Chemico-Biological Interactions 424 (2026) 111832

Contents lists available at ScienceDirect

Chemico-Biological Interactions

journal homepage: www.elsevier.com/locate/chembioint

Review Article

Embracing dishonesty: How LNT became king

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ARTICLE INFO

Keywords:
Cancer risk assessment
Mutation
Hermann Muller
Eugenics
Dose response
EPA

ABSTRACT

The US NAS BEAR I Genetics Panel in 1956 offered a profoundly influential recommendation to switch from a threshold to a linear dose response model for radiation-induced hereditary changes and key gene-mutation-mediated somatic endpoints. Historical and recent findings indicate that this NAS Report is based on falsified data from panelist William L. Russell that significantly inflated panelist estimates of radiation-induced mutational risks and that panelist James Crow disguised massive panelist uncertainty and variability to promote acceptance of its LNT-risk-based recommendations. This paper extends those challenges to the integrity and credibility of the 1956 NAS report and of decisions by regulatory agencies such as the US EPA to adopt such recommendations by showing that: (1) the "esteemed" NAS panel lacked the necessary expertise to guide the country on radiation-induced genetic risks based on a strikingly anemic research publication record on that topic, (2) selection of the panelists was influenced by commitments toward an evolutionary-based eugenics framework, (3) a key eugenics strategy involved the adoption of an LNT model for risk assessment to reduce population genetic variability, and (4) Muller persuaded the Panel chair to add Crow to blunt an anticipated challenge to Muller's genetic load hypothesis by population geneticists based on enhanced fitness in heterozygotes at optimized radiation exposures, which challenged his eugenics framework. In addition, the selection of a Panel scientifically weak regarding radiation experiments may have been intentional to allow Muller to dominate Panel activities consistent with his eugenics perspectives. It appears likely that Muller and Crow conspired to manipulate Panel activities, leading to the adoption of LNT for radiation risk assessment, as part of the larger RF plan to control the direction of human evolution via eugenics principles and practices.

1. Introduction

In 1956 the United States National Academy of Sciences (NAS) Biological Effects of Atomic Radiation (BEAR) I Panel made the recommendation for regulatory and public health agencies to shift from the use of a threshold to a linear non-threshold (LNT) dose response model for hereditary and cancer risk assessment [1]. This recommendation would prove to be far reaching with respect to radiation and chemical carcinogen risk assessment, the cost of compliance with regulations, impact on technology development and on the general awareness of risks and their dose response relationships. The BEAR I Genetics Panel LNT recommendation provided the basis for the emission standards of the nascent US nuclear power industry [2] and the fundamental cancer risk assessment framework for the Federal Radiation Counsel which would become the radiation division within a newly created EPA starting in 1970. The BEAR I Genetics Panel's so-called LNT dose response "vows" of 1956 were renewed 16 years later with the next US NAS expert committee [i.e., Biological Effects of Ionizing Radiation (BEIR) I] and passed on to EPA. The EPA soon implemented this guidance in their establishment of cancer risk assessment policies and practices for both radiation and chemical carcinogenesis [3–9]. James Crow, Chair of the 1972 BEIR I Genetics Committee, had been a highly influential member of the 1956 BEAR I Genetics Panel and was a close friend of Hermann J. Muller, making the LNT policy continuity a virtual certainty, a symbol of Muller's legacy [9,10].

The BEAR I Genetics Panel risk assessment recommendations have remained intact for 70 years, which is quite a testament to the actions of this Panel, which was broadly promoted as being an extraordinary grouping of radiation genetics talent and vision [11–13]. The first paragraph of the Leviero [11] *New York Times* article stated that "A committee of outstanding scientists reported today that atomic radiation, no matter how small the dose, harms not only the person receiving it but also all his descendants." The next paragraph would claim that "it was the most comprehensive United States effort to determine how the

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https://doi.org/10.1016/j.cbi.2025.111832
Received 21 July 2025; Received in revised form 14 November 2025; Accepted 17 November 2025

future of the human race might be affected by the unleashing of nuclear power." While this image has long remained intact, it may come as a surprise that most of the geneticist Panel members had never published an article on radiation-induced mutations. Indeed, almost all of them had very limited research on the topic prior to their selection on the Panel, reflecting a general lack of appropriate expertise. Thus, the question must be asked as to how and why these panelists were selected.

The present paper extends our recent publication showing that panelist William Russell provided falsified mouse mutational data that led to significantly elevated mutation risk assessments and that this was not detected/discovered by the panelists [14]. It also extends a recent detailed assessment of how each panelist made his estimates, even with the falsified data of Russell and how these estimates were manipulated by Crow to minimize Panel uncertainty and variability to enhance acceptance of Panel recommendations [15]. Making matters even more complicated and troubling is that the present paper shows that the research experiences of most members of the Panel were deficient with regard to induction of mutations by radiation, the principal focus of the Panel. Even with those panelists having some experience, it was quite limited and mostly not timely. These novel perspectives challenge the basis for how/why this group of panelists was selected which led to the manufactured image that they constituted an amazing group of talented experts whose opinions were not only the most informed and credible but also closely converged, thereby providing apparent greater assurance of the consistency and reliability of their guidance. The paper explores the possibility that the selection to the Panel by the Rockefeller Foundation (RF)/NAS may have been strongly influenced by their high dependence upon the RF for research funding and their scientific and ideological support for eugenics as a means to direct human evolution.

It is probably unknown to most risk assessors that the concept of LNT was viewed as a fundamental feature of a eugenics-based strategy for directing human evolution [16]. Within this context, this paper will assess how Muller tried to prevent the influence of Theodosius Dobzhansky, also an LNT proponent, who challenged Muller's genetic load mutation hypothesis via what were then recent discoveries in population genetics and how this was framed within a eugenics context. The integration of these diverse actions (e.g., Russell data falsification, use of the falsified data for key risk estimates, decision to exclude key panelist estimates by Crow, exclusion of the Dobzhansky perspective and the integration and guiding of Panel activities within a eugenics framework to affect the direction of human evolution) resulted in the LNT recommendation by the Panel that became adopted in the US and worldwide. The integration of these diverse developments reveals that the scientific foundation for hereditary and cancer risk assessment was based on a series of critical mistakes, deliberate misrepresentations of the scientific record, massive self-interest and an overriding desire to affect the direction of human evolution via eugenics principles and practices.

2. Criteria for selecting the BEAR I Genetics pPanel: science, ideology, and self-interest

The action of the RF to fund and direct the NAS BEAR I Genetics Panel is seen here as a tactic in a much larger scheme to advance a massive geopolitical narrative/initiative of social engineering to guide no less than the future course of human evolution [17]. Julian Huxley's Galton Lecture [18], as presented to the Eugenics Education Society (February 1936) and which displayed the "enlightened" (i.e., reformed), non-racist eugenics framework, acknowledged that the goal of eugenics is "to control the evolution of the human species and guide it in a desirable direction". The plan was a breathtaking one, far greater in scope than campaigns against cancer, Alzheimer's Disease, hunger, human rights and other exceedingly important goals. These valued goals were seen as being subservient to the control of human evolution.

The present paper provides insight to the question of why the RF offered major financial and related support for Muller through the many ups and downs of his professional career as well as its efforts to secure for him an appropriate professorship over many years. Its efforts were finally successful with his appointment at the University of Indiana, where its "silent" influence was substantial, being helped by Frederick B. Hanson, Muller's long-term RF grants manager and LNT supporter [19]. The RF tried especially hard to ensure Muller's success and acceptance at the University of Indiana by giving substantial funding for genetics research in Muller's new Biology Department for a number of other faculty members (e.g., Tracy Sonneborn, Ralph Cleland), in an effort to make the often argumentative and disagreeably assertive Muller more "appreciated" and welcomed rather than one to be avoided and isolated. Muller came to Indiana bearing gifts, all stamped RF [19]. In many ways, the RF was setting the stage for their US academic "investment" such as Muller, to be activated, as a sophisticated type of figurative "Manchurian Candidate". This was also the case for some of the other geneticists on the BEAR I Genetics Panel who had also received goods and services from the RF making it likely that they would accept the call to deliver, which in this case, was to serve on the BEAR I Genetics Panel and to deliver LNT to the U.S. and world community.

The Chairman of the NAS BEAR I Genetics Panel was Warren Weaver, the longtime research director of the RF [20]. In retrospect, the placement of Weaver, a non-geneticist, to manage/direct this Panel, may well have been done, at least in part, to protect their unique investment in Muller, who was doing his best to challenge and alienate the US Atomic Energy Commission (AEC), so much so that they prevented him from speaking at the first Atoms for Peace Conference in Geneva, a highly publicized decision that they would eventually regret [8,9].

The BEAR I Genetics Panel was constructed largely based on the experience and goals of Weaver. The RF had long been funding genetics researchers, especially those whose research and related activities were broadly directed to the area of eugenics [20]. The following section briefly considers how the concept of eugenics came to impact their selection to the Panel.

2.1. Weaver's RF strongly supported Caltech: connection to Beadle, Dobzhansky, Lewis, Pauling and Sturtevant

The RF investment in the Biology and Chemistry/Chem-Engineering Departments at Caltech would affect the recruitment of **Beadle, Sturtevant and Dobzhansky** to the BEAR I Genetics Panel. Yet these three genetics leaders had published only one research paper on radiation-induced mutations, a nearly decade old 1947 paper by Dobzhansky and Spassky [21]. This is not unlike the case with Caltech entomology professor Edward B. Lewis, who was working under the direction of George Beadle, then the Department Chair, having a first, yet, major publication on radiation and cancer risk assessment in the journal *Science* in 1957 [22]. Furthermore, Pauling, like Muller and Beadle, another Nobel Prize winner, was a dominant force challenging the AEC with respect to nuclear issues, including those relating to LNT. In fact, Lewis and Pauling had numerous interactions on these matters during the development of Lewis's 1957 *Science* paper [23,24]. In the case of Sturtevant, he addressed the issue of genetics and eugenics in various publications in supportive but in rather cautious and academic ways. It was clear that Sturtevant saw the value of eugenics principles within society. Even a decade after the convening of the BEAR I Genetics Panel, Sturtevant [25] wrote "It is evident that animal breeders have, by selection from mixed populations, produced many reasonably uniform breeds, possessing desired characteristics and including many individuals more extreme in these respects than any found in the original population. There is no reason to doubt that similar results could be obtained with human populations." However, Sturtevant raised the key question of "Who sets the goals?" within a democratic society and how could some version of the eugenics agenda be acceptably attempted. Prior to the move to Caltech, Sturtevant had worked at Columbia for about 15 years under the direction of Thomas Hunt Morgan who was a very strong supporter of eugenics during these early years [26], with both becoming less strident and coercive, a perspective that resonates

with the above quote from the now much older Sturtevant. Thus, the eugenics impact on Sturtevant started early in his career, morphed over time, being impacted by strong forces such as Morgan and the directions of the RF.

2.2. The RF funding of Muller

Muller had been identified by the RF as a major talent that they wanted to support, from the mid-1920s onward [19]. In the case of the now older and mature professorial Muller, it seems likely that by the mid-1950s he still appreciated the need to please one's funder. For example, in a September 22, 1955, letter of Muller to Beadle [27] (just two months before the start of the BEAR I Genetics Panel activities), he captured the plight of the academic researcher who is always in search of funds. Muller recounted a recent meeting with Warren Weaver in which he had expressed concerns with a recommendation of Weaver, then president of the American Association for the Advancement of Science (AAAS), not to have the AAAS undertake a major review of radiation health effects since the NAS was going to do so. Muller then stated: "I was, so to speak, taking my scientific life in my hands in doing this because our group here would like to get a renewal of our Rockefeller Grant, beginning next year."

Calabrese and Shamoun [17] have tracked the relationship between the RF and Muller, especially with respect to his being awarded the Nobel Prize, some of which will be briefly summarized in a subsequent section. Not only would the RF target **Muller** (see Appendix) for the BEAR I Genetics Panel, but Weaver would use Muller to identify possible Panel members, who would satisfy the scientific and ideological needs of the RF and Muller. Those appointments directly linked to Muller include Cotterman, Crow, Glass, Sonneborn and Stern.

2.3. Muller influenced Panel appointments

2.3.1. Tracy Sonneborn (see Appendix)

Muller affected the appointment of his University of Indiana office mate, Tracy Sonneborn, who was an officer in the American Eugenics Society [28]. Sonneborn had no research publications in radiation-induced mutations.

2.3.2. James Crow (see Appendix)

According to Crow [29], yet another officer in the American Eugenics Society [28], he was selected to be a member of the BEAR I Genetics Panel as an "afterthought" to add a population geneticist who could aid Muller to counter possible arguments of Dobzhansky in support of his population genetics balance hypothesis for natural selection [30]. In fact, after all the other panelists had been selected, Muller made his pitch to Weaver to add Crow [29]. Crow [29,31–33] had gone to graduate school at the University of Texas to study with Muller, only to learn that Muller was off to Germany and the Soviet Union, never to return to Texas. Crow and Muller would reunite when Crow was a professor at Dartmouth College and Muller was a professor at Amherst College in the early 1940s [19], as the travel time via car was only about 2 h between the two campuses. Approximately two years before the start of the BEAR I Genetics Panel activities, Muller and Crow had begun to work together on genetic issues associated with consanguinity, at that time a very powerfully focused area of eugenics research [34]. This was at about the time the classical-balance controversy between Muller and Dobzhansky started to erupt [16,30], preparing the way for the Muller request to Weaver regarding the addition of Crow to the BEAR I Genetics Panel.

2.3.3. Bentley Glass (see Appendix)

Another Muller connection was the appointment of Glass, who was Muller's last Ph.D. student at the University of Texas. Despite a weak publication record, Glass would also become a prominent national leader in the eugenics movement, especially trying to incorporate it into secondary school education in the US and worldwide, an activity for which he recruited Muller during the mid-late 1950s, that is, during the meeting time of the Genetics Panel [35]. Glass and Muller made a concerted effort to integrate explicit eugenics principles and practices into the US secondary school curriculum, only to be denied this possibility largely due to highly negative response to an eugenics advocacy article by Muller [36] in *Science* at a critical time during the process of textbook finalization [35].

It is sometimes very hard to grasp the real intentions of leaders, such as Glass. For example, it may be asked whether Glass's use of revisionist, more socially acceptable language, was merely a tactic to promote his philosophical and political agenda? Or, was he an authentic reformed eugenicist as his writings suggested? In Glass's case, his repeated claims to be a reformed eugenicist may be best viewed within the context of a commentary that he offered in *Science* in 1971 [37]:

> "*I reiterate that the right that must become paramount is not the right to procreate, but rather the right of every child to be born with a sound physical and mental constitution, based on a sound phenotype. And again, just as every child must have the right to full educational opportunity and a sound nutrition, so every child has the inalienable right to a sound heritage. Perhaps that can be achieved on a voluntary basis, through educational understanding, genetic diagnosis and wise counseling. That, of course would be preferable. But if such means prove insufficient for the task, social compulsion may indeed be the only alternative, whether we like it or not.*"

These comments are striking and demand the question: who was the "real" Bentley Glass? Was Glass a hard-core eugenicist or an authentic reformist? Based on his own statements, Glass appears to unmask his real philosophical identity, which he so long kept successfully disguised. This situation may be best elucidated in the assessment of physician and ethicist Leon [38] who affirmed that: "Bentley Glass convicts himself by his own defense."

2.3.4. Charles C. Cotterman (see Appendix)

Muller also had a close professional relationship with Lawrence Snyder, a genetics professor at Ohio State University. Snyder was a significant leader in the American Eugenics Society.[2] Charles Cotterman, a Ph.D. student of Snyder, was also selected to be on the BEAR I Genetics Panel even though he had no publications on the topic of radiation-induced mutations. Muller developed a close working relationship with Cotterman and Snyder as they created the *American Journal of Human Genetics* in the late 1940s, with Muller becoming the first president of the American Society of Human Genetics and Cotterman becoming the editor in chief of the Society's journal, *American Journal of Human Genetics*, in 1948. He was the editor for a six-year term that was set by the Society guidelines [40]. At this time Cotterman was on the faculty at the University of Michigan along with **James V. Neel** (another member of the BEAR I Genetics Panel), with both working in the University's Heredity Clinic until 1953 under the direction of Professor Dice,[3] who had a very strong eugenics focus [41].

2.3.5. Curt Stern (see Appendix)

Stern was a close colleague of Muller, hiring Muller to serve as consultant on the Manhattan Project [8]. Prior to his involvement in the Manhattan Project, Stern had never published a paper on radiation-induced mutation. Stern also never published another paper on radiation-induced mutation after the completion of the Manhattan

[2] Snyder, the American Society of Human Genetics (ASHG) president (1950), stated that feeblemindedness was one of traits that are "so undesirable that the race will do well without them" and favored sterilization as an efficient means to prevent their proliferation within the population [39].

[3] Dice was another ASHG (1951) president who also supported the eugenics application of sterilization but did so within a voluntary framework [39].

Project research, which resulted in three papers on that topic published in the 1948–1949-time span. Stern was a long-time member of the American Eugenics Society. Stern also had a long-term relationship with the RF, first receiving a fellowship in 1932. He also contributed articles on eugenics to *Eugenics Quarterly* [42] and *Scientific American* [43] and a long chapter in his Genetics [44] textbook. In his 1952 article in *Scientific American* [43], he wrote: **"The eugenics movement proposes to make the process less haphazard in the future by applying national designed population policies, looking toward the elimination of genetic misfits and an increase in the number of those with superior genetic endowments"** (emphasis added) (page 72). This perspective permeated the Stern textbook as well. Stern was invited to join the BEAR I Genetics Panel but turned it down due to strains from recent extensive travel [5,8].

2.3.6. James V. Neel (see Appendix)

Neel was also a BEAR I Genetics Panel member with many professional connections to the field of eugenics, starting with his Ph. D. advisor, Curt Stern, at the University of Rochester. Neel would also work under the direction of Professor Dice at the University of Michigan in its Heredity Center after he received his MD degree, also being a colleague with Cotterman [39]. Of further importance is that Neel had a more than 50-year collaborative working relationship with William Schull on the atomic bomb research project [41]. Schull was also a long-standing member of the American Eugenics Society. [28]. Much of Neel's research on hereditary diseases and their gene frequencies and familial relationships had a eugenics component. While Neel's research on the effects of the atomic bombs on offspring of survivors was extremely relevant for the activities of the BEAR I Genetics Panel, Muller dismissed the negative findings as illusionary at the first BEAR I meeting [45], in effect precluding it from being considered by the Panel. This resulted in an extremely strained relationship between Muller and Neel. In addition, the relationship between Muller and Neel was further strained because Neel tended to support Dobzhansky's balance hypothesis [45,46].

The selections of **George Beadle** and **Milisav Demerec** were affected by the fact that both had the same Ph. D. advisor at Cornell University, Rollins A. Emerson, who was one of the three major early figures in American genetics, along with William Castle of Harvard and Thomas Hunt Morgan of Columbia. Emerson had a long interest in eugenics and became one of the signers of the Eugenics Manifesto [47].

2.3.7. George Beadle (see Appendix)

Beadle would move from Cornell to Caltech to eventually take over the leadership of the program that Thomas Hunt Morgan had developed after he left Columbia. Morgan built his new program with strong funding from the RF, funding that continued under the leadership of Beadle [20]. RF support for the research of Emerson at Cornell was also the case, with continuous funding for several decades [48]. Thus, Emerson was a key target of the RF research funding largesse. During his period of becoming the new chair of the BEAR I Genetics Panel, Beadle [49] published a paper in *Eugenics Quarterly* on "Man's evolutionary future". The paper that Beadle published in the *Eugenics Quarterly* was based on his presidential address given to the AAAS in New York City on December 27, 1956.

2.3.8. Milisav Demerec (see Appendix)

With respect to Demerec**,** he moved immediately from Cornell to start work at the Carnegie Institute at Cold Spring Harbor in 1923. It became the leading center in the world promoting the adoption of a eugenics perspective, especially during the early years of Demerec's involvement [50]. Prior to his involvement with the BEAR I Genetics Panel, Demerec had shown an interest in gene mutation in microorganisms and fruit flies. However, he had only published two papers on the effects of ionizing radiation/X-rays on the induction of mutations approximately a decade prior to the BEAR I Genetics Panel meetings (Table 1). It should be noted that Demerec helped to direct the research program of Bruce Wallace, a former Ph. D. student of Dobzhansky [64] and also a member of the American Eugenics Society [28].

Table 1
Data based study publication record (not including only a reported abstract or review/discussion, opinion papers) of NAS BEAR I Genetics Panel concerning ionizing radiation-induced mutations prior to appointment to the Panel (November 1955).

Genetics Panel Members	
Geneticist	Publications
Milislav Demerec	[51,52]
Bentley Glass	[53,54]
Bertwind P Kaufmann	[55,56]
Hermann J Muller	[57–59]
William L Russell	[60]
Invited But Were Unable to Serve	
Curt Stern	[61–63]
Theodosius Dobzhansky	[21]

2.3.9. Bertwind Kaufmann (see Appendix)

Demerec also likely influenced the appointment of Bertwind Kaufmann to the Panel since he was a staff scientist working under the direction of Demerec at the Carnegie Institute [65]. The publication record of Kaufmann indicates he had published two papers on the effects of ionizing radiation on mutations in the 1947–1949 period [66,67].

2.3.10. Theodosius Dobzhansky (see Appendix)

Theodosius Dobzhansky was invited to join the BEAR I Panel. Dobzhansky was also a signatory of Muller's Eugenics Manifesto [47], as well as being part of the TH Morgan, Columbia and Caltech connection. Dobzhansky had long-standing collaborations with Sturtevant and Wright, both members of the Panel. Dobzhansky was long interested in the concept of mutation but studied it within the context of population genetics, variation and natural selection with a combination of laboratory and field studies. Prior to the BEAR I Genetics Panel he had published only one paper [21] that included an aspect of radiation-induced mutation, with that study involving fruit flies [68].

2.3.11. Sewall Wright (see Appendix)

Sewall Wright was a graduate of Lombard College. While there, Francis Brute Key taught Wright botany and inspired his interest in genetics, introducing him to original research, reading important articles on Darwinism and genetics and directed him toward graduate study. Key left Lombard College in 1912 to work for Charles Davenport at the Eugenics Records Office at Cold Spring Harbor [69–72]. Of further relevance is that Wright obtained professional training at Cold Spring Harbor over several summers. Wright received his Ph.D. from Harvard under the direction of William Castle, who was a leading eugenicist, especially during the era when Wright was at Harvard. After receiving his Ph.D., Wright became a professor at the University of Chicago teaching a course on Evolution and Eugenics [69]. Furthermore, Wright and Dobzhansky were very close collaborators during and after the period of the creation of the Eugenics Manifesto. With respect to research publications on the concept of radiation and mutations, Wright did not have any prior to the BEAR I Genetics Panel. However, Wright [73] published a review of a paper by Robley Evans [74] concerning radiation and mutation.

2.3.12. Clarence C. Little (see Appendix)

The influence of the RF on eugenics research extended to the activities of Clarence C. Little, another Panel member, at the Jackson Laboratory in Maine as will be summarized below. Little directed a long-term research project for RF with eugenics applications via the use of animal models. However, Little did not publish any papers on the effects of radiation-induced mutations. Little was a student of William Castle at

Harvard University who, as noted above, had a very strong interest in eugenics [75]. Castle served on the Eugenics Record Office's board at its founding. He also published the text ***Genetics and Eugenics: A Textbook for Students of Biology and a Reference Book for Animal and Plant Breeders*** [76]. The Castle book ended with a chapter titled "The possibility and prospects of breeding a better human race." This chapter was structured within a eugenics framework.

In 1919, Little accepted a position as research associate and assistant director of the Station for Experimental Evolution, Carnegie Institute, Cold Spring Harbor, N. Y. [77]. The station at Cold Spring Harbor was created by Charles Davenport, a foundational member of the early American Eugenics movement. In 1922, Little became the president of the University of Maine, moving to the University of Michigan in 1925. One year of Little's University of Michigan presidency overlapped with his role as president of the American Eugenics Society (AES) in 1929. He was a director of the AES from 1923 to 1939, both during and after his tenure ended at Michigan [78].

Little was an enthusiastic supporter of the American Eugenics movement and was one of the founders of the American Eugenics Society [79]. Directly following the Second International Congress on Eugenics, of which he was the Executive Secretary, the American Ad Interim Committee was created and later evolved into the American Eugenics Society. Little served on the Committee's Board of Directors along with Henry Fairfield Osborn and Charles Davenport, whom many consider to be the originator of American Eugenics. The major achievement of Little's presidency was the expansion of the AES State Committees for which he appointed State Directors and enhanced the Society's capacity to lobby state and local institutions. During a public lecture associated with the 1932 6th International Congress of Genetics in Ithaca, NY, Little stated: "We favor legislation to restrict the reproduction of the misfit ….by compulsory sterilization …." [78].

2.3.13. Alexander Hollaender

Weaver also had a long-standing professional friendship with Alexander Hollaender, starting when both were at the University of Wisconsin in the late 1920s and early 1930s [80,81]. After Weaver left to become the research director of the RF in 1932, he initiated a funding program with Hollaender to research the biological effects of radiation. This relationship with Hollaender would extend for several decades and be a key factor in the invitation of Hollaender to the BEAR I Genetics Panel. It was largely via this connection that Weaver would invite **William Russell** (see Appendix) to join the BEAR I Genetics Panel [10]. Russell was a native of the UK, doing his Ph.D. at the University of Chicago under the direction of Sewall Wright. After receiving his Ph.D., Russell was hired by Little to do research on genetics at the Jackson Laboratories in Maine. Little arranged for Russell to move to ORNL as a result of marital difficulties that affected Russell's job.

The construction of the BEAR I Genetics Panel was thus a complex mixture of high-level genetics talent, linked to organizational connections within the RF, as well as Muller and other leaders in the genetics community. For the most part, the research and publications of the Panel members in the key area of the capacity of radiation to induce mutations was astonishingly weak. Of the 15 geneticists invited to be on the Panel, eight had no record of publications on the topic of the Panel (Table 2), that is, the capacity of ionizing radiation to induce genetic damage with a prime focus on gene mutation in reproductive cells. Of the remaining seven with publications in this area, there was a total of ten publications, with only two being published within the previous five years (Table 1).

The overwhelmingly weak publication record of the Panel in the area of major concern stands in marked contrast with the image created for the Panel, as being the most esteemed Panel imaginable. It is necessary to recognize that the Panel as a group was comprised of a group of very accomplished, talented geneticists who were very seasoned professionals. However, the majority of these panelists were being asked to render judgements on areas of genetics that they had essentially no substantial experience with, based on publication record, which is a standard and objective measure. Even for those with an authentic publication record, their publications were generally quite limited, and many years removed from the Panel activities. Thus, as a group they were not a good fit for the challenge. The striking lack of genuine experience in the area under review would be reflected in their substantial personal uncertainties of mutation risk estimates and the extent to which they differed amongst themselves. It is also noteworthy that at that time scientific knowledge was woefully inadequate to make meaningful estimates of hereditary risks of radiation exposures. These concerns arose even as the Panel was directed to follow courses of action that were designed to reduce such concerns. That is, even though the guidance was to develop independent estimates reflecting their different backgrounds, Crow tried to make their efforts more similar in a disguised fashion that was challenged and exposed by James V. Neel [15]. Thus, the view that the country was being guided by the "best of the best" for the problem at hand was a manufactured image that the NAS created and that media, such as the *New York Times*, promoted. The panelists did not challenge this highly inflated image.

Table 2
Geneticists on the BEAR I Genetics Panel who had no publications on the topic of the capacity of ionizing radiation to induce genetic damage with a prime focus on gene mutation in reproductive cells.

Geneticist	Number of Publications
George Beadle	0[a]
James Crow	0
Charles Cotterman	0
Clarence Little	0
James Neel	0
Tracy Sonneborn	0
Alfred Sturtevant	0
Sewall Wright	0

[a] Beadle used X-rays and UV in the early 1940s to induce mutations in *Neurospora* for biochemical investigations related to his concerns about gene-enzyme relationships. None of this research was related to radiation-induced risks.

3. Eugenics and the NAS BEAR I Genetics Panel

Given that this Panel was not well-equipped to address the task for which it was intended, the question needs to be asked as to how members were selected. There was a very strong eugenics thread that permeated the membership, with differential levels of involvement being manifested. Indeed, seven of the invited geneticist panel members [i.e., Beadle [49], Crow [82,83], Dobzhansky [84–87], Glass [88], Muller [89], Neel [90], and Stern [42] would publish in the journal *Eugenics Quarterly* during the meeting years of the Panel. Little also published in *The Eugenics Review* [91], but prior to the activities of the Panel. Even the Chairman, Warren Weaver [92], published in the *Eugenics Quarterly*, showing a close connection between the RF and eugenics during the time of the Panel. Despite the considerable visibility of the actions of the BEAR I Genetics Panel, there has surprisingly been little notice taken that the majority of panelists contributed articles to the *Eugenics Quarterly* journal during the time of Panel activities [16]. As shown later in this article, there was a relationship between the LNT concept and the Eugenics concept that was mutually supportive and that provided guidance for Muller, especially in his disputes with population geneticists. From the perspective of Muller, the major issue was understanding and directing evolution via eugenics principles and actions. The implementation of LNT into governmental regulatory agency policies and practices was simply a component of the strategies and tactics for societal implementation of Muller's Eugenics Manifesto.

3.1. The RF and eugenics: a guiding hand

Paul [93–98] has documented that by 1933 the RF established an

integrated plan to understand, control and direct human behavior, with the key eugenic targets of "mentality and temperament", which would morph into the academic discipline of behavioral genetics. Within this context the RF awarded substantial grants to Oskar and Cecile Vogt's Institute for Brain Research in Berlin [96]. The Vogts directed and integrated their research within an ostensible eugenics framework[4] [17]. Similarly, in the early 1930s, the RF funded eugenics research activities at the Pathological Institute of Copenhagen which was directed by Oluf Thomsen [99], who had a strong eugenics orientation. Thomsen's student, Tage Kemp, was the recipient of RF fellowships to study with the US leading eugenicist, Charles Davenport, at Cold Spring Harbor. Kemp was to research the genetics of psychopathology. In 1936, the RF provided 90,000 dollars for the formation of an Institute of Human Genetics which Kemp directed at the University of Copenhagen. The Institute was designed to study the heritability of mental traits and to offer genetic counseling within a eugenics framework. Beginning in 1933, Kemp established an impressive publication record on a range of eugenics topics. He likewise defended the Danish Sterilization Act of 1929. With funding from the RF, Kemp chaired the first International Congress on human genetics in Copenhagen. In 1957, he developed strategies and procedures to prevent hereditary diseases such as mental disabilities and "feeble mindedness" [100]. It is also important to note that Kemp was designated to be the chair of the session of the Atoms for Peace Conference in which Muller was scheduled to speak. While the AEC withdrew the invitation to Muller a month prior to the conference, Kemp arranged for Muller to give a 5-min summary of his non-presented paper. The invitation was ultimately denied by the conference organizers [101].

The RF also developed a strong interest in non-human oriented eugenics which was led by academics conducting experimental research. For example, the BEAR I panelist Alfred H. Sturtevant, in his 1954 presidential address to the Pacific division of the AAAS [102], emphasized that "man is one of the most unsatisfactory of all organisms for genetic study". He stated that there was overwhelming evidence indicating that the same biological principles operate in humans and higher animals. In fact, most of the evidence concerning practical features of human genetics is best based on experimental research via organisms rather than on human studies. Human research, according to Sturtevant, is especially unsatisfactory for the most important of all human differences, namely mental capabilities. This perspective inspired the RF to fund research with Clarence C. Little, another BEAR panelist, at the Jackson Laboratory to put a eugenics framework into experimental practice. This led to a 13-year study starting in the mid-1940s using five dog breeds to assess how genetics affected the development of intelligence and temperament. This funded project ended in 1956 [96], just when the National Academy of Sciences (NAS) Biological Effects of Atomic Radiation (BEAR) I Genetics Panel began its activities.

The RF was not alone in their strategies and programs to enhance eugenics. The Carnegie Foundation created a eugenics research center at Cold Spring Harbor, beginning in 1910, an effort directed by Charles B. Davenport, the most prominent eugenicist in the US [103]. The RF strategy was to internationalize their eugenic efforts, targeting multiple leading countries, and various key scientific leaders, with Muller being far from unique. Muller was seen as one of a range of long-term "investments" in the RF vision to influence the direction of human evolution. Thus, John D. Rockefeller Jr., who directed RF beginning in 1897, had a major interest in eugenics [20], with a special interest regarding population control. Rockefeller would become a member of the American Eugenics Society and a trustee of the Bureau of Social Hygiene [104]. He would write to Charles Davenport, the director of the Carnegie funded Eugenics Records Office, stating that prison sentences for weak-minded women would have eugenic value should these women "be kept from perpetuating their kind … …until after the period of childbearing had passed" [104].[5] Since eugenics was viewed as a viable science during that time, it was easy for the RF to support organizations and individuals that fit into this framework, such as Muller. Finally, it should be noted that the RF President Raymond Fosdick in 1952 [106], just three years before the creation of the BEAR I Genetics Panel, emphasized that the RF's investments in the natural sciences were being directed/guided by some of the very questions that it had been addressing twenty years earlier, such as whether it was possible for scientific leaders to "develop so sound and extensive a genetics that we can hope to breed in the future superior men." Thus, the guiding elements of eugenics were still affecting major directions of the RF at the time of the BEAR I Genetics Panel creation.

4. Rockefeller Foundation and Muller

Muller realized his dependence on the RF. We previously reported that Muller had discovered serious scientific misconduct in a series of critically important papers that were used by Muller and others to support the LNT co-authored by Frederick Hanson, who had been serving as his RF grants manager from about 1935 onward until his death a decade later, and who intervened at the University of Indiana for his professorial position [107]. Despite the seriousness of the possible charges involving Hanson, Muller would not proceed to publicly challenge/correct the scientific record and allege the misconduct, possibly to ensure that his funding would not be affected. Muller kept this quiet until after Hanson died. Even then, Muller was extremely cautious in trying to avoid offending his RF sponsor, sharing this secretive story with a close academic colleague, Everett Dempster. It is likely that this story would have never been revealed had Muller's letter to Dempster not been preserved. Muller had two self-serving reasons for remaining quiet, protecting his RF funding and not exposing important limitations in his LNT argument [107]. It is also important to note that the challenged papers of Hanson and Heys were subsequently cited by multiple BEAR I Genetics Panel members, including Glass, Demerec, and Kaufmann and even by Muller, who had discredited the research. One of those papers [108] was also cited in the significant Manhattan Project paper of Caspari and Stern [61] that Muller had carefully reviewed prior to submission. This review occurred within months of his critical letter about the Hanson and Heys research. Yet Muller had retained his criticism of the Hanson research since 1933. Why Muller didn't suggest to Stern that this "fraudulent" citation of [108] should not be cited is unknown but raises a new ethical issue.

In retrospect the RF sought to use Muller to promote its eugenic goals within a major geopolitical scheme to direct evolution. Muller appears to have understood the big picture within which he was being

[4] In 1932 Muller spent a sabbatical year at the Vogt's Institute in Germany. This visit would prove to be pivotal for Muller as Folke Henschen, a member of the Nobel Prize committee and a profoundly engaged eugenicist, would also be similarly engaged in research at the Vogt Institute. Muller and Henschen became quite friendly with Muller greatly impressing Henschen. Henschen targeted Muller to receive the Nobel Prize, with him becoming Muller's powerful advocate. In his final year (1946) as chair of Nobel Prize Committee, Henschen was able to achieve his goal for Muller [17].

[5] Rockefeller's scientific director, William Welch, a professor at Johns Hopkins University, was a member of the Board of Scientific Directors of the Eugenics Research Office, assisting in directing the funding of eugenics research. Welch would also become the AAAS President (1907) and the President of the US National Academy of Sciences (1913–1916) [105]. Thus, support for eugenics in the US reached strikingly high influential levels, creating cooperation between many philanthropic organizations, federal and state government entities, and high-level academic scientific leadership. This also occurred in many prominent US academic institutions (e.g., Harvard, Yale, U of Chicago, UCal/Berkeley) having formal courses on eugenics from about 1915 to 1940 and active research programs. Eugenics in the US during the early decades of the 20th century was therefore widespread and internationally influential [105].

manipulated, protected and promoted by the RF. These actions would both control and promote Muller and his ideas, including the cultural and scientific support for his Nobel Prize nomination and then to promote his ideas following receipt of the award. Muller's receiving the Nobel Prize was also a major achievement for the RF that supported him for two decades, often in challenging times and circumstances [17]. Not unlike the situation with the NAS BEAR I Genetics Panel, none of the individuals nominating Muller for the Nobel Prize had the scientific credentials to evaluate his research, yet nearly all were staunch eugenicists, including, Folke Henchien, chair the Nobel Prize Committee [17]. Hermann Muller had now achieved international status and was reasonably "manageable" if the RF provided him and his university with adequate funding. Of further importance is that the RF, via the leadership of Warren Weaver, was also funding the Karolinska Institute, including the research activities of Einar Hammarsten and Torbjorn Caspersson, members of the Nobel Prize Committee, who awarded Muller the prize [17]. Thus, Muller, Hammarsten and Caspersson had substantial research support from the RF with the same grant manager at the time he received the Nobel Prize [109].

5. The BEAR I Genetics Panel mission

In retrospect, the BEAR I Genetics Panel had critical challenges that highlighted and exposed their limitations. Of particular importance is that the Panel was challenged to do the impossible. The Panel was asked to estimate the effects of ionizing radiation on the US population for hereditary and other effects that could be mediated by genetic impacts, including somatic effects, not only for the present generation but for many generations to come. To make this task even more challenging, they were expected to deliver their report to the government, country and world communities in about six months. Even though the Panel members knew each other reasonably well, had often worked together in many different academic and advisory capacities, and had some deeply held common beliefs about radiation, genetic processes/functions/mechanisms, mutations, genetic diseases, and genetic research methods, all were treading on new territory when it came to applying what they knew to a spectrum of societal concerns that they were being forced to confront in the nuclear era.

This NAS Panel activity was led by Warren Weaver of the RF, who was an ideal chair from the perspective of the RF in some respects, as he appreciated the magnitude of the challenge, the impossible time constraints, and that he was pushing all panelists beyond their comfort zones of knowledge and experience. Weaver's role in this activity was much less that of a typical administrative Panel chair but like a leader administering a series of scientific stress tests to the Panel and evaluating how the members responded. It would be from Weaver's version of these Panel-based scientific stress tests that US and global radiation risk assessment policies and practices would emerge. Weaver and the Panel knew that the world was waiting, and that it was their responsibility to provide the national and world communities their opinions and guidance—lest some less qualified group (in their opinion) take on the challenge. While the Panel picked some so-called low-hanging (i.e., easy) fruit right away (i.e., adopting LNT as their central belief without debate), Weaver was waiting for the Panel to take on the tougher challenges with his take-home scientific stress test, exploring the questions of greatest interest, the answers to which would likely guide the country for the foreseeable future.

The scientific challenges that Weaver posed to the Panel geneticists were formalized in a letter dated February 8, 1956 [110], two days after these issues were discussed in detail at a meeting of the BEAR I Genetics Panel on February 6, 1956, as revealed in the Panel transcripts [111]. Forcing all to assume a linear dose response relationship, a basic assumption that the Panel had agreed to on February 5th, Weaver requested that each geneticist provide an independent and documented technical report that provided quantitative estimates to the following three informational requests:

(1) the total genetic damage expected among all descendants of a single generation exposed to 10r,
(2) the cumulative damage through 10 generations from a single generation exposed to 10r, or the damage at the 10th generation from 10r per generation up to that time,
(3) the damage expected in the first offspring generation.

The assumed number of children born following the exposure of all parents in the United States in one generation (30 years) to 10 r (above background radiation) was estimated to be 160 million but was later normalized to a population size of 100 million. Weaver asked the panelists to provide their reports via the mail prior to their next meeting in less than 3 weeks, on March 1. A 10 r dose over 30 years would mean that their total exposure would be about three times higher than that of parents exposed only to background radiation.

Following the March 1, 1956, Panel meeting, Weaver assigned panelist James Crow, a close colleague of Hermann Muller, to organize the submitted technical reports, putting them into a framework that would facilitate the Panel's discussion and evaluation of the various reports. The Crow function was designed to be a type of helpful, quasi-administrative one. Crow was one of a few younger panelists, amongst an impressive grouping of senior geneticists, such as George Beadle, Milislav Demerec, Hermann Muller, Clarence Little, Alfred Sturtevant and Sewall Wright.

Given the unchartered scientific areas that the Weaver stress test was exploring with the panelists, it was believed that it was important for each of the geneticists to take the questions posed by Weaver and try to solve them using his own research and related experience to derive respective answers/estimates. This group of geneticists was broadly based, with expertise ranging from those studying bacteria, paramecia, various insect models, such as fruit flies, mammalian models such as mice and dogs, and human research, ranging from clinical evaluations to population-based research. This diversity was seen as a strength to the group, as it suggested that if these questions from Weaver could be approached in their own unique research domain–and if their quantitative risk estimate answers closely converged–it would result in considerable confidence in their estimates and ensuing policy recommendations. This was planned at the start, and it seemed like a reasonable foundation upon which to proceed. However, problems began to emerge even before the end of the Feb 5/6 meeting, with Tracy Sonneborn and C. C. Little showing great reticence, feeling ill-equipped for the task as shown in Panel transcripts. In fact, they would not take up the challenge of Weaver. In a more animated fashion, James Neel would repeatedly challenge Weaver on the very point of his imposed stress test. Neel believed that science had not yet matured enough to reliably take up the Weaver challenge. It was Neel's opinion that the uncertainties were so great that any estimates that would be provided would be unacceptably uncertain and that recommendations for the public and world communities would be, in fact, illusionary [5,8]. Neel felt that it was a waste of time and, even dangerous, should the Panel be so bold as to act upon their estimates. Despite these challenges, Weaver would make the decision to proceed, raising the stakes to a higher level, essentially ignoring the rather impassioned, yet rational, suggestions of Neel, who had been directing a massive study of the effects of radiation on the occurrence of genetic effects in the offspring of parents exposed to the effects of the atomic bomb blasts for the past decade.

5.1. The Muller-Crow connection

Of the 12 geneticists on the BEAR I Genetics Panel, nine accepted the challenge of Weaver to provide quantitative responses to his posed questions, along with an explanatory accompanying write up [5,8]. The technical reports of the nine geneticists were never shared with the scientific community or general public by the BEAR I Genetics Panel, despite requests to do so. Calabrese and Selby [15] have assessed each of the nine reports and compared their respective approaches. Providing

such an analysis and documentation facilitates the historical assessment of the origins and applications of the LNT model in radiation risk assessment. Of particular importance is that seven of the nine geneticists used the mutation estimates provided by Russell from his massive mouse study. The mutation rate provided by Russell was in serious error as his control group had a mutation rate that should have been at least 120 % (2.2-fold) higher [112]. This error was only discovered some forty years later, leading to major reevaluation by an expert international panel. Based on the expert panel's recommendation, the Department of Energy (DOE) compelled the Russells to correct their error in the scientific literature. Selby [113], who discovered the discrepancy and brought it to the attention of the DOE, is of the opinion that the hiding of the mutational data by the Russells was a deliberate decision. Nonetheless, what this means is that all the estimates made using Russell's data were contaminated–being considerably elevated, when there was a much smaller, and perhaps no, treatment effect with the same data if they had been properly corrected for the error detected many decades later. Making matters worse is that even though the DOE forced the Russells to correct the scientific record, they did not require the Russells to urge the correction of other major reports, such as the NAS 1956 paper, that depended on their mutation risk estimates for policy development. This situation amounted to having US national policy that was founded on the NAS 1956 analysis and report being based on incorrect and falsified data. Further, this situation got even worse, as will now be shown.

5.2. Crow takes control of the Panel

The process of assessing Panel activities has raised important questions concerning the unusually significant role of James Crow in Panel activities, his relationship with Hermann Muller and how their interactions may have affected the development of Panel recommendations concerning radiation risk assessment in a manner that reinforced and assured the adoption of Muller's stringent and ideologically oriented perspectives. Weaver assigned Crow the seemingly minor administrative role of organizing the submitted technical reports (i.e., Beadle, Crow, Demerec, Glass, Kaufmann, Muller, Russell, Sturtevant, Wright) in order to facilitate subsequent Panel discussions. However, Crow took this administrative assignment as a license to make decisions on what was "acceptable" and what should be excluded. This action of Crow would change the course of risk assessment history in the US and global community.

Following his review of the technical reports of the nine geneticists, James Crow removed the mutation risk estimates of Demerec and Wright [5,8]. Crow indicated that he removed these estimates because their values were far removed from other submitted values, being much lower. The fact that Crow removed these estimates was not related to any scientific criticism. It had to do with the fact that if the scientific community and general public were aware that there was much disagreement amongst the panel members, it was feared that it might affect the adoption of Panel recommendations. Crow wanted to hide the disagreements and preserve the illusion that this was an expert Panel that understood the expressed challenges and had valuable insights for addressing such issues—while the opposite was actually the case. The exclusion of the Demerec and Wright estimates was the only way to avoid disaster according to Crow, and he convinced Weaver that this was necessary. However, how could Weaver and/or Crow make the decision to remove the estimates of two experts without causing considerable conflict and internal dissension, ultimately affecting the viability of the entire activity? The actions of Crow were playing right into the hands of Neel who pleaded with Weaver and Crow via letters and memos [114–118] to not follow the mutation risk assessment path of Weaver, as there was too much uncertainty. Weaver and Crow would need strong support to back up their decision to drop the Demerec and Wright estimates even if they felt justified to make such a decision in the first place. Could Crow have really made this decision on his own? Certainly, Weaver was not in the position to provide sound guidance as he was not even a geneticist. He was installed by Detlev Bronk to manage the Panel toward the goal of making an LNT-based recommendation. In fact, Bronk had created a Genetics Panel, separate from the Medical Panel, for the first time in the history of hereditary risk evaluations because the Medical Panel was dominated by those who preferred the threshold over the LNT dose response model [3]. The only way for the LNT risk model to emerge was via the creation of a Genetics Panel and to stack that Panel with LNTers. That is exactly what Bronk and Weaver did [5,8]. However, they did not anticipate the controversy that emerged with the mutation risk estimates.

Perhaps Crow and Weaver derived confidence for taking such drastic action from a February 25, 1956, letter of Muller to the Panel [119]. In this letter Muller immediately showed their problem and challenge. The "experts" had enormous uncertainty. Muller writes "the table shows …. …. ….the enormous latitude possible in our estimates of the more immediate damage and their consequent unsuitability of presentation to the public or to non-geneticists as representing the major basis for our conclusions regarding the need for rigorous protection from radiation." It is clear that Crow got the message and repeatedly brought this to the attention of Weaver [5,8]. It was quite clear at this point, that following Muller's clear articulation of the problem, the Panel knew that this was a serious issue and it had to do with the fact that there was still too much uncertainty and lack of confidence in their predictions and that to share these with the public and politicians who mostly needed calmed reassurance would never work. We know what happened next as described above. Given the seriousness of the situation, its looks as if Crow was given the "green light" to alter the research record and that all the panelists understood why it was necessary. It is thus perhaps not surprising that none raised their voice in protest when they saw in the final report the statement that only six (instead of the actual nine reports) were used as the basis of the conclusion that there was a rather good consensus among the experts. By drastically altering the research record without informing the journal/readership with a compelling explanation, the entire Panel committed scientific misconduct. The detailed analysis of the wide disparity of outcomes by the nine panelists who accepted Weaver's challenge shows how extraordinary this coverup was [15].

The actions of Russell to provide falsified data that massively overstated mutational risks and of Crow to remove the estimates of Demerec and Wright, in many ways, determined the fate of the BEAR I Genetics Panel report, supporting its recommendation to switch from a threshold to a linear dose response model for radiation-induced genetic damage. That was an essential decision to provide the means to estimate the number of population-based "genetic deaths" within the first (F_1) and subsequent generations in the BEAR I Report. The LNT approach of the panelists, which was adopted by regulatory agencies (e.g., EPA) in the late 1970s, was to estimate risks in a linear fashion to extremely low doses spread over a generation of 30 years with little, if any, consideration of background disease incidence, variation and detection capacity. The LNT dose-response recommendation and its risk assessment applications would be widely accepted and applied to hereditary and cancer risk assessment for radiation and chemicals [8,9]. These decisions have affected regulatory policies and practices worldwide now for more than six decades, greatly impacting numerous scientific, technical and public health developments, providing the foundation for nearly stopping the expansion of the nuclear power industry.

5.3. A long disguised (perhaps unrecognized) issue within the BEAR I genetics panel: The Dobzhansky connection

The removal of the Demerec and Wright mutation estimates by Crow was problematic because they were derived from highly experienced senior geneticists, with Demerec being the most experienced genotoxicity researcher on the Panel, while Wright was recognized as a genuine international leader in population genetics. While both were educated at different institutions (Harvard-Wright; Cornell-Demerec) and worked in

their long professional careers at different institutions (University of Chicago and the Carnegie Institute, Cold Spring Harbor), they did have a significant common theme that may have affected the actions of Crow with respect to the removal of their independent mutation estimates. This commonality long preceded the creation of the BEAR I Genetics Panel.

The commonality was founded on a dispute between Muller and Dobzhansky over a fundamental issue within evolutionary theory, that would involve Wright and Demerec taking the side of Dobzhansky. The Dobzhansky perspective was developed in collaboration with Wright during the 1940s [120] in their research on population genetics and later markedly enhanced by Bruce Wallace, who obtained his Ph. D. under the direction of Dobzhansky at Columbia University soon after the end of World War II. Wallace then became a researcher under the direction of Demerec at the Carnegie Institute from 1951 to 1958, then moving to Cornell University. The conflict between Muller and Dobzhansky centered on the role of natural selection in evolution, the outcome of which both Muller and Dobzhansky believed would greatly affect a broad spectrum of public policy initiatives [16,64].

From Muller's perspective, natural selection acts principally to reduce genetic variation. More specifically, Muller believed that optimal genes for most traits had already been selected for, typically being found in a homozygous state [16]. For a given species, Muller believed therefore that there was one, and only one, normal allele for each gene locus. He believed that alleles that differ from the normal one must be harmful to some extent to the carrier. As a result, new mutations would be typically selected out. In contrast to Muller, Dobzhansky and his longtime collaborator Sewall Wright insisted that genetic variation within species is the rule, not the exception, since selection for variation is essential to ensure the evolutionary plasticity of the species. According to the Dobzhansky and Wright perspective, natural selection would strongly favor a different genotype portfolio that would enhance survival chances in an ever-changing environment [16]. This would become the classical (Muller)-balance (Dobzhansky) controversy in evolutionary biology, a conflict that would become central to the professional activities of Dobzhansky and Muller by the early to mid-1950s and end with the death of Muller in 1967. A letter from Muller to Raphael Falk on November 16, 1963 [121], after nearly a decade of intellectual conflict with Dobzhansky, summarized the state of their dispute:

> **"It was too bad that you couldn't be at Princeton, where we had a kind of gladiatorial combat from which both sides finally emerged apparently uninjured, so far as each side thought of itself, but demolished, so far as each side thought of the other. At the end, Dobzhansky held out his hand for me to shake and I grasped it firmly, saying "I think you may in time come around after all", at which everybody laughed, and the meeting broke up."**

That the Dobzhansky-Wright versus Muller debate would spill over into the question of the genetic effects of radiation was not unexpected since it would involve the critical issue of whether mutation-induced variation might sometimes be advantageous and selected for. If that were the case, then the effects of radiation on populations would be considerably less harmful than Muller predicted with his focus on the inevitability of genetic deaths for organisms that experienced germ cell gene mutations.

It was within this framework of the Muller-Dobzhansky debate that the research of Wallace emerged and became significant [15,122–128]. Furthermore, the Muller-Dobzhansky controversy also became central to the ongoing eugenics debate. For Muller, eugenic practices were desirable since they are variation-reducing practices [16]. Thus, key issues with which Muller had been associated could typically be framed within an evolutionary and eugenics context. Muller's concern with radiation and advocacy of eugenics was consistent with the classical natural selection hypothesis which he advocated.

The basis of Dobzhansky's challenge to Muller's classical selection perspective was born in the development of population genetics by the renowned biostatistician, Ronald Fisher, in 1930 [129]. Based on research on selection pressure within a statistical framework, it soon became clear to Fisher that both alleles (A/a) would be retained at stable equilibrium frequencies in a population if heterozygous subjects displayed the largest average fitness. These findings were supported by Nabours and Kingsley [130], who used Mendelian crosses to induce heterosis (i.e., enhanced fitness of the heterozygote) for lethal factors in the locust. The proportion of lethal gene heterozygotes attained in the F_2 generation, as well as in backcrosses, consistently exceeded the predicted values. Similar findings would be reported by Dobzhansky [131–133] and in a series of papers in the 1940s by himself and Wright [120,134]. They showed that inversion heterozygotes of *Drosophila paraobscura* typically exceed the homozygotes in fitness [64, page 15]. The heterosis concept was seen within an evolutionary biology context and was applied to the question of the effects of radiation on the population. The Dobzhansky perspective argued that radiation-induced mutations could be advantageous in the heterozygous condition depending on the environmental context. This was the question that Dobzhansky posed, and it was tested by the newly minted Ph. D. student, Bruce Wallace, under the direction of Demerec at Cold Springs Harbor with Atomic Energy Commission funding.

There are other relevant insights into the professional relationship of Muller and Crow that are germane to the issue of whether and how Muller may have influenced Crow to remove the estimates of Demerec and Wright. In 1954, the AEC held a genetics conference at Argonne National Lab for the future of genetics research funding, giving the young Wallace a high-profile presentation role [64]. The next step would be more provocative as the AEC fed a very supportive view of the Wallace findings into a major US News and World Report [135] article in March 1955. This story downplayed Muller's Genetic Load hypothesis while promoting the new and striking Wallace findings of population-based radiation-induced benefits. Making things even more challenging for Muller was the fact that Dobzhansky [30] gave a major address at the June 1955 International Genetics Symposium at Cold Spring Harbor, making the case for his balance view at the expense of the classical hypothesis of Muller [31].

The Dobzhansky presentation established, in effect, a line drawn in the sand between Muller and himself on the issue of population genetics, hybrid vigor, radiation-induced mutations and the limitations of Muller's genetic death and load concepts. Dobzhansky [30] indicated that one could support the concept of linearity at low dose for radiation-induced mutations while not accepting the Mullerian implications that all the induced mutations would translate eventually into genetic deaths. He argued that induced mutations could often result in enhanced evolutionary-based fitness.

At the center of this confrontation with Muller by Dobzhansky was the research of Wallace, which the AEC had decided to promote. While the questions and challenges raised by Dobzhansky were striking and gathered much attention, the scientific foundations of the Muller-Dobzhansky dispute are complex, requiring a sustained research agenda. Even some seven decades later the questions and challenges posed by these two genetics leaders of the 1950s have taken on a scientific life of their own, reflecting new understandings and limitations in the perspectives of both Muller and Dobzhansky. Thus, the goal of the present paper is not to provide an in-depth evaluation of the arguments regarding the classical and balance hypotheses in the evaluation of radiation-induced mutations at the levels of the individual and population but to evaluate this controversy within the time frame of the BEAR I Genetics Panel activities and whether and to what extent it affected the Panel deliberations and why.

Soon after the CSH Symposium in June 1955, at which Dobzhansky [30] made his major presentation on the classical-balance controversy, Muller took a family trip to Europe, where he would give a major presentation in Lindau, Germany, on his radiation-induced mutation

research to a large grouping of Nobel Laureates that would occur on July 15th. A month later Muller was scheduled to speak at the first Atoms for Peace Conference in Geneva. However, a few days after the Lindau presentation Muller was to learn that his presentation was dropped by the UN following the decision of the AEC. This created a major storm in the genetics community; the bottom line was that Muller was out and that Wallace, who was also invited, was in [101]. It was clear that the intellectual battleground had crystallized with a focus on the emerging topic of radiation-induced mutation within a population genetics framework, all within about a year's time.

The Muller episode at Geneva became an international affair, with major stories first by the *Washington Post* [136,137], then followed by the *New York Times* (September 18, 1955, page 54, no author given) [138] as well as with a prominent editorial in the journal *Science* by George Beadle [139], amongst many other venues, all of which excoriated the AEC, embarrassing the Eisenhower administration [16]. Despite these highly visible criticisms of the AEC, little did the public know that the AEC worked to ensure that their views on radiation, mutagenicity and health would be well represented. In this case, the AEC ensured that Bruce Wallace, a leading *Drosophila* researcher, with long-term AEC funding, would share the results of his research that challenged Muller's views on radiation genetics including his genetic death concept in the battle over what governs evolutionary change.

5.4. Did the "classical" view of Muller or the "balance" view of Dobzhansky win the day?

At the Geneva meeting, Wallace's presence made Muller's exclusion even more painful for Muller and his followers. Making it worse for Muller was the publicity that Wallace received leading up to and during this prominent conference. A striking example of such publicity was seen in an article in *US News and World Report* entitled: The Facts about "A-Bomb" Fall Out, with the subtitle: "Not a Word of Truth in the Scare Stories" [135]. One of the striking statements in the article was:

> **AEC Tests show: Fruit flies, raised for 128 generations in highly radioactive surroundings, did not degenerate, as expected. Instead, they ended up a better race of fruit flies–hardier, more vigorous, more reproductive, with better resistance to disease (March 25, 1955, p. 25).**

Particularly interesting is that Muller obtained an entrance pass in order to attend the session that he was supposed to speak in. He got to hear the genetics presentations of Toby Carter from the UK, and William Russell [140] and Bruce Wallace [126] from the USA. Muller viewed Wallace's participation in the Atoms for Peace Conference as disturbing. What disturbed him most was that Wallace was going to dispute his genetic death/genetic load hypothesis with a more optimistic, but in Muller's opinion, strikingly wrong view of the future. The experiments of Wallace [141], which were first reported in 1951, had exposed laboratory populations of fruit flies to varying doses of radiation, to see what types of differences in average fitness might occur. Most notable was the finding that a population receiving an acute level of radiation had a significant increase in relative fitness over the control group. The increase in fitness did not go away, generation after generation, even after far more than 100 generations [122]. Thus, the presentation of Wallace, in effect, added so-called "insult to injury,", that is, to make a bad situation even worse for Muller.

Muller followed up that meeting with a letter to Mogens Westergaard, a Danish mutational geneticist who did experiments on *Neurospora* as his model organism [142]. Muller stated in the letter: **"The other American speaker [i.e. Bruce Wallace], although paying lip service at the end to the radiation danger, had as the chief purport of his talk the relative evanescent character of the damage and of the long term benefits as seen in flies and as estimated, by analogy for man**". According to Beatty [16], it was quite clear that Muller had considerable difficulty in differentiating Wallace the person and scientist from the version that was given in the various newspaper/magazine articles. At the same time Muller was also angry with the AEC that had his invitation withdrawn while Wallace, a rather newcomer to the field, had replaced him, a Nobel Prize recipient. Well into the fall of 1955 Muller was still upset with the AEC about his removal as a speaker as seen in a quote of a Newsweek article [143] with Muller declaring "Sticking your head in the sand won't protect your genetic structure."

Muller saw what was emerging and so he attempted to take matters a bit more into his own control. During 1955 Muller went outside his laboratory domain and developed a research collaboration with James Crow, a population geneticist, who sided with his classical view. After the BEAR I Genetics Panel had been invited in the fall of 1955 [29,31], Muller decided that he needed a high-level professional population geneticist on the BEAR I Genetics Panel to blunt and challenge Dobzhansky should the need arise. Thus, in an after-the-fact move, Muller persuaded Warren Weaver to add James Crow to the already completed Panel, giving Muller a very compatible colleague, in effect a Muller team member, whom he thought could counter and neutralize Dobzhansky in Panel debates. Even though Weaver had enormously helped Muller with the addition of Crow, more good news was coming Muller's way. That is, the invited Dobzhansky was taking a sabbatical to do field research in Brazil for an extended number of months and was forced to decline participation on the Panel, after Crow was now an official member. Of further importance is that Dobzhansky did not have the political savvy of Muller in that he did not ask Weaver to add a substitute, like Wallace, who would be an excellent replacement to counter both Muller and Crow. Thus, even though 1955 had started rather negatively for Muller with respect to the political and scientific disputes over the classical and balance theories, the situation had taken several major turns to his advantage, with Weaver clearly siding with him, the addition of Crow and the absence of his nemesis, Dobzhansky [16]. The Weaver-Muller collaboration would prove to be a strategically powerful move that supported a Muller perspective as well as affecting the course of the BEAR I Panel activities that have impacted risk assessment and international regulations for the next 70 years.[6]

As the Muller dispute with the AEC spilled into the fall of 1955, the United Nations created the Scientific Committee on the Effects of Atomic Radiation (UNSCEAR). The US delegation included multiple geneticists, such as Dobzhansky and Crow, who was firmly in the Muller "Classical" camp. According to Beatty [16], once Muller learned that Crow would be attending the UN meetings [144], he wrote Crow, "expressing relief and some anxiousness." Muller wrote to Crow saying "I had not known that you were to be asked to be consultant at the meeting of the UN Committee on radiation, but I am, of course, delighted that this is the case … ….I hope that you will be there … for the critical genetics discussion. Your letter enclosing Kimura's article in opposition to the balance theory has just arrived adding more coals to the same fire. I am really delighted, and I hope that you will be able to make enough of it clear to the UN committee to moderate the doubts they may otherwise throw on the so-called "classical" hypothesis" [145]. In fact, Muller was served well by the Crow's presence on UNSCEAR. Its report [146] in paragraph 79 of Annex H stated that "the Committee is compelled to assume that the general genetic structure of human populations corresponds more closely to the classical model in so far as this relates to known genes having individually detectable effects … …. It must be

[6] After the present paper was submitted, a letter was obtained by EJC from the Dobzhansky preserved papers (October 2, 1957 Dobzhansky letter to Bentley Glass) indicating that when he joined the BEAR Genetics Panel after his sabbatical (after the publication of the major BEAR I report in June, 1956), he requested that Bruce Wallace be added to the Panel. The addition of Wallace would have countered/balanced the presence of Crow. However, this request was not approved by George Beadle, the Chair of the Panel, greatly disappointing Dobzhansky.

emphasized that upon all the hypotheses discussed here, the great majority of radiation-induced mutations will be to alleles which are in the first instance harmful and unlikely to be retained in the population."

This was exactly what Muller was hoping for. Dobzhansky made his case but lost the vote within the Committee. Nonetheless, the fight would go on as he forced the Committee to note [146] in paragraph 77 of Annex H that "In recent years this [Classical] view has been increasingly strongly challenged by some, especially in connexion with the accumulation of extensive evidence concerning the prevalence and the superiority in many respects of structural heterozygotes in natural populations of *Drosophila*. … . It has also been argued on more general grounds that heterozygosity is the adaptive norm in most loci and that heterozygotes are in fact intrinsically better able to adapt themselves and maintain their own stability in the face of challenging environmental conditions. A recent experiment by Wallace seems to indicate that even random unselected radiation-induced heterozygosity in general confers an advantage, at least upon individuals otherwise homozygous for certain pairs of arbitrarily chosen chromosomes in laboratory populations of *Drosophila*". However, the absence of a Dobzhansky or Wallace on the BEAR I Panel prevented the importance of Wallace's results from appearing in the various reports of the Panel even though, as noted by Calabrese and Selby [15], a few Panel members stressed the potentially great importance of Wallace's results when responding to Weaver's challenge to estimate hereditary risks.

It is not certain how this debate would have affected the application of LNT for hereditary and cancer risk assessment. In contrast, the population genetics research of Dobzhansky and Wallace was showing that "populations" were more fit, in general, living longer with the presence of more radiation-induced mutations. Epidemiology has mostly targeted specific diseases, such as cancer and heart disease, instead of the concept of population "fitness". It is easily seen that the simpler approach of Muller would be more readily understood and accepted by the general public. In their 1963 book *Radiation, Genes and Man*, Dobzhansky and Wallace failed to address the issue of how their radiation-induced enhanced population fitness model would affect hereditary and cancer risk assessment [147]. While this was their opportunity, it was likely that this issue was simply too complex and scientifically premature, while the reductionist model of Muller was more easily explained (not that it makes it correct). Thus, in most respects, Muller had much less to fear from the Dobzhansky and Wallace challenges than he expected. Dobzhansky and Wallace simply were at the very early stages of a scientific learning curve, where their results were interesting and provocative but far from having any practical implementation within the regulatory community or society in general. However, a December 23, 1970, letter of Wright to Russell [148] seemed to capture "risk assessment" implications of the Wallace findings. He noted that "I find it difficult to reconcile their [the Gofman and Tamplin risk estimates (see Ref. [2])] estimates of the dangers of 5 r per generation to the persistence of the human population with the way in which *Drosophila* populations flourish under 1000r or more per generation [147] or the relatively slight effects of 100's of r's which you find as I understand it, in your mouse colony." It is clear from the comments of Wright that the Wallace findings were significant when placed in a broader biological context for the evaluation of the effects of radiation.

The Wallace findings indicated that the Muller view had serious limitations, and they reinforced other challenging data such as from the massive studies of Russell. In fact, the situation was even more damaging for the Muller perspective because the Russell data were falsely elevated by at least a massive 2.2-fold, which enhanced the shift in attention away from the fruit fly model system to the mouse system. It is important to note that Wallace was not a member of the NAS BEIR I Genetics Panel, which Crow chaired, a factor that ensured that the Dobzhansky-Wallace perspective would not have representation. The shortcomings of Muller's genetic load hypothesis would become apparent by the mid-1960s (see [149,150] for a review), with even a subsequent large-scale multigenerational (up to 82 successive generations exposure to 200 rad) radiation mutagenesis experiment in mice by Spalding providing results that were strikingly inconsistent with Muller's genetic load hypothesis [151].

Many details described above show that Muller and Crow had a close working relationship that was quite active at the very start of the BEAR I Genetics Panel and that Muller would try to motivate Crow to affect committee meeting outcomes. In fact, Muller did not stop with Crow for the UNSCEAR meeting but also wrote to Shields Warren [152], Harvard University, chair of the committee to which Crow had been added, to put additional pressure on that committee. Muller used this letter to Warren to criticize the views on radiation-induced mutations by Dobzhansky. He was especially adamant since Dobzhansky's views indicated that Muller's estimates of risk were an order of magnitude too high. Warren shared the Muller letter with Dobzhansky, who later wrote to the prominent Harvard evolutionary biology professor, Ernst Mayr, indicating that: "As you doubtless know, Muller is violently opposed to the ideas on human evolution which I like to express. As is his habit, he translates scientific opposition into personal enmity. In fact, I am told that what really maddens him is my support of Wallace's work, which he thinks is not merely wrong but wicked" [85]. This brief glimpse into the modus operandi of Muller adds further to the speculation that he may well have played a role in affecting Crow's decision on the Demerec and Wright estimates.

With respect to the BEAR I Panel, Demerec, Kauffman, and Wright included strong reference to the research of Wallace in their individual technical reports and related letters that Weaver/Crow would evaluate and exclude from Panel consideration. It is of value within the present context to consider what these three panelists wrote concerning the research of Wallace.

5.4.1. Demerec [153]:

"I believe that we should not disregard the results of Wallace's experiments, in which Drosophila populations have been exposed to continuous irradiation for more than a hundred generations. These results are different from what a geneticist would expect them to be on the basis of the information now available from studies of genetic effects induced in irradiated individuals. They show definitely that the irradiation has had a considerably smaller effect on the populations than was expected. It seems fairly incautious to dismiss this evidence from our considerations on the assumption that these results were due entirely to the high incidence of embryonic death brought about in Wallace's experiments by crowding, and on the further assumption that there is not a high incidence of embryonic death in human populations. The effectiveness of this objection would be considerably reduced if an estimate which places the average pregnancy wastage in man as high as 70 %–80 % should prove to be correct. The effective crowding, however, can be tested experimentally, and such experiments are planned for the near future, provided the level (in terms of number of workers) of the AEC grant supporting this research is not reduced."

5.4.2. Wright [154]:

"An increase in mutation rate would tend to have a slight over-all detrimental effect on shifting gene frequencies slightly from the equilibrium points under selection alone, but the capacity for adaptive adjustments in such a population are so great that this effect would never amount to much. Bruce Wallace's experiment with Drosophila populations illustrate this capacity under drastic radiation for 130 generations."

5.4.3. Kaufmann [155]:

"Bruce Wallace has discussed with me at some length problems of the genetic structure of populations to which he has directed his thoughts during recent years. It seems to me that his deliberations should be entered in the record for the consideration of the genetics panel, but unfortunately Wallace is away today and I cannot clear with him about his wishes for distribution of the remarks listed below. Nonetheless I am

presenting them for your information and will consult with Wallace when he returns to Cold Spring Harbor if you think his comments should be passed on to members of the Genetics Panel."

"Wallace states that we know very little about the genetic structure of human populations–the role of heterosis in determining man's "fitness." He notes that a population utilizing a balanced polymorphic system loses a certain proportion of its members but the existing population gains through the superior fitness of the surviving individuals. Such a system is known to exist in many species and its existence in man has not been disproved. To evaluate radiation damage to a population built on a system of this sort requires (1) evidence for the existence of the system, (2) information regarding total prenatal mortality, and (3) an estimation of the fraction of this prenatal mortality that is the result of the polymorphic system. If it develops that man does use a system of this sort, then the importance to the population of a newly induced mutation must be related to the present frequency of "deleterious" mutations and this must be related once more to the present prenatal mortality which presumably accounts for the sacrificed homozygotes."

"Rough calculations have been made on the relative importance of newly induced mutations under various assumptions regarding prenatal mortality. The following extreme assumptions have been made: Homozygosity for a substantial block of genes is lethal; (2) "A substantial block" is taken as the polygeneticist "super-gene", the number of which equals (1 plus Chiasma frequency) times (haploid chromosome number). In man it is assumed that 50 is a reasonable estimate; (3) An effective number of "alleles" for such "statistical" genes related to genetically caused prenatal mortality. If m equals the number of alleles and S the proportion of original zygotes surviving, $(m - 1/m)\ 2 = S$. If it is assumed for purposes of discussion that the importance of the new mutants is 1 when prenatal mortality equals 10 %, the relative importance for our higher mortality are as follows: S, 20 per cent, relative importance 0.2; 30 per cent, 0.09; 40 per cent, 0.06; 50 per cent, 0.03; 60 per cent, 0.02; 70 per cent, 0.01; 80 per cent, 0.008, 90 per cent, 0.005. This is why Wallace believes that an investigation of prenatal mortality (or more importantly the development of experimental approaches to replace the pregnancy wastage techniques) is essential for an understanding of radiation damage."

"Admittedly such discussion is directed toward a consideration of populations rather than individuals, and one must think of individuals in considering radiation damage to human populations. Thus, there seems little alternative at the moment but to approach the problems before us in terms of the models we have chosen and the calculations we have made. But Wallace has also noted that it would be disastrous for geneticists to accept these models as final and to direct all future experimental evidence toward quantifying the variables demanded by the model for a selection under its conditions."

It is thus apparent that at least three members of the panel—all three of whom were among the nine who accepted Weaver's challenge to estimate hereditary risks—considered Wallace's findings to be noteworthy. However, their views about Wallace were rebuffed, the fact that they had contributed estimates was ignored, and the views of Wallace had no discernible impact on the final reports.

5.5. *Muller reveals himself*

In an August 27, 1956, letter to George Beadle [156], the newly appointed chair of the BEAR I Genetics Panel, Muller addressed mounting concerns that the Panel needed to provide the scientific community with a document that contained the scientific foundations upon which the Panel's recommendations were based. The two official reports of the Panel (i.e., NAS, 1956-paper in *Science* [1] and the Report to the Public [157] lacked the needed rigor and normal detailed and supporting documentation. Even though the Panel had considerable prestige, they needed to do far more than expect the world community to accept what they had to say without the needed backup documentation. Muller wrote: "As for the preparation of a technical report to back up our more popular one, it seems to me that it would involve us in a lot of thankless work and disputatious rehash of points we have already considered, as well as in airing our dirty linen before the public unnecessarily. After all, only geneticists would be competent to judge the validity of our technical report and geneticists do not need it because they have access to the technical reports already in the literature, some of which are in summary form. So far as I can see, it would be a matter of quarrelling over what would be the most important points to put in and to what extent they were valid, things on which I thought we had agreed to disagree. **Why, for instance, should I enter into a public dispute with Demerec on whether a bacterial generation should be taken as corresponding just as closely to a human generation as a drosophila generation does. This is only one little example out of many. If you had heard Jim Neel's paper before the WHO meeting (perhaps he will send you a copy of it if you ask him) you will see how radically he and I differ regarding not merely the significance of the drosophila data for man but even regarding what the drosophila data show with respect to Drosophila itself. Similarly, I think I would have to disagree with Wright concerning frequency and importance of small detrimental mutations as contrasted with the conspicuous ones known as lethals and visibles".**

It thus becomes quite clear that the two targeted estimates for removal from the Genetics Panel estimates by Crow were the same two that Muller had serious concerns with. While this may be a coincidence, Crow [29] noted in a personal reminiscence article that after the entire BEAR 1 Genetics Panel had been invited, Muller made a personal request to Weaver that he be added. Thus, Crow was given a major career boost by a Nobel Prize winner, no less. Crow indicated that Muller wanted the presence of an expert in population genetics on the Panel. This was the very area in which Dobzhansky was challenging Muller, along with his former Ph.D. student, Bruce Wallace. Crow stated that he had begun to have a professional collaboration with Muller. That occurred because Muller wanted Crow to help him find a way to estimate the human spontaneous mutation rate from consanguinity data. Their eventual conclusion was that the majority of the inbreeding effect was the result of incompletely recessive loci [34]. As noted above, Muller made a formal written request to Crow to protect his interests with respect to the classical-balance natural selection debate on the UNSCEAR Panel. As noted earlier, the resulting final report of that committee likely pleased Muller.

In another remembrance type article, Crow [31] pointed out Muller's intense conflict with Dobzhansky on the classical-balance matter, pointing out (page 369) that "Muller was very much concerned with his crusade against radiation at this time and he did not want to make any concessions that would in any way weaken the genetic load argument. I once suggested putting into a committee report that there were many mutants whose advantage or disadvantages depend on the background genotype; for example, a mutant gene decreasing size could be advantageous in a very large person but disadvantageous in the small one. He didn't disagree with the statement, but he argued strongly against putting it in the report, fearing that it would dilute the impact."

"When he was trying to persuade, Muller addressed his remarks to the subject at hand and the intended audience. When he was writing about evolution and eugenics, he emphasized selectable variants; when he was writing about radiation risk, he emphasized the harmfulness of mutations. Muller inevitably overstated his arguments, whatever the subject". (pp. 369–370). Nine pages later (page 379), Crow noted that **"it is clear that Muller's alarmist views of the hazards of radiation have prevailed, rather than Dobzhansky's more moderate views. In my opinion, Muller was too effective in cautioning against radiation risks, with the result that the public now has an irrational fear of low dose radiation relative to other risks. The fear, I suppose, has resulted more from the assumption of no threshold for carcinogenic effects than from the dread of genetic effects. In any event, the battle that Muller waged was certainly won: the present standards for radiation safety are more stringent**

than even he dared advocate." (pg. 379).

Now that Muller had long since passed away, Crow was free to place Muller's efforts in perspective. In fact, some eight years later, Crow [32] echoed the same view, but this time, he gave himself equal blame to Muller for the success in working together to create the irrational fear of low dose radiation in the general public. Note that the irrational fear did not even take into account the massive exaggeration of mutation risks generated by the BEAR I Genetics Panel due to the Russell fraudulent data. Thus, the so-called irrational fears that he and Muller helped to generate were even far more extreme than he even realized [14]. The interesting thing is that even though Crow saw this as a real issue, at least by the mid-1980s, there is no evidence that he tried to correct what he believed to have been scientific and policy errors. His several public apologies are buried in the middle of very long articles, with no evidence that they had been noted except in several of my (EJC) publications, decades later.

Upon reflection, we find that long-term policy decisions are not necessarily right or wrong or based on the best possible information. In the case of the switch to an LNT model with the recommendation of the BEAR I Genetics Panel we find that:

The panel did not have extensive research experience with either radiation-induced mutation or extrapolation biology. They had an image created by the NAS and the media that was misleading and largely false. No one checked their scientific qualifications and objectively evaluated them. There is an overwhelming, but incorrect, general impression/belief that the BEAR I Genetics Panel members were top experts on radiation genetics, based on experience/publication record.

The Panel was stacked with those whose beliefs coincided with what the leadership wanted because every Panel member supported the LNT model, with extremely little debate and no serious challenging of the LNT model. It was a biased Panel. It would have been acceptable to have biased scientists on the Panel, but the biases should have been reasonably balanced. There was a colossal failure to provide such a balance on the BEAR I Panel.

The failure by the BEAR I Panel to have any discussion concerning the strengths and weaknesses of the threshold and LNT models represented a serious failure to serve the needs of the scientific community and the country. This can certainly be a criticism leveled at Weaver as chair.

The manipulation of the Panel membership by Muller to add an extra population geneticist to counter the assumed presence of Dobzhansky is also striking and his intentions were also hidden. Likewise, Muller was accorded a major strategic advantage when Dobzhansky was not replaced with someone who could have effectively presented his viewpoint. This paper reveals that Weaver knew that Crow and Muller had a special relationship that resulted in Crow being appointed to the Panel. Yet, Weaver assigned Crow the pivotal task of organizing the mutation estimates, which certainly seems to have exceeded his designated responsibilities. Crow ended up rejecting the estimates of three panelists who had shown interest in the research of Bruce Wallace and the balance perspective of Dobzhansky. Based on the February 25, 1956, letter of Muller to the Panel about extreme Panel [119] lack of agreement and profound uncertainties and the impossibility of sharing this with the public, there were various ways in which Crow could have addressed the thorny problem. For example, he quickly removed the estimates of Demerec and Wright, but he retained the estimates of Beadle and Glass, whose estimates revealed extreme uncertainty. The publication of the Panel in *Science* [157] drastically misrepresented the uncertainty that existed among Panel members even after Crow decided to delete three of the nine responses [15]. The more technical report of the BEAR I Panel [1], the one published in *Science*, based its few statements that suggested that there was reasonably good agreement among experts exclusively on estimates of the number of radiation-induced mutations expected in 100 million children under the assumption that in future generations (sometimes possibly hundreds or thousands of generations later) every induced mutation would be selected out of the population—that being a genetic death according to Muller's concept of genetic load. The statements supporting this claim were shown to exaggerate considerably the extent of agreement even after deleting the estimates of Demerec, Wright, and Kaufmann [15]. As we also documented, the differences between Panelists became much more extreme when they attempted to estimate actual health effects found in those 100 million children. Risk estimates based on Muller's genetic death concept should now be considered meaningless [151]. At least two members of the BEAR Panel (Wright and Russell) showed by their responses that they considered it essential to have estimates of actual health effects in first generation children to provide meaningful risk estimates. No such useful data were then available. The only data then available on actual health damage in the F_1 generation were death before weaning age in mice or death before emergence as adults in *Drosophila*. Those were the data that Panelists applied to reach their highly divergent estimates of damage in the F_1 generation, and no mention of their often extremely different estimates was made in their Reports. Those risks were applied to 100 million children without even acknowledging that many of those deaths would relate to deaths occurring in humans long before recognized pregnancy.

The goal of the Panel leadership should be to provide a fair process for evaluation, not to create a framework that tends to ensure a desired outcome. However, Weaver and Bronk wanted a desired outcome that also placated Muller.

Crow's several reminiscent articles, long after the deaths of Muller, Weaver and other key players, clearly revealed his special appointment and his capacity to be highly influenced by Muller on this and other similar committees. Muller used Crow as a type of internal mole to ensure that his perspective would prevail. Crow admitted to these actions late in life, finally coming to the understanding that society's best interests were not served by the extremism of Muller and his complicity with it.

6. Eugenics and LNT

Carlson [19] wrote that eugenics was the recurring theme of Muller's life, his so-called "leitmotif", beginning with the first paper written as an undergraduate in which he offered his perspective of a eugenics-based world. This would remain the central theme of Muller's life, with it becoming more obvious when he was in poor health near the end of his life some five decades later,[7] when he was extensively involved in commercial plans to make available to women the sperm of exceptional men. In a July 4, 1965 letter, Muller emphasized he was determined to work on the sperm preservation project, despite his major health issues "because I look on it as the most important work of my life, and certainly of the later part of my life." [158].[8]

The three major philosophical/ideological directions of Muller involved:

(1) his commitment to a socialist communist state,
(2) the linear non-threshold (LNT) concept for health protection and
(3) the eugenics concept.

Of these three powerful directions, the socialist communist state and

[7] As Muller was reaching the end of his active academic leadership, he became strikingly active in the area of eugenics leadership. In the late 1950s and early 1960s he made numerous presentations on eugenics at conferences including as a partial listing: Future of Man Symposium (September 1959, New York City), the Darwin Centennial Conference (November 1959, Chicago), the Minnesota Human Genetics League (September 1960, Minneapolis), the Academy of Art and Science conferences (September to December 1960, Boston) [159].

[8] The genius sperm bank dream of Muller became a reality in 1980, being initially named after Muller. (Hermann J. Muller Repository for Germinal Choice). The activity was shut down in 1999.

the hereditary and cancer risk assessment/LNT focus were subservient to and supportive of the eugenics cause. With respect to the social communist perspective, Muller was attracted to it because it had the goal of making the social and economic environments equalized across society. Once the so-called massive environmental variables had been, in effect, neutralized for all, the basis for differences amongst individuals would be seen as largely genetic and his eugenics framework could then become operational.

The LNT concept was important to Muller not only to protect the genome but also to create a technological means to improve it, again serving the broader goals of his eugenics perspective. Improving the genetic health of the population was the central focus of Muller, with all other factors being integrated to support it. Muller wanted intelligent and well-meaning leaders to direct the future of human evolution, finally putting aside the Darwinian "natural selection" principle for humans, strongly believing that human intelligence, even though often affected by politics, power-issues and biases, would be better than a ruthless and demanding evolution based on natural selection processes.

The regulatory-based LNT concept was at the center of reducing genomic variability, preventing genes that had already been selected for from becoming mutated. He thought that reaching the goal of switching from a threshold to an LNT model would result in far stricter exposure standards, thereby resulting in fewer gene mutations and the preservation of already optimally selected genes. This was central to Muller's population genetics model. It was also at the core of the profound debate between Muller and Dobzhansky, the latter who favored the balanced population genetics model. While Dobzhansky and his followers also supported the LNT model, they rejected the idea that all mutations would lead to harmful population-based effects. They argued that the real life of populations contradicted the reductionist-based laboratory research of Muller, which led to his LNT model. This was a crucial challenge to the Muller viewpoint, but it was far more difficult to study and required much longer time periods to complete studies. Nonetheless, the data supporting the balance hypothesis were credible and substantial. These findings suggested that there may be an optimal level of population-based exposure to radiation that would maximize fitness within the population. In limited discussions it was suggested that this optimum exposure level might be perhaps an order of magnitude higher than what Muller was suggesting.

Muller was successful in blunting the impact of Dobzhansky on the BEAR I Genetics Panel. As pointed out earlier, Muller arranged via Weaver to add Crow to the Panel to provide the necessary intellectual backup. Wright, Demerec and Kaufmann made strong pitches for the balance hypothesis to be discussed by the Panel. Demerec and Kaufmann even went so far as asking Weaver to invite Bruce Wallace to meet with the Panel. However, there is no record that Weaver attempted to do this. In addition, Muller was doing his best to counter Wallace and his striking findings. This put Wallace in a very difficult position, as he was a recent Ph.D., who was getting surprising but fascinating results. He was, after all, challenged by a very aggressive Nobel Prize winner who could make or break his young career.

The classical hypothesis of Muller prevailed, leading to the seemingly unambiguous and easier-to-describe LNT model, instead of a model that could have been encumbered with a spectrum of scientific nuances from the study of complex populations. Of further importance is that, after the BEAR I Genetics Panel report was released in 1956 [1,157], the pressure to challenge Muller's view on this matter calmed because Wallace left Carnegie Institute for an academic appointment at Cornell. Further, the next major distraction for Muller would be the December 1958 report in *Science* by Russell showing strong evidence of a dose-rate effect and the likelihood of DNA repair being very effective at lower dose rates and lower doses [160], which again turned Muller's scientific life upside-down [10].

The demise of the eugenics movement was one of the consequences of the defeat of Nazi Germany in World War II. Even though the eugenics concept was still alive, with leaders such as Hermann Muller strongly advocating for a more scientific and non-racist view, the concept had, nonetheless, become politically and socially irreversibly damaged, with the academic offspring of the "Mullerian" generation now turning a progressively deafer ear to the beliefs and passions of their highly accomplished professorial mentors, such as Muller and Glass. These next generation academic offspring were quickly, but quietly, attempting to get some needed distance from their esteemed mentors and their views. The next generation of geneticists did not want their careers to be damaged by this "racist' driven scientific dogma that had the distinct potential to be a career killer–even though it was a core belief of their academic mentors [161]. For example, even James Crow, University of Wisconsin, a true Mullerian, received notable criticism related to his publications in *Eugenics Quarterly* in the 1950s and 1960s when the University proposed an Institute on Evolution in his honor [162].

The fact that the visibility of the eugenics concept movement strikingly declined over the past ~80 years has led to the impression within the scientific community that the eugenics concept never achieved a high level of importance, being merely a slight aberration, for the geneticists of the Muller era, who provided society with its hereditary and cancer risk assessment framework. However, as this paper clearly demonstrates, eugenics had a tremendous influence on the BEAR I Genetics Panel. Eugenics was also a dominant perspective within the genetics community and many aspects of medicine (e.g., genetic psychiatry, dealing with a range of neurodegenerative diseases, such as schizophrenia) for the first half of the 20th century, shaping and directing scientific and social policies [26,103].

The central theme of the present paper is that the US BEAR I Genetics Panel was selected by leaders of the RF/NAS to advance their eugenics agenda via the recommendation for the linear non-threshold dose response model for hereditary risk assessment. The RF plan was an ingenious one because while the broader eugenics concept received the "death sentence" with the defeat of the Nazis, the LNT portion of the overall plan was retained by infiltrating the environmental movement, thereby permitting the LNT model in critical ways to continue to enhance the eugenics agenda without being seen or understood as doing so by the leaders of the environmental movement and their vast supporting cast of legislative, government, academic and major media groups.

While some aspects of the eugenics movement had made a deep dive into historical oblivion after World War II ended in 1945, environmental health, including hereditary and cancer risk assessment, was on the ascendancy. The remaining parts of the eugenics movement needed a clear, but scientifically viable, platform. A key strategy to enhance the remaining eugenics agenda emerged with the awarding of Muller with the Nobel Prize in 1946. It is ironic that just when this several decade RF-inspired effort to have Muller awarded the Nobel Prize was achieved as a geopolitical eugenics strategy, the main aspirations of the eugenics movement seemed to be quickly receding because the eugenics goal of a science-directed human condition was quickly and emphatically rejected. However, the brilliant and unrelenting Muller and the RF found a unique way to promote a central theme of the eugenics movement—that being the LNT model. This could be achieved via effectively positioning the LNT dose-response to become the central feature of the environmental/public health/regulatory science movement, thus reaching a much-desired goal with vast applications.

The long reach of Muller is shown herein to be considerably more complex than previously imagined. With respect to his principal goals and those of his Nobel Prize nominators, Muller's reach, as seen within the context of the rejection of eugenics, became rapidly diminished, becoming a colossal failure [95]. However, Muller's research rapidly became viewed as "visionary", and was integrated within the environmental movement, becoming a significant inspiration of the Rachel Carson book, *Silent Spring* [163], with Muller, and his gene mutation "claims" and his Proportionality Rule/LNT model providing a major part of the foundation for hereditary and cancer risk assessment, thereby providing a central and guiding framework for the US Environmental

Protection Agency (EPA). This message then, ironically, spread globally in a stunningly rapid manner [164]. Thus, the central theme of the US EPA led hereditary and cancer risk assessment is actually based on—and developed from—important goals of the eugenics movement. It seems likely that many who have felt justified in supporting the Precautionary Rule/LNT model—based on their assumption that both concepts were founded upon sound science—have never realized that their original acceptance was an outgrowth of–and profoundly influenced by–the eugenics movement. Curiously, a similar subtle eugenics strategy is seen with the implementation of elite co-educational colleges. While such colleges have been an idea that has resonated across the US, such colleges support the highly eugenic goal of bringing together highly gifted males and females that could lead to new families and offspring, much like Muller would have strongly supported, that is, a modern, non-coercive means to achieve some of his eugenics goals.

The failure to discern the historical foundations that led to awarding the Noble Prize to Muller prevented the scientific community and society from recognizing that this award to Muller was largely a tactic to manipulate the world community to adopt a transformative social engineering eugenics concept, by powerful governmental and wealthy grant funding social engineers at the RF, the Carnegie Foundation, and other similar groups. Further enhancing this ideological "take over" of the Nobel Prize to enhance a social engineering concept is that it also strongly encouraged its followers to overstate and even misrepresent the scientific quality of Muller's research, while, at the same time, failing to acknowledge its limitations that have now been documented. An illustration of this was the Nobel Prize ceremonial speech of Caspersson [165] for the Muller award [17].

This eugenics-based ideological takeover philosophy was quickly, yet quietly, adopted by the leadership of the modern environmental movement as was apparent from the described actions of the US NAS BEAR I Genetics Panel and their political and governmental enablers, and then later by entities such as the Federal Radiation Council and the US EPA, who continued to apply the same flawed science of Muller that was intended to bolster the eugenics movement to become the central principle of the environmental precautionary principle [9]. In an odd but strikingly parallel fashion, the eugenics movement, like the environmental movement, was based on a "precautionary principle". The eugenics movement had been mainly driven by a perceived need to quickly prevent targeted groups from reproduction even though it lacked a well-founded understanding of the genetic foundations of the diseases in question and their environmental influences. With a goal that also embraced rapid policy change, the environmental movement followed a similar ideological path and applied the fear-based precautionary principle, oftentimes leading to crippling and irrational societal decisions, with a history of often failing to protect while actually enhancing harm and being enormously costly [9]. In many respects, this eugenics activist philosophy has been successful since it seems likely that many leaders who support the use of the LNT dose response model have never recognized that they are really pawns in a much bigger historical and societal game. Their unawareness makes the capacity to manipulate them much easier and far more efficient.

7. Conclusion

This paper integrates multiple factors that led to the LNT recommendation of the BEAR I Genetics Panel. These involved data falsification by Russell, failure of the panelists to detect the Russell deception, the falsification of the research record by Crow to minimize estimates of Panel uncertainty and variation, the blunting of criticism of Muller's classical (genetic load) hypothesis by those who supported the balance hypothesis, the stacking of the Panel with RF-grant funded LNT advocates, who lacked appropriate expertise in the area of radiation-induced mutation while being guided by a RF-mediated eugenics philosophy that strongly favored adoption of LNT, as part of a larger RF plan to control the direction of human evolution via eugenics principles and practices. The big picture conclusion is that the major public health and regulatory agency adoption of the LNT for risk assessment has had a sordid history that has been hidden for nearly 80 years. Despite this extremely troubled and fraudulent past, the scientific community and regulatory agencies have shown themselves to be reluctant to address this issue, which has involved the complicity of multiple Nobel Prize winners and other major scientific elites and high-profile organizations like the US NAS. The history of LNT is a skeleton in the closet of the EPA, the NAS and other major public organizations that continue to embrace it as being central to their mission without addressing the extremely flawed historical and scientific foundations and dishonesties upon which it stands. It seems that the time for action is long overdue.

CRediT authorship contribution statement

Edward J. Calabrese: Writing – review & editing, Writing – original draft, Conceptualization. **Paul B. Selby:** Writing – review & editing, Writing – original draft, Conceptualization.

Use of generative AI and AI-assisted technologies

No AI software was used in the preparation of this manuscript.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: Edward Calabrese reports financial support was provided by US Air Force. Edward Calabrese reports financial support was provided by ExxonMobil Foundation. If there are other authors, they declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgments

EJC acknowledges longtime support from the US Air Force (AFOSR FA9550-19-1-0413) and ExxonMobil Foundation (S18200000000256). The U.S. Government is authorized to reproduce and distribute for governmental purposes notwithstanding any copyright notation thereon. The views and conclusions contained herein are those of the author and should not be interpreted as necessarily representing policies or endorsement, either expressed or implied. Sponsors had no involvement in study design, collection, analysis, interpretation, writing and decision to and where to submit for publication consideration.

APPENDIX

NAS BEAR I Genetics Panel

Beadle, George: He was a graduate student of Rollins A. Emerson at Cornell University. Emerson was a dominant figure in the area of genetics and was very supportive of societal applications of eugenics from agriculture to human populations. He was a signer of the Eugenics Manifesto [47]. Despite this association, there is little evidence that Beadle had an active/ideological role in the area of eugenics. The research of Beadle was basic, with a focus on molecular mechanisms. He was not involved with the study of human genetic diseases that influenced others (e.g., James V Neel) noted below that more directly confronted the issue of eugenics.

Cotterman, Charles C.: He received his Ph.D. at The Ohio State University under the direction of Laurence Snyder. Snyder was a leader in the area of medical genetics and eugenics. In 1947 Snyder, together with Hermann J. Muller, Herluf Strandskov and Cotterman, founded the American Society of Human Genetics. Muller was the first president, in

1949, with Snyder following a year later [166].

Crow, James F.: Director of various American Eugenics Society (AES) activities in the area of population genetics from 1971 to 1974; 1979–1981 [28]. Crow was a strong supporter of the classical view of natural selection, which drew him into collaborations with Muller and conflicts with Dobzhansky. Crow influenced his Ph.D. student Newton Morton, who became a well known population geneticist, regarding the classical perspective. Crow was quoted as follows: "The right to reproduce at will is regarded as a basic human right. I cannot see this remaining true much longer.", a comment similar to those offered by Muller and Garrett Hardin.

Demerec, Milisav: He started his professional life in the Genetics Department of the Carnegie Institute at Cold Spring Harbor (CSH), the center for leadership and research on eugenics in the US and the global community. The Genetics Department at CSH in 1921 was formed by the combination of the Station for Experimental Evolution and the Eugenics Records Office. Demerec was director from 1935 to 1941, succeeding the well-known geneticist Albert F. Blakeslee, who reported inducing gene mutation some six months before Muller. Blakeslee was also a member of the American Eugenics Society.

Dobzhansky, Theodosius: He was an officer of the American Eugenics Society (AES). He was a director of population genetics related activities from 1954 to 1973. He was also Chairman of the Board from 1969 to 1975. He had notable graduate students with strong eugenics backgrounds and leadership roles, including Bruce Wallace and Richard C. Lewontin [28].

Glass, Bentley: Member of the AES beginning in 1956. He became a Director of selected activity areas of the AES from 1958 to 1971. In 1971, when Bentley Glass was President of the AAAS, he emphasized that "in a world where each pair must be limited, on the average, to two offspring and no more, the right that must become paramount is not the right to procreate, but rather the right of each child to be born with sound physical and mental constituents, based on a sound genotype. No parent will in the future will have to burden society with a malformed or a mentally incompetent child." (Cited in Ref. [98]) [28].

Little, Clarence C.: He presented a paper at the Second International Eugenics Conference in 1921 entitled: "Inheritance of a predisposition to cancer in man." He was President of the American Eugenics Society from 1928 to 1929; a Director within the AES from 1923 to 1935; Major leader of American Birth Control League [28].

Kaufmann, Bertwind: He joined Demerec at the Cold Spring Harbor in 1937 and became its director following the retirement of Demerec in 1960.

Muller, Hermann J.: Member of the American Eugenics Society beginning in 1921. Muller presented a paper at the Second International Eugenics Conference in 1921 entitled "Mutation. [28]. Related information: Muller's Ph. D. student, Clarence P. Oliver (1898–1991) became the first director of the Dwight Institute at the University of Minnesota for the Promotion of Human Genetics. The Institute had a strong emphasis on eugenics via research, education and family counseling activities. Oliver continued in this leadership position for five years, then moving to the University of Texas where Muller did his Nobel Prize research to become the director of a new Institute for Human Genetics. Oliver was on the editorial board of Mankind Quarterly whose goals were to "reverse ideas of equality which were spreading and leading to school desegregation." Other colleagues of Muller were members of the AES such as John T. Patterson, his Department Chair, George Snell, who did a Post-doc with Muller from 1931 to 1933 and who became a Nobel Prize recipient in 1980. Theophilus Painter, a widely acclaimed Drosophila cytogeneticist, was also active in the eugenics movement.

Neel, James V.: He received his Ph.D. under the guidance of Curt Stern, and he also established the second academic research institute for human genetics. Neel's research was always at the center of eugenics debates, but Neel was very scientifically oriented. Given that he was a national leader in the assessment of hereditary diseases, he would inevitably be drawn into issues related to eugenics throughout his career. He also worked with leaders who were far more activist than he was. Thus, he was very sensitive to the issue of eugenics and very careful to maintain a scientific, not ideological, perspective on the topic.

Russell, William L.: Received his Ph.D. under the guidance of Sewall Wright. It is not known if he took the eugenics related course taught by Wright. Russell worked for five years at The Jackson Laboratory in Maine, in close association with Little, who had a powerful leadership presence in the area of eugenics. It is interesting to note that George Snell, a Nobel Prize recipient, was Russell's coworker at the Jackson labs and Snell also was a member of the American Eugenics Society.

Sonneborn, Tracy M.: Sonneborn was a longtime AES Member. He was a member of the Biology Department of the University of Indiana and a close colleague and friend of Muller. Under the leadership and direction of Frederick Osborn, Sonneborn was recruited for and joined the Board of Directors of the American Eugenics Society (Paul, 1997) [28].

Stern, Curt: Stern was also a long-time member of the AES. Stern's attitude toward eugenics is reflected in the following quote: "To state that reproductive selection against severe physical and mental abnormalities will reduce the number of affected from one generation to the next by only a few per cent does not alter the fact that these few percent may mean tens of thousands of unfortunate individuals who, if never born, will be saved untold sorrow" [44]. He was a recipient of a Rockefeller Foundation Fellowship on several occasions [28].

Wright, Sewall: AES Advisory Council 1927–1935. He was also a member of the Illinois State Eugenics Board starting in 1929. Mrs. Wright was also a member of the AES. Wright was most closely associated with the Dobzhansky wing of the AES, throughout his career, including debates with Muller within the BEAR I Genetics Panel. Wright taught a course at the University of Chicago entitled: "Evolution, Genetics, and Eugenics." [28, 69, 72].

Data availability

No data was used for the research described in the article.

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