



Letter to the Editor

How did Hermann Muller publish a paper absent any data in the journal *Science*? Ethical questions and implications of Muller's Nobel Prize

ARTICLE INFO

Keywords

Cancer risk assessment
Mutation
Linear dose response
Hermann J. Muller
Ionizing radiation

ABSTRACT

The present paper reports the discovery of an October 26, 1927 letter of Hermann J. Muller concerning the owner and editor of the journal *Science* that suggests an agreement that could have led to Muller's publication in *Science* – absent any data – which was contributory to both his professional reputation, and perhaps his being considered for and awarded a Nobel Prize.

1. Introduction

In 2018 [1] we raised the question of how Hermann J. Muller published a paper in the journal *Science* which claims to provide the first evidence of radiation-induced gene mutation. Of note is that this paper did not contain any data, but instead, only discussed data from experiments that were conducted. Furthermore, subsequent examination of Muller's Nobel Prize research notebook reveals that the *Science* paper reported only those findings based upon the two of three experiments, given that the work for the third experiment was not completed until approximately a month after aforementioned paper was published in *Science*. Furthermore, the data of the three experiments were published as a single paper in 1928 in a non-peer-reviewed conference proceedings [2]. That proceedings paper lacked a methods section, citations, and did not provide even a cursory discussion of the relevant published literature. According to Muller (letter from Muller to Edgar Altenburg on July 8, 1946) [3], the conference proceedings' paper was published verbatim as read at the meeting, strongly supporting the assumption that it was not subjected to peer review (given its rather irregular absence of methods, citations and/or review and discussion of relevant previously published work).

2. The Muller - Cattell connection

We query how it was therefore possible for Muller to publish his non-data supported manuscript in the prestigious journal *Science*. Why would J. Mckeen Cattell, the owner and senior editor of *Science* and the *American Naturalist* at that time publish the Muller "discussion" prior to completion of the experiments, and sans data of any kind?

It was well known that Muller's doctoral advisor, Thomas Hunt Morgan, was a close friend and colleague of Cattell's. The *Science* paper was published 12 years after Muller received his doctorate, and he had a strained relationship with Morgan at that time [4]. However, recent discovery of Muller's correspondences may offer insight to how Cattell and Muller might have arranged for publication of Muller's manuscript. Muller's paper was published in *Science* on July 22, 1927. He presented his data at a conference in Berlin during the week of September 11–17,

1927. A newly found letter from Muller to Milislav Demerec [5], written five weeks after the Berlin meeting, noted that Cattell had asked him (i. e., Muller) to identify papers of appropriate relevance and quality, and to then contact the authors and invite them to republish their paper(s) in Cattell's journal, *American Naturalist*. In this letter, Muller informed Demerec that "Dr. Cattell agreed to accept for publication there any paper which I thought suitable."

We ask, why the (by that time) world famous Muller would take the time to identify researchers' papers, read each carefully, and then write to those scientists on behalf of Cattell, encouraging them to submit their work to the *American Naturalist*? Should his requests be accepted, it would then involve the responsibility of Muller to provide (or at least direct) peer reviews; and should these papers require revision(s), then additional review by Muller would be needed for Cattell, pursuant to any decisions on whether the revised manuscripts should be accepted for publication.

Perhaps Muller's letter to Demerec offers an important view to Muller's relationship with Cattell during the time leading up to the publication of Muller's paper in *Science*. The letter suggests that Cattell sought some 'quid pro quo' from Muller as befitting Cattell's "gift" of the professional favor of publishing a paper in (his) journal *Science* despite the lack of data, and prior to completion of the contributory experiments. This decision all but assured status primacy for Muller in the critical area of gene mutation, and certainly could be regarded as contributory to his being considered for, and eventually, being awarded a Nobel Prize.

Even more explicit correspondence between Cattell and Muller might offer a more complete, and thus compelling, explanation of how Muller published a paper (lacking any data) published in *Science*. Nevertheless, Cattell evidently received a significant "pay back" from Muller given the reciprocal notoriety conferred to *Science* upon Muller's winning the Nobel Prize. For example, the paper of EC McDowell [6] was republished in the *American Naturalist* in exactly the same manner as it was published in the Proceeding of the 5th International Genetics Congress consistent with the arrangement of Muller and Cattell, presumably due to the intervention of Muller.

<https://doi.org/10.1016/j.cbi.2022.110204>

Received 16 August 2022; Received in revised form 14 September 2022; Accepted 26 September 2022

Available online 7 October 2022

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3. Remaining questions

It may well be that there is more to this story. Yet, Muller's correspondence to Demerec adds an important piece to the historical puzzle, and in this way, may provide both a more complete picture – as well as salient signposts for historians of science, and ethicists to follow along a path of further inquiry and research in this matter.

Taken together, we offer that Muller's correspondences give rise to the following questions:

1. When did Cattell and Muller reach their formal working arrangement?
2. How did Cattell get interested in the 5th International Genetics Congress presentations?
3. Why didn't Cattell ask another prominent scientist, such as Demerec, a very-well noted radiation geneticist - rather than Muller, to take on the Muller arrangement as doing so would have avoided any appearance - or evidence - of conflict of interest for either person? However, such an arrangement would have been potentially very time consuming and not a trivial request, and it is even surprising that Muller even agreed to do it.
4. Did Muller recognize the potential criticisms and ethical violations that would arise after publishing his paper in *Science*, given his work – and relationship - with Cattell?

Perhaps most unusual is that Muller did not publish his data-based paper in the *American Naturalist*, when it was clearly available to him, but instead chose to remain only in the non-peer reviewed Proceedings [2] - where very few would see and cite it. It could be speculated that this venue would limit the capacity of peers to review and criticize his findings. While Muller ([7]-letter to Carl G Hartman, April 11) would claim that his 1928 PNAS [8] paper indeed presented his major findings, it should be noted that this work is largely a review of the findings, and does not enable assessment of the original data. In sum, we are compelled to ask, whether Muller felt he had something to hide, and if so, what?

Funding

EJC acknowledges longtime support from the US Air Force (AFOSR FA9550-19-1-0413) and ExxonMobil Foundation (S1820000000256). The U.S. Government is authorized to reproduce and distribute for governmental purposes notwithstanding any copyright notation thereon. The views and conclusions contained herein are those of the author and should not be interpreted as necessarily representing policies or endorsement, either expressed or implied. Sponsors had no

involvement in study design, collection, analysis, interpretation, writing and decision to and where to submit for publication consideration.

Author contributions

EJC; conceptualization, original draft, editing. JG. Writing, review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

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

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