Contents

Contents

Chapter 1

122° 02' W, 38° 04' N

Port Chicago, at coordinates 122° 02' W, 38° 04' N, is 30 miles northeast across San Francisco Bay from the city of San Francisco and a few miles up the Sacramento River from Carquinez Strait where, at the city of Martinez, the river in ages past has cut a gap through the low hills that circle the bay on all her flanks. Through that narrow, deep gorge the Sacramento makes her debouchment into the upper San Francisco Bay, at San Pablo Bay, carrying the mainly seasonal flow of her many tributaries that drain the Cascade Mountains in the north, the massive Sierra Nevada Range east along the Nevada border and, from the south, the California Central Valley from as far as the Tehachapi Mountains in southern California.

Twice a day high tides from the Pacific Ocean flood through San Francisco’s Golden Gate across San Francisco Bay. At Carquinez Strait the rising tides often obstruct the diminished summer flow of the Sacramento and the pent flow of the river over millennia has formed a large tidal bay, Suisun Bay, that arches to the north just east of Carquinez Strait. Port Chicago, on the southern shore of Suisun Bay, is within the North Temperate Zone—north of the Tropic of Cancer and south of the Arctic Circle. At latitude 38' N, Port Chicago is slightly more than one-third the distance from the equator to the North Pole.

Port Chicago and most of coastal California enjoy the Mediterranean type climate, mild wet winters and dry summers with sunny days predominant throughout the year. California’s Mediterranean climate results from a combination of atmospheric and oceanic conditions on the windward Pacific Ocean. In summer a vast, cool semipermanent high pressure atmospheric cell usually develops between California and Hawaii, the Pacific High. Low pressure summer storms from the western Pacific move against the stable periphery of the Pacific High and are deflected north from the California coast to the coasts of Oregon, Washington and British Columbia. During normal winters the Pacific High weakens and moves south toward the equator and winter storms flow without obstruction to the California coast from the breeding ground of North American winter weather among Alaska's western Aleutian Islands.

Summers in California are dry and winters are wet, with the heaviest 74 inches annual rainfall in the northern part of the state and 9 inches at the Mexican border. San Francisco on the coast usually receives 22 inches of rain a year; 30 miles inland at Port Chicago 16 inches are typical, of which a scant 0.03 inch is the average precipitation for July. The Port Chicago climate year-round is mild. July high temperatures range between 75° F and 92° F; rarely are July nighttime temperatures less than 55°.

At Port Chicago during July surface winds blow typically from the west-northwest with an average speed of 8 miles per hour; the annual average wind speed is 7.5 miles per hour. Port Chicago, however, has a pattern of regular afternoon and early evening wind that is not characteristic of most of the San Francisco Bay area; Carquinez Strait and the Sacramento River course provide a windgap through the hills that surround the bay and the system is a major feeder of cool coastal air to the summer-heated central valleys to the east. Afternoon summer winds through Carquinez Strait, across Suisun Bay and up the course of the Sacramento are most intense in mid- to late afternoon but diminish an hour or two after sunset when the hot, rising air mass above the solar-heated interior valleys begins to cool. These were the conditions at 10:30 PM the evening of July 17, 1944 when a massive explosion at the Port Chicago Naval Magazine changed the course of history.

The Mediterranean climate of coastal California is so mild and carefree, and food is so plentiful that most people who are born there and those who have moved there stay there, and that has been true for 10,000 years. California was home to the largest and most varied concentration of American Indians north of Mexico. The immigration of Asian Mongoloid peoples to North America from the northeast of the Asian continent occurred at the beginning of the present Holocene Epoch 10,000 years ago when most of the North American Continent south to the present border between Canada and the United States was covered by a massive ice cap that extended from the west to the east coast. The present western area of Alaska and the northeast Asian continent were, however, ice free. The northern continental ice sheets, as much as two miles thick, held so much of Earth's water that the world's oceans were then 360 feet lower than today; the Bering Sea was dry and provided a land bridge that joined the Asian continent from the present East Cape, Siberia, to Alaska on the North American continent.

As Earth's climate warmed the ice caps melted, sea levels rose to their present depths and the land bridge was submerged, but when the Bering Sea was dry, or much lower and offered a chain of small islands now submerged, Asiatic peoples followed the rising summer sun to the east. Probably not much different from the motives of men who in historic time have left a homeland to cross the seas, those early Asiatic immigrants were at least curious to discover what lay beyond the horizon and, if modern times are a guide to Man's behavior, we can speculate they were seeking new opportunities in a new land or had been compelled to their journey by violence and oppression, environmental or other hardships of their homeland. The first men and women who made that expedition were followed by many thousands more who had learned of the new land from those who returned, for it was their descendants who during thousands of years eventually first dominantly populated North, Central and South America. Immigration from the Asian continent to North America across the Bering Sea land bridge was, however, probably not the only population source of the Americas; increasing paleogenetic evidence indicates the likelihood of trans-Atlantic and trans-Pacific migrations that also occurred some 10,000 to 12,000 years ago.

From the northwest area of contemporary Alaska where those early wayfarers reached North America at the end of the last Ice Age most followed a warming inland valley south that cut a gap between the great ice sheets that covered most of Canada. At the southern limit of the glacial ice in the present area of Montana many of the immigrants moved southeastward into the American plains and from there those wanderers originated the many scattered Indian tribes who occupied North America east of California at the time the first "documented" Europeans arrived in the late 1400s. Others of those first Asian immigrants who arrived at the terminus of the glacial ice turned west following ancient rivers that flowed from the melting ice cap to the Pacific Ocean and eventually in their ramblings they found California.

Cooler and greener then, those early immigrants shared the land with the last of the mammoth and mastodon, saber tooth cats, Pleistocene horses, American camels and many other species from that time that are now extinct, but man survived and thrived in that luxuriance of nature. As the climate of the Northern Hemisphere warmed, the great northern ice covers melted, the oceans rose, North America was separated by the Bering Sea from Asia and the easier movement of prehistoric immigrants by land from Asia to North America slowed but did not cease; as Eskimos sometimes do today, those early immigrants crossed the frozen winter sea ice and during the summer thaw paddled skin boats between the continents.

By A.D. 1500, 350,000 Indians occupied California's 156,000 square miles, mostly in the coastal areas and in the central valleys where they were established in small "tribelets" usually of no more than 100-130 persons, extended family who spoke one of the 135 Indian dialects then current in California derived from two linguistic stocks (Penutian and Hokan) that together subsumed at least 35 distinctive languages. When the first Spanish explorer to reach coastal California above the Baja California Peninsula, Juan Rodríguez Cabrillo, sailed into what is now San Diego harbor on September 28, 1542 the native population throughout California were established in those small extended family groups, each with a delineated territory of usually not more than 100 square miles that in the early Spanish idiom were called *rancherías*. Anthropologists and ethnographers believe that very little communication and interaction was present even between neighboring *rancherías*, but the remarkable language skills, especially of the male California Indians, when the Europeans first arrived seems to suggest at least that the young men, as might reasonably be expected, traveled far and wide in search of trade and adventure, which necessitated the development of a considerable aptitude to learn the languages and dialects of the regions they visited. Although the adult men, women and children usually didn't travel beyond the perimeter of their individual *rancherías,* it's difficult to be persuaded that the young men didn't ramble and interact with their neighbors where they lived, and even more distantly.

The California Indians were a Stone Age people without the wheel, beasts of burden or written records; they obtained their food hunting small game, occasionally deer and elk, fishing, gathering clams, mussels and abalone along the Pacific shore, and the acorns, seeds, grains, berries, edible plants and roots that Nature provided in abundance. Agriculture was mostly unknown to the California Indians, except the cultivation of tobacco, and in the southeastern region along the Colorado River the Yuma and Mojave practiced some flood plain agriculture in manner similar to the river valley Indian cultures of Arizona and New Mexico. The Indians throughout California were superb basket-makers, arguably the best in the world, but they made no pottery except, again, in the southeastern region where the craft was somewhat known by cultural diffusion from the Indians of the Southwest.

The southern shore of Suisun Bay at Port Chicago and the central portion of what is now Contra Costa County in an area roughly centered on Mt. Diablo was home to the *rancherías* of the Bay Miwok Indians. The Bay Miwok were associated ethnologically with the larger language groupings of the Eastern Miwok who inhabited the delta of the Sacramento and San Joaquín Rivers east of Port Chicago and within a 200 miles wide band of territory across California's Central Valley, into the foothills and lower levels of the Sierra Nevada Range; however, the Bay Miwok in the Port Chicago area may have spoken a unique language, Saclan.

The north shore of Suisun Bay and a 100 miles wide territory northward for two hundred miles was home to the Patwin Indian grouping, whose language was linguistically related to the language of the Miwok but was a fundamentally different language; the Patwin tribelet in the vicinity of the present city of Vallejo were known during the Spanish period as the Suisun and were considerably more numerous than the Bay Miwok. Immediately to the west of the southern shore of Suisun Bay along the southern rim of Carquinez Strait, the Carquines Indians were at home on their *rancherías*. The Carquines tribelet were the northern limit of the 40 tribelet groups of the Coastanoan Indians, known more often today as the Ohlone, who inhabited the eastern shore of San Francisco Bay as far south as the present city of San Jose, the western shore of San Francisco Bay, the San Francisco Peninsula and peninsula seacoast south to Monterey Bay. The language and dialects of the Coastanoan were fundamentally different from the language of the Bay Miwok, which is surprising considering that the entire area inhabited by those several tribelets was not much more than 1,000 square miles.

No archeological explorations have been made at Port Chicago or anywhere on the southern shore of Suisun Bay to investigate the Bay Miwok Indian past but the district was an area of Indian habitation during the previous 3,000 to 4,000 years. The impermanent, encampment characteristics of that habitation would have left few remnants to be discovered today, but before 20th century development claimed and removed them more than 400 ancient shell mounds and kitchen middens of the Carquines and other of the Coastanoan Indians were located on the eastern shore of San Francisco Bay. Although ethnographers believe there was little communication between even neighboring Indian tribelets it’s very difficult to imagine that the Bay Miwok would not have often taken that one day walk from Suisun Bay to San Francisco Bay where those massive shell mounds were located. That one day walk would have offered the Bay Miwok an entirely different variety of food from the salt waters of San Francisco Bay and San Francisco Bay shoreline than what was available from the brackish waters of Suisun Bay.

The largest of those shell mounds, the Ellis Landing shell mound at the city of Richmond south of Carquinez Strait, was a one day walk for the Bay Miwok living among the hills and plains on the southern shore of Suisun Bay. The Ellis Landing mound was 450 feet long, 250 feet wide, 30 feet high and contained an estimated 1,260,000 cubic feet of broken shells, principally clam and mussel varieties intermixed with oyster, cockle and abalone. The mound had been accumulated during a period of human habitation in the vicinity of that site during 3,000 to 4,000 years. A two-week excavation conducted by University of California archeologists in 1907, which explored less than one-tenth of the mound, discovered 265 manmade artifacts, 126 human skeletons and fifteen house pits. The bulk of the Ellis Landing mound was then removed to permit development of the Richmond city shoreline.

Destruction of the Ellis Landing shell mound immediately after the partial 1907 exploration of that site determined that essentially all that might have been known from archeological findings about the Carquines tribelet of the Coastanoan and their neighbors the Bay Miwok, was lost but the earliest Spanish explorers who trekked over-land to the area from Monterey Bay and those who later reached San Francisco Bay by ship have provided some sketchy but important information about those Indians in diaries kept by literate members of those expeditions.

By about A.D.1300 the many distinct California Indian groups had established themselves in permanent locations where white men found them in historic times, 200 years later. The human population density per square mile of California in 1500 was at least four times that of any other aboriginal population in what is now the United States, but the year 1532, followed by the Spanish and then American settlement of California, marks the beginning of the end for most of that population.

The Spanish *conquístador* Hernán Cortés had sailed with Diego Velázquez to conquer Cuba in 1511. From Cuba, Cortés established a Spanish colony in Mexico (New Spain), where he arrived in 1519; he then burned his ships to effectively commit his entire military force to the conquest of the Aztec Empire, which was complete by 1521. The next year he established a shipyard at Zacatula on the Pacific coast of Mexico. Ship construction without local supplies and the accustomed European materials necessary to rig and outfit a ship was extra-ordinarily difficult but the first vessels were completed in 1526.

In 1532 Cortés sent two small ships in exploration northwest up the coast; neither of the two returned. The following year he dispatched the expedition that discovered Lower California, the Baja California Peninsula. The original commander of the expedition, Diego Becerra, was killed in a mutiny at sea and Baja California was discovered by the pilot of the expedition, Fortún Jiménez, who had led the mutineers. Jiménez landed at the bay known today as La Paz—an irony of identification since Jiménez and twenty of the Spanish expeditionary force and an unknown number of La Paz Indians were killed there in battle.

The traditional honor for the European discovery of Upper California is given to Juan Rodríguez Cabrillo, who first sighted that land from the sea in 1542. Cabrillo had sailed from Navidad on the Mexican west coast in two small, poorly provisioned ships manned by conscripts on June 27, 1542. On September 28 he entered the harbor of San Diego, which he named San Miguel. The Cabrillo expedition then made a series of anchorages in southern California above San Diego at Catalina Island, on the coast opposite Catalina at San Pedro, northward of the Palos Verdes Peninsula at Santa Monica and Ventura, at several points along the Santa Barbara Channel coast, and on the Channel Island of San Miguel. As winter closed upon the California coast, Cabrillo rounded Point Conception but strong northwest winds prohibited progress north of Point Conception.

The expedition returned south into the Santa Barbara Channel and wintered at lee anchorage at San Miguel Island where, a month earlier, Cabrillo had broken an arm in an accident and where he died in January 1543, apparently consequent to an infection of that injury. Cabrillo’s dying wish was that the crew would continue north under the leadership of the expedition’s chief pilot Bartolomé Ferrolo when better weather would permit. The expedition did continue north up the west coast and when the ships turned back on March 1, 1543 the crews had reached a point off the coast of southern Oregon but had not landed north of Point Conception.

In April 1543 with the crews desperately sick with scurvy and nearly starved the *San Salvador* and the *Victoria* sailed into their home port in Mexico at Navidad. The expedition had not found evidence of an ice-free Northwest Passage, the mythic Strait of Anián that hope held would connect the Atlantic and Pacific Oceans in the middle latitudes and thus permit more rapid and less arduous passage between the oceans than the perilous Straits of Magellan at the southern tip of South America. And the expedition had not noticed the inconspicuous and often fog-hidden Golden Gateway entry to San Francisco Bay.

The next landing of Europeans on the coast of Upper California was made by the English in 1579. In 1577 with a privateer's commission granted by Queen Elizabeth, Admiral Francis Drake was named by the Elizabeth to head the first English expedition to circumnavigate the world, for the principal purpose of offering challenge to the Spanish dominance of the Philippine and Molucca "Spice Islands" trade in the Indonesian Malay Archipelago. Once through Magellan's straits Drake and the *Golden Hind* turned north up the South American west coast, plundered a few Spanish Manila galleons along the Peruvian coast, and reached the Alta California coast slightly above the entry to San Francisco Bay in June 1579. There Drake made an emergency landing to careen and caulk the *Golden Hind,* a vessel of 100 tons burden carrying 30 tons of captured Spanish treasure, mostly silver.

Descriptions of the California Indians inhabitant in the locality where the *Golden Hind* put in at the present Drake's Bay, recorded by members of the crew, have permitted anthropologists to identify those Indians as the Coast Miwok group whose territory included the present Drake's Bay, about forty miles north of San Francisco at the present Point Reyes National Seashore, and Bodega Bay twenty miles further north. The point on the coast where Drake put in is sometimes disputed, but his journal reports he landed in a bay marked by "white bancks and cliffes," which characterize the coast at Drake's Bay but not further north, where some scholars advocate the *Golden Hind* put in. Those beautiful shoreline cliffs at Drake's Bay, brilliant on a clear sunny day, reminded Drake of the white cliffs of Dover on the southeast English coast opposite the French port of Calais. Drake named California, *Nova Albion*—New England—and claimed the land in the name of Queen Elizabeth.

The tribelets on their *rancherías* in the area of San Francisco Bay and the California Central Valley were usually no more than a group of neighboring villages, usually with a central village in a permanent location, which was the political and social center of the tribelet, and three or four smaller *rancherías* that were periodically moved a short distance when the homes had deteriorated and the location was degraded by the unsanitary consequences of temporary habitation. Although from a somewhat unsympathetic European cultural view-point, one early observer of the coastal California Indians reported, “When the collection of bones and other food refuse thrown on the floor became too offensive, and the fleas and other vermin too numerous even for the Indians, they merely set fire to the house and built a new one in another spot.” There is, however, a tendency in the opposite direction of cultural evaluation to idealize the “Noble Savage.” Between the extremes, which no doubt did exist, lay the daily existence of the average Indian family in widely varying circumstances of personal and social accomplishment.

The anthropologist Alfred L. Kroeber wrote, “The Californians were shorter and smaller skulled than the Indians of the eastern shore of the continent, but were superior in these respects to the Aztecs and the Mayas. They had not the copper complexion, the aquiline features, nor the proud bearing usually associated with the American Indian, but were flat-nosed and broad-faced, with an apathetic carriage.” The apathetic carriage and lack of proud bearing that Kroeber found among the California Indians at the beginning of the 20th century is not, however, reported by the earliest Spanish explorers who found the Indians energetic, with great endurance as dancers and runners, proud and self-confident individually and culturally. When Kroeber met the California Indians their culture had been destroyed and the original population of self-sufficient people had been reduced to beggary by mass murder, disease, starvation, alcohol, and all the consequent mental and emotional hurts and harms that those conditions had produced, in manner very much akin to the circumstances of African-American enslavement before the Civil War and the social, political and economic consequences to African-Americans during more than 100 years following the Civil War—and with all the same ethnocentric prejudices in place necessary to deny the humanity of the subjugated and the enslaved.

However, considering that the Indians of California in their primitive state were essentially a Stone Age people, absolutely without education in our sense, there is more logic to be amazed at their capacity to acquire the various branches of knowledge than to be shocked by their lack of it. Those of the Indians who came in contact with the first Spanish learned with astounding rapidity to speak and pronounce Spanish clearly and accurately. Contemporary Europeans and later linguists who studied the languages and dialects of the California aboriginals, when those languages were still current, found the languages and dialects spoken among the Indians to provide a syntax and vocabulary as complex as any Indo-European language, whether employed by the Indians in physical description, conceptual elaboration, or the abstract metaphysics of ontology and cosmology.

The Indian catechumens under instruction in the Spanish Catholic missions easily learned to read music and to sing concordantly, and in the church chorals they learned to intone the Latin with impeccable accuracy. Their ability to acquire mechanical arts is witnessed in the magnificent remains of the mission church buildings from the Spanish period that were erected by Indian workmen under the direction of the padres. The Franciscan missions provided industrial schools in which the Indians rapidly acquired skills in carpentry, weaving and agriculture. Without experience of any domesticated animal but the dog prior to the arrival of the Spanish, the California Indians quickly became herdsman of whom it was said there were none better in the world. However, the often perverse and vicious treatment the Indians received when in the care of the padres in the mission settings must be remembered as a brutality comparable to any that one segment of humanity has imposed upon another.

Coast Miwok in the area of Drake’s Bay, the Lake Miwok in the area of Clear Lake and the Eastern Miwok, including the Bay Miwok in the Port Chicago area, were Penutian-speaking peoples whose languages are now extinct. The Penutian grouping of American Indian languages was spoken in various areas along the West Coast from British Columbia to central California and also, inexplicably, by the Zuñi of central New Mexico, 1,000 miles to the east. Penutian, one of two language phylums among the aboriginal California Indians, consisted of four recognized language families, comprehending 23 distinct languages of which seven were spoken by the Miwok at Drake's Bay, Clear Lake and from the southern shore of Suisun Bay (Saclan language?) across the California Central Valley and into the Sierra Nevada Range; also among the Penutian-speaking California Indians were the Bay Miwok's neighbors on the north shore of Suisun Bay, the Patwin, and the Maidu, Wintun, and Yokut. Penutian-speakers apparently came from the north. Except present-day New York City, the diversity of languages spoken among the California Indians vastly surpassed the diversity of any other world area of comparable size.

By 1848 when the United States acquired California the estimated 350,000 native population of California del Norte (Upper or Alta California, above the Baja Peninsula) present in 1500 had been reduced to 100,000; between 1849 and 1855 the massive influx of fortune-seekers from every part of the world during the California Gold Rush resulted in the death of 50,000 California Indians by disease, ruthless massacre of entire native settlements, the willful destruction of the Indians' food stores and occupation of their acorn-gathering grounds. By 1880, 20,385 survived. In 1910 the anthropologist Kroeber estimated a native California Indian population of 16,000. The United States Bureau of Indian Affairs in 1933 put the number at 21,977. By 1970 the U.S. census had increased the number to 91,018, on and off reservations, and today some 150,000 California Indians are counted, although their once clear ancestral identity among the state's ethno-genic groups is essentially lost.

In September 1768 Gaspar de Portolá, a Spanish officer of the Catalán Dragoons and newly commissioned governor of Spanish California with some 100 foot soldiers and cavalry trekked 500 miles north from San Diego and reached Monterey Bay on the coast 60 miles south of San Francisco Bay. The following year on November 4, 1769 Portolá and party were the first Europeans to see San Francisco Bay, from the hills south of the present city of San Jose. Following the march from Monterey Bay to the southern fringe of San Francisco Bay Portolá returned to Monterey Bay and then to San Diego.

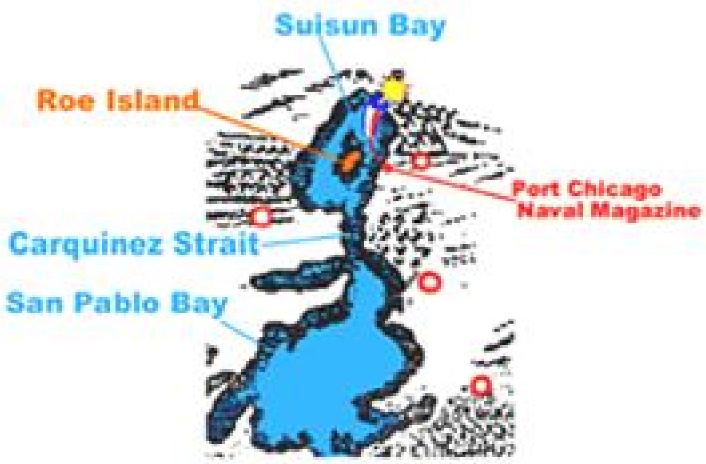
By March 1772 the existence of San Francisco Bay was established but the narrow and often fog-shrouded Golden Gateway into the bay from the Pacific Ocean had not yet been entered by any reported European ship. By March 1772 Gaspar de Portolá had been ordered out of California to Mexico. His successor, Pedro Fages, again from Monterey Bay, mounted an expedition up the east shore of San Francisco Bay, known as the Contra Costa, in search of a land route to Point Reyes on the seacoast north of San Francisco Bay. Fages, his small military troop and the diarist of the expedition, Fray Juan Crespí, moved up the Contra Costa past the present site of Berkeley. From Berkeley the Golden Gate was clearly visible across the bay and Fages charted the position of that single entryway into the bay, which information on August 5, 1775 would guide the first recorded European ship to her entry into San Francisco Bay, the Spanish ship-of-war *San Carlos*.

Along the way up the Contra Costa the Fages party encountered without hostility the inhabitants of the Coastanoan Indian tribelets and *rancherías* of that area, descriptions of whom Fray Crespí sketched among his diaries. Although the Fages party were the first known Europeans to pass upon the eastern shore of San Francisco Bay Fray Crespí, apparently with some humor, noted that among those Contra Costa Indians were men who, with their black beards and taller stature, much resembled the Spaniards. Since beards are unknown except among a few of the California Indians in the far north, who exhibit some scant facial hair, it is intriguing to imagine the romantic conte of a Spanish ship that entered upon the bay but never returned to home port, or the salvaged members of a shipwrecked crew a generation or more earlier who had found their way to the Eden-like *rancherías* of the Contra Costa where they were, no doubt, received as minor gods until their human nature showed the contrary to be true.

North of Berkeley, the Fages expedition reached the mouth of Carquinez Strait through which the Sacramento River enters San Francisco Bay. The expedition party were the first Europeans to see the upper reach of San Francisco Bay, known as San Pablo Bay on the north shore of which lies the present city of Vallejo opposite the island that would be the first west coast naval base of the United States, Mare

Island. In 1942 the Port Chicago Naval Magazine would be established as an administrative annex of the Mare Island Naval Yard and Magazine.

From Carquinez Strait, named by derivation from Karkin, the name of an Indian village in that region at the present site of the town of Martinez, the Fages party moved across the hills and gullies of the rough terrain on the south rim of the strait to the southern shore of Suisun Bay. On the opposite rim, Fray Crespí noted the presence of several Indian villages, which would later be recognized as the southern most population of the Patwin, separated from their southern neighbors, the Carquines and Bay Miwok, by the Sacramento River.



Detail. "Map of the great harbor of San Francisco, examined and sketched by Brevet Frigate-ensign of the Royal Navy José de Cañizares, first sailing master, of the Department of San Blas, [the said harbor being] situated on the west coast of California, north of the equator, on the Pacific Ocean, in 37 degrees 44 minutes north latitude engraved [on wood] by Manuel Villavicencio in the year 1781."

○ = Rancherias of friendly Indians trading in tobacco and fish.

[Colors added]

Captain Fages is credited with the European discovery of Suisun Bay and as the party moved along the grassy plain of the southern shore, at the site of the present Port Chicago Naval Magazine National Memorial, they passed "five large villages of very mild heathen . . . [where the party] were well received . . . and presented with some of their wild food." From Suisun Bay the Fages expedition followed the southern bank of the Sacramento a few miles past the present site of Port Chicago to the confluence of the San Joaquín River with the Sacramento, at which point Fages determined the river and the river delta obstructed a direct land route to the north and the party returned to Monterey in some duress, lacking adequate provisions, across what is now central Contra Costa County.

The *San Carlos* had entered first upon San Francisco Bay on August 5, 1775, but San Francisco Bay and most of the west coast were then essentially uncharted and remained as unaffected by European culture as during the previous millennia. On the east coast an extensive rural and urban mechanized, industrial civilization and culture had developed, principally of the British and French variety. The British colonies eleven months later would declare themselves independent of the British crown. In April 1776 the Spanish naval captain José de Ortega sailed into San Francisco Bay and conducted the first extensive exploration of the bay along the area of the present city of San Francisco and somewhat south, and on the opposite shore along the Contra Costa north to Carquinez Strait.

The records of the Ortega survey include the most complete early descriptions of the Coastanoan and Bay Miwok Indians, which were followed by the picturesque observations of the diarist of the 1810 Moraga expedition that reconnoitered much of the surrounding territory. Other diarists in the early 1800s recorded perceptive accounts of the villages, inhabitants and the culture of the Carquinez Strait and Suisun Bay Indians of both shores, but there is sadly very little but anecdotal sketches of that lost culture that lived so long where a National Memorial now commemorates those men later killed and injured in the Port Chicago explosion.

Before their ruin, the Carquines Indians, the Patwins and the subgroup of the Eastern Miwok, the Bay Miwok of the Port Chicago area were strong and brawny, intelligent and a joyful race who loved and sang and danced and worked and died there, and it is fitting that those strong men who loved and sang and danced and worked and died in the Port Chicago explosion were preceded in that place by men of equal stature and character. Of the Indians in the immediate Suisun Bay area we know only Chief Solano of the Suisun (the Patwin), portrayed in the diary of the 1840s Mexican-born California pioneer General Mariano Vallejo as “A fine figure of a man, six feet, seven inches in height and broad in proportion.” Solano County on the northern shore of Suisun Bay was named in honor and recognition of Chief Solano. The Port Chicago Naval Magazine National Memorial on the southern shore of Suisun Bay was established by the United States Congress to commemorate the sacrifice and contribution to the nation’s World War II efforts of those 320 strong and dedicated civilian men, the officers and enlisted men of the United States Navy, Marine Corps and Coast Guard who died July 17, 1944 in the massive Port Chicago explosion.

Photographs and illustrations credits.

Detail, “Map of the great harbor of San Francisco, 1781.” Source: *The First Spanish Entry Into San Francisco Bay, 1775, etc.*, John Galvin, editor. San Francisco: John Howell Books, 1971; p. 104.

The provenance of this map is not credited by this source. John Howell Books ceased business many years ago and the editor, John Galvin, cannot be otherwise presently identified. The map is reproduced here by the courtesy of Ms. Guadalupe Martínez, Southwest Collections, New Mexico State Library, Santa Fe.

Chapter 2

Paul Masters, Captain Archie Kuntze and Madame Sun

On 7 December 1941 a surprise attack by the Empire of Japan on the U.S. naval base at Pearl Harbor, Island of Oahu, Hawaii, and other Oahu military bases sank or heavily damaged 21 ships, destroyed or damaged 323 aircraft, killed 2,388 military personnel and civilians and wounded 1,178. On 8 December in Washington, D.C. President Franklin Delano Roosevelt asked Congress "to declare that since the unprovoked and dastardly attack by Japan on Sunday, December seventh, a state of war has existed between the United States and the Japanese Empire."

At 4:10 p.m. E.S.T., 8 December 1941 the Senate and House of Representatives of the United States of America in Congress assembled resolved "That the state of war between the United States and the Imperial Government of Japan which has thus been thrust upon the United States is hereby formally declared; and the President is hereby authorized and directed to employ the entire naval and military forces of the United States and the resources of the Government to carry on war against the Imperial Government of Japan; and, to bring the conflict to a successful termination, all of the resources of the country are hereby pledged by the Congress of the United States."

On the morning of 11 December the Government of Germany declared war against the United States. President Roosevelt immediately requested the Congress to recognize a state of war between the United States and Germany. That day the Senate and House of Representatives of the United States of America in Congress assembled jointly resolved "that the state of war between the United States and the Government of

Germany which has thus been thrust upon the United States is hereby formally declared."

Paul Masters was born 30 December1908 in Stockdale, Ohio, the son of William and Stella Masters; Paul's parents were well educated and several of their relatives were associated with colleges in the Midwest. By the late 1920s the family, including Paul's brothers Omer and Miles, had moved to New Mexico where Paul attended and graduated from the University of New Mexico, Albuquerque. In 1936 Paul and his first wife Charlie were married; they had met while students together in a writing class at the university. Paul's major had been history and Charlie had studied drama and creative writing. Paul and Charlie then moved to New York City where Paul took a Masters Degree in Public Health at Columbia University. Subsequently Paul enlisted in the U.S. Army and completed Officers Candidate School, but he was very quickly discharged from the Army because of asthma.

In a few pages of sketchy autobiographical notes that describe her life in Santa Fe and Los Alamos during the war, written in 1948 before she died of bone marrow cancer, Charlie accounts that before her arrival at Los Alamos she had developed what she described as "my Secret Service connection and my availability to State Department files." Family members say that Charlie had been trained in counterespionage, but no details of the circumstances of her training are known. In the summer of 1943 the couple were in Chicago, Illinois. Five years later Charlie wrote in her autobiographical notes, "My orders, which had reached me in Chicago after months of anxious waiting on my Civil Service application, were short and non-enigmatic in the extreme. They merely told me to report to 109 E. Palace Ave. in Santa Fe on such-and-such a day." She wrote that she first arrived in Santa Fe "on that fall day in 1943." Presumably Paul was with her or soon followed, but Paul is not mentioned in that brief account of her life during the war years.

Charlie's 1948 autobiographical notes reveal that she was not the counterespionage super-sleuth that the reader hopes those pages would reveal. She was afraid of heights, which she called claustrophobia rather than acrophobia, and she was suspicious of all men who didn't dress respectably, and especially she was suspicious of men who didn't dress respectably and who also appeared not to have shaven for several days or showed more advanced progress in the development of beards or moustaches. Those beards and moustaches Charlie determined were probably theatrical props donned to disguise the faces of the foreign agents she observed lurking in Santa Fe and subsequently on the Hill at Los Alamos. Sloppily dressed men with beards and moustaches would have marked as foreign agents many of the young scientists at Los Alamos.

Paul and Charlie Masters examining pot shards, Los Alamos, NM, 1944

In her first days sleuthing around the Plaza area of Santa Fe and in the bar of La Fonda hotel she found cause to report to Los Alamos security a suspicious, slouching person in ill-fitting disreputable clothing who proved to be Los Alamos Laboratories Director J. Robert Oppenheimer. It seems that Charlie Masters soon found her apt calling at Los Alamos as a substitute teacher in the school established for the children of residents on the Hill.

Paul Masters had been discharged from the Army after a brief tour in the humid summer climate of the American southern states, which had provoked his asthma. After his Army discharge he went to Santa Fe to be with Charlie, and he was subsequently employed in the photographic laboratory at Los Alamos. The date Paul began employment at Los Alamos cannot be established without the onus of making a Freedom of Information Act request to Los Alamos to obtain that information, but Paul certainly had been hired at Los Alamos before the beginning of summer 1944; he continued in Los Alamos employment until the end of the war in the Pacific.

For many years before he went to Los Alamos Paul had been an avid amateur photographer, which meant in those days that he had developed skills in photographic darkroom processes. He was imaginative in that work and generally a man with a bright and inquisitive mind who quickly learned the often complex sciences including chemistry and optics that distinguish a master photographic technician from one who is technically competent but not especially inventive or innovative in the work.

The nation collectively during the war urgently tried to assign persons to employment, duties and responsibilities in which their talents and experience, such as they may have had, would be most effectively utilized in the war effort. Many men, of course, had no unique experience, education or training and those men were sorted out by aptitude testing when they volunteered or were drafted into military service, and by that process all the personnel needs of the military services were filled. It was not always an efficient selection and assignment process; men who had been trained by the services for one particular rating or duty would find the duties they were assigned had no logical or practical association with the training they had received.

As example, twin brothers from Bennington, Vermont, were drafted into the Army and trained together for 5 months as motorcycle mechanics. Shortly after the 6 June 1944 Allied forces D-Day European invasion across the English Channel onto a 50-mile stretch of the French Normandy coast the brothers were posted to duty in France, where U.S. forces had few motorcycles to repair or maintain. An abrupt change of assignment found the twins, who were only 5 feet of physical stature, working together as driver and operator of one of the Army's enormously huge, heavily armored, wheeled tractor-trailer units used to retrieve and transport disabled combat tanks from battlefield areas to repair facilities and, on the way to retrieval assignments, to return repaired tanks to the front lines. The brothers' relatively small physical stature facilitated their movement within the interior wreckage of disabled tanks where entry was often necessary to disengage the tank's drive train so the massive bulk of those disabled fighting machines could be more easily winched onto the retriever trailer.

The photographic laboratories at Los Alamos were a very important component of the atomic bomb research and development work done at Los Alamos and Paul joined a team of photographic experts and technicians who were among the most accomplished in the world. New photographic equipment and techniques were developed there to record the sequence of events in the study of nuclear and bomb physics that occurred and changed so quickly that existing photographic equipment could not record the changes. There has not yet been a thorough history written of the developments in equipment and photographic techniques adapted and invented at Los Alamos during the war to advance the weapons programs, but that is itself an extraordinary story in its own ways as significant as the development of the atomic bombs, and Paul Masters was at the center of those developments.

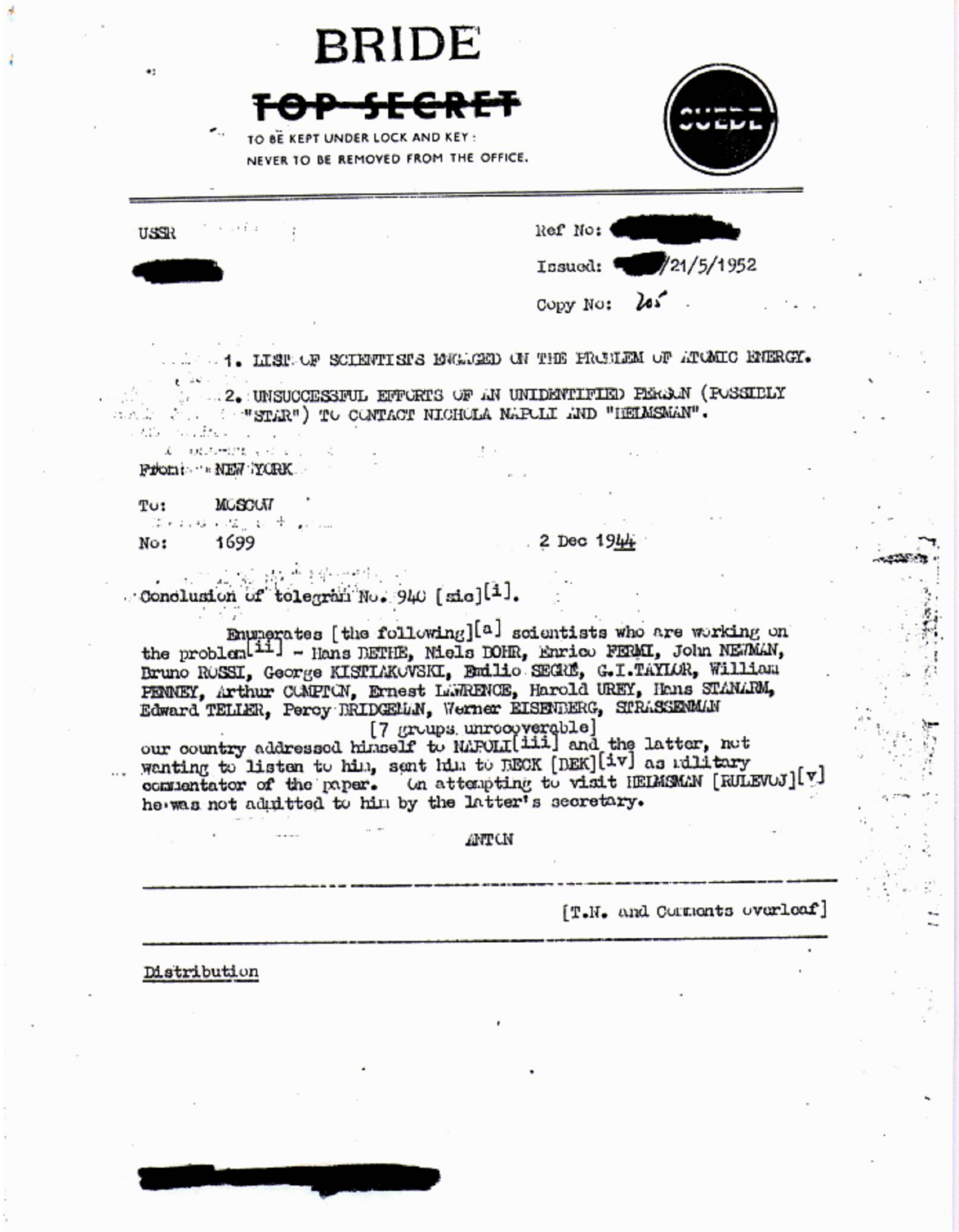
Paul and Charlie were employed at Los Alamos after the entire course of their lives to that time had been carefully reviewed by Army Intelligence inquiry and they were found to be satisfactory candidates to join the secret work at Los Alamos. Before his employment was effective Paul was required to sign a document by which, under penalty of law, he agreed not to disclose any of the secret or otherwise classified materials and information to which he would be privy in the course of his duties.

Paul was in no way a subversive of the nation’s interests nor an enemy sympathizer, and he certainly would never have disclosed atomic secrets to anyone, but as a young man he was the type of guy who would take a signature cigarette ashtray from a hotel room where he had enjoyed a vacation stay; he collected a few souvenirs along the way to remind him of what he had done and where he had been. And so with the not-unusual propensity to abscond with a hotel or casino ashtray and other small appropriated souvenirs he did, with some frequency, take home unauthorized copies of Los Alamos documents during the war.

A part of Paul’s duties in the photo laboratories at Los Alamos was to operate a large blueprint-like copying machine to make copies of all kinds of non-classified, confidential and secret documents too large for the then usual mimeograph copiers. During the war years Paul would sometimes make extra, unauthorized copies of documents as souvenirs. He would fold them neatly and carry them home in his shirt breast pocket. All but one of the known documents that Paul purloined from Los Alamos were copies of organizational charts that showed who was assigned to the different Los Alamos work divisions at various times during the war; laboratory personnel were shifted around between the divisions as the bomb work progressed and different talents and scientific aptitudes were needed by different groups.

Every known unauthorized document copy that Paul made and removed from Los Alamos represented a very serious violation of the security oath he had signed when he was employed by the Manhattan Project; the actual employer of Los Alamos personnel was, and is, the University of California. At that time any information that named the scientists at Los Alamos and specified the work areas in which they were employed would have been of enormous benefit to foreign espionage intelligence gatherers, spies, interested in the details of the nation’s atomic bomb program because the scientific expertise and research interests of those men were known internationally before the war, and knowledge of their work area assignments at Los Alamos would have provided useful information about the directions in which the work at Los Alamos was proceeding.

The Russians, notably, sought to obtain as many of the scientific and technical details of the atomic bombs in development at Los Alamos as possible in order that Russia could develop a workable atomic bomb at the earliest possible date; if the Russians knew who was working at Los Alamos, and the work they were assigned to do, a great deal could be known about the progress of the work and toward which persons Russian espionage efforts should be directed to obtain very specific information about the bombs, their science and technology. Russian interest in learning who was participant at Los Alamos is frequently noticed among the Venona files, as for example the New York to Moscow telegram No. 1699 of 2 December 1944:



BRIDE

~~TOP SECRET~~

SUEDE

TO BE KEPT UNDER LOCK AND KEY:
NEVER TO BE REMOVED FROM THE OFFICE.

USSR

[redacted]

Ref No: [redacted]

Issued: [redacted]/21/5/1952

Copy No: 205

1. LIST OF SCIENTISTS ENGAGED ON THE PROBLEM OF ATOMIC ENERGY.

2. UNSUCCESSFUL EFFORTS OF AN UNIDENTIFIED PERSON (POSSIBLY "STAR") TO CONTACT NICHOLA NAPOLI AND "HELMSMAN".

From: NEW YORK

To: MOSCOW

No: 1699 2 Dec 1944

Conclusion of telegram No. 940 [sic][i].

Enumerates [the following][a] scientists who are working on the problem[ii] - Hans BETHE, Niels BOHR, Enrico FERMI, John NEWMAN, Bruno ROSSI, George KISTIAKOVSKI, Emilio SEGRÉ, G.I.TAYLOR, William PENNEY, Arthur COMPTON, Ernest LAWRENCE, Harold UREY, Hans STANARM, Edward TELLER, Percy BRIDGEMAN, Werner EISENBERG, STRASSENMAN

[7 groups unrecoverable]

our country addressed himself to NAPOLI[iii] and the latter, not wanting to listen to him, sent him to BECK [DEK][iv] as military commentator of the paper. On attempting to visit HELMSMAN [RULEVOJ][v] he was not admitted to him by the latter's secretary.

ANTON

[T.N. and Comments overleaf]

Distribution

[redacted]

This Venona document names scientists working on "the problem" including those prominent in the Manhattan Project: Hans Bethe, Niels Bohr, Enrico Fermi, John von Neumann, Bruno Rossi, George Kistiakowsky, Emilio Segré, Geoffrey I. Taylor, William Penney, Arthur Compton, Ernest Lawrence, Harold Urey, Edward Teller, Percy Bridgeman, and others. The organizational charts that Paul Masters removed from Los Alamos would have been of enormous assistance to Russian espionage, but that was not Paul's intent nor purpose. The documents he copied were souvenirs to remind him of what he had done and where he had been.

Of even more significance, Paul made an unauthorized copy of the document "History of 10,000 ton gadget" and removed that copy from Los Alamos in his shirt pocket. The "History" that Paul copied and removed to his home is a comprehensive mathematical model of the progression and physical effects that were anticipated to be the consequences of the test detonation of an atomic bomb that would be, some months after the "History" was composed, conducted in the New Mexico desert at Trinity site, 16 July 1945. There is a great deal of very precise information that can be learned from this document about the specific design parameters and technology of the bomb described by this document and about the physical and military effects that would result from combat use of the bomb. By the date the "History" was composed any thoughtful person in possession of that document would have recognized that the Manhattan Project had evidently complete confidence that an atomic bomb of energy yield equal to the detonation of 10,000 tons of TNT was not only feasible but was anticipated to be tested and combat ready in the near term.

When Paul Masters removed the "History of 10,000 ton gadget" from Los Alamos he knew in a general way that the document was more historically significant than the laboratory organizational charts he had previously taken from the laboratory and that he would, thereafter, continue to add to his collection as the war progressed. This particular document, folded as it had been to fit his shirt pocket, he put in a white No. 10 letter envelope that he cached in a cardboard box in the garage of his home where, with a variety of insignificant photographic dark-room paraphernalia and a selection of unused mid-1940s photographic chemicals and enlarging papers, it remained 35 years until he moved with his second wife, Louise, to El Castillo retirement apartments in Santa Fe.

When Paul and Louise moved into El Castillo, Paul cleared out their former home, and the garage, and donated those possessions they no longer wanted or needed to the church that Louise attended, Santa Fe's Christ Evangelical Lutheran Church, to be sold at the church's spring 1980 rummage sale. Among those donations was the World War II cardboard packing box that held the 1940s vintage photographic darkroom paraphernalia, chemicals and enlarging papers, an Army Air Forces 35 mm educational film strip entitled "The Properties of Photographic Lenses" in its original 1943 canister, and the "History of 10,000 ton gadget."

On that lovely spring Santa Fe Saturday morning when the church opened the doors of its parish hall to admit those who would rummage through the discards and junk offered for sale there I had been an amateur and sometimes professional photographer nearly 30 years, and in the immediate post-World War II years as an American boy in Paris I had spent many pleasant Sunday afternoons after dreary sermons in the American Church, located on the Quai d'Orsay, rummaging through the vast treasure trove of the Paris flea market. I had become an accomplished rummager and in later years I took particular interest in the discovery of out-of-date photo supplies with which I could make photographic prints of peculiar character and excellence.

As it happened that morning, my 10-year old son Carlo had taken a long, hot shower before breakfast and he had fainted after some 20 minutes in the shower, had fallen out of the shower stall and had struck his head a glancing blow on the toilet bowl, a sound that I detected from the living room as an alarming dull thud. He didn't seem to be seriously injured, and he wasn't, but as a precaution I took him nearby to the hospital emergency room for examination; on the way home from the hospital I passed the church and saw a rummage sale in progress. The emergency room physician had instructed the boy to lie quietly for awhile, which he willingly did in front of the television, entertained by the Saturday morning cartoons, while I went to the rummage sale.

In the center of the parish hall under a long folding table covered with rusted gardening hand tools, broken plumbing and automobile parts, decayed paperback books, broken children's toys, old shoes, and worn-out clothing I was delighted to find a cardboard packing box that, with more old clothing and other discards, held some vintage photo materials. Those I removed and set aside to purchase and I continued to forage to the bottom of the box where I found a No. 10 letter envelope, somewhat yellowed with age, that obviously from its bulky distension had a substantially-sized folded paper within. I removed the paper from the envelope, carefully unfolded it, and I held a sheet of browning paper some 14 inches from the left to the right margin, 8.5 inches from top to bottom, and covered with columns and rows of cabalistic arithmetical notations.

The title of the document includes the term “gadget,” which I knew to have been a World War II code term that designated the Manhattan Project atomic bombs. The legend of the document that appears vertically along the left margin begins in Step 1 with the term “Detonation”; Step 8 introduces the term “Ball of fire,” which is repeated in Steps 10 and 11; and the legend includes the terms “Shock wave,” “Blast wave,” and “Radiation.” It was immediately apparent to me that the document pertained somehow to the history of the atomic bombs that had been developed during World War II at Los Alamos, 25 miles northwest from Santa Fe across the Rio Grande. As I looked quickly at the document before I purchased it that morning for $0.25, I noted that the bottom line said the ball of fire of the 10,000 ton gadget would mushroom out at 18,000 feet in typical Port Chicago fashion. I was 41 years old that spring.

It is curious in retrospect that many years before 1980, when I began my study of the Port Chicago explosion, my life had several times intersected the Port Chicago history. My home during my high school years in the mid-1950s was within the gates of the Terminal Island Federal Prison where, by the end of 1944, the 50 Navy enlisted men who were convicted of mutiny by Navy court-martial following the Port Chicago explosion had been imprisoned. In the mid-1960s while I was a student at The University of California, Berkeley, I was prosecuted by Alameda District Attorney James Frank Coakley for offenses arising from my activities in the Berkeley Free Speech Movement; Lieutenant Commander Frank Coakley, a Navy lawyer in 1944, had led the mutiny court-martial prosecution of the Port Chicago 50. At Berkeley I participated in protest demonstrations at the gates of the Port Chicago Naval Magazine, from which facility a major portion of the munitions employed by U.S. forces then foundering in the Vietnamese war were transshipped. In spring 1967 my career in investigative journalism and military history began at the Treasure Island Naval Station where the court-martial of the Port Chicago 50 had been held in 1944.

I become a newspaper reporter.

At the beginning of the year 1967 I recognized I would need an additional three units of academic credit to graduate that spring from the University of California, units that were not included in my scheduled classes. Journalism professor Pete Steffens, son of the American muckraking journalist Lincoln Steffens, assigned 3 units of independent study that required I compare the reporting of a news story of my choosing that had been reported by the local newspapers and by the national newspapers or news magazines. For reasons that I no longer definitely remember I chose for that assignment to compare local and national newspaper coverage of a very recently concluded San Francisco area Navy court-martial that had been reported by the *San Francisco Chronicle*, and nationally. Navy Captain Archie C. Kuntze had recently been convicted by court-martial convened at San Francisco's Treasure Island Naval Station of illegally importing one bolt of Thailand silk cloth into Vietnam on a military aircraft.

Probably the captain's offense seemed to me ludicrous in the context of the disasters of the then-current war in Vietnam, to which I had actively objected since my radicalization in the Free Speech Movement. The irony of Capt. Kuntze's charge and conviction of illegally importing one bolt of Thai silk into Vietnam quickly assumed preposterous proportions when I learned from newspaper accounts that prior to his court-martial Capt. Kuntze, in Saigon, had the assignment of contracting and supervising all the military construction associated with the buildup of U.S. forces in Vietnam, as well as overall responsibility for the supply of all U.S. troops in Vietnam.

News reports of the Kuntze court-martial provided the information that for a period of two years prior to his removal from those duties, one-tenth of all the world's trade shipped on all the world's oceans had passed across Capt. Kuntze's desk; in the usual circumstances of business he had 5-6 million U.S. dollars cash in his office safe to cover incidental expenses. And the Navy prosecuted him for one bolt of contraband silk? It didn't make sense, and I decided I ought to have a conversation with the captain who was then resident in the Bachelor Officers Quarters (BOQ) at Treasure Island Naval Station waiting to be separated from the Navy.

A visit by a university journalism student with an officer at the BOQ at Treasure Island might seem an occurrence generating few complications, and it would have been routine if my wardrobe and appearance had not consisted of royal blue velvet pants, a tie-dyed dress shirt, complementing necktie, bare feet in hippie sandals, long hair halfway down my back, an Indian beaded headband, and a student's beard that was less than stylishly trimmed. The Marine guard at the Treasure Island base gate for some reason mistook me for a woman and cordially addressed me as Ma'am.

However, during the years I had lived on Terminal Island during high school and early college years my father wore the uniform of the United States Public Health Service (USPHS), which except the buttons is identical with an officer's uniform of the United States Navy. He had four stripes on his sleeves, the equivalent of a Navy captain, and it was impossible to distinguish a USPHS officer in uniform from a Navy officer in uniform without close inspection of the officer's buttons or the insignia on the officer's cap and sleeves. We had family privileges at the large Terminal Island Naval Station and Shipyard nearby our home on the island. On many hot southern California sunny summer days lounging by the outdoor pool of the base Officers Club I had become well acquainted with the charming daughters of the base admirals and through those daughters I knew their mothers and fathers.

During those years on Terminal Island I had pleasant and friendly relations with many Navy admirals and their families; my brother Lieutenant (jg) Victor Conrad Vogel—a Navy flyer who was killed ten years later when his plane went down in the Mediterranean during his final approach to a carrier landing—had married an admiral's daughter.

I was acquainted with the manner in which Marine guards respected Navy officers and respected the business of Navy officers. I knew that although I would take some insult from the Marine guard at Treasure Island because of my manner of dress I would be admitted to my prearranged business with Capt. Kuntze at the BOQ.

Also reported by the news accounts of Capt. Kuntze's court-martial was the information that in Saigon the captain had a flamboyant Chinese-Vietnamese mistress who frequently used the captain's staff car—which had shockingly wide whitewall tires—and the captain's driver for prodigal shopping trips around the city, passing brazenly through the crowded streets with the car's horn and siren blaring to carve a way through the congestion of Saigon's traffic jams with U.S. and Navy command flags flying from the front fenders. It was blatant, in the sense of being both offensively loud and insistent, as well as shameless. The Office of Naval Intelligence, the Central Intelligence Agency, and the Federal Bureau of Investigation had all tried to locate this woman, Madame Sun, in order to require her testimony at the captain's court-martial, but Madame Sun had not been found.

In my first interview with Capt. Kuntze he told stories of massive corruption in the process of military construction contracting in Vietnam that had favored the Texas construction firm Brown, Root and Jones, and he alleged that President Lyndon Johnson's wife Lady Bird Johnson had been the recipient of several million dollars in illegal kickbacks by the firm for the influence her husband had provided. The Browns of Houston, Texas were, in fact, LBJ political and financial supporters for many years. Once we started talking about President and Lady Bird Johnson and the corruption Capt. Kuntze alleged governed military construction contracts in Vietnam we didn't talk much about his one bolt of contraband silk. The more important information was so surprising that when I returned home I called the *San Francisco Chronicle* Military Affairs Correspondent Charlie Howe and told him what I had been told. Charlie had reported Capt. Kuntze's court-martial for the *Chronicle*. Charlie initially explained to me that Capt. Kuntze had been court-martialled on that silly charge as the quickest and least tangled way the Joint Chiefs of Staff had devised to remove the Navy from the principal role it then held in the military buildup in Vietnam and transfer that role to the Army, which thereafter was the dominant service branch onshore.

In a meeting together a few days later Charlie challenged me to carry a concealed miniature tape recorder and a microphone, masked as a fountain pen, onto the Treasure Island base, into the BOQ and to secretly record Capt. Kuntze's statements as I walked him through a repeat of the conversation we had previously had concerning the alleged misdoings of Brown, Root and Jones, President Johnson and Lady Bird. I didn't hesitate to agree although Charlie warned of the legal consequences I would be subject to if I were discovered on the base with that concealed apparatus.

I had already run the Marine guard gauntlet of insults once at the Treasure Island base gate successfully and I had no doubt I could do it again. The tape recorder was a miniature reel-to-reel device about the size of two decks of playing cards back-to-back, and the microphone was in appearance a slightly oversized fountain pen; a thin black wire connected the two. It would be necessary that I cough loudly or make some other loud noise that would cover the noticeable, audible click when I activated the recorder by moving the machine's on/off switch to the operating position. And so one noontime in the spring of 1967 I entered the Treasure Island base with a tape recorder hidden in a top-opening leather satchel-type briefcase stuffed with an abundance of concealing papers and notepads, and with a microphone clipped upright on the top edge of one of the briefcase compartment separators.

All went well and I arrived in Capt. Kuntze's room on the third floor of the BOQ, put the opened briefcase on the floor between where we sat facing each other, managed to kick over a nearby desk chair and activated the recorder while I extracted a note pad and real ballpoint pen with which to take notes of our conversation, as I had previously done. I spoke the date, while making notes, and said I was glad to meet again with him, Captain Kuntze, in his room in the Treasure Island BOQ and I began a reconstruction of our prior conversation. About three minutes later a pneumatically operated jackhammer in the court-yard below the room's open window began breaking up a concrete slab. Capt. Kuntze rose and closed the window, but the racket was essentially unabated and all but the first three minutes of the recording was inaudible. Those three minutes, however, were enough to win the small wager of dinner and drinks I had made with Charlie that I would succeed in that covert mission.

Charlie was an amazing guy who had spent three hideous years as a prisoner of war in North Korea; that experience made him interesting to know, because he had seen the world from a different perspective, but in the more usual interactions he was socially inept and often lived over-and-over in his dreams the horrors he had experienced in that North Korean prison. He was a superb military affairs analyst and reporter. After a few years he took an editorial position in New York City with McGraw-Hill; later he retired to a shack without running water on Cleopatra Hill in the mostly abandoned old copper mining town of Jerome, Arizona, and subsequently he just disappeared.

During my second meeting with Capt. Kuntze he had said several things in conversation that convinced me Madame Sun was then in the San Francisco Bay area and had been during the captain’s court-martial. When I told Charlie I believed Madame Sun was living somewhere nearby we started a new adventure. Through the Department of Motor Vehicles we learned that Captain Kuntze owned a Cadillac automobile that was garaged in San Francisco. We followed him in his driving excursions for a week, but he made no contact with Madame Sun; from the telephone company we obtained records of the phone calls he had received and made from the BOQ. He had received nor made any off-base calls except to and from his mother in Southern California. I then decided that if Madame Sun were in the country she had probably come in on a Chinese passport, so I went to the Chinese consulate in the City’s Chinatown and asked to see the records of all persons who had entered the West Coast with Chinese passports during the previous six months.

Those records covering several years were entered as manuscript notes on 3 x 5 inch index cards, many hundreds of them haphazardly thrown into a large storage box in a corner of the room behind the secretary’s work area. The twelfth of those index cards that I extracted from the jumble of that box recorded that Madame Sun, using one of her known aliases, had entered the United States at Seattle three months earlier on a student visa; the contact address provided for her was the retail establishment of a very expensive Chinese furrier on San Francisco's Union Square.

During my young boyhood my home had been a big white antebellum mansion on the high ground of the "Narcotics Farm"—the several thousand acres of the USPHS hospital and federal prison for narcotic addicts at Lexington, Kentucky, where my father was medical officer-in-charge and warden. Among the federal prisoners at Lexington were craven marijuana-smoking Negro musicians, pathetic heroin addicts of all races but always young, and recovering Chinese-American opium addicts; some of the latter had been granted "trustee" privileges and were the white-coated houseboys, cooks and laundry boys in our home. Those men had been in many ways my surrogate uncles, and they had taught me as much of their Cantonese language as I could absorb, but which I had never learned adequately to maintain an adult conversation. I had, however, thorough fluency in the Pidgin English spoken by those men and I could faultlessly front myself as a representative of the Chinese Students Association who wished to establish contact with the student Miss Sun. Her telephone number would not be given, but I could write to her at her home address in San Francisco. That's all Charlie and I needed. We had succeeded where the Office of Naval Intelligence had failed.

The rest of that story is short. Charlie had an old Volkswagen camper with curtains that could be drawn to cover all the rear windows. I borrowed my dad's 35 mm Leica camera and 135 mm telephoto lens; we parked across the street from the front entry to the very nice Nob Hill apartment house where she lived and, hopefully, I would photograph Madame Sun and Capt. Kuntze as they made an exit from that grand foyer arm-in-arm to the street. We sat there most of the day, eating stale Chinese food from soggy paper containers, drinking bottled beer and pissing into the empties. Madame Sun did not appear, but at day's end Charlie produced from his briefcase—the same briefcase I had carried onto the Treasure Island Naval Station—an application for employment with the *San Francisco Chronicle* and he asked if I would like a job with the paper when I graduated a month later.

I didn’t have any other plans. I completed the employment application, submitted some journalism class papers as writing samples, was interviewed by the City Editor Abe Mellinkoff, was graduated from Berkeley, refused my diploma because it has been signed by Governor Ronald Reagan, took a week off to go camping in Yosemite, and started my first day as a city desk reporter with the paper, driving from home in Berkeley to work in my 1962 forest green Austin-Healey 3000 MK II sports car.

Photographs and illustrations credits.

Paul and Charlie Masters examining pot shards, Los Alamos, NM, 1944. Source: Courtesy of Mr. and Mrs. Wayne Clark. Used with permission.

“History of 10,000 ton gadget.” Source: Author’s files and Los Alamos National Laboratory.

Chapter 3

San Francisco Chronicle to NMERDI

I should have been as happy with a reporter's job with the *Chronicle* as a sailor on shore leave in Hong Kong but the assignments I received from the city editor, Abe Mellinkoff, were dull compared to the adventure that had gotten me the job. I was particularly thrilled, however, to work reporting assignments with *Chronicle* photographer Joe Rosenthal whose Pulitzer Prize-winning photograph of five Marines and a Navy corpsman raising an American flag atop Mount Suribachi during the battle for Iwo Jima was well known to me. However, the reporting assignments I had were mostly dreary and I sought a repetition of my Treasure Island escapade, which I soon found by courtesy of Haight-Ashbury Free Clinic Medical Director Dr. David Smith. At that time, the 1967 Summer of Love, the Clinic had been open since June and I had retired my Berkeley attire for the usual dress shirt, coat and tie of a working professional, but when occasion required I could re-costume and mingle modishly with the Haight-Ashbury crowd and not be recognized as a reporter from the alien Establishment newspaper.

It is appropriate now to mention that my father Victor H. Vogel, MD, had retired from the United States Public Health Service several years earlier. In the spring of 1967 the third edition of the classic book he had coauthored in 1954 with the sociologist and linguist David Maurer, *Narcotics and Narcotic Addiction,* had been issued by the publisher Charles Thomas and Sons. Three years earlier my dad had been appointed drug tsar of the State of California by Governor Edmund "Pat" Brown. In 1966 Gov. Brown lost his bid for a third term as California's governor to Ronald Reagan and the new governor in 1967 immediately reappointed my dad to the position he held in the Brown administration.

My dad was then the nation's leading Establishment authority on the subject of narcotics and narcotic addiction. By 1967 the book by Maurer and Vogel was everywhere in the country the definitive informational text for the medical profession, law enforcement agencies and the judiciary. Therefore when I introduced myself to David Smith at the Haight-Ashbury Free Clinic as a *Chronicle* reporter and my father's son, and said I wanted to know exactly what the hallucinogenic drug STP was, and where it had come from, I had the answer a week later.

STP had become the latest preferred mind-bender in the Haight-Ashbury and was causing some very bad trips that local emergency room physicians did not know how to properly treat because no one knew what the drug was. Since no one knew where the drug had come from, it was impossible to immediately know the drug's chemical composition and if tranquilizers like Thorazine would help or hinder recovery from the drug's effects. There was also considerable anxiety in the medical community, which anxiety had been reported by the *Chronicle*, that the drug might cause permanent cognitive and genetic damage to those who used it. David Smith gave me a telephone number and said I should wait two days, call the number and speak with the man who would answer the call. Today, Dr. Smith heads the 22 San Francisco Bay area facilities of Haight Ashbury Free Clinics, Inc.

Three days later I called the number and learned I was speaking with Alexander Theodore Shulgin, Ph.D. Sasha, as he is known by acquaintances, introduced himself as the chemist who had developed STP for Dow Chemical Company at its laboratories in the San Francisco East Bay. This was significant news because *Chronicle* reporting on the drug to that time had intimated that STP was probably a Hippie-Communist conspiracy concocted to destroy the minds and stir-fry the gene pool of American youth. Although abominated by the political Left because of the company's massive and very profitable production of the napalm used by the U.S. military in Vietnam, Dow

Chemical was in the view of the Establishment, and the *Chronicle* city editor, a moral and ethical pillar of America's industrial might and genius.

Editor Mellinkoff, who must be commended for the newspaper's frequent editorial plumping for Guide Dogs for the Blind, would later align the influence of the *Chronicle* in lavish praise and support of the up-and-coming psycho of the San Francisco People's Temple, the Rev. Jim Jones, whose pious pilgrimage ended with the mass suicide and murder of 913 of his flock—276 of them children—when at gunpoint on 18 November 1978 they drank their cups of cyanide-laced Kool-Aid at "Jonestown," French Guyana. Editor Mellinkoff was not alone among the Establishment and the Establishment's entourage of "good" liberals in that collective delusion of the Rev. Jones' sanity and sanctity; others were California State Assemblymen Willie Brown and Art Agnos, pastor of San Francisco's Glide Memorial Church Rev. Cecil Williams, NAACP President Joe Hall, American Indian Movement (AIM) leader Dennis Banks, gay activist Harvey Milk, California Governor Jerry Brown, California State Senator Milton Marks, San Francisco Mayor George Moscone, and Ben Brown, a member of President Carter's Transition Team.

By the time I put my finished copy of the STP "Inside Story" on the city editor's desk I had shown that Dow Chemical had produced scores of hallucinogenic drugs and that the United States Army was also active in developing those drugs at several ultra-secret laboratories, notably the Army's Chemical Center and School at Fort McClellan, Alabama. The Army had produced many hallucinogenic drugs includeing LSD, JB 314 and Agent BZ, which the Pentagon described to me as "our standard incapacitating agent." Agent BZ could be sprayed in combination with a faintly oily base and would be absorbed almost immediately through the skin, which convenience led the Army to consider plans to use the same airplanes that sprayed defoliants and Agent Orange in Vietnam to lay down an hallucinogenic mist of Agent BZ over Hanoi and Haiphong harbor with the anticipation that the U.S. Marines would quickly take that harbor and capital city from the defenders, befuddled in hallucinogenic disorientation, and the Marines would win the war.

Shulgin, my article reported, complained that rumors in the San Francisco and Berkeley drug scene had named him as the man responsible for the leak of the drug's formula from Dow. "That," he said, "is an absolute fabrication." I was told that Sasha's son had graduated from Berkeley about the time that STP had first entered the Bay Area drug scene. Those rumors intimated the chemical formula for STP had been a graduation present that permitted the son to earn his fortune before the Government would declare the drug illegal. In any case, that was one of the rumors I heard but did not report. Students interested in learning more about Alexander Theodore Shulgin and his life's work in psychopharmacology can start their inquiry with the cover story of the June 5, 2000 issue of *Time* magazine, "The Lure of Ecstasy" by John Cloud:

"Alexander Shulgin, 74, the biochemist who in 1978 published the first scientific article about the drug's [Ecstasy, MDMA] effect on humans, noticed [a] panacea quality back then. The drug 'could be all things to all people,' he recalled later, a cure for one student's speech impediment and for one's bad LSD trip, and a way for Shulgin to have fun at cocktail parties without martinis . . . Many of the [late 1970s] therapists had heard about MDMA from the published work of former Dow chemist Shulgin. According to Shulgin (who is often wrongly credited with discovering MDMA), another therapist to whom he gave the drug in turn named it Adam and introduced it to more than 4,000 people."

The article from which this excerpt is taken is available at:

http://www.time.com/time/magazine/article/0,9171,997083,00.html.

City editor Mellinkoff was not delighted with my initiative and reporting that showed STP had somehow entered the San Francisco drug scene from the research laboratories of Dow Chemical Company. I had researched and written the story on my own time, had not told him I was working on it, and immediately he had read the lead paragraph he was livid and refused to publish the story. He actually threw my copy back at me across his desk. I was prepared for that likelihood and I had contacted the San Francisco bureau of *Life* magazine and had a firm offer of $5,000 for the story if Abe wouldn't publish it. I was then earning an annual salary of $3,400 at the *Chronicle* and was fully prepared to go with the *Life* magazine offer if Abe refused, and I told him so. Abe was not accustomed to being jerked around by anyone, and never by a cub reporter, but he picked up his telephone and called the Dow Chemical regional vice president and after two minutes of conversation decided the *Chronicle* would publish the story. Two days later it was printed at the top center of the front page and would have had the banner headline that morning except Henry J. Kaiser died during the night and that notice got the banner in the late-hour page make over.

I become a magazine editor.

During the next two months I became aware that my future at the *Chronicle* was doubtful and Charlie Howe told me a position was open for an assistant editor in the San Francisco bureau of the McGraw-Hill trade monthly *Electronics*. Apart from the well researched and well written articles I had done for the *Chronicle* my only commending qualification to be an assistant editor with the world's then leading electronics industry news magazine was that I had studied relativity theory one semester at Berkeley with Professor Edward Teller. The electronics industry had entered the world of solid state physics as the basis of the transistor and integrated circuit semiconductor business burgeoning in Silicon Valley down the San Francisco Peninsula in Palo Alto, Santa Clara and Sunnyvale.

The premise of relativity theory dictates that measurements, and even the interaction of an observer, changes the thing you're trying to measure or observe. Reality, it turns out, is not at all what everyone had supposed it to be as described by classical Newtonian physics. David Lindley, who did research in cosmology and particle physics at Cambridge and Fermilab near Chicago, has written that the uncertainty principle implicit in quantum theory, which Einstein never liked, says you can't always get what you want—a phrase made popular by Mick Jagger of The Rolling Stones, who had a different view of reality than most of us. The uncertainty principle applies not only to the limits of our knowledge about the Universe and women but also, I have observed, to our knowledge about Port Chicago. We can ask questions about that reality and try to get at the truth, but we may not be entitled to unequivocal answers.

Opportunities for investigative reporting with *Electronics* magazine were few but by early July 1968 I had written, and the magazine had published, a comprehensive article I had done that detailed the critical need to achieve radiation resistance for integrated circuits used by the Atomic Energy Commission and other high-reliability programs underwritten by the Government. The Navy, for example, wanted chips to be used in the guidance circuits of the fleet ballistic missiles Polaris and Poseidon, circuits that would have 100 per cent reliability or a failure rate less than 1 per cent after final acceptance testing had excluded 9,500 chips from every 10,000 that were purchased. Those circuits would be required to operate with that reliability in space in the Van Allen radiation belts, in an environment contaminated by a nuclear explosion, or near a nuclear reactor, or source of isotope radiation.

Apollo and Minuteman missiles were subject to the same need for high reliability, radiation hardened circuits. Until that time post-irradiation specifications for critical-mission circuits had been subject to negotiation and done on a “best effort” basis but Government policy had become specific to the point that radiation tolerance wasn’t any longer open to negotiation. Government-specified post-irradiation operating characteristics were required to be met and only those suppliers that could manufacture to those requirements would stay alive in the market.

Certain variables could be controlled in manufacture at the “wafer” stage to enhance radiation tolerance: voltage requirements needed be minimized; dielectric isolation needed to be used to control spontaneous generation of photocurrents; and supply currents needed to be limited by using thin-film resistors in collector legs. Not all firms that had been chip suppliers to the Government missile programs could compete. The cost of research and development necessary to achieve the new and ever tightening radiation hardening specifications, for those must lucrative defense applications markets, could only be met by the larger companies and those that received favored-supplier contracts from the Albuquerque, New Mexico, U.S. Department of Energy Sandia Corporation that provided technology to those companies, technology that had been developed by Sandia’s in-house circuit designers. Under the old rules vendors might have supplied one or two out of perhaps 25 to 30 semiconductors for a specific project, but under the new rules a single firm would supply most or all the integrated circuits or discrete components necessary to one high-reliability program. Many small firms were sunk by the industry giants Fairchild Semiconductor and Motorola; even Raytheon Semiconductor was frozen out.

I was surprised and pleased when we in the San Francisco bureau received our copies of that issue of the magazine from New York to find I had been promoted from assistant editor to San Francisco regional editor. The San Francisco area was the world hub of the new electronics. Shortly after that article was published I was invited to visit the magazine's Manhattan editorial offices. The managing editor shook my hand, reached for his wallet, gave me a $20 bill, and told me to get my long hair cut before we had lunch with the publisher at noon. I complied. I had sometimes experienced a considerable wait while my bona fides were checked when I presented myself to the front desk security at Fairchild and other of the military contractor firms I needed to deal with in the course of my employment with the magazine.

I become a novelist.

In spring 1969 I retired from McGraw-Hill, with some capital appreciation derived from electronics firms' initial stock offerings I had purchased, and my wife and I drove east to rural northern Vermont where I began work on the Great American Novel, *A Voyage in Search of a Soul.* When she became pregnant we moved to a small apartment in Washington, DC, on Capitol Hill a few blocks from the House of Representatives and Senate. There I continued the novel, which I finished in spring 1971 in a small rented house that we and our infant son occupied on a cliff 200 feet above the Pacific Ocean at Elk in Mendocino County, California. Through a connection facilitated by my mother-in-law, the daughter of the psychologist and philosopher Otto Rank (1884-1939), I submitted the completed novel to Robert Cornfield, then senior editor at The Dial Press. Although he declined the book what he wrote to me on 12 April 1971 greatly encouraged my work as a writer.

"In some ways, the book dissatisfies me, but first I want to say a bit about what I admire, for the faults are only those of this particular book, while your successes are obviously those of your gifts and talents and they'll be with you in all your writing. First, there is the calm, measured, literate style. A real ability to deal with the functions of written language. You can play, parody, relax, switch modes, deal with the ineffable without posturing or seeming pretentious. If you don't think that a rare gift, I'll punish you by making you spend a day reading manuscripts here. Also, you have managed a tangible, smelling, seen world—one I've never met before but which after reading you I'd be able to recognize. You have managed to become distinctive and idiosyncratic and convincing."

The reasons editor Cornfield declined the book were also well explained: "The book moves so poetically that the secret of the form remains too closed. The narrative isn't clear; the fractioning is finally disruptive. It might have been better if you had forced a clear form on the book. I think the metaphor isn't aid enough. I know I am suggesting a more conventional manner, but I think the meditative way of the book needs that sort of frame. The patience you demand is a right price for the reward, and, to continue at great strain the metaphor, I'd like the marketplace and terms clearer."

I am charged with felonious assault.

Three months later I was in a San Francisco County jail cell charged with one count felonious assault on a police officer engaged in the performance of his duty; one count felonious assault on a police officer engaged in the performance of his duty, with a deadly weapon; one count inciting to riot; one count disturbing the peace. In the narrative of that arrest that I published the following December in the short-lived weekly newspaper of the City, the *San Francisco Fault*, I created the tangible, smelling, seen world I had experienced which few of the City's residents would recognize and most would never know but for my telling of it:

"Jail is a refuge and you want to forget, but there is no drug, no nightingale to lull you to a drowsy numbness, no narcotic to drink and leave the world unseen. No way to fade far away, dissolve and forget. You're at the epicenter of the evil perpetrated by men upon men; you're at the heart of darkness. There's a black man in your cell block. A big, muscular man, dripping from fever, without a shirt, who, with his hand in his pants paces his cell like a leopard, talking incoherently hour after hour as he masturbates. When the doors to the cell block are locked for the night, he puts up the second mattress in his cell so that he cannot be seen lying in his bunk, and for two hours he screams and moans until he vomits. He's charged with homicide."

The conditions of my no-contest plea to charges of trespass and resisting arrest following the Berkeley Free Speech Movement Sproul Hall sit-in specified a one year non-supervised probation; had I been arrested on any criminal charge during that year, it was six months in the Alameda County slammer, so I stayed well back of the front lines at protest demonstrations that year. This time I had stayed back from the front lines in a protest demonstration at San Francisco City Hall, but when the violence unexpectedly erupted well in front of me I was swept into the melee by a contingent of San Francisco police that moved in to rescue one of their own who had become isolated and taken down—a particularly distressing situation because the young Maoists who had taken the officer down had also taken his badge, neither of which was a good thing to do.

I was looking at the possibility of 20 years in prison if convicted of those felonies. I hadn't injured the policeman I was charged with assaulting, but that was only good fortune—his and mine. Another second and I would have been in abysmal trouble, but in that second I received a stunning police baton blow to the head from behind and my purpose was—I say by the grace of God—checked. A year later all the charges were dropped when I agreed not to sue the City and County of San Francisco for the wrong of the severe and very brutal beating I took intermittently during a slow drive in a police cruiser through the City's back alleys from the time my head wound was treated at the county hospital until I was booked one hour later. Every detail of the stops we made where violence was done to me in those alleys, out of the public eye, I reproduced in prose for the readers of the *San Francisco Fault;* it was a tangible, smelling world often alleged but seldom seen until small video camera-recorders brought examples of that violence to witnesses in every home.

San Francisco to Santa Fe.

The next six years, until summer 1978, were difficult and pleasant. I earned most of a living for my family scraping and painting the exterior of San Francisco Victorian houses to which was added the income of commercial assignments in theatrical photography and photographic assignments of four somewhat regular clients, the Bank of America and Crowley Maritime Corporation, *High Times* and *Head* magazines. In the early 1970s I had followed a suggestion that Sasha Shulgin made that I research the life and writing of the mid-19th century American author Fitz Hugh Ludlow, best known today for his first book that has been continuously in print since 1857, *The Hasheesh Eater*. Excerpts of the completed biography were published in the *Berkeley Barb* and other magazines but I was unable to find a publisher for the whole work, which finally evolved to an adventure novel for adolescent boys, *Muscarine,* completed in 1989. The seven years research I had done for the Ludlow biography and bibliography had taken me to several dozen research libraries from the San Francisco Historical Society to Harvard University and many private and institutional libraries between the coasts. I had experienced the enjoyments and thrills of historical research, learned how to find what I wanted, and I had acquired a particularly useful skill that enabled me to read an entire document page or microfilm screen in a single glance.

In summer 1978 I moved the family to Santa Fe, New Mexico. Santa Fe is 7,000 feet above sea level. The first winter there I worked outdoors at the Santa Fe ski basin at 10,000 feet operating the facility's main chairlift—an outdoors winter spent hefting significantly overweight Texans, on skis for the first or second time in their lives, into precarious seats on a mile-long moving machine that had a notorious tendency to eat Texans. The following summer I took work as the custom color photographic printer in the local and very fine shop that served the large amateur and professional community of photographers in Santa Fe, The Darkroom; and I took my place among that community of photographic artists in frequent exhibits of my Eastman Kodak

Dye Transfer prints in joint shows with long-time Santa Fe resident Eliot Porter, the great master of the dye transfer print, and his assistant Jim Bones. But more substantial earnings were required than the artist's endeavor provided.

My next door neighbor, a divisional administrator in state government's Energy and Minerals Department, told me her department was looking for a scientific and technical editor to administer the state's energy research and development programs. I was very fortunate that my demonstrated competence as a scientific and technical editor and writer, and my record of productive interactions with scientists, engineers and high-level business people found favor with the Research and Development Division of the New Mexico Energy and Minerals Department in Santa Fe, where I was hired in late summer 1979 to sort out and coordinate the state's multi-discipline energy research and development program administered by that department.

Those programs then received state appropriations of $5 million annually and comparable matching Federal and industry funds. The research and development funds administered by the Energy and Minerals Department provided research support mostly to academic researchers at the state's universities and colleges; in consequence I quickly became acquainted with most of the professors of the physical sciences at The University of New Mexico in Albuquerque, New Mexico State University at Las Cruces, the New Mexico Solar Energy Research Institute at Las Cruces, New Mexico Institute of Mining and Technology at Socorro and that Institute's Petroleum Recovery and Research Center. A fair number of those men who received research funding from the Energy and Minerals Department fund had been Manhattan Project scientists at Los Alamos during the war and had taken academic positions in the state after the war.

During the decade of the 1970s funds flowed to the New Mexico state government at record levels, principally from severance taxes the state collected from oil and gas producers, the state's coal mining industry and, very importantly, from the state's uranium mining and ore processsing industry. The year 1978 was the peak of the post-war uranium mining boom in New Mexico, driven by the bloom of nuclear power plants in construction and planned across the country. New Mexico produced 8,539 tons of uranium concentrate in 1978 with a market value of $370 million. That production represented 46 percent of the total U.S. production of uranium concentrate, known as yellow cake. The accumulation of money in the state's reserved Severance Tax Income Fund from all the state's energy and mineral extraction industries during the period of high energy prices during the 1970s was then well over two billion dollars. But in 1979 most forecasters in the state's energy sector anticipated an impending oil glut and also anticipated that uranium purchases by the nuclear power industry would soon collapse.

The uranium market was saturated and could be supplied for years from utility companies' inventories that had been amassed during a long post-war period of Government subsidy for the uranium mining industry and the uranium enrichment process at Oak Ridge, Tennessee. Government industry subsidies produced uranium enriched for utility purposes at such low cost that it was essentially a giveaway program to the nuclear power utilities. Even if nuclear power plant construction continued its then roaring bonanza, any rational forecast expected utility uranium purchases to radically decline. Canada and Australia were entering the uranium mining and milling market with better ore and lower productions costs, so it was clear to analysts that the New Mexico uranium industry was headed for collapse, which it shortly did. In 1986 the New Mexico uranium industry paid excise and severance taxes of $2.2 million and produced only 6 percent of the total U.S. production of uranium concentrate.

With the expectation in late 1979 that cheap oil would soon become abundantly available from Organization of Petroleum Exporting Countries (OPEC) the fiscal future of the State of New Mexico was readily perceived and understood. The comparatively high-priced oil and gas produced in the U.S. generally, and New Mexico particularly, would be driven out of the market. The state's income from severance taxes on oil and gas production would nosedive; combined with the loss of uranium severance taxes expected to result from abatement of utility purchases of New Mexico uranium, the state government faced an impending fiscal calamity.

New Mexico state government had been financed in large measure for decades by severance tax revenues from oil, gas, coal, and uranium production. Of those revenues during the 10 years prior to 1979 the legislature had generously appropriated monies from the General Fund and Severance Tax Income Fund for the wide range of energy research and development studies administered by the state Energy and Minerals Department. In total some $20 million had been appropriated by the state for that program's purposes. In addition to budget funds awarded to specific research projects an appropriate percentage of the project grant was added to cover overhead expenses at the schools where the research was conducted. Including matching Federal government and industry funds, some less than $40 million had been made available to the state research program with the result that more than 350 research projects had been completed or were ongoing in autumn 1979.

However, in recognition of an impending fiscal crisis, state legislators began to look at the expenses of that program more critically and asked if there had been any projects funded that had any practical application to enhancement of the value of the state's energy resources, their development, production and marketing. Legislators who asked questions about the program found that the organization of the program over the years hadn't provided any ready inventory of the 350 projects that had been funded, what those projects had proposed or what had been accomplished. The Energy and Minerals Department needed someone, at a moderate salary level, with adequate scientific and technical competence to organize and analyze the program project files, to discover if anything of practical value had been accomplished, and to respond to the legislature's request for a detailed report of the overall history of the program, its costs and benefits.

A few examples of the research funded by the state's program will give a sense of the complexity of most of the project studies: "Methane production from carbon oxides over borohydride-reduced transition metals," "Engineering methods for predicting productivity and longevity of hot-dry-rock geothermal energy reservoirs in the presence of thermal cracks," "Relationship of pore structure to fluid behavior in low permeability gas sands," "Extraction of radionuclides from low-grade ores and mill tailings," "A roof type solar distillation plant for the Lake Valley Chapter of the Navajo Indian Reservation in New Mexico," "The effect of carbon dioxide on the phase behavior and viscosity of coal liquids." The research program funded work in all subjects of energy research and development: oil (especially enhanced oil recovery technologies), natural gas, coal, the nuclear fuel cycle, geothermal, hydropower, wind, solar, synthetic fuels, biomass, waste conversion, energy conservation, the energy requirements of agriculture, general study of the socioeconomics of energy production and use; the program also created and funded a wide variety of energy information literature and community awareness programs to benefit the state's residential and business consumers.

By the beginning of 1980 I completed a comprehensive report and analysis of the 350 projects that had been and currently were funded by the program. The Secretary of the Energy and Minerals Department transmitted the report to Governor Bruce King; the governor approved the report and sent it to the legislature with his recommendation, endorsed by the energy secretary, that an institute administratively attached to but separate from the Energy and Minerals Department should be established to administer the research program. A few months later the legislature enacted the proposal and the New Mexico Energy Research and Development Institute (NMERDI) was established, on paper, in Santa Fe. I very much objected to adoption and use of the Institute's acronym. Spanish, old Castilian Spanish, and modern "Spanglish" compete with English as the dominant language spoken in northern New Mexico, but a French cultural influence and a significant French community have been remarkable in Santa Fe since 1850 when the Roman Catholic Diocese of Santa Fe was created and Jean Baptist Lamy received Papal appointment as the first Bishop of Santa Fe and later, in 1875, Archbishop of New Mexico and Arizona. Two famous books keep Lamy's memory alive. One is Willa Cather's superb historical novel *Death Comes for the Archbishop,* first published in 1927. The other is Paul Horgan's 1975 Pulitzer Prize-winning biography, *Lamy of Santa Fe.* There is a French consulate and a French community in present Santa Fe. *Merde* is the French colloquial word that translates to English as "shit," and I was concerned the Institute's acronym, NMERDI, looked too much to French speakers in Santa Fe as a curious acronym for a New Shitty Institute.

The enactment that created NMERDI included an appropriation of $5 million to design and construct the modest office building we thought appropriate to the undertaking; $500,000 to purchase a Beechcraft Queenair turboprop “mini-airliner,” principally for the governor’s use, but with the Governor’s pilot available for NMERDI transportation requirements; an initial appropriation of $5 million to fund new and continuing projects close to the commercialization stage with the goal of economic development; and the legislature provided a generous operating budget for the new Institute. I was appointed by Governor King to implement establishment of the Institute. It was at that juncture in spring 1980 that I recovered the “History of 10,000 ton gadget” which Paul Masters 35 years earlier had purloined from Los Alamos Laboratories.

Chapter 4

NMERDI, Edward Teller, Men and Boys

By the time the state legislature created the New Mexico Energy Research and Development Institute (NMERDI) I was acquainted with most of the men at the state's institutions of higher learning who were principal investigators of the projects funded by the energy research program; a number of those men during the war had been Manhattan Project scientists at Los Alamos. All those former Los Alamos scientists to whom I showed the "History" between May and August of 1980 were intrigued by the reference that document makes to Port Chicago and fascinated that a document had ever gotten out of Los Alamos that provides so much detail about the design and effects of the explosion of an atomic bomb, but none of them knew the name Port Chicago. They agreed, however, that Edward Teller, the "Father of the H-bomb," would certainly be able to identify Port Chicago and say what that reference in the document signified.

A few days study of that document had persuaded me that the "History" had been written preceding the 16 July 1945 atomic bomb test at Trinity site in New Mexico. If the "History" had been composed after that date, the document would have specified the Trinity fireball as "typical" of an atomic bomb detonation rather than the "Port Chicago" fireball; Paul Masters had told me he had removed the document from Los Alamos during the autumn of 1944. Port Chicago was a piece of my experience in Berkeley during the mid-1960s that I had forgotten—repressed, perhaps; it had been a difficult time. I had no recollection of my visits there 15 years earlier. Not until September 1980 did I recognize that Port Chicago was a place I had been.

I had studied undergraduate physics with Teller at Berkeley and had gotten to know him somewhat personally because admission to the class required prospective students to write a short essay on some aspect of relativity theory and adequately defend that essay in conversation with him; students in the class individually met regularly with him during the semester.

Professor Teller approved my essay and defense, admitted me to the class, and later introduced me to quantum theory, Max Planck, Werner Heisenberg and John von Neumann.

Port Chicago was nowhere mentioned in the indexes of the general histories of the Manhattan Project available in the New Mexico State Library; those scientists of my acquaintance who had been at Los Alamos during the war had never heard of Port Chicago, but evidently, whatever Port Chicago was, it had been fundamental to Los Alamos in the preparation of a detailed mathematical model that described the detonation of an atomic bomb. Although my acquaintances who suggested I speak with Ed Teller about the document did not foresee that meeting could be a problem, I did.

By the beginning of August 1980 I had asked those men if it were possible that an atomic bomb had been tested by Los Alamos prior to Trinity and if "Port Chicago" could be a code word that designated that test. If such a thing had happened, they had not known of it; but they were mindful to say that Los Alamos during the war had been deliberately organized and compartmentalized for security purposes so that very few people knew much more than the details of the specific areas of work to which they were assigned. As much as my acquaintances knew from personal experience, and later reading, the first atomic bomb test had been conducted at Trinity site, 16 July 1945.

That test at Trinity, they said, had simultaneously proved the theory of large scale nuclear fission explosions and proved the particular technology and design of the bomb detonated at Trinity, which was detonated in combat a few weeks later at Nagasaki, Japan, 9 August 1945. That the feasibility of large scale nuclear fission explosions had not been first proven experimentally on a smaller scale than the Trinity test was manifestly contrary to the usual method of scientific research and development. The conventional scientific method would dictate an experimental small bang as a proof of theory before an experimental big bang was attempted.

The arguments advanced by those former Los Alamos scientists associated with the state's energy research programs to explain that deviation from customary research and development practice were that bench-scale experiments conducted by the Project had so adequately proven the theory of large-scale nuclear explosions that a small-scale proof of the theory was unnecessary. That reasoning seemed unreasonable to me. For a project as important to the immediate national defense during the war as the atomic bomb was, and which was expected to determine the postwar maintenance of world order—and that was as expensive as the atomic bomb development program was—it seemed to me inconceivable that a small-scale proof of the theory of nuclear weapons would not have been made, if that test had been possible. If that small-scale experimental detonation had been possible, had it been done at Port Chicago?

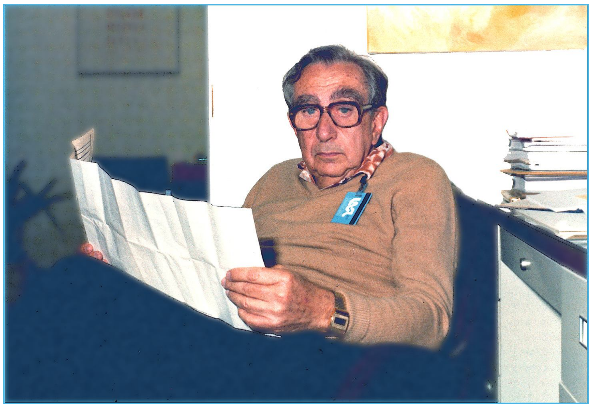
Teller, I expected, would be reluctant to talk about the document I had and Port Chicago, if the "History" did refer to an unannounced bomb test and Port Chicago had been that test. If I proposed a discussion of that subject with him I expected he would decline to meet for that purpose, so I tricked him. Teller, I learned, spent most of his time at Lawrence Livermore Laboratory, east of San Francisco in the city of Livermore, but he also spent a good part of his time at Los Alamos Scientific Laboratory (LASL) where he had an office and where he worked and met with his Los Alamos-based colleagues. Two years later, Los Alamos Scientific Laboratory was renamed Los Alamos National Laboratory (LANL).

Meeting with Edward Teller at Los Alamos.

Early in August 1980 I called Teller's secretary at Los Alamos and identified myself, honestly, as the editor of the state's energy newsletter, *Energy Source*, and said I wanted to interview Dr. Teller at Los Alamos for an article I would write for *Energy Source* that would consider Dr. Teller's views on the energy crisis. I said I would have a camera with me to take a picture of Dr. Teller to illustrate the article.

Within a few days the early afternoon of 27 August was set for that meeting. I arrived with the original and a copy of the "History," a Nikon motor-drive camera with a 50 mm lens, a role of color negative film loaded in the camera, a powerful exposure-attenuated photographic strobe and battery pack.

We talked awhile about Berkeley in the mid-60s and I said that before we commenced the interview I would take the photos I wanted. He agreed. To take his mind off the photography I suggested he read something I had brought with me and I handed him the original "History of 10,000 ton gadget." He protested in his usual gruff manner that he didn't need anything to take his mind off the photography, to which I responded with like gruffness that he should read it anyway and then talk to me about the document. Ed glanced at the document and immediately became engrossed with what it obviously offered in history from Manhattan Project days.


Dr. Edward Teller (1908-2003) reading "History of 10,000 ton gadget," Los Alamos Scientific Laboratory, 27 August 1980.

I started flashing the big strobe and exposing quick photos with the motor driven Nikon while he read; then I moved my position so that the upper right corner of the document was visible in the frame and asked him to turn his head and look directly into the camera lens, which he did. The document, so immediately interesting as it was to him, had indeed distracted his attention from the photography. He may even have been unaware I was taking pictures and he seemed oblivious of the flash that was lightening the room. When I had a sufficient number of photos exposed I put the camera down and asked him specifically what the bottom line reference to Port Chicago signified. He studied the bottom line a moment and said it was Port Arthur. I said, "No, Ed. It's not Port Arthur. It's Port Chicago."

The Texas City disaster.

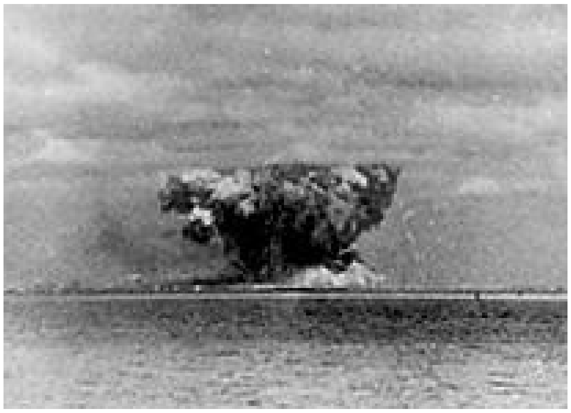
Port Arthur is on the Texas coast of the Gulf of Mexico, 65 miles east of the state's principal maritime port at Texas City on the Galveston Bay shipping channel; the Texas City deepwater port facilities serve Houston and industrial southeast Texas. On 16 April 1947 the bulk carrier S.S. *Grand Camp* exploded at approximately 9:15 A.M. while moored to Pier "O" at Texas City. Subsequently the local piers, adjacent warehouses, nearby industrial plants, tank farms, and two other ocean going vessels became involved; one of those two ships was the cargo vessel S.S. *High Flyer* which also exploded. The number of persons known to have been killed in the Texas City disaster was 433 and 128 persons were listed as missing, for a total of 561 persons killed or presumed to have been killed. The number of persons injured in the explosions ranged in the thousands; property loss totaled $67 million. S.S. *Grand Camp* very frequently is misnamed S.S. *Grandcamp* in the literature.

Grand Camp, a French-owned vessel, was the former American Liberty ship S.S. *Benjamin R. Curtis* built at Los Angeles, California, in 1942; *Grand Camp* was the same class vessel as the Liberty ship which had exploded 17 July 1944 at the Port Chicago Naval Magazine, the S.S. *E.A. Bryan:* length 422.8 feet, breadth 57 feet, depth 34.8 feet, gross tons 7,176, net tons 4,380, 5 cargo hatches. The cargo aboard the *Grand Camp* when it came to dock at Texas City consisted of 59,000 bales of sisal binder twine; 2,501 bales of leaf tobacco; 9,335 bags of shelled peanuts, 380 bales of compressed cotton, 16 cases of small arms ammunition, and inconsequential quantities of other, common commodities. At Texas City longshoremen began loading pallets of 100-pound multi-ply paper bags of ammonium nitrate into the *Grand Camp* at 1 P.M. on 11 April 1947. By 8:20 the morning of 16 April, 882 tons of ammonium nitrate were loaded in the ship's No. 4 lower cargo hold and 1,459 tons in No. 2 lower hold. The ship's ammonium nitrate cargo had been produced by the U.S. Government during World War II to be used in the manufacture of military explosives and was destined to be recycled as fertilizer in France.

Described as "fertilizer compound," the ammonium nitrate loaded in the *Grand Camp* originated at three U.S. Army ordnance plants, one in Iowa and two in Nebraska. The United States Coast Guard Court of Investigation convened in the days following the explosion to inquire into and investigate the fire that started aboard the *Grand Camp* and the subsequent ship explosions found that the shipping officers of the ordnance plants had violated applicable Interstate Commerce Commission regulations governing the transportation of explosives and other dangerous articles by describing the highly explosive ammonium nitrate shipment as "fertilizer compound". The Record of Proceedings of the Court was issued 24 September 1947 and found that the fire on board the S.S. *Grand Camp* which preceded the explosion was of undetermined origin.

Because the explosive and oxidizing characteristics of the shipment were not identified, the ship's operator was not properly advised that the shipment was explosive. No specific instructions were issued to longshoremen or the ship's crew with respect to smoking on the *Grand Camp* during loading operations, nor were the longshoremen issued specific instructions on the storage of ammonium nitrate. Bags of ammonium nitrate which were broken or torn during loading into the *Grand Camp* were not refilled or repaired but were stowed in the holds in violation of Coast Guard regulations governing the handling and stowage of that material. The Court of Investigation found that "hardly without exception all persons concerned with the handling, stowage and transportation of the cargo displayed a lack of knowledge of the provisions of regulations governing the safety of the operations either by land or water."

Smoke was noticed emerging from the No. 4 hatch of the *Grand Camp* at 8:15 on the morning of 16 April, but no flames were visible. Four portable fire extinguishers were lowered into the hold and expended without effect on the smoking cargo. Ten minutes later flames appeared and an alarm was sounded on the ship's whistle. A fire hose was lowered into the hold, but the ship's captain ordered that no water should be played on the cargo. All personnel were ordered out of the hold, the hatch was battened down, the hold's ventilators were closed, and steam was introduced into the hold by the ship's steam smothering system, which had the effect only to raise the temperature of the explosive cargo. The 41 men then present on the ship left the vessel and assembled at the outer end of Pier "O". Five of the assembled men left the pier before the explosion, but of the thirty-six men who remained on the pier to watch the drama only two survived the explosion.



Smoke cloud above the explosion of S.S. *Grand Camp.* After 2-4 minutes the smoke cloud has pierced through the cloud ceiling 2,000 feet above Galveston Bay

At 9:15 A.M., approximately fifty-five minutes after the fire was discovered, the 882 tons of ammonium nitrate in No. 4 lower hold detonated and in close sequence caused the 1,400 tons in No. 2 to detonate, resulting in the complete destruction of the S.S. *Grand Camp.* A photograph taken from a position several miles from the explosion, and 2-4 minutes following the complete detonation of the *Grand Camp,* shows that the towering smoke cloud above the explosion had pierced through the cloud ceiling at a height probably 2,000 feet above Galveston Bay.

Twenty-seven members of the Texas City Volunteer Fire Department that responded to the alarm had begun firefighting operations on and about the ship when the No. 4 hold exploded. All were lost. Their four pieces of apparatus were destroyed to the extent that only parts of one piece could be identified. Several pictures of the fire department preparing for action, the ship burning, and the firemen applying water form part of the record of the Proceedings of the U.S. Coast Guard Court of Investigation.

The Court of Investigation found that the explosion of the *Grand Camp* generated tremendous pressure, "but appeared to have lacked the shattering destructive characteristics of an equivalent amount of a [TNT] nitro-high explosive." Within a radius of one-half mile of Pier "O" the missile pattern that resulted from fragmentation of the ship was one missile to every two square feet. Missiles ranged in size from a rivet head to a portion of the ship's structure estimated to weigh 60 tons.

The S.S. *High Flyer* was a World War II type C-2 cargo ship built at Oakland, California, in 1944 and was slightly larger than the *Grand Camp.* S.S. *High Flyer* was moored to Pier "A", 700 to 800 feet south of the *Grand Camp.* Prior to its arrival at Texas City the *High Flyer* had loaded 1,050 tons of sulphur in the No. 2 hold and 950 tons of sulphur in No. 4. At Texas City, 961 tons of ammonium nitrate had been loaded in the No. 3 lower hold.

Small areas within the *High Flyer's* ammonium nitrate and sulphur cargoes were probably immediately set smoldering by red-hot steel fragments thrown off from the explosion of *Grand Camp.* Sparks and smoke were observed coming from one of the ship's forward holds three hours later at 11:20 A.M. But not until 6:00 P.M. did rescue personnel searching the badly damaged *High Flyer* see fire in the ship's No. 4 hold. Soon the sulphur cargo in Nos. 2 and 4 holds was on fire, and smoke was issuing from the ammonium nitrate cargo in No. 3. The fires burned slowly for five to six hours, but by 12:55 the morning of 17 April the ship's sulphur cargo was fully ablaze and the area was ordered evacuated. Fifteen minutes later at 1:10 A.M. the 961 tons of ammonium nitrate in the *High Flyer's* No. 3 hold exploded. Because the area had been quickly evacuated the loss of life as a result of this explosion is reported as one; the injured reported as from 35 to 100. The explosion completely destroyed the *High Flyer*.

Meeting with Edward Teller at Los Alamos, continued.

When I had exposed a sufficient number of photos which showed Dr. Teller in his office at Los Alamos Scientific Laboratory reading the "History of 10,000 ton gadget" I put the camera down and asked him specifically what the bottom line reference to Port Chicago signified. He had studied the bottom line a moment and said it was Port Arthur, by which he intended reference to the Texas City disaster. I answered, "No, Ed. It's not Port Arthur. It's Port Chicago."

About then he knew I had deceived him and that I had no intention to interview him on the subject of the energy crisis but was there to talk about Port Chicago. His response was abrupt: "I believe you have a classified document. You should take it immediately to the Classification Office. I will deny I have ever seen this document. I will deny I have discussed Port Chicago with you." Fortunately the photos were successful. I did then use his secretary's telephone to call the laboratory Classification Office and said I had a document in my hands that Ed Teller thought probably should be classified and Ed said I should carry it over to that office immediately.

I met the chief of the Classification Office in the foyer of the laboratory Administration Building and suggested we walk outside to the flag pole in the grassy area opposite the building entrance. As we walked I learned that lately many atomic bomb documents were discovered by the public and brought to his attention, but they always turned out to be bogus or insignificant. This one, I said, was the real thing and it was significant.

In Dr. Teller's office I had written on the duplicate copy of the "History" I had with me that it was the "Property of Peter Vogel." The document had no other mark of authorship or ownership, nor was any classification mark on it. When we had settled the question of the document's authenticity I gave him the copy I had marked as my own property and said I wanted to know what the bottom line reference to Port Chicago signified. I also provided one of my business cards that bore the authoritative gold Seal of the State of New Mexico and identified me as the New Mexico State Energy Information Coordinator in the Resource and Development Division of the New Mexico Energy and Minerals Department, Bruce King, Governor.

The classification officer said he would call me when he had an answer and did two weeks later. To learn what Port Chicago was I was instructed to look in the entry for Disasters in the *Encyclopedia Americana*. There I first learned of the 17 July 1944 explosion and disaster at the Port Chicago Naval Magazine. The next week I went to the State Library in Santa Fe and found the *Time* magazine article "Strange Cargo" that reports that disaster in some detail. Particularly I noticed that witnesses had said the explosion that evening at 10:30 had illuminated the landscape for many miles around in a brilliant flash of white light, bright as noonday. Implicitly, it must be understood, the brightness of noontime at Port Chicago on a clear day—in July around San Francisco Bay the noontime sun is often diffused to a gray pallor though the shroud of summer coastal fog.

I then turned to *The New York Times* editions for the days following the explosion and found there photographs of the destruction at Port Chicago and a much more extensively detailed account of the disaster. Although not of particular note in 1944, the paper did report that most of the men killed and injured at Port Chicago were Colored. Following the sequence of Port Chicago stories that had appeared in that newspaper during several months following the explosion I first learned of the Port Chicago work stoppage by those Negro sailors at the base who had survived the explosion without serious injury and were then set to be tried by Navy court-martial on the charge of mutiny-in-wartime. The Port Chicago story got more interesting with every fact that I learned.

The indefinite suspicion I had that a nuclear bomb test had been conducted somewhere obscurely identified in the "History" as Port Chicago was reinforced by what I read in *Time* magazine and *The New York Times.* It had been a massive explosion, perhaps the largest manmade explosion in history to that date; the frequently reported "blinding" white flash that illuminated the landscape at the moment of the Port Chicago explosion was startlingly suggestive of the brilliant "blinding" white flash associated with the Hiroshima atomic bomb explosion, especially as depicted in the book I had read by John Hersey, *Hiroshima.* The pitch and tone of Ed Teller's response to his review of the "History of 10,000 ton gadget," and his response to the bottom line reference to Port Chicago in that document, had also increased that suspicion.

Edward Teller had told me without hesitation on 27 August that he would deny, which is to say he would lie, that he had ever seen the "History" and had discussed Port Chicago with me. When a person emphatically and without reservation commits to a lie to conceal a truth or fact, the truth or fact that is to be concealed by that lie can be no more important than a personal foible or minor indiscretion that would, if known, cause the individual an acute passing embarrassment, but that others would rank as a petty burlesque of mankind's more notable and momentous falsehoods. However, common homely logic readily educes the inference that if Edward Teller would brassily lie to conceal the fact that we had discussed Port Chicago and lie to conceal the fact that he had seen and carefully read the "History of 10,000 ton gadget," something about Port Chicago was more complicated than a personal foible or minor indiscretion.

The first business after the legislature established the New Mexico Energy Research and Development Institute was to lease office space for the new enterprise, equip and furnish those offices, hire the first support staff, and then begin the search for a suitable location in Santa Fe to construct our $5 million office building. Two months later in very nice leased, new modern offices with panoramic views east to the Sangre de Cristo Mountains and west to the Jemez and Manzano Mountains we began preparation of a slate of candidates from which the governor would nominate and appoint the Institute Board of Directors and Technical Advisory Committee.

The enabling legislation specified that the Institute Board of Directors should be selected from the most experienced and knowledgeable luminaries of the energy sciences, energy resource production, development and marketing whom we could persuade to give their time, wisdom and energy to direction of the Institute purposes and programs. The Institute's Technical Advisory Committee was to be composed of the most experienced and knowledgeable scientists and engineers that we could draw from industry, the state's universities and colleges. The enabling legislation specifically directed that the new Institute should, as much as possible, draw from the scientific and technical expertise of the state's national laboratories: Los Alamos Scientific Laboratory and Sandia National Laboratory.

By appointment of the governor I moved from the Energy and Minerals Department to the Institute in the position of Liaison Officer where my responsibilities immediately included explaining the purposes and programs of the Institute to those persons whom the governor would nominate to the Institute Board of Directors. The first letter of nomination the governor sent went to Los Alamos Scientific

Laboratory Director Donald M. Kerr, Jr. If LASL director Kerr would agree to serve on the board we thought others whom the governor had selected to nominate would be willing to serve as well. If Don Kerr declined the governor's nomination we would not be able to attract the stellar Board of Directors we needed. The Technical Advisory Committee, lacking a blazing Board of Directors, would be helpful but not composed of the world-class scientists and engineers whose expertise we also needed. In mid-September 1980 I made an appointment to meet Dr. Kerr at Los Alamos to discuss with him the purposes and programs of the Institute and, I hoped, to persuade him to accept the governor's nomination and be the board's cornerstone of excellence.

Don Kerr's was a corner office on the third floor of the administration building at Los Alamos, also with very nice panoramic views of the Jemez Mountains that rise to their peaks behind Los Alamos which lies at 9,000 feet on their eastern slope. During the first 20 minutes of our meeting I made my pitch for the Institute and that he should accept the governor's nomination to the board. The mandate of the national laboratories includes the directive to assist and cooperate with state governments where the laboratories are located as may be beneficial and appropriate, so the governor's request for his service on the board was comprehended by the compass of his office.

Don then agreed to serve on the board, as he said, if others of comparable stature would also agree to serve. That would not be a problem. With Don's agreement to serve, others of comparable stature would line up for the opportunity to join the board. That business concluded, I took the "History of 10,000 ton gadget" out of my briefcase, handed it to him, directed his attention to the bottom line, and said we'd be very happy to have his expertise and guidance on the Board of Directors, but there was the matter of Port Chicago. I was, I told him, reasonably convinced the Port Chicago explosion had been a nuclear weapon test conducted by Los Alamos; that I intended to make that a public issue; and if he accepted the governor's nomination he was going to have to deal with me because Governor King had appointed me to the Institute and I was, ex officio and by statute, Secretary of the Institute Board of Directors.

Don was noticeably miffed by the complications that unexpected topic would contribute to his service on the board and immediately proposed that Ed Teller, whom Don knew was then at Los Alamos, would refute that idea; Don reached for the telephone on his desk to get Ed on the line for that purpose and I said, “Don’t bother. I’ve already spoken with him about it and I respect the man too much to take up more of his time.” Don then said to me, “You’ll never be able to prove it.” I said that I’d do the best I could, and would he serve on the board if I could get other men of comparable stature?

He agreed and that ended our first meeting. From that time we worked three years together. There were challenging incidents that came up between us during those years but which were always more farcical, I thought, than grave. For example during the first meeting of the board, in executive session, the agenda included review of “Institute Personnel Qualifications.” Don frostily challenged the scientific and technical qualifications I brought to the position I held; I voiced a frosty remark in response, and the board approved the qualifications of existing Institute personnel but disapproved the new $5 million Institute office building.

Anyway, I served at the pleasure of Governor King, and the governor was so well pleased with my work with the program that he had also appointed me an honorary colonel in the New Mexico National Guard. Additionally I served on the Energy Committee of the New Mexico Society of Professional Engineers, was a member of the industry-critical New Mexico Carbon Dioxide Enhanced Oil Recovery Research Review Committee, served on the Executive Committee of the New Mexico Solar Energy Association Board of Directors, had negotiated a new Queenair for the governor’s office, and frequently performed traditional New Mexico folk music at the governor’s parties with the band I sang with, *Los Travadores de Santa Fé.* It would have been difficult to persuade the governor to be displeased with my service.

The board members probably did expect Don to challenge the fitness of my qualifications for employment in the Institute and as designated secretary of the board because in conversations with those men before each had accepted the governor's nomination I had explained that Don and I had what I described as a well-tempered factual dispute ongoing on the subject of the 1944 explosion and disaster at Port Chicago in California that had killed 320 men, most of them Negroes, and the role of Los Alamos in that disaster.

The board members had accepted that condition before they joined the board, and in the next years they all became generally interested by the Port Chicago history that I forewent no suitable opportunity to discuss with them, or anyone who would listen. To understand the pertinence of those men's interest in the Port Chicago work I was developing it is appropriate to acknowledge who those men were who served on the Institute Board of Directors. Los Alamos Scientific Laboratory director Kerr was elected chairman of the board; former Los Alamos Manhattan Project physicist, Provost of the University of New Mexico McAllister H. Hull, Jr. chaired the Institute's Technical Advisory Committee. Members of the Institute Board of Directors were:

Robert O. Anderson, Chairman of the Board, Atlantic Richfield Company

Jack M. Campbell, former Governor of New Mexico,

Edward F. Hammel, Consultant to the Office of Planning and Analysis, Los Alamos National Laboratory,

Frank S. Hemingway, retired Technical Director, White Sands Missile Range,

Larry Kehoe, Secretary of the New Mexico Energy and Minerals Department,

Donald M. Kerr, Director, Los Alamos Scientific Laboratory,

Dean A. McGee, Chairman, Executive Committee, Kerr-McGee Corporation.

Dean A. McGee, who died September 15, 1989, was the most ethical, honest, even-tempered, affable, open-minded and modest man I have known who coupled that remarkable character with the usual fierce competition and self-interests of even a small business. But Dean McGee conducted a worldwide billion-dollar energy business behemoth and maintained the calm, unirascible character of his person which inspired an entirely deserved personal and business confidence among his associates that, no doubt, permitted him business opportunities unavailable to others whose temperament and modes of interaction were more strident.

He was a geologist who received his B.S. degree from the University of Kansas and served Phillips Petroleum Company for ten years as chief geologist. He resigned in 1937 to become vice president of Kerlyn Oil Company, predecessor to the Kerr-McGee Corporation. He became executive vice president of Kerr-McGee in 1942, president in 1954, and chairman of the board and chief executive officer in 1967.

The Kerr-McGee uranium mining and milling operations at Ambrosia Lake, New Mexico, during the term of his service on the state’s energy research institute board were in standby shutdown, but a wonderland of tidy suspended animation to visit. The world market price for refined uranium ore, yellow cake, was considerably below the break-even production costs of the domestic uranium industry. The worldwide nuclear power industry—then the big market for uranium—held substantial uranium reserves; guaranteed federal government uranium purchases and price support had nearly ceased. The associated costs of standby shutdown at Ambrosia Lake were very high and included the huge electricity costs necessary to operate the mammoth pumps that continuously dewatered the inflow of groundwater from the mines; the cost of cleaning up that water pumped from the mines contributed another large expense. The uranium boom that originated in the late 1940s was over. During 1989 Kerr-McGee completed the sale of all the company’s worldwide uranium interests.

During the period of our acquaintance in the early 1980s Dean McGee and Kerr-McGee Corporation were confronted with the enigmatic plutonium poisoning of a company employee, Karen Silkwood. My original article on the Port Chicago explosion, which alleged the explosion had been an unannounced nuclear weapon test, had been published in spring 1982 and was the subject of frequent discussion among the men and several women associated with the Energy Research and Development Institute programs. Mr. McGee was, of course, considerably acquainted with the physics and chemistry of the small explosions that are part of petroleum resource prospecting and minerals mining operations and he readily contributed what he could to my understanding of the "History of 10,000 ton gadget." Whatever evil was done in the Karen Silkwood affair I am confident from the experience of my three years acquaintance with Mr. McGee that he was ignorant of any wrong action or purpose intended or committed by any person in his employment, for whom his own character and behavior should have been the founder's paradigm to transcend any employee's inclination to criminal character and behavior.

The progress of my study of the Port Chicago explosion has been facilitated by my practice of speaking to anyone and everyone about the explosion and my view that the explosion had been a very well concealed nuclear weapon test conducted by Los Alamos. I am certain the dominance of that theme in my conversations with others has often been tedious and annoying but that's the way it had to be done, and the practice often produced pertinent information that I would not otherwise have learned. Sharing a car with old and new friends en route to a party one evening in Santa Fe in early summer 1981 I learned that one of the guests with whom I traveled was a meteorologist at LANL, to whom I expounded my view as we drove along that the Port Chicago explosion had been a nuclear fission weapon test secretly conducted by Los Alamos. Either I have forgotten or I never knew the reason that meteorologist had studied the Port Chicago explosion, but he asked me if I were aware of the extensive Port Chicago explosion documentary files that were available in the Archives at Los Alamos laboratory. I was not aware those documents existed at Los Alamos.

A few days later I telephoned to the laboratory archivist Walter Bramlett and told him I had been told the Archives held a large collection of documents pertaining to the 1944 Port Chicago explosion in California and that I would like to have access to those files. Walt Bramlett had come to Los Alamos in 1945 as the first postwar archivist when the only archived materials at the lab were those that had originated with the Manhattan Project. Walt, I believe, knew the wartime Los Alamos Manhattan Project materials, classified and not, and all the unpublished details of wartime Los Alamos better than any other person there before he arrived or who has followed his tenure of 37 years as the laboratory's archivist, and he brought a sense of humane humor to his work.

Walt told me without a trace of humor in his voice how it was really a shame I hadn't called about the Port Chicago materials two weeks earlier because they had since been culled from the Archives and destroyed. The laboratory Archives, he said, were getting much too large for the available space and would be unmanageable without occasionally culling materials for which there had been no expressed interest during several years. All the Port Chicago files, and they had been extensive, were gone. In response I said I hadn't wanted to hear that, wondering at that moment if my reputation had preceded me, and he laughed and said of course he had all the Port Chicago files, but I would have to ask the laboratory Classification Office to review them before I could come to the Archives and go through those materials.

I wrote the letter and within a month the Classification Office had reviewed all those 7 linear feet of Port Chicago documents and approved that they were cleared for my access. I spent intermittent afternoons in the Archives for a month and became acquainted with those materials in detail; I made extensive notes on the materials, copied one page only, and got to know Walt Bramlett well enough that we would go out to lunch together and talk about Port Chicago and my view that the explosion had been a test of a nuclear fission weapon. Walt, in my view of things, came as close to a direct acknowledgment that the Port Chicago explosion had been the test I claimed it was as anyone had done. After a full New Mexican lunch and a few beers I allowed as how I didn't understand that we could have done that to our own men. To understand that we could have done that to our own men Walt said I had to remember that the men killed in the explosion were mostly niggers.

In 1981 in New Mexico, Negro was still the current epithetic noun used to identify members of the Negroid ethnic division of the human species, especially one of the various peoples of central and southern

Africa and their descendants in the New World. Black man and black woman were terms that had some currency among the more liberal progressive-minded residents of the state in 1981, but the dignity of full humanity and fully participating social, economic and political citizenship conveyed by the descriptor African-American was a long time coming into that area.

When Walter Bramlett used the term niggers in 1981 to describe most of the men killed in the Port Chicago explosion it seemed to me he wanted to be sure by that epithet that I understood where the Negro ranked in the general public and corresponding Armed Services perception of the order of humanity in 1944. Most Americans then did not account that Negro males should be classified as men, and they were certainly not to be counted among our men and our boys. The longstanding prejudice of naming African-American men “boys” makes it somewhat difficult to remember that all the African-American men who died at Port Chicago were, in fact, essentially boys; few were older than 21 and many of those who had volunteered in military service were big boys who lied and said they were 18 when they were actually 16 and 17 years old; some of the boys who died at Port Chicago probably were younger than 16.

Photographs and illustrations credits.

Dr. Edward Teller (1908-2003) reading "History of 10,000 ton gadget," Los Alamos Scientific Laboratory, 27 August 1980. Source: Photograph by Peter Vogel.

Smoke cloud above the explosion of S.S. *Grand Camp.* After 2-4 minutes the smoke cloud has pierced through the cloud ceiling 2,000 feet above Galveston Bay. Source: Houston, Texas, *Chronicle* newspaper.

Chapter 5

"History of 10,000 Ton Gadget": The Authors and the Bomb it describes

The "History of 10,000 ton gadget" is the most comprehensive mathematical description of the progression of the explosion of a World War II atomic bomb that the public will ever see. The larger portion of the document is manuscript; the document's legend that runs from top to bottom on the left margin is typescript. The original document consists of two sheets of paper put together with transparent "Scotch" tape. The mathematical data in manuscript notation were written across the 14 inch dimension of one 8.5 x 14 inch sheet of "legal" size paper; the legend was typed down the 8.5 inch margin of one 8.5 x 11 inch sheet of "letter" size typing paper. The two sheets of paper were trimmed and taped together, first on back of the document. With the two sheets of paper taped together on the back, the horizontal and vertical lines that divide the data and legend entries were drawn, and a strip of tape was applied to the face of the document along the vertical line that divides the typescript and manuscript portions of the document. An outline of the tape that joins the two sheets on the face of the document can be seen along the length of that vertical line.

In my first article on Port Chicago, published in the Spring 1982 issue of *The Black Scholar*, the document was reproduced and carried a copyright in my name. I claimed ownership of the document by right of possession, but clearly Paul Masters had thieved the document from Los Alamos. The right of ownership by possession is usually contravened if the licit owner of stolen property can be determined and I had determined that Los Alamos was the licit owner of the document. I held the document in a bank safety deposit box a few months less than five years. In late 1984 in the first basement of the J. Robert Oppenheimer Library at Los Alamos National Laboratory I met with Los Alamos Archivist Roger A. Meade and put the document into his hands as a voluntary gift, a donation made to the laboratory Archives. The following year I listed the gift as a charitable donation for federal tax purposes and claimed a deduction equal to the cash expenses I had made to establish authenticity of the document and to determine its licit owner, plus the $0.25 I had paid to acquire the document at the Christ Evangelical Lutheran church rummage sale in spring 1980; the donation and deduction were approved without dispute by the Internal Revenue Service.

The authors of the "History of 10,000 ton gadget."

The "History" carries no information that permits identification of its authors. In January 1981 I began a study of the Manhattan Project historical literature to determine who were the authors of the "History." The public information office at Los Alamos lab suggested I start a general study of the Project history with David Hawkins' *Manhattan District History, Project Y, The Los Alamos Project*, Volume I, LAMS-2532 (Los Alamos, 1961). I found that paragraph numbered 11.20 of Hawkins' Los Alamos history describes a part of the work accomplished at Los Alamos immediately following 1 August 1944; the description of that work provided by paragraph 11.20 is a point-by-point recapitulation of the information presented in the "History of 10,000 ton gadget":

"11.20. Much more extensive investigation of the behavior and effects of a nuclear explosion were made during this period than had been possible before, tracing the history of the process from the initial expansion of the active material and tamper through the final stages. These investigations included the formation of the shock wave in the air, the radiation history of the early stages of the explosion, the formation of the 'ball of fire,' the attenuation of the blast wave in air at greater distances, and the effects of blast and radiations of [sic] human beings and structures . . . General responsibility for this work was given to Group T-7, with the advice and assistance of W.G. Penney."

Paragraph 11.20 of Hawkins' history reported that after 1 August 1944 general responsibility for investigation of the behavior and effects of a nuclear explosion had been given to Los Alamos Group T-7, with the advice and assistance of W.G. Penney. Further close reading of Hawkins' history showed that Los Alamos Laboratories Theoretical Division Group T-7 (Damage) had been formed in November 1944 by a change of name. Theoretical Division Group T-7 had been the former Group O-5 (Calculations) of the Ordnance Division. Both O-5 and T-7 were reported to have been led by Joseph O. Hirschfelder.


Joseph Oakland
Hirschfelder
(1911-1990)

It seemed to me probable that Joseph Hirschfelder in his work with Groups O-5 (Calculations) and T-7 (Damage) would have been linked with the preparation of the "History of 10,000 ton gadget" and therefore acquainted with the Port Chicago explosion and the Port Chicago explosion fireball. Joseph Oakland Hirschfelder in 1981 was a mathematician and theoretical chemist at the University of Wisconsin, Madison. He had, I also learned, been chairman of the editorial board that produced the first comprehensive, publicly available technical account of the way nuclear fission weapons work and their effects: the 1950 U.S. Atomic Energy Commission text, *The Effects of Atomic Weapons.*

Hirschfelder had taken a double Ph.D. in theoretical physics and chemistry at Princeton University in 1936 under Eugene P. Wigner, later a prominent Manhattan Project physicist. After receiving his Ph.D., Hirschfelder spent an additional year as a postdoctoral fellow with John von Neumann at the Princeton Institute for Advanced Study. In 1937 he went to the University of Wisconsin as a Wisconsin Alumni Research Foundation research associate. At the beginning of the war for about two years Hirschfelder was with the National Defense Research Committee (NDRC) in Washington, DC, where he worked as head of the Interior Ballistics Group on a wide variety of problems including the thermodynamics of propellant gases and the fluid dynamics and combustion in the barrels of guns, mortars, and rockets. John von Neumann arranged Hirschfelder's transfer to Los Alamos in early 1944 where Hirschfelder was a group leader through the end of the war. During his time as a group leader at Los Alamos, Hirschfelder worked with Hans Bethe and John Magee on the dynamics of nuclear explosions including, specifically, the formation of the fireball and shock wave. In 1945-46 Dr. Hirschfelder was head of theoretical physics at the Naval Ordinance Test Station at Inyokern, California (China Lake), and in 1946 he was chief phenomenologist at the U.S. atomic bomb tests at Bikini Atoll in the South Pacific.

In 1946 Hirschfelder returned to Madison to become a full professor in the Department of Chemistry. He then established the University of Wisconsin Naval Research Laboratory which he directed until 1959 when it was reorganized as the University of Wisconsin Theoretical Chemistry Institute. Joseph Hirschfelder died 30 March 1990. The prestigious Joseph O. Hirschfelder Prize in Theoretical Chemistry with its $10,000 stipend is awarded annually by the University of Wisconsin Institute of Theoretical Chemistry. The Hirschfelder Prize, established in 1991, was made possible by a gift from the chemist's widow, the mathematician Elizabeth Hirschfelder.

Hirschfelder, I was confident in 1981, would certainly have known in 1944-45 how to distinguish the distinctive spherical fireball typical of a nuclear fission explosion from the amorphous turbulent mass of hot luminous gases characteristic of the explosion of conventional TNT-based munitions. The roiling cloud of hot, luminous gases that results from a chemical explosion, of which TNT and dynamite explosions are examples, is of fundamentally different appearance from the initial discrete spherical fireball of a nuclear fission explosion because of the enormous temperature and heat differences that distinguish a nuclear fission explosion from a relatively cold chemical explosion. A principal area of Joseph Hirschfelder’s work at Los Alamos was to calculate and predict the behavior of the distinctive fireball of a nuclear explosion and his work in that study was made with the thermodynamicist Hans Bethe, who first predicted the distinctive fireball characteristic of a nuclear explosion.

In 1981, I spoke with Hirschfelder at the University of Wisconsin at Madison, provided to him a copy of the “History of 10,000 ton gadget,” and asked him what his role had been in the preparation of that document and what signified that document’s reference to the Port Chicago ball of fire as having been typical of a nuclear explosion.

Professor Hirschfelder, I had been told, ever refused to discuss his wartime activities at Los Alamos as he did when I spoke with him. He declined to discuss the document or to explain the document's reference to Port Chicago.

William George Penney
(1909-1991)

Paragraph 11.20 of David Hawkins' history reported that W.G. Penney had provided advice and assistance to Hirschfelder's Group T-7 (Damage) after 1 August 1944 in that group's investigation of the behavior and effects of a nuclear explosion. William George Penney was born at Gibraltar 24 June 1909; his death came 3 March 1991 in East Hendred, England. During the early 1930s Penney spent two years at the University of Wisconsin, Madison, before he received a doctorate from the University of Cambridge in 1935. In spring 1944 William George Penney was Professor of Applied Mathematics, Imperial College of Science and Technology, University of London.

Later knighted for his service to the Commonwealth in the development and successful test of Britain's first atomic bomb, 3 October 1952, Penney often is designated the "Oppenheimer of Britain." Prior to World War II Penney's area of scientific specialty was the physics of hydrodynamic waves, both shock waves and the more familiar ocean waves, known as "gravity" waves. During 1943 and early 1944 Penney designed and supervised development of the mobile breakwaters that would be emplaced by the Allies off the Normandy beaches during the opening phases of the D-Day invasion to degrade the potentially treacherous interaction of energetic Atlantic ocean waves and the personnel and water craft that would invade the Normandy beaches through the hazards of the Atlantic rollers.

Geoffrey Ingram (G.I.) Taylor was an extraordinary British physicist one generation older than William Penney. During World War II, as he had during World War I, Taylor applied his scientific expertise to military problems including the propagation of blast waves in both air and underwater explosions. Almost from the beginning of the Manhattan Project Geoffrey Taylor was a consultant to the Manhattan Project program at Los Alamos; he was, in the final account, a major theoretical and practical scientific contributor to the intricate design of the atomic bomb tested at Trinity site in New Mexico and detonated in combat at Nagasaki. Early in 1944 Taylor had arranged that William Penney should join the atomic bomb development program at Los Alamos. Penney departed London for Los Alamos shortly before D-Day and arrived at Los Alamos in the third week of June 1944.

Penney's principal assignment at Los Alamos was to develop theoretical predictions of damage effects from the blast wave of an atomic bomb. But his expertise in the hydrodynamics of ocean waves was enlisted to theoretically investigate the effects of underwater atomic bomb detonations; theoretical investigation was augmented by an experimental program of very small explosions conducted in the Anchor Ranch explosion pond at Los Alamos. On 17 July 1944 theory supported by the minuscule explosions made in the Anchor Ranch pond yielded to analysis of the water waves that resulted from the Port Chicago explosion; seventy-five per cent of the weight of explosive detonated at Port Chicago was submerged, below the water line, in the lower cargo holds of the exploded ship *E.A. Bryan.*

There is no doubt in my mind that, in addition to Penney's participation in analysis of the water waves that resulted from the Port Chicago explosion, Penney also participated in review of the various Los Alamos analyses of the Port Chicago explosion blast wave in the air, analyses made in the days, weeks and months following the explosion. Prediction of the damage effects from the blast wave of an atomic bomb was William Penney's principal assignment at Los Alamos.


William Sterling Parsons, USN
(1901-1953)

William Penney's significant contributions to the wartime work at Los Alamos can be broadly comprehended with recognition that within a few weeks of his arrival he was added to the core group of scientists there who made all key decisions in the direction of the program. The others with whom he shared that duty and responsibility were Los Alamos Laboratories Director J. Robert Oppenheimer; Los Alamos Laboratories Associate Director Captain William Sterling Parsons, USN; the physicist, theoretician and mathematician John von Neumann; and the brilliant physicist and operational planner Norman Ramsey.

Penney was an observer at the 16 July 1945 bomb test at Trinity site in New Mexico; on 9 August 1945 he witnessed the bombing of Nagasaki

J. Robert Oppenheimer
(1904 – 1967)

from one of the observation planes that accompanied the Nagasaki mission bomber *Bock's Car;* he was a member of the U.S. team of scientists and military analysts who entered the rubble of Hiroshima to assess the effects of the atomic bomb that was detonated there 6 August 1945. At Bikini Atoll in July 1946 he joined Joseph O. Hirschfelder and other scientists from Los Alamos who had responsibility for the program to test two U.S. atomic bombs at Bikini in Operation Crossroads. After completion of the analyses of the Bikini tests Penney returned to England to undertake development of the first British atomic bomb.

I had no opportunity to speak with Lord Penney until the summer of 1990 when I provided to him a copy of the "History of 10,000 ton gadget" and asked him what his role had been in authorship of that document and what he had known of the Port Chicago explosion. Sir William told me he had had no knowledge of the Port Chicago explosion, and had not been acquainted with the "History of 10,000 ton gadget" until he received a copy of that document from me. The Port Chicago explosion, he said, had not been discussed within the scope of his associations during the time he was with the Manhattan Project at Los Alamos. Specifically, Penney said neither Los Alamos Laboratories Director J. Robert Oppenheimer nor Los Alamos Laboratories Associate Director Captain William Parsons had ever mentioned the Port Chicago explosion in his hearing.

August 1945, Tinian Island, South Pacific. Left to right: Captain William Sterling Parsons, USN, 1901-1953, Associate Director Manhattan Project Los Alamos Laboratories; bomb commander, Hiroshima combat bombing mission. Rear Admiral William R. Purnell, USN, Navy member Atomic Bomb Military Policy Committee. Brigadier General Thomas Farrell, USA, Administrative Deputy Director Manhattan Project. Source: U.S. National Archives.

That assertion was either an error of memory or a deliberate misrepresentation of fact. I did not have the impression in telephone conversations with Sir William, nor from the text of his letters to me, that his recollection of events related to his participation in the development of the first nuclear fission weapons at Los Alamos was clouded.

Paragraph 11.20 of Hawkins' official Manhattan Project history attests that William Penney provided advice and assistance to the work of Joseph Hirschfelder's Los Alamos Group T-7

(Damage). The work of Group T-7 in defining the phenomenology of nuclear weapons explosions is shown to have been summarized by the “History of 10,000 ton gadget,” in which the Port Chicago explosion fireball is characterized to have been typical of a nuclear explosion.

There is documentary evidence which conclusively shows that William Penney was cognizant of the Port Chicago explosion prior to 31 August 1944 and that he contributed to scientific analyses of the effects of the explosion. The 16-page report, “Effects of the tidal wave in the Port Chicago explosion of July 17, 1944,” was researched and written by the civilian Los Alamos physicist Maurice Mandel Shapiro. Ph.D., and transmitted 31 August 1944 by Capt. Parsons to his superior officer Rear Admiral William R. Purnell in Washington, DC. Admiral Purnell was the Navy member of President Roosevelt’s three-man Atomic Bomb Military Policy Committee. In paragraph H, page 11, of his report on the Port Chicago tidal wave Dr. Shapiro wrote:

Dr. Maurice Mandel Shapiro,
Chief Scientist Emeritus,
Laboratory for Cosmic Physics,
U.S. Naval Research Laboratory
(1915-2008)

“Another consideration which throws some light on the probable wave height has been suggested by Dr. W. G. Penney. If the initial wave behaved as a solitary wave, then it would have tended to instability as its height approached a value equal to the depth of the water. Since the depth at the point in question [the southern tip of Roe Island] was about 5 feet, the wave would probably have attained no greater height than this.”

On page 15 of this undated report on the effects of the Port Chicago tidal wave, but which was transmitted 31 August 1944, Dr. Shapiro wrote, “It is interesting to compare the wave effects in the Port Chicago explosion with those observed in our model experiments in the explosion pond at Anchor Ranch. We shall apply similitude relations deduced by W. G. Penney in his hydrodynamic theory of surface explosions. In this theory it is assumed that a known impulse is delivered to a water surface over a finite circular area. . . .”

This Port Chicago explosion tidal wave analysis concludes on page 16 with Dr. Shapiro’s remark, “Considering the large error involved in these estimates, the presence of shelving at the Roe Island bank, and the fact that the mean depth of water in the channel was much less than

200 feet, the agreement between the Port Chicago wave amplitudes and those predicted by Penney's theory is good."

The Anchor Ranch technical area at Los Alamos and the experimental explosion pond constructed there actually took that name from the name of the privately owned Anchor Ranch adjacent to Los Alamos Ranch School for Boys; both properties had been taken by the Government in late 1942 to establish the site of Los Alamos Laboratories.

William Penney had mathematically formulated his hydrodynamic theory of surface explosions before his arrival at Los Alamos in late June 1944; but experiments in which he participated that were conducted at the Anchor Ranch explosion pond with explosions of two ounces of pentolite at the surface of water two feet deep provided small scale demonstrations of Penney's theory, and the Port Chicago explosion provided field-scale confirmation of that theory. In fact, all the physical phenomena of the Port Chicago explosion would later provide comparative examples and effects data important in analysis of the first British nuclear bomb test, Operation Hurricane conducted 3 October 1952 off the west coast of Australia in the Monte Bello Islands. Operation Hurricane was organized and directed by William Penney and proved a bomb his team of British scientists had designed and built; that bomb was fundamentally the same device described by the "History of 10,000 ton gadget."

The circumstances of the first British atomic bomb proof made in Operation Hurricane in several important ways were remarkably correspondent to the circumstances of the Port Chicago explosion, in which the first U.S. atomic bomb was proven. The 25 kilotons TNT equivalent energy yield of the weapon detonated in Operation Hurricane was 25 times greater than the nominal 1 kiloton TNT equivalent energy of the first U.S. nuclear fission weapon proven at Port Chicago, but the similarities of the two explosions begin with recognition that both explosions originated and were "barricaded" within the confines of a blue water ship.

The British weapon detonated in Operation Hurricane was located within the hull of the aging 1944 River-class frigate HMS *Plym. Plym* was a relatively small, fast, shallow-draft gunboat which displaced

1,370 tons and was 301 feet overall length. *Plym* was anchored 400 yards off Trimouille Island beach in water 40 feet deep; the center of gravity of the explosion was 10 feet below the water line. At Port Chicago, the exploded Liberty ship *E.A. Bryan* was a deep draft, wide beam cargo ship that displaced 14,245 tons and was 441 feet overall length. The *E.A. Bryan* was moored to a pier 300 yards off the shoreline of the Port Chicago Naval Magazine and the center of gravity of the main munitions explosion within the ship's cargo holds was 10 feet below the water line, in water depth a little less than 40 feet. Those circumstances are typical of merchant ships either dockside or at anchor in the majority of the world's maritime harbors and ports.

Operation Hurricane and the weapon proof conducted at Port Chicago were both intended to ascertain the military effects of a nuclear weapon carried by ship into a port facility or harbor and detonated. The Port Chicago explosion had provided baseline data for a 1 kiloton tactical atomic bomb detonated in a port, and those Port Chicago data were augmented by data obtained from the Bikini atomic bomb tests that involved a variety of ships, large and small. The principal objective of Operation Hurricane, beyond a proof of the first prototype British nuclear weapon, was to ascertain the immediate and long-term radiation effects of an atomic bomb that might be smuggled by ship into a British port and detonated. In the early 1950s the threat of an atomic bomb carried into a port cached among the cargo of an innocent-looking merchant vessel, and then detonated, was of great concern to the maritime British with their many ports and harbors. That same threat to the port or harbor of any of the world's maritime nations is no way diminished today, and the miniaturization of nuclear fission weapons greatly facilitates concealment of an atomic bomb among a ship's containerized merchant cargo.

Destruction of a maritime port was the first suggested military application for a U.S. atomic bomb. Albert Einstein's letter of 2 August 1939 to President Franklin Delano Roosevelt proposed the delivery of an atomic bomb to a port by boat, considering that the weapon would likely be too heavy to be transported and delivered in combat by aircraft. In that letter Einstein informed the President that nuclear fission was on the threshold of development and that atomic bombs would be the first practical consequence. Einstein explained:

“This new phenomenon would also lead to the construction of bombs, and it is conceivable—though much less certain—that extremely powerful bombs of a new type may thus be constructed. A single bomb of this type, carried by boat and exploded in a port, might very well destroy the whole port together with some of the surrounding territory. However, such bombs might very well prove to be too heavy for transportation by air.”

In the spring 1982 publication of my first article on the Port Chicago explosion I attributed authorship of the “History of 10,000 ton gadget” to Joseph Hirschfelder with the assistance of William Penney. I made that attribution despite Joseph Hirschfelder’s 1981 refusal to confirm or deny his contribution to the “History,” and eight years before my 1990 discussions and correspondence with William Penney. More than 10 years after I published that attribution of authorship, the Manhattan Project history *Critical Assembly*, prepared by the U.S. Department of Energy and published by the Cambridge University Press in late December 1993, confirmed that attribution on pages 343-344:

“By January 1945, Hirschfelder and British physicist William J. [sic] Penney had gathered a great deal of data from Britain on the structural damage caused by German high-explosive bombs. These data proved extremely useful in the group’s further calculations, and by the next month it had developed a hypothetical ‘history’ of the explosion of a nuclear weapon with the explosive power of 10,000 tons of TNT.”

Although Paul Masters recalled in conversation with me in 1980 that he had removed the “History” from Los Alamos in autumn 1944, the information provided by *Critical Assembly* sets January or February 1945 as the date that document was prepared. The date that document was prepared, a minor detail, remains uncertain. Hirschfelder and Penney by January 1945 had gathered a great deal of information on structural damage caused by German high-explosive bombs that fell on England; but the phrase “by the next month” is vague. Interpretation of that phrase could mean the “History” had been prepared by the beginning of February 1945 or by the end of that month.

The type of bomb described by the "History of 10,000 ton gadget."

The "History of 10,000 ton gadget" is a technical document that provides a complex mathematical model of the detonation and anticipated physical effects of the atomic bomb proof-fired at Trinity site in New Mexico 16 July 1945. That bomb design, with combat modifications, was detonated at Nagasaki 9 August 1945. The energy of the weapon described by the "History" is equivalent to 10,000 tons of TNT (10 kilotons, or abbreviated as 10 kt). The document mathematically models a "nominal" 10 kt atomic bomb explosion.

During the theoretical period of the atomic bomb program a 10 kt TNT equivalent atomic bomb was calculated as the minimum energy of explosion that would fulfill the Manhattan Project's specific mandate to produce a militarily-decisive atomic bomb for use during the war. A bomb of nominal 10 kt TNT equivalent energy yield was therefore established as the practical objective of the Project.

As represented by the "History," a nominal 10 kt atomic bomb was used as the basis for general theoretical descriptions of an atomic bomb explosion, but the effects of atomic bombs of greater and lesser energy could be computed easily from the benchmark description of a 10 kt explosion. A bomb of 10 kt yield, or greater than 10 kt, would constitute a strategic, militarily-decisive weapon. Bombs of energy less than 10 kt would be applicable to tactical military uses, but one or several tactical nuclear bombs would not necessarily be militarily decisive. The contemplated strategic weapons of 10 kt, and greater energy yield, would destroy the major part of a city, as occurred at Hiroshima and Nagasaki; tactical weapons would destroy lesser, limited targets as, for example, a military or commercial maritime port as occurred at Port Chicago.

The prototype weapon design that was proof fired at Port Chicago was a nominal 1 kt tactical device, but the realized energy yield of the prototype detonated at Port Chicago was much less than the nominal 1 kt combat potential of that weapon. The explosive energy of the Port Chicago device, in order to conserve the very limited supply of fissionable uranium available by July 1944, was intentionally limited to the minimum that was calculated to be necessary to sustain a productively efficient, recognizable nuclear fission chain reaction explosion; reduction of the amount of fissionable uranium available in the device to that minimum constrained the energy yield of the Port Chicago device to about 300 tons TNT equivalent. That minimal nuclear fission explosion, however, sympathetically detonated all the conventional military munitions loaded in the exploded ship and those that were emplaced next to the ship on the Port Chicago Magazine ship loading pier. Those conventional munitions exploded with an energy equal to the high order detonation of 1.5 to 2.1 kt of TNT. The combined explosive result was equivalent, as an order of magnitude, to the 1 kt energy yield that prototype tactical weapon was forecast to produce when it would be optimized for combat use. The fireball and succeeding column of flame that instantly rocketed to 10,000 feet above the primary nuclear explosion at Port Chicago was easily recognized by those who had predicted that typical characteristic of a nuclear explosion.

The Trinity/Nagasaki weapon described by the "History of 10,000 ton gadget" was a spherical implosion design. Within a heavily armored exterior steel ballistic case, an inner spherical steel encasement contained the spherical, functional bomb components. The fissionable component of the Trinity/Nagasaki bomb was a ball of essentially pure plutonium-239 located as the central core of the weapon; that active component of the weapon has been reported by some accounts to have been the size of a grapefruit, reported by other accounts to have been the size of a chicken egg, and suggested by some writers to have been the size of the human eyeball. The 21 kt energy yield of the Trinity device, however, reasonably suggests the plutonium core was more grapefruit-sized than less.

The plutonium core contained within it a small manufactured mechanism called the urchin, so called because it physically resembled the common spiny sea urchin that, in death, leaves a bulbous, bumpy five sided penta-radial, thin calcareous shell up to four inches across seen washed onto ocean beaches. In life the sea urchin protrudes a dense array of brittle spines that are the animal's principal defense against being eaten and the means by which it can gather food and carve a protective niche for itself in soft rock. The urchin within the plutonium core of the Trinity/Nagasaki bomb physically resembled the live sea urchin: spine-like projections from the centrally located urchin extended into the plutonium core. When the bomb was detonated, which is to say imploded by a surrounding mantle of conventional high explosives, the plutonium core was radically compressed; urchin and urchin spines were crushed and disrupted. Seven grams of beryllium and 50 curies of a polonium^{210} alpha source, segregated in the urchin spines, were instantaneously mixed. When exposed to alpha particles, beryllium emits neutrons. The resulting beryllium-polonium nuclear reaction released vast numbers of neutrons within the plutonium core and thereby pervasively initiated the explosive neutron-induced plutonium nuclear fission chain reaction.

The abundance of neutrons produced by the urchin promoted initiation of the nuclear fission chain reaction and greatly increased the efficiency of the chain reaction, but as the detonation progressed only a few percent of the plutonium core atoms of the Trinity/Nagasaki weapon were subject to fission before the heated core began to expand and plutonium atoms within the core that had not fissioned became more widely separated than in the compressed state. As the core expanded, and the distance between plutonium atoms increased, the likelihood that a fission-inducing neutron would collide with any Plutonium nucleus and induce fission was diminished. Misses rather than hits became more probable in the expanding fissile core. To partly overcome the tendency of the bomb to blow itself apart before the chain fission reaction was as complete as possible the plutonium core was enclosed within a heavy shell of depleted uranium, the tamper. Depleted uranium (DU) is highly refined uranium from which most of the atoms susceptible to fission have been removed. The tamper also served to reflect neutrons that reached the periphery of the core; those neutrons, without reflection into the core, would have been lost to the continuing chain reaction.

Depleted uranium, like lead and gold, is a material of very great density. Depleted uranium has a very high mass density compared to cotton. The energy required to move a material body of very high mass density is much greater than the energy required to move a material body of very low mass density of the same size: a puff of breath will move and rapidly accelerate a 1-inch cotton ball, but a puff of breath will not disturb a 1-inch ball of uranium. The tendency of a body at rest to remain at rest or of a body in motion to stay in motion in a straight line unless disturbed by an external force is defined in physics as the inertia of that body. The inertia of uranium is very great compared to cotton. The inertia of depleted uranium in motion, its momentum, recommends its use as the material of which armor piercing anti-tank rockets and gun projectiles are manufactured because an accelerated uranium projectile is very difficult to stop; a projectile of depleted uranium traveling at 3,000 feet per second will pass through several inches of military armor steel plate; higher velocities increase the penetrating power of the projectile.

Difficult to stop, a mass of depleted uranium is also difficult to move. The plutonium core of the Trinity/Nagasaki weapon was enclosed by a depleted uranium tamper that significantly resisted the expansion of the heated core and contained the fissioning material very briefly, but for the sufficient small fraction of a second necessary to permit the fission chain reaction to proceed more completely through the core material than would have been realized without the confining effect of the tamper. However, the rapidly increasing pressure of the expanding plutonium core very quickly overcame the inertia of the depleted uranium tamper and the tamper was disintegrated and vaporized.

Surrounding the core and tamper of the Trinity/Nagasaki bomb design was a spherical mantle of molded high-explosive blocks, tightly fitted together and each shaped in design so that when detonated simultaneously the combination achieved the effect of a focusing optical lens. When the explosive blocks were detonated most of the released energy was focused inward toward the core of the weapon; predominantly an *implosion* rather than an *explosion*. When the explosive blocks were simultaneously detonated they produced a powerful, inward moving, focused spherical pressure wave, the detonation wave; the detonation wave progressed rapidly through the detonating explosive to the interface of the explosive with the depleted uranium tamper. The detonation wave at the tamper interface produced a pressure of several million pounds per square inch uniformly on the surface of the tamper. The tamper under that influence became radically compressed and transferred the energy of the detonation wave against the plutonium core. In motion, the momentum of the now exceedingly dense, compressed tamper moved inward against the core, against the resisting mass of the plutonium core, which was of essentially the same mass density as the tamper before compression.

The millions of pounds per square inch pressure exerted by the tamper compressed the core plutonium to about the size of a mote. During compression of the core the urchin spines were ruptured, which released swarms of neutrons to effectively initiate the nuclear fission chain reaction. As the full power of the fission chain reaction exploded, the tamper was disintegrated and vaporized by the shock wave of energy released by the fission reaction. The expanding shock wave disintegrated and vaporized the inner spherical steel encasement and immediately disintegrated and vaporized what remained of the bomb’s armor-plate exterior case. The shock wave then emerged into the surrounding atmosphere where it expanded with great speed and tremendously destructive force as a very hot, high-pressure blast wave in air.

How a nuclear explosion proceeds.

The sudden liberation of energy by an explosion, chemical or nuclear, causes a sudden increase of temperature and pressure surrounding the explosion; materials present in the explosion are converted to very hot, luminous gases that expand rapidly and create a pressure or shock wave in the surrounding environment. The characteristic of a shock wave is a sudden increase of pressure at the wave front expanding into the surrounding medium—air, water or earth—with a gradual decrease of pressure behind the front. A shock wave in air is generally called a blast wave because it resembles and is accompanied by a very strong wind. In water or in the ground, shock wave, rather than blast wave, is the proper term because in water and ground the effect is like that of a sudden impact.

At very early times in the development of a nuclear explosion, beginning in less than a microsecond, the explosive shock wave is formed and driven by the energy of the expanding bomb debris; the temperature at that moment is several tens of millions of degrees. Because of that intense heat, all the fission products, bomb casing and other weapons parts are converted to the gaseous form. Within less than a millionth of a second of the detonation of a nuclear fission weapon, the extremely hot weapon residues radiate large amounts of energy, mainly as X rays. Approximately 85 percent of the energy of the explosion during this early stage is the kinetic energy of nuclear fission fragments present in the form of “soft” X rays. Within the X-ray portion of the electromagnetic spectrum soft X rays have relatively longer wavelengths and relatively lower energies than “hard” X rays. The initial energy of the explosion is distributed between soft X rays and shock wave energy. The proportions are determined by the nature of the medium in which the bomb explodes.

When an explosion takes place in a medium of high density—like water or earth—a larger percentage of the X-ray energy of the fission fragments is immediately converted to heat energy than is the case when an explosion takes place in the less dense medium of air. In a water or earth medium the emitted X rays are quickly stopped and their energy converted to intense local heat, which reduces the energy available to the shock wave; in the less dense medium of air, X rays travel a relatively greater distance before an interaction with the more widely separated atoms and molecules of the atmosphere. Consequently, in air a greater portion of the energy of explosion is available to blast wave. The X-ray energy imparted to the atoms and molecules of the atmosphere is so great and the temperature generated so high that an instantaneous brilliant flash of visible white light is emitted by those superheated gasses. In a nuclear explosion in air, where the air density does not differ greatly from sea level, most of the X rays, which constitute the primary thermal radiations, will be absorbed within a few feet of the explosion. It is in this manner that the initial fireball is formed in an air or surface burst.

The characteristic white flash of light generated by a nuclear explosion immediately precedes formation of the explosion fireball; the fireball follows the luminous flash and remains luminous for several seconds or minutes, depending on the energy yield of the weapon. The surface temperature of the fireball, upon which the brightness, or luminance, depends, does not vary greatly with the total energy yield of the weapon. The observed brightness of the fireball in an air burst close to sea level is roughly the same, regardless of the energy yield of the weapon.

As an explosion in air proceeds, the blast wave expands into the surrounding atmosphere until the energy of the blast wave has been dissipated by the resistance of the air that the wave front encounters and moves through. The blast wave finally ceases to exist as a manifestation of the explosion when the pressure at the wave front is equal to the ambient air pressure, which at sea level is 14.7 pounds per square inch.

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Joseph Oakland Hirschfelder (1911-1990), Professor of Chemistry, University of Wisconsin, Madison. Source: University of Wisconsin, Madison. Used with permission.

William George Penney (1909-1991), postwar Director of Atomic Weapons Research and Development at Aldermaston, England; Chairman, Board of the Atomic Energy Authority, U.K. Source: School of Mathematics and Statistics, University of St Andrews, St Andrews, Fife, Scotland. Used with permission.

J. Robert Oppenheimer (1904-1967), Director Manhattan Project Los Alamos Laboratories. Source: Los Alamos National Laboratory.

August 1945, Tinian Island, South Pacific. Left to right: Captain William Sterling Parsons, USN (1901-1953), Associate Director Manhattan Project Los Alamos Laboratories; bomb commander, Hiroshima combat bombing mission. Rear Admiral William R. Purnell, USN, Navy member Atomic Bomb Military Policy Committee. Brigadier General Thomas Farrell, USA, Administrative Deputy Director Manhattan Project. Source: U.S. National Archives.

Maurice Mandel Shapiro, Ph.D. (1915-2008), Chief Scientist Emeritus, Laboratory for Cosmic Physics, U.S. Naval Research Laboratory. Source: Photo by Dr. Arthur E. Smith. Copyright by Dr. Arthur E. Smith. Used with permission. Photo available at:

http://home.hiwaay.net/~smithae/shapiro.html.

Chapter 6

“History of 10,000 ton gadget”: Critical explanation and analysis

The Document.

The complexities of the “History of 10,000 ton gadget” will present no obstacle to readers whose interest and study have been directed to mathematics and physics, but for those whose interests have inclined to other areas of accomplishment an introduction to the terms and relationships of which the “History” is constructed will be helpful. The first document term that requires definition appears in the legend entry for Step 6 of the document at the left margin: isothermal sphere.

Isothermal sphere.

Much of the vocabulary of the physical sciences consists of ancient Greek and Latin words that have very specific meaning handed down without change for two thousand years and longer. But if a descriptive word required in a modern scientific context is not available from the Greek or Latin an appropriately descriptive word may be invented for modern use, a word that can combine defined Greek or Latin words or word elements. *Isotherme* is a French word that means “having the same temperature,” but the French is a combination of the Greek *isos*—meaning same or equal—and the Greek *therme,* which means heat; *therme* is itself derivative of the Greek word that means hot, *thermos.* An isothermal sphere, in the context of the “History of 10,000 ton gadget,” refers to one of the earliest phenomena of a nuclear explosion noted in Step 6 of the “History.” The isothermal sphere is a sphere of very hot gases of practically equal temperature throughout.

At the periphery of the isothermal sphere a very powerful initial shock wave is formed. The shock wave at the front of the isothermal sphere immediately propagates radially from the stable isothermal sphere and produces the intensely luminous ball of fire (fireball) that is typical of a nuclear explosion; from the periphery of the fireball a radially propagated energetic blast wave emerges. The isothermal sphere makes no significant contribution to the immediate military effects that result from the detonation of a 10 kt atomic bomb except that the isothermal sphere is so very hot that any material substance within 126 meters of the detonation will be converted to vapor.

Destruction of a military target by a nuclear fission weapon is comprehended mainly by the effects of a powerful blast wave in air but the more distant military consequences of a nuclear explosion can be significantly augmented by the thermal (heat) radiations emitted by the fireball that emerges from the isothermal sphere. Furthermore, accurate anticipation of the thermal characteristics and physical behavior of an isothermal sphere as it endures and rises above a nuclear explosion can be useful as a basis to predict the military and environmental effects of radioactive fallout since most of the dangerous fission byproducts of an atomic bomb detonation are concentrated in the isothermal sphere. The changing radius, pressure and temperature of the isothermal sphere permit calculation of the height the radioactive debris contained by the isothermal sphere will rise above the explosion, and thereby to predict how the radioactive debris will probably be precipitated locally and distantly according to the atmospheric conditions and winds prevailing at the maximum altitude to which the radioactive materials will be raised by the residuum of the very hot isothermal sphere.

Scientific abbreviations used in the document.

The scientific abbreviations written along the top line of the “History” identify the type of information that appears in the columns beneath those abbreviations; for most readers those abbreviations require explanation.

(t Millisec). The time (t) required to complete each of the 11 Steps identified in the legend is expressed in milliseconds (1 second is equal to 1,000 milliseconds). The ancient Greeks divided the day into 24 hours but why, in the obscure past, the hour was divided into 60 minutes and the minute divided into 60 seconds, is unknown. Speculation proposes that the number 60 was taken for those divisions because that number has the mathematical facility of "harmony and elegance"; 60 is divisible by a large number of small numbers without remainders: 1, 2, 4, 5, 6, 10, 12, 15, 20, 30. Time is not an absolute constant but is affected by motion and gravity. In an experiment conducted in 1971 atomic clocks were carried on two high-speed aircraft. One traveled eastward in the rotational direction of the Earth, and one westward. After the flight, the onboard clocks were found to have either lost or gained time (relative to a ground-based atomic clock) depending on their direction of travel, thus confirming a predicted effect of relativity.

(R Meters). R is the symbol that denotes radius, a line segment that joins the center of a circle or sphere with any point on the circumference. The diameter of a circle or sphere is twice the radius. The distance from the original position of the exploded bomb system to the unobstructed periphery of the effects of the explosion is defined as the radius (R) of the explosion and the unit of measure is the meter (m). The meter was redefined in 1960, after much dispute, as the length equal to 1,650,763.73 wavelengths of the orange-red radiation of the krypton 86 isotope in a vacuum. The customary U.S. measure of one foot is equal to 0.3084 meter and the U.S. measure of one yard, three feet, is thus slightly less than one meter. At the moment of detonation the bomb system is .70 meter radius. Completion of each Step in the progression of the explosion modeled by the "History" has increased the radius from the detonation point at which the expanding effects of the explosion are distinct.

(*$\overset{o}{R}$ ***m/sec).*** The small dot above the R symbol for radius denotes that the changes of radial distance entered in the previous column of data will be considered. In this column the speed at which the radius of the system changes will be calculated as the measure of radial change (meters) relative to the time (seconds) that comprehends that change expressed as meters per second (m/s). Radial change within the bomb system, measured in meters, is initially small. When the bomb is detonated the radius of the system is immediately decreased by implosion and compression from .70 meter to .23 meter; that change of radius is accomplished in .067 millisecond and is therefore a very rapid change, accomplished at the rate of 7 x 10^5 meters per second. Following the initial speed and reduction of the bomb system radius by implosion, the radius of the bomb system begins to expand in explosion, but the rate of expansion is slower than the initial rate of change that was accomplished by the originating implosion.

(P bars). P is the symbol employed by physicists to designate pressure. A barometer measures and displays atmospheric pressure; the unit of measure that expresses atmospheric pressure is the bar—derived from the Greek word *baros,* which means weight. One bar is the unit of pressure equal to the pressure of the weight of the atmosphere exerted upon the surface of the Earth at sea level, 14.7 pounds per square inch (psi). The figures in this column report the different air pressures that accompany the explosion at different times and corresponding distances. As the shock wave at the front of the isothermal sphere separates from the isothermal sphere and moves out from the center of the explosion as a blast wave in air, the pressure at the blast wave front diminishes. The initial energy of the blast wave is progressively distributed to a larger wave front area as the circumference of the (spherical) blast wave front expands from its origin; therefore the pressure at any point on the expanding blast wave front is diminished. The pressure, or energy, of the blast wave front at increasing distance is also diminished because energy of the blast wave is expended to overcome the resistance of the atmospheric gases and suspended particulate matter through which the blast wave expands.

(T). The temperature (T) of the explosion at various times is expressed in degrees Kelvin (°K), a temperature scale in which absolute 0 equals -273.16° on the Celsius (C) temperature scale. The Celsius temperature scale registers the freezing point of pure water as 0°C and the boiling point of pure water under normal atmospheric pressure as 100°C. The Celsius scale in popular use is still frequently called the Centigrade scale but in 1948 by international agreement the centigrade scale was officially designated by the name of the man who devised it, the Swedish astronomer Anders Celsius (1701-1744). The Fahrenheit scale, generally used in the United States to report weather temperatures, is named for its inventor Gabriel Daniel Fahrenheit (1686-1736), a German physicist who lived in Holland. For scientific purposes the Fahrenheit scale is cumbrous because it sets the freezing point of a salt-water mixture, rather than pure water, as 0°F and the boiling point of a salt-water mixture at 212°F under normal atmospheric conditions. For general weather reporting purposes when the temperature in Phoenix, Arizona, is 121°F most Americans know it's very hot in Phoenix; and when it's -60°F in Fargo, North Dakota, most Americans know it's really cold in Fargo. A temperature of 60°F converts to 15.555°C and 288.655°K.

The subscripts: s, i and o used in the document.

We notice that the first two data column headings, P bars and T (°K), that appear along the top line of the "History of 10,000 ton gadget," which we have reviewed, each carries the subscript, s: P_s bars and T_s (°K). To the right of those columns are four data column headings that carry the subscript, i: R_i Meters, P_i bars, T_i (°K), and the data column to the right of T_i (°K), which introduces the subscript, o: p_i/p_o.

The values presented in the columns identified with the subscript, s, are the pressure and temperature of the shock wave (s), through Step 6. At the beginning of Step 7 the shock wave has hydrodynamically separated from the isothermal sphere front and has become the expanding blast wave. For the purposes of this document the changing pressures and temperatures of the expanding blast wave, beginning in Step 7, are represented as extensions of the shock wave (s). The values calculated in the four columns identified with the subscript, i, represent the changing radius and pressure, and the estimated temperature and estimated gas density variations of the isothermal sphere (i) as it decays.

p_i/p_o.

The descriptive notation in the data column heading p_i/p_o seems to be the lower case letter p, but is the 17th letter of the Greek alphabet, rho, in the lower case, ρ. Rho, in the lower case, is the symbol used in physics to represent air density and is used to express the concept of the aggregate mass of molecules per volume of air, composed mostly of nitrogen (78 per cent) and oxygen (21 per cent). Estimations of the changing density of the gas present in the isothermal sphere were necessary to allow quantitative prediction of the characteristics of the shock wave that would develop from the isothermal sphere in the course of the explosion of a 10,000 ton gadget.

The variable properties of a gas are the gas pressure (P), temperature (T), mass (M), the volume (V) that contains the gas, and the gas density (p). These variables are related to one another, and the values of these properties determine the state of the gas and its thermodynamic characteristics.

Thermodynamics is the branch of physics which deals with the energy and work of a system and was born in the 19th century as scientists were first discovering how to build and operate steam engines. An atomic bomb is an engine that does work. The energy of a nuclear explosion is the product of nuclear fission, and the work of a nuclear bomb explosion is the application of that energy to military purposes, principally as the energy of the shock and blast wave in air. The changing density of the isothermal sphere gas significantly affects the characteristics of the shock wave that emerges from the isothermal sphere.

The characteristics and thermodynamic properties of air at sea level are often employed as a comparative standard for studies that describe the characteristics and thermodynamics of gas behavior in conditions that differ from sea level. The standard density of air at sea level is 1.229 kg/m^3 at 15°C and pressure 101.3 k-pascals (metric), or .00237 $slug/ft^3$ at 59°F and pressure 14.7 lbs/in^2 (English). The Greek letter rho in the lower case (and sometimes, r) is the symbol which specifically designates air density; when used with the subscript, o, the density of air at sea level is represented, p_o.

The data in this column estimate the changing density of the isothermal sphere gas. The isothermal sphere variables of pressure and radius, and therefore volume, were calculated with reasonable certainty as they would change during specific intervals of time. From those calculated values of pressure and volume, the temperature and density of the isothermal sphere gas could be generalized, but with a considerable factor of uncertainty because the necessary solving calculations were so complex. The data in the T_i (°K) column and the data in the p_i/p_o column, which are derived from the calculated pressure and volume of the isothermal sphere, "may be wrong by a factor of 2."

In their 18 July 1945 Los Alamos Report 296, "Opacity and thermodynamic properties of air at high temperatures," Joseph Hirschfelder and John L. Magee wrote, "It seems to us highly desirable that accurate tables of the thermodynamical properties of air be computed [for the high temperatures involved in a nuclear explosion]. This project would be easy to set up but the actual computations are sufficiently difficult that it would require approximately ten people for one year."

From Step 3 through Step 5 of the "History of 10,000 ton gadget" the density of the exploding bomb system declines from an initial density equal to the interior of the Sun to a value that, in Step 6, is usefully comparable to the density of air at sea level. In Step 6, when the radius of the isothermal sphere is 10.50 meters and the pressure is 16,160 bars, the density of the isothermal sphere gas, p_i, is estimated to have declined to 1.48 the density of air at sea level, p_o. Through the process of Step 7, as the radius of the isothermal sphere and fireball expand and their respective pressures decrease, the density of the isothermal sphere gas diminishes radically. In Step 8, when the fireball and isothermal sphere are fully expanded, and the pressure in each has declined to approximately 2 bars and 1 bar, the density of the isothermal sphere gas is extremely low: .0015 the standard density of air at sea level.

Illumination.

Representations in this column estimate the intensity of unobstructed light produced and perceptible at 10,000 yards consequent to the explosion of a 10 kt atomic bomb in the lower atmosphere. In Step 6 the first flash of brilliant light emitted by the isothermal sphere, perceived at 10,000 yards, is calculated to be 36 times the illumination of the Sun received directly on one square foot of Earth's surface at noon on a clear day in the middle northern latitudes at the summer solstice. All photometric concepts are based on the idea of a standard candle flame. Ordinary outdoor scenes in daylight have an average luminance of several hundred candles per square foot. More technically, when light falls upon a surface it produces illumination (illuminance); the usual measure of illuminance is the foot-candle, which is one lumen falling on each square foot of receiving surface. The lumen is defined as the amount of luminous flux radiated by a small point-source of one candle power into a cone having a solid angle of one steradian. The metric scale measure of illuminance is the lux; for conversion purposes 1 foot-candle is equal to 10.76 lux. A good discussion of steradians, or square radians, is found at:

http://www.physlink.com/ae174.cfm

Type of radiation.

Radiation, as that term is used in physics, denotes the emission or propagation of waves or particles including light, sound, radiant heat and the particles emitted by radioactivity either directly from unstable atomic nuclei or as a consequence of a nuclear reaction. A black body is an idealized radiation absorber and emitter that provides a useful concept to determine the non-radioactive radiation emissions of a nuclear fireball and isothermal sphere. Although in fact only a reasonable approximation, the assumption of black body behavior for the fireball and isothermal sphere provides an adequate model from which to calculate fireball and isothermal sphere thermal radiation and visible light characteristics. For a black body the distribution of radiant energy over the spectrum can be related to the surface temperature by Planck's radiation equation. From the Planck equation it is possible to calculate the rate of energy emission of a black body for a given wavelength. The Stefan-Boltzmann law dictates that the total amount of energy radiated per square centimeter per second by a black body in all directions in one hemisphere is related to the absolute temperature of the black body. The total radiant energy emitted by a fireball of any radius, and the total radiant energy emitted by an isothermal sphere of any radius can be readily calculated, as well as the spectral composition of that energy at any black body temperature.

Thermal radiation received at a distance from a nuclear explosion is fairly characteristic of a black body at a temperature of about 6,000 to 7,000°K, and at any distance is inversely proportional to the square of the distance from the blast point, but atmospheric scattering and absorption markedly decrease the ultraviolet with increasing distance. Assuming the black body characteristics of the fireball and isothermal sphere, the predicted temperature variations over time manifest by those radiant sources permit anticipation of the military effects resulting from thermal radiation; the intensity and spectral distribution of visible light emitted by a nuclear explosion in air can also be known.

One of the important differences between a nuclear and a conventional high explosive (HE) weapon is the large proportion of the energy of a nuclear explosion released in the form of thermal radiation that can cause fire damage and personal injury. The military consequences of thermal radiation can be important at greater distances than the destruction and damage caused by the blast wave. Ultraviolet, visible and infrared radiation from the fireball traveling with the velocity of light arrives at every distance from the explosion in advance of the blast wave. Local fires ignited by those prompt thermal radiations can become firestorms in forests, fields and through large urban areas when the blast wave wind arrives and creates the conditions of a blast furnace among those discrete fires, as occurred at Hiroshima (12.5 kt) and Nagasaki (22 kt). For those explosions, respectively 1,670 and 1,640 feet above the ground, solid materials on the ground immediately below the burst attainted surface temperatures of 3,000° to 4,000°C (5,400° to 7,200°F); solid materials at 3,200 feet (.61 mile) reached 1,800°C (3,270°F). Persons exposed to a flux of heat at those temperatures will not survive.

In development of the World War II nominal 10 kt atomic bomb the forecast of its military consequences included a general determination of the distances at which materials of different sear and combustion temperatures in a target area would be damaged or ignited by thermal radiations emitted by the fireball. Ignition or heat damage would depend on the spectrum and rate of thermal energy emission by the fireball, and the corresponding spectrum and rate of thermal energy absorption of materials at different distances. Some 35 per cent of the total energy of a nuclear fission air burst in the lower atmosphere is thermal radiation energy, of which the fireball is the principal source, with some contribution from the isothermal sphere from which the fireball emerges.

Calculation of the intensity of light that would be radiated by the detonation of the nominal 10 kt atomic bomb, and the spectral character of that light, was necessary to know the hazard of transient or permanent damage that direct or indirect observation of that light by observers positioned 10,000 yards from the explosion would experience; specifically, how the cornea, lens and retina of the eye might be damaged by the intensity and spectral character of that light. Scientific and military observers of the weapon test to be conducted at Trinity site were expected to be located 10,000 yards from ground zero. An additional important calculation was to know with certainty the distance from the explosion, and at what times, the crew of an aircraft that would deliver an atomic bomb in combat would require eye protection to preclude temporary or permanent blindness by exposure to that initial flash of light.

The spectrum and intensity of light received at 10,000 yards at .182 millisecond from the isothermal sphere behaving as a black body at 82,000°K surface temperature is estimated to be 36 suns—the initial flash of intense light. Formation of the fireball one-half millisecond later, at .628 millisecond, radiates as a black body of 30,300°K, which reduces the illumination received at 10,000 yards to 29 suns.

Following the initial one-half millisecond intense flash of light from the detonation of the 10,000 ton gadget described by the “History” the luminance of the explosion perceived at 10,000 yards immediately diminishes to 3.3 suns and then to 0.14 sun. However, at 38 milliseconds the luminance increases to 0.80 sun, and at approximately (~) 160 milliseconds the luminance increases to 1 sun. The luminance of 1 sun is sustained for 2 milliseconds before decreasing to 0.10 sun and finally declining to 0.001 sun at 200,000 milliseconds.

Timeline of a nuclear explosion described by the "History."

Step 1.

At the moment the detonation commences the time is 0 milliseconds. The radius of the gadget is .70 meter which includes the plutonium core, tamper, and mantle of molded HE blocks surrounding the tamper. The .70 meter radius does not include the bulky ballistic case that accommodated the gadget when adapted to combat delivery. The pressure of the system is the ambient atmospheric pressure at sea level, 1 bar.

Step 2.

Much less than one millisecond (.067 millisecond) is required for the detonation wave to propagate entirely through the HE blocks that enclose the tamper and core to reach the depleted uranium tamper interface. The radius has been reduced from .70 meter to .23 meter. The HE mantle was anticipated to be .47 meter thick. The explosive blocks of which the mantle was composed and which detonated the Trinity/Nagasaki weapon were not fabricated in their final and optimal form until late spring 1945, but the quantity of HE necessary to impart the needed energy of implosion was known by late 1944, so the indicated .47 meter thickness of the HE blocks is probably close to the actual thickness of the HE mantle of the gadget detonated at Trinity and the weapon detonated at Nagasaki. The change of radius was .47 meter, the lapsed time .067 millisecond, which gives an average speed for the detonation wave of 7×10^5 meters per second. The speed of radial change is constant through Step 3. The pressure of the system in Steps 2-4 is not calculated but would be equal to the interior pressure of the Sun.

Step 3.

At the end of .127 millisecond the imploding detonation wave has fully compressed the tamper and interior plutonium core (active), theoretically to 0 meter radius. The urchin has been vaporized and the neutrons produced by the urchin have provided a sufficiently large number of neutrons throughout the compressed plutonium core to efficiently initiate and sustain a comprehensive nuclear fission chain reaction. The temperature of the fully compressed system has reached 58,000,000°K.

Step 4.

At .128 milliseconds the heat and pressure of the fissioning plutonium core have expanded the core to .18 meter at an average rate of 2×10^5 meters per second. The highly compressed tamper, which has imparted its pressure to the core, has rebounded to a considerably less compressed state and for a brief moment resists and contains expansion of the core. During that moment of containment the multiplication of neutrons resulting from the fission process has induced fission as completely as will be achieved before the shock wave of the fissioning core impacts the tamper. The tamper under the influence of the shock wave is disintegrated and vaporized. The fissioning core expands beyond the radius at which the nuclear chain reaction will continue and is essentially complete. Much of the energy of the system has devolved to the energy of the shock wave and X radiations; the temperature has consequently dropped to 7,600,000°K.

Step 5.

At .132 milliseconds the shock wave has passed through the radial space occupied by the HE mantle prior to detonation and has disintegrated and vaporized the gadget's steel encasement. The radius of the exploding system has increased to .92 meter at an average speed of 2×10^5 meters per second. The pressure of the system is now calculated to be 29,000,000 bars. The temperature of the system has dropped to 760,000°K which accounts the conversion of thermal energy to the kinetic energy of the shock wave and energy radiated as X rays.

Step 6.

At .182 millisecond the isothermal sphere is formed with a radius of 10.50 meters; the speed of radial expansion has diminished to 3.6 x 10^4 meters per second. The shock wave has hydrodynamically separated from the isothermal sphere to become the blast wave. The legend entry for Step 6 reports that “Radiation squirts out,” which is a picturesque but not scientifically precise description of the massive emission of visible, thermal, X ray, gamma and neutron radiation from the isothermal sphere. The pressure of the isothermal sphere is 16,160 bars and the temperature that of a black body at 82,000°F, cut off at 10,000 yards in the ultraviolet by atmospheric absorption. The momentary luminance of the isothermal sphere received at 10,000 yards is estimated to be 36 suns.

Step 7.

Summary.

During the approximately 38 milliseconds of the explosion described in Step 7 the blast wave, which has propagated and separated from the isothermal sphere, expands into the atmosphere and the fireball forms. The radius of the fireball increases; its pressure and temperature diminish. The radius of the isothermal sphere increases; its temperature and pressure diminish. The pressure of the blast wave at the fireball front is greater than the pressure of the isothermal sphere throughout Step 7; the temperature of the fireball front is much less than the temperature of the isothermal sphere throughout Step 7. However, the temperature of the fireball is sufficiently high that all the radiations of the isothermal sphere, which is enclosed within the fireball, are confined and blocked from emission and view by the thermal opacity of the fireball during most of the period described by Step 7.

Blast wave and fireball during Step 7.

The radius of the blast wave front, which is still coincident with the fireball front, increases from 21 meters to 126 meters, a factor of 6. The rate of expansion rapidly diminishes from 1.7 x 10^4 to 1,300 meters per second. The pressure decreases from 3,360 bars to 20 bars, a factor of 168. The fireball surface temperature decreases from 30,300° to 500°K, a factor of 60.5.

At the beginning of Step 7 the fireball radiates as a black body of 30,300°K and is sufficiently hot to be essentially opaque to all radiations of the interior isothermal sphere. The initial luminance of the explosion perceived at 10,000 yards in Step 7 is produced from the surface of the fireball and decreases as the fireball cools and the luminance decreases from 29 suns to 0.14 sun at 14.280 milliseconds. However, at 14.280 milliseconds the temperature of the fireball (1,500°K) has cooled sufficiently to become transparent to most of the isothermal sphere radiations; in consequence, at the end of Step 7 the luminance of the much hotter isothermal sphere has increased the perceived luminance from 0.14 sun at 14.280 milliseconds to 0.80 sun.

Isothermal sphere during Step 7.

The radius of the isothermal sphere increases from 15.70 to 94.60 meters, a factor of 6; the radius of the fireball has also increased by a factor of 6. The initial pressure of the isothermal sphere in Step 7 is 2,020 bars, which is one-third less than the initial Step 7 fireball pressure of 3,360 bars. By the end of Step 7 the pressure of the isothermal sphere is reduced to 9.4 bars, a factor of 215, and compares to the final Step 7 pressure of the fireball of 20 bars. The initial temperature of the isothermal sphere in Step 7 is 67,000°K. By the end of Step 7 the temperature of the isothermal sphere has been halved to 33,000°K. The initial Step 7 temperature of the fireball, in comparison, has been reduced from 30,300°K to 500°K, a reduction factor of 61. The isothermal sphere at the end of Step 7 is 66 times hotter than the fireball surface because the opaque fireball had briefly blocked most energy-reducing radiations from the isothermal sphere; the heat and temperature of the isothermal sphere are correspondingly maintained.

Black body radiation during Step 7.

At the commencement of Step 7 the isothermal sphere radiations have been cut off by the fireball which is sufficiently hot at 30,300°K to have become opaque to the intensely hotter central isothermal sphere. The radiations of the explosion at .628 millisecond are essentially those of the 30,300°K fireball, a black body of 30,000°K. When the fireball temperature is diminished to 8,300°K at 2.774 milliseconds the radiations of the explosion continue to be essentially those of the fireball, a black body of 8,000°K. However, at 14.280 milliseconds the temperature of the fireball has declined to 1,500°K and is now partially transparent to the 39,000°K black body isothermal sphere radiations. In consequence, the radiations of the explosion are of a black body of approximately 4,500°K, hotter than the 1,500°K fireball but cooler than the 39,000°K isothermal sphere. As the fireball continues to cool and becomes progressively more transparent to the isothermal sphere radiations the black body temperature of the explosion increases to 6,000°K at the end of Step 7.

Step 8.

At approximately 160 milliseconds the fireball is fully expanded to approximately 220 meters radius. The rate of radial change is slowed to 500 meters per second; the pressure at the fireball front is now only approximately 2 bars. The energy of the fireball has been dissipated by radiation emissions and cooling by expansion, and immediately the 20,000°K isothermal sphere becomes the dominant radiating body and is perceived at 10,000 yards with the luminance of 1 sun and radiating as a black body of approximately 10,000°K. The maximum radius of the isothermal sphere, 155 meters, is formed coincidentally with the maximum radius of the fireball, approximately 220 meters. The isothermal sphere then visually appears as an intensely hot, brilliant spherical core within the larger fireball which has cooled sufficiently to become transparent to the intense light of the isothermal sphere.

Step 9.

At approximately 2,200 milliseconds (2.2 seconds) the blast wave has traveled 1,200 meters from the locus of the explosion to arrive at the "damage area." The blast wave is advancing at 332 meters per second, less than the 440 meters per second (1,150 feet per second) speed of sound at sea level—the blast wave at the damage area is subsonic. The luminance perceived at 10,000 yards has diminished to 0.10 sun and the isothermal sphere radiates as a black body of less than (<) 5,000°K. The pressure at the blast wave front in Step 9 is defined to be 5 psi "overpressure." Overpressure is that pressure, expressed in pounds or fractions of a pound per square inch, which exceeds the ambient atmospheric pressure: 1 bar, or 14.7 psi, at sea level. Overpressure, as will be discussed later, is a measure that permits prediction of blast wave-induced structural damage. A peak overpressure of 5 psi, which is an impulse pressure of 5 pounds per square inch in excess of ambient pressure, will destroy most structures; an overpressure of 2.5 psi will induce sufficient damage that most structures affected by that overpressure will be rendered useless. In the Port Chicago explosion the limiting radius of 2.5 psi overpressure was 2,500 feet (771 meters), or one-half mile.

Step 10.

At 28 seconds the blast wave has traveled 10,000 meters (10,000 yards) from the detonation point. The speed of the blast wave is 332 meters per second; the overpressure at 10,000 yards is .18 psi, which is not sufficient to rupture the human ear drum. The diminishing luminance of the explosion is 0.01 sun. The temperature at the blast wave front has cooled nearly to the ambient atmospheric temperature. At Trinity site observers in the open who were positioned 10,000 yards from ground zero experienced the blast wave as a gentle gust of warm wind. The fireball has reached a height of 2,000 feet and has begun to disintegrate to flame-hot turbulent gasses radiating as a black body much less than 5,000°K and rising above the detonation point, no longer a discrete ball of fire but a lengthening column of flame that at the top will form a mushroom cap as the top cools and is obstructed in greater ascent by the atmosphere.

In the Port Chicago explosion the initial, discrete ball of fire reached a height of 2,000 feet and was completely disintegrated into turbulent convection currents that resolved to a column of flame which expanded and billowed at the top as it rose. The top of the column of flame from the Port Chicago explosion reached an altitude of 7,000-10,000 feet. The column of flame was red at the top and brightened from orange to yellow at the base.

Step 11.

Finally, at 200,000 milliseconds (200 seconds; 3 minutes and 33 seconds) the hot, turbulent and luminous gasses produced by the detonation of the 10,000 ton gadget at Trinity would rise and cool by expansion and convection. The ball of fire and supervening column of flame at Trinity was expected to resolve to a dark mushroom-capped smoke cloud that would ascend to 18,000 feet in “typical Port Chicago fashion.” At Port Chicago on that moonless night of 17 July 1944 the dark smoke cloud above the explosion was invisible against the dark night sky and we have no account of the final height achieved by the top of the smoke cloud top that arose from the Port Chicago explosion.

[Note. The “History of 10,000 ton gadget” does not account one militarily significant artifact that results from the detonation of a 10 kt weapon, but which is an artifact that also occurs in consequence of any explosion and is proportional to the energy yield of an explosion. In the wake of a radially expanding blast wave the pressure of the atmosphere in the area behind the blast wave front does decrease below the ambient, pre-explosion pressure of the atmosphere. In the area behind a blast wave a negative pressure phase results and a wind, proportional to the energy of the explosion, will blow in toward the locus of the explosion. The wind produced by that negative or suction phase of a large explosion can amplify the destruction caused by the blast wave when structures weakened by the blast wave are demolished by the negative phase wind. Personal casualties will also increase among those who survived the blast wave when the negative phase wind translates the wreckage left by the blast wave into a barrage of missiles.

Photographs and illustrations credits.

“History of 10,000 ton gadget.” Source: Author’s files and Los Alamos National Laboratory.

Chapter

7

Ship Explosions:
USS *Maine, SS Fort Stikine,* SS *Mont Blanc*

Combustion. Combustion is a chemical change, especially oxidization, accompanied by the production of heat and light. All forms of fire and explosion are subtypes of the larger term, combustion.

Fire. Fire is a rapid and persistent combustion that releases heat and light, especially the heat-releasing (exothermic) combination of a combustible substance with oxygen. Depending on the nature of the combustible substance and the availability of oxygen to the chemical reaction a fire can burn quickly or slowly.

Deflagration. A deflagration is a fire in which a highly combustible substance burns very rapidly and produces exceptionally great heat and light, but without generating a high pressure wave.

Explosion. An explosion is type of combustion characterized by a sudden, rapid and violent release of mechanical, chemical, or nuclear energy from a confined region; especially, such a release that generates a radially propagating high pressure shock or blast wave accompanied by a loud, sharp report, flying debris, heat, light, and fire.

High explosives. High explosives (HE) are used in military ordinance, blasting and mining. High explosives have a very high rate of reaction, high pressure development, and a detonation wave that moves faster than the speed of sound (1,400 to 9,000 meters per second). High explosives include “primary explosives” (e.g., nitroglycerin) that can detonate with little or no stimulus or shock and “secondary explosives” (e.g., dynamite, TNT), that require a strong stimulus or shock provided by a detonator such as a blasting cap. High explosives detonate “high order.”

Low explosives. Low explosives change into gases by burning or combustion. These are characterized by deflagration and do not generating a high pressure wave. Low explosives are characterized by a lower reaction rate than high explosives. Low explosives (gun powder is the only common example) produce a range of reactions from deflagration to a “low order” detonation, in which the detonation wave generally moves slower than 2,000 meters per second.

Fireships. Ships-of-war, commercial ships, pleasure ships and pleasure craft have been liable and subject to damage and destruction by fire since the beginning. One of the first boatmen paddling a primitive bundle of floating reeds who set out to cross water while carrying live coals from his breakfast fire was certainly the first man to have his boat burn out from under him. In warfare, long before the invention and development of chemical high explosives and gunnery projectiles, fire had been employed as an instrument of naval combat. The fireship has been used from the very beginning of directed naval combat. A boat specific or adapted to the purpose would be laden with combustible materials including barrels of tar and oil, oil-soaked sail canvas and masses of rope, and ignited. Towed by a sailing ship or a crew of oarsmen in a longboat, or propelled unmanned by current or wind, the burning fireship would get alongside an enemy combatant with the intent to set it ablaze.

One exemplary use of a fireship in naval combat was observed and reported by an eyewitness to a naval battle between the Greeks and Turks at the beginning of the Greek War of Independence, 1821-32. The Greek rebellion within the Ottoman Empire was a struggle which resulted in the establishment of an independent kingdom of Greece, but the Greeks were actually waging a holy war because it was not simply Greek against Turk but Christian (Greeks) against Muslim (Turks). The Greek cause was saved by the intervention of the European powers, with U.S. encouragement, who favored the formation of an autonomous Greek state. On 20 October 1827, Great Britain, France and Russia sent their naval fleets to Navarino (now Pylos), where they destroyed the Egyptian fleet which was allied with the Ottoman Turks. Although this loss severely crippled the Ottoman forces, the war continued, complicated by the Russo-Turkish War, 1828-29. A Greco-Turkish settlement was finally determined by the London protocol of 3 February 1830 which declared Greece an independent monarchical state under European and Russian protection.

Ioannis D. Frangoudis, a Cypriot clerk on the Greek ship-of-war *Heracles* commanded by Captain Arargyros Hadji-Anargyros, provides a description of the destruction wrought by a fireship:

“About 4 o’clock in the morning of 27 May 1821 Captains Ghikas D. Tsoupas and Konstantinos Babas arrived. At this moment the wind being favorable, a north-west wind, Papanikolis’ fireship put up the sign of battle. Then we attacked, together with a few ships from Spetsae [a small island in the Argosaronic Gulf of Greece], firing at the enemy fiercely and crying ‘hurrah,’ our cries echoing in the nearby mountains and valleys.

“To encourage the men in the fireship we went so deep into the enemy’s fire that the shells of its cannons went through the shrouds of our own ship to hit the boats behind us. We finally saw our purpose succeeding, that is to say the fireship was secured to the enemy ship and the fire started spreading from the prow to the whole vessel. We all went mad with joy and ran about the deck crying loudly ‘Great is the Lord,’ shelling the burning boat incessantly. Meanwhile the enemy sent back a rain of bullets, propelled by the fire and intensified by the despair of the Turks, who were doing their best to chase us away so as to be able to jump into the sea and escape from the flames. Our men, exalted and full of courage, wanted to board the burning frigate, but our Captain wisely prevented them.”

Spontaneous combustion.

Fire and explosion in a merchant ship's fuel or cargo, or among the munitions of a warship, does not require an active source of ignition. Spontaneous combustion is defined as the ignition and burning of a mass independently of contact with any burning body. One uniquely contemporary example of spontaneous combustion was detailed in a Federal Food and Drug Administration Public Health Advisory issued June 27, 1996: "Potential Risk of Spontaneous Combustion in Large Quantities of Patient Examination Gloves."

In the spring and summer of 1995, the spontaneous combustion of powder-free latex medical patient examination gloves caused fires in four areas of the U.S. The fires all occurred in warehouses and involved large quantities of non-sterile, powder-free, chlorinated latex gloves stored on pallets. Investigators ruled out arson and concluded that high warehouse temperatures accelerated an exothermic chemical reaction on the chlorinated gloves to the point where the latex ignited. The same gloves in substantial quantity carried as cargo in the poorly ventilated confinement of a ship's cargo holds and subject to the intense summer heat of the Tropics would offer a serious threat of spontaneous combustion and consequent fire hazard to the ship and crew.

Innumerable ships and sailors have been and will be lost consequent to the spontaneous combustion of ships' cargoes. Spontaneous combustion in the sweltering cargo holds of the sailing ships that transported the prodigious tonnages of coal that fueled the Industrial Revolution was a frequent cause of disaster among those colliers. When the world's sail-driven commercial and naval fleets were superceded by coal-fired steam engine propelled ships, spontaneous combustion in those ships' coal fuel bunkers was similarly hazardous.

USS Maine, 15 February 1898.

USS *Maine* was a 6,682 ton second-class battleship built at the New York Navy Yard and commissioned in September 1895. The precipitating cause of the Spanish-American War (21 April-13 August 1898) was the explosion and sinking of the *Maine* in the harbor of Havana, Cuba, at 9:40 the evening of 15 February 1898. More than five tons of powder charges for the battleship's six- and ten-inch guns exploded, virtually obliterating the forward third of the ship; 260 American servicemen were killed in the explosion. Debate has continued for 100 years whether the *Maine* explosion was caused by spontaneous combustion and undetected fire in the battleship's coal bunker, the heat of which detonated the ship's adjacent forward powder magazine, or if the detonation of a naval mine placed against the ship's hull by unknown saboteurs had caused the magazine to explode.


USS *Maine*, circa 1895-1898

Sensational and inflammatory newspaper reports of the incident, in which the Hearst and Pulitzer newspapers took the active lead, provoked public opinion in hostility toward the government of Spain, which then controlled Cuba and other of the Caribbean islands, as well as the Philippine Islands in the Pacific. In the Caribbean, U.S. opinion and policy generally favored the objectives of a homegrown Cuban insurrection which at that time sought to expel the Spanish governor and Spanish military from Cuba to permit establishment of local sovereignty. An exceptionally grotesque and repressive Spanish administration had by that time resulted in the starvation and death of 100,000 Cubans. Many U.S. citizens were morally outraged; but establishment of local sovereignty in the Caribbean was also anticipated to be favorable to U.S. political influence and economic interests in Cuba, which the Spanish had resisted.

A four-week Board of Inquiry investigation by the U.S. Navy Department concluded that a mine had been detonated under the ship, but the board did not attempt to determine how or by whom that mine had been placed. The newspapers' warmongering diatribes insinuated that agents of the Spanish government had placed the mine that, when detonated in an act of sabotage, triggered the catastrophic explosion of the ship's gunpowder stores. On 21 April, President William McKinley ordered the Navy to begin a blockade of Cuba; Spain followed with a declaration of war on 23 April 1898. Congress responded with a formal declaration of war on 25 April, retroactive to the start of the blockade. But Spain, in 1898, was politically, economically and militarily unable to mount a significant war effort.

On 1 May, Assistant Secretary of the Navy Theodore Roosevelt directed Commodore George Dewey to take his Pacific Squadron of six ships to the Spanish-controlled Philippine Islands area to engage the Spanish Pacific fleet, which Dewey surprised at anchor in Manila Harbor. During a 7-hour attack, remembered as the Battle of Manila Bay, Dewey's squadron sank all ten ships of the Spanish fleet. None of the ships of Dewey's squadron was disabled and only eight U.S. sailors were wounded, while 381 of the Spanish navy and marines were killed. The Spanish were defeated in the Spanish-American War, and the U.S. moved to the status of a world power in the Pacific and Caribbean areas—probably because of an explosion aboard the USS *Maine* caused by spontaneous combustion in a coalbunker.

In 1911 a second Navy Board of Inquiry confirmed the 1898 board finding that a mine had been the precipitating cause of the explosion, but technical experts at the time of the investigations disagreed with both boards' findings and advocated that spontaneous combustion of coal in the bunker adjacent to the forward reserve powder magazine was the most likely cause of the explosion of the *Maine*. In 1976 U.S. Navy Admiral Hyman G. Rickover published his book *How The Battleship Maine Was Destroyed.* The admiral engaged two experts on explosions and their effects on ship hulls who concluded that the damage caused to the ship was not consistent with the external explosion of a mine. The most likely cause, they speculated, was spontaneous combustion of coal in the bunker next to the forward reserve powder magazine, that the intense heat of an undetected coal fire, transferred through the separating bulkhead, had detonated the powder magazine.

Because of the historical importance of the sinking of the USS *Maine* as the precipitating event of the Spanish-American War, the cause of the explosion will likely be debated another hundred years. Most recently, in 1996-97, the National Geographic Society underwrote another comprehensive study that employed computer-modeling to attempt to resolve the cause of the explosion. That study was reported in the February 1998 issue of *National Geographic Magazine* and abstracted by the historian Thomas B. Allen in the January 1999 issue of the United States Naval Institute periodical, *Naval History*.

SS Fort Stikine, 14 April 1944.

The most detailed and engaging account of the Friday 14 April 1944 explosion of the merchant munitions ship SS *Fort Stikine* in Bombay harbor was written by John Ennis: *The Great Bombay Explosion*, published 1959 in England (Cassell) and New York (Duell, Sloan and Pearce). The book has been out of print for 40 years but is one of the core masterpieces of the particular variety of historical narrative literature that engagingly and factually recount enormously destructive ship explosions, books that are based in comprehensive documentary research, that draw extensively from contemporary newspaper accounts and many personal interviews conducted by the authors with individuals who were participant in the event and survived, eyewitnesses of all sorts, and others affected in different ways by the event.

SS *Fort Stikine,* North Sands Class freighter.

SS *Fort Stikine* was a coal burner of 7,130 tons, 441 feet of length and carrying 1,395 tons of wartime munitions. Cause of the explosion was spontaneous combustion among 9,000 bales of cotton, loaded at Karachi, that were a part of her cargo. Of fringe interest, *Fort Stikine* carried among her cargo 124 gold bars each weighing 28 pounds, valued in total at two million Pounds Sterling ($1,821,000). The gold had been shipped from London to stabilize the Indian currency, the Rupee, then sagging in value because of wartime economic disruptions and fear of an invasion of India from Japan. The ship explosion blasted those gold bars throughout the immediate area onshore and into the harbor waters. Most of the gold bars are reported to have been quickly recovered from different parts of the city, but during the 1970s two of the gold bars were recovered during normal harbor dredging operations and returned to the British government.

In 1944 India was still a fairly content British colony, and India provided courageous, well trained and well disciplined troops to the Allied cause and many valuable military bases, war materiel, and supply services. Bombay, India’s principal seaport, was the clearing-house, distribution center and storehouse of war materiel that supported the China-Burma-India (CBI) Theater of Operations. The closing of the Burma Road left only one route open to supply Chinese and American troops fighting the Japanese on the Chinese mainland: the exceptionally hazardous air route maintained by the U.S. Army Air Corps flying from India’s Assam Valley over “The Hump” of the Himalaya Mountains into China.

Fort Stikine was one of 90 Canadian-built wartime cargo vessels of the North Sands Class that were very similar to the U.S.-built Liberty ships; *Fort Stikine* was built and launched July 1942 at Prince Rupert Drydock & Shipyard, Prince Rupert, British Columbia, Dominion of Canada, purchased by the U.S. War Shipping Administration and transferred to British operation and ownership. The ship’s principal cargo was wartime munitions, but before coming into the Victoria Docks at Bombay she had made a number of port calls along the Indian Ocean coast to take on whatever additional cargo the crew could manage to squeeze into her holds and onto her deck. At Karachi she took on the cotton bales that would ultimately destroy 18 ships and a large part of Bombay.

Prior to shipment loose ginned cotton is compacted into bales under high pressure to reduce the volume of the shipment. Extreme tropical heat and humidity in the poorly ventilated cargo hold that contained the cotton bales raised the temperature at one or more places in the cotton cargo to the point of spontaneous combustion, which process was encouraged by the pressure within the compacted cotton bales; that compaction augmented the absorption and retention of heat. The ship had come to dock before the first wisps of smoke from the smoldering cotton were noticed rising from the ventilators. A red flag signifying an explosive cargo had not been raised on the ship's mast so the potential for disaster presented by fire aboard the *Fort Stikine* was not recognized. For awhile before the municipal fire brigade was summoned, the crew with limited equipment tried unsuccessfully to extinguish the fire.

Flames were beginning to appear from the No. 2 cargo hold when the first municipal fire brigade and equipment arrived. More firefighters and equipment were called until even the most antiquated pumpers had been hauled to the docks and every available firefighter was at the scene. An hour and a quarter after the first fire brigade arrived, the forward section of the ship's hull, which contained the fire and 611 tons of explosives, displayed a bright cherry-red glow. At 4:06 P.M. the forward section of the ship exploded killing hundreds of spectators, all the 66 gallant officers and men of the Bombay Fire Brigade, and destroying most of Bombay's firefighting equipment.

That disaster was bad enough, but that first explosion launched flaming cotton bales through the air to distances up to one mile from the explosion. In the areas surrounding the docks those flaming bales of cotton fell into and ignited the packing-case shacks and shanties of the Bombay slums, which had mostly been reduced to kindling wood by the first blast wave. Soon 300 acres (120 hectares) of docks, warehouses and most of the Bombay slums were in flames. No system of firefighting water hydrants had been laid in the slums and, anyway, all the city's firefighting equipment had been destroyed. The fires burned two days and two nights, until there was nothing left to burn. Thirty-four minutes after the forward section of the ship exploded, 784 tons of munitions in the burning aft section of the ship exploded.

Eighteen merchant ships in Bombay harbor were either sunk or severely damaged. The number of persons killed and injured by the two explosions is impossible to determine because the class of slum dwellers, the Untouchables in Indian society, who were most severely affected by the disaster, were uncounted. Estimates of the number believed to have been killed range between 336 and 1,376, but some writers assert the number killed was more likely 6,000. Five hundred persons are reported to have been hospitalized, but most of the impoverished residents of the Bombay slums who were injured in the explosion would not have sought medical treatment at public facilities; they were cared for by family and friends and most were not counted among the reported casualties.

A good eyewitness account of the *Fort Stikine* explosion by John Garside, 19 years old at the time, is available at the WWW link below. John, a British Royal Navy D.E.M.S. gunner aboard the SS *Fort Crevier*, another of the North Sands Class, watched the fire from 400 yards through binoculars and was knocked unconscious by the blast. Equivalent to the complement of U.S. Navy Armed Guard gunners aboard armed U.S. merchant ships during the war, British-flagged Defensively-Equipped Merchant Ships (D.E.M.S.) carried a complement of Royal Navy gunners. John remembers, "The blast picked me up and dropped me into an open coal bunker. When I came to and scrambled out on deck we had been on fire for some time."

http://members.tripod.com/~merchantships/fortcrevier1.html

It is interesting to notice in John Ennis' book *The Great Bombay Explosion* that the U.S. Army Corps of Engineers is credited for the many photographs reproduced there of the fires and devastation that resulted from the explosion. Army Corps of Engineers Major General Leslie R. Groves from 1942 was the military commandant of the Manhattan Project, and he would probably have learned those photos had been taken at Bombay by Corps photographers and obtained copies for the Manhattan Project. The 14 April 1944 Bombay explosion occurred 3 months before the Port Chicago explosion, but among the presently identified and declassified Port Chicago explosion documentary materials held by Los Alamos National Laboratory Archives no mention of the Bombay explosion is made. The photos of the firestorm that swept the Bombay docks and slums following the explosion of the *Fort Stikine* would have been a valuable preview of the firestorm that was anticipated to ravage any Japanese city that would be targeted for combat use of the atomic bombs. Manhattan Project scientists at Los Alamos had, however, thoroughly studied the available literature descriptive of effects of the World War I explosion of the ammunition ship *Mont Blanc* in the harbor of Halifax, Nova

Scotia, British Dominion of Canada. Most of North End Halifax was burned to the ground.

SS Mount Blanc, 6 December 1917.

The harbor at Halifax, Nova Scotia, on the Canadian Atlantic coast at approximately the latitude of Bangor, Maine, is an ice-free natural harbor, long and deep, lying on an axis roughly running to the northwest from the Atlantic harbor mouth. Entry is from the southeast through a strait, cleverly named The Narrows. When an inbound ship has cleared The Narrows, the town and maritime docks of Halifax are seen off the port bow on the south shore of Halifax Harbor. Off the starboard bow, opposite Halifax, lies the city of Dartmouth on the north shore of Halifax Harbor. Ships that will not immediately go to dock along the Halifax waterfront proceed several miles northwest through Halifax Harbor to anchorage in the much larger inner harbor, Bedford Basin.

In December 1917, during World War I, Halifax was the principal East Coast deepwater maritime Canadian port. Ships that had been loaded at the Halifax docks with supplies for the war in Europe, food, materiel, munitions and troops, gathered at anchorage in Bedford Basin to assemble the slow, wallowing convoys of 15 or more merchant ships that would somewhat be protected from German surface and submarine attack during the trans-Atlantic voyage by an escort of naval warships. Ships waiting to be loaded at Halifax also made anchor in Bedford Basin. Ships loaded with war materiel at the ports of New York City and Boston steamed north to Halifax to join the trans-Atlantic convoys forming there. Halifax harbor was a very busy port on 6 December 1917, and the civilian population of Halifax was very much increased because of the port's wartime employment; several thousand Canadian troops were stationed at Halifax in garrison or in barracks waiting transportation to the European war.

The French ship SS *Mont Blanc* had sailed from New York City to Halifax to join an assembling convoy but arrived 5 December after the antisubmarine nets closing Halifax Harbor at night had been raised, and she necessarily waited at anchorage to enter the harbor until 7:30 the morning of 6 December. *Mont Blanc* was a munitions carrier. Her cargo holds were lined with wood affixed to framing with copper nails, which could not spark if struck by steel, but an hour later when the *Mont Blanc* collided bows-on in the harbor with the Norwegian cargo ship in ballast, the SS *Imo,* although her bow was only moderately gashed, barrels of highly inflammable liquid benzene stored on her forward deck were ruptured, and the flow of benzene from those ruptured barrels was ignited by the spectacular cascading barrages of sparks generated as the ships' steel bows ground together. A raging fire that could not possibly be extinguished immediately engulfed the bow of the *Mont Blanc.*

Smoke cloud formed above the explosion of the SS *Mont Blanc.*

The captain, pilot and crew of the *Mont Blanc* knew the ship carried an explosive cargo of 200 tons of TNT, 10 tons of gun cotton, 25 tons of benzene, and 2,300 tons of picric acid. They expected the ship would explode immediately. They launched the ship's lifeboats, abandoned her, and rowed with all haste to the Dartmouth shore where they warned everyone they passed to take cover as they ran to refuge behind a low hill; all the crew of the *Mont Blanc* were saved except one of the crew who was fatally injured by one of the 3,000 tons of steel fragments of the ship blasted miles through the air when the ship exploded at 9:05 A.M., 20 minutes after she was abandoned. The massive smoke cloud formed above the explosion of the *Mont Blanc* is said to have risen one mile (5,280 feet).

Gun cotton (fulmicotton) dates from 1845 and is cotton fiber treated by immersion in a mixture of nitric and sulphuric acids, transformed into a paste and compressed. Gun cotton was primarily used during World War I as an artillery projectile propellant, but because it is corrosive and brisant the deterioration of gun barrels was rapid. Picric acid (trinitrophenol) is highly explosive and extremely heat, flame, shock, and friction sensitive. The high explosives lyddite and melinite are composed mostly of compressed or fused picric acid. Picric acid is often used as a booster to detonate a less sensitive explosive, such as TNT. Picric acid is toxic by all routes of entry, inhalation, ingestion and dermal, and reacts with metals to form metal picrates which are also highly explosive and toxic.

The captain, pilot and five members of the *Imo'*s crew were among the 1,900 persons immediately killed in the explosion, which also injured 9,000; hundreds later died of their injuries. The captain of the *Mont Blanc* had not ordered the red flag hoisted on the ship's mast, which would have signaled that the burning ship carried explosive cargo. The abandoned, flaming ship drifted to the Halifax piers, brushed one and set it afire before she came against Pier 6 and grounded. Members of the Halifax Fire Department responded and were positioning their pumper to the nearest hydrant when the *Mount Blanc* disintegrated.

Large crowds of sightseers had run from the downtown area to the docks to watch the spectacle; most were killed. On the slope of the hills that rise behind the waterfront, families gathered at their windows to watch the ship burn in the harbor below. The blast wave blew-in every window in North End Halifax. Thousands of the injured were lacerated by flying glass; 1,000 persons received severe eye injury; surgeons removed 250 eyes that could not be saved.

The blast wave flattened almost all buildings within an area of two square kilometers, converting the mostly wooden homes and structures to kindling wood; 1,630 homes were destroyed. Then the tidal wave from the explosion rushed ashore and many of the injured and uninjured in downtown Halifax were drowned. The water wave raced across the harbor to the Dartmouth shore, swamping and sinking ships and boats as it moved. On the Dartmouth shore the wave gained intensity as it funneled into Tufts Cove where it washed away the entire settlement of Micmac Indians encamped there. In Halifax, following the rain of steel fragments, many coal- and wood-burning stoves that were overturned and thrown around by the blast wave ignited the rubble of buildings collapsed by the blast wave and 325 acres of the city burned. That night a blizzard burdened the city's agony with 16 inches of snow; 6,000 persons were without shelter from the storm.

View of the Halifax disaster, looking south, 6 December 1917

On the morning of the explosion when news of the disaster reached Boston, Massachusetts, the city immediately mobilized an extra-ordinary relief effort and rushed medical teams and medical supplies to Halifax, followed by ship- and trainloads of tents, food, clothing and bedding, and hundreds of volunteer construction workers with the materials and supplies necessary to begin reconstruction of the city. In continuing thanks, the citizens of Halifax every year send the towering Christmas tree that is mounted in Boston's Prudential Plaza.

Los Alamos documentary records: Comparison of the Halifax and Port Chicago explosions.

Captain William S. Parsons, USN: Memorandum of 24 July 1944.

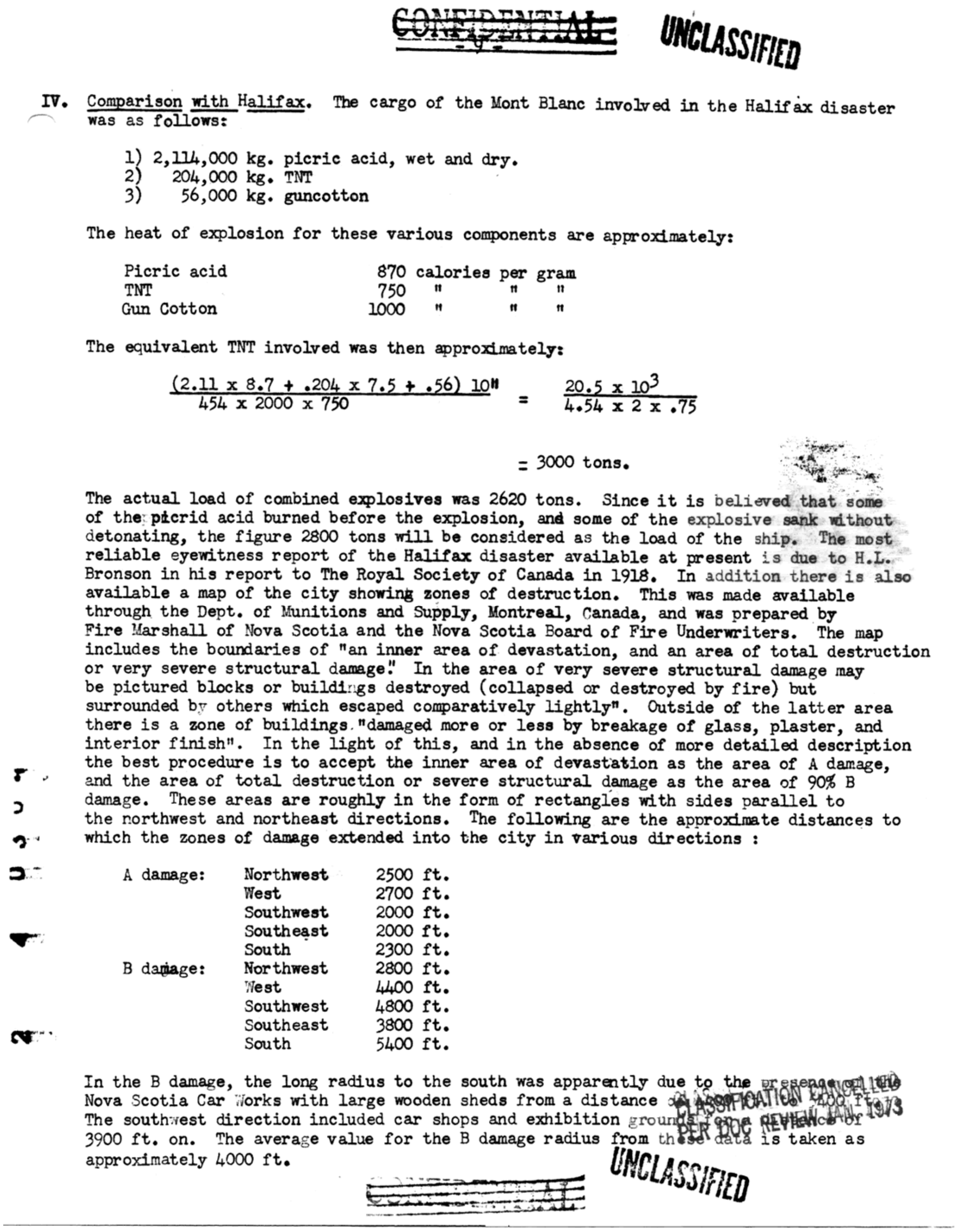
Among the declassified Port Chicago explosion literature available in the Archives of Los Alamos National Laboratory, the first comparison of the Halifax and Port Chicago explosions was made by Navy Captain William S. Parsons in his memorandum of 24 July 1944, "Port Chicago Disaster: Preliminary Data." The memorandum was addressed and transmitted to Rear Admiral William R. Purnell, Navy member of the Government's 3-man Atomic Bomb Military Policy Committee:

“Comparing loss of life to the Halifax disaster, it appears that all but some five of the victims at Port Chicago were right on top of the explosion, in a position corresponding to some 25 crew members and fire fighters at Halifax. Thus, in comparison for remote victims is Halifax about 1,475, Port Chicago less than 5. If the two explosions are considered to be of the same order of magnitude, the difference in loss of life can be attributed to the fact that Port Chicago was designed for large explosions.”

Ensign George T. Reynolds, USNR: Analyses of structural damage from the Halifax and Port Chicago explosions; discussion of seismic effects of the Halifax explosion.

A more comprehensive comparison of the 6 December 1917 Halifax explosion with the 17 July 1944 Port Chicago Naval Magazine explosion was prepared by Los Alamos scientist Ensign George T. Reynolds, USNR, and transmitted by Capt. Parsons from Los Alamos 31 August 1944 to Adm. Purnell of the Atomic Bomb Military Policy Committee; Adm. Purnell was Capt. Parsons’ commanding officer. Captain Parsons was Ens. Reynolds’ commanding officer.

Ensign Reynolds’ two-page comparison of the explosions reproduced here is Section IV of the body of his report, “Port Chicago: Analysis of damage due to air blast and earth shock,” which is Enclosure D of Capt. Parsons’ 31 August 1944 memorandum to Admiral Purnell, “Port Chicago Disaster: Third Preliminary Report.”



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IV. Comparison with Halifax. The cargo of the Mont Blanc involved in the Halifax disaster was as follows:

1) 2,114,000 kg. picric acid, wet and dry.
2) 204,000 kg. TNT
3) 56,000 kg. guncotton

The heat of explosion for these various components are approximately:

Picric acid	870	calories	per	gram
TNT	750	"	"	"
Gun Cotton	1000	"	"	"

The equivalent TNT involved was then approximately:

$$\frac{(2.11 \text{ x } 8.7 + .204 \text{ x } 7.5 + .56)\ 10\text{"}}{454 \text{ x } 2000 \text{ x } 750} = \frac{20.5 \text{ x } 10^{3}}{4.54 \text{ x } 2 \text{ x } .75}$$

$$= 3000 \text{ tons.}$$

The actual load of combined explosives was 2620 tons. Since it is believed that some of the picrid acid burned before the explosion, and some of the explosive sank without detonating, the figure 2800 tons will be considered as the load of the ship. The most reliable eyewitness report of the Halifax disaster available at present is due to H.L. Bronson in his report to The Royal Society of Canada in 1918. In addition there is also available a map of the city showing zones of destruction. This was made available through the Dept. of Munitions and Supply, Montreal, Canada, and was prepared by Fire Marshall of Nova Scotia and the Nova Scotia Board of Fire Underwriters. The map includes the boundaries of "an inner area of devastation, and an area of total destruction or very severe structural damage." In the area of very severe structural damage may be pictured blocks or buildings destroyed (collapsed or destroyed by fire) but surrounded by others which escaped comparatively lightly". Outside of the latter area there is a zone of buildings "damaged more or less by breakage of glass, plaster, and interior finish". In the light of this, and in the absence of more detailed description the best procedure is to accept the inner area of devastation as the area of A damage, and the area of total destruction or severe structural damage as the area of 90% B damage. These areas are roughly in the form of rectangles with sides parallel to the northwest and northeast directions. The following are the approximate distances to which the zones of damage extended into the city in various directions :

A damage:	Northwest	2500 ft.
	West	2700 ft.
	Southwest	2000 ft.
	Southeast	2000 ft.
	South	2300 ft.
B damage:	Northwest	2800 ft.
	West	4400 ft.
	Southwest	4800 ft.
	Southeast	3800 ft.
	South	5400 ft.

In the B damage, the long radius to the south was apparently due to the presence of the Nova Scotia Car Works with large wooden sheds from a distance of [illegible] ft. The southwest direction included car shops and exhibition grounds for a distance of 3900 ft. on. The average value for the B damage radius from these data is taken as approximately 4000 ft.

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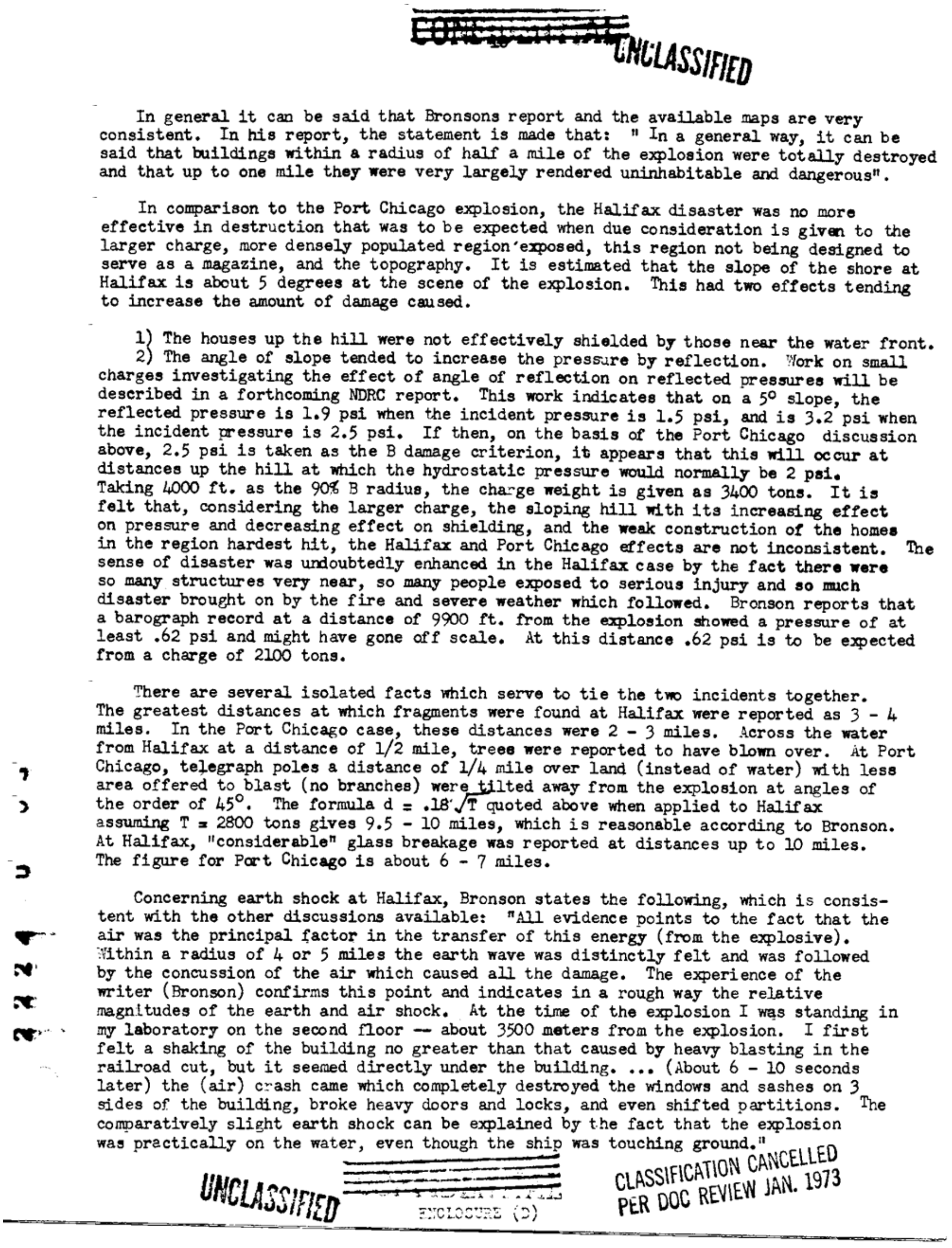

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In general it can be said that Bronsons report and the available maps are very consistent. In his report, the statement is made that: " In a general way, it can be said that buildings within a radius of half a mile of the explosion were totally destroyed and that up to one mile they were very largely rendered uninhabitable and dangerous".

In comparison to the Port Chicago explosion, the Halifax disaster was no more effective in destruction that was to be expected when due consideration is given to the larger charge, more densely populated region exposed, this region not being designed to serve as a magazine, and the topography. It is estimated that the slope of the shore at Halifax is about 5 degrees at the scene of the explosion. This had two effects tending to increase the amount of damage caused.

1) The houses up the hill were not effectively shielded by those near the water front.
2) The angle of slope tended to increase the pressure by reflection. Work on small charges investigating the effect of angle of reflection on reflected pressures will be described in a forthcoming NDRC report. This work indicates that on a 5° slope, the reflected pressure is 1.9 psi when the incident pressure is 1.5 psi, and is 3.2 psi when the incident pressure is 2.5 psi. If then, on the basis of the Port Chicago discussion above, 2.5 psi is taken as the B damage criterion, it appears that this will occur at distances up the hill at which the hydrostatic pressure would normally be 2 psi. Taking 4000 ft. as the 90% B radius, the charge weight is given as 3400 tons. It is felt that, considering the larger charge, the sloping hill with its increasing effect on pressure and decreasing effect on shielding, and the weak construction of the homes in the region hardest hit, the Halifax and Port Chicago effects are not inconsistent. The sense of disaster was undoubtedly enhanced in the Halifax case by the fact there were so many structures very near, so many people exposed to serious injury and so much disaster brought on by the fire and severe weather which followed. Bronson reports that a barograph record at a distance of 9900 ft. from the explosion showed a pressure of at least .62 psi and might have gone off scale. At this distance .62 psi is to be expected from a charge of 2100 tons.

There are several isolated facts which serve to tie the two incidents together. The greatest distances at which fragments were found at Halifax were reported as 3 - 4 miles. In the Port Chicago case, these distances were 2 - 3 miles. Across the water from Halifax at a distance of 1/2 mile, treee were reported to have blown over. At Port Chicago, telegraph poles a distance of 1/4 mile over land (instead of water) with less area offered to blast (no branches) were tilted away from the explosion at angles of the order of 45°. The formula $d = .18\sqrt{T}$ quoted above when applied to Halifax assuming $T = 2800$ tons gives 9.5 - 10 miles, which is reasonable according to Bronson. At Halifax, "considerable" glass breakage was reported at distances up to 10 miles. The figure for Port Chicago is about 6 - 7 miles.

Concerning earth shock at Halifax, Bronson states the following, which is consistent with the other discussions available: "All evidence points to the fact that the air was the principal factor in the transfer of this energy (from the explosive). Within a radius of 4 or 5 miles the earth wave was distinctly felt and was followed by the concussion of the air which caused all the damage. The experience of the writer (Bronson) confirms this point and indicates in a rough way the relative magnitudes of the earth and air shock. At the time of the explosion I was standing in my laboratory on the second floor -- about 3500 meters from the explosion. I first felt a shaking of the building no greater than that caused by heavy blasting in the railroad cut, but it seemed directly under the building. ... (About 6 - 10 seconds later) the (air) crash came which completely destroyed the windows and sashes on 3 sides of the building, broke heavy doors and locks, and even shifted partitions. The comparatively slight earth shock can be explained by the fact that the explosion was practically on the water, even though the ship was touching ground."

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Ens. Reynolds’ comparison of the Halifax and Port Chicago explosions categorizes structural damage with the terms A, B, and C damage. In Section I of his complete report of the Port Chicago damage due to air blast and earth shock, of which Section IV is his comparison of the

Halifax and Port Chicago explosions, Ens. Reynolds defines the parameters of A, B, and C damage.

A damage, means a building completely demolished.

B damage, means a building damaged beyond repair.

C damage, means a building temporarily uninhabitable, but repairable.

In discussion of the A, B, and C damage caused by the explosions at Halifax and Port Chicago, radius means the distance from the source of the explosion to points where 90 percent of typical buildings have experienced A, B, or C damage. Typical buildings are taken “as structures estimated to have the same resistance to damage as well constructed frame dwellings of American types.” Considering damage at the Port Chicago Naval Magazine, but not damage in Port Chicago town, Ens. Reynolds notes that “some extrapolations and interpolations are necessary because of the nature of construction at the ammunition depot.”

Ensign Reynolds calculates the energy of the Halifax explosion to have been equivalent to 2,800 tons of TNT. Captain Parsons in his 31 August memorandum to Adm. Purnell reports that blast damage from the Port Chicago explosion “was consistent with a high order detonation of 1,500 to 1,600 tons of TNT.” The 2,800 tons charge weight of the Halifax explosion was 1,250 tons (80 percent) greater than the 1,500-1,600 tons TNT equivalent charge weight of the Port Chicago explosion. But as Capt. Parsons wrote to Adm. Purnell, the explosions should be considered to be “of the same order of magnitude.” In scientific quantification an increase of one order of magnitude is the same as multiplying a quantity by 10.

If the Halifax explosion had been one order of magnitude greater than the Port Chicago explosion, rather than of the same order of magnitude, the charge weight of the Halifax explosion would have been 15,000-16,000 tons TNT equivalent, or 10 times the 1,500 to 1,600 tons TNT equivalent charge weight of the Port Chicago explosion. There is, however, a reality quirk about large explosions that will be discussed later: The physical effects of explosions involving a charge weight greater than 1,000 tons of TNT, or the nuclear equivalent, augment only by the cube root of the charge weight of explosive. To determine the physical effects of a 10,000-ton TNT equivalent atomic bomb the effects of a 1,000-ton bomb are multiplied, not by 10, but by 4.

For the Halifax explosion, Ens. Reynolds reported “the average value for the B damage radius . . . is taken as approximately 4,000 feet.” For the Port Chicago explosion, Capt. Parsons reported in his 31 August memorandum to Adm. Purnell, “While no typical structures were near to the B-limit radius, estimates based on blast damage to freight cars at 1,400 to 1,500 feet, and to frame buildings at 3,500 to 5,500 feet, gave 90% B-damage to typical American dwellings at a radius of 2,500 feet.” Class B damage is associated with a blast wave peak over-pressure of 2.5 psi.

Ensign Reynolds provides some discussion of the seismic effect, the earth shock, which resulted from the Halifax explosion, but the Halifax explosion literature does not mention that any seismographic station recorded the Halifax earth shock. Records of the Port Chicago explosion earth shock were made at twelve seismographic stations in California and one in Nevada. Those records will be reviewed and analyzed in a subsequent chapter.

Of particular interest in Ens. Reynolds’ discussion of the Halifax earth shock is a quotation he provides from a report on the explosion written by the Halifax scientist H.L. Bronson and published, 1918, by the Royal Society of Canada: “Within a radius of 4 or 5 miles the earth wave was distinctly felt and was followed by the concussion of the air which caused all the damage.”

At his position 3,500 meters from the Halifax explosion Bronson reports that the air blast arrived 6-10 seconds later than the earth shock. Recognition that at distances 4 or 5 miles from the Halifax explosion the earth shock arrived at least 6-10 seconds before the air blast wave arrival will be important in discussion of the number of explosions that occurred at Port Chicago. Many eyewitnesses to the Port Chicago explosion construed the violence of the earth shock to have been the manifestation of one explosion and the succeeding violence of the blast wave to have been the manifestation of a second explosion.

Dr. Maurice M. Shapiro: Comparison of the water disturbances caused by the Halifax and Port Chicago explosions.

Dr. Shapiro’s sixteen-page report, “Effects of the tidal wave in the Port Chicago explosion of 17 July 1944," is Enclosure E of Capt. Parsons’ 31 August 1944 memorandum to Adm. Purnell, “Port Chicago Disaster: Third Preliminary Report.” Dr. Shapiro’s sixteen-page report includes the three-page Appendix II, “Comparison of the water disturbances in the Port Chicago and Halifax explosions,” from which the following extracts are taken.

“In the Halifax catastrophe a cargo of approximately 2,800 tons of TNT equivalent was involved, whereas in the Port Chicago disaster 1,550 tons exploded. The 3,000 ton ship Mont Blanc, which blew up in Halifax Harbor, was a smaller vessel than the Liberty ship SS EA Bryan, the former having a length of 330 feet and a beam of 40 feet, and the latter 440 feet and 55 feet, respectively. However, the two ships did not differ much in draft, the former drawing 20 feet of water, and the latter 22.5 feet just before they exploded. Each ship was close to shore, the SS EA Bryan being about 450 feet away, whereas the Mont Blanc was believed to have touched ground immediately before the explosion. The depth of water at the explosion site was less than 25 feet at Halifax, and approximately 35 feet at Port Chicago. However, in both ships the explosive cargo appears to have been situated at a level just below the water line, and both events must be considered as practically surface explosions. That is to say, for both cases the mechanism involved in the creation of the tidal wave was the delivery of an impulse to the water by a charge near the surface. In neither case was the charge sufficiently deep, relative to its size, to permit the formation of a dome by a pulsating underwater gas bubble and the spreading of a wave train following the collapse of the dome.

“[For the Port Chicago explosion] the wave height 1,300 feet away was of the order of 6 to 7 feet, and 3,200 feet away it was about 5 feet. [Scant quantitative information on the wave height for the Halifax explosion] permits us to set a rough upper limit of about 12 feet for the wave height at a distance of 800 feet from the explosion.

“It may be reasonably guessed that at Halifax the water flowed up a distance of 1,200 plus or minus 300 feet from the shore; at Port Chicago the farthest incursion of the water was about 1,800 feet from the banks, but this flow occurred in a shallow ravine or slough. From these facts it may be concluded that the tidal wave, like the other effects of the Halifax explosion, was a greater disturbance than that in the Port Chicago disaster.”

Photographs and illustrations credits.

USS *Maine*. Source: U.S. Naval Historical Center, Photograph NH 60255-A. Photographed circa 1895-98.

SS *Fort Stikine,* North Sands Class freighter. Source: Robert G. Halford. *The Unknown Navy: Canada's World War II Merchant Navy*. Ontario: Vanwell Publishing, 1995.

Smoke cloud formed above the explosion of the SS *Mont Blanc*. Source: Nova Scotia Provincial Archives.

View of the Halifax disaster, looking south, 6 December 1917. Source: National Archives of Canada; panorama sections C-019944, C-019948, C-019953. Photographer: W.G. MacLaughlan.

Ensign George T. Reynolds, USNR. "Port Chicago: Analysis of damage due to air blast and earth shock": Enclosure D of Captain William S. Parsons' 31 August 1944 memorandum to Admiral Purnell, "Port Chicago Disaster: Third Preliminary Report." "Section IV. Comparison with Halifax," pages 9-10. Source: Los Alamos National Laboratory.

Chapter 8

Ship Explosions: Black Tom Island, SS *Mary Luckenbach,* SS *Robert Rowan,* USS *Mount Hood*

Black Tom Island, New Jersey, 30 July 1916.

The Black Tom Island, Jersey City, New Jersey, munitions explosions the early morning of Sunday 30 July 1916 in the New York City harbor involved an undetermined quantity of military munitions and other wartime explosives that had been accumulated well beyond legal limits at and in the vicinity of the ship loading pier at Black Tom Island. The munitions and explosives were waiting shipment to Britain and Russia for use against Germany in the First World War, which the United States had not yet entered. The initial explosion of perhaps 1,000 tons of TNT followed an initial shipboard fire and was followed for several hours by sympathetic explosions of nearby munitions stockpiles and the stores of munitions loaded in close-by railroad cars and barges.

The barge *Johnson 17*, moored to the pier at Black Tom Island, was loaded with 50 tons of TNT and 417 cases of detonating fuses. At 2:45 A.M. flame suddenly shot up from one of the munitions-loaded railroad cars on the pier. Simultaneously, another blaze flamed aboard *Johnson 17*. At 3:08 A.M. the earth shook, the sky lit up and Black Tom roared; *Johnson 17,* other munitions-laden barges near the pier and dozens of munitions-laden railroad cars exploded, essentially in one massive detonation. There was a pause of seconds, then a second mighty thunderclap. Over Jersey City the sky was brilliantly lit in a saffron hue. Red-hot shrapnel bombarded the brick walls of warehouses, plowed deep into the planking of the pier and splattered down on the waters of New York Harbor as a sizzling-hot rain.

In Manhattan, Staten Island and Brooklyn, and along a 15-mile stretch of the Jersey shore, men, women and children were thrown from their beds. In thousands of homes, windows facing the explosion were blown-in and windows on opposite walls were blown-out. Terrified by “a rumbling of thunder” and “a deafening roar” thousands of people in the greater New York area rushed in bare feet over broken window glass into the streets, clad in pajamas or nightgowns. In all of lower Manhattan few windows remained intact in any building, including the city’s first skyscrapers. Patrons stepping out of night clubs in Manhattan were brusquely hurled back against the doors by a hot, powerful blast across the water from Jersey City. The arrivals of the earth shock and following blast wave 90 miles away in Philadelphia were sufficiently forceful to awaken most of that city’s residents.

On Ellis Island, less than a mile from the explosion, the Ellis Island Immigrant Processing Center was battered by the blast wave and deluge of shrapnel. Total destruction of the Immigration station and hundreds of deaths and injuries were prevented when two blazing munitions barges that had drifted against the Ellis Island seawall were pulled away by railroad company tugboats and towed into Upper New York Bay where they exploded and sank. Newly-arrived immigrants were led out of their dormitories into a downpour of hot coal cinders. Explosions, with decreasing frequency, continued until dawn. Black Tom was gone, without a trace of the pier, the warehouses, the railroad cars, or the locomotives on and around the pier. Although only four persons were killed by the explosion, destruction and damage were finally calculated at $40 million.

The cause of the initial fire aboard the barge *Johnson 17* and the cause of the fire in the first railcar that ignited were finally established to have been acts of sabotage committed by German agents who had unchallenged access to the unguarded pier. The site of the Black Tom Island explosion is now included in New Jersey’s Liberty State Park, a picnic and recreation area along the Hudson river and adjacent to the old Jersey Central Railroad terminal. A detailed account of the explosion and the long investigation that finally determined that sabotage had been the cause of the disaster is found in Jules Witcover's book, *Sabotage at Black Tom: Imperial Germany's Secret War in America, 1914-1917;* Chapel Hill, NC: Algonquin Books, 1989. "The Black Tom Explosion," an article by H.R. Balkhage and A.A. Hahling in *American Legion Magazine,* August 1964, provides a useful summary account.

Significantly, in 1916 U.S. history Margaret Sanger (1883–1966) was arrested for conducting a birth-control clinic in Brooklyn, NY. The previous year she had been indicted for sending birth-control information through the mails. She organized the first American (1921) and international (1925) birth-control conferences and in 1923 formed the National Committee on Federal Legislation for Birth Control. The same year Jeannette Rankin (1880–1973), a Republican, was elected the first woman in the United States to serve in Congress, 1917–1919; she was also elected a member of the 77th Congress, 1941–43. Rankin voted against the declaration of war on Germany in 1917 and in 1941 cast the only vote in the U.S. House of Representatives against entering World War II. A member of various antiwar organizations, in 1968 she led the Jeannette Rankin Brigade, a peace group, to Washington, DC, to protest the U.S. war in Vietnam.

SS Mary Luckenbach, 13 September 1942.

SS *Mary Luckenbach,* without WW II armament.

SS *Mary Luckenbach* was a World War I vintage cargo ship, one of more than 100 of the Hog Islander Class freighters built, 1918-1920, by the American International Shipbuilding Company at Hog Island near Philadelphia, Pennsylvania. She was propelled by a single screw and, in calm seas, capable of a then speedy 11.5 knots; the Hog Islanders were the world's first large fleet of ships that burned oil rather than coal. SS *Mary Luckenbach*, a typical Hog Islander, was a 7,600 ton cargo ship, 389 feet in length and 54 feet at the beam. The Hog Islander profile showed a raised forecastle and poop deck, and a midship island. A very interesting article of investigative historical research, “The Saga of Hog Island, 1917-1921: The Story of the First Great War Boondoggle,” by James J. Martin, is available at:

http://www.blancmange.net/tmh/articles/hogisle.shtml

In early September 1942, SS *Mary Luckenbach* was one of 39 merchant ships, one or two rescue ships and several oilers that, with a large naval escort, composed Arctic Convoy PQ18 on the treacherous Murmansk Run from Scotland and Iceland to the northern Russian ports of Murmansk on the Barents Sea and Archangel further east and considerably south on the White Sea. The route turned the northernmost point of Norway at North Cape and thence southeast along the Kola Peninsula to Murmansk and then southeast and south to Archangel. The port of Murmansk is above the Arctic Circle but below the southern limit of sea ice, and is kept ice free in winter by the Norwegian Current, an extension of the relatively warm North Atlantic Current. Ships of the Murmansk Run convoys that survived attacks by German submarines positioned along the route and waves of German bombers that flew to meet the convoys from bases at Bodo and Banak, Norway, transported supplies critical to the Russian armies resisting the German invasion. The Soviet Union was attacked by Germany 22 June 1941. On 8 September 1941 the Germans had fully encircled Leningrad (Saint Petersburg) and began a siege that lasted 900 days, until 27 January 1944. During January and February 1942, 200,000 residents of Leningrad died of cold and starvation. At least 640,000 people died in Leningrad during the siege and some estimates put the number at 800,000.

Except the arctic runs to Murmansk and Archangel only the deepwater ports on the Persian Gulf were available to supply the Russian interior, but the long rail lines and roads from the Gulf to the Russian front, and their limited capacity, could not carry the vast quantities of munitions and food required by the Russian armies to defeat and force the retreat of the German invasion. During World War II, 40 convoys with a total of more than 800 ships, including 350 under the U.S. flag, started the

Murmansk Run. One of every eight was sunk by German bombs, torpedoes, mines, or the weather. The ships that got through to Murmansk and Archangel delivered 10-20,000 aircraft, 5,000 tanks, 375,000 trucks, 8,700 tractors, 51,500 jeeps, 1,900 locomotives, 15,000,000 pairs of boots, rifles, machine guns, auto tires, radio sets, and all the other equipment the West could provide needed by the Russian armies fighting on the Eastern Front. Most ships on the Murmansk Run carried some quantity of munitions and explosives among their cargoes. SS *Mary Luckenbach* carried 1,000 tons of TNT.

Location of Bear Island, Murmansk, Archangel

There are several narrative and historical accounts that describe the composition and passage of PQ18, but no two agree in the most significant details. Different accounts report the *Mary Luckenbach* exploded on 12, 13, 14 or 15 September 1942. The U.S. Coast Guard gives the date as 13 September, which is used here. My description of PQ18 is a composite account derived from the several available sources and provides a representative picture of the convoy and its passage. The most comprehensive account of PQ18 is Peter Smith's *Arctic Victory: The Story of Convoy PQ18;* London: Crecy, 1975 and 1994.

PQ18 had assembled in Loch Ewe, northwest Scotland, and sailed 2 September 1942 for Iceland where the convoy's naval escort was increased. The convoy was protected by the largest naval escort of any of the wartime convoys on the Murmansk Run. Close cover was given by the British escort aircraft carrier HMS *Avenger*, the anti-aircraft cruiser HMS *Scylla* and sixteen fleet destroyers; farther out three British heavy cruisers and their destroyer escorts shadowed the convoy. More distant cover was provided by the battleships HMS *Anson* and HMS *Duke of York*, together with a light cruiser and destroyers to the northeast of Iceland. British Home Fleet submarines patrolled off the Norwegian Lofoten Islands and northern Norway.

On 13 September the Liberty ship SS Oliver Ellsworth was torpedoed by the German submarine *U-589* and sunk. The U.S. Coast Guard compilation "U.S. Merchant Ship Losses During the Second World War" reports the *Oliver Ellsworth* went down in the World War II "Graveyard of the Arctic" at 75.52° N, 7.55° E, near Bear Island that is half-way between north Norway and the south cape of Spitsbergen (Svalbard). Later that day more than 40 German aircraft attacked the convoy. Junkers-88 torpedo-bombers dropped some 70 torpedoes at about 1,000 yards range against the long lines of slow freighters. Eight ships were sunk, including the *Mary Luckenbach* which was hit by an aerial torpedo. The U.S. Coast Guard gives her final location as 76° N and 10° E, which shows PQ18 had progressed somewhat to the north and east after the *Oliver Ellsworth* was sunk. According to the log of the Liberty ship SS *Esek Hopkins* the torpedo that destroyed the *Mary Luckenbach* was dropped by a burning plane and the explosion of the *Mary Luckenbach* is said to have destroyed that plane and another as well. Therefore, the ship exploded within several seconds of the torpedo hit.

SS *Mary Luckenbach* disintegrated when her cargo of 1,000 tons of TNT detonated in one massive explosion. The number of her merchant crew and Navy Armed Guard complement are given inconsistently by different sources, 38-46 for her merchant crew and 16-27 for her Navy Armed Guard complement; all perished. Robert Hughes, Royal Navy Volunteer Reserve (RNVR) Gunnery Control Officer on HMS *Scylla* provides a description of the explosion in his book, *Flagship to Murmansk (Through the Waters*); London: William Kimber & Co., Ltd., 1956: "A stupendous column of smoke was rocketing to heaven, and as we looked an immense glow lit the column, and great cerise, orange-and-yellow fragments arched outwards towards us . . . [later] the great smoke column was still thousands of feet high and mushrooming out

SS *Mary Luckenbach* explosion.
Turbulent cloud of flame,
at estimated 5-10 seconds.

where it met the clouds. At its base flames still flickered and the following ship was altering course to avoid them."

SS *Mary Luckenbach* explosion. Stem of the mushroom cloud, at estimated 15-20 seconds

Nothing was left of the ship except a pillar of smoke when rescue craft arrived to look for survivors. Nearby, the blast shook SS *Schoharie*, also a Hog Islander, as though she had been torpedoed, throwing men flat on the deck while fragments of hot steel from the exploded ship crashed down upon her from bow to stern. On the Liberty ship SS *Nathaniel Greene,* Captain George Vickers had just swung his ship away from one of several aerial torpedoes when the *Mary Luckenbach* blew up. He thought at first that his ship had been hit and ordered the crew to lifeboat stations. The blast threw gunners from their stations, smashed the galley, broke doors, and also showered the vessel with debris, including shell casings from the *Mary Luckenbach*'s guns. Captain Richard Hocken of the Liberty ship SS *William Moultrie,* steaming in the same column immediately astern of the *Mary Luckenbach,* said that when his ship passed over the spot, "there was nothing left of her at all—not even a raft—no wreckage, not even a match box; hardly a ripple on the surface of the sea."

The three photos that show the explosion of the *Mary Luckenbach* vividly document the progression of the explosion as viewed from three different ships in the convoy. The first photo was taken, I estimate, 5-10 seconds after the moment of detonation and shows the initial cloud of turbulent flaming gasses generated by the explosion. Taken within a few seconds of the explosion, this remarkable photograph required a man capable of very quick mental and physical responses, and who had a camera at hand. Since the convoy was under German air attack at that moment I am inclined to credit the photograph to an unknown Navy Armed Guard anti-aircraft gunner whose mental and physical responses as a gunner enabled him to quickly target and shoot this photo.

SS *Mary Luckenbach* explosion. Smoke column, at estimated 2-3 minutes

The second photo, which I estimate was taken some 15-20 seconds following the detonation, shows the stem of the cloud of smoke that has formed a mushroom cloud. The third photo in the sequence shows the column of smoke that has pierced through the cloud ceiling 2-3 minutes after the explosion; the base of the cloud cover is probably 2,500 feet above the water.

Convoy PQ18, after fighting nearly all the way, arrived at its destination, Archangel, 21 September. Planes and U-boats had sunk thirteen of the convoy's ships at a cost of six German U-boats and 41 aircraft during one week of fighting. In recognition of gallantry during the passage of PQ18 the U.S. Maritime Administration awarded the officers and seamen of the *Nathaniel Greene* and the *William Moultrie* the Gallant Ship Citation Ribbon, and both ships were awarded the Gallant Ship Plaque. The texts of the citations read:

SS Nathaniel Greene

"During a long voyage to North Russia, SS *Nathaniel Greene* was under incessant and violent attack by enemy planes and submarines. In most gallant fashion, and in spite of many crew casualties, she consistently out-maneuvered and out-fought the enemy, finally discharging her vital cargo at the designated port. After effecting temporary repairs to her battered hull and rigging, she took part in the North African Campaign. Bound for her last port, with limited cargo, she was torpedoed, and in a sinking condition was successfully beached.

"The stark courage of her heroic crew in battle against overpowering odds caused her name to be perpetuated as a Gallant Ship."

SS William Moultrie

"Emerging victoriously from an extremely battered convoy, numbering many sunken ships, SS *William Moultrie* arrived at the scheduled North Russian port and discharged her vitally needed cargo. Expert maneuvering and coordinated gun control during the highly concentrated submarine and bombing attacks over a period of one week prevented crew casualties and brought the series of actions to a successful conclusion. “The stark courage of her heroic crew in defeating a relentless enemy caused her name to be perpetuated as a Gallant Ship.”

For conduct during the passage of PQ18 Captain Richard Hocken of SS *William Moultrie* was awarded the Merchant Marine Distinguished Service Medal, with the citation:

“His ship, the SS *William Moultrie*, in a convoy which suffered heavy losses, fought through a week of continuous attacks by enemy bombers and submarines to deliver her cargo of war material to a north Russian port. In the course of the long, running battle, the ship was directly attacked 13 times and was credited with downing eight planes and with scoring hits on 12 others. During the first attack on the convoy, the *William Moultrie* distinguished herself by shooting down three torpedo planes and assisting in the destruction of six more. The following day her guns shot down four more of the attacking planes and damaged five. Later, after successfully repelling another attack by planes, four torpedoes were sighted heading for the ship. The guns fired on them, exploding one and the other three were eluded by skillful seamanship. Captain Hocken, master of a gallant ship and a gallant crew, exhibited qualities of leadership and high courage in keeping with the finest traditions of the U.S. Merchant Marine.”

SS Robert Rowan, 11 July 1943.

During Operation Husky the Allies launched an invasion of Sicily from North African bases, 9-11 July 1943. General George S. Patton landed two and a half divisions of the U.S. 7th Army at Licata and Gela, on the island’s south coast, and Field Marshal Bernard Law Montgomery (“Monty”) landed four and a half divisions of the British 8th Army at several places on the southeast coast, near Syracuse. The Sicilian invasion was accomplished without significant opposition on the beaches but inland, before the island was captured after a campaign of 39 days, ten divisions of General Mussolini’s Fascist Italian army, supported by two German divisions, took a toll of 31,158 Allied forces killed, wounded or missing. Estimated German casualties were 37,000; 7,000 were captured, but 60,000 German troops were evacuated across the Strait of Messina to Italy. Casualties among Italian troops are estimated to have been 130,000.

Benito Mussolini was Italy's Fascist dictator from 1922 to 1943. On 29 September 1943 Marshal Pietro Badoglio, who represented the Italian Government because Mussolini had fled from Rome, surrendered to U.S. General Dwight David Eisenhower, Commander-in-Chief, Allied Forces, North Africa. On 28 April 1945, just before the Allied armies reached Milan, Mussolini and his mistress Clara Petacci were captured by Italian partisans near Lake Como as he tried to take refuge in Switzerland. He was summarily executed and his body was later strung up by its heels on the Piazzale Loreto in Milan. The same day, 28 April, south of Milan, the Brazilian Expeditionary Force surrounded the German 148th Grenadier and Italia Bersaglieri Divisions. The German commander surrendered the following day and during the next twenty-four hours the Brazilians collected more than 13,500 German prisoners. Often neglected in summary discussions of the Allied forces that contributed significantly to the capture of Italy's valley of the river Po and Milan were the Japanese-American 442d Regiment, the 1st Brazilian Infantry Division, the free Italian Legnano Combat Group (U.S. II Corps and Fifth Army, and the 6th South African Armored Division.

Helping to lift the great American and British armies and their supplies to the Sicilian invasion beaches were scores of Liberty ships that had assembled in North African ports over a period of many weeks. Most of the Liberty ships had temporary accommodations for some 200 troops in addition to the civilian merchant marine crew and Navy Armed Guard complement. On 11 July at 1:50 P.M. a fleet of perhaps 35 Junkers Ju-88 vertical bombers were overhead targeting their bomb loads on the ships assembled in the harbor at Gela.

The munitions-laden Liberty ship SS *Robert Rowan,* carrying her crew and troops of the U.S. 18th Infantry, took a bomb in her forward hold which started an uncontrollable fire; the ship was expected to explode immediately. All other ships in her immediate area began moving out of range. SS *Robert Rowan*'s captain ordered her abandoned. Before the *Robert Rowan* exploded 20 minutes later at 2:15 P.M. all 421 men aboard her when the bomb hit were taken off by PT boats (patrol torpedo boat) and transported to the destroyer USS *McLanahan* (DD-615). After the men from the *Robert Rowan* were taken aboard *McLanahan*, in the words of George E. Smith, USN, of *McLanahan*'s Engineering Department, "we shagged out of there." The noise of the explosion, he said, "was indescribable," and the explosion "strung parts of that ship all over the area."

Placing an ammunition ship at risk of concentrated German bomber attack in the transport attack area of the harbor at Gela signified that the *Robert Rowan*'s mission was to provide re-supply ammunition to the troops that had landed. Ships at anchor or maneuvering in the attack transport area would not have carried naval warship ammunition or bombs; therefore the *Robert Rowan*'s ammunition cargo certainly did not include the tons of concentrated TNT explosives of naval depth charges and aerial bombs and consisted principally of artillery, mortar and gun ammunition. Ammunition of those types generally will not explode "high order" in a fire and most of it will burn furiously, deflagrate, or detonate "low order." SS *Robert Rowan* burned furiously for 20 minutes before she exploded.

SS *Robert Rowan* explosion

Although the explosion of the *Robert Rowan* was a mammoth detonation and possibly equivalent to a high order explosion of 500 tons of TNT, the ship did not entirely disintegrate; she was broken in half and only partially submerged when she sank to the shallow harbor bottom. Her intact portions continued to burn with intermittent small explosions through the afternoon and night. Above the initial fire, and following the first large explosion, a huge pillar of black and white smoke rose to an estimated 4,500 feet; at the lower levels, up to perhaps 2,000 feet, the cloud was fitfully punctuated by the aerial explosions of artillery shells and tracer ammunition thrown up from the devastated hulk as the remainder of her munitions cargo burned and detonated in relatively small bursts. At 6:00 A.M., 13 July, the above-water remains of SS *Robert Rowan* were still smoking.

USS Mount Hood, 10 November 1944.

The ammunition ship USS *Mount Hood* (AE-11) was built by the North Carolina Shipbuilding Company, Wilmington, North Carolina; launched 28 November 1943; acquired by the Navy on loan-charter basis, 28 January 1944; converted by the Norfolk Shipbuilding & Dry Dock Co., Norfolk, Va., and the Navy Yard, Norfolk, Va.; and commissioned 1 July 1944. She was 459 feet of length, 63 feet at the beam, and displaced 13,910 tons. At 8:50 the morning of 10 November 1944 USS *Mount Hood* was at anchor at berth 380 in Seeadler Harbor, Manus Island Naval Base, Admiralty Islands northwest of New Guinea. She was acting as a floating ammunition depot, simultaneously receiving munitions by lighters from, and delivering munitions to, other ships in the harbor. It was fortunate that the explosion did not occur one month earlier when Seeadler Harbor was packed with at least 600 ships and possibly as many as 1,000, including those of the U.S. Seventh Fleet commanded by Vice Admiral Thomas Cassin Kinkaid.

USS *Mount Hood*

Admiral Kinkaid was at that time himself under the direct command of Army General Douglas MacArthur, and the Seventh Fleet was then known as “MacArthur’s Navy.” The Seventh fleet was staging for General MacArthur’s invasion of Leyte Island in the Philippines and the Battle for Leyte Gulf, 23-26 October 1944; the Battle for Leyte Gulf is generally considered by naval historians to have been the greatest naval battle ever fought. At the time the *Mount Hood* exploded there were no major combatant ships in the harbor at Manus, but there were 272 cargo vessels, troop transports, oilers, and other noncombatant auxiliary vessels at anchor; 37 large ships and 56 smaller craft in the harbor within a 2,400-foot (approximately one-half mile) radius of the *Mount Hood* were severely damaged, mainly by the initial barrage of steel fragments from the disintegrated ship. The radius of Class B damage from the explosion of the *Mount Hood* was essentially the same as for the Port Chicago explosion.

Aboard USS *Mount Hood* the ship's crew and other Navy personnel were working all the ship's five cargo holds when she exploded in one massive detonation that entirely disintegrated and fragmented the ship; personnel casualties on the *Mount Hood* and on other vessels totaled 45 known dead, 327 missing and 371 injured. Witnesses saw a small explosion, about the size of a single bomb, that was followed a few seconds later by the main explosion. The Navy Board of Investigation convened to inquire into the explosion determined, "There were approximately 3,800 tons of ammunition aboard. This included bombs, projectiles, fixed ammunition, rockets, both bodies and motors, smokeless powder, aerial depth bombs, and nose fuses. Torpex loaded depth bombs were apparently coming on board."

Of the approximately 3,800 tons of ammunition cargo weight aboard the *Mount Hood,* the projectiles and fixed ammunition, rocket bodies and motors, smokeless powder and nose fuses among that cargo, whatever their amount, would not have detonated high order but would have burned in the explosion or detonated low order. The *Mount Hood* Board of Investigation findings did not determine nor estimate the portion of the 3,800 tons of munitions cargo weight aboard the exploded ship that would be expected to burn or detonate low order, but we will make the estimate that 33 per cent of the 3,800 tons of munitions cargo weight aboard the *Mount Hood* were munitions that would be expected to burn or detonate low order. Thirty-three per cent of the munitions cargo weight aboard the *E.A. Bryan* were determined to be munitions that would burn or detonate low order in the explosion of that ship.

A 33 per cent reduction of the 3,800 tons munitions cargo weight aboard the *Mount Hood* provides a probable total high explosive munitions cargo weight aboard *Mount Hood* of 2,546 tons. This estimate will permit ready comparison of the high explosive munitions cargo weight aboard the *Mount Hood* with the high explosive munitions cargo weight aboard the ship *E.A. Bryan* that exploded at Port Chicago.

Records in the Archives of Los Alamos National Laboratory that describe in detail the circumstances of the 17 July 1944 explosion of SS *E.A. Bryan* at the Port Chicago Naval Magazine pier show that 4,373 tons of munitions cargo weight were aboard the *E.A. Bryan* at the time of the explosion. But of those 4,373 tons of munitions cargo weight, Los Alamos determined that 1,434 tons (approximately 33 per cent) were 5-inch and 3-inch anti-aircraft projectiles and 40 mm. cartridges that would have burned or detonated low order in the explosion of the *E.A. Bryan*. The total high explosive munitions cargo weight aboard SS *E.A. Bryan* was thus 2,930 tons, which compares with the 2,546 tons high explosive munitions cargo weight aboard USS *Mount Hood.*

The high explosive munitions cargo weight aboard the *E.A. Bryan* thus exceeded, by 384 tons, the high explosive munitions cargo weight aboard the *Mount Hood.* However, if the TNT charge weight of those 384 tons of high explosive munitions is considered that difference is seen to be insignificant in the context of the two ship explosions.

The cargo weight of the high explosive munitions aboard the two ships can be analyzed to ascertain the TNT charge weight of those high explosive munitions. Los Alamos determined that the TNT charge weight of the 2,930 tons high explosive munitions aboard the *E.A. Bryan* was equal to 1,577 tons of TNT, which is essentially 53.59 per cent of the cargo weight of the high explosive munitions aboard the *E.A. Bryan.*

The 2,546 tons cargo weight of high explosive munitions aboard the *Mount Hood* was, therefore, equal to 1,364 tons of TNT (2,546 x 53.59 per cent = 1,364). The TNT charge weight aboard the *E.A. Bryan* was 1,577 tons. The explosion of the *Mount Hood* thus involved 213 tons of TNT less than the explosion of the *E.A. Bryan* (1,577 - 1,364 = 213).

In the context of the two ship explosions the difference of 213 tons of TNT is insignificant.

For comparative purposes the Port Chicago explosion of SS *E.A. Bryan* and the explosion of USS *Mount Hood* involved essentially the same TNT charge weight of World War II high explosive munitions of the types employed by the U.S. in the Pacific Theater of war. The explosions of the *E.A. Bryan* and the *Mount Hood* should be indistinguishable in all their physical manifestations, but the two explosions are readily distinguishable by the considerable difference of height to which the column flame and smoke cloud rose above each of the two explosions..

Previously classified Manhattan Project documents in the Archives of Los Alamos National Laboratory document that, at Port Chicago, a column of flame rose 7,000-10,000 feet above the explosion; no measure of the height to which the smoke cloud ascended above the Port Chicago explosion is provided by those documents, presumably because the dark smoke cloud was invisible against the black, moonless night sky. But, as we have learned from review of the several explosions detailed in this and the previous chapter, the height to which the smoke cloud rose above the Port Chicago explosion was necessarily many thousands of feet higher than the 7,000 to 10,000-foot height to which the column of flame rose above Port Chicago.

The column of flame rising from the Port Chicago explosion was, however, easily discernable against the dark night sky at 10:30 the evening of 17 July 1944. The rising column of flame was precisely observed by the pilots and one copilot of two Army Air Corps airplanes flying line-of-sight toward Port Chicago when the explosion occurred. First Lieutenant Sidney P. Phillips of the Reno Army Air Base, pilot of a C-49 cargo plane, was 5-8 miles from the explosion at altitude 9,000 feet; his copilot was Second Lieutenant Fred Dregor, Jr. Second Lieutenant R.A. Smith, also of the Reno Army Air Base (type of plane unreported), was 3.5 miles from the explosion at 7,000 feet altitude.

The two pilots and copilot Dregor provided excellent descriptions of the towering Port Chicago column of flame in testimony to the Navy

Court of Inquiry that investigated the explosion. Those testimonies are summarized in two of Capt. William Parsons' reports from Los Alamos that were addressed and transmitted to Adm. William Purnell of the Atomic Bomb Military Policy Committee. In his 4 August 1944 memorandum, "Port Chicago Disaster: Second Preliminary Report," Captain Parsons informed Adm. Purnell, "Two Army airplanes witnessed the explosion, the pilots agreeing that the flame rose to 8,000 feet." In his 31 August 1944 memorandum to Adm. Purnell, "Port Chicago Disaster: Third Preliminary Report," Capt. Parsons wrote, "The explosions resulted in a column of flame which expanded and billowed at the top as it rose, and reached an altitude of 7,000 to 10,000 feet."

The column of flame from the Port Chicago explosion, which expanded and billowed and reached an altitude of 7,000 to 10,000 feet, may be instructively compared with the column of flame that rose 10,000 feet above the 16 July 1945 nuclear explosion conducted at Trinity site in New Mexico. Major General Leslie R. Groves, military director of the Manhattan Project, was an observer at the Trinity explosion. In his 18 July 1945 memorandum to Secretary of War Henry Lewis Stimson the General reported the characteristics of the fireball, column of flame, and smoke cloud that formed above the Trinity explosion:

"For a brief period there was a lighting effect within a radius of 20 miles equal to several suns in midday; a huge ball of fire was formed which lasted for several seconds. This ball mushroomed and rose to a height of over ten thousand feet before it dimmed . . . A massive cloud was formed which surged and billowed upward with tremendous power, reaching the substratosphere at an elevation of 41,000 feet, 36,000 feet above the ground, in about five minutes, breaking without interruption through a temperature inversion at 17,000 feet which most of the scientists thought would stop it."

The expectation held by "most of the scientists" that the temperature inversion would stop the ascent of the rising smoke cloud from the Trinity test is first mentioned in the "History of 10,000 ton gadget," which mathematically modeled and predicted the effects of the Trinity test. Step 10 of the "History" predicted the Trinity ball of fire and succeeding smoke cloud would “mushroom out at 18,000 ft in typical Port Chicago fashion.” The scientists expected the cooling top of the rising smoke cloud from the Trinity test would lack sufficient heat-driven buoyant force to pierce through the heavy, cold air at the base of the temperature inversion which is typically layered at 18,000 feet. The smoke cloud that rose above the 1,000 tons of TNT that exploded aboard SS *Mary Luckenbach* lacked sufficient heat-driven buoyant force to pierce through the cold, heavy cloud cover lying only 2,500 feet above that explosion. Robert Hughes, Gunnery Control Officer on HMS *Scylla* reported the smoke cloud above the explosion of SS *Mary Luckenbach* mushroomed out “where it met the clouds.”

Although no measurement of the height of the smoke cloud above the Port Chicago explosion is found among presently available Los Alamos documents that describe the explosion, the rising smoke cloud from the Port Chicago explosion would have been imaged and tracked by radar installations operating, at the time of the explosion, at the Fairfield-Suisun Army Air Base, now Travis Air Force Base, 20 air miles north of the Port Chicago Naval Magazine. The Fairfield-Suisun AFB radar units were of two types: the microwave SCR-584 automatic tracking radar, a particularly fine precision radar used for aircraft positioning data and antiaircraft gun laying, and the microwave SCR-615B fixed radar unit. Both systems, with a range of 40 miles, worked very well for cloud imaging and storm tracking. For many years the National Oceanic and Atmospheric Administration (NOAA) used an unmodified SCR-584 as a weather radar.

General Groves’ 18 July 1945 report to Secretary Stimson includes, as quoted text, a report from the General’s liaison officer positioned at the Alamogordo Army Air Base, 60 miles from Trinity site. That unnamed officer reported:

“The original flash lasted approximately 10 to 15 seconds. As the first flash died down, there arose in the approximate center of where the original flash had occurred an enormous ball of what appeared to be fire and closely resembled a rising sun that was three-fourths above a mountain. The ball of fire lasted approximately 15 seconds, then died down and the sky resumed an almost normal appearance.”

Although the energy yield of the Trinity explosion was one order of magnitude greater than the energy yield of the Port Chicago explosion, the characteristic temperature of the nuclear fireball and succeeding column of flame produced by the Trinity explosion was the same as the temperature of the fireball and succeeding column of flame produced by the Port Chicago explosion. The ball of fire and column of flame produced by the Port Chicago explosion were typical of a nuclear explosion, and were accurately used by Joseph O. Hirschfelder and William G. Penney to predict the characteristic behavior of the fireball and column of flame that did result from the Trinity nuclear weapon test. General Groves' report on the Trinity site test is available at:

http://www.nuclearfiles.org/menu/library/correspondence/groves-leslie/corr_groves_1945-07-30.htm

In Capt. Parsons' "Port Chicago Disaster: Third Preliminary Report," Appendix I of Enclosure D, "Report of Ensign G.T. Reynolds, USNR," we find a summary of the Court of Inquiry testimony of pilots Phillips and Smith, and Phillips' copilot, Dregor. Lieutenant Smith flying at 7,000 feet altitude testified, in part, that he had witnessed "one pyrotechnic display or one mass of billowing flame extending to at least 1,000 feet above the plane . . . The flame was first observed as 2,000 to 3,000 feet high and it continued to build up for about 15 seconds to a height of at least 8,000 feet."

USS *Mount Hood* explosion

Lieutenants Phillips and Dregor flying at 9,000 feet reported, in part, "There was a terrific white flash with a smoke ring about 3 miles in diameter. Then in the center a terrific flash 'whooshed' up to at least 9,500 feet." Ensign Reynolds reported in this document that the observations of Lt. Smith were "apparently good information," but "not as good" as the observations of Lieutenants Phillips and Dregor, which are only summarized here but will be provided in their entirety in a later chapter.

For the explosion of the *Mount Hood*, the Navy Board of Investigation had determined by 9 December 1944, from observations made by competent eyewitnesses, that above the daytime explosion of the *Mount Hood* the “flame and smoke” had risen to a height of 7,000 feet. It must be understood that the maximum height achieved by the smoke cloud was 7,000 feet and that the flame from the explosion of the *Mount Hood* would necessarily have risen to a much lesser height; probably, in my estimation, not more than 1,500 feet above the explosion. The USS *Mount Hood* Board of Investigation determined, “The flame and smoke from this explosion extended about 1,000 feet in radius and rose 7,000 feet.”

Concerning the cause of the explosion of the *Mount Hood* the Board of Investigation ruled that the evidence indicated “the possibility of the detonation of TPX-loaded [torpex] depth bombs while it [sic] was being loaded into Number 3 or 4 hold. Detonation could have been caused by striking the hatch with the bombs on the way down or dropping them into the hold carelessly.”

For additional information on the explosion of USS *Mount Hood* see: “Flash of Darkness” by Dale P. Harper, *World War II* magazine (publication date not available), and “The USS *Mount Hood* Explosion” at:

http://www.geocities.com/Athens/Acropolis/3535/mthood.html

For additional photos of the explosion of USS *Mount Hood:* U.S. Navy Historical Center.

Photographs and illustrations credits.

SS *Mary Luckenbach*, without WW II armament. Source: Courtesy Peter Thompson, Department of Economics, Florida International University. Photo available at: http://www.fiu.edu/~thompsop/liberty/hog_island.html

Map showing location of Bear Island, Murmansk, Archangel. Source: Mapquest.com

SS *Mary Luckenbach* explosion. Turbulent cloud of flame, at estimated 5-10 seconds. Source: www.culture24.org.uk/places+to+go/south+west/bristol/art24845

See also the same or similar photo, p. 109; Bunker, John. *Heroes in Dungarees: The Story of the American Merchant Marine in World War II*. U.S. Naval Institute Press, 2006.

SS *Mary Luckenbach* explosion. Stem of the mushroom cloud, at estimated 15-20 seconds. Source: WWW site unrecoverable.

SS *Mary Luckenbach* explosion. Smoke column, at estimated 2-3 minutes. Source: Robert Hughes, *Flagship to Murmansk (Through the Waters)*. London: William Kimber & Co., Ltd., 1956.

SS *Robert Rowan* explosion. Source: U.S. Army Signal Corps Photo, MM-43-L-1-23 (Lieutenant Longini). Available at: http://www.geocities.com/Pentagon/Base/1250/dd615smith.html

USS *Mount Hood.* Source: Bureau of Ships Collection, U.S. National Archives; photo No. 19-N-70330. Available at: http://www.history.navy.mil/photos/sh-usn/usnsh-m/ae11.htm

USS *Mount Hood* explosion. Source: U.S. Naval Historical Center photograph from the War Diary, Manus Island Naval Base, for November 1944; photo No. NH 96173. Available at: http://www.history.navy.mil/photos/sh-usn/usnsh-m/ae11-k.htm

Chapter 9

Historical Record: "The Port Chicago, California, Ship Explosion of 17 July 1944"

The many hundreds of pages of previously classified Government records that report the circumstances and physical manifestations of the Port Chicago explosion are composed, in the greater part, of extensively detailed data obtained by measurement of the actual, physical effects of the Port Chicago explosion. Those data and analyses of those data were necessary to confirm the Manhattan Project's mathematically modeled, theoretical forecasts of the destructive effects that would result from the use of atomic bombs designed to accomplish military objectives, tactical and strategic.

Most of the comprehensive data and analyses of those data that are available in Government Port Chicago explosion records are extraneous to the purpose of this book and will not be considered. Sections of available Port Chicago explosion records, for example, that precisely detail and mathematically dissect the "Percentage of plaster damage to total houses damaged" and the "Frequency distribution of number of structural members broken by buildings, area" would be neither instructive nor interesting to a general readership.

Information summaries that exist in the Port Chicago explosion records do, however, expertly condense many pages of detailed data and data analyses; those summaries are so well written that they provide excellent, succinct statements of information appropriate to the general reader's interest, which is to comprehend the artifacts of the explosion and to arrive at a determination of its cause. Information summaries and germane portions of the text of Port Chicago explosion records presented in this and subsequent chapters will be exactly transcribed, except obvious typographical errors in an original record will be corrected. For example, "testimoney" will be correctly spelled.

"The Port Chicago, California, Ship Explosion of 17 July 1944." Army-Navy Explosives Safety Board Technical Paper No. 6. Washington, D.C., 1948.

This record is approximately 165 pages including text, maps, charts, tables and diagrams; the record was declassified 29 March 1957 by Commander H.E. Jennings, USN, for the Armed Services Explosives Safety Board (ASESB), now the Department of Defense Explosives Safety Board (DDESB). Many of the maps, charts, tables and diagrams in available copies of this record are so poorly copied that they are only partially legible. I obtained one copy of this record in 1981. This record was re-classified, 1982, to be available only to "qualified" Department of Defense contractors. By the courtesy of Dan Tikalsky, retired Concord Naval Weapons Station Public Affairs Officer, one copy was deposited, 1992, with the Office of the Regional Historian, U.S. National Park Service Western Regional Office, San Francisco. That office erroneously credits Commander H.E. Jennings as the author of this report. Colonel D.C. Hall, president of the Army-Navy Explosives Safety Board, in his foreword to the report, credits Army-Navy Explosives Safety Board staff member Dr. Ralph Ilsley with "the analysis and correlation of the data and the preparation of this report." Commander Jennings approved the 1957 declassification of this report but had no role in the 1948 report preparation or writing.

Extracts from:

"The Port Chicago, California, Ship Explosion of 17 July 1944."

ABSTRACT.

> *The explosions on 17 July 1944 at Port Chicago, California, of about 3,500,000 pounds of explosives in railroad cars on the pier and in the holds of a ship resulted in the death of 320 people, injuries to 390, and property damage estimated to be $13,000,000. The 10,000 claims submitted to the U.S. Navy Board of Investigation and the voluminous testimony of the U.S. Navy Court of Inquiry have been reviewed and utilized so as to present a factual narrative of important aspects of the explosion such as types and magnitude of injuries, zones of major damage, types of damage to houses and contents, magnitude of damage in relation to direction of structure to blast wave, damage by the water wave, magnitude and type of missiles, extent and depth of true crater, the 'false' crater, decrease in magnitude and types of damage with greater distance from the pier and relation of formulas of limiting distance of structural damage – British and American – with actual facts.*

The damage relationships by types, magnitude, direction and distance from the pier are recorded in the report by description, charts, tables, maps and in many cases by the determination of a formula for the fitted curve for the amount of damage per locality.

The U.S. Naval Magazine, Port Chicago, California.

The location of Port Chicago as a Naval Magazine was chosen with extreme care, in a sparsely settled area, with deep tidewater along the northern boundary and with two transcontinental railways on the southern boundary. The ship loading pier was built to transfer ammunition from railroad cars directly into deepwater ships. After several modifications the pier was completed in May, 1944, so that two ships could be handled simultaneously on opposite sides of the pier. An additional facility, consisting of a marginal pier with two ship loading berths in tandem, was in later stages of completion at the time of the explosion. The Naval Magazine of Port Chicago was primarily a transfer facility and the magazine's responsibility started with the receipt of loaded railway cars and ended when the cargo had been stowed in ships or barges.

The completed pier had three tracks, and at each edge a loading platform 18 feet wide and car floor high. Railway cars were spotted opposite the holds into which the material was to be loaded. The center track was used primarily for switching, but occasionally railway cars were spotted on the center track opposite a hatch and the material handled through the car just emptied. The physical limitations of the pier prevented unnecessary concentration of ammunition on the pier.

The material was taken out of the cars, placed on the platform under the ship's booms, hoisted on board, and stowed in the holds. The ships were loaded on a three-shift schedule to meet the required ammunition shipments. In collaboration with Port Chicago and Service Force, the Port Director prepared a loading plan for each ship and, as agents for the operators of the ship, submitted it to the Captain of the Port for a loading permit. The separation of various classes of explosives and the stowage of the same in merchant ships were determined according to regulations in effect on 17 July 1944; namely, "Regulations Governing Transportation of Military Explosives on Board Vessels During present Emergency." (U.S. Coast Guard, 1 October 1943.)

A senior loading officer was in charge of all loading and qualified junior loading officers were on the pier during all periods of activity.

Land security from unauthorized intrusion was maintained by a Marine Sentry System. Waterfront security was maintained by Coast Guard patrol boats. Prevention and control of fires was planned by adequate fire apparatus on land, fire watch, pumpers on loading pier and a Coast Guard fire barge secured at the end of the ship loading pier. Smoking was prohibited except in specified areas. Automobiles and trucks were not permitted on the pier beyond the pier office.

*Factual Details Immediately Prior to the Explosion.**

(*Throughout most of the report, the singular form will be used although two distinct explosions were verified and the possibility of three indicated.)

Ships and Pier.

The S.S. Quinault Victory, a new vessel of the Victory type, was moored starboard side to, headed east at the outboard berth. Port Chicago Naval Magazine personnel were rigging the ship for loading and all hatches except No. 5 were about ready to load at the time of the explosion. Dunnage and loaded cars of ammunition and bombs were spotted on the pier beside the Quinault Victory in preparation for the initial loading and stowage at midnight.

The S.S. E. A. Bryan, a new vessel of the Liberty type, had completed one trans-Pacific trip and after undergoing voyage repairs had been assigned to Port Chicago for a cargo of ammunition and bombs. It moored on 13 July 1944 and thereafter loaded continuously day and night until the explosion of 17 July 1944 at about 10:19 P.M. Pacific War Time. At the time of the explosion there were approximately 4,600 tons of ammunition and bombs, containing 1780 tons of high explosives and 200 tons of smokeless powder, in or being loaded in the various holds. The E. A. Bryan was moored starboard side to, headed west at the inboard berth. Sixteen railroad cars, loaded with various types of ammunition and bombs, were spotted on the pier. The cars had approximately 430 tons of cargo containing 150 tons of high explosives and 10 tons of smokeless powder.

[Note. The “Abstract” of this Army-Navy Explosives Safety Board record states that “about 3,500,000 pounds of explosives in railroad cars on the pier and in the holds” of the *E. A. Bryan* were available to the Port Chicago explosion. “Explosives” as used here means the

TNT charge weight of the munitions on the pier and in the holds of the *E. A. Bryan;* 3,500,000 pounds equates to 1,750 tons of TNT.

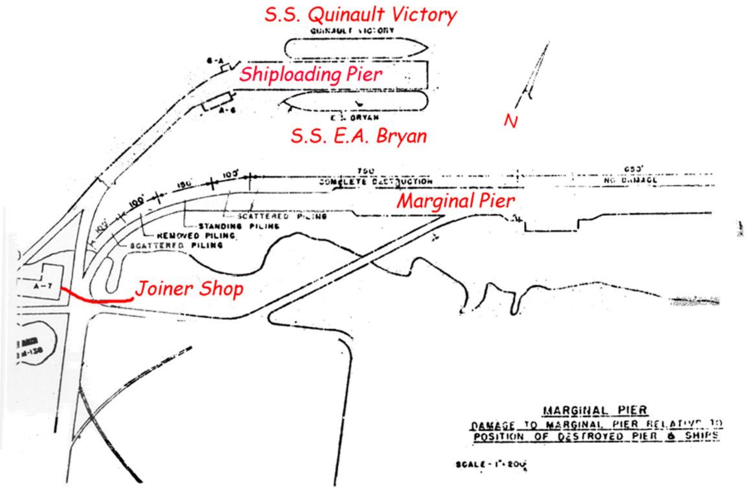
[In Chapter 8 we established that a TNT charge weight of 1,577 tons was on board the *E. A. Bryan.* The 150 tons of high explosive (TNT) in cars on the shiploading pier mentioned in the record paragraph above, when added to the 1,577 tons of TNT aboard the *E. A. Bryan,* gives a total TNT charge weight for the Port Chicago explosion of 1,727 tons or 3,454,000 pounds of TNT. The difference between 3,454,000 and 3,500,000 pounds, which latter number is cited in the “Abstract,” is 46,000 pounds or 23 tons. In the context of the Port Chicago explosion a difference of 23 tons TNT charge weight is insignificant—“about 3,500,000 pounds” were available to the explosion.

[However, in a later section of this record the National Defense Research Committee (NDRC) is reported to have used 4,272,000 pounds (2,136 tons) of TNT to represent the energy of the Port Chicago explosion. The 409 tons difference between 1,727 and 2,136 tons is a significant difference. An additional 409 tons charge weight cannot be accounted by the total high explosive munitions documented to have been in railroad cars on the pier and on board the *E. A. Bryan*. An augment of 409 tons of explosive energy to the Port Chicago explosion can, however, be accounted by the “few hundred tons of TNT equivalent” that Atomic Bomb Military Policy Committee member James Conant predicted on 4 July 1944 could be the energy produced by the anticipated proof of the Mark II atomic bomb.]

The assistant to the Senior Loading Officer had started an inspection of the pier at 9:30 P.M. and left four or five minutes before the explosion at 10:19 P.M. According to him, the Quinault Victory had not started loading but the E. A. Bryan was loading as follows: Incendiary bombs in No. 1 hold; depth bombs in No. 2 hold; tail vanes in No. 3 hold; fragmentation cluster-bombs in No. 4 hold; and 40 mm. [ammunition] in No. 5 hold. He did not notice anything unusual as to the loading methods and no unusual problems were reported to him. The night was dark, clear, and cool; the wind was force 1 to 2 from the southwest, and the tide had been flooding for one hour.

Immediate Vicinity of the Pier.

Within about a mile of the pier were situated the administrative, storage, service, and barracks buildings of the Naval Magazine. Except for the usual personnel on duty the greater part of the remaining personnel had retired for the night. The moving picture show in the new recreation building had been attended by about 700 personnel but had let out about 32 minutes before the explosion. Three employees of a construction company for the marginal pier were working overtime in the company office which was built on shore immediately south of the two piers. At the approach end of the main pier was the joiner shop, and working therein were five civilians and a Marine private. In the revetment area, filled with about 140 loaded cars of ammunition and bombs, an engine crew was shifting cars for the directed needs of the pier transportation officer. Marine guards, some with trained dogs, were patrolling various posts, one of which included the approach end of the loading pier outward to the main bend.



Diagram, Port Chicago Naval Magazine, ships and piers

In the channel, an empty oil tanker had approached a point – 1,200 feet – approximately midway between the loading pier and a lighthouse situated directly across the channel on Roe Island. A Coast Guard patrol boat had just passed the loading pier a few minutes before the explosion. A tug with a barge in tow was about 2,800 feet to the northwest of the pier.

At Port Chicago station, a mile to the south of the pier, a passenger train of the Southern Pacific Railroad had just arrived at the station at the time of the explosion. A greater part of the people of the town of Port Chicago, one to two miles south of the pier, were either in bed or were preparing to retire. In the theater with a seating capacity of 386 people, there were about 195 people watching the movies. A touring circus had a one-day engagement at Port Chicago and although the show was finished the “big top” had not been taken down and packed away.

In the air at 9,000 feet a C-49 cargo plane was flying from Oakland to Sacramento and the pilot and co-pilot estimated they were about one and one-half minutes away from the scene of the explosion or about four or five miles away at their rate of travel – 150 miles per hour. Another plane was flying at about 7,000 feet and was three miles away proceeding north.

The Explosions.

The interpretation of the recordings of seismographs in the general vicinity of San Francisco, California, although not conclusive, indicated that two explosions took place between 2218:47 and 2218:54½ Pacific War Time, 17 July 1944, and that the second was greater than the first. Witnesses described the first explosion as sharp and loud as contrasted with the second which was deeper and poorly defined. Furthermore, the first explosion appeared confined as it ascended in a column of boiling and billowing mass of burning gases with a mushrooming top. The outside was darker than the inside and showed flashes of orange, red, and variations of the same. The first flash was brilliant white changing later to yellow and reddish-orange as the column rapidly gained altitude. The second explosion was not confined and spread in all directions from the pier area as a center. The second explosion apparently culminated in the mass detonation of all remaining explosives, especially those of the E. A. Bryan.

The pilot of the plane, cruising at 9,000 feet and four to five miles away, described the explosion as a terrific white flash with a large smoke ring that spread out in a horizontal plane; within the terrific flash he recognized pieces of white hot metal as it mounted at least 500 feet higher than the elevation of the plane; only one was seen which lasted for ten to fifteen seconds. The pilot of the plane cruising at 7,000 feet and three miles away saw an original flash with its billowy flame and pyrotechnics display. The column appeared to have reached an altitude at least 1,000 feet above the plane. (He believed the billowy mass reached its maximum height in about 15 seconds.) A few seconds after the original flash, the pilot said the plane received a terrific concussion as if it had been hit by something; however, on landing he could find only scratches under the wing surface and on the side of the fuselage.

The officer-in-charge of Roe Island Lighthouse, which is situated directly across the channel from the loading pier – 3,280 feet – described the first explosion as “shaking the lighthouse violently, smashing in all the windows and tossing furniture around.” There was no evidence of fuel oil having been sprayed on Roe Island as a result of the water wave.

On the other hand, the Patrol Boat U.S. Y.P. Miahelo II, in the channel about 500 yards from the pier [Note. Should be 1,400 yards—4,200 feet], was heavily sprayed with fuel oil when the water wave broke over the boat. The combined effect of water, blast, and missile damage resulted in a “constructional loss” of the patrol boat.

An oil tanker, the M.S. Redline, was damaged severely by the blast wave, water wave, and missiles. The salvage value was estimated to be about twelve per cent of the actual replacement cost. Fisherman’s channel lights Nos. 1 and 2 were destroyed and Suisun Bay lighted buoy No. 4 was struck by a missile and sank.

The buildings of the Naval Magazine were damaged extensively; sporadic damage to structural members of buildings was proven up to 13 miles – Suval [railroad] Station, California; plate glass was broken up to 35.5 miles – Petaluma, California; and a legitimate claim for plaster damage was reported at 48 miles – Calistoga, California.

Damages as a Result of the Explosions.

As a result of the explosions the following deaths were recorded:

Number Recorded & Identified	Number of Deaths	
17	202	enlisted personnel of loading crews of E.A. Bryan and Quinault Victory.
5	9	officers associated with loading division
1	67	officers and crew of both vessels.
0	30	officer – 1 – and enlisted men – 29 of armed guard of both vessels.
2	5	enlisted men of Coast Guard fireboat
1	1	Marine Sentry on approach end of pier.
1	3	Civil Service employees of train crew on pier
3	3	employees of construction company for new pier.
30	320	totals

A total of 81 bodies were recovered but only 30 bodies were identified.

Except for the Marine sentry walking his post on the approach end of the pier and the three employees of the construction company, all others killed must have been at the outer part of the pier in or about the E. A. Bryan, the Quinault Victory, and the fireboat. It was testified that the Marine sentry probably died from multiple wounds as a result of being struck by missiles. The three employees of the construction company, who were working in temporary offices on shore almost directly south of the E. A. Bryan, likewise were killed probably by missiles. Although the joiner shop at the land edge of the pier entirely collapsed, the five persons therein at the time of the explosion escaped without major injuries.

Personnel and Civilian Injuries.

Twenty-five of the 30 men of the Navy Armed Guard crew of the *E.A. Bryan* killed in the explosion

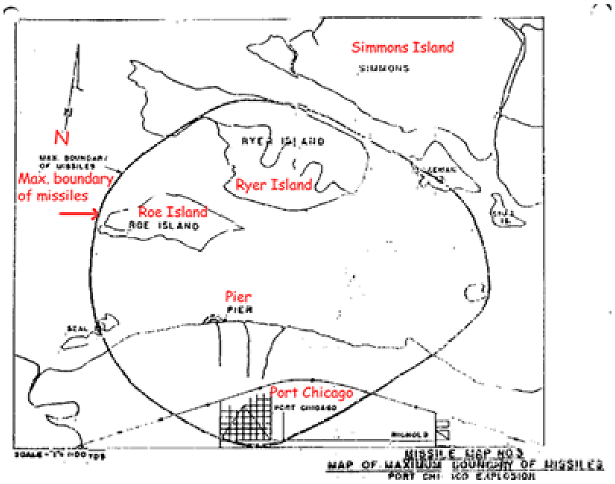
Most of the injuries to civilians and naval personnel of the Naval Magazine were superficial, resulting from shattered glass from windows and doors. The total injured listed from all causes was 390 of which 237 were Navy; 6 Marine Corps; 4 Coast Guard; 5 Maritime Service; 25 Civil Service; and 113 civilians. . . . It is important to note that 54 per cent of all injuries were in the vicinity of the eyes.

Property Damage.

The damages to property were estimated to be 12.5 millions of dollars. [Note. Government property damage, including destruction of the Government-owned *Quinault Victory* and *E. A. Bryan,* were estimated to be $9,892,034, which was 79 per cent of the total estimated damages to property.]

Loading Pier and Ships.

Except for 200 feet of the approach end, the loading pier was destroyed along with a diesel locomotive, 16 carloads of ammunition and bombs, and a utility building 100 x 26 x 14 feet which were on it. Many missiles, recognized as parts of the railroad cars, were found on Roe Island to the north and a few were found in the vicinity of the revetments to the south. The joiner building, situated on land close to the approach to the pier and at a distance of 1,000 feet from the center of the pier was demolished but three men working therein were rescued without serious injury. These men were the closest to the explosion that survived. [Note: elsewhere, five men and one Marine are reported to have been in the joiner shop.] A Marine sentry walking his post on the approach end of pier was killed, probably by missiles.



Map of maximum boundary of missiles.

The Coast Guard fireboat at the end of the pier was destroyed and a diver was able to recognize the twisted remains by entangled fire hose. A body of one of the enlisted personnel was found in the wreckage.

The ships, E. A. Bryan and Quinault Victory, were destroyed and the former furnished the greater part of the steel missiles which showered the area. Large pieces, later identified as parts of the hull of the Quinault Victory, were found imbedded in the muddy bottom of the bay north of the pier. One piece of keel, 60 to 70 feet long with its propeller attached, was lying upside down and could be seen at low tide. The keel was creased and buckled in one place and was sheared at the end opposite the propeller. The hull showed a large hole about 15 to 20 feet from the propeller. Offshore, from the visible portion with propeller attached, was found a portion of the mast with bow headed downstream. The bow was cut off apparently at the bulkhead.

The position of those two large parts identified as originating from the Quinault Victory indicated that the stern had moved through an angle of 180 degrees whereas the bow had moved through and angle of less than 25 degrees.

Barracks and Administration Area.

In the barracks and administration area, situated immediately south of the revetment area, the long axis of most of the buildings was parallel to the direction of the blast wave. The most seriously damaged buildings were an old recreation building – about 94 per cent; the laundry building – 81 per cent; the officers' lounge – 75 per cent; and the new recreation building – 73 per cent.

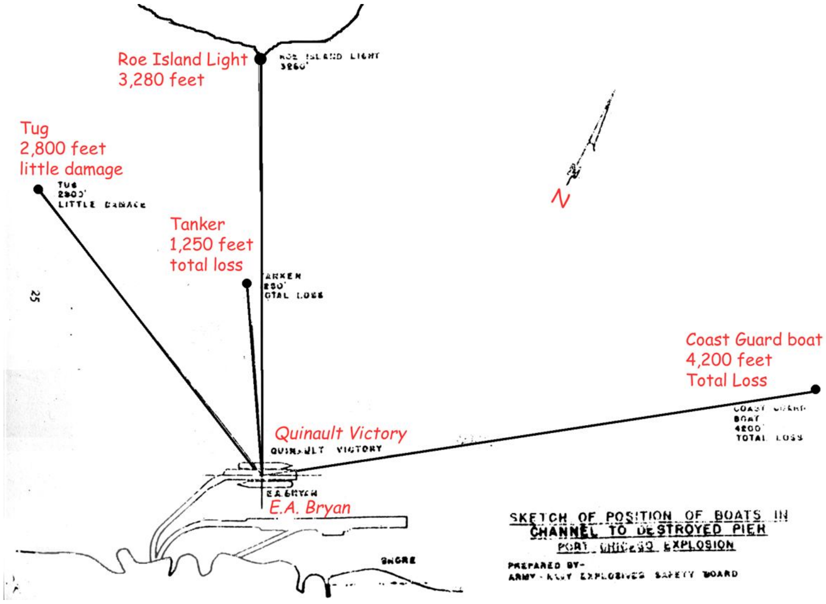
Nine barracks buildings, each two-story 42' x 150', with frame construction on concrete piers, wood floors, rustic siding and composition roofs, had the long axis about parallel to the direction of the blast wave. These buildings had damage of about 20 per cent of the cost of construction. The north wall of several barracks buildings were demolished by the pressure wave; the south walls were slightly damaged; wall panels were loosened; partitions were damaged; window glass was destroyed; sash and frames were damaged.

Waters Adjacent to the Naval Magazine.

The following ships were in the channel, within a mile of the loading pier, at the time of the explosion:

Name	Type	Distance from pier
M.S. Redline	Empty oil tanker of about 388 tons.	1,250 ft.
Governor MBM	Tugboat with barge loaded with bulk gasoline.	2,800 ft.
Y.P. Miahelo II	Coast Guard Patrol boat	4,200 ft.

The position of the boats to the pier at the time of the explosion is indicated by the sketch map on Page 25. The M.S. Redline sustained damage both from the blast wave and from missiles with some additional damage from the water wave. The superstructure was almost gone, all the tank tops were broken open, and the steel plates were bent, some even twisted and cut. The engine room was penetrated by a 14-inch projectile and another penetrated not only the top of the deck house but also two bunks therein. The engine was knocked out so that it became necessary to tow the tanker to a wharf. After appraisal of the cost of repairs it was estimated that the M.S. Redline was 88 per cent damaged. Of the crew of eight, the chief engineer received a cut on the head and an injured shoulder; the pilot had a cut on the neck and a broken ankle; a seaman had a bruised head and glass in one eye; another seaman had a cut on one hand and a bruise on the other. Four members of the crew were uninjured.



Sketch of position of boats in channel to destroyed pier.

The Governor MBM, a tugboat towing a barge loaded with bulk gasoline, passed the loading pier shortly before the explosion. The tugboat carried a crew of three, one on the barge and two on the tugboat. The two men on the tugboat were knocked out temporarily but did not have any other injuries. The boat was shaken badly by the water wave as well as the blast wave. The boat began to leak shortly after the explosion but by frequent pumping it was possible to keep her in service for several months after the explosion.

The patrol boat Y.P. Miahelo II, at the time of the explosion, was headed away but in the direct line with the pier. It carried a crew of four but only one was awake and on duty. As a result of the explosion the ears of the crewman on watch at the wheel were affected for about 15 minutes; another crewman had one ear temporarily affected; and two crewmen suffered no ear pressure. The explosion knocked out the engines, took the overhead off the wheelhouse, sprayed oil over the boat, smashed out all glass and caused other damage to the boat. After an extensive survey of the damage the boat was considered to be a "constructive total loss." The crewman on watch at the wheel was cut on the face and back by flying glass but the other crewmen received cuts only on the feet as a result of walking on broken glass.

It is interesting to note that the M.S. Redline oil tanker was on the starboard beam, the Y.P. Miahelo II was on the starboard quarter [Note. Quarter: The general direction on either side of a ship located 45 degrees off the stern], and the Governor MBM was on the starboard bow of the exploded ship, the E. A. Bryan.

In contrast to witnesses on land, who were unable to differentiate portions of the two ships and the pier within the exploding area, the witnesses on the above-mentioned boats testified to such happenings as:

(1) Saw head end of a ship foremost mast forward go up in the air.

(2) Saw pieces of dock in air with pilings attached.

(3) Saw funnel-shaped area 200 feet in air, on top of which was bow of ship with mast attached.

(4) In first flash saw shoreline and inside ship [*E. A. Bryan*]; in second explosion the inside ship and pier seemed to go together.

(5) Could see outline of dock and ship, looked like center of fireworks mostly on the ship that hadn't gone up yet. [E.A. Bryan].

Roe Island Lighthouse.

On Roe Island directly across the channel and 3,280 feet from the pier, a lighthouse was occupied by the keeper, his wife, two children, and an assistant keeper. The keeper and his wife were in the kitchen in the rear of the house at the time of the explosion. The first explosion knocked out the navigation light, broke all windows in the house, blew furniture about and shook the house violently. Although the two children were showered with glass neither one had a single scratch. The assistant keeper and the parents of the children likewise were not injured.

The keeper of the light testified as to his recollections while in the kitchen during the first explosion and during the second explosion while looking out the upstairs window, especially as to the 20 to 30 foot water wave coming toward the lighthouse from the direction of the pier. Although this unusual water wave for Suisun Bay put the lighthouse boat 40 feet back on the beach and tore down bulkheads, there was no evidence of oil sprayed on Roe Island. On the other hand, the Y. P. Miahelo II patrol boat was sprayed heavily with fuel oil, apparently by either the same or similar water wave that was observed by the lighthouse keeper.

A large number of missiles were found on Roe Island including several lengths of railroad car rails and many pieces which were recognized as parts of railroad cars. The significance of these particular missiles in relation to the possible origin of the explosion will be considered later in the section on missiles. Some parts of the bodies of the loading details and ships’ crews were recovered on Roe Island. Some parts were washed on the island probably by the so-called “tidal wave” and other parts probably were blown directly onto the island by the force of the explosion.

Southern Cities and Towns.

Port Chicago.

The town of Port Chicago, lying adjacent to the barracks and administration area of the Naval Magazine and 1 to 2 miles from the loading pier, not only received extensive damage from the blast wave but also was hit by scattered missiles from the exploding ship. Although the main street of Port Chicago was roughly parallel to the direction of the blast wave, many occupants of commercial buildings suffered extensive damage to merchandise by the breakage of large plate glass windows and cases. Window glass breakage, plaster damage and other superficial damage, and personal property damage to household furniture was extensive in the residential parts of the town.

Many automobiles in Port Chicago were damaged by the blast wave and some by missiles. Steel tops were mashed in, windows broken, doors sprung, fronts and fenders dented. For example, an automobile parked beside the Southern Pacific Depot had a 3 inch diameter hole torn in its steel body by the penetration of a metal slug; the ribs in top were broken; doors were sprung and all glass was broken. Another car, parked in front of the depot, had its top and sides blown in, all glass broken and upholstery out. A woman occupant of this car was not injured. A car, parked across the street from the theater, was struck by a missile, and two doors, a running board, and a door post were crushed.

A two-ring circus was in town the night of the explosion and the damage to their trailer equipment was surveyed by experienced men from a nearby government installation. It was verified that nineteen tires were destroyed on twelve of the eighteen trailers. The claims for damages to automobiles parked on the streets of Port Chicago did not indicate that any tires were destroyed either by missiles or by the blast wave.

In the town of Port Chicago only one building, an unoccupied shop which lacked proper maintenance, completely collapsed. A large storehouse in fair condition partially collapsed and the remainder had to be torn down. The sidewall of a theater partially collapsed but the patrons vacated the building before the roof fell. Several poorly constructed frame buildings, used for commercial purposes, had partially collapsed side walls and roofs. As a whole, the structural damage to all types of buildings was mainly to the roof and associated members and was minor as compared with the dollar amount of superficial and glass damage.

The injuries associated with structural damage were few. The magnitude of personal property damage by broken glass, flying glass, and broken doors indicates, however, the possibility of a greater number and more serious injuries if the explosion had occurred when people were active and walking in rooms rather than late at night when most people had retired. The greater protection of a horizontal position and lack of panicky actions no doubt resulted in the low injury rate. There is evidence that many of the windows and doors had blown in before the occupants were aroused sufficiently to move about and be subjected to the hazards of the second explosion.

Glass Breakage.

Window Glass.

Every claim for glass breakage was recorded separately, by cities and towns, as to the sizes and number of panes of glass broken, the type, such as single strength (ss), double strength (ds), plate, and wire, and the dollar amount for replacement. Later a tabulation was prepared showing, by cities and towns, the number of claims. These data were transferred to a percentage relationship of the total houses damaged in a city or town. The glass damage criterion for cities and towns indicates a straight line relationship with the southern cities showing a more consistent trend than those of either the western or eastern cities. This exhibit indicates the rapid decrease in damage with greater distance from the pier. Of particular interest is the apparent limit of breakage of similar types of glass between 22 and 24 miles. This apparent limit of breakage of small sizes will be considered in the section on plate glass, especially as to its significance in relation to mathematical calculations by the National Defense Research Committee of probable window glass breakage of a particular size at twenty-five miles from the Port Chicago explosion.

Plate Glass.

Plate glass breakage was extensive with much damage to merchandise displayed in store windows. The amount of breakage, in general, follows a straight line relationship with greater distance from the pier.

The National Defense Research Committee, in a chapter on the rupture of glass (“Study of the physical vulnerability of military targets to various types of aerial bombardment.” NDRC Report No. A-385, pp. 297-298. Confidential) chose Port Chicago as an example for the use of certain equations derived therein for the determination of the radius of glass breakage by sizes. On the basis of the weight of explosives detonated of 4,272,000 pounds and glass breakage up to 25 miles given in an earlier abstract of the explosion and on the assumption of "face-on" conditions, it was stated, "12 x 18 x 0.12 inches glass should not be broken, but larger panes such as 24 x 24 x 0.12 inches would be expected to be broken under favorable conditions."

A review of the basic sheets on glass breakage shows that the greatest distance of breakage of window panes comparable in size to 12 x 18 x 0.12 inches was at Oakland, California, a distance of about 22 miles from the pier. The theoretical calculation by N.D.R.C., therefore, is substantiated by complete data on actual window breakage as a result of the Port Chicago explosion.

[Note. Reference in the two paragraphs above is made to NDRC Report No. A-385, a report that employed comprehensive data on window glass breakage caused by the Port Chicago explosion to formulate equations to predict the radius of glass breakage, by sizes, which would result from various types of aerial bombardment. The TNT charge weight cited from Report A-385 for the Port Chicago explosion, 4,272,000 pounds or 2,136 tons, is said to have been "given in an earlier abstract of the explosion." Report A-385 presumably provides a specific citation for that "earlier abstract of the explosion," but Report No. A-385 cannot be located; consequently neither the title of that "earlier abstract of the explosion" nor the Government agency that produced it is known.]

VIII Appendix. C. Suisun Bay Crater.

In Mud Bottom. (Quoted from, Records of Proceedings of a Court of Inquiry, The U.S. Naval Magazine, Port Chicago, California, July 21, 1944.)

"Soundings of the bottom of Suisun Bay in the vicinity of the U.S. Naval Magazine Ship Pier were made between February 26 and March 11, 1944, and again between July 25 and July 29, 1944. A comparison of the maps prepared in these two survey yielded information about the crater formed in the Port Chicago explosion of July 17, 1944. The contour maps and profile diagrams indicate that a crater about eight feet in maximum depth was scoured out by the explosion of the S.S. E.A. Bryan. As might be expected, the crater by no means exhibits circular symmetry in a horizontal plane, but is roughly in the shape of an oval or ellipsoid, whose major axis is parallel to the direction of the exploding vessel. The bottom of the crater is located directly under the center of the ship. At a depth of 33 feet (approximately the mean depth near the pier prior to the explosion) the crater diameter along the major axis of the ellipsoid was roughly 600 feet, and along the minor axis, nearly 300 feet."

In Hard Bottom. (Extracted from: "Soil Investigation Naval Magazine, Port Chicago – Soundings to determine extent of crater created in Suisun Bay by the recent explosion." Contract report by L. Cedric Macabee to Public Works Officer, Navy Yard, Mare Island, 28 March 1945.)

The Bureau of Yards and Docks of the Navy Department authorized a survey of the area of the exploded ship by means of probings, boring, and other devices so as to ascertain the extent of the original crater at Port Chicago. An area of about 700 square feet was investigated by probing through the loose mud overlying the crater area to the undisturbed surface of the hard bottom of the original crater.

A rectangular grid of the area was laid out with lines 100 feet apart except within the critical area where the lines were 50 feet apart. A special probing tool, one-inch round, was pushed down at 10 to 25 foot intervals along the lines to determine the depth of the hard bottom below the mud line. The survey started in November 1944 and was completed in March 1945. The lateral force of the explosion was evident to the contractor, as he reported, by the probable total removal of the soft mud from the vicinity of the blast and its partial return with the water in the form of a tidal wave. There were, therefore, two tidal waves that resulted from the explosion: The first moved outward from the explosion center onto low-lying shoreline areas and across Suisun Bay to Roe Island; the second, carrying a large volume of soft mud, rushed back into the region where the explosion had occurred to restore the volume of water that had been expelled outward by the explosion in the first tidal wave.

Miscellaneous findings of the report included the following:

(1) Steel obstructions were encountered at 81 feet (elevation minus 81) below mean low low water (MLLW).
(2) Many local small deep holes appeared in the hard bottom in the crater area.
(3) Mud balls up to four feet in diameter were found on the mud bottom by salvage divers.
(4) Pilings of the shiploading pier were broken off at the mud line.
(5) Heavy parts of a ship, later identified as parts of the Quinault Victory, were found northward in the Suisun Bay shipping channel, approximately 2,000 feet from the origin of the explosion.
(6) Two distinct craters were identified on the south side of the shiploading pier and a lesser crater on the north side of pier.

The extent of the original crater was expressed by the contractor's report as follows:

"It is our opinion the craters as shown on contour map of depth of probing, cross-sections of the area, the evidence of individual smaller holes and metal considerably below the general outline of the craters blasted out in the 'hard bottom' of the bay give the location of the crater of the blast and the tremendous downward force . . . The extent of the mud crater caused by the explosion was possibly 800 feet in diameter measured from the center of the blast."

Photographs and illustrations credits.

Diagram, Port Chicago Naval Magazine, ships and piers. Source: “The Port Chicago, California, Ship Explosion of 17 July 1944.” Army-Navy Explosives Safety Board Technical Paper No. 6. Washington, D.C., 1948; page 13.

Twenty-five of the 30 men of the Navy Armed Guard crew of the E. A. Bryan killed in the explosion. Source: Courtesy of Thomas R. Bowerman, http://www.armed-guard.com/02peo.html.

Map of maximum boundary of missiles. Source: “The Port Chicago, California, Ship Explosion of 17 July 1944.” Army-Navy Explosives Safety Board Technical Paper No. 6. Washington, D.C., 1948. VIII. Appendix B. Missile Analysis; page 7.

Sketch of position of boats in channel to destroyed pier. Source: “The Port Chicago, California, Ship Explosion of 17 July 1944.” Army-Navy Explosives Safety Board Technical Paper No. 6. Washington, D.C., 1948; page 25.

Chapter 10

TNT and torpex charge weight, probable causes and origin of the Port Chicago explosion.

Analysis of the craters formed by the Port Chicago explosions in the bay bottom in the vicinity of the ship loading pier elucidates the origin and progression of the explosions. A survey, by soundings, of the bay bottom in the vicinity of the pier had been made five months before the explosion. Soundings were again made between 25 July and 29 July 1944. A more precise and ingenious method of survey of the bay bottom in the vicinity of the pier was subsequently authorized by the Bureau of Yards and Docks of the Navy Department and the findings were reported 28 March 1945 by the contractor, L. Cedric Macabee, which produced “Contour Map No.1. Map of Crater on Hard Bottom.” Contour Map No. 1 is poorly reproduced in the available copies of the Army-Navy Explosives Safety Board Port Chicago explosion report. I have added identifying text and outlined the shape of the ships *Quinault Victory* and *E. A. Bryan* and the pier. I have added text to identify isolated bay bottom craters and labeled those Craters Nos. 1, 2, 3, 4, and 5. Unfortunately most of the detailed information provided by copies of the original Contour Map No. 1 is unreadable.

Army-Navy Explosives Safety Board Port Chicago explosion report. Contour Map No.1. Map of Crater on Hard Bottom

All the evidence presented in the Army-Navy Explosives Safety Board Port Chicago report and the Proceedings of the Port Chicago Court of Inquiry is conclusive that the first explosion, accompanied by a brilliant flash of light, occurred either within one of the forward two cargo holds (Nos. 1 and 2) of the *E. A. Bryan*, or on the pier in the vicinity of the Nos. 1 and 2 holds of the *E. A. Bryan.* As will be shown, the first explosion did occur on the pier rather than within in the No. 1 or No. 2 cargo hold of the *E. A. Bryan.* Because the ships *E. A. Bryan* and *Quinault Victory* were, respectively, moored inboard and outboard of the Naval Magazine pier headed west and east, the first explosion may also be said to have occurred on the pier opposite the stern cargo holds Nos. 4 and 5 of the *Quinault Victory*.

Crater No. 1 was formed directly beneath the pier at the position adjacent to the No. 2 hold of the *E. A. Bryan*, and is the crater formed by the first explosion. The widening of Crater No. 1 at the starboard (right) side of the *E. A. Bryan* shows that the force of the first explosion that formed Crater No. 1 impacted and was partly reflected by the steel hull and bulk of the ship *E. A. Bryan.* The shock and blast force of the first explosion broke the *E. A. Bryan* abaft the No. 2 hold at the position where the force of the first explosion impacted the hull and bulk of that ship. The first explosion on the pier demolished the joiner shop at 1,000 feet, broke glass in the town of Port Chicago at a mile and a half to the south, in the lighthouse 3,200 feet to the north, and in the Coast Guard Patrol boat 4,200 feet to the east of the pier. The first explosion also broke apart the stern from the *Quinault Victory* and launched the stern section of the ship's keel, with the propeller attached, into a high arc to where it fell into Suisun Bay 2,000 feet from the explosion.

The force of the first explosion that broke the *E. A. Bryan* abaft the No. 2 hold displaced the intact cargo holds Nos. 1 and 2 of the *Bryan* 90 feet to the southwest (to the port or left side of the center line of the ship) where the munitions in the No. 2 hold exploded forming Crater No. 2.

The first explosion was very powerful and a review of the varieties and weight of munitions that were on the pier in railroads cars spotted opposite the Nos. 1 and 2 holds of the *E. A. Bryan* and opposite the Nos. 4 and 5 holds of the *Quinault Victory* will define the energy of the first explosion.

Two documentary records are available that identify the 16 railroad cars that were on the pier at the time of the explosion, and those records provide an inventory of the types of munitions contained by those cars and the cargo weight of those munitions. The two documentary records, however, do not consistently report the position of each railroad car on the pier at the time of the explosion. The two documents were assembled immediately after the Port Chicago explosion. One was prepared by personnel of the Naval Ammunition Depot Mare Island (NADMI) and was used by the Port Chicago Navy Court of Inquiry as the basis of the Court's fact-finding; the second was prepared by Los Alamos scientific staff and was used as the basis of Los Alamos analyses of the explosion.

The investigator's problem is to decide which of the two documentary records presents the true position of the railroad cars upon the pier at the time of the explosion. Because this investigation of the Port Chicago explosion is principally directed to elucidate the role of the Manhattan Project Los Alamos Laboratories in the Port Chicago explosion the records available in the Archives of Los Alamos National Laboratory that describe the position of the railroad cars on the Port Chicago Magazine pier at the time of the explosion will be taken as valid—except one important error in that Los Alamos record that will be described.

NAD No. 83044-1, "Explosive Material on Pier & on board S.S. Bryan prior to detonation on 17 July 1944 at U.S. Naval Magazine, Port Chicago, California"

PDF

By 30 August 1944 NADMI had prepared the schematic diagram NAD No. 83044-1, "Explosive material on pier & on board S.S. 'Bryan' prior to detonation on 17 July 1944 at U.S. Naval Magazine, Port Chicago, California," with the note, "Exact arrangement of cars on center track is unknown, but presumably were spotted for thru-loading." "Thru-loading" means that munitions that arrived on the pier in railroad cars that were spotted on the pier's center track would be manually transferred by the ship loading crews from those cars on the center track through the open side doors of emptied railroad cars spotted on the outside track. Thru-loaded munitions would be emplaced on the pier opposite the cargo holds of the ship to be loaded. From that position on the pier alongside either ship the munitions would be stacked on pallets or loaded into cargo nets and hoisted aboard by deck-mounted winches, associated booms and cables and lowered into the cargo holds where additional ship loading personnel would stow the munitions according to a loading plan and block the munitions in place with wooden dunnage.

Diagram NAD No. 83044-1 represents that all 16 cars on the pier at the time of the explosion were spotted between the two ships. Six of the 16 railroad cars on the pier are represented by this document to have been located in the vicinity of the Nos. 1 and 2 holds of the *E. A. Bryan,* and this diagram represents that six cars of the 16 cars on the pier were spotted on the center track.

Among the Port Chicago explosion records in the Archives of Los Alamos National Laboratory is a schematic diagram, untitled, which represents that ten cars, rather than 16, were spotted on the pier between the two ships at the time of the explosion. Six of the 16 cars identified by the diagram made at Los Alamos are shown to have been positioned on the approach wing of the pier. The diagram made by Los Alamos represents that no cars were spotted on the center track. I have titled that Los Alamos diagram, "Los Alamos diagram of explosive material on pier prior to detonation on 17 July 1944."

Los Alamos diagram of explosive material on pier prior to detonation on 17 July 1944

This Los Alamos diagram shows the location of 16 cars on the pier and on the approach wing of the pier, and identifies the munitions contents of each car. The manuscript notations that identify the position and contents of each car are easily legible in the original document but the outlines of the pier and ships are not, so I have clarified those. It should be noted that this Los Alamos diagram is drawn as seen from the north. The pier and ships are depicted from a perspective on Suisun Bay rather than seen from the Port Chicago shore. Seen from the perspective from Suisun Bay, the *Quinault Victory* is outboard of the pier, headed east, in the foreground.

This Los Alamos diagram represents that four railroad cars were spotted in the vicinity of the Nos. 1 and 2 holds of the *E. A. Bryan.* Spotted at the No. 1 hold of the *E. A. Bryan* is one carload of M-7 incendiary bombs. Spotted at the No. 2 hold of the *E. A. Bryan* this Los Alamos document shows one carload Mk-47 bombs—350 pound, DB AN-Mark 47 aerial depth bombs (DB) filled with torpex.

On the outboard side of the pier, spotted at the No. 4 hold of the *Quinault Victory,* this Los Alamos document shows one carload of M-33 bombs—1,000 pound, AP AN-M33 armor-piercing (AP) aerial bombs filled with TNT. This Los Alamos document also represents that one carload of M-65 bombs was spotted at the No. 5 hold of the *Quinault Victory*. The M-65 was a 1,000 pound, GP AN-M65 general purpose (GP) bomb filled with TNT.

The NAD No. 83044-1 and Los Alamos diagrams differ in their representation of the position of the two carloads of M-65 bombs that were on the pier at the time of the explosion. The document prepared by Los Alamos represents that one carload of M-65 bombs was spotted at the No. 5 hold of the *Quinault Victory* and one at the No. 3 hold of the *Quinault Victory*. But NAD No. 83044-1 represents that one carload of M-65 bombs was spotted at the No. 3 hold of the *Quinault Victory* and one at the amidships position of the *Quinault Victory*.

The NAD diagram report of the positions of those two carloads of M-65 bombs amidships and at the No. 3 hold of the *Quinault Victory* is correct. The Los Alamos diagram which represents that one carload of M-65 bombs was spotted at the No. 5 hold of the *Quinault Victory* is incorrect, as determined thus:

If one carload of M-65 bombs had been spotted at the No. 5 hold of the *Quinault Victory* the explosion of that car would have formed a discernable crater in the bay bottom beneath the position of that car, but Crater Contour Map No. 1 does not disclose a crater at that location. A distinct ellipsoidal crater, however, is revealed on Crater Contour Map No. 1 beneath the pier at the amidships position of the *Quinault Victory* and that crater extends eastward to the position beneath the pier at the ship’s No. 3 cargo hold. The location of that ellipsoid crater corresponds to the position reported by NAD diagram No. 83044-1 to have been the location of two end-to-end cars loaded with M-65 bombs.

Furthermore, the bow (No. 1) and stern (No. 5) cargo holds of Liberty and Victory munitions ships were not loaded with heavy per-cubic-foot weight high explosive bombs. M-65 bombs would not have been designated as cargo to be loaded into the No. 5 hold of the *Quinault*

Victory. Typically the Nos. 1 and 5 cargo holds of those munitions ships were loaded with lighter per-cubic-foot cargo—gun projectiles, cartridges, and also M-7 incendiary bombs which did not have a heavy fragmentable steel or iron case. A ship heavily laden at the bow and stern does not maneuver in turns as easily as a ship relatively lighter laden at the bow and stern. A center of mass amidships also greatly increases the steadiness of a ship's floating equilibrium.

The NAD and Los Alamos documents that diagram the position of the cars on the pier agree that two carloads of M-7 incendiary bombs were on the pier at the time of the explosion. Both documents identify the M-7 bomb to have been an incendiary "cluster" bomb. In the World War II military literature available to me I have been unable to find any reference to the M-7 (Mark 7, Mk-7, M7, or M-7) incendiary cluster bomb. I have found reference to two U.S. World War II incendiary cluster bombs used in the Pacific Theater of War, the 500-pound M-17 which was a cluster of 110 4-pound M-50 magnesium incendiary bombs, and the 220-pound M-19 which was a cluster of 36 6-pound jellied oil M-69 bombs. The otherwise unidentified M-7 is, however, mentioned by the Port Chicago Navy Court of Inquiry as a consideration in the Court's endeavor to establish probable causes of the explosion:

> "52. That the initial explosion occurred in the vicinity of the inboard end of the pier near the bow of the E. A. BRYAN, probably among components being handled on the pier or being loaded into No. 1 or 2 holds. The sharp distinct sound and the brilliant white flash lead to the belief that the initial detonation was that of an M-7 cluster or Mark 47 depth bomb. . . ."

The NAD and Los Alamos documents agree that one of the two cars of M-7 incendiary cluster bombs on the pier was spotted on the outside track at the No. 1 hold of the *E. A. Bryan.* But the two diagrams differ in their representation of the position of the second carload of M-7 bombs. The diagram prepared by Los Alamos represents that the second of those two cars was positioned on the approach wing of the pier, but NAD diagram No. 83044-1 represents that the second car of

M-7 bombs was spotted on the center track opposite the *Bryan*'s No. 1 hold.

NAD diagram No. 83044-1 can be shown to have erroneously reported the position of the second carload of M-7 incendiary bombs. The second carload of M-7 bombs was not located on the center track opposite the *Bryan*'s No. 1 hold but was, as represented by the diagram prepared by Los Alamos, located on the approach wing of the pier. That error of NAD diagram No. 83044-1 is proven thus:

Two railroad cars of ammunition reported by NAD diagram No. 83044-1 to have been spotted on the pier between the two ships at the time of the explosion were later found by salvage divers intact on the mud bottom, below the destroyed portion of the western approach wing of the pier. Both cars were full and the dunnage had not been removed as the first preparation to unload the cars' munitions contents. One of the two cars found intact on the mud bottom contained M-7 incendiary bombs; the second contained Mk-47 aerial depth bombs.

In March 1947 an officer of the staff of the Army-Navy Explosives Safety Board interviewed the senior member of the salvage company who was in charge of the actual salvage operations at the pier and according to his description of the contents of the cars, "the officer reported that one car must have contained incendiary clusters and the other air depth bombs. The cars were found just beyond the trestle of the undestroyed western approach portion of the pier. One was lying upright and the other in a slightly tilted position as if they had rolled off the tracks." [Reference: "The Port Chicago Ship Explosion of 17 July 1944," Army-Navy Explosives Safety Board, VIII Appendix. D. Origin and Number of Explosions; footnote, page 4.] The two cars "were blown into the bay without exploding and subsequently were raised and buried on Ryer Island." [Reference: "The Port Chicago Ship Explosion of 17 July 1944," Army-Navy Explosives Safety Board, Section III. Structural Damage; footnote, page 11.]

As one measure of the inaccuracies of NAD diagram No. 83044-1 those two cars later recovered intact and fully laden from the mud bottom are erroneously represented to have been spotted between the two ships opposite the Nos. 1 and 2 holds of the *E. A. Bryan:* "MK-7

CLSTR” bombs at the No. 1 hold and “350# DEPTH TORPEX” at the No. 2 hold. In consequence of that error the munitions contents of those two railroad cars were incorrectly assumed by the Navy Court of Inquiry to have contributed to the explosion and incorrectly assumed by the Court to have been probable origins of the explosion.

Having now noticed that one error of several in NAD diagram No. 83044-1, which errors led the Court of Inquiry to several mistaken findings, we turn to discussion of the railroad cars and their munitions contents that were on the pier at the time of the explosion.

We are able to refer to another Los Alamos document to establish important information about each of the railroad cars that was on the pier. That document is, “The following cars were on the pier during the explosion.” From this document we may learn which of the two ships was to receive the contents of each railroad car on the pier at the time of the explosion, the railroad company that owned each car on the pier, the railroad company’s identifying number for each car, the munitions contents of each car, the cargo weight of the munitions in each car, and the point of origin of each car. Manuscript notes made upon this document, as received from Los Alamos Archives, are legible and define if the contents of each car would detonate high order, low order or would make no contribution to the energy of the Port Chicago explosion.

Los Alamos National Laboratory: The following cars were on the pier during the explosion

We have shown that one carload of M-7 incendiary bombs was spotted on the outside track at the No. 1 hold of the *E .A. Bryan*. From Los Alamos document, “The following cars were on the pier during the explosion,” we learn that car was either DRGW (Denver & Rio Grand Western) car No. 68697 or C&O (Cincinnati & Ohio Railroad) car No. 10645. Both were designated to be loaded aboard the *E. A. Bryan* (PC# 80); both contained 30 tons of M-7 incendiary cluster bombs; both originated at the Hawthorne, Nevada, Navy Ammunition Depot, now the Hawthorne Army Depot.

However, in the aggregate load of munitions on the pier and loaded as cargo aboard the *E. A. Bryan* the M-7 incendiary cluster bombs would have held an insufficient charge of TNT to have contributed significantly to the TNT charge weight of the Port Chicago explosion, and that would also have been true for any type of incendiary bomb. A very small TNT charge is sufficient to effectively disperse the bomb's incendiary material.

The document, "Los Alamos diagram of explosive material on pier prior to detonation on 17 July 1944," and NAD diagram No. 83044-1 agree that one carload of Mk-47 bombs was spotted on the outside track at the No. 2 hold of the *E. A. Bryan*. The Mk-47 bomb was a 350 pound, torpex-filled aerial depth bomb. This carload of Mk-47 bombs was either NJI&I (New Jersey, Indiana & Illinois Railroad) car No. 4149 or ATSF (Atchison, Topeka & Santa Fe Railroad) car No. 143756. Both cars were designated to be loaded aboard the *E. A. Bryan* (PC# 80); both contained 54 tons of Mk-47 bombs; both originated at the Hawthorne Navy Ammunition Depot. One of those two cars containing Mk-47 was found intact and fully laden on the mud bottom just beyond the trestle of the undestroyed portion of the pier; that carload of Mk-47 bombs was not consumed in the explosion.

Immediately opposite the No. 4 hold of the *Quinault Victory* was one car which contained M-33 bombs—1,000 pound, TNT-filled AP AN-M33 armor-piercing (AP) aerial bombs. This was either ATSF car No. 147190 or SAL (Seaboard Air Line Railroad) car No. 19442. Both cars were designated to be loaded aboard the *Quinault Victory* (PC# 79); both contained 53 tons of M-33 bombs; both originated at the Indian Island, Washington, Naval Magazine, now the Naval Magazine Indian Island.

The location of the railroad cars on the Port Chicago ship loading pier, as reported by NAD diagram No. 83044-1, suggests that the carload of M-33 bombs spotted on the outside track at the No. 4 hold of the *Quinault Victory* was to have been loaded into the No. 4 hold. Following that transfer the emptied car would have been moved off the pier to the west and the next car eastward on the pier, spotted at the amidships position, would have been moved westward to be opposite the No. 4 hold and that carload of M-65 bombs loaded into the No. 4 hold. The M-65 bombs held by the next car eastward on the pier's outside track, spotted at the No. 3 hold, would have been loaded into the No. 3 hold.

In summary, according to Los Alamos records corrected to show that no railroad car was spotted at the No. 5 hold of the *Quinault Victory*, three bomb-laden railroad cars were on the pier between the Nos. 1 and 2 holds of the *E. A. Bryan* and the Nos. 4 and 5 holds of the *Quinault Victory*. Manuscript notes on the Los Alamos document, “The following cars were on the pier during the explosion,” provide the TNT and torpex charge weight of the munitions loaded in each of those three cars.

Car spotted at:

E. A. Bryan, No. 1 hold.

M-7 incendiary cluster bombs.

Cargo weight: 30 tons;

TNT charge weight: effectively none.

E. A. Bryan, No. 2 hold.

Mk-47 aerial depth bombs.

Cargo weight: 54 tons;

Torpex charge weight: 39 tons (73 % of the cargo weight).

Quinault Victory, No. 5 hold

No munitions at this position.

Quinault Victory, No. 4 hold.

M-33 AP aerial bombs.

Cargo weight: 53 tons;

TNT charge weight: 8 tons (15% of the cargo weight).

The total amount of explosives available to the first explosion on the pier in the vicinity of the inboard end of the pier between the bow of the *E. A. Bryan* and stern of the *Quinault Victory* is thus determined to have been 137 tons cargo weight containing 47 tons of TNT and torpex.

The detonation of 47 tons of TNT and torpex on the pier between the bow of the *E. A. Bryan* and the stern of the *Quinault Victory* certainly generated a sufficiently energetic shock wave to break the *E. A. Bryan* abaft the No. 2 hold and to displace the broken forward portion of the ship—the bow and cargo holds Nos. 1 and 2—to the position 90 feet southwest of the pier where Crater No. 2 demonstrates that the munitions cargo of the No. 2 hold of the *E. A. Bryan* detonated. The force of the first explosion was also certainly sufficient to break the stern apart from the unloaded, high-riding *Quinault Victory* and to have been the impetus that impelled the stern section of the ship's keel, with the propeller attached, in a high arc through the air to the position 2,000 feet to the north of the pier.

The first Port Chicago explosion indisputably occurred on the pier between the Nos. 1 and 2 cargo holds of the *E. A. Bryan* and the Nos. 4 and 5 cargo holds of the *Quinault Victory* where three munitions-laden railroad cars were positioned. The second, massive explosion that followed the first explosion on the pier by several seconds included the essentially simultaneous detonation of the cargo of bombs that had been loaded into Nos. 2, 3 and 4 cargo holds of the *E. A. Bryan*, as well as the unexploded munitions remaining on the pier. The cargo within the ship's Nos. 1 and 5 holds (projectiles, cartridges and M-7 incendiary bombs) did not contribute significant energy to the explosion because that cargo burned or detonated low order.

Los Alamos
document
"S.S. E.A. Bryan"

Los Alamos document "S.S. E. A. Bryan" inventories the munitions cargo loaded into the *E. A. Bryan* prior to the explosion. The total TNT and torpex charge weight of the ship's cargo was initially calculated by Los Alamos to have been 1,552 tons. That total of 1,552 tons TNT was reported in Capt. Parsons' memorandum to Adm. Purnell dated 24 July 1944, "Port Chicago Disaster: Preliminary Data."

That 1,552 tons total represented an erroneously calculated TNT charge weight for the M-64 bombs loaded into the ship's No. 2 hold—erroneously calculated to have been 142 tons. Los Alamos personnel subsequently correctly recalculated the TNT charge weight of the M-64 bombs in hold No. 2 to have been 167 tons, rather than 142 tons. Captain Parsons reported a recalculated total of 1,577 tons to Adm. Purnell in his memorandum dated 4 August 1944, "Port Chicago Disaster: Second Preliminary Report." The total TNT and torpex charge weight of the ship's cargo, initially reported as 1,552 tons, was increased by 25 tons of TNT to 1,577 tons.

Port Chicago Naval Magazine, California, PC #80 –
S.S. A.E. [sic] Bryan. Approximate load at 2330 – 17 July 1944.

Corresponding information prepared by Naval Magazine Port Chicago to document the munitions that had been loaded into the cargo holds of the *E. A. Bryan* is reproduced by the document, "Port Chicago Naval Magazine, California, PC #80 – S.S. A. E. [sic] Bryan. Approximate load at 2330 – 17 July 1944."

I have made a compilation of the information provided by the three documents presented here to summarize the cargo weight of the munitions loaded into the holds of the *Bryan*, the TNT or torpex charge weight of those munitions, and to show if those munitions burned, exploded low order or high order. Those documents are: "S.S. E. A. Bryan," "Port Chicago Naval Magazine, California, PC #80 – S.S. A. E. [sic] Bryan. Approximate load at 2330 – 17 July 1944," and the compilation I have given the title, "Approximate munitions load aboard the *E. A. Bryan* at 2330, 17 July 1944."

"Approximate munitions load aboard the *E.A. Bryan* at 2330, 17 July 1944."

To ascertain the origin of the first explosion the problem for the investigator is to determine which of the three railroad cars on the pier in the vicinity of the Nos. 1 and 2 holds of the *E. A. Bryan* was the first to explode and thereby initiated the second, massive explosion.

I exclude the possibility of an accidental detonation in or about the car spotted opposite the No. 4 hold of the *Quinault Victory,* which car contained M-33 bombs. Although the hatch of the No. 4 hold of the *Quinault Victory* had been opened before to the explosion, the transfer of cargo from that car or any car on the pier into the *Quinault Victory* had not commenced prior to the explosion. The contents of the car opposite the No. 4 hold of the *Quinault Victory* were most probably undisturbed. The findings of the Navy Port Chicago explosion Court of Inquiry state: "Loading [of the *Quinault Victory*] should have started by midnight. Dunnage and loaded cars were spotted on the pier for this purpose." The 1948 Army-Navy Explosives Safety Board report on the explosion states, "Port Chicago Naval Magazine personnel were rigging the ship [*Quinault Victory*] for loading and all hatches except the No. 5 were about ready to load at the time of the explosion."

I minimize as a possibility that the accidental detonation of one or several M-7 incendiary cluster bombs opposite the No. 1 hold of the *E. A. Bryan* would have produced a sufficient shock to sympathetically detonate nearby high explosive munitions. The accidental detonation of one or several M-7 incendiary bombs in or about the car laden with M-7 incendiary bombs would have dispersed incendiary material and ignited extensive areas of the wooden pier that, aflame, eventually would have caused nearby high explosive munitions to burn or explode, but that process would have required minutes rather than seconds.

An explosion in or about the car of Mk-47 bombs spotted at the No. 2 hold of the *E. A. Bryan* is the only presumptively effectual origin of the first explosion on the pier in the vicinity of the Nos. 1 and 2 holds. That first explosion on the pier initiated the second and larger explosion that, as defined by the Court of Inquiry, "consisted of the detonation – substantially simultaneously – of the ammunition in ten holds of the E. A. BRYAN. That this was initiated by the detonation of a component or group of components, or hot fragments from the first explosion which entered the holds either through the ship's side or through the open hatches."

Liberty and Victory ships were constructed with five large openings in the deck, the hatches leading to the ships' five cargo holds. Each of the five cargo holds was divided into upper and lower holds; therefore the Court of Inquiry mentions ten holds. The heaviest cargo—bombs, in the case of a munitions ship—was loaded into the lower holds to establish the ship's center of gravity well below the waterline to mitigate the possibility of capsize in rough seas and during fast, full-rudder turns. Lighter-weight more bulky cargo was loaded into the upper holds. At the time of the explosion only the five lower holds of the *E. A. Bryan* had been loaded.

Accidental detonation of a torpex-filled Mk-47 bomb on the pier is frequently cited by commentators on the Port Chicago explosion to have been the cause of the explosion. The 1944 Navy Court of Inquiry proposed as the first in the order of probable causes the "presence of a supersensitive element which was detonated in the course of handling." In definition of a supersensitive element the court specified:

> "a. One wherein a thin film of high explosives is present because of defects in the manufacture of the case or faulty filling of that particular component. (This condition could have occurred in the Mark 47 and the Mark 54 depth bombs.)
>
> "b. One which has become prematurely armed by reason of damage to the safety features either in transit to the magazine or in the handling after arrival. (This condition could have occurred in the M-7 incendiary bomb clusters.)"

The court's reference to a defective Mark 54 depth bomb as a probable cause of the explosion is not plausible; 315 tons of torpex-filled Mark 54 depth bombs had been loaded into the No. 4 hold of the *E. A. Bryan* the day preceding the explosion but could not have been a cause of the first explosion on the pier. Mark 54 bombs were not anywhere on the pier at the time of the explosion.

It has been noted that the Navy Court of Investigation that inquired into the cause of the 10 November 1944 explosion of the USS *Mount Hood* in Seeadler Harbor, Manus Island, reported "Torpex filled depth bombs were apparently coming on board."

RDX and Torpex.

Following World War I, TNT replaced wet gun cotton as the explosive utilized as the main charge filler for underwater bombs and torpedoes. In 1920 the chemical compound cyclonite, actually cyclotrimethylene trinitramine, was identified in Germany. It is more powerful than TNT and the British renamed it RDX for Research Department Explosive. It is the primary ingredient in plastic explosives.

RDX provided the basis for a new class of explosives particularly suited to underwater military uses. RDX is a white crystalline solid, has a high degree of stability in storage, and is considered the most powerful and brisant of the military high explosives. It has a very plastic, dough-like consistency and RDX explosive charges can be shaped for special detonation effects. RDX forms the base of the current military explosives Composition A, Composition B, Composition C, HBX, and H-6, and is sometimes referred to as hexogen (Russian). Apart from its explosive hazard, breathing RDX dust can cause epilepsy and amnesia.

On at least one occasion Osama bin Laden's associates in Al Qaeda terrorist network were reported to have used RDX, and the U.S. Federal Bureau of Investigation (FBI) determined that RDX was the explosive used in the attack on the *Arleigh Burke* class guided missile destroyer USS *Cole* (DDG-67),12 October 2000 in the Yemen port of Aden. Seventeen sailors were confirmed or presumed dead in that attack. RDX is the explosive most frequently utilized by terrorists worldwide.

RDX is at least 50% more effective than TNT as an underwater explosive against ships. During World War II, RDX was difficult to make safely and therefore, compared to TNT, considerably more expensive to produce in large quantities. During the war U.S. explosives researchers compounded a mixture of TNT (37-41%), RDX (41-45%) and 18% aluminum that was known as torpex. The addition of aluminum to the mixture of RDX and TNT was found to accomplish a prolongation of the pressure wave. The process of converting torpedo warheads and depth charge loadings from TNT to torpex began with an order for 20 million pounds of torpex in early 1942. The first torpex filled antisubmarine torpedo warheads followed late the same year. Torpex-filled aerial depth- bombs, for example the Mk-47, appear to have been introduced in late 1943.

During World War II, after about January 1944, as the manufacture of torpex in the U.S. provided that material in quantities sufficient for application to its optimal military purposes (aerial depth bombs, depth charges and torpedoes), the use of torpex by the Navy increased. Torpex provided a higher explosive energy and higher detonation velocity (24,600 feet per second) than RDX (22,700 to 23,700 feet per second) or TNT (21,800 to 22,400 feet per second).

In 1945 torpex was replaced by HBX, in the 1960s by H-6, and in the 1970s by PBX. Although commonly used today without the admixture of TNT, sometimes RDX and TNT are mixed in what is called Cyclotol or C-6 (Composition 6), but RDX alone is more commonly used as C-4 (Composition 4). Prior to the 1945 introduction of HBX, which included a stabilizing wax component, experimental testing with torpex indicated that torpex had a greater sensitivity to heat and shock than TNT, but in no instance is the accidental detonation of a World War II torpex-filled torpedo, aerial bomb or depth charge documented.

The aluminum component of torpex-filled ordnance did produce an intense flash of white light in explosion. One carload of Mk-47 bombs exploded on the pier in the first explosion (39 tons of torpex) and certainly produced a brief flash of intense white light in the immediate area of the pier. The second, massive explosion included the detonation of 54 cargo tons of Mk-47 bombs in the No. 2 hold (39 tons of torpex) and 315 cargo tons of Mk-54 bombs in the No. 4 hold (225 tons of torpex). The torpex contribution to the second explosion was 264 tons.

Newspaper accounts of the explosion reported that at the city of Napa, a distance of 30 miles across flat terrain and a few low hills, the landscape was illuminated as if by the noonday sun. Whether the detonation of 264 tons of torpex could have produced a flash of white light of sufficient lux to so brilliantly illuminate the landscape at that distance can not be ascertained from the available literature. Certainly the brilliant flash of white light produced by the detonation of 264 tons of torpex and the simultaneous explosion of “blinding” white light produced by the detonation of the Mark II weapon were sufficient to produce full daylight illumination at Napa 30 miles away.

The accidental detonation of a torpex-filled Mk-47 bomb was suspect as a possible cause of the Port Chicago explosion and torpex-filled munitions are mentioned as cargo being handled at the time of the explosion of the USS *Mount Hood*. But since there is no known documented instance of an accidental torpex munitions explosion during World War II munitions handling operations, improperly filled or otherwise, the probability of an accidental detonation of a Mk-47 torpex aerial depth bomb on the pier at Port Chicago must be considered in that context.

News media documentary accounts done in recent years of the Port Chicago mutiny have touched peripherally on the cause of the explosion and settled on the accidental shock-induced detonation of a Mk-47 bomb to have been cause. In 1944 the Navy Court of Inquiry listed the accidental detonation of a Mk-47 bomb first in the rank of probable causes; that expert opinion satisfied the purposes of those news media inquiries, which chiefly portrayed the circumstances of the mutiny. In fact, the likelihood of an accidental shock-induced detonation of a Mk-47 bomb at Port Chicago was negligible.

A World War II torpex aerial depth bomb was not shock sensitive and none is documented to have accidentally exploded in handling operations. Released from an airplane the Mk-47 depth bomb stuck the water surface with a very considerable force of impact. If torpex were remarkably sensitive to shock-induced detonation those bombs would have had been essentially wasted ordnance because they would frequently have detonated on impact with the water surface rather than sinking intact to a subsurface depth at which the detonation of those bombs might disable the target of those bombs, an enemy submarine.

Crater Contour Map No. 1 shows that the entire carload of Mk-47 bombs spotted at the No. 2 hold of the *E. A. Bryan* detonated instantaneously in the first explosion. The Court of Inquiry, however, assumed that an accidental detonation of a single explosive element on the pier had been necessary to cause the detonation of that carload of Mk-47 bombs, but no conclusive evidence of that presumed first detonation of a single explosive element was offered in testimony. Despite the absence of testimony that could identify the precipitating detonation of a single explosion element the court found:

“That the initial explosion occurred in the vicinity of the inboard end of the pier near the bow of the E. A. BRYAN, probably among components being handled on the pier or being loaded into No. 1 or 2 holds.”

Section 51 of the court’s “Finding of Facts, Opinion and Recommendations” provides the court’s ranked order of probable causes of the explosion:

> “a. Presence of a supersensitive element which was detonated in the course of handling.
>
> “b. Rough handling by an individual or individuals. This may have occurred at any stage of the loading process from the breaking out of the cars to final stowage in the holds.
>
> “c. Failure of handling gear, such as the falling of a boom, failure of a block or hook, parting of a whip, etc.
>
> “d. Collision of the switch engine [operating on the pier] with an explosive loaded car, possibly in the process of unloading.
>
> e. An accident incident to the carrying away of the mooring lines of the QUINAULT VICTORY or the bollards to which the QUINAULT VICTORY was moored, resulting in damage to an explosive component.
>
> “f. The result of an act of sabotage. Although there is no evidence to support sabotage as a probable cause, it cannot be ignored as a possibility.”

For the text of the Court’s “Finding of Facts, Opinion and Recommendations,” see:

http://www.history.navy.mil/faqs/faq80-4n.htm

The court ranked sabotage last in the order of probable causes of the explosion. Sabotage is an act which damages property or obstructs productivity or normal functioning, such as committed by enemy agents against a nation in war. Deliberate detonation of the carload of Mk-47 bombs spotted at the No. 2 cargo hold of the *E. A. Bryan* with the purpose to effect the detonation the Mark II fission bomb and to conceal the detonation of that bomb within the larger explosion of the *E. A. Bryan*'s massive cargo of TNT and torpex munitions was not sabotage. But that is the means I impute as the origin of the Port Chicago explosion. The Mark II weapon was concealed among the cargo of crated aerial bomb tail vanes loaded 16 July 1944 into the No. 3 hold of the *E. A. Bryan* and was set with aerial depth bomb or depth charge hydrostatic pressure-activated fuses to detonate the Mark II at a pressure of 3-4 atmospheres in excess of sea level ambient atmospheric pressure; that necessary pressure above the ambient was propagated by the detonation of the carload of Mk-47 bombs.

The proof detonation of the Mark II at Port Chicago was conducted pursuant to determination by the nation's top civilian and military authorities that the resultant deaths and injury of civilians and military personnel, the destruction of private and Government property and war materiel, and the temporary obstruction of normal functioning at the Port Chicago magazine would be justified by the unwelcome proof that large scale nuclear fission weapons were in fact feasible. In July 1944 those weapons, if proven feasible, were expected to provide a sure means to successfully end World War II and, in the Pacific Theater, to avoid the more than 100,000 U.S. military casualties anticipated if U.S. forces would be required to force the Japanese surrender by invasion of the Japanese home islands. The death of 320 men at Port Chicago was a small fraction of the 100,000 U.S. casualties that would certainly have resulted if an invasion of Japan by U.S. forces had been necessary to defeat the Empire. But of more continuing significance, the 1944 U.S. military and scientific forecast of postwar reality recognized that if nuclear fission weapons were in fact proven every future enemy of the United States would eventually acquire a capability to attack the United States or the nation's interests anywhere in the world with nuclear weapons.

The reader should well ask why the proof of nuclear fission weapons was conducted in circumstances that would result in the death and injury of U.S. civilians and military personnel, the destruction of private and Government property and military materiel, and the temporary disruption of normal operation of the Port Chicago Naval Magazine. Several reasons provided the logical imperative for that decision. Military secrecy was one of those reasons.

The proof detonation of the Mark II weapon was effectively concealed from notice by the artifacts of the massive explosion of conventional munitions that were in place at Port Chicago. It was important in July 1944 that Germany, Japan and Russia should not know that the U.S. had proven the feasibility of nuclear fission weapons and consequently could be expected to have a near-term nuclear weapon combat capability. If the Mark II had been proof fired anywhere in an isolated area which did not provide an apparent and plausible cause for that explosion, firm speculation or actual discovery that a fission weapon had been detonated would quickly have passed through the existing foreign espionage networks. The typical fireball and column of flame produced by an isolated nuclear fission weapon explosion would have been clear evidence of a nuclear explosion to scientists, U.S. and foreign, working on the development of fission weapons.

Even though the proof of the Mark II uranium hydride bomb had been concealed by the Port Chicago ship explosion and that proof was known only to a few U.S. military officers, top civilian officials and Los Alamos scientists, by 16 March 1945 Professor Igor Kurchatov in Russia had received sufficient information about development of the U.S. uranium hydride bomb through the espionage network that he considered it possible “the [uranium hydride] atomic bomb has already been executed and that uranium 235 has been separated in major quantities.” How much the Russians had learned about the Mark II uranium hydride bomb and how the Russians obtained that information will be taken up more extensively in a later chapter that will recount development of the Mark II weapon. Igor Kurchatov directed the Soviet Union's nuclear weapons program from its inception in February 1943 until his death in 1960.

Not less important than the military need to conceal the proof of the Mark II was the military need to learn as much as possible, from that very expensive proof in terms of fissionable material, about the potential military effects of large scale fission weapon explosions, and especially the effects of a nuclear weapon detonated in an enemy port or harbor facility which, in 1944, was the only feasible combat application for the atomic bomb. Prior to 17 July 1944 no explosion of energy yield greater than 1,000 tons of TNT had been sufficiently well documented to provide absolute baseline data on the effects of large explosions. Every measured destructive and damaging effect of the Port Chicago explosion could be, and was, utilized by Los Alamos scientists to confirm the mathematically calculated effects of multi-kiloton explosions. The measured destruction and damage that resulted from the Port Chicago explosion included a comprehensive range of equivalent military and civilian target elements: harbor installations, a variety of ships at different ranges, airplanes in flight at different altitudes and distances, typical military structures and munitions storage facilities, civilian residential and business structures close-in and distant, automobiles, a passenger train, above and below ground utility installations. The earth shock and air wave at a variety of distances near and far were precisely taken by recording seismographs and barometers . . . the list of measured effects was very extensive and included, significantly, the effect of such a large explosion on the morale of surviving military personnel. Most of the surviving military personnel at the base rallied quickly.

Tragic and bitterly sorrowful as the Port Chicago proof of the Mark II was for those persons injured in the proof and for those who suffered the death of family in that proof that cost of human suffering was the nation’s first payment required to assure that the nation would be prepared for the age of nuclear weapons. Title II of Public Law 102-562, the “Port Chicago National Memorial Act of 1992,” established the Port Chicago Naval Magazine National Memorial to recognize those who served at the facility, to honor the memory of those who gave their lives and were injured in the explosion, and to commemorate “the critical role Port Chicago played in the Second World War and the historic importance of the explosion.”

It is now necessary to show that sufficient fissionable U^{235} had been produced by the Manhattan Project to permit the detonation of one Mark II weapon on 17 July 1944. The amounts of U^{235} produced by the Manhattan Project for each of the years 1943, 1944 and 1945 were, during the war, protected from disclosure by a Top Secret classification. The U^{235} production data for the years 1943 through 1949 today are still protected from disclosure by a Top Secret/Not Declassifiable designation. U^{235} production data for the years 1943 through 1949 have not yet been administratively released by the U.S. Department of Energy, but on 5 December 1980 I obtained the U^{235} production data for the years 1943 through 1949 from two offices of the U.S. Department of Energy. Those data show that, by the end of 1943, the Manhattan Project had produced sufficient U^{235} to permit the detonation of eight of the Mark II weapon each utilizing 9 kg U^{235}. During 1943, 74 kilograms U^{235} were produced by the Manhattan Project.

Photographs and illustrations credits.

“Crater Contour Map No.1. Map of Crater on Hard Bottom.” Source: “The Port Chicago, California, Ship Explosion of 17 July 1944,” VIII Appendix, C. Suisun Bay Crater. Army-Navy Explosives Safety Board: Washington D.C., 1948. Original detail enhanced by author, January 2002.

“Explosive material on pier and on board S.S. ‘Bryan’ prior to detonation on 17 July 1944 at U.S. Naval Magazine, Port Chicago, California.” Source: Prepared by Naval Ammunition Depot, Mare Island (document: NAD No. 83044-1) reproduced in, “Port Chicago Naval Magazine Explosion on 17 July 1944: Court of Inquiry Convened at the U.S. Naval Magazine, Port Chicago, California, 21 July 1944.” [U.S. National Archives, Pacific Sierra Region, Record Group 181, 12th Naval District Commandant's Office, General Correspondence Series, 1946.] Available online at:

http://www.history.navy.mil/faqs/faq80-4b3.jpg.

“Los Alamos diagram of explosive material on pier prior to detonation on 17 July 1944.” Source: Los Alamos National Laboratory Archives, Collection A-84-019, Series 5, 319.1, “Port Chicago Loading Schedules, 7/17/44 - 7/18/44” (Folder 29-2) [Formerly Folder 37-7].

“The following cars were on the pier during the explosion.” Source: Los Alamos National Laboratory Archives, Collection A-84-019, Series 5, 319.1, “Port Chicago Loading Schedules, 7/17/44 - 7/18/44” (Folder 29-2) [Formerly Folder 37-7].

“S.S. E. A. Bryan.” Source: Los Alamos National Laboratory Archives, Collection A-84-019, Series 5, 319.1, “Port Chicago Loading Schedules, 7/17/44 - 7/18/44” (Folder 29-2) [Formerly Folder 37-7].

“PC #80 – S.S. A. E. [sic] Bryan.” Approximate load at 2330 – 17 July 1944. Source: Los Alamos National Laboratory Archives, Collection A-84-019, Series 5, 319.1, “Port Chicago Loading Schedules, 7/17/44 - 7/18/44” (Folder 29-2) [Formerly Folder 37-7].

“Approximate munitions load aboard the *E. A. Bryan* at 2330, 17 July 1944.” Source: Compiled by author from Los Alamos document, “S.S. E. A. Bryan,” and Port Chicago Naval Magazine document “PC #80 – S.S. A. E. [sic] Bryan. Approximate load at 2330 – 17 July 1944.”

Chapter 11

Manhattan Project U^{235} production data, 1943-1945

During the summer and autumn of 1980 those former Manhattan Project Los Alamos scientists with whom I was acquainted in the programs of the New Mexico Energy Research and Development Institute had told me that to plausibly argue an atomic bomb had been detonated at the Port Chicago Naval Magazine I would need to show that the Manhattan Project had produced a sufficient quantity of fissionable material by 17 July 1944 to enable a nuclear fission bomb to be detonated on that date. Correlatively, I would need to know the quantity of fissionable material required to produce an atomic bomb of sufficient explosive efficiency to yield an energy of explosion equivalent to the energy of the Port Chicago explosion.

By December 1980 I had determined conclusively from the published Manhattan Project literature that production of plutonium in weapon quantity had not been feasible before the first plutonium producing reactor at Hanford, Washington, had commenced reliable operation at the end of December 1944. Therefore, if a nuclear fission weapon had been detonated 17 July 1944 at Port Chicago that weapon would necessarily have employed U^{235} as the fissionable material. What I required then to sustain my argument that a fission weapon had been detonated at Port Chicago was the U^{235} production data for the years 1943 and 1944, and then to ascertain if the quantity of U^{235} produced by 17 July 1944 had been sufficient to enable a nuclear fission weapon detonation equivalent to the energy of the Port Chicago explosion.

The available published Manhattan Project historical and narrative literature asserted that the Project had produced only sufficient U^{235} "to fill the belly of Little Boy"—Little Boy, the gun assembly Mark I weapon employed in combat at Hiroshima, which utilized U^{235} as the fissionable material. The literature asserted that the Project with great uncertainty of the outcome had barely produced sufficient U^{235} by 6 August 1945 to enable combat use of the one Little Boy bomb detonated at Hiroshima, but that claim of the literature was anecdotal and was not supported by factual U^{235} production data for the period prior to 6 August 1945. The Project's U^{235} production data for the years prior to 1950 were classified; even today U^{235} production data for the period prior to 1950 have not yet been administratively released by DOE.

The quantities of U^{235} and plutonium produced by the Manhattan Project during World War II were better kept secrets than the technology and design of atomic bombs. Circulation of general and specific information about the technology and design of the atomic bombs in development at Los Alamos was controlled by the most effective security policies and practices Gen. Groves could devise, with the purpose to restrict that information to persons in the Project "who had a need to know" that particular detail of the Project. But with several hundred people working on details of the program at Los Alamos, and in university-affiliated research laboratories, some details of the work inevitably did leak and were transmitted through the espionage network to the Russians. As Russian spymaster Anatoli Sudoplatov is reported to have told authors Leona and Gerald Schecter in 1994, the security of wartime secrets at Los Alamos would have been considerably more effective if Gen. Groves had ordered that the shirt pockets of all men leaving that facility were searched. Los Alamos photographic technician Paul Masters had removed a carefully folded copy of the document "History of 10,000 ton gadget" from Los Alamos in his shirt pocket.

Some details of the Project's work inevitably would leak and indeed some details of the work were transmitted to the Russians as the Project advanced during the war. The significance of those leaked details to the postwar development of Russia's first atomic bomb continues to be debated, but from the military point of view the leak of fragmentary details descriptive of the technology and design of U.S. atomic bombs was not as significant as a leak of information that would disclose the number of atomic bombs that the U.S. could employ in combat at any time during the war, or in later years as any military action might require. If a combatant can know the limit of an enemy's supply of arrows, bullets or atomic bombs, and thus know when that supply is significantly reduced or expended, that knowledge can be used in a variety of ways to achieve tactical and strategic advantage.

Given effective delivery systems, the number of atomic bombs that can be employed in combat at any time is dependent on the quantity of fissionable materials that has been produced. Therefore, the quantity of fissionable materials produced during the war was the most closely guarded secret of the Manhattan Project's weapons program. Only those few persons with some direct function or responsibility for U.S. military planning had a need to know the quantities of fissionable materials that had been produced.

Those persons were the Joint Chiefs of Staff and the Atomic Bomb Military Policy Committee, which committee provided that essential information to the Joint Chiefs. A spring 1943 secret memorandum issued by Gen. Groves ordered that within the Manhattan Project only the General himself, J. Robert Oppenheimer and Navy Capt. William Parsons would be informed of the quantity of fissionable materials as those materials were produced. However, it seems reasonable that Gen. Groves' deputy in the Project, Brigadier General Thomas F. Farrell, was informed and that the two civilians principally responsible for the Project's production of U^{235}, Philip H. Abelson at the Naval Research Laboratory and Earnest O. Lawrence at the Manhattan Project Oak Ridge site, could have calculated the quantity of production (Abelson) or would have been directly cognizant (Lawrence).

Before I contemplated a plan to locate and obtain the Project's U^{235} production data for the period prior to 1950 I decided I should ascertain the quantity of U^{235} that had been utilized by the Hiroshima Mark I bomb in order to know if the production data, when I obtained them, would show sufficient material by 17 July 1944 to enable a test of the

Mark I “Little Boy” bomb. In 1980 the quantity of U^{235} expended by the Mark I bomb at Hiroshima had not been published, but I had obtained important information about the design of that weapon that helped to calculate that quantity.

The Hiroshima Mark I weapon was a gun assembly design. One subcritical projectile of U^{235} was fired from the breech of a modified Navy 5" Navy anti-aircraft gun barrel (tube). At the muzzle end of the gun tube a fissionable “blind target” component of the Mark I weapon was composed of three or four discrete concentric rings of U^{235} partially sheathed in a depleted uranium tamper. The target rings and their tamper were contained within a very heavy steel encasement robustly thread-mounted to the muzzle end of the gun tube. The accelerated projectile entered the target case, which stripped the projectile’s tamper; the projectile stripped of its tamper penetrated the concentric cavity of the target rings and was immediately stopped by the blind target case, which arrest assembled the projectile and target rings in a supercritical mass. By installation of one, two, three or perhaps four target rings the energy yield of the Mark I weapon could be varied. Neither the tampered projectile nor the tampered target assembly could exceed one tampered U^{235} critical mass for the particular geometries of those components.

The Classification Office at Los Alamos had told me the critical mass of U^{235} in a tampered sphere is 15.5 kg; a tampered U^{235} mass presenting non-spherical geometries, as were the geometries of the projectile and target rings, would permit a greater subcritical U^{235} accumulation than the spherical geometry. I knew that the fission of 1 kg U^{235} would yield about 22,000 tons TNT equivalent at 100 percent efficiency, but I did not know the efficiency achieved by the Mark I weapon.

The Mark I gun tube was manufactured at the Washington, DC, Navy Gun Factory and was modified from the standard 5" anti-aircraft gun tube in several ways. The tube was not rifled because the spin imparted to a projectile by the rifling of a gun barrel is advantageous to the stability of a projectile in exterior ballistic trajectory but was unnecessary to the Mark I projectile which did not enter an exterior ballistic trajectory and moved only within the interior of the gun tube, from the breech to the muzzle and target case.

The metal weight of the gun tube was significantly reduced because the Mark I gun would be fired only once and would not require the usual durability of an anti-aircraft gun barrel or gun tube subject to the strain and wear that result from multiple firing. The unidentified alloy of which the tube was forged was a lighter and stronger metal than the steel used in the conventional 5" anti-aircraft gun tube, but was sufficient with that lighter weight, with the enhancement of radial expansion (autofrettage) construction, to prevent rupture of the tube by the several tons per square inch gas pressure that resulted from the deflagration of the propellant charge.

I knew that the acceleration imparted to the Mark I U^{235} projectile within the gun tube had been accomplished by the same weight powder charge used to accelerate the standard 50-pound projectile for which the 5"/35 caliber Navy anti-aircraft gun was designed. I knew that the same powder charge weight imparted approximately the same rate of acceleration to the Mark I projectile as to the conventional 50-pound 5"/38 projectile. Therefore the weight of the Mark I U^{235} projectile would necessarily have been close to 50 pounds (22.68 kg). But I knew the Mark I U^{235} projectile had been partially sheathed in a depleted uranium tamper which represented some measure of the approximately 50-pound Mark I projectile.

Furthermore, I had learned that the Mark I U^{235} projectile was, for the 5" gun tube, a subcaliber projectile and was supported at rest in the tube and during acceleration by a sabot carrier. A subcaliber projectile of less weight than the gun’s standard projectile, if fired with the same powder charge as that employed to propel the gun’s standard projectile, achieves a significantly higher muzzle velocity (hypervelocity) than the muzzle velocity achieved by the gun’s standard and heavier projectile. A hypervelocity plutonium projectile was contemplated for the Mark I weapon as a means to utilize plutonium in that weapon, but irreducible impurities in the plutonium produced by the Hanford reactors determined that even a hypervelocity plutonium projectile would not permit a rate of projectile acceleration sufficiently rapid to avoid partial detonation (predetonation) before the projectile and target components were fully assembled.

Although I have seen several documents in the Archives of Los Alamos National Laboratory that briefly discuss the application of sabot-carried projectile technology to the Mark I weapon the only published note, of which I am aware, that discusses that subject is found in the 1993 DOE publication *Critical Assembly* (Cambridge University Press) on page 84 and in the associated footnote:

"[Charles L.] Critchfield had worked on sabots before coming to Los Alamos. Because Oppenheimer believed that the projectile critical masses would need sabots, he considered Critchfield vital to the gun effort. Trained as a mathematical physicist, but also adept at ordnance experimentation, Critchfield was an ideal choice to translate gun concepts into experimental models. Born in Shreve, Ohio, Critchfield grew up in Washington, D.C., and attended George Washington University, where he became a protégé of Gamow and Teller. In 1943, while working for the Geophysical Laboratory on a project to perfect sabots, Critchfield was approached by both Oppenheimer and Teller and persuaded to join the project."

The diameter of a subcaliber projectile is smaller than the interior diameter (bore or caliber) of the gun tube or gun barrel from which the subcaliber projectile will be fired. A capability to utilize subcaliber ammunition in available guns in earlier military history was often useful or critical; the basic method of utilizing subcaliber projectiles was first devised by the French as early as 1848. Originally the capability to use subcaliber projectiles was important on the battlefield if, for example, the standard ammunition for a battery of 5" caliber guns was expended but a supply of 3" ammunition was available.

The wonderful but now difficult to find 1948 book *Rockets, Guns and Targets* provides a good summary of the sabot projectile research done by several contractors to the U.S. National Defense Research Committee (NDRC) and Office of Scientific Research and Development (OSRD) during World War II. The book is one volume of the series "Science in World War II" written and edited by OSRD staff and published by Little, Brown and Company, Boston. This volume of the series was edited by John E. Burchard. In the left margin the reader will find a link to Burchard's 7-page, 29 July 1944 report, "Damage survey at Port Chicago, California." Dr. Burchard's Port Chicago report was transmitted to Rear Admiral Julius A. Furer, Coordinator of Research and Development, U.S. Navy, via Vannevar Bush, Chairman, National Defense Research Committee. What appears to be a blind carbon copy of Burchard's report is held by Los Alamos National Laboratory Archives.

John E. Burchard,
Damage Survey at Port
Chicago, California,
29 July 1944 – 7 pages

A 1-page manuscript note dated August 26, 1944 and signed "O." (Oppenheimer) provided by Los Alamos Archives in association with Burchard's Port Chicago report to Adm. Furer is also available as a link in the left hand margin. Oppenheimer's comment on Burchard's report states, "This seems a lot rougher than, but not inconsistent with, what our people reported & concluded." This note is the only certain evidence so far discovered that J. Robert Oppenheimer was personally involved in review and analysis of scientific reports descriptive of the Port Chicago explosion.

J. Robert Oppenheimer,
26 August 1944
comment on John E.
Burchard's Damage
Survey at Port Chicago

Twenty-two years of investigation into the Port Chicago explosion have produced tantalizing evidences of several as yet undiscovered Government reports and analyses that pertain to the explosion. One of those evidences is recognition that the copy of John Burchard's Port Chicago explosion report held by Los Alamos Archives was, at some later date, transmitted as "Enclosure (F)" of an undiscovered report. Demonstrably that undiscovered report to which Burchard's report was made enclosure "Enclosure (F)" originated at Los Alamos. The typescript notation "Enclosure (F)" which is added at the bottom of the first and last pages of the Los Alamos copy of Burchard's report was made on the same typewriter that produced Capt. Parsons' Port Chicago Disaster memoranda to Adm. Purnell. Similarly, the copy of Capt. Parsons' 31 August 1944 memorandum, "Port Chicago Disaster: Third Preliminary Report," as that copy was received from Los Alamos Archives, shows that memorandum was, after 31 August 1944, made "Enclosure (B)" of an undiscovered Port Chicago explosion report. Probably Capt. Parsons' 31 August 1944 Port Chicago memorandum to Adm. Purnell as “Enclosure (B)”, and John Burchard’s Port Chicago explosion report as “Enclosure (F)”, will be found to be parts of the same report, when that report is discovered.

I here transcribe John Burchard’s comment on page 319 of *Rockets, Guns and Targets* that reports the circumstances that prompted Burchard’s report on the Port Chicago explosion. References are made to the OSRD (OSRD divisions were identified by alphabetical designators) and the NDRC (NDRC divisions were identified by numerical designators). The NDRC was established during 1940 by Carnegie Institute President Vannevar Bush. In June 1941 Bush persuaded President Roosevelt to form the OSRD with Bush as director; Bush thereafter reported directly to President Roosevelt. Ongoing work conducted by the NDRC was folded into the OSRD in June 1941. Both organizations were established to mobilize civilian U.S. scientific personnel, their resources and competencies in support of the war effort.

“More often than not the apparent reluctance of the Services to seek the fullest co-operation [of NDRC and OSRD civilian scientists] was a matter of preoccupation or indifference rather than of veiled opposition. For example, when the great explosion occurred at Norfolk [Virginia] in September 1943, it did not occur to the Navy to request the admitted experts on damage working for [NDRC] Division 2 for assistance in evaluating the physical effects of the disaster. Yet such an evaluation was of great concern to the many [NDRC and OSRD] groups then interested in larger [atomic] bombs and far more powerful explosives [nuclear fission], who leaped for every piece of data however fragmentary which would or might bear on the question. When Burchard asked permission to send Bowman to make such an assessment, it was not only readily granted but Bowman was provided with every facility including observation aircraft, photographers, and guides. His report was distributed by the Navy. Yet, when the even greater explosion occurred some months later at Port Chicago (July 1944), again the matter had to be called to the attention of the Navy, which was again very co-operative, this time with Burchard, who made the survey en route home from the Pacific. The Navy was, of course, making extensive reports of both incidents but not from this [nuclear weapons] angle."

Because the scientific research that was contracted by the OSRD during the war to investigate sabot projectiles has not been generally reported, and because the Manhattan Project histories have, with one exception, failed to note that the Mark I weapon utilized a sabot projectile, and was the only World War II U.S. weapon to use a sabot projectile, we digress briefly from the principal topic of this chapter to reproduce text from *Rockets, Guns and Target* descriptive of the NDRC and OSRD sabot projectile research. The text is abstracted without the complete original continuity from Chapter XXXV, "The Quest for Hypervelocity," which reports the history of NDRC Division 1. Hypervelocity is the term which categorizes the velocity of projectiles that exceed the velocity of common military projectiles and has relevance to several applications including the greater armor-piercing capability of hypervelocity projectiles.

The principal contractor for NDRC sabot research was the University of New Mexico under the direction of Dr. E. J. Workman. Workman's progress and final reports have not been located nor can any Los Alamos archival records be discovered that substantially document the development of the Mark I sabot projectile. But because "By the end of December, 1942, Workman could report that various designs of sabot projectiles had already been developed and were adaptable to nearly all existing guns" the presumption is reasonable that Workman developed the projectile sabot carrier for the Mark I modified Navy 5" caliber gun tube in collaboration with Critchfield after Critchfield arrived at Los Alamos in 1943. It is of note that during World War II Germany did develop hypervelocity armor-piercing sabot-carried projectiles that disabled a great number of the Allied forces' combat tanks during the North African Campaign.

" 'Sabot' is the French for 'wooden shoe,' and in an ordnance context means the part used to fill the space between a small projectile and a larger gun bore; it is made detachable and is to be dropped as the projectile leaves the muzzle. Such devices were used as early as 1848 in order to adapt special projectiles for use in available guns; they were generally made of wood (hence sabot). In World War I the French used sabots to adapt 37-mm. ammunition for use in the 75-mm. gun. This gun had a low rifling pitch and the light projectiles were unstable in flight and none too accurate. American ordnance experts, mindful of this experience, and despite awareness of a reviving interest abroad, were not very interested in the sabot projectiles as practical ammunition. At the time when Division 1 took up the cudgels the sabot had a bad name in American military circles and the division and its contractors therefore faced an uphill fight against opposition which was not entirely made up of the technical difficulties inherent in the problem.

“The active interest of Division 1 in developing a sabot projectile was aroused by letters from [Vannevar] Bush to [Richard] Tolman on the 23rd of March, 1942. The Divisional staff was of course already familiar with the early history of the sabot but now it began to study its potentialities in earnest.

“In August, 1942, the University of New Mexico was awarded a contract for the design and development of subcaliber projectiles under the direction of Dr. E. J. Workman. Ultimately $230,000 [or, elsewhere, $208,000] was allocated for the work there. By the end of December, 1942, Workman could report that various designs of sabot projectiles had already been developed and were adaptable to nearly all existing guns as well as being suitable for mass production. The report stressed the advantages of the sabot projectile in its greater chance of scoring a hit on a moving target [because of a flatter trajectory] and its superior armor-penetration qualities.”

To calculate the quantity of U^{235} expended by the detonation of the Mark I weapon at Hiroshima I worked four months with a four-function electronic calculator to determine, as I published my finding in 1982, that “the active nuclear component of the weapon detonated at Hiroshima could have been as much as 60 kilograms of U-235. More probably, however, the total U-235 component of that weapon was nearer to 45 kilograms.” Seven years later, in summer 1989, I met with Stanford University Linear Accelerator Center (SLAC) physicist Pierre Noyes, a strange man then sympathetic with the styles of social improvement instituted by Chairman Mao Tse-tung in China and President Fidel Castro in Cuba.

In that meeting Professor Noyes told me that a Japanese physicist the previous year had undertaken to calculate the quantity of U^{235} employed in the Hiroshima bomb. The Japanese physicist's finding and mine were within 3 percent of the same range of weight, which difference arose because we started each with a slightly different degree of U^{235} enrichment in the material utilized by that weapon. In 1990 I found the very important Manhattan Project manuscript document written by Atomic Bomb Military Policy Committee alternate member Harvard University President James B. Conant, "Findings of Trip to L.A. [Los Alamos], July 4, 1944," which defines "50 '25' [U^{235}] kg" to be the active component of the Mark I weapon.

During the months that I had been occupied with those calculations, and with my duties in the Energy Research and Development Institute, I had made inquiries among my contacts in the Department of Energy, particularly in the Grand Junction, Colorado, offices, to learn which DOE offices, and who particularly in those offices, would have access to the U^{235} data for the years 1943-1949. As the Energy Information Coordinator for the State of New Mexico I said I wanted to put together a table of U^{235} production data that would demonstrate the state's historical contribution to that production. The several men I spoke with at DOE, Grand Junction, told me they had never seen that data and had frequently wondered what the numbers would be for those years.

By late November 1980 my DOE contacts had identified two men, one in each of two DOE offices, who my contacts had ascertained would have access to the U^{235} production data for the years 1943-1949: Don M. Cox in the DOE Enrichment Office Division at Oak Ridge, Tennessee, and Jim Staggs in the DOE Office of Uranium Resources Enrichment, Planning and Analysis Branch in Washington, DC. On 5 December 1980 I spoke by telephone with both those men and verbally obtained the U^{235} production data for the years 1943-1949. At that time both men believed those data had been declassified by the terms of a recent general declassification order that covered a wide range of Manhattan Project documents.

Peter Vogel to Don M. Cox, letter of 9 December 1980

On 9 December I transcribed the production data I had received verbally on 5 December from Cox and Staggs into a 2-page letter to Cox, which I transmitted on the letterhead of my office in the New Mexico Energy and Minerals Department, Energy Resource and Development Division. Because I considered that information and those data would be of significant historical importance as well as important historical significance I made a reporter's notes of all those inquiries that led to my 5 December request for those data and on 10 December I mailed those notes and a copy of my letter to Don Cox to David Weir, co-founder of the Oakland, California, Center for Investigative Reporting.

Don Cox, at Oak Ridge, provided the U^{235} production data for the years 1943 through 1949 in kilograms "equivalent top product" [ETP] which he explained is uranium enriched to 93.15 per cent U^{235}, and is uranium enriched in the U^{235} isotope to the degree requisite to the most efficient use in nuclear fission weapons. Kilograms ETP are approximately converted to kilograms U^{235} by multiplying units of ETP by 0.93. Therefore, the data provided by Cox show 74 kg U^{235} were separated during 1943, and 93 kg were separated during 1944.

Jim Staggs, in Washington, DC, provided the U^{235} production data for the years 1943-1949 in Separative Work Units (SWU), which he explained were approximately converted to kilograms U^{235} by dividing the number of SWU by the atomic weight of the U^{235} isotope, 235. Therefore, the 15,000 SWU accomplished during 1943 equates to 63.5 kg ETP which, multiplied by 0.93, gives 59 kg U^{235} separated during 1943. For 1944, 20,000 SWU equates to 85 kg ETP and 79 kg U^{235}.

The two sets of U^{235} Manhattan Project production data provided by Cox and Staggs document that either 74 kg or 59 kg were produced during 1943. Either quantity by the end of 1943 provided the requisite 50 kg U^{235} to permit the detonation of one Mark I weapon at Port Chicago the evening of 17 July 1944, which would have produced an explosive energy equivalent to 12,500 (12.5 kt) tons of TNT.

Although the requisite 50 kg U^{235} existed by 17 July 1944 to permit the detonation of a 12.5 kt energy yield Mark I weapon at Port Chicago the total energy yield of the Port Chicago explosion was equivalent only to 1,577 or 2,100 tons of TNT. The energy of the Port Chicago explosion was therefore grossly inconsistent with the detonation of a Mark I weapon, even if the weapon had been configured to the minimum energy option permitted by variation of the number of fissionable elements (rings) that could be installed into the Mark I's blind target component affixed to the muzzle end of the gun tube.

The gross inconsistency between the energy of the Port Chicago explosion and the minimum energy of the Mark I weapon was perplexing and seemed to defeat the thesis that an atomic bomb had been detonated at Port Chicago—until 1993 when I discovered the Manhattan Project's essentially unreported development of the Mark II weapon that required only 9 kg U^{235} to produce a nuclear fission explosion equivalent to 1,000 tons of TNT. On 4 July 1944 the Mark II with a nominal yield of 1 kt TNT equivalent was forecast to produce, from an optimal air burst, Class B damage within an area of 2-5 square miles and correspondingly less if the weapon were detonated in a surface burst, as was the circumstance of the Port Chicago explosion.

An optimal air burst occurs when a weapon is detonated at the correct height in air above a target to maximize structural damage beneath the exploded weapon by optimal distribution of the generated blast wave overpressure to the target area. At a height above optimal the radius of effective overpressure in the target area is reduced by blast wave energy dissipation in air. At a height below optimal the radius of effective overpressure in the target area is reduced by partial conversion of blast wave energy to earth shock and by above-horizontal angular reflection of the blast wave from the earth or water surface lying directly below the burst.

The detonation of a weapon essentially upon an earth surface produces the least efficient utilization of blast wave energy if the military objective is the destruction and damage of surrounding surface structures. This inefficiency arises, first, because a very considerable portion of the energy of an earth surface detonation is directly coupled to the earth and transmitted as earth shock, which may only slightly affect surrounding surface structures. The second reason a surface burst is ineffective in propagating an efficient blast wave that will affect local structures is that a large fraction of the energy generated by a surface burst is immediately reflected by the earth surface straight up from the point of the explosion.

In addition to that portion of the blast energy reflected straight up from the explosion, some of the blast energy of a surface burst is reflected at all angles intermediate between vertical and horizontal. Most of the blast energy that is reflected from the earth in a surface explosion is wasted to the purpose of causing destruction and damage to local structures—except tall buildings surrounding the point of a surface explosion will suffer the incident and earth-reflected blast wave from the bottom of the structure to the top. That effect of earth-reflected blast energy significantly contributed to the destruction and damage caused by the proximate surface detonation that occurred 19 April 1995 at the Murrah Building in Oklahoma City. For structures of lower height only that portion of the blast wave propagated essentially horizontally from a surface burst will be militarily effective.

If destruction of a large area is the military objective, as was the objective in the bombing of Hiroshima and Nagasaki, the burst must be made above the target at the correct height to optimally radiate an effective overpressure to the greatest radius given the energy yield of the weapon. Adjustment can be made to the height of the burst to induce an earth shock of sufficient magnitude to weaken the structural integrity of some particular class of structure, which weakened structures would then be collapsed by the pressure of the following blast wave.

On 4 July 1944 the optimal air burst of a 1 kt Mark II weapon was forecast to cause Class B damage within an area of 2-5 square miles depending on the surface terrain of the target area and the durability of target structures. The Port Chicago surface explosion of 1,577 to 2,100 tons TNT equivalent resulted in Class B damage to a radius of 2,500 feet, which calculates to an area of 0.7 square mile (Area = $3.14r^2$, where r [radius] equals 2,500 feet). Optimal military use of the atomic bombs in development by the Manhattan Project in 1944 would require airplane delivery and a fusing mechanism that would guarantee that a bomb released from a delivery aircraft would detonate at the optimal height above the intended target.

By the end of 1943 the Manhattan Project had produced either 74 or 59 kg U^{235}. The availability of 9 kg to permit the proof of a Mark II weapon at Port Chicago 17 July 1944 is thus established. But we can also interestingly examine the 1943, 1944 and 1945 production data to assess the validity of the anecdotal claim of the Manhattan Project historical literature that by 6 August 1945 only sufficient U^{235} was available to enable detonation of one Little Boy Mark I weapon, the weapon detonated at Hiroshima, which employed 50 kg.

During 1943 and 1944 a cumulative total of either 167 kg (Cox) or 138 kg (Staggs) U^{235} were separated. Those two totals, reduced by the 9 kg U^{235} expended in the proof of Mark II, allowed a remaining total of either 158 kg or 129 kg U^{235} at the end of 1944. Additional separation was accomplished during year 1945—either 289 kg (Cox) or 197 kg (Staggs) U^{235}.

However, the material form of the U^{235} produced by the Manhattan Project required conversion to metal and other fabrication processes before the material could be disposed in a weapon. I conclude that only the U^{235} produced during the first six months of 1945 should be added to that which was available at the end of 1944 in order to ascertain the quantity of U^{235} available for weapon use by 6 August 1945. The annual production data provided by Cox and Staggs are not broken down by month of production, so some estimate must be made of the fraction of 1945 production that was accomplished by the end of June 1945.

If one-half the 1945 production had been accomplished by the end of June, either 144.5 kg (Cox) or 98.5 kg (Staggs), were available from 1945 production for weapon use by 6 August 1945. Those amounts separated during the first six months of 1945, added to material remaining from 1943 and 1944 production, totaled either 302.5 kg (Cox) or 227.5 kg (Staggs)—sufficient material for either 6 or 4 of the Mark I bomb.

If one-third the 1945 production had been accomplished by the end of June, either 96 kg (Cox) or 98 kg (Staggs) were available from 1945 production by 6 August 1945. Those amounts separated during the first six months of 1945, added to the material remaining from 1943 and 1944 production, totaled either 254 kg (Cox) or 194 kg (Staggs)—sufficient material for either 5 or 3 of the Mark I bomb.

If one-fourth the 1945 production had been accomplished by the end of June, either 72 kg (Cox) or 49 kg (Staggs) were available from 1945 production by 6 August 1945. Those amounts separated during the first six months of 1945, added to the material remaining from 1943 and 1944 production, totaled either 230 kg (Cox) or 178 kg (Staggs)—sufficient material for either 4 or 3 of the Mark I bomb.

My sense of the matter is that one-fourth the total 1945 U^{235} production had been accomplished by the end of June, so that by 6 August 1945 the total available quantity of U^{235} was either 230 kg or 178 kg—equivalent to either 4 or 3 of the Mark I bomb. But I introduce one more variable into this assessment of the number of Mark I bombs that were available by 6 August 1945 for operational purposes.

There is archival documentary evidence and germane attestations in the historical literature which show that Chief of Staff General Marshall planned the use of 9 of the Mark II tactical weapons to prepare three beaches of the Japanese home islands for an Allied amphibious invasion if that invasion had been necessary to finally defeat the Empire. General Marshall planned the use of three of the Mark II to effect the destruction of beach obstructions and shore defenses on each of three invasion beaches; three of the Mark II were planned to effect the destruction of defensive installations and troops immediately behind the three invasion beaches; and three of the Mark II were planned to be employed against Japanese troops and military equipment that U.S. military planners anticipated would advance to meet the invasion from more distant locations.

Those 9 Mark II weapons would each require 9 kg U^{235}, for a total of 81 kg which quantity would have been reserved from the total available by 6 August 1945. Subtracting that 81 kg reserve from the totals of either 230 kg (Cox) or 178 kg (Staggs) U^{235} available by 6

August 1945, the U.S. had available either 149 kg or 97 kg U^{235} to provide the operational capability of either 2 or 1 of the Mark I strategic weapon and the reserve operational capability of 9 of the Mark II tactical weapon. The number of either 2 or 1 of the Mark I is accordant with the anecdotal claim of the Manhattan Project historical literature that by 6 August 1945 only sufficient U^{235} was available to enable the detonation of one Little Boy Mark I weapon, the weapon detonated at Hiroshima, which employed 50 kg U^{235}.

All the U^{235} produced during 1943 and that produced during 1944, prior to 17 July, was the conjunct result of two isotope separation processes: the liquid thermal isotope separation method developed by Philip Hague Abelson at the Naval Research Laboratory and the electromagnetic isotope separation process developed by Earnest Orlando Lawrence of the University of California, Berkeley, Radiation Laboratory. Chapter 12 provides review of those two technologies and how they operated in conjunction to produce the first U^{235} in weapon quantity during 1943, 1944 and the first six months of 1945.

Photographs and illustrations credits.

"John E. Burchard, Damage Survey at Port Chicago, California, 29 July 1944." Source: Los Alamos National Laboratory Archives, Collection A-84-019, Series 5, 319.1 Port Chicago Disaster Reports, 7/17/44 - 11/16/44 & undated (Folder 29-1) [Formerly Folder 37-6].

"J. Robert Oppenheimer, 26 August 1944 comment on John E. Burchard's Damage Survey at Port Chicago, California." Source: Los Alamos National Laboratory Archives, Collection A-84-019, Series 5, 319.1 Port Chicago Disaster Reports, 7/17/44 - 11/16/44 & undated (Folder 29-1) [Formerly Folder 37-6]

"Peter Vogel to Don M. Cox, letter of 9 December 1980." Source: Correspondence, Peter Vogel.

Chapter 12

U.S. World War II U^{235} isotope separation: E. O. Lawrence and Philip H. Abelson.

Chapter 11 published two corresponding sets of 1943-1949 U^{235} production data obtained 5 December 1980 from the U.S. Department of Energy Enrichment Office Division at Oak Ridge, Tennessee, and the U.S. Department of Energy Office of Uranium Resources Enrichment, Planning and Analysis Branch in Washington, DC. Those two data sets show that during 1943 the Manhattan Project did separate either 74 or 59 kg of U^{235}. In the years since those 1943 U^{235} production data were provided by DOE many critics have disputed, and several have denied, the validity of those DOE data.

To sustain my argument that sufficient separated U^{235} had been produced by 17 July 1944 to enable a nuclear fission weapon detonation on that date at the Port Chicago Naval Magazine it has been necessary to discover corresponding documentary attestations to authenticate that either 74 kg or 59 kg U^{235} had been separated during 1943. Study of the many published books of the Manhattan Project historical literature yields no attestation that any quantity of U^{235} had been separated during 1943. The author's task has been to satisfactorily confute that universally accepted precept of the Manhattan Project historical literature. Information published in this chapter will show that Philip H. Abelson, working at the United States Naval Research Laboratory with the liquid thermal diffusion uranium isotope separation method, did separate the U^{235} isotope during 1943 in quantity sufficient to permit the detonation of at least one Mark II bomb utilizing 9 kg U^{235} by 17 July 1944.

Beginning in 1940 at the U.S. Bureau of Standards in Washington, DC, with his first essentially bench-scale demonstration of the liquid thermal diffusion method Philip Abelson did achieve his first measureable uranium isotope separation in April 1941. That achievement was such a significant achievement in progress of the United States Navy's early interest in the development of nuclear fission energy as a means of ship propulsion that two months later, in June 1941, the Naval Research Laboratory (NRL) provided funding for further development of Abelson's uranium isotope separation by the liquid thermal diffusion method. Accordingly, Abelson moved his work from the Bureau of Standards to the NRL at Anacostia, also in the District of Columbia. Between March 1941 and December 1942 at the NRL Abelson designed and constructed the NRL liquid thermal diffusion pilot plant, which came on-line with successive improvements during 1942. During the six months following January 1943, namely February through July 1943, the NRL pilot plant "produced 236 pounds of UF6 [uranium hexafluoride] possessing isotope separation. The quantity and the separation were greater than had been obtained by the gaseous diffusion method at that time."

That information published by Dr. Abelson in 1998 is the first published verification that U^{235} separation had been accomplished in quantity during 1943. Additional information provided by a declassified letter dated 15 September 1943 from James Conant, alternate civilian member of the Atomic Bomb Military Policy Committee, to Rear Admiral William Purnell, the Navy member of that committee, also verifies that U^{235} separation in quantity had been accomplished during 1943 by the Naval Research Laboratory. That letter also shows that by 15 September 1943 all the enriched uranium hexafluoride ("hex") produced by Abelson at the NRL during, at least, those six months of 1943 had been, or immediately would be, transferred by the Navy to the Manhattan Project.

The per cent U^{235} concentration of that material, at least 236 pounds (107 kg), has not been disclosed in declassified documents but, as shown below, that material included defined quantities of varying, but known, U^{235} concentration. That material in possession of the Manhattan Project was available for immediate enhanced enrichment, if necessary, by Ernest O. Lawrence's electromagnetic method to the 20 per cent U^{235} concentration necessary to permit the detonation of at least one 9 kg U^{235} Mark II weapon by 17 July 1944.

James Conant's letter of 15 September 1943 to Admiral Purnell reads in the paragraph numbered (3):

15 September 1943 letter of James Conant to Rear Admiral William R. Purnell.

> "We understand that there is still available at the Naval Research Laboratory approximately 80 pounds of hex, made up of several lots of different known composition. If this material, together with the analyses of the several samples, can be made available to those now engaged on the project under the general direction of the Military Policy Committee for experimental purposes, the favor will be deeply appreciated, and an equivalent amount of base material will be supplied in exchange. The arrangements for this would be made through General Groves' office."

The liquid thermal diffusion and electromagnetic isotope separation methods will be discussed in detail later in this chapter, but presently a brief review of several typical assessments of the success of World War II uranium isotope separation method that are pervasive in the Manhattan Project historical literature will introduce a fraction of comic relief into an otherwise sedating historical discourse.

Assessments of U^{235} separation accomplished during World War II.

Wartime U^{235} production data was one of the most closely guarded secrets of the Manhattan Project, and the U.S. Department of Energy (DOE) has not yet administratively released the U^{235} production data for the years 1943-1949. During late 1996 and early 1997 DOE Secretary Hazel O'Leary toured the country to promote the DOE's "Openness Initiative," part of which was "a commitment to informing the public on information regarding the total figures for United States highly enriched uranium production, acquisition, and utilization.” That information was scheduled for publication in September 1997 as the “Highly Enriched Uranium Report: The First 50 years.” The report was advertised to “provide information regarding uranium enriched in the U-235 isotope to a level of 20% or greater” for the years 1945-1996.

In a January 1997 meeting with Secretary O’Leary in San Francisco I gave to her the 1943-1949 U^{235} production data I had obtained in December 1980 from the two DOE offices and said those data should be basic to the forthcoming DOE “Uranium Report.” One month later DOE announced that publication of the “Uranium Report” was indefinitely suspended because, “It is taking considerable time to locate, identify, and access the huge quantity of highly enriched uranium data which includes production, blending, shipment, etc. Further, analysis, verification, and validation of this data are taking considerable effort.” At the time of this writing, 17 April 2002, the DOE “Uranium Report” has not been published.

The Government has withheld publication of those data with the result that anecdotal accounts of World War II U^{235} production, which lack any authentic documented basis, have become established by repetition in the historical literature and consequently credited in the public perception as fact. The Government has not published false information to mislead public perception of the actual amount of U^{235} production accomplished during the war, but since World War II the Government has tacitly permitted the promulgation and general acceptance of fiction in that matter. Appropriately critical reading of all hitherto published accounts of wartime U^{235} separation plainly shows that one author following another has creatively paraphrased the same few anecdotal assertions that have persisted in the literature since the first official account of the Manhattan Project was published in 1945 by Henry DeWolf Smyth, *Atomic Energy for Military Purposes*. Examples from current and recent Manhattan Project historical literatures illustrate how bizarre and antic those accounts of wartime U^{235} production can be and which are, despite their absurdity, generally credited as competent historical record.

The Nuclear Weapon Archive has posted at this web site,

http://nuclearweaponarchive.org/Usa/Med/Med.html,

that “September 1944 marked a difficult period: . . . The total production of highly enriched uranium to date was only a few grams.” The FAS Web site provides no basis of documented fact for that assertion. That same assertion has been repeated in the literature since the end of World War II. Because Government records of U^{235} production that would substantiate or refute that assertion are classified that assertion is unverifiable; nevertheless FAS offers that information without the cautionary admonishment of competent science that customarily is made in circumstances of unverifiable fact, and FAS is satisfied to offered the public, as useful information, an indefinite number: A “few” is an indefinitely small number that conveys a qualitative sense of quantity, but not quantitative fact.

Richard Rhodes’ 1986 book, *The Making of the Atomic Bomb*, which won the National Book Award for its elephantine literary accomplishment, offers information that should be—but is not—instructively correspondent to the FAS assertion that “September 1944 marked a difficult period: . . . The total production of highly enriched uranium to date was only a few grams.” Richard Rhodes reports on page 600, “A minimum of 100 grams per day—3.5 ounces—of 10 percent U235 came through beginning in late September 1944.” Rhodes specifically refers to production of the Oak Ridge, Tennessee, electromagnetic plant, Y-12, but the information provided by Rhodes for September 1944 is no more factually quantitative than the information provided by the FAS for the same period.

A one-day “minimum” production of 100 grams of 10 per cent U^{235} equates to 10 grams of U^{235}. Ten grams of U^{235} is reasonably the equivalent of “only a few grams” that the FAS asserts were produced during September 1944. But if two days at Y-12 in late September 1944 produced a minimum of ten grams, the total minimum September production was 20 grams, which is twice more than a few grams. Five days minimum production in late September would have produced 50 grams, which is five times more than a few grams, and might also be expressed as an indefinite number that is approximately several times more than a few, but not many times more than a few. In fact, we don't know how many days in "late September" Y-12 did produce a minimum of 10 grams. We don't in fact know how many days constitute the period of "late September." We don't know if any day's production at Y-12 in late September perhaps exceeded the minimum daily production of 10 grams; if the daily 10-gram minimum was exceeded, we do not know how many grams in excess of the 10 grams minimum might have been produced.

This is the kind of nonsensical numbers jumbling that recalls Medieval speculations that disputed the number of angels that could dance on the head of a pin. Information that would be historically meaningful is not the daily minimum or daily maximum potential output of the Y-12 plant, but the actual quantity of material that was produced during the late days of September 1944—the results. The actual results are, of course, classified data and the result of that ignorance has been that authors, like the weaver of cloth for the "Emperor's New Clothes," weave an imaginary fabric and the world admires the design. Few readers would be satisfied with an historical account of the 1944 Kentucky Derby which reported only that a minimum of two horses had run the Derby in early May 1944. A satisfactory historical record would report the race results. The 1944 Kentucky Derby was won by Pensive, owned by Calumet Farm, trained by Ben A. Jones, and ridden by one of the greatest jockeys of the mid-century, C. McCreary, in a time of 2:04 1/5. The maximum number of horses that can run the Kentucky Derby is 20; the actual number that ran in 1944 was 20, and the fastest Derby time, 1:59 2/5, was run by Secretariat in 1973. That's useful historical information.

On page 602 Rhodes reports the anecdote that has been repeated since the end of the war, "Early in 1945 Oak Ridge began shipping bomb grade U235 to Los Alamos." This report is as comprehensive as a report that Santa Claus began delivering Christmas toys to deserving boys and girls in late December 1945. Santa Claus and his Elves had spent most of the year 1945 producing those toys, but Santa didn't distribute those toys until the appropriate time. I am decidedly persuaded that Los Alamos scientists received bomb grade U^{235} at the time and in quantities that General Groves determined appropriate. General

Groves had no practical reason to provide any of the U^{235} produced during 1943 and 1944 to the scientists at Los Alamos in quantity above what was immediately necessary to experimental purposes or, when useful, bomb construction.

General Groves had more than one compelling reason to withhold any and all U^{235} that had no immediate experimental or weapon use. That deliberate procedure of dispensing only the amount of U^{235} useful at any time did guarantee that the actual amount of U^{235} available at any time during the war was unknown to the scientists working at Los Alamos, which ignorance limited the possibility that information might leak to the enemy and allow some military advantage to the Japanese, the Germans or even to our ally, the Russians.

Furthermore, General Groves was a very much experienced personnel manager, on a mammoth scale; he'd gotten the Government's Pentagon Building in Washington, DC, completed in less time than anyone hoped it could be done, and he knew that people will work more diligently, and with more dedication and resourcefulness, if the purpose to be accomplished is urgent, or is represented to be urgent—a race against time or, as the title of one book of Manhattan Project history characterized that urgency, "Scientists Against Time." The programs of the Manhattan Project were urgent, but by creating the exaggerated impression that timely production of the fissionable material for the Mark I U^{235} bomb was always in doubt General Groves spurred the scientists at Los Alamos to greater urgent effort to complete a workable plutonium Mark IV bomb, which for several years General Groves and others in the military and at Los Alamos knew would be necessary to the postwar development of the hydrogen fusion bomb.

The Mark IV technology and plutonium would be the basis of the trigger mechanism for the hydrogen bomb. General Groves was concerned that if the Mark IV were not successfully completed before the end of the war Government funding in peacetime for that development might not materialize. The hydrogen bomb would certainly be developed eventually by the Russians, and military preparedness required that the U.S. also have the H-bomb to counter that expected Russian threat. Successful completion of the Mark IV, focused spherical plutonium implosion technology before the end of the war was of very, very great importance to the postwar national defense. The scientists at Los Alamos were kept in ignorance of the fact that the prototype Mark II bomb had been successfully proof fired at Port Chicago, and they were kept in ignorance of the actual quantities of U^{235} that had been produced. The urgency of scientists working against time to produce the Mark IV plutonium weapon gave a very great impetus to that effort.

Uranium isotope separation methods, 1943-1944.

Centrifugal uranium isotope separation.

Early in the U.S. atomic bomb development program some theoretical and experimental studies were made to evaluate the potential of centrifuges to separate the uranium isotopes. Theory and experiment proved that the lighter and heavier uranium isotopes would segregate in distinct bands from an originally homogenous natural uranium material subjected to centrifuge rotation at high speed. Thousands of centrifuges would have been required to separate the U^{235} isotope in bomb quantity, and no efficient means of extracting the separated isotopes from the experimental centrifuges was immediately practicable. Furthermore, rapidly spinning machines have a notorious tendency to spontaneously disintegrate which, in the case of a uranium isotope separation centrifuge, results in the loss of the contained material. The centrifugal method was abandoned very early in the U.S. program; the history of that method has been well reported in the literature and will not be repeated here.

Of contemporary interest, Iraqi President Saddam Hussein’s efforts in the late 20th century to produce U^{235} for weapon use employed the centrifugal method; German manufacturers sold those machines, in pieces, to Iraq in violation of an applicable United Nations embargo. Saddam Hussein was frustrated in that undertaking by the same problems the U.S. had encountered in 1940 and 1941: the required machines are fragile and often disintegrate in high speed operation, and it’s difficult to remove the useful isotope from a centrifuge without significant contamination by the dominant but useless U^{238} isotope.

Gaseous diffusion uranium isotope separation method: Oak Ridge, K-25.

The gaseous diffusion uranium isotope separation method or process, also known as the screen diffusion method or process, and constructed at Oak Ridge as the K-25 plant, has been adequately reported in the available Manhattan Project literature and will not be reviewed here. This gaseous diffusion method made no contribution to U^{235} separation during 1943 and 1944.

Electromagnetic uranium isotope separation: Ernest O. Lawrence, Oak Ridge, Y-12.

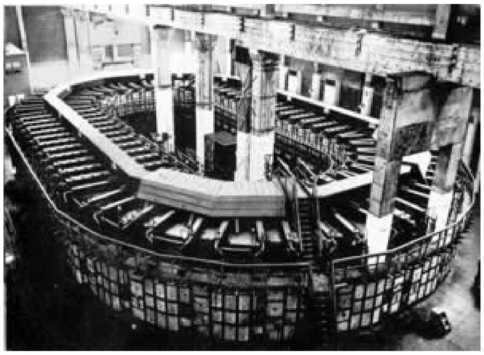
Ernest O. Lawrence, Director, University of California Berkeley Radiation Laboratory (1901-1958).

In 1939 A. O. C. Nier at the University of Minnesota had separated a minuscule amount of U^{235} by mass spectroscopy and immediately Ernest O. Lawrence, Director of the University of California Berkeley Radiation Laboratory, calculated that improvements to a machine he had invented would enable large scale separation of the U^{235} isotope. That machine was the cyclotron. In 1939 Lawrence received the Nobel Prize in physics “for the invention and development of the cyclotron and for results obtained with it, especially with regard to artificial radioactive elements.”

The cyclotron—later in the war known as the calutron, for **Cal**ifornia **U**niversity cyclo**tron**—utilized a huge electromagnet and the associated prodigious magnetic field between the magnet’s poles to accelerate atomic particles. A monoenergetic beam of ions of naturally occurring uranium when passing between the poles of the calutron magnet, in a vacuum, splits into several streams according to their momentum, one per isotope, each characterized by the particular radius of curvature induced by the magnetic field. Collecting cups at the ends of the semicircular trajectories caught homogenous streams of the different uranium isotopes.

Lawrence’s 1939 cyclotron magnet was inadequate to more than experimental isotope separation and a gargantuan magnet enclosed in a comparably-sized calutron would be required to separate the U^{235} isotope in quantity greater than laboratory samples. Electromagnets of the size necessary to the purpose had never before been made. Production of U^{235} in quantity sufficient to the requirement of an atomic bomb by this, the electromagnetic isotope separation method, would require many such prodigious magnets each in a separate calutron.

In April 1940 the Rockefeller Foundation pledged $1.4 million to the cost of Lawrence’s proposed 184-inch cyclotron magnet. In November 1941 a committee of the National Academy of Sciences reported that Lawrence's proposed method of separating the U^{235} isotope should be pursued by the Manhattan Project (then known as the S-1 project) as well as several other methods that seemed less certain. The Office of Scientific Research and Development (OSRD) then contributed $400,000 to Lawrence’s development of the calutron. Two years later, in March 1942, Lawrence successfully enriched the U^{235} isotope in a sample of uranium by a factor of five; General Groves ordered construction of an industrial scale plant at Oak Ridge, Tennessee, code named Y-12, that would employ Lawrence’s calutrons to separate U^{235} in weapon quantities.


Oak Ridge, Tennessee,
Y-12 Alpha electromagnetic isotope separation “racetrack”

During 1942 Lawrence and his associates at Berkeley worked pretty much night and day to refine the calutron design to multiply the demonstrated factor of five enrichment. The first calutrons constructed at Oak Ridge were built in a large oval configuration and officially designated Alpha units but, in code talk, were soon referred to as Lawrence’s “racetracks,” an important recurrent term. General Groves initially had ordered construction of 96 calutrons at Oak Ridge to be combined into each of five production racetracks, but in March 1943 the General authorized construction of a second installation of calutrons known as Beta units, which were to receive the slightly enriched uranium product of the Alpha units and to increase the U^{235} concentration produced by the Alpha units to bomb grade enrichment. Ground was broken at Oak Ridge on February 18, 1943 for the electromagnetic isotope separation plant designated Y-12. In a letter to General Groves dated 3 August 1943, Lawrence wrote, “The first racetrack [designated “XA”] will go into operation November 1 and succeeding racetracks follow at monthly intervals.”

Richard Rhodes’ book on page 492 offers the claim that “At the end of 1943 Y-12 was dead in the water with hardly a gram of U235 to show for all its enormous expense,” but apparently Y-12 was raised from the dead to productive life during the 31 days of January 1944, following the transfer of 236 pounds of enriched uranium hexafluoride from the Naval Research Laboratory to the Manhattan Project. An early February 1944 exchange of letters between Military Policy Committee alternate member James Conant and E. O. Lawrence documents that spontaneous vivification. On 11 February, in cryptic reference to the Y-12 Alpha “racetracks,” Conant wrote to Lawrence at the Berkeley Radiation Laboratory, “I hear your horses are running well and all prospects for the future are rosy. Many congratulations and best wishes.” Five days later Lawrence responded, “Many thanks for your kind words. Prospects are indeed rosy and there is every reason to expect that the job will go through in time to be a factor in the shape of things to come.” By early February 1944, entirely as a consequence of the enriched uranium hexafluoride produced by the NRL that had been transferred to the Manhattan project during 1943, the Y-12 electromagnetic separation plant was not “dead in the water.”

United States U^{235} isotope separation accomplished during 1943 and 1944 was the conjunct product of the Naval Research Laboratory liquid thermal diffusion pilot plant operated by Philip H. Abelson and E. O. Lawrence’s Oak Ridge Y-12 electromagnetic Alpha racetracks. Uranium hexafluoride, enriched to various known but classified U^{235} concentrations, produced by the NRL during 1943 was accumulated by the Navy and when transferred to the Manhattan Project, before and after 15 September 1943, was converted to uranium tetrachloride and utilized as U^{235}-enriched feed stock for Lawrence’s Alpha calutrons. Several sources in the Manhattan Project historical literature report that slightly enriched uranium tetrachloride was successfully enhanced to bomb quality U^{235} concentration, 93 per cent, in one pass through any one of Lawrence's 96 Alpha calutron units; Dr. Abelson has confirmed that assertion during telephone conversations with me made during the mid-1990s.

The liquid thermal diffusion isotope separation method was greatly more efficient than Lawrence's electromagnetic method to double the 0.7 per cent occurrence of the U^{235} isotope in natural uranium to 1.4 per cent. Lawrence's Alpha calutrons were greatly more efficient than Abelson's liquid thermal diffusion method to increase 1.4 per cent U^{235} product to any higher degree of enrichment from 20 per cent up to efficient bomb grade enrichment of 93 per cent U^{235}.

Liquid thermal diffusion uranium isotope separation: Philip H. Abelson and the NRL.


Capt. William S. Parsons, USN, and Dr. Philip H. Abelson (1913-2004).

The most succinct description of the liquid thermal diffusion method and the history of the development of that method is provided by Philip Abelson in a Biographical Memoir of NRL scientist Dr. Ross Gunn published by the National Academy Press in 1998. Every reader is emphatically encouraged to go to the complete text of that memoir, for Ross Gunn was a scientist of extraordinary accomplishment in a wide range of scientific disciplines, especially in his service to the Navy and including his contributions to NRL development of the liquid thermal diffusion isotope separation method. Philip Abelson's Biographical Memoir of Ross Gunn is at:

www.nap.edu/html/biomems/r**gunn**.pdf

In the interest of more widely disseminating Dr. Abelson's tribute to Ross Gunn, as well as Dr. Abelson's account of the historical development of the liquid thermal diffusion method, several paragraphs of that Biographical Memoir are here carried forward to this readership.

"From 1927 to 1947 Gunn was a research physicist on the staff of the U.S. Naval Research Laboratory. In 1934 he was appointed technical adviser for the entire laboratory. In that role he interacted with important naval personnel. In March [June?] 1939 he wrote a memorandum to Admiral H. G. Bowen, chief of the Navy's Bureau of Ships, outlining the tremendous advantages that could be expected from the use of atomic energy in submarine propulsion.

“Immediately after the announcement of the discovery of uranium fission in early 1939, Ross Gunn became a keen observer of and participant in developments relevant to nuclear power. He was particularly interested in its possible application to propulsion of submarines.

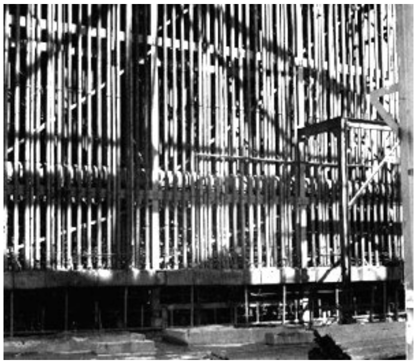
“By mid-1940 it had become evident that the rare 235U was fissionable and that a chain reaction creating nuclear power was likely to be achieved. Gunn learned that I [Abelson] was conducting experiments on uranium isotope separation and arranged to provide me with financial support. I was then an employee of the Carnegie Institution of Washington. I obtained my first tiny isotope separation using equipment manufactured by me, but housed at the National Bureau of Standards. The method involved liquid thermal diffusion of uranium hexafluoride (UF6). The simple apparatus consisted mainly of three concentric tubes 12 feet long. The inner tube was heated by steam. A second tube was maintained at 65° C. The third tube served to contain the 65° C cooling water. The UF6 occupied the space between the walls of the inner and middle tubes. Runs on this column were made in April 1941, when a measurable isotope separation was obtained.

“When Gunn learned that I had achieved a small separation of uranium isotopes, he invited me to join the staff of the Naval Research Laboratory, where enhanced supplies of high-pressure steam could be made available. In June 1941 the move was made. A series of experiments was conducted to determine the optimum spacing between the hot and cool walls. In June 1942 a column 36 feet long heated by 100 psi of steam produced an isotope separation factor of 1.11. This success led to an expanded effort that included authorization to build and operate fourteen columns 48 feet long. It also led to the procurement of a propane-fired boiler capable of delivering 1,000 lb/in^2 of steam. For a time, the facility at the Naval Research Laboratory was the world’s most successful separator of uranium isotopes.

[Interpretive note for the paragraph below: The Manhattan Project predecessor agency, code named the S-1 project, was headed by University of Chicago physicist Arthur H. Compton who was widely then perceived as an ineffective and ineffectual leader. Many wartime documents composed after the August 1942 establishment of the Manhattan Project continued to refer to the S-1 project rather than the Manhattan Engineer District (MED) project, as S-1 was reorganized after 17 September 1942 by Army Colonel Leslie Richard Groves within the Manhattan, New York, District Office of the Army Corps of Engineers.]

“Ross Gunn, who was a member of the federal government’s S-1 uranium committee, communicated results of the isotope experiments to committee chairman Lyman J. Briggs in August 1942. This led in October 1942 to a visit to the Naval Research Laboratory by General Leslie R. Groves and Admiral W. R. Purnell. Later, in January 1943, a special committee assembled by the Manhattan District inspected the installation. The committee was impressed by the simplicity of the equipment and commented favorably. A Naval Research Laboratory report submitted to the Bureau of Ships by Gunn in January 1943 pointed to the advantages of using enriched uranium in nuclear reactors. It would be a necessary step in creating a nuclear-powered submarine. The report also stated, ‘A liquid thermal diffusion plant costing one to two million dollars could provide the necessary separated isotopes.’ ”

“During the next six months, improvements were made in the construction of the separation columns. At the same time, the pilot plant produced 236 pounds of UF6 possessing isotope separation. The quantity and the separation were greater than had been obtained by the gaseous diffusion method at that time.



Liquid thermal diffusion uranium isotope separation columns, Philadelphia Navy Yard.

“Gunn decided that an expansion of production capabilities of the liquid thermal diffusion method was warranted. Doing so would provide an alternative if the Manhattan District’s magnetic and gaseous diffusion methods failed. A survey of naval establishments showed that large-scale sources of high-pressure steam could be made available at the Naval Boiler and Turbine Laboratory at the Philadelphia naval base. Authorization to build a 306-unit plant at Philadelphia was obtained on November 27, 1943.

Rear Admiral Earle Mills, assistant chief of the Bureau of Ships, signed the project order.

“In June 1944 the Philadelphia plant was approaching completion. J. Robert Oppenheimer learned of the progress and recognized that a supply of partially separated uranium would increase the production of an electromagnetic plant at Oak Ridge. He communicated with General Groves, who sent a reviewing committee to the Philadelphia plant. Its report was favorable and led to the decision to build a 2,142-column plant at Oak Ridge. Construction there was rapid. The $20-million facility [named S-50 and built in less than three months] achieved production that shortened the duration of World War II by eight days.

“Secretary of the Navy James Forrestal presented the Navy Distinguished Civilian Service Award to Ross Gunn on September 4, 1945. The citation included:

> “ ‘For exceptionally distinguished service to the United States Navy in the field of scientific research and in particular by reason of his outstanding contribution to the development of the atomic bomb . . . For his untiring devotion to this most urgent project, Dr. Gunn has distinguished himself in a manner richly deserving of the Navy’s highest civilian award.’

“Immediately after the end of the war Gunn returned to the concept of the nuclear submarine. Methods of detecting diesel-powered submarines had advanced greatly. In the latter part of World War II large numbers of German submarines had been destroyed. I was tasked with becoming familiar with the current state of nuclear reactors, particularly those using enriched uranium. I was provided with access to experimental programs at Oak Ridge and Argonne, and I participated in criticality experiments of enriched uranium assemblages.

“Gunn also took part in obtaining blueprints of the most advanced German submarine. Analysis showed that the energy system of the submarine could be replaced by a shielded nuclear reactor. In September 1946 a report on the feasibility of a nuclear submarine was submitted to the Bureau of Ships. Later, Admiral Hyman Rickover directed a highly successful development and construction of nuclear submarines. However, some part of the credit for nuclear submarines belongs to Ross Gunn."


Dr. Ross Gunn
(1897-1966).

Ross Gunn made the first proposal to utilize uranium as a fuel for submarine propulsion in a 1 June (March?) 1939 memorandum to the NRL Director Admiral Bowen, "Submarine submerged propulsion: Uranium power source." Admiral Hyman Rickover is generally credited, and properly so, with the practical development of the Navy's first nuclear-powered submarine after World War II, but immediately following the war Rickover "was still an undistinguished, little-known captain doing the routine work of mothballing excess navy ships in the Pacific. Months would elapse before he found his way into the nuclear field. By then research on nuclear power for ships was already under way within Groves' Manhattan Project. [Admiral William S.] Parsons' close relations with Groves help explain the readiness of the general to undertake this research. As the chief nuclear advisor to [Admirals] Forrestal and Nimitz, Parsons helped promote nuclear power for ships as a high navy priority."

A discussion of the roles of Admirals Parsons and Rickover is found in Albert Christman's *Target Hiroshima: Deak Parsons and the Creation of the Atomic Bomb*, U.S. Naval Institute Press, 1998, from which the above quotation is taken.

Christman's biography of Admiral Parsons, described by one critic as an "in-house gloss" because of Christman's prepossessed opinions as a retired Navy employee, is nonetheless the only present comprehensive study of Admiral Parsons' life and naval career and is certainly recommended reading. Admirals Parsons and Rickover were members together of the Annapolis Class of 1922. Which of the two admirals deserves to be acknowledged the most accomplished technical officer of the 20th century U.S. Navy will be disputed for many years, but as more information will be published descriptive of the modest and reclusive Admiral Parsons' and his accomplishments I am confident that history will accord that distinction which I have felt appropriate for 20 years.

By September 1980 I was aware that the liquid thermal diffusion method of uranium isotope separation had contributed significantly to U.S. wartime production of the U^{235} isotope in weapon quantities. In 1984 I began conversations and correspondence with Philip Abelson to learn the actual contribution of that method which he had developed beginning early in 1940; Dr. Abelson is editor emeritus of the American Association for the Advancement of Science journal *Science*. He has been very generous in correspondence and discussion of the technology and development of the process, but politely he always has declined to disclose the actual quantity of U^{235} that had been separated by that method during the war, citing the secrecy agreement he had accepted and signed during the war which established his continuing legal obligation to withhold that information.

In spring 1939, soon after it was announced that uranium fission had been achieved, Philip Abelson completed the requirements to receive his Ph.D. in physics from Berkeley where he worked with J. Robert Oppenheimer and E. O. Lawrence. That spring season on his office blackboard, with Abelson present, Oppenheimer drew a rough sketch of a U^{235} gun assembly weapon. Oppenheimer told Abelson that a weapon of that design was theoretically feasible but practically so only if an industrial scale method of separating the U^{235} isotope could be developed. With his Ph.D. awarded, Philip Abelson moved to the Carnegie Institution in Washington, DC, where he began development of the method for uranium isotope separation that would subsequently be accomplished entirely with U.S. Navy funding at the NRL and the Philadelphia, Pennsylvania, Navy Yard. The product of the Navy's liquid thermal diffusion uranium isotope facilities, which product was owned by the Navy, did in combination with the capabilities of Lawrence's first calutrons at Oak Ridge account for all the separated U^{235} available to the Manhattan Project during 1943 and 1944.

Significantly, Dr. Abelson writes in his Biographical Memoir of Ross Gunn that the liquid thermal diffusion isotope separation method "achieved production that shortened the duration of World War II by eight days.” Dr. Abelson, a modest man as was his mentor and colleague Dr. Gunn, does not report in that memoir that following the end of World War II he, Philip Abelson, was recognized by United States Congressional memorial for his development of that method of uranium isotope separation, which Congress somehow calculated did shorten the duration of the World War II by eight days. In discussion of that honor Dr. Abelson chuckles in perplexity, unable, as is the reader, to decipher any sensible meaning from that recognition.

On 12 September 1945 the Japanese representatives signed the official “Instrument of Surrender,” and World War II ended that day. Is the implication of that Congressional recognition that the Japanese representatives would have surrendered eight days later, on 20 September, if the U^{235} produced by the Abelson method had not enabled the combat use of the U^{235} Mark I weapon at Hiroshima on 6 August? Lacking other more specific determined meaning, that recognition must be a figurative acknowledgment by the United States Congress that Philip Hague Abelson and the liquid thermal diffusion method of uranium isotope separation method did contribute uniquely and exceptionally to the successful termination of World War II.

Dr. Abelson also writes in his biographical memoir of Dr. Gunn that Secretary of the Navy James Forrestal presented the Navy Distinguished Civilian Service Award to Ross Gunn on 4 September 1945. The citation included a recognition that will surprise careful readers of Richard Rhodes’ *The Making of the Atomic Bomb*, who will find in the book’s three indexed references to Ross Gunn no suggestion of Gunn’s “outstanding contribution to the development of the atomic bomb.” The Navy award citation reads in part that Dr. Gunn was recognized,

> “For exceptionally distinguished service to the United States Navy in the field of scientific research and in particular by reason of his outstanding contribution to the development of the atomic bomb . . . For his untiring devotion to this most urgent project, Dr. Gunn has distinguished himself in a manner richly deserving of the Navy’s highest civilian award.”

The significance of the Navy Distinguished Civilian Service Award to Dr. Gunn on 4 September 1945—even 8 days before the 12 September 1945 Japanese surrender—means Dr. Gunn was the first person of all those associated directly or indirectly with the Manhattan Project to receive Government recognition of his contribution to the development of the atomic bombs. The significance of the contributions made by Ross Gunn and Philip Abelson and the liquid thermal diffusion uranium isotope separation method developed with their direction to the successful conclusion of World War II will be truly comprehended only when the quantity of U^{235} produced that method shall be publicly known.

To augment this chapter that has been principally concerned with Philip Abelson's contributions to the successful termination of World War II by development of the liquid thermal diffusion uranium isotope separation method, other particulars of Abelson's exceptional role in the nation's atomic bomb program are of interest.

Early in the game, U^{235} separation in bomb quantity and quality required a license from Philip Abelson because he had been granted a U.S. patent on the only then feasible method of producing uranium hexafluoride (UF_6) in industrial scale quantities; UF_6 gas when appropriated heated enters its liquid phase, and it was liquid UF_6 that was the basis of Abelson's liquid thermal diffusion uranium isotope separation method. UF_6 in its gaseous phase, at about 60°C, was also essential to the gaseous diffusion uranium isotope separation method which, after January 1945, began undocumented but no doubt scanty separation of the isotope. The gaseous diffusion method conducted at the Oak Ridge K-25 plant did not, during the war, make a critical contribution to U^{235} production, although in the postwar decades the gaseous diffusion plant has been the only major U.S. facility to separate the isotope.

There is an apocryphal story which is probably true but has not yet been thoroughly documented that General Groves seriously blew his emotional relief valve when he had to acknowledge that the Oak Ridge K-25 gaseous diffusion plant would not be able to operate without a patent license from Philip Abelson, which license would legally permit Manhattan Project contractors to produce the vast quantities of UF_6 gas necessary to operation of the K-25 plant. Without much delay Abelson was persuaded to sell his patent to the U.S. Government for $1.00, but it is alleged by usually reliable sources of rumor that one condition of the sale of that patent required General Groves to guarantee to the Navy, on demand, those quantities of refined uranium metal necessary to continued uranium isotope separation at the Navy's Philadelphia Yard liquid thermal diffusion facility, which facility was Philip Abelson's personal province. The Navy's interest in postwar U^{235} production at Philadelphia anticipated that uranium power reactors would be developed as a means of naval ship propulsion, especially submarines. Philip Abelson did lead the Navy's first feasibility study of nuclear-powered submarines.

Appendix A provides transcriptions of all those documents in my possession concerned with development of the liquid thermal diffusion uranium isotope separation method during the period 9 September 1940 through 21 September 1944. These documents, which will not be otherwise available to the reader interested in the detail these documents provide, also provide references to additional, comprehensive NRL reports on the process which I have been unable to locate.

Photographs and illustrations credits.

“15 September 1943 letter of James Conant to Rear Admiral William R. Purnell.” Source: National Archives Microfilm Publications, “Bush-Conant File Relating to the Development of the Atomic Bomb, 1940-1945; Records of the Office of Scientific Research and Development Record Group 227,” reel No. 10, frames Nos. 152, 153.

“Oak Ridge, Tennessee, Y-12 Alpha electromagnetic isotope separation ‘racetrack’ .” Source: U.S. National Archives.

“Liquid thermal diffusion uranium isotope separation columns, Philadelphia Navy Yard.” Source: U.S. National Archives.

“Ernest O. Lawrence, Director University of California Berkeley Radiation Laboratory.” Source: The University of California.

“Capt. William S. Parsons, USN, and Philip H. Abelson.” Source: Philip H. Abelson.

“Dr. Ross Gunn.” Source: U.S. National Archives.

Chapter 13

Mark II: July 4 – August 17, 1944

Within the entire commercially published Manhattan Project historical literature there is only one specific mention of the Mark II by that designation. That instance is found in *The New World, 1939/1946,* which is Volume I of a two-volume U.S. Department of Energy-funded history of the U.S. Atomic Energy Commission published, 1962, by the Pennsylvania State University Press and subsequently republished by the University of California Press. *The New World, 1939/1946* was written by DOE contract historians Richard G. Hewlett and Oscar E. Anderson, Jr. In addition to the information that Hewlett and Anderson provided about the Mark II there are presently three identified Manhattan Project documents that also name the Mark II and supply additional information about the Mark II.

James B. Conant (left) with Vannevar Bush after witnessing the atomic bomb explosion at Trinity site, Alamogordo, New Mexico, 16 July 1945.

Those three documents, all from the summer of 1944, are dated 4 July, 27 July and 17 August. All three documents were written by Atomic Bomb Military Policy Committee alternate member, Harvard University President James B. Conant. The document dated 27 July reports events of 17 July 1944, the day of the Port Chicago explosion. No publicly known document dated before 4 July 1944 names the Mark II; no publicly known document dated after 17 August 1944 names the Mark II. Hewlett and Anderson do not identify the documentary sources they had consulted to prepare their description of the Mark II, but comparison of that text with the text of the three identified Manhattan

Project documents that name and describe the Mark II discloses that those three documents were the source of the description of the Mark II that Hewlett and Anderson published in *The New World.*

The information descriptive of the Mark II disclosed in the text of *The New World* and the information descriptive of the Mark II disclosed in the text of the three presently identified germane Manhattan Project documents permits the following composite description of the Mark II and the state of its development during the period 4 July–17 August 1944:

Mark II was a low-efficiency implosion bomb suitable for use with either U^{235} or plutonium (Pu^{239}). The nuclear fission chain reaction achieved by the Mark II utilizing a U^{235} active would be the result of slow (thermal energy) neutron fission. On 4 July the predicted energy yield of the Mark II was 1,000 tons TNT equivalent. On 17 July a test of the Mark II was predicted to yield a "moderate" explosion equivalent, at minimum, to "only a few hundred tons of TNT." By 17 August the "upper limit of effectiveness" achieved by the Mark II was known, but that information is classified. On 17 August, the Mark II could be developed for combat use in 3 or 4 months time and the upper limit of effectiveness could be "raised somewhat."

We will now review in detail the four available descriptions of the Mark II. The report of the Mark II provided by Hewlett and Anderson appears on pages 251-252 of the first edition of *The New World.* In autumn 1990 Richard Hewlett acknowledged in telephone conversation that he had been the lead author of that segment of *The New World* that describes the Mark II; he could then remember only one of the documentary sources he had consulted, James Conant's "Historical Note. Written July 27, 1944."

"Findings of Trip to L. A. [Los Alamos] July 4, 1944"
James Conant's report to General Groves.

On **4 July 1944** James Conant informed Gen. Groves by memorandum that detonation of the Mark II utilizing a 9 kg U^{235} active, or detonation utilizing a 2 kg plutonium active, was expected to yield a nuclear

James Conant, “Findings of Trip to L.A. [Los Alamos] July 4, 1944”

fission explosion equivalent to the detonation of 1,000 tons of TNT. An optimal air burst of the Mark II with an energy equivalent of 1,000 tons TNT was expected to cause Class B damage (damage beyond repair) to an area of 2-5 square miles. Ten square miles of Class B damage was the goal Los Alamos set for optimal development of the Mark II. Conant informed Gen. Groves on 4 July 1944 that the Joint Chiefs of Staff should be informed they could “count on the Mark II for the purposes of operational planning,” but Conant informed Gen. Groves the “Mark II will require one proof firing before the design is ready for use against the enemy.”

During most of 1944 James Conant visited Los Alamos once each month to review technical and scientific developments and problems; during those visits he met with Los Alamos Director J. Robert Oppenheimer and others members of Oppenheimer’s scientific staff. Conant would then make written reports of those visits addressed to Gen. Groves, which summarized his “findings.” Conant either preferred travel by train or Gen. Groves had prohibited him to travel by airplane. For his trips to Los Alamos Conant boarded the Chicago-to-Los Angeles “Southwest Chief” at Chicago, Illinois, and rode one day and one night to Lamy, New Mexico, a few miles southeast of Santa Fe. A car and driver from Los Alamos would meet the “Chief” at Lamy and deliver Conant to Los Alamos.

On 23 June 1944 Conant wrote “Dear Oppie” that he planned to arrive at Lamy Sunday, 2 July, on the “Chief.” He wrote, “Please do no feel that you or George [Kistiakowski], or any hard working scientist who needs his Sunday off, should come to meet me . . . I am planning to leave on Thursday [6 July] at 10:15 A.M.” In consequence of this visit Conant wrote the document, “Findings of Trip to L. A. [Los Alamos] July 4, 1944,” which is presently discovered only in his manuscript draft.

The second page of this report to Gen. Groves begins with Conant’s advisement: “Recommend the following report to the ‘top,’ assuming one is confident of [SENSITIVE INFORMATION DELETED].” We cannot in fact determine from the text of this document what person or persons Gen. Groves would understand Conant to mean by the “top,” but

President Franklin D. Roosevelt had designated five persons to have determination of general policy in the Project, namely, Vice President Henry A. Wallace, Secretary of War Henry L. Stimson, Chief of Staff General George C. Marshall, James B. Conant, and Vannevar Bush. Those five men composed the President’s General Policy Group, also known as the “Top Policy Group.” Conant’s recommendation to Gen. Groves that he provide this report to the “top” most probably is correctly interpreted to mean the Top Policy Group. Assuming the Top Policy Group to be the correct interpretation of Conant’s allusion to the “top,” those four men, Conant himself, and Gen. Groves may be said to have been cognizant of the information that Conant reported in this document.

Two other men were probably cognizant of the information this report provides about the Mark I and Mark II bombs—General Wilhelm D. Styer and Rear Admiral William R. Purnell. On 23 September 1942 the Top Policy Group, designated by President Roosevelt to have determination of general policy in the Project, had appointed the Military Policy Committee “to consider and plan military policy” relating to the Project. Members of the Military Policy Committee were Vannevar Bush, James Conant as Bush’s committee alternate, Gen. Styer for the Army, and Adm. Purnell for the Navy. Because this document, “Findings of Trip to L. A. July 4, 1944,” forecasts the availability of the Mark I and Mark II bombs for military use, we must reasonably infer that Gen. Styer and Adm. Purnell were among those men cognizant of the information this document discloses about the Mark I and Mark II bombs.

Furthermore, I have no doubt that President Franklin D. Roosevelt was, at least in summary, also informed of the information this report discloses about the Mark I and Mark II bombs. Of the Top Policy Group, Vice President Wallace, Secretary of War Stimson, Chief of Staff General Marshall, and Vannevar Bush did report directly to the President. Conant’s report to the “top” forecasts the availability of the Mark I and Mark II bombs for military use, and those forecasts, he wrote, were “certain enough to be used as a basis for operational planning by the JCS [Joint Chiefs of Staff] . . . One can count on either 4 Mark I bombs (20-40 sq. [square] miles class B damage) or 20 Mark

II bombs (2-5 sq. miles class B damage).” It is inconceivable that this information would not have been immediately conveyed to the President—that atomic bombs of superlative power could and certainly would be made, by what date and in what number those bombs would be available.

Although Conant’s 4 July 1944 forecasts were certain enough to be used as a basis for operational planning by the Joint Chiefs of Staff, he does stipulate in this report that the “Mark II will require one proof firing before the design is ready for use against the enemy.” That one required proof firing of the Mark II would, if successful, fulfill the requirement Conant would stipulate 13 days later, on 17 July, that a test of the Mark II was necessary to prove the Mark II a “fairly sure thing,” and which proof would permit a decision to put the Mark II on the shelf, and would permit Los Alamos to work on the Mark III with less nervousness.

The text of James Conant’s 4 July 1944 report, which he recommended that Gen. Groves provide to the “top,” is transcribed below. The reader should keep in mind the important fact that James Conant was not ever informed of the exact quantity of enriched uranium produced during the war. In this report Conant does report that U^{235} was then “in full scale manufacture,” but all references to the availability of U^{235} he identifies to be assumptions based on “present indications.” In one paragraph of this document, which does not occur in that portion of the document that is his report to the “top,” he asks, “What is the schedule for ‘25’ production in Sept., etc.?”

Conant did not have a need to know the exact U^{235} production results and Gen. Groves believed that secret information would be best kept during the war, and in the postwar years, by those few men who had a specific need to know that information to plan and implement use of the bombs for military purposes. From the several thousand wartime document pages of Conant’s authorship that I have carefully read, I have taken the conviction that Conant chose not to know the actual quantity of U^{235} that had been produced at any time during the war.

As shown in Chapter 12, Conant was aware that significant quantities of U^{235} had been produced during 1943, but he did not know to what degree that material had been enriched. I am confident that during the period 4 July–17 August Conant did not know that sufficient U^{235} had been separated to permit the required test of the Mark II. In my opinion, by 27 July 1944 Conant had deduced that the Port Chicago explosion had been the one proof firing of the Mark II that he had informed Gen. Groves on 4 July was necessary. In my opinion, on 27 July Conant made his "Historical note" to evidence in his own historical record, and for posterity, that the proof of the Mark II at Port Chicago had been planned, and that Oppenheimer was cognizant that test would occur later that same day, 17 July 1944. The text of Conant's 4 July 1944 report to Gen. Groves, which he recommended be transmitted to the "top," reads thus:

> "**Recommend the following report to the 'top' assuming one is confident of [SENSITIVE INFORMATION DELETED].**
>
> " 'We are confident that one bomb can be dropped on the enemy on Aug. 1 [1945] with every prospect of a success; the area of class B damage will be 20-40 square miles. Additional bombs could be dropped every six weeks thereafter. These bombs use the method of assembly (Mark I) which we are now confident will work for one of the two products under manufacture [i.e., U^{235}]. It suffers from the disadvantage that relatively large quantities are required. Work is now being pushed at [one word unreadable] speed on a second method of assembly (Mark II) which use[s] considerably smaller amounts of material. This in turn allows earlier delivery of a bomb and a greater number of bombs during the next twelve months. Initially this second method (Mark II) will represent a less efficient use of material but eventually after all the development work is complete it will probably prove a much more efficient bomb than Mark I. The present indications are that the first Mark II bomb (class "B" damage, 2-5 sq. [square] miles) will be ready in March and 3-6 such bombs can be produced before July 1. This forecast in regard to Mark II while extremely probable can not be made with the same confidence as the statement about Mark I since the research and development of the Mark II bomb is less far advanced and by the very nature of construction Mark II will require one proof firing before the design is ready for use against the enemy. These forecasts which we believe are certain enough to be used as a basis

> for operational planning by the JCS [Joint Chiefs of Staff] involve only material “25” [U^{235}] which is now in full scale manufacture. If the production of “49” [plutonium] which should commence in a few months is according to schedule the output of Mark II bombs before July 1 should be increased by two or three bombs. For the six months following July 1, 1945 one can count on either 4 Mark I bombs (20-40 sq. miles class B damage) or 20 Mark II bombs (2-5 sq. miles class B damage) as a minimum with a possible increase in production of 50% and a possible increase of effectiveness per bomb of 1.5-3 fold in area of class B damage.’ ”

James Conant, “Findings of Trip to L. A. [Los Alamos] July 4, 1944”; page 4 text enlargement

Page 4 of Conant’s “Findings of Trip to L. A. July 4, 1944” is mostly legible, with difficulty, but may not be legible in the reproduction of that page available earlier in this chapter. Therefore, a significant portion of the text of page 4 of this document is enlarged and a transcription of that text is provided. This particular text segment discloses that the Mark II was susceptible to use with either a 9 kg U^{235} or 2 kg plutonium active and would produce an explosion equivalent to 1,000 tons of TNT. The enlarged text of that page reads thus:

Assume 9 [kg] “25” by Jan 1 [1945], 1 test end of Jan, 1,000 T – “B” damage 2-5 sq miles (goal of 10 sq miles)

Assume 9 “25” Feb 15, 1 gadget Mar 1

Assume 50 “25” by July 1, 1 test & 1 gadget Mar 15; 1 gadget April 15; 1gadget June 1; 1 gadget July 1 [5 gadgets at 9 kg U^{235} each = 45 kg]

Assume 2 “49” [plutonium] by Jan 1, one test Jan

Assume 10 “49” by July 1, one test, 4 gadgets by July 1

1 Gun gadget by Aug 1 in the bag, “B” damage 20-40 sq. miles

If [text illegible] “25” by [one word illegible], then 1 G every 6-8 weeks

"Historical Note. Written July 27, 1944.

James Conant, "Historical note. Written July 27, 1944"

James Conant. Reports events of 17 July 1944.

On the afternoon of **17 July 1944** James Conant in conversation with Los Alamos Laboratories Director J. Robert Oppenheimer urged that a test of the Mark II be conducted "as soon as possible" because, Conant said, the Mark II and was "almost a sure way" to produce a "moderate" nuclear fission explosion but, Conant added, a test of the Mark II might yield "only a few hundred tons of TNT equivalent." A successful test of the Mark II, Conant urged Oppenheimer, would permit Los Alamos "to put Mark II on the shelf" and development of more powerful bombs at Los Alamos could proceed "with less nervousness." Oppenheimer agreed a test of the Mark II was "a distinct possibility" but he told Conant "it was too early." The Port Chicago Naval Magazine explosion occurred several hours later, the evening of 17 July 1944.

Richard Hewlett's one paragraph that discloses information about the Mark II is derived from information exchanged in a 17 July 1944 conversation, at Chicago, Illinois, between Atomic Bomb Military Policy Committee alternate member, Harvard University President James B. Conant and Los Alamos Laboratories Director J. Robert Oppenheimer. The substance of that conversation is reported in James Conant's "Historical Note. Written July 27, 1944". Hewlett's abstract and paraphrase of that "Historical note" reads thus:

"Those July [1944] days at Los Alamos were on the discouraging side. With the gun method out for plutonium, implosion remained the only hope for using the Hanford [plutonium] production. When Conant talked privately with Oppenheimer at the Chicago conferences, he found him pessimistic about the chances of developing it quickly. Conant suggested that the laboratory make plans for a low-efficiency implosion bomb suitable for both uranium 235 and plutonium. It seemed to him an almost certain way of utilizing some atomic energy, even if only the equivalent of a few hundred tons of TNT. Should the Los Alamos staff develop this bomb to the point where it seemed a fairly sure thing, they could set it aside as Mark II (the uranium gun bomb being Mark I) and go to work with less nervousness on Mark III, an implosion weapon that would require less metal and be more powerful. Oppenheimer agreed that this was a distinct possibility but thought it too early to tell."

Richard Hewlett's text provides this July 1944 information about the Mark II:

> Mark II was a low-efficiency implosion bomb susceptible to use with either U^{235} or plutonium. James Conant considered the Mark II an almost certain way to produce a nuclear fission explosion that would yield a minimum energy equivalent to a few hundred tons of TNT. Mark II was in development at Los Alamos, but not yet so sufficiently advanced that the bomb could be considered a fairly sure thing. If Los Alamos would so sufficiently develop the Mark II that it would seem a fairly sure thing, Mark II could be set aside and Los Alamos could work on a more powerful bomb, the Mark III, with more confidence. J. Robert Oppenheimer agreed that development of the Mark II was a distinct possibility, but he "thought it too early to tell." Although the Mark II would require more uranium or plutonium metal than the Mark III, Mark II would be a less powerful bomb.

Hewlett's abstract and paraphrase of Conant's "Historical note" does not, however, report the most important information about the Mark II that Conant's "Historical note" does disclose:

In conversation with Oppenheimer at the University of Chicago, several hours before the Port Chicago explosion, James Conant urged Oppenheimer that a test of the Mark II be conducted "as soon as possible." The Mark II, Conant said, "seemed to be almost a sure way of getting some atomic energy released." Conant's "Historical note" specifically characterizes the utilization of atomic energy that could be achieved by Mark II as a "moderate explosion." Conant further told Oppenheimer on 17 July that a successful test of the Mark II, "even if the resulting explosion were only a few hundred tons of TNT equivalent," would permit a Los Alamos decision that the Mark II could be "put on the shelf." And a successful test of the Mark II, Conant told Oppenheimer, would permit Los Alamos to "work on a Mark III with less nervousness," which is to say the theory of large scale nuclear fission weapons would have been proven by that test of the Mark II. Conant also reported in his "Historical note" that Oppenheimer responded "it was too early" to expect a test of the Mark II, but a test of the Mark II “was a distinct possibility.” At 10:30 P.M. Pacific War Time Zone—00:30 A.M. Central War Time Zone, 18 July in Chicago, Illinois—the Port Chicago Naval Magazine exploded. Mark II had been tested.


Eckhart Hall, University of Chicago

Some information about that 17 July 1944 meeting in Eckhart Hall on the University of Chicago campus during which Conant and Oppenheimer discussed the Mark II is disclosed by a SECRET 11 July letter to Conant from University of Chicago Metallurgical Laboratory Project Director Arthur H. Compton. The letter, signed by Associate Project Director Norman Hilberry and received by James Conant 14 July, reads thus:

> “The next meeting of the Project Advisory Board will be held Monday evening July 17, 1944 at Eckhart. We are arranging for dinner at 6:00 o’clock preceding the meeting. The agenda will consist of two items:
>
> 1. Post-war plans for the Project as a guide for present changes in Project policy and organization.
>
> 2. The importance of light water moderated units in the over-all Project program and the effect on transfers of associated personnel required if the program is to be pushed.”

I here provide a transcription of James Conant’s manuscript, “Historical note. Written July 27, 1944”; the two manuscript pages of this document are also here reproduced.

“Historical note. Written July 27, 1944.

“On Monday July 17, 1944 [end of the line unreadable] conferences were held in Chicago involving the following people: A. H. Compton, J. R. Oppenheimer, C. [Charles] A. Thomas, J. B. Conant. And a special meeting in the evening attended by the above and Dr. Fermi & Gen. Groves & Col. [Colonel Kenneth D.] Nichols. The disquieting prospect first discovered [1 word unreadable] JBC by JRO on the visit to L. A. [Los Alamos] on July 4, (and confirmed by experiments reported on teletype of July 11, 1944 attached) was considered. It was concluded that the evidence was so clear that '49' [plutonium] prepared at Hanford could not be used in the gun method of assembly that all work on the purification of '49' & on the '49' gun should be dropped (see attached letters).

"On Tuesday the decision to discontinue the chemical work [on plutonium] was announced by A. H. C. and C. A. T. to the group leaders at Chicago in somewhat cryptic terms. (The true story undoubtedly had leaked all around the shop, however!)

"Dr. Oppenheimer was not very optimistic about a speedy resolution of the implosion method which is now left as the only hopeful way of using 49. JBC in conversation with JRO urged that as soon as possible, plans be layed for [SENSITIVE INFORMATION DELETED] with moderate explosion as this seemed to be almost a sure way of getting some atomic energy released even if the resulting explosion were only a few hundred tons of TNT equivalent. If this could be considered a fairly sure bet it could be put on the shelf as 'Mark II' (the gun for '25' [U^{235}] being Mark I) and people could work on a Mark III using [SENSITIVE INFORMATION DELETED] and correspondingly sure U. ε. with less nervousness. JRO said it was too early but this was a distinct possibility.

"Attached papers deal with this and related problems and status of work in July 1944."

In August 1998, in consequence of an active intercession by the former Secretary of Defense, Stanford University Professor William J. Perry, the National Archives at College Park, Maryland, took action on a Freedom of Information Act (FOIA) request I had made two years earlier to obtain the complete text of Conant's "Historical note." The National Archives retained the classification of the two instances of redacted text which appear in the "Historical note" citing DOE classification codes NWDD 961083-1, DOE b(3) and DOE d(3).

The National Archives also reported that those papers which are cited in the concluding paragraph of Conant's "Historical note," and which are said there to be attached, could not be located. Specifically, the National Archives reported that Document 2 of the FOIA request, namely, "Attached papers deal with this . . ." was not located "in the withdrawn items or the open files."

We now consider the descriptions of the Mark I, Mark II and Mark III bombs, and the differences between them, that Conant discussed in conversation with Oppenheimer 17 July 1944 and ten days later recorded in this “Historical note.” Those differences provide information about the design, technology and state of development of the Mark II by 17 July 1944.

Mark I.

Conant’s “Historical Note" identifies the Mark I as “the gun for ‘25’ [U^{235}]”— the gun-assembly Little Boy bomb. In earlier chapters we established that the Mark I utilized a highly enriched uranium active—uranium enriched to 90 or 93 per cent U^{235}. Conant’s “Historical note” confirms that the Mark I was not susceptible to use with plutonium; he wrote, “ ‘49’ [plutonium] prepared at Hanford could not be used in the gun method of assembly.”

Mark II.

Richard Hewlett reported in *The New World* that the Mark II was “an implosion bomb,” but he does not provide any documentary reference that we might review to confirm his attestation that the Mark II was an implosion bomb. Conant’s “Historical note” does not disclose that the Mark II was an implosion bomb, nor does Conant’s “Findings of Trip to L. A. 4 July 1944” disclose that the Mark II was an implosion bomb. As will be shown below, the Mark II was in fact an implosion bomb.

Hewlett also reported that the Mark II was a “low-efficiency” bomb, but again he does not provide any documentary reference that we might review to confirm that the Mark II was a low-efficiency bomb, nor does he identify the standard of comparison which determined his assessment that the Mark II was a low-efficiency bomb.

The efficiency of a nuclear fission bomb is expressed as that percentage of a bomb’s available fissile atomic nuclei that will fission before the bomb’s active material is so sufficiently heated, and consequently expanded, by the release of fission energy that the nuclear fission chain reaction ceases. A fission bomb would achieve 100 per cent efficiency if every available fissile nucleus did fission before the fission chain reaction ceased. We are able to use the Mark I bomb detonated at Hiroshima as a standard to which the efficiency of the Mark II can be compared.

We have seen that the complete fission of 1 kg U^{235} would produce an energy of explosion equivalent to the explosion of 22,000 tons of TNT. Detonation of the Mark I at Hiroshima, which employed a 50 kg U^{235} active, produced an energy of explosion equivalent to 12,500 tons of TNT. The complete detonation of 50 kg U^{235} would have produced an energy of explosion equivalent to 1,100,000 tons of TNT; therefore the efficiency of the Mark I bomb is calculated to have been 1.14 per cent. Current designs of gasoline automobile engines operate in the range of 25-28 per cent fuel efficiency, or less.

In his “Findings of Trip to L. A. July 4, 1944” Conant forecast that the Mark II, with a 9 kg U^{235}, would yield an energy of explosion equivalent to 1,000 tons of TNT. The complete fission of 9 kg U^{235} would produce an energy of explosion equivalent to 198,000 tons of TNT. The efficiency of the Mark II, with a 9 kg U^{235} active, is therefore calculated to have been 0.51 per cent. The Mark II, with an efficiency of 0.51 per cent, was a low-efficiency bomb in a comparison with the 1.14 per cent efficiency of the Mark I.

Hewlett also reported in *The New World* that the Mark II was suitable for both U^{235} and plutonium, but he does not provide any documentary reference that we might review to confirm his attestation that the Mark II was suitable for both U^{235} and plutonium, and Conant’s “Historical note” does not name the fissile material that could be utilized by the Mark II. Conant’s “Findings of Trip to L. A. July 4, 1944” does, however, report that the Mark II, with either a 9 kg U^{235} active or a 2 kg plutonium active, would yield an energy of explosion equivalent to 1,000 tons TNT.

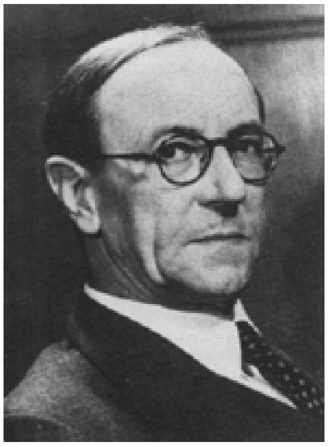
Close analysis of the text of Conant’s “Historical note” does in fact disclose that the Mark II was designed to utilize U^{235} and, more specifically, that the Mark II was designed to use slightly enriched uranium, rather than the highly enriched uranium for which the Mark I was designed. The information that the Mark II was designed to use a slightly U^{235}-enriched uranium active is gleaned from the text of the Conant’s “Historical note” which reports that, given a successful proof of the Mark II, “people could work on a Mark III using [SENSITIVE INFORMATION DELETED] and correspondingly sure U.ε. with less nervousness.”

<u>Note</u>: Current terminology in the commercial nuclear power reactor industry defines slightly enriched uranium fuel to be 2-5 per cent U^{235}, highly enriched fuel to be 20-30 per cent U^{235}, and fully enriched fuel to be greater than 90 per cent U^{235}. In preceding chapters, here, and hereafter I use the terms “slightly enriched uranium” and “highly enriched uranium” as those terms were current in Manhattan Project usage: respectively, 20-30 per cent U^{235} and 90-93 per cent U^{235}.

This text of Conant’s “Historical note” discloses a fundamental difference between the Mark II and Mark III, which difference enables us to determine that the Mark II was susceptible to use with a slightly enriched uranium active. In a comparison with the Mark II, Conant wrote that the Mark III would use the “correspondingly sure U.ε.” Readers acquainted with the fundamentals of nuclear physics will immediately recognize that the Greek alphabetical character epsilon, ε, is the symbol that designates the “fast fission factor” in highly enriched uranium, and that Conant here distinguishes between the Mark III the efficiency of which would depend on operation of the fast fission factor, necessarily, in a highly enriched uranium active, and the Mark II the efficiency of which would <u>not</u> depend on operation of the fast fission factor and, therefore, necessarily, a slightly enriched uranium active. All other readers will require an explanation of the fast fission factor (ε).

Fast fission

The neutron is an atomic particle that carries neither a positive nor a negative charge—the neutron is neutral. The neutron was first identified in 1932 by the British physicist James Chadwick who received the 1935 Nobel Prize in physics for that discovery. Neutrons produced by the nuclear fission reaction are the essential source of neutrons available to sustain a nuclear fission chain reaction in either a uranium-fueled nuclear power reactor or a uranium fission bomb. The fission of one U^{235} nucleus produces an average of 2.5 free neutrons. Because the average number of free neutrons produced by the fission of one U^{235} nucleus is greater than the one neutron expended to induce that fission, a nuclear fission chain reaction is feasible.



James Chadwick
1891 - 1974

The greatest number of neutrons produced by the nuclear fission reaction begin their journey in their own minute space as fast, high energy neutrons with an average kinetic energy in the range of 1 million electron volts (1 MeV). In early February 1939 the Danish physicist Niels Bohr recognized that the principal isotopes of natural uranium, U^{235} and U^{238}, must have different fission properties. He predicted, in publication, that the least abundant uranium isotope, U^{235}, would be easily enough destabilized to be fissioned by slow neutrons, namely, a neutron that has a kinetic energy no greater than 1eV—one million times less than the original energy of the greatest number of neutrons produced by the fission process.

The initial high energy of a fission-produced neutron can, however, be reduced or moderated to the energy most likely to induce fission in the U^{235} nucleus, which is to say, reduced to slow or thermal neutron energy. The predominant isotope of natural uranium is the non-fissionable U^{238} isotope. Two possible effects result from the collision or impingement of a high energy fast neutron with a U^{238} nucleus. The majority of high energy neutrons (higher than 1 MeV) that collide with a U^{238} nucleus are captured by that nucleus, and those captured neutrons are lost to the process of a nuclear fission chain reaction. Alternatively, a high energy neutron that collides with U^{238} nucleus may not be captured but will be partially slowed by inelastic scattering from that nucleus. That scattered neutron is somewhat energy moderated, but to effect fission of a U^{235} nucleus that partially energy-moderated neutron must be reduced to slow energy by elastic scattering from collisions with lighter nuclei that may be naturally present or may be introduced artificially. When a neutron collides elastically with another nucleus at rest in the medium, it transfers some of its energy to that nucleus. The maximum transfer of energy occurs when the target nucleus is comparable in mass to the impinging neutron.

Among all atomic nuclei, the mass of the hydrogen nucleus is most comparable to the mass of the neutron. Water (H_2O), which consists of two hydrogen atoms and one oxygen atom, is a good neutron energy moderator, but the particular hydrogen isotope (protium) of which the water molecule is composed has a fairly high propensity to capture and hold an impinging neutron, thereby removing those captured neutrons from the fission process. Three isotopes of hydrogen are known. The most abundant is protium (H, with a single proton) followed by deuterium (D, with one proton and one neutron) and the least abundant tritium (T, with one proton and two neutrons).

Of the three hydrogen isotopes, deuterium (also known as the deuteride isotope of hydrogen) manifests the least propensity to capture and hold an impinging neutron, and therefore offers the greatest probability, among the hydrogen isotopes, that an impinging neutron will be elastically scattered, rather than captured, and an energy reduction accomplished. After a series of, on average, 18 elastic collisions an initially high energy, fast neutron is moderated to slow or thermal energy. So, if the deuteride hydrogen isotope, deuterium, could be separated from natural hydrogen, that isotope would be the best possible neutron energy moderator.

For a uranium fission bomb with a slightly U^{235}-enriched active, a significant percentage of the U^{238} nuclei present in natural uranium would have been removed from the active material by isotope separation, which would significantly reduce the number of U^{238} nuclei in the active and, therefore, reduce the number of fast neutrons lost to the fission chain reaction by U^{238} capture. Thereby, a greater number of fast neutrons produced by U^{235} fission would be available to be elastically scattered by collision with a deuterium nucleus and, in consequence, moderated to the slow neutron energy most effective to sustain the bomb's U^{235} nuclear fission chain reaction.

In 1921 Harold Clayton Urey (1893-1981) entered the University of California and in 1923 was awarded the degree of Ph.D. in Chemistry, by which time he was well acquainted with J. Robert Oppenheimer at the Berkeley campus of the University of California. Urey spent the following year in Copenhagen at Niels Bohr's Institute for Theoretical

Physics and then returned to Johns Hopkins University as an Associate in Chemistry. In 1929 he was appointed Associate Professor in Chemistry at Columbia University. In 1931 he devised a method for the concentration of any possible heavy hydrogen isotopes by the fractional distillation of liquid hydrogen, which led to his discovery of deuterium and the Nobel Prize in Chemistry in 1934 for that discovery. With E. W. Washburn, Urey then quickly evolved the electrolytic method for the separation of hydrogen isotopes. During the period 1940-1945 Urey was Columbia University's director of war research.

Harold Clayton Urey
1893-1981

During early February 1939 Niels Bohr had predicted, in publication, that the least abundant uranium isotope, U^{235}, would be easily enough destabilized to be fissioned by slow neutrons. On 5 February 1939, J. Robert Oppenheimer wrote to the physicist George Uhlenbeck, then a visiting professor at Columbia University, "I think it really not too improbable that a 10 cm [centimeter] cube of uranium deuteride (one should have something to slow the neutrons without capturing them) might very well blow itself to hell."

J. Robert
Oppenheimer to
George Uhlenbeck,
5 February 1939

In this 5 February 1939 letter to Uhlenbeck, Oppenheimer first proposed what Los Alamos would develop to be the low-efficiency Mark II uranium hydride bomb, in which the deuteride hydrogen isotope was used to moderate the energy of fast fission neutrons to slow (thermal) energy neutrons in a slightly U^{235}-enriched active.

There are, therefore, so far three men identified who can be said to have been principal to the development of the Mark II: James Chadwick, who discovered the neutron by which artificially induced nuclear fission was achieved in the Mark II; Harold Urey, who discovered the deuteride hydrogen isotope by which the neutron energy moderation requisite to the nuclear fission chain reaction in the Mark II was achieved; and J. Robert Oppenheimer who first proposed that a slow neutron fission of uranium deuteride "might very well blow itself to hell."

By March 1940 British experiments showed that both fast and slow neutrons would induce fission in U^{235}, but to accomplish fast neutron fission of uranium the U^{235} isotope would need to be separated from the U^{238} isotope to the degree of 90 to 93 per cent. The result of that concentration of U^{235} would increase the proximity of U^{235} nuclei and that proximity would permit U^{235} fission by fast fission neutrons. Fission of highly enriched uranium by fast neutrons was, by 1940, designated the fast fission factor and identified by the Greek alphabetical character epsilon, **ε**. In March 1940 refugee German scientists Otto Frisch and Fritz Peierls, living in England, proposed to the British government that an atomic bomb would be feasible if the least abundant and most readily fissionable of the uranium isotopes, U^{235}, were separated from its occurrence in natural uranium, which would eliminate the depletion of neutrons in the system by non-fission neutron capture by nuclei of the U^{238} isotope. An accumulation of essentially pure U^{235}, Frisch and Peierls argued, would be susceptible to fission entirely by fast neutrons. That proposal was the basis of the Mark I gun assembly bomb and the Mark III.

"Report to Gen. Groves on Visit to Los Alamos on August 17, 1944"
James Conant's report to General Groves.

Report to Gen. Groves on Visit to Los Alamos on August 17, 1944

On **17 August 1944** in this memorandum to Gen. Groves, James Conant reported the decision had been made at Los Alamos "that Mark II should be put on the shelf for the present. If all other implosion methods fail, Mark II can be taken off the shelf and developed for combat use in 3 or 4 months time." He additionally reported in this memorandum, "If all other implosion methods fail, it may be necessary to work on the Mark II to see if at least the upper limit of effectiveness [SENSITIVE INFORMATION DELETED] cannot be raised somewhat." In conclusion of this memorandum, in his "Note on explosive damage," Conant informed the General, "It was agreed that Class B damage was damage beyond repair. For the phrase to be of significance the type of structure must also be named. It was agreed that for dwelling houses the area of 90% Class B damage was about as follows for 1,000 tons of TNT: 90% Class B damage = 0.5 mile radius = 0.75 square mile."

The reader will recall that on the afternoon 17 July 1944 (reported 27 July 1944) Conant stipulated in conversation with Oppenheimer that a decision to put the Mark II on the shelf would require that a successful test of the Mark II had been accomplished. Because on 17 August 1944

Conant reported the decision "that the Mark II should be put on the shelf for the present," the reader may infer that the stipulated successful test of the Mark II had been accomplished in the period between the afternoon of 17 July and 17 August 1944.

The reader will recognize that Conant's 17 August 1944 "Note on Explosive Damage" refers to explosive damage that "was damage beyond repair" and that "the area of Class B damage was . . . 0.5 mile radius." The reader will reasonably want to know what particular explosive damage Conant reports "was damage beyond repair" and what particular explosive damage Conant reports did occur within a 0.5 mile radius, where and when. The explosive damage to which Conant refers is not named in the declassified portions of this document, and informally the National Archives, College Park, Maryland, has told me that the explosive damage to which Conant refers is not named in the classified portions of this document.

Earlier chapters of this book have established that the radius of Class B damage that did result from the Port Chicago Naval Magazine explosion was 2,500 feet, which is a 0.5 mile radius (1 mile = 5,280 feet; 0.5 mile = 2,640 feet). Specifically, Los Alamos physicist Ensign George T. Reynolds, USNR, wrote in his 27 July 1944 "Report on Port Chicago, July 20-24, 1944": "From all observations, smoothing out directional effects, the average B radius is considered to be 2500 feet."

Ensign Reynolds' "Report on Port Chicago, July 20-24, 1944" is Enclosure (C) of Capt. William Parsons' 4 August 1944 memorandum to Atomic Bomb Military Policy Committee member Adm. William R. Purnell, "Port Chicago Disaster: Second Preliminary Report."

The decision made at Los Alamos, reported by James Conant on 17 August 1944, to put the Mark II on the shelf was made specifically in consequence of the Port Chicago explosion. The upper limit of the Mark II's effectiveness was known specifically in consequence of the Port Chicago explosion. James Conant's 17 August 1944 report to Gen. Groves that the Mark II could be developed for combat use in 3 or 4 months time was made specifically in consequence of the Port Chicago explosion.

Previous chapters have shown that the fireball and column of flame that did result from the Port Chicago explosion were typical of a nuclear fission explosion and could not have been generated by the explosion of the 1,750 tons TNT and torpex charge weight of munitions emplaced upon the Port Chicago Naval Magazine pier and loaded as cargo aboard the Liberty ship *E. A. Bryan,* which was moored to the Port Chicago Naval Magazine ship loading pier.

Mark II: The autocatalytic uranium hydride lateral implosion experimental device.

Vice Admiral Frederick L. Ashworth, USN, Ret.

In spring 1993 Los Alamos National Laboratory Archivist Roger Meade advised me that Adm. Frederick L. Ashworth was resident in Santa Fe, New Mexico, and Meade recommended I make arrangements to meet the admiral to discuss my preparation of a biography of Rear Admiral William S. Parsons. I was then in the middle of seven years at Stanford University and there employed in one of the molecular biology research laboratories in the Department of Biological Sciences. The Stanford University libraries hold one of the most comprehensive collections of Manhattan Project historical literature and materials of any university library. That collection was assembled to provide research materials of particular interest to two Stanford history professors and their students. In earlier years, the history of the Manhattan Project had been a defined curriculum emphasis for students in the Stanford History Department.

In summer 1993 I met Adm. Ashworth to discuss my proposal to research and write a biography of Adm. Parsons. Captain Parsons, before his assignment to Los Alamos, had been assigned to the office of National Defense Research Committee Chairman Vannevar Bush, to coordinate NDRC and Navy development of the proximity fuze, also known as the VT (variable time) fuze. (See: Ralph B. Baldwin, *The Deadly Fuze: Secret Weapon of World War II.* San Rafael, California: Presidio Press, 1980) During that time Commander Ashworth had been Capt. Parsons' "fly boy," which is to say that usually wherever Capt.

Parsons was flown by Navy aircraft, Commander Ashworth was the pilot.

Captain Parsons arrived on duty at Los Alamos in May 1943 but when he began his assignment there he was persuaded by Gen. Groves not to travel by air and, with that restriction, Commander Ashworth's duty as Capt. Parsons' pilot was not required. However, three months after the Port Chicago explosion, in October 1944, Commander Ashworth was assigned duty at Los Alamos where he reported to Capt. Parsons and was his deputy. Their work together there, built upon their several years of prior acquaintance, enabled a remarkable association in the Project and a friendship that continued until Adm. Parsons' death in 1953. On 6 August 1945 Capt. Parsons was the bomb commander on the Hiroshima combat mission, and three days later Commander Ashworth was the bomb commander on the Nagasaki combat mission. In summer 1993 Adm. Ashworth's knowledge of Adm. Parsons, the man and naval officer, was the most comprehensive of any person living.

Tinian Island, prior to 6 August 1945. Left to right: Norman Ramsey; Capt. William S. Parsons, USN; Edward Doll; Col. Ernest Kirkpatrick, USA; Commander Frederick L. Ashworth, USN

Admiral Ashworth agreed that a biography of Adm. Parsons was very much needed. The Manhattan Project historical literature published by 1993 did only in several instances briefly mention the role of the United States Navy in the Project. That deficiency of the historical record existed because Adm. Parsons, Adm. Ashworth, nor the Navy service had written any detailed account of the Navy contributions to the Project. The Army had caused to be written and published a thorough account of the Army participation in the Project; Gen. Groves' autobiographical account of his role in the project had been published.

Another contributing cause of that deficiency lies in the fact that the memoirs of those civilian scientists who were associated with the Project assert mainly the authors' contributions to the project and ignore or minimize those aspects of the project of which they were ignorant or in which they could claim no principal credit. Furthermore, those academic historians whose publications have largely influenced the public perception of the Project history have been more sympathetic in common collegial association with the civilian scientists associated with the Project than with those few members of the Navy who were associated with the Project, whose appellations were Capt. and Comdr. rather than the collegial Prof. and Dr.; that collegial bias has caused the Navy role in the Project to have been minimized by academic historians, where it has not been ignored.

The needful task of writing the first biography of Adm. Parsons eventually was done by Albert Christman who, as a civilian Navy employee, had made a good start in the necessary research during the late 1960s and early 1970s at the China Lake Naval Weapons Station in California but soon thereafter abandoned the work. Following 1993 Christman was either persuaded or directed to complete that biography, *Target Hiroshima,* published by the U.S. Naval Institute Press in 1998.

In 1981, I had been admitted to the Archives at Los Alamos National Laboratory to review the 7 linear feet of documents, declassified at my request, held by the Archives that pertain to the Port Chicago explosion; prominent among those documentary resources are the extensive reports and analyses of the Port Chicago explosion transmitted by Capt. Parsons to Adm. Purnell from 24 July through 16 November 1944.

When Adm. Ashworth and I concluded our discussion of my proposal to write a biography of Adm. Parsons, I turned our conversation to the Port Chicago explosion, which necessarily would be an element of that biography because the principal reports and analyses of that explosion had been prepared by Capt. Parsons, and the major component analyses of those reports had been prepared by Los Alamos scientists Maurice M. Shapiro and Ensign George Reynolds under Capt. Parsons' direction.

I asked Adm. Ashworth if he had been aware of the Port Chicago explosion, at that time; he replied he had not arrived at Los Alamos until three months later. I explained that I had made a study of all the materials descriptive of the explosion that I had been able to locate during 13 years, including those reports that Capt. Parsons had transmitted to Adm. Purnell, and the "History of 10,000 ton gadget" which asserted that the ball of fire generated by the Port Chicago explosion had been typical of a nuclear fission explosion. I explained that I had also located three pertinent Manhattan Project documents, all authored by James Conant and dated from 4 July through 17 August 1944.

On 4 July 1944, I said, Conant had informed Gen. Groves that a bomb, which Conant named the Mark II, was available to the Joint Chiefs of Staff for the purposes of operational planning. In that report, I said, Conant forecast that the Mark II, with either a 9 kg U^{235} or 2 kg plutonium active, would yield an energy of explosion equivalent to 1,000 tons of TNT. In that report Conant had also informed Gen. Groves that the Mark II would require one proof firing before it could be available for use against the enemy.

On 17 July, I said, Conant had urged Oppenheimer to conduct a test of the Mark II as soon as possible, even if the energy yield were only equivalent to several hundred tons of TNT. A successful test of the Mark II, Conant then told Oppenheimer, would permit a decision by Los Alamos to put the Mark II on the shelf, and work on the more powerful bombs could proceed with less nervousness.

On 17 August 1944, I said, Conant informed Gen. Groves that the Mark II could be developed for combat use in 3 or 4 months times, but Conant reported that Los Alamos had decided that the Mark II should be put on the shelf unless all other implosion methods failed. On 17 August, I said, Conant had reported the upper limit of effectiveness for the Mark II to Gen. Groves, which he felt could be somewhat raised. I said all the information provided by Conant's 17 August report to Gen. Groves had been determined in specific consequence of the Port Chicago explosion.

Taken together, I said, those three documents and the "History of 10,000 ton gadget" had persuaded me that a proof of the weapon that James Conant identified as the Mark II had been the cause of the Port Chicago explosion. However, I said, I had been unable to learn more about the Mark II than it was a low-efficiency implosion design suitable for use with either a 9 kg U^{235} or 2 kg plutonium active and that the predicted energy yield of the Mark II ranged between a few hundred tons of TNT and 1,000 tons.

I then asked Adm. Ashworth if he were able to provide more specific information about the design and technology of the Mark II than I had discovered. In response, Adm. Ashworth identified the Mark II to have been "the autocatalytic uranium hydride lateral implosion experimental device."

Lacking Adm. Ashworth's specific identification of the Mark II, I would not have been able to develop a comprehensive history of the development of that weapon, because nowhere in the presently declassified Manhattan Project documentary materials is that identification made, nor can that identification be deduced or inferred.

Frederick Lincoln Ashworth graduated from the United States Naval Academy and completed the Naval Postgraduate School course in ordnance engineering shortly before the Japanese attack on Pearl Harbor in 1941. After service in the Pacific Theater of Operations, then as Capt. Parsons' pilot, and following his assignment at Los Alamos, in company of then Commodore Parsons, Ashworth was assigned to Washington to lead the Navy into the nuclear age. Ashworth participated in the July 1946 Bikini atomic tests as Adm. Parsons' deputy. For the period August 1955–September 1957, Capt. Ashworth was Commander of the Naval Ordnance Test Station (NOTS), China Lake, California. After leaving China Lake, and elevated to the rank of rear admiral, Ashworth became Commander of the Sixth Fleet. Elevated to the rank of vice admiral, Ashworth was named Deputy Commander in Chief of the Atlantic Fleet.

Lamentably, a full account of Adm. Ashworth's life and United States Navy career has not been written. Two important filmed interviews with the admiral were conducted by the late historian Stanley Goldberg for the Smithsonian Institution and are available in the Smithsonian Videohistory Collection, "The Manhattan Project" (RU 9531. Collection Division 5: "Alberta": Session Seventeen, June 5, 1990, and Session Eighteen, June 6, 1990). Jerry Miller's *Nuclear Weapons and Aircraft Carriers: How the Bomb Saved Naval Aviation.* (Washington, D.C.: Smithsonian Institution Press, 2001) is a very important history that provides some discussion of Adm. Ashworth's naval career. See also, Albert Christman's two-volume history of the China Lake, California, Naval Weapons Station (Naval Ordnance Test Station, NOTS): *Sailors, Scientists, and Rockets* (Washington, D.C.: U.S. Government Printing Office, 1971) and *The Grand Experiment at Inyokern* (Washington, D.C.: U.S. Government Printing Office, 1978).

Photographs and illustrations credits.

"James B. Conant with Vannevar Bush after witnessing the atomic bomb explosion at Trinity site, Alamogordo, New Mexico, 16 July 1945." I am confident this photo was taken by the light of the Trinity fireball. The illumination of this photo is certainly not that of a camera-mounted flash bulb exposure, because the illumination comes from above Conant's right shoulder rather from the photographer's straight-on position, as is evident by the orientation of Conant's shadow cast on the background. The orientation of Conant's shadow reveals that the source of illumination is some 30 degrees above the horizontal. Conant and Bush were at least 10,000 yards from the explosion. Both men face away from the source of illumination. Conant and Bush are pictured in a moment of solemn acknowledgment that the purpose of their endeavor, to produce a militarily-decisive atomic bomb, has been accomplished. Source: Massachusetts Institute of Technology Museum, photograph VB120, "Vannevar Bush with James B. Conant after witnessing the first atomic bomb explosion at Alamogordo, NM, July 16, 1945." Used with permission.

"James Conant, "Findings of Trip to L. A. July 4, 1944." Source: National Archives Microfilm Publications, "Bush-Conant File Relating to the Development of the Atomic Bomb, 1940-1945; Records of the Office of Scientific Research and Development Record Group 227," reel No. 1, frames Nos. 828-833.

"James Conant, 'Findings of Trip to L. A. July 4, 1944.' " Page 4 text enlargement. Source: National Archives Microfilm Publications, "Bush-Conant File Relating to the Development of the Atomic Bomb, 1940-1945; Records of the Office of Scientific Research and Development Record Group 227," reel No. 1, frame No. 830.

James Conant, "Historical note. Written July 27, 1944." Source: National Archives Microfilm Publications, "Bush-Conant File Relating to the Development of the Atomic Bomb, 1940-1945; Records of the Office of Scientific Research and Development Record Group 227," reel No. (unrecoverable), frames Nos. 112, 113.

"Eckhart Hall, University of Chicago." Source: University of Chicago.

"James Chadwick, 1891-1974." Source: University of California.

"Harold Clayton Urey, 1893-1981." Source: University of California.

"J. Robert Oppenheimer to George Uhlenbeck, 5 February 1939." Source: Smith, Alice Kimball and Charles Weiner, *Robert Oppenheimer: Letters and Recollections.* Cambridge, MA: Harvard University Press, 1980. Reference by the courtesy of Jonothan Logan.

James Conant, "Report to Gen. Groves on Visit to Los Alamos on August 17, 1944." Source: National Archives Microfilm Publications, "Bush-Conant File Relating to the Development of the Atomic Bomb, 1940-1945; Records of the Office of Scientific Research and Development Record Group 227," reel No. 8, frames Nos. 114-117.

"Tinian Island, prior to 6 August 1945." Left to right: Norman Ramsey, Project Alberta deputy director; Capt. William S. Parsons, USN, head of Project Alberta; Edward Doll, head of atomic bomb fuzing team; Col. Ernest Kirkpatrick, USA, coordinator of Project Alberta overseas construction; Commander Frederick L. Ashworth, USN, Alberta operations officer and Parsons' military alternate. Source: Courtesy of Morris Jepson, Capt. Parsons' electronics assistant on the 6 August Hiroshima combat mission.

Chapter 14

Mark II:
February 5, 1939 – August 24, 1943

As shown in Chapter 13, the Mark II was an implosion weapon susceptible to use with either slightly enriched uranium or plutonium as the active material. In his memorandum to General Leslie Groves, “Findings of Trip to L. A. [Los Alamos] July 4, 1944,” James Conant forecast that the Mark II with a 9 kg U^{235} active would produce a nuclear fission explosion equivalent to the detonation of 1,000 tons of TNT and, if detonated in an optimal air burst, would result in structural damage beyond repair (Class B damage) within an area of 2-5 square miles. James Conant’s 4 July 1944 memorandum to Gen. Groves advised the General that the Mark II was certain enough to be used by the Joint Chiefs of Staff for the purposes of operational planning, but Conant stipulated the Mark II would necessarily be once proof fired before the design could be available for use against the enemy. Conant’s 4 July 1944 memorandum informed the General that “present indications” permitted the forecast that 3-6 of the Mark II would be available before 1 July 1945 and for the six months following 1 July 1945 either 4 of the Mark I Hiroshima-type bomb or 20 of the Mark II would be available.

On 17 August 1944 James Conant reported to Gen. Groves that, in consequence of the Port Chicago Naval Magazine explosion of 17 July 1944, Los Alamos had agreed the Mark II should be put on the shelf unless other implosion methods of bomb assembly should fail of development, that the upper limit of effectiveness of the Mark II was known and could be somewhat improved, and that the Mark II could be developed for combat use in 3 or 4 months time.

Chapter 13 showed that J. Robert Oppenheimer first proposed a uranium deuterium nuclear fission bomb on 5 February 1939, and Chapter 13 showed that the active fissionable material of the Mark II was a compound of slightly U^{235}-enriched uranium metal and deuterium. Chapter 13 also disclosed that in summer 1993 Vice Admiral Frederick L. Ashworth, USN, Ret., in conversation with this author identified the Mark II to have been the autocatalytic uranium hydride (deuterium) lateral implosion experimental device.

Uranium hydride

Uranium metal and the isotopes of hydrogen, including the deuterium isotope (the deuteride), can be compounded in several different proportions; all compounds of uranium and hydrogen are uranium hydrides. In the language of chemistry, metal hydrogen compounds, in which the metal forms covalent bonds with hydrogen, are not properly called metals. Lithium deuteride and uranium deuteride, as examples of uranium hydrides, are nonmetals because the deuterium (H_2) in the compound exists in the -1 oxidation state while the metal exists in a positive oxidation state. A uranium metal deuteride compound is most efficiently accomplished at a temperature of 225˚ C in a refractory-lined, sealed container, usually steel, known in the metallurgical sciences as a “bomb”; the result is a fine, black, pyrophoric powder, UH_3. Because, being pyrophoric, UH_3 will spontaneously ignite in air a half dozen small fires each week in the uranium metallurgy work areas at Los Alamos were usual during spring and summer 1943 until methods had been developed to constrain the material’s pyrophoric nature.

Two men were responsible for development of the Manhattan Project uranium hydride program, Frank (F.H.) Spedding working at the Ames campus of the University of Iowa and Cyril Stanley Smith, who joined the Project at Los Alamos in April 1943 from the American Brass Company. Spedding’s group at Ames first produced a uranium hydride compound, originally thought to be UH_4; Spedding was also responsible for development of an industrial scale method of producing uranium metal, by which was accomplished World War II production of all the uranium metal necessary to Manhattan Project scientific and weapon purposes. Los Alamos learned of the possibility of large scale, controlled uranium hydride production in April 1943, apparently also a process developed by Frank Spedding.

When Cyril Smith arrived at Los Alamos his first undertaking was development of facilities and the technology there to produce uranium hydride in quantity which, because of its high concentration of neutron energy moderating deuterium, was to be the active material for the uranium deuteride weapon that Oppenheimer had proposed 5 February 1939 and which on 4 July 1944 Conant first identified as the Mark II. The first work in uranium metallurgy at Los Alamos, directed by Cyril Smith, was the preparation and powder metallurgy of uranium hydride. We are fortunate to have available Cyril Smith's previously inaccessible "Semimonthly Reports of the Metallurgy Group," to J. W. Kennedy, Nos. 1-14 for the periods ending July 15, 1943, to February 29 1944. These reports provide an extensively detailed account of the early Los Alamos work in uranium hydride and plutonium metallurgy. Smith's reports are reproduced in Edward F. Hammel's recollections, *Plutonium Metallurgy at Los Alamos, 1943-1945*. Los Alamos: Los Alamos Historical Society, 1998. Hammel served with scientific distinction, perceptive good humor and wit on the Board of Directors of the New Mexico Energy Research and Development Institution during the years of my affiliation with the Institute board.

Manhattan Project historian David Hawkins: the UH bomb

David Hawkins' *Manhattan District History: Project Y, the Los Alamos Project* (Volume I) provides the most succinctly accumulated details of the history of the developments that produced the Mark II. A concatenated abstract of all references in the Hawkins history to those developments is provided as Appendix B, but will be here summarized in abstract; paragraph numbers of the Hawkins history are omitted here but are provided in Appendix B.

Hawkins' text makes frequent mention of the "hydride gun." Until February or August 1944 consideration was given to the use of uranium hydride as the active material for use with the gun assembly bomb design (Mark I). Very little declassified information is available about the hydride gun program. Hawkins reports that development of the hydride gun continued, “until February 1944, by which time the hydride gun had been abandoned”; the U.S. Department of Energy Los Alamos history, *Critical Assembly. A Technical History of Los Alamos during the Oppenheimer Years, 1943-1945*, reports, “At Teller’s suggestion T-Division investigated uranium hydride . . . by August 1944, interest in the hydride gun had disappeared.”

There are, however, several evidences made available to me since 1982 which raise the possibility that a nuclear fission detonation of energy yield in the range of 50-100 tons TNT equivalent was achieved by a hydride gun assembly, probably using a modified 3”/50 Navy anti-aircraft gun equipped with an unrifled tube, at 21:00 hours on 26 December 1943 at the Alamogordo Bombing Range in New Mexico, on a playa in the vicinity of Oscuro Peak. Physical evidence of that test, if it occurred, is recognized in aerial photographs, seismic records, and Landsat thermal images of that area in which a circular scar of high thermal index may represent the area of thermally-fused sand which would have been the consequence of that detonation. Two Army veterans told the National Association of Atomic Veterans in 1982, or earlier, that 100 U.S. Army volunteers had been proximate to that detonation in slit trenches and in the open.

Short-wave radio transmissions intercepted by U.S. Army Intelligence in May 1944, broadcast from the U.S., and which detail the activities of a group of 12 Spanish Fascist espionage agents operating in the U.S., report that “a chemical explosion which reached a temperature of 1,000,000 degrees occurred at Alamogordo. We will all die.” The Spanish agent who made that report was, several weeks later, reported by his compatriots to have been shot and killed in Las Vegas—whether Las Vegas, New Mexico, or Las Vegas, Nevada, cannot be determined. Probably U.S. Army Intelligence caught him and he was summarily executed. This particular information was discovered by NHK News researchers working in Washington, D.C. and was made available to this author by NHK News executives in meeting at Thanksgiving 1982 in Santa Fe, New Mexico.

In 1984, New Mexico Institute of Mining and Technology geophysicists affiliated with the programs of the New Mexico Energy Research and Development Institute identified for this author the relevant Landsat images.

Several of David Hawkins' mentions of the uranium hydride bomb development are transcribed here:

> "Another virtue of the hydride program not mentioned in paragraph 4.13 was the interest taken in the preparation and fabrication of this material. Studies were begun, among the first undertaken by the metallurgists, in the art of preparing high density compacts of this material. The result was that although after a year or so it was known that the hydride would not yield an efficient weapon, this material could be easily fabricated, and was used in making experimental reactors.
>
> "Aside from the metallurgy of active materials—uranium hydride, uranium, and plutonium—several techniques were developed for the fabrication of materials with important nuclear properties, notably boron and beryllia. These were techniques of powder metallurgy, and the object in both cases was to attain the highest possible densities. The main pressure for the production of boron came again from the hydride gun program, for which it would be difficult to dispose a sufficient number of critical masses of hydride into gun and target.
>
> "In this connection the Laboratory undertook to procure large amounts of boron enriched in B^{10}, which constitutes about 20 percent of the normal boron. A method for the separation of B^{10} had been developed by [Harold] Urey, and was further developed by him at the request of the Los Alamos Laboratory. A pilot plant was constructed in the fall of 1943, to develop the method and to provide experimental amounts of the separated isotope. Early estimates (February 1944) set the needed production rate of the isotope at a figure comparable to the production of separated uranium. Plant construction was undertaken by Standard Oil of Indiana. Difficulties in construction and a decreasing probability that boron would be used in large amounts caused a decrease in the scheduled capacity of the plant by 25 per cent.
>
> "Even after there was reasonable assurance that a bomb made of hydride would not be used, and especially not a hydride gun, it was decided to maintain production of the B^{10} isotope because of its

potential usefulness in an autocatalytic bomb, if one could be developed.

“The attack on the many-velocity [neutron] problem had proceeded simultaneously with the work described above, in the sense of investigating methods by which the many-velocity problem could be reduced to a series of one-velocity problems. The problem posed itself naturally in connection with the investigation of the uranium hydride bomb, for in this case the energy degradation of neutrons from elastic collisions with hydrogen was one of the essential characteristics of the chain reaction. Quite early, methods were found for treating the hydride problem, with a continuum of velocities, under quite unrealistic assumptions, such as an infinite medium of core material . . . By July 1944, however, a method had been developed which was applicable to a spherical core and tamper.

“In the case of hydrogenous material it could not be assumed that neutrons were scattered isotopically. It was found however, semi-empirically, that this fact was adequately accounted for by the use of the transport cross section, as in the case of the all-metal diffusing medium.

“After the formation of the Uranium and Plutonium Metallurgy Group in April 1943, the work described below was done primarily in that group, and was placed in a separate group in June 1944. The first work in uranium metallurgy at Los Alamos was the preparation and powder metallurgy of its hydride. This compound had been successfully produced on the project by Spedding’s group at Ames, and the existence of the possibility of large scale, controlled production was learned of at Los Alamos in April 1943. The employment of the hydride in a bomb was still being seriously considered. Consequently, metallurgical investigations concerning uranium hydride were in order. The early literature identified the compound as UH_4 but primary work in the formation of the hydride indicated that UH_3 was closer to the true formula. That this was so was verified independently by the chemists.

“The metallurgical work was modified by bomb requirements with the result that methods of producing hydride in high density form and the elimination of the pyrophoric characteristic became important problems. Compacting of the hydride by cold pressing and hot pressing methods was attempted as well as the possibility of hydride formation under high pressures applied externally to the massive material being treated. This work generally led to the establishment of many control factors in the hydride formation process.

"The work on the pressure bomb method of producing high density hydride compacts was curtailed when success was achieved with the formation of uranium-plastic compacts. The research on the latter began during February 1944, the objectives being to prepare compacts in desired geometric shapes in which the hydrogen-to-uranium ratio varied. This feature could readily be accomplished by the employment of uranium powder and a suitable hydrogenous binding agent. It was also possible largely to eliminate the employment of the hydride and thus reduce the number of fires. In the early days of this work, a half dozen small fires a week were not unusual. The plastic bonding agents employed, among others, were methacrylate, polyethylene and polystyrene. Compacts were thus made with uranium-hydrogen compositions corresponding to UH_3, UH_4, UH_6, UH_{10} and UH_{30} which were used for various experiments by the physicists.

"The flow of beta stage enriched uranium received from the Y-12 plant was generally as follows: the material was received as a purified fluoride and reduced directly to metal. For hydride experiments the metal was converted to hydride and formed by plastic bonding. When hydride or metal experiments were completed, the material was returned for recovery, as in the meantime were crucibles, liners, and other containers that had been used in fabrication. Recovered solutions were converted to hexanitrate, extracted with ether, and precipitated as reduced oxalate. The oxalate was ignited to oxide and converted back to the original tetrafluoride."

Mark II: Autocatalytic fission bomb assembly

In chemistry, a **catalyst** is a substance that is usually present in small amounts relative to chemical reactants which, in a chemical reaction, modifies the rate of the chemical reaction. Especially a catalyst modifies the rate of a chemical reaction by an increase of the rate of a chemical reaction. **Catalysis** is the action of a catalyst which modifies the rate of a chemical reaction, especially as the action of the catalyst increases the rate of a chemical reaction. **Autocatalysis** is catalysis of a chemical reaction by one of the chemical products of the reaction. An autocatalytic chemical reaction does itself produce a catalyst which increases the rate of the chemical reaction in which it is present, which chemical reaction does produce more of the catalyst and, therefore, does create a condition favorable to continuation and increase of the chemical reaction.

Analogously, an angry argument among persons produces more anger, which is a catalyst that increases an already angry argument and promotes a condition favorable to continuation and augmentation of an angry argument. An angry argument can be said to be autocatalytic because it does, itself, create the catalyst that continues and increases the intensity of an angry argument.

An autocatalytic method of nuclear fission bomb assembly is one which, as it progresses, does itself create a chemical reaction that will increase a condition favorable to effectuate a nuclear fission chain reaction. However, in the case of the Mark II the generally comprehended definition of autocatalysis was reversed: Rather than an increase of a chemical reaction to augment a condition favorable to effectuate a nuclear fission chain reaction in the Mark II, a decrease of a chemical reaction was necessary to augment a condition favorable to effectuate a nuclear fission reaction.

Language is often a casualty of war but, as often, war is a catalyst to new idiomatic language constructs which may in time become catholic in use as, for example, a “Pyrrhic victory” for 2,000 years has denoted a military victory achieved with staggering loses, as occurred when Pyrrhus, the Greek King of Epirus, defeated the Romans in the Battle of Asculum (279 B.C.) but lost 3,500 men, including many of his ablest officers. When he was congratulated on the victory, Pyrrhus is reported to have said, “If we defeat the Romans in one more such battle, we shall be completely ruined.”

Specifically, in the static condition, inclusion of the boron-10 isotope (B^{10}) in the slightly U^{235}-enriched active material of the Mark II acted as a very efficient absorber of the occasional high energy neutrons produced in the Mark II active material as the result of the spontaneous fission of U^{235} nuclei. In the static condition, the inclusion of B^{10} in the Mark II active permitted the accumulation of a fissionable active greater than the critical mass. In the static condition, absent the effective B^{10} absorption of those spontaneously produced high energy neutrons in the Mark II U^{235} active, a spontaneous fission chain reaction (predetonation) in the Mark II active was certain to occur.

However, when deliberate detonation of the bomb would be initiated the action of B^{10} in the fissionable active as a high energy neutron absorber would inhibit the condition favorable to effectuate a nuclear fission chain reaction by absorption of high energy neutrons produced by and essential to continuation of the chain reaction. Therefore for operation of the Mark II, rather than an increase of a chemical reaction to augment a condition favorable to a fission chain reaction a decrease of a chemical reaction was required. Either a means could be devised to remove the neutron-absorbing B^{10} from the Mark II active as the detonation progressed or, alternatively, a means could be devised to suppress the efficiency of the B^{10} as a high energy neutron absorber as the detonation progressed.

The M.A.U.D. Committee report, July 1941

The first substantially detailed analysis of possible methods to develop a nuclear fission chain reaction as a source of power and as a weapon for military purposes was the British M.A.U.D. Committee report of July 1941. Within 30 days a copy of the M.A.U.D. report had been delivered by the British to the U.S. and was received by Director of the Office of Scientific Research and Development Vannevar Bush. At that time Adm. William S. Parsons (then Commander Parsons) was the Navy Bureau of Ordnance liaison officer with the Naval Research Laboratory; Parsons' office at the NRL adjoined that of Ross Gunn. (Ross Gunn, see Chapter 12.) Commander Parsons was then working directly with Vannevar Bush to coordinate Naval Research Laboratory, OSRD and NDRC programs in development of the proximity fuze.

The M.A.U.D. report that Vannevar Bush had received from the British in August 1941 proposed a gun assembly atomic bomb design. Parsons, then working directly with Vannevar Bush, was the Navy's most knowledgeable and experienced experimental ordnance officer; particularly he was the Navy's most knowledgeable and experienced officer in the design and manufacture of naval guns. At the end of the next four years Capt. Parsons at Los Alamos had successfully developed the most powerful gun that, to that date, had ever been made, the Mark I gun assembly atomic bomb detonated in combat at Hiroshima, 6 August 1945.

I have discovered no documentary evidence to conclusively establish that in August 1941 Vannevar Bush provided the M.A.U.D. report to Commander Parsons for his evaluation of the gun assembly atomic bomb proposed by that report, but whom else would Vannevar Bush turn to for evaluation of that British proposal for a gun assembly atomic bomb than the Navy's most accomplished ordnance officer, with whom Bush was well acquainted and whose office at the Naval Research Laboratory was just several miles down the Potomac River from the Government center of the District of Columbia where Vannevar Bush worked?

I expect when more complete research inquiry is made into Adm. Parsons naval career by future scholars the history will show that his involvement with the development of the Mark I gun assembly bomb began in August 1941 and at that time Vannevar Bush asked him to appraise the British proposal for a gun assembly atomic bomb.

Historian Al Christman has written in his biography of Parsons, *Target Hiroshima,* "In March 1943 Parsons knew nothing of the chain of nuclear events in Vannevar Bush's life that were about to encircle him as well."

We do know that Vannevar Bush, as well as Adm. Purnell of the Atomic Bomb Military Policy Committee, had recommended Parsons to Gen. Groves when the Manhattan Project was established. At Los Alamos when he arrived in May 1943 Capt. Parsons was named head of the Ordnance Division and in the weeks following the Port Chicago explosion he was named Los Alamos Laboratories Associate Director.

The M.A.U.D. report does not propose an autocatalytic method of bomb assembly, but does emphasize that an optimal fission chain reaction in U^{235} would require that fast fission-produced neutrons be energy moderated by elastic collisions with deuterium:

"Since slow neutrons are so much more effective in causing fission of 235 it is clear that the conditions for a chain reaction will be more favorable if the fast neutrons present in the system can be slowed down. This can readily be achieved by adding to the uranium a suitable compound of a light element such as hydrogen, deuterium, or carbon. The reduction in velocity of the neutrons takes place by the collision of the neutron with the light atomic nucleus . . .The loss of neutrons due to capture by deuterium is much less than that due to hydrogen."

The report also suggested that an improvement in the conditions favorable to a fission chain reaction "can be achieved by arranging the slowing-down material in alternate layers or blocks instead of a uniform mixture." The first nuclear fission power reactor, constructed at the University of Chicago by Enrico Fermi and first operated 2 December 1942, utilized natural uranium oxide and metal distributed in pockets throughout 350 tons of graphite blocks, but for the Mark II bomb the "slowing-down" material, deuterium, was compounded with slightly U^{235}-enriched uranium metal to form uranium hydride (uranium deuteride) and was thus a uniform mixture.

Autocatalysis, the Briggs report, October 1941

On 28 October 1941 National Bureau of Standards Director Lyman J. Briggs, via National Defense Research Committee Chairman James Conant, transmitted to Arthur Holly Compton at the University of Chicago Metallurgical Laboratory "a special report on chain reactions from the group dealing with the theoretical aspects of the uranium work." Section 4 of Briggs' report responds to Compton's written question, "Can the system be controlled by the 'expulsion' method?":

> "This method consists in removing neutron absorbing materials from the uranium by means of an ordinary explosion. In one of its modifications it is intended to operate in such a way as to make use of a small fraction of the released nuclear energy to eject the absorber at a rate which increases as the reaction progresses.
>
> "It is felt that this method has possibilities which should be investigated. It might lead to elimination of effects of spontaneous fission, to a simplification of the arrangement and to an increase in the

> energy evolved. It is felt that a mathematical investigation of the possibilities would be helpful.”

Autocatalysis, Edward Teller, Spring 1942

In spring 1942 meetings with J. Robert Oppenheimer and others at the University of California, Berkeley, to consider possible ways to design an atomic bomb, Edward Teller proposed the autocatalytic bomb assembly concept that would be developed to be the Mark II bomb. Teller suggested that B^{10} might be admixed with the fissionable active of a uranium hydride bomb, and he proposed that when the B^{10} was highly compressed in a nuclear explosion the absorption of neutrons by B^{10} would diminish as a result of compression, promoting an increase in the criticality of the bomb’s active and boosting the energy release from the bomb. This was, in fact, the first perception of the ionization implosion pressure principle that would be the basis of the hydrogen fusion bomb. In 1944 John von Neumann proposed that the B^{10} in Teller's autocatalytic system for the Mark II bomb be replaced by a deuterium-tritium (D-T) mixture, in which von Neumann theorized that thermonuclear ignition of the D-T mixture would occur as a result of heating and ionization compression in the conditions of an nuclear fission explosion.

Von Neumann's proposal was an important step toward the creation of a thermonuclear-boosted atomic bomb. The Mark II which was proof fired at the Port Chicago Naval Magazine 17 July 1944 was the world’s first nuclear fission bomb, but the Mark II in several ways that are not presently disclosed in the declassified literature was the first important precursor of the hydrogen fusion bomb, the H-bomb. I consider likely that an insignificant component of deuterium fusion energy was produced by the detonation of the Mark II at the Port Chicago Naval Magazine.

Autocatalysis, Manley, Oppenheimer, Serber, and Teller, November 1942

On November 26, 1942 University of Chicago Metallurgical Laboratory Director Arthur Holly Compton, in his “Report on the Feasibility of the ‘49’ Project,” considers the autocatalytic method for use with plutonium; this report is exclusively concerned with the application of plutonium to fission bombs and therefore does not consider uranium hydride bomb autocatalysis.

In discussion of an autocatalytic plutonium bomb Compton wrote in this report, “If, due to impurities or otherwise, too many natural neutrons are emitted from the ‘49’, an efficient explosion can be produced by an ‘autocatalytic’ method. This procedure requires perhaps 6 times more material than does the direct method, and is more hazardous in use, but is, nevertheless, considered practicable if the requirements make it necessary.”

Section F of Compton’s 26 November 1942 report includes “The Use of Materials in a Fission Bomb,” by John H. Manley, J. Robert Oppenheimer, Robert Serber, and Edward Teller. Page 3 of the Manley, Oppenheimer, Serber and Teller report describes the autocatalytic method for plutonium bomb assembly under the heading, “Detonation Autocatalysis”:

> “Since the autocatalytic method circumvents the difficulty of predetonation it will be illustrated by an example:
>
> “In the mass of ‘49’ small spheres (approx. 3 cm diameter) of B^{10} (or B^{10} hydride) are imbedded. The explosion is brought about by an external change such as bringing a neutron reflector closer to the bomb. In the course of the explosion the small boron bubbles will be compressed and their power of neutron absorption thereby decreased. This increases the neutron reproduction ability of the bomb and the explosion proceeds.
>
> “This method has the advantage that there is no danger of predetonation from stray neutrons caused by spontaneous fission, or by (α,n) reactions, or cosmic rays, or enemy defense measures.
>
> “The disadvantages are: 1.) The boron absorbers would make it necessary to increase the amount of ‘49’ by about a factor of 3 or more to obtain the same efficiency and by a further factor of 2 to make the experimental control safe. 2.) A good fast-neutron absorber such as separated B^{10} would be required. 3.) The calculations involved for the construction are considerably more complicated and this increases the difficulty of experimental control and testing.”

Autocatalysis, Robert Serber, April 1943

Robert Serber's Los Alamos "Indoctrination Course" was a series of lectures given by Serber at Los Alamos during the first two weeks of April 1943. The earliest document catalogued at Los Alamos is "LA-1. The Los Alamos Primer," which consists of manuscript notes taken during those lectures by Edward (E. U.) Condon.

"LA-1, The Los Alamos Primer" is available online at the link:

http://upload.wikimedia.org/wikipedia/commons/9/9c/Los_Alamos_Primer.pdf

and is commercially published: Serber, Robert. *The Los Alamos Primer. First lectures on how to build an atomic bomb.* University of California Press, 1992.

During April 1943 newly arriving members of the scientific staff at Los Alamos who attended these lectures were informed by Serber, "All autocatalytic schemes that have been thought of so far require large amounts of active material, are low in efficiency unless very large amounts are used, and are dangerous to handle. Some bright ideas area needed."

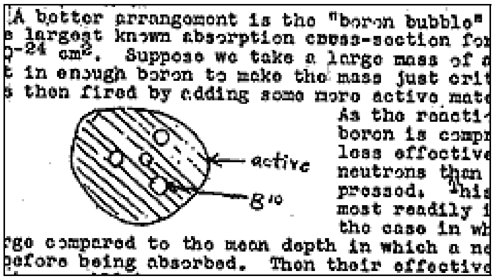
"LA-1" does not disclose why Serber described autocatalytic "schemes" as dangerous to handle, but in early autocatalytic assembly experiments at Los Alamos the safe proportions of active material and catalyst were not known. The speed of the autocatalytic reaction as the proportions of active material and catalyst were experimentally augmented or diminished was not known; and the effect on the reaction rate consequent to tamper materials variations and even minimal compression of the active and catalyst were not known. Furthermore, because those autocatalytic assembly experiments were performed with uranium hydride, the hazard of fire was ever present—uranium hydride is pyrophoric—until means had been devised to constrain spontaneous combustion of the uranium hydride.

"LA-1" proposes two methods of autocatalytic uranium fission bomb assembly. The second of the two, the "boron bubble scheme," is the method developed for the Mark II. The text of “LA-1” describes the generalized conception of that method.

The “boron bubble scheme”

Serber instructed attendees at those lectures that, with reference to the autocatalytic methods of assembly, “A better arrangement is the ‘boron bubble’ scheme. B^{10} has the largest known absorption cross-section for fast neutrons . . . Suppose we take a large mass of active material and put in enough boron to make the mass just critical. The device is then fired by adding some more active material or tamper. As the reaction proceeds the boron is compressed and is less effective at absorbing neutrons than when not compressed. This can be seen most readily if one considers the case in which the bubbles are large compared to the mean depth in which a neutron goes in boron before being absorbed. Then their effectiveness in removing neutrons will be proportional to their total area and so will drop on compression. Hence ν [the neutron number] will increase as the bubbles are compressed. If the bomb is sufficiently large this tendency is bound to overweigh the opposing one due to the general expansion of the bomb material, since the distance the edge of the bomb must move to produce a given decrease in ν increases with the radius of the bomb, whereas for a larger bomb the distance the edge of a bubble must move is unchanged, since it is not necessary to increase the radius of the bubbles but only to use more of them.”

“The boron bubble scheme.”
Source: Robert Serber,
“LA-1. The Los Alamos Primer.”



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Basically this means that in an uncompressed condition, at normal atmospheric pressure, bubbles of B^{10} mixed into the U^{235} active material of a fission bomb will be very effective high energy neutron absorbers. The fission efficiency potential of the bomb required the safe accumulation of active material substantially in excess of the critical mass. But as the minimum critical mass is exceeded the hazard of a spontaneous fission chain reaction occurring in the active material rapidly increases, for the reason that spontaneous nuclear fissions within the active material each produce an average of 2.5 high energy neutrons, and each of those spontaneously fission-produced neutrons is theoretically capable of inducing a nuclear fission chain reaction in the active material.

However, the introduction of bubbles of B^{10} within the active material to absorb spontaneously fission-generated high energy neutrons permitted the accumulation of a safe supercritical active. Thereby the difficulty of accumulating a safe supercritical U^{235} active that would, when fully assembled, enable at least a moderately efficient nuclear fission explosion was achieved by the introduction of B^{10} bubbles into the active material. The next problem was to devise a method to eliminate the B^{10} from the active material at the moment the assembly was so complete that the explosive fission reaction could proceed, and proceed without the hindrance of high energy fission-generated neutron absorption by the B^{10} bubbles. Elimination of the B^{10} from the active was known as the “expulsion” method of autocatalytic assembly.

Alternatively, and more advantageously, radical compression of the boron bubbles would have the result that “their effectiveness in removing neutrons will be proportional to their total area and so will drop on compression. Hence ν [the neutron number] will increase as the bubbles are compressed.” This method was known as the “compression” method of autocatalytic assembly, and was the method of assembly employed by the autocatalytic uranium hydride lateral implosion experimental device, named by James Conant the Mark II.

May 1943, refinement of autocatalysis

In a report dated 10 May 1943 the Los Alamos Reviewing Committee noted the “recently discovered” possibility for use of uranium hydride as the active material of a nuclear fission bomb; the report noted that Los Alamos had learned of the existence of the hydride “somewhat by accident.” In source materials available to me I have found no information that amplifies the statement that Los Alamos learned of the existence of uranium hydride somewhat by accident. Frank Spedding at the University of Iowa, Ames, first produced the uranium hydride, apparently early in 1943; perhaps Spedding didn’t consider his accomplishment of sufficient importance to merit communication to any of his Manhattan Project colleagues.

Implosion and the first Los Alamos implosion experiment, July 4, 1943

In a meeting at Los Alamos on ordnance problems late in April 1943, Seth Neddermeyer presented the first persuasive theoretical analysis of the implosion method of assembling a supercritical fissionable active. Neddermeyer showed, mathematically, that the compression of a solid sphere of plutonium or uranium by the detonation of an encasing layer of a high explosive was feasible and that spherical implosion assembly would be superior to the gun assembly method of the Mark I design because of the higher velocity and shorter path of assembly achieved by implosion.

On May 17, 1943 James Conant wrote to George Kistiakowsky, head of the Bruceton, Pennsylvania, Explosive Research Laboratory (ERL), operated on the grounds of the U.S. Bureau of Mines Experiment Station by the National Defense Research Committee Division 8 (Ralph Connor, chief): “This is to authorize the visit of S. Neddermeyer and Edward McMillan to Bruceton, and authorize you to show them whatever they may desire to see.”

During that visit the first experimental implosion of a cylinder was conducted. During a 17 April 1985 interview with McMillan conducted by Robert Seidel, McMillan recalled that visit had produced “some experiments with cylindrical implosions . . . (using an iron) pipe and making some explosives in a shell around it.” Page 88 of the DOE Los Alamos history *Critical Assembly* further reports of that visit: “Ignition of the explosives wrapped around the pipe ‘at a few points’ set up a convergent wave and one could see clearly that ‘the pipe had closed in.’ These experiments demonstrated that one could actually ‘drive matter in.’ ”

The Mark II autocatalytic uranium hydride lateral implosion experimental device was in fact an imploded cylindrical design, rather than the imploded spherical design of the Mark III and Mark IV. “Lateral” means “of, relating to, or situated at or on the side or sides.” (*The*

American Heritage Dictionary of the English Language. New York: American Heritage Publishing Co., Inc., 1971.) Lateral implosion of the Mark II defines the implosion of a cylinder by means of an explosive charge wrapped around the length of the cylindrical bomb, and with the ends of the cylinder capped so that an implosive force would be exerted with some uniformity laterally upon the cylinder wall. The uranium hydride active filled the interior of the thick-walled mild steel cylinder, which was in my estimation 3 inches of interior diameter and 2 feet long.

Multi-point detonation of the explosive which encased the cylinder crushed (imploded) the steel cylinder upon the active material along the length of the cylinder and thereby was assembled the active material of the Mark II into a highly compressed supercritical mass. The dense mass of the highly compressed cylinder walls and cylinder end caps confined the active material for the brief fraction of a second necessary for the initiation of a nuclear fission chain reaction by means of a neutron source placed within the active, and thenceforth propagation of an explosive fission chain reaction.

The high explosive which encased the Mark II cylinder was itself confined by a casing of depleted uranium or lead beneath an outer cylinder of tensile steel, which collectively acted as a tamper. In total the Mark II weighed approximately 1,120 pounds (510 kg). Navy Capt. William S. Parsons said the process of imploding a cylinder capable of momentarily containing an evolving fission chain reaction would be “like trying to squash a full can of beer without ejecting any of the beer.” The Mark II was that theoretical can of beer. No illustration of the actual construction of the Mark II is available in the declassified literature. The Mark II was essentially a nuclear fission pipe bomb.

The text of *Critical Assembly* on pages 88-90 gives a good summary of the development of cylindrical implosion technology at Los Alamos:

> “The early Los Alamos implosion research was remarkably crude. It was carried out in an arroyo on South Mesa. The first test, using tamped TNT surrounding hollow steel cylinders, was made on the Fourth of July (!) 1943, with [Capt. William S.] Parsons attending. The team centered a piece of steel pipe in a larger piece of stove

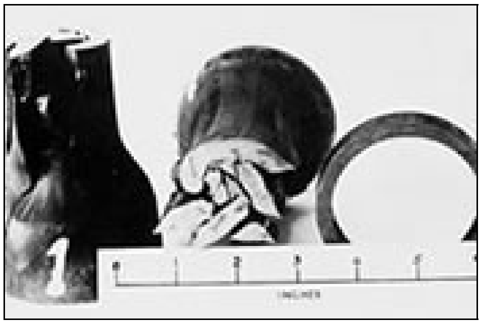
pipe, and after packing granular TNT into the annular space between the pipes, detonated the implosion using Primacord. Other versions of the experiment used powdered TNT and plastic explosive to squash mild steel pipes into solid bars. Using the 'Edison approach' [the "trial and error" method of experimentation], Neddermeyer's group repeated this basic experiment many times, varying all the parameters—the explosive arrangement, size of the pipes, and nature of the explosives. The experimental data to be analyzed consisted of a motley collection of bashed-in pipes. These data were subjected to a primitive version of the analysis, which in the program would later be referred to as 'terminal observations.' The method centered on studying the remains of imploded material after the test shots.

"Summarizing the implosion experiments done in July and August [1943], Neddermeyer wrote in one of the earliest technical Los Alamos reports:

"[In tests] 'which were of necessity done with meager equipment, the aim has been first to observe the main features of the phenomena when metal shells undergo extreme and rapid plastic flow under external pressure, and to make an empirical determination of the relation between collapse ratio and mass ratio. These experiments are being followed by observations of the velocities and times of collapse, for which several direct methods have been devised.'

"To cast the needed high explosive for these experiments, E-Division erected a small casting plant at Anchor Ranch."

Data from early implosion tests. Source: Seth Neddermeyer, "LA-18, Collapse of Hollow Steel Cylinders," August 9, 1943.



David Hawkins' *Manhattan District History: Project Y, the Los Alamos Project* (Volume I) reports in paragraph 7.53 that "the first implosion tests at Los Alamos were made in an arroyo on the mesa just south of the Laboratory on July 4, 1943. These were shots using tamped TNT surrounding hollow steel cylinders."

A meeting of Los Alamos Laboratories Governing Board on 28 October 1943 made the decision to emphasize the implosion assembly program. As reported by David Hawkins, at the end of October 1943 ordnance and engineering work at Los Alamos "was geared to the gun program, and could not be redirected overnight. By the end of 1943 the implosion had caught up with the gun in priority . . . The quantitative investigation of the hydrodynamics of the implosion proved a very difficult job . . . In the spring of 1944, the problem was set up for IBM machine calculation. These machines, which had recently been procured to do calculation on odd-shaped critical masses, were well adapted to solve the partial differential equations of the implosion hydrodynamics . . . As was not unnatural at the beginning of this new line of investigation, there was some thought given to the implosion of uranium hydride. The density of this material was about half that of uranium, and the space occupied by the hydrogen would be recoverable under sufficient pressure. Samples of hydride prepared at Los Alamos were investigated at the high pressure laboratory of W. P. [Percy] Bridgman at Harvard. Pressure density data up to 10 kilobars, still very low pressure from the point of view of the implosion, gave indication that the hydride was not in fact very easily compressible . . . During the period to April 1944 some data were obtained from terminal observation, from the HE flash photography of imploding cylinders, and from flash X-ray photography of small imploding spheres . . . The first successful HE flash photographs of imploding cylinders showed that there were indeed very serious asymmetries in the form of jets which traveled ahead of the main mass. A number of interpretations of these jets were proposed, including the possibility that they were optical illusions."

Mark II, the first nuclear fission bomb

The Military Policy Committee report of 21 August 1943

Four months after Cyril Smith began his April 1943 work at Los Alamos on uranium hydride metallurgy, and at which time Los Alamos learned of the possibility of large scale controlled uranium hydride production, and six weeks after the first cylinders were imploded at Los Alamos, the Atomic Bomb Military Policy Committee—formally the Military Policy Committee on Atomic Fission Bombs (appointed 23 September 1942)—in their "Report of August 21, 1943 on Present Status and Future Program on Atomic Fission Bombs" informed Vice President Henry Wallace, Secretary of War Henry Stimson and Chief of Staff Gen. Marshall, “There is a chance, and a fair one if a process involving the use of a hydride form of material proves feasible, that the first bomb can be produced in the fall of 1944.” Members of the Military Policy Committee were Vannevar Bush, James Conant as Bush’s alternate on the committee, Rear Admiral William R. Purnell, USN and General Wilhelm D. Styer, USA.

On 21 August 1943 the Military Policy Committee forecast a fair chance the first atomic bomb, which would employ a “hydride form of material,” could be produced in the fall of 1944. Necessarily that hydride material was uranium hydride. An undated manuscript note from James Conant to National Defense Research Committee Vice Chairman Richard C. Tolman, which contextually can be dated to the same period or earlier than the Military Policy Committee’s report of 21 August 1943 reads: “For your information and return. I guess I am satisfied that the ‘23’ [U^{233}, see note] project should be of second order presently though if they get stuck on the decontamination of ‘49’ [plutonium], ‘23’ might well prove a better bet. Furthermore if the 25 [U^{235}] hydride looks iffy ‘23’ will prove more attractive than at present since it seems pretty certain ‘49’ doesn’t form a hydride.”

[Note: “Special nuclear material” (SNM) is defined by Title I of the Atomic Energy Act of 1954 as plutonium, U^{233}, or uranium enriched in the isotopes U^{233} or U^{235}. U^{233} and plutonium do not occur naturally but can be formed in nuclear reactors and extracted from the highly radioactive spent fuel by chemical separation. U^{233} also can be produced in special reactors that use thorium as fuel. Only small quantities of U^{233} are reported to have ever been produced in the United States.]

The Military Policy Committee report of 21 August 1943 to Vice President Wallace, Secretary of War Stimson and Chief of Staff General Marshal forecast the “fair chance” that the first atomic bomb, a uranium hydride bomb, could be produced “in the fall of 1944.” That forecast was pin-point accurate. The fall of 1944 is understood in the Northern Hemisphere to be the autumn of the year, from the autumnal equinox on about 22 September to the winter solstice on about 22 December. James Conant informed Gen. Groves in the memorandum “Report to Gen. Groves on Visit to Los Alamos on August 17, 1944” that the Mark II could be developed for combat use in three or four months time, which places that development of the first atomic bomb, as a combat weapon, between 17 November and 17 December 1944 and exactly in the fall of 1944.

The Manhattan Project historical literature universally reports that a dire uncertainty of the Project's success ceaselessly harried the endeavor, but on 21 August 1943 the Military Policy Committee exactly predicted that the first atomic bomb "can be produced in the fall of 1944." The Mark II, however, was a tactical nuclear fission weapon of 1,000 tons TNT equivalent energy yield and, therefore, the Mark II was not the militarily-decisive strategic weapon of energy yield equal to or greater than 10,000 tons TNT equivalent that the Project was mandated to produce for use during the war. Consequently, on 17 August 1944 following the successful test of the Mark II on 17 July 1944, which had demonstrated the feasibility of large scale nuclear fission weapons, the Mark II was "put on the shelf" and work at Los Alamos on the more powerful militarily-decisive weapons that would be detonated in combat one year later at Hiroshima and Nagasaki proceeded with greater confidence.

The Quebec Agreement, August 14-24, 1943

Of very great historical interest is the fact that the 21 August 1943 report of the Military Policy Committee, which forecast that the first atomic bomb could be available in the fall of 1944, is dated two days after President Roosevelt and Prime Minister Churchill signed the Quebec Agreement at Quebec City, Canada, on 19 August 1943—that Military Policy Committee report is, in fact, dated during the proceedings of the 14-24 August 1943 Quebec Conference.

The Quebec Agreement of 19 August 1943 ("Articles of Agreement Governing Collaboration Between the Authorities of the U.S.A. and the U.K. in the Matter of Tube Alloys") established that cooperation between Britain and the U.S.A. in the development of atomic bombs was imperative: "Whereas it is vital to our common safety in the present war to bring the TUBE ALLOYS project [i.e. the atomic bomb project] to fruition at the earliest moment; and whereas this may be more speedily achieved if all available British and American brains and resources are pooled. . . .”

The fifth provision of the Quebec Agreement established the British and U.S. Combined Policy Committee. Among the functions delegated to the committee by Prime Minister Churchill and President Roosevelt were the activities necessary “to keep all sections of the project under constant review” and to maintain “complete interchange of information and ideas on all sections of the project between members of the Policy Committee and their immediate technical advisers.” The Quebec Agreement named the following persons to the Combined Policy Committee, and each of those men named to the committee was present at the conference:

- The Secretary of War, (Henry Stimson, United States)
- Dr. Vannevar Bush. (United States)
- Dr. James B. Conant. (United States)
- Field-Marshal Sir John Dill, G.C.B., C.M.G., D.S.O. (UK)
- Colonel the Right Hon. J. J. Llewellin, C.B.E.1 M-0., M.P. (UK)
- The Honorable C. D. Howe. (Canada)

The Military Policy Committee’s 21 August 1943 complex and fulsome SECRET report of more than 20 pages, which forecast that the first atomic bomb would be produced in the fall of 1944, is dated two days after 19 August, the date the Quebec Agreement was signed.

Certainly that report had been finalized and approved by the full Military Policy Committee before the Quebec Conference convened on 14 August. Military Policy Committee Chairman Vannevar Bush, even with James Conant’s assistance, could not possibly have finalized that comprehensive report while the Quebec Conference was in progress. Moreover, that 21 August report had been approved by the full Military Policy Committee, but committee members Adm. Purnell and Gen. Styer were not present at the Quebec Conference. Admiral Purnell and Gen. Styer had approved that finalized 21 August Military Policy Committee report before the Quebec Conference convened.

The report had been finalized before the Quebec Conference and therefore necessarily postdated to 21 August in anticipation that the Quebec Conference by that date would approve the proposed terms of the Quebec Agreement, that the proposed Combined Policy Committee would be established by the terms of that Agreement, and that during the Quebec Conference, on or after 21 August, the British and U.S. members of the Combined Policy Committee present at Quebec would meet, and the information provided by the Military Policy Committee report dated 21 August would be then disclosed to the British. Probably before the Quebec Conference ended on 24 August 1943 the British members of the Combined Policy Committee learned there was a fair chance, if a process involving uranium hydride proved feasible, that the first atomic bomb could be produced in the fall of 1944. That bomb would be the Mark II, and the British physicist James Chadwick, at the beginning of August 1944, would be among the first persons to review the reports and analyses of the 17 July 1944 Port Chicago explosion that were prepared by Los Alamos.

Photographs and illustrations credits.

“The boron bubble scheme.” Source: Serber, Robert. “LA-1, The Los Alamos Primer,” April 1943; Section 21, “Autocatalytic Methods,” page 23.

“Data from one of Seth Neddermeyer’s earliest implosion tests. The center ring is an untested cross section of the carbon steel tubing used in the first implosion experiments at Los Alamos.” Source: Neddermeyer, Seth Neddermeyer. “LA-18, Collapse of Hollow Steel Cylinders,” August 9, 1943.

Chapter 15

The 3 Horsemen, and Corruption of the Port Chicago Navy Court of Inquiry.

On 27 July 1944, the same day that James Conant wrote his "Historical note" to report his 17 July conversation with J. Robert Oppenheimer at the University of Chicago on the subject of the Mark II, Conant also wrote a letter to J. Robert Oppenheimer at P. O. Box 1663, Santa Fe, New Mexico. The letter informs "Dear Oppie":

> "I shall arrive in Lamy on the 'Chief' at the usual time on Thursday, August 17, and plan to leave on Sunday, August 20, by the 'Chief.' I am sorry the visit must be so short, but if we can arrange for another session of a couple of hours with the same group I met with on my last trip, I am sure we can accomplish a good deal in a short time. I should hope that I can spend the best part of a day with George [Kistiakowski] . . . I hope the visitation of the Nobel Prize winners went off successfully. I expect to hear a report from the General tomorrow . . . Without being over-optimistic I still reaffirm my complete confidence in your ability to make at least a mark two gadget work (one crit or better one-half!) by the first of February, but of this more when we meet."

We have here Conant's statement that on 27 July 1944 some unidentified "Nobel Prize winners" were then visiting at Los Alamos, and this letter informs Oppenheimer that Conant expected Gen. Groves to provide a report on the "visitation" of the Nobel Prize winners the following day, 28 July. Apparently, and in fact, Gen. Groves was at Los Alamos on 27 July, and Conant correctly expected the General would return to Washington on 28 July and then make a report of that visit.

General Groves, from his Washington, D.C., office, did inform Oppenheimer in telephone conversation on **10 July 1944** at 11:00 A.M. that he would "talk to JBC [James Bryant Conant] and RCT [Richard Chace Tolman] re: 3 horsemen's visit." On **12 July 1944** at 10:45 A.M. the General "called Dr. Oppenheimer, Santa Fe, N.M. re: visit of 3 horsemen to Y [Los Alamos]. To arrive July 31st and department [sic] August 3rd. Gen. Groves to send written invitations to all three."

On **12 July** the dates were set for that meeting of the "3 horsemen" at Los Alamos. The 3 horsemen would arrive at Los Alamos 31 July and depart the morning of 3 August. The meeting of the 3 horsemen at Los Alamos was set to begin 31 July and was not in progress 27 July—as Conant's letter to Oppenheimer of 27 July incorrectly reports. But Gen. Groves was, in fact, at Los Alamos the day of 27 July, did return from Los Alamos to Washington on 28 July, and did that late afternoon make a report of his 27 July Los Alamos visit to James Conant and the Military Policy Committee.

General Groves' Los Alamos visit of 27 July 1944

The General's office logbooks, known as the "Groves Diaries," for 26 July 1944 disclose that Gen. Groves left his Washington office at 5:45 P.M., "for the airport to go to Santa Fe." Given that time of departure from Washington, Groves arrived at Los Alamos the morning of 27 July. The General's office log for 28 July discloses that the Gen. "returned from the airport at 4:10 P.M." and "at 5:45 P.M. entered a meeting of the Military Policy Committee." The General's office log discloses that Vannevar Bush, James Conant and Admiral Purnell were present at that committee meeting; Gen. Styer was absent.

The National Archives has been unable to locate any minutes or notes taken during that 28 July 1944 meeting of the Military Policy Committee, so we cannot know certainly what particular matters were discussed during that committee meeting, but we can make a well-reasoned surmise—which is that Gen. Groves returned from Los

Alamos in possession of the first written analyses of the Port Chicago explosion that had been prepared by Los Alamos, and that the Port Chicago explosion and those analyses were the principal topic of discussion during that 28 July 1944 meeting of the Atomic Bomb Military Policy Committee.

The first completed Los Alamos summary report of the Port Chicago explosion, written by Captain William S. Parsons, is dated 24 July 1944. In addition to that first summary report dated 24 July, a comprehensive first analysis of the blast damage that did result from the Port Chicago explosion was completed by Ensign George T. Reynolds, USNR, at Los Alamos and dated 27 July 1944. Both documents were completed and available to Gen. Groves during his 27 July visit at Los Alamos.

The first written Los Alamos report on the Port Chicago explosion, “Port Chicago Disaster: Preliminary Data,” was Capt. William S. Parsons’ 24 July 1944 memorandum of that title addressed to Rear Admiral William R. Purnell, the Navy member of the Atomic Bomb Military Policy Committee and Capt. Parsons’ commanding officer. Ensign Reynolds’ Port Chicago blast damage analysis dated 27 July 1944, “Report on Port Chicago July 20-24, 1944,” was addressed to Capt. Parsons, Ensign Reynolds’ commanding officer. Ensign Reynolds’ report on Port Chicago would be made Enclosure (C) of Capt. Parsons’ memorandum to Admiral Purnell, “Port Chicago Disaster: Second Preliminary Report,” dated 4 August 1944. Both documents were intended for delivery to Admiral Purnell in Washington, D.C.

Captain Parsons’ 24 July memorandum “Port Chicago Disaster: Preliminary Data” and Ensign Reynolds’ 27 July “Report on Port Chicago July 20-24, 1944” were both completed and available to Gen. Groves at Los Alamos on 27 July—before the General returned to Washington from Los Alamos on 28 July. Those established circumstances educe the conclusion of fact that Gen. Groves, who was the Military Policy Committee’s executive officer, did take possession of those two reports at Los Alamos and did deliver them to the person for whom they were intended, Adm. Purnell, during the meeting of the Military Policy

Committee which did convene in the General's office on 28 July at 5:45 P.M., and at which meeting Admiral Purnell was present.

In the context of the Manhattan Project history it's impossible to discover any other matter or event of sufficient importance on or about 26 July that would have compelled the General to travel by air from Washington to Los Alamos and return 48 hours later, except the important matter of receiving the first Los Alamos analyses of the Port Chicago proof of the Mark II bomb, and making delivery of those analyses to Admiral Purnell and the Military Policy Committee.

At Los Alamos on 27 July Gen. Groves undoubtedly had general discussions of the Port Chicago explosion, as well as specific discussions of those first two explosion analyses, with Oppenheimer and others of the scientific staff at Los Alamos. During the 28 July meeting of the Military Policy Committee Gen. Groves undoubtedly introduced discussion and review of those first two Port Chicago explosion analyses made by Los Alamos, and also made report to the committee of such incidental information concerning the Port Chicago explosion that he had received in discussions with Oppenheimer and others at Los Alamos on 27 July.

However, lacking any minutes or notes that report the matters discussed by the Military Policy Committee during that 28 July meeting in Washington we can only educe as a conclusion of fact that the purpose of Gen. Groves' quick trip to Los Alamos was to receive those documents, to discuss the Port Chicago explosion with Oppenheimer and others at Los Alamos on 27 July, to deliver those Port Chicago analyses to Admiral Purnell at the 28 July Military Policy Committee meeting, and to verbally report such additional information concerning the Port Chicago explosion that the General had learned from discussions of the explosion at Los Alamos on 27 July.

The "3 horsemen"

> *"Now in my vision this is how I saw the horses and their riders. They wore red, blue, and yellow breastplates, and the horses' heads were like heads of lions, and out of their mouths came fire, smoke and sulfur. By these three plagues of fire, smoke and sulfur that came*

> *out of their mouths a third of the human race was killed." — The Revelation of John.*

I first learned from James Conant's letter of 27 July 1944 to Oppenheimer that some unidentified Nobel Prize winners had visited Los Alamos precisely at the time the first Los Alamos reports and analyses of the Port Chicago explosion had been available, but Conant's letter to Oppenheimer does not identify those Nobel Laureates.

Nowhere does the commercially published Manhattan Project historical literature report a visit by any Nobel Laureate to Los Alamos on or about 27 July 1944, but we can safely assume that the "Nobel Prize winners" whom Conant believed were visiting at Los Alamos on 27 July 1944 were men whose scientific contributions were important to the work undertaken at Los Alamos in development of the atomic bombs in summer 1944, rather than perhaps Pearl S. Buck who received the Nobel Prize for Literature in 1938.

In previous chapters are identified three Nobel Laureates whose contributions in science were fundamental to the development of the atomic bombs at Los Alamos, and specifically fundamental to development of the Mark II: Sir James Chadwick who received the 1935 Nobel Prize in Physics for his discovery of the neutron, which enabled artificially induced nuclear fission in the active uranium hydride material of the Mark II; Ernest O. Lawrence who received the 1939 Nobel Prize in Physics for the invention and development of the cyclotron, which was used by the Manhattan Project as an essential contributing technology to the U^{235} isotope separation necessary to produce the slightly U^{235}-enriched uranium hydride active of the Mark II; and Harold Clayton Urey who received the 1934 Nobel Prize in Chemistry for his discovery of the deuterium isotope of hydrogen (H_2), which was essential to the uranium hydride (deuterium) Mark II. Harold Urey had also isolated the B^{10} boron isotope essential to the autocatalytic uranium hydride Mark II, and he had developed the industrial scale methods of production necessary to produce the deuterium and B^{10} isotopes essential to detonation of the Mark II autocatalytic uranium hydride lateral implosion experimental device.

A close reading of the General's office logbooks, which are held by the National Archives at College Park, Maryland, first discloses that on **10 July 1944** at 11:00 A.M. in Washington, "Gen. Groves held a telephone conversation with Dr. Oppenheimer at Los Alamos. Gen. Groves to talk to JBC [James Bryant Conant] and RCT [Richard Chace Tolman] re: 3 horsemen's visit." On **12 July 1944** at 10:45 A.M. the General's office log reports, "Gen. Groves called Dr. Oppenheimer, Santa Fe, N.M. re: visit of 3 horsemen to Y. To arrive July 31st and department August 3rd. Gen. Groves to send written invitations to all three."

The investigator need only read a few more days through Gen. Groves' office log to discover that the General invited Nobel Laureates Chadwick, Lawrence and Urey to arrive for a visit at Los Alamos 31 July and to depart the morning of 3 August 1944.

Nobel Laureate James Chadwick and his wife Aileen had arrived in the United States from England before the end of 1943 and by early 1944 had taken up residence at Los Alamos. James Chadwick, however, was infrequently at Los Alamos more than a few days in succession because his principal diplomatic and administrative functions required his presence mostly in Washington, D.C., where he quickly established close working relationships and rapport with Secretary of War Henry Stimson, Vannevar Bush, James Conant, and Gen. Groves.

Professor Chadwick was the senior technical adviser to the British members of the Combined Policy Committee, which had been established by the fifth provision of the Quebec Agreement, signed by Prime Minister Winston Churchill and President Franklin D. Roosevelt, 19 August 1943. The Quebec Agreement defined the intent and methods of cooperation among British, Canadian and U.S. scientists to advance, for mutual security, the wartime development of atomic bombs. Among the functions delegated to the Combined Policy Committee were the activities necessary "to keep all sections of the project under constant review" and to maintain "complete interchange of information and ideas on all sections of the project between members of the Combined Policy Committee and their immediate technical advisers."

On or about 21 August 1943 at Quebec the British and Canadian members of the newly established Combined Policy Committee were informed of the information disclosed by the U.S. Military Policy Committee on Atomic Bombs "Report of August 21, 1943 on Present Status and Future Program on Atomic Fission Bombs." That report forecast, "There is a chance, and a fair one if a process involving the use of a hydride form of material proves feasible, that the first bomb can be produced in the fall of 1944."

James Chadwick was the senior technical adviser to the British members of the Combined Policy Committee and, according to the directive of that committee to maintain "complete interchange of information and ideas on all sections of the project between members of the Combined Policy Committee and their immediate technical advisers," James Chadwick would necessarily be informed that the uranium hydride Mark II had been successfully proof-fired 17 July 1944. The Combined Policy Committee as a whole consisted of:

- The Secretary of War, (Henry Stimson, United States)
- Dr. Vannevar Bush. (United States)
- Dr. James B. Conant. (United States)
- Field-Marshal Sir John Dill, G.C.B., C.M.G., D.S.O. (United Kingdom)
- Colonel the Right Hon. J. J. Llewellin, C.B.E.1 M-0., M.P. (United Kingdom)
- The Honorable C. D. Howe. (Canada)

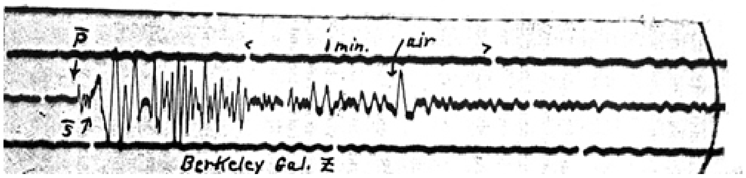
General Groves' office log records that on **20 July** at 10:35 A.M. "Dr. Chadwick called JO'L [Jean O'Leary, Gen. Groves' secretary] re: would like a priority 3 to travel by Flight 6:15 P.M. Friday [28 July] TWA [Trans World Airlines] from Wash. to Y." Nobel Laureate James Chadwick arrived at Los Alamos, Saturday, 29 July 1944.

During 1944 Professor Ernest O. Lawrence divided his time between his radiation laboratories at the University of California, Berkeley, campus and the Manhattan Project Y-12 facility at Oak Ridge, Tennessee, near Knoxville, where the Tennessee Eastman Corporation was making excellent progress in the installation of Lawrence's electromagnetic U^{235} isotope separation calutrons, ever changing in their design specifications to improve output, and training the necessary hundreds of personnel in the methods of operating the calutron race-tracks. Gen. Groves' office log records that on **13 July** at 12:20 P.M., "Gen. Groves called E. O. Lawrence, Knoxville, Tenn. re: plans to be at Y to arrive on July 31st and to depart August 3rd in the [A].M." Nobel Laureate Ernest O. Lawrence arrived at Los Alamos 31 July 1944.

From 1934 Harold Clayton Urey was Professor of Chemistry at Columbia University in New York City and, during the war was Columbia's Director of War Research, including the atomic bomb project. Professor Urey divided his time between Columbia University and the Army's Wabash Valley Ordnance Works, established in 1942, where in 1943 Standard Oil Company of Indiana had established the industrial facility to produce the deuterium necessary to the Mark II. Gen. Groves' office log records that on **13 July** at 12:40 P.M., "Gen Groves called Dr. Urey, Wabash, Indiana, to invite him to be present at Y on July 31st to stay until morning of August 3rd." Nobel Laureate Harold Clayton Urey arrived at Los Alamos 31 July 1944.

General Groves did himself return to Los Alamos from Washington to be present for the visitation of the Nobel Prize winners. The General's office log discloses that the General's secretary on **2 August**, "Called Gen. Groves in Santa Fe. w/weather news and summary of what had occured [sic] in his absence." The date the General arrived at Los Alamos is not disclosed by his office log, but his office log does disclose that the General departed Los Alamos 3 August.

As we have seen above, Capt. Parsons' first report on the Port Chicago explosion had been completed 24 July 1944 and Ensign Reynolds' first analysis of the blast damage that did result from the Port Chicago explosion had been completed 27 July. At least those two documents were completed and available by the date the 3 horsemen arrived at Los Alamos—Chadwick on 29 July; Lawrence and Urey on 31 July. However, several more complex analyses of the Port Chicago explosion were also available to the 3 horsemen and Gen. Groves during their visit, and before their departure the morning of 3 August.



Port Chicago explosion seismic record, "Gal. Z." One seismogram of the 17 July 1944 Port Chicago Naval Magazine explosion, made 30 kilometers from the source on the Galitzin Z recorder at the University of California, Berkeley.

Captain Parsons' memorandum to Admiral Purnell, "Port Chicago Disaster: Second Preliminary Report," is dated 4 August 1944. That memorandum includes, as **Enclosure (C)**, Ensign Reynolds' first blast damage analysis, "Report on Port Chicago July 20-24, 1944," dated July 27. Captain Parsons' "Second Preliminary Report" of 4 August 1944 also includes, as **Enclosure (D)**, Dr. Maurice M. Shapiro's undated "Preliminary Report: Observations on the Effects of the Tidal Wave, Port Chicago Explosion, July 17, 1944," and, as **Enclosure (E)**, Ensign Reynolds' undated "Report on Seismic Evidence, Port Chicago Explosion," which on the report title page is also named "Report on Port Chicago July 20-24,1944."

Captain Parsons' 4 August 1944 memorandum to Admiral Purnell, "Port Chicago Disaster: Second Preliminary Report," in addition to **Enclosures (C)**, **(D)**, and **(E)**, also provides as **Enclosure (A)**, "Marked copy of layout of U. S. Naval Magazine, Port Chicago, California"; **Enclosure (B)**, "Notes on Enclosure (A)"; and **Enclosure (F)**, "Prints of Mare Island Navy Yard Photographs Nos. . . .[38 in total]." All those Port Chicago explosion effects reports, maps and photographs, which would be Enclosures with Capt. Parsons' 4 August 1944 report to Admiral Purnell, were also available for review by the 3 horsemen and Gen. Groves during their visit at Los Alamos from 31 July through the morning of 3 August.

Moreover, the text of Capt. Parsons' 4 August 1944 memorandum to Admiral Purnell, "Port Chicago Disaster: Second Preliminary Report," informs the admiral that "two Army airplanes witnessed the explosion, the pilots agreeing that the flame rose to 8,000 feet." That particular information was also available to the 3 horsemen and Gen. Groves during their Los Alamos visit, which information provided the conclusive evidence that the fireball of the Port Chicago explosion had been typical of a nuclear fission explosion and, therefore, that the Mark II had been successfully proof fired at Port Chicago.

We must show by what means all those documents and information were available at Los Alamos during the visitation of the 3 horsemen. Captain Parsons in his first Port Chicago report of 24 July to Admiral Purnell, “Port Chicago Disaster: Preliminary Data,” states that he, Ensign Reynolds and Dr. Shapiro “arrived at Mare Island [Navy Yard] about noon 20 July and, with Captain Crenshaw, proceeded to Port Chicago.” Four days later, on 24 July, the party had returned to Los Alamos where Capt. Parsons wrote his first Port Chicago disaster report of 24 July and where, by 27 July, Ensign Reynolds had completed his first Port Chicago blast damage report.

Aerial photographs of the destruction at the Port Chicago Naval Magazine had been made during the early afternoon of 18 July by Mare Island Navy Yard. More than 100 photographs on the ground of the destruction were made by Mare Island on 18 July and immediately subsequent days. Prints of those photographs of the Port Chicago Naval Magazine destruction were available to Capt. Parsons and party prior to their return to Los Alamos 24 July, as were the “Marked copy of layout of U. S. Naval Magazine, Port Chicago, California” and associated notes.

All the information necessary to Dr. Shapiro’s “Preliminary Report: Observations on the Effects of the Tidal Wave, Port Chicago Explosion, July 17, 1944” had been investigated and collected by Dr. Shapiro from 20 July through 24 July. Ensign Reynolds in his report on the seismic evidence states, “On Monday [24 July] Reynolds and Shapiro conferred with Prof. Perry Byerly in his office at the University of California in Berkeley . . . This part of the report is concerned with what we learned from him and from the inspections of the [seismic] records.”

When the 3 horsemen arrived at Los Alamos on 29 July and 31 July 1944, the information available at Los Alamos descriptive of the Port Chicago explosion included Capt. Parsons’ 24 July “preliminary data”;

Ensign Reynolds' blast damage analysis; Ensign Reynolds' information on the seismic evidence; Dr. Shapiro's observations on the effects of the tidal wave; a marked copy of the layout of the Port Chicago Naval Magazine; and 38 aerial and surface photographs of the destruction wrought at Port Chicago. Additionally, Capt. Parsons, Ensign Reynolds and Dr. Shapiro were available to be called to augment their documented reports of the explosion by their subjective eyewitness accounts of the destruction wrought by the proof of the Mark II at Port Chicago. Hans Bethe, Joseph O. Hirschfelder, George Kistiakowski, William George Penney, and Edward Teller were also present at Los Alamos during the visit of the 3 horsemen and were available to provide additional comment and analysis of the documented and anecdotal reports of the Port Chicago explosion, as were J. Robert Oppenheimer and Gen. Groves who together had arranged the meeting of the 3 horsemen.

On 3 March 2000 this author filed a Freedom of Information Act request with Los Alamos National Laboratory Archives and immediately thereafter with the National Nuclear Security Administration, Department of Energy, Albuquerque, New Mexico, (FOIA Request 00-054-C) to obtain "whatever documentary materials you may have in the archives that are pertinent to a meeting held at Los Alamos July 31 through the morning of August 3, 1944 at which were present James Chadwick, Gen. Groves, Ernest O. Lawrence, J. Robert Oppenheimer, Harold Urey, with the probable participation of Capt. Parsons and Edward Teller and possible participation of others."

By 20 April 2000 Los Alamos Archivist Roger Meade informed DOE, Albuquerque, he had conducted a search of the "Project Y" Collection A-84-019 and the Archives Access Data Base for any documents under the keywords "Chadwick," "Groves," "Lawrence," "Oppenheimer," "Urey," "3 horsemen" and "three horsemen." Dr. Meade reported, "No responsive documents were located at the LANL Archives."

If no documentary records do exist that disclose even the fact of the meeting of the 3 horsemen and Gen. Groves at Los Alamos for the period 29 July through the morning of 3 August 1944, except those pertinent entries in Gen. Groves' office log, that meeting was clearly intended to be conducted without a single recoverable trace, but that meeting is established as a conclusion of fact.

Implicit in the entries in Gen. Groves' office log that establish the dates for the visit of the 3 horsemen and Gen. Groves to Los Alamos is the reality that the method and plan for the proof of the Mark II at the Port Chicago Naval Magazine had been determined and concluded by 12 July 1944, on which date the schedule for the meeting of the 3 horsemen at Los Alamos was set.

James Conant was apparently first cryptically informed that the proof of the Mark II had been successful in Oppenheimer's letter to him of 3 August 1944, the same date the 3 horsemen and Gen. Groves departed Los Alamos: "We are looking forward to your visit on the seventeenth and will plan to meet you at the Chief at Lamy . . . We have had the first positive indications as far as our main program goes, and although the results have not been checked, they do lend some encouragement. By the time you are out we should know pretty well how sound they are."

The "main program" was, of course, fulfillment of the Manhattan Project mandate to produce a militarily-decisive atomic bomb for use against the enemy during World War II. The "first positive indications," which had not been checked by 3 August, were the reports made by Capt. Parsons, Ensign Reynolds and Maurice Shapiro of the Port Chicago explosion that were available at Los Alamos by 3 August and were transmitted to Admiral Purnell as Capt. Parsons' 4 August 1944 memorandum and Enclosures, "Port Chicago Disaster: Second Preliminary Report."

The composite of augmented and extensively elaborated information and analyses of the effects of the Port Chicago explosion that would be developed following Capt. Parsons' 4 August 1944 "Port Chicago Disaster: Second Preliminary Report," and before James Conant's Los Alamos visit of 17 August, would be the materials and Enclosures of Capt. Parsons' 31 August 1944 "Port Chicago Disaster: Third Preliminary Report." The augmented information and elaborated data and analytical reports of that "Third Preliminary Report" would indeed show “how sound” the “first positive indications” had been that were available to Oppenheimer, the 3 horsemen and Gen. Groves by 3 August.

By the time James Conant arrived at Los Alamos 17 August 1944, evidence of every manifestation and effect of the 17 July 1944 Port Chicago explosion had been collected, analyzed, reviewed, and systematically reported by Capt. Parsons, Ensign Reynolds and Dr. Shapiro at Los Alamos. Consequently, in his “Report to Gen. Groves on Visit to Los Alamos on August 17, 1944” Conant wrote:

> “It is agreed that the Mark II should be put on the shelf for the present. If all other implosion methods fail, it could be taken off the shelf and developed for combat use in 3 or 4 months time . . . If the explosive lens development then looks very bad it may be necessary to work on improving Mark II to see if at least the upper limit of effectiveness [SENSITIVE INFORMATION DELETED] cannot be raised somewhat . . . It was agreed that Class B damage was damage beyond repair. For the phrase to be of significance the type of structure must also be named. It was agree that for dwelling houses the area of Class B damage was about as follows for 1,000 tons TNT: 90% Class B damage = 0.5 mile radius = .75 square mile area . . . For 10,000 tons TNT these figures are to be multiplied by 4.”

Captain J. S. Crenshaw, USN. Corruption of the Port Chicago Navy Court of Inquiry

On 21 July 1944 Commandant of the Twelfth Naval District (San Francisco) Rear Admiral Carleton H. Wright appointed a 3-man Court of Inquiry, “To investigate the facts surrounding the explosion of 17 July 1944.” Admiral Wright’s appointments to the court were Navy Captains Albert G. Cook, Jr., John S. Crenshaw and William B. Holden. Captain Cook was named president of the court. The transcript of the record of the court proceedings, completed after 40 days of testimony, and all official records finally concerned with the Court of Inquiry, report that “John S. Crenshaw, Captain, United States Navy” was a member of the court.

Captain Parsons’ 24 July 1944 memorandum to Admiral Purnell, “Port Chicago Disaster: Preliminary Data,” states, “My party arrived at Mare

Island about noon on 20 July and, with Capt. Crenshaw, proceeded to Port Chicago."

Captain Parsons' 4 August 1944 memorandum to Admiral Purnell, "Port Chicago Disaster: Second Preliminary Report," states, "Discussion with Capt. J. S. Crenshaw of the Court of Inquiry on 3 August, indicated that considerable progress is being made and that good evidence from eyewitnesses has been taken."

Captain J. S. Crenshaw was Capt. John Stewardson Crenshaw, United States Naval Academy, Class of 1921. To his family members in youth, to friends and acquaintances during his years at the Naval Academy, and until his death (19 May 1975), John Stewardson Crenshaw was always known as Jack Crenshaw. "Jack" is, of course, derived from John or Jacques. Usually, in public records, Capt. John Stewardson Crenshaw is identified as Capt. J. S. Crenshaw, as for example from the *Dictionary of American Naval Fighting Ships:* "USS *Frontier* (AD-25) was launched on 25 March 1945 by the Todd Shipyards, San Pedro, Calif.; sponsored by Mrs. George M. Ravenscroft, and commissioned 2 March 1946, Capt. J. S. Crenshaw, in command." At his retirement, Capt. J. S. Crenshaw was elevated to the rank of Rear Admiral.

Captain John Stewardson Crenshaw was, in fact, Capt. William S. Parsons' brother-in-law. Captain Crenshaw had been appointed to the Port Chicago Court of Inquiry specifically in consequence of Capt. Parsons' request for that appointment that he had made to Admiral Purnell.

"On 20 July, accompanied by a Los Alamos officer and a scientist, Parsons joined his brother-in law Capt. Jack Crenshaw (a member of the official inquiry into cause) at Mare Island, and they went together to the Port Chicago site."—Al Christman, *Target Hiroshima,* p. 154.

Captain Crenshaw and Capt. William S. "Deak" ("Deacon") Parsons had been acquainted for 25 years since their years together at the U. S. Naval Academy. Al Christman in his biography of Admiral Parsons, *Target Hiroshima,* wrote, "Deak's friendship with Jack Crenshaw went back to his second year at the academy, when Jack was one of the upperclassmen Deak tutored in Spanish. Now [1927] Jack was one of the five other ordnance postgraduate students with him at Annapolis." Crenshaw and Parsons had, respectively, married sisters Betty and Martha Cluverius. Betty and Martha were the daughters of Rear Admiral Wat Tyler Cluverius, USN, who as Captain Cluverius was commandant of midshipmen at Annapolis during the years Crenshaw and Parsons were there. Deak Parsons and Martha Cluverius were introduced at the rehearsal for the wedding of Betty Cluverius and Jack Crenshaw in the fall of 1928; Deak Parsons was Jack Crenshaw's best man. Martha preceded Betty and her father down the aisle of the Norfolk Navy chapel. One year later, in November 1929, Martha Cluverius and Deak Parsons were married in the Norfolk Navy chapel; Jack and Betty Crenshaw were best man and matron of honor.

Martha's father was Rear Admiral Wat Tyler Cluverius, USN; her maternal grandfather was Admiral William Sampson, USN, who was of course Admiral Cluverius' father-in-law; Deak Parsons, Martha's husband until his death in 1953, was Rear Admiral William Sterling Parsons; several years following Admiral Parsons' death Martha remarried to take as her second husband Rear Admiral Robert Burroughs, USN. The extraordinary military credits and honors of each of these admirals and the statuesque character and nobility of their wives are known to the more inquiring readers of U. S. naval history but the family history, including the succeeding generations, is an American epic which no author has not yet comprehended. The Golden Plates of the Mormon Church, as example, were discovered on Admiral William Sampson's farm in New York state. Martha Cluverius Parsons Burroughs was a woman of extraordinary character and abilities, as are her daughters, and their children's generation.

However, in July 1944 Capt. Parsons at Los Alamos arranged with Admiral Purnell to have his brother-in-law, Capt. John "Jack" S. Crenshaw, appointed to the Port Chicago Navy Court of Inquiry by Commandant of the Twelfth Naval District (San Francisco) Rear Admiral Carleton H. Wright.

By the appointment of Capt. Crenshaw to the Port Chicago Navy Court of Inquiry the judicial integrity of the court was corrupted. The

Archives at Los Alamos National Laboratory hold many documents, including transcriptions of telephone conversations between Captains Crenshaw and Parsons made during the proceedings of the court, which show that Capt. Crenshaw had, if not specific cognizance, at least sufficient reason to believe that the cause of the Port Chicago explosion had not been the accidental detonation of conventional munitions but had been the purposeful proof detonation of the Mark II nuclear fission bomb.

Captain Crenshaw did not, in the record of the proceedings of the court, disclose that information to the court, and that known cause of the Port Chicago explosion was not therefore disclosed to assist the defense of those men subsequently charged and convicted in summary courts-martial proceedings nor disclosed to assist the defense of those men subsequently charged and convicted of mutiny-in-wartime by formal court-martial proceedings in the aftermath of the Port Chicago explosion. That information was then SECRET, but even so those men convicted in summary and formal courts-martial proceedings were by that deprivation of substantive fact denied procedural due process and all those courts-martial convictions, summary and formal, should now immediately be vacated by the Judge Advocate General of the United States Navy.

Photographs and illustrations credits.

Port Chicago explosion seismic record, “Gal. Z.” One seismogram of the 17 July 1944 Port Chicago Naval Magazine explosion, made 30 kilometers from the source on the Galitzin Z recorder at the University of California, Berkeley. The Government seized all the original seismic records of the Port Chicago explosion and did not permit University of California, Berkeley, seismologist Professor Perry Byerly to publish those seismograms and analysis of those seismograms until October 1946. The same journal issue in which Professor Byerly’s analysis of the Port Chicago seismic records was published also published California Institute of Technology Professor B. Gutenberg’s “Interpretation of records obtained from the New Mexico atomic bomb test, July 16, 1945.” The Government did not permit Professor Gutenberg to publish the seismograms of the 16 July 1945 Trinity Site test; those seismograms have not since been published. Several years ago Los Alamos National Laboratory Archives found the seismograms of the Trinity Site test could not be located. Source: Byerly, Perry. “The Seismic Waves from the Port Chicago Explosion.” *Bulletin of the Seismological Society of America*, Vol. 36, No. 4, October 1946.

Chapter

16

George T. Reynolds, Russian espionage, shots Ruth and Ray, 1953

Ensign George T. Reynolds, USNR

Ensign George T. Reynolds, USNR, contributed significantly and uniquely to the reports and analyses of the Port Chicago explosion prepared under Captain William S. Parsons' direction at Los Alamos during the several months following the explosion, which reports and analyses were transmitted by Captain Parsons to Atomic Bomb Military Policy Committee member Rear Admiral William R. Purnell. At Los Alamos, Captain Parsons was Ensign Reynolds' commanding officer.

On 24 April 1944 George Kistiakowsky wrote to James Conant and named Reynolds among eleven men from among whom "we would like to have a minimum of six men." George T. Reynolds was then at Princeton University working under Professor Walter Bleakney in National Defense Research Committee (NDRC) Division 2, Structural Defense and Offense. On 25 April Conant requested Vannevar Bush to instigate Reynolds' transfer to Los Alamos; Conant added, "There will be a kick here." By 9 May Reynolds had not agreed to the transfer.

Ensign Reynolds was never a "happy camper" at Los Alamos. In one undated letter mailed from Santa Fe 14 February 1945 to NDRC Chairman James Conant, Reynolds asked Conant to find some means to arrange his transfer from Los Alamos. Reynolds was discontent at

Los Alamos because he found he was inconveniently subordinate to men of higher military rank than his own but men of inferior scientific and technical accomplishments. Furthermore, his assignment at Los Alamos did not provide enough important work for him to do. In this letter to James Conant, Ensign Reynolds wrote:

“Since I am here entirely at your request, I feel it is about time to submit an informal report. Frankly it is only just recently that I have overcome my initial disappointment at missing my Ft. Pierce [Florida] assignment, but I can now say I am trying to make the best of it . . . part of my trouble has been in not having enough important work to do . . . it has been difficult to find myself in my own field with in [sic] my own work, unable to move with the freedom that the NDRC accorded me as the result of my experience & PhD in physics . . . after 7 months I am beginning to feel the limitations of the rank of Ensign. I am extremely fortunate in having a fine Navy Commanding Officer here [Captain Williams S. Parsons]. He has been very understanding & I would not want him to think I am discontent, & so would appreciate your confidence in the matter. I realize this request borders on being presumptuous, but am making it after several weeks deliberation. I would very much appreciate hearing from you, as I am trying to maintain as many of my old contacts as possible.” Signed, *Geo. T. Reynolds*

Ensign Reynolds would have preferred to spend the years of his World War II military service with Professor Bleakney on the beautiful beaches at the U.S. Naval Amphibious Training Base at Fort Pierce, a few miles south of Vero Beach on the Atlantic coast of Florida where, no doubt, Ensign Reynolds would have made an unremarkable contribution to the nation’s war effort, as his contribution to the nation’s war effort at Los Alamos would have been unremarkable, except his definitive contributions to analysis of the Port Chicago explosion.

In an interview with Reynolds for the Rutgers Oral History Archives of World War II, conducted by Sean D. Harvey and Shaun Illingworth in Princeton, New Jersey, 29 October 1999, Reynolds narrated the events and process that led to his assignment at Los Alamos:

> “. . . Back in 1943 [sic, 1944], I had been ‘asked’ by Vannevar Bush, who was head of the OSRD [Office of Scientific Research and Development], and James B. Conant of Harvard University, who was head of the National Defense Research Council [sic; Committee], to go West and work on a project that several of my acquaintances here had already gone to work on. Everybody knew what that was. However, I didn’t want any part of it. Not for any moral reasons, it was all right with me, but I wanted the action that the amphibious warfare training promised to me. But I made a mistake. I was summoned to Washington, and I was interviewed by Conant, and he said, ‘You know, you’re the only one that we’ve been trying to get that has refused to go there. And I don’t think you’re very patriotic.’ And that’s where I made my slip. I said, ‘It’s not that I’m not patriotic. I’ve got myself a commission in the US Navy.’ ”

Reynolds’ “slip,” by which he made known to James Conant his status as an ensign of the United States Naval Reserve, resulted in immediate Navy orders that Reynolds proceed within four days to Santa Fe and there report to Captain Parsons. Ensign Reynolds did not, apparently, present himself to James Conant at that Washington meeting in Navy uniform. The investigator must wonder if Ensign Reynolds could have foreseen the consequence of his “slip”—an unwelcome assignment to Los Alamos—would he have permitted James Conant to hold the false perception that he, Ensign Reynolds, was a civilian rather than an officer of the United States Naval Reserve?

Ensign Reynolds was 27 years old and “very egotistical.” He considered that his Ph.D. in physics that he had received from Princeton University one whole year earlier, and the abundant scientific and technical experience he had accumulated as a graduate student at Princeton entitled him to much more authority and respect in the overall scientific and military community at Los Alamos than he had been accorded. Being then “very egotistical,” Ensign Reynolds must have felt he was amply justified in evasion of at least one military regulation at Los Alamos that he reckoned inconvenient to his own purposes. One military regulation in force at Los Alamos required that all his personal mail that would be sent off-base be first submitted to Los Alamos U.S. Army censors. On 14 February 1945 Ensign Reynolds mailed his complaining letter to James Conant from the U.S. Post Office in Santa Fe, by which evasion of military regulation the

Army censors at Los Alamos and his commanding officer Navy Captain Parsons were unaware of his complaints. As he wrote to James Conant, "I would not want him [Captain Parsons] to think I am discontent, & so would appreciate your confidence in the matter."

James Conant, however, ignored Ensign Reynolds' request for confidence in the matter and in 1981 caused Ensign Reynolds' letter to be reproduced in the "Bush-Conant File Relating to the Development of the Atomic Bomb, 1940-1945." Ensign Reynolds' letter, the envelope in which Reynolds mailed the letter from Santa Fe, Conant's office acknowledgment of receipt of the letter made 17 February by Ruth E. Jenkins, and Conant's own responsive letter of 9 March are all reproduced on Reel 10, Group 156 "R" of that 14-reel collection produced by the National Archives.

On 9 March 1945 Harvard University President, National Defense Research Committee Chairman, Atomic Bomb Military Policy Committee Alternate Chairman, and member of the British-American atomic bomb Combined Policy Committee James Conant wrote his temperate response to George Reynolds' chummy letter of 14 February:

> "Dear Ensign Reynolds:
>
> "I am sorry to have been delayed in replying to your letter and sorry that I was not able to see you personally and talk over your problem. I can readily understand some of the difficulties under which you have labored. I wish I could do something to help you out, but I am afraid I am not in a position to remove the limitations of which you speak.
>
> "After all, I think you would have to find consolation in the fact that these limitations would have been quite as severe in your work at Fort Pierce if not more so, but in this case you would have been dealing entirely with commissioned personnel and not mixed up with a civilian organization.
>
> "I certainly hope that in the future your work will prove more interesting than in the past and that you will feel in the long run that your transfer to this particular task was not too great a sacrifice.
>
> "Very sincerely yours,

> "James B. Conant"

In the Rutgers interview Dr. Reynolds reported his attitude and frame of mind when he arrived at Los Alamos:

> "I wasn't happy. I was assigned to the group that I knew were after me: a Harvard chemist, George Kistakowski [sic], a very colorful fellow, who is well known to the history of the atomic bomb. He recognized immediately that he had an unhappy camper there. I went to his office at Los Alamos as soon as I arrived there. He said, 'Hello, I'm glad to see you.' And I said, 'I'm not glad to be here.' He said, in his Russian accent, 'Oh, God! . . . What's wrong?' And I told him I'd gotten married, and my wife, Virginia, was down in Santa Fe on a street corner with our luggage, and I'd been taken by MPs [Military Police] into a car and brought up to the hill."

Reynolds did acknowledge in this Rutgers interview, "I was young, very egotistical . . . and of course, we thought the civilians knew more than the Navy." In the Rutgers interview Reynolds does also acknowledge that his commanding officer Captain Parsons "was a fine gentleman." Princeton University Professor of Physics Emeritus George T. Reynolds is 58 years older than in 1944 when he was young and "very egotistical," but assessing my interactions with the man these last 20 years I have found that only the qualitative degree of the adverb that he used predicatively in his self-description is less fitting today than in 1944 and would be better qualified now as "somewhat less than very egotistical." Professor Reynolds is member emeritus of The New Jersey State University at Rutgers Board of Trustees; he has spent much of his time since retirement at the Woods Hole Marine Biological Laboratory and Woods Hole Oceanographic Institution on Cape Cod, Massachusetts.

On his work in analysis of the Port Chicago explosion Professor Reynolds explained in the Rutgers interview:

> "I went to Port Chicago, and spent about a week there doing every kind of analysis I could think of to estimate the blast effect of the ammunition ship. Using collapsed oil drums, knocked over telephone poles, windows dished in miles away, sides of railroad cars, all of which could be analyzed physically, mechanically. I came up with a ridiculous answer, which was that fifteen hundred fifty tons of

> TNT went off. I said, 'Fifteen fifty, plus or minus fifty.' Today, knowing what I know about physics and the experiences that I've had in the field, I would have said, 'Well, it's somewhere between 1000 and 2000.' But not me, I was very confident of my work. When it was all said and done, and they got the bill of lading out, it turned out that there were fifteen hundred forty tons, so I was immediately considered an expert, purely by accident."

The compete Rutgers interview is available at:

http://oralhistory.rutgers.edu/Interviews/reynolds_george.html

Fort Pierce, Florida; the DOLOC Committee

In August 1943 Commander in Chief (COMINCH) Admiral Ernest J. King, USN, asked the Navy's Coordinator of Research and Development Rear Admiral Julius A. Furer to set up within NDRC a project to study the Demolition of Obstacles to Landing Operations (DOLOC). John E. Burchard, Chief of NDRC Division 2, Structure Defense and Offense, was DOLOC Committee chairman. DOLOC members included Princeton University Professor Walter Bleakney who was Deputy Chief of Division 2 and George Kistiakowsky who was Chief of NDRC Division 8, Explosives. The committee representative in England was H. P. Robertson. John Burchard's 29 July 1944 report to Rear Admiral Furer, "Damage Survey at Port Chicago, California," is reproduced in Chapter 11. The work of the DOLOC Committee began at an orientation meeting with the Navy on 22 September 1943 at which Admiral Furer presided.

The investigation of obstacles to landing operations, and their elimination by explosives, was undertaken principally in anticipation of the Allied Forces June 1944 Normandy landing. The DOLOC experimental obstacle demolition programs were conducted at Fort Pierce with large explosive charges—aerial bombs, individually placed mines, and explosives-laden remote-controlled boats—to determine the size and placement of charges that could accomplish the destruction of shallow water and submerged obstacles emplaced by the German defenders. However, the submerged craters that resulted from detonation of those necessarily large demolition charges could trap and likely would drown heavily laden Allied troops slogging ashore through the otherwise low-tide shallow water access to the beaches. Studies made at Fort Pierce determined the explosive charge weight necessary to destroy expected landing obstacles, the depth of craters resulting from those charge weights, the elevated lip of the resulting craters, and the time required for wave action to remove the stumbling-block crater lip and refill the crater.

The largest experimental charge detonated statically in the course of DOLOC investigations at Fort Pierce was 64,000 pounds, under shallow water on 3 February 1944, which cleared underwater obstacles within a circle 160 feet in diameter. On 4 October 1943 a charge weight of 6,800 pounds of TNT was similarly detonated, which cleared obstacles in a circle 80 feet in diameter. The experimental underwater detonations conducted at Fort Pierce confirmed a previous rough equation that the diameter of cleared circle in feet would equal approximately twice the cube root of the charge weight expressed in pounds—the “cube root law.”

Twice the cube root of a submerged demolition charge weight (pounds TNT) best described the crater results of the submerged demolition experiments done at Fort Pierce, but 3.70 times the cube root of the charge weight (pounds TNT) best described the results of crater experiments done on the surface of clay soil, also done by NDRC Division 2. Generally speaking, 3.70 times the cube root of the charge weight best predicts the diameter increase of all physical effects that result from a ground surface explosion, chemical or nuclear. Because the depth of the water beneath the exploded Liberty ship SS *E. A. Bryan* at the Port Chicago Naval Magazine pier was slight compared to the charge weight of the explosion, the Port Chicago explosion is usually defined as a ground surface explosion.

Measurements of the crater formed in the Suisun Bay bottom beneath the exploded Liberty ship SS *E. A. Bryan* at the Port Chicago Naval Magazine pier were immediately used by Los Alamos to confirm applicability of the cube root law to multi-kiloton explosions. **Enclosure (B)** of Captain Parsons’ “Port Chicago Disaster: Final Report” to Admiral Purnell, dated 16 November 1944, is Dr. Maurice

Shapiro's "Analysis of crater in bottom near ship pier." In that report Dr. Shapiro wrote:

> "A comparison has been made of the crater in the Port Chicago explosion with those created in a large number of TNT explosions in clay soil. In experiments performed by Division 2 of NDRC* (*'Effects of impact and explosion,' Sheet No. 3B-1, September, 1943), with charge weights ranging between 100 and 4000 pounds, the following empirical equation relating crater diameter D (feet) to charge weight W (pounds) was deduced for explosions occurring at the surface of the ground:
>
> "$D = 3.70W^{1/3}$
>
> "Applying this to the Port Chicago explosion, we have D= 3.70 x 146 = 540 feet. The crater diameters in the NDRC experiments exhibited approximate cylindrical symmetry. They were measured at the original ground surface between shear shoulders. The diameters estimated above for the Suisun Bay crater, namely 600 and 300 feet, were similarly measured at the original bed-surface under the SS E.A. Bryan. The qualitative agreement between the crater size predicted by extrapolation and the actual size is surprisingly good if one considers the distribution of charge in the ship, the location of the center of gravity of the charge 20 feet above the bottom, and most significantly, the considerable energy absorption by the intervening water."

In his Port Chicago damage survey to Rear Admiral Furer of 29 July 1944, John Burchard wrote in paragraphs 1.a, 1.b., 13, 15.a. and 15.b.:

> "1. Purpose of Survey.
>
> "a. For information it might yield as the effect of very large charges when used in bombardment. The location of the charge below the water line was of course one which would be expected to result in less damage to structures than might arise certainly from air blast in the open and probably from earth shock if the charge had been buried in earth.
>
> "b. For information as to the effect which the detonation of a large underwater charge near the shore might have on enemy underwater obstacles and nearby shore fortifications. A simulation of underwater obstacles was available in the piling supporting the piers and of shore installations by adjacent revetments used to protect loaded

freight cars. These were of standard construction of piling with earth-filled walls and might be taken as reasonably representative of a bunker, though on a large scale. On the other hand, the charge was not located for optimum results as it was supported well off the bottom by the hull of this ship.

"13. Underwater effects. Crater not yet measured. If we take [the Port Chicago] charge as 4,000,000 pounds and compare with 64,000 fired at Fort Pierce, we would expect crater radius of circa 320' from cube root law."

"15. Conclusions.

"a. The detonation of such a load among enemy obstacles would neither:

(1) guarantee a satisfactory passage, or

(2) stun the enemy long enough or cause enough casualties to impair his defenses. Our own personnel rallied immediately.

"b. The radii of positive and worthwhile damage to be expected from such charges will not exceed those postulated by the $W^{1/3}$ rule and will probably be less."

Russian espionage and the uranium hydride bomb

As shown in Chapter 13, in his letter of 5 February 1939 to physicist George Uhlenbeck, J. Robert Oppenheimer first proposed a uranium hydride nuclear fission bomb to utilize the deuterium hydrogen isotope in a U^{235} metal-deuterium compound. In development at Los Alamos, Oppenheimer's 1939 concept of a uranium-deuterium fission bomb would be named the Mark II by James Conant on 4 July 1944.

On that date in memorandum to General Groves, Conant forecast the Mark II would yield an energy of explosion equivalent to 1,000 tons of TNT; the Mark II was successfully proof fired at the Port Chicago Naval Magazine the evening of 17 July 1944. On 17 August 1944 by memorandum Conant informed General Groves of the decision taken at Los Alamos, in consequence of the Port Chicago explosion, that the Mark II should be put on the shelf, and Conant's memorandum of 17

August 1944 to General Groves acknowledges that the then known upper limit of effectiveness of the Mark II could be improved somewhat and developed for combat use in 3 or 4 months time. The Mark II uranium hydride bomb was the first practicable and proven nuclear fission weapon.

Despite all that historical significance, in his comprehensive review of the Manhattan Project history, *The Making of the Atomic Bomb*, Richard Rhodes mentions uranium hydride only on pages 610 and 611, in discussion of Otto Frisch’s bench-top critical mass experiment which Richard Feynman described allegorically as tickling the tail of a sleeping dragon—because of the distinct hazard that the experiment in progress could accidentally go awry and propagate a violently explosive nuclear fission energy release, lethal prompt radiations in the immediate area, smoke and fire, as of an aroused and angry fire-breathing mythic dragon.

On February 5, 1939 Oppenheimer proposed what would become the Mark II. On 21 August 1943 the Atomic Bomb Military Policy Committee informed Vice President Henry Wallace, Secretary of War Henry Stimson and Chief of Staff General George C. Marshall, “There is a chance, and a fair one if a process involving the use of a [uranium] hydride form of material proves feasible, that the first bomb can be produced in the fall of 1944.” Eleven months later the Mark II was successfully proof fired at Port Chicago.

On 16 March 1945, eight months following the successful proof of the Mark II, Russian nuclear physicist Igor Kurchatov wrote an assessment of the technological value of materials recently obtained by the NKGB from spies inside U.S. military bases and war plants. Among those materials that on 5 March Kurchatov was provided to review was information that the U.S. had the uranium hydride bomb concept in development. In his 16 March report to NKGB chief Lavrenti Beria, on the technological value of those materials Kurchatov had received for review on 5 March, Kurchatov wrote that the materials were of great interest. Kurchatov noted two particular ideas mentioned in those materials to be of especial interest: 1) the use of uranium hydride 235 instead of metallic uranium 235 as the active material of an atomic bomb, and 2) implosion as a method to detonate an atomic bomb.

An English translation of portions of Kurchartov's 16 March 1945 report to Beria is found as Document No. 8 in Appendix Two, pages 458 and 459, of *Special Tasks* by Pavel and Anatoli Sudoplatov with Jerrold L. and Leona P. Schecter (New York: Little Brown & Co., 1994; updated edition June 1995).

The copyrighted translation of that report, commissioned by the Schecters, reads in part:

> "The utilization of uranium-hydride 235 instead of uranium 235, as the materials suggest, is based on a great degree of probability of the absorption of low-velocity neutrons by uranium, which provides for diminishing the critical mass. The introduction of hydrogen, however, retards the entire process and may drag it out to impermissibly long periods of time. Besides, because of the low density of the substance, the critical mass needs to be increased. Therefore, it is far from obvious that the use of uranium-hydride instead of uranium will yield that significant (almost 20-fold) gain with regard to the mass, which the materials suggest.
>
> "The proposal in question can only be gauged after a stringent theoretical scrutiny of the matter. . . [Schecters' redaction].
>
> "It seems exceptionally important to establish whether the system described was studied through calculation or by way of an experiment. If the latter, that would mean that the atomic bomb has already been executed and that uranium 235 has been separated in major quantities. The materials contain a remark that seems to suggest that. In describing the implosion method it is pointed out that no experiments have yet been carried out with active material. . . ."

Because the Schecters have deleted part or parts of the whole text from their English transcription of Kurchatov's 16 March 1945 report to Beria it is impossible to know certainly from their text if "the system described" by Kurchatov is in fact the uranium hydride bomb concept, although contextually "the system described" appears to be the uranium hydride bomb concept. The Schecters have not responded to a request to obtain the deleted part or parts of their commissioned translation of Kurchatov's report to Beria. Kurchatov's 16 March 1945 report to Beria was published in the Russian Academy of Sciences journal *Questions of History of Natural Science and Technology,* No. 3, 1992 (*Voprossi Istorii Estestvoznania i Tekhniki*). The Schecters' book *Special Tasks* was lambasted by the critics in 1994, but some of the criticisms made of that first edition are addressed in the updated 1995 edition.

Joseph Albright and Marcia Kunstel in their book *Bombshell. The Secret Story of America's Unknown Atomic Spy Conspiracy* (New York: Times Books/Random House,1997) detail the life of the Manhattan Project physicist Theodore Alvin Hall, apparently known to his Russian handlers as "Mlad." Albright and Kunstel believe Mlad was the person who provided the Russians with the information that Los Alamos was working on a uranium hydride bomb. Albright and Kunstel wrote that, in the course of their interviews with Hall in the 1990s, he didn't recall knowing anything about the uranium hydride bomb, but Albright and Kunstel comment editorially, "at the time he probably did know of it."

Albright and Kunstel on page 125 propose their reasons to believe that Hall provided the Russians with information about development of the uranium hydride bomb at Los Alamos:

> "A second clue pointing in Ted Hall's direction was that the raw document that so interested Kurchatov stressed the possibility of making a bomb of uranium hydride. Because of the odd history of the uranium hydride bomb, it is possible to triangulate a sixty-day period during which that information most likely passed into the hands of the NKGB. That window lasted from late November 1944 to late January 1945—a period that contained Hall's meeting with [Saville] Sax in Albuquerque. It was only in this brief span, Los Alamos records show, that the laboratory possessed enough U-235 in the form of uranium hydride to make a critical mass. Starting in November 1944, metallurgists had converted twelve kilograms of U-235 into 1,350 small cubes of uranium hydride. The cubes were for the critical assembly experiments carried out by Otto Frisch's G-I group. Twice in those two months Frisch and his assistants did stack together enough hydride cubes to reach a chain-reacting critical mass. Very likely it was this same pile of uranium hydride cubes that [Vsevolod] Merkulov had in mind when he wrote to Beria on

> February 28: ‘The Americans already have the necessary amount of active substance for two or three bombs of lesser effectiveness.’ ”

Albright and Kunstel were not aware in 1997, nor were Beria and Kurchatov in 1945, that the Mark II uranium hydride bomb required only 9 kilograms U^{235}, nor did they know that at least that minimum quantity had been produced by Philip Abelson at the Naval Research Laboratory during 1943. Albright and Kunstel are, therefore, incorrect in their statement that only during the 60-day period between November 1944 and late January 1945 did Los Alamos possess “enough U-235 in the form of uranium hydride to make a critical mass.”

Albright and Kunstel wrote that only during that triangulated period of 60 days from late November 1944 to late January 1945 would information about the U.S. hydride bomb “most likely” have passed into the hands of the NKGB. But information about the U.S. hydride bomb concept and development could have passed to Russian intelligence anytime after Oppenheimer first proposed that concept to George Uhlenbeck in his letter of 5 February 1939.

After distribution of the Atomic Bomb Military Policy Committee report of 21 August 1943, which noted the fair chance that the first (uranium) hydride bomb could be available by the fall of 1944, the British and Canadian members of the Combined Policy Committee knew certainly that the U.S. had the uranium hydride bomb in development.

Following James Chadwick’s visit to Los Alamos 29 July through the morning of 3 August 1944 the British and Canadian members of the Combined Policy Committee knew that the first uranium hydride bomb had been successfully proof fired at Port Chicago 17 July.

Information about the U.S. uranium hydride bomb development could have reached the Russians from American, British or Canadian sources. One possibility in Canada was Allan Nunn May. “The event which unraveled the spy network in Canada was the defection of Igor Gouzenko, a code clerk in the Soviet embassy in Ottawa, in early September, 1945. This led directly to a physicist-spy, code-named “Alek” engaged in wartime nuclear research in Canada. Gouzenko's

Soviet documents revealed him to be an Englishman named Allan Nunn May, a person who had had many leftist connections in prewar years. Nunn May, never at Los Alamos himself, nonetheless obtained information of interest to the USSR. He informed them of the nature of the Trinity and Hiroshima bombs, the U-235 output of the plant at Oak Ridge, and of Pu-249 at Hanford, and passed a small sample of U-233 to Soviet agents" (T. M. Sanders, University of Michigan;

http://www-personal.umich.edu/~sanders/214/other/handouts/chr_spy.html)

Klaus Fuchs should also be considered as one person who could have provided the Russians information about the uranium hydride bomb development. Fuchs worked closely with Edward Teller at Los Alamos, and the uranium hydride bomb was dominant among Teller's program interests and efforts. No mention of the uranium hydride bomb was made in Fuchs espionage trial in England, but Fuchs had no reason to mention another particular instance of his espionage than those that were before the court.

Someday in the clouded future the Russian Foreign Intelligence Service archives may locate and release the documents that Kurchatov reviewed for Beria from 5 to 16 March 1945; it will be possible then to ascertain what remark suggested to Kurchatov that the uranium hydride bomb had been tested, and it may then be possible to ascertain the source of that information. Very few persons were cognizant that the Mark II had been successfully proof fired 17 July 1944.

According to Albright and Kunstel, Vsevolod Merkulov wrote in his 28 February 1945 report No. 1103/M to Beria:

> "There is not any definite schedule for producing the first bomb because so far the design and research works haven't been finished. It is thought that a minimum of one year and maximum five years will be required to produce the first such bomb.
>
> "As for bombs of somewhat smaller capacity [i.e., the Mark II], it is reported that already within several weeks one can expect the manufacture of one or two bombs, for which the Americans already have available the necessary quantity of active substance. This

> bomb will not be so effective, but all the same it will have practical meaning as a new kind of weapon by far superior in its effectiveness to all the currently existing kinds of weapons. The first actual battlefield explosion is expected in two or three months."

Merkulov's 28 February information about U.S. production of smaller capacity bombs "within several weeks" and the first actual battlefield explosion "in two or three months" does not correspond to any forecast in James Conant's Los Alamos site visit reports to General Groves of 4 July and 17 August 1944, nor is that information forecast in his "Report on Visit to Los Alamos – October 18, 1944," nor in his "Summary of Trip to Los Alamos, December 1944." Merkulov's information of 28 February did not come from James Conant nor anyone in his office at the National Defense Research Committee.

Albright and Kunstel continue discussion of the uranium hydride bomb on page 126:

> "By the time Sax met Hall in Albuquerque, Oppenheimer and his division leaders were indeed toying with the option of trying to make several 'bombs of lesser effectiveness' out of uranium hydride. Edward Teller's hydride-gun idea had gone into and out of fashion, but it remained a live possibility until the end of December 1944. But after Sax' visit, the picture changed overnight, making the hydride bomb a dead letter. On January 1 [1945] Oppenheimer froze the design of Little Boy [Mark I], a bomb that needed all of the Manhattan Project's stock of U-235 in the form of pure uranium metal. Oppenheimer's metallurgists were ordered to convert all 1,350 hydride cubes into metallic uranium. By early February 1945, the uranium hydride cubes were gone and the option of making several small bombs "of lesser effectiveness" had disappeared."

In fact, on 17 August 1944 James Conant reported to General Groves the decision taken at Los Alamos to put the 1,000-ton TNT equivalent Mark II on the shelf, with recognition Mark II could be taken off the shelf and developed for combat use in 3 or 4 months time if required, and with the possibility of some energy yield improvement. The **option** of making several small bombs of lesser effectiveness, i.e., the Mark II, had not "disappeared" by early February 1945; that **option** was remitted after 17 August 1944 and was not revisited until 31 March and

11 April 1953 in shots Ruth and Ray of the Upshot-Knothole series of tests conducted at the Nevada Proving Ground.

Shots Ruth and Ray, uranium hydride experimental devices

After the war Los Alamos physicists were skeptical of the usefulness of uranium hydride in weapons. Edward Teller remained interested in the concept though and, as he had at Los Alamos during the war to assure development of the Mark II uranium hydride bomb, Teller used his prominent position to push hydride weapon development when the University of California Radiation Laboratory (UCRL) weapons lab opened in Livermore, California. Ruth and Ray were both uranium hydride experimental devices designed and produced by Edward Teller and Ernest Lawrence at UCRL, later the Lawrence Livermore National Laboratory (LLNL). Ruth was the first device fielded by UCRL and was detonated 31 March 1953; Ray was detonated 11 April 1953. Both yielded an energy of explosion equivalent to 200 tons of TNT, which is the same energy of explosion produced by the proof detonation of the Mark II uranium hydride experimental device at the Port Chicago Naval Magazine 17 July 1944. Review of the reported ionizing radiation effects that resulted from shots Ruth and Ray permits approximation of the probable ionizing radiation effects that resulted from the 17 July 1944 proof detonation of the Mark II.

Ruth, named Hydride I, was detonated at 0500 hours, 31 March 1953 atop a 300-foot tower at the Nevada Proving Ground. The energy yield was 0.2 kiloton (200 tons TNT equivalent). The Atomic Energy Commission (AEC) objective was to evaluate the nuclear yield, blast, thermal and radiological phenomena produced by this experimental device. The Department of Defense (DOD) objective was to measure the effects of the detonation and evaluate the military applications of the device. The top of the cloud reached an altitude of 13,600 feet.

Ray, named Hydride II, was detonated at 0445 hours, 11 April 1953 atop a 100-foot tower at the Nevada Proving Ground. The energy yield was 0.2 kiloton (200 tons TNT equivalent). The AEC objective was to evaluate the nuclear yield, blast, thermal and radiological phenomena produced by this experimental device. The DOD objective was to evaluate military equipment, tactics, and doctrine; to measure effects characteristics and evaluate the military applications of the device; and to orient military personnel in the tactical uses of nuclear weapons. The top of the cloud reached an altitude of 12,800 feet.

The public report “Shots Annie to Ray” (Defense Nuclear Agency report DNA 6017F) does not identify the active material employed by the devices detonated in shots Ruth and Ray, but elsewhere in the Department of Energy (DOE) literature Ruth is identified as “Hydride I” and Ray is identified as “Hydride II.” Both were necessarily U^{235}-enriched uranium hydride devices. The degree of U^{235} enrichment is not reported. The popular literature, without any documentary reference, reports only one difference between the Ruth and Ray devices: the uranium hydride active for shot Ray (Hydride II) was, specifically, a uranium deuterium (^{2}H) compound; by implication the uranium hydride active for shot Ruth (Hydride I) was either U^{235}-enriched uranium compounded with the naturally occurring abundance of hydrogen isotopes, or the hydrogen (^{1}H) or tritium (^{3}H) isotopes.

It seems improbable that Edward Teller and Ernest Lawrence would have spent time, money, effort, and a quantity of separated U^{235} to develop and test a uranium hydride device, Ruth, that would employ a compound of uranium and natural hydrogen or a compound of uranium and the ^{1}H isotope. From February 1939 it was known that a uranium deuterium compound would be the most efficient uranium hydride active material. For that reason, the Mark II employed a uranium deuterium active, and the proof detonation of the Mark II effectively demonstrated the efficiency of a uranium deuterium device. Hydride I (Ruth) was intended for use as a primary in a compact thermonuclear bomb system; conceivably the test of a uranium tritium device would have provided information and data useful to that design and purpose.

However, all we know certainly is that active material of Hydride I and II, Ruth and Ray, was uranium hydride and that the test detonation of the two each produced an energy of explosion equivalent to 200 tons of TNT, which is the TNT energy equivalent produced by the Mark II uranium deuterium Mark II experimental device proof fired 17 July 1944 at the Port Chicago Naval Magazine.

Projects done in conjunction with both shots Ruth and Ray evaluated the nuclear yield, blast, thermal, seismic, electromagnetic radiation, initial gamma radiation versus distance, radioactive fallout dispersal, airborne sound, and indirect damage. Shot Ray also included cloud penetration, cloud sampling and radiochemistry analysis of the obtained samples; shot Ruth did not. In addition to the same projects done at shots Ruth and Ray, shot Ray included troop orientation and indoctrination—71 DOD personnel positioned as observers 16 or 18 kilometers from ground zero. The principal DOD exercise that accompanied shot Ray was to provide Marine Corps operational tests designed to investigate factors that might affect the use of helicopter assaults under the conditions following a battlefield nuclear detonation: flash blindness, overpressure, and ground and airborne radioactivity. Three helicopters were employed in the exercise.

The radiological effects measurements obtained from detonation of the uranium hydride devices Ruth and Ray provides information sufficient to assess the probable radiological consequences of the proof detonation of the uranium hydride Mark II at the Port Chicago Naval Magazine 17 July 1944.

Ionizing radiation consequences, Ruth and Ray

Ionizing radiation survey data for shots Ruth and Ray were reported as roentgens/hour (R/h), which is equivalent to Roentgen Equivalent in Man (REM). Many different systems and units are employed to measure and quantify ionizing radiation. The published DOE ionizing radiation survey data for shots Ruth and Ray are reported as roentgens/hour and are so reported here. Following the discussion, below, of the Ruth and Ray ionizing radiation survey findings, information is presented which correlates ionizing radiation exposure levels with short-term human health effects and mortality. One week continuous exposure to 1 R/h would be expected to produce no medical consequence. The ionizing radiation survey data obtained immediately following shots Ruth and Ray permit the conclusion that no adverse effect to short-term human health was probable in consequence of the proof detonation of the Mark II at the Port Chicago Naval Magazine. Long-term human health effects that may result from one-time or intermittent exposure to low levels of ionizing radiation is a subject debated with the same want of definitive conclusion as the debate to definitively settle the Origin of Life.

Ruth

The Ruth device, Hydride I, is reported to have been 56 inches in diameter, 66 inches long and to have weighed 7,400 pounds. A betatron is reported to have been used for initiation. The weight and dimensions of the Ray device, Hydride II, and the initiation mechanism for shot Ray are not available.


Shot Ruth – Remains of the Tower

Ruth was detonated atop a 300-foot tower in the open air. Only the top 100 feet of the steel tower were vaporized, so the fireball of shot Ruth did not exceed a radius of 100 feet and therefore did not contact the ground. Ground surface material was not vaporized by the Ruth fireball, which limited the material entrained by the Ruth fireball and rising cloud that could be distributed as radioactive fallout.

The Mark II was detonated 10 feet below the waterline, within the hull of the Liberty ship *E. A. Bryan*. The fireball generated by the proof of the Mark II at Port Chicago did contact steel portions of the ship as it initially formed, but probably did not contact Suisun Bay water. More radioactive debris was certainly produced by the proof of the Mark II—and available to form radioactive fallout—than was produced by shot Ruth, but the quantitative difference of vaporized material and particulate matter generated by the two detonations was small. The amount of radioactive fallout that resulted from the proof of the Mark II at Port Chicago was greater than that which resulted from shot Ruth, but the difference was so slight that the radioactive fallout from the two detonations can be considered to have been effectively the same.

The prompt gamma radiations emitted by the detonation of Ruth were attenuated only by the surrounding atmosphere, and earth immediately beneath the shot. Much of the prompt gamma radiations emitted by the Port Chicago proof of the Mark II was attenuated by the steel hull of the ship before it disintegrated.

There was an insignificantly greater amount of radioactive fallout available to be deposited over a wide area downwind of Port Chicago than was available to be deposited in consequence of shot Ruth, but significantly less prompt gamma radiations affected the immediate area of the Port Chicago explosion, within 1,000 feet, than affected the immediate area of shot Ruth.

One B-25 aircraft spent four hours tracking the Ruth cloud at 12,000 feet, and encountered a maximum radiation intensity of 0.1 R/hour. That reading was made at the cloud periphery because aircraft did not penetrate the Ruth cloud.

The gamma radiation spectrum of residual contamination and initial gamma exposure versus distance data were obtained by the U.S. Army Signal Corps Engineering Laboratories to characterize the gamma radiation resulting from the Ruth detonation. The initial gamma exposure data from shot Ruth have not been published, but were probably a composite measure of prompt and delayed gamma.

During the first 80 minutes following the Ruth detonation a radiation ground intensity survey was made by an H-5 helicopter at heights ranging from five to 50 feet above the ground. The highest radiation intensity, 1.0 R/h at a height of ten feet above the ground, was measured near ground zero. One C-47 and two L-20s surveyed fallout radiation intensities as far as 320 kilometers offsite at heights ranging from 500 to 800 feet. Those aircraft detected negligible amounts of radiation.

On the ground surface within a radius of 50 meters of ground zero for the Ruth detonation the radiation intensity was initially 10.0 R/hour. At 24 hours, 1.0 R/hour. At the end of 72 hours radiation intensity on the ground was 0.01 R/hour to a maximum radius of 150 meters from ground zero. The onsite fallout was minimal; intensities exceeding 0.1

and 0.01 R/hour were found as far as four kilometers from ground zero in a narrow band to the south.

Ray


Shot Ray detonation, 11 April, 1953

Radiation surveys done following shot Ray were not so thoroughly conducted as for shot Ruth.

In one of the Ray DOD exercises conducted immediately after the shock wave passed, one of the three helicopters in the exercise proceeded toward the shot area and then landed about 150 meters from ground zero. A radiation monitor disembarked and during a period of ten minutes recorded radiation levels on the ground, 150 to 1,000 meters from ground zero. The highest radiation intensity recorded on the ground was 10.0 R/h, 510 meters from ground zero. All recorded intensities except the one made at 510 meters were less than 10 R/h within ten minutes after the detonation. The maximum intensity of onsite fallout encountered 30 minutes after the shot was 25 R/h, five feet above the ground in one isolated spot. An F-84G aircraft penetration of the Ray cloud was made 45 minutes after the detonation. A peak intensity of 40 R/h was detected. The Ray cloud was not tracked by aircraft. Low-flying aerial surveys conducted offsite, up to 320 kilometers, encountered a maximum intensity of 0.05 R/h.

Ionizing radiation exposures, short term human health and mortality effects

Short-term (several days), whole-body exposure in roentgens, probable effects. Source: “Emergency Exposures to Nuclear Radiation,” TM-11-1, and “Medical Aspects of Nuclear Radiation,” TB-11-24, Office of Civil and Defense Mobilization.)

000 - 100 R	No obvious effects
100 - 200 R	Minor incapacitation
200 - 600 R	Sickness and some deaths
Over 600 R	Few Survivors

An exposure of 1 R/h for 6 hours/day in the open air is considered “safe.” Persons exposed to one month continuous exposure at 1 R/h would be expected to suffer 50 percent dead; 15 days continuous exposure to 1 R/h would be expected to produce 5 percent dead; one week continuous exposure to 1 R/h would be expected to produce neither medical consequences nor, therefore, deaths.

Using the ionizing radiation survey data reported for shots Ruth and Ray as measures of the probable ionizing radiation levels produced consequent to proof of the 200 tons TNT-equivalent uranium hydride Mark II experimental device conducted at the Port Chicago Naval Magazine it is readily apparent that even the two men who survived the Port Chicago explosion at 1,000 feet under the rubble of the Joiner Shop, at the shore end of the pier, would probably not have suffered adverse short-term health consequences as the result of ionizing radiation exposure, prompt gamma nor subsequently from any local radioactive fallout. Neither of those two survivors showed any immediate effect of short-term ionization radiation exposure and one of the two, interviewed by the news media 55 years later, neither evidenced nor claimed any adverse health effect in consequence of the otherwise brutal drubbing to which he was subject 1,000 feet from the center of the Port Chicago explosion.

Those personnel in the Port Chicago Naval Magazine barracks and administration areas 1.5 miles from the detonation of the Mark II were in no way affected by the immediate gamma radiations produced by the detonation, nor were hazardous levels of radioactive fallout probable at that distance. Similarly, civilians in the adjacent town of Port Chicago were not subject to immediate or subsequent ionizing radiation hazards. Comparison of the survey data taken of the intensity of downwind radioactive fallout from the detonations of Ruth and Ray clearly shows that no widespread hazardous intensities of ionizing radiations from fallout were probable from the Mark II detonation at Port Chicago, although local hot spots may have occurred in the then remotely populated Sacramento Valley.

Those personnel who immediately entered the area of destruction at the shore end of the destroyed Port Chicago pier to conduct search and rescue were not exposed to any substantial radiological hazard. The men who subsequently recovered human remains and who did metal fragment plots and fragment recovery in the near vicinity of the detonation of the Mark II were not exposed to any radiological hazard of consequence.

Final remarks

Twenty-two years ago at a church rummage sale in Santa Fe, New Mexico, I recovered the “History of 10,000 ton gadget,” a document that Los Alamos photographic technician Paul Masters had purloined from the Manhattan Project laboratories at Los Alamos in winter 1944-1945. The bottom line of that document predicts that the ball of fire that would result from the 16 July 1945 nuclear bomb test at Trinity Site would occur in “typical Port Chicago fashion.”

A few days study of the “History of 10,000 ton gadget” persuaded me that if competent Los Alamos scientists had characterized the Port Chicago explosion fireball as having been typical of a nuclear fission explosion then the Port Chicago explosion had, according to the doctrine of necessitarianism, necessarily been a nuclear fission explosion. I subsequently learned that the men who had written that characterization of the Port Chicago fireball, Joseph O. Hirschfelder and William George Penney, were not just run-of-the-mill competent Los Alamos scientists but were brightest among the luminaries of the Manhattan Project scientists working at Los Alamos in winter 1944-1945.

In autumn 1980 Los Alamos National Laboratory Director Donald M. Kerr, now director of the Federal Bureau of Investigation Laboratory Division, challenged me to prove, if I could, that the Port Chicago explosion had been a nuclear fission explosion. *The Last Wave from Port Chicago* is my response to that challenge.

My critics demand that I produce a "smoking gun" document in proof of the work made here, so I venture to compose that document, which is a signed and handwritten direction from President Franklin D. Roosevelt to the Secretary of War Henry Stimson, dated July 7, 1944. Because that document would be substantially redacted if it were ever available to the public, I also provide the document as it would be redacted by the appropriate Government authorities.

> "By the authority vested in me as Commander-in-Chief of the Armed Forces of the United States of America, and additionally granted to me by the Congressional Declaration of War against the Empire of Japan, I hereby direct you to authorize the Joint Chiefs of Staff and Rear Admiral William R. Purnell, USN, the Navy member of the Atomic Bomb Military Policy Committee, in cooperation with appropriate civilian scientists and Armed Forces personnel assigned to the Manhattan Project laboratories at Los Alamos, New Mexico, to secretly detonate the prototype Mark II experimental uranium hydride nuclear fission bomb at the Port Chicago Naval Magazine as soon as practicable in order to prove the feasibility of large scale nuclear fission weapons, which are essential to the present and future national security, and by that proof detonation to determine by scientific analysis of the physical consequences of that proof the anticipated military consequences that will result from such use of an atomic bomb of comparable energy in the particular circumstances of an enemy harbor or maritime port and, moreover, to utilize detailed analyses of the consequences of that proof detonation to be made at the Port Chicago Naval Magazine to establish the anticipated military effects that will be realized from the use of the more powerful militarily-decisive nuclear fission bombs now in development by the Manhattan Project, in similar or other circumstances of combat. The exigencies and imperatives of the present War require

> that the proof detonation of the Mark II prototype atomic bomb here ordered shall be made by the parties without consideration of any physical consequences to property and persons which shall inevitably arise from execution of this order."
>
> Signed, *Franklin D. Roosevelt*

The administratively redacted text of President Roosevelt's direction to Secretary of War Stimson, the "smoking gun" document, would read:

> "By the authority vested in me as Commander-in-Chief of the Armed Forces of the United States of America, and additionally granted to me by the Congressional Declaration of War against the Empire of Japan, I hereby direct you to authorize [SENSITIVE INFORMATION DELETED] to determine by scientific analysis [SENSITIVE INFORMATION DELETED] the anticipated military consequences that will result from [SENSITIVE INFORMATION DELETED] use of an atomic bomb [SENSITIVE INFORMATION DELETED] now in development by the Manhattan Project [SENSITIVE INFORMATION DELETED]."
>
> Signed, *Franklin D. Roosevelt.*

Photographs and illustrations credits.

Shot Ruth, remains of the tower. Source: Lawrence Livermore National Laboratory.

Shot Ray, detonation. Source: Lawrence Livermore National Laboratory.

Appendix A

Document transcriptions: The liquid thermal diffusion uranium isotope separation method.

Contents

1943, January 15	Letter of H. T. Wensel, Technical Aide, National Defense Research Committee, to Rear Admiral W. R. Purnell.
1943, January 19	Letter of James Conant to Lyman Briggs, Director, National Bureau of Standards.
1943, January 23 (1)	Letter of Special Subcommittee of the S-1 Executive Committee: Lyman J. Briggs, Chairman; E. V. Murphree; Harold G. Urey.
1943, January 23 (2)	Letter of Special Subcommittee of the S-1 Executive Committee, Lyman J. Briggs, Chairman E. V. Murphree; Harold C. Urey.
1943, January 25	Letter of E. V. Murphree, Standard Oil Development Co., New York, NY, to Lyman Briggs.
1943, January 28	Letter of Harold C. Urey, Columbia University, to Lyman Briggs.
1943, January 30	Letter of Lyman Briggs, U.S. Department of Commerce, National Bureau of Standards; Chairman, Special Sub-committee to James Conant. Subject: Liquid Thermal Diffusion Plant.
1943, February 13	Memorandum for Admiral Purnell from Vannevar Bush.
1943, February 19,	“Program for experiments to be carried out on the thermal diffusion method,” E. V. Murphree and H. C. Urey.
1943, February 23	Letter of Lyman Briggs to James Conant, including:
1943, February 24	Letter of James Conant to General Groves.

1943, May 11	Letter of James Conant to Rear Admiral William R. Purnell.
1943, June 19	Letter of Lyman Briggs to Colonel T. C. Crenshaw, Manhattan District Office, Corps of Engineers, New York, NY.
1943, July 10	Letter of General L. R. Groves to James Conant.
1943, July 10	Letter of James Conant to Rear Admiral William R. Purnell.
1943, July 30	Letter of James Conant to Rear Admiral William R. Purnell.
1943, September 3	Memorandum from Chief of the Bureau of Ships, signed by H. A. Ingram by direction of Chief of Bureau, to Commander in Chief, U.S. Fleet. (Attention Rear Adm. W. R. Purnell).
1943, September 9	Letter of Ruth E. Jenkins [secretary to James Conant] to Harold Urey, Columbia University.
1943, September 15	Letter of James Conant to Rear Admiral W. R. Purnell.
1944, March 4	“Paraphrase of teletype” of J. R. Oppenheimer to James Conant.
1944, March 4	Letter of James Conant to Rear Admiral William R. Purnell.
1944, March 8	Letter [“Dictated—not read–not signed”] of General Groves to James Conant.
1944, March 17	Letter of Rear Admiral W. R. Purnell, Navy Department, Office of the Chief of Naval Operations, to James Conant.

1944, March 20	Letter of Ruth E. Jenkins to Dr. J. R. Oppenheimer, Box 1663, Santa Fe, New Mexico.
1944, April 20	Letter of James Conant to Lyman Briggs.
1944, May 6	Manuscript note of James Conant to Vannevar Bush.
1944, June 3	Letter of General Groves, War Department, Office of the Chief of Engineers, Washington, to James Conant.
1944, June 3	Memorandum of Mssrs. W. K. Lewis, E. V. Murphree and R. C. Tolman to Major General L. R. Groves. Subject: “Possible Utilization of Navy Pilot Thermal Diffusion Plant.”
1944, July 25	Memorandum of Chief of the Bureau of Ships [Vice Admiral Edward L. Cochrane] to Rear Admiral W. R. Purnell, U.S.N. (Op-05).
1944, July 27	Manuscript of James Conant, “Historical note on introduction of the Abelson-Gunn process.”
1944, September 15	Letter of James Conant to Lyman Briggs.
1944, September 21	Letter of Lyman Briggs to James Conant.

Document transcriptions

1940, September 9
Letter of George B. Kistiakowsky to Lyman Briggs, Director, U. S. Bureau of Standards.

“Dr. R. Clark Jones, the co-author with Furry and Onsager of the theory of thermal diffusion which has proved itself very well in the past, has discussed the advisability of further theoretical work in connection with the Uranium isotope separation work with E. H. Land of the Polaroid Corporation by who he is temporarily employed. Mr. Land telephoned Dr. Vannevar Bush and the letter suggested that Jones talk the matter over with me. This letter is a result of our conversation.

“It appears that Jones and Furry and developed general expressions which cover the case of concentric cylinders as well as a wire inside a cylinder and that when numerical solutions become available, it will be possible to calculate theoretically the best type of apparatus (including cascade systems) for each particular problem and to know in advance the rate of establishment of equilibrium and the concentration factor to be expected. Jones and Furry, however, have not been able to obtain algebraic solutions notwithstanding extended work and Jones proposes now to use the Bush differential analyser of M.I.T. for this purpose. Professor P. M. Morse of M.I.T. has promised the machine for two months if technical help can be found to run it during this time. The necessary help means an expenditure of $600. to $1000. and Mr. Jones asked me to find out whether the necessary funds could not be made available from defense appropriations.

“Although I am personally somewhat skeptical about the ultimate usefulness of the thermal diffusion method in separating Uranium isotopes, I am quite convinced that we should have complete information on the possibilities of the method and that [the] calculations in question would be of great utility in this connection. Therefore, I want to urge a grant of $1000. for the hiring of men needed to run the differential analyser machine.

“If you believe that the matter should be handled confidentially, may I suggest that the contract for the work be given to Professor E. B. Wilson, Jr. of Harvard who has been appointed a consultant of the National Defense Research Committee, who is willing to oversee the work and who is well qualified to do so in virtue of his theoretical training.

“Another matter which I should like to mention is the question of withholding from publication an article by Furry and Jones on the thermal diffusion which has been sent to Reviews of Modern Physics and which contains enough new information to be of considerable interest.”

1942, July 27
"Extract from report of Dr. H. C. Urey dated July 27, 1942." SECRET. [The complete text of this July 28, 1942 report in reference has not been located.]

> "I understand that the Naval Research Laboratory is having some success in separating the uranium isotopes. From remarks that Dr. Gunn made some time ago, they are probably using the electrolytic mobility method in fused salts. Dr. Nier tells me they are securing 10% changes in the ratio of the isotopes with 25 gram samples, or thereabouts. This work has not been correlated with the other work of the Committee [the Uranium Committee of S-1], for reasons that I do not understand, but efforts should be made by Dr. Conant, or Dr. Bush, probably, to be sure that the work of that laboratory ties in with the general purpose of this committee.
>
> "Since giving me the above information, Dr. Nier has been requested by the Naval Research Laboratory, not to transmit information of this kind to the Columbia people [i.e., Harold Urey]. He will respect this request in the future. However, this information has come to me, and I feel that I am duty bound to pass it on to the committee."

1942, December 12

Letter from W. [Warren] K. Lewis, Massachusetts Institution of Technology; National Defense Research Committee of the Office of Scientific Research and Development; member of the Uranium Committee of S-1, to J. B. Conant, Chairman, National Defense Research Committee. CONFIDENTIAL.

> "With reference to the trip made by our committee to the Naval Research Laboratory yesterday, it is unfortunate that the work is in such an early stage of development and particularly that the Laboratory has not been able so far to envisage at all definitely its ultimate potentialities. On the way home last night I finally got the picture

sufficiently clarified in my own mind so that I was able to make an estimate of the number of stages required for a product of high purity. The figure was only eighty of the present units in series. While the estimate is highly tentative, it is certainly of such interest that the development work ought to be continued intensively.

“During the conversation the workers expressed their desire for the help of suitable experts, particularly physicists, in consultation. I suggested that I would do anything I could to make such men available through the NRDC. If they write to you along this line, you will understand the background of the request. On thinking the matter over, I feel sure that men like Sherwood and Hottel of this Department or Chilton of DuPont are even more likely to be of help than a physicist. They have worked in fields which I am sure are parallel.

“If I can help the Laboratory in any way in consultation on the matter (and I think I can) it would be a delight to try to do so.”

1942, December 14
Letter of James Conant to W. K. Lewis.

“Thank you very much for writing to me about the Naval Research Laboratory. I appreciate your suggestions and your willingness to help. I will see if anything can be done along these lines.”

1942, December 31
Letter of Vannevar Bush to Rear Admiral William R. Purnell. SECRET. [The Atomic Bomb Military Policy Committee was established 23 September 1942; Vannevar Bush was appointed chairman of the committee; Adm. Purnell was appointed to represent the Navy.]

“The Executive Committee on S-1 at its last meeting entered the following statement in their minutes: ‘The Committee expresses the hope that the work of the Naval Research Laboratory can be expedited so that a comparison can be made with other processes and that, to further the end, the S-1 Executive Committee will do all it can to help.’

“When we recently listened to the members of the Reviewing Committee they expressed the feeling that the Naval Research Laboratory needed further facilities and manpower on this particular aspect of the problem in order to carry out a very difficult piece of experimentation. The also felt that it would be well to have the experiments carefully repeated.

“I would feel much gratified if you found it possible in some way to aid the Naval Research Laboratory to proceed on this matter to better advantage. While our Reviewing Committee at the present time did not recommend any extensive work along these lines, I feel that no possibility should be overlooked, and it also appears that while the method being studied at the Naval Research Laboratory has certain disabilities, it also has certain advantages, and the whole possibilities of that particular approach ought to be rendered more clear and again evaluated.

“My Office, of course, will be glad to aid in this in any way possible. Dr. Briggs has already undertaken to assure than any information that we have that can be of service to NRL in connection with their research program along these lines is made available to them. There may be other ways in which we could assist as, for example, by aiding in seeking for appropriate additional personnel. I take this up directly with you, however, rather than with Admiral Van Keuren,* as it is a matter concerned with this special secret program.”

[* Rear Admiral Alexander H. Van Keuren, until 2 November 1942, Chief of the Bureau of Ships; succeeded by Vice Admiral Edward L. Cochrane. However, as late as 31 May 1944 Admiral Van Keuren, a naval architect, was still involved in the development of the liquid thermal diffusion method. See the document below: 1944, June 3. Memorandum of Mssrs. W. K. Lewis, E. V. Murphree and R. C. Tolman to Major General L. R. Groves, paragraph 5.]

1943, January 15
Letter of H. T. Wensel, Technical Aide, National Defense Research Committee, to Rear Admiral W. R. Purnell.

"Dr. Conant has asked me to inform you that the two reports, which the Naval Research Laboratory has sent to him for transmittal to you, are being held for approximately ten days to permit their study by a subcommittee especially appointed by Dr. Conant for this purpose.

"This subcommittee has been instructed to submit a report not later than January 25th based on their studies of these reports and of other data in regard to the Naval Research Laboratory project. Dr. Conant felt that you would prefer to have a definite recommendation from such a subcommittee, along with the report, even though this will delay the transmission to you for the time indicated.

"In the event that this procedure does not meet with your full approval, I trust you will so notify us. The time indicated above was as short a time as possible, in the opinion of the members of the subcommittee, which would be required for a study of the report[s] adequate to permit a definite recommendation to be formulated."

1943, January 19
Letter of James Conant to Lyman Briggs, Director, National Bureau of Standards.

"Dr. Bush has expressed the hope that you and the members of your subcommittee (Murphree and Urey) will make an actual visit to the Naval Research Laboratory and discuss with the people there the work that they are doing. He feels this will be important both from the point of view of your obtaining all the information and in order to improve the relations between the S-1 Committee and the Navy. I concur in his views."

1943, January 23 (1)
Letter of Special Subcommittee of the S-1 Executive Committee: Lyman J. Briggs, Chairman; E. V. Murphree; Harold G. Urey.
SECRET.

“At the last meeting of the S-1 Executive Committee, on January 14, the undersigned were appointed as a Subcommittee to review reports prepared by the Naval Research Laboratory, covering work they have carried out on separation of the uranium isotopes by liquid thermal diffusion. The Naval Research Laboratory reports are Nos. 0-1977 and 0-1981 and are dated January 4 and January 7, respectively. The Subcommittee has been assisted in its investigation by Dr. Karl Cohen and Dr. W. I. Thompson. Discussion of the reports and of the work at the Naval Research Laboratory has been held with members of the Naval Research Laboratory staff. The thermal diffusion pilot plant at the Naval Research Laboratory has been visited by the Subcommittee and Doctors Cohen and Thompson.

“The Naval Research Laboratory has made excellent progress in the separation of the isotopes by liquid thermal diffusion and they are to be congratulated on their work. In the process used, the uranium is in the form of uranium hexafluoride. The diffusion column consists of an inside nickel tube, jacketed by a copper pipe, which is in turn jacketed by a steel pipe. The uranium hexafluoride is charged to the annular space about 0.25 mm clearance between the nickel and copper pipes and held under a higher pressure than that of the vapor pressure of the uranium hexafluoride at the hot surface. Steam at the pressure required to give the working temperature desired is used as the heating medium inside the nickel tube. The outside of the copper pipe is cooled by water. With this combination, there is a rapid flow of heat from the nickel tube to the copper tube, resulting in a rapid diffusion of the uranium hexafluoride. The columns used in the Naval Research Laboratory are about 36 feet long. With this equipment and with a steam temperature of about 238°C on the inside of the nickel tube and with cooling water temperature of about 65°C, under total reflux conditions, equilibrium values have been obtained indicating an enrichment of the light uranium isotope of as high as 31% at the top of the diffusion column and impoverishment of the light uranium isotope as high as 28% at the bottom of the tube. These are not actual experimental results, but represent an extrapolation of such results.

“Last September, some earlier results of the Naval Research Laboratory were reviewed by the S-1 Committee. In obtaining these earlier results, somewhat lower temperature was used at the hot surface and the enrichment obtained was not as high as shown by the more recent work.

“In all the work to date of the Naval Research Laboratory, no appreciable amount of material has been withdrawn, either from the top or the bottom of the diffusion apparatus, so results are not available under steady production conditions, such as would exist in any cascade built up with thermal diffusion units. The Naval Research Laboratory has, however, measured the change in concentration at the top and bottom of the thermal diffusion apparatus as a function of time. From this work, it is possible to make an estimate of what would occur under steady production conditions and hence to make estimates as to the size of the cascade required and time required to reach equilibrium. The method of making these calculations is outlined in a memorandum dated January 22, attached to this letter. In this memorandum, the methods of calculation were worked out by Doctors Cohen and Thompson. Dr. Urey has made independent calculations, using a simplified method and obtained results closely approximating those of the memorandum.

“The results of calculations made on certain experiments carried out by the Naval Research Laboratory are given in table one, attached to this letter. [Note: table one (Table I) is reproduced below the Appendix text]. Three cases are considered. The first column is based on the early results obtained at the Naval Research Laboratory, which were given to the S-1 Committee last September [1942]. The second column is based on an extrapolation of actual experimental results obtained recently at the Naval Research Laboratory. The third column is based on a different extrapolation of the recent experimental results. It will be noted from the table that in the enriching section of the plant, with the newer results, about 20,000 36-foot diffusion columns will be required.

Diffusion columns of greater length than 36 feet may be used with a corresponding reduction in the number of columns. This compares with around 38,000 for the earlier results. The newer results indicate an equilibrium time of about 600 days, compared with somewhat over 800 days for the earlier results. It should be realized that the figures given here represent a considerable extrapolation of the actual experimental data. As pointed out above, the actual experimental data obtained was on a column operating under total reflux with no appreciable product withdrawal, whereas, in the calculation, columns producing product continuously are pictured. Further, in the recent data the number of experimental determinations of the composition at the top and bottom of the column as a function of time are very limited. Moreover, in the experimental work, a reservoir of normal uranium hexafluoride was connected at all times to the bottom of the column in such a way that circulation to the bottom of the column may have occurred. This leads to results difficult to interpret. For these various reasons, the calculations given in table one should be considered very approximate. The equilibrium time calculated in the table one is quite optimistic, since no allowance was made for hold-up of material in the expansion joints which will be required, and, furthermore, no allowance was made for any hold-up of material in connecting piping. In a commercial plant, corrections for these items may lead to an increase in equilibrium time of approximately 25%.

“Table one gives estimated figures of steam requirements, cooling water, and electric power. The estimated requirements of electric power may be somewhat high. Estimates of the quantities of copper and nickel required are also given in table one. For the quantity of steam involved in column two, which is estimated at 12,00,000 pounds per hour, an investment in steam production facilities of $30,000,000 is indicated. Based on figures given in Naval Research Laboratory report 0-1977, the cost of the thermal diffusion tubes proper would be around $12,000,000. Considerable additional expenditure would be required for steam and water piping, structural steel, buildings, and the like. A very rough guess at the cost of the complete plant to produce one kilogram per day of U235 at 90% purity, would be $75,000,000. This puts the estimated cost in the same region as other separation projects.

“The high equilibrium time indicated in table one represents a drawback to the project. It has been estimated that if the U235 were produced in 10% concentration, the equilibrium time would be considerably reduced, possibly to about one-third or less that given in table one. In this case, other processes would be required to bring the concentration to the desired 90% strength.

“It is felt that the Naval Research Laboratory has developed a simple and positive means of separating the uranium isotope and this method, at least qualitatively, has been well demonstrated. There would appear to be no major mechanical problems to be solved. The thermal diffusion process as developed has the very great advantage of mechanical simplicity. The system is completely closed without stuffing boxes and moving valves and should have no contamination of the product. It is felt that the process may be appreciably improved by future developmental work. For example, the results given in report 0-1981 show a decrease in equilibrium time to about 50%, due to raising the hot wall temperature of the diffusion tube from 213EC to 238EC. This change in equilibrium time is, of course, reflected in a decrease in the size of the plant. The Naval Research Laboratory feels that by the use of still higher temperatures on the hot side of the diffusion tube, even more favorable results will be obtained.

“The future development work was discussed with the Naval Research staff. At present it is planned to obtain experimental data with steady product withdrawal, under conditions so far found to be optimum. This will probably involve the use of two 24 foot diffusion tubes, one to be operating as a stripping section and the other as an enriching section. Further, the Naval Research Laboratory plans to explore the advantages to be gained by using a higher hot wall temperature. It is planned to do this first by the use of higher pressure steam and next by the use of Dowtherm [a Dow Chemical Co. biphenyl/diphenyl oxide blend eutectic heat transfer fluid applicable to either liquid phase or vapor phase heating] as a heating medium. It has been suggested that, in the single tube diffusion experiments, with no product withdrawal, a reservoir with circulation to the bottom of the tube be installed so that the experimental results may be more easily interpreted. The Naval

Research Laboratory has estimated that the experimental work outlined may be completed within two months.

"The time required to construct a one kilogram per day liquid thermal diffusion plant will depend on the degree of priority the project would have. Taking the estimated cost of $75,000,000, it would appear that some eighteen months may be required for the erection of the plant, although this could be reduced by better priorities on materials. The eighteen months period should be considered as starting April 1, of this year, at which time it is hoped that sufficient experimental data may be available for plant design. The figure given in column two of table one, for equilibrium time is approximately 600 days, or twenty months. Rounding off the construction and equilibrium period to a total of three years would give April 1946 as the time the plant would started delivering product. It is felt very likely that further development work at the Naval Research Laboratory will lead to an appreciable reduction in the plant size, which would give a corresponding reduction in equilibrium time and should also result in a decrease in construction time. It should be realized that the figure of approximately 600 days, given in Figure [table] I, represents an optimistic interpretation of the actual experimental data.

"The Subcommittee feels that large-scale application of the thermal diffusion process, as developed by the Naval Research Laboratory, will be accelerated by having some large commercial concern work with the Naval Research Laboratory from the standpoint of carrying out preliminary engineering studies which later could be readily expanded to engineering work on actual plant design. Similar arrangements have been made in the past in connection with other projects of this general type."

1943, January 23 (2)
Letter of Special Subcommittee of the S-1 Executive Committee, Lyman J. Briggs, Chairman E. V. Murphree; Harold C. Urey.
SECRET.

"In addition to the technical discussion of the liquid thermal diffusion project at the Naval Research Laboratory by the Subcommittee of Section S-1, this Committee believes that it would be well to bring to your attention possible developments on this method that may have taken place in Germany.

"The thermal diffusion method was discovered by Clusius and Dickel, who first applied it to gaseous diffusion. Wirtz and his co-workers have discussed thermal diffusion of electrolytes in water solutions and the separation of carbon tetrachloride and hexane by thermal diffusion. In the Annalen der Physique, Vol. 36, 1939 Debye and Wirtz discussed the theory of thermal diffusion in liquids. Wirtz ends his article with the remark, 'There is at present too little material for a comparison of these results with experiment. In this Institute corresponding experiments are in progress.' This is dated July 30, 1939. In the Zeitschrift für Elektrochemie, Vol. 45, 1939 Wirtz and Korsching describe experiments showing the separation of carbon tetrachloride and hexane. Also, in the Zeitschrift für Angewante Chemie, Vol. 52, p. 499, 1939 the same authors mention the separation of the hydrogen isotopes to a slight extent when the method is applied to pure water. The effect on the hydrogen isotopes is small, and may have led them to believe that the method could not be applied to the uranium isotopes.

"As stated above, this method is the discovery of this group of German scientists, and it would seem to us most probable that they would apply it to our problem. In 1939 they had had experience on the thermal diffusion of electrolytes in water solutions, and they would not be obliged to do further experiments, as Dr. Abelson was, to become acquainted with the method.

"This Committee can only estimate a possible time schedule for the German development along these lines. We will assume that they started immediately after the War started, that within 16 months, namely, by the end of 1940, they had arrived at a place where the plant could be constructed. This is somewhat better than our schedule, but we are being somewhat optimistic because of their great experience with the method in general. Assuming that it would require 36 months to built the plant and bring it to equilibrium, namely, the same assumption we are making, from April first for our own possible development, this plant would come to production by January 1, 1944.

“We must allow some months for the production of material and hence this might become an effective weapon during the first half of 1944.

“This Committee believes that it may be possible to improve the diffusion process so that the time to come to equilibrium might be reduced to one year in place of 18 months, assumed before. At the same time it is felt that with very high priorities on materials it might be possible to construct the plant within one year. On this basis the total period for erecting the plant and coming to equilibrium would be two years, which would mean that production might start January 1, 1943. Therefore, it seems possible that this material might become an effective weapon in their hands during the first half of 1943.

“Recommendations

“The Committee recommends that active steps be taken immediately to discover plants in Germany which may be designed and used for this purpose. In the first place, this method could be modified to make multiple plants possible; thus it may be necessary to look for a number of smaller plants rather than one large one. The plant or plants would be placed in a coal mining region where some five or ten thousand tons of coal a day would be available. Some 30,000 kilowatts of electrical power would be required for the total of all the units. These plants must be placed either on a river to furnish the necessary cooling water or else they will have water cooling towers. The plants will undoubtedly be heavily camouflaged and might conceivably be built into the side of a hill. The construction would hardly have been begun before January 1, 1941, but of course they might have been begun at any time after that. The plants will contain a large number of steam generation units and hence many smoke stacks.

“The thermal diffusion units may be housed in buildings, in which case they will be large and tall, perhaps 50 to 100 feet high. On the other hand, if the thermal diffusion units are placed outside, they may present the appearance of many rows of vertical pipes housed at the top and bottom. I. G. Farbenindustrie would be a reasonable company to undertake this work. Elaborate precautions to prevent bombing will be taken because the long time to come to equilibrium would make interruption by bombing especially disastrous.

"This Committee would be glad to study photographs of any plants which are being built in Germany."

1943, January 25
Letter of E. V. Murphree, Standard Oil Development Co., New York, NY, to Lyman Briggs. SECRET.

"On further considering the work of the Naval Research Laboratory, I am wondering if our report stressed strongly enough the possibility of using the Naval Research Laboratory process as an alternative to the [gaseous] diffusion process, at least for the lower part of the separation plant.

"In the lower part of a separation plant, the length of time for the thermal diffusion process is by no means as serious as for the upper part of the plant. Dr. Urey brought out this point in our discussion Saturday and the point was mentioned in our report. On thinking the matter over further, it seemed to me that the enrichment in the thermal diffusion process was about as well demonstrated as for the [gaseous] diffusion screen process and that the thermal diffusion process has considerable less unsolved mechanical problems. There is a lack of sufficient experimental data on the thermal diffusion process and this lack is more serious than in the screen diffusion process because the theory of the thermal diffusion process has not been as well established. The thermal diffusion process suffers from the further disadvantage that no adequate engineering survey has been made to determine what the approximate cost and materials required for the plant would be.

"Considering the basic simplicity of the thermal diffusion process and the demonstration that has been made at the Naval Research Laboratory of its operability, it is my recommendation that consideration should be given to it as an alternate to the diffusion screen operation for the bottom of the plant. To bring this question to a head, it will be necessary first to obtain further experimental information, particularly on a continuous flow operation. Such work was discussed with the Naval Research Laboratory people and it was our understanding that they would do it. I do feel that every effort should be made to see that this work is done promptly. In our discussion with the Naval Research Laboratory people, it was further understood that the possibility of higher hot wall temperature would be investigated to see if the process could be improved. This is also quite urgent and must be done promptly.

“I don’t feel that we really have any adequate picture of the equipment required for a thermal diffusion process. The rough figures that we made Saturday probably are not of too great significance. Some engineering group should immediately undertake a study of the process, using as a basis a somewhat optimistic interpretation of present results. This would give then a rough comparison of the cost and materials requirement of the thermal diffusion process as compared with the screen diffusion process. With this information and the experimental information mentioned above, it should be possible to decide whether the thermal diffusion process should be used as the lower part of a separation plant. I believe if prompt action is taken it may be possible to reach a conclusion on this within two or three months and possibly sooner.”

1943, January 28
Letter of Harold C. Urey, Columbia University, to Lyman Briggs.
SECRET.

“I am in complete agreement with the letter of Mr. Murphree to you as of January 25, 1943. I had felt that the report brought out a usefulness of the thermal diffusion method in the lower part of any plant that we might consider. The time to come to equilibrium may prove to be too long for the use of this method for the whole process, but still it may be possible to use the process for the lower part of the plant. The method seems to be remarkably free from many of the troubles that we experience in the other methods.

"I should like to emphasize what Mr. Murphree says in his last paragraph. I certainly do not think that we have an adequate picture of the equipment required for the thermal diffusion plant, nor of the possible troubles that we may have. I believe that some chemical firm should be given the job of studying this with the Naval Research Laboratory. The results of their study would be very interesting indeed.

"I have been unable to get Thompson today, and hence can tell you no more than I knew the other day. I will try again tomorrow and if the situation is any different from what it was I will telephone you. The report which I received through Dr. Cohen was that the equilibrium time for the plant should be 850 days instead of 600."

1943, January 30
Letter of Lyman Briggs, U.S. Department of Commerce, National Bureau of Standards; Chairman, Special Sub-committee to James Conant. Subject: Liquid Thermal Diffusion Plant. SECRET.

"The following report supplements and summarizes the one made to you under date of January 23, 1943 by the special sub-committee appointed to consider the above subject.

"(1) We recommend that immediate consideration be given to the liquid thermal diffusion process as an alternate to the diffusion screen operation, particularly for use in the bottom part of the plant. This recommendation is made because of the basic simplicity of the thermal diffusion process. By this procedure, the material is processed in a sealed system, thus avoiding contamination. No pumps to move the process material or valves with moving parts in the sealed system are necessary. Flow can be stopped when necessary by freezing the material in a pipe. The operation of individual columns has been satisfactorily carried out at the Naval Research Laboratory. That enrichment can be obtained by the thermal diffusion process has been demonstrated about as conclusively as for the diffusion screen process. The unsolved mechanical problems of the liquid thermal diffusion process are considerably less than those of the diffusion screen process. The time required to reach working equilibrium becomes much less serious when the thermal diffusion process is used only in the lower stages of the plant.

"(2) We recommend that further experimental data on the thermal diffusion process be obtained by the Naval Research Laboratory with all possible speed. This information is urgently needed because the theory of the thermal diffusion process is not as well established as that of the screen diffusion process. It is particularly important to obtain further experimental information on continuous flow operation. Higher hot-wall temperatures should also be investigated to see if this improves the process. An effort should be made to reduce the hold-up in the ends of the columns and in the communicating pipes between the columns. All of these points have been considered by the staff of the Naval Research Laboratory and work on some of them is already underway. Further information on these subjects is urgently needed as the final design is dependent upon it.

"(3) We recommend that some competent engineering group should be employed at once to undertake a study of the process, with particular reference to its use in the bottom part of the plant. This would provide a basis for making a better comparison of the thermal diffusion process and the screen diffusion process as regards cost and materials required. We consider this an urgent matter. It will help to speed up the program.

"Letters from Dr. Murphree and Dr. Urey relating to this supplemental report are enclosed.

[Manuscript notations at the bottom of this letter read: 1. "Letter to L. J. Briggs from Urey dated Jan 28, and letter to L. J. Briggs from Murphree dated Jan 25 confirmed by conversation with Miss Kingsbury by phone to have been correct attachments. R. J." (Ruth Jenkins, Conant's secretary) 2. "Gen. Groves has copy of this letter. Confirmed by phone conversation with Mrs. O'Leary (Gen. Groves' secretary) 4/28/44. R. J."]

1943, February 13
Memorandum for Admiral Purnell from Vannevar Bush.
SECRET.

"Conant and I looked over the attached. There are a large number of incomplete or incorrect statements, but I do not believe that it is necessary for us to point these out since you have access to the whole matter. We have, as you know, urged that the Naval Research Laboratory be given support in its experimental program.

"The Engineering Panel has just completed a further review of the proposed NRL process of separation. General Groves will undoubtedly present this at the next meeting of the Military Policy Committee on this subject. I judge that at that time you will wish to discuss this further with the group."

1943, February 23
Letter of Lyman Briggs to James Conant. SECRET.

"I enclose herewith a special report prepared by Dr. Murphree and Dr. Urey at the request of S-1. This report outlines an experimental program which it is hoped the Naval Research Laboratory will be willing to carry out as promptly as possible. These experiments will help to determine the way in which the liquid thermal diffusion process may be used most effectively in the cascade.

[This "special report," dated 19 February 1943, "Program for experiments to be carried out on the thermal diffusion method," is transcribed below.]

"I have reviewed this program and it meets with my approval. I suggest that it be transmitted through appropriate channels to the Director of the Naval Research Laboratory."

1943, February 19
"Program for experiments to be carried out on the thermal diffusion method." SECRET. Signed, Murphree and H. C. Urey; copies to: L. J. Briggs (3), A. H. Compton, J. M. Conant, E. O. Lawrence, E. V. Murphree, H. C. Urey. SECRET.

"The writers were asked to give recommendations as to a detailed program to be carried out to establish the application of the thermal diffusion method for separation of uranium isotopes. The program proposed is divided into two parts. The first part deals with unsteady state experiments and is primarily drawn up to determine the reproducibility of separation tubes. These experiments will also give information that can be used in calculating a cascade of thermal diffusion units to obtain a given separation from a given amount of material. The second part of the experiments suggested are steady state experiments which may more definitely establish the length of diffusion columns required in a separation plant. In particular the steady state experiments will serve as a check on the theory that has been used to extrapolate unsteady state experiments over to steady state conditions.

“Unsteady State Experiments:

“In order to build a plant making use of the thermal diffusion plan, it will be necessary to be sure that the individual units constructed behave closely in the same manner. So far experiments made by the Naval Research Laboratory indicated that with a variety of spacing between the hot and the cold tubes, a regular curve for the over all fractionation is secured. This regular curve indicates that it is possible to construct these tubes in such a way that they are reproducible, though the fact that the fractionation factor secured in these experiments follows a smooth curve may be fortuitous, and in any case gives us no estimate as to how closely it is possible to duplicate the performance in tubes of this kind. The following experiments should therefore be run in order to test the question of reproducibility of these tubes.

“The Naval Research Laboratory is building 48' tubes with spacings of 0.25 mm, though they have built in the past 36' tubes, and also have talked of 24' tubes. The experiments outlined below should preferably be done with 24' tubes. If these are not available, then with 36' tubes and, again, if these are not available, with 48' tubes. The reason for this choice is that the experiments can be done much quicker with the short tubes, while the fractionation factors secured are quite adequate for analytical purposes.

“The volume of the 48' tubes within the tube itself is, according to the best information available, about 1,600 grams of hex [uranium hexafluoride] The experiments should be done with a holdup at the top of the tube (i.e., the holdup in the expansion joint) of 200 to 500 grams, preferably 200, again because the time of the experiments would be decreased. In any case the holdup at the top of the column must be accurately known. A circulating holdup at the bottom of the tube should be about 25 kilos in order that the entire volume of the tube is only a small fraction of the reservoir at the bottom of the tube, this being desirable from the standpoint of the theoretical interpretation of the results.

“Experiments performed by the Naval Research Laboratory indicate that a 36' tube comes half way to equilibrium in about one day. Experiments should therefore be run, taking samples every six hours for a period of about four days. If the concentration curve with time at the top of such a column is approaching equilibrium, the experiment could be stopped, otherwise it must be continued. The samples taken every six hours period should not be over 1 or 2 grams so that the material withdrawn from the top of the column is only a small part of the total holdup at the top of the column, thus resulting in only a small upset of the operation of the column. Care should be taken to get a representative sample at this point.

“Such experiments should be run on a number of tubes, perhaps four, in order to see whether successive tubes made in the manner which they are using, are reproducible. It is impossible to expect that they are exactly reproducible, but it is important to know what deviation there is between tubes of this kind in order that one can estimate the performance of a plant constructed of such tubes. It is not possible to construct a plant assuming that the best performance observed can always be repeated, unless experiments are made to demonstrate this.

“Steady State experiments:

“A single test on a thermal diffusion unit, or a pair of thermal diffusion units, even though operated under steady state conditions will not definitely determine the minimum number of thermal diffusion units required in a cascade. Tests under at least two conditions, and preferably three, are required. In the program proposed below steady state experiments on a pair of columns are proposed, using three product withdrawal rates. The first of these product withdrawal rates, which is considered near the optimum, is 0.36 kg. per day. This experiment may establish the number of diffusion units required in a cascade to within about 40% of the minimum. In order to get a closer approximation to the minimum number of units required, and also to obtain an overall check of the data, runs at product withdrawal rates of 0.2 kg. per day and 0.7 kg. per day are proposed.

"For the data previously reported at unsteady state conditions in 36' columns, it is proposed that a pair of 24' columns be connected together with a feed reservoir between them. One of these 24' columns would serve as an enriching stage and the other as a stripping stage. When the column is operated under steady state conditions equal amounts of material would be withdrawn at regular intervals from the top of the enriching stage and from the bottom of the stripping stage. For the equipment proposed it is estimated the annular space of the 24' column would have a holdup of 600 grams of hex, and that the expansion joints of each unit would have a holdup of 500 grams of hex. The holdup in the expansion joint must be accurately known.

"In order to make the units symmetrical the expansion joint for the enriching tube should be at the top of the column and the expansion joint for the stripping tube at the bottom of the column. If it is not possible to put the expansion joint for the stripping unit at the bottom of the column then a small circulating reservoir having a holdup equivalent to an expansion joint should be installed at the bottom of the stripping tube.

"In order to be sure that the composition of the hex between the enriching and stripping sections is approximately that of the feed, it is felt that a circulating reservoir of 25 kg. capacity should be used. This reservoir will serve to charge the columns and furnish the material to be withdrawn.

"It is important that the material withdrawn from the columns at any one time be limited in amount so as not to upset the conditions within the columns. For the runs at product withdrawal rates of 0.2 kg. and 0.36 kg. per day, equal samples of product and waste should be withdrawn at two-hour intervals. For the run at a product withdrawal rate of 0.7 kg. per day, product and waste should be withdrawn at intervals of one hour.

"In the runs proposed during the initial part of the operation the composition of the material withdrawn from the column will be changing and will eventually approach constant composition. In order to get a reasonable approximation of this constant composition the run should extend over a period of seven days. Analyses by the mass spectrometer should be made on samples representative of the material withdrawn from the top and bottom of the column pair at twelve-hour intervals. That is, information will be available on the composition of the material at the top and bottom of the diffusion pair at every twelve hours of the seven-day run. Analyses should be made on a sample representative of the material in the reservoir between the two columns at twenty-four hour intervals."

1943, February 24
Letter from James Conant to General Groves. SECRET.

"I am transmitting to you herewith a special report prepared by two members of the S-1 Committee dealing with the experimental program at the Naval Research Laboratory. This report has been reviewed by another member of the Committee, Dr. Briggs, and meets with his approval.

"The S-1 Committee hopes that you will transmit this report to the Director of the Naval Research Laboratory. The proposed experimental program recommended by this group we believe to be of considerable importance. The results of these experiments will provide the information which will be most helpful in determining the over-all advantages of the thermal diffusion process as a part of the cascade system. I need not say that it is desirable that this information be secured at the earliest possible date."

1943, May 11
Letter of James Conant to Rear Admiral William R. Purnell. SECRET. [Reference is made in this letter to a book, which book Conant returns to Adm. Purnell. The title of the book nor identification of the chapter in reference is provided by this letter.

> "I have had a photostat made of the important chapter in this book and have forwarded it to Dr. H. C. Urey, who is the expert on this matter.
>
> "I am returning the book to you with my thanks, thinking you will probably want to forward it to our friends in the Naval Research Laboratory."

1943, June 19
Letter of Lyman Briggs to Colonel T. C. Crenshaw, Manhattan District Office, Corps of Engineers, New York, NY. SECRET.

> "I am enclosing a copy of a letter to Dr. P. F. Alexander directing him to turn over to your Office two tons of uranium oxide belonging to the National Bureau of Standards. This material was purchased from funds provided by NDRC and its transfer without charge to your Office for use in the war effort meets with the approval of the NDRC.
>
> "An acknowledgment in due course of the receipt of this material will be appreciated."

1943, July 10
Letter of General L. R. Groves to James Conant. SECRET.

> "I feel that the progress at the Naval Research Laboratory, on the problem with which we are concerned, has reached a point where it will be desirable to have this situation reviewed by the S-1 Committee.
>
> "It is possible that you would prefer to have this done by a Committee appointed by you, but not necessarily members of the S-1 Committee.

> "Would you be kind enough to take charge of this review and render a report."

1943, July 10
Letter of James Conant to Rear Admiral William R. Purnell.
CONFIDENTIAL.

> "I have been asked by General Groves to appoint a subcommittee of the S-1 Committee to review the present status of the work at the Naval Research Laboratory on the S-1 program. It was his idea that this subcommittee would compare the present prospects of that process with the other plans which are now being developed under the Military Policy Committee to see if there was any way in which the Naval Research Laboratory program could be advantageously fitted in to the general scheme which is now in progress of development.
>
> "I am proposing to appoint a committee composed of W. K. Lewis, Chairman, and Drs. Urey, Murphree and Briggs.
>
> "I am writing to inquire whether or not the Naval Research Laboratory group would be willing to receive such a committee and give them all the information it would be necessary for the committee to have to form a judgment and make a report through me to General Groves. I hope the Naval Research Laboratory would not regard such a visitation as an intrusion but rather as one more indication of the desire of the S-1 Committee to be of any assistance they could to the group which is doing such interesting and excellent work."

1943, July 30
Letter of James Conant to Rear Admiral William R. Purnell.
SECRET.

> "I am transmitting herewith a copy of a portion of a memorandum submitted through Dr. Harold C. Urey which deals with NRL Report No. 0-2047, by N. Rosen, dated April 17, 1943. I should be indebted to you if you would transmit this memorandum to those concerned in the Naval Research Laboratory.

“Enclosure. 1 copy only made of Memo to Dr. Urey from Karl Cohen dated July 16, 1943 on NRL Report No. 0-2047 - Theory of Liquid Thermal Diffusion. Sections 1 thru 4 copied. Sections 5 and 6 not sent to Admiral Purnell.”

[NRL Report No. 0-2047, “Liquid Thermal Diffusion Research Theory of Isotope Separation,” dated 17 April 1943, has not been located. NRL Report No. 0-2047 was apparently written by N. Rosen. The memo “which deals with NRL Report 0-2047" was apparently written Karl Cohen and dated16 July 1943; that memo has not been located.]

1943, September 3
Memorandum from Chief of the Bureau of Ships, signed by H. A. Ingram by direction of Chief of Bureau, to Commander in Chief, U.S. Fleet. (Attention Rear Adm. W. R. Purnell). SECRET.

“Subject: NRL Report No. 0-2127 - Fourth Partial Report on Liquid Thermal Diffusion Research [30 July 1943].

“Reference: (a) NRL sec. ltr. S-S41-5U(448), Serial #1647, of 13 Aug. 1943.

“Enclosure: (A) Two copies of NRL Report No. 0-2127.

“1. In accordance with the suggestion of reference (a), two copies of the subject report are forwarded herewith, one copy to be further transmitted to Dr. Karl Cohen, Columbia University, New York, New York.”

1943, September 9
Letter of Ruth E. Jenkins [secretary to James Conant] to Harold Urey, Columbia University. SECRET.

“At Dr. Conant’s request, I am sending you herewith NRL Report No. 0-2127, dated July 30, 1943 and entitled ‘Fourth Partial Report on Liquid Thermal Diffusion Research,’ for transmission to Dr. Karl Cohen.”

1943, September 15
Letter of James Conant to Rear Admiral W. R. Purnell. SECRET.

[The September 8, 1943 report, in the form of a letter, cited in paragraph (1) has not been located. Probably the report, in the form of a letter, cited in paragraph (1) was the result of General Groves' request to Conant of 10 July 1943 and Conant's inquiry to Admiral Purnell, also of 10 July 1943.]

> ”(1) I am forwarding herewith to you as a member of the Military Policy Committee a report dated September 8, 1943, from a committee composed of Dr. L. J. Briggs, Dr. H. G. Urey, Dr. E. V. Murphree and Dr. W. K. Lewis, Chairman, dealing with the work on liquid thermal diffusion being done at the Naval Research Laboratory. This report is in the form of a letter dated September 8, 1943 addressed to me. I shall greatly appreciate it if you would forward this document to the Naval Research Laboratory through the appropriate channels.
>
> “(2) This report was discussed at a recent meeting of the S-1 Committee at which General Groves was present. It was the strong opinion of this group in accepting the report of the subcommittee to which I have just referred that it would be most unfortunate for the entire effort if any further expansion of the work at the Naval Research Laboratory in this field were to result in the drawing away of personnel now being employed on other aspects of this program. In particular, we had in mind such men as Drs. Beams, Nye, Armistead, Snoddy and Ham of the University of Virginia. It was the opinion of both the subcommittee and the whole Committee that the most useful way in which the work referred to in the first paragraph of page 2 of the report could be carried out by the Naval Research Laboratory would be by careful study on a small scale of the problems referred to in this report.
>
> “(3) We understand that there is still available at the Naval Research Laboratory approximately 80 pounds of hex, made up of several lots of different known composition. If this material, together with the analyses of the several samples, can be made available to those now engaged on the project under the general direction of the Military Policy Committee for experimental purposes, the favor will be deeply appreciated, and an equivalent amount of base material will be supplied

in exchange. The arrangements for this would be made through General Groves' office.

"cc: Brigadier General L. R. Groves (together with copy of Report) shown only on Gen. Groves' copy and file copies."

1944, March 4
"Paraphrase of teletype" of J. R. Oppenheimer to James Conant. SECRET.

"It is believed by [Joseph W.] Kennedy that thermal diffusion methods may be effective in our problem of purification of X ten [plutonium] product. It seems probable that some of the experimental work on separation carried out at NRL may be relevant. If such is the case could you arrange for most pertinent reports sent to us. Reference YC-227. Parsons suggests this might be cleared with Purnell. Paul Fine (Dr. Tolman's office) can arrange details."

1944, March 4
Letter of James Conant to Rear Admiral William R. Purnell. CONFIDENTIAL.

"Dr. Oppenheimer, who you know is in charge of the experimental work at Y on the S-1 project, would like to have his men look at the reports from the Naval Research Laboratory on the thermal diffusion method, feeling that the technique they have developed might be of some use to them in some problem of purification. Needless to say, they do not propose to set up a separation plant at this spot in connection with the main problem.

"I have in my possession copies of the third and fourth partial reports on this process dated April 17, 1943 and July 30, 1943, numbered NRL-0-2047 and NRL-02127, respectively. I should not wish to send them to these men, however, without your permission. My own feeling is that the chances that they will find anything of use is slight, but I hesitate to turn down any request from that hard-pressed area. If I were to send these two reports and they on reading them found anything of interest, it would then be possible perhaps through you to obtain copies of the first two, but at the moment no action on your part would be needed except to let me know whether or not I would have permission to send these reports to Y solely for their use."

[Note at bottom of letter page, "NRL No- 0-2047 dated April 17, 1943 and NRL Report No. 0-2127 dated July 30, 1943 sent to Dr. J. R. Oppenheimer 3-20-44 upon receipt of telephoned approved from Admiral Purnell to Dr. Conant. Confirmation by letter to be mailed to Dr. Conant."]

1944, March 8
Letter ["Dictated—not read–not signed"] of General Groves to James Conant. SECRET.

"Here is an item that I meant to speak to you about today but it got lost in the shuffle.

"Nichols told me that he had gotten a rumor that Gunn at the Naval Research Laboratory was responsible for a story that they would be producing 10 grams of 90% material by July 1 and that the cost of the plant would be only $1,000,000.

"Do you have any ideas as to how the truth of this affair can be found out? I personally am suspicious of any cost figures unless we have been completely misinformed in the past by all of our scientific investigators who have looked into the process. There is one possibility, however, —that he is using surplus steam from a Navy yard and is not charging himself anything for it."

1944, March 17
Letter of Rear Admiral W. R. Purnell, Navy Department, Office of the Chief of Naval Operations, to James Conant. CONFIDENTIAL.

I can see no objection to forwarding the report of the Naval Research Laboratory, on the thermal diffusion method, to Dr. Oppenheimer.

Accordingly, permission is granted to forward the reports of April 17, 1943 and July 30, 1943, numbered NRL-0-2047 and NRL-0-2127, respectively, to Dr. Oppenheimer."

1944, March 20
Letter of Ruth E. Jenkins to Dr. J. R. Oppenheimer, Box 1663, Santa Fe, New Mexico. SECRET.

On Dr. Conant's behalf and in answer to your teletype of March 4 (Reference YC-227), I am forwarding to you NRL Report No. 0-2047 dated April 17, 1943 and NRL Report 0-2127 dated July 30, 1943.

If on reading these reports you find anything of interest, please let Dr. Conant know, and he will endeavor to obtain through Admiral Purnell copies of the first two partial reports on this process.

"Enclosures:

"Report No. 0-2047, April 17, 1943, 'Liquid Thermal Diffusion Research Theory of Isotope Separation by Thermal Diffusion, – I. Single Column.'

"Report No. 0-2127, July 30, 1943, "Fourth Partial Report on Liquid Thermal Diffusion Research."

1944, April 20
Letter of James Conant to Lyman Briggs. SECRET.

"I was very much interested in your remarks yesterday about the origin of the work on liquid phase separation now being carried on at the Naval Research Laboratory. Am I right in understanding that the first work on this method was done by Mr. Abelson at the Bureau and sufficiently promising results were obtained by him while there to warrant the NRL going ahead with the project vigorously when Mr. Abelson transferred to NRL? What was the approximate date of Mr. Abelson's transfer?

“I would greatly appreciate your dropping me a line on these matters, as I want to have my facts straight about this bit of history.”

1944, May 6
Manuscript note of James Conant to Vannevar Bush.
PERSONAL.

“I uncovered a rather strange and to my mind a rather unpleasant bit of past history in S-1 the other day. In a conversation with Smyth [Princeton physicist Henry DeWolf Smyth, author of *Atomic Energy for Military Purposes*, 1945] & myself Briggs was led to recount the origin of the Naval Research Lab’s method. According to his story Abelson (of Carnegie) was working at the Bureau of Standards in the earlier days of the Briggs committee and either invented or developed the idea of separation by thermal liquid diffusion and obtained results indicating that there was a chance that it would work. Gunn a member of the Briggs committee but apparently no one else became a party to this knowingly and prevailed upon Briggs to let Abelson go to the NRL where they had a lot of steam necessary for the purpose. Gunn & Abelson continued working on the Thermal Diffusion Process at NRL and kept Briggs informed of progress. Briggs felt himself under pledge not to tell anyone else. This continued till Briggs was smoked out by some questions of Urey in the summer of 1943 (my interpretation!). Briggs said that finally he felt he must get a release from the NRL and reveal to the S-1 Executive Committee NRL progress.

“I rather led Briggs on in an innocent way to tell this story. From my point of view it is very damaging to Briggs. Whether he realized it or not I don’t know but I wrote the enclosed letter [20 April 1944] and have received no reply after two weeks.

“I’m not particularly anxious to accuse Briggs of double dealing nor Gunn either. But I could get very mad in retrospect about his behavior. It is necessary to make a record now to show why the S-1 committee never pushed the Thermal Diffusion method, namely because they were never informed of it?

> "In particular shall I forget the letter I wrote to Briggs though to me his failure to reply is pretty much an admission of guilt?"

1944, June 3
Letter of General Groves, War Department, Office of the Chief of Engineers, Washington, to James Conant. SECRET.

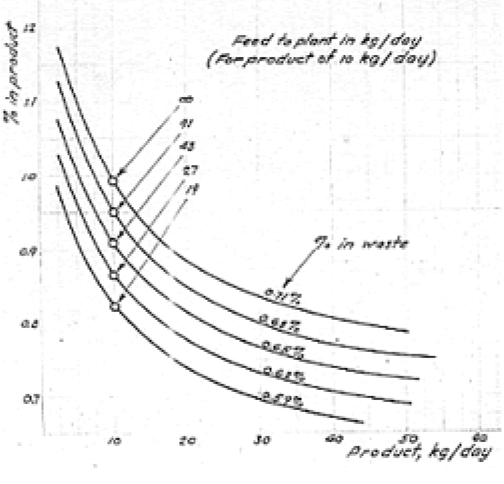
> "I am inclosing a copy of the report by Messrs. Lewis, Murphree and Tolman with the suggestion that you take it with you to Tennessee and discuss the question insofar as necessary with the people at the site. If you could then give me your advice it would be very much appreciated.
>
> "As yet Mr. Murphree and Mr. Lewis have not signed the report but I understand that they are agreeable to it.
>
> "Incl.: Report 6/3/44"

1944, June 3
Memorandum of Mssrs. W. K. Lewis, E. V. Murphree and R. C. Tolman to Major General L. R. Groves. SECRET.

> Subject: "Possible Utilization of Navy Pilot Thermal Diffusion Plant."
>
> "In accordance with the request in your letter of 31 May 1944, we have examined into the possible utilization of the Navy pilot thermal diffusion plant. Our conclusions are given in the following paragraphs which are numbered to correspond to the paragraphs in your letter. [This 31 May 1943 letter has not been located.]
>
> "1. The installation of a 100 tube plant at the Philadelphia Navy Yard is well advanced, and operation is expected to begin about July 15, 1944. The plant is provided with two high pressure boilers of sufficient capacity for operating a total of 300 tubes, and is to be provided with a low pressure boiler for operating auxiliary equipment. The Navy's tentative program for the operation of this equipment is to set aside some of the 100 tubes for special experiments on individual tubes. The remainder of the 100 would then be connected up as a cascade, with 7

stages of enrichment and 1 stage of stripping, which is estimated would give a production of about 100 grams of 25 [U^{235}] per day, at approximately 6% concentration, with a hold up of about 100 days.

“2. The estimate mentioned in your letter that the Navy plant, with 100 tubes connected in parallel, would produce 12 kilograms a day of 1% material is optimistic. The attached curves, “Feed to plant in kg/day (For product of 10 kg/day).” Curves giving the anticipated possible production of the Navy thermal liquid diffusion plant with 100 tubes connected in parallel] give the anticipated possible production of the Navy plant, with the 100 tubes connected in parallel, based on the data presented in Columbia Report No. 4-R-104 for tubes and operating conditions at NRL most nearly comparable to those to be used at Philadelphia. These curves give the plant production in kilos of metal per day, plotted against the percentage concentration of 25 in the product. Each curve corresponds to a specific concentration of 25 in the depleted material to be rejected from the plant. The feed requirements in kilograms of metal per day indicated on the plot apply on to the case of a daily production of 10 kilos of enriched material. (The corresponding figures can be readily computed for other daily outputs.)



“From these curves it can be seen that the maximum attainable production of 1% material would be a trifle under 10 kilos per day, and this would involve a very large quantity of feed material, with a very high concentration of 25 left in the reject. Probably it would be unwise to try to lower the concentration of 25 in the rejected material below

0.68%. This corresponds to the production of about 7.4 kilos per day of 1% material, with a consumption of 75 kilos of natural metal. With the same concentration in the reject material, the 100 tube plant would produce 10 kilos of 0.95% material, with a consumption of 91 kilos of natural metal. A plant of 300 tubes would obviously produce three times as much product with three times the metal consumption.

As already mentioned, the curves given are based on the assumption that the 100 tubes would be connected in parallel, and for getting quantities of product of the order of 10 kilos per day this is the most effective method of utilizing the present equipment.

“3. The two main boilers mentioned above, plus the auxiliary boiler, would be adequate for the operation of an additional 200 tubes. However, the Navy would like to obtain information on the operation of the present 100 tubes, before making decision as to details of the remaining 200. They estimate that the installation of the additional 200 could be made two months after decision to go ahead was reached. If it seemed wise from a military standpoint to push for the earliest possible maximum production of approximately 1% material, we feel that no great uncertainties would be involved in this program in going ahead with 200 more tubes of the present design. Nevertheless, this would inevitably delay the sound development of the ultimate possibilities of the thermal diffusion method.

“4. Attention should be called to the fact that 300 tubes could produce 30 kg of 0.95% concentration, with a discard of 0.68% concentration, and a daily consumption of 273 kilos of natural metal. It may also be mentioned that although a temporary utilization of the Navy plant for production would delay the investigation of the ultimate potentialities of the thermal diffusion method, useful information would nevertheless be obtained during production operation. We are of the opinion that the ultimate potentialities of the thermal method should in any case be investigated at the proper time.

“5. The information on which this report is based was obtained from a conference which we had on 31 May 1944 at the Naval Research Laboratory with Admiral A. H. Van Keuren, Captain T. A. Solberg, Commander R. H. Gibbs, Dr. Ross Gunn, and Dr. P. H. Abelson, and on a conference on 1 June 1944 at Philadelphia with Captain C. A. Bonvillian and Dr. Abelson. At both conferences we were assisted by Mr. W. I. Thompson and Dr. Karl Cohen. We desire to inform you that we found the Navy very cooperative in furnishing the information which we needed, and hope that you will express to the Naval Research Laboratory our appreciation for this cooperation."

1944, July 25
Memorandum of Chief of the Bureau of Ships [Vice Admiral Edward L. Cochrane] to Rear Admiral W. R. Purnell, U.S.N. (Op-05). SECRET.

"Subject: Special Project - Name of Method.

"Reference: (a) Dir. N. R. L. SECRET ltr. S-S41-5(U) (200) Ser. 3459 of 13 July 1944.

"1. The Chief of the Bureau of Ships concurs in the recommendation of reference (a) that the thermal diffusion process for isotopic separation which originated at the Naval Research Laboratory and is shortly to be put into operation at the Naval Boiler and Turbine Laboratory at Philadelphia be given the name Abelson-Gunn Process, in recognition of the two scientists, Dr. Philip H. Abelson and Dr. Ross Gunn, who have developed this process to its present state.

"Copy to:

Dir. NRL

Dr. James B. Conant, N.D.R.C.

Maj. Gen. L. R. Groves"

1944, July 27
Manuscript of James Conant, "Historical note on introduction of the Abelson-Gunn process."

"Pursuant to attached letter of June 3 from Gen. Groves, JBC [James B. Conant] and W. K. L. [Warren K. Lewis] Discussed with E. O. L. [Ernest O. Lawrence] & the top [Oak Ridge] Tennessee people the relation of the NRL process (later to be called the Abelson-Gunn process) to the electromagnetic process. It was agreed that the use of the expanded Phila. [Philadelphia] plant to produce 0.70% feed was of first importance and by itself would increase the output before July 1, 1945 appreciably. The question was also raised of building a NRL plant to operate on the [boiler] house of the [gaseous] diffusion plant at Tennessee. It was pointed out that for small enrichment this process was economical but for large enrichment almost impossible because of coal consumption and long hold-up time. It was recommended to Gen. Groves that a plant be built at Tennessee to feed in the electromagnetic plant enriched material thereby perhaps doubling the output of providing insurance against failure of the [gaseous] diffusion plant to come in on time."

1944, September 15
Letter of James Conant to Lyman Briggs.
CONFIDENTIAL.

"In going over my records in connection with summing up the present status of the S-1 work, I find that I do not have an answer to a letter I addressed you on April 20 concerning the origin of the Naval Research Laboratory work.

"I am wondering if I could trouble you to let me know if I am right in assuming in my summary to Dr. Bush that the first work on the NRL method was done by Mr. Abelson at the Bureau and that sufficiently promising results were obtained by him while there to warrant your transferring the work to the NRL with the understanding they would prosecute it vigorously. I take it the date of the transfer of this work was some time during the summer of 1941.

"This has all become a matter of ancient and mostly academic history now, but I am trying to record a few facts as I am clearing up a portion of my files."

1944, September 21
Letter of Lyman Briggs to James Conant. SECRET

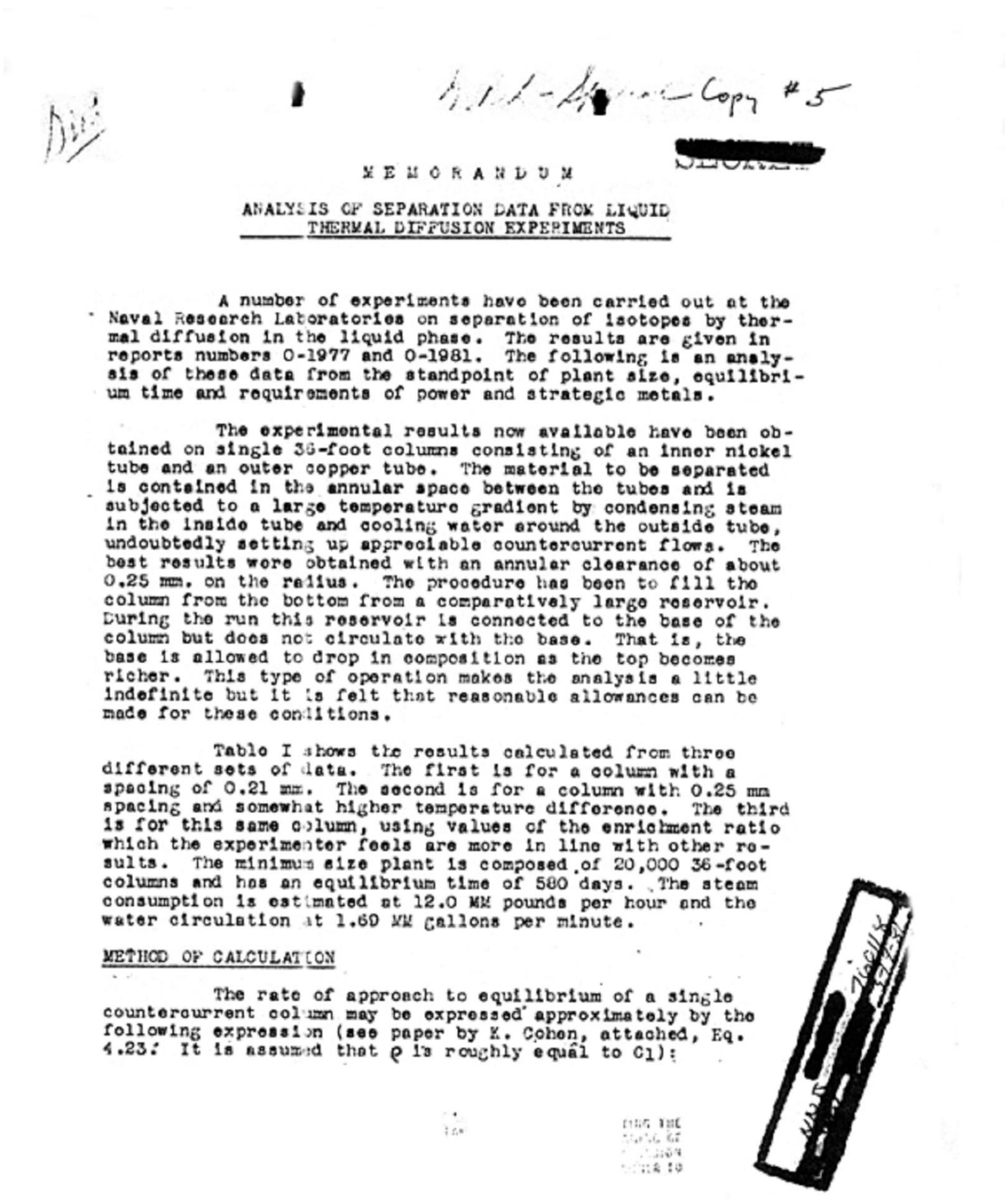
"This is in reply to your letter of September 15th. The S-1 work was initiated by the President, as you may recall, in October, 1939, and was at first supported by small allotments from the Bureau of Ordnance of the Navy and the Bureau of Ordnance of the Army. In April, 1940, I learned of Admiral Bowen's active interest in the matter. He was at that time in charge of the Naval Research Laboratory and was prepared to give some real support to the project. This support resulted in an enlargement of Fermi's work at Columbia and the initiation of the centrifuge work at the University of Virginia and at Columbia. Nier was given funds to separate a larger sample of 25 with the mass spectrograph. Meier was asked to study the separation by diffusion. Urey was given a grant for the separation of heavy hydrogen. At this time, also various other methods for separating the isotopes were studied, including separation by thermal gaseous diffusion. In the course of this work, Dr. Abelson thought he saw possibilities in the thermal liquid diffusion method. Accordingly, in September, 1940, on my recommendation, a grant was made by the Naval Research Laboratory to the Department of Terrestrial Magnetism [Carnegie Institution, Washington, D.C.] for this purpose. The facilities of the Department of Terrestrial Magnetism were not, however, altogether adequate for this type of work and accordingly I invited Abelson to come to the Bureau of Standards where he set up and operated columns about 10 feet long, with different clearances between the concentric tubes. The results of these experiments were encouraging, but Abelson was convinced that we should employ larger thermal gradients, which necessitated much higher steam pressures than were available at the Bureau. Accordingly, at Dr. Gunn's suggestion, that phase of the work was transferred to the Naval Research Laboratory in the summer of 1941 where higher steam pressures and greater shop facilities were available.

"Dr. Abelson was never on the rolls of the Bureau. The work was supported throughout this preliminary period by the Naval Research Laboratory except for such laboratory facilities and help as were contributed by the National Bureau of Standards. The move to the Naval Research Laboratory took place as I recall about July 1, 1941.

"It was the understanding, as you say, that the work would be pursued diligently and that the Committee would be kept advised of the progress that was made. Dr. Gunn and Dr. Abelson kept me fully informed regarding the work and I have in my files a letter from Dr. Gunn expressing his willingness to release the results to the Committee during the earlier stages of the work. Admiral Bowen, of the Naval Research Laboratory, requested that the project be left in the hands of the Naval Research Laboratory until its possibilities had been more definitely established. I think he feared that the method, despite its promise and simplicity, would not be looked upon with favor by the Committee because of the high operating costs. You will recall that this was exactly what happened when the process was examined by the S-1 Committee."

Following pages reproduce:

Memorandum of W. I. Thompson, "Analysis of separation data from liquid thermal diffusion experiments," dated January 22, 1943; 5 pp pages, including "table one [Table I]."



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MEMORANDUM

ANALYSIS OF SEPARATION DATA FROM LIQUID THERMAL DIFFUSION EXPERIMENTS

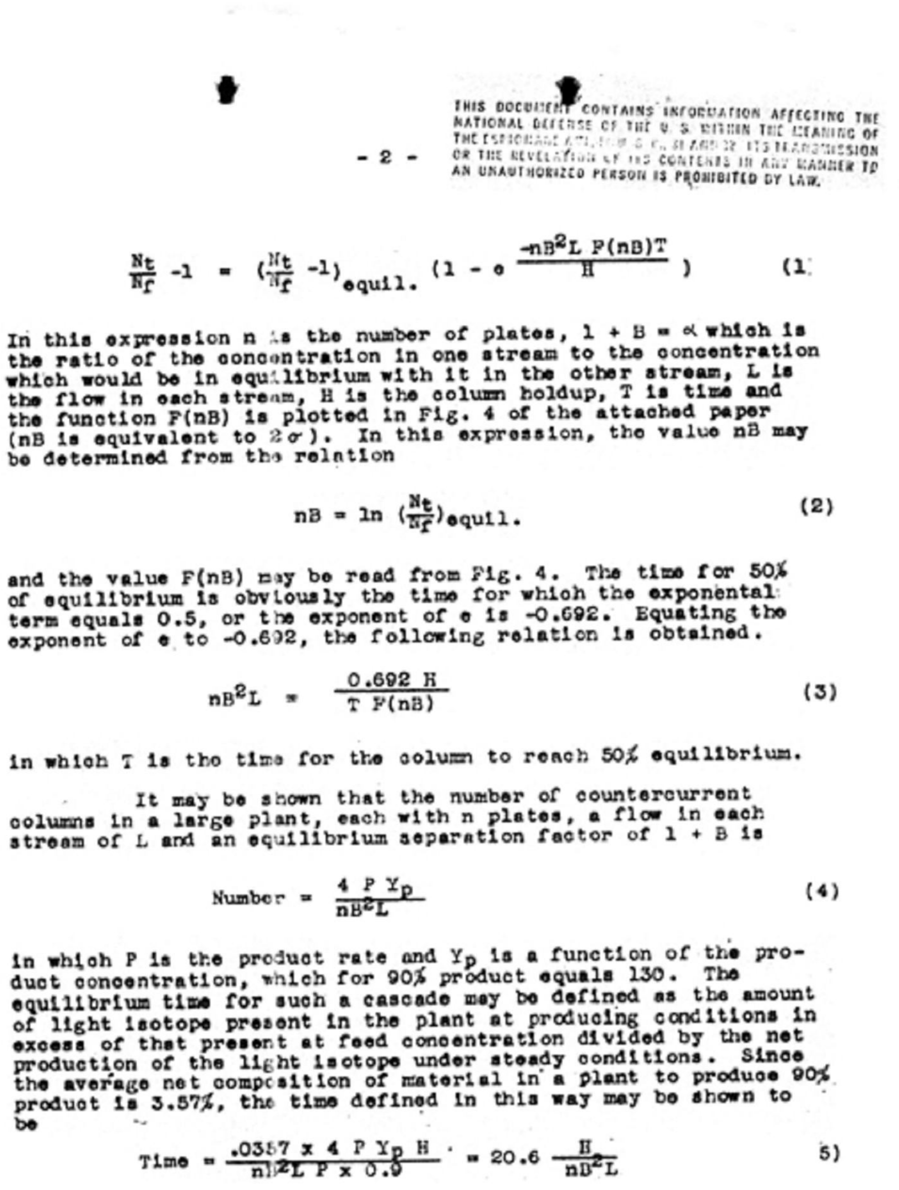
A number of experiments have been carried out at the Naval Research Laboratories on separation of isotopes by thermal diffusion in the liquid phase. The results are given in reports numbers O-1977 and O-1981. The following is an analysis of these data from the standpoint of plant size, equilibrium time and requirements of power and strategic metals.

The experimental results now available have been obtained on single 36-foot columns consisting of an inner nickel tube and an outer copper tube. The material to be separated is contained in the annular space between the tubes and is subjected to a large temperature gradient by condensing steam in the inside tube and cooling water around the outside tube, undoubtedly setting up appreciable countercurrent flows. The best results were obtained with an annular clearance of about 0.25 mm. on the radius. The procedure has been to fill the column from the bottom from a comparatively large reservoir. During the run this reservoir is connected to the base of the column but does not circulate with the base. That is, the base is allowed to drop in composition as the top becomes richer. This type of operation makes the analysis a little indefinite but it is felt that reasonable allowances can be made for these conditions.

Table I shows the results calculated from three different sets of data. The first is for a column with a spacing of 0.21 mm. The second is for a column with 0.25 mm spacing and somewhat higher temperature difference. The third is for this same column, using values of the enrichment ratio which the experimenter feels are more in line with other results. The minimum size plant is composed of 20,000 36-foot columns and has an equilibrium time of 580 days. The steam consumption is estimated at 12.0 MM pounds per hour and the water circulation at 1.60 MM gallons per minute.

METHOD OF CALCULATION

The rate of approach to equilibrium of a single countercurrent column may be expressed approximately by the following expression (see paper by K. Cohen, attached, Eq. 4.23. It is assumed that ρ is roughly equal to C_1):



- 2 -

THIS DOCUMENT CONTAINS INFORMATION AFFECTING THE NATIONAL DEFENSE OF THE U. S. WITHIN THE MEANING OF THE ESPIONAGE ACT, 50 U. S. C., 31 AND 32. ITS TRANSMISSION OR THE REVELATION OF ITS CONTENTS IN ANY MANNER TO AN UNAUTHORIZED PERSON IS PROHIBITED BY LAW.

$$\frac{N_t}{N_f} - 1 = \left(\frac{N_t}{N_f} - 1\right)_{\text{equil.}} \left(1 - e^{\frac{-nB^2L\,F(nB)T}{H}}\right) \qquad (1$$

In this expression n is the number of plates, 1 + B = α which is the ratio of the concentration in one stream to the concentration which would be in equilibrium with it in the other stream, L is the flow in each stream, H is the column holdup, T is time and the function F(nB) is plotted in Fig. 4 of the attached paper (nB is equivalent to 2σ). In this expression, the value nB may be determined from the relation

$$nB = \ln \left(\frac{N_t}{N_f}\right)_{\text{equil.}} \qquad (2)$$

and the value F(nB) may be read from Fig. 4. The time for 50% of equilibrium is obviously the time for which the exponental term equals 0.5, or the exponent of e is -0.692. Equating the exponent of e to -0.692, the following relation is obtained.

$$nB^2L = \frac{0.692\ H}{T\ F(nB)} \qquad (3)$$

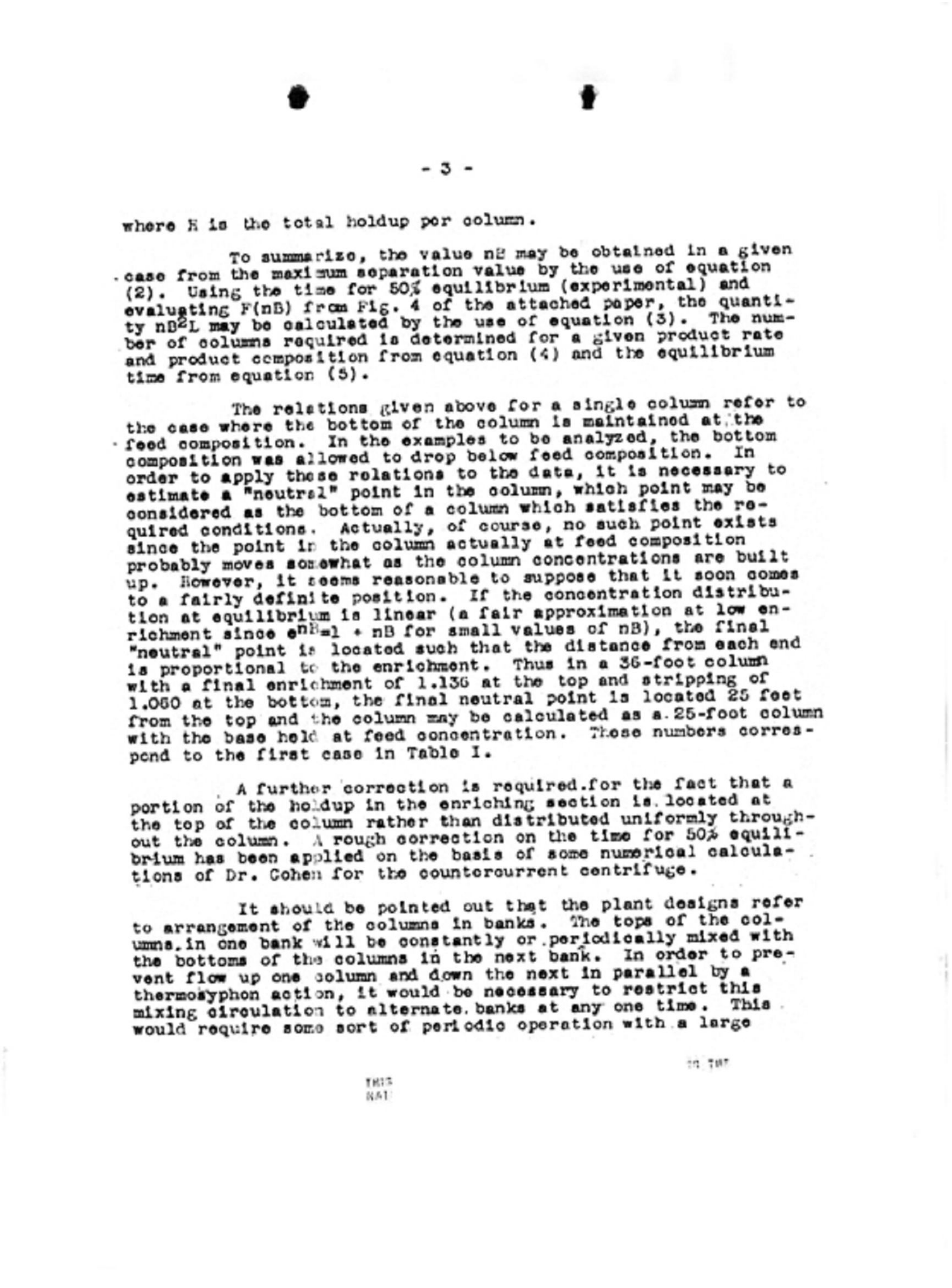
in which T is the time for the column to reach 50% equilibrium.

It may be shown that the number of countercurrent columns in a large plant, each with n plates, a flow in each stream of L and an equilibrium separation factor of 1 + B is

$$\text{Number} = \frac{4\ P\ Y_p}{nB^2L} \qquad (4)$$

in which P is the product rate and Y_p is a function of the product concentration, which for 90% product equals 130. The equilibrium time for such a cascade may be defined as the amount of light isotope present in the plant at producing conditions in excess of that present at feed concentration divided by the net production of the light isotope under steady conditions. Since the average net composition of material in a plant to produce 90% product is 3.57%, the time defined in this way may be shown to be

$$\text{Time} = \frac{.0357 \times 4\ P\ Y_p\ H}{nB^2L\ P \times 0.9} = 20.6\ \frac{H}{nB^2L} \qquad 5)$$



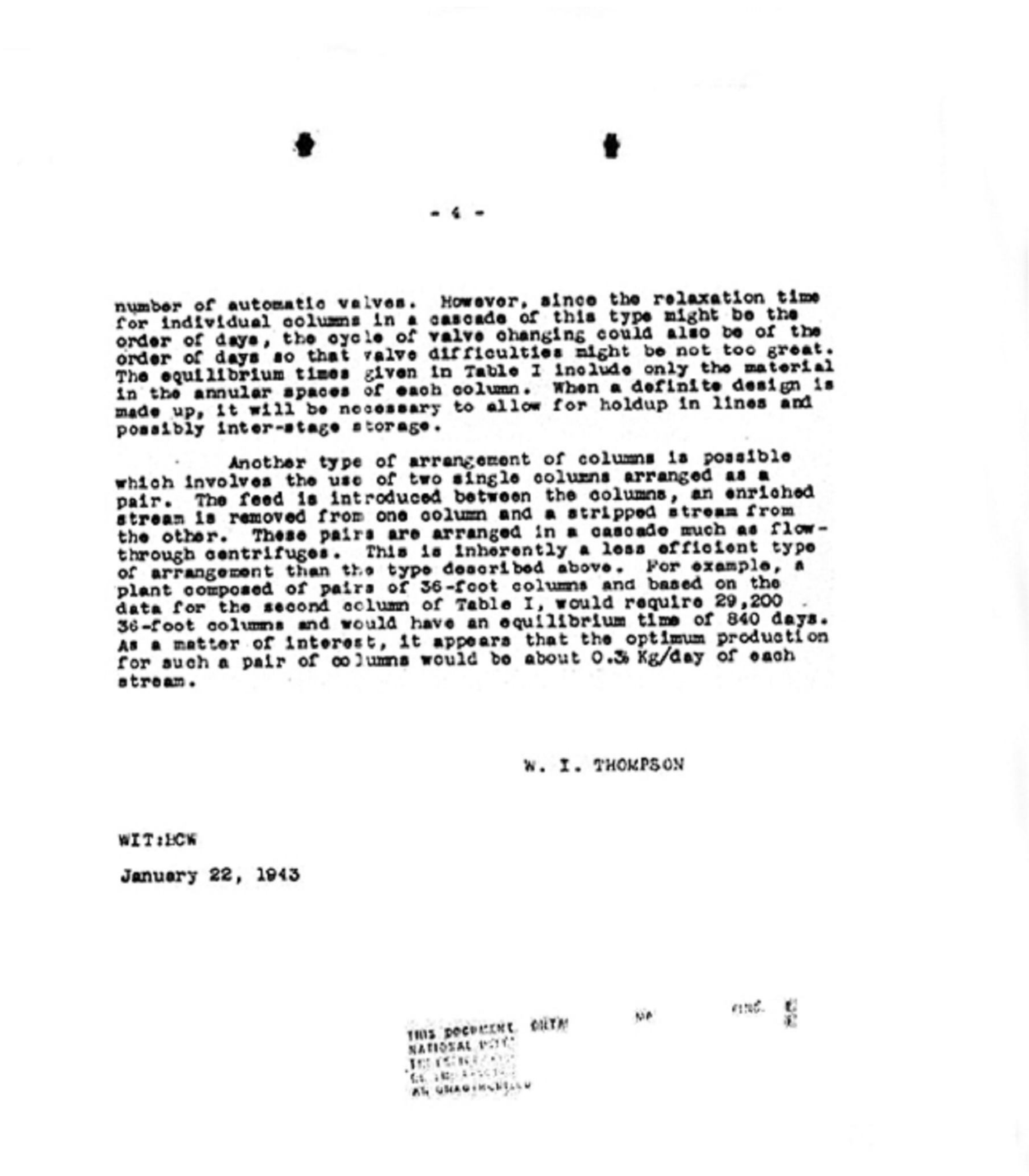
where H is the total holdup per column.

To summarize, the value nB may be obtained in a given case from the maximum separation value by the use of equation (2). Using the time for 50% equilibrium (experimental) and evaluating F(nB) from Fig. 4 of the attached paper, the quantity nB^2L may be calculated by the use of equation (3). The number of columns required is determined for a given product rate and product composition from equation (4) and the equilibrium time from equation (5).

The relations given above for a single column refer to the case where the bottom of the column is maintained at the feed composition. In the examples to be analyzed, the bottom composition was allowed to drop below feed composition. In order to apply these relations to the data, it is necessary to estimate a "neutral" point in the column, which point may be considered as the bottom of a column which satisfies the required conditions. Actually, of course, no such point exists since the point in the column actually at feed composition probably moves somewhat as the column concentrations are built up. However, it seems reasonable to suppose that it soon comes to a fairly definite position. If the concentration distribution at equilibrium is linear (a fair approximation at low enrichment since $e^{nB}=1+nB$ for small values of nB), the final "neutral" point is located such that the distance from each end is proportional to the enrichment. Thus in a 36-foot column with a final enrichment of 1.136 at the top and stripping of 1.060 at the bottom, the final neutral point is located 25 feet from the top and the column may be calculated as a 25-foot column with the base held at feed concentration. These numbers correspond to the first case in Table I.

A further correction is required for the fact that a portion of the holdup in the enriching section is located at the top of the column rather than distributed uniformly throughout the column. A rough correction on the time for 50% equilibrium has been applied on the basis of some numerical calculations of Dr. Cohen for the countercurrent centrifuge.

It should be pointed out that the plant designs refer to arrangement of the columns in banks. The tops of the columns in one bank will be constantly or periodically mixed with the bottoms of the columns in the next bank. In order to prevent flow up one column and down the next in parallel by a thermosyphon action, it would be necessary to restrict this mixing circulation to alternate banks at any one time. This would require some sort of periodic operation with a large



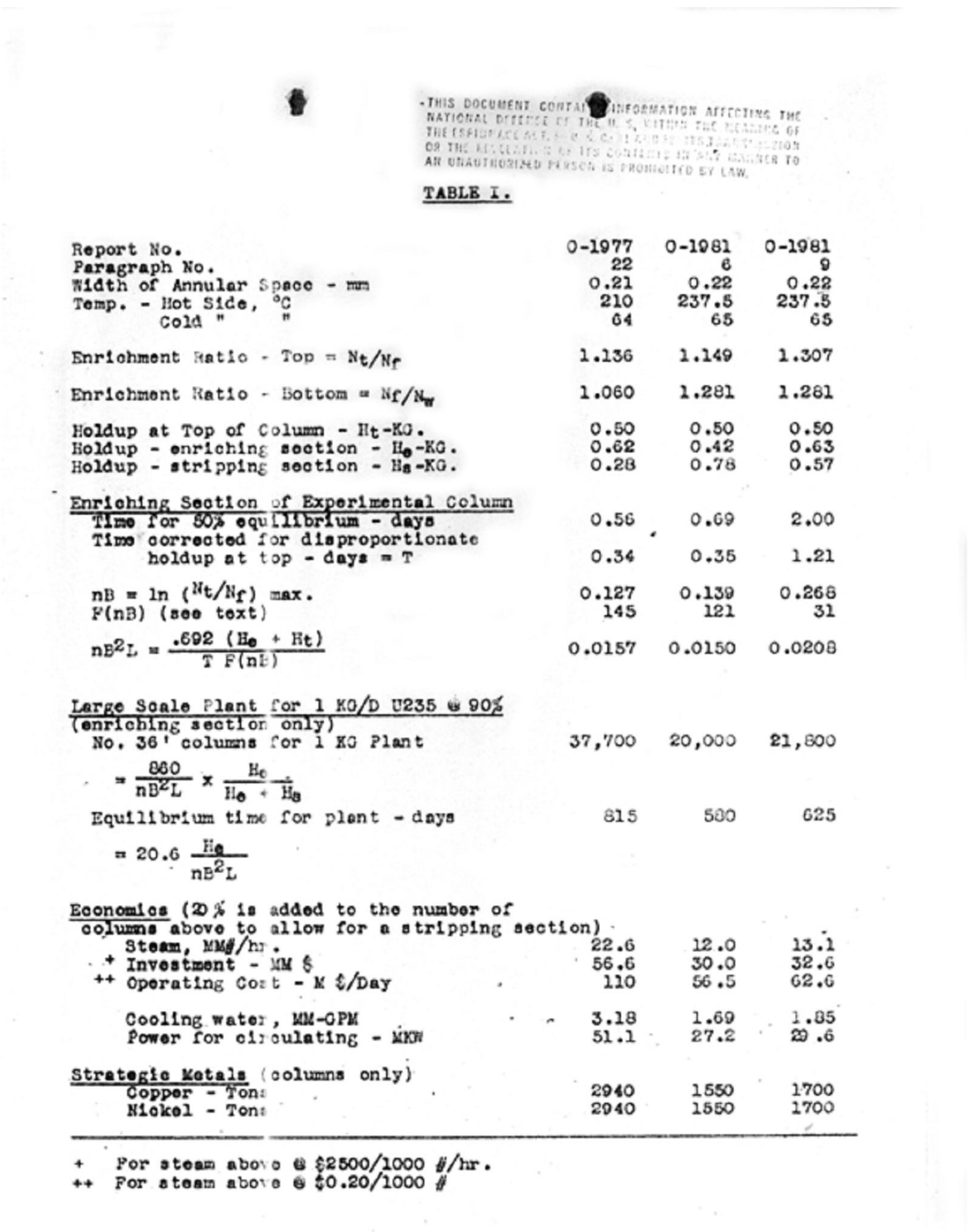
number of automatic valves. However, since the relaxation time for individual columns in a cascade of this type might be the order of days, the cycle of valve changing could also be of the order of days so that valve difficulties might be not too great. The equilibrium times given in Table I include only the material in the annular spaces of each column. When a definite design is made up, it will be necessary to allow for holdup in lines and possibly inter-stage storage.

Another type of arrangement of columns is possible which involves the use of two single columns arranged as a pair. The feed is introduced between the columns, an enriched stream is removed from one column and a stripped stream from the other. These pairs are arranged in a cascade much as flow-through centrifuges. This is inherently a less efficient type of arrangement than the type described above. For example, a plant composed of pairs of 36-foot columns and based on the data for the second column of Table I, would require 29,200 36-foot columns and would have an equilibrium time of 840 days. As a matter of interest, it appears that the optimum production for such a pair of columns would be about 0.3 Kg/day of each stream.

W. I. THOMPSON

WIT:HCW

January 22, 1943



THIS DOCUMENT CONTAI[illegible] INFORMATION AFFECTING THE NATIONAL DEFENSE OF THE U. S. WITHIN THE MEANING OF THE ESPIONAGE ACT, [illegible] ITS TRANSMISSION OR THE REVELATION OF ITS CONTENTS IN ANY MANNER TO AN UNAUTHORIZED PERSON IS PROHIBITED BY LAW.

TABLE I.

Report No.	0-1977	0-1981	0-1981
Paragraph No.	22	6	9
Width of Annular Space - mm	0.21	0.22	0.22
Temp. - Hot Side, °C	210	237.5	237.5
Cold " "	64	65	65
Enrichment Ratio - Top = N_t/N_f	1.136	1.149	1.307
Enrichment Ratio - Bottom = N_f/N_w	1.060	1.281	1.281
Holdup at Top of Column - H_t-KG.	0.50	0.50	0.50
Holdup - enriching section - H_e-KG.	0.62	0.42	0.63
Holdup - stripping section - H_s-KG.	0.28	0.78	0.57
Enriching Section of Experimental Column			
Time for 50% equilibrium - days	0.56	0.69	2.00
Time corrected for disproportionate holdup at top - days = T	0.34	0.35	1.21
$nB = \ln ({}^{N_t}/N_f)$ max.	0.127	0.139	0.268
F(nB) (see text)	145	121	31
$nB^2L = \frac{.692\ (H_e + H_t)}{T\ F(nB)}$	0.0157	0.0150	0.0208
Large Scale Plant for 1 KG/D U235 @ 90% (enriching section only)			
No. 36' columns for 1 KG Plant $= \frac{860}{nB^2L} \times \frac{H_e}{H_e + H_s}$	37,700	20,000	21,800
Equilibrium time for plant - days $= 20.6\ \frac{H_e}{nB^2L}$	815	580	625
Economics (20% is added to the number of columns above to allow for a stripping section)			
Steam, MM#/hr.	22.6	12.0	13.1
+ Investment - MM $	56.6	30.0	32.6
++ Operating Cost - M $/Day	110	56.5	62.6
Cooling water, MM-GPM	3.18	1.69	1.85
Power for circulating - MKW	51.1	27.2	29.6
Strategic Metals (columns only)			
Copper - Tons	2940	1550	1700
Nickel - Tons	2940	1550	1700

\+ For steam above @ $2500/1000 #/hr.

++ For steam above @ $0.20/1000 #

Documents Reproduced

Memorandum of W. I. Thompson, “Analysis of separation data from liquid thermal diffusion experiments,” dated January 22, 1943; 5 pp pages, including “table one [Table I].” Source: Author’s files.

“Feed to plant in kg/day (For product of 10 kg/day).” Curves giving the anticipated possible production of the Navy thermal liquid diffusion plant with 100 tubes connected in parallel. From: 1944, June 3. Memorandum of Mssrs. W. K. Lewis, E. V. Murphree and R. C. Tolman to Major General L. R. Groves; Subject: “Possible Utilization of Navy Pilot Thermal Diffusion Plant.” Source: Author’s files.

Appendix B

David Hawkins' *Manhattan District History:* Development of the Mark II.

David Hawkins' *Manhattan District History* of the Los Alamos Project is not conveniently available to most interested readers, so I have concatenated pertinent extracts from that *History* which report development of the autocatalytic uranium hydride lateral implosion experimental device which, beginning on 4 July 1944, was named the Mark II by James Conant. Hawkins does not refer to the bombs and bomb designs in development at Los Alamos by the "Mark" designations, which are identified and used in the text chapters of *The Last Wave from Port Chicago.*

I have also included in this Appendix paragraphs from Hawkins' *History* that report the activities of other persons at Los Alamos who are mentioned in the text chapters of *The Last Wave from Port Chicago:* Captain Williams S. Parsons, USN; Commander Frederick L. Ashworth, USN; Dr. Maurice M. Shapiro; Ensign George T. Reynolds, USNR, etc.

I was privileged to have several conversations with University of Colorado Distinguished Professor Emeritus of Philosophy David Hawkins from 1982 until the year preceding his death on 24 February 2002 at age 88. Primarily our discussions centered on aspects and elements of the *Manhattan District History*. Professor Hawkins' *History* is constructed primarily from the extensive notes he compiled from verbal reports and briefings that he received from those persons foremostly involved in the work at Los Alamos, where Hawkins was resident from spring 1943 though the end of the war. He wrote the *History* during 1946 and 1947.

The *History* was CLASSIFIED until 1 December 1961 when it was distributed as report LAMS-2532 (Vol. I) by Los Alamos Scientific Laboratory of the University of California, and is Volume I of the two-volume *Manhattan District History, Project Y, the Los Alamos Project.* Volume I, the Hawkins' history, is titled *Inception until August 1945.* Chapters III - VIII of Volume I report the period of Los Alamos history from April 1943 to August 1944; Chapters IX - XIX report the period August 1944 to August 1945. Volume II, titled *August 1945 through December 1946,* was written by Edith C. Truslow and Ralph Carlisle Smith; Volume II reports the period of Los Alamos history from August 1945 through December 1946.

The original two-volume *Manhattan District History, Project Y, the Los Alamos Project* was republished in one volume by Tomash Publishers, Los Angeles, California, 1983, as Volume 2 of the series *History of Modern Physics, 1800-1950,* with the title *Project Y, the Los Alamos Story*. The Tomash edition includes a new introduction and a bibliography; the original text has been edited and includes modifications, additions and deletions of the original text. At the time of this writing, 24 July 2002, one copy of the Tomash edition is offered for sale on the Internet at $89.00 (U.S.).

David Hawkins was born at El Paso, Texas, and was raised in New Mexico. His knowledge of the terrain and topography of New Mexico contributed to the selection of Trinity Site for the 16 July 1945 test of the Mark IV spherical plutonium implosion gadget. He received his undergraduate degree (1934) and Masters of Arts degree (Philosophy, 1936) from Stanford University. He completed his doctorate in probability theory at the University of California, Berkeley, in 1941 and joined the faculty there. In 1943 his friend and faculty colleague at Berkeley, J. Robert Oppenheimer, invited him to join the project at Los Alamos where Hawkins was designated official historian of the project.

In 1947 Hawkins joined the faculty of the University of Colorado, Boulder, where he taught philosophy and the physical sciences. After

World War II, he publicly criticized the Manhattan Project and lobbied in Washington for international controls on the development of nuclear weapons. Al Bartlett, a University of Colorado professor who worked with Hawkins on the Manhattan Project, called him “one of the greatest intellects I’ve ever known.” His widow, Frances Pockman Hawkins (Stanford, 1935), has described David Hawkins as a pacifist. In addition to his curriculum duties in philosophy and the physical sciences at the University of Colorado, Professor Hawkins trained math and science teachers in the education of children and wrote about the philosophy of science.

Professor Hawkins’ *Manhattan Project History* is a work he compiled and wrote from his accumulated notes and daily logbooks in which he recorded his participation in and observation of Los Alamos activities from spring of 1943 through the end of the war; conversations, verbal reports and briefings that he received as the designated Project Y historian were additional grist to his intellectual mill. Prior to declassification and public release, Hawkins’ *History* had been substantially edited by Los Alamos to delete all classified information. Hawkins’ *History* is not a document-derived history, and few primary documents are cited in the *History*.

Hawkins told me he had been unaware of the “Mark” designations of the bombs and bomb designs in development at Los Alamos. He said he had been unaware that the autocatalytic uranium hydride lateral implosion experimental device (Mark II) had been so completely developed by 4 July 1944 that James Conant was able to instruct General Groves on that date that the Mark II, with a nominal 1,000 tons TNT equivalent energy yield, was available to the Joint Chiefs of Staff for the purposes of operational planning.

He was unaware that by 4 July 1944 Conant had instructed General Groves that the Joint Chiefs of Staff should be informed the Mark II would necessarily be proof fired once before the design could be available for use against the enemy. He was, he said, only incidentally aware of the 17 July 1944 Port Chicago explosion, which is not mentioned in his *History*. He was unaware that on 17 August 1944 James Conant reported to General Groves the decision made at Los

Alamos that the Mark II should be put on the shelf and that the Mark II could be developed for combat use in 3 or 4 months time from 17 August 1944. Hawkins was unaware that on 17 August 1944 the upper limit of effectiveness of the Mark II was known, and that Los Alamos expected the nominal 1,000 tons TNT equivalent Mark II could be somewhat improved.

Typographical errors and apparently incorrect or inaccurate text in the original I have recognized editorially with the notation, [sic]; other typographical errors that may occur are my own. All text in boldface type is text to which I have added that emphasis to highlight germane occurrences of subjects and names.

Extracts from: Manhattan District History. Project Y, the Los Alamos Project. Volume I. Inception until August 1945.

Text occurrences by Subject and Name

Ashworth, USN, Commander Frederick L.
autocatalysis
autocatalytic
autocatalytic bomb
autocatalytic methods
autocatalytic methods of assembly
B^{10}
ball of fire
bomb of uranium hydride
bomb made of hydride
bomb made of uranium hydride
boron
compression or expulsion of neutron absorbers
Hirschfelder, Joseph O.
hollow steel cylinders
hydride bomb
hydride bombs
hydride calculations
hydride compacts
hydride core
hydride critical assemblies
hydride critical masses
hydride in a bomb
hydride program
hydride gun
hydride gun program
hydride of uranium
hydride-plastic cubes of composition UH_{10}
hydride problem
hydride program
hydride mixtures
hydrides
hydrogen
hydrogen-to-uranium ratio
hydrogenous
hydrogenous binding agent
hydrogenous material
hydrogenous moderator
imploding cylinders
implosion
Parsons, Captain Williams S.
Penney, William G.

self-assembling or autocatalytic method
Reynolds, USNR, Ensign George T.
Shapiro, Maurice M.
Spedding, Frank H.
Teller, Edward
UH_3
UH_4
UH_{10}
UH_{10} plastic
UH_{30}
UH_{80}
uranium hydride
uranium hydride bomb
uranium hydride gun
uranium hydride mixtures
uranium hydride program
uranium-hydrogen compositions
Urey, Harold C.
Workman, E. J.

Introduction.

1.44 Autocatalysis, Implosion. Two other methods of assembly had been proposed, and it was a part of the early program to investigate them. One of these was a **self-assembling or autocatalytic method**, operating by the **compression or expulsion of neutron absorbers** during the reaction. Calculation showed that this method as it stood would require large quantities of material and would give only very low efficiencies.

1.54 . . . Calculations had to be made for three materials :U^{235}, Pu^{239}, and also a new compound, a **hydride of uranium**, which seemed to have certain advantages over metallic uranium as a bomb material. . . .

1.56 The program included, finally, the further investigation of bomb damage, of the possibility of **autocatalytic methods of assembly**, and the proposal to amplify the effect of fission bombs by using them to initiate thermonuclear reactions.

1.62 Fission Cross Sections. Fission cross sections had been measured by the subproject under N. P. Heydenberg at the Department of Terrestrial Magnetism of Carnegie Institute, by McKibben's group at Wisconsin, and by Segre's group in Berkeley. These measurements—for U^{235}—covered the neutron energy range above 125 kev, and the range below 2 ev. When the curve for fission cross sections over the high energy was extrapolated downward, a figure was obtained for thermal energy that was much larger than the cross section actually observed. Since the extrapolated region covered the important range of neutron energies in a **bomb of uranium hydride**, measurements were planned to investigate cross sections at these intermediate energies and resolve the apparent anomaly. Fission cross sections of Pu^{239} were already known at thermal energies and at a few high energies. Here also measurements were planned to cover the entire range of energies up to about 3 Mev.

1.71 At the beginning of the Los Alamos Project . . . it was not known whether U^{235}, Pu^{239}, or both would be used, or whether the bomb material would be metal or compound. . . .

1.74 The metallurgy program included research and development on the metal reduction of uranium and plutonium, the casting and shaping of these metals and compounds such as **uranium hydride**, as well as various possible tamper materials. . . .

1.77 A corollary feature of the ordnance program has been its simultaneous investigation of alternative methods. The uncertainties of nuclear specification, and the possibility that one or another line of investigation might fail, have made such a policy unavoidable. Of the three methods of producing a fission bomb (**autocatalysis**, the gun, the **implosion**) that have been discussed, the last two were singled out for early development. **Autocatalysis** was not eliminated; but it was not subject to development until some scheme was proposed which would give a reasonable efficiency. This did not occur during the course of the project, although **autocatalytic methods** continued to receive considerable theoretical attention. Of the remaining two methods, the gun appeared the more practical; it used a known method of accelerating large masses to high velocities. The problem of "catching" a projectile in a target and starting a chain reaction in the resulting supercritical mass was obviously a difficult one, but it seemed soluble.

1.78 The method of implosion, on the other hand, was much farther removed from existing practice . . . At a meeting on ordnance problems late in April [1943], Neddermeyer presented the first serious theoretical analysis of the **implosion**. His arguments showed that the compression of a solid sphere by detonation of a surrounding high-explosive layer was feasible, and that it would be superior to the gun method both in its higher velocity and shorter path of assembly. Investigation of the method was begun almost immediately. It subsequently received two increases of priority, until at the end of the project it had become the dominant program throughout the Laboratory.

1.83 . . . [The report of the reviewing committee, dated May 10, 1943] took note of the newly discovered possibility for use of **uranium hydride**. Pointing out that the existence of the hydride had been learned of at Los Alamos somewhat by accident, the committee recommended a more systematic technical liaison between this and other branches of the larger project. It also recommended that the study of U^{233} as a possible explosive material be continued.

[**Note** on U^{233}. Rarely mentioned in the general literature as an active bomb material. “Special nuclear material” (SNM) is defined by Title I of the Atomic Energy Act of 1954 as plutonium, U^{233}, or uranium enriched in the isotopes U^{233} or U^{235}. The definition includes any other material that the Commission determines to be special nuclear material, but does not include source material. The NRC has not declared any other material as SNM. U^{233} does not occur naturally but can be formed in nuclear reactors and extracted from the highly radioactive spent fuel by chemical separation. U^{233} can be produced in special ‘breeder” reactors that use thorium as fuel. Only small quantities of U^{233} are reported to have been made in the United States.]

The Period April 1943 to August 1944.

3.1 The first period of the Los Alamos Laboratory’s existence [April 1943 to August 1944] presented the problems common to organizational beginning . . . In a position of responsibility parallel to that of the Director [J. Robert Oppenheimer] was established the Governing Board. This consisted of the Director, Division Leaders, general administrative officers, and individuals in important technical liaison positions.

3.7 The membership of the Governing Board was: Bacher, Bethe, Kennedy, Hughes (3.20), Mitchell, [Captain William S.] **Parsons** (7.3), and Oppenheimer. Later additions were McMillan, **Kistiakowsky** (7.55), and Bainbridge (7.4).

4.9 . . . A number of quite basic weapon specifications, to go to the next stage, remained undermined for a considerable length of time. One was the choice of a tamper; another was the **uranium hydride** possibility; and a third was the mechanism of assembly—gun or implosion.

4.12 . . . From a combination of relative and absolute fission cross section experiments performed over the period to August 1944, it was possible to plot fission cross section curves as a function of [neutron] energy for both U^{235} and Pu^{239} from thermal energies to several million electron volts. These results were not only used in more accurate critical mass and efficiency calculations, but also were partially responsible for the abandonment of the **uranium hydride program**; partly because they showed that the energy-dependence which would make the **hydride** an efficient weapon did not occur, and partly because, through the evidence they provided for the existence of considerable radiative capture at thermal energies, the critical mass and efficiency estimates of metal uranium bombs became more optimistic. Investigation, suggested by the behavior of fission cross sections at low energies, led to the discovery that radiative capture in U^{235} was indeed significant, and even greater for Pu^{239}. Since measurements of the neutron number had been made at thermal energies for total absorption (capture plus fission) and not fission alone, and since capture would become less important at the high energies of neutrons operative in the bomb, it followed that the effective neutron number in both materials was higher than had been assumed. As a result of these considerations, the **hydride program** was carried on after the spring of 1944 only at low priority.

4.13 Although the **hydride program** was unsuccessful, the process of learning enough to understand its limitations contributed in a number of ways to the whole program. For example, the use of the assumption that the fission cross section was inversely proportional to neutron velocity made clear the importance of inelastic scattering in the tamper. In the first approximation it had been assumed that only neutrons scattered back elastically would contribute in any important way to the reactions. But if decreasing neutron energy was compensated for by increasing the fission cross sections, this assumption could not safely be made. A lengthy series of back-scattering and transmission experiments with a considerable list of potential tamper materials was made, in which the scattering cross sections were measured for neutrons of various energies and for various scattering angles, and in which the energy degradation of scattered neutrons was also measured.

4.21 . . . At a Governing Board Meeting of October 28, 1943, the [**implosion**] program was reviewed and the decision made to strengthen and push it . . . Ordnance and engineering work was geared to the gun program, and could not be redirected overnight. By the end of 1943 the implosion had caught up with the gun in priority; by April 1944, its facilities had been greatly expanded, and enough experimental evidence was in to show the great magnitude of the difficulties that were still ahead.

4.25 The quantitative investigation of the hydrodynamics of the **implosion** proved a very difficult job . . . In the spring of 1944, the problem was set up for IBM machine calculation. These machines, which had recently been procured to do calculation on odd-shaped critical masses, were well adapted to solve the partial differential equations of the implosion hydrodynamics.

4.26 As was not unnatural at the beginning of this new line of investigation, there was some thought given to the implosion of **uranium hydride**. The density of this material was about half that of uranium, and the space occupied by the hydrogen would be recoverable under sufficient pressure. Samples of **hydride** prepared at Los Alamos were investigated at the high pressure laboratory of W. P. Bridgman at Harvard. Pressure density data up to 10 kilobars, still very low pressure from the point of view of the implosion, gave indication that the **hydride** was not in fact very easily compressible.

4.27 While theoretical investigation was familiarizing the Laboratory with the enormous potentialities of the **implosion**, its empirical study was getting under way. During the period to April 1944 some data were obtained from terminal observation, from the HE flash photography of **imploding cylinders**, and from flash X-ray photography of small imploding spheres.

4.28 Whereas the theoretical studies of the implosion assumed a symmetrical converging detonation wave, the only feasible method of detonating the HE was to initiate one or several diverging waves. It was assumed or, better, hoped that with several detonation points symmetrically spaced around a sphere, the difference would not be essential. From terminal observations some indications of asymmetry of collapse were obtained, but it was difficult to ascertain their cause. The first successful HE flash photographs of **imploding cylinders** showed that there were indeed very serious asymmetries in the form of jets which traveled ahead of the main mass. A number of interpretations of these jets were proposed, including the possibility that they were optical illusions.

4.29 Another virtue of the **hydride program** not mentioned in paragraph 4.13 was the interest taken in the preparation and fabrication of this material. Studies were begun, among the first undertaken by the metallurgists, in the art of preparing high density compacts of this material. The result was that although after a year or so it was known that the **hydride** would not yield an efficient weapon, this material could be easily fabricated, and was used in making experimental reactors.

4.30 . . . Apart from early work with the **hydride**, effort was first concentrated on the metallurgy of uranium. . . .

4.33 Aside from the metallurgy of active materials—**uranium hydride**, uranium, and plutonium—several techniques were developed for the fabrication of materials with important nuclear properties, notably **boron** and beryllia. These were techniques of powder metallurgy, and the object in both cases was to attain the highest possible densities. The main pressure for the production of **boron** came again from the **hydride gun** program, for which it would be difficult to dispose a sufficient number of critical masses of **hydride** into gun and target.

4.34 In this connection the Laboratory undertook to procure large amounts of **boron** enriched in $\mathbf{B^{10}}$, which constitutes about 20 percent of the normal **boron**. A method for the separation of $\mathbf{B^{10}}$ had been developed by **Urey**, and was further developed by him at the request of the Los Alamos Laboratory. A pilot plant was constructed in the fall of 1943, to develop the method and to provide experimental amounts of the separated isotope. Early estimates (February 1944) set the needed production rate of the isotope at a figure comparable to the production of separated uranium. Plant construction was undertaken by Standard Oil of Indiana. Difficulties in construction and a decreasing probability that **boron** would be used in large amounts caused a decrease in the scheduled capacity of the plant by 25 per cent.

4.35 Even after there was reasonable assurance that a **bomb made of hydride** would not be used, and especially not a **hydride gun**, it was decided to maintain production of the $\mathbf{B^{10}}$ isotope because of its potential usefulness in an **autocatalytic bomb**, if one could be developed. This isotope was, indeed, very useful in small quantities in counters and as a neutron absorber.

5.3 During June 1944, **R. Peierls** took charge of the **Implosion Group** [of the Theoretical Division] in place of E. Teller who formed an independent group outside the Theoretical Division (13.3). This group acquired full responsibility for implosion IBM calculations. During July 1944 Group O-5 (E-8, 7.1) joined the Theoretical Division on a part time basis, its work in the Ordnance Division being largely completed (14.1).

5.12 The attack on the many-velocity problem had proceeded simultaneously with the work described above, in the sense of investigating methods by which the many-velocity problem could be reduced to a series of one-velocity problems. This work was done primarily by **Group T-4**. The problem posed itself naturally in connection with the investigation of the **uranium hydride bomb**, for in this case the energy degradation of neutrons from elastic collisions with hydrogen was one of the essential characteristics of the chain reaction. Quite early, methods were found for treating the **hydride problem**, with a continuum of velocities, under quite unrealistic assumptions, such as an infinite medium of core material in which there was a sinusoidal distribution of neutrons. The case involving two media, i.e., core and tamper of different materials, could not be treated at first. By July 1944, however, a method had been developed which was applicable to a spherical core and tamper. This method allowed the treatment of a continuum of velocities, and was subject only to the restriction that there be no inelastic scattering in the tamper medium.

Unfortunately this inelastic scattering was not a negligible effect with the tampers that were being considered. Within a fairly short time this difficulty had been overcome, although only to the extent of allowing for three or four neutron velocity groups instead of the continuum.

5.13 In the case of **hydrogenous material** it could not be assumed that neutrons were scattered isotopically. It was found however, semi-empirically, that this fact was adequately accounted for by the use of the transport cross section, as in the case of the all-metal diffusing medium.

5.14 Other means for accounting for the continuum of velocities were adopted in special problems, such as that of calculating the distribution of thermal neutrons in the Water Boiler.

Water Boiler

5.15 One of the first practical requirements in critical mass calculation was to estimate the critical mass of the Water Boiler. These calculations were made by a variety of methods. In this case as in that of the **hydride calculations**, the slowing down was an essential factor; in fact, the boiler would be of small critical dimensions only because it slowed neutrons down to thermal velocities, taking advantage of the larger thermal fission cross section of U^{235}. The standard method, the “age theory” that had been developed by Fermi for calculating the thermal neutron distribution in piles, was inaccurate when applied to a small enriched reactor, because it required a very gradual slowing down of the neutrons. This condition was satisfied for a carbon moderator, with mass 12 times that of the neutrons; it was not satisfied with a **hydrogenous moderator** such as water, because the neutrons and hydrogen nuclei are of the same mass, and energy loss can occur rapidly. . . .

5.57 The detailed investigation of damage and other effects of [a] nuclear explosion was not pursued very far in the period under review [April 1943 to August 1944]. Some results, going beyond the rough estimates reported in paragraph 1.57 were, however, obtained in the summer and fall of 1943. There was further investigation of the shock wave in air produced by the explosion, of the optimum height for the explosion, of the effects of diffraction by obstacles such as buildings, and of refraction caused by temperature variation. There was some calculation of the energy that might be lost through the evaporation of fog particles in the air. Estimates were made of the size of the "**ball of fire**" after the explosion, and the time of its ascent into the stratosphere. The theory of shallow and deep underwater explosions was investigated, and led to the suggestion of model experiments.

5.60 Some of the more important cooperative work between the Theoretical Division and the other divisions of the Laboratory has already been mentioned; for example, the interpretations of scattering data, and calculations of the water boiler and **hydride critical masses**, and the calculations of the hydrodynamical characteristics of the implosion. . . .

5.61 One rather conspicuous example of theoretical influence on the design of experiments was the "Feynman experiment," an experiment which was never performed but whose principle was embodied in several experiments. This was simply the proposal to assemble near-critical or even supercritical amounts of material safely by putting a strong neutron absorber (the $\mathbf{B^{10}}$ boron isotope) uniformly into the core and tamper. For an absorber with an absorption cross section inversely proportional to the velocity of the neutrons absorbed, it could be shown that the effect was to decrease the multiplication rate in the system by an amount which was directly proportional to the concentration of absorber. Thus an amount of material which would be supercritical could be made subcritical by the addition of boron; from a measurement of the rate at which the neutron died out in this system, the rate could be simply calculated at which they would increase if the boron were absent.

5.64 Mention should be made here of safety calculations made by Group T-1 and later by Group F-1 for the Y-12 and K-25 plants. The Group Leader, **E. Teller**, was appointed as consultant for the Manhattan District as a whole on the dangers of possible supercritical amounts of material being collected together in the plants producing separated U^{235}.

6.29 The emphasis in fission cross section measurements was early influenced by interest in the **uranium hydride bomb**. The theory of this bomb is explained more fully in Chapter V. Suffice it to say that the practicability of this type of weapon depended on the hypothesis that the slowing down of neutrons by **hydrogen** was compensated in its delaying effect by a corresponding increase in the fission cross section with decreasing neutron energy. If this hypothesis were true, the rate at which the explosion takes place would remain the same as in a metal bomb, while the critical mass would be considerably decreased. Evidence for the inverse dependence of cross section on neutron velocity was the early work at Wisconsin (1.62) [McKibben] which showed approximately 1/v dependence from 0.4 Mev down to 100 Mev [sic; should be "down to 100 kev"]. The same law of dependence was also verified between thermal velocities and 2 ev. On the other hand when the latter dependence was extrapolated to higher energies, and the high energy curve to low energies, the two failed to cross. In fact between 2 ev and 100 kev there was found a 12-fold increase in the coefficient of 1/v to be accounted for. Since the practicability of the **hydride bomb** depended upon the actual shape of the curve in this region, it was of great importance to know approximately where the break occurred.

6.30 In this connection it was found from **boron** absorption measurements made by the electrostatic Generator Group in August 1943 that the break occurred between 25 and 40 ev. This was the first indication that fission cross sections do not follow a simple law in the epithermal region. Because the break occurred at this low energy, the possibility of a **hydride bomb** was not yet excluded.

6.36 . . . When early in 1944 the short electrostatic generator rebuilding program was completed (6.4). High currents and energy regulation to within 1.5 kev incorporated into this machine made it possible to utilize the back-angle neutrons from the Li(p,n) reaction down to less than 5 kev. Development of new counters—the so-called long counters—indicated the possibility of bringing the absolute fission cross section measurements down to the region of a few kev, where they were still extremely uncertain. This apparently simple experiment became long and involved because of difficulties in interpreting the counter data obtained. Checks by independent methods became necessary, one which gave considerably lower cross section values in the 30 kev region than had first been obtained. If this lower value of the cross section were correct, it would reduce somewhat the potentialities of the **hydride bomb**. After considerably further investigation of counters and the construction of an antimony-beryllium source of 25 kev neutrons, the lower value was finally confirmed. The principal result of these efforts was another blow to the **hydride gun program**.

6.49 . . . The notion prevailed for some time that inelastic scattering (i.e., scattering in which the neutrons, although not captured by the tamper nuclei, lose part of their energy to them by excitation) would play an unimportant role, since it would probably reduce neutrons to a very low energy where they would not contribute materially to the explosive chain reaction. Very little was known, moreover, about the variation of scattering with neutron energy. It was thought, at the time, that the most important part of the fission spectrum lay at high energies, near 2 Mev. It was felt that to a first approximation the usefulness of a tamper would be determined by the number of neutrons reflected backward to the core. . . .

6.53 By the end of October 1943, back-scattering measurements had been completed for a large list of substances, and a number of [tamper] substances, and a number of instrumental improvements had been made . . . At about this time, also, measurements of the fission spectrum indicated that the important energy range was nearer 1 Mev than 2 Mev. Results of the first experiments indicated, moreover, that earlier ideas about inelastic scattering were incorrect, and that the inelastically scattered neutrons could play an appreciable role in the functioning of a tamper. Recognition of their possible importance was made easier, also, by the current concern of the Laboratory with the **uranium hydride bomb**. The same increase in cross section with decreasing energy that made this bomb seem feasible also suggested that neutrons slowed by inelastic scattering might still make a considerable contribution to an explosive chain reaction.

6.54 For these reasons preparations were made for the study of scattering as a function of energy and scattering angle, taking account of inelastically scattered neutrons. This work was done cooperatively by the D-D and Electrostatic Generator Groups, beginning in November 1943. Back-scattering data were obtained at 1.5 Mev and 0.6 Mev, as well as 3 Mev. In addition to over-all back-scattering measurements, an experiment was performed to give specific information on the degraded neutrons as a function of primary neutron energy for the elements still in the running as scatterers.

6.56 One further scattering experiment was begun in this period, an integral experiment which would attempt to obtain information about the **hydride bomb**. The D-D source was to be surrounded by a modifying sphere mocking the **hydride core** as nearly as possible; integral tamper properties would be investigated around this core as well as neutron distribution in tamper and core. One instrumental development that occurred in this connection was a new fission detector. . . .

6.57 The first chain reacting unit built at Los Alamos was the Water Boiler, a low-power pile fueled by uranium enriched in U^{235}. It was the first pile built with enriched material, the so-called alpha stage material containing about 14 per cent U^{235}. The necessary slowing down or moderation of fission neutrons is provided in this system by the **hydrogen** in ordinary water: the active mixture is a solution of uranyl sulfate in water solution. The tamper chosen was beryllium oxide.

6.59 . . . For economy of material it was important to find the optimum concentration of the solution [for the Water Boiler]. The number of **hydrogen** nuclei had to be large enough to slow down the neutrons to thermal energies, and small enough not to capture too many of them.

6.66 Between the completion of the building in February 1944, and the first operation of the Water Boiler as a divergent chain reactor early in May 1944. . . .

6.69 The operation of the Water Boiler, like that of other controlled reactors, depends upon the very small percentage of delayed neutrons; these make it possible to keep the system below critical for prompt neutrons and in the neighborhood of critical for all, including the delayed neutrons. Although the delayed neutrons are only about 1 per cent of the total, in the region near critical the time dependence of the system—its rate of rise or fall—is of the order of the delay period; prompt chains die out constantly, to be reinstated only because of the delayed neutrons.

6.75 Toward the end of the first period of the Laboratory [April 1943 to August 1944], plans were underway in the Water Boiler Group to make critical assemblies with **uranium hydride**, and to rebuild the water boiler for higher-power operation. Both of these projects carry us over into the next period, when the work of the group was divided between two new groups; this further work is therefore reported in later sections (13.25 ff, 15.4, ff).

7.3 In May [1943] Capt. W. S. **Parsons**, USN, came to the Site for a preliminary visit. His transfer to be head of the ordnance engineering work at Los Alamos was arranged at the request of General Groves, on the recommendation of [James B.] **Conant** and [Vannevar] **Bush** and with the approval of the Governing Board. Capt. **Parsons** returned in June as Division Leader of the Ordnance Division.

7.5 After **Parsons'** first visit in May he investigated the possibilities of obtaining a competent chief engineer to head group E-6 [Ordnance Division–Engineering]. The man chosen by **Parsons** was George Chadwick, for 20 years Head Engineer of the Navy Bureau of Ordnance. Although Chadwick never resided at Los Alamos, he functioned from June to September 1943 as prospective head of this work. During this period he worked with the Bureau of Ordnance and the Navy Gun Factory on the design and fabrication of the first experimental guns, consulted at Los Alamos on the design of the Anchor Ranch Proving Ground, and in August was asked to assist in the procurement in the Detroit area of machinists and draftsmen. At this time Chadwick decided not to take the Los Alamos position. The connection with Chadwick in Detroit remained, however, and is discussed later in this section (7.12).

7.7 In the fall of 1943 Groups E-7 [Delivery] under [Norman] Ramsey and E-8 [Interior Ballistics] under [Joseph O.] **Hirschfelder** were added to this [Ordnance] division.

7.10 When **Parsons** returned to Washington after his first Los Alamos trip [May 1943], he arranged that all his connections with the Navy Department would be handled through Lt. Comdr. Hudson Moore of the Research and Development Section of the BuOrd [Bureau of Ordnance]. The most important activities of the latter was with the Naval Gun Factory and concerned the fabrication of experimental guns. Moore also handled procurement of miscellaneous ordnance materials from Navy stores, and liaison with the Navy Proving Ground at Dahlgren, VA.

7.11 At the same time **Parsons** arranged for security reasons that all Navy equipment would be shipped to E. J. **Workman**, head of Section T, OSRD [Office of Scientific Research and Development], Project at the University of New Mexico, Albuquerque.

7.20 The seriousness of the problem of getting these fantastic guns made and proved called for a great expansion of personnel, facilities and liaison in the Ordnance Division. This expansion was instituted by Captain **Parsons** upon his assignment to the project in May 1943. At this time, the attention of the division was centered immediately upon the practical problems of getting the 3000 feet per second gun made and proved. The reason for this specialization was, simply, that the proposed design of this gun was farthest removed from standard practice. The principal departures from standard design were: (1) this gun tube should weigh only one ton instead of the five tons usually characteristic of the same muzzle energy; (2) consequently, it must be made of highly alloyed steel; (3) the maximum pressure at the breech should be as high as practicable (75,000 pounds per square inch was decided upon), i.e., the gun should be as short as possible, and (4) it should have three independently operated primers.

[**Note.** Neutron-producing impurities (specifically, Pu^{240}) in the plutonium produced at Hanford, Washington, posed the likelihood of predetonation in the gun assembly Mark I weapon using a plutonium active. The rate of critical assembly accomplished by a 3,000 feet per second plutonium projectile was initially considered sufficiently rapid to preclude predetonation, if the presence of impurities in the Hanford plutonium could be significantly reduced. By 11 July 1944 Los Alamos had determined that impurities in the Hanford plutonium could not be significantly reduced. James Conant recorded in his “Historical Note” of 27 July 1944, “It was concluded that the evidence was so clear that '49' [Pu^{239}] prepared at Hanford could not be used in the gun method of assembly that all work on the purification of '49' and on the '49' gun should be dropped." The Mark I gun assembly weapon was then available for use only with slightly a U^{235}-enriched **uranium hydride** active or highly enriched uranium metal active. With either of those active materials the required Mark I projectile velocity and muzzle pressure fell within the range of conventional Navy gun design and operation.]

7.21 The Naval Gun Design Section undertook the practical problems of engineering the proposed design in July 1943. Pressure-travel curves were obtained from the NDRC [National Defense Research Committee] through R. C. Tolman. These were computed by the ballistics group at Section 1 of the Geophysical Laboratory under the supervision of [Joseph O.] **Hirschfelder** who subsequently joined the staff at Site Y and continued to supervise the work of the Interior Ballistics Group. The curves were drawn for maximum breech pressures of 50,000, 75,000, and 100,000 pounds per square inch and submitted to the Bureau of Ordnance, Navy Department.

7.22 As stated above, this was a unique problem involving special steel and its radial expansion [autofrettage], design and breech, primers and mushrooms for extra high pressures, insertion of multiple primers, and many smaller details. The absence of rifling and special recoil mechanism were the only details in which this gun could be considered simpler than standard guns. Nevertheless, the drawings were completed and approved, in a very short time, and the forgings required were ordered in September [1943]. Some delay was occasioned in the preparation of the steel because of difficulty in meeting the physical specifications. The fabrication of guns was done at the Naval Gun Factory, and required about four months at high priority. The first two tubes, and attachments, were actually received at Site Y on March 10, 1944. [For a bibliography on autofrettage, see:

http://users.rcn.com/harwood.ma.ultranet/t19.html]

7.23 The tubes received in March were of two types. Both had adaptor tubes surrounding them in order that the recoil could be absorbed in a standard single Naval gun mount. On the type A gun this adaptor made no contribution to the strength of the tube and was fitted to the gun proper only at the breech. On type B, the adaptor did support the gun tube so that it was much stronger than the bare tube would be. The purpose of type A was to allow tests of the wall strength and deformation in the high alloy gun tube, and the purpose of type B was to make specifically interior ballistic studies.

7.24 While these guns were being procured, intensive effort was put into installations, acquiring personnel and perfecting techniques for testing the guns, and in establishing the necessary channels of procurement of accessories such as propellants, primers, cartridge cases, rigging gear, and the like. The early plan was to install a proving ground, along more or less established lines, with centralized control of all operations on explosives research. The proving work was done by the Proving Ground Group [E-1, Lt. Comdr. Albert Francis Birch, USN, group leader], and the operation, loading, and care of the guns was under the direction of an experienced ordnance man from the Naval Proving Ground at Dahlgren, T. H. Olmstead. Although the plan for a proving ground became impractical for the work on high explosives when the latter work became more elaborate [i.e., Mark IV, spherical implosion design], the gun work was adequately implemented at the original proving ground at Anchor Ranch. The buildings of the Anchor Ranch included the usual gun emplacements, sand butts, and bombproof magazines, control room, and shop. Novel features were incorporated in recognition of the special nature of the proving problem. For one, the fact that it was by no means certain that high alloy tubes would not fragment when overloaded, plus the program for eventually firing the tubes in free recoil, increased the hazards of proving above the ordinary. To cope with this possibility the ground level of the gun emplacements was put above the roof of the bomb proofs, which were installed in a ravine. Also, to protect the guns, targets, etc., from public view, as well as to permit instrumentation on these units in all kinds of weather, the guns were provided with shelters that could be rolled away for the period of actual firing. Construction was started on the proving ground in June 1943 and continued at high priority. It was virtually completed in September. The first shots were fired from emplacement No. 1 on September 17, 1943, at 4:11 p.m. and 4:55 p.m. A second emplacement was completed by the following March in anticipation of receiving the special guns.

[**Note.** The gun of emplacement No. 1 was a 3"/50 Naval anti-aircraft gun equipped with unrifled tubes. If a 50-100 tons TNT equivalent uranium hydride gun fission explosion was made 26 December 1943 at the Alamogordo Bombing Range, that successful demonstration of the prototype Mark I gun assembly design was made with the 3"/50 caliber Navy anti-aircraft gun.]

7.25 The proof firing between September and March [1943-1944] was done chiefly with the 3"/50 Naval A.A. gun equipped with unrifled tubes. The purposes of these rounds were primarily to test the behavior of various propellants, to study elements of projectile and target design on 3 inch scale, and to smooth out instrumentation of the studies generally. The instrumentation was under the direction of K. T. Bainbridge. . . .

7.26 . . . One nonstandard technique that was developed specifically for the interior ballistic problem was the following of the projectile, during its acceleration in the tube, by continuous microwaves. By the time that the type A and B guns arrived, the proving ground routine, the techniques of instrumentation, and the performance of propellants were well established, at least for work at 3 inch scale. In this time interval, the burning of propellants at very high pressure was being studied upon request from Los Alamos at the Explosive Research Laboratory at Bruceton, Pa., thus adding to the preparation for the special gun.

7.27 In February [1944], the direction of Anchor Ranch was assumed by Comdr. F. Birch, with [Edwin] McMillan as Capt. **Parsons'** Deputy for the Gun. In March, the proving work swung over to testing the type B gun for interior ballistic behavior (first round March 17, 1944). By this time, however, the specifications for a lower velocity gun, to be used with U^{235}, became clear. These specifications were considerably less exacting than for the original gun envisioned for this purpose as they called for a muzzle velocity of only 1000 feet per second. Three of these guns were ordered from the Naval Gun factory in March. Some of them would be radially expanded, and a special gun mount had to be designed for them. In spite of this, they presented a much simpler problem to the Bureau of Ordnance, and no anxiety was felt for their operation.

7.28 By reason of the well-prepared experimental background, the testing went smoothly and rapidly. It was found that “WM slotted tube cordite” was the most satisfactory form of propellant at the high pressures involved. Other propellants were tried, but proved inferior. In particular, the 5"/50 Navy powder behaved erratically, as it had done before, and this was traced to worm holing of translucent grains. The Mark XV primers proved to stand over 80,000 pounds per square inch. The propellant performed properly at -50°C. The interior ballistic problem was solved, but the tube was eroded so badly that it had to be returned to the Gun Factory in April. Attention was then given to mechanical strength and deformation of the type A gun. By this time, the proving ground was working at very high efficiency. The installation of a drum camera greatly facilitated record taking, and many measurements of pressures, strains, velocities, and time intervals were made on one round. By early July [1944], the soundness of the design was thoroughly proved, and only by running the maximum breech pressure up to 90,000 pounds per square inch was it finally possible permanently to deform the gun.

7.29 By early July, however, it became clear that the 3000 feet per second gun would never be used. The necessary presence of Pu^{240} in the Hanford plutonium (4.46) decreased the minimum time of assembly of this material far below what was possible by gun-assembly methods.

7.31 Before any work was started on these developments, the plan was complicated by the further uncertainty in the amount of active materials that could be safely disposed in the [gun] projectile alone, or in the [gun] target. This was particularly important in the case of the hypothetical **uranium hydride gun**; for here the critical mass would be small, while for effectiveness a large number of critical masses would have to be assembled. Although planned primarily for the **hydride gun**, the critical mass calculations for odd metal shapes were not at the time accurate enough to rule out a possible need for such methods in the [U^{235}] metal gun model. The development of these mechanisms was a difficult undertaking which remained uppermost in the efforts of the groups concerned until February 1944, by which time the **hydride gun** had been abandoned.

7.39 In addition to the primary development of a high elevation triggering mechanism, some attention was given to underwater detonation. The goal was to detonate 1 minute after impact with the surface. This program hardly got underway, however, before theoretical considerations, based on model tests, predicted that shallow underwater delivery was ineffective. Full attention was then given to the air blast bomb. . . .

7.52 After the April [1943, Los Alamos] conference Neddermeyer visited the Explosives Research Laboratory at Bruceton to become acquainted with experimental techniques as applied to the study of high explosives. Certain types of equipment and installations used at Bruceton were considered desirable for the early **implosion** work, and plans were made for including these at the Anchor Ranch Proving Ground. While at Bruceton, Neddermeyer had his first **implosion** test fired and found encouragement in the result.

7.53 . . . The first **implosion** tests at Los Alamos were made in an arroyo on the mesa just south of the Laboratory on July 4, 1943. These were shots using tamped TNT surrounding **hollow steel cylinders**.

7.54 Interest in the **implosion** remained secondary to that of the gun assembly. There was some consideration of the possibility of using larger amounts of explosive to increase the velocity. But the impossibility of recovery and the currently incomplete instrumentation kept such things in the “idea” stage for several months. The decisive change in this picture of the implosion came with the visit of J. von Neumann in the fall of 1943. Von Neumann had had previous experience with the use of shaped charges for armor penetration. Von Neumann and **Parsons** first advocated a shaped charge assembly, by which active material in the slug following the jet would be converted from a hollow cone shape to a spherical shape having a lower critical mass value. He was soon persuaded, however, that focussing [sic] effects similar to those which are responsible for the high velocity of Monroe jets would operate within an imploding sphere.

7.55 For the development of an adequate HE [high explosive] production plant and research program as well as for general assistance to the research in implosion dynamics, the consulting services of

[George B.] **Kistiakowsky** were required by the Laboratory in the fall of 1943. In February 1944, **Kistiakowsky** joined the staff as Capt. **Parsons'** deputy for implosion. In April he assumed full direction of the rapidly increasing administrative problems of the work.

[**Note.** The documented administrative and evident interpersonal conflicts that developed between Captain Parsons and George **Kistiakowsky** prior to February 1944, as well as before and after **Kistiakowsky**'s April 1944 assumption of full direction of the implosion program, have not yet been satisfactorily detailed nor well appraised in the published Manhattan Project historical literature. Many important documents that would permit the necessary detail to evaluate the difficult interactions of the two men are presently CLASSIFIED. Among those academic-based scientists at Los Alamos, whom General Groves characterized as "prima donnas," **Kistiakowsky** was outstanding. Captain **Parsons**' character, contrarily, is best distinguished by his own aphorism, "There is no limit to the amount of good a man can do if he does not insist that he be acclaimed for his work."]

7.70 On the occasion of [Norman] Ramsey's first visit to Los Alamos in September 1943, **implosion** was just being urged by von Neumann. From this model a preliminary estimate was made of a 9000 pound bomb with a diameter of 59 inches. On the basis of these estimates the Bureau of Standards bomb group was asked, through the Bureau of Ordnance, to have wind-tunnel tests made to determine the proper flaring and stabilizing fins for such a bomb.

7.71 . . . In November 1943 Ramsey and General Groves met with Colonel R. C. Wilson of the Army Air Forces, and plans were discussed for the first modified B-29. In December the first full scale models were ordered through the Detroit Office [George Chadwick], and Ramsey and Capt. **Parsons** visited the Muroc Airbase [Muroc Lake, California; now Edwards Air Force Base] to make the necessary test station plans.

HYDRIDES

8.19 After the formation of the Uranium and Plutonium Metallurgy Group in April 1944 [sic; should be 1943], the work described below was done primarily in that group, and was placed in a separate group in June 1944. The first work in uranium metallurgy at Los Alamos was the preparation and powder metallurgy of its **hydride**. This compound had been successfully produced on the project by [Frank] **Spedding**'s group at Ames, and the existence of the possibility of large scale, controlled production was learned of at Los Alamos in April 1943. The employment of the **hydride in a bomb** was still being seriously considered (4.14). Consequently, metallurgical investigations concerning **uranium hydride** were in order. The early literature identified the compound as UH_4 but primary work in the formation of the **hydride** indicated that UH_3 was closer to the true formula. That this was so was verified independently by the chemists.

8.20 The metallurgical work was modified by bomb requirements with the result that methods of producing **hydride** in high density form and the elimination of the pyrophoric characteristic became important problems. Compacting of the **hydride** by cold pressing and hot pressing methods was attempted as well as the possibility of **hydride** formation under high pressures applied externally to the massive material being treated. This work generally led to the establishment of many control factors in the **hydride** formation process.

8.21 The work on the pressure bomb method of producing high density **hydride compacts** was curtailed when success was achieved with the formation of uranium-plastic compacts. The research on the latter began during February 1944, the objectives being to prepare compacts in desired geometric shapes in which the **hydrogen-to-uranium ratio** varied. This feature could readily be accomplished by the employment of uranium powder and a suitable **hydrogenous binding agent**. It was also possible largely to eliminate the employment of the **hydride** and thus reduce the number of fires. In the early days of this work, a half dozen small fires a week were not unusual. The plastic bonding agents employed, among others, were methacrylate, polyethylene and polystyrene. Compacts were thus made with **uranium-hydrogen compositions** corresponding to UH_3, UH_4, UH_6, UH_{10} and UH_{30} which were used for various experiments by the physicists.

The period August 1944 to August 1945

9.4 Shortly before the general reorganization of the Laboratory [1 August 1944], Oppenheimer outlined a plan to replace the Governing Board by two separate boards. The Governing Board had served as a policy making body attempting to handle general administrative problems and technical policies and serving as a medium for communicating technical developments. By the middle of 1944 it was seriously overburdened. The new plan divided the functions of the Governing Board between an Administrative and a Technical Board. Both of those bodies were advisory to the Director. The members of the Administrative Board appointed in July 1944 included Lt. Col. Ashbridge (Commanding Officer), Bacher, Bethe, Dow, Kennedy, **Kistiakowsky**, Mitchell, **Parsons**, and Shane; those of the Technical Board, Alvarez, Bacher, Bainbridge, Bethe, [James] Chadwick, Fermi, Kennedy, **Kistiakowsky**, McMillan, Neddermeyer, Captain **Parsons**, Rabi, Ramsey, Smith, Teller, and Wilson. . . .

[**Note.** As epitomized by paragraph 9.4, Captain **William S. Parsons’** administrative and scientific eminence at Los Alamos and his confederation with those most universally acclaimed civilian members of the Project Y scientific staff were so prominent that the reader must wonder what pervasive ignorance or prejudice of scholarship has excluded due notice and acclaim of that prominence from, essentially, the entire body of the published Manhattan Project historical literature. The record of Captain **Parsons’** fundamental and essential contributions to the Project and the record of the United States Navy’s fundamental and essential contributions to the Project are amply registered by the most basic of all Manhattan Project historians, David Hawkins, but that record has been slighted by, essentially, every subsequent Manhattan Project historian.]

9.6 The Intermediate Scheduling Conference was an interdivisional committee which began meeting in August 1944 to coordinate the activities, plans and schedules of groups more or less directly concerned with the design and testing of the **implosion** bomb. The committee was formalized in November [1944] with Capt. **Parsons** as chairman, [Comdr. Frederick L.] **Ashworth** (19.3), Bacher, Bainbridge, Brode, Galloway, Henderson, **Kistiakowsky**, Lockridge, and Ramsey as permanent members and Alvarez, Bradbury, Doll, and Warner as alternates . . . Eventually the conference was concerned with both the gun assembly and implosion bombs. The agenda of its meetings included chiefly procurement arrangements for items needed for the final weapons, the test program carried out in cooperation with the Air Forces, and details of the packaging and assembly of the bomb parts for overseas shipment. . . .

9.8 The intricate problems of scheduling the implosion program became the task of the Cowpuncher Committee, composed of Allison, Bacher, **Kistiakowsky**, C. C. Lauritsen [California Institute of Technology], **Parsons**, and Rowe. It was organized “to ride herd on” the implosion program, i.e., to provide over-all executive direction for carrying it out. The committee held its first meeting in early March 1945. This group met often and published semimonthly a report called the Los Alamos Implosion Program which presented in detail the current status of the work. This included the progress of experiments in each group concerned in the program, the scheduling of work in the various shops, and the progress of procurement.

9.10 Among other interdivisional committees was the Weapons Committee, organized in March 1945. It assumed to a large extent the technical responsibilities originally assigned to the Intermediate Scheduling Conference, which became primarily an administrative group. The Weapons Committee was directly responsible to Capt. **Parsons** and was organized with Ramsey as chairman and Warner as executive secretary . . . This committee was asked to assume responsibility for planning all phases of the work peculiar to combat delivery and later became part of Project A (Chapter XIV).

[**Note.** For the history of Project A, see: Harlow W. Russ, *Project Alberta. The Preparation of Atomic Bombs for use in World War II.* Los Alamos Historical Society, Los Alamos, 1984; Exceptional Books, Ltd., Los Alamos, 1990.]

9.12 Early in March 1945 two new organizations were created, with the status of divisions—the Trinity Project, and the Alberta Project—one to be responsible for the test firing of an implosion bomb at Trinity, and the other to be responsible for integrating and directing all activities concerned with the combat delivery of both types of bombs. The Trinity Project was led by Bainbridge with **Penney** and Weisskopf as consultants. Project A was led by Captain **Parsons** with Ramsey and Bradbury as technical deputies. . . .

Damage

11.20 Much more extensive investigation of the behavior and effects of a nuclear explosion were made during this period than had been possible before, tracing the history of the process from the initial expansion of the active material and tamper through the final stages. These investigations included the formation of the shock wave in air, the radiation history of the early stages of the explosion, the formation of the "**ball of fire**," the attenuation of the blast wave in air at greater distances, and the effects of blast and radiations of [sic] human beings and structures.

[**Note.** Compare the text above with the text of the document, **"History of 10,000 ton gadget"** in *The Last Wave from Port Chicago,* Chapters 5 and 6.]

Much of this information was of importance in making plans for the Trinity text. It was essential to know also the probable fate of Plutonium and fission products in the **ball of fire** and the smoke cloud ascending out of it. These calculations, plus calculations of blast and radiation, were essential in planning experiments and observations at Trinity, and in planning for the protection of personnel. Theoretical studies of damage to structures and to personnel were, of course, made in anticipation of combat use. Extensive use in this connection was made of British data on damage to various kinds of structure caused by high explosive bombs. General responsibility for this work was given to Group T-7, with the advice and assistance of W. J. [sic] **Penney**.

[**Note.** William **G**eorge Penney. This particular typographical error in the Hawkins' *History* was carried over to the text of the 1993 DOE Los Alamos history, *Critical Assembly,* on page 344: "By January 1945, **Hirschfelder** and British physicist William J. **Penney** had gathered a great deal of data from Britain on the structural damage caused by German high-explosive bombs. These data proved extremely useful in the group's further calculations, and by the next month it had developed a hypothetical "history" of the explosion of a nuclear weapon with the explosive power of 10,000 tons of TNT."

[That "hypothetical history," composed by Joseph O. **Hirschfelder** and William George **Penney,** is the document reproduced and discussed in

Chapters 5 and 6, the “History of 10,000 ton gadget.” Logically and etymologically, a “hypothetical history” is a contradictory conjunction of terms. A history by definition is a record and analysis of past events; most of the information provided by the “History of 10,000 ton gadget” is predictive of the Trinity test and is not, therefore, history. But Step 10 of the “History of 10,000 ton gadget” does, in one instance, report history, and specifically the history of the **Port Chicago explosion** in precise description of the Port Chicago ball of fire: “. . . **ball of fire** reached 2,000 ft. . . .” The column of flame from the Port Chicago explosion ascended 8,000 to 10,000 feet, but the discrete and typical nuclear explosion **ball of fire** from the **Port Chicago explosion** ascended to 2,000 feet before it disintegrated into a rising column of turbulent convection currents.]

14.1 As a result of the August 1944 reorganization of the Laboratory . . . by the end of September the organization of the Ordnance Division was . . . [7 groups, including] O-6, Water Delivery, Exterior Ballistics, M. M. **Shapiro** [group leader]. . . .

14.20 From the experimental data it was discovered, contrary to expectation, that a surface explosion produced larger gravity wave [in water] than a subsurface explosion of the same size. From a theoretical analysis, scaling laws were derived which made it possible to predict with some assurance the effects of the surface or near-surface detonation of atomic bombs. This program was the work of the Water Delivery and Exterior Ballistic Group [led by Maurice M. **Shapiro**], with the assistance of **Penney** and von Neumann. It had been begun at the end of the previous period [to August 1944] by McMillan.

15.4 The work of the Critical Assemblies Group was carried out at Omega Site, (6.64 ff) where it shared space with the Water Boiler Group. Its main work was to carry out experiments with critical amounts of active materials, including both **hydrides** and metals. It was given the further responsibility of investigating the necessary precautions to be observed in the handing and fabrication of active materials at Los Alamos, to be certain that in these operations no uncontrolled nuclear reactions could occur. When G Division acquired the definite responsibility of designing and preparing the core and tamper—the “pit assembly”—of the Trinity and subsequent implosion bombs, members of the Critical Assemblies Group were given this responsibility.

15.5 During the early period of this group's existence, a large number of critical assemblies were made with various **uranium hydride mixtures**. A relatively large amount of effort was spent in investigating these assemblies for two reasons. The first was that there was not yet enough material for a metal critical assembly without **hydrogen**. The second was that by successively lowering the **hydrogen** content of the material as more U^{235} became available, experience was gained with faster and faster reactions. It was also still not ruled out, at this time, that **hydride bombs** using small amounts of material might be built.

15.6 By November 1944 enough **hydride-plastic cubes of composition UH_{10}** had been accumulated to make a cubical reacting assembly in the beryllia tamper, if the effective composition was reduced to **UH_{80}** by stacking seven polythene cubes for each cube of **UH_{10}** plastic. Further experiments were made with less **hydrogen** and other tampers. In February 1944 [sic; should be 1945] this **hydride** was sent back to the chemists and metallurgists for recovery and conversion to metal, and the program of **hydride critical assemblies** was ended.

15.7 The most spectacular experiments performed with the **hydride** were those in which a slug of **UH_{30}** was dropped through the center of an almost critical assembly of **UH_{30}** so that for a short time the assembly was supercritical for prompt neutrons alone. This experiment was called "tickling the dragon's tail," or simply the "dragon." The velocity of the falling slug was measured electrically. Before the experiment was actually performed a number of tests were made to prove that it was safe, for example that the plastic would not expand under strong neutron irradiation, thus causing the slug to stick and cause an explosion. On January 18, 1945, strong neutron bursts were obtained, of the order of 10^{12} neutrons.

15.8 These experiments gave direct evidence of an explosive chain reaction. They gave an energy production up to twenty million watts, with a temperature rise in the **hydride** up to 2°C per millisecond. The strongest burst obtained produced 10^{15} neutrons. The dragon is of historical importance. It was the first controlled nuclear reaction which was supercritical with prompt neutrons alone.

17.4 The flow of beta stage enriched uranium received from the Y-12 plant was generally as follows: the material was received as a purified fluoride and reduced directly to metal. For **hydride** experiments the metal was converted to **hydride** and formed by plastic bonding. When **hydride** or metal experiments were completed, the material was returned for recovery, as in the meantime were crucibles, liners, and other containers that had been used in fabrication. Recovered solutions were converted hexanitrate, extracted with ether, and precipitated as reduced oxalate. The oxalate was ignited to oxide and converted back to the original tetrafluoride.

19.5 In March 1945, Project Alberta or Project A was established to provide a more effective means of integrating the activities of the various Los Alamos groups working on problems of preparation and delivery of a combat bomb than the Delivery Group by itself had been able to offer . . . Captain **Parsons** was the officer in charge of Project Alberta, with Ramsey and later Bradbury as deputies for scientific and technical matters. The organization included three groups—and administrative group known as Headquarters Staff, a technical policy committee called the Weapons Committee (9.10) and a working group of representatives from other divisions. Comdr. **Ashworth** was operations officer and military alternate for Capt. **Parsons** and served as chief of the Headquarters staff . . . Group representatives [on the Weapons Committee] included [among others] . . . Comdr. **Ashworth** [Tests at Wendover], [Hans] Bethe [General Theory], [William G.] **Penney** [Damage], [Maurice M.] **Shapiro** [Ballistics].

19.7 . . . The emphasis during this period was on supplying the many details necessary for successful operation and correcting faults which became apparent in tests . . . Liaison problems in connection with the development of bombs were of great importance during this period and were handled primarily by Capt. **Parsons** and Comdr. **Ashworth**. Among the military and semimilitary organizations and individuals involved in addition to the United States [Army] Engineers were the 20th Air Force, the Bureau of Ordnance, the Assistant Chief of Naval Operations for Material, Commander Western Sea Frontier, **Commandant 12th Naval District** [San Francisco], **Commandant Navy Yard Mare Island**, Bureau of Yards and Docks Navy Department, NOTS Inyokern [Naval Ordnance Test Station, Inyokern, California; now Naval Weapons Center, China Lake], NAD Yorktown [Naval Ammunition Depot, Yorktown, Virginia; now Naval Weapons Station, Yorktown], and NAD McAlester [Naval Ammunition Depot, McAlester, Oklahoma; now McAlester Army Ammunition Plant]. After Parsons and Ashworth went overseas much of this work was handled by Capt. R. R. Larkin, USN, who arrived at Los Alamos in June [1945].

19.9 Perhaps the most important function of Project Alberta was planning and preparing for overseas operations. As early as December 1944 the initial planning and procurement of some kits of tools and materials had begun, and these activities continued at an accelerated rate through July [1945]. In February Comdr. **Ashworth** was sent to Tinian to make a preliminary survey of the location and select a site for project activities. By March the construction needs for the Tinian Base, known as Destination, were frozen, and construction began in April.

19.10 As early as June 1944, the need had been considered for selecting personnel for field crews required in the final delivery of the bomb and in the later stages of experimentation and testing prior to delivery . . . Actually the personnel for the project teams at Tinian were selected early in May 1945, and were organized as follows:

Officer-in-Charge	Captain **Parsons**
Scientific and Technical Deputy	Norman Ramsey
Operations Officer and Military Alternate	Comdr. **Ashworth**
Team members included . . .[among 36]	Ens. **Reynolds**.

19.15 Since the earliest date previously discussed for combat delivery [of the Mark I] was August 5 (at one time the official date was August 15), **Parsons** and Ramsey cabled Gen. Groves for permission to drop the first active unit as early as August 1. [For the 6 August 1945 Hiroshima combat mission with the Mark I] Col. P. W. Tibbets was pilot of the Enola Gay, the B-29 which carried the bomb. Maj. Thomas Ferebee was the bombardier, Capt. **Parsons** was bomb commander, and Lt. Morris Jepson was electronics test officer for the bomb.

19.16 Only a few days before the scheduled drop it was decided by the technical group that it was not safe to take off with the bomb completely assembled, since a crash might mean tremendous destruction to men and materials on Tinian. Full safing could not be secured, but it was finally agree that a partial safeguard would come if the cartridge which contained the propellant charge were inserted through the opening in the breech block during flight rather than on the ground. This scheme had been considered before (14.14) but was not finally adopted until this time. Capt. Parsons, who was already assigned to the crew as weaponeer, was given the job. This decision meant that Capt. **Parsons** had to be trained in a short time to perform the operation, and also that the bomb bay of the B-29 had to be modified to provide him with a convenient place to stand while completing the assembly. These things were done and the bomb was not completely assembled until the plane was safely in flight. [For extensive elaboration see, Harlow W. Russ, *op. cit.*]

19.17 The progress of the mission is described in the log which Capt. **Parsons** kept during the flight:

6 August 1945

0245	Take Off
0300	Started final loading of gun
0315	Finished loading
0605	Headed for Empire from Iwo
0730	Red plugs in (these plugs armed the bomb so it would detonate if released
0741	Started climb Weather report received that weather over primary and tertiary targets was good but not over secondary target
0838	Leveled off at 32,700 feet
0847	All Archies (electronic fuses) test to be OK
0904	Course west
0909	Target (Hiroshima) in sight
0915-1/2	Dropped bomb (Originally scheduled time was 0915) Flash followed by two slaps on plane. Huge cloud.

1000	Still in sight of cloud which must be over 40,000 feet high
1003	Fighter reported
1041	Lost sight of cloud 363 miles from Hiroshima with the aircraft being 26,000 feet high

The crews of the strike and observation aircraft reported that, five minutes after release, a low 3 miles diameter dark grey cloud hung over the center of Hiroshima, out of the center of this a white column of smoke rose to a height of 35,000 feet with the top of the cloud being considerably enlarged. Four hours after the strike, photo-reconnaissance planes found that most of the city of Hiroshima was still obscured by the cloud created by the explosion, although fires could be seen around the edges. Pictures were obtained the following day and showed 60 per cent of the city destroyed.

19.19 The first Fat Man bomb [Mark IV] was scheduled for dropping on August 11 (at one time the schedule called for August 20, but by August 7 it was apparent that the schedule could be advanced to August 10. When **Parsons** and Ramsey proposed this change to Tibbets he expressed regret that the schedule could not be advanced two days instead of only one, since good weather was forecast for August 9 and bad weather for the five succeeding days. It was finally agreed that Project Alberta would try to be ready for August 9, provided it was understood by all concerned that the advancement of the date by two full days introduced a large measure of uncertainty. All went well with the assembly, however, and the unit was loaded and fully checked late in the evening of August 8. The strike plane and two observing planes took off shortly before dawn on August 9. Maj. C. W. Sweeney was pilot of the strike ship Great Artiste, Capt. K. K. Beahan was bombardier, Comdr. **Ashworth** was bomb commander, and Lt. Philip Barnes was electronics test officer.

19.22 On the day following the Nagasaki mission, the Japanese initiated surrender negotiations and further activity in preparing active [atomic bomb] units was suspended. The entire project was maintained in a state of complete readiness for further assemblies in the event of a failure in the peace negotiations. It was planned to return all Project

Alberta technical personnel to the United States on August 20, except for those assigned to the [General Thomas F.] Farrell mission for investigating the results of the bombing in Japan. Because of the delays in the surrender procedures, Gen. Groves requested all key personnel to remain at Tinian until the success of the occupation of Japan was assured. The scientific and technical personnel finally received authorization and left Tinian on September 7, except for Col. Kirkpatrick and Comdr. **Ashworth** who remained to make final disposition of project property.

Appendix

C

Development of the Mark II, a brief chronology

1929—Ernest Orlando Lawrence invented the cyclotron that, in development, would contribute to World War II separation (enrichment) of the U^{235} isotope necessary to Mark II active material; Nobel Prize in Physics, 1939.

1931—Harold Clayton Urey (Thanksgiving Day) discovered the deuterium isotope of hydrogen that would provide the deuterium component of the Mark II active; Nobel Prize in Chemistry, 1934.

1932—James Chadwick discovered the neutron which was essential to achieve artificial nuclear fission in the Mark II uranium deuterium active; Nobel Prize in Physics, 1935.

1939—J. Robert Oppenheimer (5 February) first proposed the uranium deuterium nuclear fission bomb concept subsequently developed as the Mark II.

1942—Edward Teller first proposed use of the B^{10} isotope to achieve an autocatalytic assembly of the uranium deuterium nuclear fission bomb concept proposed by Oppenheimer.

1942—Harold Urey and associates develop industrial scale processes to produce B^{10} and deuterium that would provide the B^{10} and deuterium components of the Mark II active.

1943—April:

Frank Spedding and his group at the University of Iowa, Ames, devised an industrial scale process to produce natural uranium metal and successfully produced the first uranium hydride compound that would provide the material of the Mark II active;

Cyril Smith begins uranium hydride metallurgy at Los Alamos;

Robert Serber delivers Los Alamos "Indoctrination Course" lectures, which describe the "boron bubble" concept of autocatalytic bomb assembly that would be developed as the Mark II autocatalytic uranium hydride lateral implosion experimental device.

4 July: Seth Neddermeyer, with **Captain William S. Parsons, USN** present, conducted the first experimental implosion of a cylinder at Los Alamos, which would be developed as the autocatalytic uranium hydride lateral implosion design of the Mark II.

14-24 August: British-American Quebec Conference, Quebec City, Canada.

21 August: In report to Vice President Wallace, Secretary of War Stimson and Chief of Staff General Marshall the Atomic Bomb Military Policy Committee accurately forecast the fair chance that the first atomic bomb, the (uranium) hydride bomb, would be available in the fall of 1944.

21-24 August: at the Quebec Conference, British and Canadian members of the Combined Policy Committee were informed of the fair chance that the first atomic bomb, the (uranium) hydride bomb, would be available in the fall of 1944. Prime Minister Winston Churchill was certainly also at that time informed of the fair chance that the first atomic bomb, the (uranium) hydride bomb, would be available in the fall of 1944.

15 September: James Conant requests Rear Admiral William R. Purnell of the Atomic Bomb Military Policy Committee to complete the transfer of 236 pounds (107 kg) enriched uranium hexafluoride from Philip H. Abelson's Naval Research Laboratory liquid thermal diffusion uranium isotope facility to the Manhattan Project, via General Leslie R. Groves, Military Policy Committee executive officer.

1944—4 July: James Conant informed General Groves, Atomic Bomb Military Policy Committee, and the Top Policy Committee by the memorandum, "Findings of Trip to L. A. [Los Alamos] July 4, 1944," that the Mark II was certain enough to be used by the Joint Chiefs of Staff for the purposes of operational planning, but the Mark II would necessarily be proof fired once before the design could be ready for use against the enemy.

10 July: at 11:00 A.M., General Groves' office log reports, "Gen. Groves held a telephone conversation with Dr. Oppenheimer at Los Alamos. Gen. Groves to talk to JBC [James Bryant Conant] and RCT [Richard Chace Tolman] re: 3 horsemen's visit."

12 July: at 10:45 A.M., the General's office log reports, "Gen. Groves called Dr. Oppenheimer, Santa Fe, N.M. re: visit of 3 horsemen to Y [Los Alamos]. To arrive July 31st and department [sic] August 3rd. Gen. Groves to send written invitations to all three."

13 July: at 12:20 P.M., the General's office log reports, "Gen. Groves called E. O. Lawrence, Knoxville, Tenn. re: plans to be at Y to arrive on July 31st and to depart August 3rd in the [a].m."

13 July: at 12:40 P.M., "Gen Groves called Dr. Urey, Wabash, Indiana, to invite him to be present at Y on July 31st to stay until morning of August 3rd."

17 July: afternoon at the University of Chicago, Conant in conversation with J. Robert Oppenheimer advocates that a test of Mark II be conducted as soon as possible which, if successful, would demonstrate the feasibility of nuclear fission weapons. Mark II could be put on the shelf, and work on the more powerful bombs could proceed with less nervousness.

17 July: at 10:30 P.M., Port Chicago explosion; Mark II successfully proof fired.

17 July: President Franklin D. Roosevelt was en route by train from Hyde Park, New York to San Diego, California. The *Presidential Special* at the time of the Port Chicago explosion was passing through New Mexico in the Santa Fe-Lamy-Los Alamos-Albuquerque area.

President Roosevelt certainly had been aware from 21 August 1943 that the uranium hydride Mark II was in development. In this author's opinion, the President would necessarily have provided the initial military and civilian authorization to conduct a proof detonation of the Mark II in circumstances determined by the Joint Chiefs of Staff and the Atomic Bomb Military Policy Committee best to assess the military potential of the Mark II as well as the military potential of large scale nuclear fission weapons, and that the President took upon his own responsibility the consequences to persons and property that would result inevitably from that proof when the Port Chicago Naval Magazine was named as the location that proof would be conducted.

Materials available in the Franklin D. Roosevelt Presidential Library, Hyde Park, New York, record that President Roosevelt sent a telegraph message from the *Presidential Special* to Generalissimo Josef Stalin after midnight 17 July 1944—the early morning of 18 July. If indeed President Roosevelt did send a telegraph to Generalissimo Stalin the early morning of 18 July that telegram would have passed through Washington, D.C., for retransmission to the USSR. I have not found any other reference to such a telegram from the President to the Generalissimo on that date, nor have I found any reference to a telegram received aboard the *Presidential Special* by which the

President would have been informed that the Mark II had been successfully proof fired the evening of 17 July 1944.

In Germany at the Potsdam Conference on 24 July 1945, following the successful 16 July 1945 Trinity Site test of the Mark IV gadget in New Mexico, President Truman informed Premier Stalin that the U.S. possessed "a new weapon of unusual destructive force." Many books have been written that ponder Stalin's stolid response to President Truman's information about the U.S. atomic bomb. Stalin's reported response was that he showed no special interest, but he is reported to have told President Truman he was glad to hear it and hoped we would make good use of it against the Japanese.

Did Stalin know prior to 24 July 1945 that the U.S. had successfully proof fired an atomic bomb? The published opinions of those men present at Potsdam 24 July 1945 who were aware of President Truman's acknowledgment that the U.S. possessed "a new weapon of unusual destructive force" are available in summary at:

http://www.dannen.com/decision/potsdam.html

Unknown to everyone at the Potsdam Conference, except probably Premier Stalin, was the 16 March 1945 information analysis provided to NKGB chief Lavrenti Beria by Igor Kurchatov which suggested that the U.S. uranium hydride bomb had been tested prior to 16 March. The NKGB was certainly aware by 16 March 1945 that the U.S. was nearing a nuclear fission bomb capability, and that information soon thereafter would have been made available to Premier Stalin.

1944—18 July: nationwide morning radio news reports of the Port Chicago explosion and front page reports were published in all the metropolitan newspapers including the *Chicago Tribune*, the *New York Times*, the *Washington Post*, and the *San Francisco Examiner*. Radio and newspaper reports were available to James Conant and Gen. Groves, both in Chicago Illinois, Harold Urey at Wabash, Indiana, and Ernest Lawrence at

Knoxville, Tennessee. None of the three Nobel Laureates ever apparently mentioned the Port Chicago explosion.

18 July: President Roosevelt visited day-long at the San Diego Naval Base prior to departure for Hawaii; the evening of 18 July the President radio-broadcast acceptance of his nomination by the Democratic Party to a fourth presidential term. In that 18 July speech broadcast from the San Diego Naval Base the President did not mention the Port Chicago Naval Magazine disaster, nor did he apparently at any later date.

20 July: at 10:35 A.M., the General’s office log reports, “Dr. Chadwick called JO’L [Jean O’Leary, General Groves’ secretary] re: would like a priority 3 to travel by Flight 6:15 p.m. Friday [28 July] TWA [Trans World Airlines] from Wash. to Y.”

20 July: at about noon Captain Parsons and Los Alamos scientists Maurice M. Shapiro and Ensign George T. Reynolds arrived at the Mare Island Navy Yard and, with Captain Parsons’ brother-in-law Captain Jack S. Crenshaw, proceeded to Port Chicago.

22 July: at 3:00 P.M., the General’s office log reports, “Gen. Groves talked to Mr. Oppenheimer in Santa Fe. Gen. Groves told O. that Greenewalt [Crawford Greenewalt, Dupont Chemical Co.] wanted him to come to W. [Wilmington, Delaware] and O. said he would arrange it after the visit of the 3 horsemen.”

24 July: the first Los Alamos analysis of the Port Chicago explosion is completed by Captain Parsons at Los Alamos for transmittal to Atomic Bomb Military Policy Committee member Admiral Purnell, “Port Chicago Disaster: Preliminary Data.”

26 July: 5:45 P.M., General Groves departed Washington airport for Los Alamos.

27 July: completion of Ensign Reynolds' blast damage assessment, "Report on Port Chicago July 20-24. 1944."

27 July: morning, General Groves arrived at Los Alamos from Washington.

28 July: morning, General Groves departed Los Alamos, returned to Washington and entered a meeting of the Military Policy Committee at 4:45 P.M.

29 July: John Burchard's "Damage Survey at Port Chicago, California," transmitted to U.S. Navy Coordinator of Research and Development Rear Admiral Julius A. Furer via National Defense Research Committee Chairman Vannevar Bush; blind copy to Los Alamos reviewed by Oppenheimer.

29 July: Nobel Laureate James Chadwick arrived at Los Alamos.

31 July: the Port Chicago Naval Magazine War Diary for July 1944 reports, "Among the officers and technicians not assigned to duty in the Twelfth Naval District who visited Port Chicago immediately after the disaster were:

> Capt. J. C. Byrnes, Jr., of the Bureau of Ordnance
>
> Capt. Radfor [?] Moses, of the Bureau of Ordnance
>
> Comdr. M.G. Johnson, Bureau of Ordnance
>
> Comdr. J. H. Sides, Bureau of Ordnance
>
> Lt. Comdr. Dexter Bullard, Bureau of Ordnance
>
> Capt. W.S. Parsons, from the Office of Chief of Naval Operations
>
> Col. Crosby Field, of the Joint Army-Navy Ammunition Storage Board

> Lt. Col. Ruel Stratton, of the Joint Army-Navy Ammunition Storage Board
>
> Professor John F. Burchard, Chairman DOLOC Committee, Office of Scientific Research and Development
>
> D. Max Beard, of the Naval Ordnance Laboratory, Navy Yard, Washington, D.C.
>
> E. Moss Brown, Naval Ordnance Laboratory, Navy Yard, Washington, D.C.”

31 July: Nobel Laureates Ernest and Harold Urey arrived at Los Alamos.

2 August: General Groves present at Los Alamos; date of arrival unknown.

3 August: morning, General Groves, Nobel Laureates Chadwick, Lawrence and Urey depart Los Alamos.

3 August: Oppenheimer letter in response to James Conant’s letter of 27 July, addressed to 1530 F Street, N. W., Washington, D.C.: “We are looking forward to your visit on the seventeenth and will plan to meet you at the Chief at Lamy . . . We have had the first positive indications as far as our main program [the atomic bombs] goes, and although the results have not been checked, they do lend some encouragement. By the time you are out we should know pretty well how sound they are.”

4 August: Captain Parsons’ memorandum to Admiral Purnell, “Port Chicago Disaster: Second Preliminary Report.” Enclosures: (A) “Marked copy of layout of U. S. Naval Magazine, Port Chicago, California”; (B) “Notes on Enclosure (A)”; (C) “Copy of Ensign Reynolds, USNR, on Blast Damage”; (D) “Copy of Report of Dr. M. M. Shapiro on Observations on the Effects of the Tidal Wave, Port Chicago Explosion”; (E) “Copy of Report of Ensign Reynolds, USNR, on Seismic Evidence”;

(F) “Prints of Mark Island Navy Yard Photographs Nos. . . . [38 photos in total].”

17 August: “Report to Gen. Groves on Visit to Los Alamos on August 17, 1944.” In specific consequence of the 17 July 1944 Port Chicago Naval Magazine explosion, in which the Mark II was proof fired, James Conant informed General Groves by memorandum that the Mark II could be developed for combat use in 3 or 4 months time and the energy yield of the Mark II could be somewhat improved. The Atomic Bomb Military Policy Committee’s 21 August 1943 forecast that the first (uranium) hydride atomic bomb could be produced in the fall of 1944 was fulfilled.

31 August: Captain Parsons’ memorandum to Admiral Purnell, “Port Chicago Disaster: Third Preliminary Report.” Enclosures: (A) “Marked copy of Map of U.S. Naval Magazine, Port Chicago C-3075-1, dated 30 June 1944”; (B) “Copy of Map of U.S. Naval Magazine, Port Chicago C-3075-1, dated 30 June, 1944”; (C) “Photograph of print showing distribution of explosive cargo in S.S. E.A. Bryan”; (D) Report of Ensign G. T. Reynolds, USNR, consisting of ‘Analysis of damage due to air blast and earth shock’ ”; (E) “Report of Dr. M. M. Shapiro on Effects of the tidal Wave, with discussion and calculations”; (F) “Key to Plate Numbers”; (G) “Photographs constituting plate numbers . . . [26 photographs in total].”

16 November: Captain Parsons’ memorandum to Admiral Purnell, “Port Chicago Disaster: Final Report.” Enclosures: (A) “Discussion of Damage to Marginal Pier and Analysis of Fragment Distribution, by Ensign George T. Reynolds, USNR”; (B) “Analysis of Crater in Bottom near Ship Pier, by Dr. Maurice M. Shapiro”; (C) Prints showing Damage to Marginal Pier.”

1953—The uranium hydride bomb was twice experimentally detonated at the Nevada Proving Ground in 1953. Shot Ruth (Hydride I; 31 March) and shot Ray (Hydride II; 11 April) of the Upshot-Knothole series were designed and proof fired under the direction of Edward Teller and Ernest O. Lawrence of the University of California Radiation Laboratory at Livermore. Ruth and Ray were essentially replications of the uranium hydride Mark II proof fired at the Port Chicago Naval Magazine, 17 July 1944. Review of the reported ionizing radiation consequences of shots Ruth and Ray provides sufficient data to conclude that no person suffered ionizing radiation consequences from the proof detonation of the Mark II at the Port Chicago Naval Magazine.

1992—28 October: Public Law 102-562, Title II, 102d Congress, signed by President George Bush, established the Port Chicago Naval Magazine National Memorial; said to have been President Bush's last official act as President of the United States.

1994—17 July: Port Chicago Naval Magazine National Memorial dedicated by the National Park Service on the Suisun Bay shoreline of the Concord Naval Weapons Station.

2002—12 March: House Resolution 3941 of the 107th Congress, Second Session, the "Port Chicago Naval Magazine National Memorial Study Act," has directed the Secretary of the Interior (Gale Norton) in consultation with the Secretary of the Navy (The Honorable Gordon R. England) to conduct a special resource study to determine the suitability and feasibility of including the Port Chicago Naval Magazine National Memorial as a unit of the National Park System. For current information on the bill, please see Congressman George Miller's "The Port Chicago Explosion" Web page at:

http://georgemiller.house.gov/ptchicmain.html

The Port Chicago Naval Magazine National Memorial lies within the Concord Naval Weapons Station, which is presently leased to the United States Army. Should the Port Chicago Naval Magazine National Memorial be established by the Congress as a unit of the National Park System the entire 12,000 acres of the essentially deactivated Concord Naval Weapons Station should be reserved by the Congress for eventual transfer to the National Park System, to be included in the Port Chicago Naval Magazine National Memorial; 200 acres are presently discussed. The Concord Naval Weapons Station is the last large pristine undeveloped San Francisco Bay shore property and should be so preserved in perpetuity as a unit of the National Park System.