Genetics in the Atomic Age:  
The Atomic Bomb Casualty Commission, 1947–1956

U.S. officials were understandably anxious about horror stories coming from Hiroshima and Nagasaki in the aftermath of the bombings. At first it was suspected (at any rate, reported) that the Japanese were sensationalizing the effects of the bombs in order to win sympathy and thus obtain milder terms of surrender. But in fact the bombs were killing in ways nightmarishly bizarre and previously unknown in war.

People seemingly unaffected by the explosions were, days later, becoming agonizingly ill. The source of the problem seemed to linger in the environment. In the United States, a chemist who had worked temporarily for the Manhattan Project speculated that Hiroshima and Nagasaki might be uninhabitable for seventy years on account of persistent radiation. Disputed by Manhattan Project officials at home, the claim caused an understandable stir in Japan.²

Residual radiation was not the only concern; there were also fears that effects of the initial exposure might not be manifested until much later. These fears could not be allayed simply by means of a geiger counter.

Reassurances by the American government aside, no one on either side of the Pacific knew for sure what to expect in this regard. The biological and medical effects of radiation had been studied intensely during the war years by Manhattan Project investigators. But that work had been done primarily out of concern for worker safety. The bomb qua weapon had been developed and studied mainly in connection with its physical blast, not its radiation-biological effects. Indeed, although Manhattan Project officials had planned ahead of time to survey the physical effects of the explosions, they had made no special provisions for investigating the biological effects. But it very soon became apparent that some such study was in order.³

Shortly after the signing of surrender papers, medical teams representing the Army, Navy, and Manhattan Project were dispatched to the scene in order to check Japanese reports of the bombings and to determine the possible dangers to Occupation forces from whatever radiation might actually persist. These groups confirmed a variety of symptoms owing to radiation exposure at the time of the blast. They disconfirmed reports of harmful amounts of residual radioactivity.⁴

Initial surveys of the more immediate radiation–biological effects of the bombs were completed by the end of the year, 1945. The groups responsible for these investigations then recommended that the National Academy of Sciences—National Research Council (NAS-NRC) undertake long-term studies. An NAS team, including representatives of the Army and Navy, began feasibility studies in 1946. Late in the same year, President Truman directed the NAS to go ahead with the investigations. To this end, in 1947, the NAS formally established the Atomic Bomb Casualty Commission (ABCC) with funding secured from the newly created successor to the Manhattan Project, the Atomic Energy Commission (AEC).⁵

Of all the original ABCC projects, the one that generated the most interest was designed to ascertain the genetic consequences of exposure to the blasts. Coincidentally enough, in 1946 the American geneticist H. J. Muller had been awarded the Nobel prize for his (earlier) discovery of radiation-induced mutation. Muller had seized upon his newfound eminence to drive home to the public the genetic hazards of radiation. The survivors of the bombs in Hiroshima and Nagasaki served him well in this regard: as he said of them, “if they could foresee the results [mutations among their descendants] 1,000 years from now . . . , they might consider themselves more fortunate if the bomb had killed them.”⁶

Early ABCC investigating teams sought and received reports of malformed babies born since the bombings.⁷ Fears of more and more malformations haunted the survivors of the bombs, the “hibakusha,” and even resulted in their being scorned as marriage partners by other Japanese.⁸ Nor were concerns about the genetic effects of radiation confined to the Japanese. They arose, for instance, among workers in expanding radiation-related fields in the United States and elsewhere. Moreover, as U.S. and Soviet nuclear testing increased during the cold-war period, the possible genetic consequences of exposure to fallout became a growing public concern worldwide.

The genetics project was also interesting for purely academic reasons. It was an immense undertaking in the young science of human genetics—representing a unique opportunity to find out about aspects of human heredity
that could otherwise be inferred only from experiments on fruitflies, mice, and other genetically manipulable organisms.

The ABCC genetics project was potentially very significant for all these reasons and more. However, the results of the study, at the time the most comprehensive investigation of the genetic effects of radiation on humans, turned out to be negative—"negative" in the technical sense of failing to demonstrate an effect, which is not equivalent to disproving the effect. The problem with negative results is that they seem to demonstrate so very little. They are, to use another term, "inconclusive." Interestingly enough, the ABCC genetics study was carried out with the prior understanding that the results would be, in all likelihood, negative or inconclusive, in large part because of certain acknowledged limitations in the nature of the investigation.

But why, then, was the project considered worthwhile? What could possibly have been gained from such results? There is not just one answer to these questions, in large part because there was more than one interested party. Like so many other projects in the postwar era of "big science," this one involved a variety of institutions: the AEC, the NAS-NRC, and the ABCC to name just the most conspicuous. The various interests of all these institutions must be taken into account, but so too must those of the individual scientists involved: the reasons that motivate an institution to pursue a particular scientific project are often quite different from the professional motives of the scientists.

The multiple significances of the project lie in part in the difference between the terms "negative" and "inconclusive." "Negative," with its emphasis on no demonstrable effect, has more reassurance value than "inconclusive." with its emphasis on no demonstrable effect but no ruling out of an effect either. The genetics project was considered worthwhile by some parties on account of its foreseen negative, reassuring results. But as we shall see, the individual scientists involved in the project were much more interested in the inconclusiveness (versus the negativity) of the results.

Complicating matters further was the possibility of conflict among the variety of perspectives and interests involved. Any account of such an enterprise must therefore also make clear the degree of complementarity of the different outlooks that renders the pursuit possible. To explain the nature and various significances of the ABCC genetics project, it is necessary to consider not only several levels of analysis, but also (accordingly) several different contexts: for example, Occupied Japan, postwar America, and the community (and subcommunities) of American geneticists. I shall try to weave these various factors into a single (if reticulated!) narrative, with just a brief analysis at the end. I aim to explain how and why the genetics project was undertaken and accomplished despite its anticipated negative or inconclusive results, thanks to, but also in spite of, the variety of interests and perspectives of the parties involved.

I have restricted my analysis to what was, in retrospect, only the first phase of the genetics project (1947–1956). The leaders of the project—who will be introduced later—are understandably concerned that I not leave readers with the impression that their work ended in 1956, when in fact it continues to the present. At the end of this essay, I refer to some recent summaries of work being done on the genetic effects of the bombings of Hiroshima and Nagasaki.

International Relations

To appreciate fully the genetics project, one must know something more about the ABCC. Similarly, the ABCC must be understood in the context of the Occupation of Japan, which lasted officially until 1952, but which had a still longer-lasting effect.

One of the chief aims of the Occupation was the "democratization" of Japan. This directive left considerable room for interpretation, especially in light of rapidly changing political circumstances. Originally it involved a broad restructuring of Japanese institutions: a new constitution and a newly structured government, an overhaul of the educational system, and important medical, agricultural, industrial, and labor reforms. These efforts also included a large-scale attempt to democratize science, in which the NAS-NRC played a major role.9

But as the Occupation proceeded and the cold war commenced, Japan came to represent much less of a threat to democracy and much more of a potential outpost in its defense. The successful completion of the Occupation became contingent upon making Japan an ally in the promotion of American-style democracy and the containment of communism.10 Scientific cooperation between Japan and the United States came to be seen as an especially important means of cementing relations.

The ABCC played a potentially very important role in fostering scientific and diplomatic cooperation between the United States and Japan. While the ABCC was administered by the NAS-NRC and financed by the AEC, it was also in large part a joint Japanese-American venture. Many Japanese researchers, physicians, and health workers of all sorts were employed. This cooperation was seen early on as not only scientifically but also diplomatically crucial. In a preliminary report, it was remarked that

a long-term study of atomic bomb casualties in collaboration with the Japanese, affords a most remarkable opportunity for cultivating international relations of the highest type. . . . Japan at this moment is extremely plastic and has great respect for the Occupation. If we continue to handle Japan intelligently during the next few years while the new policies are being established, she will be our friend and ally
for many years to come; if we handle her unwisely, she will drift to other ideologies. The ABCC or its successor may be able to play a role in this.17

Similar lines of reasoning recur throughout the history of the ABCC.

Of course, American ABCC researchers sought cooperation not only with the Japanese biological and medical communities, but also—indeed especially—with the hibakusha. Under the circumstances, it seems remarkable that the ABCC could secure cooperation from as many hibakusha as it did in the course of its investigations. As a matter of policy, the organization relied on volunteer subjects. They were compensated only by diagnosis and referral to their own doctors if ill, and if well only by being assured that they were contributing to scientific progress. One other, rather more intangible sort of compensation that the ABCC hoped to be able to provide to the hibakusha was negative findings, in order to allay fears of yet-to-be-manifested effects of the bombings.

The role of the Japanese physicians and health workers in obtaining the cooperation of the hibakusha and conducting the clinical examinations was undoubtedly very great. Otherwise, the hibakusha had to face their recent American enemies, and under very unusual circumstances at their homes or at ABCC headquarters.19

The question obviously arises whether the cooperation of the hibakusha could really be called “voluntary” under the conditions of Occupation. In fact, their cooperation reportedly decreased in 1952, coincident with the ending of the Occupation. With the lifting of the Occupation-imposed restrictions on the Japanese press, criticism of the United States, the Occupation, and institutions like the ABCC spread. ABCC “Research Committee” minutes in 1952 record discussions of new anti-American sentiments and new problems in patient cooperation attributed to the fact that “with the end of the Occupation there has been a dissemination of pictures of atomic bomb casualties and discussions on the use of the atomic bomb. Recently the fact that ABCC gives no treatment and that patients come here merely as guinea pigs has been reiterated in all these media.”20

Many of the hibakusha appreciated the thorough checkups and diagnoses that the ABCC provided for their babies.15 However, there was also dissatisfaction—though exactly how widespread it is hard to tell. The objection that the ABCC used bomb survivors as “guinea pigs” in its scientific experiments was raised more and more frequently. This perception was reinforced in the minds of many by the fact that it was ABCC policy to investigate and diagnose only, without offering treatment. As one character in Hiroyuki Agawa’s 1953 novel, Devil’s Heritage, captured this sentiment, “That’s one reason why they say that the people here are being used as guinea pigs. When the doctors use animals in their experiments, they don’t give it a thought if the animals die or if they have a tough time of it. . . . If data is obtained from these animals, that’s sufficient. There’s no need to treat them for anything. What the ABCC is doing is exactly the same thing.” Not only was the issue treated in the literature of the period; it was also commonly discussed in the Japanese press. It was the single sorest point, diplomatically speaking, for the ABCC. Ambassador to Japan Edwin Reischauer had to admit that the ABCC was something of a “political liability” in this regard.21

It is not entirely clear why the ABCC had this policy. The problem is not that ABCC researchers and administrators had no reason to give. The problem is rather that they offered so many. Early on, treatment seems to have been ruled out as a matter of Occupation policy.23 This may have reflected the Occupation’s general insistence that Japan become self-sufficient, and at the same time not be a financial burden to the Allied Powers. Along similar lines, the Occupation sought to reduce Japan’s reliance on outside sources of antibiotics (like penicillin) by helping to establish scientific and industrial means of producing such drugs within Japan.24

Later, it was argued that treatment by American ABCC physicians would be an affront to the Japanese medical community and might thus threaten the cooperation of Japanese physicians in carrying out ABCC’s research agenda.25 It was also argued that treatment by American physicians would require obtaining a license to practice medicine in Japan, which would in turn require passing appropriate tests, which would in turn require learning Japanese, and so on.26 A very different sort of justification was that, as a matter of principle, countries attend to their own war casualties.27 An argument that some ABCC and AEC officials expressed privately was that treatment would suggest atonement.28 Many Japanese indeed believed that some such compensation for American war “sins” was due them.29

ABCC researchers and administrators, for their part, were certainly sensitive to the bad public relations that resulted from having their projects construed as experiments on A-bomb victims. But privately they also had to worry about how to make their surveys conform even better to the ideals of a good experiment.30

Perhaps the only thing worse than being a guinea pig in a scientific experiment, from the Japanese point of view, was to play the same role in an experiment conducted for U.S. military, or more specifically civilian defense, purposes. Some Japanese feared that the subjects of ABCC investigations were unwittingly being used in this way.31

Criticism of ABCC aims and methods peaked in 1954 for various reasons connected with the “Lucky Dragon” incident. On March 1 of that year, radioactive ash from the first hydrogen bomb explosion (code name “Bravo,” part of test series “Castle” in the Pacific) settled on a Japanese fishing boat, the Lucky Dragon. By the time the ship returned to port, its
crew members had already begun to develop the agonizing symptoms of intense radiation exposure. The subjects of much media attention in their own country, the fishermen were daily reminders of the physiological horrors and genetic fears that the bombings of Hiroshima and Nagasaki had originally wrought almost a decade earlier.30

The initial U.S. response to Japanese alarm was unapologetic and offensive. W. Sterling Cole, chairman of the Congressional Joint Committee on Atomic Energy, was quoted in the Japanese press as suspecting that the fishermen were on a spy mission. Admiral Lewis Strauss, chairman of the AEC, in an official press release, severely minimized the condition of the fishermen. All the while, the AEC would reveal no information on the nature of the weapon tested.37

The AEC did request John Morton, then director of the ABCC, to offer "cooperative assistance" to the Japanese medical personnel overlook the condition of the fishermen, and also to report back with the facts. But Morton’s ABCC team was given a very cool reception. The Japanese press reported that a "cold war" had broken out between Japanese and American medical investigative groups, attributing the problem in part to the concern of Japanese physicians that they would be forced to take an underlaborer’s position if the Americans became involved. They felt strongly that they could handle the situation themselves.38

A more popular ground for concern about ABCC investigations of the fishermen was the that the ABCC was perceived in some quarters as "merciless" in the way it used subjects as "guinea pigs."39 Related concerns about the use of the unfortunate fishermen for military purposes were also reported in the Japanese press:

Americans appear to emphasize the maintenance of secret investigations more than treatment of the patients which Japanese physicians are seriously concerned about. Both the boat and the patients must be precious materials for the Americans. For that very reason they probably consider it natural to request the Japanese to at least cooperate in their investigation, when they are anxious to create the most powerful "retaliative force" on behalf of the free nations in the world.40

In the end, ABCC investigators were allowed to make extensive investigations of only two of the fishermen and less extensive investigations of nine of the remaining twenty-one.

The connection of the ABCC with the AEC had been politically sticky before the Lucky Dragon incident. But it became particularly problematic after that time. The NRC officer most responsible for overseeing the ABCC, Keith Canno, confided in 1955 to Detlev Bronk, then president of the NAS, that a new sponsor might be in order—perhaps the World Health Organization, or the United Nations. Both of those possibilities would disentangle the ABCC from negative associations. As he explained the problem, “the dilemma persists that ABCC is, at once, a scientific project and a diplomatic front. In the latter respect it has established itself as a significant and sympathetic component of the community in which it operates. On the other hand, it is a popular target for anti-American sentiment and will remain so as long as the project is known to be operated from the U.S.A. and to be sponsored exclusively by A.E.C.”

But by the mid-1950s, the ABCC was faltering not only for diplomatic reasons. Its scientific program—except for the genetics project—was also in disarray. In addition to looking for genetic consequences, the ABCC had been looking for other possible late-manifested effects of the bombs. In most of their studies, groups of heavily and lightly or nonexposed survivors were clinically examined and the results compared. Sufficiently greater frequencies of a disorder in a heavily exposed group were then attributed to the exposure itself. That was, at any rate, the program in principle.

During the first several years of ABCC’s studies, the focus had naturally been on documenting the areas in which people are most sensitive to radiation. Thus, early ABCC studies had managed to establish causal connections to increased rates of leukemia and cataracts.42 The areas in which humans are somewhat less sensitive to radiation were taking longer to document, as were still later developing effects. At the same time, an attrition of subjects that was perceived to be due to increased lack of cooperation was resulting in study groups too small for purposes of studying these other effects. Some control groups had even been dropped in favor of clinical examinations of as many highly exposed subjects as possible. Adding to the difficulties of maintaining long-term studies, there was considerable turnover in ABCC personnel and consequently considerable change in the research interests of the staff.

An ad hoc committee convened in 1955 to evaluate the program noted that there was too little of the “experimental viewpoint” to be found there. In an addendum to the report, a statistician on the committee, Seymour Jablons, acknowledged certain problems inherent in the circumstances that made “experiments” on the effects of the bomb very difficult. For instance, the difficulties in acquiring suitable control groups, resembling the highly exposed groups in all respects except degree of exposure, were inestimable. But, he continued, if the ABCC’s studies were to be worthwhile at all, they must at least aim to approach the standards of a good experiment.43

The committee made a number of very specific and very bold suggestions. They proposed a detailed research agenda and suggested means of recruiting well-qualified scientists for long enough stretches to implement it. They proposed monetary compensation for subjects in order to prevent excessive attrition. To free the ABCC from its association with horrors past, the committee also strongly suggested a name change.44
Many of the committee’s proposals were implemented, and the ABCC assumed at new life. Prior to these major changes it had become unclear whether the ABCC could or should continue. But as one AEC official remarked at the time, discontinuation of the program would have been a shame, and not just for scientific reasons:

The AEC has a two-fold interest in seeing that the program is not interrupted: the necessity for making the most scientifically of all available material on the effects of ionizing radiations on humans coupled with a need for assuring that misleading and unsound reports of the effects of radiation on man emanating from Nagasaki and Hiroshima are kept to a minimum. Were the United States to pull out, the vacuum created would assuredly be filled by something, and it might well be something bad, even flavored with occasional tinges of red. Especially might this be the case at Hiroshima. In such an event, both the world scientific community and the United States as a country would be the losers.15

As for the name change, ABCC administrators ultimately felt, and Ambassador Reischauer agreed, that the Japanese would not fail to recognize the change as an essentially political move, so that it would serve no beneficial purpose.16

The general reorientation in the ABCC’s research program was supposed to allow ABCC researchers to find positive effects of the bomb if indeed there were any. But the failure to find many positive effects was certainly not itself a sign of the failure of the ABCC. Indeed, around the time of the reorganization, the NRC’s consultative committee to the ABCC (the Committee on Atomic Casualties) explicitly affirmed the importance of negative as well as positive findings, especially inasmuch as those findings might allay the fears of the Japanese.17

Domestic Affairs

As many Japanese suspected, they were indeed being investigated (in part) for purposes of “civil defense” and for general information concerning the safety of Americans in the atomic age. This is (again, in part) how the AEC justified its support of, and made use of, the ABCC. For instance, in a draft press release in 1950, the AEC quite bluntly acknowledged that “the medical findings of the ABCC have important significance for military and civil defense planning in the United States” (an NRC official reviewing the draft found reason to amend this line, but only suggested adding something about the scientific as well as the other aims).18 The crew of the Lucky Dragon were of special interest for the same sorts of reasons. As ABCC Director Morton acknowledged in his confidential “Preliminary Report” to the AEC on the condition of the men, “These 23 fishermen appear to be of extreme interest to the United States, not only because of the medical lessons, but because of their importance to national defense.”19

Such specifically American ends reflected the agenda not only of the patron of the ABCC but also of its administrator, the National Research Council, an operating arm of the National Academy of Sciences. While its main agenda is the promotion of science, the NRC is also charged specifically with promoting national defense and the public welfare.20 Of course, those are not exclusive aims. The postwar period presented considerable opportunities for the NRC in both regards. Detlev Bronk, the first postwar chairman of the NRC, cited the “growing realization of the value of science, a popular appreciation of the practical accomplishments of scientific research and the increasing availability of funds for science” that had increased the opportunities of the NRC. He further made clear that he intended to take full advantage of the circumstances: “The Council has . . . a responsibility to be sufficiently adventurous in seeking opportunities for leadership and useful action in all fields of science. It is more than a waiting agency through which governmental and private organizations may seek assistance from the scientists of the country.”21

Bronk cited the ABCC as an example of “how the National Research Council can further the fundamental research of individual scientists while serving as an agency of contact between scientists and the government.”22 The ABCC project quickly became by far the largest of the government projects that the NRC administered. By the end of the 1949–1950 fiscal year, the NRC’s expenditures for the ABCC amounted to approximately 45 percent of its total expenses for government projects (the ABCC together with the AEC fellowship program, also administered by the NRC, accounted for approximately 75 percent of the NRC’s total expenses for government projects). The proportion stabilized: by the end of the 1953–1954 fiscal year, NRC’s expenditures for the ABCC amounted to approximately 40 percent of its total expenses for government projects.23

An interesting example of how the AEC, together with the NRC, sought to use the ABCC to serve national interests involves a mutual concern that Americans were overly, that is, irrationally, afraid of radiation. Consider the minutes of an early meeting of the NRC’s advisory committee to the ABCC, the Committee on Atomic Casualties:

Dr. Lyon suggested that the Committee give consideration to the psychological problems that would continue to arise in relation to the atom bomb. He felt that the government would need advice on this problem for the sake of morale and future protection.

Dr. Rivers remarked that this appeared to be an appropriate activity for the Committee to undertake. He stressed the importance of morale and
It was not only the possibility (and soon the actuality) of atomic weapons in the hands of the Soviets that Americans feared. When the testing program came home to the Nevada desert in the early 1950s, Americans learned to fear their own nuclear weapons. The escalation of the cold war, and especially the outbreak of the Korean War, had provided the rationale for a more efficient testing program in closer contact with American industrial, military, and scientific labor, and, on the other side of the cost-benefit equation, in closer proximity to Americans in general.  

The tests originated without serious public incident, thanks to a combination of good public relations work by the AEC, together with considerable patriotism on the part of residents of the nearby areas. But widespread uneasiness developed during the “Upshot-Knothole” series of tests in 1953. Several shots in this series resulted in considerable radioactive fallout, well beyond designated test areas. The public was not unaware: there were emergency measures, such as roadblocks on highways in fallout areas and warnings to inhabitants of some areas to take cover. The AEC acknowledged high levels of radiation (up to 10 roentgens) albeit in thinly populated areas. Growing fears were reflected and further fueled by reports of otherwise seemingly mysterious human and animal (especially sheep and cattle) maladies. Utah’s Representative Stringfellow asked that the tests be brought to “a speedy conclusion” because of the “alarm, bitterness and anxiety” among area residents.  

But it was the 1954 Castle series in the Pacific, and the Bravo shot in particular, that unsettled Americans the most, just as it shocked people worldwide. As Daniel Lang of The New Yorker wrote in 1955, “This [the Bravo explosion] was the shot that made the world fallout-conscious.” The dangers of relatively “local” fallout from thermonuclear weapons were themselves frightening. The crew members of the Lucky Dragon were not the only evidence; twenty-eight U.S. military personnel stationed outside the originally designated test area, and 236 natives of the Marshall Islands, also outside the test area, were heavily exposed and suffered for it, though they received care faster than the especially unfortunate Japanese.  

These events prompted an expansion of the already large test area from 67,000 to 570,000 square miles. But this broadening of the notion of “local” fallout was not the full significance of the Castle series, which resulted in increased levels of radioactivity worldwide. In 1957, with a little time for retrospection, members of the Joint Committee on Atomic Energy (the legislative committee that oversaw the AEC) reflected on the following assessment of the “fallout problem.” prepared internally for their use:

The chances are good that fallout as a subject of much controversy throughout the world would not have come up if the Castle tests and their aftermath had not come up first...  

Castle meant essentially two new facts, as far as fallout is
concerned. First it meant very heavy local fallout, some of which happened to fall out on, of all people, some Japanese. . . . Second, Castle meant nuclear clouds that pierced the stratosphere and left tiny particles of radioactive matter “locked up there” to spread around the globe and fallout over an extended length of time; that is to say, Castle meant fallout from weapons tests alone covering populated areas of the world and continuing for years afterwards.35

Within the AEC these fears and the safety questions underlying them were often seen as separate, if related, problems. There were, in other words, “public relations” or “psychological” as well as technological problems to be solved.36 Somewhat similar to the public relations problems, at least in contrast to the technological problems, were the labor relations issues facing the AEC as it tried to settle questions about adequately safe exposure levels, compensation for riskier jobs, and so on.37

One way of handling fears and concerns about radiation was to appeal to the public’s sense of patriotic duty. The public would have to accept some risks so that the United States could be militarily prepared to contain communism.38 But the best way to handle such problems was to be able to assure the public that the dangers were slight, or at least much less than many anticipated. The ABCC’s finding, together with the results of other research sponsored by the AEC’s Division of Biology and Medicine, presented problems but also prospects in this regard.

Of all the concerns about radiation, the potential genetic effects were the most frightening. Time reported in connection with the original ABCC investigations in 1947 that “what worried the doctors most was the effect the Hiroshima and Nagasaki bombs will have on generations yet unborn.”39 In 1955, Newsweek reported in the aftermath of the Bravo scare that “the principal indirect result of atomic explosions, all scientists agree, is the impact of fallout’s radiation on genes, the remote carriers of heredity in living cells.”40 If the experts were reportedly so worried, it is no wonder that Americans in general should be worried about genetic effects.

Whatever fears the public would have had otherwise were certainly made worse by the worrisome prophesies of the geneticist H. J. Muller. Muller held a position of great authority on the subject of radiation-induced mutation ever since 1927, when he had discovered the phenomenon of X-ray induced mutation. His authority was extended well beyond the community of geneticists in 1946 when he was awarded the Nobel prize for that achievement.

As for Muller’s views on the subject, Time appropriately referred to him as the “glumy Nobelman.”41 Muller was engaged in a regular media blitz, spreading the notion that the atomic bombs at Hiroshima and Nagasaki, as well as fallout from ongoing nuclear tests in the Pacific, had done and were doing considerable genetic damage. As one popular science magazine reported Muller’s reflections:

A worried crown creased his forehead. The effects on the Japanese victims were horrible; yet Muller could figure that the long-range results would be just as bad, all over again. In future centuries, as many children would be born dead or would die early of inherited weaknesses, because of bomb-caused mutations, as died from its original blast.

“There have been planted,” he has told audiences in dozens of cities . . ., “hundreds of thousands of minute time-bombs in the survivors’ germ cells, of far more delayed action than any time-bombs hitherto devised.”42

Bravo heightened concerns about biological effects of radiation in general and genetic effects in particular. In the summer of 1954, geneticists and evolutionary biologists in the United States received the following appeal from their Japanese colleagues:

APPEAL TO THE WESTERN EVOLUTIONISTS AGAINST THE HYDROGEN BOMB
BY THE JAPANESE SOCIETY FOR THE STUDY OF ORGANIC EVOLUTION

Dear Sir:

We are sending this letter to you with our heartfelt appeal to your sympathy for the inestimable victims due to the recent atomic weapons . . . It is our duty . . . to transmit the chromosomes received from our ancestors, without a bit of injury. For further proper evolution, one needs no more Hiroshimas, no more Nagasakis. . . . It is time to appeal for the abeyance of attempts of such tremendous destruction. We hope that you, the western evolutionists, would take the leadership of this appeal, because you are the men most conscious of the destructive influence due to the radiation upon the life on the earth.

The appeal was also published in Science.43

Later that year, Science carried an alarming article by the eminent Drosophila geneticist, Alfred Sturtevant, in which he claimed that “bombs already exploded [in weapons tests] will ultimately result in the production of numerous defective individuals—if the human species itself survives for many generations.”44 Sturtevant’s letter was paraphrased and excerpted widely in the popular press.45

Concerns about the perpetuation of the species aside, Americans were anxious about their own children and grandchildren. By the mid-1950s, as Daniel Lang reported at the time in the New Yorker, many Americans could more easily stomach the idea of nuclear annihilation than the specter of radiation-induced mutations being passed down to their immediate
The possibility of genetic damage to descendants of the Japanese survivors was thus also worrisome to Americans; accordingly, the press and the public placed considerable stock in the outcome of the ABCC's genetic studies.

The Genetics Project

The principal actor in the genetics project was, at least in the beginning, James Neel. Neel completed his graduate work in 1939 at Rochester under the Drosophila geneticist Curt Stern. His interests turned to humans shortly after that. In 1942 he began studies in medicine at Rochester to prepare himself for work in human genetics.

All the while Neel studied medicine at Rochester, his former teacher Stern was conducting experiments for the Manhattan Project on genetic effects of radiation at low doses. Stern was of course working on Drosophila. The mouse geneticist Donald Charles was also working for the Manhattan Project at Rochester. Neel has since said that he was aware that "hush hush" research was going on, but knew, and asked, nothing beyond that. After the war ended, however, the news quickly spread that Rochester had been a center for biomedical research related to the bomb.

By this time, Neel had obtained his M.D., had been drafted, and was stationed at Rochester as house officer. Lieutenant Neel, M.D., was not looking forward to what was surely his fate, namely being sent to an ordinary field hospital to do very ordinary medicine. As it happened, he knew an assistant to Colonel Stafford Warren, the director of the Manhattan Project's biomedical program and the organizer of the initial investigations of the bombings (Warren was a professor of radiology at Rochester). Neel asked this assistant (Joe Holland) if there were any plans to do genetic follow-up studies of the bombings and mentioned that when called to active service he would be interested in pursuing such a possibility himself. When the NAS-NRC-Army-Navy investigative team was put together very shortly after that, in November 1946, it was Lt. Neel who represented the Army.

Neel arrived in Japan to discover that a fairly large-scale study of genetic effects was also being planned by the Japanese. For the next half year, Neel familiarized himself with the design of the Japanese survey, studied the contingencies in Japan relevant to the successful completion of any genetic investigation, and designed an improved project. It was planned as an investigation of only the offspring of the exposed generation . . . prevailing conditions in Japan making only a short-term study possible. As Neel explained at the time, "What we are dealing with is not a laboratory experiment, but a situation which must be approached with due consideration of the present conditions in Japan. Under the circumstances it is impossible to look beyond the next ten or twenty years."

What Neel proposed was, in the parlance of experimental design, a "null" hypothesis test. The usual form of a null hypothesis is that exposure to some treatment (in this case radiation from the bombs) does not have a particular effect (in this case a genetic effect). To test the null hypothesis, one checks to see whether the frequency of the hypothesized effect in the exposed group is sufficiently greater than in the control group—that is, sufficiently great to refute the null hypothesis and thus establish a connection between the treatment and the effect. The point of having a sufficiently large ("statistically significant") difference is to rule out the possibility that the difference is simply a matter of chance. Thus, in the case at hand, one would expect some mutations among the radiation-exposed group as well as among the control group, since mutations occur "naturally" (i.e., for reasons other than radiation exposure). Even if the bombs had no genetic effect, the exposed group might, just by chance, have more mutations. The question, then, is whether the exposed group has a sufficiently greater number of mutations to rule out a chance discrepancy and thus to refute the null hypothesis that the explosions had no genetic effects.

Actually, Neel assumed from the beginning that the bombs must have had some genetic effect. After all, radiation was known to cause mutations in every organism ever studied. The only real question, from Neel's point of view, was whether the bombs had a genetic effect big enough to be clearly distinguishable from chance.

The sorts of mutational effects that Neel planned to study included a higher incidence of malformations and stillbirths, lower birth weights, a higher neonatal death rate, and a skewed sex ratio. As Neel acknowledged in his proposal, however, even with careful controls and large numbers of subjects, the mutational effects under investigation might very well be so infrequent—even if real—as to remain undetected. The main problem in that regard was the anticipated, one-generation term of the project. Unfortunately, in one generation only dominant mutations would be detected (except for any recessives reflected by a skewed sex ratio). And as was well known, dominant mutations are much less frequent than recessive mutations, which are themselves not all that frequent.

Neel's plans were presented to a special advisory committee on genetics composed of biologists of the stature of George Beadle, H. J. Muller, Donald Charles, Laurence Snyder, and Charles Danforth. The meeting was arranged by Neel for the purpose of getting "formal endorsement from my peers before I started spending those millions of dollars in what would probably be viewed as an inconclusive study."

The committee endorsed Neel's proposal and collectively published an announcement of the program in Science. But the announcement was even more exaggerated in its skepticism than Neel's proposal to the committee. The express purpose of the announcement was to make clear the difficulties of the study and the small likelihood of demonstrating genetic effects even if
they existed. The final paragraph of the note hammered the point home: “Although there is every reason to infer that genetic effects can be produced and have been produced in man by atomic radiation, nevertheless the conference wishes to make it clear that it cannot guarantee significant results from this or any other study on the Japanese material. In contrast to laboratory data, this material is too much influenced by extraneous variables and too little adapted to disclosing genetic effects.”

The extreme skepticism was due mainly to the participation of Muller on the committee. Muller insisted at the genetics advisory committee meetings that there were genetic effects of the bombs, whether or not they could ever be demonstrated in the offspring of the exposed generation. He also argued for presenting a very skeptical view of the genetics study to prevent the press and lay public from later misinterpreting negative findings as signifying that genetic effects were not actually produced by the bombs.

Given the extreme pessimism of the announcement of the genetics project, one might well wonder why the committee nonetheless endorsed it. The published statement was short on reasons in this regard, referring only to the “unique possibility” afforded by the situation. The significance of this remark, as elaborated elsewhere, is that as long as there was any chance whatever that such data on humans might be revealing, they should be gathered, for they would presumably (hopefully) never be available for analysis again.

 Actually, Beadle, as head of the special genetics committee, had reported to the Committee on Atomic Casualties that his group did not consider the project justifiable “on purely scientific grounds.” The grounds he cited were, however, somewhat at odds with Muller’s particular concerns. According to Beadle, the committee generally believed that the study would help combat false impressions about the bomb, for example, that it had given rise to many “monsters,” as if the real problem was not the popular underestimation, but rather the overestimation of the genetic effects. This sort of concern arose also in the larger committee’s discussion of the “morale factor” associated with the study, namely, the extent to which it would comfort the public, as well as those people working at jobs involving radiation hazards, to know that competent geneticists were undertaking this project, and the degree of reassurance that the (expected) negative results would provide. Similar concerns were also reflected in Neel’s cover letter accompanying the announcement to Science. Neel expressed the hope that the study would counteract the “lurid publicity” surrounding such discussions. In Neel’s eyes, the press, in search of the dramatic, was invariably more interested in malformation and monster stories.

The committee’s endorsement was not the only reward that Neel received for his initial investigations. It became evident early on that he and his coworkers could also collect information pertinent to more basic issues in genetics and human genetics, like the effects of consanguinous matings (matings between relatives). Areas of Hiroshima and Nagasaki had especially high incidences of consanguinity. One of the first publications of the genetics staff, in 1949, was on this topic; it was an area that Neel and his coworkers continued to explore for many years. Neel viewed the consanguinity studies as an important scientific compensation for the “meticulous bookkeeping” aspects of the main ABC genetics project. As time passed, the genetics study became professionally interesting in yet other important respects.

Neel’s personal commitment to the project is reflected in his concerns about decreased funding for ABC genetics activities in the early 1950s. This was due mainly to changing priorities of the AEC in the face of the escalating Korean conflict. Neel quite strategically urged an official of the AEC’s Division of Biology and Medicine to keep the agency’s own longer-term interests in mind, thus defending his own interests in the project:

Frankly, I believe that the bogy of the potential genetic effects of the atomic bomb is going to rise to haunt the Commission for many years to come unless they take decisive steps to insure the collection of the necessary data. By that I mean, quite simply, the continuation of the Japanese project. Unless the requisite actions are taken in the very, very near future, what is now an adequately functioning machine is going to fall apart quickly—and I’m damned if I will have anything to do with attempting to pick up the pieces if there comes a belated recognition of what has been lost.

The genetics project formally commenced early in 1948. Although Neel oversaw the genetics project, he did not direct it from Japan for the entire period (it is important to keep in mind that Neel was not professionally handcuffed by the project for its duration). He assumed a position at the University of Michigan in 1949. William Schull headed the project in Japan from 1949 to 1951. When Neel recruited Schull to the ABC, Schull had just finished his Ph.D. in human genetics at Ohio State with Laurence Snyder, Madge Macklin, and D. C. Rife. In 1951 Schull joined Neel on the faculty at Michigan. Data collection continued in Japan; data analysis was mainly the responsibility of Neel and Schull in Michigan.

Data collection itself was by no means straightforward. There were considerable difficulties to be overcome. For instance, the genetics team needed to get pregnant women to register the fact that they were expecting. Registration was very important, insofar as it provided a way to keep an eye out for pregnancy terminations of all sorts that might not otherwise be recorded. Cooperation in this regard was secured through mechanisms already in place in Japan. A ration system instituted after the war had special allowances for pregnant women: by registering their condition, they could
spect, leading Neel and Schull to view some comparisons between different exposure classes with reservations and to exclude some data altogether (these very interesting issues cannot properly be treated here—they deserve separate, detailed analysis).

During the year 1953, plans were laid for official release of the genetics results to date. Neel and Schull, in Michigan, drafted a manuscript for publication in Science. But disagreements arose between them and some of the other ABCC geneticists and researchers in Japan. One of the main issues concerned the slightly skewed sex ratios (statistically significant) in the Nagasaki, but not the Hiroshima, data. Some of the researchers in Japan questioned the use of skewed sex ratios as a measure of radiation effects anyway, and in light of the fact that the skewed ratio was found only in Nagasaki they wanted to downplay the importance of those results. As the geneticist Duncan McDonald explained to Neel, "In a preliminary report, if we cannot present clearly and understandably the full significance, complications, and limitations of our findings, we must at all costs, distasteful though it be, deliberately minimize radiation effect indications." He was apparently concerned about causing alarm without very good reason. Neel appealed to McDonald to consider the problem of the ABCC's retraction this sort of claim should not be borne out. It was easy, Neel complained, for McDonald to overlook this problem, since he, unlike Neel, was scheduled to sever his ABCC ties soon.

Neel and Schull did not think the importance of the sex-ratio findings should be minimized. To complicate matters, while Neel and Schull had been preparing their report for publication in Science, the other geneticists in Japan were preparing a report for a conference there. Neel and Schull were understandably concerned that the other group would give the Japanese a very different report—same data but very different conclusions—than would appear in Science. It would cause no small stir in Japan if a report effectively denouncing genetic results were presented there, while a report acknowledging genetic effects were published in the United States. On the other hand, Neel was unwilling to change his own manuscript in the way suggested by the other group, since what they suggested was, according to Neel, "bad science." A crisis was at hand. Rather than try to iron out matters directly with the other group, Neel convened AEC and NRC consultants to the ABCC to consider the differences of opinion and render judgment. The committee decided in Neel's and Schull's favor.

The genetics project was "terminated" in 1954 for unforeseen reasons (in retrospect, it was only temporarily terminated—see the final passages of this essay). When the program was initiated, Japan had record high birth levels. But shortly after that, the Japanese government relaxed restrictions on abortions, in part in order to ease the demand on available food resources. As a result of this and other circumstances, birthrates in Japan
declined considerably, to the point where further data collection seemed unprofitable. Rumors circulated worldwide that data collection had been stopped instead because more and more findings of ever greater statistical significance were emerging. In fact, the results did differ from the earlier results in at least one very interesting respect, namely, the sex-ratio effect disappeared (that is, to say, the sort of exposure-class comparison that had previously yielded a significant sex-ratio effect ultimately yielded a negative effect).

Also in 1954, the Lucky Dragon incident occurred, which unleashed considerable concern about the genetic effects of nuclear device testing. All the while, the geneticists with the most information about the genetic effects of radiation on humans were writing up their results.

The Significance of "Negative"/"Inconclusive" Results

The release of the completely negative/inconclusive results did not go much more smoothly than the release of the preliminary results. Neel and Schull were scooped in the spring of 1955 by then director of the AEC, Robert Holmes, who provided the information for a U.S. News and World Report article entitled "Report on Hiroshima: Thousands of Babies, No A-Bomb Effects."

The man who knows most about the effect of A-bomb radiation on human beings has this to say in a history-making report:

Children of Japanese survivors of atomic bombs are normal, healthy, happy. That's the verdict based on study of 70,000 babies, including 50,000 born to parents caught in bomb blasts.

There is no evidence that future generations of A-bomb survivors will produce deformed or retarded children. 63

When the magazine actually quoted Holmes, the inconclusiveness of the genetic study became somewhat more apparent: "The offspring of the exposed generation up to now appear to be normal, healthy and happy. What the future will show is a problem for further investigation."

Strongly objecting to these and similar statements, Muller went so far as to appeal to the NRC to bar Holmes from communicating the results of the genetic studies to the press. Of course, Holmes had not said anything that directly conflicted with Neel's and Schull's report. Although NRC officials did advise Holmes against any advance publicity for the genetic study, they could not answer his insistent queries as to what he had said wrong. Indeed, Holmes later had a summary of Neel's and Schull's manuscript prepared for him by ABC blostonicians in Hiroshima. This summary con-

cluded, "In general, it may be said that the results of the study were negative, that is, no appreciable effects on the indicators could be reasonably attributed to radiation from the atomic bombs in Hiroshima and Nagasaki." As Holmes explained to Academy officials, "This has been the main line of defense. If this statement is untrue, then we are in trouble. On the other hand, if it is correct, and I can only presume that it is, then much good has actually been accomplished in holding down slanted propaganda."

What concerned Muller, though, was not what Holmes had said—it was what he had not said. In particular, he had not said anything about the limitations of the genetics study that had led geneticists to expect negative results all along, even if radiation effects had occurred.

Around the same time (early spring 1955), the ABC genetics data were also being discussed in Nevada, where weapons testing was about to resume after a two-year layoff since the Upshot-Knothole series. The director of the AEC's Division of Biology and Medicine, John Bugher, addressed the concerns of area residents in a pre-series press conference. Concerning the possible genetic hazards of the test, Bugher argued that there was no cause for alarm.

We've been operating in Japan for several years, in fact since the war, the very important study of the long-term effects of the bombs used at Hiroshima and Nagasaki. . . . An enormous effort has been made to detect in children of persons exposed to those bombs any evidence of genetic change. Now there we're talking of people exposed to large amount of radiation—in many cases just short of a killing amount. They had doses measured in two hundred, three hundred, four hundred roentgens. It appears at the present time, that by none of the genetic criteria which we have in man, can we find clear evidence of a difference between the children exposed to these radiations and the children of a control population in which the parents were not exposed. Now we think the changes must be there, but they are not statistically significant in comparing these two populations.

Now when we come to Nevada, instead of talking about roentgens, we talk about milli- and microroentgens—a fraction of a roentgen, a thousandth of a roentgen. 69

The mostly negative results reported preliminarily in 1953 had previously been used by the AEC in a similar manner on the verge of Upshot-Knothole. 70

A flagrantly misleading report of the genetics results was published in the spring by U.S. News and World Report under the title "The Facts about A-Bombed 'Fall-Out': Not a Word of Truth in Scare Stories Over Tests." This article, informed by AEC sources, was written particularly in response to a supposed "campaign to generate fear"—fear of fallout from nuclear testing.
in the Pacific and at home in Nevada. Among the reassurances provided was the following:

How about the danger to future generations? This fear, the hardest to combat, is being played on particularly in the current campaign to view U.S. atomic tests with alarm. . . .

Much money and effort, first of all, was spent by AEC to keep close track of babies born to those survivors of the Hiroshima and Nagasaki bursts who were subjected to heavy doses of radioactivity. The assumption had been that a number of “monsters,” or at least an increase in abnormalities, would appear, particularly in offspring of two survivors who had both been subjected to near-lethal doses of radioactivity. To many scientists’ surprise, there has been no measurable increase in mutations among the 50,000 children tested. 10

Readers of U.S. News and World Report were perhaps justifiably confused by yet another article, appearing a couple of months later, on the genetic effects of the bombings of Hiroshima and Nagasaki and ongoing nuclear weapons tests, this one based on an interview with Muller, who was billed as “the scientist best able to clear up this question.” As usual, Muller insisted that “most of the children of the Hiroshima survivors who themselves showed clinical symptoms of having been irradiated did inherit mutations that were produced by the bombing—even though the statistics gathered from them were inadequate to prove this point.” 11

Throughout this period, there was much discussion about the way in which the final results should be officially released. Already a year before the publication of Neel and Schull’s final monograph, NAS officials were beginning to worry about the “Japanese resentment” that might result if all the publicity attending the release of the results were focused on America. 12 Neel and Schull decided to present their findings just before the publication of their book at the International Congress of Genetics, which was, appropriately enough, being held in Tokyo that year.

Right before that presentation, Director Holmes was approached with a request for information about the status of Neel and Schull’s report by a representative of the Japanese Atomic Energy Commission. The UN Scientific Committee had asked the JAEC for information concerning the genetic effects of the bombings, and the JAEC representative wanted Japan to be able to contribute that information. Holmes relayed the request to the NAS, along with the reasons offered by the JAEC representative: namely, “since the work had been done in Japan on Japanese people, the only ones suffering such exposure to radiation, it might be proper that the material be presented to the United Nations through a Japanese channel.” Holmes continued.

August 6 is approaching, the maidens from New York are returning, tests in the Pacific are occurring, and the agitating group in Hiroshima, small though it be, is looking for an issue. The subject of Genetics is an easy selection for them, and if we can be trapped into a known refusal to share our information it could be used to their advantage. On the other hand, if the publication is sufficiently conclusive in a favorable light then its public delivery could be timed carefully with an assurance of considerable advantage to us. Therefore, if the Genetics Study be the most significant contribution of ABCC to data, as frequently stated, then the time is probably near to lay the laurel. 13

By the time that the genetics results were officially released, Holmes had departed. But his successors, like him, found the results “sufficiently conclusive in a favorable light.” George Darling, who replaced Holmes, acknowledged this rather more explicitly in describing a difference between Japanese and American interests in the ABCC: “The Japanese scientists outside of ABCC. . . . want to follow up positive leads or turn to broader disease studies. On the other hand, . . . the American Government has an interest in being able to state from first hand knowledge that negatives found are really negatives and that they have been repeated to the point where it is unrealistic to expect further studies to produce new evidence.” 14

From the beginning, ABCC officials had hoped that they could reassure, and in this way compensate, the subjects of their studies with negative results. 15 And this became the most common and important use of the genetics data by the ABCC. Two years after the official release of the genetics findings, the ABCC, now under Darling, instituted a new policy of publishing its reports in Japanese as well as English. The first report included a brief history and overview of the ABCC, for those many readers who had never before been properly introduced. The report emphasized the special suitability of the NAS (not specifically the NRC) for this undertaking, inasmuch as it was a “nonpolitical, nonprofit organization” devoted exclusively to “increasing knowledge and serving public welfare.” 16

One of the greatest scientific difficulties faced by the ABCC, according to the report, was that radiation effects could be discerned in the masses only through long-term studies and careful use of controls. Only the genetics study met those criteria. It was cited in the report as the ABCC’s single most important project. 17

When it came to interpreting the negative results of the genetics study, a dual interpretation, “scientific” and “human,” was proposed: “From the scientific point of view this answer is incomplete because of the complexity of the problems of human genetics. . . . From the human point of view, however, results of this study are reassurance of high degree.” 18 Such
"reassurance," the report continued, should be kept in mind by those who criticize the ABCB for the fact that "it only does research" and does not treat or otherwise compensate its subjects:

The very real service ABCB renders to the community through the research studies is not generally appreciated, and understandably so, because of the intangible nature of the contribution. Considering the findings of the genetic study alone, however, it is difficult to imagine what service ABCB could have performed for the survivors if individuals or the community as a whole which would in any way compare with the reassurance parents are able to give their teenage children today as a result of this . . . study.\(^{123}\)

Darling worked hard to spread the word. As he reported to an NRC official, concerning a meeting he had with a group from Hiroshima, "The officers of the A-bomb Sufferer's League at least certainly understood that research could be a very meaningful thing when they were asked whether the genetic findings might not mean more to the teenagers who were now concerned about marriage than if ABCB had given out extra pencillin or material things.\(^{123}\)

There is some indication that the negative genetic results have at least been seen in Japan as "consolation" relative to the other positive long-term effects like the increased incidence of leukemia.\(^{123}\)

Neel and Schull meanwhile developed their own interpretation of what they preferred to call "inconclusive" findings. In their book-length monograph, The Effect of Exposure of the Atomic Bombs on Pregnancy Termination in Hiroshima and Nagasaki, finally published in 1956, they were quite explicit about the uncertainties posed by their results. As they summarized their findings,

Accordingly, we can say of the present study that under circumstances where, on the basis of what is known concerning the radiation genetics of mammals, it appeared unlikely that conspicuous genetic effects of the atomic bombs could be demonstrated, such effects have in fact not been demonstrated. The present study can in no way be interpreted to mean that there were no mutations induced in the survivors of the atomic blast. Neither, on the other hand, is the reverse interpretation—that of mutation production—permissible from this series of observation, although, on the basis of all that is known of radiation genetics, there is no real reason to doubt that mutations were produced in Hiroshima and Nagasaki. We are left with inconclusive findings, albeit findings which permit us to set confidence limits.\(^{123}\)

As for the "confidence limits," they argued that their data at least ruled out changes in the sex ratio greater than 1.6 percent if the mother was heavily exposed and 4 percent if the father was heavily exposed. The data also ruled out any more than a 100 percent increase in the malformation rate and an 80 percent increase in the frequency of stillbirths. The data did not rule out smaller effects, nor did the data confirm smaller effects.\(^{123}\)

Neel has pointed out to me how he and Schull "went over backwards" to avoid using the term "negative."\(^{123}\) In addition to "inconclusive" findings they preferred terms like "uncertainty" and explicit qualifications such as: "At the moment . . . we are reluctant to infer more than that the data remove the remote possibility of a conspicuous sensitivity of human genes to irradiation (i.e., marked mutability). Even this conclusion is open to challenge, depending on the definition given "conspicuous."\(^{123}\)

An interview of Schull in U.S. News and World Report in 1956 contrasts sharply with interpretations of the genetics project by Holmes and Darling and the AEC. Although the "no effect" interpretation found its way into the title of the article ("How about the Children of A-Bomb Survivors? New Study Shows No Ill Effects from Radiation"), Schull himself emphasized uncertainty: "We should like to be the first to recognize the unsatisfactory situation in which this study leaves us with respect to drawing firm conclusions concerning radiation-induced genetic changes in human populations. . . . We do not conclude that A-bombs did not have any detrimental genetic effects. Our conclusion is simply that such bad effects were not demonstrable."\(^{123}\)

All of this emphasis on inconclusive findings, which do not sound especially reassuring, again raises the issue of the meaning and value of the genetics project. But the results were very important from Neel and Schull's point of view—not simply in spite of the inconclusiveness but because of the residual uncertainty. To Neel and Schull, there was significance even in their statistically insignificant findings. They used these negative results positively to stake out their own particular position on the genetic hazards of radiation for humans; it was a position of extreme uncertainty, in contrast to the extremely pessimistic positions of Müller and (especially) Sturtevant. Thus, while Müller considered the ABCB genetics results to be consistent with work on Drosophila and mice that showed clearly the very harmful effects of radiation, Neel and Schull preferred to emphasize the fact that all studies of the genetic hazards of radiation, on flies and mice as well as humans, were equivocal.

The last chapter of Neel's and Schull's monograph was a thoroughgoing critique of attempts to assess the genetic hazards of radiation in Drosophila and mice, and of attempts to extrapolate from Drosophila and mice data to humans.\(^{123}\) The foundation for this critique had been laid in earlier work by Neel on various difficulties involved in estimating spontaneous
(naturally occurring) mutation rates, the baselines for evaluations of radiation effects. This work was further elaborated in Neel and Schull’s 1954 textbook, Human Heredity.\textsuperscript{19}

For instance (and this is just one example), to estimate spontaneous mutation rates from prevailing gene frequencies, one would have to know (among other things) how natural selection affects the frequency of the gene in question (the greater the selection pressure against the gene, the better its prevailing frequency will reflect its spontaneous mutation rate). To extrapolate generally, one would then have to know how natural selection usually affects genetic variation. By the early 1950s there was already considerable disagreement about general effects of selection on genetic variation, and by the time Neel and Schull were writing their manuscript the dispute was even more pronounced. Muller, for one, defended the view that selection acts mainly to reduce genetic variation; optimal genes for most traits having already been selected, recurring mutations are usually selected against. But there were others, most prominently Theodosius Dobzhanity and Sewall Wright, who argued that genetic variation within species is ubiquitous, in part because of selection, and that such stores of variation are crucial in guaranteeing the evolutionary plasticity of species. This controversy also bore more directly on the question of the genetic effects of radiation: if variation were ubiquitous and even advantageous, the effects of radiation would be somewhat less deleterious than if variation were scant and mostly disadvantageous.\textsuperscript{20}

Until one could resolve these and other issues, one would be unable to assess in any precise manner the genetic hazards of radiation. The uncertainties at the time, according to Neel and Schull, were enormous. The data, they suggested, were consistent with the most pessimistic as well as the most optimistic assessments of the hazards of radiation—ranging from Muller and Sturtevant’s position that any additional mutation owing to radiation was genetically and evolutionarily undesirable, to views such as those of the origin-of-life researcher John Kenison, published in a feature article in Science in 1955, to the effect that increased variation due to radiation might be an evolutionary advantage for humans in light of the rapidly changing environments to which they must adapt.\textsuperscript{20}

Neel and Schull vigorously defended their position of uncertainty. They acknowledged that it was a position difficult to defend in the face of pressure to settle on assessments precise enough for use in setting standards for protection. But they saw virtues in resisting the temptation to conclude too much from the data then available: “There is . . . the possibility that by refusing to be drawn into premature speculative calculations which in the nature of things will be "used" as soon as they have been set to paper, and by insisting on all possible occasions that the work that should be done actually be carried forward, the geneticist in the long run will arrive more quickly at the goal of a lasting, valid appraisal of this problem.”\textsuperscript{19}

Neel continued to press this line as a member of various expert panels convened during the mid- to late 1950s to reach agreement on the genetic hazards of radiation. Neel and Muller squared-off on a couple of these occasions: the 1955-1956 meetings of the NAS Committee on the Biological Effects of Atomic Radiation and the 1956 meetings of the World Health Organization Study Group on the Effect of Radiation on Human Heredity. At these meetings, Neel relentlessly challenged Muller’s (and others’) assumptions. He referred at the time to his position in these discussions as that of “â€œbite soixante.”’\textsuperscript{19}

Neel and Schull’s emphasis on the uncertainties surrounding Drosophila and mouse studies should also be considered in connection with their roles within the community of human geneticists. Again, Schull was trained as a human geneticist and by 1956 was accomplished in various respects other than the ABCC studies. Neel was by that time a leader in the field. For instance, he had served as president of the Society for Human Genetics in 1954 (the society began in 1948). The range of his accomplishments in this field was also much broader than just the ABCC project.

At this time, the scientists with the most extremely pessimistic positions concerning the genetic hazards of radiation for humans were fruitfly geneticists who, moreover, often belittled humans as unsuitable objects of genetic study.\textsuperscript{20} (By the way, it says something about the infant state of human genetics that one of those Drosophila geneticists, Muller, was actually the first president of the Society for Human Genetics.) Consider again Sturtevant’s influential article in Science in 1954 (the one with the alarming suggestion that genetic damage from bomb tests was leading to the extinction of the species). The title of the article, “Social Implications of the Genetics of Man,” was a peculiar one given the first sentence: “Man is the most unsatisfactory of all organisms for genetic study.”\textsuperscript{20} According to Sturtevant, the genetics of other organisms should serve instead as the basis for human genetics. It was on the basis of what was already known about those other organisms that Sturtevant so confidently predicted the demise of humanity.

Neel, who had been sent a copy of Sturtevant’s article by the editor of Science, Duane Roller, reacted sharply to it:

Each of us has his biases. As one who has staked his professional future on the coming of age of the study of heredity in man, I think I have some insight into the direction which my own take. However, I did serve an apprenticeship in Drosophila genetics. I wonder whether those who have worked with this animal realize the extent and form of their
own biases... If one looks to man for the characteristics which have made Drosophila so useful in one stage of the development of genetic thought, one will of course be disappointed. But I must hasten to add that if one looks at Drosophila for help with some of the problems with which we are coming to grips in man, one is equally disappointed.13

The inconclusive results of the ABCC project did raise questions concerning the possibility of obtaining information about the genetic hazards of radiation from human studies. But Neel had not worked for eight years on the ABCC project only to demonstrate the difficulty of research on humans and the superficialness of such research in the light of work on other organisms. The last chapter of his and Schull’s book guarded against this particular interpretation of the previous fourteen chapters. The fact that their own studies were inconclusive, they argued, signified only that more studies were needed. And given the difficulties plaguing extrapolations from other organisms, those studies would have to include humans.

Just as Neel defended his position of uncertainty in front of other geneticists on the expert panels of the 1950s, so too did he strongly defend the importance of human genetics.13 As he explained to Muller in one of his more conciliatory (but still typically firm) moods,

I expect to devote much of my remaining scientific life to providing data pertinent to reaching a decision on this matter [i.e., the genetic hazards of radiation for humans]. To me, now that—thanks in large part to your own contributions—awareness of the genetic risks of radiation is widespread, there is a real danger in crystallizing our thinking on a quantitative level too soon. If there were no prospect of collecting pertinent data for man, I might feel differently. However, I honestly believe that we are on the threshold of a new era in the study of human genetics, with every prospect of advancing our knowledge of man as rapidly in the next 30 years as we have of Drosophila in the past 30.13

Summary and Postscript

The significance of the ABCC genetics study is in the eye of the beholder. From the perspective of several of the parties involved, the study was part of a much larger effort in diplomacy. For the NAS-NRC, it was part of a very large effort to promote science through the promotion of national defense and the public welfare. From the ABCC’s own point of view, the results were reassurance for the hibakusha and thus a form of compensation for their continued participation in the other ABCC studies, or at least some consolation to them. From the AEC’s point of view, the results (and perhaps even the investigation itself) represented a useful antidote to alarming (if not alarmist) speculation concerning the hazards of atomic energy. For Neel and Schull, the results fit squarely into a position of considerable uncertainty in the ongoing scientific disputes about the genetic hazards of radiation for humans; this was in turn a useful platform for promoting the new field of human genetics.

But there is perhaps a problem here in the variety of these interpretations of the significance of the genetics project. The ABCC’s and the AEC’s uses of the genetics study might seem to involve sweeping under the rug all the difficulties in interpreting negative results. How could the results be reassuring if, as Neel and Schull seemed to suggest, they left so many wide-open questions about the extent of radiation-induced mutations? Some particular claims made on behalf of the genetics results were certainly misleading—even fraudulently—reassuring. The AEC-informed, U.S. News and World Report article, “The Facts About A-Bomb Fallout: Not A Word of Truth in Scare Stories Over Tests,” is a case in point. But were the ABCC and the AEC wrong in principle to offer the genetics results as reassurances? To be sure, this was an interpretation in each of their best interests. But that alone is not evidence of the illegitimacy of those interpretations.

Although negative/inconclusive results are commonly (if not altogether justifiably) considered to be a less than successful outcome of a scientific investigation, there are acknowledged exceptions: for instance, when the results are truly surprising. That is, when there is overwhelming anticipation that a particular causal connection will be borne out, but when in a powerful test it fails to be. Now positive results were not anticipated by geneticists (this is the most flagrantly fraudulent aspect of the U.S. News article). But positive results (or something akin to positive results) were anticipated by laypeople: positive results had to be anticipated to be feared. Continuing along that line, one might further reason that the extent of reassurance or relief provided by negative/inconclusive results depends on how great the fears of positive results are.

Not only were positive results anticipated by the lay public, many also believed that positive results had been obtained. Individual cases of malformed babies born to exposed parents continued to be reported as if they constituted positive evidence. As the cold war escalated and the atomic age advanced, there were new and stronger political incentives for this publicity. Certain antinuclear and pacifist groups made the most of such reports. For instance, it is not hard to see through the intent or imagine the effects of films such as The World Is in Dread, a 1957 Japanese production. Scenes of the ABCC labs in Hiroshima and Nagasaki are followed in the movie by pictures of genetic abnormalities in irradiated fruitflies (some with traits like one eye, which, to make things worse, supposedly appear several generations after radiation exposure); later comes a two-headed baby born to a hibakusha in 1950, followed by a baby born without a brain in 1951, and
still other major malformations follow that. According to the psychologist
Robert Lifton, from whom this description of the movie is taken, there are
distinctions made in the film between true radiation-induced mutations and
the effects of exposure to offspring in utero, but these distinctions are easily
lost in all the rapid scene changes.129

Consider, along the same lines, a peace pamphlet circulated by the
“London Committee of 100” in 1963. It shows a picture of a malformed
baby with the following caption: “This baby’s eyes and nose had merged
into one mis-shapen feature in the middle of its forehead. Its mother was at
Hiroshima on August 6th 1945; she received heavy doses of radiation. Radia-
tion from testing now taking place may cause many more mutations like
this.”130

Given these sorts of expectations and hence fears, the negative results
of the genetics study might reasonably have been consoling. Perhaps the
apparent conflict between the Neel/Schull position of uncertainty and the
ABCC/AEC position of reassurance can be resolved by taking into account
these special circumstances.

The potential conflicts between the Neel/Schull and the AEC/ABCC
positions may actually have been less serious than a couple of other con-
flicts. For instance, as we saw, the AEC’s principally American interests
were suspected by the Japanese and threatened to undermine their coopera-
tion with the ABCC’s investigations. In addition, when Neel and Schull
later decided to pursue in greater depth the nature and consequences of high
consanguinity in Japan, they sought to downplay their ABCC affiliation, in
part to avoid the guinea-pig stigma associated with the ABCC.131

The connections between the diplomatic/political and the scientific as-
pects of the ABCC and its genetics project were complicated to be sure (and
only part of the story could be told here). But intertwined they surely were.
The ABCC and its genetics project were part and parcel of the atomic age.

Although it may be viewed as whiggish to do so, it strikes me as even
more mischievous not to acknowledge in closing that the genetics project
continues to the present day, with Neel and Schull still playing important
roles in the analysis of data. The ABCC was formally succeeded in 1975 by
the Radiation Effects Research Foundation, which is jointly supported (in
principle, fifty-fifty) by Japan and the United States; the American half is
currently channeled through the Department of Energy.

The more recent genetic investigations include electrophoretic studies
of proteins from blood samples gathered in the early years of the ABCC.
The electrophoretic techniques reveal protein differences attributed to differ-
ent genes, whether recessive or dominant. By comparing an offspring’s pro-
teins with those of its parents, one can look for variants unique to the
offspring, and hence attributable to recessive as well as dominant mutations.
These findings are also inconclusive.141

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tional Education, and the American Society of Zoologists. I cannot help
feeling that with all this assistance, I should have done a much better job.
But there is more to come!

Notes

1. Memorandum of telephone conversation between General Leslie Groves
and Lieutenant Colonel Rea. 25 August 1945. Radiological Effects, File 5G, Cor-
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Times, 13 September 1945; see also Michael J. Yavenditti, “The American People
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247; Peter Wyden, Day One: Before Hiroshima and After (New York: Simon and

2. Paul Boyer, By the Bombs Early Light: American Thought and Culture at the
Dawn of the Atomic Age (New York: Pantheon, 1984), p. 188; “A Job for Hirohito,”
The New Republic, 24 December 1945, pp. 367–368; “70-Year Effect of

3. Barton C. Hacker, The Dragon’s Tail: Radiation Safety in the Manhattan
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11. Kawai, Japan’s American Interlude, pp. 27–33; Reischauer, United States and Japan, pp. 32–41; the extent of this shift is debated in Robert E. Ward and Sakamoto Yoshikazu, eds., Democratizing Japan (Honolulu: University of Hawaii Press, 1986).


17. James V. Neel to Philip S. Owen, 8 January 1948, ABCC Directors’ Correspondence, ABCC Records, NAS Archives.


19. See, for example, “Minutes of the Third Meeting of the Japanese Advisory Council for ABCC,” p. 7, J. C. Bugher Collection, Series 1, Subseries 2, Box 9, Folder 106, Rockefeller Archives Center.


22. See, for example, “Minutes of the 49th Meeting of the Advisory Committee on Biology and Medicine,” pp. 103–104.


28. John Bugher to John Morton, 16 March 1954, Fukuryu Maru Incident,


117. Ibid., pp. 5–6.

118. Ibid., p. 6.

119. Ibid., p. 10.

120. George B. Durling to R. Keith Canner, 31 January 1958, Directors’ Correspondence, ABCC Records, NAS Archives.


123. Ibid., p. 199.

124. Personal communication.


131. Ibid., p. 217.

132. See, for example, the minutes of the NAS Committee transcribed as “Proceedings, Conference on Genetics,” 5–6 February 1956, esp. pp. 76–79, 104–105, 145–146, 274–280 (Mendel’s self-characterization is on p. 275), COM NAS. Cons on BEAR, Genetic Meetings: Transcripts, NAS Archives.

133. See, for example, Muller in “What Will Radioactivity Do to Our Children?,” U.S. News and World Report, 13 May 1955, p. 76.


135. James V. Neel to Duane Roller, 7 July 1954, Box 15, “Neel #5” Folder, Curt Stern Papers, APS Library.

136. See, for example, the minutes of the NAS Committee transcribed as “Genetics Panel, National Academy of Sciences Committee to Study the Biological
12

Epilogue: The Development and Expansion of the American Society of Zoologists

In December of 1984, I attended my first annual convention of the American Society of Zoologists (ASZ), held that year in Denver. I had been invited by Larry Spencer, a biologist at Plymouth State College who enjoys an avocational interest in the history of natural history, to present a paper on work I had been doing on the natural history tradition in the early Washington Territory (after 1891, Washington State). At this symposium, which Professor Spencer had organized with Brother C. Edward Quinn (Biology Department, Manhattan College), I met other members of the Division of the History and Philosophy of Biology, a relatively new section of the society (1981), and we began to discuss how to attract professional historians and philosophers of science to active participation in the division.

Perhaps these early discussions were so tinged with bonhomie that I failed to detect the sinister aims that motivated Professors Spencer and Quinn. They were interested in identifying a historian of biology who would be willing to write a history of the ASZ, a project both of them wanted to have completed by the year of the society's centennial, 1989–1990. Luckily, I recovered my sense of critical acumen soon enough to dissuade them from coercing me to compile a book-length history of the society. Instead I volunteered to produce a pamphlet-size history of the society by 1989–1990 if they would help me obtain support to produce a multi-authored volume on the history of American biology. Happily, and largely owing to Brother