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COMMENT



Edward B. Lewis: His radiation leukemia article in *Science* journal: A turning point in cancer risk assessment

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ABSTRACT

Edward B. Lewis published what is widely considered the most influential article in the history of cancer risk assessment. He was successful in changing the radiation risk assessment focus from transgenerational birth defect concerns to cancer due to his success in getting his article published in *Science* journal, receiving a highly laudatory editorial endorsement, and quickly becoming a prominent speaker to the U.S. Congress and the focus of a major story in *Life* magazine, amongst other notably rapid recognitions. However, what is generally not appreciated is that Lewis had no education and training in the complex topics of his article, with no professional knowledge of radiation, leukemia, biostatistics, epidemiology, and risk assessment. This article explores how the inexperienced Lewis benefited from the manipulation of *Science* by leaders in the scientific community and to avoid proper peer review. Despite the recognition of serious fundamental flaws in the Lewis article, it became embraced by the ideological leaders of the radiation genetics community who ensured the Lewis-based linear no-threshold (LNT) model for cancer risk assessment would become accepted and serve as the foundation for the adoption of the precautionary principle, which is the basis for the Environmental Protection Agency (EPA) and European Union regulatory activities, and global regulatory entities.

KEYWORDS

Atomic bombs; dose response; leukemia; radiation; radioactive fallout

Introduction

This article is about how one person can make a difference. It is the story of Edward B. Lewis, then an unknown young professor of entomology, focused on development and growth of *Drosophila*, who was encouraged by his Division Chair, the very prominent and soon to be Nobel Prize recipient, George Beadle, to write an article on radiation risk assessment with the focus on leukemia (Calabrese 2020). In a short time, Lewis had completed this “assignment” and under the guidance of Beadle and other members of the U.S. National Academy of Sciences (NAS) Committee on the Biological Effects of Atomic Radiation (BEAR) I Genetics Panel found a way to avoid a proper peer review and to get his article published in *Science*, along with receiving a strikingly positive editorial endorsement by an editor whose expertise was amphibian reproduction, with no unique understanding of the critical topics of the article. This ideological focus then facilitated a further series of high-profile events/activities that led to the Lewis

article changing the national and global dialogue and, most importantly, the adoption of his flawed scientific and risk assessment perspectives for national and international policies up to the present time. While this article is about the striking story of Lewis, it is also about how the scientific community and society were duped by key scientific leaders (i.e., BEAR Genetics Panel) in an appeal to authority and how it continues. Furthermore, this article extends the earlier detailed evaluation of the Lewis (1957) key article as reported in Calabrese (2021a, 2021b) in which multiple fundamental limitations of that article were addressed in considerable detail, largely discrediting its scientific value.

The Lewis paper

On May 17, 1957, Edward B. Lewis, then a young professor in the Biology Division of CalTech, published what is considered the most significant article on cancer risk assessment of the twentieth century and to the

present time (Lewis 1957). Lewis built upon and extended the research and recommendations of the radiation genetics community and the U.S. BEAR I Genetics Panel that asserted that the dose response for radiation-induced mutations in reproductive cells for males and females was linear down to a single ionization. What Lewis did was to extend this perspective to somatic cells and cancer. He used the concept of radiation-induced mutation citing the findings of Curt Stern and Delta Uphoff (Uphoff and Stern 1949) during the Manhattan Project, to establish the mechanistic and dose-response framework with research on *Drosophila*. He then assimilated epidemiologic findings from four target populations that had experienced apparent dose-dependent increases in radiation-induced leukemia. These four groups included: radiologists with nearly a half century of supportive data, patients with ankylosing spondylitis with exposure that targeted bone marrow, children with an enlarged thymus who were treated with therapeutic doses of radiation, and the survivors of the atomic bomb explosions in Japan. While each targeted population was important and complemented the findings of other groups, the atomic bomb survivors proved to be the most significant due to the size of the population, the commitment to study this group in a longitudinal fashion and the massive effort concerning dose reconstruction to better estimate responses at much lower doses than in the other three groups.

The Lewis (1957) article has had a very significant impact within the scientific community, the U.S. Congress, and the general public, as seen by the fact that it was published in *Science*, the most prominent journal worldwide, received a glowing editorial from the editor-in-chief of *Science* (DuShane 1957), and became the object of focus and debate on the national news program in the United States called Meet The Press within one week. One week later Lewis testified to a U.S. Congressional committee which was followed by the prominent magazine, *Life*, publication of a major profile on the risk of ionizing radiation only one week later, thus elevating Lewis to further prominence. This was quite an auspicious start for Professor Lewis, especially in a highly complex field that was not even remotely related to his own area of expertise. The newly gained prominence of Lewis was soon translated into various invitations to serve on major advisory committees, in fact, the most prominent in the country, such as the National Council on Radiation Protection and Measurements (NCRPM) and continued later with membership on the Biological Effects of Ionizing Radiation (BEIR) I Committee, groups that were so highly regarded that their recommendations were often quickly implemented into national policy with little to

no debate. These policies were then quickly implemented globally.

Given this situation, it is important to consider how the Lewis phenomenon occurred. How does a young entomologist professor, who studies developmental processes in fruit flies, come to write a globally transformative article that is published by the most prestigious and selective journal in the world? And how did this article receive a powerfully positive editorial endorsement from the editor of *Science*? Could this have happened to any other fruit fly development geneticist?

It happened because of an amazingly complex causal nexus that made the impossible not only possible, but made the impossible happen. It started with Lewis being a professor in the Biology Division of CalTech. This was key since his Department Chair was George Beadle, another geneticist who had worked on corn, flies, and fungi. However, Beadle was not an ordinary experimental geneticist, as he, like Hermann Muller about a decade before, was soon to receive the Nobel Prize, only a year after publication of the Lewis (1957) article in *Science* (Calabrese 2020). Beadle was well established as an influential scientist on the national and world stages, having won numerous awards, including the Lasker Award in 1950 which frequently precedes receipt of the Nobel Prize. Of importance to the Lewis (1957) article in *Science* is that in 1955, Beadle had been president of the American Association for the Advancement of Science (AAAS), which owns and controls the journal *Science*. Of particular relevance is that Beadle served as an advisor to the Atomic Bomb Causality Commission from 1947 until 1960. As a result, Beadle had access to newly obtained/unpublished findings concerning the effects of the atomic bombs on the occurrence of adverse health effects, including the emerging concern with increases in leukemia incidence (Calabrese 2019).

Given his insider knowledge, Beadle tried to encourage members of his faculty to evaluate the effects of radiation on the occurrence of leukemia in humans, with Lewis being the only one that accepted his challenge. The timing of this encouragement by Beadle was also important as it coincided with the appointment of Beadle to be Chair of the U.S. NAS BEAR Genetics Panel. This Panel was strikingly prestigious and influential, being comprised of some of the most significant geneticists in the United States and globally, including Hermann J. Muller. In addition, the chair of the BEAR Genetics Panel that Beadle replaced was Warren Weaver, who had been the president of the AAAS the year before Beadle took office. However, Weaver remained very powerful,

being the Chair of the AAAS Board of Directors. If the BEAR Genetics Panel was not strong enough with their AAAS and *Science* journal connections, another Panel member, Bentley Glass, had also been both acting Editor-in-Chief in 1953, and one of six senior editors, at the time of the Lewis *Science* publication in 1957 (Calabrese 2021b). This impact on *Science* journal was visible and impressive as the NAS arranged to have each of its six Panels have their technical reports separately published in *Science* immediately following (June 1956) the release of their Report to the Public concerning respective BEAR Panel's recommendations. Thus, the impact of the NAS and the Panel membership, especially the Genetics Panel, on the decision making at *Science* journal was evident.

Beadle had encouraged Lewis to focus on the concept of the somatic mutation hypothesis, whereby societal/public health concerns would shift from transgenerational birth defects, which the Neel report found no evidence to support (Neel and Schull 1956; Calabrese 2020), to the occurrence of radiation-induced cancer in humans. This shift was a critical strategic decision by Beadle, as he was assuming leadership of the BEAR Panel at a tumultuous time because Muller and James V. Neel were engaged in a major and highly publicized dispute within the United States and European radiation genetics communities. Neel, who was the director of the U.S. NAS sponsored Atomic Bomb Causality Commission (ABCC) study on birth defects and a BEAR Genetics Panel member, had found no treatment-related reproductive based genetic effects in over ten years studying 75,000 offspring of survivors of the atomic blasts on Hiroshima and Nagasaki, a study in which the findings continue to the present with negative effects (Calabrese 2020). At the opening meeting of the BEAR Genetics Panel on November 20, 1955, in Princeton, New Jersey, Muller insulted Neel's study, referring to Neel's negative findings as "illusionary" and thus emphatically shutting down the possibility that Neel's new and massive data set that showed no treatment-related genetic effects would be considered by their Panel (Calabrese 2015, 2019, 2020). Neel became frustrated and angry and sent the final chapter of his report some two months later to all the members of the Genetics Panel. However, even these belated efforts had no impact, except for further frustrating Neel. In fact, even this belated attempt by Neel to educate the Genetics Panelists failed to result in the subject being discussed. This simmering conflict of Muller and Neel escalated during a World Health Organization (WHO) conference in Copenhagen in the first week of August, 1956, generating headlines in a *New York Times* story in

which two hostile geneticist campus emerged: the Mullerians (i.e., the followers of Muller as called by the *New York Times* writer) and the anti-Mullerians as led by Neel (Calabrese 2020). The commotion continued into a follow-up meeting of a WHO radiation genetics committee meeting the next week, also in Copenhagen. While efforts were made to reconcile differences between Neel and Muller, it was to no avail, and Panelists were forced to take sides in the dispute. Muller tried to use his power and influence to deny Neel the chance to speak and to publish his findings, which resulted in a threatened walkout by the British contingent (Calabrese 2020). After returning to the United States, Alexander Hollaender, Director of biological research at Oak Ridge National Labs and member of the BEAR Genetics Panel, tried to reconcile Muller and Neel by hosting a meeting of the two warring parties, but again without success, further exacerbating an already highly inflamed situation. Into this dispute stepped Beadle as the new Genetics Panel Chair. Knowing that it was not going to be possible to resolve the Neel and Muller dispute over radiation-induced mutational risks in the short term, or maybe never, Beadle refocused attention on a pressing follow-up concern, which was cancer. This would involve a major focus on leukemia, which had emerged in ABCC findings and which he had insider information on in advance of their publication. It was within this context that he encouraged Lewis to take on the assignment of conducting a radiation-induced leukemia risk assessment, even though Lewis was ill-prepared for the task and the likely hostile response by leading members of the radiation research community as led by the Atomic Energy Commission (AEC), its scientific leadership, and some of its funded researchers. This unusual situation will also be shown as creating the framework in which leaders like Beadle used the young Lewis as their stalking horse to achieve one of their long-sought after goals of changing the risk assessment model for radiation from a threshold to a linear model (Calabrese 2020). While Lewis was forced to take professional risks in publishing a highly provocative article on radiation risk assessment, he also had considerable support in very influential areas.

Even though Lewis was busy with his normal academic tasks, he finalized a draft in November 1956, some four months after Beadle had become Chair. Beadle used his authority as Chair of the Division of Biology and Chair of the BEAR Genetics Panel to get the Panel to provide their version of an informal but friendly/supportive peer review of the Lewis draft paper (Calabrese 2020). On paper, this may have sounded like a great idea, as Lewis was given the

opportunity to engage some of the most prestigious geneticists. It is not clear how many of the Panelists provided comments back to Lewis, but certainly James Crow, Alfred Sturtevant, and James Neel did (Calabrese 2019), with Neel providing information on exposure assessment. However, none of the members of the Genetics Panel were experts on cancer, leukemia, types of leukemia, childhood versus adult leukemia susceptibilities, or its etiology, including the activation of latent leukemia viruses by ionizing radiation (Kaplan 1954) as well as epidemiological methods, exposure characterization, and uncertainties in risk assessment modeling (Calabrese 2021a, 2021b). He also had no professional familiarity with medical conditions such as an enlarged thymus in children and ankylosing spondylitis in adults (Calabrese 2021a, 2021b). Every aspect of the radiation and leukemia issue was new to Lewis, yet the unprepared Lewis would take on this assignment and finish this advance draft within about four months after Beadle became Chair (Calabrese 2021a, 2021b).

In marked contrast to the Genetics Panel, the NAS BEAR Pathology and Medicine Panel was built around addressing the issues that Lewis was to assess. In fact, this panel was as prominent as the Genetics Panel but with a focus on human disease causation, with pioneering leaders in experimental models for the assessment of radiation-induced leukemia such as Jacob Furth and Henry Kaplan. In fact, Furth had initiated studies on radiation-induced leukemia in mice in 1929, had developed multiple genetic strains, differentiated the various forms of leukemia within models, documented the role of radiation in inducing cancer, and painstakingly compared the similarities and differences between human and mouse leukemias (Furth et al. 1935). Kaplan (1954) extended the findings of Furth by showing that radiation could activate latent leukemia viruses that led to the development of leukemia and lymphomas in several commonly used animal models. This panel also had Louis H. Hempelmann, who had conducted his own version of pioneer studies on iatrogenic (medical-induced) leukemia. He tracked children who had received radiation therapy for enlarged thymus glands, discovering a significantly higher risk of leukemia and thyroid cancer in that population (Simpson et al. 1955). In fact, this was one of the target groups that Lewis was studying. Shields Warren was the lead pathologist studying atomic bomb survivors in Hiroshima and Nagasaki. He was widely considered the world's foremost authority on radiation-induced leukemia in humans. In 1956, he published a landmark study in the *Journal of the American Medical Association* (Warren 1956) that

showed an increased incidence of leukemia among radiologists compared to other physicians, another of the target groups of the Lewis assessment. It was the leadership of Warren that in fact led to the creation of that ABCC in the 1946/1947-time frame. Eugene P Cronkite was a world leader in studying the effects of radiation on blood-forming organs. He was a founder of the International Society for Experimental Hematology. In 1954, just two years before the BEAR report, Cronkite served as the Project Director for the medical team that treated the Marshallese people accidentally exposed to fallout during the Castle Bravo nuclear test (Cronkite et al. 1997). While there were other similarly uniquely educated and experienced members of the Pathology Panel with direct relevance to the Lewis article, the critical point here is that Beadle failed to guide/ensure that the inexperienced Lewis would properly engage with the most relevant and experienced experts in the world that could have provided him extraordinary, insightful, and necessary scientific and medical guidance for the benefit of the scientific community and society. Even though this collective and unique expertise was readily available for Lewis, he would bypass this collective and relevant talent when it should have been obvious, certainly to Beadle and others on the Genetics Panel and the editor of *Science*.

Why would Lewis utilize the Genetics Panel and avoid the review of the Pathology and Medical Panel? While a final answer is uncertain, there was a strong measure of unease between the two Panels, as had been expressed by Muller on various occasions as early as the start of the BEAR activities. Muller was suspicious of AEC leaders like Shields Warren, Chair of the Pathology and Medicine Panel. This suspicion had history, was long lasting, and deeply personal for Muller. In 1947, Warren became the Director of the Division of Biology and Medicine of the newly created U.S. Atomic Energy Commission (AEC). The AEC was a regulatory agency that also had responsibility for assessing risks associated with radiation. By the early 1950s, it had become apparent that there was a conflict between the leadership of the AEC and key leaders of the US radiation genetics community, principally led by Muller, over the nature of the mutational/genetic risk in the low dose zone and its health implications. The AEC strongly supported a threshold dose-response model, while the Muller led radiation geneticists advocated strongly for the linear no-threshold (LNT) dose-response model (Calabrese 2015, 2019). At times, the debate between these groups became acrimonious as when Alfred Sturtevant, a future BEAR Genetic Panel member, published an

article in *Science* (Sturtevant 1954) harshly critical of the AEC, creating much public debate. Sturtevant was strongly supported by his colleagues based on letters from Muller, Curt Stern, and others (Calabrese 2019). The acrimonious debate came to a head when the AEC disinvited Muller to speak at the first Atoms for Peace Conference in Geneva in August 1955, only a few months prior to the start of the BEAR I Genetics Panel activities. The reaction of the scientific community and leading media outlets in the US was harshly critical of the AEC. In fact, Beadle (1955) wrote an editorial in *Science* supporting Muller and strongly criticizing the actions of the AEC. Since Warren was part of the AEC leadership and had been selected to chair the BEAR Pathology and Medicine Panel, there was considerable distrust that was directed to AEC risk-based actions. When the NAS placed Warren in charge of the Pathology and Medicine Panel, it likely sent an alarm signal to Muller, Beadle, and the other members of the Genetics Panel. In addition to this background, historically, geneticists including Muller, had previously been part of medical panels concerning radiation health effects in the United States. During that era, the medical community strongly favored a threshold dose-response model. This view conflicted with the linear view that dominated the geneticists (Calabrese 2015). In fact, the separating out of a Genetics Panel from the Pathology and Medicine Panel by Bronk, President of NAS, and Weaver, Director of Research of the Rockefeller Foundation, was the most strategic and influential decision in the history of the radiation risk assessment field. They knew in advance that the geneticists would advocate for a linear dose response that would be challenged and rejected by the Medical Panel. Given this history and the fact that Lewis was proposing a linear dose response perspective, it is not hard to see why Lewis might have been advised not to engage with the Medical Panel. Nonetheless, this was the group that had the most relevant education, training, and experience for providing high level peer review for Lewis.

Assuming that Lewis was a truth-seeking scientist who did not know the answer to his questions in advance, it would have been far better to learn of their perspectives before submitting the paper for publication rather than after publication. This is where the mentoring of Lewis by Beadle, and perhaps others, such as James Crow likely failed. The actions of Beadle and Lewis to avoid the challenge of the Pathology and Medicine Panel was no different than Muller demanding that the BEAR Genetics Panel ignore the new and compelling study of Neel. In both cases, the individuals

refused to engage with perspectives and data that might adversely affect their belief system. In this case, Beadle and Lewis were no different than Muller.

One must ponder how it was possible for Beadle, Muller, Crow, and others to have failed to properly guide Lewis, given his completely novice entry into this area and the significance of this activity. They had to realize that failure to seek out the guidance from experts on the Pathology and Medical Panel was not in the best interests of Lewis and the scientific community. Yet this is exactly what occurred.

One would have thought that the Lewis article would have been significantly challenged and improved with a rigorous peer review. However, the *Science* peer review offered little guidance to Lewis, with the Editor-in-Chief not requiring any author modifications.¹

It is important to note that of the 14 members of the BEAR Pathology and Medical Panel, only one direct response to the Lewis paper was published. It was by Austin Brues (1958) who originally proposed to write a letter to the editor, but “elected to expand it in view of the importance of the subject and the accumulation of new documentary material.”

Not surprisingly, Brues did not receive the endorsement of the editor of *Science* nor did he get any of the series of highlights that were afforded Lewis. It is also important to note that the Lewis paper has been cited about 2.5-fold more often than the Brues paper to the present time. In contrast to Lewis, Brues had much experience in radiation and health effects on humans, being a member of the ABCC since its creation in 1947 and also being President of the Radiation Research Society from 1955 to 1956.

Changing history

What would have happened had Lewis had the guidance, wisdom, or courage to take advantage of the opportunity to engage with the Pathology and Medicine Panel? We now know that each of the four target populations that Lewis chose to study had serious scientific concerns and limitations (Calabrese 2021a, 2021b), none of which were noted by Lewis, and that it likely would have caused a radical reframing of his article with fundamentally different conclusions. Yet the draft that was submitted to the Genetics Panel and the manuscript that subsequently submitted to *Science* did not result in correcting any of the serious flaws documented by Calabrese (2021a, 2021b) 70 years later (Table 1). This suggests that the peer review, as noted above, avoided the type of relevant

Table 1. The four lewis target populations.

Population	
Radiologists	Brues (1958) ridiculed Lewis' "low" dose methodological approach to estimate leukemia risks in radiologists, indicating that this group had received about 2,000 r over many years (Braestrup 1957), a value of no serious extrapolative relevance to low dose risk assessment. This criticism was emphasized by Mole (1958) who argued that Lewis' exposure values for the risk assessment probably underestimated exposure by up to 50-fold. Court-Brown and Doll (1958), leading epidemiologists, went even a step further by sharing their analysis on British radiologists showing that leukemia in this group displayed no enhanced risk. Of even greater public health importance, Court-Brown and Doll (1960) and Court-Brown et al. (1960) published a prospective study indicating that the incidence of leukemia in about 40,000 children whose mothers received diagnostic X-rays during pregnancy was not affected. In general, these exposures covered up to 5 rads, making the Lewis predictions on even less secure grounds. Mole (1958) and Kimball (1958) further challenged the analysis of the Japanese survivor data, pointing out that the massive dose uncertainty and an invalid representation of findings greatly exaggerated risks, resulting in a linear response, which was not justified. Each of the four areas developed by Lewis therefore felt the brunt of strong rebuttals from talented opposition.
Patients with ankylosing spondylitis	Lewis neglected to share that the authors of the ankylosing spondylitis article (Court-Brown and Doll 1957) stated in the preface of their article that due to the use of principally very high bone marrow doses "that until much more work has been done it will not be possible to decide between the alternative dose response hypothesis."
Children with enlarged thymus	The published manuscript of Lewis misrepresented the article he cited by Simpson et al. (1955) to support his LNT leukemia estimates for patients with an enlarged thymus. What Lewis failed to inform the readership of was that the authors rejected any causal relationship between the X-ray exposures and leukemia in these patients. Simpson et al. (1955) affirmed that there is "no definite conclusion to be drawn as to the relationship to the radiation exposures. This is particularly true of leukemia, which was apparently not associated with any one form of irradiation treatment with or without high radiation doses." Simpson et al. (1955) emphasized that no effect of radiation treatment "could be demonstrated in the case of leukemia."
Offspring of survivors of the atomic bombs	Lewis reported a linear dose response for leukemia in the Hiroshima and Nagasaki populations. He did this by a little trick. He combined the data of several low exposure groups. This resulted in missing the occurrence of a J-shaped dose response in all the studies for both cities from the start of reporting in the mid-1950s to the mid-1980s. The reported data contradicted the Lewis analysis. The J-shaped was reported in a <i>Science</i> journal article and later ignored. It was also discussed by the Pathology and Medical Panels and emphasized by ardent LNTer John Gofman (1981).

expertise of the Pathology and Medicine Panel. That is, the alleged peer review was grossly inadequate.

This story reveals that the scientific process is very susceptible to ideological manipulation, even at the highest levels of science, as reflected by the U.S. NAS creating a novel Genetics Panel stacked with ideologically focused LNT supporters and ensuring that its views garnered attention when information was released to the public as well as the now transparent ideological actions of *Science* journal. Yet because of the publication in *Science* and the endorsement by the editor, the external world of scientists, journalists, and the public trusted the findings, with a massive appeal to scientific authority, and still does.

Call for action

Upon review of this manuscript the *Journal of Occupational and Environmental Hygiene* Editor-in-Chief suggested the need to end this article with a type of call for action to the scientific community. Upon reflection, this article challenges editors to be fair in the review process by inviting reviewers that could offer insightful and helpful information and perspectives, something that was noticeably lost in the publication of the Lewis article. While this problem is likely widespread, in case of the Lewis article, it became critical since it had a profound effect on the scientific and regulatory communities worldwide that continues to the present. The non-research community blessings articles have had passed "peer-review" as the gold standard. The call to action is a challenge to science editors to be

committed in securing peer reviews from knowledgeable, fair, transparent, and constructive reviewers. The non-research community places a profound set of trust in scientists and our self-policing review process. It is apparent that the system is quite human, with all its fragilities. This call for action is a reminder that so much is at stake and that editors and reviewers are true guardians of society.

Notes

i. After the present article was submitted for publication consideration, correspondence of Lewis concerning radiation risk assessment was obtained from the CalTech library. Some of these letters impact the perspectives offered.

1. A December 10, 1956 letter from Shields Warren to Hermann Muller suggests that Warren had either received a copy and read it to some extent or was told about it. In the letter to Muller, Warren offered the following rather cryptic comment: "I would tend to regard Lewis's article as a very well-done exercise in logic, assuming certain premises, rather than as a definitive job." Exactly what did Warren mean with this statement? In my long academic publishing career I have never received a reviewer comment that praised my logic without commenting on my science. On April 27, 1957, about nine days after acceptance of the article and three weeks prior to publication, Lewis sent a copy of the accepted version to Warren, stating that he was now sorry for having ever shared a preliminary version, although he did not indicate with whom it had been shared.

2. A December 26, 1956 letter from Arthur Upton to Lewis stated the following as given immediately below. In addition, Upton's letter indicated a cc to Jacob Furth, a member of the BEAR Pathology and Medical Panel. Upton's letter stated as follows:

Your paper, "Leukemia and Ionizing Radiation," will make a valuable contribution to the literature. I have one critical comment to offer, concerning the final sentence in paragraph 9; namely, there is no evidence that I know of to suggest that irradiation of the bone marrow results in the myelogenous type while irradiation of the lymphatic glands and spleen leads to the lymphatic and monocytic types. On the contrary, although exposure of the bone marrow is presumably required for the induction of myelogenous leukemia, it appears from recent studies by Kaplan et al., Lorenz et al. and others that irradiation of lymphoid tissues alone is not sufficient for the production of lymphoid leukemia in mice (see the discussion of this problem on pages 14 and 15 of the enclosed manuscript, "Host Factors in the Pathogenesis of Leukemia in Animals and Man.")

3. A February 7, 1957, letter of Lewis was sent to Upton with a cc to Furth thanking for assistance.
4. In an important May 13, 1957 letter to Upton, Lewis apologized for citing and discussing information on radiation-induced leukemias in animal models. He stated that space limitations precluded the inclusion of this information. Thus, we now know with certainty that Lewis had been made aware of the Furth and Kaplan threshold supportive findings and chose not to cite it.
5. A letter on April 18, 1957 from the *Science* Editor-in-Chief DuShane indicated that the paper had been sent to at least two reviewers and was now accepted for publication. One of the reviewers had some comments and DuShane gave Lewis the freedom to modify the paper or not. Thus, it is clear that despite all the limitations of the Lewis papers that the *Science* Editor-in-Chief sent to reviewers who found little, if any, fault with the paper. In an April 22, 1957 letter to DuShane, Lewis indicated that he did make several changes to the manuscript based on the reviewer's comments.

Author contributions

CRedit: **Edward J. Calabrese**: Conceptualization, Writing – original draft.

Use of generative AI and AI-assisted technologies

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

Disclosure statement

No potential conflict of interest was reported by the author(s).

Data availability statement

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

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