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# Seeing the Oceans in the Shadow of Bergen Values

*By Jacob Darwin Hamblin\**

## ABSTRACT

Although oceanographers such as Roger Revelle are typically associated with key indicators of anthropogenic change, he and other scientists at midcentury had very different scientific priorities and ways of seeing the oceans. How can we join the narrative of the triumph of mathematical, dynamic oceanography with the environmental narrative? Dynamic methods entailed a broad set of values that touched the professional lives of marine scientists in a variety of disciplines all over the world, for better or for worse. The present essay highlights three aspects of “Bergen values” in need of greater exploration by scholars. First, how did the dominance of Scandinavian outlooks influence scientific questions across the broad spectrum of oceanography? Second, did oceanographers’ particular means of making the oceans legible through instrumentation challenge their ability to perceive the oceans differently? Third, given the immense quantity of data, was the historical legacy of the dynamic oceanographers more descriptive than they imagined?

THE AMERICAN OCEANOGRAPHER ROGER REVELLE urged his students to ask questions of the sea, not just measure it. Data collection was not enough. Ironically, one of Revelle’s principal accomplishments in life—judged from the present—was the data he began to collect. He was instrumental in initiating the measurements that now show the steady accumulation of carbon dioxide in the atmosphere since 1957. Looking back, we might regard the oceanographers of Revelle’s generation as the pioneers in viewing the earth as changing owing to the activities of humans. The Scripps Institution of Oceanography, which Revelle directed, became the world’s leading institution for global carbon-cycle research. After leaving Scripps, Revelle moved in 1964 to Harvard’s Center for Population Studies and continued to advocate this kind of research. One need only leaf through the pages of *Earth in the Balance* (1992) to see what a profound impact

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Revelle had on a young Harvard student named Al Gore, who would share the Nobel Peace Prize in 2007 for his efforts to draw attention to global climate change.<sup>1</sup>

Historians examining documents from 1950s oceanography will look in vain for this hero of the environmental movement. Oceanographers of that era were more likely to be found helping to blow up atolls in the Pacific than trying to save the earth. There are certainly links between the postwar earth sciences and the rise of concern about anthropogenic changes to the earth.<sup>2</sup> But like his contemporaries, Revelle in that era was motivated by different questions—questions historians should not ignore. We may arrive at a more informative portrait of modern oceanography by considering how oceanographers' scientific values informed their questions and practices. What were those values? Eric Mills has provided an important starting point by highlighting the clash between descriptive and dynamic oceanography. Descriptive oceanography measured and mapped and used data collected over time to assign causality for major geophysical phenomena. Dynamic oceanography also was data intensive, but it was grounded in mathematics and considered the interaction of large-scale ocean and atmospheric systems. Dynamic oceanography was particularly suited to addressing practical problems such as tracking fish migrations or iceberg flows. As several scholars have demonstrated, the prospects of oceanography for providing practical information attracted a variety of patrons, especially naval ones. For Mills, the central story for oceanography at midcentury was the rise of new mathematical methods in creating comprehensive models of ocean circulation, such as the ones proposed after World War II by Henry Stommel.<sup>3</sup>

How can we join the narrative of the triumph of mathematical, dynamic oceanography—in which Revelle played a major role—with the environmental narrative? Dynamic, model-based methods have been crucial across the environmental sciences, and they have consistently informed views about environmental challenges. Sharon Kingland has explored them in relation to ecologists, who differed sharply on the importance of modeling; essays in the *Osiris* volume *Klima* have highlighted scientists' attention to dynamic modeling as crucial to the early history of climate science. Paul Edwards has explored model building in his study of meteorology and climatology, and he ends up rejecting the firm line between dynamic and descriptive methods.<sup>4</sup> In the case of the oceans, we know that the rise of dynamic oceanography was crucial in addressing ocean circulation, but historians have only begun to understand the significance of dynamic oceanography for the earth and environmental sciences. Dynamic methods were not merely tools for resolving a specific scientific dispute about currents. They carried more baggage than that: they entailed a broad set of values that touched the professional lives of marine scientists

<sup>1</sup> David M. Hart and David G. Victor, "Scientific Elites and the Making of U.S. Policy for Climate Change Research, 1957–74," *Social Studies of Science*, 1993, 23:643–680; and Albert Gore, *Earth in the Balance: Ecology and the Human Spirit* (Boston: Houghton Mifflin, 1992).

<sup>2</sup> Jacob Darwin Hamblin, *Arming Mother Nature: The Rise of Catastrophic Environmentalism* (New York: Oxford Univ. Press, 2013).

<sup>3</sup> Eric L. Mills, *The Fluid Envelope of Our Planet: How the Study of Ocean Currents Became a Science* (Toronto: Univ. Toronto Press, 2009). Regarding oceanography's patrons see, e.g., Naomi Oreskes, "A Context of Motivation: U.S. Navy Oceanographic Research and the Discovery of Sea-Floor Hydrothermal Vents," *Soc. Stud. Sci.*, 2003, 33:697–742; and Jacob Darwin Hamblin, *Oceanographers and the Cold War: Disciples of Marine Science* (Seattle: Univ. Washington Press, 2005).

<sup>4</sup> Sharon E. Kingland, *Modeling Nature: Episodes in the History of Population Ecology* (Chicago: Univ. Chicago Press, 1985); and Paul N. Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming* (Cambridge, Mass.: MIT Press, 2010). An example from the *Klima* volume that touches on the rise of dynamical methods in a regional context is Deborah R. Coen, "Imperial Climatographies from Tyrol to Turkestan," *Osiris*, 2011, N.S., 26:45–65.

in a variety of disciplines all over the world, for better or for worse. Thus we should be attuned to the collateral effects of adopting scientific values (like those of dynamic oceanography) on institutional priorities, ways of seeing the ocean, and everyday scientific practice. I refer to “Bergen” values because dynamic oceanographers typically saw the Bergen Geophysical Institute in Norway as the intellectual root of their preference for mathematical modeling, intensive data collection, integrated studies of several disciplines, and prediction.

The present essay highlights three aspects of these Bergen values in need of greater exploration by scholars. First, dynamic oceanographers put pressure on themselves and others to ask sufficiently scientific questions, worthy of their prediction-oriented outlook, and quite often they demeaned the views of others as outdated and unscientific. How did that dominance influence scientific questions across the broad spectrum of oceanography? Second, the preferred instruments of the dynamic oceanographers—which collected data on temperature, salinity, and current direction—made the seas comprehensible in a particular way. Did that challenge oceanographers’ ability to perceive the oceans differently? Third, the adoption of Bergen values led to great triumphs of data collection and computation, but it also led to the accumulation of enormous amounts of data that never were used to solve a scientific problem. Yet those data now exist as natural historical information for scientists and environmental historians trying to understand change over time. Was the historical legacy of the dynamic oceanographers more descriptive than they imagined?

#### A BROAD REACH FOR BERGEN VALUES

Dynamic oceanographers were not just advocating mathematics; they were redefining what deserved to be called oceanography. Consider the precipitous twentieth-century decline in the reputation of the American naval officer Matthew Fontaine Maury, whose *Physical Geography of the Sea* (1855) was once considered one of the most influential oceanographic treatises.<sup>5</sup> To twentieth-century advocates of dynamic oceanography, however, it was a relic to be disowned. Reviewing a biography of Maury in the 1960s, the American oceanographer Columbus Iselin remarked, “In my opinion, he actually contributed much more to climatology and to physical geography” than to physical oceanography. This was not a compliment. For Iselin, “physical oceanography” was about ocean dynamics, not the descriptive measurement literally suggested by ocean- or geo- “graphy.” Maury’s reputation was a notable casualty in the struggles to assert Bergen approaches within major oceanographic institutions and to establish international standards. Iselin himself would get a similarly dismissive treatment after his death for his lack of mathematical acumen. He was not oceanographer enough for Henry Stommel, who wrote in Iselin’s obituary that “as a scientist he was beyond his depth.”<sup>6</sup>

For Iselin, Stommel, and others who tried to refashion the meaning of oceanography, useful science meant mathematics, integrated studies, constant data collection, and prediction. They emulated the efforts of Scandinavian researchers such as Vilhelm Bjerknes.

<sup>5</sup> Matthew Fontaine Maury, *The Physical Geography of the Sea* (New York: Harper, 1855). Although “oceanography” was a term widely used in the English-speaking world by the early twentieth century, it was not Maury’s own. His “physical geography of the sea” followed the terminology established by Alexander von Humboldt; see *ibid.*, p. xii.

<sup>6</sup> Columbus O’D. Iselin, “Maury’s Biography,” *Science*, 1963, 142(3600):1647–1648; and Mills, *Fluid Envelope of Our Planet* (cit. n. 3), p. 228 (quoting Stommel’s obituary of Iselin).

Drawing heavily on classical physics, Bjerknes had blended hydrodynamics and thermodynamics and applied them to large-scale systems in the sea and air. Bjerknes attempted to use fluid dynamics to understand ocean currents, and he developed a mathematical theorem to determine current velocity based on measurements of temperature and salt content (salinity). Branches of the marine sciences that did not embrace these values courted disdain. Climatology, for example, was perceived as the geriatric stronghold of a dying style of science. Its highest ambitions were to track changes over time. When climate-minded nineteenth-century oceanographers and meteorologists attempted predictions, they based them on probabilistic calculations according to seasonal climate data. By contrast, dynamic oceanographers of the early twentieth century plugged the latest data into equations. “In at least one country,” one climatologist wrote in the mid 1950s, “the old, the halt, and the infirm could be appointed to the climatological branch; the able-bodied men were expected to be forecasters, which was man’s work.”<sup>7</sup>

By the 1950s, many institutions in Europe and North America had embraced dynamic oceanography and were accustomed to shunning descriptive work. The Norwegian fisheries scientist Johan Hjort had suggested that one key to predicting the availability of fish was understanding ocean currents, and he linked ocean dynamics to fish population dynamics. In the early twentieth century, he helped to found the first international cooperative oceanographic body funded by governments—the International Council on the Exploration of the Sea (ICES). It turned its back on the great voyaging tradition of the nineteenth century and focused instead on intensive, repeated work in a large but limited area. In addition, the United States Coast Guard sponsored the International Ice Patrol in the North Atlantic; under the influence of Bergen-trained Edward “Iceberg” Smith, the patrol used the dynamic methods to compute current patterns and drifting ice.<sup>8</sup> The most influential textbook on Bergen-influenced mathematical and dynamic oceanography came from a German, Albert Defant, whose 1929 *Dynamische Ozeanographie* became the basic handbook of physical oceanographers in the 1930s.<sup>9</sup> Britain’s Discovery Committee sent scientists to the Antarctic Ocean in the 1920s and 1930s to survey whale resources; while on this voyage, the British oceanographer George Deacon conceptualized and wrote his seminal work, *The Hydrology of the Southern Ocean* (1937), which demonstrated that the Antarctic convergence—the “front” between cold and warm masses—circled the whole continent.<sup>10</sup>

Both of the major oceanographic institutions in the United States—Woods Hole and Scripps—had made explicit commitments to Bergen values by midcentury. Woods Hole’s

<sup>7</sup> F. Kenneth Hare, “Dynamic and Synoptic Climatology,” *Annals of the Association of American Geographers*, 1955, 45:152–162, on p. 152. On Bjerknes see Robert Marc Friedman, *Appropriating the Weather: Vilhelm Bjerknes and the Construction of a Modern Meteorology* (Ithaca, N.Y.: Cornell Univ. Press, 1989). On English efforts to make predictions based on climate data see Katharine Anderson, *Predicting the Weather: Victorians and the Science of Meteorology* (Chicago: Univ. Chicago Press, 2005).

<sup>8</sup> Helen M. Rozwadowski, *The Sea Knows No Boundaries: A Century of Marine Science under ICES* (Seattle: Univ. Washington Press; London: ICES, 2002); and Ronald Rainger, “Edward ‘Iceberg’ Smith and American Polar Oceanography,” in *Extremes: Oceanography’s Adventures at the Poles*, ed. Keith R. Benson and Rozwadowski (Sagamore Beach, Mass.: Science History Publications, 2007), pp. 133–172.

<sup>9</sup> Eric L. Mills, “Socializing Solenoids: The Acceptance of Dynamic Oceanography in Germany around the Time of the ‘Meteor’ Expedition,” *Historisch-Meereskundliches Jahrbuch*, 1998, 5:11–26; Cornelia Lüdecke, “Diverging Currents: Depicting Southern Ocean Currents in the Early Twentieth Century,” in *Extremes*, ed. Benson and Rozwadowski, pp. 71–105; and Albert Defant, *Dynamische Ozeanographie* (Berlin: Springer, 1929).

<sup>10</sup> G. E. R. Deacon, *The Hydrology of the Southern Ocean* (London: Cambridge Univ. Press, 1937). See also Eric L. Mills, “Creating a Global Ocean Conveyor: George Deacon and *The Hydrology of the Southern Ocean*,” in *Extremes*, ed. Benson and Rozwadowski, pp. 107–132.

first director, Henry Bryant Bigelow, had been trained by Alexander Agassiz, the director of Harvard's Museum of Comparative Zoology, who had taken the Fish Commission steamer *Albatross* to the Atlantic and the Pacific. But Bigelow lacked Agassiz's influence and deep pockets, so he had to reject his mentor's far-flung descriptive voyages—which no one wanted to pay for—in favor of a style that promised predictive results. He promoted intensive area study of the Gulf of Maine between 1912 and 1924, work modeled on the ICES studies and sponsored by the United States Bureau of Fisheries. Also, one of Bjerknes's students, Carl-Gustaf Rossby, had come to the United States in the late 1920s explicitly to apply Scandinavian notions of polar fronts to American weather forecasting, and he spent considerable time with oceanographers at nearby Woods Hole. Bigelow's successors as directors of Woods Hole, Columbus Iselin and Edward "Iceberg" Smith, had both gone to study in Norway and embraced the mathematical, data-intensive, prediction-oriented style of science.<sup>11</sup>

The Norwegian influence on the Scripps Institution of Oceanography, near San Diego, California, was just as definitive, as the institution struggled to be taken as seriously as Woods Hole. One scholar has characterized Scripps, circa 1940, as "Bergen West."<sup>12</sup> Scripps was dominated in the 1920s by laboratory work in marine biology; several faculty members wanted to see more integrated studies of biological, chemical, and physical processes, they wanted to coordinate with stations elsewhere, and—most of all—they wanted to go to sea. Laboratory marine biology shared little with Bergen values and seemed antiquated. In 1936, at the suggestion of Bigelow, the institution hired Harald U. Sverdrup, a countryman and protégé of Bjerknes, as director. When he came to Scripps from Norway he brought with him all the trappings of dynamic oceanography. He came into conflict with George McEwen, who had taught physical oceanography there for years and continued to favor long-range seasonal weather forecasting based on statistical calculations from climate data. Sverdrup slowly edged McEwen away from a teaching role in physical oceanography and worked to disassociate Scripps from McEwen's approach. Sverdrup and two colleagues at Scripps published the textbook *The Oceans* (1942), which described the oceans on a global scale and emphasized the connections between the branches of the marine sciences. At the same time, it asserted how many fields were subordinate to dynamic oceanography—"Although physical oceanography is to a great extent independent of other marine sciences," the authors wrote, "its results are used extensively in marine biology and submarine geology." *The Oceans* was a scientific manifesto in favor of the Bergen approach. It decried isolated, descriptive studies in favor of integrated work dealing with large-scale processes.<sup>13</sup>

Although Scripps struggled with Sverdrup's legacy—some suspected him of having Nazi sympathies during the war—the directors who succeeded him, Carl Eckart and Roger

<sup>11</sup> Jeffrey P. Brosco, "Henry Bryant Bigelow, the U.S. Bureau of Fisheries, and Intensive Area Study," *Soc. Stud. Sci.*, 1989, 19:239–264; Kristine C. Harper, *Weather by the Numbers: The Genesis of Modern Meteorology* (Cambridge, Mass.: MIT Press, 2008); and Rainger, "Edward 'Iceberg' Smith and American Polar Oceanography" (cit. n. 8).

<sup>12</sup> Deborah Day, "Bergen West; or, How Four Scandinavian Geophysicists Found a Home in the New World," *Hist.-Meereskundl. Jahrb.*, 1999, 6:69–82.

<sup>13</sup> H. U. Sverdrup, Martin W. Johnson, and Richard H. Fleming, *The Oceans: Their Physics, Chemistry, and General Biology* (New York: Prentice-Hall, 1942), p. 5. On Sverdrup's role at Scripps see Ronald Rainger, "Adaptation and the Importance of Local Culture: Creating a Research School at the Scripps Institution of Oceanography," *Journal of the History of Biology*, 2003, 36:461–500. Regarding McEwen see Eric L. Mills, "Useful in Many Capacities: An Early Career in American Physical Oceanography," *Historical Studies in the Physical and Biological Sciences*, 1988, 20:265–311.

Revelle, were also interested in the dynamics of the sea. Eckart, a physicist, wished to insert rigor and mathematic into theories of turbulence, and he was deeply interested in hydrodynamics. He resigned as director in just two years. Revelle knew how to talk up Bergen values, having himself gone to Norway to work with Bjorn Helland-Hansen. He had been Sverdrup's choice as Scripps director because of his experience at sea. Sverdrup did not want a successor who would return the institution to laboratory and inshore work—Scripps needed a long-distance, high-seas man who could make possible the extensive data collection required for large-scale, dynamic oceanography in the Pacific.<sup>14</sup> Revelle put Bergen concepts and patronage strategies into action by launching a series of expeditions, some in cooperation with other nations and all with naval patronage, to collect data in the middle of, across, in the north of, and in the equatorial Pacific. The expedition names reflected data sets rather than heroic voyages—MIDPAC (1950), TRANSPAC (1953), NORPAC (1955), EQUAPAC (1956). Revelle was adept at cooperation with government bodies, and some projects were linked to other massive commitments to the region, such as the Capricorn expedition (1952), which followed Pacific nuclear tests. The 1950s were years of empire building for Revelle, and as his reputation grew so too did that of Scripps—and so did the scale of oceanographic work. As the Scripps oceanographer Walter Munk remarked, “When Roger left in 1961, Scripps had a Navy bigger than that of Costa Rica.”<sup>15</sup> Although these expeditions were interdisciplinary, they were deeply influenced by Bergen values, even on subjects that had little to do with ocean circulation *per se*.

#### MAKING THE OCEANS LEGIBLE

How did Bergen values affect how scientists saw the oceans? One clear way of getting at this question is to pay close attention to choices of instrumentation and data. Since the interior of the ocean cannot be seen by the naked eye, it must be made comprehensible by some intervening technology—instruments, maps, equations. To borrow a term from James Scott, it must be made legible.<sup>16</sup> As historians, we must understand how scientists were reading the oceans and how their choices of instruments and data to be collected reinforced what they expected to see.

By the 1950s, advocates of dynamic oceanography were itching to test global circulation models like the one offered by the American Henry Stommel in the late 1940s. Their priorities in instrumentation reflected this goal. For them, the most fundamental measurements were of temperature and salinity, essential for calculating water density and thus for differentiating between water masses and calculating circulation. The means for sampling water at depth often was the Nansen bottle (developed by the Norwegian Fridtjof Nansen).

<sup>14</sup> Ronald Rainger, “Patronage and Science: Roger Revelle, the U.S. Navy, and Oceanography at the Scripps Institution,” *Earth Sciences History*, 2000, 19:58–89. See also Walter H. Munk, “Tribute to Roger Revelle and His Contribution to Studies of Carbon Dioxide and Climate Change,” *Proceedings of the National Academy of Sciences*, 1997, 94:8275–8279.

<sup>15</sup> Munk, “Tribute to Roger Revelle,” p. 8276. On the various Scripps expeditions see Ronald Rainger, “‘A Wonderful Oceanographic Tool’: The Atomic Bomb, Radioactivity, and the Development of American Oceanography,” in *The Machine in Neptune’s Garden: Historical Perspectives on Technology and the Marine Environment*, ed. Helen M. Rozwadowski and David K. van Keuren (Sagamore Beach, Mass.: Science History Publications, 2004), pp. 93–131; and Jacob Darwin Hamblin, *Poison in the Well: Radioactive Waste in the Oceans at the Dawn of the Nuclear Age* (New Brunswick, N.J.: Rutgers Univ. Press, 2008).

<sup>16</sup> James C. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven, Conn.: Yale Univ. Press, 1998).

As the historian Vera Schwach has suggested, the bottle was part of a “knowledge package” that was tied to courses on methods, theory, and field practice at Bergen.<sup>17</sup> The purpose of a Nansen bottle was not simply to take a measurement; it was to help delineate the boundaries of the ocean’s water masses and to track currents, with an eye to seeing how different masses interacted. Similarly, the Ekman meter (developed by a Swede, Vagn Walfrid Ekman) was designed to measure the direction and speed of currents. Together, these Scandinavian devices, including later adjustments to them, were not merely two instruments among many aboard ship—they were the most fundamental instruments in dynamic oceanography, because they helped make the oceans legible in terms of interacting, circulating water masses.

Not everyone cared about these data, even though collecting them had become standard practice. There were many instruments available aboard ship, all representing different ways of perceiving the oceans. The geophysicist Maurice Ewing once joked that the oceans were sometimes just in the way, a “murky mist” that kept him from seeing the bottom; he was more interested in taking bottom profiles of the mid-ocean ridge.<sup>18</sup> The same could be said of marine geologists wanting to take core samples or of geophysicists trying to track magnetic reversals on the ocean floor. There was a plethora of instruments—Nansen bottles, echo depth sounders, Ekman current meters, Geiger counters, bathythermographs, magnetometers, heat flow gauges, seismometers, hydrophones, gravimeters, trawling dredges, fishing nets, sediment core tubes, and who knows what else—and each potentially represented a radically different way of seeing the oceans.

One difficulty for the historian is to discern whether the standard oceanographic measurements of dynamic oceanography made scientists less able to see alternative views of the oceans. Consider, for example, the collection of radioactivity data in the sea after the Americans began to conduct hydrogen bomb tests in the Pacific region. In response to the controversial 1954 Bravo test, which covered a Japanese fishing boat with radioactive debris, Japanese marine scientists started collecting data. The Americans saw these measurements as only marginally significant. But the Japanese were seeing the Pacific Ocean as a contaminated space, and to them it made clear scientific sense to take routine measurements of radioactivity in order to understand how much the water was being altered by human activities. Some Americans, including Roger Revelle, proposed deliberately dumping radioactive debris into the ocean, to be used as an ocean current tracer: the radioactive debris could help them to see the movement of currents over long distances. Only in that context did the scientific value of taking radioactivity measurements seem obvious.<sup>19</sup>

In the 1950s, as Japanese scientists asked how radioactivity content increased over time, changing the composition of seawater, American oceanographers instead aimed to see where radioactive materials would go, carried by currents. Woods Hole scientist Valentine Worthington had suggested that the overturn of the ocean—the time for deep water to be replaced by sinking surface water—might happen in as little as 150 years, meaning that

<sup>17</sup> Vera Schwach, “At Sea with Science, at Work with Water Samplers: The Importance of Instruments in Oceanography, 1890–1920,” paper presented at the International Congress on History of Science, Technology, and Medicine, Manchester, 27 July 2013.

<sup>18</sup> Maurice Ewing, quoted in Lonny Lippsett, “At Deepwater Horizon, Basic Research Was Applied,” *Oceanus*, 2011, 48(3), <http://www.whoi.edu/oceanus/feature/at-deepwater-horizon-basic-research-was-applied> (accessed 26 Sept. 2013).

<sup>19</sup> Scientists’ views about normal levels of radioactivity in the Pacific oceans are discussed in Hamblin, *Poison in the Well* (cit. n. 15).

contaminated water deposited in the deep sea might (theoretically) rise to the surface while still dangerous. Others disagreed, estimating overturn to take as much as a thousand years.<sup>20</sup> The 1957–1958 International Geophysical Year (IGY) seemed like an ideal time to test Worthington’s ideas, and oceanographers aboard the *Crawford* did so by gathering more salinity measurements—the better to understand general circulation. Woods Hole scientists also measured fission products deposited in the sea from nuclear fallout, and these measurements typically were contextualized as tracer experiments on ocean circulation. They also collected data on carbon-14 content in water samples to reveal the age of seawater in any particular current. The assumption here was that the most relevant oceanographic studies would be of circulation. Only later did biologists challenge this by emphasizing how much the dangerous radioisotopes accumulated in sea life.<sup>21</sup>

Although Roger Revelle would later gain acclaim for his early warnings about climate change, this was not his concern when he first published about carbon dioxide accumulation in the atmosphere. Having helped to design the oceanographic program for American nuclear tests, he was attracted to problems of the ocean’s circulation. His studies of carbon dioxide were rooted in his fascination with radiocarbon (carbon-14), which could be used to judge the age of water, a crucial means of understanding the timescales of ocean circulation. Carbon-14 led Revelle to carbon dioxide, and he wanted to know how long carbon dioxide remained in the atmosphere before dissolving into the ocean. In 1957, Revelle and Hans Suess—an Austrian nuclear chemist—estimated about ten years, and they concluded the paper that described their work with a paragraph warning that the oceans could not continue to absorb carbon dioxide indefinitely.<sup>22</sup>

Although rooted in a desire to understand ocean circulation, the Revelle/Suess paper is typically associated with a different narrative, one focused on global warming and the notion that the chemical properties of the biosphere have been changing over time. This is particularly so because during the IGY Revelle and the atmospheric scientist Harry Wexler saw an opportunity to collect carbon dioxide data using more precise instrumentation (expensive spectrophotometers), and the resultant data set has become the most famous in the history of the earth sciences. By 1960, data collected by Charles Keeling detected a rise in atmospheric carbon dioxide over a short period, and that rise has continued unabated ever since, suggesting a significant change to the atmosphere’s chemical composition. This in itself would spark scientific demand for more continuous data collection to determine the extent of carbon dioxide increase and the possibility of global climate change.<sup>23</sup> However, Revelle had been motivated not by a desire to track change to the earth over time but by scientific questions about circulation, fully entrenched in Bergen thinking. The Revelle/Suess paper had been based on a limited data set, and the IGY was an opportunity to strengthen his and Suess’s studies of the interactions between the atmosphere and oceans. Are these complementary narratives or competing ones? Is this a story about the carbon cycle or about anthropogenic alterations to the earth?

<sup>20</sup> For more on the radioactive waste implications of Worthington’s work see *ibid.*

<sup>21</sup> “South Atlantic IGY Studies,” *Science*, 1957, 125(3249):637–638; and Hamblin, *Poison in the Well*.

<sup>22</sup> Rainger, “Wonderful Oceanographic Tool” (cit. n. 15); and Roger Revelle and Hans Suess, “Carbon Dioxide Exchange between Atmosphere and Ocean and the Question of an Increase of Atmospheric CO<sub>2</sub> during the Past Decades,” *Tellus*, 1957, 9:18–27.

<sup>23</sup> On the Revelle/Suess paper see Spencer R. Weart, “The Discovery of the Risk of Global Warming,” *Physics Today*, 1997, 50:34–40; and Weart, *The Discovery of Global Warming* (Cambridge, Mass.: Harvard Univ. Press, 2003).

## COLLECTING DATA IS NOT SCIENCE

Although the carbon dioxide measurements might be the most impressive unintended consequences of oceanographers' obsession with ocean circulation, others may turn out to be more significant in the long term. Specifically, I refer to the extraordinary accumulation of data over the past century. As in the case of carbon dioxide, much of the data collection started during the IGY, when scientists could draw significantly on military and political interest, along with considerable sums of money, to entice scientists and technicians worldwide to take measurements. One exemplary case was the data on mean sea level. As Henry Stommel noted, "because so many periods and scales are involved simultaneously in the variable sea level, obtaining a significant measurement of the annual variation of sea level requires many more than  $10^3$  measurements." These he expected people all over the world to collect. To arrive at monthly averages, measurements needed to be taken hourly, and to establish annual variation this needed to be done over at least four years. A shorter time frame would be mere description, making any statistically significant calculations impossible. Without these, Stommel later complained to presidential science advisor Jerome Wiesner, oceanography was just "cookbooks and tradition."<sup>24</sup>

These extensive collection efforts had the effect of making oceanography more descriptive than ever, flooding it with data, most of which sat unused. Stommel huffed in disappointment at his colleagues. Although the work on circulation during the IGY was aimed at confirming or rejecting his own idea of deep currents—the world was literally collecting data for him—he was dissatisfied at how closely IGY work resembled the near-anecdotal style of oceanography of the past. Too many scientists were just doing what they were told, collecting data that others assured them were important. There was little coordination about scientific questions. Instead, scientists reserved time aboard ship to collect more limited data sets on subjects dear to them. Stommel regarded IGY oceanography as essentially a collection of independent voyages that promised to share results. Relatively few scientists were standing by, eager to put the data to work, to prove or disprove geophysical theories. Stommel believed the IGY was a good step in the area of international coordination—much needed to accomplish what he saw as modern oceanography—but a step backward because most of its efforts involved old-style expeditions traversing the sea. Stommel guessed that the solution lay in rigorous coordination and intensive, repeated data collection, most likely with unmanned recording stations. "There is no harm in thinking at first about the ocean in various simple ways, to see how satisfactory a model one can devise," Stommel wrote in *Science*, reflecting on the IGY, "but a time comes when consideration of the next stage in complexity can no longer be postponed." He urged that scientists use technology to conduct oceanography "of the new type," to allow theory and observation to advance together.<sup>25</sup>

Stommel's and others' advocacy for more and more data was tempered by their ill feelings for those—often in the Soviet Union—who appeared to believe that collecting data was itself science. The Soviets seemed to be interested primarily in descriptive data that could quickly be reproduced in print. To the amazement of the Americans, for example, the Soviets included printing apparatus on their big ships—for near-instantaneous publication of their "science" as the ships reached ports. This mirrored the practice of the IGY, in

<sup>24</sup> Henry Stommel, "The Varieties of Oceanographic Experience," *Science*, 1963, 139(3555):572–576. On the "cookbooks and tradition" comment see Hamblin, *Oceanographers and the Cold War* (cit. n. 3), p. 235.

<sup>25</sup> Stommel, "Varieties of Oceanographic Experience," p. 575.

which scientists were required to submit results to world data centers, where they would be openly available. In order for these data centers to function, they needed an influx of data. But many were at pains to distance themselves from the notion that data collection was science. The British oceanographer George Deacon barely held back his disgust. Of course the Soviets collected lots of data during the IGY, he noted; their chief scientists were not trained to think creatively.<sup>26</sup>

Western scientists nursed a deep disdain toward their Soviet colleagues who seemed more interested in obtaining endless reams of data than in analyzing what they accumulated. The American Walter Munk described the leading Soviet scientist Vladimir Kort thus: “Oh, big, gruff man who loved to go to sea and drop Nansen bottles. Nothing complicated about him. He loved taking classical Nansen sections, and he would have been happy in his life if he had done nothing else but that.” That was Munk’s impression of Russian oceanography in general: “dull, factory-like kinds of surveying” without much innovation or creativity.<sup>27</sup> To Westerners, it smacked of the old brand of descriptive oceanography. And yet these data were precisely what Westerners had demanded. The unused data became part of a bigger trend in the environmental sciences. Despite dynamic oceanographers’ firm belief that these data were essential for answering fundamental questions in the field, not everyone found them useful or wished to analyze them. A kinder view of Soviet activities would be that the scientists had been told to cooperate, and so they did. They gathered the data that the international community of oceanographers supposedly wanted.

Despite Western suspicions, endless reams of data were not the goal of everyone in the Soviet Union. Soviet scientists at the Shirshov Institute of Oceanology also wished to transform their discipline from a descriptive science to a mathematical and predictive one. Konstantin Fedorov, in particular, later praised his mentor Vladimir B. Shtokman for attempting this transformation, and he was fond of telling students to ask questions of the ocean rather than just measure it—a directive that he may have heard from Revelle (whom Fedorov admired). Fedorov had worked alongside Stommel aboard *Atlantis II* in 1965, during the International Indian Ocean Expedition, and he got the distinguished American oceanographer’s message loud and clear: collecting data was not enough. Fedorov and Stommel conducted fundamental work on thermohaline finestructure—the stratification of the sea into small “structures,” layers, or eddies, phenomena previously masked by the use of instruments whose different scale of measurement suggested uniform mixing. After the expedition Fedorov went home and began to use the first Soviet-manufactured salinity-temperature-density probes. The navies in both countries were deeply interested in finestructure because of the effects on sonar. After the American–Soviet cooperation Fedorov was instrumental in turning the topic into a major research agenda in the Soviet Union.<sup>28</sup>

The enormous amount of data collected in the 1950s only increased in the 1960s and beyond, with the emergence of satellites as oceanographic tools. Such tools would certainly help to meet demands for a constant flow of oceanic data, but there was no guarantee that anyone would be interested in looking at those data. Many did use the data,

<sup>26</sup> On Deacon’s views see Hamblin, *Oceanographers and the Cold War* (cit. n. 3), pp. 182–184.

<sup>27</sup> Walter Munk oral history interview, conducted by Ronald E. Doel, 16 Sept. 1997, American Institute of Physics, College Park, Maryland, p. 92.

<sup>28</sup> H. Stommel and K. N. Fedorov, “Small-Scale Structure in Temperature and Salinity near Timor and Mindanao,” *Tellus*, 1967, 19:306–325; and Fedorov, *The Thermohaline Finestructure of the Ocean* (Oxford: Pergamon, 1978).

but, as the historian Erik Conway has argued, satellite oceanography marked the beginning of a new period in which producers of data worked separately from the users. Getting these “users” to “use” was no small task. Scientists and engineers at NASA’s Jet Propulsion Laboratory designed instruments to take vast numbers of measurements and developed data-processing methods to render them comprehensible. They even made the data publicly available, to promote their use beyond a few oceanographic institutions in the United States, many of which were reluctant to make the switch from conventional (and less data-intensive) ship-borne expeditions.<sup>29</sup> Part of that reluctance may have been scientists’ desire to design their own research questions rather than have them dictated by the data.

The triumphs of the dynamic approach—namely, the successful efforts to instigate massive data collection in the mid-twentieth century—had the incidental effect of providing half a century of observational data that no previous generation could have dreamed of possessing. But who will use these data, and to what end? Paul Edwards reminds us that “data friction” should be expected when data have been collected in different historical or regional contexts and that making such data commensurable is one of the greatest challenges faced by scientists. It is hard to imagine scientists sifting through a century’s worth of changes in nomenclature, appreciating the historical context while trying to comprehend the array of now-obsolete technical data systems. But perhaps the desire to document changing climate will be strong enough for them to try. Be skeptical! As the earth scientist Athelstan Spilhaus once confessed about his experiences chasing grant money, “oceanography is not a discipline; it’s a place. . . . Oh, we cheated all the way through that. Any smart guy did that. Later on, they used to call everything ecology. It makes me laugh.”<sup>30</sup> Such a view will be recognizable to any academic who has sought financial support. Given the long-standing influence of Bergen values, dismissive of descriptive studies, is it likely that oceanographers have suddenly become historically minded?

Perhaps historians will be able to decipher the data and use them to document significant changes to the ocean’s physical, chemical, and biological properties. Environmental historians look for reliable data sets to show changes in species distribution over time, to track the long-term ebb and flow of water in coastal areas, or to demonstrate ecological differences from one decade, or century, to the next.<sup>31</sup> But historians may be reluctant to work with scientists in this endeavor, especially if historical goals are subordinated to mathematical model seeking. They may find themselves treated as “data serfs” working for “model lords,” as Lance van Sittert put it so memorably when reflecting on his experience with the History of Marine Animal Populations (HMAP), a collaborative project between humanists and scientists. HMAP, part of the Census of Marine Life, has provoked serious questions about whether historians and scientists are even pursuing similar goals.<sup>32</sup> Yet as Katharine Anderson has pointed out, the purported tensions

<sup>29</sup> Erik M. Conway, “Drowning in Data: Satellite Oceanography and Information Overload in the Earth Sciences,” *Hist. Stud. Phys. Biol. Sci.*, 2006, 37:127–151.

<sup>30</sup> Edwards, *Vast Machine* (cit. n. 4); and Athelstan Spilhaus oral history interview, conducted by Ronald E. Doel, 10 Nov. 1989, American Institute of Physics, p. 68.

<sup>31</sup> Joseph E. Taylor III, “Knowing the Black Box: Methodological Challenges in Marine Environmental History,” *Environmental History*, 2013, 18:60–75. For an exploration of ecological changes in an earlier period see Matthew McKenzie, *Clearing the Coastline: The Nineteenth-Century Ecological and Cultural Transformation of Cape Cod* (Lebanon, N.H.: Univ. Press New England, 2010).

<sup>32</sup> Lance van Sittert, “The Other Seven Tenths,” *Environ. Hist.*, 2005, 10:106–109. See also the fascinating

between historians and scientists in HMAP might just as easily be framed as tensions between historically minded and model-minded scientists.<sup>33</sup> Sittert's complaint does sound like the tension between descriptive and dynamic oceanographers: some wished to document change over time, while others sought sufficient data to bolster models of oceanic systems and to identify repeatable, predictable relationships.

These divergent priorities should make us uneasy with narratives that place oceanographers, imbued with Bergen values, at the forefront of environmental thinking, as if they championed studies designed to reveal anthropogenic changes to the earth over time. Although some of their work turned out to be crucial for studies of environmental change, dynamic oceanographers spent decades deriding descriptive goals as insufficiently ambitious, and their perspectives shaped how they made the oceans legible through specific instruments, kinds of data, and models of circulation. Just as scientists and historians must be mindful of context when cobbling together significance for all these data—left over from activities designed to do something else, paid for by an array of patrons—historians of science should consider how scientific values among oceanographers shaped how they saw the oceans. Joining the narrative of the triumph of mathematical, dynamic oceanography with the environmental narrative may be possible, but it often feels like trying to look through the ocean itself, the “murky mist” that kept Maurice Ewing from seeing what he wanted to see.

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and divisive discussion of the role of historians collaborating with scientists to manage and analyze marine data in the multiple-essay “Marine Forum,” *ibid.*, 2013, 18(1).

<sup>33</sup> Katharine Anderson, “Does History Count?” *Endeavour*, 2006, 30:150–155. Anderson in particular invokes Sharon Kingsland's work, which shows the tensions between historical and model-minded scientists in the history of ecology. See Kingsland, *Modeling Nature* (cit. n. 4).