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Archaeology enters the 'atomic age': a short history of radiocarbon, 1946–1960

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Abstract. Today, the most powerful research technique available for assigning chronometric age to human cultural objects is radiocarbon dating. Developed in the United States in the late 1940s by an alumnus of the Manhattan Project, radiocarbon dating measures the decay of the radioactive isotope carbon-14 (C^{14}) in organic material, and calculates the time elapsed since the materials were removed from the life cycle. This paper traces the interdisciplinary collaboration between archaeology and radiochemistry that led to the successful development of radiocarbon dating in the early 1950s, following the movement of people and ideas from Willard Libby's Chicago radiocarbon laboratory to museums, universities and government labs in the United States, Australia, Denmark and New Zealand. I show how radiocarbon research built on existing technologies and networks in atomic chemistry and physics but was deeply shaped by its original private philanthropic funders and archaeologist users, and ultimately remained to the side of many contemporaneous Cold War scientific and military projects.

Introduction

On the evening of 9 January 1948, Willard F. Libby gave a talk on 'Archaeological ages by natural radiocarbon' at an informal supper conference for anthropologists at the headquarters of the Viking Fund (soon to be renamed the Wenner-Gren Foundation for Anthropological Research) in New York City.[1] Libby, unusually for the venue, was neither an anthropologist nor an archaeologist. Instead, he was a chemist with a PhD from Berkeley who specialized in the study of low-activity radioisotopes – an expertise he had put to use as a member of the Manhattan Project, before moving to the Department of Chemistry and the Institute for Nuclear Studies at the University of Chicago after the war.

Libby began by describing the process of cosmic-ray bombardment that produced neutrons in the upper reaches of the Earth's atmosphere, which in turn would react with atmospheric nitrogen to form the radioactive isotope carbon-14, also called

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1 W.F. Libby, 'Abstract of paper Archaeological Ages by Natural Radiocarbon Content', 9 January 1948, Frederick Johnson papers (subsequently FJ), Box 2, UCLA Library and Special Collections, Los Angeles.

radiocarbon.[2] Since the rates of neutron production had been measured by physicists, the amount of radiocarbon on Earth could be calculated, assuming all the neutrons formed radiocarbon and that the production and disintegration of radiocarbon was in a stable equilibrium. If the specific radioactivity of carbon was the same everywhere in the world, as Libby believed it was, and the rates of carbon-14 production had remained more or less consistent over the past fifty thousand years, it 'seems extremely likely that the measurement of the specific activity of buried materials will indeed give the elapsed time since the body was removed from the life cycle, i.e. since the time of death'.[3] Direct measurements without isotopic enrichment could be expected to give values back to ten thousand years. With further sample enrichment, it was possible that radiocarbon could be detected and used to date organic materials back to perhaps forty thousand years. Libby's audience of archaeologists and anthropologists was thrilled: world prehistory could have an absolute timeline. The Viking Fund capped the evening by announcing they had already pledged $18,000 in support for the project.[4]

Today, radiocarbon dating remains the most powerful research technique available for assigning absolute chronometric ages to human cultural objects and other organic materials, with an upper limit of sixty thousand years before the present.[5] Carbon dating labs can be found worldwide, usually in universities or government laboratories, and other researchers or interested members of the public can often have rare materials dated for a fee. In addition to the obvious dating applications, the study of radioactive carbon has been highly useful for understanding global atmospheric circulation and the exchange of carbon dioxide between the ocean and the atmosphere, and for identifying the early impact of anthropogenic climate change.[6] From its beginnings in the United States, radiocarbon dating laboratories were established throughout the world in the 1950s, with more than thirty labs established by 1960; a specialized journal, *Radiocarbon*, began publication in 1959.[7] For developing the radiocarbon dating technique, and in recognition of its indispensable contributions to 'archaeology, geology, geophysics, and other sciences', Willard Libby was awarded the Nobel Prize in Chemistry in 1960.[8]

Given these temporal, material and scientific characteristics – the 1950s, radioisotopes, a Manhattan Project alumnus and the wider applicability of low-activity

2 In equation form: ^{14}N + neutron → ^{14}C + proton.

3 Libby, op. cit. (1).

4 Greg Marlowe, 'Year One: radiocarbon dating and American archaeology, 1947–1948', *American Antiquity* (1999) 64, pp. 9–32. Marlowe points out (pp. 20–24) that this was essentially a pro forma announcement; the funds had already been distributed to Libby three months previously.

5 R.E. Taylor and Ofer Bar-Yosef, *Radiocarbon Dating: An Archaeological Perspective*, 2nd edn, Walnut Creek, CA: Left Coast Press, 2014.

6 Roger Revelle and Hans Suess, 'Carbon dioxide exchange between atmosphere and ocean and the question of an increase of atmospheric CO_2 during the past decades', *Tellus* (1957) 9, pp. 18–27. This work was partly funded by the Office of Naval Research; see Jon Agar, *Science in the Twentieth Century and Beyond*, Cambridge: Polity Press, 2012, pp. 398–399.

7 'Revised list of laboratories, 1960', *American Journal of Science Radiocarbon Supplement* (1960) 2, pp. 224–228.

8 Arne Westgren, 'Award ceremony speech for W.F. Libby', 10 December 1960, Nobel Media AB, 2020, at nobelprize.org/prizes/chemistry/1960/ceremony-speech.

radioactivity measurement – it would be reasonable to expect that the story of the development of radiocarbon dating should fit into or bridge multiple literatures in the history of Cold War science: the history of the growth of the earth and environmental sciences in the Cold War, the history of military investment in the sciences of long-range and low-activity radioactive surveillance, or possibly the history of the production and managed circulation of radioisotopes in medicine and biology.[9] Radiocarbon dating grew out of a long tradition of low-activity radiation studies in interwar atomic chemistry and physics, and after the war members of this professional community became the first heads of new radiocarbon laboratories outside the United States. Radiocarbon dating is fundamentally a problem about detecting and measuring low-level radioactivity, a problem that is also applicable to long-distance military surveillance of other nuclear powers.[10] Building the apparatus for dating samples by the radiocarbon method was made significantly easier by the expansion of the post-war electronics industry and the wider availability of atomic counters for experimentation.[11] Researchers at radiocarbon dating laboratories in the mid-1950s also realized that they could detect bomb blasts – specifically multi-megaton thermonuclear bomb blasts, which added so many extra neutrons to the stratosphere that world radiocarbon levels increased by 10 per cent over the 1950s.[12] On both sides of the Iron Curtain, the activist physicists Linus Pauling and Andrei Sakharov published papers in 1958 against the continuation of thermonuclear weapons testing and specifically highlighting the global increase in radiocarbon as an area of pressing international public-health concern.[13]

However, despite these many points of contact, radiocarbon dating research did not follow the typical Cold War geosciences path, and this article explains why. I argue that there were three main reasons for this divergence. First, the initial research funding for the development of radiocarbon dating as a research field came from a private entity, the Wenner-Gren Foundation, and not from military or governmental

9 See Paul Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming*, Cambridge, MA: MIT Press, 2010; John Cloud (ed.), *Earth Sciences in the Cold War*, special issue of *Social Studies of Science* (2003) 33, pp. 629–819; Simone Turchetti and Peder Roberts (eds.), *The Surveillance Imperative: Geosciences during the Cold War and Beyond*, New York: Palgrave Macmillan, 2014; Charles A. Ziegler and David Jacobson, *Spying without Spies: Origins of America's Secret Nuclear Surveillance System*, Westport, CT: Praeger, 1995; Angela Creager, *Life Atomic: A History of Radioisotopes in Science and Medicine*, Chicago: The University of Chicago Press, 2013. Radiocarbon was not produced commercially by the AEC, nor was it used in biomedical research, and its trajectory diverges significantly from those of the radioisotopes discussed by Creager.

10 Ziegler and Jacobson, op. cit. (9).

11 Peter Galison, *Image and Logic: A Material Culture of Microphysics*, Chicago: The University of Chicago Press, 1997. See especially the Introduction, and Chapters 4 and 6. On electronics, military funding and postwar physics see also the classic article by Paul Forman, 'Behind quantum electronics: national security as basis for physical research in the United States, 1940–1960', *Historical Studies in the Physical and Biological Sciences* (1987) 18, pp. 149–229.

12 T.A. Rafter and G.J. Fergusson, 'Atom bomb effect: recent increase of carbon-14 content of the atmosphere and biosphere', *Science* (1957) 126, pp. 557–558.

13 Andrei D. Sakharov, 'Radioactive carbon from nuclear explosions and non-threshold biological effects', *Atomic Energy* (1958) 4, reprinted in English translation in *Science and Global Security* (1990) 1, pp. 175–186; Linus Pauling, 'Genetic and somatic effects of carbon-14', *Science* (1958) 128, pp. 1183–1186.

sources.[14] It was developed specifically for archaeological and some geological dating applications, with no emphasis placed on making the dating system also capable of radiological surveillance for military purposes; in fact, significant energy and attention were expended on shielding the dating apparatus from detecting outside radioactive interference that threw off date calculations.

Second, although atomic chemists and physicists were instrumental in the development and spread of radiocarbon dating, their role in the emergent field of radiocarbon studies always existed in relation to its direct utility in geology and archaeology. Even when radiocarbon scientists were working on improving their experimental practice and exploring new facets of radiocarbon research, their laboratories were chiefly engaged in dating artefacts for fees that partially underwrote their other research. These obvious non-military applications also meant that the dating research could be held up as a peaceful example of the benefits of the atomic age, even while anthropologists and archaeologists gained prestige and objectivity from their associations with a high-modern atomic technology.[15]

Third, despite the fact that radiocarbon researchers were among the first to raise the alarm about the worldwide increase in carbon-14, the international conventions around nuclear fallout remained focused on conventional fission products like strontium-90, even when the evidence for the global rise in radiocarbon was picked up by some of the most prominent anti-nuclear activist scientists in the world as they made their arguments for the discontinuation of nuclear testing. In part, as Néstor Herran has shown, this was due to the lack of hard data on the negative health effects of excess carbon-14, whereas significant data were available about the impact of conventional fallout on the 'down-winder' populations near nuclear test sites.[16] Additionally, Willard Libby had moved on from his radiocarbon research to take a position as a commissioner of the US Atomic Energy Commission, where he particularly focused on issues relating to fallout. Libby's background in radiocarbon as naturally occurring and of no particular danger may also have informed his relatively low assessment of radiocarbon's risk to humanity.[17] Certainly, by the time researchers drew attention to the worldwide radiocarbon increase in 1957, the fallout narrative had already begun to take shape.

This article tracks the history of radiocarbon dating through its first decade and a half, from its inception in 1947 to the award of the Nobel Prize to Libby in 1960 and the passage of the Limited Test Ban Treaty in 1963. Building on Greg Marlowe's excellent study of the first year of radiocarbon research (1947–1948), this article expands the temporal and disciplinary scope of past histories, and reads radiocarbon alongside the

14 Susan Lindee and Joanna Radin, 'Patrons of the human experience: a history of the Wenner-Gren Foundation for Anthropological Research, 1941–2016', *Current Anthropology* (2016) 57, pp. S218–S301.

15 This began very early in the history of radiocarbon's development; see, for example, Hallam Movius Jr to Paul Fejos, 31 January 1949, FJ 1:N1.

16 Néstor Herran, 'Unscare and conceal: the United Nations Scientific Committee on the Effects of Atomic Radiation and the origin of international radiation monitoring', in Turchetti and Roberts, op. cit. (9), pp. 69–84. See also Paul Rubinson, '"Crucified on a cross of atoms": scientists, politics, and the Test Ban Treaty', *Diplomatic History* (2011) 35, pp. 283–319.

17 Willard Libby, 'Radioactive fallout and radioactive strontium', *Science* (1956) 123, pp. 657–660. See also Libby, 'A letter to Dr. Schweitzer', *Bulletin of the Atomic Scientists* (1957) 13, p. 206.

growing literature on the geo- and environmental sciences in the Cold War, concluding that radiocarbon research has a number of 'paths not taken', despite many similarities and points of connection. It begins with Libby's Chicago research in the late 1940s and the creation of the Radiocarbon Dates Committee, a short-term joint project between Libby and the Society of American Archaeologists (SAA) and the American Anthropological Association (AAA), supported by the Viking Fund (Wenner-Gren Foundation). It then explores what happened when radiocarbon dating moved out into the wider worlds of both archaeology and nuclear research, and how new radiocarbon practitioners negotiated problems of contamination and standardization in the laboratory, conflict between different disciplinary norms, and the perennial challenge of raising money and finding institutional support. As more labs were established in different parts of the world, their researchers were able to gather and then correlate data on radioactive interference produced by atmospheric bomb tests and track the formation and movement of 'bomb carbon' in the atmosphere and local ecosystems. Ultimately, however, these studies of bomb carbon had little impact on the changing discussion about the global impact of nuclear fallout. Instead, radiocarbon dating was confirmed as an archaeological and geological technology answering a different set of questions about the global environment.

The Radiocarbon Dates Committee

The supper club talk that Libby gave in New York in January 1948 was almost two decades in the making, for all that Libby had probably not anticipated an enthusiastic audience of archaeologists and anthropologists when he began his earliest work on radioisotopes in the early 1930s. As a graduate student and then faculty member at the University of California, Berkeley, Libby specialized in the measurement of naturally occurring radioactive substances, particularly of abundant isotopes showing 'feeble activity', using modified Geiger-Müller counters that he protected from background cosmic-ray interference with heavy iron shielding.[18] His work was not specifically focused on carbon isotopes, although a research group in his department led by Sam Ruben, formerly one of Libby's graduate students, began investigating carbon isotopes for use in photosynthesis studies in the late 1930s.[19] Initial experimental data for the as yet unisolated carbon-14 isotope suggested it should have a half-life of about three months, which fell in the desired window for bio-tracer research. Together with Martin Kamen, Ruben began work on creating a sample of carbon-14 in the Berkeley cyclotron, successfully isolating a sample in 1940. The half-life was determined to be not three months, but instead four thousand years, later revised upward to 5,568 years – a little ungainly for photosynthesis study.[20] Further radiocarbon (carbon-14) research was put on hold when the United States entered the Second World War in 1941 and the Berkeley chemists were mobilized for war service. Ruben and Kamen

18 W.F. Libby, 'Radioactivity of neodymium and samarium', *Physical Review* (1934) 46, pp. 196–204.
19 Creager, op. cit. (9), pp. 227–230.
20 Recent (2018) experimental data gives the half-life of carbon-14 as 5,730 years.

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were pulled into chemical weapons development and uranium separation respectively before Ruben was killed in a laboratory explosion in 1943. Libby, who had followed Ruben and Kamen's work from a distance, since he was on sabbatical at Princeton, joined the Manhattan Project, where he remained for the duration of the war, moving to a position at the University of Chicago's Department of Chemistry and Institute for Nuclear Studies in 1945.

In Chicago, Libby picked up some of his old interests in low-activity radiation detection and the study of radiation from cosmic rays, and in early 1947 he published a new paper on the natural production of radiocarbon in the environment as a by-product of the bombardment of the Earth's atmosphere by cosmic radiation.[21] In the paper, Libby's group analysed the carbon-14 in two samples of methane, one taken from petroleum sources and the other collected from naturally occurring bio-methane in the Baltimore sewers. The petroleum-sourced methane showed no carbon-14 activity, which was to be expected, since the vast geological age of petroleum deposits meant that the carbon-14 should have decayed below the threshold of discernible activity long ago. The 'young' bio-methane, however, showed amounts of carbon-14 that agreed with the amounts predicted by their model if carbon-14 was naturally created through atmospheric cosmic-ray bombardment.

Libby and his co-authors conceded that while it was conceivable that the radiocarbon in the Baltimore sewer could have been man-made, the possibility was remote: the only source of artificially produced radiocarbon in the country at the time of collection was the reactor of the Atomic Energy Commission (AEC) in Oak Ridge, and records showed that none of their carbon-14 had been dispatched to Baltimore. The next task was to determine the actual half-life of carbon-14 to a higher degree of precision than Ruben and Kamen's preliminary calculations in 1940. Libby, drawing on his Manhattan Project connections and his high status in the US nuclear chemistry community, got a purified sample of radiocarbon from the Oak Ridge reactor, and experimentally determined that radiocarbon had a half-life of 5,568 years, plus or minus thirty years.[22]

In the spring and summer of 1947, small discussions took place at Chicago among members of the chemistry and anthropology departments about the application of new studies in low-level radioactivity to dating archaeological materials. Harrison Brown, a professor of chemistry, suggested to the anthropologist Fay-Cooper Cole that it might be possible to measure absolute age by measuring artefacts' absorbed radium content, while Libby thought the same goal might be possible with radiocarbon. These conversations eventually grew to include not only Libby, Brown and Cole, but also chemist Harold Urey; anthropologists Fred Eggan, Sol Tax and Kenneth Orr; and archaeologist Robert Braidwood.[23] In a side-by-side comparison of the radiocarbon and radium dating hypotheses, written by anthropology graduate student Robert

21 E.C. Anderson, W.F. Libby, S. Weinhouse, A.F. Reid, A.D. Kirshenbaum and A.V. Grosse, 'Radiocarbon from cosmic radiation', *Science* (1947) 105, pp. 576–577.

22 Frederick Johnson, 'Introduction', *Memoirs of the Society for American Archaeology* (1951) 8, p. 1.

23 Robert S. Merrill, 'A progress report on the dating of archaeological sites by means of radioactive elements', 20 October 1947, FJ 2. Merrill's report was later published: see Merrill, 'A progress report on the dating of archaeological sites by means of radioactive elements', *American Antiquity* (1948) 13, pp. 281–286.

Merrill in the summer of 1947, radiocarbon easily proved superior to radium as the target isotope of choice. It was absorbed consistently by living organisms over the course of their lifespan, and then disintegrated into stable, identifiable isotopes. Radium was not consistently absorbed by all living things and could also be the decay product of heavier radioactive elements – although it did have the advantage that it could be used to date inorganic materials. The problem of refining the technical processes of sample preparation, radioactivity measurement and mitigating sample contamination, Merrill wrote, would fall largely to chemists and physicists.[24]

During these casual conversations about radiocarbon dating in the summer of 1947, word trickled out into the US archaeological and anthropological community that a chemist in Chicago had a wild new technique that could date otherwise undatable archaeological relics – although in many of the rumours, that chemist was the 1934 Nobel laureate Harold Urey.[25] During a summer visit to Brookhaven National Laboratory on Long Island, Urey crossed paths with the Dutch German palaeoanthropologist G.H.R. von Koenigswald, whose work at the time was supported by a collection of small and medium-sized anthropological grants, including one from the Viking Fund. Von Koenigswald reported on radiocarbon dating to the fund leadership, who immediately seized on both the exciting possibility of the technique and the status boost the Viking Fund would receive if they were giving grants to Nobel laureates.[26]

The Viking Fund was (and remains) a unique institution in the landscape of American private philanthropy. Founded by the Swedish industrialist Axel Wenner-Gren to avoid a tax liability to the IRS related to the sale of a very large yacht, the Viking Fund carved out a role as the pre-eminent funder and institutional centre for the emerging and professionalized discipline of anthropology in the United States in the twentieth century.[27] For the first twenty years of its operation, the director of research (and main force behind grant-making decisions) was Paul Fejos, a Hungarian émigré whose previous career included a stint in Hollywood directing avant-garde film and a position as the organizer of an archaeological expedition in Peru supported by Wenner-Gren, who may have wished to build goodwill and explore mining opportunities in the region as he contemplated expanding his business interests in Latin America.[28] Wenner-Gren's interest in the foundation waned to a degree over the 1940s; the renaming as the Wenner-Gren Foundation in 1951 was an attempt to strengthen the relationship and good feeling between the donor and the organization.

By 1947, the Viking Fund had begun to establish itself as a valuable patron for anthropologists in the United States, as well as achieving a certain degree of academic standing

24 Merrill, op. cit. (23), p. 286.

25 Lindee and Radin, op. cit. (14), p. S270; Marlowe, op. cit. (4), pp. 17–19.

26 Lindee and Radin, op. cit. (14).

27 See Lindee and Radin, op. cit. (14). See also Ilja A. Luciak, 'Vision and reality: Axel Wenner-Gren, Paul Fejos, and the origins of the Wenner-Gren Foundation for Anthropological Research', *Current Anthropology* (2016) 57, pp. S302–S332; and Leslie C. Aiello, 'The Wenner-Gren Foundation: supporting anthropology for 75 years', *Current Anthropology* (2016) 57, pp. S211–S217.

28 Wenner-Gren's fortune came from his company Electrolux, maker of vacuum cleaners and home appliances.

as the backers of the Viking Fund Publications in Anthropology, edited by Yale anthropologist Cornelius Osgood.[29] As Joanna Radin and Susan Lindee have noted in their invaluable study of the Wenner-Gren Foundation, the organization was uniquely able to give small to medium-sized grants to individual researchers in the United States with a minimum of paperwork and bureaucratic processing, and the grants to the radiocarbon project were exemplary in this regard.[30] Libby (once it was established that Urey was not actually part of the radiocarbon research group) was offered a grant of $5,000 by Fejos without even applying, and then received an additional $13,000 grant to spend in the upcoming year (1948) to transform radiocarbon dating from a promising idea to a workable process.[31]

Before Libby had even arrived to give his talk at the Viking Fund in January 1948, wheels were in motion at the Society for American Archaeology (on the basis of advanced intelligence from the Viking Fund network) to create an ad hoc committee of archaeologists to partner with Libby as he turned his radiocarbon dating proposal into reality.[32] Through the joint offices of the Society for American Archaeology and the American Anthropological Association, a Radiocarbon Dates Committee was formed in early 1948, consisting of Froelich Rainey from the Penn Museum in Philadelphia, Donald Collier at the Field Museum in Chicago and Frederick Johnson from the Peabody Museum in Andover, Massachusetts, who served as the committee chairman and later as the unofficial historian of the radiocarbon enterprise.[33] Libby began by asking the committee to preside over the selection of a sample of historical material that 'if possible will contain two or three pounds of elementary carbon' and whose age could be ascertained with a high degree of confidence by archaeologists, ideally something around four thousand to five thousand years old.[34] The chosen samples – wooden beams from the tombs of Zoser and Sneferu in Egypt – returned dates that aligned closely with the agreed archaeological dates for the relevant dynasties, even without the Libby group performing any isotopic enrichment on the carbon samples.[35] Following this preliminary success, the committee members encouraged Libby to request a second year of funding from the Viking Fund to continue developing radiocarbon dating as a robust archaeological

29 Lindee and Radin, op. cit. (14), p. S242.

30 Lindee and Radin, op. cit. (14), p. S270.

31 Marlowe, op. cit. (4), p. 19; Lindee and Radin, op. cit. (14), p. S270.

32 Frederick Johnson to Paul Fejos, 22 December 1947, FJ 1:N1.

33 See Frederick Johnson to Donald Collier, 16 February 1948, FJ 1:N1; and Frederick Johnson (ed.), 'Radiocarbon dating: a report on the program to aid in the development of the method of dating', *Memoirs of the Society for American Archaeology* (1951) 8, pp. 1–65. See also Willard F. Libby, *Radiocarbon Dating*, Chicago: The University of Chicago Press, 1952; and Paul Fejos, *The Wenner-Gren Foundation for Anthropological Research: The First Ten Years 1941–1951 as the Viking Fund*, New York: The Viking Fund, 1951, pp. 22–24. Yale geologist Richard Foster Flint also assisted the committee and served as a liaison to the American Geological Society.

34 W.F. Libby to Frederick Johnson, 24 March 1948; W.F. Libby to Froelich Rainey, 9 April 1948; and Johnson to Libby, 19 April 1948, all in FJ 1:1.

35 W.F. Libby to Paul Fejos, 13 December 1948, FJ 1:1.

technology; he received an additional $20,000 to cover equipment and operating costs for his laboratory in 1949.[36]

Before funding for 1949 was even guaranteed, Libby had begun work on a worldwide assay of environmental radiocarbon, in order to establish that radiocarbon was consistently present in the biosphere at varying latitudes and elevations. In an October 1948 letter to Frederick Johnson, Libby set out a list of locations that were still unrepresented in his survey: 'Northern Canada, New Zealand, modern Egypt, the Union of South Africa, Siberia, Japan or China, Iceland, the Philippines, Hawaii, Pitcairn Island or other islands in this group, and French West Africa at about 20°N and 0°W'.[37] This collecting task fell largely to the archaeologist members of the committee, who mobilized their professional network, relying on American academics who did regular work in particular parts of the world, who could use their own contacts to gather samples and funnel them back to the radiocarbon team. In the next year, after the initial worldwide assay was complete (and had confirmed that worldwide radiocarbon content was equal, regardless of locale), the committee used a similar point-man method to gather samples for testing from certain geographic regions.[38] Hallam Movius Jr, at Harvard, for example, was responsible for organizing the collection of samples from Scandinavia and Western Europe, while Robert Braidwood, at the University of Chicago, coordinated and shared his own specimens from Mesopotamia and the Middle East. However, projects in American archaeology dominated the list of target artefacts and sites at this stage of systematic dating work.[39]

Johnson was also concerned that the committee might be relying overmuch on exclusively archaeological samples, writing to Libby that they should attempt to 'establish the program on as broad a basis as possible'.[40] Samples of peat bogs were of high interest to the committee, but it was difficult to find bogs in the United States that had not already been drained. At Yale, ecologist Edward Deevey wrote to a colleague in England to ask for samples of British peats, and Hallam Movius used contacts he had cultivated on the European continent to arrange correspondence and an exchange of samples between the Radiocarbon Dates Committee and the National Museum of Denmark.[41] Gaining specific samples was sometimes challenging, but it was rarely impossible; someone on the Radiocarbon Dates Committee could usually find a willing collaborator who could either share material or volunteer to contact someone else who could send a piece of wood or sample of peat to the United States for Libby's laboratory to analyse.

36 Libby to Fejos, 13 December 1948; Johnson to Rainey and Collier, 10 December 1948, FJ 1:2; Rainey to Fejos, 27 January 1949, FJ 1:2; Libby to Fejos, 13 December 1948, FJ 1:1; Johnson to Libby, 28 January 1949, FJ 1:1; Johnson to Fejos, 3 January 1949, FJ 1; Fejos to Johnson, 11 January 1949, FJ 1.

37 Libby to Johnson, 14 October 1948, FJ 1:1.

38 Johnson to Fejos, 3 May 1949, FJ 1.

39 James B. Griffin to Johnson, 21 May 1949, FJ 1.

40 Johnson to Libby, 8 April 1949, FJ 1:1.

41 Edward Deevey to Harry Godwin, 17 October 1949, FJ 1; Hallam Movius to Johnson, 18 October 1949, FJ 1.

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The committee had always been intended as a limited-term endeavour, meant only to establish that Libby's radiocarbon process actually worked for dating archaeological materials. After the publication of some preliminary dates in the second half of 1950, the committee announced the authoritative release of the first dates list on 21 June 1951. Other universities, among them the University of Michigan and the University of Pennsylvania, expressed interest in setting up radiocarbon dating labs of their own. Libby, who had moved labs in Chicago over the summer, was dealing with some delays caused by the movers accidentally smashing up his radiocarbon apparatus.[42] Libby had no plans to remain the sole radiocarbon authority and welcomed the expansion, providing blueprints of the dating apparatus on request to other researchers. While Libby welcomed the continuation of the Radiocarbon Dates Committee, or its reconstitution as a radiocarbon data clearing house, overseen by the AAA, Johnson and the other archaeologist committee members demurred, arguing that not enough laboratories had expressed interest for the continuing administrative burden to be worth the effort.[43] Instead, they discharged the Radiocarbon Dates Committee as a formal entity in November 1951, leaving the new laboratories and the members of the emergent research field to determine publishing venues and research directions on their own terms, with no centralized archaeological or anthropological oversight.

Disciplining radiocarbon

The Radiocarbon Dates Committee project was possible because of the collaboration between archaeologists from well-established institutions with large collections of artefacts and long-standing collaborative networks of field researchers, on the one hand, and the new and rapidly expanding world of atomic chemistry and physics, on the other. The ease with which the committee had come together was largely due to the influence and financial largesse of the Viking Fund, which in some ways obscured the interdisciplinary gap that the radiocarbon project had to bridge. 'The laboratory procedure involves theories in physics and chemistry', wrote the committee members in 1951, 'which for the most part are outside the experience of almost everyone who has a sample to be dated'.[44] The decision to discontinue the Radiocarbon Dates Committee was in part because it had outlived its organizational purpose, but also in part because the archaeologists and the Libby group were growing tired of dealing with each other.

Tensions emerged within the project as early as 1949, both between the Radiocarbon Dates Committee and the sample-submitting archaeologists, and among the members of the committee itself. Archaeologists were motivated to send the committee material from ongoing excavations in part because they stood to directly benefit from learning how old a site or artefact was, in absolute rather than relative terms. This raised the question: who

42 Libby to Johnson, 9 October 1951, FJ 2.

43 Johnson to Libby, 11 October 1951, FJ 2.

44 Frederick Johnson, Froelich Rainey, Donald Collier and Richard F. Flint, 'Radiocarbon dating: a summary', in Johnson, 'Radiocarbon dating', op. cit. (33), p. 62.

owned the chronological data that were being produced? In June 1949, Donald Collier wrote to Frederick Johnson,

> I favor the sending by Libby to collaborators of progress reports on the whole project, if the collaborators understand and agree that the data contained *are for information but not publication*. I think this device would be extremely useful in keeping the collaborators informed and increasing their understanding of the problems and difficulties involved …[45]

Libby was interested in establishing proof of concept; if a machine needed to be recalibrated, or contamination required a date to be revised a few hundred years in one direction or another, he was only concerned with identifying and correcting the problem.[46] A few hundred years could make a tremendous difference to an archaeologist, especially one hurrying to get a description of a new site excavation into publication. In a speech to the Society for American Archaeology in May 1950, Johnson had to specify that any preliminary dates reported should be understood as incidental data from a physics experiment performed by Libby and his collaborator James Arnold, and that archaeologists who submitted samples for testing should not treat the age estimates they received in return as their personal intellectual property.[47] But at the same time, Libby seemed to show little awareness of how these dates mattered for archaeologists or the disruptive effect he was having on the field; it was as if an archaeologist was telling a physicist with great confidence and reasonable authority that the commonly accepted value for the mass of a proton was wrong. In December 1950 Johnson wrote to Jesse Jennings, an archaeologist at the University of Utah and the editor of the journal *American Antiquity*,

> Libby is a physicist of top rank … However, he has no idea, even now, of the broader implications of what he has done or is doing. He cannot yet appreciate the importance or interest of his dates beyond the tables he prepares in the narrow confines of his laboratory.[48]

A few months later, Johnson began another letter to Jennings with 'Libby always does things in the damndest fashion'.[49] Libby had begun writing his own book on the radiocarbon process, targeted at an audience of nuclear physicists and chemists, focusing on sample preparation and apparatus construction. Now he wanted to include reprints of material that Johnson and Jennings had produced for *American Antiquity*, to add a little archaeological colour and interest for his audience. The problem wasn't one of competition – Johnson recognized that the audience for a monograph on nuclear chemistry was not really the same as the devoted readership of *American Antiquity* or of the *Memoirs of the Society for American Archaeology* – but he was irritated by Libby's high-handed attitude. 'On second thought', he added to Jennings, 'it might be an idea to "force" the nuclear brethren to get a Memoir if they had to have one'.[50]

45 Collier to Johnson, 10 June 1949, FJ 1:2, emphasis in the original.

46 See memorandum from Richard Foster Flint, 25 May 1950, FJ 1:2; Johnson to Libby, 31 May 1950, and Libby to Johnson, 7 June 1950, FJ 1:1.

47 Frederick Johnson, 'Present status of carbon 14 research', paper read at the Society for American Archaeology Annual Meeting, Norman, Oklahoma, 20 May 1950, FJ 2.

48 Johnson to Jesse Jennings, 15 December 1950, FJ 1:3.

49 Johnson to Jennings, 18 April 1951, FJ 1:3.

50 Johnson to Jennings, 18 April 1951, FJ 1:3.

For archaeologists, the benefits of radiocarbon dating were tempered by frustration. Radiocarbon dates were reported with margins of uncertainty that could be as high as a few hundred years; while this error shrank with improvements in the counting method and the introduction of sample enrichment, this mostly meant that many of the earliest radiocarbon dates had to be heavily revised – to the great frustration of archaeologists who were wrestling with how to rework existing archaeological chronologies based on new radiometric dates. The system of reporting dates was ungainly, given in the format '*n* years before present'. Archaeologist Lee Abel, at the Museum of Northern Arizona, pointed out the annoyance of having to do extra maths to use radiocarbon dates in ordinary archaeological chronologies, which also required knowing in what year the dating test was conducted.[51] Additionally, radiocarbon date tests were expensive: in 1952, the cost to a laboratory of running one dating sample was between two hundred and six hundred dollars, which was passed on to the archaeologist.[52]

These objections, in general, were more likely to come from archaeologists who specialized in recent North American prehistory, whose artefacts had already been placed in robust (if contentious) chronological systems based on the interpretation of cultural succession. Researchers working in the American Southwest were also able to rely on tree ring dating (dendrochronology) for their region, which partly explains the preponderance of vocal radiocarbon sceptics from institutions in Arizona.[53] Already, though, it was clear that the radiocarbon contrarians were fighting a losing battle. When it worked, radiocarbon was a revelation for archaeology, resolving some long-standing debates and otherwise unanswerable questions in the field, and as techniques were improved it worked often enough to justify both faith and expenditures.[54]

Radiocarbon arrived at a transitional moment in American archaeology, which was struggling to professionalize and establish itself both domestically, in relation to both the natural and the human sciences, and internationally, in relation to European and Latin American archaeology.[55] Radiocarbon Dates Committee member Froelich Rainey wrote early in the project in May 1949 that 'this seems a good time to set the Europeans and the South Americans right back on their heels with the announcement of what I consider one of the most remarkable of archaeological techniques'.[56]

The most worrying problem for the future of radiocarbon dating was the increasing radioactive contamination of both research laboratories and archaeological field sites

51 Lee Abel, 'Radiocarbon dates: a suggestion', *American Antiquity* (1953) 19, p. 158.

52 'Notes and news: radiocarbon dates', *American Antiquity* (1952) 18, pp. 89–90. These costs were separate from the start-up investments in a laboratory.

53 See discussion in Henry O. Thompson, 'Science and archaeology', *Biblical Archaeologist* (1966) 29, pp. 114–125; and Frederick Johnson, 'A quarter century of growth in American archaeology', *American Antiquity* (1961) 27, pp. 1–6.

54 For a contemporary (positive) view see W.S. Broecker and J.L. Kulp, 'The radiocarbon method of age determination', *American Antiquity* (1956) 22, pp. 1–11.

55 On the complex relationship between archaeology and the natural sciences see discussion in Patrick E. McGovern, 'Science in archaeology: a review', *American Journal of Archaeology* (1995) 99, pp. 79–83.

56 Rainey to Johnson, 11 May 1949, FJ 1:2.

as the United States moved into the nuclear age. University of Arizona archaeologist Wesley L. Bliss wrote to *American Antiquity* in October 1951 that 'the writer is convinced that many of the published dates cannot be considered as valid', and that 'no attempt should be made to re-evaluate indiscriminately the archaeological chronology using radiocarbon dates, until the possible factors for contamination have been eliminated or minimized'.[57] At the 1952 meeting of the Society for American Archaeology, the radiocarbon sessions were dominated by discussions on current and continuing problems:

> All discussants (physicists) referred to the special current and continuing problem of contamination by radioactive dust in the atmosphere from atomic explosions, since 1945 but now, since the Nevada tests, more serious: samples must be placed in airtight containers immediately upon collecting.[58]

In the face of an external threat, the fragile interdisciplinary alliance in radiocarbon dating held. Both groups stood to lose if, as one anthropologist gloomily prognosticated in 1954, atomic weapons tests made radiocarbon dating obsolete.[59]

The contamination controversy forced archaeologists to decide whether they were going to commit to using radiocarbon and taking it seriously, and to adjust both their expectations and their field practices and specimen management to decrease opportunities for radioactive contamination. In the lab, the physicists and chemists who were enthusiastically adopting the new radiocarbon technology were equally game for adapting Libby's original apparatus and procedure towards the same goals. Although the 1950s saw radiocarbon researchers also become interested in exploring global carbon circulation and how carbon-14 accretes in different sectors of the biosphere and environment, leading to new research programmes spinning off from the dating projects in oceanography and atmospheric sciences, technique refinement for improved dating remained at the core of the emergent discipline. Despite the early objections and frustrations, archaeological concerns and interests were present in radiocarbon dating from the beginning, and this interdisciplinary alignment shaped the future finances, personnel and technical development of the field, and its trajectory in the Cold War scientific world.

The global laboratory

Although the Radiocarbon Dates Committee had established that it was possible to date archaeological and other carbon-based specimens by measuring the carbon-14 content, all of the proof-of-concept test runs had been conducted in Libby's laboratory at the University of Chicago's Institute of Nuclear Studies, which was probably one of the best-resourced sites for nuclear chemistry research in the world at the time. When researchers outside the United States began setting up their own radiocarbon

57 Wesley L. Bliss, 'Facts and comments: radiocarbon contamination', *American Antiquity* (1952) 17, pp. 250–251.

58 'Notes and News', op. cit. (52).

59 Carleton Coon, *The Story of Man: From the First Human to Primitive Culture and Beyond*, New York: Knopf, 1954, p. 154.

laboratories, they adapted the Libby group's protocols to local material circumstances. They also almost immediately began tinkering with procedures and practices, looking for ways to mitigate contamination risks, reduce the impact of instrument variability, simplify the sample-preparation procedure and reduce the time required to run a sample count.[60]

The basic components of a radiocarbon dating system consisted of a central Geiger-Müller counter modified with a screen-wall and a sliding sample cylinder that shifted the sample in and out of position to allow the measurement of both the sample activity and the contemporary background radiation.[61] The central counter was then surrounded by a ring of unmodified Geiger-Müller counters as an anti-coincidence shield that would register any exogenous rise in the background radiation and allow that activity to be subtracted from the counts registered in the main screen-wall counter, and the entire apparatus was encased in an eight-inch-thick steel case with a sliding lid, like a very sturdy breadbox. Libby provided the blueprints to his experimental set-up on request to any researcher, and his 1952 book *Radiocarbon Dating* included detailed descriptions of his procedures and practices.

The use of Geiger-Müller counters had been common in cosmic-ray physics and low-activity radiochemistry since the 1930s; Libby's screen-wall design in 1948 was based on a system he had created to study neodymium and samarium as a postdoc in the mid-1930s.[62] By the late 1950s, the parts list for a new radiocarbon laboratory in Sydney, Australia, for example, also included a variety of additional electronic equipment – multi-channel pulse analysers, anticoincidence units, signal amplifiers – that streamlined the counting process; many of these items entered circulation in physics laboratories as war surplus.[63] The main counter system, however, was basically a twenty-year-old technology, built on the most common type of commercially available radiometric counter. The Australian lab seems to have encountered no limitations on importing or purchasing the necessary technology, other than three-to-four-month delivery wait times for electronic components ordered from overseas.

In 1959, *Radiocarbon* published a complete list of dating laboratories around the world: sixteen in Europe, fourteen in the United States, three in Canada and three in Australasia.[64] Six more labs, including one in Moscow, appeared on the 1960

60 Respectively see Włodzimierz Mościcki, 'On the use of $CO_2 + CS_2$ filled G.M. counters for age determination', *Acta Physica Polonica* (1953) 12, pp. 238–240; C. Ballario, M. Beneventano, A. De Marco, F. Magistrelli, C. Cortesi and T. Mantovani, 'Apparatus for carbon-14 dating', *Science* (1955) 121, pp. 409–412; T.A. Rafter, 'The preparation of carbon for C^{14} age measurements', *New Zealand Journal of Science and Technology B* (1953) 35, pp. 64–89; and E.C. Anderson, Hilde Levi and H. Tauber, 'Copenhagen natural radiocarbon measurements I', *Science* (1953) 118, pp. 6–9.

61 See complete description in W.F. Libby, *Radiocarbon Dating*, 2nd edn, Chicago: The University of Chicago Press, 1965, Chapters 4 and 5.

62 On Geiger-Müller counters see discussion in Galison, op. cit. (11), Chapter 6.

63 Galison, op. cit. (11), p. 35. The parts list comes from the records of the Department of Nuclear and Radiation Chemistry, University of New South Wales, Sydney. See J.H. Green to Vice-Chancellor J.P. Baxter, 13 October 1959, Department of Nuclear and Radiation Chemistry papers, UNSW Records and Archives, Sydney. Unfortunately, Libby's description of his apparatus does not go into equivalent detail about the suppliers and exact makes and models of his electronic equipment.

64 'List of laboratories', *American Journal of Science Radiocarbon Supplement* (1959) 1, pp. 215–218.

list.[65] The Wenner-Gren Foundation continued to support radiocarbon projects, while the Rockefeller Foundation gave money for an inclusive Geochronology Laboratory at Yale University.[66] In other instances, discussed below, governmental entities decided radiocarbon dating was a matter of some national scientific importance.

Looking at how radiocarbon moved outside the United States also makes it clear how the dispersal and uptake of radiocarbon dating methods built on a foundation of inter-war research on the uses of radioactive isotopes as tracers in chemistry and biology. One of the earliest European radiocarbon labs was in Denmark, at the University of Copenhagen, directed by physicist Hilde Levi. Originally from Frankfurt, Levi was the last Jewish woman to receive a PhD in science from the University of Berlin in 1934. She left Germany for Copenhagen immediately after graduating and joined George de Hevesy's lab at Niels Bohr's Institute for Theoretical Physics in 1934, moving to the privately funded Carlsberg Laboratory in 1940 after the German invasion, until her colleagues and friends helped her escape to Sweden in 1943.[67] In Sweden, Levi joined the Wenner-Gren Institute for Experimental Biology at the University of Stockholm – which, like the Viking Fund, was founded by Axel Wenner-Gren in 1937 for a tax write-off – and then returned to Denmark in May 1945 to take a leading position in an interdisciplinary group working on applications of radioisotopes as tracers in biological research.

One challenge Levi's group faced was that they had been cut off from new scientific developments in radioisotope studies by the war. To get around this difficulty, she applied for and was awarded a one-year fellowship from the American Association of University Women (AAUW) to travel to the United States as a visiting researcher, and left Copenhagen in October 1947. After brief visits to Brown, Harvard and MIT, Levi arrived at the Institute for Nuclear Studies at the University of Chicago and began working with Libby on 'the modern development of the counting technique, especially using carbon as a tracer'.[68] She remained in Chicago until April 1948, working on the development of radiocarbon dating and exploring problems in the counting of extremely low levels of radioactivity and how to isolate the carbon samples from other radioactive impurities like potassium-32.[69] Levi found that she could 'hardly learn more within my special field of interest at any other institution', and spent far more time at Chicago than she had originally intended, although she made time to visit universities around the Midwest and East Coast, as well as the Carnegie Institute and the National Institutes of Health in Washington, DC and a symposium on isotopic tracers in biology at Cold Spring Harbor.

On her return to Copenhagen, Levi was approached by the National Museum of Denmark about the possibility of building a radiocarbon dating laboratory. With

65 'Revised list of laboratories, 1960', *American Journal of Science Radiocarbon Supplement* (1960) 2, pp. 224–228.

66 Fejos, op. cit. (33).

67 Hilde Levi AAUW fellowship file (subsequently HL), American Association of University Women archives, Washington, DC.

68 HL fellowship report 1, 30 November 1947.

69 HL fellowship report 2, 5 September 1948.

funding secured from the Carlsberg Foundation to support construction costs and equipment purchases, construction began in the autumn of 1951. The first list of dates covered a series of geological samples chosen by representatives from the National Museum and the Danish Geological Survey; the second list, covering dating experiments run between February 1953 and May 1955, dated archaeological samples from the earliest period of the European Neolithic.[70] The laboratory also dated samples of muscle and liver tissue from the famous 'bog body' known as Grauballe Man, placing the preserved remains in the Roman Iron Age, roughly 310 CE, plus or minus 100 years.[71]

Because the Danish laboratory had such an early start, it quickly emerged as an organizing force in European radiocarbon studies. This coordinating project was significantly aided by Levi's continuing research relationships with radiocarbon scientists in the United States, and her long-standing friendships with most of the major figures of European physics, many of whom had come through the Bohr Institute during her postdoctoral years. Levi was the critical bridge between the American and European radiocarbon committees: in the words of one American observer, Levi knew 'everyone and the cat' in European physics.[72]

For researchers in radiocarbon labs on both sides of the Atlantic in the early 1950s, it was very apparent that the Europeans were working at a massive material disadvantage to their colleagues in the United States. In 1954, the organizers of a radiocarbon conference in Copenhagen deliberately chose not to invite American researchers, because they wanted to promote contacts among European research laboratories rather than routing all research collaboration through the better-funded labs in the United States.[73] Hilde Levi laid out the parameters of the issue for Frederick Johnson:

> With few exceptions, the European labs are in the very beginning of their work; it therefore seemed to me that American workers, if present, necessarily would dominate, both because of their much-advanced techniques and the extensive research projects they have under way. They would cause the European beginner to remain rather silent, and while he would hear how much better others are doing, he would not get his own troubles discussed.[74]

At the same time, it was critical, Levi argued, that European radiocarbon researchers have the opportunity to travel to the United States to learn new techniques from their admirably over-resourced American colleagues. Holding an international radiocarbon conference in the US rather than Europe would provide infrastructure and institutional support for European radiocarbon researchers to visit multiple US labs and make valuable connections. The only catch was that most of the European researchers Levi knew would be unable to cover their own transatlantic travel costs.[75] Levi and Johnson applied for a significant grant from the Wenner-Gren Foundation and found that their money went significantly farther if the conference was held in the UK, and a smaller

70 Henrik Tauber, 'Copenhagen natural radiocarbon measurements II', *Science* (1956) 124, pp. 879–881.

71 Henrik Tauber, 'Tidsfæstelse af Grauballemanden ved Kulstof-14 Måling' (Dating of Grauballe Man by carbon-14 measurement), *Kuml* (1956) 6, pp. 160–163. *Kuml* is helpfully published with summaries in English.

72 Johnson to James Arnold, 14 October 1955, FJ 4.

73 Levi to Johnson, 3 November 1954, FJ 2.

74 Levi to Johnson, 3 November 1954, FJ 2.

75 Levi to Johnson, 2 December 1954, FJ 2.

number of American radiocarbon researchers made the trip from west to east instead. This turned out to be the best option, and Cambridge University played host to an international conference on radiocarbon dating for researchers from both sides of the Atlantic in July 1955.[76]

Levi went to the US in 1947 with the intention of catching up on new developments in radioactive tracer research, 'from which scientists in the liberated countries have been cut off for so many years'. What she found was an entirely new domain of research in the making, which shaped her work and life for the next two decades. She wrote more than fifty articles on radiocarbon and other uses of radioactive tracers in biology and physiology and served as a consultant to the Danish National Health Service on radiation safety and exposure limits. In 1966 she wrote, 'I have tried to become, and I think succeeded in becoming, a good research worker or scientist, recognized as such in the field in which I am specialized.'[77] The Second World War created disjuncture in Levi's life and professional career, but it also created conditions that allowed her to build a career in a dynamic new field.

Radiocarbon research was one of many fields that absorbed expertise and investment opportunities left over from the wartime mobilization of nuclear physics and chemistry, especially in smaller countries that did not or were unable to invest in research reactors or weapons development in the 1950s. In New Zealand, where the end of the war brought the return of highly trained scientists who had been seconded to work with the British and American atomic bomb projects, limited local supplies of uranium and other radioactive minerals scuttled a short-lived government plan to build an experimental reactor.[78] This meant, however, that New Zealand had plenty of people who were well suited to working on radiocarbon, especially at the New Zealand Department of Scientific and Industrial Research (DSIR) in Wellington. In 1950, DSIR head F.R. Callaghan proposed the creation of a Wellington radiocarbon laboratory to 'stop the geologists from arguing' over issues that included the timing of past volcanic eruptions and the extinction of the moa.[79]

In 1951, radiochemist T. Athol Rafter and physicist Gordon Fergusson, who specialized in variations in cosmic background radiation, wrote to Libby in Chicago to ask for advice about setting up a laboratory, and soon began preparing carbon samples for measurement. They published their first findings in 1953, accompanied by detailed explanations of the DSIR group's improved experimental methods for decreasing environmental contamination and new efforts to run date tests using carbon dioxide instead of a solid carbon sample.[80] While in Chicago (and probably most other radiocarbon

76 Hilde Levi, 'Radiocarbon dating: conference in Cambridge', *Nature* (1955) 176, pp. 727–728.

77 HL reply to AAUW questionnaire for 1966, marked received 18 April 1966.

78 Ross Galbreath, *DSIR: Making Science Work for New Zealand*, Wellington: Victoria University Press, 1998, pp. 140–169; and Rebecca Priestley, *Mad on Radium: New Zealand in the Atomic Age*, Auckland: Auckland University Press, 2012, pp. 57–63.

79 Rodger Sparks, 'Athol Rafter', *Radiocarbon* (1996) 38, pp. v–vi.

80 See discussion in G.J. Fergusson, 'Activity measurement of samples for radiocarbon dating', *New Zealand Journal of Science and Technology B* (1953) 35, pp. 90–108; and T.A. Rafter, 'Carbon dioxide as a substitute for solid carbon in ^{14}C age measurements', *New Zealand Journal of Science and Technology B* (1955) 36, pp. 363–370.

laboratories) the sample preparation and the counting equipment were colocated, Rafter and Fergusson split their research between Rafter's lab in Wellington and Fergusson's lab in Lower Hutt, fourteen miles away over rough mountain roads, which meant that it was likely that all the carefully placed carbon had fallen off the surface of the counter cylinder by the time it arrived.[81] Rafter was finally able to move to join Fergusson in Lower Hutt in November 1952, taking over a new workspace in the DSIR's evocatively named No. 2 Shed.

As the number of laboratories around the world increased, so too did opportunities for researchers to discover new forms of radioactive cross-contamination. In 1947, at the outset of the radiocarbon dating project, there had only been five nuclear detonations in history, and none of those had had a yield greater than about twenty kilotons.[82] In the autumn of 1952, the United States tested its first high-yield hydrogen bomb in the Pacific, Ivy MIKE, with a yield of ten megatons, roughly a thousand times larger than the yield of the Hiroshima explosion.[83] The critical difference between the megaton and kiloton bombs was, of course, that the megaton bombs were a thousand times larger, but also that they produced free neutrons in the upper atmosphere in significant enough numbers that they began to increase the production of free carbon-14 in the Earth's atmosphere. In radiocarbon terms, the megaton bomb tests were an anthropogenic cosmic-ray event.

The New Zealand lab was the first radiocarbon research site established in the Pacific, and in nearest relative proximity to the American hydrogen bomb tests. In November 1954, as soon as they were fully organized in their now-unified laboratory in the No. 2 Shed, Rafter and Fergusson began monitoring atmospheric carbon dioxide in order to track the contemporary carbon-14 baseline. Their first results were reported in early 1955, and although they agreed largely with the expected values, there were preliminary indications that the carbon-14 content of the atmosphere was measurably increasing. They suspected, initially, that this might be due to some kind of seasonal variation related to carbon exchange, or, alternately, 'a C^{14} enrichment of the atmosphere by atomic explosions'.[84] Further sampling in 1956 and 1957 led Rafter and Fergusson to conclude that the carbon-14 content of the atmosphere of the southern hemisphere had increased by just under 5 per cent in the past four years alone; a similar increase was measured for carbon-14 uptake in the southern hemisphere biosphere. They termed this increase the 'atom bomb effect', and hypothesized that the increase in excess carbon-14 was likely to be even higher in the northern hemisphere, where all the publicly acknowledged weapons tests to date had taken place.[85]

81 Rafter, op. cit. (60), p. 76.

82 Trinity (July 1945), Hiroshima and Nagasaki (August 1945), and the two test detonations Able (June 1946) and Baker (July 1946) during Operation Crossroads at Bikini Atoll.

83 Alex Wellerstein, 'What if Truman hadn't ordered the H-bomb crash program?', *Restricted Data: The Nuclear Secrecy Blog*, 18 June 2012, at http://blog.nuclearsecrecy.com/2012/06/18/what-if-truman-didnt-order-h-bomb-crash-program.

84 Rafter and Fergusson, op. cit. (12).

85 The British atomic tests in Australia at Emu Field, Maralinga and the Montebello Islands in the early 1950s were obviously in the southern hemisphere, but conducted under circumstances of extreme secrecy.

As Fergusson and Rafter's reports suggest, radiocarbon labs were in a position to observe bomb blasts and potentially violate nuclear secrecy, if they were able to discern the effects of secret tests, but at the same time they were unable to actually confirm how many bomb tests had taken place, or independently determine the yield of the bomb tests to date. Radiocarbon variation could be caused by a few different factors, and there were many unknown variables still at play in the calculations of the variability of historic and present global carbon-14 levels. Rafter and Fergusson, for example, did not seem to know about the British bomb tests at Maralinga in 1952. This could be because the Maralinga bombs (mere kiloton blasts) were too small to have a measurable impact on atmospheric carbon-14 data, but it also suggests that radiocarbon labs made for poor spy stations. At no point does it appear that a radiocarbon laboratory actually spilled nuclear secrets or raised the alarm on otherwise undetected bomb tests; all the times where radiocarbon researchers mentioned fallout interference, it was a post hoc assessment, probably derived by matching observed background spikes to bomb tests reported in the newspapers.

'Atom bomb effect' later became a touchstone for debates on the 'nuclear-free New Zealand' policy in the 1980s, but in the 1950s it had a decidedly smaller impact.[86] Linus Pauling cited Rafter and Fergusson's article in his own article on the short- and long-term effects of atmospheric weapons testing, but international conventions about fallout remained focused on conventional fission products.[87] Radiocarbon researchers, likewise, had been dealing with inconvenient interference from conventional fallout for several years, and were also aware that the 'ordinary' rate of radiocarbon production had fluctuated in the past; while investigating anomalous radiocarbon activity measurements in wood from the nineteenth century, researcher Hans Suess had shown in 1955 that the increased carbon dioxide output of the Industrial Revolution had led to lower-than-normal levels of atmospheric radiocarbon during the previous century.[88] Fallout was definitely visible to radiocarbon labs in a very direct way, but their impact on the wider world's understanding of fallout was low.

For Rafter and Fergusson, what was more immediately interesting was that their bomb carbon observations offered a way to gain insight into mixing patterns between the atmospheres of the northern and southern hemispheres. 'If information on the power of all the weapons exploded to date were available', they wrote, 'the rate of C^{14} increase in the Northern Hemisphere with time could be calculated fairly accurately.'[89] Military weapons tests had created a natural experiment which made new knowledge about atmospheric mixing and the carbon cycle apparent to trained observers; inconveniencing some radiocarbon dating labs was just a side effect.

86 See discussion in Priestley, op. cit. (78); Galbreath, op. cit. (78).
87 Herran, op. cit. (16).
88 Hans E. Suess, 'Radiocarbon concentration in modern wood', *Science* (1955) 122, pp. 415–417.
89 Rafter and Fergusson, op. cit. (12).

Conclusion

Willard Libby was the sole recipient of the 1960 Nobel Prize in Chemistry 'for his method to use carbon-14 for age determination in archaeology, geology, geophysics, and other branches of science'. In his presentation address, Nobel committee chairman Arne Westgren emphasized the importance of Libby's experimental skill, obtained through long years of study of low-activity radioactive substances, which had allowed him to bring his 'brilliant impulse' to study the carbon-14 content of biological materials to fruition as an indispensable part of research work in many areas of science.[90] The success of the radiocarbon dating method resulted from this combination of initial insight coupled with low-activity radiation measurement, with general methods adjusted for the specific circumstances of working with carbon-based samples.

Thinking counterfactually, could radiocarbon's story have gone differently? Could radiocarbon dating have become part of the Cold War military surveillance network, or subject to prohibitions on knowledge-sharing across international borders? A dating technique based on another radioactive mineral might have been pulled into worldwide surveys of fissile material, while a dating apparatus that used more specialized radiation counters might have been subject to either industrial or military export and import controls. If the initial budget had been supplied by the Atomic Energy Commission, or the US Department of Defense, radiocarbon observation could have become part of the suite of long-distance radiation surveillance projects – although, until the advent of the megaton weapons, conventional nuclear weapons were not producing debris that only a radiocarbon scientist could observe or identify. Even after the beginning of thermonuclear weapons tests, radiocarbon scientists were only contributing to an already extant conversation, and their particular concerns were shaped by the fact that radioactive contamination was making it more difficult to study the human past – a poignant and relevant issue, but not one that surpassed concerns about the unrestrained testing of nuclear warheads starting a global conflict or bringing about the annihilation of all life on Earth.

The Nobel Prize capped a decade of growth and development in radiocarbon research, and, even though the prize was given to a chemist, confirmed the central role of archaeology in shaping radiocarbon as a dating – and not a surveillance or fallout-monitoring – technology. Radiocarbon practices and detectors were ordinary, rather than secret, and after the Radiocarbon Dates Committee released the first official list, information about radiocarbon dating seems to have circulated freely throughout international intellectual networks. While individual labs faced local challenges specific to their material, political or geographical circumstances, these differences fuelled innovation and growth in the field.

By the time of Libby's Nobel Prize in 1960, the stakes of low-activity radiation research had undergone significant changes. More than ever, the world was aware of the threat that radiation, especially radioactive fallout, posed to the health and well-being of people and the global environment. In Stockholm, though, radiocarbon was

90 Westgren, op. cit. (8).

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framed as a peaceful, useful isotope, gently illuminating the human past, helpfully identifying mineral and oil deposits, creating useful new knowledge about the Earth and its processes. In his speech at the Nobel banquet, Libby expressed his hope that this work had 'in some small way … helped people to learn about the atom and the way in which it works and may have helped them to see that isotopes … hold much promise for the future'.[91] 'We hope', Libby concluded, 'that this honor you have done us will bring the time of further realization of these benefits closer and will help all mankind to live better and be happier through the atom and isotopes'. The conditions for the expansion of many of the geo- and environmental sciences were set by Cold War military priorities – radiocarbon dating, ultimately, happened not to be one of them.

91 Willard F. Libby, 'Speech at the Nobel banquet', 10 December 1960, Nobel Media AB, 2020, at nobelprize.org/prizes/chemistry/1960/libby/speech.