

## Big Science and Big Data in Biology: From the International Geophysical Year through the International Biological Program to the Long Term Ecological Research (LTER) Network, 1957–Present

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### ABSTRACT

This paper discusses the historical connections between two large-scale undertakings that became exemplars for worldwide data-driven scientific initiatives after World War II: the International Geophysical Year (1957–1958) and the International Biological Program (1964–1974). The International Biological Program was seen by its planners as a means to promote Big Science in ecology. As the term Big Science gained currency in the 1960s, the Manhattan Project and the national space program became paradigmatic examples, but the International Geophysical Year provided scientists with an alternative model: a synoptic collection of observational data on a global scale. This

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The following abbreviations are used: AEC, Atomic Energy Commission; CSAGI, Comité Spécial de l'Année Géophysique Internationale; ESA, Ecological Society of America; IBP, International Biological Program; IBP Papers, Papers of the International Biological Program, Archives of the National Academy of Sciences, Washington, DC; ICSU, International Council of Scientific Unions; IGY, International Geophysical Year; IGY Papers, Papers of the International Geophysical Year, Archives of the National Academy of Sciences, Washington, DC; IUBS, International Union of Biological Sciences; LTER, Long-Term Ecological Research; LTER Papers, Ocean Informatics Design Studio archives, Scripps Institution of Oceanography, La Jolla, CA; NAS, National Academy of Sciences; NIH, National Institutes of Health; NODC, National Oceanographic Data Center; NSF, National Science Foundation; ONR, Office of Naval Research; RP, Roger Revelle Papers, MC6, Scripps Institution of Oceanography Archives, La Jolla, CA; ORNL, Oak Ridge National Laboratory; SCAR, Scientific Committee on Antarctic Research; SIO, Scripps Institution for Oceanography, La Jolla, CA; USNC/IBP, U.S. National Committee for the IBP; USNC/IGY, U.S. National Committee for the IGY; WDC, World Data Centers.

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*Historical Studies in the Natural Sciences*, Vol. 40, Number 2, pps. 183–224. ISSN 1939-1811, electronic ISSN 1939-182X. © 2010 by the Regents of the University of California. All rights reserved. Please direct all requests for permission to photocopy or reproduce article content through the University of California Press's Rights and Permissions website, <http://www.ucpressjournals.com/reprintinfo.asp>. DOI: 10.1525/hsns.2010.40.2.183.

new, potentially complementary model of Big Science encompassed the field practices of ecologists and suggested a model for the natural historical sciences to achieve the stature and reach of the experimental physical sciences. However, the program encountered difficulties when the institutional structures, research methodologies, and data management implied by the Big Science mode of research collided with the epistemic goals, practices, and assumptions of many ecologists. By 1974, when the program ended, many participants viewed it as a failure. However, this failed program transformed into the Long-Term Ecological Research program. Historical analysis suggests that many of the original incentives of the program (the emphasis on Big Data and the implementation of the organizational structure of Big Science in biological projects) were in fact realized by the program's visionaries and its immediate investigators. While the program failed to follow the exact model of the International Geophysical Year, it ultimately succeeded in providing a renewed legitimacy for synoptic data collection in biology. It also helped to create a new mode of contemporary science of the Long Term Ecological Research (LTER Network), used by ecologists today.

KEY WORDS: Big Science, International Geophysical Year (IGY), International Biological Program (IBP), Roger Revelle, data-driven research, data management

In 1961 Alvin Weinberg, director of the Oak Ridge National Laboratory and a member of the President's Science Advisory Committee, reflected on the phenomenon he called Big Science. He pointed out that since World War II, academic research had increasingly become bonded to big government and big industry, transforming science from an individual initiative into a collective enterprise, with big interdisciplinary government-funded teams of researchers as a major feature of this novel organizational form of scientific research.<sup>1</sup> After Weinberg's influential essay, Big Science became identified with the changes in the organization of scientific research that placed scientific production in line with postwar modernization and economic growth, appropriating the lessons of wartime mobilization of science. In the 1960s, as the term Big Science gained currency in the United States, the Manhattan Project and the national space program—centralized, large-scale scientific research efforts of unprecedented magnitude—had, for many people, become paradigms of Big Science.

Big Science is usually understood as an organizational form of science that was exemplified in the postwar weapons laboratories, high-energy physics

1. Alvin M. Weinberg, "Impact of Large-Scale Science on the United States," *Science* 134 (1961): 161–64; see also Alvin M. Weinberg, *Reflections on Big Science* (Oxford: Pergamon Press, 1967).

operations and installations, rockets, and superconductors.<sup>2</sup> In this paper we seek to extend the historiographic account of Big Science, drawing attention to a specific mode of large-scale organization of research that differed from the one implemented in high-energy physics laboratories and the U.S. (or Soviet) space program. The International Geophysical Year (IGY, 1957–1958), we argue, provided scientists with a model of Big Science that was different in some crucial ways from physicists’ undertakings. One of the important features of the IGY was its data-driven mode of research, as contrasted with the hypothesis- or instrument-driven mode of most physicists’ work and with the platform-driven character of much of the space program.<sup>3</sup>

While geophysicists in the 1950s were interested in many theoretical questions, the IGY, as a Big Science project, largely lacked targeted theoretical drivers. Its foci were supplied by various motivations to collect data around the globe. To be sure, individual scientists may have had theories that informed their methodologies and interpretative frameworks; however, the motivation for the overall project was not to test any hypotheses or theories but, primarily, to collect data. Although Big Science in physics and geophysics shared many organizational features, the Big Science of the IGY was distinguished by its emphasis on and the visibility of Big Data—a synoptic collection of observational data on a global geographic scale. The IGY World Data Centers (WDC), created to keep and organize the IGY data, were not only one of the very important aspects of the success of the IGY, they were also an innovative offshoot

2. See for example: James H. Capshew and Karen Rader, “Big Science: Price to the Present,” *Osiris*, 2nd series 7 (1992): 3–25; Peter Galison and Bruce Hevly, eds., *Big Science: The Growth of Large-Scale Research* (Palo Alto, CA: Stanford University Press, 1992); Catherine Westfall, “Rethinking Big Science: Modest, Mezzo, Grand Science and the Development of the Bevalac, 1971–1993,” *Isis* 94 (2003): 30–56; Daniel Kevles, “Big Science and Big Politics in the United States: Reflections on the Death of the SSC and the Life of the Human Genome Project,” *Historical Studies in the Physical and Biological Sciences* 27, no. 2 (1997): 269–97.

3. On the distinction between “data-driven” and “hypothesis-driven” modes of research, as distinct scientific epistemologies, see D. B. Kell and S. G. Oliver, “Here is the Evidence, Now Where is the Hypothesis? The Complementary Roles of Inductive and Hypothesis-Driven Science in the Post-Genomic Era,” *Bioessays* 26 (2003): 99–105; Ulrich Krohs and Werner Callebaut, “Data without Models Merging with Models without Data,” in *Systems Biology: Philosophical Foundations*, ed. F. C. Boogerd, F. G. Bruggeman, J. S. Hofmeyr, and H. V. Westerhoff (Amsterdam: Elsevier, 2007): 181–213; Sabina Leonelli, “On the Locality of Data and Claims about Phenomena,” *Philosophy of Science* 76 (2009): 5–14. For historical accounts of theory-driven and instrument-driven research in physics see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997). On platform-driven research in the space program see David de Vorkin, *Science with a Vengeance: How the Military Created the U.S. Space Science after World War II* (New York: Springer-Verlag, 1992).

of the IGY, and its only institutionalized form that was a significant scientific, institutional, and social resource.

The International Biological Program (IBP, 1964–1974), conceived shortly after the end of IGY, intended to emulate it by setting up a worldwide research initiative to accumulate a vast array of datasets on different living phenomena on a global scale, deploying standardized methods and interdisciplinary collaborations. The IBP is often referred to as one of the first realized Big Science projects in biology, and is often cited anecdotally as a biological version of the IGY.<sup>4</sup> Chunglin Kwa, in discussing the organization of the Grassland Biome—the most Big Science–like project launched under the auspices of the IBP—pointed out that the IBP adopted many features of the organizational culture typical for Big Science as described by Alvin Weinberg.<sup>5</sup> We argue in this paper, however, that although Big Science of the large physics and engineering operations was a reference point for the IBP science, the model for the IBP was not physics but geophysics. The history of the attempts to emulate the IGY in biology provides a window into a particular model of Big Science, driven by data and centered on field observations. In the United States in particular, the IBP was seen by its planners as a means to promote a Big Science model of research in biology and to transform ecology—one of the most naturalistic and “little science” fields of biology—into a modern Big Science.

The model of Big Science provided by the IGY was closer to extant field practices of ecologists than the model provided by the Manhattan Project or the space program. It nevertheless met substantial difficulties when the institutional structures it required were set in motion. Data organization turned out to be a major problem, as attempts to emulate the IGY World Data Centers—to store and to distribute data and the information accumulated during the IBP—failed notoriously, creating bad publicity for the program and a long-lasting memory

4. See Joel B. Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology* (New Brunswick, NJ: Rutgers University Press, 1992); Frank Benjamin Golley, *A History of the Ecosystem Concept in Ecology: More Than the Sum of the Parts* (New Haven: Yale University Press, 1993); Chunglin Kwa, “Representations of Nature Mediating between Ecology and Science Policy: The Case of the International Biological Programme,” *Social Studies of Science* 17 (1987): 413–42; Chunglin Kwa, “Modeling the Grassland,” *Historical Studies in the Physical and Biological Sciences* 24, no. 1 (1993): 125–55; Toby A. Appel, *Shaping Biology: The National Science Foundation and American Biological Research, 1945–1975* (Baltimore: Johns Hopkins University Press, 2000); Sharon E. Kingsland, *The Evolution of American Ecology, 1890–2000* (Baltimore: Johns Hopkins University Press, 2005); Conrad Waddington, “The Origin,” in *The Evolution of IBP*, ed. E. B. Worthington (Cambridge: Cambridge University Press, 1975), 4–11.

5. Kwa, “Modeling the Grassland” (ref. 4), 127.

of felt failure. Tracing the history of the IBP from its conceptualization in 1958 to its transformation into the Long-Term Ecological Research program (LTER) after the official end of the IBP in 1974 allows us to see, nevertheless, that the original emphasis on Big Data and the Big Science organizational features in biological research were finally fully legitimized by the program's visionaries and its immediate investigators. The Big Science programs in biology failed to follow the exact model of IGY, but they ultimately succeeded in providing a renewed legitimacy for synoptic data collection in biology. Large-scale data collection in biology, intrinsic to nineteenth-century natural history, astronomy, oceanography, and other examples of Humboldtian science, acquired a new significance in the 1950s and 1960s as a part of the Big Science enterprise in a world shaped by the post-atomic age and Cold War sensibilities. In the process, the template for Big Science in biology was transformed by the negotiations among scientific communities, funding bodies, and the U.S. public.<sup>6</sup>

#### **BIOLOGY AND OTHER NON-GEOPHYSICAL SCIENCES IN THE IGY**

The International Geophysical Year is remembered as a remarkably successful cooperative scientific effort of worldwide simultaneous geophysical observations, which created new forms of international scientific collaboration and organizational infrastructure for large-scale data-driven research. Well before the official end of the program in December 1958, it had become clear that the worldwide, coordinated cooperative effort of simultaneous geophysical observations had proven more valuable than even the most optimistic forecasts. Occurring at the time of the Cold War, the IGY transcended national and professional boundaries, created new forms of international scientific collaborations, and produced valuable scientific data and results. Chief among these were the launching of the first artificial satellites, beginning with Sputnik as a Russian contribution to the IGY; the exploration of Antarctica; and the detection of Van Allen belts (radiation belts around Earth named for their discoverer, James Van Allen).<sup>7</sup> The success and scale of the IGY captured

6. This paper is largely, although not exclusively, an American story. While there was an international dimension to these programs, the American IBP story more than in other countries was shaped by the efforts of American scientists to negotiate the recognition of the Big Science biological program by funding bodies, scientific communities, and the public.

7. On the history of the IGY see Rip Bulkeley, "The Sputniks and the IGY," in *Reconsidering Sputnik: Forty Years since the Soviet Satellite*, ed. Roger D. Launius, John Logsdon, and Robert

the imagination of the larger scientific world and engendered enthusiasm for projects that previously had not been considered possible or even imaginable, such as the International Year of the Quiet Sun, International Cooperation Year, International Year for the Preparation of Disarmament, and the Freedom from Hunger Campaign.

The possibility of including biological sciences within the scope of IGY activities was discussed from its earliest planning days. At the fifth meeting of the U.S. National Committee for the IGY (USNC/IGY) in March 1955, the Executive Committee adopted a resolution encouraging “the implementation of all types of non-geophysical research during the IGY, particularly in the biological sciences.”<sup>8</sup> At the suggestion of Hugh Odishaw, the Executive Secretary of USNC/IGY, an advisory committee was formed to evaluate proposals in these other fields. In 1956 and early 1957, a series of USNC/IGY meetings was devoted to discussing the possibility of collecting botanical and zoological data and specimens, and of launching studies of human psychology and physiology, as well as marine biology and geology of the Antarctic region.<sup>9</sup>

Although the idea to extend the IGY beyond geophysical areas was supported by many members of the Executive Committee, a few, including the IGY’s major architect, Lloyd Berkner, expressed reservations “in view of the possibility of causing . . . financial embarrassment.”<sup>10</sup> The special status of the Antarctic Research program provided an opportunity to extend the IGY programs beyond geophysics by attracting additional funding sources rather

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W. Smith (Amsterdam: Harwood Academic Press, 2000), 125–60. Patrick McCray, “Amateur Scientists, the International Geophysical Year, and the Ambitions of Fred Whipple,” *Isis* 7 (2006): 634–58; Allan A. Needell, *Science, Cold War, and the American State: Lloyd V. Berkner and the Balance of Professional Ideals* (Amsterdam: Harwood Academic Publishers, 2000); Fae L. Korsmo, “Shaping Up Planet Earth: The International Geophysical Year (1957–1958) and Communicating Science through Print and Film Media,” *Science Communication* 26 (2004): 162–87; Fae L. Korsmo, “The International Geophysical Year of 1957 to 1958: An Analysis of Why Both the Executive and Legislative Branches of the U.S. Government Funded the IGY 1957 to 1958,” *Science, People and Politics* 2, no. 1 (2007), available online at <http://www.gavaghancommunications.com/sp-pkorsmoigy.html> (last accessed 3 Mar 2010); Christy Collis and Klaus Dodds, “Assault on the Unknown: The Historical and Political Geographies of the International Geophysical Year (1957–8),” *Journal of Historical Geography* 34, no. 4 (2008): 555–73; Simon Naylor, Katrina Dean, and Martin Siegert, “The IGY and the Ice Sheet: Surveying Antarctica,” *Journal of Historical Geography* 34, no. 4 (2008): 574–95.

8. “Advisory Committee for Special Studies,” IGY Papers, Drawer 28, Regional Programs: Antarctic: Special Studies, Medical-Biological: General Correspondence, 1955–1958.

9. Hugh Odishaw, 8 Mar 1957, IGY Papers, *ibid.*

10. “Advisory Committee for Special Studies,” IGY Papers, *ibid.*

than by diverting funds allocated for geophysics programs to other areas of research (the latter scheme was apparently Berkner's concern). The recognition of the military and diplomatic importance of the Antarctic region, and the generous support of the IGY Antarctic component by the military, was enough to justify the extension of the IGY activities there. As Berkner's biographer, Allan Needell, has pointed out, the national security and scientific motivations behind the IGY blended when the idea of the IGY gained momentum. The IGY developed its programs with multiple motivations and multiple goals, having important political and intelligence dimensions along with scientific ones.<sup>11</sup> Likewise, the inclusion of non-geophysical research programs within the IGY was motivated by an amalgam of political, intelligence, and scientific goals.

The unique environment of the Antarctic region provided the rationale for envisioning projects in several biological domains, related to its distinctive microflora and fauna, the adaptability of humans in extreme conditions, and even, as one of the proposals stated, "to examine speculative theories on 'life from outer space.'"<sup>12</sup> And then there was dentistry. The Committee of Dentistry of the Division of Medical Science at the National Academy of Sciences (NAS) insisted that the possibilities for dental research, which included testing various materials and devices as well as psychosomatic factors and stress on human physiology, receive special consideration. Dental disorders, which caused considerable trouble during the early polar expeditions, became the justification for including within the IGY Antarctic program a sizable dentistry research effort, supported by the U.S. Navy's Bureau of Medicine and Surgery.<sup>13</sup>

In January 1957 the Executive Committee approved the inclusion of non-geophysical studies in IGY Antarctic programs. This, in turn, provided the IGY planners with a precedent to consider other IGY-related but non-geophysical programs outside the Antarctic environment. The Executive Committee authorized Odishaw to establish a special committee for non-geophysical IGY programs in the areas of "first, geology, tectonology and volcanology; second, geography, cartography and exploration; and third, medicine, biology (including studies of flora and fauna), and psychology (particularly studies of man in

11. Needell, *Berkner* (ref. 7).

12. "Antarctic Microbiology Research Program," draft of 25 Mar 1957, IGY Papers (ref. 8).

13. "Non-Geophysical Research during 1957 Period of the IGY Antarctic Program," 4 Feb 1957, IGY Papers, *ibid.*

arduous environment).<sup>14</sup> The resolution adopted by the Executive Committee encouraged establishment of the special advisory committee “to facilitate the participation of American biologists [in the IGY] . . . and to encourage . . . biologists to contribute to future world-wide scientific surveys.”<sup>15</sup>

The Advisory Committee for Special Studies, responsible for the evaluation of the projects in biological and medical sciences proposed for inclusion in the IGY program, was established in March 1957. Committee membership included Frank L. Campbell, the representative of the NAS Division of Biology and Agriculture; W. Consolazio of the National Science Foundation (NSF); Charles L. Dunham, the Atomic Energy Commission’s (AEC) Director of the Division of Biology and Medicine; Ernest Allan of the National Institutes of Health (NIH); Hayden Cox of the American Institute of Biological Sciences; Howard Page of the Psychological Sciences Division of the Office of Naval Research (ONR); and O. E. Reynolds representing the Biological Sciences Division of ONR.<sup>16</sup> The approved projects were in the areas of hydrobiology (mostly those that emphasized ecological and productivity studies), marine and terrestrial microbiology, botany, and zoology (studies of flora and fauna of Antarctica and programs in bird and seal banding), physiology (studies of a human’s ability to adapt to extremely severe environments), and psychology (studies on the influence of stress, isolation, and deprivation on the personnel of the polar stations).<sup>17</sup>

The biological projects under the auspices of the IGY were less integrated than the geophysics’ work, partly because of the variety of financial sources involved. In contrast to the centralized worldwide coordination of the geophysical research initiatives during the IGY, the biological activities were without exception the result of individual initiative, with support coming from diverse sources, including USNC/IGY, ONR, and the U.S. Naval Support Force, U.S. Navy (for dentistry) and U.S. Air Force (supporting projects in

14. “Advisory Committee for Special Studies: USNC/IGY Ninth Meeting, November 7, 1955,” IGY Papers, *ibid.*

15. “Resolution Concerning Participation of Biologists in the International Geophysical Year,” n/d, IGY Papers, *ibid.*

16. R. C. Peavey, W. M. Smith “Establishment of Advisory Committee for Special Studies,” 21 Mar 1957, IGY Papers, *ibid.*

17. As Fae Korsmo documents, biological research was also done in conjunction with glaciological projects under the auspices of IGY. Fae L. Korsmo, “Glaciology, the Arctic, and the U.S. Military, 1945–1958,” in *New Spaces of Exploration: Geographies of Discovery in the Twentieth Century*, ed. Simon Naylor and James Ryan (New York: I. B. Tauris, 2010), 125–47.

physiology and psychology).<sup>18</sup> USNC/IGY played the role of clearinghouse and evaluated the scientific merit of the submitted proposals.<sup>19</sup>

The organizational flexibility and diversity of funding sources furthered the continuation of the IGY-initiated biological programs into the post-IGY years. The Antarctic programs in biology, in particular, continued to flourish, as the emphasis of the other post-IGY Antarctic programs shifted to other fields, particularly geology. During the final stages of the IGY, the Scientific Committee on Antarctic Research (SCAR) was organized to continue and coordinate national activities on this continent. In the 1958–1959 austral summer, six long-term biological programs were launched as part of the U.S. Antarctic Program. In summer 1959–60 biologists and geologists outnumbered other specialists in all teams participating in the U.S. Antarctic Research Program.<sup>20</sup> In 1962–1963 fifteen field biological programs operated in Antarctica and an exploratory survey was made to investigate possible sites on the Palmer Peninsula for the first U.S. Antarctic station designated primarily for biological research.<sup>21</sup>

Biological programs launched under the auspices of the IGY became the first testing ground for the planning of the IBP. As the report submitted to the First General Meeting of the IBP noted, “the activities of SCAR over the past eight years have in certain respects paralleled those envisaged by IBP.”<sup>22</sup> In 1964, when the planning for the IBP started to take shape, biological research in the Antarctic included programs in terrestrial ecology and productivity studies, soil bacteriology, conservation, freshwater research (including biomass measurements and measurements of primary production), marine biology, and research on human adaptation.

Many proposals for biological research within the IGY, submitted to the IGY Committee for Special Studies, emphasized the value of worldwide ecological and productivity studies conducted simultaneously in the Antarctic and the Atlantic, Western Pacific, and Mediterranean Oceans, in conjunction with geophysical projects. The inclusion of the non-geophysical programs within the IGY not only extended its scope, but also the very concept of the worldwide survey of the Earth, complementing geophysical data with biological data to

18. *IGY Bulletin* (1957–1965), Sep 1960.

19. S. R. Galler to William Smith, 8 Mar 1957, IGY Papers (ref. 8).

20. *IGY Bulletin*, Sep 1960.

21. *IGY Bulletin*, Sep 1963.

22. M. W. Holdgate, “Biological Activities of SCAR,” IBP Papers, Series 6: ICSU, Folder Special Committee on Antarctic Research (SCAR) 1964.

create a broader and unifying framework of the studies of the environment. Within this framework, the properties of the Earth were regarded as determined not only by the chemical and physical properties of the Earth but also by biological activities, which in turn were controlled, at least partially, by the physical properties of the environment. Hence, if the living organisms could be studied as definitive indicators of physical and chemical processes, then the worldwide surveys should include monitoring changes in world fauna and flora as correlated with geophysical changes.<sup>23</sup>

### **The Proposed Inclusion of the Social Sciences within the IGY**

A broad vision of the IGY not only linked geophysical sciences to biology but also, through biology, the physical to the social sciences. In July 1957, a meeting on biology and IGY included a proposal for a “Committee for Biological and Social Sciences in Conjunction with the IGY,” submitted by medical physicist Stanley H. Clark and sociologist Joseph B. Ford.<sup>24</sup> The IGY program, they suggested, could be connected to social-scientific questions, such as bioclimatology, biometeorology, medical and psychological studies, and sociological, ecological, and criminological investigations.<sup>25</sup> Clark and Ford proposed to include Ludwig von Bertalanffy as a committee member, not only stressing the necessity of interdisciplinary communication but also implicitly suggesting the possibility of importing a theoretical framework from systems theory to facilitate the kind of interdisciplinary communication envisioned in their proposal.<sup>26</sup> In the 1950s Bertalanffy had promoted general systems theory as a framework for the formulation of concepts and principles of organization valid for systems in general. That is to say that underlying structural similarities or isomorphisms in different fields—for example, physics, biology, engineering, sociology—provided a basis for their unification.<sup>27</sup> Clark and Ford stressed not only that the social sciences could profit from inclusion in the IGY, but that the IGY might also profit from

23. “Resolution Concerning Participation of Biologists in the International Geophysical Year,” n/d, IGY Papers (ref. 8).

24. Frank Campbell, Memorandum on “Biology and IGY,” 5 Aug 1957, IGY Papers, *ibid.*

25. Stanley H. Clark and Joseph B. Ford to Detlev Bronk, 12 Feb 1957, IGY Papers, Drawer 31, Miscellaneous IGY Files: Coordinating Committee for Biological and Social Sciences: proposed.

26. *Ibid.*

27. On the “systems movement” inspired by Bertalanffy in postwar America, see Debora Hammond, *Science of Synthesis. Exploring the Social Implications of General Systems Theory* (Boulder: University Press of Colorado, 2003).

the social sciences.<sup>28</sup> Moreover, the IGY itself could be studied by social scientists, perhaps leading to a better understanding of science.<sup>29</sup>

This proposal in particular, and the possibility of introducing the needs of social science into IGY planning in general, were discussed in August 1957 at a meeting with the representatives of governmental agencies, such as NIH, ONR, NSF, AEC, the Fish and Wildlife Service, and the Office of the Surgeon General.<sup>30</sup> Although the idea received attention, the committee concluded that the proposal was too late in coming, too far from the core of the program, and perhaps a bit too random.<sup>31</sup>

The establishment of the Coordinating Committee for Biological and Social Sciences in conjunction with the IGY also appeared to be more difficult to accomplish than the Committee for Special Studies that oversaw projects in earth sciences, biological sciences, and medical sciences. NAS executive officers S. D. Cornell and R. C. Peavey, responding to a request by Hugh Odishaw to review the proposal, expressed their concerns pointing out that the proposal appeared to be “somewhat different” from earlier proposals to include non-geophysical sciences in the IGY programs that suggested the establishment of the committees for “(i) medical and physiological projects; (ii) zoological and botanical projects, and (iii) associated physical sciences, e.g., geology, volcanology.” They concluded: “We are aware that we must deal without delay with proposals in the latter category.”<sup>32</sup>

So the proposal died, but not without making an impression on Odishaw, who revived some of its arguments in an article published in *Isis* in 1962. Odishaw called scholars’ attention to the IGY’s World Data Centers as a unique offshoot of the IGY that attempted “formally to establish a method for interchange of raw or semi-processed data . . . [and where] decisions on data interchange were made by the scientific community [and] in each field, the specialists themselves determined the nature and form of data interchange.” The IGY and its data centers could become a subject for future historians, sociologists, and students of public affairs.<sup>33</sup>

28. Frank Campbell, Memorandum on “Biology and IGY,” 5 Aug 1957, IGY Papers (ref. 8).

29. Stanley H. Clark and Joseph B. Ford to Detlev Bronk, 12 Feb 1957, IGY Papers (ref. 25).

30. Frank Campbell, Memorandum on “Biology and IGY,” 5 Aug 1957, IGY Papers (ref. 8).

31. *Ibid.*

32. “Establishment of Coordinating Committee for Biological and Social Sciences in conjunction with the IGY,” memorandum of 8 Apr 1957, IGY Papers (ref. 25).

33. Hugh Odishaw, “What Shall We Save in the Geophysical Science?,” *Isis* 53, no. 1 (1962): 80–86.

So while the IGY did not include the social sciences, Odishaw recognized its social implications as a model of scientific cooperation and data exchange. The IGY World Data Centers had been created to process the data resulting from IGY activities, and Odishaw highlighted them as an innovative and valuable offshoot. Indeed, the World Data Centers were a very important (although little emphasized) aspect of the success of the IGY, and were the only institutionalized form of it that continued to operate after the year was over.<sup>34</sup> Moreover, the WDC were used by the IBP planners as a model for the organization of their data, too.

### IGY World Data Centers and Biological Data

The organization of the IGY data, and the assurance that it would be properly stored, indexed, catalogued, and available for researchers, was the subject of intense deliberations for two years preceding the IGY. It was clear from the outset that vast quantities of data would be collected; with ten thousand scientists and amateur observers from more than sixty nations taking measurements and observations at more than two thousand stations around the world, this expectation was easily fulfilled.<sup>35</sup> According to the participants' reports, published in the *Annals of the IGY*, the representatives from the United States and the Soviet Union competed to establish centers for the complete set of IGY records during one of the first organizational meetings of Comité Spécial de l'Année Géophysique Internationale (CSAGI) in 1955.<sup>36</sup> Ultimately, three centers were created: one in the United States (WDC-A), one in the USSR (WDC-B), and one center intended to serve Western Europe and the Pacific (WDC-C). Each center consisted of a number of scientific institutes in different geophysical disciplines that provided storage for the IGY data. Data reaching one center would be exchanged with the other two, insuring that each center held a complete set of the data.<sup>37</sup> IGY publications presented this triplicate organization of WDC as a means to assure the safety of the information and the geographic accessibility to users.<sup>38</sup>

34. See Stanley Ruttenbergh and Henry Rishbeth, "World Data Centers—Past, Present and Future," *Journal of Atmospheric and Terrestrial Physics* 56, no. 7 (1994): 865–70; Fae L. Korsmo, "The Origins and Principles of the World Data Center System," *Data Science Journal* 8 (2010): 55–65.

35. On amateur observers contributing to the IGY see McCray, "Amateur Scientists" (ref. 7).

36. *IGY Bulletin*, no. 2 (Aug 1957). For the detailed history of the origin of the concept of the IGY WDC, see Korsmo, "Origins" (ref. 34).

37. *Annals of the IGY (1957–1970)*, vol. 2 A (1957–1958), 367–68.

38. Hugh Odishaw, "International Geophysical Year: A Report on the United States Program," *Science* 127 (1958): 115–28; Odishaw, "What Shall We Save?" (ref. 33).

However, as Diana Crane has pointed out, the map of the data centers mirrored the geopolitical map of the Cold War world, even as the WDC concept enshrined ideals of scientific internationalism.<sup>39</sup>

By the beginning of the IGY, all three centers were established and functional. The WDC-A in the United States was divided by disciplines among eleven institutions that together represented all IGY disciplines.<sup>40</sup> Working groups in each discipline designed detailed data management plans, establishing procedures for data submission and data services, and provided scientists with instructions concerning which data to forward to the center, what format those data should take, what categories of data the centers would store, and the policies regarding the time schedule for dispatch of data, plans for interchange of data between the branches, and possible forms of final publications.<sup>41</sup> The nature and form of data stored at WDC varied. In some disciplines, copies of original records were exchanged (ionogram films, all-sky camera observations of aurorae); in others, tabular observational data were collected (e.g., meteorological observations and cosmic ray count rates). In several disciplines, such as glaciology, rocketry, and satellites, the volume of raw data exchanged was small and data were provided mostly in the form of reports and publications.<sup>42</sup>

The question of where to keep the biological data collected under the auspices of the IGY was not resolved or even seriously posed during the program's lifetime, although the WDC had been planned only for data collected by geophysicists and in closely related fields. While it was assumed that IGY geophysical data would be used by scientists in other fields, the reverse was not assumed: IGY biological data were not supposed to be kept in the World Data Centers.<sup>43</sup>

In practice, biological data collected in conjunction with the IGY projects (such as oceanographic cruises) mostly ended up with geophysical data at the

39. Diana Crane, "Transnational Networks in Basic Science," *International Organization* 25, no. 3 (1971): 585–601. In the post-IGY time the principle of multiple data set copies was modified and the WDC data policy concerning the keeping of three identical copies was not extended to newer types of data (ICSU Panel on WDC, *Guide to the World Data Center System*, 5th ed. (Boulder, CO: World Data Center A, NOAA/NGDC, 1987)

40. *IGY Bulletin*, no. 2. Each U.S. institution that served as an archive center for the IGY had "a history of active scientific interest and competence in the given geophysical discipline": Odishaw, "What Shall We Save?" (ref. 33).

41. *Annals of the IGY* (1957–1958), 614–20.

42. On military limitations and regulations concerning data on rockets and satellites and on the experiments in space see Walter Sullivan, *Assault on the Unknown: The International Geophysical Year* (New York: McGraw-Hill Book Company, 1961).

43. M. Oakles, "Inter-Office Memorandum," 28 Mar 1957, IGY Papers (ref. 8).

WDC anyway. The biological observations were included as one of the provisional types of data that would flow to the IGY WDC-A: Oceanography, one of eleven sub-centers that comprised WDC-A.<sup>44</sup> The *WDC Guide* specified that biological data forwarded to the IGY WDC-A: Oceanography should provide the quantity and composition of plankton layers in the standardized format, accompanied by a chart showing the positions of cruise stations at which the observations were made.<sup>45</sup> Such data could then be transferred in a machine-readable form (which at that time meant punched IBM cards), run through an IBM 709 computer operated at the Texas A&M College's Data Processing Center, and then displayed in a way suitable for the work envisioned. In practice, biological material collected during the IGY's oceanographic cruises turned out to be much more diverse and heterogeneous than anticipated. In 1961, Luis R. A. Capurro, the Associate Director of the IGY WDC-A: Oceanography, together with Maxwell S. Doty, Professor of Botany at the University of Hawaii, tried to assemble all biological data on plankton collected during the IGY in WDC-A: Oceanography. They succeeded in displaying a large amount of biological data in a serial IGY-WDC publication.<sup>46</sup> However, this experience demonstrated many difficulties in organizing and handling the biological data on marine productivity. The plankton measurements were obtained with widely differing techniques (including the radiocarbon method, pigment analysis, and estimations of plankton biomasses), and were not standardized and thus hard to compare. As a result, the presentation of a coherent collection of biological measurements was achieved, but at the cost of the exclusion of vast amounts of data.<sup>47</sup> These difficulties resurfaced a few years later during the IBP.

### **THE IGY AS A MODEL FOR THE IBP: CREATING A CONCEPTUAL AND INSTITUTIONAL FRAMEWORK FOR A LARGE-SCALE DATA-DRIVEN PROGRAM IN BIOLOGY**

As the IGY came to a close, biology emerged as the scientific field most likely to profit from a similar large-scale international effort. Insiders' histories of the IBP trace the beginning of the program back to an historical conversation

44. *Annals of the IGY* (1957–1958), 629.

45. ICSU Panel on WDC, *Guide to the World Data Center System* (Boulder, CO), 300.

46. Maxwell S. Doty and Luis R. A. Capurro, eds., *Productivity Measurements in the World Ocean* (IGY Oceanography Report No. 4, 1961).

47. Only data obtained by the radiocarbon ( $C^{14}$ ) method and phytoplankton pigment analyses have been presented in the publication: *ibid.*, iii.

similar to the one that launched the IGY. As the story goes, in March 1959, Lloyd Berkner, past President of the International Council of Scientific Unions (ICSU) and one of the major visionaries of the IGY; British physicist Rudolph Peters, then President of ICSU; and Italian geneticist Guiseppe Montalenti, then President of the International Union of Biological Sciences (IUBS), began to envisage the biological version of the IGY.<sup>48</sup> What was initially called the International Biological Year was conceived by the Executive Board of ICSU in October 1959 as a logical continuation and an extension of the forms of organization of scientific research propelled during the IGY. However, the idea of a single year (or even eighteen months, as the IGY turned out to be) was soon rejected as too limited a time period to accomplish the organized collection of a significant amount of biological information.<sup>49</sup> Instead, a five- to seven-year International Biological Program was proposed, to be operated according to the same organizational scheme used for the IGY. The national committees for the programs would be set up in each participating country under the auspices of the National Academy of Sciences (NAS).

Although the general organizational scheme of the IBP was similar to the IGY—big and centralized, financed, and structured around large collaborative projects involving constant sharing of resources and data among various institutions—the model quickly broke down. In the United States, it soon became apparent that the IBP functioned in a much less centralized and hierarchical manner than the geophysicists' program. The IBP accommodated far more small-scale investigations and local initiatives. In 1967, by the end of the preparatory phase and the beginning of the operational phase of the program, the IBP planners observed that “administration of the US/IGY was more centralized than that now planned for the US/IBP.”<sup>50</sup> Funding, particularly,

48. In the United States, the idea to have a biological program similar to IGY was discussed in the NSF as early as in 1956; see Appel, *Shaping Biology* (ref. 4), 228. On the history of the IBP see Kwa, “Representations of Nature” (ref. 4); Kwa, “Modeling the Grassland” (ref. 4); Chunglin Kwa, “Local Ecologies, Global Science: Discourses and Strategies of the International Geosphere-Biosphere Programme,” *Social Studies of Science* 35 (2005): 923–50; Golley, *History of the Ecosystem Concept* (ref. 4); Sharon E. Kingsland, *The Evolution of American Ecology, 1890–2000* (Baltimore: Johns Hopkins University Press, 2005); Hagen, *An Entangled Bank* (ref. 4). For the insiders' accounts of the IBP's history see: E. B. Worthington, ed., *The Evolution of IBP* (Cambridge: Cambridge University Press, 1975); W. Frank Blair, *Big Biology: The US/IBP* (Stroudsburg, PA: Dowden, Hutchinson & Ross, 1977).

49. Worthington, ed., *Evolution* (ref. 4).

50. The report highlighted at the same time the positive features of a decentralized mode of operation. “Information on the International Biological Program prepared for the Subcommittee

was much less centralized. For the IGY, the U.S. Congress had appropriated funds for the new research programs initiated by USNC/IGY, and specifically identified them in the National Science Foundation budget. Once identified, the disbursements of funds for the IGY were made in centralized fashion, at the request of the USNC/IGY technical panels. The panels also made the evaluations and decisions concerning these disbursements. In contrast, the financial support for the USNC/IBP's research projects was provided directly "to the proposal originator by the appropriate granting agency on the basis of evaluations and decisions made within the agency."<sup>51</sup>

An even greater difference between IGY and IBP was the lack of agreement among biologists on a conceptual core that would provide the justification for a worldwide cooperative scientific undertaking in biology.<sup>52</sup> Why was it difficult for biologists to identify a unifying theme for a worldwide biological program? In the IGY, the Earth itself was the unifying theme, and had long provided geophysicists with motivation and justification for international cooperation.<sup>53</sup> Its physical properties, from the upper atmosphere to the deep ocean floors, and from the tropics to the poles, provided geophysicists with a material object—the globe itself—on which they focused their program. Biologists, on the other hand, as F. Golley put it in his insider's historical account of the IBP, "were required to create a global purpose" for cooperation, being unable to turn the existing material object of their science into a unifying theme.<sup>54</sup> The debate about the conceptual core of a worldwide biological program persisted throughout the program, as the aims and purposes of the IBP were repeatedly questioned, reshaped, and refocused throughout its lifetime.

In 1961, when ICSU formally established the Planning Committee for the IBP, the conceptual areas of the planned program were defined by the areas of interest of the IBP promoters at ICSU: human heredity (suggested by IUBS

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on Science, Research and Development of the Committee on Science and Astronautics of the U.S. House of Representatives," 30 Aug 1967, IBP Papers, Series 8: Government, Folder Congress: Subcommittee on Science R&D: Information of IBP Prepared by NAS-IBP Staff.

51. Ibid.

52. Ibid.

53. For the history of large-scale projects in the earth sciences see Naomi Oreskes, "Earth Sciences, History of," *Encyclopedia of Physical Science and Technology, Third Edition*, Volume 4 (2002): 761–73; Ronald E. Doel, *Solar System Astronomy in America: Communities, Patronage, and Interdisciplinary Research, 1920–1958* (New York: Cambridge University Press, 1996); Ronald E. Doel, "Constituting the Postwar Earth Sciences: The Military's Influence on the Environmental Sciences in the USA after 1945," *Social Studies of Science* 33, no. 5 (2003): 635–66.

54. Golley, *History of the Ecosystem* (ref. 4), 111.

President G. Montalenti), plant genetics and breeding (suggested by G. L. Stebbins, the Secretary-General of IUBS), and conservation and the study of “natural biological communities which are liable to undergo modification or destruction,” proposed by Jean Baer, Chairman of the Division of Zoology of IUBS.<sup>55</sup> But only the latter theme felt global. Moreover, when Conrad Waddington assumed the leadership of IUBS in 1961, he was quite critical of the entire enterprise “of organizing something on a large worldwide scale, comparable to the IGY,” with “no firm grasp of how it should be financed or organized” and with a program comprised of “a small number of rather definite projects in fairly restricted areas.”<sup>56</sup> Waddington later admitted that his first impulse was “to kill the whole thing before it went any further.”<sup>57</sup>

But he didn't kill it. Instead, under Waddington's leadership, the proposal for IBP started to take shape. During 1962–1963 its core theme was narrowed to ecology, with emphasis on studies of biological productivity, food supply, and the human population. In 1964, the program formally began under the unifying theme “The Biological Basis of Productivity and Human Welfare.” Waddington later explained the change of the focus: “[We] felt that the only possible line would be to formulate a programme around something which was indubitably of major social and economic importance for mankind as a whole. . . . The most attractive field [for the IBP], I thought, was something to do with the way in which solar energy is processed by the biological world into the formation of complex molecules which man can use, as food or otherwise.”<sup>58</sup> The focus on biological productivity and Earth's biological resources also suggested a certain continuity with the IGY, as it assumed the collection of basic physical and biological environmental data on a global scale, transgressing the disciplinary boundaries between geophysics and biology. The unifying theme of global biological productivity required simultaneous, worldwide observations by methods that would ensure the standardization of data, similar to how it had been implemented during the IGY. The standardization of methods of observation was emphasized as an important feature of IBP-supported projects: “The IBP will afford means of standardizing observations and for establishing communication between investigations internationally.”<sup>59</sup> Global environmental measurements also promised to link

55. Waddington, “Origin” (ref. 4), 4.

56. *Ibid.*, 5.

57. *Ibid.*

58. *Ibid.*

59. “Information” 30 Aug 1967, IBP Papers (ref. 50), 10.

the biological sciences to oceanography, meteorology, and other disciplines concerned with the environment in a broad sense.

But there was a problem. Although the focus on biological productivity and human welfare provided biologists with a rationale for a global program, these themes held little interest for most of the immediate investigators. To many biologists, these themes did not sound like genuine *scientific* problems, or at least not *biological* research problems. Duke University zoologist David Livingston wrote, “it is important to all of us as *people*, but to few of us as *biologists*. There are interesting social, psychological and physiological aspects to the problem, but it is not a biological problem in the sense that cracking the DNA code or understanding the role of species diversity in natural communities are biological problems. The success of the IGY was due to its concern with genuine geophysical problems. It was not promoted to prevent earthquakes, control the weather or make skyscraper construction safer, but to understand the world. Our [biological] international program ought to have the same aim.”<sup>60</sup>

In the 1960s, as ecology and environmental politics evolved to the point where they became inseparable in the public imagination, the topic of biological productivity in its relation to environment and overpopulation problems had come to denote an overtly political concern. As Barry Commoner’s biographer Michael Egan put it, “professional ecologists found their discipline under siege by political activists.”<sup>61</sup> The political overtones of ecological and environmental problematics made the American planners and prospective investigators of the IBP skittish. During the 1960s, the rise of the radical social criticism and post-atomic cultural (and countercultural) movements created what historian Tina Stevens has called the “culture of post-atomic ambivalence” in which at least some American intellectual elites struck a posture of autonomy and independence from political interests.<sup>62</sup> The President of the National Academy of Sciences, Frederick Seitz, underscored that “the USNC should

60. D. A. Livingston to Frank Campbell, 14 Feb 1964, IBP Papers, Series 1: USNC/IBP: Ad hoc, Folder Membership: D. C. Frey: Survey of Biologists re Interest in IBP (emphasis added). Of course, the IGY *did* have pragmatic goals related to rocketry and communications; see Allan A. Needell, *Science, Cold War, and the American State: Lloyd V. Berkner and the Balance of Professional Ideals* (Amsterdam: Harwood Academic Publishers, 2000).

61. Michael Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism* (Cambridge, MA: MIT Press, 2007), 103.

62. Tina M. L. Stevens, *Bioethics in America: Origins and Cultural Politics* (Baltimore: Johns Hopkins University Press, 2000).

endorse the study of population biology [qua biology] . . . but should exclude controversial political overtones.”<sup>63</sup>

But the most important concerns remained scientific. To many people the whole project seemed misconceived. In the planning phase of the IBP American biologists accused the planners of endorsing a “me too” approach rather than justifying the IBP through genuine scientific needs.<sup>64</sup> Cornell ecologist Lamont Cole called the IBP “a boondoggle designed to ride the coattail” of the IGY, as he saw “nothing in IBP that would require international cooperation” nor any “attempt to make a case for simultaneous studies.”<sup>65</sup> Others, such as ecologist David Livingston, agreed.<sup>66</sup>

Biologists saw the IBP as mimicking the IGY without acknowledging the epistemic differences between the geophysical and biological sciences. Many stressed the differences between their practices and those of geophysical science, especially in their discussion of the standardization of data and measurements. This was the major requirement of a “synoptic data effort,” as Hugh Odishaw defined the impetus of the IGY.<sup>67</sup> But standardization appeared not only unrealistic in many areas of biological sciences, but also *undesirable*: such an effort would impede the development of their science rather than promote it. Harvard biological oceanographer Gordon Riley, for example, thought it was a waste of time:

I do not know how to make meaningful measurements of marine productivity. Chlorophyll and C<sup>14</sup> presumably can be standardized. But it so happens that nonliving organic matter frequently outweighs the phytoplankton, and a significant and eventually usable fraction of primary production is secreted by

63. Minutes of the 19–20 Mar 1966 meeting of the USNC/IBP, IBP Papers, Series 2: USNC/IBP, Folder Meetings: 1920 March 1966. Ironically, Seitz later engaged in many overtly political activities; see Naomi Oreskes and Erik M. Conway, *Merchants of Doubt* (New York: Bloomsbury, 2010).

64. Ecologist W. Edmondson summarized the common sentiment, noting: “When the matter first came up some years ago it was presented as having a biological equivalent of the International Geophysical Year, and the problem then seemed to be what could be done. Looking back on it, it is interesting that the decision seemed to be first ‘let us do something’ and later ‘what shall we do?’” W. T. Edmondson to David G. Frey, 9 Mar 1964, IBP Papers (ref. 60).

65. Lamont C. Cole to Frank Blair, 6 Mar 1964, IBP Papers, Series 1: USNC/IBP: Ad hoc, Folder Membership: Chairman: S. A. Cain. Survey of Biologists re Interest in International Biological Program (IBP).

66. D. A. Livingston to Frank Campbell, 14 Feb 1964, IBP Papers (ref. 60).

67. Hugh Odishaw, “International Geophysical Year,” *Science* 128, no. 3339 (1959): 1599–609.

the phytoplankton into the water and therefore cannot be measured by the C<sup>14</sup> method. These are matters for exploratory research. There are no standard methods. We are not sure yet just what it is we need to measure.<sup>68</sup>

Botanist Francis Raymond Fosberg viewed standardization as an effect rather than a cause of scientific progress:

For ecologists to be required to use specified methods . . . would . . . be a backward step. In the field of productivity most of the methods that I have heard of are so completely unconvincing that it would seem to be catastrophic to freeze any of them. I would much prefer to encourage originality and hope that some methods that would really measure productivity, or, more correctly, production, might develop . . . [T]he attempts to get existing workers to change what they are doing . . . do not appeal to me.<sup>69</sup>

Ecologists had already warned IBP planners about the difficulties they had experienced during the IGY when they tried to organize the biological data using the IGY/WDC system. Indeed, as the early experience with biological programs under the auspices of the IGY had shown, the IGY mode of research implied an inevitable centralization, leading to homogeneity in research methods and approaches. “Do the anticipated benefits from a ‘Big Science’ kind of biology outweigh those that we presently enjoy from the ‘Little Science’ nature which has always been the core of biology?” Fosberg asked.<sup>70</sup> The answer, for Fosberg at least, was no.

Ecologists were not unique in their negative assessment of the implications of Big Science projects. Alvin Weinberg, who was responsible for the original rhetorical framing of Big Science, also warned scientists and the public about the consequences of the “pathologies” the Big Science mode of scientific research embedded.<sup>71</sup> Because of their size and complexity, Big Science projects could not be possible without becoming embroiled in institutional, bureaucratic, and national politics. Centralization of the research decision-making through centralization of facilities was another issue that compromised the ideal

68. Gordon A. Riley to Bostwick H. Ketchum, 5 Mar 1964, IBP Papers, Series 1: USNC/IBP: Ad hoc, Folder Membership: B. H. Ketchum. Survey of Biologists re Interest in IBP. On Riley see Eric L. Mills, *Biological Oceanography. An Early History, 1870–1960* (Ithaca, NY: Cornell University Press, 1989).

69. F. Raymond Fosberg to Stanley Cain, 1 Apr 1964, IBP Papers (ref. 65).

70. F. Raymond Fosberg to Stanley Cain, 4 Apr 1964, IBP Papers, *ibid.*

71. Capshew and Rader, “Big Science” (ref. 2).

of science as the free pursuit of knowledge. While maintaining faith in the fundamental health of American science, and using Big Science rhetoric to advocate even larger funding allocations, Weinberg nevertheless argued that the funding for large-scale research should be confined to national laboratories, “to prevent the contagion” of Big Science from spreading.<sup>72</sup>

Others agreed. For many scientists in the 1960s, Big Science was a disturbing disease rather than a healthy growth of science to a more mature state. The Rockefeller Institute’s biologist Paul Weis, for example, wrote about “irrelevance, triviality, redundancy, lack of perspective, [and] an unbounded flair for proliferation” as “just some of the symptoms of Big Science in biology.”<sup>73</sup> Fosberg warned the planners of the IBP that “biologists, in their envy of the support afforded ‘Big Science’ in the fields of physics and oceanography, are attempting to change the character of biology, and particularly ecology, from its present emphasis on individual effort to something like the impersonal group effort nature of the multi-million dollar programs in the physical sciences.”<sup>74</sup> This, he concluded, was not a good thing.

Stanley Cain, one of the members of the Ad Hoc U.S. Committee on IBP, responded to these concerns by expressing an unusual faith in the U.S. Congress, suggesting that Congress would not fund the IBP if it were not good science, or at least good for science.<sup>75</sup> The die, however, was already cast, for the IBP was not merely modeled after the IGY, but actually born *within* it.

### **Roger Revelle and the Early Planning of the IBP**

The experience of the IGY affected and influenced the early planning of the IBP. The continuity between the two programs was assured through oceanographer Roger Revelle, who introduced many of the IGY ideals in the planning of the American contribution to the IBP.

Revelle entered the field of oceanography in the 1930s, when the subject was scarcely known in the United States. As Director of the Scripps Institution for Oceanography (SIO) since 1951, Revelle had continued the efforts of his

72. Alvin M. Weinberg, *Reflections on Big Science* (Oxford: Pergamon Press, 1967). Likewise, De Solla Price presented similar ideas in his essay on the dangers of large-scale research entitled “Disease of Science,” in Derek J. De Solla Price, *Science Since Babylon* (New Haven: Yale University Press, 1961). See discussion in Capshew and Rader, “Big Science” (ref. 2).

73. Paul Weiss, “Experience and Experiment in Biology,” *Science* 135 (1962): 468–71.

74. F. Raymond Fosberg to Stanley Cain, 4 Apr 1964, IBP Papers (ref. 65).

75. Stanley Cain to Lamont C. Cole, 10 Mar 1964, IBP Papers, *ibid*.

teacher, Harald Sverdrup, to transform this small institution into one of the major centers of oceanography in the world, promoting postwar oceanography through the powerful combination of military support and fundamental research.<sup>76</sup> Revelle participated in the IGY from its early stages as a member of the Technical Panel on Oceanography at USNC/IGY, and was elected to the National Academy of Sciences in 1958, during the IGY.<sup>77</sup> In 1961 he moved to Harvard University, where he established the Center for Population Studies, which he headed as director from 1964 to 1974 and continued as chair until 1978.<sup>78</sup>

In late 1964, when the National Academy of Sciences set up the U.S. Ad Hoc National Committee on the IBP, Revelle was asked to lead the program during its three-year planning period. Waddington was pleased with this appointment, remarking with satisfaction: “Americans put in charge someone with the reputation of a real thruster.”<sup>79</sup> Many American biologists, however, wondered why a physical scientist was heading the biological program. Cornell ecologist Lamont Cole suggested that it was “most unfortunate that the problem [of planning the IBP] was not at least handed to the two ecologists (A. E. Emerson and G. E. Hutchinson) who are the members of the [National] Academy [of Sciences].”<sup>80</sup> In fact, the NAS had solicited nominations for the IBP planning committee from the Ecological Society of America (ESA) and other biological societies.<sup>81</sup> Stanley Cain, a leading American ecologist and a former ESA president, was appointed a vice chairman of USNC/IBP. However, with no ecologists on the National Science Board, which directed NSF policy, the possibilities were limited for ecologists to lobby for their own candidates.<sup>82</sup>

76. Naomi Oreskes and Ronald Rainger, “Science and Security before the Atomic Bomb: The Loyalty Case of Harald U. Sverdrup,” *Studies in the History and Philosophy of Modern Physics* 31B (2000): 309–69; Ronald Rainger, “Patronage and Science: Roger Revelle, the U.S. Navy, and Oceanography at the Scripps Institution,” *Earth Sciences History* 19, no. 1 (2000): 58–89; Ronald Rainger, “Constructing a Landscape for Postwar Science: Roger Revelle, the Scripps Institution and the University of California, San Diego,” *Minerva* 39 (2001): 327–52; see also Chandra Mukerji, *A Fragile Power-Scientists and the State* (Princeton: Princeton University Press, 1990).

77. As a member of the ICSU Joint Committee on Oceanography since 1954, Revelle participated in the working groups designing the IGY’s oceanographic component (“International Council of Scientific Unions—8th General Assembly, Sep–Oct 1958,” RP, Box 7, Folder 49).

78. In this new position Revelle coupled the concerns of population change with broader environmental and social issues (RP, Revelle’s Oral History, 1984).

79. Waddington, “Origin” (ref. 4), 10.

80. Lamont Cole to Frank Blair, 11 Feb 1964, IBP Papers (ref. 65).

81. Stanley Cain to Lamont Cole, 10 Mar 1964, IBP Papers, *ibid*.

82. This point was made in Kwa, “Representations of Nature” (ref. 4), 416.

Despite these criticisms, Revelle was a feasible candidate for a chairman of the committee, given his strong credentials in international science, his reputation as a visionary institution-builder, and the declared purpose of the Ad Hoc Committee: not to promote the IBP among biologists but to explore the organizational and conceptual possibilities to implement the program in America. Revelle presented himself not as a physical scientist, but as an oceanographer, whom he jokingly defined as “a sailor who uses long words.”<sup>83</sup> Following a tradition that dated from Henry Bigelow, the first director of the Woods Hole Oceanographic Institution, Revelle saw oceanography as the scientific field that embraced any kind of research that might be done from a ship.<sup>84</sup> Bigelow had promoted the unity of three major oceanographic disciplines—geological, physico-chemical, and biological—bounded together by the “uniformity of the sea” and by the unifying influence of the international coordinating institutions aimed at synthetic investigations.<sup>85</sup> Revelle built on this vision of oceanography as a science that stretches from geotectonic research of the earth’s crust beneath the ocean to studies of oceanic life in all its interaction.<sup>86</sup> Biology, for Revelle, was one part of this bigger project.<sup>87</sup> During Revelle’s time at SIO, Revelle endorsed a bold vision of a “new marine biology” that relied upon and employed modern methods and theoretical principles.<sup>88</sup> As Ronald Rainger has pointed out, Revelle saw the “new marine biology” as a link that would fill the gap between the two branches of modern biology: molecular biology, rooted in physical and chemical sciences, and evolutionary biology, which linked natural history with genetics and relied on population genetics and probability models as a cornerstone of “neo-Darwinian synthesis.” The “new marine biology,” as an examination of organisms in relation to their “chemical ecology on the one hand, and the analysis of how turbulence, diffusion and movement of water masses influence population structure, on the other,” would bridge the two branches of modern biology in what Revelle called “a new synthetic

83. Sullivan, *Assault on the Unknown* (ref. 42), 346.

84. Henry B. Bigelow, *Oceanography, Its Scope, Problems and Economic Importance* (Boston: Houghton Mifflin Co., 1931).

85. *Ibid.*, 9.

86. Rainger, “Patronage and Science” (ref. 76).

87. *Ibid.*

88. See Rainger, “Constructing a Landscape” (ref. 76). Revelle’s involvement with biology continued in the late 1950s as the chairman of the Oceanography and Fisheries Panel of the Committee on the Biological Effects of Atomic Radiation. See Thomas F. Malone, Edward D. Goldberg, and Walter H. Munk, “Roger Randall Dougan Revelle, 1909–1991,” *Biographical Memoirs of the National Academy of Sciences* 75 (1998): 3–23.

biology.” In this way, Rainger concluded, “Revelle was calling for ‘a new biology,’ but one based on his views of physical oceanography.”<sup>89</sup>

This vision of a new biology that transcended disciplinary boundaries and was united by the subject of its study—the Earth’s biosphere—shaped Revelle’s vision for the IBP in a manner that was informed by the IGY: an extension of the geophysical description of the Earth to include its living component. Revelle saw the IBP as a way to investigate and correlate the dispersal patterns of geological, geophysical, atmospheric, and biological changes in the biosphere, where “man is becoming a geological and biological agent.”<sup>90</sup>

Exploring the Earth through “measuring the Earth” was the credo of the IGY, “a key to a complete understanding of the planet we live on.”<sup>91</sup> Measuring and quantifying “a thin web of living creatures”—the “web of life”—became one of the IBP’s major goals.<sup>92</sup> At the same time, this required an adjustment of the existing institutional and conceptual organization of the research in the life sciences. Revelle explained to the U.S. Congress: “the geophysicists [during the IGY] were generally engaged in a straightforward business of making various kinds of measurements . . . [I]n our case [of IBP] we have to find out what kind of measurements to make . . . We have to develop new methods and new techniques, and we have to train people because the kind of scientists who are needed for this program just do not exist in sufficient number.”<sup>93</sup> New techniques included the worldwide biological surveys by satellites, “to improve our description of the biosphere,” and to achieve “greater compatibility of methods of measurement and adequate arrangements for data handling.”<sup>94</sup> The planning for the IBP included not only research programs but also a program for training biologists “in modern methods of investigating natural and managed ecosystems.”<sup>95</sup> Ecology would be central to the IBP: “Ecology has been a science which inevitably tended to lag behind the laboratory biological sciences. . . . The time has come [for] ecology. [IBP] is a device for pushing ecology and

89. Rainger, “Patronage and Science” (ref. 76), 42.

90. IBP Press Release, 21 Sep 1967, IBP Papers, Series 2: USNC/IBP, Folder Meeting: General: 28–29 Oct 1967.

91. Werner Buedeler, *The International Geophysical Year* (Paris: UNESCO, 1957), 60, 68.

92. Subcommittee on Science and Astronautics, U.S. House of Representatives, Hearings, 90th Cont., 1st sess., 9 May, 6 Jun, 12 Jul, 3 and 9 Aug 1967, 10.

93. *Ibid.*, 9.

94. *Ibid.*, 3.

95. David Pimentel to Roger Revelle, 5 Jan 1968, IBP Papers, Series 2: USNC/IBP, Folder Meeting: Agenda: 20–21 Jan 1968.

for formalizing our support and our interest in ecology among all the scientists of the United States.”<sup>96</sup>

Neither Revelle’s vision for “a new synthetic biology” nor his plan for the IBP met with much enthusiasm among his biological colleagues at SIO, and still less among the broader biological community. Many biologists with whom Revelle had worked at SIO in the 1940s had been skeptical about his understanding of ecology and marine biology, complaining that he obtained his view of these fields “from the work of other people, often [with] too much enthusiasm and too little critical assessment.”<sup>97</sup> Similar reservations were expressed by ecologists twenty years later. In early 1968, when the USNC/IBP was ready to move into its five-year operational stage, Revelle resigned from the chairmanship of USNC/IBP, and was succeeded by Frank Blair, zoology professor from the University of Texas and member of the Ad Hoc IBP Committee. Blair later depicted Revelle’s early leadership of IBP as evidence of an “arrogant disregard for biology on the part of NAS/NRC [who] place[d] a physical scientist at the head of the U.S. effort in the International Biological Program.”<sup>98</sup>

The complaints expressed by Blair, however, didn’t amount to a conceptual disagreement. Both leaders of the USNC/IBP endorsed the systems approach in biology, which they promoted as a way to organize and focus ecological research. Revelle saw the role of the IBP as stimulating the “new approaches to worldwide biological surveys . . . needed to improve the level of description of the biosphere.”<sup>99</sup> The ecosystems concept would refine “biological surveys,” turning them into a “modern” science, one which relied on sound theoretical principles and employed modern methods. Reconciliation of the measurement of the “web of life” with comprehensive studies of the ecosystems would bring

96. *The International Biological Program: Its Meaning and Needs*, Report of the Committee on Science and Astronautics, Subcommittee on Science, Research, and Development, 90th Congress, 2nd sess., 1968, 25.

97. Rainger, “Patronage and Science” (ref. 76), 2. As a result, in the late 1940 biologists at Scripps had strongly opposed Revelle’s candidacy for Director, fearing that his emphasis and orientation would make biology secondary to physics and chemistry (*ibid.*).

98. Blair, *Big Biology* (ref. 48), 21. Revelle himself, however, did not make any negative remarks upon his apparent dismissal. The President of NAS, Frederick Seitz, described Revelle’s attitude toward the change in the IBP leadership in a most cheerful way to Frank Blair: “I spoke with Roger Revelle with regard to the changing of chairmanship of the International Biological Program. He was deeply pleased to learn that you had accepted the position as the new chairman. His pleasure was multiple since he, in fact, had proposed you in the first place.” (Frederick Seitz to Frank Blair, 19 Dec 1967, IBP Papers, Series 2: USNC/IBP, Folder Membership, 1966–1968).

99. Roger Revelle, “International Biological Program,” *Science* 155 (1967).

biologists onto the same page as physicists: “Our programs are not surveys, but [are intended] to solve problems. They are problem-solving oriented, e.g., analysis of ecosystems.”<sup>100</sup>

### The IBP: Big Science and Big Data

Since the 1950s the Atomic Energy Commission had been the largest supporter of systems ecology in the United States.<sup>101</sup> The USNC/IBP relied on the resources of the Oak Ridge National Laboratory (ORNL) in Tennessee and Brookhaven in New York, the leading American centers of ecosystems ecology and radiation ecology at the time of the IBP planning. Eugene Odum, a consultant for the ORNL, and Oak Ridge systems ecologists, such as George Van Dyne, Stanley Auerbach, and Jerry Olson, were the major designers and contributors of the IBP “biome projects,” the part of the IBP focused on the ecosystems studies.<sup>102</sup>

Although only one part of the IBP, the biome studies were its most ambitious component. They divided the country into five large ecological regions (tundra, grassland, desert, coniferous forest, and eastern deciduous forest), and selected the representative sites where each of the biomes was to be studied and modeled. The Tundra Biome program focused its work at Point Barrow, Alaska; the Desert Biome projects were carried at several sites, one of which, at Curley Valley, Utah, functioned as coordinating center; the Coniferous Forest Biome program had two sites, the Cedar River basin near Seattle and H. J. Andrews Experimental Forest in Oregon; the Deciduous Forest Biome program had five sites; and the Grassland Biome, based in Fort Collins, Colorado, and directed by Van Dyne, was supposed to function as a model and coordinating center for other biome studies.<sup>103</sup>

100. Draft Minutes of the meetings, IBP Papers, Series 2: USNC/IBP; Folder Meetings: Minutes: 20–21 Jan 1968. In retrospect, Revelle saw the endorsement of the system approach as the most valuable achievement of the IBP (Revelle to Paul Kramer, nd. (-1975), RP, Box 7, Folder 40).

101. On the role of AEC in the development and transformation of ecological research see Stephen Bocking, “Ecosystems, Ecologists, and the Atom: Environmental Research at Oak Ridge National Laboratory,” *Journal of the History of Biology* 28, no. 1 (1995): 1–47. Ron Doel points out that one of the underlying reasons for the AEC to engage in the ecosystem concept was the prospect of a wide range of atomic tests that could be done under Project Plowshares (personal communication).

102. Other IBP programs included the Subcommittee on Productivity of Freshwater Communities, Subcommittee on Human Adaptability, and the Subcommittee on Productivity of Marine Communities.

103. Golley, *History of the Ecosystem* (ref. 4); Kwa, “Modeling the Grassland” (ref. 4).

The goal was to produce total ecosystem models for each biome, the models were expected to provide the basis for “manag[ing] the ecological systems of the planet.”<sup>104</sup> Each biome site involved large teams of field scientists, administrative managers, computer programmers, and modelers, a collective effort designated to produce comprehensive models of entire ecosystems.

Intensive total-systems studies were based on measurements of all major components of the ecosystem: gross production, respiration, net production, soil and topographic settings, macroclimatic and microclimatic variables, etc. Despite earlier arguments, the planners of each IBP program now emphasized the necessity of data and method standardization, insisting that the studies will “involve many independent observers and must follow standards . . . so that data obtained are strictly comparable.”<sup>105</sup> The agreed-upon measurement techniques included isotope techniques for tracing food chains and estimating rates of nutrient cycling; remote sensing, including aerial photography, infrared scanning, radar, sonar, and underwater TV; biotelemetry, as well as various physical and chemical methods, such as chromatographic techniques, nuclear magnetic resonance, atomic absorption, spectrophotometry, calorimetry, nitrogen analyzers, and respiration chambers with automatic gas analyzers.<sup>106</sup>

Despite biologists’ initial insistence that their science was different from physics—even geophysics—and needed different approaches and new organizational structures, the administrative complexity of the IBP, which required interdisciplinary and multiple-institution participation, led to the organizational features (such as centralized infrastructure and standardization of methods and measurement techniques to ensure the large-scale collaboration) not dissimilar from the type of Big Science developed at the atomic physics laboratories. By the end, Oak Ridge ecologists at least, if not many others as well, strongly believed in the applicability of Big Science policy to ecology.<sup>107</sup>

But what about the IBP data and the data centers? Tentative plans to establish data banks—central storages where datasets accumulated in the biome

104. *IBP: Its Meaning and Needs* (ref. 96), 2.

105. USNC/IBP Subcommittee on Productivity of Freshwater Communities, “Program Statement of the Subcommittee on Productivity of Freshwater Communities and Subcommittee on Productivity of Terrestrial Communities of the U.S. National Committee for the International Biological Program” (Washington, DC: National Research Council, Division of Biology and Agriculture, 1966), 6.

106. *Ibid.*, 8.

107. See Kwa, “Modeling the Grassland” (ref. 4).

programs would be made available for scientists and decision-makers, both within and outside the U.S. IBP—were usually presented as concluding lines at the end of the biome studies proposals.<sup>108</sup> Even in the most organized biome—the Grassland Biome—the data bank was not set up until the late stages of the project.<sup>109</sup>

For Revelle, however, data centers were not something to be placed in the end of the proposal and then forgotten. For an oceanographer, Big Science meant Big Data, which implied the organization and management of large datasets. Following the IGY pattern, Revelle emphasized the need for the establishment of the data centers for the IBP at the *beginning* of the program, to ensure effective data handling. At the USNC/IBP meetings, Revelle was the major speaker on data issues: minutes recorded Revelle’s urging the establishment of data centers for the IBP:

Dr. Revelle opened the meeting . . . with a discussion on the need for information handling and data centers . . . In Dr. Revelle’s view the problems associated with data centers are: (a) to have the information in a form in which it can be used by different people for different purposes, and (b) to have it readily available upon request. In this connection Dr. Revelle said that a data center is a library in which data are collected, sorted, processed, and made easily retrievable to scientists upon request . . . Dr. Revelle charged the group to think through the kinds of data that should be collected on uniform data sheets in each field and what the rules should be for storage and retrieval.<sup>110</sup>

Revelle saw the IBP as a mechanism for the exchange of information not only across disciplines, but also across political barriers. Revelle was a long-time participant in the International Pugwash Conferences on Science and World Affairs, begun in 1957 to bring scientists from around the world to discuss the social and political implications of science.<sup>111</sup> Pugwash conferences had also been concerned with international cooperation in science, and the development of innovative international organizations as a vehicle to influence international science policy and larger transnational and international policy in a post-World

108. A. A. Brooks and R. L. Sayers, “Documentation and Submission of Data Sets,” Oak Ridge National Laboratory internal publication, 1972, Coniferous Biome Data Bank copy, LTER Papers.

109. Kwa, “Representations of Nature” (ref. 4).

110. Minutes of the meeting USNC/IBP, 25–26 Feb 1967, IBP Papers, Series 2: USNC/IBP, Folder Meetings/Agenda: 25–26 Feb 1967.

111. RP, Revelle’s Oral History, 1984.

War II world.<sup>112</sup> The Pugwash conferences listed organizers and major visionaries of the IGY among its participants. The IGY was in some sense an alter ego of Pugwash, as two intertwined groups both associated their history with the International Polar Years as an epitome of the international ideal of cooperation in science.<sup>113</sup> If international cooperation was the goal, then the exchange of scientific data was the means. The flow of scientists and their data back and forth across the Atlantic and the Iron Curtain acquired a special meaning in the divided Cold War world, securing “an important continuing channel of communication . . . between ‘East’ and ‘West’ countries.”<sup>114</sup>

The problem of storage, retrieval, and exchange of information—the material basis and the “hard currency” of the international cooperation—became a recurrent theme at Pugwash meetings in the 1960s, culminating in 1964 with the proposal for the organization of the World Centre of Scientific Information.<sup>115</sup> A long-term member of the Pugwash Continuing Committee, Bentley Glass, noticed that these recommendations pointed to the need for the development of new systems of worldwide, systematic, and coordinated storage and retrieval of scientific information “in the fields of science where none yet exist.”<sup>116</sup> In the discussion of the state of the IBP at the Pugwash conferences, participants paid as much attention to questions of the appropriate storage and possible institutionalization of the IBP data as to the scientific contents of the program and urged governments to support IBP projects.<sup>117</sup>

112. Crane, “Transnational Networks” (ref. 39).

113. J. Rotblat, *Pugwash—The First Ten Years: History of the Conferences of Science and World Affairs* (London: Heinemann, 1967).

114. *Ibid.*, 116. During the 1960s and 1970s the IGY/WDC system was reorganized. Most of World Data Centers were merged with the national data centers created by the military soon after the termination of the IGY. As Fae Korsmo pointed out, while the military services were interested in having the geophysical data readily available, the WDC system did not operate fast enough to process the data into the standards suitable for military operations. The WDC for Oceanography, which was operated during the IGY by Texas A & M College, was merged in 1961 into the National Oceanographic Data Center (NODC) established by the Navy. Likewise, the Air Force took steps to establish a centralized nationwide solution for the geophysical data: Korsmo, “Origins” (ref. 34). Although the data in NDC/WDC centers continued to be generally available for researchers (including foreigners), the symbolic meaning of WDC as an icon of scientific internationalism vanished when WDC acquired new status as primarily national rather than international centers.

115. See Rotblat, *Pugwash* (ref. 113), 180; Bentley Glass, “Pugwash Interest in Communications,” *Science* 159 (1968): 1328–31.

116. Glass, “Pugwash Interest” (ref. 115).

117. See, for example, minutes of the conference on “Cooperation in the Life Sciences” (1961) in Rotblat, *Pugwash* (ref. 113), 114–16, and 180.

Despite this support, the concept floundered and the data centers for IBP were not realized. The success of the geophysicists in establishing the IGY World Data Centers did not provide a template for biologists to do the same. Why? The authors of the independent evaluation of three of the five biome programs, commissioned by NAS and conducted by Battelle's Columbus Laboratories in late 1974, indicated three major reasons for the failure to create IBP data centers: (1) the field scientists and modelers were content with exchanging data through personal contacts, rather than through a centralized system, (2) protocols and formats for data were either lacking or not followed, and (3) some of the researchers were reluctant to release their data to the data banks.<sup>118</sup> But this still doesn't explain why biologists experienced these difficulties and geophysicists apparently did not. A partial answer involves the uneasy relations between traditional ecology and the new systems ecology that Revelle, as well as his successor Frank Blair, tried to promote.

### **Challenges of Systems Ecology and the IBP Data**

Systems ecology gained its momentum in the context of postwar anxieties about nuclear weapons and the high hopes prompted by the spectacular growth of science and technology. A belief in the prospects for human control of nature and enthusiasm for cybernetics on the one hand, and public enthusiasm for ecology during the age of growing environmentalism on the other, contributed to the sense that ecology was the most important new science of the day.<sup>119</sup> A systems approach in ecology drew from cybernetics, information theory, thermodynamics, physical equilibrium theory, statistical ecology, and computer science in developing models of large ecosystems with the information about its components and linkages. The examination of input and output properties (such as energy flow and trophic relations) seemingly opened the way to explain and predict ecosystem performance under changing environmental conditions, and hence to provide a base for rationalization of resource management.<sup>120</sup> Mathematical modeling of large ecosystems, with its large-scale funding and large institutional infrastructures,

118. Rodger Mitchell, Ramona A. Mayer, and Jerry Downhower, "An Evaluation of Three Biome Programs," *Science* 192 (1976): 859–65.

119. See Golley, *History of the Ecosystem* (ref. 4); Kwa, "Modeling the Grassland" (ref. 4); Kingsland, *Evolution of American Ecology* (ref. 48).

120. Golley, *History of the Ecosystem* (ref. 4).

gave ecology, traditionally associated with natural history, a “modern scientific cast”—at least in some people’s minds.<sup>121</sup>

The systems approach provided a way to view the environment in an integrated, unified manner that made it particularly attractive for the purposes of the IBP.<sup>122</sup> As one IBP proposal explained:

[T]he focal point . . . is to improve our understanding of whole ecosystems. Throughout this study, the whole system will be kept continually in view. No matter how narrow or detailed some of the projects may be, their relation to the whole will be a dominant theme. . . . The system approach is ideally suited to this task. The entire study will be used to develop an ecosystem model which can be used to assess the behavior of the system. In turn, the assessment results will be used to guide and evaluate further study. The activities and techniques associated with synthesis of information into the whole, with ecosystem model design, development and implementation, and with evaluation of model results for study guidance and ecosystem management are collectively called systems analysis.<sup>123</sup>

At the same time, the ecosystems approach was still deterministic, as it reduced the complexity of natural systems to the small set of variables that were thought to determine their essential features.<sup>124</sup>

On the level of organization of the IBP data, the Biome Analysis-of-Ecosystems program developed tentative plans for data banks to be organized at each site.<sup>125</sup> The procedures for compiling numerical material and storing it in data banks, as well as the standardized data set documentation forms, which included investigator names, location of research, parameters measured, key words, restrictions on dissemination of the data, and a brief description of the data set and experimental method, were adopted in each of the biome sites,

121. Kingsland, *Evolution of American Ecology* (ref. 48), 156.

122. Unifying role of systems approach was emphasized by advocates of ecosystems concept. See Eugene Odum, “The Emergence of Ecology as a New Integrative Discipline,” *Science* 195 (1977): 1289–93.

123. “Report of the Working Session on Grassland Ecosystems” 5–8 Oct 1967, IBP Papers (ref. 90).

124. *Ibid.*

125. Some IBP programs utilized the former IGY World Data Centers reorganized into National Data Centers after the end of the IGY. Thus, some of the data from IBP upwelling ecosystems program flew to NODC (*U.S. Participation in the International Biological Program*, U.S. National Committee for the International Biological Program, Report No. 6 (Washington, DC: National Academy of Sciences, 1974), 23).

but rather late in the development of the IBP projects.<sup>126</sup> Despite all the efforts to provide infrastructure and organization for the data-driven program on a large scale, the early worries about standardization of the methods, techniques, and data proved to be justified. As predicted, standardization turned out to be difficult. Several different ways of measuring C<sup>14</sup> fixation, as well as several different techniques for investigating primary production, were used by the IBP ecologists who studied productivity of marine ecosystems. In the end, researchers failed to achieve the level of data standardization necessary for producing a synthetic account of the IBP results. Marine biologist M. Dunbar had to admit at the IBP General Assembly:

[P]resenting a coherent general account of our present knowledge of marine production . . . is not simple. . . . No mathematical genius is required to convert milligrams to grams, or even saturation values of oxygen concentration to milliliters per liter, but to convert milligrams of carbon fixation per square meter per day to milligrams per cubic meter per year is impossible without additional information which is often not supplied. Biomass values expressed in units per volume or per area per day give quite different sorts of information from those conveyed by average values per year, etc.<sup>127</sup>

As a result, the computing technology could not automate the use, combination, or synthesis of the data generated in biome studies from a variety of methodologies and in a variety of different formats. Systems analysts complained that the data did not meet their criteria for model parameters, and blamed their colleagues for failing to understand that “systems analysis . . . is not a cure for poor data.”<sup>128</sup>

Moreover, despite plans to the contrary, data from nearly all the field projects were given to the modelers directly, rather than through the formal procedures established for data exchange.<sup>129</sup> As the authors of the independent evaluation of biome programs reported, more than ninety-five percent of researchers contacted in the process of evaluation had contributed data to the modeling teams of the biome programs, but the data were originally collected “with no

126. Brooks and Sayrs, “Documentation and Submission,” LTER Papers (ref. 108).

127. M. L. Dunbar, “Productivity of Marine Ecosystems,” in *Productivity of the World Ecosystems: Proceedings of V General Assembly of the Special Committee for the International Biological Program* (Washington, DC: National Academy of Science, 1975), 27.

128. “Report of the Working Session on Grassland Ecosystems” 5–8 Oct 1967, IBP Papers (ref. 90).

129. Mitchell, Mayer, and Downhower, “Evaluation of Biome Programs” (ref. 118).

knowledge of the appropriate sampling procedures and data format.”<sup>130</sup> By mid-1973 the biome programs accumulated a total of 1,182 datasets in the data banks, where each dataset represented about 5,000 measurements, but the data in the data banks were not readily available and the data were largely in the custody of the principal investigators.<sup>131</sup>

Each biome project experienced a tension between the interests of the modeling teams, who had the leading position within the biome studies, and the interests of the individual ecologists who collected the data. Field biologists complained that they were degraded to mere data collectors, “clipping and weighing” grass for the Biome Data Bank.<sup>132</sup> The systems analyst, on the other hand, would reproach ecological colleagues as “they cut down trees and weighed them, and I did everything else.”<sup>133</sup> Indeed, a systems analyst had to play a double role in the IBP programs: although their explicit function was to develop the models by integrating the data derived from the studies, their work also included data management and training of ecologists in the use of computers and mathematical methods for systems analysis.<sup>134</sup> Seen from the perspective of an ecologist, however, it was the mathematician-modeler who required guidance. For the ecologists T. C. Foin and S. K. Jain “biologists need to guide mathematicians in dealing with real world phenomena.”<sup>135</sup>

These tensions mirror what Sharon Kingsland has called a “continuing dialectic between mathematician and biologist” in the history of ecology.<sup>136</sup> Where the mathematician saw equilibrium and uniformity at the cost of ignoring individual characteristics, the biologist saw individuality and heterogeneity at the cost of generalization. Nowhere was this dialectic more evident than in the attempt of the IBP to reconcile the mesoscale systems ecology—given its institutional and conceptual ties to cybernetics and space programs—with “earthly ecology” rooted in the traditions of natural history. The immediate goal of the

130. *Ibid.*, 864.

131. Data as presented in *U.S. Participation* (ref. 125), 23; discussed in Mitchell, Mayer, and Downhower, “Evaluation of Biome Programs” (ref. 118); Kwa, “Modeling the Grassland” (ref. 4).

132. Kwa, “Modeling the Grassland” (ref. 4).

133. Long, “Forest and the Mainframe” (ref. 4), 55.

134. One of the problems with organization of the IBP data banks was the lack of “people between the specialists and the modelers able to interpret the data.” Golley, *History of the Ecosystem* (ref. 4), 127.

135. T. C. Foin and S. K. Jain, “Ecosystems Analysis and Population Biology: Lessons for the Development of Community Ecology,” *BioScience* 27, no. 8 (1977): 532–38, on 538.

136. Sharon E. Kingsland, *Modeling Nature* (Chicago: University of Chicago Press, 1985).

IBP—to examine the biological basis of productivity as related to human welfare—was expected to be reached by the ecosystems approach through the analysis of energy flow, trophic relationships, and nutrient cycles of large ecosystems represented by biomes. For traditional ecologists, however, this approach didn't leave room for the individuality and heterogeneity of biotic phenomena. As zoologist Dennis Crisp put it, “This abstraction leaves out of account the faunistic composition, community structure, feeding behavior and food preferences of the organisms concerned. The juicy steak and the old leather boot become equals in the eyes of [the] calorimeter.”<sup>137</sup>

### THE IBP: FAILURE OR SUCCESS?

In 1974, the evaluation of the U.S. IBP effort turned into a heated debate. The NAS had commissioned a report on the American contribution, released to the public in January 1975. Its criticism was harsh. *Science* reported that the program was criticized for providing research funds to “second-rate researchers who wouldn't have qualified for grants under the regular NSF grant programs,” that “the biome studies have accumulated masses of data while failing to establish chains of cause and effect that could lead to deeper understanding of how ecosystems work,” and that the results of the predictive models were almost impossible for non-specialists to use.<sup>138</sup> The *Science* reviewer concluded that one “just couldn't see, for the money spent, that we had advanced our understanding.”<sup>139</sup>

The major charge of critics was that the IBP “failed to live up to its own rhetoric.”<sup>140</sup> The failure to establish data centers was a particular frustration, a “disappointment” for the late IBP scientific director Barton Worthington.<sup>141</sup> But it wasn't only the management of data that failed: the core goal of creating comprehensive models, which would mimic the behavior of biome-wide ecosystems, turned out to be impossible, too.<sup>142</sup> With increasing criticism of “cybernetic totalism” in the 1960s, the cybernetic deterministic approach to

137. Dennis J. Crisp, “Secondary Productivity in the Sea,” in *Productivity of the World Ecosystems: Proceedings of V General Assembly of the Special Committee for the International Biological Program* (Washington, DC: National Academy of Sciences, 1975), 71.

138. C. H., “NAS Report on International Biological Program,” *Science* 187 (1975): 663.

139. Ibid.

140. Philip M. Boffey, “International Biological Program: Was It Worth the Cost and Effort?” *Science* 193 (1976): 866–68.

141. Worthington, ed., *Evolution of IBP* (ref. 48), 60.

142. Golley, *History of the Ecosystem* (ref. 4).

ecosystems modeling—with its assumption that the trophic pyramids and the set of environmental factors can determine the model of the ecosystem and that systems, once adequately modeled, could be controlled in a cybernetic regime—came under sustained attack. Many ecosystems modelers participating in the IBP shared these concerns. At the outset, ecosystems modelers had hoped to overcome the deterministic view of ecosystems by embracing probabilistic perspectives.<sup>143</sup>

By the end of the program the entire approach of all-encompassing models—whether deterministic or probabilistic—was declared “dead or near a dead end.”<sup>144</sup> George Innis, a systems analyst at the Grassland Biome, warned in his presentation for the USNC/IBP Coordinating Committee that “the deterministic view of ecosystems” had no future and had to be changed. The ecosystems modeling approach endorsed in IBP biome projects, intending to provide a comprehensive description of the whole system, was, he argued,

mechanistic in the large for being holistic in the small . . . in the sense that the large-scale dynamics of the ecosystems are presumed to be explained by the interactive dynamics of the components of the ecosystems. The components on the other hand . . . are described holistically, that is, there is no attempt to describe the mechanisms which operate, but a black box treatment is used. While this may or may not be appropriate, many biologists feel that there are properties of the ecosystem and there are theories about ecosystems which apply to the system as a whole and are not explained by the aggregation of the components. We have been unable to incorporate such theories in our current ecosystem simulation approaches.<sup>145</sup>

Yet, despite the criticism and self-criticism of the IBP ecosystems studies, the program, in yet another form, endured. The NSF expressed its willingness to support the Analysis-of-Ecosystems component of the IBP “for a considerable period after 1974.”<sup>146</sup> After the IBP formally ended in 1976, what had

143. “Report of the Working Session on Grassland Ecosystems” 5–8 Oct 1967, IBP Papers (ref. 90).

144. George S. Innis, “Future Directions in Ecosystem Modeling,” 26–27 Jul 1973, IBP Papers, Series 2: USNC/IBP, Folder Meetings: 26 Jul 1973.

145. *Ibid.* As a solution Innis suggested to revise the modeling approach by seeing the ecosystem as a “self-organizing system.” In practice this meant shifting from the models of large ecosystems to the “library of submodels,” or smaller modules that can be replaced, revised, and adjusted independently depending on the data and simulation experiments. In other IBP biome sites the projects aimed at modeling large ecosystems were replaced by early 1970s by largely descriptive programs with the elements of ecosystem analysis. See Alexios R. Antypas, “Translating Ecosystem Science into Ecosystem Management and Policy: A Case Study of Network Formation” (PhD dissertation, University of Washington, 1998).

146. Minutes of Meeting, 21 Jan 1972, IBP Papers, Series 2: USNC/IBP, Folder Meetings: Minutes, 17–21 Jan 1972.

been the IBP's line in the NSF budget became the base budget for the ecosystems studies program, which, since the mid-1970s, has been the NSF's biggest program in environmental biology.<sup>147</sup> In this way, funding was continued for what was considered the best of the IBP programs. And in 1977 the NSF approved a new, post-IBP program initially entitled Long-Term Ecological Measurements, then renamed the Long-Term Ecological Research program (LTER). Officially started in 1980, LTER was at first part of the NSF ecosystems studies program. Eventually it was assigned its own program officer: James T. Callahan, the same person who had been the program officer for the IBP.<sup>148</sup> So the IBP lived on as the new LTER program in the NSF. The IBP did not so much end as evolve—or perhaps transmute—from the original concept of a biological “year” to its virtual opposite—a program focused explicitly on the long term.

At least in the minds of the NSF program officers, the strong continuity between the IBP and LTER was straightforward. The NSF program officer who closely observed the ecosystem research for twenty-five years assessed the impact of the IBP retrospectively:

I have consistently said that . . . biome projects solidified and scientifically legitimized . . . ecosystem science. Which today is very strong and healthy and competitive. It created the track and the track record in the mainline journals for the publication of results at that level of biological complexity. It brought about the creation and dedication of topical sessions at the annual meetings of the major societies, it drove the creation of the ecosystem program at the NSF, and whatever successes it had contributed strongly to the institutionalization and the continuation of that program into the present . . . a lot of things, all of which are blocks in that structure that has become that construct of ecosystem science.<sup>149</sup>

The LTER program continued much of the work envisioned by the IBP on a revised basis, both organizationally and conceptually. Some IBP research sites (the H. J. Andrews Experimental Forest, Coweeta Hydrologic Laboratory, Konza Prairie, and Niwot Ridge research sites) became the first research sites of the LTER. Like the biome research during the IBP, the LTER research was based on interdisciplinary teams of ecologists working in various research sites,

147. Appel, *Shaping Biology* (ref. 4).

148. Dave Coleman, Dac Crossley, and James Gorz, “In Memory of James T. ‘Tom’ Callahan,” *LTER Newsletter* (1999).

149. Cited in Antypas, “Translating Ecosystem Science” (ref. 145), 96.

at universities, and with various partners such as the U.S. Forest Service, Department of Energy, and the Park Service.<sup>150</sup> While LTER strived to develop the comparative studies of trends and dynamics across sites that are characterized by their inter-annual and spatial variability, modeling as the primary focus was abandoned by the LTER ecologists, many of whom were former participants in the IBP. Conceptually, the LTER ecological studies shifted focus from the static state of an ecosystem (largely adopted during the IBP biome studies) to the analysis of disturbances, identified by long-term observations.<sup>151</sup>

Seemingly mundane questions of data collection—how data are gathered, organized, stored, and shared—were given pride of place in the planning of LTER from its inception.<sup>152</sup> From the development of site-based data management, established as an integral element of each LTER site, emerged a role of an information management and a community of practice.<sup>153</sup> Funding for the LTER program from the NSF stipulated that data generated from research be adequately documented, archived, and made available for intersite data exchange.<sup>154</sup> Learning from the experience of the IBP, the LTER fostered the idea that data management should be integrated into the research process from the initial stages of site and research planning.<sup>155</sup> At the first LTER Forest Science Data Bank, the data managers who worked in the data bank's Quantitative

150. J. F. Franklin, C. S. Bledsoe, and J. T. Callahan, "Contributions of the Long-Term Ecological Research Program," *BioScience* 40, no. 7 (1990): 509–23.

151. Gene E. Likens, ed., *Long Term Studies in Ecology: Approaches and Alternatives* (New York: Springer-Verlag, 1989). As Golley has noted, the hope to create big comprehensive models of the entire ecosystems was deficient as a whole, since this approach wrongly assumed the ideal existence of a single state in any given ecosystem: Golley, *History of the Ecosystem* (ref. 4).

152. See, for example, *A Pilot Program for Long-Term Observation and Study of Ecosystems in the United States. Report of a Second Conference on Long-Term Ecological Measurements, Woods Hole, Massachusetts, February 6–10, 1978* (Washington, DC: NSF Program in Biological Research Resources, 1978).

153. K. S. Baker, B. J. Benson, D. L. Henshaw, D. Blodgett, J. H. Porter, and S. G. Stafford, "Evolution of a Multisite Network Information System: The LTER Information Management Paradigm," *BioScience* 50 (2000): 963–78; K. S. Baker and F. Millerand, "Infrastructuring Ecology: Challenges in Achieving Data Sharing," in *Collaboration in the New Life Sciences*, ed. J. Parker, N. Vermeulen, and B. Penders (London: Ashgate, in press); H. Karasti, K. S. Baker, and E. Halkola, "Enriching the Notion of Data Curation in E-Science: Data Managing and Information Infrastructuring in the Long Term Ecological Research (LTER) Network," *Computer Supported Cooperative Work* 15 (2006): 321–58.

154. OSU Forest Science Data Bank Newsletter, vol. 2, no. 1 (1983); LTER papers.

155. The role of data managers was played either by the IBP scientists-ecologists or (in most cases) by the modelers or system analysts.

Services Group had a combination of statistical, ecological, and biological training, in addition to their technical skills in data management.<sup>156</sup>

The emphasis on statistics and the visible role of the statistician in the first LTER data bank reflected a general trend: the statistician and biometrician replacing the IBP ecosystems modeler as a manager of the ecological data collections. These new data managers, with training in statistics, were often well educated in ecology and biological sciences, since statistics and biometrics were regarded as part of a basic training of an ecologist.<sup>157</sup> This contrasted with the IBP modelers, who usually had their training in physics, information theory, and econometrics, and considered the simulation modeling as an exquisite “art” that “should be attempted only by one highly trained in mathematics and computer programming,” as Thomas Kirchner, ecologist and data manager at the Natural Resource Ecology Laboratory (Colorado State University), put it.<sup>158</sup> The replacement of highly complex whole ecosystem modeling by more accessible statistics mirrored the shift from IBP’s mathematical-physical approach to the descriptive LTER ecology that reestablished links with statistics and biometrics.

But the diminution of some of the ambitions of the IBP during post-IBP developments didn’t turn it into “little science.” LTER science was still Big Science, involving large-scale collaboration and resource sharing among a variety of institutions and encouraging groups in different locations studying

156. Although during the IBP, as many reviewers noted, proper management of the data did not become an established tradition, several data banks were established during the time of the program, for example, the Forest Science Data Bank at Andrews Experimental Forest (*OSU Forest Science Data Bank Newsletter* 2, no. 2 (1983), LTER Papers). See also S. G. Stafford, P. B. Alaback, K. V. Wadell, and R. L. Slagle, “Data Management Procedures in Ecological Research,” in *Research Data Management in the Ecological Sciences*, ed. W. K. Michener (Columbia: University of South Carolina Press, 1988), 93–113. Susan Stafford, the long-time data manager of the FSDB, had an educational background both in ecology and statistics, with a PhD in applied statistics.

157. As ecologist Frederick Smith noticed, by 1968 biometrics became largely the domain of ecologists, because geneticists, who were the major proponents of biometrics in the first half of the twentieth century, had changed from mathematical to a chemical orientation. As a result, teaching courses in biometrics in many departments shifted from the geneticists to the ecologists. See Frederick Smith, “The International Biological Program and the Science of Ecology,” *Proceedings of the National Academy of Sciences* 60, no. 1 (1968): 5–11.

158. Thomas B. Kirchner, “PREMOD and MOMAID: Software Tools for the Construction and Analysis of Simulation Models,” in *Research Data Management in the Ecological Sciences*, ed. W. K. Michener (Columbia: University of South Carolina, 1986), 345–71, on 346.

different biomes to coordinate their research strategies and to share data and instrumentation. It was big biology but with a new template.

In her study of *Arabidopsis* researchers, Sabina Leonelli suggested distinguishing *centralized big science*, “launched and coordinated by few (often only one) leading institutions which acquire funding and distribute it among interested groups from other institutions on condition of complying with a given research agenda” from *decentralized big science*, where participant laboratories agree on the set of issues to be investigated, but each laboratory carries out its own research in its own way.<sup>159</sup> Standardization of practices and a theoretical framework is not necessary to pursue the latter type of collaboration, she suggests, although participants share information and resources among one another.

ILTER science seems to occupy an intermediate position between these ideal types. The LTER arrangements can be described as a hybrid that addresses the specifics of local biomes (and accommodates the diversity of local institutional arrangements) while at the same time bringing site-specific researchers into an ongoing, long-term, all-site dialogue, in which a shared set of research aims encourage comparative intrasite analyses. No one LTER site or set of participants is singled out to take a leading role permanently; rather, scientific leadership is distributed and delegated across the network as a whole. LTER researchers refer to this mode of research “LTER *network science*,” emphasizing that LTER seeks to support comparative analysis through a mix of local site-based biome studies as well as cross-site studies. Thus, LTER appears to have benefited from lessons learned during the IBP, building tighter connections between data managers and ecologists, and viewing the standardization of methods and of measurement techniques as complex issues to be addressed over time, individually at the sites, and as a community within the LTER network.<sup>160</sup> Long-time LTER ecologist John Magnuson explains, “The opportunities for really new science lay in LTER network science.”<sup>161</sup> Or at least this is how it now looks to participants in hindsight.

159. Sabina Leonelli, *Weed for Thought: Using Arabidopsis Thaliana to Understand Plant Biology* (PhD dissertation, Vrije Universiteit Amsterdam, 2007), 42.

160. LTER grew from six sites that composed the network in 1980 to twenty-six sites by 2005; see *LTER Network. Celebrating 25 years of Excellence in Long Term Ecological Research* (LTER Network Office, 2005).

161. John J. Magnuson, “Reflections on LTER Beginnings, Challenges, and the Future,” *Network News* (2006).

## CONCLUSION

Although an expanding literature addresses the questions of large-scale collaboration in science, historians are only beginning to assess and document patterns that shaped collaboration involving big and dispersed teams of scientists and a variety of different scientific institutions.<sup>162</sup> In some cases, scientists harshly disparaged such efforts initially, but later participated in revisionist history in which the success of the project was assumed all along. In other cases, scientists have declared success, with little discussion of whether the original objectives of the projects were fulfilled. Although the IBP was declared a failure, those who drew that conclusion perhaps underestimated the social technologies it set in motion. Despite much criticism (including self-criticism), the IBP was instrumental in the creation of centers for ecosystems studies in major universities, which consequently provided an infrastructure for ecosystems research and a network for researchers, helping to elevate ecosystems research into an attractive career choice. During the IBP almost two thousand scientists were trained in ecosystems studies, which led in turn to the diffusion of the ecosystems idea outside the narrow and specialized intra-ecology debate.<sup>163</sup> By the 1990s the ecosystems approach to understanding ecological phenomena and framing resource management had become common, even dominant.<sup>164</sup>

At the outset, the idea of the large-scale “biological survey” was not generally seen as a sufficient justification for a program in biology, in contrast to the IGY, where it was. The “synoptic data effort,” as Hugh Odishaw defined the impetus of the IGY, was located in the IBP *inside* the theoretical framework of systems ecology, which emphasized comprehensive modeling and tended to privilege theory (now manifested as modeling) over “mere” data collection. Yet, at the same time, the IBP nevertheless instantiated a large-scale, data-driven effort as its cornerstone, ensuring that data would play a central role in the overall program, as it had in the IGY.

162. Following the lead of the American Institute of Physics, a number of historians, sociologists, and ethnographers have launched projects attempting to document large-scale collaborations (see *American Institute of Physics' Study of Multi-Institutional Collaborations. Phase I: High-Energy Physics* (New York: American Institute of Physics, 1992); *American Institute of Physics' Study of Multi-Institutional Collaborations. Phase II: Space Science and Geophysics* (College Park, MD: American Institute of Physics, 1995); for an overview see Ivan Chompalov and Wesley Shrum, “Institutional Collaboration in Science: A Typology of Technological Practice,” *Science Technology Human Values* 24 (1999): 338–72.

163. Golley, *History of the Ecosystem* (ref. 4).

164. *Ibid.*

With the development of LTER, history came full circle, or at least traced a helical path: long-term data collection resumed its pride of place in ecology. The position that had been dominant in the 1950s regained substantial weight in the 1980s. Centralized data storage and a certain level of standardization of data were finally achieved. But something else had changed: the way in which those data were gathered, processed, stored, and exchanged. Data were now stored in a mix of site-based, network-based, and theme-based digital data repositories, accessible online via the Web. Although data management activities are still not generally considered glamorous activities within the life sciences, national and global data-archiving projects have gained momentum in the culture of the post–Cold War world shaped by information technologies.

To return, then, to the question of Big Science, the history of the IGY, IBP, and LTER underscores how large-scale data collection has been an important part of Big Science in the second half of the twentieth century, beyond physics and in domains where complex instrumentation and gadgetry have played only a supporting role.<sup>165</sup> To put it perhaps a bit too simply, in many historical accounts, at least until very recently, Big Data were not as much a part of the Big Science story as big laboratories and big machines.<sup>166</sup> A notable exception to this is the Human Genome Project and the rise of genomics and other data-driven “-omics” disciplines, which have recently attracted the attention of historians and philosophers.<sup>167</sup> This new emphasis on genomics in historiographic literature has, at least to some extent, overshadowed the role and impact of earlier large-scale, data-driven initiatives in post–World War II biology. Perhaps more important, it has also tended to downplay the continuing role of natural historical practices of data collection in biology, and perhaps the earth sciences as well: data collected

165. On the culture of “gadgeteering” see Paul Forman, “Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940–1960,” *Historical Studies in Physical and Biological Sciences* 18, no. 1 (1987): 149–229.

166. On data in Big Physics, see Peter Galison, *Image and Logic* (ref. 3). On data collection as central to the history of earth science, see Oreskes, “Earth Sciences” (ref. 53).

167. On “omics” disciplines see Krohs and Callebaut, “Data without Models Merging” (ref. 3). On the Human Genome Project and related developments, see J. L. Heilbron and Daniel Kevles, “Finding a Policy for Mapping and Sequencing the Human Genome: Lessons from the History of Particle Physics,” *Minerva* 26 (1988): 299–314; Leonelli, *Weed for Thought* (ref. 160); Sabina Leonelli, “Circulating Evidence across Research Contexts: The Locality of Data and Claims in Model Organism Biology,” *LSE Working Papers on the Nature of Evidence: How Well Do ‘Facts’ Travel?*, no. 25/08 (2008); Bruno J. Strasser, “Collecting and Experimenting: The Moral Economies of Biological Research, 1960s–1980s,” *Preprints of the Max-Planck Institute for the History of Science* 310 (2006): 105–23.

outside the laboratory, and often without specific reference to particular theoretical positions and aspirations. In both biology and geology, scientists have long believed that data collection could be the end in itself, one that would lead to the advancement of science, independent of a specific theory that those data were intended to test. The history of the IGY, IBP, and LTER shows that many natural scientists continued to hold firmly to that belief in the second half of the twentieth century, and evidently still do today.

While Big Data has been a less emphasized part of the postwar Big Science story, it is not less important. The history of the IBP, as the first attempt to launch Big Science in biology after World War II, gives us useful insights into the peculiarities of collecting and organizing biological data on a large scale, the institutional structures it required, and the controversies it generated. In the IBP and LTER, data collection—as an *end product*—achieved legitimacy in biology as an integral component of the postwar Big Science mode of research.

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