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# The Gofman-Tamplin Cancer Risk Controversy and Its Impact on the Creation of BEIR I and the Acceptance of LNT

Edward J. Calabrese[1]

[1] School of Public Health and Health Sciences, Department of Environmental Health Sciences, University of Massachusetts, Amherst MA, USA

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**Summary**

*The major public dispute between John Gofman and his colleague Arthur Tamplin and the United States (US) Atomic Energy Commission (AEC) at the end of the 1960s and during the early 1970s significantly impacted the course of cancer risk assessment in the US and worldwide. The challenging and provocative testimony of Gofman to the US Senate in early 1970 lead to the formation of the US National Academy of Sciences (NAS) Biological Effects of Ionizing Radiation (BEIR) I Committee in order to evaluate the accuracy of claims by Gofman and Tamplin that emissions from nuclear power plants would significantly increase the occurrence of genetic defects and cancers. BEIR I recommended the adoption of the linear non-threshold (LNT) dose response model for the assessment of cancer risks from radiation exposures. The US EPA adopted this recommendation and generalized it to incorporate chemical carcinogens, thereby affecting cancer risk assessments over the next decades. Despite the scientific limitations and ideological framework of their perspectives, Gofman and Tamplin are of considerable historical importance since they had essential roles in affecting the adoption of LNT by regulatory agencies.*

## 1. Introduction

From 1969 through to the early 1970s, a major public dispute occurred between John Gofman and his colleague Arthur Tamplin and the United States (US) Atomic Energy Commission (AEC) that significantly impacted the course of cancer risk assessment in the US and worldwide. This paper shows that the provocative testimony of Gofman to the US Senate in early 1970 [1] spurred the creation of the US National Academy of Sciences (NAS) Biological Effects of Ionizing Radiation (BEIR) I Committee to determine the accuracy of claims by Gofman and Tamplin that nuclear emissions from power plants would cause widespread genetic defects and cancers. The actions of Gofman and Tamplin proved to be highly influential since BEIR I [2] recommended that the US Environmental Protection Agency (EPA) [which had replaced the Federal Radiation Council (FRC)] adopt the linear non-threshold (LNT) dose response model for the assessment of cancer risks from radiation exposures. The US EPA [3] accepted this recommendation and generalized it to include chemical carcinogens [4], thereby affecting cancer risk assessments to the present day. Despite their highly criticized analyses and strong ideological

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Corresponding Author: Edward J. Calabrese, School of Public Health and Health Sciences, Department of Environmental Health Sciences, Morrill I N344, University of Massachusetts, Amherst MA 01003; E-mail: edwardc@schoolph.umass.edu

perspectives, Gofman and Tamplin proved to be of great historical significance because they played essential roles in catalyzing the adoption of LNT by regulatory agencies in the US and around the world and in preventing the worldwide expansion of nuclear power.

The process by which the EPA adopted LNT for cancer risk assessment is clearly rooted in the 1956 recommendation of the US NAS Biological Effects of Atomic Radiation (BEAR) I Genetics Panel [5] that reproductive and genetic risk assessments for ionizing radiation needed to switch from a threshold to an LNT dose-response model. However, how this recommendation affected the adoption of LNT by the EPA for cancer risk assessment is complicated and needs some clarification, especially since the report of the subsequent BEAR II Genetics Panel in 1960 [6] did not support the application of LNT for radiation-induced cancer risk assessment due to uncertainties in low-dose extrapolation [7]. The current paper explains how the adoption of LNT by the US EPA for cancer risk assessment occurred, discusses its scientific foundations, describes necessary precipitating events, and characterizes key personality traits that helped affect the switch from threshold to LNT. The story is an outgrowth of the Gofman and Tamplin controversy that dominated the debates on radiation risk assessment and on the adoption of nuclear power within the US circa 1970 [8].

Herein it is shown that the recommendations of the 1956 BEAR I Genetics Panel [5] on exposure limits for ionizing radiation were used by both the Federal Radiation Council (FRC) and the AEC to establish radiation emission standards (January 1, 1961) for the first series of nuclear power plants built in the US. Subsequently, Gofman[1] and Tamplin would challenge the public health foundations upon which these FRC and AEC radiation emission standards were based, claiming these standards yielded unacceptable risks of cancer and genetic diseases and needed to be tightened by at least a factor of 10. Their claims were based on the premise that the LNT dose-response model is valid and they occurred at a time when society greatly feared adverse health effects from radiation exposures. The widespread acceptance of LNT and the real fear of radiation combined to create serious controversy that eventually would end the expansion and development of nuclear power plants in the US. Ultimately, Gofman and Tamplin would have to leave Lawrence Livermore, an AEC entity, as it became for them a professionally inhospitable workplace. This paper will also show that the historically dominating influences of the NAS BEIR committees on LNT and cancer risk assessment for over 50 years had their origins in the controversies between the scientists Gofman and Tamplin and the FRC and AEC.

## 2. The FRC/AEC Create Nuclear Plant Emission Standards Based on BEAR I Genetics Panel Recommendations

In 1956 the BEAR I Genetics Panel [5] recommended that the contribution of man-made ionizing radiation to an individual not exceed 10 rem per reproductive generation of 30 years, with a focus on genetic-based reproductive endpoints. This recommendation assumed that exposure from medical and related uses were already accounting for about half of the 10 rem. Thus, they took the remaining 5 rem exposure value, and divided it by 30 years, obtaining a value of 0.17 rem/year for an acceptable level for population-based exposures. The BEAR I Genetics Panel asserted that there was no safe level of exposure to ionizing radiation and made estimates for genetic damage based on the LNT model. The Panel did not address cancer risk estimates.

At that time, the lowest absorbed dose of ionizing radiation that was believed to produce a statistically significant increase in cancer incidence from a medical or epidemiological viewpoint was $\geq$100 rem [11]. The natural background radiation for

1 - John Gofman had a Ph.D. in nuclear chemistry from the University of California at Berkeley (UCal/Berkeley) under the direction of Glenn Seaborg, who received the Nobel Prize for his discoveries of transuranium elements. Their combined efforts played a significant role in the development of the atomic bomb. After receiving his Ph.D., Gofman received an MD from the University of California at San Francisco. Gofman then became a professor at the UCal/Berkeley, doing significant research in the area of cardiovascular disease with a focus on HDL/LDL, receiving multiple highly prestigious awards. In 1963 Gofman accepted an offer from Seaborg, now director of the Atomic Energy Commission (AEC), to direct its radiation research and risk assessment program at the Lawrence Livermore Laboratory [9, 10].

most areas of the world is between 0.05 to 0.20 rad/year. Consequently, the 0.17 rem/year value became adopted by the US AEC for emission exposure standards for US nuclear power plants. The concern over approximately doubling background exposures was mitigated by the absence of evidence to show that living in high background radiation zones (> 0.75 rad/year) was medically harmful. Thus, the 0.17 rem/year value of the BEAR I Genetics Panel for genetic damage morphed into FRC guidance and AEC regulatory/legal emission standards that Gofman and Tamplin would then use to assess cancer risks.

No significant public dispute arose over the 0.17 rem/year emission standard when operations began on the first of several nuclear power plants. However, this situation changed markedly as disputes arose over emissions of the proposed Monticello nuclear plant, which was to be built about 35 miles northwest of Minneapolis, Minnesota [12]. These disputes would begin in 1966 when certain faculty at the University of Minnesota with public interest concerns demanded that the AEC provide answers to questions about the risks associated with exposures from ionizing radiation in drinking water. Although their concerns were somewhat parochial, this would change in 1968 when Dean Abrahamson, a University of Minnesota Professor, contacted his friend from graduate school, Donald Geesaman, who was working at the Lawrence Livermore Laboratory under the immediate supervision of Arthur Tamplin. Tamplin received his Ph.D. from UCal/Berkeley under the direction of John Gofman and was now working again under Gofman's supervision at Livermore, where they had both arrived in 1963. Their mission was to evaluate the environmental and public health concerns of radiation, which was an outgrowth of the AEC Plowshare Program/Atoms of Peace, an initiative of the previous Eisenhower administration. They were also evaluating the world-wide distribution and possible effects of radionuclides from above-ground testing and those inadvertently released to the environment from underground testing.

Abrahamson had gone to graduate school with Geesaman at the University of Nebraska and contacted him to ask for assistance in this effort to evaluate health concerns associated with the Monticello project. Geesaman shared the concerns of Abrahamson with Tamplin and Gofman, who initially did not consider the emissions from nuclear power plants to be a serious concern as, by comparison, they had been focusing on other potentially higher exposures. Nonetheless, Tamplin eventually became interested and convinced Gofman that a deeper consideration of the issue was needed [12].

During this period, the nuclear physicist Ernest Sternglass of the University of Pittsburgh published a highly provocative paper [13] claiming that above ground testing in the US over the past decades was responsible for about 400,000 infant deaths and two million fetal deaths. Sternglass received enormous publicity after being interviewed on numerous national media outlets and writing many popular spin-off and follow-up articles for the general press [14]. The Sternglass assertions became of great concern to the AEC, and Gofman was asked to evaluate them since he directed the AEC/Livermore radiation health assessment program. This evaluation was actually undertaken by Tamplin [15], who concluded that Sternglass had grossly overstated the potential harm by about a factor of ten for infant deaths. Tamplin was said to have become a "hero" at Livermore, as his paper had discredited the principal claims of Sternglass [10, 16]. However, Tamplin was not fully dismissive, claiming that premature infant deaths were likely, given an uncertainty range, with up to about 4,000 premature deaths possible. The AEC wanted Tamplin to publish the refutation of Sternglass in a genetics journal with a limited audience and not in a widely read journal, like the *Bulletin of Atomic Scientists* [10]. This suggestion was considered highly inappropriate by Gofman and Tamplin who then resisted this recommendation with a highly inflammatory response[2] that began what would become a major series of rapidly esca-

2 - Gofman [10] tells the story of how he had a discussion with two senior AEC officials on this matter, both (i.e., John Totter and Spofford English) whom he knew quite well. In fact, English had been a fellow graduate chemistry student with him at UCal/Berkeley. Gofman asserted they wanted to "white-wash" the findings and told them his opinions in offensive language. It is likely that his personal style contributed to both the publicity his opinions received as well as the hostility he would encounter at the AEC and elsewhere [17].

lating disputes with the AEC. The Tamplin [15] publication in the *Bulletin of Atomic Scientists* and their active engagement with the controversy over the Monticello emission standards redirected Gofman and Tamplin to the issue of low-dose radiation exposures and cancer risks. During the Monticello evaluation process, they became convinced that the dose response for radiation-induced cancer risk was linear, with no safe dose [10, 12, 16].

Gofman and Tamplin used their experiences with Monticello and the AEC nuclear power plant emissions to develop their version of cancer risk assessment[3]. During the summer and early fall of 1969, Gofman and Tamplin had finalized a manuscript on the topic. These efforts resulted in Gofman [11] making a plenary presentation to a conference at the Institute for Electrical and Electronic Engineers (IEEE) on October 29, 1969, in San Francisco[4]. It was at this time that Gofman and Tamplin made their case for LNT as it applies to low doses of ionizing radiation, thereby raising criticisms with the AEC emission standards for nuclear power plants.

In his presentation Gofman stated that:

> "… a hard look at what data do exist leads us to have grave concern over a burgeoning program for the use of nuclear power for electricity and for other purposes, with an allowable dose to the population at large of 0.17 rem of total body exposure to ionizing radiation per year. A valid scientific justification for this "allowable" dose has never been presented[5], other than the general indication that the risk to the population so exposed is believed to be small compared with the benefits to be derived from the orderly development of atomic energy for peaceful purposes."
>
> "… Unfortunately, all the hard data concerning dose-effect relationship in man are for total doses about 100 Rads. Our estimates, therefore, of the effect per rad are, to be conservative, based upon a linear extrapolation from high dosages down to very low dosages…"

Although Gofman and Tamplin did not identify the origin of the 0.17 rem/year value of the FRC/AEC, as noted earlier, it had its roots in the 1956 report of the BEAR I Genetics Panel.

Gofman and Tamplin then applied this value (0.17 rem/year) in a new way, that is, to use LNT to estimate the increase in cancer incidence. They did so by assuming there was a 1% increase in tumor incidence rate/year/rem (i.e., based on an assumed doubling dose (DD) of 100 rem) with this being built upon a natural cancer incidence in the US of approximately 280 people affected/100,000 people/year. When they applied this rate to 100,000 people over the 30-year period, the 0.17 rem exposure translated into 14 newly induced cancers/year. If everyone in the US were exposed to 0.17 rem/year from birth to 30 years, the total exposure greater than background would be 5 rem. Assuming that the risk for all forms of cancer plus leukemia is an increase of 1% in incidence rate/rem, this yields 5 x 1=5% increase in cancer incidence rate. Based on these calculations, Gofman and Tamplin estimated 14,000 additional cancers per year to the US population over 30 years of age. They next added 2,000 more cancers to the total after assuming enhanced

3 - Gofman's conversion to an LNT belief is not clearly presented in his writings. However, it is surprising that he does not highlight the influence of Hermann J. Muller and the perspectives of the radiation genetics community. Rather, Gofman appears to have been more affected by the epidemiological research on leukemia as reported in the late 1950s by Alice Stewart and Richard Doll and in the 1960s by Brian McMahon whose research he commonly cited. On December 18, 1969, Alice Stewart wrote to Gofman thanking him for his December 9th letter and articles and sharing new findings supporting a linear dose response.

4 - The invitation was arranged via an engineering colleague of Gofman's at Livermore [10].

5 - The NAS BEAR I Genetics Panel [5] had made mutation damage estimates and had addressed this question as it was based on 10 rem exposure. If Gofman and Tamplin had dug more deeply into this question, they would have learned that the most prestigious radiation geneticists in the country (i.e., BEAR I Genetics Panel) displayed profound uncertainties and very large differences between each other even when forced to accept LNT when making estimates. For example, panelist George Beadle (Nobel Prize recipient-1958) provided a range of damage uncertainty estimates from a low of 100,000 to a high of 200,000,000 mutational events from 10 rem. It was such extreme examples of uncertainty that created great concern within the BEAR I Genetics Panel. It would eventually lead them to deliberately hide their massive uncertainties and interindividual expert differences from the scientific community and the public [18]. If such uncertainties/differences were revealed, they felt that the public would be unable to consider their policy recommendations seriously. How this information would have affected Gofman and Tamplin is uncertain. However, they would have readily seen that even the expert radiation geneticists were confused, having little confidence in their estimates.

radiation susceptibility by young children, making the total increase to 16,000. Although the additional 2,000 cancer cases were without a biologically based numerical justification, some speculation was offered concerning the possibility of a much-accelerated DD for X-ray-induced cancers resulting from *in utero* exposures. However, this estimation of extra cancers was not added to the total due to the *in utero* exposures. The 16,000 cancer cases were soon morphed into 32,000/year when Gofman and Tamplin decided that the DD for radiation-induced cancer could be decreased in half (i.e., from 100 to 50 rads), thereby increasing the radiation-induced cancer potency by 2-fold[6]. The presentation of Gofman at the October 29th conference [11] generated no national media publications, only a modest article in a San Francisco paper [12]. However, this presentation was known to AEC leadership and raised concerns [10]. This situation would change in less than a month.

Before considering that change, it should be noted that Gofman and Tamplin misinterpreted the meaning of a DD when calculating their sensational estimate. It is easy to understand how someone might be confused by the term DD because that word pair obviously suggests that there is a doubling of any effect of interest when the DD of radiation is applied. However, that interpretation is incorrect for cancer incidence (a somatic effect in irradiated individuals). The term "doubling dose" was presented on page 25 of the BEAR I [19] Genetics Panel Report to the Public when discussing its attempt to estimate "tangible inherited defects" that are present in the first-generation following exposure of a human population to a "doubling dose" of radiation. Just as it would be for estimates of induced cancer in irradiated people, the text of the Panel's report shows that it would be incorrect to conclude that there is a doubling of "genetic effects" already in the first generation. The BEAR I [19] Genetics Panel assumed that the present level of genetic effects in 1956 was 2% in children in the population of the United States. That is, of 100,000,000 children, about 2,000,000 million would experience [harmful] effects of medical importance without any additional exposures to man-made radiation. The Panel's paragraph on this topic then stated: "If we [mankind] were subjected, generation after generation, to an additional DD of man-made radiation, then this present tragic figure of 2,000,000 would gradually increase by 2,000,000 more cases, up to an eventual new total of 4,000,000 [that being a new genetic equilibrium]. It would, to be sure, take a very long time to reach this equilibrium double value. Perhaps 10% of the increase, or 200,000 new instances of tangible inherited defects, would occur in the first generation." Note that this is for an exposure to the DD for many generations[7].

Gofman and Tamplin initially assumed a DD of 100 rem. The BEAR I [5, 19] report did not specifically recommend that value. The Panel did write [19]: "The lowest figure which has been responsibly brought forward for the DD is 5 r, and the largest estimates range up to 150 r or even higher. Recent work with mice (which are, after all, mammals)

6 - A decade after the Gofman and Tamplin [11] cancer risk estimates (1% cancer increase/year/rem) relating to emissions from nuclear power plants, leading groups such as the US BEIR Committee and other advisory groups reported cancer and genetic risk approximately 10-fold lower for the same exposure duration (i.e., 30 years). These estimates were also driven by an LNT model assessment but with a shallower slope [20].

7 - According to Sankaranarayanan and Wassom [21], the BEAR I Genetics Panel provided the first estimate of genetic risks over both the first and subsequent generations of offspring assuming similar exposures with each generation. The Panel developed an indirect method which was called the "doubling dose" approach, based on population equilibrium theory. The equilibrium theory is founded on the assumption that the stability of mutant gene frequencies within populations indicates a so-called balance between two opposing entities: spontaneous mutations (i.e., these occur and become part of the population gene pool at a given rate per generation and natural selection which eliminates the same mutation via early death/failure to reproduce). When the so-called "equilibrium population" is then exposed to radiation, more mutated genes enter the gene pool and are then the object of natural selection, with the population achieving a "new" equilibrium-between both mutation and selection. The duration (i.e., generations) to achieve the new equilibrium and the rate of occurrence are contingent on the duration of exposure, the genetic endpoint, induced mutations and the intensity of selection. The equilibrium theory was continued with the creation of BEIR I [2] but with the quantitative estimates modified by Russell's discovery of dose-rate, and refined to address various types of mutations such as autosomal dominant, sex-linked and multi-factorial diseases, providing estimates of the number of generations needed to achieve the theoretical equilibrium for each endpoint type.

gives some basis for thinking that the DD is not as high as 150 r. The experience in Japan gives some basis for thinking that the DD is larger than 5 r". Considering that the Panel otherwise ignored the massive study led by James Neel in Japan, this seems to be an almost insulting single use of his extensive data that revealed no apparent induced hereditary effects in the population exposed to atomic bombs [22]. Certainly, for the time, the value of the DD used by Gofman and Tamplin is reasonable even though their application of it makes no sense.

### 3. Background Assumptions of the Gofman and Tamplin Risk Estimation

On page 75 of the book *Poisoned Power* [23] Gofman and Tamplin provide a rationale for their radiation risk assessment methodology. They state that "countless geneticists have repeatedly cautioned society about the danger of allowing any increase in the rate at which any type of mutations is introduced into the general population". This statement is consistent with the recommendation of the BEAR I Genetics Panel [5] though not specifically cited in the book. Gofman and Tamplin [23] state that "geneticists know very well that background radiation induces mutations". They go on to cite a 1970 September 8 affidavit by Joshua Lederberg [24], a Nobel laureate, before the Public Service Board of Vermont. They claimed that Lederberg stated that the present FRC/AEC standard of 0.17 rem/year allows for a 10% increase in mutation rates. They then quote Lederberg who stated that the present standards should be more stringent being not more than about 1% of the spontaneous mutation rate. Lederberg then applied this concept to other environmental mutagens such as a host of chemical mutagens. Gofman and Tamplin [23] (page 80) then stated that "natural radiation probably accounts for about 5-10 percent of diseases and premature deaths due to genetic diseases. Since there were ~320,000 cancer plus leukemia deaths in the US annually as of 1970, Gofman and Tamplin assumed that background radiation would account for about 10% or 32,000 ([23] - pages 258, 260). This is the basis for how Gofman and Tamplin converted/forced their methodology to derive the 32,000 annual cancer cases, using the 1% increase in cancer incidence/year/rad, 4,000 more cancers from the very young and reducing the DD in half. They seemed to know the numerical target goal (i.e., 32,000 cases) and altered the model parameters to achieve this value.

It is important to note that the genetics community to whom Gofman and Tamplin refer was led by Hermann J. Muller and his radiation geneticist colleagues leading up to the BEAR I Genetics Panel recommendations for LNT in 1956. What Gofman and Tamplin omit is that Muller and Mott-Smith [25] addressed the issue of background radiation for mature spermatozoa in fruit flies. These are cells that lack most genetic repair processes. They determined that background radiation could account for no more than $1/1300^{th}$ of the control group mutations in Muller's Nobel Prize winning research. That is, it would be nearly impossible to measure such a background dose treatment effect in such a biological model. While a case has been made for a higher background radiation mutation rate for humans due to their longer reproductive life, Gofman and Tamplin failed to cite the massive findings of James V. Neel that did not reveal a significant mutation effect in the offspring of atomic bomb survivors following 75,000 subjects with copious publications from the 1950s to the present [22]. Yet these findings received enormous publicity and were widely published in the peer reviewed literature by Neel and colleagues. Thus, the underlying functional assumptions of Gofman and Tamplin did not consider the Muller and Mott-Smith and Neel data. These data were contemporary to the research of Gofman and Tamplin, challenging the summary statements of the above cited "numerous geneticists". In addition, although Gofman and Tamplin cited the comments of Joshua Lederberg to support their case, they also failed to cite the written comments of Lederberg (October 16) [26] to the Pennsylvania State Senate. In these comments Lederberg stated that he did not support the Gofman and Tamplin cancer risk assessment on mechanistic grounds, concluding that their estimates were "highly implausible". This was also similar to comments by Marvin Schneiderman [27], a biostatistician for the US National Cancer Institute (NCI) and later a staff person for the NAS. He noted that the Gofman risk estimates were "too

high by a factor of 10 even accepting all of his assumptions." These two individuals could not be construed as being agents of the AEC.

### 3.1. Gofman and Tamplin Risk Assessment Presentations

Gofman received an invitation to testify before the Sub-Committee on Air and Water Pollution, Committee on Public Works of the US Senate that was chaired by Edmund Muskie. The invitation had nothing to do with the October 29th IEEE presentation but resulted because he was an Associate Director of Livermore. However, Gofman's presentation on November 18, 1969, was not about Livermore practices but was an extension of the earlier IEEE conference presentation. The presentation to the Senate was entitled: *Federal Radiation Council Guidelines for Radiation Exposure of the Population at Large—Protection or Disaster?* Perhaps the strongest conclusion from this presentation was the following: "… we are speaking out in the strongest terms against the current guidelines for radiation exposure to the population at large. We are urging the Atomic Energy Commission itself to join us in seeking early downward revision of the Federal Radiation Council Guidelines". (page 674).

Gofman and Tamplin also stated: "The only sensible thing to do right now is to reduce drastically the Federal Radiation Council dose allowable to the population at large by least a factor of 10". (Page 666).

In contrast to the presentation in San Francisco, the Senate appearance generated considerable high-level national publicity, and even followed Gofman back to his job at Livermore. Although the Livermore leadership comments were not explicitly critical of what he had said, he was told that it would be necessary, in the future, to obtain clearance/approval of such presentations and related publications before they are given/published [10, 16]. AEC leadership claimed that they did not want to prevent him from doing his job but they did not want to be surprised. It would not be long until Gofman would test this statement.

Five weeks later, on December 28, 1969, Tamplin was scheduled to make a similar challenging presentation at the American Association for the Advancement of Science (AAAS) annual conference in Boston that had a special section on nuclear power [10]. Playing by the new AEC oversite rules, Gofman and Tamplin shared the proposed presentation material with the Livermore administration. To their great disappointment, there was much censoring of their proposed comments[8]. This infuriated Gofman and Tamplin and created heightened controversy and dispute. In the course of the dispute, Gofman claimed to have informed the key organizers at the AAAS that Livermore was a "scientific whorehouse and practices censorship… and anything coming out of the Livermore lab is not to be trusted" [9] and indicated that Tamplin would not give the presentation. With the emotions quite high over the issue of censorship, the Livermore administration backed down, permitting Tamplin to make his presentation without their influence and paying his travel to the meeting. However, the battle lines were drawn between Gofman and Tamplin and the AEC administration at Livermore and Washington, DC. The next confrontation would be about a month after the AAAS meeting. This time it was in Washington, DC, during the third week of January, 1970.

Gofman and Tamplin quickly concluded that they were facing a major confrontation with the AEC administration. However, Gofman may have felt to some extent protected since he could return to his professor position at UCal/Berkeley and perhaps also because his Ph.D. mentor, the Nobel Prize Recipient, Glenn Seaborg, was the director of the AEC, with political contacts in the White House. In addition, it was Seaborg who personally recruited Gofman for his AEC position in 1963. Based on Gofman's recounting of this period, it is not clear that he appreciated the pressure that he had put Seaborg under and how he was testing his former mentor and now director of his organization. Gofman decided that the new AEC strategy to stop his pro-LNT message was not going to be censorship, as they had apparently won that confrontation,

8 – The censoring action led to a volatile confrontation between Gofman and Michael May, a long time AEC administrator. According to Gofman [10], he told May exactly what he had conveyed to the AAAS personnel, with the same explicit language.

but would involve discrediting their message and scientifically embarrassing them. This would be attempted by bringing in multiple AEC-funded prestigious academic researchers who would also testify at the forthcoming Senate hearings. Gofman claimed that he had seen this strategy in action by the AEC over the past years with others. Thus, he anticipated a significant challenge and confrontation [10, 16].

So how did Gofman and Tamplin prepare for this major anticipated showdown in front of the Senate Committee on Atomic Energy? According to Gofman [10, 16], over the next three weeks he and Tamplin wrote 14 manuscripts on the topic of radiation, LNT, and cancer. Of these 14 manuscripts, there would be ten published in the proceedings of the Congressional hearings [28-37], ten would be specific to scientific topics (e.g., organ specific cancers, such as bone, breast, lung, etc., and other related topics) and a summary paper[9]. The other papers would be targeted for publications in different venues. Their strategy was therefore to be the aggressor, to take the challenge to the AEC via the use of the Congressional hearings. They adopted a strategy that was designed to take the AEC administration by surprise, and to hit the topic from so many angles, within a brief period of time, that it would not be possible for the AEC to be organized well enough to counter the Gofman and Tamplin offensive. In addition, since this was being carried out in the US Congress, Gofman and Tamplin were hoping to present an overwhelmingly convincing case that would compel the FRC and AEC to face political pressure and to drastically change their environmental and public health practices. Besides the strong focus on cancer risks, Gofman and Tamplin also estimated that the genetic effects in the population would produce a 5-50% increase of serious diseases and a quantitatively corresponding increase in the yearly death rate. This perspective contributed to their demand for a greater than ten-fold reduction in the radiation exposure standards.

As might be expected, the Gofman performance ramped up an already heightened controversy and the dispute became highly visible within and outside the government, affecting the media, the scientific community, the AEC, and the Livermore and Berkeley communities. Gofman and Tamplin were clearly viewed as "the enemy within," as Gofman would commonly characterize the situation [9]. These were two highly visible AEC scientists, with Gofman being a major leader who publicly challenged and embarrassed his organization and his former advisor. This would also lead to Gofman getting involved in major public debates with talented scientists from the AEC side (e.g., UCal/Berkeley Professor Thomas Budinger) with large attendances, all very formidable affairs, with huge implications [23, 38]. Gofman and Tamplin also became involved with litigation to remove from the federal government (i.e., AEC) the authority to regulate radiation emission standards for nuclear power plants. This case eventually advanced to the US Supreme Court where the Justices ruled against the legal arguments of Gofman and Tamplin. Besides lawsuits, Gofman and Tamplin pursued other publicly conspicuous spin-off activities that only exacerbated tensions between them and the AEC [12]. For example, in 1971 Gofman would help create and become the director of the Committee of Nuclear Responsibility (CNR) (Wikipedia), an activist group dedicated to stopping the threat of nuclear power. However, one of Gofman's activities was quietly overlooked but became influentially significant; it was the response of the US Senate to his Congressional testimony.

On January 28, 1970, only a week after Gofman's Senate testimony, Robert H. Finch, the Secretary of Health, Education and Welfare, sent a letter to

9 - Gofman [9] would state: "In about three weeks we wrote fourteen scientific papers. I'd never done anything like that in my life." The fact that they researched, assembled, drafted, revised and finalized 14 papers in about three weeks is nearly impossible to imagine, especially for those in the domain of scientific publication. The massively accelerated effort would affect the quality of the papers, the failure to properly assess the literature and to properly understand the complexity of each technical area. This made their efforts an easy target for experts in their respective fields. Ironically, it was this criticism that Gofman and Tamplin were trying to counter, yet their strategy actually enhanced it. A reflection of the limited scientific quality of their manuscripts supports the fact that little effort was made to publish these papers in peer reviewed scientific journals. The purpose of the effort was more political than scientific as Gofman and Tamplin understood that the issue would be decided at the highest political levels rather than in a drawn-out scientific process. Gofman and Tamplin's instincts and strategy would prove to be correct.

Senator Edmund S. Muskie with the following recommendation:

> "Drs. Gofman and Tamplin have raised the question of whether the present FRC guidelines are still acceptable. In the past ten years, since the formulation of the FRC basic guides, sufficient additional information has developed from epidemiological studies and animal[10] experiments so that revaluation of such guidelines is believed to be warranted.
>
> In view of our concern with the potential hazard of ionizing radiation in the environment, and as chairman of the FRC, I am recommending that the Council institute a careful review and evaluation of the relevant scientific information that has become available in the past decade. I am recommending that this reevaluation provide, as definitely as possible, estimates of the risk associated with low levels of environmental radiation as a basis for review of the adequacy of current FRC guidelines as applicable to projected radiation levels. Based on projected exposure classes of radiation sources, such as nuclear power reactors, other peaceful uses of nuclear energy, and radiation from consumer products would also be considered."

The FRC would soon contract with the NAS/National Research Council (NRC) to establish the Advisory committee (i.e., BEIR I) to perform the type of review noted above by Finch. So acute was the controversy that, even before a study could get underway, Cyril Comar, Chair of the BEIR I committee, wrote to Charles Dunham, who had moved from the AEC to be head of the NAS Division, informing him that all leading radiation advisory organizations, domestic and international, are not in agreement with the Gofman and Tamplin analyses and recommendations [39]. However, Comar concluded that even though no evidence supported the Gofman and Tamplin position, "the allegations and widespread public concern generated by their actions has forced the committee to take up the issue as stated by Finch." [39].

## 4. The Gofman - Tamplin Affair in Perspective

The major conclusion of the present assessment is that this episode in environmental and public health history was an example of misguided scientific activism dressed in the garb of apparent high-powered science that patently failed to apply the gold standard for ensuring scientific quality: the process of peer review. In essence, stoking the public's fear of radiation with exaggerated claims of deadly diseases was used to influence the political process, instead of the scientific peer-review process, to accept an unproven (and possibly invalid) scientific model (LNT), thereby hindering the development and expansion of nuclear power plants in the US and around the world. In retrospect, the actions of Gofman and Tamplin were quite successful in ensuring that ionizing radiation would be viewed as acting without a threshold and, therefore, was the cause of, or significantly contributory to, a vast range of cancers and genetic related diseases.

It is hard to find two scientists who were more successful than Gofman and Tamplin in helping to achieve a major societal transformation. Their actions were highly significant in affecting the long-term cancer risk assessment policies of the US and many countries, and they did so without being a part of either the advisory committees that set these polices in motion or of the agencies involved in regulatory decision making. In fact, Gofman and Tamplin were a type of scientific/societal catalyst that activated a crucial step that was necessary to make the LNT policy changes occur. However, Gofman and Tamplin knew very well that, as AEC insiders, their professional careers within this organization were at great risk, not only because of their specific passionate opposition to the goals of their organization, but also because of the leadership style of Gofman. In the case of Gofman, he had a very generous and long-term funding arrangement at Livermore, without having to write competitive grants. He had an ideal arrangement for

10 - It has recently been discovered that William Russell, Oak Ridge National Laboratory, choose not to publish a large-scale animal study on lifespan and cancer risk involving a very large single (600 R) X-ray exposure to the male parent (~ 1959). No treatment related effects were observed. Russell would publish the findings some 35 years later in a coordinated effort to win a lawsuit in the UK [40]. It is not known how these findings may have impacted the low dose radiation risk assessment debate; however, it seems certain that it would have been used by the AEC to support their position and would have forced Gofman and others to address these findings. The Russell study was a very strong effort, even providing compelling evidence nearly 35 years later in the face of more stringent radiation standards.

a talented academic researcher. Yet, he risked and lost it in his principled quest to challenge the AEC to both rethink LNT and change its commitment to nuclear power. Although not as prominent as Gofman, Tamplin also put his career at the AEC in great jeopardy, and he was the first against whom AEC directed its professional emasculations. In the end, both men were compelled to leave the AEC, with Gofman eventually returning without his generous funding to UCal/Berkeley. According to Gofman [10, 16], his promised National Cancer Institute (NCI) follow-up funding also fell victim to AEC vindication. Gofman would take an early retirement and spend the rest of his professional life challenging the nuclear industry and strongly supporting the LNT model. Yet, despite his strong efforts to write modestly impactful books over the next three decades and to testify in multiple venues, Gofman had given up his academic base and had lost much power and influence.

Gofman may not have realized it, but he and Tamplin actually had won the major battle by instigating the NAS to create BEIR I and having NAS fill BEIR I members with many key supporters of Hermann Muller's LNT model (e.g., James Crow, William Russell, Edward B. Lewis). In 2021 the medical historian and colleague of Gofman, Henry Blackburn [41] wrote an insightful and sympathetic reflection on Gofman's life. In a follow up email communication by Blackburn [42] to the author (EJC), he revealed that Gofman lost everything in the process except his wits, but he still remained a positive and happy person. Many who would come to know the Gofman-Tamplin and AEC story would probably see them as courageous; this would also likely be the case for those having scientific and policy disagreements with Gofman and Tamplin.

Yet, within their truly courageous story, there is considerable and, at times, troubling complexity. The current assessment presents Gofman as knowingly venturing into a more-or-less "self-destructive" professional mission. To confuse and distract the AEC in the deployment of its professional resources against Gofman and Tamplin following Gofman's Congressional testimony, these men preemptively wrote 14 papers in three weeks on radiation cancer risk assessment that could be used to fully support Gofman's testimony and, thus, potentially ward off post-testimony criticisms from the AEC. This massive publication effort seemed even more daunting after the realization that Gofman and Tamplin had virtually no background experience in cancer risk assessment. Such a preemptive strategy signifies that Gofman and Tamplin were well aware of the AEC forces arrayed against them and also that the battle to be won was in the form of a political judgement rather than a scientific argument. Each of these 14 papers was designed to challenge leading authors, professionally and non-professionally, in key areas of cancer risk assessment. It is hard to comprehend the decision to undertake such a strategy, and yet the strategy ultimately managed to achieve its goal of sustaining the credibility of Gofman and Tamplin with key high-level elected officials, especially Senator Muskie. As expected, the Gofman testimony spurred presentations from leading experts in multiple areas of cancer risk assessment and radiation-induced mutations. In contrast to the sensational and personalized style of Gofman, the opposing perspectives were standard, hard-hitting professional rebuttals, avoiding personal attacks. It is generally recognized that many weaknesses and flaws in the analyses of Gofman and Tamplin were exposed during the rebuttals of the opposing scientists. However, whether the criticisms were accurate and on target was really not the overriding issue, especially since the scientific criticisms had been directed at a lay audience of elected officials and non-scientists. In fact, Gofman and Tamplin had won the debate by convincing the senators to create the BEIR I Committee very soon after the Gofman testimony. This "triumph" was glowingly underscored by Tamplin [39] soon after the publication of the NAS BEIR I Committee report in 1972. Tamplin [39] clearly had taken great satisfaction in their (Gofman and Tamplin) achievement, which was the endorsement of LNT by NAS BEIR I [2]. In the end, the BEIR I Committee was dominated by LNT-supporting committee members who readily endorsed the LNT recommendations of NAS BEAR I [5], but also decided to include a provision on cancer risk assessment. This decision would prove transformative within society as the LNT recommendations were soon generalized by EPA to encompass chemicals as well.

## 5. Summary Evaluation of the Gofman/Tamplin Cancer Risk Assessment Approach

Given the above background, the next section briefly evaluates the analysis of Gofman and Tamplin [23, 43] that was used to challenge the emission standards of FRC/AEC. Their cancer risk assessment involved the leukemia data from the Japanese atomic bomb studies and the findings of Court-Brown and Doll [44, 45] for ankylosing spondylitis (AS), which involved leukemia and other cancers. Gofman and Tamplin followed the summaries provided by the International Commission for Radiation Protection (ICRP) [46] that were extensions of the 1957 report of Lewis [47], who made the first quantitative risk assessment for leukemia from these two populations.

The principal difference between the Lewis approach and that of Gofman and Tamplin was that Lewis also considered leukemia in two other populations (i.e., radiologists and children with enlarged thymuses that had been treated with X-rays to reduce their sizes) and did not consider other cancers; Gofman and Tamplin applied their cancer risk assessment to the FRC radiation emission guidelines that had been adopted by the AEC for nuclear power plants. The Lewis approach has been strongly criticized for each one of the four population groups he had evaluated [48, 49]. With respect to the AS, Gofman and Tamplin cited the 1965 paper of Court-Brown and Doll, which was an extension of their earlier findings (1957).

The study on AS and radiation-induced leukemia was a substantial effort funded by the British Medical Research Council. In the preface of the final published report [44] the Council wrote the following: "the present investigation was undertaken in the hope of obtaining an indication of the effects of small doses of radiation on human beings. From the nature of the case this could not be obtained directly, for few of the patients had received less than a mean dose of 250 r to the bone marrow; but it was hoped that a sufficiently precise relationship between the high doses of radiation studied and the corresponding increased incidence of leukemia could be derived to allow extrapolation to be made with reasonable confidence to lower levels of dosage. **"Unfortunately, this hope was not fully realized, for it is possible to derive more than one type of dose response relationship for the data," (emphasis added).** The authors of the report also stated to the Council in the preface of the report **"that until much more work has been done it will not be possible to decide between the alternative hypotheses." (emphasis added).** However, Lewis [47] failed to share this information with the reader while using the study to promote his goals.

The analysis of Gofman and Tamplin [23, 43] also failed to acknowledge the limitations expressed by the funding agency and by the researchers themselves. In addition, the dose to the spinal marrow used in the Gofman and Tamplin [23, 43] analysis was quite extreme, being 880 rad as the "average" dose. The disease estimation procedure involved a direct extrapolation from the 880 rad to the emissions standards of the FRC/AEC. The 1965 paper of Court-Brown and Doll did not disavow or modify their concerns and restrictions as clearly indicated in the 1957 paper. The principal value of the 1965 paper was the emergence of other cancers at what they called "heavily irradiated sites", a circumstance with the same very high to low dose extrapolative limitations. Yet, Gofman and Tamplin never mentioned these factors nor were they challenged to do so.

With respect to the Japanese survivor studies, the report of Gofman and Tamplin [23, 43] relied upon a summarized report that integrated an accumulating number of leukemia cases over time. Furthermore, there were several revisions concerning the exposure assessment to various types of radioactive agents in the cities of Hiroshima and Nagasaki by the Atomic Bomb Causality Commission (ABCC)/Radiation Effects Research Foundation (RERF) as occurred in 1957, 1965, 1986 and 2002 [48].

Gofman and Tamplin [23, 43] simply adjusted their risk assessment calculation to be applied to the FRC guidelines based on cancer risk/year/rem assuming LNT. However, what they failed to do was to reconstruct an iterative dose response for leukemia cases throughout the 1950s and 1960s, as reported by Calabrese [48] who revealed a highly consistent J-shaped dose response throughout the 1950s, 1960s, 1970s, and 1980s, over a 40-year

period. These estimates were based on following the original data, and each of the exposure reconstructions for each city (e.g., 1957, 1965 and 1986). These findings indicate that the linear dose response assumption of Gofman and Tamplin [11, 23, 43] was not supported. Thus, in the principal assessment that was directed to challenging the FRC guidelines, the core data and approaches used by Gofman and Tamplin were not supportive of their LNT hypothesis. These criticisms that challenged the LNT and the Gofman and Tamplin approach were provided in the 1970 Congressional Record, which contained the Gofman testimony/articles. However, the J-shaped response had been reported by Wald [50] and even discussed by the US NAS BEAR I Pathology Panel in 1956 [48, 51]; however, this group failed to pursue this viewpoint, probably because it did not conform to the existing paradigm, even though the J-shaped findings were a consistent feature of the data[11].

Gofman and Tamplin attempted to distract AEC experts and challenge their analyses by attacking published cancer dose-response studies for the radiation of multiple organs. One such criticism by Gofman and Tamplin [23, 43] was of radium studies conducted by MIT professor Robely Evans that supported the threshold model. This criticism led to a dispute with Evans over his sarcoma data. Evans indicated that his data were so dramatically non-linear that the linearity hypothesis displayed a statistical probability of less than 1 in 200,000,000 chance of occurring. Evans concluded that the odds against the linear assertions of Gofman and Tamplin were astronomical and not be even remotely supportable [54]. Gofman and Tamplin would also challenge the findings of William Russell that X-rays and gamma rays display a dose-rate effect in the male and female reproductive cells (i.e., stem-cell spermatogonia and primary oocytes), such that at low dose rates the damage is repaired. In the case of females, Russell [55] reported that it would take a dose-rate exposure some 27,000-fold greater than background before exceeding the repair capacity of the oocyte[12]. Russell claimed that there was a threshold dose-rate response in the primary oocytes but not in the stem-cell spermatogonia. It should be known that Russell [55] presented these findings at the May 5-7, 1969 conference at the Lawrence Livermore Laboratory that Gofman helped to direct.

Gofman and Tamplin [33] also provided their analysis of bone cancer in dog studies at the University of Utah to the Senate entitled: *Osteosarcoma Induction in the Beagle Dog with Alpha Emitting Radionuclides,* it was also submitted to the FRC a month later. Professor Charles Mays [56], who evaluated the Gofman and Tamplin paper, sent them a letter on March 25, 1970, concerning this paper:

> "… No doubt you wish for these organizations to be favorably impressed with the results of your calculations…
>
> Unfortunately, your manuscript contains a number of errors. For your convenience I enclose a copy with the numerical mistakes corrected in red for easy identification. This provides the opportunity to correct your manuscript before the official version of the Hearing is printed. Altogether, your 10 pages of text contains 71 numerical mistakes… However, errors of omissions of a much more serious nature exist… You have only selected those levels

11 - In 1981 Gofman [52] would finally address the issue of the J-shaped dose response for leukemia for both Hiroshima and Nagaski. His published analysis was not a strong one as it would cite only a single study of blood lymphocyte mutations as a biomarker for exposure that included only 18 people at Hiroshima whose exposures were beyond 2.4 Km from the hypocenter [53]. Gofman mistakenly claimed that there were 36 subjects from both cities. About half of the subjects reentered the city soon after the bombing, thereby receiving further exposures. No information was available on how this small sample was selected, their gender, occupation(s) and other relevant variables. Yet, Gofman would use this study to dispute the reliability of the massive efforts to reconstruct exposure estimates by the ABCC/RERF over the past thirty years in both cities. He claimed that the "control" subjects in both cities living furthest away from the hypocenter were exposed to about 6 rads more radiation than adjacent low dose exposure groups, thereby accounting for the J-shaped dose response for both cities. While the data were simply too limited and fragmentary for any conclusion, Gofman [52] used this study to reject the J-shaped dose response findings as being due to a low dose beneficial response or simply chance.

12 – In contrast to Bond, Evans and others who published written rebuttals to Gofman and Tamplin, William Russell presented a formal seminar at Oak Ridge National Laboratory in 1971 but published no follow up paper directed toward Gofman and Tamplin. According to Paul Selby (personal communication), then a graduate student of Russell, Russell gave his presentation to an audience that was much larger than normal. Selby indicated that Russell did not think very highly of the Gofman and Taplin assessment and exposed many flaws in their analysis.

> which happen to support your pre-conceived "law". You have disregarded those which do not. This is hardly likely to impress the scientific community nor anyone else for that matter. It is no new discovery that good fits to any line through data can be made by discarding the points which do not fit.
>
> In view of the importance of an accurate evaluation of the true cancer risk at low skeletal doses, and your biased selection of data, I am preparing a summary of all of our pertinent osteosarcoma induction results up to 1 April, 1970."

Months later (August 24, 1971) Mays wrote to the Senate stating that he had sent to Drs. Gofman and Tamplin their manuscript that contained the 71 errors and others of omission. He stated that: "It is with dismay I have learned that Drs. Tamplin and Gofman have not corrected their manuscript which related to our work, although they knew well in advance that their manuscript contained 71 numerical error (yes, seventy-one) and it deliberately omitted that part of our data which failed to support the linear hypothesis."

Gofman [57] would subsequently rebut some comments concerning the more trivial errors pointed out by Mays. However, Gofman was surprisingly unresponsive to the assertion that he and Tamplin disregarded data that did not fit with their LNT model.

Gofman [57] would also direct his rebuttal to comments of Victor Bond who provided a plethora of criticisms [58, 59] of the Gofman and Tamplin paper on breast cancer that was based entirely on the experimental research of Bond. Bond pointed out that Gofman only presented data on one study, ignoring data from other experimental studies and rodent strains in which radiation-induced mammary cancer risks were considerably less, and also cases where risks were less than control group (i.e., J-shaped dose response). That is, Gofman and Tamplin were very selective, using only data that supported their perspective, ignoring other non-supportive findings and not sharing their basis for such selection. Bond also pointed out that mammary tumors in the rats are not clinically relevant to human breast tumors, another point omitted/not addressed by Gofman and Tamplin. Of further relevance is that Bond [58] indicated that he estimated the annual risk from cancers in the US to be associated with the AEC exposure standards at 3,400 cases as an upper bound but with the risk being from zero to the upper bound with "the most probable value far below this figure" (i.e., 3,400). Gofman responded to this estimate in the following manner. He wrote that the AEC's Dr. Victor C. Bond's "conservative" cancer estimate for the FRC 170 millirad emission standard would yield "3,200 extra cancer deaths per year", with no mention of the Bond upper bound restrictions and related comments. Again, one finds that Gofman and Tamplin mischaracterized what Bond wrote, thereby giving a false representation. Furthermore, the reference that Gofman and Tamplin cited on page 107 listed the value at 3,200, not 3,400 cases per year. The "trivial" mistake of 3,200 vs 3,400 cases is also reminiscent of the Gofman and Tamplin papers being careless with details, as pointed out by Mays. This same issue is also seen in the 1981 book of Gofman [52] on the J-shaped leukemia data in which he did not provide easily obtainable information on several non-trivial critical study features that were materially relevant to the study.

## 6. The BEIR I Committee

When the NAS BEIR I Committee [2] assessed the effects of the atomic bomb explosions on the leukemia incidence of survivors, it relied upon papers that combined exposure groups in the 2.00-2.49 Km range with those at 2.50+ Km from the hypocenter into a single control group. These cumulative summaries of reported cases were established using the 1965 revised exposure assessment (i.e., TD65). By combining the lower dosed exposure groups, the respective papers indicated that the leukemia incidence at Hiroshima and Nagasaki was consistent with a linear dose-response model. In practical terms this meant that the "control" group included all subjects whose exposures were ~ < 5 rads [48]. The combining of the lower dosed groups in this manner was strongly criticized by Gofman [52] as being inappropriate for the data analysis, leading to incorrect associations in the critical low-dose zone. As noted above (see footnote 9), Gofman recognized the occurrence and consistency of the

J-shaped dose response of leukemia in Hiroshima and Nagasaki.

The present assessment indicated that the BEIR I Committee failed to properly assess the long series of cumulating radiation response data on leukemia. They settled for an LNT-biased analysis that grouped all data ~< 5 rad, thereby creating a biased analysis that assured an LNT conclusion. At the least, the BEIR I Committee should have acknowledged the occurrence of the J-shaped dose response for each city and attempted to account for these observations as did Gofman [52], and as was done later by Cuttler [60] and Calabrese [48]. The recognition of the failure of NAS BEIR I [2] to provide such an analysis is highly problematic from a scientific perspective.

## 7. BEAR I Geneticist Reflects on the Gofman - Tamplin Controversy

It is interesting to note that Sewall Wright, a member of the 1956 BEAR I Genetics Panel, wrote to William Russell on December 23, 1970, concerning the Gofman and Tamplin challenge to the FRC/AEC ionizing radiation emission standard. Wright [61] noted that: "They state that the evaluation of risk has been approached in the WORST possible fashion but it is not clear to me what they proposed unless it is a complete ban on all man-made radiation".

It is clear from the letter of Wright to Russell that the Gofman-Tamplin argument was in the far extreme and not consistent with current understanding of the role of genetics in human diseases, including cancer, leading to greatly exaggerated disease estimates. Similar concerns were raised by many others, as noted above, concerning Gofman during this time period. Nonetheless, in many respects, the positions of Gofman and Tamplin and Sternglass, seen in retrospect, appear to be like a negotiation in which opposing parties start with their highly polarized position. As noted earlier, Gofman and Tamplin were not successful in their attempt to eliminate the threshold-supporting authority of the AEC for nuclear power plant emissions, but they were successful in affecting the actions of elected officials, media, and the public concerning fear of all doses of ionizing radiation, no matter how small.

Wright further wrote that "They estimate that a dose of 5 additional rads up to age 30 would lead to a 5 to 50% increase in death rates (or 150,000 to 1,500,000 extra deaths per year in the U.S. population). They state that they derive this from the assumption that all human disease is due wholly to heredity, that the unfavorable heredity is due wholly to radiation, and that human beings would live forever but not this… Actually, heritability is not very great for most human diseases including cancer and mutations due to radiation are not the sole cause."

> "I find it difficult to reconcile their estimate of the damage of 5 r per generation… with the… relatively slight effects of 100's of r's... in your mouse colony."

## 8. Concerns with the Veracity of Gofman and Tamplin

On page 97 of the 1971 book *Poisoned Power,* Gofman and Tamplin [23] write that when they made their cancer estimates during the October 29, 1969, IEEE conference presentation, "we anticipated no opposition whatsoever to our scientific findings. We expected the nuclear electricity industry and the US AEC to welcome our report on the cancer plus leukemia, especially since the findings were being made before a massive burgeoning of the nuclear electricity industry. At that time (October, 1969), we had not given any special thought to the nuclear industry. In fact, in our preoccupation with a careful analysis of the hazard per unit of radiation received by the people we have thought the nuclear electricity as one of the most innocuous of the Atomic Energy programs, a view we have now had to alter radically." Thus, Gofman and Tamplin emphasized that at the end of October 1969 they had not given any "special" focus on the nuclear industry and radiation risks. The following information projects doubt on the veracity of this statement:

Gofman and Tamplin attended a conference at Livermore over March 5-7, 1969, on the "biological implications of the nuclear age". Tamplin presented a paper at the Conference that addressed human health risks of radioactive material from fallout. Gofman provided the conference summary.

Professor Dean Abrahamson from the University of Minnesota attended the conference and used it to meet Donald Geesaman, Arthur Tamplin and John Gofman. Abrahamson, Tamplin and Gofman participated in discussions on the radiation emission standard of 0.17 rem/year for nuclear power plants, the 0.5 rem exposure at the boundary of the facility, and the scientific foundations for these values. Thus, Gofman and Tamplin were aware by early March 1969 of the key issues and concerns of Abrahamson. What is also clear is that they learned that the 0.17 rem/year standard had been applied to genetic risk, not cancer. It would be in this application that Gofman and Tamplin would create much concern and attention. Of considerable importance is that Gofman and Tamplin did not acknowledge that the BEAR II Genetics [6] and Medical/Pathology [62] committees disavowed the use of linear extrapolation to estimate cancer risks from low-dose radiation exposures. Furthermore, Gofman and Tamplin actually forged ahead and practiced what the BEAR II (1960) Genetics and Medical/Pathology committees had explicitly recommended against, without ever citing that their approach contradicted BEAR II (1960) recommendations.

For the past several years before the 1969 conference at Livermore, Abrahamson challenged the AEC on issues related to its radiation emission standards and sought information and assistance from Geesaman and other key scientists at AEC (i.e., Gofman and Tamplin) who Geesaman recommended.

These four individuals (i.e., Abrahamson, Geesaman, Gofman and Tamplin) discussed public health issues related to the proposed Monticello nuclear power plant.

By summer of 1969, Gofman and Tamplin were convinced of the significance of radiation exposure issues raised by Abrahamson regarding the generation of electricity by nuclear power plants.

According to Semendeferi [12], Gofman and Tamplin had received at least two invitations by August 1969 to participate in meetings on nuclear power plants and public health issues related to emission standards for radiation exposures. One invitation for a meeting in September 1969[13] concerned the Rowe Yankee nuclear power plant in Vermont. The other meeting to be held on October 10 and 11, 1969, dealt with the Monticello nuclear power plant in Minnesota. These invitations were extended by Abrahamson.

The Vermont meeting: Gofman and Tamplin [43] wrote that "Tamplin went to Vermont with the sole purpose of trying to get the AEC to present an estimate of the biological effects of exposure at the FRC radiation protection guideline." The September 13, 1970 edition of the New York Times (page 35) [63] stated that: John Gofman also testified in Montplier, Vermont, and asserted that an additional 32,000 annual cancer cases could potentially occur due to nuclear power plants and that this estimate was very conservative. Semendeferi [12] stated that the AEC experienced its first public setback for generating electricity from nuclear power at that meeting. This was principally due to the actions of Tamplin and Gofman who publicly tried to compel the AEC to provide numerical estimates of health risks from exposure to radiation at the FRC emission guidelines. Semendeferi [12] also stated that at the Vermont meeting the AEC stigmatized Gofman and Tamplin as critics of the nuclear power industry.

The Minnesota meeting: Harry Foreman, who organized the Minnesota symposium, sent an invitation to Tamplin on August 4, 1969. In the letter he stated "The atmosphere in Minnesota is highly charged vis-a vis nuclear energy and doubts by reputable scientists (such as yourself) may well result in a furor that could drive nuclear power plants from the state forever…". According to Gofman and Tamplin [43] Tamplin formally argued at the Minnesota meeting that "the guideline dosage for exposure of the population was inappropriately too high and that no one should consider exposing the population to anything close to the guideline dosage."

According to Semendeferi [12] (page 80), the participation of Gofman and Tamplin in the nuclear power issue markedly strengthened the position of the Monticello opposition. Local newspapers highlighted the views of Gofman and Tamplin with

13 - The Rowe Yankee nuclear power plant in Vermont was the first major facility in the US, starting operations in 1961.

respect to the Monticello dispute. Tamplin [64] published an article based on his presentation at the Minnesota meeting in which he proposed to apply their risk assessment methodology to the effluents of nuclear power plants. He concluded his paper stating that "I view the burgeoning nuclear power industry with a great deal of anxiety. My impression is that these power plants should be designed so as to approach absolute containment of radioactivity."

Documentation of the actions of Gofman and Tamplin from early March to mid-October, 1969, contradicts their statements in the book *Poisoned Power*. That is, before the October 29, 1969 IEEE conference, Gofman and Tamplin claimed they had not given special thought to the possibility of nuclear power plants exposing the public to harmful levels of radiation. However, this claim of Gofman and Tamplin is contradicted by their documented involvements prior to October 29, 1969, in preparing, traveling, and participating in meetings on radiation exposures from nuclear power plants as well as in their pre-October written and spoken critiques of the AEC. Furthermore, Gofman's IEEE presentation was framed around the nuclear power plant emission standards that Abrahamson had already introduced to him well in advance of the October 29, 1969, meeting. This documentation indicates that the presentation at the IEEE conference had been significantly affected by prior considerations of issues related to health concerns with emissions from nuclear power plants. These claims of Gofman and Tamplin in *Poisoned Power* were incorrect and dishonest. Perhaps they were trying to create the image of an honest broker with no stake in the game since concealing their previous involvements and positions on radiation emissions from nuclear power plants, as they did in *Poisoned Power*, would seem to help foster their "honest broker image". In my opinion, the multiplicity of events and the close timing of the writing of the book to the actual events makes a convincing case that Gofman and Tamplin were deceitful. Other actions by Gofman and Tamplin also displayed a pattern of deliberate deception and manipulation, such as misrepresenting the findings of Bond, Evans, Mays and others in Congressional testimony while, at the same time, failing to share and/or explore contravening evidence such as the massive mutational data of Neel on the children of Japanese survivors of atomic bombs [22] and the issue of background mutations addressed by Muller and Mott-Smith [25].

## 9. The Enabling of Gofman and Tamplin - It Started with BEAR I Deceptions

The political success of Gofman and Tamplin within the Senate and joint Congressional committees of the US Congress was significantly affected by prior activities of the radiation genetics community. The most significant scientific aspect was the leadership of the US NAS BEAR I Genetics Panel that recommended a switch from a threshold to a linear dose response model in 1956 for radiation-induced genetic risks. This occurred amid enormous positive and unchallenged publicity. What the public did not know from the BEAR I Genetics Panel reports and publicity was that the panel deliberately misrepresented the scientific record concerning the extent of professional uncertainty and variation amongst the geneticists concerning risks from radiation-induced mutation [65]. The public/scientific communities did not know that the panel removed the three most divergent estimates of panel members to give the impression of conformity and agreement amongst the remaining six estimates (now hidden from the scientific community) and that the range of uncertainty among the remaining six was much more extreme than the panel reported. The public/scientific communities also did not know that another three members refused to even provide estimates based on the vast uncertainties. On page 146 of the Gofman and Tamplin [43] book '*Population Control' through Nuclear Pollution*, Tamplin states: "The question is not with your ability to detect [illness/disease], if indeed it cannot be detected, it is what is the numerical value, theoretically. Obviously, if you cannot detect it, there is no other way you can arrive at it. If the present levels of radiation protection guidelines have been set,…..by a group of competent scientific individuals who have weighed this situation carefully, then that must mean that they have an idea of what the precise effect would be, theoretically at least, on a scientific basis." One sees here that Gofman and Tamplin would rely on the authority of

groups like the BEAR I Genetics Panel to provide the country with the best scientific understandings. What Tamplin did not know was that the BEAR I Genetics Panel committed scientific misconduct, hiding their uncertainties and misrepresenting the scientific record [65, 66].

This same panel also refused to give standing to the 10-year atomic bomb offspring mutation study of James V. Neel, who himself was a panel member, explicitly because the findings did not show a treatment related effect. Neel would eventually challenge Hermann Muller on this matter, but only after the major NAS BEAR I [5] Genetics Panel reports were released to the public [22].

During this period of Panel meetings, William Russell, another BEAR I Genetics Panel member, completed a large longevity and cancer study with mice that showed no treatment effect in the offspring of highly exposed males (see footnote 8). Almost certainly because of the negative findings, Russell deliberately hid these results from the scientific community, failing to submit the research for publication and keeping it secret from major advisory committees, some of which he was a member. Russell would eventually publish this research some 35 years later to help win a major lawsuit in the United Kingdom [40].

In 1960 the BEAR II Genetics Panel released two reports, a technical [6] and public summary [67], updating their 1956 publications. In this updated reporting, the technical report of the Panel indicated that it was inappropriate to estimate cancer risks at low doses/dose rates due to unacceptable uncertainties in the extrapolation process. However, this most critical statement was omitted from the public report. Yet, it is hard to imagine a more significant conclusion, and it being one not shared with the public.

A detailed evaluation of the LNT cancer risk assessment methodology used by Gofman and Tamplin reveals that they assumed that the LNT dose-response model developed specifically for assessing risk of radiation-induced genetic mutations could also be used for assessing risk of radiation-induced cancers. In other words, without experimental proof, they assumed that the two biological processes of mutagenesis and oncogenesis were essentially equivalent and that the LNT model could be used interchangeably to predict mutagenic and oncogenic risks. However, the mechanistic processes of mutagenesis and oncogenesis have been known to be vastly different and complex processes for quite some time by many prominent scientists. In fact, the BEAR II Genetics and Medical/Pathology Panels may have also thought so at the time as they strongly rejected use of the LNT dose-response model for cancer risk assessment. If Gofman and Tamplin knew that the BEAR II 1960 Genetics panel [6] explicitly rejected the use of LNT for a low dose cancer risk assessment, they should have been honorably obligated to acknowledge it and explain their rationale for using it. However, since this did not happen, Gofman and Tamplin either knowingly ignored the BEAR II rejection of LNT in cases of cancer risk assessments or were completely ignorant (unaware) of the BEAR II rejection. Since knowingly ignoring it without explanation would be considered a dishonorable act and ignorance of it would imply incompetence, neither action could be considered acceptable or laudable behavior for prominent scientists such as Gofman and Tamplin. In any case, the end result was that Gofman and Tamplin used LNT to make predictions of cancer risks that stoked public fears and convinced anxious senators to legislate a legacy of LNT-biased BIER committees, perpetuating LNT ideology.

Furthermore, Gofman and Tamplin calculated that background radiation was the cause of 10% of the cancers and leukemias (based on a faulty understanding of what the DD is, as discussed above) and the cause of 5 to 50% of the genetic diseases and deaths annually occurring in the United States. What Gofman and Tamplin failed to realize was that the NAS panel of eminent BEAR I geneticists actually failed to come to any quantitative consensus on the nature of the dose response in the low dose zone. In fact, Gofman and Tamplin would be misled by the BEAR I Genetics Panel [5] misrepresentation of the research record and, therefore, it can be legitimately argued that this BEAR I misrepresentation was ultimately responsible for their faulty methodological approach to risk assessment as it afforded Gofman and Tamplin the license and freedom to promote their LNT agenda.

The letter and statement by Sewall Wright in this paper is highly critical of the Gofman and Tamplin cancer risk assessments and is clearly important on its own merit. However, it may be interesting to speculate further on what may have happened if certain LNT deceptions had not occurred. For instance, had Gofman and Tamplin known that Wright's LNT-based estimate of radiation-induced mutational risk in humans was one of three estimates removed by the BEAR I Genetics Panel [5] for the expressed purpose of improving the statistical spread among estimates, Gofman and Tamplin may have acted with greater scientific objectivity and integrity in 1969 than they otherwise did. Similarly, had Gofman and Tamplin known about the cover-up results from the Russell lifespan and cancer study in 1959 instead of 35 years later [40], they may have again acted with greater scientific integrity in 1969 than they otherwise did. These are only two examples of the many LNT deceptions documented over the past decade or so [22, 48, 65, 66]. The examples above illustrate specifically how the early LNT deceptions of BEAR I and Russell spawned and gave rise to the later new deceptions of Gofman and Tamplin.

## 10. Conclusions

In light of the above assessment of the activities of Gofman and Tamplin, it becomes obvious why the Atomic Heritage Foundation declared that Gofman was instrumental in the adoption of the linear no-threshold model and in the wider acceptance of the somatic risks of ionizing radiation. What they didn't say was that he achieved this goal via an amazing causal nexus:

- being a highly regarded graduate student of Nobel Laureate Glenn Seaborg, who discovered plutonium for the atomic bomb;
- becoming a physician;
- being a very accomplished UCal/Berkeley professor, with much knowledge of the physical and biological sciences;
- being appointed by AEC Director Seaborg as an Associate Director of the Livermore in charge of radiation health effects;
- becoming involved with the Monticello nuclear power plant dispute on the side of opponents;
- having an inspired and talented colleague (Tamplin);
- being the ultimate insider/now called whistle blower that appealed to the media, assuring widespread publicity;
- linking his publicity with his traits of being highly provocative and unabashedly challenging;
- being hard-working;
- being extremely courageous;
- being prone to exaggerate risks and misrepresent the facts to manipulate societal fears of dreaded diseases such as leukemia, cancer, birth defects and other genetic diseases in order to win his political battles (which he did) while not being forced to be subjected to rigorous peer review.

The historical foundations of LNT are incomplete without a recounting of the Gofman-Tamplin affair and an accounting of its effects on the process of cancer risk assessment up to the present time. Since these two scientists weren't key researchers, members of key committees such as BEAR I, or major decision makers, Gofman and Tamplin have obviously been overlooked regarding their huge impact on the history of LNT. However, this unique partnership of courageous risk takers challenged the administrative, scientific, and political leadership at the highest levels, despite their numerous flaws, limitations, and questionable ethics. The LNT story is also incomplete without grasping the significance of the impact of the misrepresentation of the scientific record by the BEAR I Genetics Panel on their uncertainties for estimating radiation-induced mutation at low doses and the cover up actions of Russell and their effects on the Gofman and Tamplin story. Further, if Gofman and Tamplin had known that the 1960 BEAR Genetics and Medical Panels were strongly against low-dose extrapolation for cancer risks, the Gofman-Tamplin affair may never have occurred.

Nonetheless, the Gofman-Tamplin affair did occur as it stoked the public fears that provided the political rationale and incentive for the US Congress to instruct the NAS to form the BEIR I Committee in 1970. The BEIR I Committee, which was packed with pro-LNT scientists, not surprisingly

recommended the EPA adopt and use the LNT dose-response model in assessing cancer risks from radiation exposures. Having been so well served by the NAS BEIR I Committee, the EPA created a succession of BEIR Committees (I - VII, thus far) that have "rubber stamped" LNT for over a half century, despite an ever-accumulating mass of countervailing evidence [18]. Such a succession of BEIR Committees may never have occurred without the unique story and impact of John Gofman and Arthur Tamplin.

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