

‘A Dispassionate and Objective Effort:’ Negotiating the First Study on the Biological Effects of Atomic Radiation

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Abstract. The National Academy of Science’s 1956 study on the Biological Effects of Atomic Radiation (BEAR) was designed to provide an objective analysis to assess conflicting statements by leading geneticists and by officials in the Atomic Energy Commission. Largely because of its status as a detached, non-governmental evaluation by eminent scientists, no studies have had a broader impact on the development of biological thinking in regard to nuclear policies. This paper demonstrates that despite the first BEAR study’s reputation as an objective and independent study, it was the product of careful negotiation between Academy scientists, the Atomic Energy Commission, and Britain’s Medical Research Council. This paper also reveals the fragility of the consensus that produced the Academy’s report, the range of political uses of the report, and the subsequent disaffection of the scientists who took part in it.

Keywords: Atomic Energy Commission, BEAR Committee, Detlev Bronk, fallout, genetics, Harold Himsworth, Medical Research Council, National Academy of Sciences, Nuclear Testing, Warren Weaver

Introduction

The first study of the Biological Effects of Atomic Radiation (BEAR) by the National Academy of Sciences, completed in 1956, was born from controversy. American testing of a hydrogen bomb in 1954 had blanketed the Japanese fishing boat *Fukuryu Maru* with radioactive debris, killing one of the crew, giving the others radiation sickness, and temporarily ruining Japan’s fish market. The word “fallout” entered public consciousness, as did the possibility that the Atomic Energy Commission (AEC) was hiding the truth about the dangers of atomic radiation. And for the first time, laypersons witnessed a serious divergence in the scientific community that fell along disciplinary lines – geneticists were breaking ranks from other “atomic” scientists by claiming that all levels of radiation exposure increased the number of mutations in human genes, and that these mutations should be considered harmful.

Hermann J. Muller, who had won the 1946 Nobel Prize (in Physiology or Medicine) for his discovery of the increase in mutations from X-rays, criticized the American government's claims that its nuclear tests were safe. Despite the impression given by the AEC, there was no threshold of safety, he said; it was simply a matter of more radiation producing more harmful mutations. Although the AEC had been studying the genetic consequences of radiation exposure since 1946, through the Atomic Bomb Casualty Commission, it had found no conclusive evidence of long-term damage. Thus laypersons everywhere confronted two contradictory positions from authoritative voices about the possible biological dangers of atomic radiation.¹

As 1954 drew to a close, the Board of Trustees of the Rockefeller Foundation decided to finance an independent evaluation of atomic radiation, with a view toward issuing an authoritative statement that could not be accused of being an AEC whitewash. The trustees agreed that a new study of biological effects should be done impartially under the auspices of a non-governmental body. They turned to Detlev Bronk, who recently had become president of the Rockefeller Institute and who was also the president of the National Academy of Sciences (NAS). Because the NAS was composed of leading American scientists without requisite government ties, it appeared perfectly suited to make, as a later press release claimed, "a dispassionate and objective effort to clarify the issues, which are of grave concern and great hope to mankind." By June 1956, their report was complete. It grew from the work of six separate committees, or panels, the most significant of which were the Pathology Panel and the Genetics Panel. These addressed the threats to human life and to human descendents. The four other panels addressed more specific environmental topics – meteorology, the oceans, agriculture, and waste disposal. There was considerable overlap among the groups, because they were all concerned with how dangers from radiation could reach human beings.²

¹ On the *Fukuryu Maru* see Lapp, 1958. Muller's views can be found in Muller, 1955. On the Atomic Bomb Casualty Commission, see Lindee, 1994; Neel, 1998.

² The trustees taking an interest in this independent evaluation included Robert Lovett, former Secretary of Defense; Arthur Sulzberger, the publisher of the *New York Times*, Henry Moe, the secretary of the Guggenheim Foundation, and Thomas Parran, former head of the Public Health Service. The account of BEAR origins is given by Bronk in Transcript, Afternoon Session, Study Group on Disposal of Radioactive Wastes, 23 Feb 1956, folder "ORG: NAS: Coms on BEAR: Disposal and Dispersal of Radioactive Wastes: Meeting Transcript, Feb 1956," National Academies Archives, Washington, DC (hereafter NAS Archives). p. 6. Quote is taken from "NAS-Atom Rad Press Release, 8 April '55," folder "Pub Rel: General, 1955-1962," NAS Archives.

No scientific assessment has had a broader impact on the development of biological thinking in nuclear policy in the United States and abroad than this first BEAR study. The conclusions reached by the BEAR committees in 1956 stood the test of time, forming the basis of later Academy reports and informing decisions about nuclear testing, radioactive waste, and the development of civilian atomic energy in subsequent decades. Despite universal agreement among historians about its influence, none have analyzed the first BEAR committees in detail.³

The Academy's 1956 report was a product of delicate negotiation across institutional, disciplinary, and even national lines. The influence of the negotiated product – the report itself – endured far longer than the ephemeral consensus that created it. Although it ostensibly was written independently of government influence, the NAS relied heavily not only on AEC data but also the AEC personnel who served on the committees and who alone had access to classified information. Also, the report was issued on the same day – and reached essentially the same conclusions – as one by Britain's Medical Research Council (MRC). Although influential historians have taken this as a coincidence, there was nothing accidental about it. The NAS and the MRC made personal contacts, traded drafts, and coordinated release dates to ensure conformity and to maximize the effect of their reports.⁴ In addition, the Academy acted to ensure the proper coverage of the reports in the media, particularly through the *New York Times*, owned by a Rockefeller Foundation trustee, and the *Scientific American*, which asked the Academy to write its own headline. Further, the policy implications of the report were not as clear-cut as the AEC and President Eisenhower claimed; the fragility of the BEAR negotiation was clearest in the gradual disaffection from the AEC of leading participants in the BEAR study, including the chairman of the Genetics Panel, who disliked how the report was used.

³ On the influence of the BEAR report see Divine, 1978, pp. 319–321, and Hacker, 1994, pp. 185–189. The Academy reiterated its conclusions in 1960, and then again in 1972 in the report on the Biological Effects of Ionizing Radiation (BEIR), though the latter warned more strongly about the somatic (bodily) effects than did the previous reports. See Walker, 2000. An overview of the scientific issues confronting the BEAR committees can be found in Kopp, 1979.

⁴ In otherwise excellent books, Robert A. Divine calls the simultaneous release of the two reports a “remarkable coincidence,” and Barton C. Hacker simply echoes Divine by calling it a coincidence. See Divine, 1978, p. 79, and Hacker, 1994, p. 185.

No Repression of a Single Scientific Fact

The public face of the BEAR committees made them seem independent of the AEC, but they were separated only in a technical sense. Even if these studies were not financed by the AEC, other NAS studies certainly were, such as those of the Atomic Bomb Casualty Commission (ABCC). The Academy had no intention of antagonizing the AEC, with which it already had enough friction in keeping the ABCC afloat.⁵ As for the AEC, it never adjusted to the fact that the BEAR study was financed by the Rockefeller Foundation, and it intervened often, commenting on drafts and arguing its point of view. The Academy went out of its way to placate the AEC throughout the whole BEAR process. For example, the AEC feared that the mere existence of the BEAR committees implied negligence on its part. To ease this distress, Detlev Bronk ensured that the initial press announcements were carefully worded to highlight not only the independent nature of the study but the fact that the AEC was enthusiastic about cooperating by providing necessary data. Although the money would come from the Rockefeller Foundation, the Academy noted that this did not mean the AEC had neglected the problem over the years. The AEC had spent over \$165,000 on its biological and medical program since 1950, the press release noted, with over half of that sum devoted to the effects of radiation on living organisms.⁶

All of the panels were potentially controversial, but only the Genetics Panel had contention built into it from the start. Although the Pathology Panel covered tough issues about occupational exposure and the risks to human health, this panel was headed by Shields Warren who, as former head of the AEC's Division of Biology and Medicine (1948–1952), could be expected to adopt the AEC's basic assumptions – after all, he had authored them. But the Genetics Panel included scientists who publicly had criticized the AEC, particularly its Chairman, Lewis Strauss. Two such critics were Alfred H. Sturtevant, of the California Institute of Technology, and Hermann J. Muller, of Indiana University. Both had been students of geneticist Thomas Hunt Morgan at Columbia University and had taken part in the pioneering experiments there on the genetics of *Drosophila* flies in the 1910s. Sturtevant was widely known for developing techniques of chromosome mapping, and Muller won a Nobel Prize for his work producing genetic mutations with X-rays. They

⁵ On the financial problems with ABCC, see Putnam, 1998.

⁶ “NAS-Atom Rad Press Release, 8 April '55,” folder “Pub Rel: General, 1955–1962,” NAS Archives.

knew that there was a direct relationship between the amount of radiation and the frequency of mutations; thus they did not believe there was a threshold below which such mutations would not be produced.⁷ Certainly there was no definitive proof of the absence of such a threshold. But as Sturtevant once put it to an AEC scientist, “theory gives no basis for a threshold, and experiment does not suggest it.”⁸

Sturtevant, Muller, and others were surprised when, after the *Fukuryu Maru* incident, AEC Chairman Lewis Strauss denied the possibility that nuclear tests could harm humans. In a White House press release on March 31, 1954, Strauss stated that although American and Soviet bomb tests had raised the background level of radiation, it still was “far below the levels which could be harmful in any way to human beings.” The statement was unambiguous and struck many scientists – especially geneticists – as dishonest.⁹

At the time of Strauss’s statement, Sturtevant was president of the American Association for the Advancement of Science (AAAS). For his presidential address at the annual meeting, Sturtevant planned to contradict Strauss and discuss the genetic harm from nuclear tests. He sent the AEC a copy of the address beforehand. John C. Bugher, the director of the AEC’s Division of Biology and Medicine, objected strongly to Sturtevant’s assertion that the bombs already exploded from tests “will ultimately result in numerous defective individuals – if the human species itself survives for many generations.” Bugher called this statement “absurd and scientifically indefensible.” It was absurd because the long-term exposure to fallout was smaller than the natural background radioactivity from radium in soil and even smaller than that of the potassium in human bodies. The sweeping generalizations in Sturtevant’s speech would require prompt correction by the AEC, Bugher warned.¹⁰

Sturtevant ignored the warning and made his statements anyway on June 22, 1954, at the AAAS meeting in Pullman, Washington. And as promised, Bugher publicized a strongly worded correction, repeating the words “absurd and scientifically indefensible” to characterize Sturtevant’s views.¹¹ Sturtevant’s address then was published in *Science*,

⁷ On the work at Columbia, see Allen, 1978, and Sturtevant, 1965.

⁸ A. H. Sturtevant to Earl L. Green, 25 Apr 1955, folder 11.3, Papers of A. H. Sturtevant, California Institute of Technology Archives, Pasadena, CA (hereafter Sturtevant Papers).

⁹ Strauss is quoted in Sturtevant, 1954, p. 406.

¹⁰ John C. Bugher to A. H. Sturtevant, 16 Jun 1954, box 11.1, Sturtevant Papers.

¹¹ Bugher’s public response is discussed in R. A. Brink to A. H. Sturtevant, 28 Jun 1954, box 11.1, Sturtevant Papers.

after a contentious peer review process in which ABCC geneticist James V. Neel urged Sturtevant to tone down the paper and to adopt the AEC's policy of comparing radiation effects from nuclear tests to natural radiation. Sturtevant refused to make this comparison, and felt that it was beside the main issue. "My specific point," he wrote to Neel, "was that Chairman Strauss had, in an official press release, made an inexcusable statement, which every geneticist knows to be wrong. That point still stands, in spite of the reaffirmation by the AEC since my talk was reported in the newspapers."¹² The article, called "Social Implications of the Genetics of Man," rejected the comparative approach and stated that the genetic effects from radiation were cumulative, added to the large doses already received by man from the natural environment.¹³

Muller's relationship with Strauss was just as antagonistic. After winning the Nobel Prize in 1946, he became an increasingly vocal critic of unnecessary radiation exposure to humans, particularly through medical X-rays. His long-standing belief, that a sound social policy should recognize genetic harm from radiation, made him disapprove of the AEC scientists' unwillingness to acknowledge it.¹⁴ A taste of Muller's views was published in *Science* in June 1955. The first sentence reminded readers of the perversions of science by Hitler and Stalin, and the article went on to warn about distortions of science through official policy in the United States.¹⁵ Making such comparisons gained him few allies in the AEC. Strauss barred him from participating in the First International Conference on the Peaceful Uses of Atomic Energy, which took place in Geneva in August 1955, though Muller was told that the United Nations had removed him due to space limitations. Strauss told President Eisenhower's National Security Council that the reason he barred Muller was that he wanted all the American delegates to have security clearance, something he was not willing to grant Muller.¹⁶ When newspapers revealed the AEC's role, Strauss defended his action by arguing that Muller's mention of Hiroshima made his paper inappropriate to a conference devoted to the peaceful uses of atomic energy. But the incident struck many scientists as a clear case of the AEC trying to quash certain scientific views – a point made on the pages of *Science* by another geneticist, George Beadle.¹⁷

¹² A. H. Sturtevant to James V. Neel, 12 Aug 1954, box 11.1, Sturtevant Papers.

¹³ See Sturtevant, 1954.

¹⁴ On Muller's anti-radiation views, see Paul, 1987, and Carlson, 1981, chs. 28–30.

¹⁵ See Muller, 1955.

¹⁶ See Glennon, 1990, doc. 77.

¹⁷ Beadle, 1955, p. 818.

These dramatic confrontations promised to make the Genetics Panel volatile. Muller, Sturtevant, and Beadle all were members, as was James V. Neel, whose attitudes toward the AEC were more conciliatory. Shields Warren, not a geneticist, also participated in the Genetics Panel, somewhat offsetting the influence of Sturtevant and Muller, and overall the panel was a mix of academic and government scientists whose outlooks about the dangers of nuclear testing and atomic energy differed markedly.¹⁸ To head this potentially tumultuous group, the Rockefeller Foundation appointed mathematician Warren Weaver, former AAAS president. Weaver had been the director of the Rockefeller Foundation's Division of Natural Sciences since 1932. His high regard for genetics research was reflected in the long history of Rockefeller patronage of institutions such as the California Institute of Technology, where two of the panelists worked: Sturtevant and Beadle.¹⁹ As a mathematician, he presumably could not be accused of having *a priori* theoretical assumptions.

Although the NAS promised a balanced view of all existing knowledge, the genetics data remained firmly under the control of the AEC, to be released to the BEAR committees at its own discretion. This annoyed some of the geneticists who thought that perhaps the AEC was trying to repress data it had collected over the past decade in the Atomic Bomb Casualty Commission. Muller in particular did not initially believe that the BEAR committees could have any independent voice. In a letter to Beadle, he revealed his suspicion that Detlev Bronk had played a role in his exclusion from the Geneva conference. It was too early, he wrote, to judge the integrity of the BEAR committees, until the scientists had time understand "to how much and what sort of pressures, direct and indirect, they may be subjected." Beadle agreed, expressing "grave doubts" about the wisdom of having a few security-cleared scientists represent all legitimate viewpoints.²⁰

Speaking for the AEC, John C. Bugher insisted that this skeptical view of the commission's secrecy was unfounded. He argued that the AEC wanted to release as much data as possible, but was obligated to

¹⁸ The Genetics Panel, chaired by Warren Weaver, included George W. Beadle, James F. Crow, M. Demerec, G. Failla, H. Bentley Glass, Alexander Hollaender, Berwind P. Kaufmann, Hermann J. Muller, James V. Neel, W. L. Russell, T. M. Sonnenborn, Alfred H. Sturtevant, Shields Warren, Sewall Wright, and C. C. Little. "Genetic Effects of Atomic Radiation," 1956, p. 1157.

¹⁹ See Kay, 1993.

²⁰ H. J. Muller to George Beadle, 22 Sep 1955, and George Beadle to H. J. Muller, 5 Oct 1955, box 5.36, Papers of George W. Beadle, California Institute of Technology Archives, Pasadena, CA (hereafter Beadle Papers).

sit on it while the ABCC scientists analyzed the information at their own pace. It was simply an ethical problem, he said. The people conducting the study ought to have a chance to publish first, and they were evidently doing it as expeditiously as they could. The AEC could not help it if the ABCC scientists were slow. He insisted that “there is no repression of a single scientific fact.” Eventually the ABCC became a major source of information about both pathological and genetic effects for the BEAR committees, as analyzed by ABCC geneticist James Neel.²¹

The AEC’s strategy during the BEAR study was to persuade the Academy scientists that they were, in large part, advising on atomic energy policy. The AEC turned the BEAR study from a detached survey of existing information into a means for recommending specific figures of permissible radiation exposure. Bugher argued that the results of the genetics study should be put in the context of exposure to the whole population, to paint a broad picture of effects over large numbers of people. Only then could the significance of radiation be seen in proper proportion. Examining effects on populations rather than demonstrable effects on individuals appeared to be the only way to gauge the probable effects of genetic mutations, and to make quantitative assessments that might help to make policy decisions about exposure. Bugher was enamored of the work of University of Rochester pathologist Harry Blair, who had measured the effects of radiation in terms of statistical life expectancy. To Bugher, this seemed likely to yield the kinds of policy-oriented results he wanted, and he recommended that the geneticists adopt a similar, practical approach. He warned geneticist Hardin Jones, of the University of California’s Donner Laboratory, that thus far the work on genetics “has all been speculative and there simply is no sound set of data which leads to a sharp quantitative statement relative to the eventual effect of enhanced mutation rates on human populations.”²² Despite their existing feelings about the possible harmful effects, he said, the BEAR scientists should avoid jumping to quantitative conclusions.

The BEAR scientists and the AEC haggled over whether to offer specific recommendations of lifetime dose. The problem was that the probable policy recommendations might be difficult for the general public to swallow. In atomic power facilities, for example, they

²¹ John C. Bugher to Hardin Jones, 29 Jul 1955, folder, “ADM: ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Atomic Energy Commission,” NAS Archives.

²² John C. Bugher to Hardin Jones, 29 Jul 1955, folder, “ADM: ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Atomic Energy Commission,” NAS Archives.

inevitably would require different permissible exposures for older workers than young (in fact, these regulations already were in place but not enforced). If the basic requirement was to minimize the number of mutations in a population that will be passed on to the next generation, then one needed only to ensure low levels during the human reproductive period. Estimates of that period differed: 45, 40, 35, and 30 years were typical. The last may appear to be an incautious cultural assumption, and none of these parameters leaves any leeway for older men who have younger wives still in the child-bearing years. But again, such estimates seemed sufficient for a population approach. Yet, as Warren Weaver summarized the thoughts of Charles Dunham, the new director of the AEC's Division of Biology and Medicine, "it would probably be somewhat difficult to convince the older workers that it was all right for them to have the larger exposures, and in general it is difficult to assign jobs on a basis other than that of skill." But still, these were the kinds of figures for which the AEC pressed the NAS, wanting definitive recommendations.²³

The AEC's influence ran deeply within the BEAR study. Its scientists not only cooperated by sharing data, but many of them served on BEAR committees and subcommittees, and thus the NAS experts and the AEC experts were often the same people, even if the organizations appeared independent of each other. They also relied heavily on information gathered by the AEC, and on the technical expertise of scientists working directly with or for the AEC. Within the waste disposal committee, for example, the subcommittee charged with acting as liaison to Warren Weaver's Genetics Panel was made up entirely of AEC scientists who had already been dealing with occupational hazards, human radiation experiments, and/or ground contamination – Herbert M. Parker of Hanford, Karl Z. Morgan of Oak Ridge, and Forrest Western of the AEC's Division of Biology and Medicine.²⁴ In addition, the BEAR scientists were not given security clearance. Instead, some of the members already were cleared at the highest level – Q – and they were expected to raise red flags if anyone's estimates or conclusions were way off the mark. As Waste Disposal Panel Chairman Abel Wolman awkwardly tried to summarize it, "what it means is that we

²³ Warren Weaver to Genetics Panel, 20 Feb 1956, folder, "ADM: ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Atomic Energy Commission," NAS Archives.

²⁴ Minutes of Meeting, "Study Group on Dispersal and Disposal of Radioactive Wastes, National Academy of Sciences, February 23 and 24, 1956," folder "Biological Effects of Atomic Radiation: Disposal and Dispersal of Radioactive Wastes: Meetings: Feb 1956," NAS Archives. P. 4.

should think as if we were Q cleared and write as if we were not.” This security veil established a knowledge barrier within the BEAR study that privileged AEC views.²⁵

Within the Genetics Panel, the key scientific point of contention materialized between Muller and Sturtevant on one side, and Sewall Wright on the other, and it involved the methodology of assessing risk. Muller’s view, that they ought to see the problem as one of the harmful load of mutations on a population, struck his opponents as an oversimplified view of the role of mutations. This conflict would endure for many years and became the “classical” vs. “balance” debate in genetics.²⁶ Warren Weaver persuaded the panelists to compromise, to include both methodological views in the report or in appendices, as long as the ultimate recommendations remained consistent. As another panel member, James F. Crow, later pointed out, “The irony is that there was no disagreement about the recommendations of the committee, only about the way in which they were justified.”²⁷

As for the AEC’s desire for recommendations, the geneticists were afraid of the implications of any figure they might set. They recognized that it needed to come from them, rather than some other body that did not appreciate the harmful effects to human descendents. But they also recognized that setting a figure would contradict their own position that all radiation was damaging and that there was no threshold of safety. Beadle wanted to abolish the term “permissible dose,” because they could trace almost all of their problems to the use of that word. It was too easy to manipulate the concept. “A beautiful example of a true, but misleading, statement comes right out of [Commissioner Willard] Libby’s remarks. He says the amount of radioactive fallout is only a small fraction of the permissible dose. This is accurate, but completely misleading, because he doesn’t say that ‘permissible dose’ doesn’t mean a thing at all.”²⁸ Anyone could take such a figure and use it to prop up a false notion of safety. The problem was to avoid throwing a “magical figure” at the public, at Weaver put it, because it could be interpreted as a threshold value. Even cases in which activities were controlled, like reactor facilities, sometimes workers received higher doses because of

²⁵ Glenn on, 1990, doc. 77. Transcript, Afternoon Session, Study Group on Disposal of Radioactive Wastes, 23 Feb 1956, folder “ORG: NAS: Coms on BEAR: Disposal and Dispersal of Radioactive Wastes: Meeting Transcript, Feb 1956,” NAS Archives. P. 6.

²⁶ A discussion of the historical development of the classical and balance views is in Beatty, 1987.

²⁷ Crow, 1987, pp. 370–371.

²⁸ Minutes of Second Meeting, Chicago, Illinois, Feb 5 and 6, 1956, BEAR Genetics Panel, box 17.1, Beadle Papers.

repairs needing to be done. These exposures would increase the number of mutations in their offspring. Such acute exposures, they agreed, should be handled by people who were not going to have more children, perhaps by workers over the age of 35, or even 45. Bentley Glass joked, "Or by eunuchs."²⁹

The members of the Genetics Panel agreed on one thing: most people did not seem to appreciate the implications of genetics. It was far easier to understand direct effects on the body. Alexander Hollaender recounted his efforts to explain radiation effects to engineers and physicists. He recalled, "When I discussed the genetic implications, they tried to pooh-pooh it, but when I told them that each r unit could cut down a week of his life, they got excited. [Each of them] called up to find out how much radiation he had gotten. Somehow or other, the danger to future generations does not sink in." The other panelists agreed that it was hard to make it vivid. People cared about themselves, and about their children, but beyond that the implications were too abstract. Bentley Glass suggested that what was needed was a kind of genetic consciousness paralleled by medieval monastery life or, to give a more modern example, by having a vasectomy.³⁰

The geneticists finally arrived at the recommended figure of 10 r for lifetime exposure, but this arbitrary amount was negotiated across the conference table. It was William Russell's figure, and he stood by it – "but I won't be pinned down on the reason for picking it." It seemed reasonable to Weaver that if any group should pick an arbitrary figure, it ought to be the Genetics Panel. But others still were wary of its implications. James V. Neel said that he would accept it if they were willing to say that it was only necessary for the moment, but not ideal in the long term. It would not hurt the human race very much and certainly would not do irreparable harm – to the race as whole – but they should not present it to the public as a safe threshold. "If this is a statement for practical reasons, which the national interests demand, that is one thing, and I am all for it." But if it was issued as a statement which they, as scientists, certified to the public as safe, on the basis of detailed calculations, that was another matter. To this Muller pointed out that they were there as scientists and on that reasoning they should not give out a figure if it had no scientific basis. But in the end they

²⁹ Minutes of Second Meeting, Chicago, Illinois, Feb 5 and 6, 1956, BEAR Genetics Panel, box 17.1, Beadle Papers.

³⁰ Minutes of Second Meeting, Chicago, Illinois, Feb 5 and 6, 1956, BEAR Genetics Panel, box 17.1, Beadle Papers.

agreed that something had to be decided in the meantime – something practical, set not by the AEC but by geneticists.³¹

Strauss and the AEC scientists proved confident in the commission's ability to control information and, because of the population approach, they knew that the results would leave room for their own interpretation of data. Consequently, they came to the conclusion that the independent report of the NAS would serve the commission's interests very well, because the commission could use the results to justify existing policies. In fact, the AEC took the position that it had been the agency to request the study in the first place. This is what Chairman Strauss told the National Security Council in May 1955, and it may well be that the AEC requested the study for the express purpose of consolidating its position with the weight of authority only the Academy could provide.³² By the Spring of 1956, awaiting the publication of the first BEAR report, Sterling Emerson of the AEC told Warren Weaver's Genetics Panel that he hoped they would consider their role as, at least in part, an advisory one to the AEC. The scientists in the Division of Biology and Medicine of the AEC would likely use the NAS findings as a basis for launching research projects and determining permissible doses of exposure. Thus the AEC proved more than cooperative. Yet this advisory status – which was not the BEAR study's express purpose – gave the AEC the justification to insist that its views be taken into account.³³

International Friendship and Negotiation at its Very Best

In Britain, the Medical Research Council (MRC) launched its own investigation of the biological effects of atomic radiation. The MRC had been created in 1914 as an extension of Britain's first national health insurance plan. When Britain's Ministry of Health was created after the First World War, in 1919, the Medical Research Council had continued as a separate entity, to promote objectivity and prevent research agendas from being dominated by the needs of a particular political administration. This long-standing separation and reputation for objectivity seemed to make the MRC ideally suited to study the

³¹ Minutes of Second Meeting, Chicago, Illinois, Feb 5 and 6, 1956, BEAR Genetics Panel, box 17.1, Beadle Papers.

³² On the National Security Council, see Glennon, 1990, doc. 32.

³³ Warren Weaver to Genetics Panel, 12 Apr 1956, folder "ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Atomic Energy Commission, 1956–1958," NAS Archives.

biological effects of radiation. The prospects of a nuclear Britain seemed sure in the early 1950s, with major facilities being formed for research, reactor development, and weapons design. It too would need answers to queries about fallout, radioactive waste, and other health-related issues.³⁴

The AEC suggested that the American and British bodies keep in touch with each other. One of the commissioners on the AEC, Willard Libby, spoke to Bronk on the telephone about it a few weeks after the press release announcing the American study, saying he might wish to contact Sir Harold Himsworth, who had been Secretary of the MRC since 1949.³⁵ In May 1955, Bronk received a letter from Himsworth describing in generalities the study being undertaken in Britain. It was to be a review of the extant scientific information on the medical aspects of radiation, including bodily and genetic effects, which ultimately would result in an official report, or "White Paper." Himsworth was eager to see some informal collaboration between the two bodies, but avoiding the appearance of any official connection. "What I have particularly in mind," he wrote, "is a desire that the individual scientists on our respective Committees should feel perfectly free, as individuals, to discuss with each other any problems in the field with which we are engaged and that you and I, as the respective Chairmen, might feel free to enter into private correspondence on any points which it might seem good use to do so." Himsworth observed that his suggestion amounted to little more than keeping the normal scientific channels open.³⁶

All parties were keenly aware of the crisis in confidence that would occur if the independent reports arrived at significantly different conclusions. Over the next few months, the NAS and the MRC traded preliminary draft reports on an informal basis. For example, when geneticist James Neel had put together some preliminary conclusions about the Atomic Bomb Casualty Commission, Himsworth's committee received unofficial copies. By December, the MRC committee began to plan its draft reports, which would make clear how completely the British and Americans were in agreement, and where some omissions might be addressed. Himsworth added the Neel work "which you so generously have arranged for us to see" to the MRC's own nation-wide

³⁴ Green, 1952, p. 100.

³⁵ W. F. Libby to Detlev Bronk, 27 Apr 1955, folder "ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955-1960," NAS Archives.

³⁶ Harold Himsworth to Detlev Bronk, 4 May 1955, folder "ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955-1960," NAS Archives.

survey of cases of a specific disease, ankylosing spondylitis, treated by whole-body radiation.³⁷

Once these reports were written, another issue confronted the two committees: when should they be published? Himsworth and Bronk agreed that it was important not to appear simultaneously, which would make them appear to be products of collusion. Yet they should not appear far apart either, as that might weaken the force of having the same results achieved in concurrent independent evaluations. “If you were going to press within a month or two of us,” Himsworth wrote to Bronk, “that would seem to me to be ideal.” He warned that anything considerably later would make Himsworth’s position rather difficult, because he would be forced to try to delay an already-prepared report. “I am under some pressure, as it is,” he confided, “and only two days ago the Prime Minister had to say in reply to a Parliamentary question that our report might perhaps be ready in two or three months.” This was in early December – by Easter, he said, the pressure would be very strong, even irresistible if the report was already written, awaiting the American one. Actually, Himsworth would end up waiting even longer than that.³⁸

Although Himsworth did not envision any major points of conflict, he did not want to leave it to chance. To ensure the harmony between the two reports, Himsworth made a trip to the United States in early April 1956. He wrote Bronk beforehand that “my main concern is to compare notes so as to make sure that we are as much in accord as I have been assuming,” and he put himself at Bronk’s disposal for the three days of his visit.³⁹ Warren Weaver met personally with Himsworth during this visit. By that time, each group had preliminary reports that they discussed with each other. Weaver was comforted to see general agreement between the two groups’ findings. Some minor differences resulted in different measurement assumptions. For example, figures for medical and background radiation exposure during one’s reproductive lifetime were not the same, because of different estimates of what constituted one’s reproductive lifetime.⁴⁰

³⁷ On Neel’s role, see Neel, 1998, 1994. Harold Himsworth to Detlev Bronk, 9 Dec 1955, folder “ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960,” NAS Archives.

³⁸ Harold Himsworth to Detlev Bronk, 9 Dec 1955, folder “ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960,” NAS Archives.

³⁹ Harold Himsworth to Detlev Bronk, 6 Feb 1956, and Detlev Bronk to Harold Himsworth, 18 Feb 1956, folder “ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960,” NAS Archives.

⁴⁰ Warren Weaver to Harold Himsworth, 6 Apr 1956, folder “ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960,” NAS Archives.

The American geneticists' population approach was the one serious difference between the two studies. The British group, probably because Himsworth's was a medical council, focused largely on individual human genetics. This led the British group to examine particular disorders, such as inheritable mental problems. From Weaver's perspective, this led the MRC to adopt a "personal" rather than "population" approach, resulting in the British group's reluctance to concede that all radiation was harmful genetically. Because almost all of the American group's findings rested on this assumption, Weaver was intensely motivated to find some common ground. In the Americans' view, the population approach forced the researcher to consider that, even if mutations did not cause a disease in one's child, the detrimental effects did not necessarily manifest themselves in the first generation. "This is also at least partly responsible for your feeling that it may be questionable to say that all mutants are roughly equivalent from the point of view of harm," Weaver argued. "Can this not be, at least in part, because you have been more concerned with the specific effect on individuals, and perhaps chiefly on the first generation, and therefore not so much concerned with the long-run effects and with the large numbers of slight detrimental?" Immediate effects, Weaver acknowledged, would differ widely. But over long periods of time, Weaver wondered if the British would agree with the Americans that these effects would average out.⁴¹

The difference could be chalked up to approach – the American group was trying to set guidelines for large numbers of exposed individuals, whereas the British group was satisfied with unspecified risks to particular individuals. The Americans asked: what is acceptable national policy, based on expected rates of mutation? The British asked: what are the odds that one's child will have a disease caused by radiation? A British scientist might tell some expecting parents that, because of exposure to radiation, their child had one chance in 1500 to have a mental disorder – very heartening news to a worried parent. But taken over a whole population, this meant something very different, because there are a lot more people exposed to radiation than 1500. Some people certainly would have the disorder, and it could be quantified. Only this quantifiable figure for the population could serve as the basis for national policy. The British approach, Weaver wrote to Himsworth after their discussions, "may be of great comfort to individual parents

⁴¹ Warren Weaver to Harold Himsworth, 6 Apr 1956, folder "ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960," NAS Archives.

who are specially fearsome of mental disorder; but does this serve to guide society to a wise course of action?"⁴²

Another major difference developed from British anxiety about the attitudes of the general public. In Weaver's words, Himsworth found "our recommended dose figures a little, shall I say, startling." The reason was not so much the figures themselves, but rather the inclusion of any specific figures at all. Why blend scientific evaluation of the biological effects with policy goals by making specific recommendations? The Americans were planning to recommend 10 r for population exposure over one's reproductive life, and 50 r for occupational exposure. Himsworth found the whole idea of specific recommendations unpalatable, because they inevitably would be misinterpreted by someone and could be manipulated in the press. And recommending different levels of population exposure and occupational exposure would be difficult to justify if a specific figure was named; it would open a host of social and legal problems.⁴³

The difference of opinion weighed heavily on Weaver's mind in the few days after Himsworth's departure from the United States, and he again wrote to him. He acknowledged their mutual dilemma of analyzing harm based on limited scientific study. Individual human genetics and population genetics had not advanced enough that one could combine precision with completeness. "And yet decisions must be made," he said. "The geneticists do not escape their social duty by standing mute – for that decision leads to consequences, just as clearly as does a decision to face the difficulties and give as much help as possible." He added that it was probably a good thing that the two reports were different in approach. He tried to characterize it this way: the British were more precise about a smaller problem, whereas the Americans were more general about the bigger picture.⁴⁴

These divergent views caused anxiety on both sides of the Atlantic. Although Himsworth had said earlier that he hoped to get the report out by Easter, his April 1956 trip to the United States convinced him that there was still more work to be done. He planned to get the report

⁴² Warren Weaver to Harold Himsworth, 6 Apr 1956, folder "ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960," NAS Archives.

⁴³ Warren Weaver to Harold Himsworth, 6 Apr 1956, folder "ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960," NAS Archives.

⁴⁴ Warren Weaver to Harold Himsworth, 9 Apr 1956, folder "ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960," NAS Archives.

to Parliament by the second week of June, and he asked Bronk to hold the American report until then. He was still mulling over the issues presented to him by Weaver when international debates about the peaceful uses of atomic energy convinced his own committee members to oppose supporting a specific 10 r dose recommendation. Both Himsworth and Bronk agreed to pause for a moment and try to reason out the differences. Bronk decided to wait for the British to meet again and discuss the issues raised by the Americans, and asked the magazine *Scientific American* to hold off reporting on the American work.⁴⁵

Increasingly both groups were beginning to regret that, despite their informal correspondence, they had not ensured even closer coordination from the beginning. It seemed clear that they were about to issue two reports with different findings. In late April, Sir John Cockcroft, the director of Britain's Atomic Energy Research Establishment and also a member of Himsworth's committee, paid Bronk a personal visit. Over breakfast, the two men agreed that they needed to find a way to present a common front. Cockcroft worried about public relations, and felt that any disagreements between the American and British groups would be confusing to the general public. Bronk then suggested that the two groups make a list of significant questions and issues, and state the various positions of each committee. Then the information could be exchanged between the two committees, so that there would be no surprises and each group would be well-prepared to address any inquiries from the press about differences between the reports, making it easier to play down the differences. Bronk wrote to Himsworth, "I recall that last summer you and I agreed that it would be well not to have collaboration; but now I wonder whether you would not think it well to have a mutual understanding before release. Cockcroft, I think, liked the idea."⁴⁶

As the release dates of the reports approached, the informal exchanges grew more specific. The boundaries between the two groups blurred, and autonomous evaluation turned to careful negotiation. In May, Himsworth wrote to Bronk about "a particular bone-seeking isotope," which their data seemed to show was accumulating at a rate that would sooner or later "encroach significantly upon the available margin of safety." They had not yet decided how to handle the issue, and Himsworth wrote that "I should be very glad to know what your people are thinking

⁴⁵ Harold Himsworth to Detlev Bronk, 20 Apr 1956; Detlev Bronk to Harold Himsworth, 30 Apr 1956, folder "ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955-1960," NAS Archives.

⁴⁶ Detlev Bronk to Harold Himsworth, 30 Apr 1956, folder "ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955-1960," NAS Archives.

on this subject.”⁴⁷ Himsworth also sent in confidence the drafts of the British committee’s findings regarding hazards, and the specific recommendations the MRC planned to make to the British government. As a friendly reminder, Himsworth emphasized the confidential nature of the report: “Our reports to Parliament are regarded as in the confidential-secret category until they are presented so, unless you want to visit me in the Tower, would you confine the circulation of these papers to those concerned with your report with whom you are in confidential relationship.”⁴⁸ Just as Himsworth sent copies of these preliminary findings, Bronk sent copies of the American one to the MRC prior to its official release in June. Himsworth apologized that the Americans did not receive a copy of the official, final version of the British report until it was released – bound, as it was, “by the rules of Parliamentary privilege.”⁴⁹

In the end, the two reports were published simultaneously on June 12, 1956. The benefits of making consistent points at the same moment outweighed the need to avoid the appearance of collusion. Besides, both the MRC and the NAS had enough influence to ensure that selected media outlets emphasized that the two reports had not been authored together and that despite minor differences, the results were essentially the same. Himsworth happily reported: “At this end, therefore, the timing with regard to publicity has gone just as we wished. I hope it was equally satisfactory over on your side of the Atlantic.” He was very pleased that all of the newspaper articles he saw made due notice of the similarity between the two reports while “each, fortunately, reproduces my assurance that the Reports were prepared without collusion.” The biggest difference that most people observed was not on scientific matters, but rather that the American report was more of a popular account that the lay person could understand, whereas the British report was more technical. To Bronk, Himsworth beamed: “Need I say how much your frank and candid cooperation in this matter has meant to me. It is a good basis on which to continue our trans-Atlantic relationships.” Bronk was equally enthusiastic in his reply, praising Himsworth’s kindness and thoughtfulness in the whole sensitive and

⁴⁷ Harold Himsworth to Detlev Bronk, 10 May 1956 (a), folder “ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960,” NAS Archives.

⁴⁸ Harold Himsworth to Detlev Bronk, 10 May 1956 (b), folder “ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960,” NAS Archives.

⁴⁹ Harold Himsworth to Detlev Bronk, 13 Jun 1956, folder “ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960,” NAS Archives.

complicated process. He called their association over the past year “international friendship and negotiation at its very best.”⁵⁰

If We Could Write the Headline

Bronk and the other committee members were anxious about the reception of their report, hoping not only to see some positive reaction but also to protect its status as an independent evaluation. Having the American and British reports follow the same general lines was a step in the right direction, preventing serious discord at home and abroad. But as he was preparing comments on the preliminary reports of the committees, Bronk began to mobilize efforts to ensure a positive response to the eventual publication of the reports. He called a meeting of committee chairmen to discuss just how, in a perfect world, they would like the report to be received by the press. Already the NAS was in close contact with *Scientific American*, helping one of its writers, Ted Rosenbaum, to craft a summary along the lines that they wished. One of the purposes of the meeting, in fact, was to arrive at “[a]greement as to the essence of the announcement or as Mr. Rosenbaum puts it, ‘if we could write the headline for the newspapers of the world what headline would say what we wish to say?’” They needed to coordinate plans to make a joint public announcement from all of the chairmen during the week of the report’s release. The summary report, which eventually was published in *Scientific American* and formed the basis of most public understanding of the BEAR work, was written by these committee chairmen, with the other committee members having no hand in it.⁵¹

The Academy hired the American Institute of Physics’s public relations director, Eugene H. Kone, to handle the publicity for the report. Kone was planning a press conference on the release date of June 12, for the committee chairmen to field questions about the report. It was crucial, from his point of view and that of the Rockefeller Foundation sponsoring the entire BEAR enterprise, to keep as low a profile as possible until the event, hoping to avoid rampant speculation, such as that which preceded the announcement of the Salk vaccine in April 1955. There was no luncheon, and there was no special check list issued.

⁵⁰ Harold Himsworth to Detlev Bronk, 13 Jun 1956; Detlev Bronk to Harold Himsworth, 3 Jul 1956, folder “ORG: NAS: Coms on BEAR: Cooperation with other Organizations: Medical Research Council of Great Britain, 1955–1960,” NAS Archives.

⁵¹ Charles I. Campbell to the Chairmen of the Committee, NAS Study of Biological Effects of Atomic Radiation, 11 May 1956, folder “Pub Rel: General, 1955–1962,” NAS Archives.

“I would rather underplay it,” Kone said, to avoid sensational coverage. Kone asked that Bronk remind all of the chairmen that leaks to the press were entirely inappropriate – they were vulnerable to partial truths or distortions of all kinds, and information needed to be controlled.⁵²

Although there were some tough questions at the press conference about exposure from fallout and waste disposal, the chairmen conveyed the unified message that no harm had been done yet. But there were some bumpy spots. For example, one reporter asked why there were no political recommendations, and Bronk explained that the NAS was not charged with that responsibility. This was, after all, simply a scientific evaluation. Both Weaver and Roger Revelle, the chairman of the panel on oceanography, understood quite well the implication – what were the policy recommendations? Did they challenge the AEC’s? What was the government going to do about them? They referred the reporter to the parts of the document with specific figures, but Bronk firmly interrupted to point out, “I prefer not to refer to them as political.” The subject in question was of course fallout, and the chairmen were in solidarity in playing down any harm that might have been done. Shields Warren pointed out that the tests themselves were done safely: “There is absolutely no effect on the individuals at the tests over and above those that are discussed here [on the population as a whole].” There were some exceptions to that, including accidental exposure in 1954 to the inhabitants of the Marshall Islands and some Americans near the test site, and another instance when some Americans were handling radioactive materials improperly and received burns on their hands. “Those are the only two exceptions. There is absolutely no risk in the witnessing of a test or taking part in the general preparations.” Later in the press conference Warren interrupted to correct himself: of course the Japanese fishing boat incident also had to be considered an exception to the rule of absolute safety. But the Japanese had disregarded American instructions, he pointed out.⁵³

The Rockefeller Foundation, which funded the whole project, was extraordinarily pleased with the result and, more importantly, with its reception. Lindsley F. Kimball, vice-president of the foundation, was pleasantly surprised at Bronk’s and Weaver’s management abilities, given the wide range of opinions and personalities of individual scientists. This seemed particularly true among geneticists, who were the

⁵² Eugene H. Kone to Charles I. Campbell, 18 May 1956, folder “Pub Rel: Press Conferences: June 1956,” NAS Archives.

⁵³ Transcript of press conference, “The Biological Effects of Atomic Radiation, June 12, 1956,” folder “ADM: Public Relations: Press Conferences: 1956 Jun,” NAS Archives, pp. 31–36.

source of much of the disagreement and agitation about long-term radiation effects, and they managed to avoid any of them breaking ranks and dissenting against the report's findings. "I agree with you," Kimball wrote to Bronk, "that Warren Weaver performed at least a minor miracle in keeping a few prima donna scientists playing ball on the same team." He congratulated Bronk for his leadership in making the whole project a success.⁵⁴

Although the NAS tried to control coverage of the entire process, and to issue carefully worded statements to the press, reporters were not always friendly. Bronk could expect the *New York Times* not to be hostile; after all, its president, Arthur Sulzberger, was one of the Rockefeller trustees who had suggested the BEAR work. The *New York Post*, however, seemed to take an opposite line. Before the reports were ready, the *Post* needled the public with hints that the Academy was not doing its job properly, with headlines like "Danger: Men Not Working" and "Science Academy Still Sitting on that A-Radiation Study." Journalist Robert G. Spivak described the Academy as a quasi-governmental agency that most scientists regarded as "a rather ponderous institution that cannot be hurried." Spivak reportedly telephoned the Academy in October 1955 to find out about its progress and was "laughingly told" that it had only gotten started. He said the report had the "blessing" of AEC chairman Strauss, while quoting approvingly from scientists such as Linus Pauling and Joseph Rotblat, both of whom publicly had warned against the dangers of radiation.⁵⁵ These particular articles were collected by the NAS to assess their public relations efforts after they were marred by the conference debacle between Muller and Strauss. The *Post* dutifully had reported the incident and editorialized, based on Spivak's finding that little had been done thus far, that "[w]e think this is as shameful and shocking a disclosure of human negligence and/or inertia as we have encountered in many a year."⁵⁶

Bronk considered the *New York Times* as the benchmark of American public opinion, and thus he put a lot of stock in how this particular newspaper would receive the BEAR report. He was not disappointed. Although one headline described radiation as a "peril to the future of man," the newspaper followed the report's summary very closely and

⁵⁴ Lindsley F. Kimball to Detlev Bronk, 21 Jun 1956, folder "ADM: Public Relations: Press Conferences: June 1956," NAS Archives.

⁵⁵ Robert G. Spivak, "Science Academy Still Sitting on That A-Radiation Study," *New York Post* (9 Oct 1955), cutting in folder, "ADM: Pub Rel: *New York Post*," NAS Archives.

⁵⁶ "Danger: Men Not Working," *New York Post* (9 Oct 1955), cutting in folder, "ADM: Pub Rel: *New York Post*," NAS Archives.

reiterated the geneticists' general claim that all radiation is damaging and that more research was needed to assess potential harm more precisely.⁵⁷ Sulzberger wrote to Bronk to congratulate him on what he found to be "a fascinating and disturbing report." Bronk's reply gushed in thanks for the "magnificent coverage" and the "splendid account" that made all the work and deliberations among scientists worthwhile: "Once again my admiration for the *New York Times* increases, although I must say I wonder how my admiration can continue to increase and still remain within the bounds of reason. And once again the assistance of the *New York Times* gives me confidence in the future of American culture."⁵⁸ Bronk was gratified to think that the public might receive their report intelligently, and that newspaper outlets might summarize it without misconstruing its conclusions – the *New York Times* was the right paper for the National Academy of Sciences.

The Breakdown of Consensus

Despite the initial rush of enthusiasm, there were many headaches for the NAS after the report's publication. Geneticists and the AEC had found common ground; the report acknowledged the dangerous implications of all mutations, but the statistical (or population) approach allowed the AEC to put these into comparative terms and thus dilute their significance. But it soon became clear that the report simply reinforced the position taken previously by geneticists, against the official views of the AEC, that all radiation was harmful. Observers came to the unpleasant conclusion that Lewis Strauss's previous proclamation – that radiation was harmless – was simply false. And worse, Strauss seemed to be trying to muzzle scientists who thought otherwise. Muller was defended by most of his academic colleagues. He had decided to attend the 1955 international conference, even though he had been barred from speaking, and he received a standing ovation as he sat in the audience. Yet still Strauss felt comfortable in claiming, just a month prior to the report's release, that radioactive waste did not pose a serious problem. He said so as he and Sir Edwin Plowden, chief of Britain's Atomic Energy Authority, visited Shippingport, Pennsylvania,

⁵⁷ Anthony Leviero, "Scientists Term Radiation a Peril to Future of Man," *New York Times* (Jun. 12, 1956), clipping in folder "ADM: Pub Rel: *New York Times*," NAS Archives.

⁵⁸ Arthur Hays Sulzberger to Detlev Bronk, 16 Jun 1956; Detlev Bronk to Arthur Hays Sulzberger, 30 Jun 1956, folder "ADM: Public Relations: Press Conferences: June 1956," NAS Archives.

where the first civilian atomic power plant was being built. He even objected to the phrase “atomic waste,” which he thought unjustifiably dignified the problem.⁵⁹ The *New York Post*, which had criticized the NAS for its sluggishness, preceded the BEAR report’s release by accusing the AEC of inadequate safety precautions – a headline ran “Doctors Find Atom Laxity Threatens Workers’ Children.” The article claimed this was the view of a number of atomic scientists it had surveyed, and it cited Muller’s work. The *Post* charged that the AEC was trying to suppress negative scientific information, to avoid international criticism being leveled against the United States.⁶⁰

Now that the NAS report had been released, such sentiments reached a far wider audience than that of the *New York Post*. The American Broadcasting Company (ABC), for example, aired similar complaints over the radio. A June 18 radio broadcast of the program “Edward P. Morgan and the News” reminded Americans that the world has been playing with matches and that “some invisible flames are already burning.” The program observed the decline in public trust in the AEC: “With all the grim jokes currently making the rounds to the effect that ‘cancer is good for you,’ ‘surgery makes for longer life,’ et cetera, there is a temptation to accuse the Atomic Energy Commission of trying to sell us radioactivity as a kind of vitamin pill.” Although the AEC had not engaged in outright merchandising, the program noted, it had achieved the same effect “by its persistent pooh-pooing of the dangers of atomic radiation and often outright refusal to give the public the facts.”⁶¹ Few laypersons had realized, for example, that there were in fact no “safe” levels of exposure – that was the opposite of what Strauss had said. In addition, few realized that these dangers were already present, due to medical X-rays, atomic tests, and waste disposal practices. All of this information, now set to print by the eminent scientists at the NAS, stood in sharp contrast to the reassuring pronouncements of the AEC.

By October, the contrast in viewpoints diverged even more sharply. Laurence H. Snyder, the president-elect of the American Association for the Advancement of Science, observed in a public lecture that the threat

⁵⁹ On Muller’s standing ovation, see Beadle, 1955, p. 818. On the statement at Shipingsport, see “Atomic Waste No Problem Yet,” 1956, p. 43.

⁶⁰ “Doctors Find Atom Laxity Threatens Workers’ Children,” *New York Post* (Jun. 6, 1956), clipping “ADM: ORG: NAS: Coms on BEAR: Genetic: General,” NAS Archives.

⁶¹ Transcript, “Edward P. Morgan and the News,” Monday, June 18, 1956, American Broadcasting Company, folder “ADM: ORG: NAS: Studies of Biological Effects of Atomic Radiation: Press Conference: Reaction,” NAS Archives.

of genetic damage to the human race necessitated a diplomatic agreement to control testing. Queried about Snyder's statement by the press, Bronk referred the question to a staff member (unnamed), who said that nuclear testing could increase by ten times and still be considered safe. When this statement was published in several national newspapers, it sent geneticists into an uproar. Sturtevant, for example, said that this simply was not the case – and in addition, the data used during the BEAR study had recently been found to be too optimistic, so the danger was even greater than they knew. Bentley Glass, another member of the Genetics Panel, said that the NAS spokesperson's interpretation was limited to thinking only of the United States. Other countries would, no doubt, want to test nuclear weapons and develop nuclear power – the cumulative effect would be beyond any acceptable measure of safety.⁶²

As the 1956 presidential election came near, Eisenhower used the NAS report to argue that radiation from testing was negligible compared to what was received from background radiation and medical X-rays. Others used the same report to justify breaking with the Eisenhower Administration's nuclear testing policies. The Federation of American Scientists, for example, supported Democratic candidate Adlai Stevenson, who had proposed halting the tests. As Eisenhower continued to defend the need to test hydrogen bombs, scientists increasingly took issue with his reasoning – including some scientists working for the AEC. A group of scientists at the University of Rochester declared that saying radiation exposures carried comparatively negligible risks was not the same as calling them safe. On the contrary, they claimed: "There is good reason to believe that they may not be safe." Scientists, however, were by no means in agreement. Although the most serious objections came from geneticists, leading physicists were more supportive of the Eisenhower Administration. Nobel Laureate Arthur Compton, for example, said that the nuclear tests were necessary despite the risks, "to maintain our freedom."⁶³

Such disagreements highlighted the breakdown of negotiated consensus under political pressure. Political will often determined the emphasis placed on data. AEC Chairman Strauss supported the

⁶² "Ten-Fold Rise in A-Tests Seen as Safe," *New York Herald-Tribune*, n. d., clipping; A. H. Sturtevant, "The H-bomb Fallout: A Scientists' View," *New York Herald-Tribune*, n. d., clipping; Weldon Wallace, "Spread of H-Tests Will Build Genetic Threat, Glass Says," *New York Times*, n. d., clipping, folder "ADM: ORG: NAS: Coms on BEAR: Genetic: General," NAS Archives.

⁶³ Walter Unna, "Experts Dispute Ike on Fallout," *Washington Post and Times-Herald* (Oct. 26, 1956), clipping, folder "ADM: ORG: NAS: Coms on BEAR: Genetic: General," NAS Archives.

Eisenhower administration's goals of nuclear testing and widespread adoption of peaceful atomic energy. Thus he played down risks, and used language that tended to minimize the harmful effects.⁶⁴ The BEAR study's population approach made this easy. Although it provided a broad view of the large-scale effects of radiation, it also left the door open for a multitude of emphases. The AEC's favored method was to contrast harm from fallout radiation with harm from natural radiation. In a December 1956 letter to the AEC's Division of Information Services, Charles Campbell (who coordinated much of the BEAR work) complained to the AEC about how it interpreted the BEAR study. "Among the geneticists there are the 'relativists' whose camp your writer seems to join, and the 'absolutists,'" Campbell wrote. "The latter like to keep matters clear by pointing out that two additional handicapped persons per thousand means something like 300,000 handicapped children per generation in the United States alone. They point out that that is a lot more human suffering than the statistic 'about 22 handicapped persons per thousand live births – instead of 20' would suggest."

The AEC typically compared any harmful effects to the existing unavoidable natural, "background" radiation from cosmic rays and radioactive rocks, or from necessary medical X-rays. By contrast, the "absolutists" pointed out that even comparatively minute increases meant that a great deal of people would be affected adversely.⁶⁵

The resignation from the Genetics Panel of its chairman, Warren Weaver, showed the breakdown of consensus most clearly. Although he had not resigned in protest, his subsequent actions betrayed his sense of dismay and frustration at the AEC, particularly Chairman Strauss. In the days immediately following the report's release, he had been asked to take part in numerous radio and television programs. The day after the press conference he gave an interview with John Daly, for his ABC news broadcast, and this was followed by several others for other stations and interviewers. He did not necessarily want to do it, he wrote to Bronk, but "I hardly see how we can take the position that this is a matter of great public concern, and then refuse to make all reasonable efforts to reach the public."⁶⁶ After handing over the chairmanship to George Beadle in the months that followed, Weaver continued to speak

⁶⁴ Pfau, 1984, pp. 182–199.

⁶⁵ Charles I. Campbell to Morse Salisbury, 19 Dec 1956, folder "ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Atomic Energy Commission, 1956–1958," NAS Archives.

⁶⁶ Warren Weaver to Detlev Bronk, 15 Jun 1956, folder "ADM: ORG: NAS: Studies of Biological Effects of Atomic Radiation: Press Conference: Reaction," NAS Archives.

in a variety of venues – news, educational institutions, and Congress. Senator Hubert Humphrey had asked him to testify before his Subcommittee on Disarmament. In doing so, the themes of geneticists' disagreements became increasingly clear, and Weaver no longer felt obligated to play down the risks. Weaver surprised the senators by making specific criticisms of Strauss. He wrote to Beadle, "I remarked that it seemed to me decidedly unfortunate that a high official of the AEC had, in an official statement to the American public, referred to the fallout from the testing of weapons as being harmless." Weaver was quick to point out that the AEC was cooperative throughout the BEAR study, and exerted no pressure on the NAS scientists. But the damage was done. The person who had done so much to keep scientists together during the BEAR study now was breaking ranks.⁶⁷

Weaver was drifting out of the AEC's camp, but sheepishly so. He was well aware of his role in obfuscating the risks of radiation. Although all of the committee members approved the full report, only the chairmen advised and approved the summary report for the public printed in *Scientific American*. In retrospect, Weaver regretted this, especially because he played a larger role in the wording than the other chairmen. Weaver was keen to take responsibility for it but to insist that his precise wording should take him off the hook:

The actual phrase in the red report [for *Scientific American*], stating that "Thus far, except for some tragic accidents affecting small numbers of people, the biological damage from peacetime activities (including the testing of atomic weapons) has been essentially negligible" (my underlining), is in one sense an accurate statement. The word negligible, at least to me, connotes a comparison. It is negligible compared with something else which is not negligible. And the word is still further softened by the adjective essentially.⁶⁸

The underlining and parentheses in the above quote were Weaver's. He was trying to explain to his successor why he, too, had seemed to downplay the harm from radiation. Yet he insisted that Strauss had gone too far.

The whole affair soured relations between Weaver and Strauss. Appalled at Weaver's comments to the Senate, Strauss called him on the

⁶⁷ Warren Weaver to George Beadle, 24 Jan 1957, folder "ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Atomic Energy Commission, 1956–1958," NAS Archives.

⁶⁸ Warren Weaver to George Beadle, 24 Jan 1957, folder "ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Atomic Energy Commission, 1956–1958," NAS Archives.

telephone and they argued. Strauss insisted that he too had only been using the word “negligible,” and that he had taken the word directly from Weaver’s report. But Weaver was thinking not of a recent statement, but one made after the hydrogen bomb test that had blanketed the *Fukuryu Maru* with fallout. After tracking down the official 1954 press release from the White House, Weaver telephoned him back and read the precise wording to him: fallout was “far below the levels which could be harmful in any way to human beings.” He also told Strauss that in the actual report signed by the Genetics Panel, not the one prepared for *Scientific American*, the word “negligible” only occurred twice, and both times was preceded by the word “not.” But from Strauss’s point of view, Weaver was unfairly digging up a statement made two years ago. It seemed wrong for Weaver to bring this up now, years after the fact and after Weaver had extricated himself from the BEAR study. Strauss pointed out that at some time in history a physicist might have denied the existence of mesons, and no one would have held that against him after their discovery. Weaver did not buy it, on the grounds that a physicist would be the first person, after the meson was discovered, to announce his mistake. But more important than this hypothetical example was “the fact that the 1954 statement was not correct *when made*” (Weaver’s emphasis). Weaver pointed out that he had always found the statement “unfortunate” and planned to do something about it “when the time seemed ripe.” Because Strauss had never made any effort to correct that statement at the same level of publicity as the first, Weaver wrote that it was his duty to say something about it. He knew he would be criticized but was glad he had done it, because one could rest assured that the AEC would never make such a baldly false statement about the effects of radiation again.⁶⁹

Weaver wrote to Beadle, “Mr. Strauss clearly feels that I used him badly.” Over the next six months, Weaver seemed to have moved into the camp of those who sharply criticized American atomic energy policy. And Strauss was not the only one who thought so. Linus Pauling asked Weaver to join in the small group of original signers in his petition to end nuclear weapons testing (Pauling would later win the Nobel Peace Prize for his efforts), but Weaver was shy. Although he agreed with the petition in principle, he explained to Bronk that “I simply am not a signer of general statements,” and he felt he would disagree with what the public interpreted it as saying. For similar reasons, he declined

⁶⁹ Warren Weaver to George Beadle, 24 Jan 1957, folder “ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Atomic Energy Commission, 1956–1958,” NAS Archives.

an invitation to participate with Bertrand Russell and other prominent scientists from numerous countries in a meeting in Canada about the perils of nuclear weapons (these became known as the Pugwash Conferences on Science and World Affairs).⁷⁰

In the United States, radiation controversies only intensified in the months between the BEAR report's release and the election. Although the BEAR committees had attempted to set the record straight and ease people's minds with the most accurate information, they had the opposite effect. The BEAR study was supposed to be apolitical, but the election year pushed science far into the realm of politics. The chairman of the Pathology Panel, Shields Warren, stuck with the AEC. He stepped into the fray when he challenged Adlai Stevenson's claims about the danger of radiation. Nuclear testing could continue at the present levels for another thirty years without harmful genetic effects, he argued against Stevenson. Although he did not say so directly to Stevenson – he had sent a telegram to Lewis Strauss comparing fallout to naturally occurring radiation – Strauss made Warren's message public. Stevenson made an easy target, because his running mate – Estes Kefauver – had made the bold and unfounded claim that nuclear testing would throw the earth off its axis.⁷¹ But then Warren himself stood accused of covering up information in his committee's contribution to the BEAR study. He was particularly vulnerable to criticism because he had worked closely with the AEC since its inception. The *Washington Post* ran a story claiming that some of the committee members were dissatisfied with the results but had been steamrolled by Warren. But after inquiring into the matter, the Academy found no one willing to speak up.⁷²

During the election, and for months and years to come, Eisenhower pointed to the BEAR study as scientific justification for nuclear testing and peaceful atomic energy. Eisenhower selectively used the BEAR study for his own purposes, stating that the “continuance of the present rate of H-bomb testing – by the most sober and responsible scientific judgment – does not imperil the health of humanity.” He made specific reference to the study by the National Academy of Sciences, but his statement was carefully worded. Echoing Shields Warren's letter to

⁷⁰ Warren Weaver to George Beadle, 24 Jan 1957; Warren Weaver to Detlev Bronk, 12 Jun 1957, folder “ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Atomic Energy Commission, 1956–1958,” NAS Archives.

⁷¹ “Atomic Expert,” 1956, p. 2.

⁷² Charles I. Campbell to the Members of the Committee on Pathologic Effects of Atomic Radiation and its Subcommittees, 26 Nov 1956, folder “ADM: ORG: NAS: Coms on BEAR: Pathologic: Summary Reports: Press Coverage: *Washington Post*,” NAS Archives.

Lewis Strauss, the president put the issue in comparative terms. Radiation exposure from continued nuclear testing, he claimed, “is, and would be, only a small fraction of the exposure that individuals receive from natural sources and from medical X-rays during their lives.” On the bright side, continued testing enabled the military to develop weapons of greater precision, which would help reduce the problems of fallout in the long run.⁷³

After Eisenhower won the election, the BEAR study’s conclusions were incorporated thoroughly into the policies of government. These conclusions, though optimistic, were hardly as patently reassuring as the president’s words on the eve of the election. In fact, the 1956 BEAR study did much to temper scientists’ unreserved enthusiasm for atomic energy. Briefing a colleague for a Foreign Service Institute lecture, Charles Campbell reaffirmed that it was true that all radiation was harmful genetically. Still, as those favoring nuclear testing argued, this had to be considered in proper proportion. The problem, Campbell said, “is to arrive at a balance between inevitably harmful effects and the good that may come from medical X-rays and, in the opinion of some, the continued development of nuclear weapons.” The harmful somatic effects, which would be emphasized in later Academy studies, were known in 1956 but could not be demonstrated with certainty. Campbell observed that even small amounts of radiation could reduce one’s life-span or cause leukemia, but these findings were not yet conclusive. Even if it was likely in many scientists’ eyes that certain long-lived isotopes ultimately would cause tumors, the lack of conclusiveness to their studies made them powerless to stop the political will that favored nuclear testing and atomic power.⁷⁴

Conclusion

The 1956 BEAR report was far from being a detached, independent evaluation. Instead, its conclusions were negotiated not only among scientists, but also with the Atomic Energy Commission and with Britain’s Medical Research Council. The purpose of the report was to give the American people – and the world generally – an authoritative assessment that could reconcile the public disagreements between government officials and leading scientists. This purpose must be kept in

⁷³ “President’s Text,” 1956, p. 8.

⁷⁴ Charles I. Campbell to Douglas Cornell, 28 Nov 1956, folder “ADM: ORG: NAS: Coms on BEAR: Cooperation with Other Organizations: Department of State,” NAS Archives.

mind, because it was the purpose that the Academy tried and ultimately failed to serve. Its main contribution was in establishing the illusion of uniformity in scientific judgment about what levels of radiation exposure ought to be considered safe. This is not to say that the BEAR report itself was a whitewash; quite the contrary, it was scientific negotiation at its most successful. For a brief moment, it balanced the goals and expectations of a host of interests. But the results of the report were used repeatedly by the AEC and the Eisenhower administration to play down the risks of fallout by calling them minute additions to the bath of natural radiation in which humans already lived. Many of the scientists who had taken part in the BEAR study were disgusted by what they considered a gross misinterpretation of their work. In the end, the BEAR study established guidelines that were reinforced in later studies, ensuring its long-standing influence. However, the BEAR committees failed in their basic objective of bringing the two sides of the debate together. The AEC had what it wanted, namely an independent evaluation to help bolster the legitimacy of its policies. And with the post-BEAR disaffection of key voices in the scientific community, the controversy was stronger than ever, fueling the distrust of government that would mark nuclear history for the decades to come.

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