Weighing the Earth from a Submarine: The Gravity Measuring Cruise of the U.S.S. S-21

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A central topic in current history of science is the emergence of research traditions. Andrew Carnegie believed in the historical driving force of the "exceptional man," but contemporary historiography sees the solitary scientific genius as a rare and poetic exception. Most scientific work is done in communities of men and women, researchers and technicians, leaders and followers, who cluster around particular forms of scientific practice [Latour and Woolgar, 1979; Rudwick, 1985; Latour, 1987; Pickering, 1992]. Research groups may amalgamate around a common methodological approach, such as an observational technique, a form of experimentation, or a specific piece of instrumentation. Or the unifying agent may be a research focus: a disease, an animal, a geographic locale.

Several recent historical studies have discussed the emergence and evolution of research traditions in American science [Galison, 1985; Warner, 1986; Pauly 1987; Galison and Assmus, 1989; Servos, 1990; Maienschein, 1991]. Maienschein [1991] has emphasized that new research traditions are commonly formulated by the gradual evolution and subtle transformation of an existing tradition. Such a "birth" of a new tradition would be impossible to "date." However, new research traditions may also emerge that are linked to identifiable historical events, while a root tradition lives on as a complementary or competing approach. Marine geophysics in America is an example of the latter. With roots in continental geodesy, marine geophysics emerged from an existing institutional and theoretical framework in the early 20th century. By mid-century, it had become a widely recognized, independent research tradition, in the process of developing its own institutional and theoretical framework. In the 1960s, the data produced from this tradition had a transforming impact on all of the Earth sciences. How did this new research tradition emerge? The purpose of this paper is to answer this question by examining the first American attempt to obtain marine geophysical data: the gravity-measuring cruise of the U.S.S. S-21 submarine, a joint initiative between the Carnegie Institution of Washington (CIW), the U.S. Coast and Geodetic Survey (USCGS), and the U.S. Navy. The S-21 expedition marks the birth of marine geophysics in America. Its history illustrates a confluence between theory and utility that characterized much of American Earth science in the late 19th and early 20th century.

The Earth, the Heavens and the Carnegie Institution of Washington
History of Geophysics Volume 5

THE S-21 SUBMARINE EXPEDITION

In September 1928, the Dutch geodesist F. A. Vening Meinesz (1887-1966) arrived in the U.S. to measure gravity aboard a U.S. Naval Submarine. The goal of the S-21 submarine expedition was to measure the acceleration of gravity in the Gulf of Mexico and the Caribbean, or, as William Bowie (1872-1940), Chief of the Geodesy Division of the USCGS, put it, to "weigh the Earth from a submarine" [Bowie, 1929d]. Vening Meinesz arrived in Washington on September 25, calibrated his apparatus at USCGS headquarters, and installed the device on board the submarine. On October 2, the U.S.S. S-21 sailed from the Naval Yard in Hampton Roads, Virginia, accompanied by two Eagle boats in case of emergency, and headed for the Gulf of Mexico (Figure 1). To assist with the scientific work and learn the technique of gravity measurement at sea, two American scientists accompanied Veining-Meinesz on board: Fred E. Wright, a petrologist at the Geophysical Laboratory (GL) of the CIW, and Elmer B. Collins, principal scientist of the Naval Hydrographic Office. The outward trip took the scientists down the Atlantic Coast to Key West and across the Sigsbee Deep to Galveston, Texas. At Galveston, they turned around, and returned across the Mississippi Delta, along the coast of Cuba, across the Bartlett and Nares Deeps, and into Guantanamo Bay (Figure 2). The final leg brought them home on November 27, 1928 [Lamson, 1930; Wright, 1929; Bowie, 1930; US-NA RG24 18W4: Logbook of the USS S-21, p. 605-613; CIW GL Misc File 1908-1935, Curtis Wilbur to John Merriam, June 19, 1928].

The cruise was a tremendous success. The submarine covered a distance of 7000 miles in just under two months, measuring gravity at forty-five stations at sea. Five

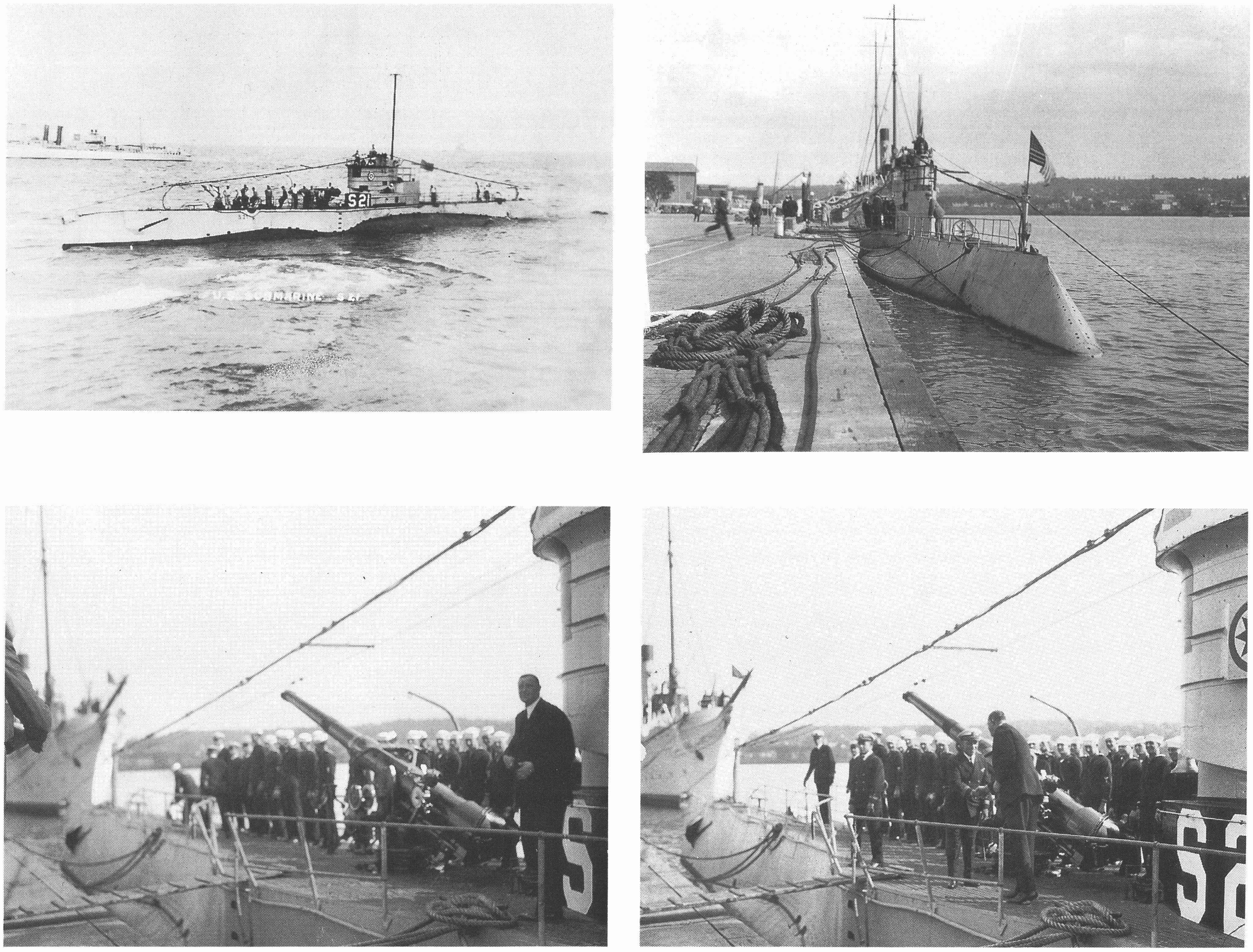

Fig. 1. Photos from the S-21 expedition. Upper left: The U.S.S. S-21 at sea. Upper right: The S-21 docked at Hampton Roads, prior to leaving on the gravity-measuring cruise. Bottom photos: Vening Meinesz on board the S-21, greeting officials of the U.S. Navy. Navy Secretary Curtis Wilbur and several other officers attended the send-off. The man shaking hands with Vening Meinesz may be the ship's captain, Lt. J. L. Fisher. Pictures taken by F. E. Wright.

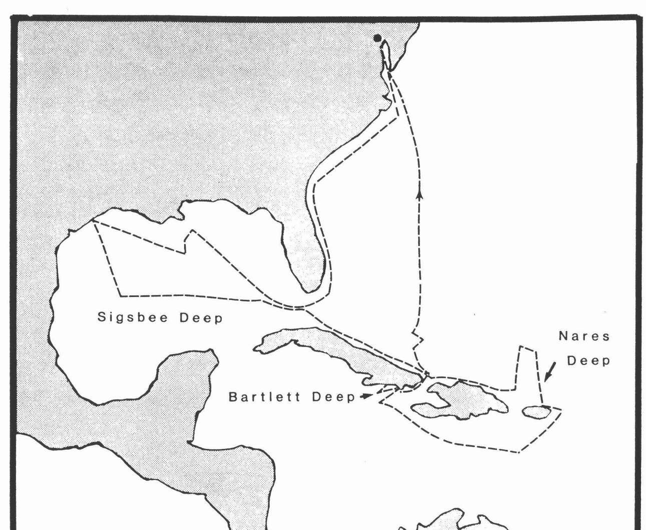

Fig. 2. The route of the S-21 expedition. After Vening Meinesz and Wright, 1930.

additional measurements were made in harbor at Hampton Roads, Key West, Galveston, Guantanamo, and St. Thomas. Of these, only Galveston had previously been the site of gravity measurements, for a total of 49 new gravity stations [Vening Meinesz, 1929]. At each station, depths were recorded at five-minute intervals using a sonic depth finder to allow for accurate topographic corrections. Additional soundings throughout the cruise assisted in topographic and geological interpretation of the results. In calm seas, measuring stations were located at periscope depth [42-45 feet]. In rough water, the submarine submerged to depths of 60-90 feet to minimize the effects of surface waves. Throughout the cruise, measurements were taken of longitude and latitude for accurate positioning, temperature, humidity, and barometric pressure for corrections to the pendulum, and the direction and velocity of the ocean current to correct for rotational velocity in computing centrifugal acceleration. The typical dive time was 35-60 minutes, of which about half an hour was spent in measurements, but in one case, measurement was completed in only 18 minutes on a total dive of 22 minutes.

The cruise was considered a success in part because of the tremendous efficiency of the operation. Efficiency had long been a preoccupation of American geodesists for both practical and intellectual reasons. In the nineteenth century, geodetic measurement in the young country had been a notoriously slow and difficult process. Hours or days were spent moving equipment by hand or horse; men were frequently injured climbing rocks and beating back brush; insects, sickness, and exhaustion were frequent companions; and gravity pendula were big, heavy, and required a solid base [Manning, 1988]. In forested or uneven terrain, survey teams had to build wooden towers to obtain levelling data; in the early 20th century it had been suggested that the USCGS should abandon triangulation in such cases and resort to simple traverses. Bowie refused such compromises with their resulting loss of accuracy and precision, and instead promoted the use of trucks (instead of horses) and the development of the 'Bilby' portable steel levelling tower [Bowie, 1932]. Nevertheless, in the 1920s it still commonly took more than a week to complete a single gravity station [Fleming and Piggot, 1956, p. 328]. In his annual reports, Bowie described the increasing pressure to improve the Survey's productivity to satisfy growing demand for geodetic data [NA-US RG23 USCGS, Series 33, Annual Office Reports]. The prospect of a radically more efficient operation was thus enticing both for reasons of personal safety and in response to the financial pressures endemic in this federal agency.

Productivity pressed the Coast Survey for intellectual reasons as well, because a large quantity of data was essential for an accurate model of the figure of the Earth. In geodesy, more data meant better science. Towards this end, Bowie's predecessor, John Hayford (1868-1925), had pioneered the application of the theory of isostasy in the interpretation of geodetic data. Hayford's innovation was to use the concept of Pratt isostasy, with its simplifying assumption of a uniform depth of isostatic compensation, to streamline the laborious calculations required to reduce raw field data. Hayford's strategy dramatically increased the efficiency of calculation, and resulted in a new model for the figure of the Earth: the "Hayford spheroid," adopted in 1924 as the standard of the International Union of Geodesy and Geophysics [Hayford, 1909; Hayford and Bowie, 1912; Bowie 1922; Burger, 1931]. The S-21 expedition was considered a fulfillment of Hayford's legacy, measured in part by the *quantity* of data produced: about 1/6 as many gravity measurements as had *ever* been collected in the terrestrial U.S. [Wright, 1929].

ORIGINS OF THE S-21 EXPEDITION

The Hayford model for the figure of the Earth was considered a foremost accomplishment of American Earth science. In a discussion of a paper by Bowie presented at the Royal Geographical Society in 1923, British geophysicist Harold Jeffreys called the Coast Survey work "one of the outstanding scientific achievements of our time" [Bowie, 1924, p. 44; also see Burger, 1931]. Colonel Sir Sidney Burrand, retired Superintendent of the Trigonometrical Survey of India, recalled that "When Hayford's method had been introduced into India we realized that it would mark an epoch in the history of geodesy" [Bowie, 1924; p. 36]. Burrand said he had first heard of Hayford's work from the great German geodesist Friedrich Helmert (1843-1917), "the foremost geodesist of the age," and recalled Helmert's realization, "with sadness, that Hayford had hit on the solution which he had been vainly seeking" [Bowie, 1924, p. 36].

Hayford's innovation was two-fold. First, using the Pratt assumption of a uniform depth of isostatic compensation, he streamlined the calculations of the deflection of the vertical used in trigonometrical surveys, vastly increasing the rate at which these calculations could be done [Hayford, 1909; also see Bowie, 1922; 1924; 1927; and 1929e]. Then, working with Bowie, he developed a new method of calculating isostatic anomalies based on variations in the acceleration of gravity [Hayford and Bowie, 1912]. This work suggested that the major features of the Earth's crust were isostatically compensated. It appeared that the theory of isostasy held true, and therefore gravity measurements could proxy for traditional geodetic data [see Bowie, US-NA RG 23 USCGS, Entry 30, General

Correspondence, "Terrestrial Magnetism" 1908-1914].

As suggested above, Hayford was primarily motivated by issues of efficiency. A civil engineer by training, his choice of the Pratt model was not based on geological considerations, but on facility of calculation. In his words, "The assumption [of Pratt isostasy] was adopted as a working hypothesis, because it happens to be that one of the reasonable assumptions which lends itself most readily to computation" [Hayford, 1909, p. 147; also see Vening Meinesz and Wright, 1930, p.11 and Reingold, 1970, p. 188-9]. Bowie's contribution was to use this computational strategy as the basis of a theoretical interpretation of the Earth's structure and origin. Bowie argued that if all the major physiographic features of the Earth were isostatically compensated then they must be very old. The Earth must be essentially stable. In his words, "If the earth's crust is in isostatic equilibrium . . . then we are justified in assuming that the isostatic condition has obtained since earliest geological times" [Bowie, 1924]. For this reason, Bowie became an adherent of the Darwin-Fisher hypothesis of fissiparturition, which placed the origin of the continents and oceans during break-up of a proto-crust when the moon separated from the Earth early in planetary history [Bowie, 1929a; Bowie, 1935b; Yale University Archives, Charles Schuchert Papers: Bowie to Schuchert June 17, 1927, Box 19 Folder 166; and October 11, 1928, Box 21 Folder 181]. As a corollary, it followed that few if any major stresses were being sustained in the crust at present. This was one reason why Bowie rejected the idea of continental drift: there simply weren't sufficient stresses to move continents.

By the mid 1920s, the practical success of the Pratt-Hayford model and Bowie's promotion of its geological results had increased acceptance of the underlying geological theory: most American geologists accepted isostasy theory as a general statement of crustal dynamics. In 1925, William Bowie declared in *The New York Times* that isostasy theory had been "proved" [Bowie, 1925]. Shortly, he claimed this in more scholarly journals as well [Bowie 1927; Bowie, 1929a; Bowie 1929c]. However, despite his optimistic public pronouncements, Bowie was well aware of a major lacuna in the empirical data base: a complete absence of data from the oceans basins. Furthermore, the available land-based data were almost entirely concentrated in Europe, Asia, and North America. This raised questions both about the geodetic models produced and any geological interpretations placed upon them. Thus there were two motivations to go to sea. One was to continue to improve estimates for the figure of the Earth. To sustain the Hayford spheroid, or replace it with something better, more geographically widespread data from hitherto unexplored regions were needed. Second, to apply isostasy theory broadly applied to problems of crustal dynamics, data from the entire Earth, not just the continents, were essential. Did isostatic equilibrium obtain over the oceans basins? To answer this question, one needed to measure gravity at sea. As Fred Wright put it, "The late Dr. Hayford told me years ago that if a gravity apparatus for use at sea were available, a single ship could, in the course of a year, contribute more important data bearing on the figure of the Earth and the theory of isostasy than have been collected in the last generation on land." [CIW-GL Misc File 1908-1938 #2: Wright to Merriam, December 27, 1928].

The obstacle to Hayford's ambition was technical. Gravity measurements were based on the well-known relation between the acceleration of gravity and the period of a pendulum, $g = 4 \pi^2 L / T^2$, where T is the period of the pendulum and L is the length. If the bulk Earth were a regular spheroid with topography superimposed on it, then one could predict the value of gravity at any location based on its elevation. Differences between measured and predicted values would reflect either divergences from the calculated figure of the Earth or uncompensated isostatic anomalies [Bowie, US-NA RG23 USCGS Entry 30, General Correspondence, "Terrestrial Magnetism," 1908-1914]. However, this conclusion presupposed no external disturbances. The only forces acting upon the pendulum should be the acceleration of gravity and the centrifugal acceleration caused by the Earth's rotation. Random accelerations encountered on board a ship would render measurement impossible. Attempts to measure gravity at sea had failed; the isostatic condition of the oceans remained unknown [Vening Meinesz and Wright, 1930; Laudan, 1980]. However, in the early 1920s, the situation changed.

Vening Meinesz was a civil engineer by training, employed by the Geodetic Commission of Holland. While American geodesists labored to overcome black flies and the Ozark Mountains, Vening Meinesz and his Dutch colleagues struggled with ground vibrations induced by storms and waves in coastal Holland. In the early 1920s, Vening Meinesz designed a gravimeter that would work in unstable conditions. Two pendula of nearly the same vibrational period swung in the same vertical plane, and the difference in their angles of elongation was photographically recorded. If the two pendula are equally affected by extraneous horizontal acceleration, then the difference between them is due to the acceleration of gravity at that location. That is, the difference between the two pendula is equivalent to a single "virtual" pendulum free of horizontal disturbance. In application, the device actually used three pendula in the vertical plane, with the outer two set in motion to create two pairs of pendula swinging in opposite phase. In addition, Vening Meinesz's gravimeter contained three "dummy" pendula to record temperature and humidity inside the apparatus, and to record the motion of the entire device. The Meinesz apparatus overcame the effect of horizontal acceleration that otherwise disturbed the motion of a single pendulum at sea [Wright, 1929; Vening Meinesz and Wright, 1930; Laudan, 1980].

In 1923, Vening Meinesz tested his device on the Netherlands K-II submarine expedition to Indonesia. A second cruise to Indonesia in the opposite direction—the K XIII, which travelled westward from Holland via the Panama Canal—resulted in a complete circumnavigation of the globe. On the latter expedition, Vening Meinesz completed a detailed survey of the enigmatic Java Trench. Over 200 measurements of gravity-at-sea were obtained—a staggering level of productivity [Bowie, 1929d; Veining Meinesz and Wright 1930, p. 1; Vening Meinesz, 1932]. The success of these expeditions caught Bowie's attention, and he began to discuss the results with Arthur Day, director of the CIW Geophysical Lab.

Bowie and Day saw the significance of Vening Meinesz's work. On a practical level, this was Hayford's ambition realized, with the potential to solidify—or radically transform—existing geodetic practice. On a theoretical level, it was an opportunity to test the theory of isostasy across the globe. In the spring of 1928, Bowie and Day approached C. S. Freeman, Superintendent of the U.S. Naval Observatory, with a plan to invite Vening Meinesz to measure gravity aboard a U.S. Naval submarine. The Navy would supply the submarine, the Carnegie Institution would invite Vening Meinesz and pay for his trip, and the Coast Survey would provide the base station for calibrating the gravimeter and the personnel and computational expertise to perform the data reductions [Vening Meinesz and Wright, 1930, p. 1-2; CIW GL Misc File 1, 1908-1935]. CIW would also supply Fred Wright to assist with the investigations. In the early 1920s, Wright had developed a torsion gravimeter to use density differences to investigate volcanic processes, and he was anxious to learn the details of Vening Meinesz's technique [Fleming and Piggott, 1956; Wright, 1916; Wright and England, 1938; Wright 1941; and CIW GL Misc. File, 1908-1935 #1]. In June 1928, Navy Secretary Curtis Wilbur agreed to the proposal, with the stipulation that CIW would be sure to pay for all of Vening Meinesz's personal expenses, including meals on board the submarine [CIW GL Misc. File 1908-1935 #1, Wilbur to Merriam June 19, 1928].

Although some geologists had questioned the conclusion of complete and local equilibrium [e.g. Barrell 1919, 1927] Bowie plowed ahead in working through the geological implications of the Hayford-Bowie result. If isostatic disturbances were to be found, they should be in areas of recent loading, such as river deltas. Positive gravity anomalies were therefore expected in coastal regions where large quantities of sediment were accumulating, but not over the deeper portions of the ocean which were thought to be ancient, fully equilibrated, features. Indeed, following the earlier work of Dutton, Bowie believed that sedimentation was the *only* cause of isostatic imbalance [Bowie, 1925; 1929b; 1929d; 1931a; 1931b; 1935b]. But Vening-Meinesz's cruises had suggested the opposite result. Measurements over the Nile Delta revealed no gravitational anomaly despite the huge volume of sediment known to be accumulating there [Bowie, 1927; 1929a]. Furthermore, unexpected negative anomalies had been found over the Java Trench. Vening Meinesz's preliminary interpretation of the Java Trench was that it was a recent or even active downwarping in the crust that was not yet adequately compensated. This suggested that, contrary to Bowie's views, there *were* active stresses present in the crust [see, for example, discussion by Evans in Bowie, 1924, p. 43-44]. Bowie's initial impulse was to explain away the missing anomaly over the Nile Delta by suggesting that isostatic adjustment was even more rapid than had hitherto been supposed. Quoting from the geologist and Arctic explorer Fritjof Nansen, in reference to the isostatic rebound of Fennoscandia, Bowie suggested that perhaps "the earth's crust . . . approaches its level of equilibrium much more closely than even the most extreme advocates (like Hayford) of perfect isostasy have considered to be possible" [Bowie 1929b, p. 589]. Yet the negative anomalies over the Java Trench remained to be explained.

GOALS OF THE S-21 EXPEDITION

The itinerary of the S-21 expedition was designed to take on the theoretical issues raised by Vening Meinesz's preliminary results. As Eleanor Lamson, Head of the Computing Section that analyzed the results, put it, the goal was "to include as many stations as possible which would assist in solving the geophysical and geological problems in and near the West Indies" [Lamson, 1929]. The overall theoretical framework was explained in detail by Wright in his preliminary report of the cruise, sent to John Merriam, CIW president, in February 1929:

> Gravity measurements over the land areas of the Earth prove that the earth's crust is everywhere in a state approaching equilibrium. Wherever large departures do occur, they indicate excess or deficiency of load and these in turn produce stresses in the Earth's crust. It is an axiom in geology, as in other matters, that extremes are temporary in character; high mountain masses are not eternal but are soon worn down and effaced. If they are actually extra loads on the Earth's crust, they give

rise to abnormally high gravity values. [Thus g]ravity anomalies above a certain value serve to locate unstable portions of the Earth's crust where movements are taking place so rapidly that compensation has not kept pace with the disturbance and where therefore earthquakes due to shearing stresses are likely to occur. A knowledge of these factors, especially of the order of magnitude of the stresses active in mountain building, earthquakes, and other crustal movements, is fundamental geologic theory and can best be ascertained by gravity measurements over areas not yet compensated. [Therefore i]n planning the itinerary of the cruise of the S-21, the effort was made to select areas that promised results of interest, such as the continental shelf, the Sigsbee Deep and the Mississippi Delta of the Gulf of Mexico, the Bartlett Deep of the Caribbean Sea, the Nares deep north of Porto Rico [sic] and the normal value of gravity over the deep portions of the Caribbean Sea and of the Atlantic Ocean. [Wright 1929]

Thus the plan was to cross the Mississippi Delta to check for an expected positive anomaly, and the Nares, Bartlett, and Sigsbee Deeps, to check for negative anomalies. The Deeps were especially close to the hearts of Coast Survey scientists, as the discovery of the Sigsbee Deep by their colleague Charles Sigsbee was considered one of the major accomplishments of 19th century hydrography. As historian Thomas Manning has put it, the discovery of great depths was a passion of 19th-century hydrographers and Sigsbee's discovery was one of the deepest [Manning, 1988, pp. 37-38]. Sigsbee's work was followed by that of hydrographer John Bartlett. The Sigsbee and Bartlett deeps perplexed American geologists [Anonymous,1888]. Why should there be differences in elevation in the ocean basins greater than those known on land? What geological forces formed them? Existing tectonic theories gave no account of any significant heterogeneities in the ocean floor. Until the late 19th century, they had never needed to. However, in the early 20th century, preliminary geodetic work in Puerto Rico revealed deflections of the vertical in excess to that attributable to the mass of the island, or the mass deficiency of the surrounding ocean, suggesting that isostatic anomalies might be associated with these Deeps [Bowie, 1926a] .

Earthquakes were also relevant to the itinerary. Some geologists argued that off-shore earthquakes were proof of large-scale crustal movement in the ocean basins. This idea could be tested by looking for isostatic disturbances:

If, for instance, the crust is subject to tangential stress [horizontal compression], its position may [change] and this gives rise to an excess of mass that reveals itself in positive isostatic anomalies. . . . If, therefore, we succeed in determining the true isostatic anomaly for a certain part of the earth's crust, we may obtain data on the trend and the magnitude of the tectonic stresses existing in that region. [Vening Meinesz and Wright, 1930]

In other words, isostatic anomalies could not only reveal the location of active stresses in the crust, but also should be proportional to the magnitude and direction of those stresses, thus providing a test of the idea of continental drift.

The problem of drift was prominent in the minds of several American geologists in the late 1920s, including Merriam and Wright. In October 1928, while Wright and Vening Meinesz were on board the S-21, Merriam arranged for the Dutch scientist to receive a copy of Alex Du Toit's new book, *A Geological Comparison of South Africa with South America* [1927; CIW General Files, Meinesz folder, Memorandum, October 1, 1928]. Du Toit's study had been funded by the CIW to test the evidence of geological similarities between the two continents attributed to continental drift. Wright was directly connected with this work. He and Reginald Daly of Harvard University had proposed Du Toit's project to the CIW, after travelling with the South African geologist on a CIW-sponsored field trip in 1922. In South Africa, Wright and Daly had also met G. A. F. Molengraaf, Holland's leading expert on the geology of the East Indies, and a proponent of some form of continental drift because of its apparent applicability to the geology of the East Indies [CIW General files, Wright folder; Molengraaf, 1928; van der Gracht, 1928; Laudan, 1986]. Upon his return from South Africa, Wright became Du Toit's principal contact at the CIW, and Merriam turned to Wright to review Du Toit's preliminary manuscript before accepting it for publication.

Wegener's theory of drift suggested definite predictions about the isostatic condition of the oceans. These predictions might be tested by the gravity-measuring expedition. Vening Meinesz and Wright explained:

In areas subject to orogenic movement, [large] stresses . . . are to be expected; gravity measurements in such areas are especially important and serve to increase knowledge of the factors that tend to disturb the shape of the Earth's crust. [Therefore m]any of the measurements made on the voyage of the S-21 were over areas in which crustal movements have recently taken place. [Vening Meinesz and Wright, 1930, p. 11]

Gravity anomalies might provide a test of continental drift.

RESULTS OF THE S-21 EXPEDITION

The results of the S-21 expedition confirmed the suggestion that isostatic compensation was considerably more complicated than Hayford and Bowie had thought. Like the Nile Delta, the Mississippi Delta seemed to be almost entirely compensated, "in spite of the fact that each year a load [of] nearly 12 billion tons [of sediment] is being laid down" [Wright, 1929, p. 9]. This suggested that the oceanic crust was staggeringly weak, and that equilibration was virtually instantaneous, "proceeding concomitantly with the deposition of the load." [Vening Meinesz and Wright, 1930, p. 77]. On the other hand, the Nares Deep was indeed the site of a large negative isostatic anomaly, suggesting that it was a recent geological feature "in which shearing stresses of large magnitude are present" [Wright, 1929, p. 9]. Furthermore, the negative anomaly extended beyond the length of the Deep proving that the anomaly was not caused by the mass deficit of the Deep, but rather that both anomaly and Deep were caused by movement of the Earth's crust in geologically recent time. The Bartlett and Sigsbee Deeps were partly compensated, suggesting that they were older features.

But the most unexpected result of the cruise was the discovery of a systematic positive isostatic anomaly throughout much of the Gulf of Mexico, in an area of relatively uniform depth that they labelled the "Gulf of Mexico Plate." This positive anomaly increased abruptly by an order of magnitude at the edge of the continental shelf. These findings utterly contradicted the expectation of general isostatic equilibrium over the table portions of the ocean basins. A major isostatic disturbance had been revealed that had little or no topographic expression. Furthermore, the overall results seemed to give contradictory indications about the strength of the Earth's crust. On the one hand, the results from the Delta suggested a very weak crust, responding almost instantaneously to sedimentary load, consistent with the traditional Hayford-Bowie view. On the other hand, the positive anomalies over the Gulf of Mexico "plate" suggested a strong crust supporting regionally extensive excess loads. Vening Meinesz and Wright were at a loss to explain these results, and the possibility of a new explanatory framework began to develop in their minds:

> The theory of isostasy has been so well established that it is not easy to understand actual excess loads on the Earth's crust over areas of such vast extent. If stresses, active in the crust, are responsible for them, then the engineering difficulty arises of explaining their maintenance over such great areas. [Vening Meinesz and Wright, 1930, p. 76]

It appeared that the crust needed to be extremely strong and extremely weak at the same time.

The authors also discussed their data as a test of the hypothesis of continental drift. Vening Meinesz noted that some positive anomalies had been detected on the Pacific side of the American continent during his earlier expeditions, which might be taken as evidence in support of drift: westward migration would cause buckling of the crust, leading to excess mass and positive anomalies. However, the S-21 expedition had revealed positive anomalies on the *Atlantic* side: the opposite of that predicted on the theory of westward drift.

> The fact that . . . pronounced positive anomalies are found at the foot of the continental slopes bordering the Pacific Ocean might seem to point toward a westward drift of the American continent in accord with the theory of Wegener—because they might be explained—in connection with the time element required to re-establish equilibrium—by the effect of the pressure exerted by the continent on its front side; but in this case one would expect a negative anomaly on the lee side, i.e. the east coast. [Vening Meinesz and Wright, 1930, p. 76]

On the face of it, the North American continent appeared to be moving in both directions simultaneously. Interpreted in terms of Wegener's theory of *westward* drift, "the evidence . . . so far as it goes, is not in favor of this hypothesis" [Vening Meinesz and Wright, 1930, p. 76]. But neither did the evidence confirm existing views.

A NEW FRAMEWORK FOR RESEARCH

The results of the S-21 expedition led Vening Meinesz and Wright to two conclusions at odds with the mainstream of American geodetic thinking. First, they concluded that major regional stresses *were* present in the oceanic crust. The ocean basins were neither a passive substrate for floating continental rafts, nor were they a fully compensated region of higher than average crustal density. They were geologically active provinces sustaining regionally extensive tectonic stresses. Second, they concluded that if the crust did sustain significant stress, then it must not be so weak as the Hayford-Bowie school supposed. It must contain "some residual strength." These conclusions implied a re-thinking of isostatic processes and their relation to geological change. A change in Wright's perspective is clearly apparent in one passage, which was significantly revised between his preliminary report to the CIW and the final report published with Vening Meinesz one year later. Wright's axiom, quoted above, that "in geology, as in other matters . . . extremes are temporary in character" was modified in his final report, and placed in a quite different context. It was no longer his own opinion, but a truism found in the geological literature, which was perhaps being interpreted in faulty or misleading ways:

> In geological literature emphasis is placed on the fact that extremes in topography, such as lofty mountain ranges and great ocean deeps or troughs, are geologically young features and essentially evanescent in character. They are the culminating centralized effects of the action of mountain-making or orogenic forces; there we may expect to find departures from normal gravity equilibrium or balance. In view of the fact that the magnitude of orogenic forces is quite beyond direct study in the laboratory, it is necessary, if we would evaluate them, to study their effects in the field where they are now active. We know from a study of the rocks themselves what changes [these stresses] produce and how large are the masses they can move. But the mechanical relations are so complex and the quantities involved so prodigious that we can not, by any direct method, measure the order of magnitude of the forces themselves. Gravity measurements afford the only available approach to this problem which is fundamental to geological theory [Vening Meinesz and Wright, 1930, p. 53].

This statement was a direct indictment of colleagues who presumed to understand the mechanics of the Earth's crust based on grossly scaled-down experimental studies or theoretical models rife with unverified assumptions. It was clear to these two scientists that the Earth's crust simply was not understood. Measuring gravity at sea demonstrated the inadequacy of existing knowledge and concepts. The causes of large-scale deformation over geological time-frames needed to be studied via their observable and measurable effects. Thus, Vening Meinesz and Wright concluded their report by prescribing an explicit research program:

> [T]he contribution of the S-21 cruise to the theory of isostasy and to fundamental geological theory is of lasting value and should form the starting point for other measurements of similar nature over more extended areas. The regions surveyed in a reconnaissance way should be explored in detail with both the gravity apparatus and the sonic depth finder in order to obtain accurate information on the physiography and the distribution of the gravity anomalies and of the stresses present in the Earth's crust. It is believed that much of the detailed gravity and sonic depth-finding work in the Caribbean area can be done on a submarine temporarily assigned for a few weeks to the task. [Vening Meinesz and Wright, 1930, p. 4]

The proposed program began immediately, as Vening Meinesz embarked in 1929 on another Dutch cruise to the East Indies, which confirmed and expanded his earlier results [Vening Meinesz, 1932; Vening Meinesz et al., 1934; Vening Meinesz, 1941]. Meanwhile, the USCGS organized an immediate land-based expedition to Haiti, Cuba, and Puerto Rico, and Bowie made arrangements for future submarine expeditions. Bowie was confident that the S-21 was "only the beginning of the use of American submarines on gravity surveys" [Bowie, 1929d, p. 220]. And in April 1929, the Hydrographic Office announced plans for additional submarine-based investigations [Freeman, 1929]. These plans came to fruition in the *International Expedition to the West Indies*, led by Richard Field of Princeton University in the early 1930s, with junior colleagues Harry Hess and Maurice Ewing. A series of further expeditions followed [Field et al., 1933; Field, 1937; Ewing, 1937; Hess 1937; Worzel, 1965], and Hess and Ewing subsequently became leaders in marine geophysics and the development of plate tectonics [Bullard, 1975; Bates et al., 1982; Menard, 1986; Allwardt, 1990]. The S-21 expedition spawned a new research tradition, beginning with measuring gravity at sea, soon expanding to include geological magnetics and seismic profiling (C. Drake, pers. comm., 1993; Bates et al., 1982]. The result was a new *kind* of scientific data—marine geophysical data—which ultimately led not only to a new explanatory framework for isostasy, but for all of the Earth sciences.

A RESEARCH TRADITION THAT WASN'T

From the start of the S-21 project, John Merriam and Arthur Day planned to continue gravity studies on the ship *Carnegie* after Vening Meinesz had gone home. This was the principal reason why Fred Wright was sent on the cruise: his prior experience with gravity apparatus suggested that he could facilitate a technology transfer [F.E. Wright, U.S. Patent no. 1,579,273, 1926; CIW GL: Patents folder: F.E. Wright]. Recall that the rationale for using submarines was to avoid the effect of surface disturbances by submerging below wave base; the S-21 measurements demonstrated that rolls up to 6° could in fact be tolerated [Vening Meinesz and Wright, 1930]. Day was not convinced that the *Carnegie* would prove this stable, and expressed hesitation about spending large amounts of CIW funds on instrumentation that might not function. However, he agreed to purchase the equipment given assurances from the Navy Secretary it would be "no imposition to . . . install such an apparatus upon appropriate submarine cruises in the future" if the work on the *Carnegie* did not succeed [CIW-GL Misc. File #2

1908-1938, Day to Merriam, January 9, 1929]. With this back-up, the CIW Executive Committee approved the purchase of a Meinesz gravimeter from its Dutch manufacturer, to be installed on the *Carnegie* when it next docked in San Francisco.

The gravimeter arrived in Washington in August 1929. Wright calibrated it and travelled with it to San Francisco. At this point the program was taken over by Scott Forbush of the Department of Terrestrial Magnetism [CIW GL Misc File 2 1908-1938; Bowie to Merriam, August 30, 1929; Merriam to Bowie, Sept. 8, 1929]. Gravity measurements during the seventh cruise of the *Carnegie* between September 3 and November 24, 1929 yielded some preliminary data indicating a large major negative anomaly over the Tonga Deep bordered on both sides by positive anomalies [CIW-DTM General Files 1900-1935, Forbush File; Forbush, 1946]. However, as Day had feared, the ship's instability limited the number and reproducibility of these results. Forbush was confident that the problems could be resolved, but before he had a chance to try, on November 29, 1929, the ship *Carnegie* tragically burned [Forbush, 1946; Harland, 1967]. The gravity program on the *Carnegie* was over. Future work would be done on submarines.

The Stabilization of a New Research Tradition

The S-21 gravity-measuring cruise marked the start of a new research tradition. This raises the question of what begets a successful research program. How was a 56-day cruise transformed into a decades-long research program? Here, we can identify three agents that helped to define and unify the emergent tradition: a set of theoretical questions, an instrument, and a geographic locale.

A set of theoretical questions: Gravity measurement at sea grew out of continental geodesy, a field rooted in cartography and driven by unapologetically utilitarian concerns. But the theoretical implications of geodetic work and the significance of isostatic compensation for models of Earth processes had been widely recognized in the late 19th and early 20th centuries. The S-21 cruise was a nearly seamless extension of the land-based geodetic tradition, both in terms of the measurements being made and the hopes for their significance. However, the observational results obtained challenged some of the fundamental beliefs of that tradition, and thereby brought into sharp focus a set of theoretical questions that related problems in isostasy to larger geological issues. The most important of these questions was the relation between isostatic disturbances and the forces that generate large-scale geological features in the crust. In articulating this issue, Vening Meinesz and Wright created an intellectual justification and framework for continued work centered on geological, rather than geodetic, questions. The recognition of this new context was facilitated by Wright's broad geological background.

An instrument: The tradition of measuring gravity at sea was also the direct result of instrumental development by an individual motivated by a practical problem. Vening-Meinesz's technical innovation permitted accurate measurements to be made that were previously impossible. In the S-21 expedition, technology drove science. Without this technology, the research tradition could not have developed no matter how interesting or important the problem to be solved. Furthermore, as Robert Kohler has suggested in a different context, instruments may help to facilitate cooperation among scientists from diverse disciplinary (or institutional) backgrounds by permitting them to share a common technique; this occurred in the 1930s, when large numbers of physicists joined geologists to create marine geophysics (Kohler, 1991, pp. 358-64, esp. 360). But what of the use of submarines? Was this an essential element, or merely a romantic but coincidental detail? As discussed above, the reason for submarine-based work was to minimize random accelerations at sea by submerging below wave-base. But the goals of the CIW, and the results on the S-21, suggested that the work might be done on a conventional ship. Had the *Carnegie* not burned, the initial difficulties might well have been worked out, and the research program could have continued under civilian auspices. But note that Day's willingness to purchase the gravity apparatus for the *Carnegie* was *contingent* upon the military back-up. Submarines may not have been essential, but the logistical, financial, and psychological support of the Navy was.

A geographic locale: Geologists commonly organize their research programs around a specific locale or a physiographic province, but geophysicists often reject the intellectual premises of "localism" [Le Grand, 1986; 1988, esp. pp 80-89, and refs. cit therein]. Indeed, some scholars have attempted to *define* geophysics in contradistinction to geology as founded in a concern with the structure of the globe as a whole, or with processes that transcend local manifestations [Wood, 1985]. Elements of this view may be true, perhaps for historical reasons, but the S-21 expedition suggests that such a distinction is an oversimplification. In the geophysical work described in this paper, the geographic locale of the ocean basins was a unifying intellectual theme: the *raison d'etre* of this work was to explore and understand an uncharted physical province. Before long, the theoretical questions began to change, and so did the instruments, but the focus on the oceans remained [Laudan, 1980].

Three unifying factors—theoretical, instrumental, and geographical—gave conceptual focus to the emergent research tradition. Institutional back-up from the U.S. Navy gave civilian scientists the confidence to embark on an extended and expensive research program. But what created the successful collaboration in the first place? Why did a private philanthropic institution become involved in a joint venture with a government agency and the military? How did a group of scientists convince the Navy to allow them to occupy a submarine for the better part of two months? The answers to these questions are essential to understanding the extensive Navy-civilian cooperation that so radically affected the Earth sciences in the middle and later parts of the 20th century—cooperation which began well before the start of the second World War. (On earlier links between geology and the U.S. military, see Goetzmann, 1959, 1966; on contemporary links, see Mukerji, 1989). One key element in the S-21 collaboration was the facilitating role of scientific and government administrators: William Bowie at the Coast Survey, John Merriam at the Carnegie Institution, and Curtis Wilbur, Secretary of the U.S. Navy. Each of these men had reasons for supporting the S-21 expedition; institutional and personal ties between the Navy, the USCGS, and the CIW were critical in creating an expedition that required the resources of all three.

THE ROOTS OF COLLABORATIVE RESEARCH

Two important factors in the initiation and success of the S-21 collaboration were the organizational prowess of William Bowie and the institutional resources to which he had access. Bowie's work at the Survey had given him decades of experience organizing field parties for scientific and technical purposes, and from this field-oriented and team-driven tradition sprung a drive towards collaborative scientific research initiatives [Fleming, 1951; Whitten, 1992]. In 1939, Bowie became the first recipient of an American Geophysical Union medal bearing his name, and dedicated in his honor to rewarding "unselfish cooperative research" [Fleming, 1951]. But Bowie's position at the Coast Survey not only inspired him towards collaborative efforts, but also permitted him to supply the specific computational expertise and personnel required for the S-21 project.

The Coast Survey was a uniformed service, and a long institutional tie with the U.S. Navy—a tradition dating back to Alexander Dallas Bache—would have suggested the possibility of access to a submarine [see Dupree, 1985, esp. pp. 133-34; Reingold, 1991, p. 112]. However, Bowie's work at the Coast Survey did not actually place him in contact with the Navy Secretary. Rather, it was Merriam who provided Bowie's first letter of introduction to Curtis Wilbur [LC JCM, Merriam to Wilbur, April 11 1925, 9507—Box 134]. Merriam, president of the National Research Council (NRC) prior to joining the CIW, was highly connected in Washington political and scientific circles [Stock, 1951], and Merriam and Bowie became acquainted through their mutual work on the NRC. They also served together on the National Academy of Sciences Committee on Oceanography formed in the late 1920s [LC JCM Box 22 Bowie folder, Letters through 1932]. Also linked to this "Washington network" for geology [see Hevly, this volume] was Arthur Day, chair of the NRC section on "geophysical chemistry."

Merriam was highly receptive to the concept of collaborative research, although for different reasons than Bowie. Merriam believed that his institution would better serve Andrew Carnegie's aim of improving the condition of mankind if it worked in concert with others. In a report written for Merriam in 1933, Fred Wright argued that the CIW could not only contribute more scientifically through cooperative efforts with other researchers, but that the institution should be a role model in this respect: "[T]he major contribution that can be made by the Carnegie Institution of Washington is that of teaching, by example, the value of cooperation in the attack on scientific problems" [LC JCM Box 185 Wright folder; Letters through 1934; Report by F. E. Wright for Merriam, 25 October 1933, p. 3.] Indeed, Wright suggested it was peculiar to think otherwise: "The Army and the Navy realize the need of pulling together according to carefully prepared plan. Team work in sports is axiomatic . . . " . Furthermore, in the short history of the institution it had already become apparent that "exceptional men" were exceptionally rare. Fortunately, there was an alternative which might achieve comparable results: groups of good men acting in concert. Cooperative research could also facilitate knowledge and technology transfer, multiplying the impact and effect of the "exceptional man" when he was found [Ibid., p. 4].

Merriam's commitment to the cooperative and interdisciplinary approach outlined in Wright's report stemmed at least in part from his personal experience in paleontology, a discipline which had been rife with priority and interpretive disputes. It was Merriam's opinion that many of these problems stemmed from the excessively specialized nature of paleontology which

> tends to narrow itself and through this development to become relatively ineffective. It would be good for paleontology to have physics, chemistry, biology and all the other sciences working on the problem of life just as it would be good for geology, and possibly in a few instances physics, to know the outcome of paleontology. [LC JCM Box 185; Wright folder, Merriam to Wright, March 11, 1932]

Thus Merriam supported interdisciplinary projects as diverse as the *Carnegie Committee on the Surface Features of the Moon* and *The Committee for the Study of the Color of the Water of Crater Lake* [LC JCM; also see Yoder, 1989; Doel, this volume; and Hetherington, this volume]. The S-21 expedition appeared a particularly suitable venture in several respects: it was large-scale and easily publicized, thus helping to fulfill the goal of setting an example for the rest of the scientific community; it was organized around an exceptional man whose expertise would be shared and ultimately transferred; and it involved cooperation with the Coast Survey and Navy, organizations with experience in collaborative work. But what of this third component in the S-21 expedition: the U.S. Navy? Why did the Navy agree to tie up one of its submarines in an apparently esoteric scientific enterprise? Navy Secretary Wilbur actively supported not only the original expedition, but also the concept of a long-term collaborative venture. Why?

Part of the Navy's enthusiasm arose from Wilbur's technophilic philosophy. Wilbur was driven by a fundamental belief that the history of the Navy was the history of technological advance, with technology on the side of the victors. Any Navy that did not keep up with technological innovation would soon become the victim of one that did. In stump speeches given on behalf of presidential candidate Herbert Hoover, Wilbur focused specifically on Hoover's technical background as his greatest attribute and qualification for the job of commander-in-chief. In various other public addresses, Wilbur emphasized the controlling impact of science and technology in the modern world in general and the military in particular. In the Navy, technology had been and would continue to be critical in the obvious areas of power and navigation, but Wilbur argued that the impact was *pervasive*, from the diesel engines that replaced the boiler room to "the electric potato peelers in the modern mess." [LC CDW, Box 2, Speeches 1924-27]. Whilst the contribution of potato peelers to victory might not be measurable, other innovations had turned out to be decisive, often in unexpected ways. But given that the Navy had no research labs of its own, support for civilian scientists had to be considered part of the Navy's mandate. Thus Wilbur declared the Navy's arms "outstretched to the inventor and the scientist":

> What does it mean to have a radio-controlled, electronically operated Navy? It means constant experimentation, constant research, constant development. What does it mean to have aircraft incorporated into the Naval forces, directing its gunfire by radio from the air, assailing its enemies in the air, and on the water and beneath the water? It means invention, research, improved planes, improved engines. . . . [LC CDW Box 2, Speeches 1924-1927, "Recent accomplishments in the Navy," ca. 1927, p 2]

Wilbur furthermore accepted the stereotype that creativity and invention were a particular strength of American culture, and should therefore be a strength of the American military. He recounted the following apocryphal story:

> It is said that when the German Kaiser was considering the question of war with America he said he had no fear of the American army or the American Navy or the wealth and resources of the American people, but he did fear the American's inventive genius . . . [Therefore] it is essential and indeed vital that the American genius for invention should be constantly stimulated along lines looking toward success in war, as well as in peace [LC CDW Box 3, Press releases, 1924-1929: Sept. 5, 1924; p. 1-5].

Since genius was to be found in diverse places, the task of the military was to find it, support it, and remind it of military concerns. One particular concern of Wilbur was U.S. dependence on imported Mexican oil, and the risk of a wartime "energy crisis." Therefore, he took a strong interest in the work of geologists, and advocated the development of a strategic petroleum reserve:

> Five years ago, the Federal Oil Commission reported that according to the best judgment of the oil industry the Nation's supply of crude oil would be exhausted in ten years, that is in 1934. This would mean that the Navy, which is almost wholly oil burning, would be helpless, unless new oil was discovered or new equipment provided. . . ." [LC CDW, Box 2, Speeches 1924-1927, "Progress in the Navy," ca. 1929, p. 5]

He also wrote:

> That an ample supply [of oil] should be available within our own continental borders in time of war needs no argument. The commodity so absolutely vital to the very life of the nation must not be dependent upon foreign supply and overseas transportation, else disaster and defeat will be inevitable, for even if there were overseas sources upon which we could draw freely without international complications as to violations of neutrality, etc., it would be necessary to call so heavily on the fleet for protection of the lines of communication that the armed forces in the actual combatant areas must needs be seriously depleted. What we need and must have are huge ground reserves of oil capable of being drawn

upon without delay to supply the petroleum products needs by the armed forces and industries in the event of war, and a sufficient amount of overground storage to take care of our week to week wants [LC CDW Box 2, "Speeches, 1929 and undated, "Petroleum and National Defense"].

A strategic petroleum reserve would prevent short-term supply cut-off during hostilities, but Wilbur wanted to be proactive to ensure the long-term supply by funding research into alternative energy sources:

> [W]e must remember that petroleum, like other mineral raw materials, is a wasting asset. No other mineral among those essential to the well being of the country has shown so rapid an increase in production and also in the rate of depletion. The question is how long can we keep up the pace we are going and still have any reserve stored in the natural reservoirs. . . . The authoritative estimates by geologists is that the original supply of petroleum in the United States is forty per cent gone . . .
>
> [Therefore] the Navy has been planning, experimenting and testing a new plan of supplying fuel. The oil shale reserves of the Navy and the nation are tremendous. If the Navy can develop a method of producing oil from oil shale, in commercial quantities and with reasonable economy, the problem of national defense, so far as fuel for aircraft and for ships, will be solved . . . [LC CDW "Petroleum and National Defense," Box 2, "Speeches 1929, and undated" and Speeches, 1924-1927, "Progress in the Navy," p. 5].

While promoting research on alternative fuel sources, Wilbur also continued to advocate technological innovation in the conventional arenas of power and navigation. In July 1924, Wilbur organized a meeting of "representatives of the executive departments and scientific establishments of the Government of the U.S. Navy" to discuss the importance of further conventional exploration of the oceans and the need for technological improvements in this area. He reminded his colleagues that the oceans were still far from fully explored, and that there remained many "unsounded depths and undiscovered secrets" beneath the waves [LC CDW, Box 3: Press releases, 1928 " July 1, 1924 [sic]]

The S-21 expedition was consistent with Wilbur's goals on a variety of levels. Gravity measurement at sea was related clearly to the overall physiography of the ocean basins. Wilbur's general sympathy to innovation would have predisposed him to look favorably on an expedition to test a new technological device. His preoccupation with energy resources would have made him sympathetic to having geologists on board. The type of data being produced—improved information about the shape of the Earth—was directly relevant to the latitude corrections required for gyroscopic measurement of true north and, indeed, for all navigational problems [LC PCDW, Box 3 Press releases, "Extracts," p. 5]. Wilbur's professional interests for the Navy thus coincided on several levels with the research needs of the scientists of the S-21 expedition. Wilbur fostered a technocratic milieu in which scientific research was perceived as beneficial to the long-term interests of the Navy, and in which the Navy was on the lookout for research of potential practical relevance. The Navy would support science so that scientists would be on board [in this case, literally] when the Navy needed them. The Navy would know when potentially important innovations arose, and scientists would keep the Navy in mind when they did their work [cf. Mukerji, 1989]. The result was a symbiosis between a military establishment on the look-out for relevant technological innovation and a scientific establishment seeking logistical and material support to expand its domain of research.

BASIC OR APPLIED RESEARCH?

Nathan Reingold has eloquently argued that historians have been hindered in their understanding of American science by adherence to the dichotomous categories of "basic" and "applied" research, which perhaps exist more for contemporary historians than they did for past American scientists [Reingold and Reingold, 1981, pp. 1-6; Reingold, 1991, pp. 60-61]. The S-21 expedition is a case in point. William Bowie and Felix Vening Meinesz were geodesists employed to provide their countries with better geodetic control, but this required a fundamental concern with the shape, the structure, and even the history of the Earth. Consequently, these men thought about, talked about, and published articles on issues that we would label "theoretical" geology. They called themselves geodesists, but they published papers in *Nature*, *Science*, and the *Bulletin of the Geological Society of America*, as well as in the publications of their sponsoring institutions.

When Bowie referred to the S-21 expedition, he repeatedly wrote of "the *purpose* and scientific *significance* of the determination of gravity at sea." [e.g. Bowie, 1929d; emphasis added]. Bowie viewed his work as simultaneously practical and theoretically significant. Utility and knowledge arose congruently from the same activity. There is no evidence that Bowie and his colleagues saw these two goals as mutually exclusive, competing, or that one was an excuse for the other. Nor is their any evidence that the pressure to do "applied" research had a "debilitating effect" on their theoretical work [see Reingold, 1991a p. 59]. Rather, practical and theoretical goals arose congruently from the nature of geodetic work. Bowie, Vening Meinesz, and Wright were all exceedingly productive scientists, and part of the pleasure of their work seems to have been that it *was* both purposeful and significant.

Likewise Fred Wright aimed for "a more accurate determination both of the figure of the Earth and of the state of equilibrium of its surface features" [Vening Meinesz and Wright, 1930, p. 1]. The Earth's figure was primarily a practical problem, the state of equilibrium was primarily a theoretical one. Two goals were accomplished through one activity. Wright never suggested that one or the other was his "real" goal. This might seem surprising from a Carnegie man, employed full-time at an institution dedicated to advances in fundamental knowledge and conceived of as a "sheltered enclave for basic research" [Reingold and Reingold, 1981, p. 3; also see Servos, 1984; Reingold, 1991; Kohler, 1991]. But the concept of science as service was not alien at the Carnegie. During the first World War, Wright had researched and supervised the production of optical quality glass; this work resulted in a number of scientific papers during and after the war [Schairer, 1954]. Nor did Wright's colleagues dismiss these practical applications: Wright's memorial in *American Mineralogist* emphasized the importance of his work on the ternary system CaO-Al_2O3-SiO_2 in the development of Portland cement.

Vening Meinesz and Wright's science and invention developed concurrently. Wright developed his torsion gravimeter because of its relevance to geological problems; Vening Meinesz was well enough aware of geological issues to put his geodetic invention quickly to work on them. Among Wright's other inventions he counted a device for distinguishing between cultured and natural pearls and a method to decrease the "scratching proclivities" of scouring powder. There is no evidence that these useful inventions offended Wright's patrons in any way; on the contrary, Merriam approved and sponsored Wright's patent application for the gravimeter [CIW General Files: F. E. Wright folder]. Perhaps this only seems surprising in retrospect, given current notions of what it means to do "pure" science. But recall that the CIW founders were steeped in the traditions of American federal science, and hence of federal *service* [Reingold, 1991, p. 218-219; Yochelson, this volume]. The founders of the CIW had done science *in* the federal government, and in the 1920s the scientists there continued to do science *with* the federal government.

The notion of science as service was developed explicitly by Wright in later years when he wrote a report for Merriam on the "Public Relations Problem of the Institution" [Wright, 1937]. Wright emphasized that the purpose of the CIW was fundamentally the same as Andrew

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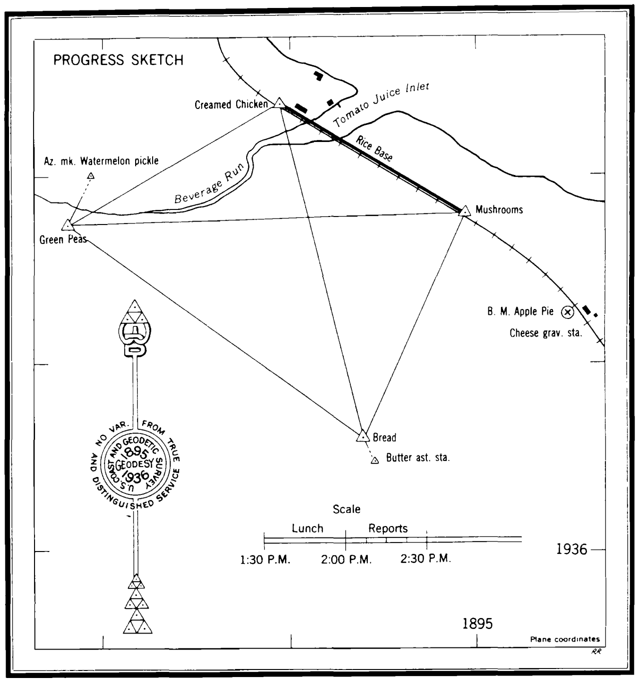
DR. WILLIAM BOWIE
U. S. Coast and Geodetic Survey
Washington, D. C., January 4, 1937



Reports:
Reconnaissance and Control . . . . C. L. Garner
The North American datum of 1927 . . . R. S. Patton
Least-Squares Adjustments . . . . H. G. Avers
Geophysical Phenomena . . . . W. D. Lambert
Isostatic Compensations
and Gravity Anomalies . . . . William Bowie

Fig. 3. The luncheon program for the retirement of [Captain] William Bowie from the U.S. Coast and Geodetic Survey, 1937. Note the north arrow, which points in only one direction, thus visually encapsulating the ethos of the Coast Survey: "true and distinguished service."

Carnegie's other endowments: all were dedicated to "progress of the human race." Thus, closely parallelling Bowie's view of the government scientist, Wright saw the task of the CIW scientist as two-faceted: "a] to contribute to progress in science through research of the highest quality; and b] to interpret the results of research in terms of their value to mankind" [Wright, 1937, p. 1]. Part of the task of interpretation included an obligation both to fellow citizens and scientists to explain and disseminate one's results. Scientists would benefit by being forced to confront the meaning of their endeavors. The two aspects of inquiry and interpretation—research and application—were seen as synergistic, not competing. In Wright's words, "these two objects are intimately linked and actually represent different phases of the same problem" [Wright,1937, p. 2]. Ultimately, CIW would suffer if it failed to maintain this essential link, for the life of this "semi-public" institution was inescapably linked to public support of it [Wright 1937, p. 3-4].

Thus although one might expect a different sense of purpose at the CIW and the USCGS, in fact a similar ethos and sense of mission facilitated cooperation. In the same year that Wright was expressing this verbally at the CIW, the ethos of science as service was encapsulated visually by the program for a luncheon in honor of William Bowie on the occasion of his retirement. Bowie's career was illustrated metaphorically by a map representing the various courses of the meal, but in place of the usual multiple arrows for true, magnetic, and grid north, this map displays only one arrow, pointed firmly in the direction of "true and distinguished service" (Figure 3).

CONCLUSION

The gravity-measuring cruise of the U.S. Submarine S-21 was simultaneously an endeavor in basic and applied science, an outgrowth of a research tradition in which utility and knowledge were, if not precisely identical, at least intimately related functions [Reingold, 1978, p. 171]. Reingold [1978, p. 167] has cited the U.S. Coast and Geodetic Survey as embodying "classic American strategies for combining culture and service, theory and practice, mass and elite," but this strategic confluence was promoted and practiced by colleagues at the Carnegie Institution of Washington as well. Among the American Earth scientists discussed in this paper, this combination fostered a mindset in which civilian-military collaboration was a natural outgrowth of shared interests and overlapping realms of concern.

A belief in the essential interconnectedness of theory and practice led American Earth scientists to the application of instrumental traditions of geodesy into theoretical realms of geology. The result was the emergence of a new research tradition: marine geophysics. This tradition crystallized around a conceptual question, an instrument with which to answer that question, and a particular place in which to use that instrument. A military establishment committed to technological advance provided material and psychological support without which the emergent tradition might well have foundered.

ARCHIVAL SOURCES

The principal archival sources used in this work are the CIW General Files, the Records of the U.S. Coast and Geodetic Survey and the John C. Merriam papers at the U.S. Library of Congress. Most of the CIW materials on the S-21 expedition are found in the Files of the Geophysical Lab [CIW-GL], or in the Director's Files. The CIW internal reports cited are as follows:

•Wright [1926]: CIW General Files; Patents: Wright

•Wright [1929]: CIW-GL Director's file #2, 1905-1935.

•Forbush [undated] "Gravity measurements on the Carnegie" and "The Meinesz gravity apparatus on the Carnegie" in CIW General Files 1900-1935, DTM: Scott Forbush folder.

The Records of the USCGS are in the US National Archive, Record Group 23 and are cited here as [US-NA USCGS]. They were inventoried by Nathan Reingold. The most useful Coast Survey materials are Series 33, Annual Office Reports, especially Box 774, 1920-1925, and 775-776, 1926-1933. Also useful are the published "Report of the Director of the U.S. Coast and Geodetic Survey", Government Documents. These give a good summary of the objectives and working conditions of the Agency.

There is a tremendous amount of material in the Merriam papers, AC 9507 at the Library of Congress, here cited as [LC JCM]. These include extensive correspondence on numerous aspects of American geology and geophysics in the early 20th century. These papers desperately need a finding guide!

Additional sources consulted include the Curtis D. Wilbur papers (Library of Congress, AC 3172), cited as [LC CDW]. These papers give an interesting snapshot into the mindset of the Navy Secretary through his speeches and other public addresses. There is no personal correspondence. The logbook of the S-21 expedition is in US-NA, Record Group 24 [US-NA RG24 LS21 Area 18W4 pp 605-613]. It confirms the accounts given in the published reports. There was essential nothing in the Records of the U.S. Naval Observatory [US-NA, RG 78, Entry 14 General Correspondence, 1925-1929] and the inventories are very poor. Lastly, there are the Collected papers of William Bowie, National Oceanic and Atmospheric Administration. These two volumes include unpublished speeches, published papers, and miscellaneous materials such as invitations to speeches, luncheon programs, etc. These give a very good sense of the range of William Bowie's scientific and professional activities. There is no personal correspondence.

ACKNOWLEDGEMENTS

I am grateful to the following persons for help in accessing historical materials: Grace Sollers, U.S. National Oceanic and Atmospheric Administration, Charles Whitten, retired, U.S. Coast and Geodetic Survey; Susan Vasquez, Carnegie Institution of Washington; Richard von Dornhoff, U.S. National Archives and Record Administration; Barbara de Felice, Dartmouth College Libraries, and Helen Wright

Greuter and Finley Wright, daughter and son of the late F. E. Wright, who provided the photographs of the S-21. Ron Doel, Charles Drake, and Greg Good graciously provided comments on an earlier draft of this paper. I am indebted to the police and librarians at the U.S. Library of Congress for not arresting me when I accidentally put several original letters in my pile of photocopies; fortunately, the materials I had were "worthless." Financial support from Dartmouth College, Burke Research Initiation Fund, is gratefully acknowledged.

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