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**From:** Edward Calabrese
**Sent:** Thur 11/2/2017 9:39:31 AM
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Ed

# Societal Threats from Ideologically Driven Science

Edward J. Calabrese

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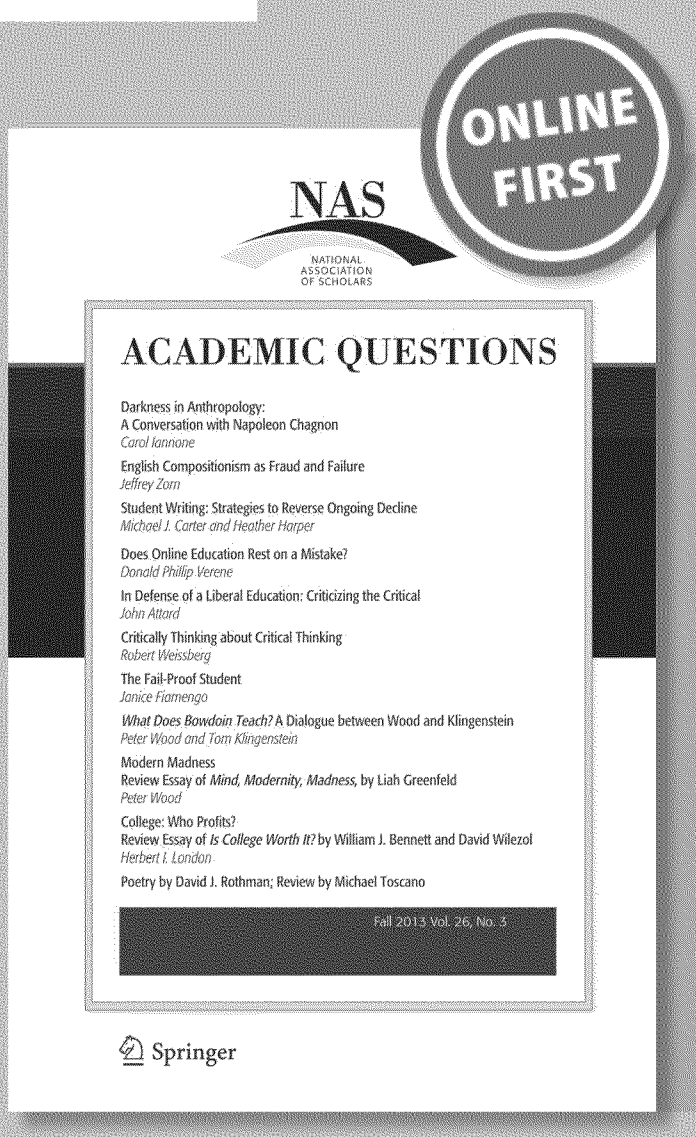


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UNSETTLED SCIENCE

# Societal Threats from Ideologically Driven Science

Edward J. Calabrese



I remember clearly my first week in graduate school in the entomology department at the University of Massachusetts Amherst in 1971. One of my fellow graduate students had just reported on a potentially important finding relating to a type of circadian rhythm, the twenty-four-hour cycle of biological processes that many organisms exhibit. The key observation occurred between 1:00 a.m. and 4:00 a.m. He was going to confirm his findings the following day. For reasons that were not shared, my advisor had some doubts about this "major" discovery and decided to be present at the lab between 1:00 a.m. and 4:00 a.m., along with the department chair (in retrospect, a bad sign).

When the student arrived at the lab the next day, my advisor asked whether the significant findings had been confirmed. The student acted very excited, claiming to have confirmed the result, and showed the data. The only problem was that during those early morning hours the student had not been in the lab, where my advisor and chair sat waiting and waiting to see him. The novel discovery proved to be a hoax, and in less than an hour the student had cleared out his office and was never to be seen again.

As for me, I got his office and an eye-opening education on honesty in science, life in general, and the consequences of unethical behavior.

I have never been too preoccupied with issues of honesty over the years because everyone I have worked with has seemed to be truthful about their science. Plus, we have tended to work in very close teams with multiple people checking what everyone else was doing. There have been many disagreements on all aspects of studies and data interpretation, but no challenges on the honesty issue. I have read William Broad and Nicholas Wade's 1982 book Betrayers of the Truth, all about

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fraud and deceit in science, but its stories and scenes seem to belong to a different world from mine. Most of the individuals caught in fraud appear to be in a mad race for some type of academic glory, whereas my life in science has been far more quiet, sedate, and mostly fun.

## Muller and the History of Dose-Response

The issue of honesty and deceit in science would reenter my life exactly forty years after my first week in graduate school. It all started very quietly. I had written a substantial review paper on the history of the linear dose-response, how it came to be accepted and used by regulatory agencies. The dose-response refers to the means by which drugs and other chemicals and physical agents, like radiation, affect biological systems and how this may be influenced by both the total amount and the rate of agent administered. As has long been my custom, I often send a copy of the draft manuscript to a group of knowledgeable friendly critics prior to journal submission. On this occasion one of the friendly critics, a very experienced expert in the area of genotoxicology, wrote that I had not explained the role of the Nobel laureate Hermann J. Muller and his significance in this area as deeply and insightfully as it probably needed to be covered.

He did not claim to be an expert on Muller, who was awarded the 1946 Nobel Prize in Physiology or Medicine for his discovery of the production of mutations by means of X-ray irradiation, but simply had a strong hunch that I was missing an important part of the story. Based on my respect for this person's past insights and help, this was more than enough for me to put the paper on hold to learn all about Muller's life and accomplishments.I obtained numerous articles by Muller, multiple articles about him, a substantial biography, and his December 12, 1946, Nobel Lecture. I even found a 1957 lecture he gave to other Nobel winners posted on the web. It was interesting to hear his voice, see his mannerisms, and follow his train of thought.

I started with Muller's earliest papers and followed his career until the very end. Then I read and studied his biography and Nobel Lecture. This method was expansive, since it also forced me to look at the lives of other leading scientists of his era who worked with him in one way or another in the area of radiation genetics. This study led me to the previously unexplored world of the history of science, especially the history of radiation, mutation, cancer, and public health. From reading multiple dissertations I came to appreciate the incredible depth and insight that such historians of science offer, and I was upset that I had never

really taken the time to learn about and from these efforts. So much had I missed! It was a bit like peeling an onion.

The story of Muller and his era became progressively more interesting and offered much insight into the scientific process. Little did my friendly critic know that his comment had reawakened in me a latent gene for the unrelenting search for historical truth. In fact, in my freshman year of college I had started out as a history major and then got so inspired by my zoology course that I switched to biology. Now it seemed that I was coming full circle.

I became particularly fascinated with parts of Muller's life because he was a professor at Amherst College (1940–1945), located in the town where I live, something I did not know. I tracked down the house he lived in, which was just a short walk to the college and about 1.5 miles from my home. I learned much about his work on the Manhattan Project with the famous geneticists Curt Stern and Ernst Caspari, and its impact on dose-response. My critic was correct: Muller was very important in the history of dose-response and risk assessment. In fact, I learned that Muller created the term "proportionality rule" in 1930 to describe the linear dose-response and played a key supportive role in the initial creation of the LNT single-hit model in the mid-1930s. The LNT dose-response model assumes that the response is directly proportional to dose down to a single molecule. In marked contrast, the threshold dose-response assumes that there is a safe level of exposure as long as the exposure is below the threshold dose.

## Manhattan Project and Dose-Response

While doing this historical digging, I noticed a potential disparity between what Muller stated in his Nobel Lecture and what I had come to learn about key findings in the mutation study of Caspari and Stern, on which Muller was a paid consultant. In his Nobel Lecture, Muller was quite emphatic that the threshold dose-response model was not scientifically credible and needed to be replaced by the LNT model for risk assessment. I found this very curious, since in August 1946 Caspari finished his major study on the effects of chronic ionizing radiation on mutations in Drosophila and found a threshold response.

The genetic damage component of the Manhattan Project was conducted at the University of Rochester under the direction of Stern. It represented the most significant research ever in this area. It had a very strong research team, improved quality control, large-scale studies, and excellent technical support, among other factors. While Stern, Muller, Caspari, and the rest of the Rochester team

were expecting that Caspari would confirm their belief in linearity, he didn't. In fact, just the opposite happened. His data demonstrated a threshold dose-response.

This was the proverbial fly in the ointment. Had Caspari's data supported a linear dose-response, it would have provided a major boost for the goal of replacing the threshold model with LNT held by Muller, Stern, and most others in the radiation genetics community.

This made me wonder whether Muller had seen the Caspari findings prior to giving the Nobel Lecture. I figured that he probably had not seen them since he never could otherwise have made the statement that he did about the lack of possibility of there being a threshold. Here was the best study to date, one in which Muller was an active and influential consultant and knew the quality of the people and research effort.

How could he ignore it, or worse still, dismiss it?

I needed to find out what Muller knew and when he came to know it. I contacted some historians of science and they had no insights on this question, so I ended up purchasing all the communication I could identify between Muller and all the Stern team members. Late one afternoon I received between six hundred and eight hundred pages of correspondence and related material.

I reviewed all the material that evening and found the so-called smoking gun. I learned that Stern had sent Muller the manuscript that he and Caspari had prepared on the study on November 6, 1946, after having alerted Muller in September to expect it. Muller acknowledged receipt of the Caspari manuscript and offered preliminary comments on it in a November 12, 1946, letter to Stern.

In the letter Muller acknowledged that these findings seriously challenged the LNT model, that the study needed to be replicated, that Stern needed to get the funds to do this, and that Caspari was a very competent researcher and that Muller could not dismiss the study due to inexperience or other reasons. Thus I knew for the first time that Muller had seen the Caspari findings one month prior to giving his Nobel Lecture and had an excellent sense of its significant implications, and that it could not be dismissed but needed to be repeated.

This new information troubled me. I put myself in Muller's position: If I were about to receive the Nobel Prize, could I ever state that there was no possibility that the threshold model was biologically plausible after seeing the Caspari study findings? In fact, his recommendation for a major replication directly contradicted this comment. The replication was not trivial and would take a year and require the help of multiple technicians, plus one as experienced as Caspari or Stern to direct it. I felt that the best I could do on this matter, if I were in Muller's situation, would be to acknowledge that the shape of the dose-response in the low dose zone remained a viable research question and needed to be resolved. I might have stated that while I believe that the

linear dose-response is most likely the correct view, this needed to be assessed further. And yet, while Muller acted like a scientist in his communications with Stern, in his public demeanor he was deceitful and very ideological —everything a scientist should not be. To act this way during the most significant moment in his professional life revealed important character traits in Muller, including those of dishonesty, risk-taking, manipulation, and arrogance.

I held out hope that he may have had new insights that led him to criticize the study and that would provide an explanation for his rejection of Caspari's threshold conclusion. However, a detailed seven-page letter to Stern dated January 14, 1947, reaffirmed the November 12, 1946, letter. With this now in hand I came to the firm but unsettling conclusion that Muller was deliberately deceptive in his Nobel Lecture and used this opportunity to achieve a long-dreamed-of goal to have LNT as the default model for cancer risk assessment. This was his chance and, apparently, the ends justified the means—again, a rationalization that scientists should never accept.

In 2012, I published this Muller Nobel Lecture story in the toxicological literature.[1] It quickly generated a series of criticisms, mostly ad hominem attacks on my character and research achievements. These were in part related to the fact that Muller could not defend himself along with other earlier defenders of the LNT model. These critics may not have been aware that Muller had himself criticized the work of a deceased scientist, Lewis J. Stadler, who had challenged Muller's gene mutation interpretations from 1931 until his death in 1954 and likewise could not respond to Muller's criticisms in 1956.

## Dose-Response and Deception

The deception issue would not end with Muller's Nobel Lecture, but would serve as the tip of even more troubling revelations. My initial follow-up was to make a detailed evaluation of the Manhattan Project's genetics/radiation research and see what I could learn from it. With respect to the Caspari research, I learned that Stern at first refused to accept the validity of these findings, claiming that the only reason that Caspari observed a threshold was due to a control group that had aberrantly high mutation rates that led to the threshold rather than linearity. To his credit, Caspari dug into the literature and presented convincing evidence that the control group was not aberrant but normal. To his credit, Stern backed

[1] Edward J. Calabrese, "Muller's Nobel Prize Lecture: When Ideology Prevailed over Science," Toxicological Sciences 126, no. 1 (2012): 1–4; "Muller's Nobel Lecture on Dose-Response for Ionizing Radiation: Ideology or Science?" Archives of Toxicology 85, no. 12 (2011): 1495–98; and "Key Studies Used to Support Cancer Risk Assessment Questioned," Environmental and Molecular Mutagenesis 52, no. 8 (2011): 595–606.

down—that is, the Caspari control was now considered normal. Did this mean that Stern gave up the effort to minimize the influence of the Caspari findings? Not in the least—but how did he do this?

It was subtle and it took both Stern and Caspari to do it, the latter oddly cooperating with efforts to undermine his own study, perhaps due to his sensing of what was important to Stern, his influential supervisor. First, a detailed reading of the paper revealed that essentially the entire discussion centered on why their data should not be accepted until it could be learned why this study showed a threshold, while a companion acute study lead by Warren Spenser completed a year before showed a linear dose-response. In many ways this was a false argument, since the two studies had more than twenty-five methodological differences and the issue could never have been practically resolved. They had to know this.

Second, the Caspari study was superior to the Spenser study in multiple ways: it was performed second, used better equipment and facilities, and improved temperature controls, among other features. In addition, much was learned during the Spenser study that was transferable to Caspari's efforts. Further, a detailed review of the Spenser study revealed a long list of problems that Stern, Muller, and others apparently never detected. All of these issues have now been documented, and some are serious.

The bottom line is that Stern and Muller did not want the Caspari paper to see the light of day, and if it did, they wanted to seriously compromise its impact. This view is actually reflected in Muller's January 14, 1947, letter to Stern.

The story gets even more intriguing as we now consider the attempt to replicate Caspari's findings. In fact, it gets much worse, as the historical record shows to what lengths Stern and Muller and others under their influence, or spell, would go to twist the truth to advance their ideology. Sometimes this resulted in direct lies, other times in data manipulation, censoring, and other forms of obfuscation and misleading behavior.

In the first replication study paper, for example, Stern and Delta Uphoff, a master's student at the University of Rochester, concluded that her control was aberrantly low and that this led to data that could not be properly interpreted. This was based on extensive written communication with Muller. Muller had a massiveamountofcontrolmutationdatainstudiesdealingwiththeaforementioned dispute with Lewis Stadler on the nature of gene mutations. In multiple letters that I obtained, Muller unequivocally supported the Caspari control as normal and the Uphoff control as aberrant. This write-up was sent to the Atomic Energy Commission by Stern and was classified. When Stern published the

findings a year later, he and Uphoff neglected to inform the scientific community that one year earlier the data that they were now publishing had been uninterpretable (their own written characterization) and that her control group was aberrant based on the data in the published literature and in Muller's massive database.

A second example involved Muller writing in the scientific literature that the study by Caspari that challenged LNT should not have credibility because of its aberrantly high control group values. Of course, he had the data to support the Caspari findings and had done so in writing in a series of letters with Stern. Despite the duplicity of Muller on this issue, he was never challenged by Stern or Caspari—even though they knew that Muller had directly contradicted his letters to Stern and his publications.

## The National Academy of Sciences and LNT

It would be bad enough if the story stopped here, but it didn't. It became even worse. The next noteworthy developments occurred when the National Academy of Sciences (NAS) created its Biological Effects of Atomic Radiation (BEAR) I Committee in 1955 and announced its seminal recommendation to switch to linearity in June 1956. This was actually the big ideological payoff for all the past efforts to ensure the success of the LNT. It represented collusion, I should say, inbreeding at the highest levels: the Rockefeller Foundation funded the BEAR committee; Detlev Bronk, president of the NAS, was also president of the Rockefeller Institute for Medical Sciences (later Rockefeller University); and Bronk chose the chairman of the Genetics Panel from the Rockefeller Foundation.

Transcripts reveal that the chair was enticing the panelists with more Rockefeller grant money. The goal was to get the scientific community and the public to go linear, simple as that. For this to happen, data had to be censored. In addition, the Genetics Panel had to show that it was in close agreement on the scientific basis of radiation-induced mutation risks, which their individual estimates were designed to show. However, the panel was so split in their scientific conclusions of radiation-induced mutation risks that if they were shared with the public, the policy recommendations of the panel would have no credibility—or so the panel members, such as Jim Crow, a University of Wisconsin professor of genetics, strongly believed and wrote about in correspondence with Chairman Warren Weaver.

## My Conclusions and Their Consequences

The findings to support my conclusions have been published in considerable detail.[2] They reveal that the Genetics Panel misrepresented the research record in the journal Science on several key matters, all of which were needed to get their policy views accepted. The panel voted on these matters, including deciding not to show their data and not to provide any written justification for their conclusions. Thankfully, these highly prestigious scientists preserved their correspondence reports and notes, which permitted me to discover their deceptions—both as individuals and, more surprising, as an NAS committee—and eventually piece this story together. The 1956 NAS BEAR I Genetics Panel report and its LNT recommendations would become the most significant document in the seventy-year history of cancer risk assessment. The acceptance of their guidance is the historical basis of why the U.S. and numerous other countries adopted the LNT. As the twig is bent so grows the tree.

Two years ago Jerry Cuttler, an active researcher on LNT and radiation, wrote to Marcia McNutt, editor-in-chief of Science, to request that the 1956 article of the BEAR I Genetics Panel be retracted due to my documentation of its deliberate misrepresentation of the scientific record and the major and continuing historical significance of this paper.[3] The situation was complicated from the start, since McNutt was also a finalist to become the next president of the NAS, and her name was already posted on the NAS website as such.

In such a situation, McNutt should have recused herself from deciding on this issue. Since the then-outgoing NAS president Ralph J. Cicerone was strongly disputing my challenging papers at the time, McNutt's conflict of interest with deciding upon the retraction request and her desire to become the next NAS president is obvious. Yet despite her finalist status—and she did become NAS president—McNutt did not recuse herself. Her decision was to deny the request. (The appendix to this article contains three key e-mail exchanges on this issue.) It was also disturbing that no apparent set of checks and balances existed within Science's organization to ensure proper oversight on such matters.

The story of LNT, therefore, is one of leading scientists, from the time of Muller's Nobel Lecture in 1946 to today, being driven by ideology and/or self-interest. This should not be who we are as scientists, nor what we should accept.

[2]Edward J. Calabrese, "On the Origins of the Linear No-Threshold (LNT) Dogma by Means of Untruths, Artful Dodges and Blind Faith," Environmental Research 142 (2015): 432–42.

[3]Edward J. Calabrese, "LNT gate: How Scientific Misconduct by the U.S. NAS Led to Governments Adopting LNT for Cancer Risk Assessment," Environmental Research 148 (2016): 535–46.

## APPENDIX

Marcia McNutt, e-mail message to Jerry Cuttler, August 11, 2015
Subject: Science Paper, Genetic Effects of Atomic Radiation; Evidence of Scientific Misconduct

Dear Dr. Cuttler:

We considered carefully your concerns about the controversy with respect to the linear no-threshold (LNT) dose-response model for assessing the risk of radiation-induced cancer. You have requested that Science retract a 1956 paper that takes a position on this issue. Standard practice in Science and other journals would be not to consider the retraction of an article more than just a few years old except in extraordinary circumstances. New discoveries are constantly advancing the frontiers of science, and unless we had some statute of limitations on retractions, we would be constantly retracting old articles after the field has moved on. We can imagine certain exceptions in cases of papers that are still highly influential. In considering this specific request to Science, we asked the following questions:

(i) Is the 1956 Science paper trustworthy? We concluded that we cannot produce the information we need to answer this question 60 years post publication to the standards that would be required to consider a formal retraction. The authors are no longer living. We do not even have a record of the Science editorial standards of that era, much less a review jacket for that paper. This case is so old we would never be able to reconstruct the evidence from all parties involved in our editorial decision.

(ii) If the paper is not trustworthy, is the matter a problem of scientific quality or scientific integrity? Because we cannot answer (i), we cannot answer (ii). However, I will note that many of the concerns raised in the Calabrese paper would fall under the classification of science quality, not science integrity. They would not be grounds for retraction of a paper 60 years after the fact.

(iii) Does this Science paper still have the "pervasive influence" claimed in the article by Calabrese? We consulted an independent expert whose positions indicate that s/he has no extreme positions on this matter, one way or another. His/her considered view is that the 1956 Science paper was one of hundreds of papers over the past half century on this broad topic, and certainly the use of the LNT model by almost all the regulatory agencies, world wide, is now based on a lot more than the NRC report and Dr. Muller's work. For example,

> if you take a look at the series of NRC "BEIR" [Biological Effects of Ionizing Radiation][4] reports, in the more recent ones there is no particular emphasis on Muller's work, with the arguments now more based on endpoints that more directly relate to radiation-induced cancer.

Based on this analysis, we do not see any reason to consider revising our policy for this paper. Science considers this case closed and will not reconsider the decision.

Dr. Marcia K. McNutt
Editor-in-Chief, Science family of journals
American Association for the Advancement of Science
1200 New York Avenue N.W.
Washington, D.C. 20005

Edward A. Calabrese, e-mail message to Marcia McNutt, August 19, 2015
Subject: NAS 1956 Paper Retraction

Dear Dr. McNutt:

I read your e-mail letter to Dr. Cuttler, rejecting his request (and others) to retract the NAS BEAR I, Committee Genetics Panel published in Science in June, 1956, due to its multiple incidents of serious falsification and fabrication. I have carefully studied your five reasons for this decision.

While I commend you for your directness and transparency in sharing the basis of the decision, I have concluded that your analysis of the issue was faulty on each of the five reasons (see attached or below) and contradicted by the factual record in a number of cases. While I know you wrote that the decision was "final," I hope that you will be open to the new analysis and that you will reconsider this issue.

Sincerely,
Edward J. Calabrese, Ph.D.
Department of Environmental Health Sciences
School of Public Health and Health Sciences
University of Massachusetts
Amherst, MA 01003

[4]See, for example, National Research Council of the National Academies, Health Effects of Exposure to Low Levels of Ionizing Radiation: BEIR VII Phase 2 (Washington, DC: The National Academies Press, 2006).

Issue #1: Is the situation extraordinary given the 60 year time lag?

The situation is extraordinary because the LNT model for cancer risk assessment continues to dominate all regulatory agencies, affects clinical treatments, environmental regulations, clean-up costs, medical treatment strategies, all needlessly wasting massive resources. In fact, it is widely believed that the recommendations by the NAS BEAR I Committee, Genetics Panel to switch from threshold to the LNT model was the most significant event in the history of risk assessment. It is also extraordinary because substantial contemporary toxicological discoveries have revealed serious failings with the LNT model with findings more consistent with the threshold and hormesis models.

Issue #2: New discoveries are constantly advancing the frontiers of science:

Contrary to your statement, my letter did not challenge an older paper (i.e., NAS Genetics Panel Science paper, 1956) based on new discoveries such as DNA repair, adaptive responses, apoptosis, and hormesis that could create non-linear dose-responses. It is, however, challenging this paper because it falsified and fabricated the research record and it continues to affect, in significant ways, the beliefs and actions of regulatory agencies, influential governmental and non-governmental organizations, educational institutions, materials and practices, and leaders in the risk assessment field—all without their knowledge that the Genetics Panel paper in Science is now recognized as being based on fraud and deception.

Issue #3: Is the Science paper trustworthy? You claim that this is not knowable because: new standards for evaluation; because the authors are not alive; and the 1950s recordkeeping is poor and without knowledge of how this paper was reviewed.

The issues of falsification and fabrication are historically founded and have long been addressed by professional standards in the sciences and their journals. My published articles have shown that the research record was deliberately altered in the Science paper by the Genetics Panel and I possess and cited the text of letters and memos documenting the scientific misconduct and the reasons why the falsification/fabrication was done. The fact that none of the Panel members are alive is adequately compensated by the factual record which is substantive and unequivocal, with high internal and external consistency. It is not significant to the present case whether the Genetics Panel paper in Science received a peer review, as most reports by high level advisory committees are usually stand-alone and not subject

to standard peer-review processes, as are papers of individual scientists. Nonetheless, all papers need integrity and honest reporting. My published papers have shown that the BEAR I Genetics Panel failed in this regard in multiple and critical ways, affecting key conclusions and acceptance of their findings by the scientific community, governmental agencies, and the general public.

Issue #4: Is the problem one of scientific quality or integrity?

You do not provide any specific evidence, but offer a general statement that many examples cited in the Calabrese (2015) paper concerned scientific quality rather than integrity. The fact that there were important issues raised about scientific quality (e.g., the obvious description of Jim Crow's research method) does not detract from the integrity issue. The key point is that it was because of the poor data quality that the Panel decided to cover up their scientific weaknesses (i.e., poor quality) so that their goal of a switch to LNT could occur. The central issue is that the Panel was not honest and altered the research record to promote this goal. I suspect that if the data quality were good, they would not have "needed" to lie and deceive. However, their LNT goal was more important than truth.

Issue #5: The continuing "pervasive influence" of the 1956 paper:

You cite an unnamed knowledgeable independent consultant who told you that the LNT is now based on many more papers than the NRC report and Muller's work. First, the Calabrese (2015) paper never states that the LNT was based on Muller's research. It states that Muller used his influence to promote acceptance of the LNT by being dishonest in his spoken and written words, all of which were documented. The paper traced the initial acceptance of the LNT to the work of Curt Stern and his students and these were highly criticized in the Calabrese paper. It was the Stern papers that the BEAR I Genetics Panel based their beliefs upon and cited in subsequent Congressional testimony (1957). You stated that the more recent BEIR reports do not base their recommendations on Muller's work and focus now on cancer. In multiple papers I show that within one year of BEAR I, that major advisory groups had generalized the Genetics Panel recommendation from genetic risk to cancer risk assessment. We have also documented that the U.S. EPA in the late 1970s specifically relied on the BEAR Genetics Panel 1956 recommendation when it adopted LNT, showing clearly that your assertions are incorrect. More specifically, Roy Albert, Chair of the EPA Carcinogen Group, in his 1994 paper in Critical Reviews in Toxicology, has reported that EPA adopted the LNT model of the Atomic Energy Commission (who

adopted the BEAR I, Genetics Panel report) that had been applied to estimating risk for fallout from atomic weapon tests. He stated that it was clear, simple, and easily understood and was plausible based on the linearity of the mutation response (see BEAR I) within the framework of target theory. He then noted that "any difference between chemical carcinogens and ionizing radiation could be waved aside as both cause genetic damage." Thus, the BEAR I report in Science served as the critical foundation for the current EPA LNT cancer risk assessment.

A vast number of published papers with experimental data contradict the LNT model. In fact, the mega-mouse (24,000 mice) study of the FDA to estimate the shape of the dose-response in the low dose zone showed a striking hormetic dose-response for bladder cancer as emphasized by a 14-member expert panel of the Society of Toxicology Detailed Japanese studies with DDT showed clear hormetic dose-responses for carcinogenicity. Numerous whole animal cancer bioassays with ionizing radiation show reduced cancer risks and life extension at low doses in multiple models. These and numerous other findings, along with the above conceptual developments (DNA repair, adaptive response, etc.) all happened after BEAR I. If anything, the LNT model decision should have been reversed except for the ideological grip that has long enveloped this field.

In summary, this response addresses each issue that your letter used to support your rejection of the request to retract the NAS 1956 Science paper due to research misconduct. The evidence presented here provides an objective basis for you to reconsider the proposal to retract the 1956 NAS Genetics Panel Science paper. The evidence is convincing that misconduct did occur, and the issue is too important to continue to ignore. Science has a professional and moral responsibility to correct this continuing scientific deceit.

Marcia McNutt, e-mail message to Edward A. Calabrese, August 19, 2015
Subject: NAS 1956 Paper Retraction

Dr. Calabrese:

I happened to be at a large gathering of distinguished scientists today, most of whom have published in Science, and I asked them the following question:

"Do you believe it would be permissible for Science to retract your paper (or any other researcher's paper) based on evidence put forth by a third party claiming

scientific misconduct, without allowing you the opportunity to rebut the claims?"

There was not a person who believed that it would be appropriate or ethical for Science to retract a paper under those circumstances. Examples that were given by this distinguished group for why due process needed to be given to both sides before action is taken included:

- Possibility of conflict of interest on the part of the third party;
- Situations in which so-called "evidence of misconduct'" was taken out of context and either misinterpreted or purposely misrepresented;
- Limited knowledge of third parties as to the entire story; e.g., believing that a result was based on X when it was based on Y.

You obviously answer "yes" to the question above, otherwise you would not continue to press this issue, but you are the only person I have encountered so far of that opinion. Science will not be changing its policy.

Please respect that the matter is closed.

Sincerely,
Marcia McNutt
Editor-in-Chief, Science family of journals
American Association for the Advancement of Science
1200 New York Avenue N.W.
Washington, D.C. 20005