Paper

Confessions of an LNT Heretic: What Happens When You Challenge Biomedical and Environmental Dogma: Rebuttal of Beyea Commentary 2024

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Abstract—**This article provides both a personal reflection concerning major professional and scientific challenges that can occur when evaluating the concept of hormesis and the historical foundations of cancer risk assessment/linear non-threshold (LNT) dose-response model and a detailed response to a recent critic. The assessment briefly captures what happened when an established mid-career scientist (i.e., the author) somehow went "astray" and challenged the central dose-response dogma of the scientific community "establishment" and regulatory agencies. It highlights what can and did happen to me when I was perceived to be a threat to vested interests; and how professional forces became animated, mobilized, and organized to marginalize me and my ideas/research to destroy my reputation and even get me removed from my position of having a tenured university full professorship.**

This historical background and personal story provide insights on their own but also necessary context when addressing new or recycled criticisms generated out of a mixture of legitimate scientific questions and/or ideological bias. There are also deep and vexing frustrations due to my substantial successes in the hormesis and cancer risk assessment areas. This framework provides a necessary backdrop to address recent criticisms of the Health Physics Society (HPS) documentary, *The History of the Linear No-Threshold Model*, and my publications on this topic that were the principal foundations for this documentary. In brief, Beyea's evaluation in the *Health Physics Journal* (*HPJ*) gives the impression that it is a broad evaluation of my research on the historical foundations of cancer risk assessment. Yet, Beyea addressed only a very limited set of discoveries that were discussed in the documentary. However, there have been numerous significant discoveries (about two dozen) published since the release of the documentary that he failed to acknowledge. All the new discoveries support, greatly enrich/extend, and do not contradict any aspect of the documentary.

In addition, the Beyea assessment represents a recycling of information from his previous two highly compromised papers of nearly a decade ago. These papers, including the present one, display his confirmation bias that is strongly associated with his failure to use primary source materials, a source of novelty and significance in my historical research on cancer risk assessment. Failure to use primary sources greatly diminishes the historical and scientific value of the Beyea article, making it susceptible to secondary source opinion misinterpretations, frank errors, and bias, as repeatedly shown herein. In addition, Beyea attempts to damage my personal and professional standing/reputation, possibly violating ethical guidelines of the *HPJ*. Thus, the scientific basis for his comments is generally trivial and often devoid of historical foundation and accuracy, while failing to be representative of my body of work over the past two decades. The failure of Beyea to use a primary source-based research methodology, especially when such documents are copious, generally available, and essential for historical research on cancer risk assessment, represents a fundamental flaw that should have been addressed and corrected in the peer review process. Failure to do so led to the publication of a historically flawed and unreliable paper and calls into question the fairness of their review process.

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INTRODUCTION

As an introduction to the present paper, I document my history with the concept of hormesis and the historical foundations of cancer risk assessment/linear non-threshold model (LNT) and how I became embroiled in expansive controversies continuing for more than three decades without stop, career threatening actions, and other conflicts. This information will better frame the context of the Beyea (2024) paper and my response to it.

I have been a faculty member at the Universities of Illinois/Chicago and Massachusetts/Amherst, Schools of Public Health, Department of Environmental Health Sciences now for over 50 y, with the vast majority at the University of Massachusetts/Amherst (UMass). Over my first nearly 20 y, I developed a well-funded and productive toxicology laboratory and became substantially involved in significant epidemiological studies (e.g., assessing the effects of elevated levels of sodium in drinking water on the blood pressure of elementary school children) (Calabrese & Tuthill 1977; Tuthill & Calabrese 1981).

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We also estimated the amount of soil that children ingest (Calabrese et al. 1989; Stanek et al. 2012), with our values becoming the foundation for US EPA exposure estimates that were also applied in most other countries, along with the discovery that there was an increase in miscarriages in those employed in the manufacture of computer chips in so-called clean rooms (Pastides et al. 1988). Our findings received worldwide attention and led to major changes in cleanroom work activities and materials used. My toxicology laboratory research focused on the concept of differential susceptibility/interindividual variability to toxic substances; that is, why some people (i.e., high risk groups) become adversely affected at doses that others apparently don't (Calabrese 1996a, 1996b). This was studied via the use of animal models that mimicked various human disease conditions. In addition, I wrote numerous books from the late 1970s to the mid-1990s on various aspects of high-risk groups and differential susceptibility dealing with factors such as age, sex, diet, and genetics; how to extrapolate animal studies to humans; and how to predict effects from complex mixtures (Calabrese 1978a, 1978b, 1980, 1981, 1983, 1984, 1985, 1986, 1991, 1996a, 1996b; Calabrese & Dorsey 1984; Calabrese & Kenyon 1991).

These efforts led to my being appointed to numerous National Academy of Sciences (NAS) expert committees, the NATO Countries Safe Drinking Water committee, and numerous other similarly high-level advisory committees. I was a member of the NAS panel that voted to end smoking on commercial aircraft in the mid-1980s. I also developed the first and only single exposure carcinogen database over the decades of the 1980s/1990s based on 6,000 published experimental studies (Calabrese & Blain 1999). I was invited several times to share this database and its applications with various NAS Committees, including one addressing acute exposures. This research became a central feature that guided some of their critical recommendations. I was also part of a group of four scientists (which included John Doull, one of the true fathers of toxicology in the US) who were invited by EPA and the American Water Works Research Foundation to undertake a 28-city speaking tour to many leading federal and state environmental/public health organizations, most leading groups in the water works industry, several major research universities, and a number of large conferences related to EPA's then newly proposed rules for risk assessment of volatile organic contaminants. In the course of these and other achievements, UMass accelerated my tenure decision and promoted me to full professor within 6 y instead of the normal 14 y, a clear sign they were more than pleased with my progression.

My life, however, would change in a rather profound way early in 1985 when I received a brochure concerning a conference on radiation hormesis scheduled for August in Oakland, CA. The brochure described radiation hormesis as a phenomenon that displays biphasic dose responses, with low doses being stimulatory and generally (but not always) protective while higher doses were just the opposite. This description caught my attention as I had seen this biphasic dose response in my first research experience as an undergraduate in 1966 in a plant physiology course, working with a synthetic plant growth inhibitor. I got immediately interested and turned this observation into a research obsession, making it faculty-guided research, and conducting about 40 substantial experiments that showed a biphasic dose response consistently when the plants were grown in soil or hydroponics. We published a paper on this research, never knowing the term hormesis (Calabrese & Howe 1976).

Soon after I received the conference brochure, I called the conference director, Leonard Sagan of the Electric Power Research Institute, to tell him of my earlier plant research that I now thought may have been an hormetic response. He then invited me to develop a paper on chemical hormesis and offered to pay my travel expenses and waive the registration fee to attend/participate in the meeting. This conference paper renewed my interest in biphasic dose responses and the concept called hormesis. I became interested in how significant the concept was, how generalizable it might be, and its public health and medical/therapeutic implications. Furthermore, I was quite interested in how it might impact environmental risk assessment, which was using the linear model for carcinogens and the threshold model for non-carcinogens. I began to wonder whether these models that had been long used in risk assessment had ever been adequately tested and validated, even though I had fully supported their use in my past books, articles, and as a member of various high-level committees. I did not think that by questioning the validity of the linear or threshold response models that I was being anything other than a scientist. How "wrong" I was to ever question what would amount to perhaps the central dogma of many powerful and politically active environmental health scientists and toxicologists in and out of government. It was amazing to see how quickly and brutally the field can act to prevent "deviation." Many of these establishment scientists acted as if they were not free to question beliefs or past judgements about the nature of the dose response in the low-dose zone. It appears that they viewed this issue more ideologically, as a type of job/grant security based on an environmental-based precautionary principle religious tenet. Somehow their training in graduate school to challenge all assumptions was thrown out of the window when it came to the central dose-response dogma of toxicology and risk assessment.

I saw the issues of the nature of the dose response in the low-dose zone and hormesis as critical questions

needing to be addressed and better understood. I sought funding to explore the topic from multiple sources, with initial funding from several federal agencies and a number of private-sector groups, all within the UMass academic framework, governance, and oversight. It was simply one of four distinct lines of externally funded research in which I was actively involved at that time. This is how the academic research game works: external funding governed via university rules/procedures is the key.

During the decade of the 1990s, we constructed a large hormesis database to assess the generalizability of the hormesis concept and the range of its applications. By the end of the 1990s, these efforts led to the receipt of multiple large grants and the beginning of a long series of papers. In 2003, a major event happened when I published a paper in *Nature* that was placed into that journal's publicity package (Calabrese & Baldwin 2003). This led to numerous newspapers, magazines, and other outlets writing stories about me and the hormesis concept. Nearly overnight I became well known, the center of this activity. Even the journal *Science* did a five-page story on hormesis and put my picture in the issue (Kaiser 2003a, 2003b). *US News and World Report* did its own major story, again with a big personal picture (Boyce 2004). The *Wall Street Journal* also did its own major story (Begley 2003). Very quickly the hormesis concept was taking center stage. For the first time, my work on the concept of hormesis was being taken seriously, including what it could mean for biology, medicine, public health, and the EPA and its risk-based actions and regulations. It is interesting to note that while the single exposure carcinogen database was developed in parallel with the hormesis data base, the hormesis concept attracted major funding, whereas the single exposure concept (Calabrese & Blain 1999) did not attract media attention and significant funding. This resulted in me following the academic hormesis funding opportunity, leaving behind the single exposure work after two decades of nurturing.[1]

With this positive publicity came the awakening of a vast opposition to the hormesis concept and me. Hormesis was soon seen by such ideologically oriented scientists more as a threat to the past 30 y of EPA regulation and risk assessment practices than an interesting scientific concept/issue. An organized effort began to oppose many of the articles that I published on hormesis, either by publishing opposing articles or writing letters to the editor. In some cases, the tone and wording were very aggressive, with the beginnings of attempts at character assassination, some of which were caught in time by journal editors to be modified, while others were published as-is. Within this context, an effort to create an environmental ethic that had hormesis and me as its target was published (Elliott 2006). The proposed environmental ethical standard would be applied to only one paper published (Calabrese & Baldwin 2003), with the author (Elliott 2006) also designating himself as the sole judge and jury concerning whether our paper passed or failed his ethical standard. Of course, I failed and was guilty as "charged." This grossly unfair and clearly "targeting" paper (Elliott 2006) required a rebuttal (Calabrese 2007), one of a long line of such defenses and explanations that I have made over the course of the past 30 y.

Suddenly, I had become the face of a morally diminished scientist, an academic who had taken a wrong turn in professional life, lost his way, compromised his ethics, and needed to be challenged, put in place, and if that didn't work, then ridiculed or worse. Very quickly I went from being part of the insider establishment and a member of many of the major committees to a person who had to be challenged, attacked, discredited, and marginalized. The opposition was threatened by the hormetic dose-response discoveries and their implications that had been emerging with our papers. I also noted that my submitted manuscripts often received six or more reviewers instead of the normal two to three reviewers, and this happened in multiple journals. It had several effects: while it did lead to many more initial rejections, it also had the ironically positive effect of improving most papers, with all sorts of helpful comments/criticisms that I needed to address, making my subsequent hormesis papers more effective and far better contributions to the field.

An extremely troubling sign of the strategy and tactics of the opposition occurred at an hormesis research group meeting in my office. One of our biostatistics faculty team members, from the Math/Stat Department at UMass, publicly told our group of 14 people that he was quitting our research group because he was told that if he kept working with Calabrese on the hormesis project, he would likely not get tenure the following year. This was a first for me at UMass. I quickly learned that underground ideologues existed and were not afraid to strike fear--and they were good at it. According to this biostatistics professor, the Calabrese research was said to be undermining the past gains to protect the environment and public health and would result in increased risk of harm. The Calabrese research on hormesis was not to be supported or tolerated. So, the young biostatistics assistant professor quit our group, moved on, and did get his tenure. There are many other similar stories of either blatant professional bullying or personal attacks, or the hidden ones that the untenured biostatistics professor shared. I knew for sure now that I had many opponents and some vicious enemies. Some of these enemies were close to home, being

[1]I have spent considerable time exploring the scientific boundaries of how my hormesis, single exposure carcinogen and my LNT research tracts fit together in a scientifically coherent manner.

part of the UMass faculty and possibly even its administration, and they would look for ways to threaten my professional activities/success. If they would go so far as to threaten the future of this untenured assistant professor with two small children, what would they attempt to do to me, given the chance? I knew then that my life would never be easy at UMass or probably at most major academic locations, but the work on hormesis on which I had embarked still remained fascinating to me, and the science was fun. Within the same UMass context of unseen opposition, just a few years ago an article/interview in *The Huffington Post* with a UMass colleague in my building (and my actual next-door neighbor) indicated that over a long period of time he had organized articles and letters-to-the-editor to oppose and challenge many of my hormesis papers, which he thought were simply wrong-headed. He never was one of the co-authors of the challenging publications; he simply remained hidden in the UMass swamp, sticking needles in a voodoo doll and waiting to see my reaction. This story was shared with *The Huffington Post* soon after he had retired and moved. Such is life when challenging the central beliefs (Thacker 2019).

In 2010, as I was recovering from a broken pelvis suffered in a bicycle riding accident, Professor Shrader-Frechette from the University of Notre Dame wrote a blatant attack article about me and my hormesis research, even putting my name in the title in a highly disparaging way (Shrader-Frechette 2010). She would not stop there as she placed my name in the title of two additional papers (Shrader-Frechette 2012a, 2012b). I have never before, or since, seen this done in a professional domain. Shrader-Frechette had previously attacked (but never had placed their names in the titles of her papers) Sir Richard Doll, the discoverer of the smoking and lung cancer association for which he was knighted, and Bruce Ames, of Ames Test fame and a recipient of the US President's National Medal of Science (Smith et al., 2021), for some of their industry related consulting efforts. Now I had made it to that rare level with the likes of Doll and Ames. However, I did them one better by getting my name in the title not once, but three times. It should be noted that the highly flawed and judgmental environmental ethics paper of Elliott (2006) cited earlier was written by one of Shrader-Frechette's former Ph.D. students, showing that the apple did not fall far from the tree. The Shrader-Frechette paper was then shared with my then Vice Chancellor for Research and my then Dean via an anonymous single paragraph letter from someone in the UMass community requesting that I be subjected to a major "trial" for possible ethical and research integrity issues, with the outcome possibly being the loss of my position after some 35 y at UMass in good standing. This is an experience that I don't recommend. After many months of review, evaluation, and meetings with lawyers from the University President's office, I was finally "vindicated" and allowed to survive and thrive. The official records were to be held for 7 y following legal requirements and then destroyed. Several months before the 7-y period ended, I requested a copy of the complete file but was told that it already had been destroyed, apparently violating the 7-y rule.

During this period, I had been exploring the history of the dose response. This eventually led to my doing a deep dive into the life and science of Hermann Muller, the Nobel Prize recipient. I was trying to write a major paper on the history of the dose response, integrating hormesis with LNT and threshold and was told by a friendly critic that I needed to explore the life and scientific accomplishments of Muller in greater depth in order to do justice to this topic.

With this insightful but gentle nudge, my academic focus on Muller began. It was all that simple. I took to heart the suggestion and put all else aside, trying to read everything written by and about Muller, a massive effort. It became obvious that detailed study of Muller was essential and rewarding. These efforts started in 2008 and led me into the world of the history of science and the discipline of professionals in that field, their sources, and their methods. I educated myself to their trade in order to better understand Muller, and it proved invaluable. I discovered that Muller was deceptive/dishonest at his Nobel Prize speech in December 1946, promoting the linear model at the expense of the threshold model for radiation-induced gene mutations. My two papers on this topic that challenged Muller's veracity and character (Calabrese 2011, 2012) generated a new army of highly talented and aggressive opponents, much like the anti-hormesis crowd. It started with the two biographers of Muller, who made it clear that not only was I wrong but that I was mean spirited and unfair, not in Muller's league as a scientist. I was the problem, not Muller. A writer from The Netherlands interviewed me in depth on the Muller Nobel Prize story and had a contract to have his article published in *Science*. When he sent it to the *Science* editor (I have a copy), the editor forced him to totally rewrite it, with me no longer being the interesting and groundbreaking professor but one who had lost his way and his ethics (Crok 2011). All this happened while my fate was being decided by the UMass administration.

An example of ideological hegemony may be seen in the following bizarre vignette. In December 2017, I was invited via a telephone call and a follow-up confirming e-mail to give a Plenary talk for a Special Session sponsored by the American Academy of Health Physics (AAHP) at the forthcoming 2018 annual meeting of the Health Physics Society (HPS) in Cleveland, OH. All my

travel, lodging, and registration fees would be waived. I was excited because this was a clear sign that my papers on the historical foundations of cancer risk assessment were resonating throughout the radiation community. Furthermore, giving an invited Plenary presentation by one who was not a member of the AAHP or HPS was a special honor and privilege. However, about 6 wk later, my excitement came to an abrupt end. I received a telephone call from the person who invited me, now disinviting me. He told me he was sorry, but there was an administrative change or something. The bottom line was that I was now out. There would be no Plenary talk for me, no all-paid trip to the conference. I did not believe the explanation since I had directed too many conferences and had invited too many Plenary speakers. The simple truth is that Plenary speakers almost never get disinvited. I was far too experienced to fall for that story. In fact, the situation is just the opposite. The conference organizers are so pleased when the Plenary speakers accept. I then spoke to his boss and then to his boss's boss, but all gave the same answer. I was told that I was out and not to call back again.

As bad as this story was, I wanted to know the truth, and 4 y later the truth emerged unexpectedly from a Freedom of Information request by me. I obtained emails of prominent members of the HPS, who just happened to be leaders in the Centers for Disease Control (CDC), the National Committee for Radiation Protection (NCRP), and EPA. I learned that three AAHP/HPS leaders somehow realized what a "danger" I was to their society and the world, and they made up the flimsy administrative glitch excuse. They also got one of their members (i.e., John Boice, the long-standing president of the NCRP) to replace me. Now John would be the Plenary speaker, not me. This act was simply another in a long line of professional decisions/actions to marginalize me and my research, a job undertaken by a self-appointed AAHP/HPS group. It was effective, bloodless, and perfectly seamless. Only I knew I had been taken down in a professional sense. Yet the sad truth behind this story was reported by Cardarelli (2024) based on my obtaining the primary documents, the now damning emails of the group. The group was successful, but now they had been exposed. The major point of this tale is that I have been the target of powerful yet very talented and beloved members of the radiation community who also know how and are willing to take down a person who is comparably achieved but who thinks differently. They make sure that their side wins, whatever means it takes. It is how they play the game, and few know about it or understand it.

I was under the microscope and had become public enemy #1 to four groups that worked in different ways and who were plotting their next series of challenges. There was the anti-hormesis group, the LNT supporters, and the field of radiation genetics that Muller created, nurtured, and matured. I had attacked their beloved leader and hero. I also knew that there was a silent and powerful group at my institution that was watching me, ready to act in any way they could. Thus, my life had become one of constant attacks, debate, and large-scale opposition. I also had become targeted by many now former friends, some of whom had been close friends, who were now part of the opposing forces.

Yet, I continued to be highly motivated and productive in both the hormesis and history of cancer risk assessment domains, transforming these fields. As for hormesis, in the 1980s the terms hormesis or hormetic were cited only about a dozen times per year in the Web of Science database. Now it approaches/exceeds 20,000 per year, quite the growth and achievement. That is, despite the opposition, threats, and attempted professional and personal embarrassments and humiliations and loss of close "friends," this idea prospered and has been adopted by many scientists worldwide who use it to transform and advance their own research agenda and enhance human health practices. In the case of LNT, my numerous new discoveries have transformed the entire historical foundation of cancer risk assessment, turning it upside down, exposing mistakes, corruption, and bureaucratic malpractice at EPA and throughout the world. My name has long been mud with those groups that oppose what I have done on hormesis and LNT. The most recent NAS radiation committee voted to not even use the term hormesis nor to cite my LNT publications (Cardarelli et al. 2023).

This lead-in sets the stage for the next focused attack, this time by Beyea (2024) in the *HPJ*. Beyea has tried this before (Beyea 2016, 2017) and was shown to be incorrect on many issues as far as I am concerned, somehow missing obvious and well-documented findings and failing to approach the research with the most appropriate methods of the history of science. In fact, these failings made communicating with him via letter to the editor nearly pointless. Furthermore, LNT history is highly dependent on primary sources, that being the world of the professional historian of science, something that I have tried to become proficient at over the past 15 y. In the case of Beyea, he still lives fulltime in the world of secondary sources that are subject to the biases and errors of others and repeatedly so. His research is largely embedded within confirmation bias. He simply does not make the necessary efforts to do the job accurately and fairly. Yet, the editors of the *HPJ* failed to understand this major point, selecting reviewers who apparently had little, if any, understanding of the role of primary vs. secondary sources, and they published his manuscript, which does not even remotely satisfy academic standards (Beyea 2024). Such actions do little to

inspire confidence in the peer review process, when the editor and reviewers apparently fail to adequately appreciate the nature of the methods used in history of science research.

This prologue was intended to show that my activities in the hormesis and LNT/cancer risk assessment domains have been highly successful. They have not only withstood the test of time and numerous critics but have also attracted large numbers of scientists to the hormetic dose-response concept, its mechanistic research, and its vast biomedical, public health, and therapeutic applications. The LNT story has been especially challenging to the regulatory and research communities because it shows that the entire field of cancer risk assessment was corrupted from the start by revered leaders, including Muller and many others (Calabrese 2015a, 2015b, 2019a, 2019b, 2024). As noted, I was once one of Muller's admirers as well, but I was able to set that aside and independently evaluate his field, now having essentially rewritten it. It is understandably embarrassing for the "establishment" and regulatory agencies to be told that they have long been wrong, misleading the public when they have been shown exactly where the errors occurred. This is the story of my LNT effort in a nutshell. However, it has created considerable hostility and with its successes has generated much frustration in some who oppose what I reported. I will now summarize my disagreements specifically with the Beyea (2024) paper.

THE BEYEA PAPER

The Beyea (2024) paper purports to be a criticism of the activities of the HPS committee that created a 22-episode documentary on the historical foundations of cancer risk assessment. I was first contacted in July 2021 about giving a taped interview of indeterminant length but probably on the order of 2-3 h. The interview was taken in late October 2021 and lasted over two consecutive days, about 14 h in duration. It was placed on the HPS website in April 2022 after editing and insertion of numerous required historical documents using mostly primary sources that provided documentation for my comments. The HPS committee wrote a subsequent paper summarizing the documentary that was published in the *HPJ* (Cardarelli et al. 2023). Thus, the paper by Beyea reflects an activity now over 3 y old. While the paper of Beyea is highly critical of the actions of the HPS committee, including the documentary and their paper, he mostly directed his focus on some of the areas over which I was questioned and my related publications. The following narrative documents numerous errors/omissions in the Beyea paper, showing his paper to be highly unreliable in that it failed to disclose conflicts that could bias the peer review, requiring retraction.

The following evaluation and criticism of the Beyea paper is massive, highly detailed and extraordinary, something no editor or publisher wants for their journal. This level of effort was necessary to demonstrate for the reader how biased, inaccurate, unacceptable, and unreliable the Beyea paper is. In over 50 y of academic peer reviewing and having been engaged in numerous past professional debates and disputes, I have seen nothing that even remotely compares in the lack of quality of the published paper and its associated peer review and editorial judgments. Over two dozen areas have been highlighted and documented in detail, most of which contain multiple significant research errors and failed professional judgments. The detailed criticism is necessary not only to correct the mass of mistakes and unacceptable scholarship by Beyea but to document poor editorial judgments of the *HPJ*. This paper represents the so-called perfect storm, which combines the poor scholarship and bias of the author with the lack of knowledge and perspective of the peer reviewers and poor judgement on the editor's part. The paper is so riddled with mistakes, fundamental misunderstandings, and bias that it is far beyond being even a highly unreliable paper. The Beyea paper represents the "poster-child" for what a retracted paper would look like based on the retraction criteria of the Committee of Publication Ethics (COPE). The extent of unreliable information is massive and has been documented in considerable detail. There is not an area that is addressed by Beyea that does not contain unreliable information. His paper seems to fit perfectly the definition of what is unreliable, and this fits the COPE definition of what should be retracted. Since the *HPJ* follows the guidance of COPE in such matters, the Beyea paper needs to be retracted based on the information provided herein. In addition, a similar and equally massive criticism of the Beyea paper is provided in the companion paper of Cardarelli (2025), which details a long string of similar flaws that specifically relate to the HPS documentary, showing numerous striking mistakes and other professional failings of Beyea and exposing again the deficiency of the peer review process and editorial actions. That such a failed paper by Beyea could have passed a peer review at the *HPJ* calls into question the judgement of the editors of this journal. A reading of the Cardarelli paper and the present one will demonstrate a massive system failure, from Beyea to the journal leadership. Let the story begin.

Beyea fails to use primary source documentation

Beyea's analysis is entirely based on what is present in the public domain within the realm of journal publications, dissertations, NAS activities/reports, and

government reports. The unique methodology and assessment of my research makes extensive use of preserved private (i.e., primary) documents such as correspondence, research notebooks (like Muller's Nobel Prize research notebook) and other unique historical materials, which I shared with the HPS staff as required during the development of the documentary. The Beyea paper reflects a failure to seek the most accurate and fundamental information. By failing to obtain primary documents, the Beyea strategy for evaluation and communication leads to a mistaken impression among the journal and readership by providing a false impression of professionalism. Beyea (2017) has even acknowledged in earlier writings that he failed to use or even try to obtain primary documents. Yet, he still continues along a path that is easier, but far less historically and scientifically reliable. As long as journals such as *HPJ* permit this, they participate in this flawed process.

The disagreements that occur on the present topic of my research are largely in the realm of the history of science. Professional assessments by those researching in this domain require and demand the use of primary sources to the extent possible/reasonable to avoid the bias and potential errors of various secondary sources. For example, Beyea (2017) tried to characterize mutation risk estimates of the BEAR I Genetics Panel (1956) based, as he writes, "solely on secondary sources by historians," leading him to offer unverified speculative estimates when the primary documents were available. Such reliance can also lead to missing essential papers in the scientific literature that are beyond the technical education of most historians. Thus, the approach of Beyea placed his research in the form of double jeopardy as will be demonstrated herein. Thus, Beyea's lack of proper research can lead to serious mistakes and failure to detect Panel bias/deceptions and mistakes, missing fundamental findings as will be discussed in more detail later.

Beyea (2016) also made a serious error in his evaluation of the BEAR I Genetics Panel, relying on the memory of an aged Bentley Glass concerning how massive radiation risk assessment disagreements were handled by the Panel. If Beyea had read the meeting transcripts (which he claims he had) and letter exchanges of the Panel (which he makes no effort to discover or obtain), Glass's failed memory would have been obvious and the basis of his serious error. This error of Beyea compounded the problem, resulting in yet another serious error concerning Panel scientific misconduct activities that my research exposed and documented with primary source materials. Such mistakes of Beyea were emphasized in my rebuttal to his earlier paper (Calabrese 2016). As will be shown later with the case of James Neel, Beyea failed to learn from these past mistakes, only to repeat the process again. The net result is a newly published paper (Beyea 2024) that once again does not meet acceptable professional standards. This error reflected not only the failure to use primary sources, but it allowed Beyea to demonstrate his confirmation bias (i.e., that the BEAR I Genetics Panel did not commit scientific misconduct when the evidence based on primary sources indicates that it did), making a mistake that would support his bias and misinform readers.

The Beyea (2016) criticism of Calabrese (2015a) was commented upon by Cuttler (Cuttler, 2016). The statement below is quoted from the Cuttler (2016) paper, providing a unique insight into the quality of the Beyea paper and his professional scholarship:

"Beyea attempts to discredit the very serious charges against the NAS of the 1950s by Calabrese (2015) without delivering specific challenges about any of the official transcripts of the meetings and the many signed items of correspondence that Calabrese listed, and does not challenge any of the analysis and conclusions. Instead, Beyea states that no one should believe what Calabrese has written until they read the rebuttals of Cicerone & Crowley (2014) and Crowley et al. (2015). Beyea omits mentioning that this Cicerone and Crowley response did not address the factual basis of Calabrese's numerous assertions; it merely tries to defend the reputation of the NAS, led by Cicerone. Nor does he mention the Calabrese rebuttal (Calabrese 2014). Beyea does not clarify that the Crowley et al. rebuttal is totally unrelated to the Calabrese historical assessment; it only defends the BEIR VII report. Thus, this statement by Beyea is misleading and implying that there was factual criticism of the Calabrese historical assessment when there was none.

There are no theories; the stated activities and decisions of the 1950s actually happened. Beyea's "most detailed" letter (Beyea 2016), which offered a series of alternative interpretations, has elucidated a 13,000-word rebuttal from Calabrese (2016). In his rebuttal, significant newly uncovered evidence is presented which supports and extends the findings of Calabrese (2015), reaffirming the conclusion that the Genetics Panel should be evaluated for scientific misconduct for deliberate misrepresentation of the research record in order to enhance an ideological agenda. This critique documents numerous factual errors along with extensive and deliberate filtering of information in the Beyea letter (Beyea 2016) that leads to consistently incorrect conclusions and an invalid general perspective."

The Cuttler perspective offers an independent appraisal of the Beyea scholarship and finds it "leads to consistently incorrect conclusions and an invalid general

perspective." That is, the Beyea (2016) letter to the editor is not a reliable document, reflecting poor scholarship, bias, and a capacity not to be able to recognize the profound limitations documented in his letter. This is now a well-worn path that Beyea has carved out for himself. These same types of professional failings are glaringly and repeatedly seen in the present Beyea paper and highlighted in this rebuttal.

The primary source approach of Calabrese versus the secondary source approach of Beyea

The exclusive use of secondary sources can be problematic in many ways, especially as the historical foundations of cancer risk assessment extend over a century with about five generations of scientists passing on their modified and imperfect understandings of past findings and how to interpret and apply such information. This process may be affected by cultural changes, language, slight changes in word meanings, education, political perspectives, professional rivalries, funding strategies, and other factors that can and often do change over time. Beyea only taps into the system sometime after original understandings have been translated into secondary sources, not having any understanding of what primary source material existed, what was selected and why, and what was ignored/not selected. Beyea relies upon the authority of other secondary source writers he tends to agree with, rather than checking original sources and determining for himself. For example, the entire BEAR I Genetics Panel and genetics field, in general, accepted Muller's interpretation of the Ray-Chaudhuri dissertation data (Ray-Chaudhuri 1939) that were used to support LNT as published in a non-peer reviewed Conference Proceedings (Ray-Chaudhuri 1944). However, no one learned that Ray-Chaudhuri failed to identify in his dissertation where the locations of the control and treatment incubators were until I reported it (Calabrese 2022a, 2022b). Thus, it is not known what the possible "background" gamma exposures to the control group may have been as noted in my earlier papers (Calabrese 2022a, 2022b). It could have been as high as 24 R. It was necessary to get a copy of the unpublished dissertation and assess the details of the research.

Furthermore, I obtained letters between Muller and Ray-Chaudhuri which demonstrated that Ray-Chaudhuri failed to report key threshold supporting experimental data in the dissertation (Calabrese 2022a, 2022b). The data were provided in correspondence to Muller during the time of his dissertation research. Thus, obtaining primary source material is critical in the search for scientific truth and relates to what is included, what is left out, and why. My efforts have consistently attempted to do this. Beyea has never done this with any of his papers, including the 2024 *HPJ* forum article. When trying to compare my research approach with that of Beyea on the topic of the historical foundations of cancer risk assessment, it is like comparing apples with oranges. In general, they are not on the same professional level. This comparison is played out in dozens of specific aspects of my research on the historical foundations of cancer risk assessment. The failure of Beyea to research at the most fundamental level will generally lead to an inadequate and uncertain outcome and one that can more readily lead to confirmation bias. It is also frustrating for the reader because Beyea and I are not searching for truth at the same level. I saw this firsthand when Beyea and I had our first professional exchange about 8-9 y ago. I showed how inadequate and often incorrect his analyses and interpretations were. The Beyea approach was inadequate from the perspective of an historian of science and the proper pursuit of truth, and it was not worth responding to his second paper. Now in his 2024 *HPJ* forum article, he continues the same modes of operation, and this has satisfied the inadequate standards of the editorial staff of the *HPJ*.

Primary references are particularly valuable since they can serve to test, challenge, or validate interpretations used in articles that rely entirely on secondary sources. Primary sources can provide information that would not normally be readily available as well as offer critical specific confirming or discrediting insights. As noted above, knowledge of the primary source allows the reader to learn what was known, accepted, left out---and possibly why. This is lost when only secondary sources are used, as is the case with Beyea's approach. Primary sources also provide unique contributions on their own, as my research on Muller and the history of cancer risk has shown. From the first reporting in 2011 on Muller and his Nobel Lecture (Calabrese 2011) to current papers, the extensive use of primary sources has contributed to many of my 150 papers on the topic in unique and substantial ways. For example, the primary literature showed that Muller received and reviewed the Ernst Caspari threshold-supporting manuscript a month before his Nobel Prize Lecture, also praising Caspari as a researcher (Calabrese 2011). A letter 1 mo after the Nobel Prize lecture, Muller confirmed his support for the Caspari findings but stated that it needed more confirmatory research (Calabrese 2011). Other letters showed Milisav Demerec, Director of Genetics at the Carnegie Institution, asking Caspari what can be done to save the "hit" model after seeing the Caspari threshold supportive findings (Calabrese 2015a, 2015b). Other letters have Muller indicating that the Caspari control group mutation frequency closely matched that in his (Muller's) own lab but that the data of Uphoff failed to do so. Based on this, Stern wrote to Caspari indicating that the Uphoff data were no longer considered reliable but that his (i.e., the Caspari data) were now accepted as valid due to the Muller letters (Calabrese 2015a, 2015b).

This is simply revealing a massive string of examples in which my use of primary sources added considerable insight and significance to the Muller and LNT story. The primary sources therefore led me to show Muller's subsequent dishonesty in the early 1950s when he later claimed just the opposite of what he earlier shared in multiple letters with Stern. Muller now conveyed the message that Uphoff's control data, which supported an LNT conclusion, could be trusted but Caspari's control data could not because he characterized it as being aberrantly high. The primary sources therefore caught Muller in a profound deception, but Stern and Caspari were afraid to speak up, to be whistleblowers (Calabrese 2023a, 2023b). Via letters, it was also learned that many radiation geneticists, including James Neel and Don Charles of the Manhattan Project, strongly supported the threshold/Caspari supporting paper of Robley Evans in 1949 (Evans 1949), but they were afraid to challenge Muller publicly (Calabrese & Selby 2023a). The letters also revealed that Muller attacked the threshold supportive studies on mutation of James Neel on the children of survivors of the atomic blasts not only because Muller disagreed with the threshold concept but also, in part, with regard to a battle over potential funding in order to prevent human population studies research from diverting his laboratory grant monies, assuming a zero-sum funding game (Calabrese 2020). This shows that Beyea not only fails to appreciate these and many other previously hidden historical findings but also highlights the fact that Beyea's research never enters the world of primary source research. The contrast between what my work has found and what his few publications offer is striking. There is always a story behind the story that can transform what we believe to be the truth, but Beyea never saw it. The record shows that without such discoveries and revelations provided by the primary source material, the story of Muller's life and the history of cancer risk assessment would be far less insightful, still full of deceptions that the scientific community would think are truths and would still be teaching their students, thereby passing on falsehoods and lies as truths, as has long been the case. The failure of Beyea to use the most reliable historical research methods and documents makes his findings unreliable.

Beyea's big mistake: Who do you trust? The historian or the scientist?

In 2017, Beyea noted that he relied upon the "detailed" thesis of Christopher Jolly from Oregon State University (OSU) to guide him on how the BEAR I Genetics Panel assessed and estimated radiation-induced genetic risks. In essence, Beyea decided to place his reputation as a scientist on the experience, technical expertise, knowledge, and judgment of the history of science dissertation by Jolly (2003) rather than digging out the details and evaluating all the potential complexities himself. This was quite a decision by Beyea, with very unfavorable consequences, as will be soon revealed. Jolly obtained the technical reports of the nine BEAR I Genetics Panel members who provided estimates of mutation risks and their methodologies. However, the dissertation offered no technical analyses of how each geneticist derived their estimates, including no critiques or assessments of possible errors, refinements, or limitations of their respective approaches. His analysis was highly descriptive, as one might assume such an historically oriented dissertation would be. On page 319 of his dissertation, Jolly noted that five of the geneticists. (i.e., Beadle, Glass, Kauffman, Crow, and Muller) used Russell's ongoing mouse experiment mutation rate in their estimates. Russell's estimate was also dependent upon it, although he used a different methodological approach.

Jolly's dissertation has a profound omission, which amounted to a major error that his dissertation committee failed to detect and which Jolly, therefore, failed to address. Jolly failed to report that the Russell (Russell and Russell, 1997) (Russell and Russell, 1996) massive mouse radiation mutation study significantly overestimated mutation risks since the control group had a very low mutation rate due to underreporting large numbers of cluster mutations. This omission had been hidden by the Russells for four decades until it was inadvertently discovered and reported to the US Department of Energy. A correction of the Russell findings of 120% (2.2-fold) was imposed by the US Department of Energy (DOE) based on the recommendations of an international expert panel that was convened in 1995 to assess possible Russell research misconduct. The Russells (1996, 1997) corrected this error via publishing the revised/corrected results in the Proceedings of the National Academy of Sciences (PNAS). The whistleblower geneticist, Paul Selby (1998a, 1998b), testified during the Russell proceedings and offered his own technical correction in two follow-up publications, claiming that the Russell errors were even greater than they admitted to, being in range of 5- to 7-fold.

The Panel also investigated the Russells for possible scientific misconduct based on arguments that the actions of the Russells were deliberately hidden for decades from Oak Ridge National Laboratory (ORNL) scientists and staff and the broader scientific community since it would have undermined the value of their mouse model, profoundly affecting the significance of their research, their very prominent roles in the field, long-term funding of this research program, and the estimation of radiation risks at low doses, which had taken a dominant role. In fact, William L Russell was central to yet another major cover-up, this time with the prominent pathologist and

future Director of the US National Cancer Institute (NCI), Arthur Upton. In this case, a lifetime mouse study of Russell and Upton showed no radiation treatment effect on longevity and cancer incidence, a striking finding that could have impacted the radiation risk assessment debate in the early 1960s and later years. Russell and Upton ensured that it would not; they suppressed the findings (that is, keeping the data hidden) by never publishing the findings or making presentations at conferences or to appropriate governmental agencies despite the major significance of the findings. The discovery of this story occurred because 33 y after the study had been completed and hidden, Upton was hired as an expert witness for the defense in a major litigation in the UK on radiation health effects. Upton contacted Russell, convincing him to publish the data in a prominent journal since it would be useful in the court case for the defendants. This story became of interest as it emerged in conversations between Selby and me. The details of this story are documented in considerable detail in Calabrese & Selby (2022).

The DOE did not dispute the facts presented at the Hearings concerning the possible scientific misconduct of the Russells, but it did not lead to a scientific misconduct charge, possibly because William Russell was over 85 y old, retired, and with a distinguished career. Nonetheless, the important point was that the control mouse mutation data of the Russells was incorrect to a biologically meaningful degree. It was the incorrect data of the Russells that six BEAR I geneticists used in their calculations that Jolly highlighted and depended upon. If the Panelists in 1956 had used the now-Russell-corrected data, the Panelist risk assessments would have been significantly affected since the corrected findings showed no statistically significant effect, thereby greatly diminishing support for the LNT model.

The Selby challenge of the Russells therefore proved to be a seminal event in the history of risk assessment. It demonstrated that the dose response of the several-decades-long Russell study, involving up to about five million mice, was no longer supporting a linear dose response model but a threshold and possibly hormetic dose response model. However, this debate was highly technical and handled quietly in high level specialized scientific publications without media publicity, although it certainly deserved it.

This very notable change in radiation mutation risk assessment was not cited in the Jolly dissertation. Jolly never listed the relevant Russell or Selby publications nor the Department of Energy hearings and its official transcripts, all which occurred at least 5 y before the Jolly dissertation. Thus, the relevant information was in scientific literature and in prominent journals, and Jolly failed to acknowledge and address it.

The Jolly dissertation (Jolly 2003) had a major focus on the BEAR I Genetics Panel and mutation risk estimates and how these actions affected the LNT and radiation risk assessment. Enter Jan Beyea. As noted above, Beyea (2017) proclaimed that he professionally relied on the Jolly dissertation. Beyea also never criticized Jolly for missing the Selby-Russell story and its transformative risk assessment implications. In fact, Beyea also failed to cite the Russell and Selby articles and the DOE dispute in his publications on the LNT topic, now stretching over a decade, including the recent paper (Beyea 2024) in the *HPJ*. Beyea built his scientific beliefs on the compromised Jolly dissertation. Calabrese (2017c) was the first to transform the Russell-Selby story and clarify some of its risk assessment implications, especially with respect to the BEIR 1972 report.

This historical perspective clearly shows that the research methodology of Beyea, which depended on the assessment of Jolly, led to a reaffirmation of an incorrect historical narrative, misleading the scientific and regulatory communities. This error of the BEAR I Genetics Panel impacted many aspects of society, including the emission standards for nuclear power plants and the risk assessment practices of the EPA and regulatory agencies worldwide. The paper of Beyea (Beyea 2024), which depended on the flawed analysis of Jolly, continues to promote and propagate significant errors, showing that it can be difficult for science to be self-correcting.

The compromised Jolly dissertation and its adoption by Beyea improperly affected multiple topics that Beyea included in his recent *HPJ* paper (Beyea 2024). These included the Russell dose rate debate, the BEAR I Genetics Panel activities and its risk assessment applications, the reflections of James V. Neel on the Genetics Panel activities, and the recommendations of LNT for risk assessment and their impacts on the EPA. It also impacted a broad spectrum of his unjustified criticisms of the HPS documentary. Each of these areas has its own story and will be discussed later in this paper. For example, even though there are major discussions about errors and manipulation of data by the BEAR I Genetics Panel and mistakes that Beyea made, his reliance on the flawed Jolly dissertation make many of the follow-up disputes moot since they were flawed from the start, using the incorrect data of Russell as the big driver. The efforts of the BEAR I Genetics Panel were flawed from the start, flaws that Calabrese found and documented. However, these were flaws that Beyea simply copied from the Jolly dissertation and passed them on.

Not only did the OSU dissertation committee fail to correct Jolly on this crucial matter but so too it was missed by the reviewers of the *HPJ* who failed to properly evaluate and judge these significant mistakes of Beyea. The foundational discoveries based on the Selby efforts

were missed by the *HPJ* reviewers and the associate editor, calling into question the competence of the reviewers and the editorial staff and/or the fairness of the review process. In the end what does this mean for the Beyea paper and the *HPJ*? It indicates that the Beyea paper provided unreliable information on a fundamental area and that then contaminated large segments of his analysis. This was a massive and self-propagating error, like a disease spreading throughout the body, resulting in a high degree of unreliability of the information in this paper, satisfying the criteria of COPE for article retraction. In the end, the decision of Beyea to abandon his responsibility for the papers he published and to depend on an independent third party is simply the wrong and lazy path to follow. In this case, this decision led to the blind leading the blind and then getting opinions accepted by the *HPJ.*

Finally, it should be noted that Beyea (2024) cited the Calabrese (2017c) paper in which I applied the Russell mutation rate correction to the BEIR I (1972) Committee assessment. It is important to note that in this case, Beyea failed to explicitly acknowledge the decades-long error of the Russells, that the Russells-based incorrect data was used by six geneticists on the BEAR I Panel, that the estimated 5 million mutations that were predicted would have been reduced to values not distinguishable from controls, and that his major reliable source of guidance (the Jolly dissertation) failed to report this development and its implications. This gives a further insight into the objectivity of Beyea, which is found to be strikingly deficient and the distorted view of the science that he presented to the readership of the *HPJ*. Surely, the editors of the *HPJ* would have desired that Beyea shared the full story.

Failure to cite critics who were Muller's biographers: What is the point here?

Beyea criticizes the HPS documentary and me for not citing criticisms of Carlson and Schwartz who wrote detailed biographies of Muller (Carlson 1981; Schwartz 2008). Both individuals offered sharp criticisms of me in the journal literature. Schwartz's criticism was part of an interview (Crok 2011), while Carlson offered criticism in the form of a publication with the last section devoted to criticism of me (Carlson 2017). The material presented in those criticisms was itself highly inflammatory, being mostly personally directed and not addressing the substance of my discoveries. These efforts, which are now nearly 15 y old, did not identify mistakes in my papers but rather represented different views based upon one's perspective. For example, an evaluation of the Muller books by these two authors indicates that neither identified any of the information that I presented in my more than 150 publications on the historical foundations of LNT and Muller's role in this process. Given this massive number of publications and diverse range of criticisms that my publications have directed toward Muller and his colleagues and the radiation genetics field and LNT, one would have thought that the Schwartz and Carlson books would have at least contained brief passing reference to some of these questions and limitations. However, their books are devoid of such information so central to Muller's professional life. How is this possible for award-winning biographers? Thus, my research is as much an illustration of the limitations of their biographies as it is an evaluation of Muller, including his strengths, limitations, and significance.

In addition, Beyea neglects to criticize the authors of these Muller biographies. He simply uses their criticism of me, with no consideration of their limitations. Moreover, Carlson admits in his book that he decided not to obtain the FBI files of Muller, which were likely to contain extremely important and novel information. Why? Carlson did not obtain the information because of a request of the Muller family. What kind of biographer would pass up such information on the subject of the biography? Was there a quid pro quo between Carlson and the family? Yet Beyea fails again to dig into the story. Why? Nonetheless, the Schwartz (2008) and Carlson (1981) biographies are valuable additions to the field despite their limitations in capturing the "real" Muller. However, because Schwartz and Carlson are really part of the "establishment," they get its praise and a pass on their limitations and are given the chance to direct their animus toward me, who criticizes Muller, the hero of their books. That Beyea criticizes the HPS team and me for not citing those works represents a lack of perspective and understanding of this context by Beyea. Schwartz and Carlson apparently know little or nothing about what I discovered about Muller, making their contributions about the topics rather marginal and more background-like.

The same criticisms of Beyea could be offered concerning the debate between the NAS President and me. The President of the NAS, in his rather uninformed attack on me, was compelled to defend the indefensible; that is, Muller's actions and the dishonesties of the NAS BEAR I Genetics Panel and his NAS Presidential predecessors (Calabrese 2014). For example, the past NAS president, Detlev Bronk, lied to the general public when he indicated that the BEAR I Genetics Panel authored the major report to the general public on radiation and mutation risks when it was instead written by a paid third party and never shared with the Panel and contained numerous significant errors (Calabrese & Giordano 2022a, 2022b). The NAS or its president never corrected this false information, and none of the BEAR I Genetics Panel members, including Muller, had the courage of their ethical convictions to challenge Bronk and attempt to set the record straight even though they were aware of the errors

and deceptions and discussed these matters in private letters I have cited. This was another major item discovered by me that was not addressed by Beyea, one of dozens of examples. Again, this was discovered by access to primary sources, that is, the correspondence of the BEAR I Genetics Panel. Therefore, the Beyea narrative is unreliable on this topic as well.

Beyea lacks understanding of how the HPS documentary originated, the level of funding, the magnitude of the needed volunteer efforts and rigorous demands

Beyea excoriates Dr. Cardarelli and his colleagues (the HPS Executive Director, the Chair of the Public Information Committee, and Past President Barbara Hamrick) for their efforts to educate the membership of the HPS on LNT during his tenure as HPS president (2021-2023). These individuals invested considerable time in scientific literature preparation, finding the right venue for recording the interview, traveling across the country, conducting the interview (including hundreds of hours of subsequent editing of the interview), and purchased high-end computer and software materials to produce the videos. These are some examples of the level of effort that these HPS members provided to produce the documentary, which brought more visits to their website than any other product ever produced by the HPS. These actions need to be recognized and appreciated because they provide important context, but this was ignored by Beyea. Such treatment was both unfair and inaccurate. The overall unpaid volunteer effort required many hundreds of donated hours. They made a choice concerning how to spend their voluntary time and very limited resources. Furthermore, Cardarelli was in a position to make an informed judgment about the cancer risk assessment and LNT issue because he had a long history of addressing this issue within his job at EPA. He also documented these perspectives in a detailed paper on radiation and cancer risk assessment, a paper that the EPA refused to let him publish. When he eventually did publish it, the agency moved to terminate his employment (Cardarelli 2024; Cardarelli & Ulsh 2018). Cardarelli was well aware of the overriding controversies, opposing views, experts in the field, publication records, and how these developments with respect to LNT developed over time and the role of his professional society in those actions. The Beyea paper fails to present a fair representation of their knowledge, commitment, the quality of their work and their attempts to be fair minded.

In addition, when considering the tone of his paper, it should have been necessary for Beyea to point out that I did not seek to be interviewed for the documentary by the HPS. I was specifically approached by the HPS to be interviewed. I also played by the rules that were established by the HPS. That is, they would not provide me with questions in advance; I was not permitted to answer questions over again to try to get a better answer or to get it right. I had only one chance at expressing myself on a very broad spectrum of questions. In addition, I agreed to work with the HPS team and provide them with all the documentation that they demanded within a reasonable time. I fulfilled that obligation fully. This information needed to be provided in the Beyea manuscript because it speaks to character, the nature of the HPS documentary, and the lack of rigor and fairness of the *HPJ* peer-review activity.

The documentary was a real-time interview without papers in front of me for reference. It was a documentary interview that was based upon my capacity to think in real time and respond to questions that were asked. This is not an easy task for any individual to pull out many thousands of facts from papers that are many years old and some stretching back longer than a century in the historical record. However, Beyea was highly critical rather than again being fair and understanding that this process was extremely challenging mentally and physically and yet I agreed to do this, realizing that 14 h of interviews in such a fashion would have tested anyone's abilities to an extreme degree. Further, I requested no compensation for this major effort nor was any offered. Beyea offers no acknowledgment of this situation. He also fails to appreciate the intense demands that the HPS placed upon me, the person that they chose to interview. While I greatly appreciated their interest in this issue, they were not easy task masters, just the opposite. In fact, it is uncertain how many others would have accepted the terms and conditions of the HPS as I did. Beyea is so focused on trying to discredit me for challenging the status quo that he fails to be fair in his evaluation of me regarding the circumstances and my capacity to respond to the types of rigorous challenges that were imposed on me by the HPS team. Yet, despite all the challenges and demands, the general consensus was extremely favorable and highly regarded, even if information turned the history of the field upside down. The documentary became viewed as a must-see story.

Beyea: confirmation bias directs his methods

Beyea makes no effort to address the issue of what his criteria for evaluation were. He made no effort to show the positive and novel findings of my research and its contribution to the field, again showing extreme bias. With respect to the first "Question," he challenges the health physics paper by Cardarelli and colleagues (Cardarelli et al. 2023) because he claims that it is not a comprehensive

review of the area of cancer risk assessment and LNT, including more recent findings. He missed the principal point that their paper was a summary of the general perspectives and conclusions offered in the HPS documentary, which was an historical evaluation. The material in this section by Beyea is therefore lacking in relevance and in fairness to the HPS team effort. Beyea then offers a variety of criticisms of my work. However, these are mostly directed toward rehashing criticisms that he provided nearly a decade ago (Beyea 2016, 2017). This is seen in his discussions related to the Manhattan Project experiment with Curt Stern and Delta Uphoff. What is critically missing in his rehash of the earlier papers is the updated findings by me that established that the Uphoff experimentation included two simultaneous variables that made any findings from her experimental work impossible to evaluate from a causality perspective, making this rehashed debate of little meaning. In addition, the data from two of her three experiments remain missing after nearly 80 y (Calabrese et al. 2023a, 2023b). Likewise, he notes the Ray-Choudhuri study that was used to support linearity at low doses, but again he fails to acknowledge recent publications that show serious weaknesses in that research (e.g., the failure to identify the location of incubators for the fruit flies and the inability to quantify potential exposure to the control group which could have been as high as 24 R) (Calabrese 2022a, 2022b). These are examples in which Beyea emphasizes arguments that were nearly a decade old, and he neglects new developments in the field.

Beyea is wrong and misrepresents the Calabrese evaluation of BEIR I and William Russell

In section 2.4 of his paper, Beyea boldly stated that "Calabrese claims that the national academies committee that authored the 1972 report ignored the dose rate dependency of genetic mouse data." In that same section, Beyea then writes that "Calabrese would have us believe that it was true." Yet, Beyea noted in the previous sentence "how incredible" it was that Calabrese would make such a claim, especially since Russell was a member of that committee. Apparently, Beyea (2024) did not address that Selby & Calabrese (2023) (see page 9, right column) contradicted Beyea's statements by actually quoting from the BEIR I report (1972) (page 65) and discussing the issue that Beyea claims Calabrese says they ignored. The following quote on the topic of dose rate dependency of mouse genetic data---the topic that Beyea claimed that Calabrese said was not addressed. Yet the information now provided shows just the opposite of what Beyea claimed. Not only did I show that the BEIR Committee discussed it, I emphasized it with a long quote, discussed it, and criticized their actions on the matter. The material from the BEIR 1 (1972) and the discussion is explicitly provided by Selby & Calabrese (2023). Here is the quote used by Selby & Calabrese (2023): "The BEIR I Committee stated: 'The finding of a dose-rate effect for mutation induction in mouse spermatogonia and oocytes raised anew the question of whether there might be a threshold dose or dose rate below which all mutational damage would be repaired. Exploration of a range of dose rates provides no evidence of a threshold dose rate for mutation induction in mouse spermatogonia…Therefore, we shall make the prudent assumptions that there is no threshold dose rate in the male and that the dose response at low dose rates is linear.'"

In fact, that paper placed that quote in a section labeled no less than "William Russell's views on threshold" (page 9, left column). The published statement of Beyea, therefore, is yet another misrepresentation of my research record. Furthermore, Beyea neglected to even cite the Selby & Calabrese (2023) reference that provided the information that discredited his statements. Yet, Beyea is allowed to make repeated incorrect and outrageous claims not only due to a lack of scholarship but also due to the failure of the *HPJ* peer-review process, their lack of knowledge, attention to detail and fact checking, and the lack of oversight at the level of the associate editor and editor-in-chief.

Equally important is that Beyea also missed our complementary discussion of dose rate by the BEIR I (1972) Committee (Selby & Calabrese 2023) that evaluated background radiation dose rate and epidemiologic studies of cancer. This dose rate discussion did not directly involve the Russell research. Thus, Beyea repeatedly missed the key factors leading to his incorrect conclusions.

Making the situation even worse for Beyea, his readers, and the *HPJ* review process and journal system is that Beyea actually cited the paper of Calabrese (2017b) titled "The threshold vs. LNT showdown: Dose rate findings exposed flaws in the LNT model part 2." How a mistake led BEIR I to adopt LNT but then neglected to acknowledge that dose rate was a central component of the paper as may be inferred from the title itself. The paper includes a detailed evaluation of the BEIR I (1972) report, regarding the role of the Russell dose-rate data in the assessment and how the major error of the Russell control group led the BEIR report to profound errors. This paper was the second of a two-part debate over the threshold vs. LNT models for risk assessment and the long-standing debates/disputes between Muller and Russell, culminating in the BEIR I (1972) meeting (Calabrese 2016). Again, Beyea fails to cite the other key papers (Calabrese 2016, 2017c; Selby & Calabrese 2023). In addition, the issues were explicitly discussed in the HPS documentary in episodes 19-20. These episodes not only contradict his statements

but also confirm what is written herein about the Calabrese papers on this topic. These collective failures by Beyea on this topic make his comments about the documentary and my published papers unreliable.

Beyea falsely claims that Calabrese accused BEIR 1 of scientific misconduct

The previous section has demonstrated that Beyea's assertion that "Calabrese claims that the national academies committee that authored the 1972 BEIR report ignored the dose rate dependency of genetic mouse data" was false (Beyea 2024). Further, Beyea ends Section 2.4 (page 10) by stating, "There is no basis for inferring scientific misconduct." This statement by Beyea is alleging that I accused the BEIR I Committee (1972) of scientific misconduct. He provides no evidence to support such an allegation. While it is true that I made such an allegation concerning the 1956 BEAR I Genetics Panel, this was never the case for the 1972 BEIR 1 Committee. The allegation of scientific misconduct is a serious matter, and it is also equally serious when one alleges that someone makes such a claim when it did not occur. The allegation of Beyea not only is wrong but it damages my reputation, suggesting that I made such accusations when they are not justified. The seriousness of this error alone is grounds for retraction. One would have expected that this allegation by Beyea would have been noted by at least one of the reviewers and demand that Beyea provide documentation. However, this was not the case. Likewise, this example represents another serious failure of editorial oversight at the level of the associate editor overseeing the peer review and the editor-in-chief.

Correction of another Beyea error on Muller and LNT: When did Muller start supporting LNT?

Beyea writes that, "By 1948, Muller would drop the 'we believe' caveat in a presentation he made trying to convince doctors to reduce high doses in medical practice, but this was 2 y after his Nobel Prize address." In point of fact, the period between his Nobel Prize Lecture and that quotation was about 3.5 mo (April 1, 1947), not Beyea's 2 y. There was a delay of approximately 2 y before his address was published. It was within 4 mo after the Nobel Prize Lecture that Muller gave a lecture to the New York Academy of Medicine during which he affirmed his Nobel Prize lecture message, stating that there was absolutely no threshold dose for mutations and that induced mutational response was proportional to total dose. His presentation, however, was not published in the Academy's journal until 1948. Thus, it is highly misleading for Beyea to claim that Muller only changed his perspective 2 y later. More importantly, Beyea also fails to acknowledge that in 1930 Muller created the original linear dose-response model concept, calling it the Proportionality Rule, with his publications in that 1930 era strongly supporting linearity down to a single ionization (Calabrese 2015a, 2015b, 2019a, 2019b). Thus, Muller is nothing if not consistent in this linearity view, again contradicting the statements of Beyea. In addition, Beyea appears to actually believe in the expressions of support for the LNT by Muller. In this regard, it is fascinating that in private communications on 4 February 1949 with Robley Evans, the highly regarded health physics professor at MIT, concerning ionizing radiation and gene mutations, Muller admitted that "Many of the quantities are only roughly known even for Drosophila, and we are admittedly extrapolating too far in applying this to man…." Muller then advocated for a Precautionary Principle due to the lack of knowledge and/or certainty. Muller asked Evans not to share this information with others, a request that Evans honored (Calabrese 2023a, 2023b; Calabrese & Selby 2023a). While Beyea thinks he knows the real Muller, this private communication suggests that he might not. However, the real point here is that once again, Beyea doesn't even give himself or the *HPJ* readership a chance to wrestle with these questions because his methodology is faulty, and the biased information he provides the reader reflects this. Because the statements of Beyea are incomplete and provide an incorrect perspective, they are not reliable.

Other examples of bias: Beyea misrepresents when Calabrese began research on hormesis

Beyea coyly links my professional interest in hormesis in order to attract funding or other resources, being very opportunistic, directing the readership's attention to the highly critical papers of Rust (2019) and Shrader-Frechette (2010) that I commented on/criticized in the Introduction. However, what Beyea failed to do is inform the readership that I had first discovered the occurrence of hormesis as an undergraduate student in the fall of 1966 and did several years of research on this topic, publishing it in a peer reviewed journal as noted earlier (Calabrese & Howe 1976). This story is now well known. He also failed to indicate that I was invited to present a paper on examples of chemical hormesis at the first ever conference on radiation hormesis in August of 1985 with the peer-reviewed proceedings published in the *HPJ* (Calabrese et al. 1987). Beyea therefore unfairly and incorrectly mischaracterized my entry to this field and how I got involved in it in the first place. This shows that not only was Beyea incorrect, but it reflects strongly upon the bias that he took to this question as this information is widely known and publicly available. It also showed his failure to interview the object of his focus (i.e., me), but he chose to derive conclusions based on,

once again, secondary sources or his own biased assumptions. His information and comments on this matter are therefore unreliable.

Failure to properly represent the body of Calabrese's research on the topic

I have approximately 500 papers published on the topic of hormesis and approximately 150 papers published over the last 15 y on LNT and cancer risk assessment. Beyea should have been fair to the *HPJ* audience in his description of me by informing the readers that my hormesis and LNT papers have been published in the peer-reviewed literature and that I was following and adhering to the procedures and expectations of professionals in the field. That is, I subjected my work and myself to the peer review process with my papers being published in dozens of journals. This means that my work was read/overseen by a large number of different editors as well as being peer reviewed by hundreds of different peer reviewers from numerous countries. This means that I adhered to the procedures and expectations of professionals in the field. Yet, Beyea omits this from his description of me. In fact, Beyea spent much time trying to convince others that I duped the scientific community with my hundreds of papers and presentations because I am such a great communicator and a good writer. While I can thank him for his awkwardly offered praise, the truth of the matter is that the research findings rise or fall on their own. His argument appears to praise my talents but indirectly insults thousands of research scientists that support, cite, and extend my work into their own domains.

Beyea gives the impression that he has evaluated my research in a comprehensive manner. However, this is far from the case. Below are listed more than 60 areas of my research on the topic of cancer risk assessment and LNT not addressed by Beyea (2024), with each having impacts on the historical foundations of cancer risk assessment and the LNT. While Beyea (2024) selects the topic areas for his evaluation, he also needs to place his selection within a professional context with respect to the body of my research in this area, something he failed to do. This affects his capacity to generalize conclusions. Based on my evaluation, Beyea addressed an extremely low percentage of the areas that I have published on the topic, making his efforts nonrepresentative of the body of my work. Thus, his capacity to try to generalize his findings is not supported and therefore his major conclusions are unreliable.

Significant findings by Calabrese not addressed by Beyea (2024)

Muller: Early important findings. Beyea failed to report that Muller's concept of evolution was incorrect. This profoundly affected the development of the LNT-single hit model. It resulted in not incorporating a repair component into the model (Calabrese et al. 2022). This perspective dominated the radiation genetics community through the BEAR I Genetics Panel meetings and is fully documented in the BEAR Panel meeting transcripts.

Beyea did not address how Muller created the Proportionality Rule, its many limitations, and how it affected the development of the LNT-single hit model (Calabrese 2015a, 2019a).

Beyea did not assess the historical occurrence of peer review by the genetics research community during the time of the Muller Nobel Prize paper in *Science* (Calabrese 2019a, 2025).

Beyea did not address how Muller got his Nobel Prize winning paper published in *Science* journal without showing data (Calabrese 2018a; Calabrese 2019a).

Beyea failed to address the seriousness of the fact that Muller's Nobel Prize research was not peer-reviewed (Calabrese 2015a, 2018a, 2019a, 2019b).

Beyea fails to discuss that Muller would admit that he did not induce gene mutations, discrediting the LNT-single hit model that has been used by the EPA (Calabrese 2019a, 2024)

Beyea failed to report that Muller did not induce gene mutations but mostly gene deletions (Calabrese 2015a, 2019a, 2024).

Beyea failed to note that Muller's post-doctoral researcher (i.e., George Snell) was unable to replicate Muller's gene mutation "findings" in mice using the same approach and study design. Muller failed to cite the Snell research, possibly because he knew he was being nominated for the Nobel Prize and the Snell research would damage his chances to win the award (Calabrese & Selby 2024a).

Beyea failed to report that the dose-rate that Muller used in his Nobel Prize study was 100 million-fold greater than background and the difficult issues in extrapolation to background from such a massive dose rate (Calabrese 2019b, 2024).

Beyea failed to note that background radiation could only account for 1/1,300 of the so-called gene mutations in the Muller control group. This means that >99.9% were caused by factors other than background radiation (Calabrese 2019b, 2024).

Beyea did not address the criticism of Muller's gene mutation interpretation by Barbara McClintock that was strongly supported by Altenberg (Calabrese 2019a)

Beyea did not capture the role of Stadler in challenging the gene mutation hypothesis of Muller and how that debate ended; it favored Stadler over Muller (Calabrese 2015a, 2019a).

Beyea ignored what happened when Muller and Stadler worked together to test the gene mutation explanation of Muller via the dissertation of Lefevre. This amounted to the final and devastating criticism of the reverse mutation hypothesis of Muller (Calabrese 2019a, 2022a, 2024).

Why didn't Beyea acknowledge that Muller produced mostly gene deletions? Also why didn't he acknowledge that Muller's close colleagues, such as Jim Crow and others, strongly supported the Stadler position and that it altered their radiation risk assessment views? They concluded that Muller and the BEAR I Genetics Panel had greatly overstated health risks (Calabrese 2019a).

Beyea failed to address how Muller and other geneticists worked together to defend Stadler during the McCarthy/Rad Scare era (Calabrese 2017a, 2019a).

Of importance is that Beyea did not report that Muller disavowed the research of Hanson and Heys, while Beyea has relied upon these discredited papers (Calabrese & Giordano 2023) without addressing Muller's comments.

Why didn't Beyea report that Muller acknowledged in letter communication with Robley Evans that the data supporting risks with Drosophila were inadequately based and that human data were far worse? Evans was asked by Muller not to share this information with anyone (Calabrese 2023b).

Muller and ethics. Beyea ignored how/why Muller took credit for Stern's major discovery of chromosome structure in the mid-1920s. How resolution of this issue led to Muller's participation in the Manhattan Project was not addressed by Beyea (Calabrese 2019a).

Why didn't Beyea assess the role of eugenics in affecting Muller receiving the Nobel prize and the selection of the BEAR I Genetics Panel by the Rockefeller Foundation (RF) (Calabrese & Shamoun 2025)? This is especially important since Muller's receiving the Nobel Prize would have a major impact on the acceptance of the LNT.

Muller-Ray-Chaudhuri research and LNT. Beyea failed to report that the design of the failed Ray-Chaudhari study was the foundation of the Manhattan Project genetics study (Calabrese 2022b).

Beyea did not report on the letters between Muller and Ray-Chaudhuri showing that the dissertation removed data that supported a threshold (Calabrese 2022b, 2024).

Beyea failed to report that the Ray-Chaudhuri dissertation did not include the location of the incubators used in his studies (Calabrese 2022b, 2024).

Beyea did not assess the possible dose of radiation in the Ray-Chaudhuri study to the control group and that it could be as high as 24 R (0.24 Gy) (Calabrese 2022b, 2024).

Beyea failed to report that Ray-Chaudhuri published his dissertation research in a non-peer reviewed conference proceeding, just like Muller did nearly 20 y before (Calabrese 2022b, 2024).

Manhattan Project. Beyea failed to assess how Muller's control group's mutation rate compared with the data of Caspari and Uphoff. It matched closely with Caspari, discrediting Uphoff control group (Calabrese 2019a).

Beyea did not report that Muller completely misled the scientific community several years later by stating that the Caspari controls were aberrantly high and the Uphoff were normal, totally contradicting his earlier written communications with Stern. This raises fundamental scientific and ethical questions that have been ignored (Calabrese 2019a, 2024).

Beyea did not report that Stern, Muller, Uphoff, and the entire BEAR 1 Genetics Panel failed to discover that the Uphoff study had two simultaneous variables that invalidated her study that the BEAR Panel and Lewis depended on (Calabrese et al. 2023a)

Why didn't Beyea report that Stern and Muller and the BEAR I Genetics Panel failed to criticize the Spencer study for combining treatment groups with the same total dose but with different dose rates? Worse still, they depended on these findings (Calabrese 2019a, 2024).

Beyea failed to comment on the ethics of Stern, who published both the Spencer and Caspari papers in the journal *Genetics* while he was the editor and avoided peer review based on the timing of submission and acceptance (Calabrese 2019a, 2024).

Beyea did not report that the key chronic study experimental data of Uphoff have been missing for 80 y (Calabrese 2015, 2019a, 2022a, 2024).

Beyea failed to report that Stern tried to get Muller not to promote the research of Ray Chaudhuri at the Nobel Prize speech but to cite the threshold supportive findings of Caspari (Calabrese 2015a, 2019a, 2024).

BEAR Genetics Panel. Beyea failed to report that at the time of the BEAR Genetics Panel creation, Detlev Bronk was President of the US NAS, President of the Rockefeller Institute for Medical Science (later the Rockefeller University), on the Board of Trustees of the Rockefeller Foundation, and directed the US government's assessment of genetic and cancer risk assessment. Bronk was in a very conflicted position (Calabrese 2014, 2015a, 2019a).

Beyea failed to recognize the importance of the Russell control group error and correct/adjust the BEIR 1 and BEAR 1 mutation risk estimates and their public health and risk assessment implications (Calabrese 2024).

Beyea neglected to report that the BEAR I Genetics Panel knew of significant errors in their report to the public and never offered to correct them. They never publicly corrected that they did not write the report attributed to them. They never challenged the false information by the President of the NAS (Calabrese 2024).

Beyea failed to note that Muller tried to prevent Neel from reporting his findings showing negative results on the induction of birth defects in the 75,000 children he had studied over 10 y after the atomic bomb explosions in Japan and publishing them in a WHO Committee report in the fall of 1956 (Calabrese 2020).

Beyea reported that the BEAR I Genetics Panel reviewed and evaluated the Neel atomic bomb findings when it didn't. Beyea claimed to have access to the BEAR I Panel transcripts when the issue was discussed. This suggests that Beyea did not read these transcripts or ignored the findings (Calabrese 2020).

Beyea did not report that the BEAR 1 Genetics Panel provided the foundations for radiation emission standards for nuclear power plants based on the incorrect findings of Russell (Calabrese 2023a).

Beyea neglected to report that Muller had his close colleague James Crow appointed to the Panel after the Panel had been created to help him in anticipated debates with Dobzhansky concerning his genetic load premise (Crow 1995).

Beyea did not report that the research of Dobzhansky and Wallace challenged the genetic load hypothesis of Muller while still supporting an LNT model (Wallace 1991).

Beyea failed to report that the three youngest geneticists (Crow, Neel, Russell) of the BEAR I Genetics Panel years later disavowed their conclusions and greatly reduced their risk estimates (Calabrese 2024).

Lewis: Major radiation cancer risk paper in Science 1957. Beyea failed to report that Edward B. Lewis did not report the J-shaped dose response for leukemia in atomic bomb exposed populations (Calabrese 2021a, 2021b)

Beyea failed to report that Lewis did not report that radiation induced leukemia in other studied populations was not supported at lower doses (Calabrese 2021a, 2021b).

Beyea neglected to report that Lewis presented key factually inaccurate findings during Congressional Hearings on risks from radiation in 1957. These Hearings greatly affected US policies (Calabrese 2021a, 2021b).

Why didn't Beyea report on the impact of the Lewis paper on the NCRP and their recommendation of an LNT (Calabrese 2021a, 2021b)?

Post BEAR 1 activities

Gofman and Tamplin. Beyea did not report how the Gofman-Tamplin debate led to the creation of the BEIR 1 Genetics Committee in 1972 (Calabrese 2023a)

Beyea neglected to show how Gofman-Tamplin was discovered to be dishonest in public testimonies and its significance (Calabrese 2023a).

Russell. Beyea failed to report that Russell discredited the LNT model based on his presentation in 1970 at a major conference in France (Calabrese 2015, 2019a).

Beyea did not report on the Selby-Russell controversy and the DOE Hearings on the matter and its potential significance for risk assessment (Selby & Calabrese 2023).

Beyea neglected to report that Russell and Arthur Upton covered up a major radiation cancer study and its potential impact on government regulations in 1959/1960 (Calabrese & Selby 2022, 2023c).

Neel. Beyea did not report that both Russell and Neel would later change their radiation doubling dose values to exceed 100 R (1 Gy), directly challenging the BEAR I Genetics Panel estimates. (Calabrese 2020)

Beyea failed to report that the Neel negative genetic damage findings of the Atomic Bombs studies have been sustained for over 70 y (Calabrese 2019a, 2020, 2022a, 2024).

Other. Beyea didn't report that the so-called Gold Standard of the NAS/RERF cancer studies failed to collect smoking data for males for 20 y and for females for 24 y and that the collected data did not appropriately match control group data (Prentice et al. 1983).

Beyea failed to report that background metabolism produces about 200 billion times more oxyradicals than background radiation (Calabrese & Selby 2023a).

Beyea neglected to report that Muller misrepresented the scientific record at the first Pugwash meeting in 1957 (Calabrese & Selby 2024c).

Beyea did not report that multiple studies tested the genetic load hypothesis of Muller, such as the 82 generation study of Spalling, showing no significant effects (Calabrese & Selby 2024b).

Beyea failed to report that the additive to background LNT model assumption was not supported in a large study by Calabrese (2018c).

The Beyea smear campaign method

Beyea attempts to paint me as an industry focused, money-hungry academic. He cites a newspaper article in the *LA Times* (Rust 2019) that led to subsequent publications in *Mother Earth* and *The Huffington Post* that

were highly critical of me. Beyea uses a quote from the *LA Times* that links me to obtaining funding from the tobacco industry. Beyea then tries to be coy in his assessment of this, leaving the question up in the air---but the point is that Beyea tried to associate me with the tobacco industry after implying that I pursued hormesis for financial reasons. This is a type of professional and personal character smear, and it should not have been in the *HPJ* paper. If Beyea had been interested in being fair, he could have interviewed me to get both sides of a story and the reviewers/editor of the *HPJ* should have required it. In fact, I have testified in only one tobacco case, on the side of the plaintiffs, with the plaintiff winning the smoking lung cancer class action case against the tobacco industry. This information was provided during the *LA Times* interview, but the writer failed to include it. This failure of Beyea to seek out information on both sides of an issue shows again the bias that Beyea took to his efforts and that his perspective is misleading and unreliable.

Within this same context, Beyea cited the work of Shrader-Frechette who started her paper about me by claiming that I was responsible for an industry defendant winning a toxic tort case with the family of a deceased worker who died of brain cancer that was associated with exposure to ethylene oxide, a multiple organ carcinogen in animal models. Beyea did not report that Shrader-Frechette failed to inform the readership of the court's official reason why the case was won by the defense. The official court document (Allen v. Pennsylvania Engineering 1996) indicated that all of the plaintiff's expert witnesses were not permitted to testify because they were not deemed as being experts. Their credentials were not accepted by the court. No experts, no plaintiff's case - this is the end of the story. I had nothing to do with that decision. Thus, the plaintiff could not have a trial since it no longer had any testifying experts. The case was dismissed with prejudice without a trial. Again, one finds that a false narrative was provided to the readership instead of what really happened. Yet somehow Beyea's paper passes a clearly biased and ideologically based "fake" academic peer review that lacks professionalism.

Shrader-Frechette also claimed that I hid my industrial funding because an "official" CV on a UMass Marine Science website failed to list any industrial funding (actually it did not include any funding). This was a major point in her paper, and it became important in getting my UMass "inquisition" started based on UMass records. The answer should have been easy. My CV is now over 160 pages with over 1,100 publications and about 800 presentations listed (about 150 pages back then). The UMass group in charge of the website condensed the CVs of all participating faculty into a single page for each person following the same format. Thus, they reduced my CV by more than 99%, from about 150 pages to one page. The UMass administrators of that program determined what they wanted, not me. It included my educational background/degrees, job history, honors and awards and areas of expertise and the single page was quickly filled. Thus, the statement of Shrader-Frechette that I hid my industrial funding not only lacked credibility, it was a type of character smear.

Yet, the UMass administration (former Vice Chancellor and my former Dean) became influenced by her writing style, aggressive claims, and perhaps because she was a professor of ethics from the esteemed University of Notre Dame and used her unsupported claims to put me through a major formal ethics and research misconduct "trial." How could this have been possible? How could the administration have been so easily duped? In addition, as these UMass administrators should have known, my UMass funding is publicly available, including from when I started in 1976 as listed in the Office of Grants and Contracts website at UMass, as is the case for all UMass faculty, including theirs as well. There is no way to hide anything.

Yet, Shrader-Frechette's publication vehicle, the journal *Synthese*, printed these false assertions, acting upon these false narratives. Thus, Beyea cites a highly biased article that contained false information. I provided documentation during my "trial" that discredited the assertions of Shrader-Frechette, based on letters supplied by the UMass Marine Science group. They verified how CVs were modified/greatly reduced by their staff and made to be uniform across all the faculty and that I had nothing to do with their process. It was so easy to rebut that it should never have been published or used to impact UMass administrators. Much of the remaining information in the Shrader-Frechette paper followed the same biased path, with some other examples even more egregious than the two listed herein. In fact, I prepared a 50-page document to defend myself in the UMass trial to rebut her allegations, many of which were blatantly false. Again, Beyea failed to do a professional job, relying on false and highly biased secondary sources. Once again, Beyea followed a similar pattern and never made an attempt to interview me. By the way, Shrader-Frechette never interviewed me nor corrected the factual record, a similar pattern of Beyea.

Beyea rehashes the past and ignores current discoveries

Beyea spends most of his time rehashing the past issues of concern to him with almost no consideration of new discoveries made by me since the time of the documentary. I have published about 25 new discoveries with respect to LNT and cancer risk assessment from an

historical perspective that occurred after the HPS documentary. A new paper by me places these 25 new discoveries in a broader and more understandable context since the documentary (Calabrese et al. 2024).

Some of these discoveries directly contradict statements made by Beyea. For example, Beyea cites a 1928 *Science* publication by Patterson (Patterson 1928) as supporting LNT. The paper of Calabrese and Selby (2024d) about Patterson shows that Paterson's claim was not justified. Beyea cites a paper by Hanson & Heys (1928) supporting LNT. He, again, failed to cite a paper by Calabrese and Giordano (2023), which noted that Muller himself in a private letter reported that papers published by Hanson and Heys from the late 1920s into the 1930s could not be trusted because of falsification of the research record. Beyea also failed to cite the paper by Calabrese and Selby (2024a) concerning the research of George Snell, future winner of the Nobel Prize for immunogenetics in 1980, whose work in Muller's lab could not verify Muller's gene mutational findings when extended to mice. These several examples show either that Beyea is unaware of new developments in the field or is biased or perhaps a combination of both. Again, this research required the use of primary sources in order to make important new discoveries. In this case, Beyea failed to discuss our published papers as well as failing to obtain any primary source material, making his conclusions unreliable.

Beyea gets the Neel atomic bomb-genetic effects report study wrong

Beyea also focused on research presented by James Neel concerning the effects of the atomic bomb on the offspring of exposed individuals. My research (Calabrese 2020) showed that Neel gave a copy of his 241-page final report to the president of the NAS and offered it for review to the newly created BEAR I Genetics Panel. It was commented on in that fashion at the opening of the BEAR I Genetics Panel (November 1955). Yet, under the leadership of Muller, the BEAR I Genetics Panel never evaluated it because Muller publicly stated that its findings were illusionary (Calabrese 2020). Beyea fails to bring this up in his paper. Due to Beyea's lack of use of primary sources, he also failed to share that Neel would subsequently send the key final chapter of his major report to all the members of the BEAR committee so that they would at least be aware of it. The key information of the Neel study therefore was never discussed by the BEAR 1 Genetics Panel. This led to angry major public disputes between Neel and Muller in the summer of 1956 at a conference in Copenhagen that was written about in the *New York Times* and within a subsequent WHO meeting as discussed below. The details of that key episode are now summarized below.

Part 1---The Panel did not evaluate the Neel study data: Here is the story

In 2020, I published a detailed paper that explained why and how it came to be that the major genetic damage study of Neel and Schull, which became ready just at the time of the BEAR I Genetics Panel, was unceremoniously brushed aside (Calabrese 2020). However, Beyea (2024) states on page 5 (left column) that the BEAR 1 Genetics Panel assessed the major study of James Neel and his colleague William Schull on the genetic effects of the atomic bomb blasts on the offspring of exposed Japanese adults. He challenges my conclusion that the Panel chose not to assess Neel's findings due to the powerful influence of Muller. Beyea then states that "the principal sources of data for these recommendations were the mouse experiments of WL Russell, the data from human studies playing at that time---properly---a minor role (see Neel 1994)." Let us examine the facts.

We will find that Beyea got the issue wrong because he failed to properly assess the primary source of information, the BEAR I Genetics Panel transcripts; that is, the Gold Standard and related Panel letter exchanges. The BEAR I Genetics Panel did not assess the major Neel & Schull (1956) study that was handed to the Panel on a silver platter by Neel at the time of the very first meeting, thanks to Muller's influence. Secondly, both Beyea and Neel got it wrong. The BEAR I Genetics Panel recommendations (e.g., LNT) were not based on the Russell mouse data but on the historical reliance on fruit fly data and Muller's major evolutionary biology mistake when he assumed that there was no such thing as genetic damage repair (Calabrese et al. 2022). It is clear that the memory of Neel, now some four decades after the BEAR I Genetics Panel meeting, was faulty as shown by the transcripts. Beyea accepted the memory of the aged Neel rather than carefully checking the transcripts. Beyea made the same mistake of not checking the transcripts with Bentley Glass in his earlier paper as noted above (Beyea 2016). I was able to show definitively that Glass's memory was also faulty, again by using the transcripts of the BEAR I Genetics Panel (Calabrese 2017b).

The Neel story describes not only that incident but also that it led to a major battle between Muller and Neel that erupted 3 mo after publication of the BEAR I Genetics Panel Report in June 1956. The "explosion" occurred at a WHO meeting (First International Congress of Human Genetics) in Copenhagen, during which Muller and Neel got into such a conflict that it led to the *New York Times* writing a story about the battle between the Mullerians and the anti-Mullerians (i.e., the followers of Neel). The fight was over the use of Neel's epidemiological data for human risk assessment. The battle of the two titans of genetics spilled over the next week at a WHO Study

Group meeting in Copenhagen. At that meeting, Muller tried to prevent Neel from speaking and contributing his paper to the Group Proceedings. Neel was saved when the British contingent threatened to boycott the meeting. The hostilities continued in the US as Alexander Hollaender hosted a failed "reconciliation" meeting. The point of this brief summary is to show that all was not well with the BEAR I Genetics Panel as a result of Muller using his power and influence to block the use of the Neel & Schull (1956) study from use by the Panel. Just think how most researchers would react if their 10-y magnum opus on human genetics was blocked by a fruit fly researcher. The reader will see that Beyea's belief that the Panel reviewed the Neel & Schull (1956) report is an illusion. This battle and severe acrimony between Muller and Neel never would have occurred if Muller hadn't yanked the study from the Panel with Neel never anticipating that anyone would have done such a thing.

Here is the summary story [page 3, left column of Calabrese (2020)]: Based on transcripts of BEAR 1 Genetics Panel meeting, in opening comments on November 20, 1955, NAS President Bronk circulated among the Panel members 10 NAS reports concerning radiation, including an earlier version (date not identified) of Neel's ABCC atomic bomb offspring survivor study. After the statements of Bronk, Panel Chairman Warren Weaver inquired concerning the availability of Neel's updated and expanded Atomic Bomb Casualty Commission (ABCC) offspring survivor-genetics study. Shields Warren, who was both a BEAR 1 Genetics Panel member and chair of the ABCC, stated that Neel's major report would be available soon. Later in the meeting, James Crow would state, "We need to know more about man himself, about the effects of radiation." This comment was reinforced by Tracy Sonneborn, who stated, "I agree with Crow that we need intensive efforts to acquire information in regard to man. No amount of extrapolation is as relevant as the direct information on man himself." However, Muller soon put a halt to this discussion, focusing on human data with his statement that "We should be aware of reliance on illusionary conclusions from human data, such as the Hiroshima and Nagasaki data, especially when they seem to be negative." Following that exchange in the panel meeting, the issue of Neel's Hiroshima and Nagasaki study and his apparent "illusionary conclusions" were never discussed. At that key time during the meeting Neel was silent, letting Muller's criticism stand. Later that same day, Neel made a very limited rejoinder, indicating that "the proper study of mankind is man" but with no specific application to the Muller criticism and no follow-up action. The issue therefore of the availability, use, and importance of Neel's Hiroshima and Nagasaki study was never again brought up for discussion. It was also not included in the meeting summary and the Panel's goal statement by Weaver.

It is crucial to emphasize that in November of 1955 (exact date unknown) Neel indicated that he had sent Muller a mimeographed copy of the updated and expanded Neel-Schull (1956) report on the ABCC atomic bomb study. While Neel may have been under the impression that Muller had read the report, Muller (1956) noted in a letter to Neel (24 October 1956) nearly a year later that he had never read this report, being too busy (Calabrese 2020). Muller's dismissive and critical comments about Neel's report during the opening of the BEAR I Genetics Panel session were therefore speculative, premature, and judgmental, as Muller had admittedly never read the report and was not knowledgeable of its contents.

After those disappointing developments at the meeting of the BEAR I Genetics Panel, Neel wrote to Weaver with a copy to Muller on 23 January 1956 (Neel 1956a), stating that, "As you know, for some time now, I have been deeply involved in studies on the potential genetic effects of the atomic bombs in Hiroshima and Nagasaki. Dr. Wiiliam J Schull and I have been working for the past two years on the report on this experience and expect to get the final manuscript off to the press by April 1, 1956."

That last chapter of the Neel report (i.e., Chapter 15) (Neel & Schull 1956) indicated that the state of current knowledge regarding radiation-induced genetic damage of humans and any other animals was so inadequate that speculation concerning the long-range effects of radiation on the genetics of the human population was, according to Neel, "...to say the least, extremely risky." Even though Weaver strongly desired specific, quantitative risk assessment guidance, Neel emphasized that the human data from his ABCC research as well as the animal model data did not support this. In contrast, Muller indicated that any estimates that would be provided by the Panel would ignore Neel's study.

The next meeting of the BEAR I Genetics Panel was held on 5-6 February 1956. The most significant development during that two-day meeting was the adoption of a strong consensus that emphasized the belief in the linear no-threshold dose response for radiation-induced mutations based on a presentation by Tracy Sonneborn. At the end of the meeting on 6 February, Weaver challenged the geneticists to estimate at least three transgenerational adverse genetic effects that would occur in the US population, including those starting with the next generation (F1) and then out to 10 generations should the parents of the current generation be exposed to 10 R of ionizing radiation or if the exposure were 10 R for each generation. The imposition of the LNT assumption by Weaver was designed to enhance a convergence/agreement of damage estimates among the panelists, eliminating a major source of potential interindividual variation in predictions. Each geneticist on the Panel was to construct his own independent damage

estimation for the questions posed by Weaver and then send via US mail a detailed report before the next meeting on 1 March. In fact, three of the geneticists refused to participate since they thought that reliable estimates could not be provided. The science was not yet good enough. One of those refusing to participate was James Neel. He provided his multiple scientific reasons in letters to Weaver, which I have documented (Calabrese 2020).

This proposal of Weaver concerned Neel beyond his refusal to participate, prompting him to write to Weaver on 14 February 1956 (Neel 1956b), in which he stated, "You will recall that I stated at the recent meeting my conviction that our knowledge was far too fragmentary to permit a meaningful quantitative statement of this problem. My reasons for this are spelled out in some detail in Chapter 15 of the monograph which Dr. Schull and I are now preparing on the Japanese study. Although there are dangers in presenting this one chapter apart from this entire monograph, nevertheless I have decided to send a copy of this chapter to every one of the committee as soon as possible, probably within the next two weeks. While I do not expect to make many converts, this will perhaps make the reasons for my stand somewhat clearer." On 21 February 1956, Neel followed through and sent a letter to the BEAR I Genetics Panel, attaching Chapter 15 (Neel 1956c). He stated that, "You will recall that at our recent meeting in Chicago I expressed certain reservations concerning our ability to develop worthwhile predictions concerning the genetic effects of radiation. The paper supplies at least part of the details on which that position is based."

Together the transcripts and summary written by Bentley Glass (Glass, 1956) provide a more substantive reconstruction of the panel meeting on 5-6 February1956. The Glass write-up matches closely in content and time with the transcripts but not fully. For example, Glass noted that in the meeting, Neel said he would have Chapter 15 of the study mimeographed and sent to all members of the panel. However, according to the transcripts, Neel never made such a statement in the meeting. Neel may have mentioned this to Glass during a break or after the session had ended or not. As noted above, Neel would later write to Weaver on 14 February 1956 (Neel 1956b) about sending Chapter 15 to the entire panel. The 241-page report by Neel & Schull (1956) contained highly detailed research chapters on each end point and contained sections on research design, study methods, results, discussions, and perspectives. No statistically significant findings were reported for any of the end points.

Even though the study of Neel & Schull (1956) was a major development, there is no evidence that the NAS leadership made it available to the panel. In fact, the evidence contradicts this position. Neel only made Chapter 15 available, as it was focused on risk assessment issues, some two months after the Panel started their work, that is, in the second half of February 1956.

Yet, Neel's actions as seen in his 21 February 1956 letter (Neel 1956c) to the Genetics Panel shared only one of the 15 chapters. His memo expressed little, if there was any urgency but only a desire to be better understood. Further, the Panel members had prepared and sent their specific estimates of genetic damage to Crow before or about the time of receiving Chapter 15 from Neel. Although Neel could have used the next meeting to explain the negative findings of the atomic bomb study and his contrary position, this never happened. During the follow-up 1 d meeting on 1 March 1956 in New York City, six geneticists, including Neel, did not attend, thereby precluding discussion of his ABCC study findings and its potential for substantially impacting the Weaver assessment memo. In fact, a review of each of the assessments submitted by the 9 of the 12 geneticists who provided an independent written estimate reveals that none cited the material provided by Neel.

This reconstruction of the BEAR Genetics Panel activities within the Panel meeting activities demonstrates that Beyea is incorrect in his representations and failed to share this information with the readership. Beyea claims that the "...data [of the Neel study] were considered but Calabrese apparently did not find it." The above reconstruction indicates that the Neel report was available but was not shared and considered by the Panel. In fact, Neel was so concerned that he himself, not the chair of the Panel, even personally sent the Panel members Chapter 15 (only) toward the end of February. The evidence indicates that the receipt of the chapter did not affect any of the nine reports that addressed the Weaver assignment given on 6 February 1956. Weaver provided a follow-up letter (8 February 1956) (Weaver 1956a) to the Panel, clarifying the instructions for their risk estimation project. This story was spelled out in detail in the Calabrese (2020) paper, which Beyea cited but failed to reliably report.

Part 2---How LNT was adopted by the BEAR I Genetics Panel

This section shows how confused Beyea's understandings are with respect to how the BEAR I Genetics Panel derived the LNT. In many respects, it is a complex mixture of misunderstandings and mistakes that indicate that Beyea lacks an understanding of what he is alleging. These errors are now presented from two differing perspectives, which I am calling for simplicity's sake Beyea Mistakes #1 and # 2. Beyea has accomplished the seemingly impossible, creating two distinctly different mistakes on the same topic but via differing approaches and assumptions.

Beyea mistake # 1. Beyea makes the conclusionary statement that the LNT-based hereditary risk assessment for ionizing radiation was based largely on the mouse research of William Russell (Russell 1951). He based this conclusion on the nearly 40-y-old memory of James Neel, as given in his autobiography (page 321) (Neel 1994). The problem with this appeal to authority is that Neel's memory had faded and that Beyea had failed to check the transcripts of the BEAR I Genetics Panel meetings, especially on 5 February 1956, the first day of major Panel work after an introductory meeting the past November. In this telling meeting, the transcripts reveal that Tracy Sonneborn wrote a general guiding statement of principles for the Panel to follow. The framework of Sonneborn was based on four principles: i.e., that all doses of ionizing radiation were (1) harmful, (2) irreversible, (3) cumulative, and (4) displayed a linear dose-response relationship. The presentation by Sonneborn at the meeting did not provide reference citations. No member of the Panel challenged these perspectives. In fact, at the first meeting of the Panel back in November, Sturtevant asserted his scientific contempt for the medical profession that still strongly supported an "anachronistic belief" in the threshold dose-response model. Sturtevant stated that he had "no doubt about the correctness of the linear dose response." He also stated that any effort to further document support for it would only be for the propaganda value in order to "educate and convince the non-geneticists." The Panel's single-minded uniformity of belief regarding the nature of the low-dose response was profoundly significant as it tended not only to limit discussion and preclude debate but also to ensure adoption of their preconceived beliefs.

The apparent uniform belief was derived from the proposal of Muller in 1930 for a Proportionality Rule for the dose response for ionizing radiation and gene mutation (Calabrese 2015a, 2019a, 2019b; Calabrese & Selby 2023c; Calabrese et al. 2022). This Proportionality Rule was a precursor term for the LNT-single hit model that contained no genetic repair component. This idea was based directly upon Muller's belief that natural mutations were so infrequent that evolutionary novelty could not occur if nature had evolved the capacity to repair induced genetic damage. This was based on large scale observations of fruit flies in which only about 400 visual mutations were observed from observations of about 25 million flies. Thus, the original thinking behind the linear dose response was derived from Muller and his research with fruit flies. Later studies with fruit flies were used to support this conclusion, including some studies with fruit flies by Hanson & Heys (1928)[2], Oliver (1930), Spencer & Stern (1948), and Uphoff & Stern (1949). None of these studies were strong enough to test the LNT assumption in the low-dose zone. This dominant perspective of Muller was still quite apparent at the BEAR I Genetics Panel, as seen once again on 5 February 1956, with Sonneborn who stated that, "Ordinary consideration of life inescapably involves exposure to irradiation and other mutagenic agents, quite apart from the additional exposure due to the atomic age, medical use of irradiation and man-controlled superimposed mutagenic agents." Based on this framework, Sonneborn further stated that, "Inescapable mutation provides an ample means for evolutionary advances for genetic adaption to changing conditions of life. It also involves mainly genetic damage under present conditions. Additional mutations only add further damage without materially increasing the capacity to adapt and evolve. Given inescapable mutations, genetic adaption and evolution depend upon selection, not upon more numerous mutations." These perspectives can be traced based to the LNT single-hit paper of Timofeeff-Ressovsky et al. (1935) that formulated the linear dose response single-hit model. Thus, the historical foundations of LNT were established in the belief system of the radiation genetics community under the leadership of Muller and others for at least two decades prior to the creation of the BEAR I Genetics Panel.

With respect to the mouse research of Russell (1951), it gets introduced into the discussion only after Weaver challenged the geneticists of the Panel to estimate genetic damage from exposure to 10 R from 1 to 10 generations, under the assumption that the LNT model was operating. Nine panel members took up this challenge using the models with which they were most familiar, including bacteria, fruit flies, mice, and humans. Thus, the challenge of Weaver built in the assumption of linearity so that none of the applications used by the Panel members were designed to prove LNT, which was the most fundamental focus of the Panel. They only used it after it was given to them for application. The Russell data had no impact on the adoption of the LNT model recommendation of the BEAR I Genetics Panel. Thus, the conclusion of Beyea is incorrect.

Beyea mistake # 2. There is yet another mistake that Beyea made with his interpretation of the Neel (1994) book concerning the role of mouse data in the 1956 BEAR I Genetics Panel Report. Beyea quotes from the Neel book on page 321: "The principal sources of data for these recommendations were the mouse experiments of WL Russell, the data from the human studies playing at that time ---properly—a minor role." However, a reading of the Neel (1994) page 321 reveals that Beyea got the story wrong, totally wrong. The Neel paragraph begins stating, "Our Committee was reconvened in 1959. There

[2]Muller considered the Hansen and Heys research to be fraudulent as discussed and cited in this article (see Calabrese & Giordano 2023).

had been one extremely important genetic development since our original charge." The one major new discovery was that of Russell et al. (Russell et al., 1958), who published a groundbreaking paper on 18 December 1958 in the journal *Science*. It showed for the first time that dose rate rather than total dose predicted the mutational damage in mouse spermatogonia and oocytes. The findings suggested the existence of DNA repair. The findings were so important that Russell was nominated several times for the Nobel Prize. The findings, in fact, refuted two central claims of the 1956 Genetics Panel report that all radiation-induced mutational damage was cumulative and irreversible. Russell contacted Muller several weeks before the *Science* publication out of respect and to give him a heads up on these findings. These findings were commented on in the next BEAR Genetics Panel report in 1960. It summarized in a few sentences the Russell findings and that these findings had been confirmed with Drosophila. This confirmation was done in Muller's laboratory. Despite the striking findings and that they had discredited the cumulative dose and irreversible damage belief, the Panel felt that the quantitative features of these new findings remained to be clarified and no changes in guidance were offered in their 1960 report. The principal point here is that Beyea related these major findings of the mouse data of Russell et al. in 1958 back to being the basis of the recommendations of the 1956 report, which they weren't. In fact, the Russell et al. (1958) findings were discrediting the 1956 BEAR I Genetics Panel findings and their recommendations. Thus, Beyea did not understand and appreciate or perhaps even know of the findings of Russell and Muller, their timing and how they were evaluated by the BEAR Genetics Panel in their 1960 Report. Plus, Beyea incorrectly claimed that these findings were the basis for the 1956 recommendations when they could not have been and, in fact, discredited them. In addition, they did not even affect recommendations in 1960.

The entire story of the 1958 Russell et al. findings, how Muller replicated these findings, and how they impacted the 1960 BEAR report were the main focus of Episode 19 of the HPS Documentary. This suggests further that Beyea never watched Episode 19 where the story was told in great detail, sharing multiple primary documents that verified key aspects of the above summarized story. Beyea's assessment of the role of the Russell mouse data for the BEAR I Genetics Report is shown to be incorrect and thus unreliable. Beyea also failed to understand the paragraph on page 321 of Neel (1994), creating even more errors in his paper, making this section also unreliable.

Beyea continues to misread the Neel autobiography creating more misrepresentations

On page 227 (top) of his paper, Beyea quotes Neel's 1994 autobiography with Neel very positively reflecting on what a good job the 1956 BEAR Genetics Panel did and that the Report has stood the test of time with no significant criticisms. At least this is how Neel remembered it. Now if only Beyea and Neel had read the same Neel autobiography more closely. In Chapter 12 of that autobiography on spontaneous mutations, Neel revealed that the background mutation rate was much higher than leaders like Muller believed back in the middle decades of the last century. As Neel writes on page 227 **"A genetic generation ago, when mutation was envisioned as a much rarer event than the evidence now suggests, H.J. Muller envisioning the human species as delicately balanced as regards its ability to cope with (i.e., eliminate) deleterious mutations, could adopt an extreme viewpoint concerning the genetic risks of increased exposures to radiation. But if the species is already coping with substantial 'spontaneous' mutations — to be sure, in ways not well understood — this alters the perspective on the risks of radiation."** Thus, Neel was clearly stating that Muller greatly overestimated the relative risk of background-radiation-induced mutation to spontaneous causes.

In the next chapter of the Neel autobiography, on page 243, Neel writes in the 30 or so years since the 1956 BEAR I Genetics Report, the doubling dose for spontaneous mutation has been revised to values now **"...about four times higher for humans than the 'official' rate, no small revision."** Based on follow-up research by Neel and a colleague, Neel noted **"that when all the data available were analyzed (and all data sets given equal weight), including some results that had been looked at askance because they didn't reveal much of an effect, the average doubling dose for acute radiation was 1.35 Gy. Applying the dose conversion factor 3 mentioned earlier [i.e., from the Russell dose rate data] to extrapolate to the doubling dose of low-level chronic radiation, we obtained a value of 4.1 Gy (i.e., 410 rads), in unexpectedly good agreement with the conclusions from the human data"** (page 244). This value compares with the range of 5-50 rads that was debated by the BEAR I Genetics Panel in their 1956 discussions. What Neel reveals is that the doubling dose estimates of the BEAR I Panel were off by a lot, about a factor of 10.

In light of this development, one has to reflect on the Neel comment that there were no significant criticisms. Yet there is Neel saying that the 1956 Panel overestimated the risks by about an order of magnitude. Also, this book was published in 1994, two years before the DOE made the Russells correct their control group data, which was too high by 120%. When such a correction was applied to the BEAR I estimates, there was no significant treatment effect at low doses, thus fully invalidating their mutation risk estimates. These comments show

that Beyea was incorrect in using Neel to claim that the BEAR I Genetics Panel did a great job that has withstood the test of time. Even Neel's comments on doubling dose contradict this, and Beyea should have reported this contradiction. Furthermore, Beyea also failed to report the criticism of the DOE just 2 y after Neel's autobiography book was published. The entire DOE story was given in the HPS documentary. Beyea also failed to report that the President of the NAS Detlev Bronk refused to share the technical reports and other work products of the BEAR I Genetics Panel with the scientific community, with most of the reports using the fraudulent data of Russell for their estimates and also hiding the fact that the highly divergent estimates of Demerec and Wright were excluded (Calabrese & Selby 2025), thereby hiding the large degree of uncertainty and variability within the Panel. This action of Bronk would have further prevented possible criticism of the BEAR I Genetics Panel report.

Beyea misrepresents Neel's view of the BEAR I Genetics Panel risk estimates

In his Question 2 section, Beyea portrays the views of James Neel largely based on Neel's (1994) reflective autobiography. Beyea summarized the elder Neel as being pleased with the BEAR I Genetics Panel report for its clarity and that the report did "justice" to the data. Thus, according to Beyea, Neel was pleased with the outcome of that Panel's activities. However, "the real" Neel of the BEAR I Genetics Panel had a different view than what was "remembered" by the elder Neel, then about four decades later. Exactly how pleased was the "1956" James Neel with how the Panel handled the data? Let us take a look.

It is critical to note that Neel refused the request of Warren Weaver, made on 6 February 1956, to the 12 geneticists on the Panel to estimate the genetic effects of 10 R (0.1 Gy) to the human population over 1 and 10 generations due to the excessive uncertainties of any estimates. On 8 March 1956, Neel wrote to Weaver (Neel 1956d) after seeing the estimates of the nine geneticists who submitted estimates. Neel wrote: "I have been going over with great interest the estimates of the various members of the committee as they have been coming in. I still remain an unreconstructed rebel with respect to the question of whether we are in a position to make meaningful quantitative estimates concerning the genetic impact of radiation on human populations. I seem at this point to be completely overruled, but I shall go down with flags flying and guns booming to the last."

On 16 March 1956, Neel again writes to Weaver (Neel 1956e) telling him that he has shared his major ABCC report with the British counterpart study committee, which had spent a great deal of time going over the first draft of his report on the Japanese experience. He indicated that the British were now very familiar with "many reservations which I had when it comes to the matter of calculating genetic damage from a given dose of irradiation at the present time." He noted that his thinking was much closer to the British committee than his own BEAR I Genetics Panel. In that same 16 March 1956 letter, Neel claimed that any possible convergences of Panel estimates were highly biased due to the actions of James Crow, who tried to force the geneticists to use similar estimates for gene number, mutation rates and other parameters for mice and fruit flies, creating likely false agreements.

One month later, on 17 April 1956, Neel again writes to Weaver on the same issue (Neel 1956f). Neel writes: "…in my role of the loyal opposition, there is at least one point that I would like to pound away at. In your memorandum of April 9, you say the geneticists do not escape their social duties by standing mute, for that decision leads to consequences. I hold that sometimes it takes more courage to stand mute and not be swept along with the crowd. In this case, I honestly believe that our calculations are tenuous to a degree where they may mislead rather than instruct. The geneticist has social responsibilities, but he also has responsibilities as a scientist. One is that in the area as critical as this one is, he must be aware of letting his conjectures get too far in advance of the facts. It is to me an exceedingly tenable position, having stated the general genetic argument, to say flatly that we know so little about the quantitative aspects of the problem that as good scientists we can only recommend that a major effort be thrown in that direction." Neel then praises part of the report by stating: "…for myself I would like to give a hearty endorsement to the part of our report that states the problem."

Neel therefore is quite clear where he stands on the challenge that Weaver gave to the geneticists and Neel's belief that the science, at that time, was not mature enough to provide adequate guidance. Of great importance is that Neel then writes: "I would like to be rather specifically dissociated from those parts of our report which attempt a quantitative treatment of the genetic risks of irradiation, in terms of the numbers of mutations produced, in which give specific dosage recommendations as regards 'permissible' levels of irradiation for populations of workers in Atomic Energy installations."

In the 12-13 May 1956 Agenda for the Panel meeting in New York City, Section 3 addresses the issue raised by Neel in his 17 April 1956 letter (Neel 1956f). The Agenda states the following: "Dr. Neel considers that the quantitative estimates…are not sufficiently definitive or sufficiently firmly justified to be useful. He therefore wishes to dissociate himself from these particular portions of the report."

The above series of letter quotes and now the Panel Agenda for 12-13 May 1956 show that Neel was consistent in his view that the science did not support providing reliable estimates of radiation-induced genetic risks. So strongly did he feel about this issue that he refused to participate, wrote about his views several times to Weaver, and demanded that his name not be associated with those sections of the report. This is a strikingly clear and consistent view. It is a perspective that also challenges the information provided by Beyea who relied upon the 1994 autobiography of Neel and his then 40-y-old memory of the Panel activities. The gold standard is what was written as shown here. It is more reliable than a 40-y-old memory. Beyea had an obligation to share with the reader the full story on the Neel views. However, again, by failing to discuss primary sources, Beyea ends up providing an unreliable perspective to the readership. This activity of Beyea is common and distorts the historical and scientific reality.

BEAR 1 and the fraudulent derivation of LNT: Ignored by Beyea

In addition to his numerous errors of commission, Beyea likewise makes important errors of omission. For example, in the case of the US NAS BEAR 1 Genetics Panel, in 1995, the US Department of Energy (DOE) forced William and Liane Russell to correct major errors of underreporting cluster mutations in their control group over their decades of research based on a major investigation (Russell and Russell 1996, 1997). This resulted in an adjusted/corrected 120% increase in control group values, showing how massive the Russells' error was. Of considerable importance is that many BEAR 1 Genetics Panel members used the flawed data of the Russells in their genetic risk estimates, a point that was emphasized by Selby (2020), a paper not cited by Beyea. If the BEAR 1 Genetics Panel had used the correct data of the Russell study, the radiation-induced mutation risk estimates would not have been statistically significant; that is, there was no treatment effect for males and females, supporting a threshold and not an LNT model that all the BEAR I Panel members were compelled to assume/use (Calabrese & Selby 2025). If Russell had not provided the Panel with the incorrect control group data or if the Panel had properly questioned Russell, as Selby would later do in the mid-1990s, the 1956 report of the Panel would have been profoundly different. This fact was not mentioned by Beyea, yet it was well known. Calabrese (2017b and 2017c) had shown that Russell's uncorrected data were used by BEIR 1 (1972) for hereditary risks as well. Beyea also failed to report that William Russell, a panel member, provided the flawed data to the Panel and used it to attempt to derive his own risk assessment estimates. The entire episode discredits all the efforts of Crow and the Panel to estimate mutational risk effects. It also discredits the Tables of damage shown by Crow, the historians of science, Jolly and Beatty, and others. In summary, the initial data were flawed, and that error continues to propagate to the present time, thereby propping up a false LNT narrative in US regulations and policies.

Failure to share this story with the readership is a major failing of the Beyea paper, and it misled the readership on this critical issue. His article provides unreliable and incorrect information on foundational issues.

Bayea also misinformed the readership about the extensive and unacceptable uncertainty and variability of the geneticist mutation risk estimates even when using the flawed data provided by Russell. Many memos James Crow sent to Warren Weaver (which my work has documented on this topic) detail the concerns of Crow (see the next section for the Crow memo assessment). Secondly, I made my uncertainty/variability estimates based on the incorrect data from Russell. A comparison of the mutation estimates for the F1 generation was provided for the nine participating geneticists, including the estimates of Demerec and Wright, based on a composite assessment by Crow (12 March 1956), thereby having the same criteria for a fair direct comparison. This direct comparison was provided by Calabrese (2015b) in the Supplementary section, a reference not cited by Beyea. In addition, the Wright estimates ranked among the lowest of those listed by Crow in his three categories of damage. In the case of Demerec, he reported F1 damage only; it was by far lower than even Wright's estimate. The point here is that Beyea is incorrect again with his characterizations, which are also framed within the context of using the incorrect data of Russell. Beyea could have arrived at a similar conclusion had he used the primary literature or reviewed the Calabrese (2015b) paper.

Beyea tries historical reconstruction lacking primary documents: overwhelming evidence supports scientific misconduct by the BEAR 1 Genetics Panel: Beyea wrong once again

Of considerable importance is that the James Neel insistence that current science was not adequate to provide reliable risk estimates of radiation-induced genetic damage was an amazingly accurate predictor of the vast uncertainties in estimates and variations that were provided by nine BEAR I Genetics Panel geneticists. Alfred Sturtevant, a geneticist member of the Panel wrote to Weaver on 20 February 1956 expressing extreme frustration with the assignment (Sturtevant 1956). Sturtevant wrote: "After going through these calculations I come out with a feeling that they are rather futile. At almost every step it has been necessary to make a guess, often

with little to go on, and with no real basis for setting limits within which the true value probably lies." What Sturtevant was expressing here was the phrase, "garbage in, garbage out." He admitted that he could not do a professional job, let alone offer the country and entire world legitimate high-level guidance. The world community was hoping that the Panel could provide this. However, it was quite evident that it could not deliver. What would be shared with the world was largely a professional mirage that misled everyone. The fact that this effort was undertaken and published was worse than a joke. It was a high-level failure of their ethical responsibilities. Yet Sturtevant and others would keep their massive ignorance and uncertainties all in-house, protected and quiet.

Weaver saw the Sturtevant letter about his horrific uncertainty and yet wrote in *Science* (page 1163, right column): "Each thus said, in effect: 'I feel reasonably confident that the true value is greater than my minimum estimate and less than my maximum.' My best judgment, as stated in a single figure, is what I have labeled the most probable estimate." The statement of Weaver was terribly dishonest, especially with Sturtevant writing "...all that I really feel confident is the sign of the effect…," thereby directly contradicting the statement in *Science* by Weaver, a fact that Beyea was unable to know and acknowledge because he only uses secondary sources.

With such independent perspectives by the most prestigious people in the field, such as Neel and Sturtevant, there is little surprise that the estimates that Crow received from the nine participating geneticists were all over the place, a situation such as "the blind leading the blind." Being a mathematician and not a geneticist, Weaver had no idea what he was demanding when he created this mess, forcing a crisis upon Crow.

This massive individual geneticist uncertainty and lack of agreement amongst the geneticist experts created a serious concern with James Crow and would lead to a troubling professional and scientific coverup. Crow informed Weaver via several memos that if the scientific community and public became aware of such uncertainties, then none of the potential Panel recommendations, such as those dealing with public health and occupational/industrial health workplace practices, would likely have impact and be adopted. All this could easily have been predicted had Weaver taken seriously Neel's longstanding experience. Within 5 d of being assigned the task of organizing the risk estimates of the nine geneticists, Crow informed Weaver of their predicament. On 7 March 1956, Crow wrote to Weaver (Crow 1956a) saying that, "Upon looking at the estimates I realize…that nobody seems to have very much confidence in them." Crow then made what would be the key, yet ominous, statement: "…I shall pool these as best I can." So, what did he mean by this?

Consider first the assignment/responsibilities that Weaver had given Crow

In a 2 March 1956 brief summary (Weaver 1956b) of the March 1 meeting, Weaver indicated that "Jim Crow was made the chairman of a subcommittee (he can commandeer help on this if he wishes) to go through all the damage estimates, compare them, and display assumptions, methods, input, and results in some sort of chart or graphic form." This description was very specific, and it did not indicate that Crow was authorized to exclude individual contributions nor to make judgments as to which estimates were the most/least credible or what should be shared with the scientific community or general public. Crow's role as subcommittee Chair was important but limited to organizing and integrating the submitted estimates in a coherent manner so that the entire Panel could intelligibly, efficiently, and objectively view the submitted estimates. As we shall see, Crow went beyond what Chairman Weaver had authorized, making decisions concerning the estimates to keep and what to exclude, without any Panel discussions, apparent consensus, and votes.

Now some 5 d later, Crow wrote to Weaver on 12 March 1956 (Crow 1956b), informing him that he had excluded the estimates of Demerec because they were based on different assumptions than those using fruit fly and mouse models that led to, "...a greatly different value than the others obtained." That is, the estimates of Demerec were far lower than those using the fruit fly and mouse models. In the case of Wright, his assessment counted, "...mutations causing conspicuous effects in postnatal life." In point of fact, Crow misrepresented what Wright had actually done. Crow stated that the risk estimate report of Wright was based on, "counting mutations causing conspicuous effects in postnatal life." However, the Wright document states that his analysis included the "conspicuous detrimental effects on viability or fecundity." Thus, the effects estimated by the Wright analyses would not have been restricted to those occurring in postnatal life, thereby contradicting the statement of Crow. This would prove to be a significant error by Crow, affecting the outcome of the Panel by Crow's decision to exclude the Wright estimates. Crow also noted that Wright's methods were "greatly different," but whatever that meant is not clear. However, Wright was considered the most outstanding mathematical talent on the Panel, being far beyond those considered excellent. It is doubtful that Crow would challenge any detailed evaluation of Wright. As in the case of Demerec, the risk estimates provided by Wright based on human data were far lower than those derived from the fruit fly and mouse estimates.

On 24 March 1956, it appears that Crow knew he was on scientific thin ice, writing to Weaver (Crow 1956c) that, "This may be too late to be helpful, but for what

they are worth here are my comments. I have considerable doubt about the wisdom of the set of estimates in section XII. The limits are too wide, so maybe no estimate is better." He went on to propose an alternative based on a recommendation from Joshua Lederberg (i.e., a 1958 Nobel Prize recipient along with panel member George Beadle). Lederberg recommended that they base the estimates upon mouse data, dropping even the fruit flies. He claimed that it could give very precise estimates even if surrounded by considerable uncertainty. Lederberg noted that "much of our information in medicine (for example, safe concentrations of industrial poisons and insecticides) comes from extrapolation from experimental animals, extrapolations subject in many cases to greater uncertainties than we are dealing with in our study." In fact, it was too late as he continued to move the quantitative estimate agenda forward. In that same letter, Crow informed Weaver, "I think you give the impression that the seven estimates were independent, whereas in fact six of them made almost identical assumptions." In fact, based on the 24 March memo, it appeared that Neel had an impact on Crow.

In his 29 March 1956 letter to Weaver (Crow 1956d), Crow reveals that he had made a decision that the estimates of the Panel will be based on the data from fruit flies and mice only! Despite his unauthorized and therefore unjust exclusion of the bacterial and human data, Crow was still greatly concerned with the dominating and overriding political issue, stating that: "The limits presented on our estimates of genetic damage are so wide that the reader will, I believe, not have any confidence in them at all. I suggest one of two things: a) omit the estimates entirely, or b) give a single best estimate of the number of mutations, or a narrow range of estimates, based on direct extrapolation from mouse and Drosophila. We then state that those are based on mouse data and let the reader add his own safety factor."

It is clear from the statements of Crow that even with the improper inclusion of only the fruit fly and mouse estimates, their agenda was still in major trouble. If they had included the bacterial and human estimates, it would have gone beyond having no confidence in their estimates at all, the equivalent of professional ridicule for this very distinguished Panel. In fact, the Panel would have been exposed as not very expert at all, a group that was acting outside its professional wheelhouse, a blow to the prestige of the National Academy of Sciences as well, perhaps even more so.

On 21 May 1956, nearly 3 mo after Crow first raised his concerns to Weaver on 7 March 1956 about massive uncertainty among the BEAR I Geneticists with respect to their genetic damage/mutation risk estimates he still cannot shake this issue. In his letter to Weaver, Crow writes: "I don't like to include that table on page 22; in fact, I thought we had agreed not to do this. The wide limits, and the wide differences between the different estimates of the limits made by the different persons, stem largely from differences of opinion as to the uncertainty in going from mouse and Drosophila to man…. Once again, I urge that we not include the table at all."

Crow and Weaver quickly found themselves between a so-called rock and a hard place, with no apparent escape. Acting in a bizarre manner, the only way out of this dilemma was for Crow to constrain the estimates to those based only on fruit flies and mice. However, Crow and the Panel would still have the problem of the remaining uncertainty being too great. How could he and the Panel find a way around this remaining major scientific roadblock. It was actually easy, but one had to be dishonest to achieve it.

Here is how Crow and the Panel solved their problem. They got around this remaining excessive uncertainty and variability issue by now claiming that the uncertainty was 100-fold, something they must have felt would work well with the scientific community, even though it was highly variable with two of the six "expert panelists" expressing uncertainty of 2,000-fold. The Panel was simply overwhelmed with uncertainty but did not want to acknowledge it publicly. The Panel then refused to share their findings, that is, how they derived this value, when members within the scientific community challenged the Panel. It soon became a career-threatening drama for all the members of the often-cantankerous Panel. They even got NAS President Bronk to participate in this scientific fraud by convincing him to deny a request from outside scientists to have access to the work product of the Panel.

This secret was hidden for 70 y, until I discovered the scientific coverup. This story has been documented in my publications and summarized in the documentary. By removal of the estimates of Demerec and Wright, it became clear that the overall variability in the group estimates would be markedly reduced, suggesting a way to deal with the high variability issue. However, the story was still not complete as there remained the Kaufmann estimate for human responses. His estimates were the third from the lowest, just above those of Demerec and Wright. So, Crow easily removed the Kaufmann estimates, now bringing the total usable estimates to six from the original nine, even though they were as legitimate as any that remained. Within this context, it is important to see what the conclusion of the Kauffman Report stated: "Calculations show that under the defined conditions the visible genetic damage resulting from chronic exposure to 10 r per generation is small in comparison with that of spontaneous origin." This general conclusion matched that offered by Demerec as well, yet it would remain

hidden by the actions of Crow and the Panel. In fact, this was most likely the reason why this report was dropped by Crow.

When the Panel published their technical report in *Science* journal in June 1956, the Panel included information ONLY for the results of the first generation (F1 generation). The *Science* paper did not provide any information on the biological models and methods used. That is it. Even though estimates were requested by Weaver for "at least" three areas (first generation with one radiation dose, 10 generations with one radiation dose, and 10 generations with 10 radiation doses/one per generation), the only information EVER provided to the scientific community by the Panel was the estimated response of a single generation (i.e., F1 generation) as summarized in the *Science* publication. In fact, this was enough to get their point and concerns across with no need to further complicate the issue. This F1 estimation information was also provided in the reports of Demerec, Wright, and Kaufmann—but Weaver, Crow, and the remaining Panel members prevented their estimates from being shared in any fashion. Furthermore, even though the *Science* journal paper indicates that six geneticists contributed the estimates, no specific information was provided that identified who provided the estimates or what any specific Panel member estimates were. The report of the Panel was largely derived by deliberate scientific misconduct and with an organized and firm cover up, all passed up to the President of the National Academy of Sciences. The cover-up also involved the journal *Science* since Bentley Glass, a member of the Panel, was also a senior editor at *Science* at the time. It became quite the scientific cabal.

Beyea tries to dispute this story. In his approach Beyea never obtains the primary information, relying on secondary sources. In the case of the Panel, this approach of Beyea is inadequate. For example, I had to make an extraordinary effort to get complete files on all memos, letters, and draft reports of the entire set of all the players in this risk assessment drama. I did this because I was concerned that no single person's file would be complete. So, I purchased the files from all the participants and organizations and painstakingly pieced them together to see how much overlap and completeness there was. Then I worked to create a master file that would be as complete as possible, with thousands of pages and hundreds of documents. These are documents that would reveal the story that I have published and briefly summarized here. The "hidden" story was hidden for a reason, and the literature is hard to identify, obtain, and integrate. Of critical importance is that Beyea tries to create his version of the story without obtaining the original documentation, where the facts can be found. Having undertaken the massive effort of reconstruction and then privately sharing my developing story for critical feedback with several interested senior scientists with a profound interest in the topic prior to submitting my work for publication, I can only view the efforts of Beyea in this area as grossly inadequate, within the context of thinking that he could develop an historically based explanation without examining the primary source documents. While Beyea may offer his speculative hypotheses and criticisms, they are based on grossly inadequate foundations that are not supported in the factual record.

Several specific examples of what Beyea employed to support his hypothesis that there was no scientific misconduct by the BEAR I Genetics Panel will now be assessed. Beyea cites one source as the basis of his hypothesis, that being the paper by the historian of science, John Beatty (2006). Beatty stated that Wright, Neel, and Demerec "...explicitly refused to contribute a number to the final report." We know that this is the case for James Neel, as it has been thoroughly documented with his story being deeply integrated into the present assessment. However, in contrast to the very explicit and documented criticism of the Weaver assignment by Neel (i.e., refusing to provide detailed estimates), neither Wright nor Demerec acted in the manner of Neel. They contributed highly detailed assessments that must have taken considerable time to produce and finalize and provided them to Weaver within several weeks of the given assignment. Careful reading of their cover letters and reports provides no evidence that supports the undocumented assertion of Beatty about the so-called, "explicit refusals" of Wright and Demerec.[3] No evidence supporting the statement was given in the detailed dissertation of Jolly (2003), which had a strong focus on the BEAR I Genetics Panel. My repeated requests to Beatty for clarification of this issue via emails (12 February 2025 and 18 February 2025) and recorded telephone messages received no response. In addition, multiple detailed letters/memos from Wright and Demerec to Weaver sent from March to May, that is, after the estimates were provided, offer no evidence of the "explicit refusals" or refusals of any kind. In the case of Wright, he became embroiled in an acrimonious debate with Muller over Muller's assertation that mutations should be considered within the framework of a "genetic death" concept. Wright's opposition to

[3]In his discussion, Beyea states, "Calabrese is apparently unaware that one of the excluded three committee members (Sewall Wright) did not want his estimate used (Beatty 2006) and the other two did not include all expected mutations in their calculations, so their reports were reported separately in the report." Beyea was "apparently unaware" that I sent an email to Beatty on 24 February 2016 stating: "On page 63, it is stated that several distinguished geneticists, including Wright, Neel, and Milislav Demerec, explicitly refused to contribute a number to the final report. I am aware of the Neel refusal but not of Wright and Demerec. Could you provide me with the source that supports that statement?" Beatty acknowledged this email, noted that his papers were in storage and would provide a response/answer to me. He never provided an answer to this request. The point of this story is to highlight that Beyea continues to make mistakes and offer unreliable speculations and is shown to be often wrong. In this case, his style is to cast doubt about my professionalism and knowledge to the reader.

this interpretation started mildly in writing as early as 17 April 1956 in a letter to Weaver (Wright 1956).

The issue soon became very inflamed with Panel member supporters on either side. Threats were made by Muller and Wright that they would not sign the final report if the other side's view on the genetic death concept predominated. Weaver diplomatically resolved this issue with the story behind the story told 40 y later by Crow (1995). This dispute was not related to the original Weaver assignment. Thus, there is no evidence that the assertion of Beyea is supported. Of importance, once again, is that Beyea depended entirely upon a single, and very limited, in scope, secondary source, when it was necessary to obtain primary documents. Beyea also stated that the Wright and Demerec findings, which they "explicitly refused to contribute to a number in the final report," were nonetheless acknowledged and cited in the Panel report, but there is no evidence that this was the case. In fact, the findings of no specific geneticist are identified in the *Science* journal publication or the Report to the Public. In addition, Beyea did not seem to understand that the only endpoint reported in the *Science* journal paper was that of the mutation estimate after one generation. This information was provided by Demerec, Wright, and Kaufmann, as documented by Crow, and therefore could not be a basis for their exclusion. In addition, the entirely documented set and sequence of the Calabrese description of the BEAR Panel activities was not challenged in the Beatty (2006) report.

Beatty (2006) also makes a misrepresentation of the factual record on the Panel activities. He stated on page 64 of his 2006 paper that, "While it is true that six of the geneticists of this committee considered the following problem, it is truer that all thirteen geneticists were asked to participate and seven declined." The statement of Beatty is in error in several ways. First, while there were 13 original geneticists on the Panel, Professor Charles Cotterman from Baylor University withdrew after the 6 February 1956 meeting due to academic obligations, leaving only 12 geneticists (Weaver 1956c). Beatty (2006) mistakenly infers that Cotterman would have affirmatively declined to offer genetic risk estimates. However, in the telegram to Weaver, Cotterman stated in capital letters: **"Under other pressures have been unable to work on project or read communications for many weeks and therefore have no suggestions stop as conditions show no prospect of change regret I must resign as useless committee member stop travel allowance and documents will be returned for redistribution."** It is clear from the Cotterman telegram that Weaver shared with Neel on 28 March 1956 that Beatty was incorrect to suggest that Cotterman remained on the Panel but refused to do the assignment. Secondly, Beatty stated that seven geneticists declined to participate and did not provide estimates, when in fact, I possess the submitted copies and cover letters of the nine participating geneticists received by Weaver. That Beatty would write in this manner suggests that he did not possess copies of these critical nine submitted reports and their cover letters at the time he published his 2006 paper on which Beyea relies. Based on the above information, the assertions of Beyea are not supported. As such, the *HPJ* reviewers and editorial leadership should have required that a higher-level professional effort be made by Beyea. Beyea persisted in his absolute reliance on secondary sources, such as the Beatty (2006) paper in the present case. The present assessment shows that the Beatty paper provided incorrect information of a highly relevant nature that has never been corrected in the record. Yet, this secondary source is the Gold Standard for Beyea rather than obtaining the primary documents himself. This assessment indicates that Beyea failed to do an appropriate professional effort, providing unreliable information.

More Beyea errors concerning the BEAR I Genetics Panel risk estimates

An important concern is that Beyea did not carefully read the BEAR I Genetics Panel 1956 *Science* journal paper. It informs the reader that Panel geneticist estimates are based on a single 10 R (0.1 Gy) exposure with effects estimates in the next or F1 generation. That is, the estimate is presented for a single (i.e., the next) generation, not the 10 generations that Beyea claims or possibly Total Damage that affects all descendants.

In the Genetics Panel 1956 *Science* paper, it was stated that *"Six of the geneticists of this committee considered the following problem: suppose the whole population of the United States received one dose of 10 roentgens of radiation to the gonads. What is the estimate of the total number of mutants which would be induced by this radiation dose and passed on to the next total generation of about 200 million children?"* (italics added).

Making the situation far worse is that the poorly reproduced figure in the Jolly dissertation (p. 350) that Beyea depended upon removed one name entirely from the F1 graphed estimates and so poorly reproduces three other geneticists' names such that they cannot be read under the largest computer provided magnification and then superimposing a further magnification of 20-fold via a magnifying glass. In addition, the Jolly figure failed to even describe the graphed information concerned; that is, there was not even notification of the type of genetic damage given (i.e., F1 genetic damage estimates). This poor and totally unacceptable figure reproduction by Jolly that

Beyea used prevents the reader from assessing differences in geneticist estimates and their uncertainties. Once again, the failure of Beyea to obtain primary documentation led to Beyea being incorrect in his fundamental understanding of what the estimates of damage were for each geneticist and of the Crow presentation. That Beyea would not attempt to obtain the original Crow estimates in light of the grossly inadequate Jolly reproduction raises serious questions about his methods. Beyea passes this confusion and error on throughout his entire paper. These limitations and mistakes contaminated the recent Beyea paper and affect anyone who uses or cites the earlier Beyea (2016) paper.

This situation became greatly aggravated when Beyea failed to acknowledge the subsequent primary literature memo of Crow to Weaver in which he claimed that estimates would now be based on only fruit flies and mice, leading to the dropping of the Wright estimates, which were by far the lowest of the remaining estimates, realizing that Crow had earlier dropped the estimate of Demerec based on his research with bacteria, which provided the lowest overall estimate for the F1 generation.

It is now clear that the estimates for the F1 generation were the only ones reported in the official technical BEAR 1 Genetics Panel report in *Science*, June 1956. It is also clear that Beyea relied on the flawed graph in the Jolly dissertation and did not obtain/consult the multiple memos of Crow on the issue with his inclusion and exclusion of studies that were strongly influenced by how they affected the occurrence of geneticist uncertainty and variability.

Thus, in summary, the 12 remaining geneticists of the Panel were invited to provide estimates, but only nine did. Crow initially excluded the values of Demerec because they were far lower than the other reported values. Nonetheless, the Demerec estimate was based on extensive research with bacteria in his own laboratory. That estimate was very professionally done with "data for 28 genes with regard to frequencies of spontaneous and X-ray induced visual mutations." From these studies Demerec reported the radiation dose needed to double the spontaneous mutation frequency, a critical need for the Panel, yet Crow discarded it. In fact, the report of Demerec offered its own experimental findings that were extraordinarily comprehensive. Demerec only provided estimates for the F1 generation, claiming that estimates out to 10 generations were too speculative. Crow provided the estimates of the remaining eight geneticists in his 12 March 1956 letter (Crow 1956b) for three genetic damage categories. It was clear that the estimates based on human data for Wright were consistently far lower than those provided by fruit fly and mouse data. In the next memo Crow indicates that the estimates of risk would be based on only these two species, thus dropping the human estimates (Crow 1956c).

In summary, Beyea made key mistakes in his 2016 paper that were never corrected and affected the Beyea (2024) paper in *HPJ.* The incorrect understandings of the Crow geneticists' mutational risk estimates, his misreading of the Crow graph via the use of the inadequate graph reproduction of Jolly (2003), and the failure to research the evaluations/judgments of Crow via the primary literature led to these multiple errors. These errors then led to additional data interpretation errors and strikingly incorrect criticisms of my research publications as noted in this paper. The information that Beyea provides is factually incorrect and interpreted improperly, providing information and interpretations to the reader that are not reliable. In addition, and most important, is that the entire Crow estimate exercise is scientifically meaningless since numerous Panel members based their estimates on the flawed and incorrect Russell data.

Furthermore, Beyea displays the same total reliance on secondary sources of a dissertation by Seltzer (2007) on the BEAR 1 Genetics Panel. In this case, Beyea noted that Bentley Glass recalled that the large initial disagreements among the geneticists on radiation induced genetic risks were resolved during an evening meeting, with general consensus being displayed the following day. Thus, according to the Glass recollection, the issue that I have focused on was never a real issue but quickly resolved by the geneticists of the BEAR 1 team after their nightly meeting. Since Beyea relies only upon secondary sources and since Glass was present at these meetings as a real eyewitness and participant, Beyea sided with the Glass story. However, I pointed out that the 85-y-old Glass, now with a 40-y recollection, got the story wrong, based on the Gold Standard, the meeting transcripts (Calabrese 2017b). Glass confused a meeting of the geneticists at their 20 November 1955 meeting at Princeton with the activities of their 6 February 1956 meeting in Chicago. There are transcripts for both meetings that allow one to figure out whether Glass was correct or not. This confusion of Glass is readily documented if one consults the primary transcripts to provide a type of test for the quality of the failing memory of the elder Glass. However, the key point herein is that Beyea emphasizes that when needed, he tries to find reports of professional historians of science, such as their papers and available dissertations. However, as in the case with Jolly (2003), Beyea failed to discern that Jolly's poorly reproduced Crow figure led to erroneous insights and conclusions. In the case of the Glass memory, Beyea relied upon the reporting by Seltzer (2007) that was accurate but was incorrectly interpretated by Beyea, possibly because Seltzer did not adequately cover the 5-6 February 1956 meeting in Chicago with similar transcriptional records. The major point is that Beyea's approach to research in this area is often

professionally precarious and not reliable. This approach of Beyea could never have survived a rigorous graduate student experience, but it passes the peer review of *HPJ.*

There are additional serious problems that Beyea's research approach creates. That is, I have addressed a range of issues concerning the historical foundations of cancer risk assessment. Some of these issues have been considered by historians of science but many have not been considered by them. What this means is that Beyea allows himself to address historically what I have done, if it might have been addressed by historians of science whose research he can learn of and access. Thus, his approach only addresses a very small fraction of what I have published, and for that small proportion of the topic, he used secondary sources that have misled him, been misinterpreted by him, or otherwise have not often covered precisely the same issues as I have. The bottom line is that his research method does not have the required rigor. Again, the lack of reliability of the Beyea approach was missed by the peer reviews and editorial leadership of the *HPJ*.

Beyea makes more errors concerning the BEAR 1 Genetics Panel genetic damage/mutation risk estimates. This is again caused by his total reliance on secondary sources, which in this case is the dissertation of Jolly (2003) and the paper of Beatty (2006). The new error is that Beyea failed to detect that James Crow used estimates for genetic damage for Beadle, Muller, and Sturtevant for the 10-generation/single 10 R (0.1 Gy) exposure [as shown in the Beatty paper (p.63)] based on their exact F1 generation data. That is, he transposed F1 data into a 10-generation category, thereby under reporting damage estimates by about tenfold for each of these individuals. Beyea missed this error because he did not use Crow's letter to Weaver on 12 March 1956 that graphed all panel estimates, and he failed to match these values against the technical documents of each of the panelists, again all primary documents. The mistake of Crow was not detected by Jolly or Beatty but by me. This means that the Jolly and Beatty reports are also in error and Beyea used these as his special Gold Standard, his academic bibles. The point is that Beyea had no chance to detect these important errors unless he used primary sources. By missing this error Beyea was unable to see that this "error" of Crow resulted in reducing interindividual variability and masked the magnitude of disagreement among the panelists. This is another in a very long list of examples where Beyea provides incorrect and unreliable information, and he has no idea that he is doing so because his methodology precludes the proper evaluation. Regardless, the information and interpretations of Beyea in these areas are incorrect. This was also missed by the peer review process, again showing that it acted in many ways like a non-discriminating sieve, simply allowing a massive flow of unreliable information generated by Beyea to be published. As noted above, even though this report has documented numerous errors and failing of the Crow genetic damage evaluation process, the entire Crow estimate exercise on which Jolly, Beatty, and Beyea based their genetic damage risk estimates and risks estimates is scientifically meaningless, since they were based on the flawed and incorrect Russell data right from the start (Calabrese & Selby 2025).

Beyea uses the data of Hanson and Heys that Muller claimed was fraudulent

Hanson and Heys published a number of papers concerning radiation-induced mutations that were used to support the LNT model. Hanson had in fact had a sabbatical in the fall of 1927 with Muller at the University of Texas. The series of papers by Hanson and Heys have been widely used as strong support for the linear position. I have commonly cited these papers in many past publications. However, several years ago I uncovered a letter from Muller to a senior academic geneticist colleague that told the story that he believed that the papers Hanson and Heys collaborated on were most likely fabricated and not reliable, and that they should never be cited. Muller never made these damaging remarks public, possibly because Hanson had become his grant manager at the Rockefeller Foundation, and he may not have wanted to affect his funding from that organization. Nonetheless, I have published a detailed assessment of the Muller criticisms of the Hanson and Heys research (Calabrese & Giordano 2023). However, Beyea (2024) continued to cite the research findings of Hanson and Heys without sharing the now revealed serious concerns of Muller. This failure of Beyea to identify these concerns and address them in his paper adds yet another reason why the information presented in his paper is unreliable.

Other mistakes of Beyea and deliberate misrepresentations

1. Beyea cited the papers of Whiting (1928) and Weinstein (1928) as having replicated Muller's findings. There was little point to noting this. As was the case with Muller, Weinstein used extremely high doses and dose rates. Muller's dose rate was about 100 million times background (Calabrese 2019a, 2019b). This was similar for Weinstein who followed the Muller protocol. As for the Whiting paper, no data were provided to evaluate, and it was performed with a wasp model, while Muller used the fruit fly model. There was also no reporting of the doses used or the sample sizes employed. Thus, it is not clear why this was cited or even why it was even published in the first place. Yet Beyea oddly used it as support for Muller's findings. Based on the lack of information provided in the

Whiting (1928) report, it suggests that Beyea may not yet even read the Whiting paper. Thus, using a study with no data provided and no experimental protocols on even the most basic of requirements is very troubling. Thus, it is not clear why this paper was cited. Furthermore, the Muller research produced mostly moderate to large/massive gene deletions and not the vast point mutations Muller incorrectly proclaimed in his title (i.e., Artificial Transmutation of the Gene) (Muller 1927a). These references were poorly selected by Beyea, making the Beyea argument unreliable.

2. Beyea cites the paper of Bentley Glass (1991), which was written when Glass was about 85 y of age. I have documented that Glass's memory was fading as he failed to correctly remember the BEAR I Genetics Panel meeting activities. He claimed that differences of mutation risk judgments were resolved overnight (the evening of 6 February 1956) when all the Panelists went back to their hotel rooms and then met again the next day, settling all the major differences. The actual facts are that all the Panelists went home that day for a month. They then developed their genetic/mutation risk effects assessments and mailed these independent assessments, which were highly divergent mutation estimates (Calabrese 2017b). Thus, Glass's recollection was incorrect based on original/basic documents which unequivocally refute Glass's recollection and the Beyea story line. Beyea (2016), however, cites the Glass paper as reliable when it isn't. Again, Beyea's use of this reference makes his conclusions not only questionable but also something that cannot be relied upon.
3. Beyea cites a summary by Glass of the Neel research given at a 1954 AEC conference (NAS 1955). This citation has no relevance to the fact that the BEAR I Genetics Panel chose not to review the Neel and Schull final 1956 report. Neel offered this final major report to the Panel in expectation that it would be formally reviewed and even had a special mailing to Muller prior to the first Panel meeting. Muller would later claim in a subsequent letter to Neel a year later that he never read it, being too busy. The comments of Beyea are procedurally and scientifically irrelevant (see Calabrese 2020). Beyea appears unaware of this insight and the information I had access to and appears not to know the basis for selection of material. Thus, the fragmented and grossly limited perspective he offered on this matter cannot be relied upon.
4. Beyea noted that Neel stated that the Russell (1951) mouse studies were the principal sources of genetic risk recommendations. The most important point was missed again by Beyea. This was the fact that the Russell data had a massive error in their control group as uncovered and reported by Selby (2020), the former graduate student and long-term colleague of the Russells. Correcting the error showed that the dose response for males and females was most consistent with a threshold dose response, not a linear at low dose response (Selby & Calabrese 2023). Thus, the comments of Beyea missed the central point and certainly cannot be relied upon.
5. Beyea states that, "What Calabrese is essentially claiming is that linearity should be dropped because the 1956 Committee supposedly messed up the slope calculations." This is yet another misrepresentation of what I said in the 22 episodes. I did not say that LNT should be dropped in the documentary. In fact, in Episode 22, I was asked what the road forward is in light of the past 21 Episodes. What I suggested was that the EPA adopt a "model optimization" process that would incorporate key elements of the LNT, threshold, and hormesis models. I had published several papers on this topic (Calabrese 2015a, 2016) and then demonstrated how it would work during the documentary. I indicated the combined model should be adopted for use now. The three models could then compete in the scientific community based on new studies and see which emerges as the best. This is laid out in Episode 22 for the interested reader. However, the key point for this Commentary is that Beyea misled the journal and readership, once again. This time, the actions of Beyea are immediately exposed by a review of my cited papers and the watching of episode 22, and the reader can determine who is accurate: Beyea or me. Thus, how does Beyea keep making inaccurate statements that can easily be shown to be false — falsehoods that are apparently missed by the peer reviewers and still get the associate editor of *HPJ* to publish a document that grotesquely misleads the readership? At best, this is another example of confirmation bias, not only for Beyea but for the associate editor as well, and perhaps even for the reviewers. Consequently, the perspectives offered by Beyea on these matters are incorrect and therefore unreliable.

Muller and the Nobel Prize and gene mutation: Beyea gets it wrong again

Beyea tries to claim that I was wrong to assert that Muller should not have received the Nobel Prize because he did not induce gene mutations. Beyea claims that any mutation would do. But Muller's claim was being the first to induce gene mutation as seen in the title of his key 1927 *Science* journal paper: Artificial Transmutation of the Gene (Muller 1927a). Muller would unsuccessfully try to induce gene mutation via a reverse gene mutation process in an

effort to prove that he had not simply punched holes in his chromosomes (Lefevre 1949, 1950). Little known is that Muller & Dippel (1926) tried to clarify the gene mutation issue in the 1925-1926 time-period by assessing the occurrence of reverse mutations with over 35,000 flies even before his claim regarding gene mutations in 1927 (Muller 1927a). However, this early effort to reverse gene mutations was not successful, becoming a well-hidden finding.

The concept of chemical and radiation-induced mutation preceded Muller's Nobel Prize research by more than three decades. Prominent efforts were widely reported by numerous researchers initially led by Daniel McDougal, working in the area of plant genetics, and making major headlines just after the turn of the century (i.e., early 1900s) (Campos 2006; Kingland 1991). Others before Muller had shown that recombining broken chromosomes could lead to transgenerational phenotypic changes, as this was addressed many times by Muller. One of Muller's contemporary fruit fly competitors, James Mavor, reported that he had induced chromosome "mutation" (Mavor 1929). In his retrospective assessment, Mavor (1929) stated that "It may be properly said, however, that the induction of non-disjunction of the sex chromosome which the present writer had shown can be produced by X-rays is in effect the induction of a chromosomal mutation." Needless to say, the prior production of such chromosome mutations by Mavor did not even yield him a single nomination for the Nobel Prize. Why? Because it was not unique and not at the level of major biological significance like the alleged "gene mutations" of Muller.

Muller received immediate worldwide acclaim after the announcement of his X-ray-induced gene mutation findings, even spreading back to his campus with over 1,000 attending a lecture on his discovery at the University of Texas soon after his Berlin presentation of his findings. Muller (1928) would also strongly emphasize that all prior attempts to induce "gene" mutation had failed prior to his findings, making his discovery unique. He had to distinguish his major advances from the others, and he did this successfully. The underlying significance was that Muller was claiming that evolution was largely driven by gene mutation and not massive genetic disruptions such as "chromosomal" mutations. However, subsequent research has shown that Muller also did not induce gene or point mutations but mostly modest to massive gene deletions (Calabrese 2019a, 2019b). Thus, there is the obvious question of the legitimacy of Muller receiving the Nobel Prize for producing mutations, not gene mutations. If it were for producing chromosome-related mutations, others would have qualified long before Muller. Carlson (1989), Muller's biographer and last Ph.D. student, wrote: "[Muller] had startled his colleagues by announcing that for the first time in the history of life on earth, the hereditary material of living organisms — in this case, the Genes of fruit flies — had been artificially mutated." There were many other similar praising declarations by contemporary genetics researchers placing Muller's gene mutation findings in a unique place in the history of science. Beyea boxes himself in and there is no escape. The Beyea position on this topic is unreliable. It was this type of thinking and unwarranted argument, so evident in his first paper, that made it simply unproductive for me to respond to this second paper regarding me published many years ago.

Beyea ignored serious ethical failures of the NAS and the BEAR I Genetics Panel

Beyea failed to disclose that the 1956 NAS BEAR I Genetics Panel "Report to the Public" was not written, approved, or even read by the Panel prior to its release to the media/public. Yet an entire article was written on this matter and published in *HPJ* (Calabrese & Giordano 2022a). The Report to the Public was written by an independent third party. It was represented by the NAS as being the Report of the BEAR I Genetics Panel. Many letter exchanges among the panelists document this fact. These letters also revealed major concerns, frustration, and anger with such actions, including there being important errors in that Report to the Public, which was falsely attributed to them (Calabrese & Giordano 2022a). Yet, none of the Panel members came forward to correct the record. The misrepresentation of this Report to the Public represented a major ethical failure of the NAS President, Detlev Bronk. Likewise, the failure of the Panel to correct the record, rather than to keep it hidden, is also a major ethical failure. While Beyea fails to use primary sources, the Calabrese and Giordano (2022a) paper provided the detailed documentation of these ethical transgressions. The failure to report such ethical failings of the President of the NAS and the Panel as reported by me shows that Beyea's representation of ethical issues is not reliable.

Beyea missed the obvious: Calabrese is not just a Muller critic but reports positive activities

Beyea often misses important papers and misleads himself, reviewers, editors, and now readers of the *HPJ.* For example, Beyea failed to note that I published an article about how Muller came to the defense of his chief scientific rival and critic, Professor Lewis Stadler, University of Missouri, for issues concerning an FBI investigation into communist party activities in the late 1940s during the McCarthey Era (Calabrese 2017a). Muller showed amazing support and leadership in trying to help Stadler save his career. In the end, Stadler's career was saved, thanks in part to Muller. Beyea failed to show to the reader that I not only discovered this episode in the lives

of Muller and Stadler but also obtained primary source material that documented it and wrote a peer reviewed article on the topic (Calabrese 2017a). If I were only looking to find fault with Muller, this article never would have been written and published. In fact, I was the investigator who discovered it and helped to make it part of the public record, something that Schwartz (2008) and Carlson (1981) did not do. Again, Beyea failed to share this information with the readership.

Muller ignores peer review to promote his Nobel Prize chances: Beyea missed the key references again

Beyea agrees with my assertations that Muller did not subject his Nobel Prize research papers to peer review (Calabrese, 2025; Calabrese & Giordano 2022b). Beyea then tries to excuse the actions of Muller, saying that peer review was not widespread and established. In fact, he makes the claim that it took until the 1970s for this to occur, citing one recent reference. A detailed review of journal administrative primary sources and journal articles indicates that many of the major journals had adopted a peer-review process by the 1920-1930 time-period. Well established documentation exists for journals such as *Plant Physiology* [start of peer review (PR)-1925] (American Society of Plant Biologists website 2025), *Journal of Bacteriology* (start of PR-1917) (Porter 1974), *Journal of Experimental Zoology* (start of PR-1904) (Harrison 1945), *Journal of General Physiology* (start of PR-1918) (Andersen 2005), *Journal of Physiology* (start of PR-1927) (Bynum 1976), *Journal of Experimental Medicine* (start of PR-1905) (Flexner & Flexner 1993), *American Journal of Botany* (start of PR-1933) (Smocovitis 2014), *Genetics* (start of PR-1917) (Dunn 1965), *Immunology* (start of PR-1916) (Emrich 2016), *The Biochemical Journal* (start of PR-1912) (Biochemical Society 1985), *Journal of Clinical Investigations* (start of PR-1924) (Brainard 1959), and many others. Some of the journals provide detailed information concerning the number of manuscripts received, accepted and rejected. (e.g., *Journal of Experimental Medicine, Journal of Bacteriology* and others). Also, although not specifically cited due to space, documentation was obtained by purchasing numerous letters (e.g., George Shull, first editor of *Genetics*; Simon Flexner, editor of *Journal of Experimental Medicine*; and James McKeen Cattell, editor of *American Naturalist, Scientific Monthly, Science*), peer-review files, and other relevant documentation for many of these journals, with several journals being extremely copious. For example, these letter exchanges provide the actual peer review and basis for acceptance, revision, or rejection of manuscripts.

In the case of the *Journal of Experimental Medicine*, the materials extend for multiple decades (1915-1941) with detailed assessments of hundreds of papers that I possess. Muller's advisor, Thomas Hunt Morgan, was on many of these above editorial boards (e.g., *American Naturalist, Genetics, Journal Experimental Zoology*) and helped to establish and participate in the peer review process while Muller was working under his direction. In addition, Albert F. Blakeslee, who reported radium-induced gene mutation in *PNAS* some 6 mo prior to Muller's paper in *Science* (Gager & Blakeslee 1927), was used by *Science* journal as a peer reviewer for genetic research performed in the laboratory of Thomas Hunt Morgan on 6 December 1926 (Blakeslee 1926) 7 mo before the key Muller *Science* publication. This finding alone is sufficient to discredit the assertions of Beyea. Likewise, Blakeslee was also a reviewer for *Genetics* as seen in correspondence with the editor-in-chief (Blakeslee 21 & 24 September 1928). Thus, it is important to note that the single reference cited by Beyea on peer review was of a general nature and failed to address the occurrence of peer review in relevant biological journals in the Muller era, especially in the area of genetics. Thus, the information provided by Beyea was grossly inadequate to address the key questions, whereas what I have provided is far more properly designed to answer the question, by getting the actual peer reviews from the preserved files of the editor at the very start of the journal *Genetics,* a finding that discredits the Beyea statements on the matter. Beyea provides an inaccurate description of the scientific culture that Muller was educated and trained in and how it had peer review integrated within it. The information that Beyea provided therefore is unreliable for the question raised, and this should have been challenged in the peer review process.

Muller was thus acculturated into the peer review process as a graduate student, starting in 1911. In addition, the key journal, *Genetics*, started peer review just at the time Muller had finished his Ph.D. The comments of Beyea are inconsistent with the historical record. The fact remains that Muller was well aware of the peer review process, had participated in it, and avoided it for his most important paper.

Of telling importance to this issue is that the owner and editor-in-chief of the *American Naturalist* and *Science* journals, James McKeen Cattel, asked Muller to be a guest editor for the *American Naturalist* journal and to acquire excellent papers that had been presented at the Fifth International Genetics Congress in Berlin, September 1927. This was the meeting where Muller presented his Nobel Prize research for the first time. Muller was a guest editor who was to oversee the review process and make decisions on acceptance or rejection of manuscripts [Muller letter to Demerec, 26 October 1927 (Muller 1927b)]. Yet, Muller curiously neglected to have his own Nobel Prize winning research paper peer-reviewed for subsequent publication in the *American*

Naturalist journal. Muller was also on the editorial board for *Advances in Genetics* where Demerec was the editor-in-chief and was also involved with the manuscript review process (Demerec letter to Muller, 11 May 1953). The collective findings show that the argument of Beyea that peer review was a recent phenomenon fails to match the evidence provided here. As in many of the other examples in this paper, such information is best obtained from primary sources, which is information Beyea did not use.

Why not publish in history of science journals? The answer is easy to understand

Beyea argues that I should have published in history of science journals. The problem with Beyea's argument is that citations of papers in even the most prominent history of science journals and with the most prominent historians are grossly anemic. A check of the H-index was made for five of the most prominent US historians of science and the addition of Beatty from Canada who Beyea relies on, and who have published in the area of the history of cancer risk assessment. Their scores ranged from 5-15, whereas my H-index is 112. While these papers can be excellent, they tend to have limited circulation, with little impact outside their narrow disciplinary domain. If I were to follow the Beyea line of argument/suggestion, then my well-cited papers would be effectively hidden under an academic basket, read and cited by few, thereby having far less impact. Thus, Beyea's line of argument is not convincing. I have published my papers in a diverse but relevant range of high-level journals that reach most scientists, with all being cited in Pub Med and the Web of Science and other indices.

More on the Stern-Uphoff case: The critical experimental data have been missing for nearly 80 y

These authors promised to publish a manuscript that would be "a more detailed account of the work" in addition to being part of a single table in a paper lacking methods and materials, experimental protocols, etc., all usually considered elements of a scientific paper. This paper became the key foundation for both the BEAR I Genetics Panel switch to linearity and the work of Edward Lewis to support linearity. Yet, the findings were never published and have been missing nearly 80 y. They were relied upon when making far-reaching decisions---yet Beyea failed to mention this, providing another reason why his conclusions are unreliable.

Challenges along the way: Muller's daughter refuses to let Calabrese have access to key documents in the Muller files at the University of Indiana

Hidden among the many critics are strategies to prevent legitimate research. For example, I have made repeated requests to obtain/purchase documents from the Muller collection within the University of Indiana Library. However, on multiple occasions the library has indicated that Muller's daughter refused to permit me to obtain certain research materials from this state-supported University library that were donated about 60 y ago when the daughter was a young adult. Yet, the information that I have requested was shared with Carlson. On this matter, the FBI files on Muller that I have finally obtained have opened up entirely new areas of inquiry that have led to new manuscripts (Calabrese et al. 2025).

More false charges

In 2.6, Beyea asserts that a false claim was made that the 1956 NAS committee "engaged" in financial misconduct. Beyea never states who, when, where, and what was the "claim." Beyea never defines the term "financial misconduct," nor does he define the term "engaged." The Beyea Commentary that followed the allegation provided no evidence that was even remotely relevant to the allegation. Yet the reviewers and editors failed to require that Beyea offer a clarified and properly defined set of allegations with supportive evidence to which an accused person could respond. In the present case, how are the accused supposed to defend themselves? The Beyea approach is simply to accuse and then offer nothing that is specific that could be refuted. When a "crime" is committed, it is important to know when, where, and how it occurred so that an accused person has an opportunity for a legitimate defense. In the case of Beyea, he failed to properly define the accusation, allowing no opportunity for self-defense. This situation will ensure that his accusation lives on.

When is a bribe a bribe? The case of Warren Weaver — Why Beyea's explanation cannot be trusted and is not reliable

Beyea acknowledged that Warren Weaver, Chair of the BEAR 1 Genetics Panel, made a "bribe offer" to the Panel based on the book of Berg and Singer (Berg & Singer 2003). The bribe statement was made very early in the morning of 5 February 1956 (page 35 in the transcripts). The statement was made within the context of Weaver being the Director of Research at the Rockefeller Foundation (RF) for over two decades and having been in charge of awarding research and programmatic grants to various members of the BEAR 1 Genetics Panel. He also understood how important grant funding was to academic researchers. It is their professional lifeline. Weaver was now in charge of the BEAR I Genetics Panel, which was also being fully funded by his organization, the RF, that gave the money to the US NAS. It is also important to appreciate that the President of the NAS, Detlev Bronk,

was a long-time member of the RF Board of Directors as well as the President of the Rockefeller Institute for Medical Research (soon to have its name changed to the Rockefeller University). This is correct; Bronk held both Presidencies at the same time and would find himself in a highly conflicted position. Beyea attempts to dismiss the provocative bribe-like statement of Weaver. Beyea claimed that Weaver didn't really mean what he said. According to Beyea, what Weaver was really trying to do was to show his "determination to get a report finished." The support for this statement, according to Beyea, was from the book of Berg and Singer (2003), a biography of George Beadle, a BEAR I Genetics Panel member. With that as backdrop, let us now read the paragraph from Berg and Singer (2003) (page 230) that contains the bribe reference:

"The panel met again in February 1956 in Chicago. Weaver understood that inspite of the disagreements within the committee, they had to construct a report that all panel members could approve. He came to Chicago prepared to succeed. He proposed that the initial reports of all the panels, including genetics, be brief and incorporated into a single, plain-spoken document; a draft of the genetics section was in his briefcase. He also held out a bribe; the Rockefeller Foundation would provide substantial support for fundamental genetic research so that in future, more knowledgeable recommendations could be made. Weaver read from the draft at length, with minor interruptions from the geneticists and a few on his part to apologize for any errors in science. Beadle, who was pretty quiet during most of the presentation, finally gave him the go ahead: 'You just keep going. We don't need any professional popularizers, all we need is you. We will put the commas in and make the corrections and put the qualifications in and we will have a fine job here'. Muller agreed. Then Weaver knew that he would have a finished brief report within months."

Let us now consider what Muller said. Immediately after the comments of Beadle, Muller stated (page 34 in the transcripts): **"I think we would all much rather have you do it than any professional popularizer, excepting the word 'professional' as applied to popularizer."**

What was occurring was that Weaver was expecting each of the six NAS panels dealing with nuclear issues to produce a brief and easily understood document. These reports would be assembled into one NAS report in a well-coordinated and timely fashion. Taking the initiative, Weaver created what might be a first draft of such a popular summary report, depending on the response of the Genetics Panel. In his opening remarks, Weaver emphasized that their Panel report should claim that the dose response was linear down to a single ionization, there was no repair, and the damage was cumulative and irreversible. In fact, it sounded as if Muller had written the script for Weaver. Weaver used his opening remarks for what he hoped to be the Panel report to shred the long-time dose response paradigm of the medical community, which was the threshold dose response model. The statement of Weaver therefore was far from trivial. It was the opening salvo in the battle over the future of hereditary and cancer risk assessment. What Weaver heard was very encouraging. The immediate comments of Beadle and certainly of Muller indicated that if Weaver wanted to write the report, that was fine with them. Weaver was in full alignment with what Muller had been advocating since 1930. However, now Muller would have the power of the RF and the NAS behind him. No one complained. The implications were that it was better for Weaver to do this than hiring an independent third party to write this popular document. Based on what Beadle and Muller stated, the role of the Panel was likely to be advisory, ensuring that Weaver's brief summary was accurate, well written, and fully supportive of their views.

About a minute later, Weaver offered the bribe statement on page 35 of the transcript:

"There may be some very practical results — and here is the dangerous remark — don't misunderstand me. We are just all conspirators here together. I am not talking as an officer of the Rockefeller Foundation but I will bat my head in the Rockefeller Foundation to try to get a very substantial amount of free support for genetics if at the end of this thing we have a real case for it. I am not talking about a few thousand dollars, gentlemen. I am talking about a substantial amount of flexible and free support of genetics. I will bat my head off to get it at the end of this if we have a really good case for it."

Several pages later Weaver stated that his current draft is now eight pages, and he would expect that the final draft would be less than 15 pages. He then stated that "Now this is what I am supposing, but at the moment I am asking you what are the last two or three pages are going to look like." Dr. Douglas Whitaker of the National Research Council and the Rockefeller Institute of Medical Research then noted that the Pathology Panel was thinking along the same lines, producing a short report, hoping to get it done in about a month.

Within this context, it seems clear that (1) Weaver wanted to strongly advocate for a radical change in radiation risk assessment, a switch from a threshold to a linear model; (2) the report should be short but readily understandable, and non-technical; (3) that Weaver wanted to quickly move the process forward; and that (4) the Panel

was highly supportive and could be useful in making sure that Weaver captured what was needed.

Why would Weaver make such outlandish and insulting bribe-like comments to the Panel may be hard to understand. Why would he say that "…here is the dangerous remark….We are just all conspirators here together." Then Weaver talks about new funding for geneticists that is significant, flexible, and substantial. In the end, the statement of Weaver to a Panel that he is directing is highly inappropriate. Panel members, in fact, should have first immediately challenged Weaver on the dangerous remark and conspirator comments. What exactly was he talking about, as if they didn't know? Why were these comments linked to funding? Not a single panelist challenged him or requested clarification. The panel did no favors for Weaver or themselves by remaining quiet. Further, no one reported these bizarre bribe-like comments of Weaver to President Bronk. The comments are scandalous and could have been devastating had they been shared with the media. The fact that no member of the panel registered a challenge or complaint to Weaver or Brock is very telling.

Weaver knew that the Genetics Panel report would be the so-called lynchpin of the entire RF-NAS effort with respect to the general public. He especially noted this strong belief to the Panel in his opening remarks. Thus, success or failure would depend on how transformative their report would be. Given Weaver's position in the RF and its funding of the entire endeavor, and under the banner of the US NAS, there was heightened opportunity for the Genetics Panel to be the transformative entity, especially with some of the star power on the panel. However, Weaver also wanted to ensure success by having a Panel that was all on the same page with no minority report muddying up the process. Within this context, Weaver's comment makes sense. He was sweetening the pot, holding out the promise for a vast increase in research funding for the genetics community, including the big players on his Panel that were already receiving such funding as well as keeping possible dissenters in line.

The interpretation of Beyea has no support. In fact, this is not what was written in the Berg and Singer (2003) chapter as shown here. Furthermore, his write up fails to place the comments of Weaver and Panel members in context. Beyea should have used the primary source material rather than relying on the book chapter of Berg and Singer (2003) that itself also failed to provide the broader context. Surely, the editors of the *HPJ* cannot realistically believe that the information provided by Beyea on this matter for the reader was adequate to make an informed judgment. He presents a view that cannot be relied upon.

Beyea's failure to disclose consultant association

Beyea noted in his acknowledgment section that George Hoffmann served as a consultant for his paper published in *HPJ*. What Beyea failed to tell the reader was that Hoffmann had a prolonged professional relationship with me in the area of dose response and hormesis. The information that Beyea should have disclosed in the paper is as follows. Hoffmann served as a consultant for about 6 y in conjunction with a long-term grant on hormesis to the University of Massachusetts with me as the Principal Investigator. The consulting activities developed following an initial invitation by Hoffmann, editor at *Mutation Research*, for me to write a peer-reviewed article on hormesis in late 200l. The paper was written and passed peer review and was then published in 2002 (Calabrese 2002). Two years later Hoffmann invited me to give a seminar (20 October 2004) on hormesis and meet with students at Holy Cross College, Worcester, MA, where he was a long-time professor in the Biology Department, teaching genetics. After the seminar and meeting with students, Hoffmann hosted a reception in my honor at his home, an activity well attended by faculty and students. These positive interactions and mutual professional interests would soon lead to me to invite Hoffmann to become collaboratively involved on my Air Force-funded research grant as a consultant. Hoffmann became very involved with the project, in ways far exceeding normal consultant activities, as he became a coauthor on six high level papers (Calabrese et al. 2006, 2007a, 2007b, 2008, 2010; Nascarella et al. 2009), participated in multiple ways in my UMass hormesis conferences, wrote commentaries and articles on hormesis (Hoffmann 2009; Hoffmann & Stempsey 2008), and even made a trip to Mexico to give a presentation on hormesis as a spin-off activity of this association. I also invited Hoffman to be the moderator of sessions at the annual Hormesis Conference which occurred in 2007, 2009, and 2011 (see International Dose Response Society website, Conferences section). Of further significance is that I invited Hoffmann to give one of only two plenary addresses in 2008. In addition, Hoffmann offered considerable assistance to one of my Ph.D. students (i.e., Marc Nascarella) on an hormesis dissertation. Nascarella formally acknowledged the assistance of Hoffmann in the Acknowledgements section of the dissertation. Thus, Hoffmann and I had developed an unusually friendly and productive professional relationship at a high level of academic involvement. Hoffmann regularly attended and fully participated in the hormesis project's research team meetings, being well known to all team members. All research ideas, activities, manuscripts, challenges, and group strengths and limitations, funding strategies, and related matters were shared with him. Hoffmann also became very actively involved in

the activities of the International Dose Response Society under my direction.

He was fully aware of the criticisms of my research and me as well as strategies/plans on how to respond to such criticisms. For example, Hoffmann was fully aware of the criticisms of Elliott (2006) dealing with his proposed environmental ethic that targeted me, as noted earlier, and my rebuttal commentary (Calabrese 2007). Hoffmann was fully informed by me when University of Massachusetts administrators placed me on "trial" in an academic misconduct investigation in the spring of 2011 as a result of the administration being sent an anonymous letter along with a recent article by Professor Kristen Shrader-Frechette (2010) that was highly critical of me both with respect to ethics and scientific issues, as discussed earlier. In my defense, I personally prepared/wrote a 50-page document that I asked Hoffmann to confidentially review and offer comments on how it could be strengthened. Hoffmann also knew that I had been vindicated during the UMass "trial" based upon the detailed information that rebutted the Shrader-Frechette article, with him knowing the entire set of information provided about her work. Yet, Beyea cited this episode to draw readers to the compromised Shrader-Frechette paper. In addition, the Shrader-Frechette paper had no relevance to the LNT documentary, the focus of the Beyea paper, and the LNT issue. My relationship with Hoffmann was not only one built on mutual scientific interests but also on trust, with all such details on the most sensitive issues being shared with him.

One of the hormesis projects that was undertaken during Hoffmann's role as a consultant at UMass involved assessing the occurrence of hormesis for various mutational endpoints, with one focused on the manifestations of hormesis in the Ames assay. A manuscript was developed on this topic and was accepted at *Mutation Research* (Calabrese et al. 2012). This occurred during a time when Hoffmann was fully involved in team meetings/research activities. Without my knowledge and that of anyone on the UMass team, Hoffmann and a colleague published a rebuttal to our hormesis and Ames assay paper (Zieger & Hoffmann 2012). This was an unheard-of act and especially shocking given the nature of our professional and personal relationship and that he did this while being a long-term consultant, professional friend, and confidant, advising me and my research team on these matters. During the development of his rebuttal paper, Hoffmann participated in group meetings, was exposed to our research plans and publication activities, including our mutation paper, all the while charting his rebuttal. It should be noted that, in the Zieger & Hoffmann (2012) publication, Hoffmann inexplicably neglected to report any "Conflicts of Interest," thus raising the issue of a possible ethical failure for non-disclosure. However, Hoffmann acknowledged his association with me in his other cited papers (Hoffmann 2009; Hoffmann & Stempsey 2008). As a result of these circumstances, it was necessary for me to terminate Hoffmann from my project and related activities. Hoffmann therefore had been a UMass hormesis project insider, with unique knowledge of me and all aspects of the hormesis project and my other professional activities. When Beyea used the services of Hoffmann as a consultant to comment/advise on his article about my interview for the HPS documentary and my long research involvement on the historical foundations of cancer risk assessment, he was surely obligated to inform the journal editors, reviewers, and the readership of the nature of Hoffmann's relationship with me and how and why that relationship ended. Hoffmann clearly has long-standing historical and personal "skin" in the game and could have brought strong negative bias and unique "insider" knowledge that affected Beyea and influenced his personal and professional criticisms of me. Yet the *HPJ* readership was deceived by Beyea, creating a false impression of Hoffmann's background. The outline of this story, without names and institutions given, were shared with multiple ethicists, with each one independently concluding that Beyea needed to disclose relevant information about the Hoffmann incident in his acknowledgement section, something he did not do. Failure to properly disclose this information represents a serious ethical failing of Beyea. It creates a situation in which any possible damage to my personal and professional reputation cannot be adequately corrected, with — for example — a subsequent erratum/correction.

The issue of Beyea's failure to disclose the Hoffmann relationship with me is relevant to the COPE Retraction Guidelines which state: *"Retractions might be necessary for unethical research practices, compromised peer review, or undisclosed conflicts of interest that could bias interpretation of the work or recommendations by peer reviewers"* (italics added).

The above detailed discussion of the Hoffmann-Calabrese interactions is directly relevant to the statement by COPE concerning, "undisclosed conflicts of interest that could bias interpretation of the work or recommendations by peer reviewers." It shows that Beyea failed to disclose conflicts between Hoffmann and me about my research and character attributes that were also targeted by Beyea and that these could have biased the interpretation of my research and recommendations by peer reviewers. Also not acknowledged by Beyea is that Hoffmann had longstanding personal/professional relationships with a number of the key geneticists whom I severely criticized and determined to have committed scientific misconduct in my articles and in the documentary. These relationships

of Hoffmann with the key leaders started when he was a graduate student at Oak Ridge National Laboratory. In fact, Hoffmann would share information in group UMass conversations concerning the camaraderie as a graduate student that he experienced based on numerous Sunday morning hikes led by Alexander Hollaender, who had been on the BEAR 1 Genetics Panel, and was in charge of the research of William and Liane Russell, both of whom were also strongly criticized by me. Hoffmann had strong historical ties to many other leaders in the genetics field, especially via his role as a staff officer at the NAS, assisting such scientists in their committee meetings, and as journal editor for Mutation Research, activities that involved developing and maintaining strong professional and personal relationships. As noted, my research was strongly critical of many of these people. Beyea should have felt obligated to share this information with *HPJ* to ensure that the reviewers would be aware of numerous conflicts and biases that Hoffmann brought to this process that could affect the peer review process. Based on the information presented, the COPE Retraction Guidelines indicate that the Beyea paper should be retracted.

As in the case with George Hoffmann, Beyea failed to identify the interactions that one of his consultants, Kevin D. Crowley, has had with me concerning the historical foundations of cancer risk assessment and the LNT issue. In 2014, Crowley co-authored a letter to the editor with the President of the US NAS, Ralph J. Cicerone, which was highly critical of several of my publications (Cicerone & Crowley 2014). I rebutted the accusations of Cicerone, and Crowley in a follow-up letter to the editor (Calabrese 2014). These were highly contested letters. A year later Crowley again co-authored a letter of rebuttal to a different article (Calabrese and O'Connor 2014), again on the issue of LNT with the clear focus on our criticism of the US NAS BEIR VII report (Crowley et al. 2015). Crowley is the Director of the Nuclear and Radiation Studies Board of the US National Research Council, a high-level position within the US NAS. Thus, one can readily see how Crowley could have co-authored the letter to the editor with the President of the NAS criticizing me if the issues I had raised were of sufficient seriousness. Crowley was also the NAS Project officer in charge of the assessment of the Fukushima Nuclear Accident. In this context, Beyea was a member of that committee with Crowley.

Beyea had an ethical obligation to inform the *HPJ* of the adversarial relationship that Crowley and I have had for the past decade, with it reaching to the heights of the President of the NAS, no small involvement. Since Crowley holds a position of considerable prestige within the radiation community, readers of the Beyea article would likely have the impression Beyea obtained guidance from a highly objective source in the US NAS. The readership needed to know that Crowley and I strongly disagreed on many issues related to the LNT story. This level of transparency is essential for an author such as Beyea to provide and for a journal to ensure, neither of which occurred. It is also important to note that I have continued my debate and challenges to Marcia McNutt, the current President of the NAS, for whom Crowley works. My dispute with McNutt was given an entire episode (i.e., Episode 14) of the HPS Documentary. This should also have been acknowledged by Beyea but wasn't because it is directly connected to the Crowley involvement. As noted earlier in the present paper, a recent NAS radiation committee explicitly refused to cite my LNT publications or to use the hormesis term in their report, each an attack on me and my work, showing the endemic hostility of the NAS leadership to my research on the LNT area and the sensitivity of that organization to my criticisms. Thus, Beyea again failed to provide the necessary transparency. This is a serious matter as it involves serious ethical failures by Beyea as manifested in his *HPJ* publication.

JOURNAL TRANSPARENCY: A BASIS FOR HONESTY AND FAIRNESS

It is apparent that the peer review process of the Beyea manuscript was seriously flawed and highly biased based on the number of deficiencies/errors, basic methodological inadequacies (e.g., reliance of only secondary source materials), as well as the many examples that strongly suggest that Beyea could not have watched multiple episodes of the HPS documentary due to numerous glaring errors (see a full documentation of this point in the Cardarelli paper), some of which the present paper identified.

This issue speaks to journal and editor integrity. I assume that journals and editors may defend the position that the peer review process cannot be violated under any circumstances. I support identity protection of the reviewers, but not what they wrote. One can decouple what was written from who wrote it. For example, reviewer comments in the present case could be briefly summarized by a journal editor. In such a situation, there would be no ethical reason not to share the summarized information minus the identification of the reviewer.

Given the above concerns, it is necessary to see how the reviewers evaluated this paper. Assuming two reviewers, did both positively evaluate the Beyea paper, recommend acceptance and justify their respective judgments? Did they offer substantive criticisms? If so, what were the nature of these criticisms? Did either recommend that the paper be rejected? If there was a negative review, what was the basis of the rejection? How was this dealt

with by the Associate Editor? This allows the reader to more fairly judge the actions of the reviewers and the Associate Editor. Did reviewers come to the same independent conclusion as Cardarelli and me; that is, that Beyea most likely did not watch the entire documentary as well as multiple episodes while commenting on them. If no reviewer brought this issue forward, this would also create a specific concern for the quality of their reviews. Given the criticisms directed at the Beyea paper herein and the documented complementary paper by Cardarelli, it is necessary that the journal editor display adequate transparency as described above to restore damaged journal credibility. A final point relates to new ethical concerns. Since Beyea claims to have watched the entire documentary, yet overwhelming evidence suggests otherwise, it raises ethical violation concerns that the Editor needs to address within the context of retraction of the Beyea 2024 paper.

FINAL STATEMENT

Nearly every conclusion offered by Beyea is inadequately researched and is often found incorrect or misleading, which is unfortunately reinforced and enabled by his failure to perform professional historical research. The Beyea article is plagued with instances of providing the reader with unreliable information concerning both the HPS documentary and my publications. The information is so erroneous that it seems quite apparent that Beyea could not have even watched large segments of the documentary. He also fails to cite key papers of mine that contradict many of his inaccuracies. The big picture is that Beyea does not have a good understanding of the topic on which he wrote, and it shows in dozens of ways as demonstrated here. In addition to his large number of mistakes, massive potpourri of misinformation, and substandard research methods, Beyea's assessment represents a serious misrepresentation of my research record and its contributions to the field. Instead of recognizing the value of the substantial number of historical discoveries that had remained hidden for nearly a century, Beyea brushes them aside, and directs an attack on my character and career efforts, lacking objectivity and reasonable fairness. Beyea also displayed profound ethical failings when he did not provide proper disclosure information about potential strong biases against me by one of his identified consultant experts. The Beyea story is not unique but represents another in a rather long line of ideologically motivated publications.

That Beyea's manuscript passed the peer review of the *HPJ* is more than a serious warning sign to the editorial leadership, the society membership, and the publisher. What is their accountability? What was the nature of the peer reviews and how were they acted upon? As one can see from my assessment, the published Beyea paper is terribly flawed, often not providing reliable information. Did the reviewers simply miss the numerous flaws that I documented?

In this specific case, the Editor-in-Chief has indicated that he relied entirely on the decisions of a designated Associate Editor, without any oversight, review, or knowledge of the selection of the reviewers, comments of the peer reviewers, and how they were handled. Yet, the editor-in-chief is supposed to ensure a fair and professional process. So, how did the Editor-in-Chief ensure this since he did not have any information as to what occurred? Based on subsequent discussions, he indicated that "fairness" is ensured by permitting me to respond with the present Commentary article to the Beyea paper. Within this context, real unfairness becomes apparent when it is realized that the editors failed to notify me of the accepted Beyea paper so that I could offer possible comment at the same time as the Beyea paper. I learned about the acceptance of the Beyea paper nearly 4 mo after it was accepted and online via my own search. A major flaw with the editor's fairness "policy" is its inherent unfairness in that the Beyea paper will continue to exist and will be used for political purposes even if I am successful in my rebuttal and show that almost all of what he asserted was not justified and not reliable. In fact, opponents of the documentary who serve on the HPS Board of Directors demanded that the three articles of Shrader-Frechette — with my name unceremoniously placed in the titles -- be posted with the documentary on the HPS website, a demand that was not approved even though her articles had no linkage to the history of cancer risk assessment. This is a sign of the nature of the politicized process. Thus, these papers, and the now published Beyea Commentary, even if largely discredited, can have a very long functional societal half-life and can get recycled, as in the present situation.

This is a good example of why the nature of peer review demands both fairness and high professional competence, including necessary fact checking, as it serves as a critical check on such political activism and its infiltration into the scientific process, while ensuring accuracy and that the information provided is reliable. For example, the *HPJ* reviewers should have been very knowledgeable of the documentary and should have watched it again (all 10 hours!) during the peer review process in order to provide a more informed peer review. If they had done so, they would have known that Beyea's allegation that I argued for the elimination of LNT was false based on watching episode 22. However, it seems that none of the reviewers knew this fundamental issue, allowing the false Beyea statement to go unchallenged. The same is the case

for his inaccurate statement that I claimed that the BEIR I Committee did not consider the issue of dose-rate. In this case as well, it is quite clear that Beyea could not have watched episodes 19-21 nor have read my relevant publications, which discredited his statements. Thus, the decision of the Editor-in-Chief is important because it prevented me from challenging the Beyea paper in real time, delaying my response by possibly 10 mo, and perhaps longer, thereby allowing Beyea to get his evaluation more solidified in the minds of readers before it is challenged. The long delay might also make readers wonder why I have not responded, perhaps now incorrectly thinking that Beyea may be correct. It represents a significant "silent editorial strategy" that ensures a profound lack of fairness. Yet, the Journal claims to follow the ethical guidelines of COPE.

The Editor-in-Chief remarkably claims that this is how the *HPJ* is being fair: BUT fair to whom? To most fair-minded individuals, this gives an undeserved advantage to Beyea and is harmful to the field as well as to me. Further, I have been informed that my paper would undergo the *HPJ* version of peer-review, a process that I have shown in this paper to be unfair, largely uninformed, and untrustworthy. Who would be the reviewers? If they are the same people who advocated/supported publication of the Beyea paper, then my paper would be almost as critical of them as it is of the Beyea paper. This situation would then place my manuscript within yet another unfair and severely compromised setting. Yet, these decisions are hidden from the author and the readership, again an unfair situation. This entire current process needs proper transparency, showing the originally submitted manuscript of Beyea, summarized reviewer comments, and what changes were made — the full process. If no or very few changes were made in the Beyea review, then what does that mean both for how the Beyea paper was treated and for my paper?

Fairness and responsibility demand that such papers need to be evaluated fairly by knowledgeable and unbiased peer reviewers. This is the ethical obligation and responsibility of the Editor-in-Chief, his designated associate editor, and the publisher — an obligation that was not fulfilled in this case, with each having a failed responsibility. This also appears to have occurred in the publications of the Shrader-Frechette (2010) and Elliott (2006) papers for other journals and editors.

Scientific societies are valued because of their commitment to objectivity, accuracy, and fairness, a standard that was not achieved in this case for any of these three areas at the *HPJ.* In fact, the number of serious errors and omissions in the Beyea paper are far beyond repair even with the publication of the present paper, which has tried to correct the record. There are still numerous other errors that remain to be corrected. All major scientific journal publishers have retraction policies. The first priority of these publishers is to ensure that the information is reliable. In fact, the first priority according to COPE for retraction is the following: "…clear evidence that the findings are unreliable." Knowing that this was the chief criteria of COPE for retracting, each area of this paper that identified unreliable findings of Beyea were noted as unreliable. The COPE criteria demand retraction. Not to do so by the Journal would compromise the very ethics that the Journal claims to uphold.

Given this situation, the *HPJ* has a clear obligation to retract the Beyea paper, in order to inform the reader of excessive unreliable information. The journal failed in its evaluation of the Beyea paper. It now needs to correct that error by retracting the Beyea paper. In addition to the issue of providing copious unreliable information, Beyea's actions have violated the COPE Retraction Guidelines as noted above, for "undisclosed conflicts of interest that could bias interpretation of the work or recommendations by peer reviewers." This provides yet another documented basis for *HPJ* to retract the Beyea paper. Failure to retract the Beyea paper would represent an abject failing of the *HPJ* leadership and further compromise the standing of the Journal.

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DATA AVAILABILITY

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

USE OF GENERATIVE AI AND AI-ASSISTED TECHNOLOGIES

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

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