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Roger Revelle’s Discovery

*Before scientists would take greenhouse effect warming seriously, they had to get past a counter-argument of long standing. It seemed certain that the immense mass of the oceans would quickly absorb whatever excess carbon dioxide might come from human activities. Roger Revelle discovered that the peculiar chemistry of sea water prevents that from happening. His 1957 paper with Hans Suess is now widely regarded as the opening shot in the global warming debates. This essay not only describes Revelle’s discovery in detail, but serves as an extended example of how research found essential material support and intellectual stimulus in the context of the Cold War.¹ (For another case study of funding see the essay on funding Keeling’s measurements. For general observations see my *Reflections on the Scientific Process*, as seen in *climate studies*.)*

In the mid 1950s, not many scientists were concerned that humanity was adding carbon dioxide gas (CO₂) to the atmosphere by burning fossil fuels. The suggestion that this would change the climate had been abandoned decades earlier by nearly everyone. A particularly simple and powerful argument was that the added gas would not linger in the air. Most of the CO₂ on the surface of the planet was not in the tenuous atmosphere, but dissolved in the huge mass of water in the oceans. Obviously, no matter how much more gas human activities might pour into the atmosphere, nearly all of it would wind up safely buried in the ocean depths.

There was a hidden flaw in this argument. It was discovered as an indirect consequence of work at the University of Chicago by Willard Libby and collaborators, scientists who had given the question scarcely a thought. In the mid 1950s, they were busy with an exciting new technique—the use of radioactive carbon-14 to find the age of ancient materials. The origins and motives of this technique were diverse, and so therefore was its support. The Chicago group was funded chiefly by the University and by a foundation dedicated to supporting anthropology and archeology. A crucial auxiliary technique, the enrichment of carbon isotopes, was developed because the isotopes were useful for medical research. But as was usually the case in nearly every field of postwar science, a substantial part of the support was indirectly related to the Cold War. Libby acknowledged that he was “also indebted to the Air Force... for a contract for the development of low-level counting techniques during 1949...” The Air Force had negligible interest in the dates of Egyptian mummies, but it had a hearty concern for delicate radioactivity measurements (one likely use was detecting residues from Soviet nuclear bomb tests). As Libby discreetly remarked, Chicago people built up expertise in measuring radioactivity “in other connections.” The separation of isotopes was likewise under investigation for military as well as

¹ This essay is copied with minor modifications, mainly stylistic, from Weart (1997).

civilian uses. Thus carbon-14 research could incorporate more highly developed techniques than would have been available for archeology alone.¹

The new techniques were co-opted for CO₂ research by Hans Suess, a chemist who left the Chicago group for a research post at the U.S. Geological Survey. He devised a plan of measuring carbon isotopes in the annual rings of old trees, which he collected from various regions with the help of staff in the National Park Service and the Department of Agriculture. This was science in a traditional vein, funded by government agencies with little idea how it could have practical use for climate studies, a simple pursuit of knowledge. Suess and his colleagues simply wanted to find how carbon cycled through the atmosphere. Their aim was partly to satisfy general geochemical curiosity, but mainly to use the knowledge to make the carbon-14 dating technique more reliable.²

In 1955 Suess detected a “contamination” of recent wood by stable carbon. This carbon had to come from the burning of fossil fuels. For carbon-14 is continually renewed in the atmosphere as cosmic rays strike nitrogen atoms and convert them to the isotope, but in ancient coal and oil, the radioactive carbon-14 has all decayed away. Although Suess’s measurements were very preliminary, his report pointed to a new research opportunity. Just as he had found how CO₂ was taken up in old trees, one might now measure how it was taken up by the oceans. As far back as 1951, Libby had speculated that study of dissolved carbon “might shed some light on the problem of circulation in the deep basins.”³

Ocean uptake could be split into two factors—the chemistry of CO₂ absorption in surface water, and the mixing of this water into the whole volume of the oceans. The chemical reactions, at least, seemed to be well known. Observations in the 1930s had established the key data (such as how the partial pressure of CO₂ in sea water varied as a function of acidity).⁴ Yet while the chemical reactions were “known,” the system had subtleties that confused everyone who attempted calculations. As one scientist reported in 1957, “Recent estimates of the residence time of a molecule of CO₂ in the atmosphere, before entering the sea, range from 16 hours to the order of 1,000 years.”⁵

¹ Libby (1967); Libby (1955), quotes from v, 8, on isotope separation, see Aristid V. Grosse, interview by J.J. Frmenc, 1970, AIP, pp. 74-75.

² James Anderson, interview by Spencer Wearth, June 1996, AIP.

³ Suess (1955); the ocean uptake question was raised especially by measurements of carbon-14 in sea shells. See papers by Suess and Craig in Craig (1957b), pp. 50-92, see pp. 50-52, 54 (copy filed as Doc. 1 C1528 W3142 No. 8, SIO); Libby (1951), p. 26.

⁴ Although often overlooked, the data (collected by Kurt Buch) were widely available in Sverdrup et al. (1942), pp. 189-211; Harvey (1955), ch. 10, see also 5.

⁵ Craig (1957a), p. 2; for the large number (“the surface waters can absorb only a small fraction of the extra CO₂ in a period of several hundred years”), see Plass (1956a), p. 149; the small number was from Dingle (1954), who used an 1899 measurement of water mixed with ordinary salt, which behaves very differently from the more chemically complicated sea water.

Ignorance was nearly as great for the mixing of ocean waters. The best guess in the early 1950s was that it took centuries for the oceans to turn over, but everyone knew this was only a guess.¹ A few carbon-14 measurements suggested that the water at the bottom might be two thousand years old, and some thought that “fossil water” could linger there even longer. In 1954, a Chicago scientist, Harmon Craig, found a carbon-14 age of several centuries for the surface waters, which suggested a bit faster mixing, although still quite sluggish. Another scientist, however, suggested that the North Atlantic might be capable of turning over in as little as a decade. In 1955, an oceanographer admitted, “nobody knows whether it takes a hundred years or ten thousand.”²

This oceanographer was Roger Revelle, a dynamo of a scientist and administrator who was orchestrating a rapid expansion of the Scripps Institution of Oceanography in California. Sitting on a dramatic cliff overlooking the Pacific, the prewar Scripps had been a typical oceanographic establishment, quiet and isolated with its small clique of a dozen or so gossiping researchers and a single ship. It relied on private patronage, which faltered when the Depression bit into the Scripps family’s funds. The postwar Scripps was growing into something quite different, a complex of modern laboratories. Under Revelle the Institution won increased long-term support from the University of California, and on this foundation piled funds for research projects from a variety of other sources. Chief among these was the U.S. Navy’s Office of Naval Research (ONR), the natural patron for any research related to the oceans. Revelle had built up contacts and personal prestige during wartime work for the Navy on sonar detection of submarines and other urgent problems. Now with ONR support Revelle enlarged Scripps’s prewar interests—fisheries and related traditional oceanographic topics—with advanced research in many other directions, above all geophysics.³

One of Revelle’s many good ideas was to use part of the money to hire Suess and Craig to pursue carbon-14 studies. By December 1955 Revelle and Suess had joined forces, combining their separate areas of expertise to study carbon-14 in the oceans.⁴ Revelle habitually collaborated with anybody who came near him. He kept a large number of puzzles simmering on the back burner, a stew of ideas available for research. Among these ideas, postwar events had brought to the fore of Revelle’s thinking a few problems that would happen to be crucial for understanding greenhouse effect warming.

¹ E.g., Revelle and Maxwell (1952).

² Kulp et al. (1951), p. 116; Kulp (1953); Craig (1954), pp. 138-39; “fossil water” (from glacial age lingering until ca. 1000 years ago followed by rapid “renewal”): Worthington (1954); “nobody knows:” Roger Revelle, “The oceans and the Earth,” talk at American Association for the Advancement of Sciences symposium, Dec. 27, 1955, typescript, folder 66, Box 28, Revelle Papers MC6, SIO. For Kulp’s 10,000 years, see Broecker (2000), p. 5.

³ Rainger (2000b).

⁴ Suess to Revelle, January 31, 1955, and Craig to Revelle, copy to Suess, March 15, 1955, folder 24, box 20, Hans Suess Papers mss. 199, Mandeville Dept. of Special Collections, Geisel Library, University of California San Diego, La Jolla, CA. Joined forces: Suess to James R. Arnold, 5 December [1955], folder 17, box 2, Suess Papers..

First, sea water chemistry. At the outset of his career in the early 1930s, Revelle had become one of the world's few experts on how carbon and calcium compounds and other molecules interact in sea water. He had learned that sea water is a highly complex soup. Its acidity is determined not only by the ingredients most obvious to chemists, like carbonate ions derived from CO₂, but also by much else, even the obscure element boron. The concentrations of various types of ions are set through intricate chains of processes, far from intuitively obvious, which scientists understood only in a limited and confused way into the 1950s and well beyond.¹ Revelle moved on to other questions.

The testing of nuclear weapons at Bikini atoll in 1946 brought him back to ocean chemistry. With a long history of close relations with the U.S. Navy, he was the natural choice to lead a preparatory study team. In an "Oceanographic Section" under Commander Revelle (he had taken the naval rank during the war), 80 scientists and aides went to Bikini. They were mainly concerned with the bomb's potential for launching tsunamis or destroying fisheries. But on the side, Revelle assigned a few scientists to study the natural chemical activity in the atoll's waters.² From this point into the mid 1950s, many pages of calculations in Revelle's unpublished papers show him working intermittently (as usual in collaboration with others) on drafts of papers on how carbonates are dissolved and precipitated in tidal pools. There were puzzles involving CO₂ that he could not manage to resolve.³

Second, ocean mixing. Here too, Revelle had a long-established curiosity, and here too, nuclear energy pushed the topic forward. The wastes from nuclear reactors had to be disposed of somewhere, and the ocean floor seemed a likely choice. In 1955, when Revelle spoke of studying ocean circulation, he emphasized the need to bury the "unbelievable quantities of radioactive substances" expected to pour from civilian reactors.⁴ At that point, of course, most of the extant radioactive wastes had been produced by military rather than civilian reactors. Stretching the point, another oceanographer would later include studies of ocean circulation and waste disposal in a list of "Applications of Marine Geophysics to Military Problems."⁵

¹ Revelle (1987), p. 6; Dickson (1992).

² Rainger (2000a); "History of Oceanographic Section JTF-1," 11 May 1946, with cover memo by J.B. Dow, 27 May, and other papers, folder 29, box 6, SIO Subject Files AC6.81-16, SIO..

³ See papers in folder 34, box 153, Roger Revelle Papers MC6A, and folder 63, Box 28 and folder 3, box 155, Revelle Papers MC6, SIO. The end result was Revelle and Fairbridge (1957).

⁴ Revelle also mentioned the importance of ocean currents for long-range weather prediction and for the movement of nutrients affecting the fertility of the seas. Revelle, "The oceans and the Earth," cited above. An early paper involving ocean mixing was Revelle and Maxwell (1952).

⁵ Maurice Ewing, speech (probably to BuShips), 1964, folder "Applications of marine geophysics to military problems 1964," Maurice Ewing Collection, Center for American History, University of Texas at Austin.

There was a more immediate problem. In 1954, fallout from an American thermonuclear test injured the crew of a Japanese fishing vessel. The entire Japanese nation became panicky about the safety of eating fish from the Pacific Ocean. Besieged by public anxieties, the U.S. Atomic Energy Commission (AEC) stepped up its program of research on where fallout ends up in the environment. Revelle became involved in the problem as chair of a National Academy of Sciences committee that was assigned to study the effects of radioactive material on fisheries. Revelle himself was interested chiefly in the disposal of wastes. But he was also in touch with Libby, who was now at the AEC and heading its study of fallout, including the contamination of surface waters by bomb tests. Research on ocean mixing had become a topic of international importance.¹

Meanwhile Revelle was studying (as usual with collaborators under an ONR contract) the results of a May 1955 test of a nuclear “depth bomb” that the Navy and AEC had exploded a few thousand feet underwater. His team found that the radioactive residues had spread out in remarkably thin sheets, stretching a hundred square kilometers but only a meter thick. Many years later Revelle recalled his surprise that “the water from one layer doesn’t exchange with the water from another layer.” It was one more example of how the huge resources of government nuclear programs made novel observational methods and data available for many kinds of research. In a 1955 report using the depth bomb test data, Revelle’s group concluded that “radioactive wastes introduced into the upper layer might remain there for many years, and would be diluted by a volume of water only a fiftieth to a hundredth the volume of the ocean.”² The same would apply to almost anything else introduced into the upper layer—including CO₂, a compound for which the Navy and AEC had no interest whatsoever.

Revelle, however, was getting personally interested in the gas. He had heard the lonely voices of G.S. Callendar and Gilbert Plass, who were arguing that if CO₂ ever did accumulate in the atmosphere, it would have a strong greenhouse warming effect. Not long after Revelle took up the problem, he began to say publicly that it was possible there could be great and perhaps harmful effects as early as the end of the century.³ Meanwhile he and Suess began to apply

¹ Libby to Revelle, 14 June 1956, June-August 1956 correspondence, box 2, Revelle Papers AC16, SIO. Research under Revelle’s direction on the “fate of radioactive materials introduced into the sea” is reported in Scripps Institution of Oceanography (R. Revelle), “ONR Progress Report No. 40,” 6 July 1956, copy in box 36, folder 14, James Arnold Papers MSS 112, Geisel Library, University of California, San Diego..

² Revelle, “Preparation for a scientific career” (1988), based on an interview by Sarah L. Sharp, 1984, University of California Regional Oral History Office, Berkeley, CA (copies at Bowling Green State University, Ohio, and SIO), 12a, 52. R. Revelle, T.R. Folsom, E.D. Goldberg, and J.D. Isaacs, “Nuclear science and oceanography.” United Nations International Conference on the Peaceful Uses of Atomic Energy, Geneva, 1955, Paper A/CONF.8/P/277, copy in folder 19, box 154, Revelle Papers MC6A, SIO, see 5, 13..

³ Lloyd Norman for Chicago Tribune Press Service, as seen in *Washington Post*, 19 March, 1956.

carbon-14 methods to the question of how the oceans take up CO₂. The opportunity for such research was plain to other scientists too. Craig, in close touch with Revelle and Suess, undertook similar calculations with some variations in the equations and assumptions. Independently, James Arnold of Princeton in collaboration with Ernest Anderson of the Los Alamos laboratory developed another variant of the research. They were following up their recent interest in tracking radioactive materials such as bomb fallout. Arnold and Anderson were soon exchanging ideas with Craig, Revelle, and Suess, and all agreed to publish papers simultaneously.¹

Revelle, Suess, and Craig all acknowledged funding as usual “under contract with the Office of Naval Research.” This was a general contract to Scripps, covering a number of research projects, driven partly by various potential applications and partly by pure scientific curiosity. For example, in his July 1956 progress report to the ONR, Revelle mentioned the CO₂ studies in connection with measuring ocean turnover. He went on to report that Craig had spent most of his time that spring working on the question of ocean disposal of radioactive wastes.²

Anderson, at Los Alamos, was supported by the AEC. He could justify travel for discussions with Craig and Revelle, he wrote, “because of their interest in the waste disposal problem... This is, of course, for Los Alamos’ benefit.”³ His collaborator Arnold drew support from a more obscure source, the U.S. Army’s Office of Ordnance Research. The Army officers were willing to fund research on abstract nuclear theory and geochemistry, and neither Arnold’s proposal nor his reports hinted at any link to ordnance. “If I wrote an open proposal,” Arnold recalled, “and the Department of Defense chose to support it, and didn’t make me get a Q [top secret] clearance and work with them and whatever, it was fine with me.”⁴

The studies by Arnold and Anderson, Craig, and Revelle, and Suess all used much the same research plans and data, and came to much the same conclusions. As Revelle and Suess put it, “the average lifetime of a CO₂ molecule in the atmosphere before it is dissolved into the sea is of the order of 10 years.”⁵ Further, all three studies used carbon-14 data to estimate that the oceans

¹ The ocean “mixing problem” first appears in their correspondence in letters Anderson to Arnold, 3 October 1955, and reply, 18 October 1955, box 1, folder 11, Arnold Papers. For Arnold’s established interests in fallout ca. 1955, see e.g., exchanges with Edward Martell, box 7, folder 2, and with Ralph Lapp, box 13, folder 10..

² Scripps Institution of Oceanography (R. Revelle), “ONR Progress Report No. 40,” 6 July 1956, copy in box 36, folder 14, Arnold Papers. See Craig, “Disposal of radioactive wastes in the ocean,” in National Research Council (1957), 34-42.

³ Anderson to Arnold, 14 December 1956, box 1, folder 11, Arnold Papers..

⁴ Arnold, interview by Weart, June 1996. University of Chicago (James Arnold), “Studies of natural and induced radioactivity. Proposal to the Office of Ordnance Research for renewal of contract Da-11-022-ORD-1248,” July 1954, box 12, folder 7, Arnold Papers. See also Arnold’s report, box 12, folder 7..

⁵ Revelle and Suess (1957), p. 18; Arnold and Anderson found “one or two decades” for CO₂ lifetime, Arnold and Anderson (1957), p. 28; Craig’s value was 4-10 years, Craig (1957a), p.

turned over completely in several hundred years. Given the leisurely time scale that most people assumed for the future growth of industrial emissions, that seemed fast enough to swallow up extra CO₂.

Yet there remained Callendar's insistent claim that measurements of CO₂ in the atmosphere showed a marked increase. If he was right, one way out for Revelle and his colleagues would be to assume they had not taken proper account of how forests and other biomass took up carbon. The magnitude of that process was so poorly known that it would allow fudging the numbers. "I am not too happy about the whole thing," Suess remarked.¹

The draft calculations by the three teams had all addressed mainly the steady-state rates of exchange between atmosphere and ocean. But the crucial question for global warming was a transient effect, the net flux of new CO₂ into the water. And as Revelle knew from the carbonate chemistry problems he had been rethinking since the Bikini atoll studies of 1946, sea water is hypersensitive to change. To match an increased level of CO₂ in the atmosphere, the number of CO₂ molecules in the water would rise only as one factor in a cascading readjustment of the proportions of many types of molecules. In technical terms, sea water is a "buffered" solution, resisting the change in acidity that an increase of carbonates would involve. When some CO₂ molecules were absorbed, their presence would alter the balance through a chain of reactions, and in the end some CO₂ molecules would be expelled back into the atmosphere. To reach the new equilibrium, Revelle now calculated, the water needed to absorb only about a tenth as much gas as a simple-minded calculation would suppose. While it was true that most of the CO₂ molecules added to the atmosphere would wind up in the oceans within a few years, most of these molecules (or others already in the oceans) would promptly be evaporated out.

Revelle could scarcely present that as a new discovery, for the reactions were all tabulated in data known since the 1930s.² But nobody had worked out this particular implication of the data. Greenhouse warming had seemed to nearly all scientists a subject of no practical significance. The few researchers who had looked into it had been interested in the oceans mainly because sea water would absorb CO₂ on cooling and evaporate it on warming, which might help explain the famous puzzle of the ice ages. People had accordingly calculated how the CO₂ content of water changed with temperature. They had seen no reason to take up the problem, which turned out to be quite different, of calculating how much new CO₂ the sea water could absorb at a given temperature.

Even after Revelle made the calculation, its implications were hard to grasp. When Arnold, visiting Scripps in January 1956, wrote about it to Anderson, his collaborator was skeptical of

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¹ Arnold to Suess, 8 January 1956, and reply, 9 February 1956, folder 17, box 2, Suess Papers. The possibilities were discussed by Revelle and Suess (1957), pp. 25-26; also Arnold and Anderson (1957), p. 31, but Craig was explicitly concerned only with the steady state.

² Harvey (1955), ch. 10.

“Revelle’s idea that 80% of the CO₂ added to the atmosphere will stay there” (he pointed to the uncertainties in biomass uptake). Craig too found much uncertainty in such processes, and Revelle’s insight had scant influence on the other two papers when they were published.¹ Even in Revelle’s own paper with Suess, the bulk of the text reflected the pair’s original belief that the oceans were absorbing most of the new CO₂. The brief paragraph on the factor of ten stood apart like an isolated thought. In the archives it is visibly an addition, Scotch-taped onto the original draft.²

Revelle did revise a curve in the paper that he had calculated for the future of atmospheric CO₂, finding now that the concentration of the gas should be rising after all. But in this calculation he assumed that industry would emit the gas in future at the same rate as at that time. Few people yet recognized that population and industrialization were shooting up exponentially. So Revelle predicted CO₂ would level off, a few centuries hence, with a total increase of 40% or less.

Different ideas were beginning to stir. The geochemist Harrison Brown in particular was sketching out a more realistic vision of a future with exploding population and industrialization. In 1953, he made what seemed like a radical prediction that world population could reach 6 billion by 2050 (it actually reached that before 2000).³ Revelle had heard these ideas, and on reflection he understood that industrial emissions were not really constant. Before he sent off the paper he had written with Suess for publication, he added a remark that the accumulation of gas “may become significant during future decades if industrial fuel combustion continues to rise exponentially.”

By way of conclusion, Revelle remarked that “Human beings are now carrying out a large scale geophysical experiment of a kind that could not have happened in the past nor be reproduced in the future.” When he wrote this sentence, which has since been quoted more than any other statement in the history of global warming, he was not warning against future perils. He did feel some concern about potential harm over the long run, and had begun to point to the problem in public. But the word “experiment” sounded benign and progressive to Revelle as to most scientists, and in this paper he only meant to point out a fascinating opportunity for the study of geophysical processes. People’s attitude toward the rise of CO₂, he would write in 1966, “should probably contain more curiosity than apprehension.”⁴

¹ Arnold to Anderson, 8 January 1958, and reply Anderson to Arnold, 16 January 1956, box 1 folder 11, Arnold Papers.

² The incongruity of the paragraph had already been clear to me on repeated readings of the published paper, but it was gratifying confirmation to find the paragraph (and the caption for figure 2) typed on a different kind of paper and taped onto the earlier version: copy of Revelle-Suess submission, 28 August 1956, folder 63, box 28, Revelle Papers MC6, SIO.

³ Brown (1954), preface dated April 1953, p. 99, see also p. 167 on China.

⁴ The “experiment” sentence is undoubtedly Revelle’s, corrected in his hand on the typescript, where it originally began, “Society is now...” Revelle and Suess (1957), pp. 18, 19, 20, 26. Already in early 1956 Revelle had said, “From the standpoint of meteorologists and

Revelle did not make much of his discovery in this 1957 paper, which described it only in passing and obscurely. His main reason for writing the paper was probably to show that the subject deserved attention. His conclusion pointed to the next step — “An opportunity exists during the International Geophysical Year to obtain much of the necessary information.” A historian who looked into the matter judged that “this famous paper was basically a grant proposal.” Revelle soon did pull in some of the funds allocated for the International Geophysical Year so that he could pursue CO₂ measurements.¹ (See the essay on *Money for Keeling*.)

Another two years passed before Bert Bolin and Erik Eriksson explained the sea water buffering mechanism in clear terms, and emphasized what it meant. Unlike Revelle, they figured industrial production would indeed climb exponentially, and they calculated that atmospheric CO₂ would probably rise 25% by the end of the century. Now the small community of geophysicists began to grasp that they could not rely upon the oceans to absorb all the emissions of fossil fuels.²

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oceanographers we are carrying out a tremendous geophysical experiment of a kind that could not have happened in the past or be reproduced in the future.” Testimony of Roger Revelle, US Congress, House 84 H1526-5, Committee on Appropriations, Hearings on Second Supplemental Appropriation Bill (1956), p. 467. Revelle also described the rise of CO₂ as an “enormous experiment” to a March 1956 study group, cited by Rainger (2004). “More curiosity” (hedged by noting that even a small temperature rise could bring “more serious changes” such as a shift of rainfall patterns): Revelle (1966), p. 41; on Revelle’s views at the time, see also Weiner (1990), p. 58.

¹ Revelle and Suess (1957), p. 26; Bowen (2005) p. 110-11.

² Bolin and Eriksson (1959).