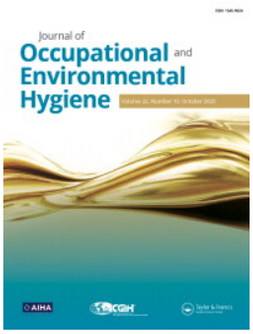

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COMMENT

Hermann J. Muller: Another ethical improbity: Deceiving the journal *Science*

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ABSTRACT

This article describes how Hermann J. Muller attempted to persuade the editor of the journal *Science* to obtain a review of his book on eugenics entitled: *Out of the Night: A Biologist's View of the Future*. Muller sent the editor of *Science*, James McKeen Cattell, a letter soliciting a review of his forthcoming eugenics book. Muller's letter is of historical significance for several reasons. It highlights how Muller characterized his own standing on the issue with respect to his geneticist colleagues, especially those in the United States. Of even greater importance is the striking lack of transparency that Muller employed in attempting to persuade the editor, not only to publish a review of his book, but also to select the reviewer(s) from a list of suggested geneticists/biologists he provided. The Muller letter is significant since it shows that Muller was deceptive in his communication with the editor. He failed to disclose his personal and professional relationships with each of the six proposed reviewers and their long histories of support and advocacy for eugenics-based societal policies. The present example of Muller's ethical improbity adds to a substantial listing of similar actions that are linked both to his propensity for inappropriate self-promotion and ideological advocacy as seen in activities dealing with eugenics, chemical and radiological risk assessment, hereditary and cancer risk assessment, health physics practices, and the development of secondary school biological curriculum. The current paper gives a rare glimpse into ethics and bias in the scientific community and raises a series of new challenges to the culture of science and its dependence on honesty and transparency. While Muller may be appreciated as a scientist of great talent and achievement, he also displayed personal failings that undercut the integrity of scientific research.

KEYWORDS

Eugenics; history of science; mutation; peer review; precautionary principle; research ethics

Introduction

Hermann J. Muller was a national and world leader of the eugenics movement. According to Carlson (1981), Muller gravitated to the concept of eugenics as a young adult college student. Muller would subsequently adopt stringent eugenics beliefs, offering recommendations for both positive and negative eugenics practices and integrating these into his personal and world views. Nonetheless, Muller subsequently claimed to have repudiated racial and class features of this eugenics framework, based, in part, on improved understandings of the difficulty in removing low frequency genes within the population by various means, including coercion, such as forced sterilization, which he advocated at least as late as 1932 (Calabrese and Shamoun 2025). Muller would become a prominent leader in the reform eugenics movement which he viewed as more science-based, with an improved understanding of the gene-environment interaction (Paul 1984). Despite the enlightened euphemisms of Muller and his reform eugenicist colleagues, there remains some question of whether the transformation was genuine or rather part of a strategy that camouflaged his intent. The latter could be the case, as depicted in a comment of Bentley Glass, a Muller protege and former Ph.D. student of Muller's (Calabrese et al. 2025). In Glass's case, his repeated claims to be a reform eugenicist, similar to Muller, may be best considered in light of a commentary that he offered in *Science* in 1971 (Glass 1971):

> 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 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

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> 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 prompt the question of who the real Glass was, and for that matter, Muller himself. Based on his own writings, Glass appears to unmask his true philosophical identity, which he so long kept successfully hidden. Perhaps this is best elucidated in the words of physician and ethicist Leon Kass (1971), who asserted that "Bentley Glass convicts himself by his own defense."

With this historical backdrop as an introduction, the present paper reports on a letter by Muller in which he requested that the editor of *Science*, James McKeen Cattell, solicit a review of his forthcoming eugenics book entitled: *Out of the Night: A Biologist's View of the Future* (Muller 1936). Muller's letter is of historical significance for several reasons. It highlights how Muller characterized his own standing on the issue with respect to his geneticist colleagues, especially those in the United States. Of even greater importance is the striking lack of transparency in the deceptions that Muller employed in attempting to persuade the editor, not only to publish a review of his book, but also to select the reviewer from a list of suggested geneticists/biologists he provided. Muller failed to disclose his extensive relationships with these individuals and their individual long histories of eugenics advocacy. Muller tried to brush this issue away by writing that he did not have an a priori arrangement with any of the six listed possible reviewers. This does not excuse the failure to disclose his associations with each recommended reviewer. Muller also misled the editor by saying that these listed people would likely not be predisposed to be "antagonistic" to his perspective, when, in fact, they would likely be enthusiastically supportive of the Muller perspective. In fact, each had a record that would strongly suggest a powerful advocacy for the Muller position. Quotes from Muller's letter to the *Science* editor (Muller 1935) include:

> From my knowledge of American geneticists and general biologists, however, I fear that you would not be likely to find one with a social viewpoint sufficiently developed to enable him to tolerate the book at all …
>
> And I know that, even among those relatively few who might not be so hopelessly offended in this respect, nearly all possess already formed antagonistic opinions with respect to the main thesis of my book, its positive stand on the question of the applicability of genetics to man in a radical manner … If then it is to receive the serious consideration which I of course hope it will merit to some degree, it will have to be discussed by one of those rare persons whose opinion is not weighted against it in advance.

Muller then suggested five British and one US geneticist as possible reviewers: Julian Huxley, John Burdon Sanderson (JBS) Haldane, Cyril Dean Darlington, Sydney C Harland, Phineas Wescott Whiting, and Joseph Needham. Muller also mentioned Edgar Altenburg as a possible reviewer but then noted his long-standing friendship with Altenburg. This action appears to have been strategic and disingenuous, trying to give the impression to Cattell that Muller was trying to be fair minded when he was not. This also appears to have been a means to further disguise his relationship with the other suggested reviewers. In fact, Cattell likely already knew the close relationship of Muller and Altenburg since Muller, Altenburg, and Cattell were at Columbia University at the same time. Cattell had a close and long-standing personal and professional relationship with Morgan, Muller's former Ph.D. advisor. Further supporting a personal relationship amongst Muller, Altenburg, and Cattell is that Muller and Altenburg had also studied with a daughter of Cattell in the now famous Morgan "fly room" laboratory.

Muller went on to say:

> I do not actually know how the above-named men would stand as we have no understanding with one another about the matter, but on judging merely by my general knowledge of them that they would be more likely than others not to be, a priori, and on principle, quite condemnatory of the book. Now as there will undoubted be plenty of highly adverse reviewers of the book in other journals, I am hoping that you will be so liberal as to give a chance for its own viewpoint to be at least in some part represented, by choosing your viewer (if any) from among one of the persons …
>
> I do not ask for a review by some special friend, or even by anyone I know in advance to agree with me …

Relationship of the reviewers to Muller and the concept of eugenics

Of the six reviewers recommended by Muller for his eugenics book, five would sign the Eugenics Manifesto that Muller drafted and published in the journal *Nature* with a total of 22 coauthors, listed alphabetically (Crew et al. 1939). The remaining listed reviewer, Phineas Whiting, who did not sign the Eugenics Manifesto, had a long relationship with Muller.

Julian Huxley

Most of the listed reviewers had a unique prior relationship with Muller and each other, but some, such as Sydney Harland, were more obscure but no less relevant. For example, Julian Huxley hired Muller in 1915 for Muller's first academic position at the Rice Institute (now Rice University) in Houston, TX. The relationship between Huxley and Muller was mutually enjoyable, both professionally and personally, according to Muller's biographer (Carlson 1981). Huxley was also a longtime member of the British Eugenics Society and became its vice president from 1937 to 1944 and president from 1959 to 1962. Muller and Huxley also maintained an extensive correspondence throughout their careers.

JBS Haldane and Cyril Darlington

JBS Haldane was a longtime prominent member of the British Eugenics Society and an adviser to Cyril Darlington during his Ph.D. research from 1927 to 1932. It was during his graduate student program (1927) that Darlington joined the British Eugenics Society. Preserved papers of Darlington indicate that he communicated with Haldane on eugenics issues as early as 1926. In a similar manner, Darlington began his correspondence with Muller as early as 1931. As for Haldane, like Huxley, he had an extraordinary range of talents, displaying great scientific achievements and leadership. Haldane also had a long personal and professional relationship with Huxley. Muller, as well as Thomas Hunt Morgan's fly room team members, were enthusiastic supporters of the Darlington research, starting with his striking Ph.D. findings that provided considerable support for Mendelian genetics along with providing a firm basis for genetics-based evolutionary biology (Carlson 1981).

In 1937, when Julian Huxley was director of the London Zoo, he learned of Muller's imminent departure from the Soviet Union. He initiated an effort to get Muller appointed for Haldane's position at the John Innes Institute in the UK and generated support from Darlington. According to Carlson (1981), Huxley was very excited about the possibility of hiring Muller. He contacted Muller and was confident that it would happen, starting in September of 1937. However, the offer to Muller was not forthcoming due to insider politics when the Lamarckian, WB McBride, successfully blocked the offer (Carlson 1981). Once that professional door closed for Muller, Huxley again came to the rescue, connecting him to Francis Albert Crew, Director of the Institute of Animal Genetics at Edinburgh, who offered him a position where he could again direct genetic research with graduate students (Carlson 2009; Beaven 2023). These perspectives illustrate the relationship that Muller had with these individuals (i.e., Huxley and Darlington) and his standing with them. Crew would eventually become a coauthor of the Eugenics Manifesto.

Sydney C. Harland

Sydney C. Harland was listed by Muller as a possible reviewer for his eugenics book. According to Davis (2018), Harland was a British botanist who was recruited by the Imperial College of Tropical Agriculture in Trinidad to educate and train students in agriculture with a strong influence on genetic applications. In 1931, Harland (1931a) created a public debate with a eugenics-based article entitled "Race Admixture" in a local journal called *The Beacon*. The article was seen as inflammatory since Harland claimed that Blacks were genetically inferior to Caucasians with respect to intelligence. This led to a serious follow-up debate within *The Beacon* (Harland 1931b; James 1931; Mendes 1931; Mentor 1932). The debate led Davis (2018), some eight decades later, to conclude that Harland displayed a eugenics framework that was symbolic of colonialism in Trinidad. Even though Harland was a plant geneticist, he transformed his botanical perspectives to guide his beliefs about the basis of human personality traits based on a eugenics framework. How this rather "local" but intense dispute may have led to Muller suggesting Harland as a potential reviewer has been partially clarified.

In his autobiography, Harland (2001, 82) noted that he attended the International Genetics Congress at Cornell University in Ithaca, NY during August 1932. At this meeting, where Muller gave one of the Plenary talks, Harland was introduced to Nicolai Vavilov, the prominent Soviet geneticist and friend of Muller. Harland indicated that Vavilov sought him out and said he had read all of Harland's publications. Harland was even given the use of Soviet consular vehicles when he visited New York City after the meeting at Cornell University. Several months later, in early 1933, Vavilov visited Harland, traveling to Trinidad to tour Harland's genetic research facilities and ongoing research. Harland then took sabbatical leave, which involved a prolonged visit to the Soviet Union based on an invitation from Vavilov. The timing of the visit by Holland to the Soviet Union coincided with Muller's relocation to the Soviet Union.

During this visit to the Soviet Union, Harland attended a banquet in honor of Muller (October 1933). According to Harland, he sat next to Muller during the event (p. 91). Muller had left Germany for the Soviet Union in late September 1933. Muller therefore had several opportunities to meet Harland and get acquainted with his research and attitudes toward eugenics. At some point, Muller recognized Harland as a fellow eugenicist, and Harland would ultimately sign the eugenics manifesto.

Harland would also have a relationship with Cyril Darlington as they jointly authored an obituary article on the life of Vavilov after he died of starvation in 1942 in a Soviet prison camp (Darlington and Harland 1945). Harman (2003) also linked Darlington to Harland and Haldane within a mutual BBC broadcast concerning science and the Soviet Union in the Lysenko era, a theme that similarly resonated with Muller (Carlson 1981).

Phineas Wescott Whiting

Phineas Wescott Whiting (1887–1978) received his undergraduate degree in applied biology in 1911 and a Masters' degree in 1912, both from Harvard. While at Harvard, he became interested in genetics and studied under the direction of William Castle. Others in his peer group who worked with Castle at this time included Clarence C Little and Seawall Wright, who would later become members of the NAS BEAR 1 Genetics Panel that provided the profoundly important recommendation to adopt a linear dose response model (also called the linear no-threshold (LNT) model) for hereditary risk and with later application to radiological and chemical cancer risk assessment (Calabrese 2015, 2019). Leslie C. Dunn was also a member of this highly achieved group of genetics graduate students. Whiting's research was directed to the genetics of coat color in domestic cats and rats. By 1915, he had moved to the University of Pennsylvania to complete his Ph.D. under the direction of Clarence McClung who was among the first to associate chromosomes with the newly hypothesized Mendelian factors of heredity. During his Ph.D. program, he directed his research to the genetics of a parasitic wasp which remained his focus for the next 50 years.

Whiting became acquainted with Muller as a graduate student, meeting at Woods Hole during the summer months where many graduate students in genetics from the major east coast universities researched and socialized, especially during the early decades of the 20th century. So close was their relationship that Muller contacted Whiting in January 1927 concerning his newly observed X-ray-induced gene mutations long before he even submitted his famous paper to *Science* (Muller 1927). Muller informed Whiting of his preliminary gene mutation findings and strongly encouraged Whiting to attempt to replicate the occurrence of X-ray-induced gene mutations in his wasp model. Whiting did this research with Muller's direction starting experiments as early as January 1927 and published a supportive paper (Whiting 1928; Richmond et al. 2012).

Whiting used these various educational experiences to develop a broad range of professional contacts including Little, Muller, Painter and many other future leaders in the genetics community. After graduation, he had several short-term academic positions including his faculty position in 1921 at the University of Iowa in the area of eugenics. During this early period, Whiting developed a broadened interest and published articles on eugenics, birth control, human heredity, and selection in socialist publications such as the *Birth Control Review* and *The Nation's Health*. In March 1925, Whiting gave a lecture at the 6th International Neo-Malthusian and Birth Control Conference entitled: "Selection, The Only Way of Eugenics" that was subsequently published in *Birth Control Review* (Whiting 1925). Whiting's Ph.D. wife Anna Rachel Whiting, was similarly focused as her husband, teaching a college course on heredity and eugenics at Catawba College, North Carolina (Richmond et al. 2012).

Whiting (1936) eventually published a positive review of the Muller eugenics book for the *Journal of Heredity* titled "Communist Eugenics." This suggests that Muller may have written the same letter suggesting reviewers and requesting a book review to editors of multiple journals. This is also supported by the fact that Huxley (1936) also wrote a highly supportive review of the Muller book titled "Marxist Eugenics" in the *Eugenics Review*.

Joseph Needham

Of the six recommended reviewers, only Needham does not appear to have had a direct personal and/or professional relationship with Muller. Furthermore, Needham was not trained as a geneticist but as an embryologist. While Muller was principally interested in germ cell mutations, Needham was focused on the underlying mechanisms of embryo development. Thus, while probably most geneticists were markedly

affected by the claims of Muller to have induced gene mutations, this would not have been the focus of Needham. What brought Needham and Muller together on the topic of eugenics was that both were committed Marxists well before they became famous scientists: Muller in 1927 and Needham from 1931 onward. By 1936, Needham had achieved a high level of international scientific recognition as seen by his invitation to give the highly prestigious Silliman Lecture at Yale University that was published under the title "Order and Life" (Needham 1936). By 1926, Needham showed political convictions similar to Muller supporting workers in the General Strike that year while becoming part of a communist and fellow traveling scientists' group that included very high-level performers such as Julian Huxley, J. B. B. Haldane, J. D. Bernal, and Lancelot Hogben (Wersky 1988). It is also interesting to note that Needham wrote a book via a pseudonym (Holorenshaw 1939) to connect the ideas of 17th century radicals with 20th century Marxism that was promoted by the Left Book Club (Hill-Andrews 2019). Muller would also arrange to have his eugenics book promoted and distributed by the same Left Book Club. While Needham never formally joined the Communist Party, he was active in various left wing causes that sometimes impacted his scientific interests (Hill-Andrews 2019). The linkage of Muller with Needham was likely influenced by their mutual close relationship with Julian Huxley. In fact, while Huxley would help facilitate the possibility of Muller obtaining academic appointments, Huxley played a determining role in Needham becoming the first scientific director of UNESCO in 1945 at the conclusion of World War II (Gudon and Rodbard 2000). Needham's acculturation to the eugenics perspective is seen with his publication of four papers in the journal *Eugenics Review* starting in 1930 (Needham 1930, 1932a, 1932b, 1942). Of further significance is that from 1933 onward, Needham and CW Waddington developed a high-level research collaboration on the genetics of embryological development. This research would eventually lead to Waddington traveling to Cal Tech for a prolonged research visit with Thomas Hunt Morgan to explore the role of gene activation for embryological development. Waddington would transform embryological research within a conceptual framework for the new field of epigenetics, the term that he originally coined. Waddington emphasized that inheritance was not just a process of transmission but also was affected by cytoplasmic factors affecting the genetic message. This led him to focus on the role that the environment might have in informing the individual's biology. This type of environmentalism was strongly influenced by his political beliefs, as he was a Fabian socialist that strongly believed in dialectical materialism. Waddington's perspective on biology was strongly influenced by a group of Marxist scholars in Cambridge called the Theoretical Biology Club, with which Joseph Needham had a central influential role (Peterson 2016). Within this context, it is of significance that Needham was committed ideologically and logistically to the Bolshevik program that was reported by the Soviet delegation at the International Congress on the History of Science and Technology in London in 1931, with Needham hosting the delegation (D'Abramo 2018). Finally, even though Muller did not list Waddington as a possible reviewer for his book, Waddington was also a coauthor of the eugenics manifesto that Muller drafted. Waddington would also engage Darlington in a major BBC broadcast (January 1959) on "The Control of Evolution in Man" which was driven by their eugenics perspectives that still strongly promoted the Muller perspective. The transcript was published in the *Eugenics Review* (Glass et al. 1959).

Discussion

Muller (1935) stated in his letter to Cattell: "I do not ask for a review by some special friend or even by some who I know in advance to agree with me…" The information provided above on each of the six recommended reviewers contradicts the above quoted statement, showing it to be lacking in transparency. However, Cattell (1935) suggested to Muller that the book may not be reviewed since the journal was interested more in science rather than in unresolved controversial matters. Cattell indicated that reviews should be of books "that definitely contribute to the advancement of science. I assume that your book takes up controversial matters that cannot be settled by the objective methods of science." However, the fact that Science would not publish a review of Muller's eugenics book does not excuse Muller's failure to disclose to the editor his relationship with each of the recommended reviewers or his failure to properly characterize the historical relationships of each reviewer to eugenics and their respective histories of strong advocacy of a eugenics agenda. The letter of Muller is also of significance since it shows a deliberate strategy whereby Muller not only deceived the editor of *Science* but also deceived a person that he had known for about 25 years, whose family members

knew each other well, and who was the editor who allowed him to publish his groundbreaking paper on X-ray-induced mutation (Muller 1927) without showing any data. This publication decision by Cattell to publish Muller's paper without supportive data thereby permitted Muller to achieve primacy in this scientific arena, a major step toward his subsequent receipt of the Nobel Prize.

Such actions of Muller resonated with his characterization by James Crow, Muller's longtime colleague and friend (Crow 1987). Crow and Abrahamson (1997) claimed that Muller, as greatly achieved and gifted as he was, often exaggerated positions in debate in attempts to win arguments. To Muller's supporters, like Crow, this was both unnecessary and disappointing. This pattern of behavior has now been exposed in numerous recent publications across a range of topics with Muller doing whatever it takes to win the argument or action-related decision, ranging from modest to massive exaggeration (e.g., grossly overstating evidence that supported his reverse mutation hypothesis [Lefevre 1949, 1950] to blatant bias/possible dishonesties [Calabrese 2019, 2020, 2022a, 2022b, 2023a, 2024; Calabrese and Selby 2024a, 2024b, 2024c]). One can now examine the nature of the relationships of the individual reviewers proposed by Muller and eugenics both before and after 1935. For Muller, there is no escape from his dishonesty.

For example, even though Muller made the claim that the X-ray-induced transgenerational phenotypic changes that he reported were due to gene mutations, coining the term "point mutation," he was challenged by his close colleague and longtime friend, Edgar Altenburg, to prove that he had not simply punched holes in the chromosomes via the massive doses of radiation administered. Muller acknowledged this specific comment by Altenburg in a subsequent paper (Muller 1928). Another example is that, as soon as he returned from presenting his groundbreaking findings from Berlin in September, 1927, Muller assigned Frederick Hanson, a professor from Washington University on sabbatical with him, to quickly begin investigating whether he could show that the high doses of X-rays that he used could induce what Muller called "reverse" mutations in an effort to demonstrate that the transformed gene was intact and functional and not largely deleted or otherwise massively altered via various types of chromosomal rearrangements (Calabrese and Giordano 2023). Thus, Muller knew early on that he had not proven his case. The reverse mutation research quest was adopted by multiple research teams given the critical question that had been posed. However, after a decade of multiple and diverse research efforts, the reverse mutation approach of Muller resulted in a major failure (Lefevre 1949, 1950). During this period, the concept of the "position" effect was established in which an intact part of a chromosome could be physically translocated to another chromosome and affect the gene expression of the newly adjacent genes, causing some of the same effects he had shown (Muller 1947; Carlson 1981). In addition, Lewis Stadler, a prominent plant radiation geneticist from the University of Missouri, claimed that Muller had confused an observation with a mechanism, arguing that the high doses of X-rays that he applied acted almost entirely, if not entirely, at the level of gross chromosomal changes (Calabrese 2015, 2017, 2018a, 2019). Muller's postdoctoral student, George Snell (1935), was unable to support the gene mutation hypothesis but showed that X-rays induced chromosomal translocations in mice using the same study design but with lower doses (Calabrese and Selby 2024a, 2024b). By the mid-1930s, Muller's biographer and last graduate student, Elof Carlson (1981, 207), specifically stated that Muller had come to doubt that he had induced gene mutation. Finally, in 1956, in both private correspondence with Warren Weaver, chair of the US NAS BEAR I Genetics Panel (Muller 1956a), and in published material (Muller 1956b), Muller admitted the obvious, that Stadler and others were correct. In fact, Muller's former laboratory research director, William Lee, and colleagues showed this was the case at the nucleotide level (Fossett et al. 1994). Thus, while Stadler had finally won the two-decade debate with Muller, it took a little longer for this to be explicitly acknowledged by those most closely associated with Muller such as Crow and Abrahamson (1997) and separately again by Abrahamson (1996), Muller's former graduate student. The significance of this turn of events is that Muller received the Nobel Prize (1946) for claiming that he had induced gene mutation, which was known not be the case by many, including Muller himself and his closest colleagues. Nonetheless, Muller skillfully used the receiving of this most prestigious award in science, the Nobel Prize, to transform the field of cancer risk assessment by adopting the LNT dose response model despite having knowledge that his foundations for the LNT model had been successfully challenged and largely refuted.

Muller's influence on the global scientific community and regulatory agencies around the globe with respect to chemical and radiological risk assessment, hereditary and cancer risk assessment, health physics

practices, environmental contamination practices, the U.S. Environmental Protection Agency and other regulatory agencies; and the area of toxic torts, has been long, enduring, and highly significant.

In addition, detailed recent investigations of Muller's career have revealed that he did not induce gene mutation in his research described (without data) in his "breakthrough" paper in *Science* but mostly modest to massive gene deletions (Calabrese and Selby 2023), was wrong that genetic damage could not be repaired ([Calabrese and Selby 2023], that induced mutations/tumor formation were additive to background [Calabrese 2018b], and that the dose response relationship for ionizing radiation and chemical carcinogens was linear down to a single ionization and a single molecule [Calabrese 2019, 2022b]). Evidence shows that Muller's substantial research achievements on X-ray-induced mutation were incorrect (i.e., he induced modest to massive gene deletions rather than gene mutations) and that his application of these findings to the areas of hereditary and cancer risk assessment was based on scientific errors and self-interest guided by a geopolitical ideological framework. In fact, the field of cancer risk assessment was derived from the uncritical acceptance of Muller's perspective, which then led to the adoption of the precautionary principle as a regulatory framework in the U.S. and many countries around the world, which he first proposed in discussion/debate in 1949 with the well-known health physicist Robley Evans (Calabrese 2023b). The collective findings indicate that the historical foundations of cancer risk assessment and their current practices, which have been so influenced by the scientific and policy leadership of Muller, need substantial revision.

Conclusion

The present paper provides a novel insight into the character of Hermann J. Muller, the 1946 Nobel Prize recipient. The paper demonstrates that Muller was dishonest in his communications with the editor-in-chief of the journal *Science*, who was also a long-time professional associate with whom he had a long-standing personal relationship. This episode in the professional life of Muller represents yet another in series of his ethical improbities that were related to enhancing his career at the expense of others. Muller's actions have had profound impacts on the regulatory frameworks for chemical and radiological risk assessment, hereditary and cancer risk assessment, and environmental contamination around the world. The magnitude of these impacts has yet to be determined, but at a minimum, the basis in which regulations passed based on a dose response relationship for ionizing radiation and chemical carcinogens linearity down to a single ionization and a single molecule, was knowingly perpetuated by Muller for decades, even after he was aware that his landmark *Science* paper was shown to be incorrect.

This episode highlights the complexity of Muller and his desire to promote his career and agenda at the expense of honesty. The current paper gives a rare glimpse into ethics and bias in the scientific community and raises a series of new challenges to the culture of science and its dependence on honesty and transparency. While Muller may be appreciated as a scientist of great talent and achievement, he also displayed personal failings that undercut the integrity of scientific research. This is a call to action for the scientific and regulatory communities to revise the historical foundations of cancer risk assessment and their current practices, which have been so influenced by the scientific and policy leadership of Muller.

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