



Reflections in Mutation Research

Scientific feuds, polemics, and ad hominem arguments in basic and special-interest genetics[☆]

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ARTICLE INFO

Article history:

Accepted 19 January 2017

Available online 3 February 2017

Keywords:

Muller

Calabrese

Ad hominem arguments

Conflict of interest

Peer review

Low-dose radiation exposure

Journal policy to prevent ad hominem arguments

ABSTRACT

Scientific disputes are commonly presented and settled in journal publications. Most are resolved by a weighing of evidence and new findings. In some cases the arguments are personal and in the form of ad hominem attacks on the personality or integrity of an author of a journal article. Many famous scientists (e.g., Galileo, Newton, and Hooke) used ad hominem arguments in responding to their critics. William Bateson, W.F.R. Weldon, William Castle, and H.J. Muller used ad hominem arguments in their publications until the end of World War I, when editorial policy of journals changed. Motivating some of the attacks are philosophic differences (such as holistic or reductionist approaches to science), ideological differences (such as Marxist or Capitalist outlooks), politics (such as Cold War depictions by East and West on fallout from nuclear testing), or conflicts of interest (which can be professional or financial such as the debates over nontraditional and orthodox medicine or over tobacco smoking and health). Most of the time, the disputes are motivated by honest disagreements over the interpretation of the data. A recent surge (2009–2016) of ad hominem attacks by Edward Calabrese has appeared disparaging H. J. Muller, E. B. Lewis, other twentieth-century contributors to radiation genetics, and the National Academy of Sciences. They address the mutational effects of low-dose radiation exposure. Calabrese's attacks have led to responses by geneticists in the field of mutagenesis, by agencies criticized by Calabrese, and by students and colleagues of those who have been accused of deception by Calabrese. This article reviews some of the history of ad hominem arguments in science and the background to the attacks by Calabrese. I argue that Calabrese's characterization of Muller and his supporters is unjust, misleading, and hurtful. I also propose some methods for dealing with or preventing ad hominem attacks in professional journals.

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1. Introduction

Scientists learn that their fields change not only by the addition of new data, new technology, new experiments, and new ideas, but also by the necessary conflict of contending ideas that abound each generation. I first became aware of the extensive presence of controversy in the progress of science when, in 1965, I wrote the first draft of *The Gene: A Critical History* [1]. I was using the reprint library at Woods Hole, Massachusetts, at the Marine Biology Laboratory. As I took notes on my 5 × 8 cards and sorted these out for each chapter, I was struck by the intensity of debates among respected geneticists. From the mid nineteenth century to the

1960s that I was covering for a history of how the idea of the gene arose and developed, I found that disputes were far more likely than a simple discovery and acceptance of new knowledge. Some of the disputes were quickly settled by new rounds of experiments. Some lingered for decades. Until the end of World War I the disputes were often in print in professional journals. For the journal *Nature* the disputes were carried out in the original articles setting off the dispute and moved to the letters to the editor. When the dispute got repetitious (usually after about three exchanges of correspondence) the editors cut off the debate in print. My second surprise was that the name-calling and nastiness of the arguments disappeared not only in *Nature* but in the major professional journals where geneticists sent their manuscripts. I have asked librarians for help in tracking down how this policy arose and who had the authority to engage other editors in shifting the debates away from ad hominem attacks, but I still don't know how this happened.

[☆] This article is part of the Reflections in Mutation Research series. To suggest topics and authors for Reflections, readers should contact the series editor, G.R. Hoffmann (ghoffmann@holycross.edu).

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As I immersed myself in the history of science, I learned that scientific disputes go back to the earliest efforts to understand the universe. Platonic dialogues show Socrates' (470–399 BCE) great skill in “leading the witness” to contradictory consequences of ideas that sound good but are not well thought out. His success in making his opponents look foolish led to his downfall, with false accusations of impiety, corrupting the youth of Athens, and leading Athens to defeat by Sparta. Galileo (1564–1642) ridiculed his scientific opponents in his debates with them and in print with his first books introducing the Copernican solar system. This invited a torrent of criticisms of him and a run-in with the authority of the Church, which his critics claimed was challenged by his presentation of Copernicus' model as factual. Galileo's continued ridicule of these opposing arguments in print led to his trial and conviction resulting in his house arrest for the rest of his life.

Isaac Newton (1642–1727) and Robert Hooke (1635–1703) despised each other, each accusing the other of stealing ideas in their working out of the laws of optics and moving bodies. They were both members of the newly formed Royal Society. It led to Newton avoiding the Society while Hooke was alive. Hooke's personality was combative. He had many disputes with other scientists on priorities for discovery and the interpretation of their work. He served as the Society's experimentalist and tested each new instrument or finding submitted to the society. He also wrote and published the Society's first best-seller, *The Micrographia*, that established the cell (which he named) as a unit composing the cork tissue he examined under his microscope [2]. After Hooke died and Newton became President of the Royal Society, he had all of Hooke's portraits removed and they disappeared or were destroyed. Newton also accused Gottfried Leibniz (1646–1716) of stealing the idea of differential and integral calculus from him. He claimed it was not coincidental independent discovery but information Leibniz got from Newton's correspondence with other scientists known to Leibniz.

Charles Darwin (1809–1882) tried to avoid controversy by delaying publication of his theory of evolution by natural selection for some twenty years and by having his friends, especially botanist Joseph Hooker (1817–1911) and zoologist Thomas Huxley (1825–1895), respond to critics. But books and articles appeared criticizing him as a sloppy thinker or a thief who merely enlarged ideas of his predecessors. When Darwin's *The Origin of Species* appeared it was extensively reviewed. Adam Sedgwick (1785–1873), a geologist Darwin respected, after reading *The Origin of Species*, wrote to Darwin on November 24, 1859:

“If I did not think you a good tempered & truth loving man I should not tell you that . . . I have read your book with more pain than pleasure. Parts of it I admired greatly; parts I laughed at till my sides were almost sore; other parts I read with absolute sorrow; because I think them utterly false & grievously mischievous— You have *deserted*— after a start in that tram-road of all solid physical truth— the true method of induction” [3]. Richard Owen (1804–1892) and Samuel Butler (1835–1902) were two of his more prominent critics. Owen was a respected naturalist and Butler, while known for his fiction and numerous writings, opposed the reductionism in Darwin's approach and favored a holistic view of life and the universe verging on pantheism. After Darwin's death the ad hominem arguments became more extensive and the attacks were inspired by religious belief, as evolution became anathema to the newly developing fundamentalist movements in Great Britain and especially in the United States.

2. Disputes and personal attacks in the history of genetics

We owe to William Bateson (1861–1926) the terms genetics, genotype, phenotype, heterozygous, homozygous, and allele. He studied embryology at Cambridge and went in 1883 to Johns Hopkins

University to work with William Keith Brooks (1848–1908). Brooks convinced Bateson that the future field to study was heredity. Bateson returned and devoted himself to studying variation and published in 1894 a book on what was known about it [4]. He introduced the idea of homeotic mutations and meristic mutations. The homeotic mutations misplaced the location of organs. The meristic mutations duplicated parts like extra digits on limbs or extra vertebrae. Bateson argued that these were discontinuous events and could lead to new species characteristics. He fought bitterly with the prevailing late nineteenth century Darwinian school led by Francis Galton (1822–1911), Karl Pearson (1857–1934), and W. F. R. Weldon (1860–1906). That group founded a field of biometrics (Gaussian curves and their departures) and had their own journal, *Biometrika*, that served as a vehicle for articles and editorial comment denouncing Bateson. When *Biometrika* did not publish Bateson's articles, he wrote and subsidized the publication of his own book in 1902, *Mendel's Principles of Heredity: a Defense* [5]. It was the discontinuity of hereditary traits studied by Mendel that appealed to Bateson, and we owe to Bateson the many years of battle he devoted to promoting Mendelism against a far greater and accepted statistical approach to studying speciation and heredity used by Weldon and Pearson. It took another 20 years before R. A. Fisher combined Mendelism with the new field of population genetics to create a mathematical approach to evolution [6]. When Weldon collapsed and died of a heart attack, his widow blamed Bateson for causing his death. Bateson had written a scathing response to Weldon's criticism and faint praise of Mendelism and Bateson claimed, “I am disposed to think that unaided he is – to borrow Horace Walpole's phrase – about as likely to light a fire with a wet dish clout (sic) as to kindle interest in Mendel's discoveries by his tempered appreciation” [7].

Bateson's enthusiasm for Mendelism caught fire in the US. One of the first converts to Mendelism was William Castle (1867–1962). He began studying contrasting traits in mammals. He showed that albinism was a recessive trait. He worked on coat color in mice, guinea pigs, and rabbits. He believed in “genetic contamination” when traits were heterozygous and that this accounted for the variable expression of many traits that otherwise behaved as dominant or recessive characteristics. Opposing Castle's views were the members of T.H. Morgan's (1866–1945) Fly Lab, especially H.J. Muller (1890–1967). In 1914 Muller published a paper arguing that the variability of Mendelian traits was not due to genetic contamination but to “residual inheritance” or what could also be called modifier genes [8]. Castle and J.C. Phillips had presented their evidence that piebald rats owed their variation to genetic contamination [9]. Muller presented his evidence that the extracted recessive was not contaminated and commented “It is difficult to believe that this suggestion of Castle and Phillips was not made in a spirit of mysticism, when we consider also their suggestion that the gene may undergo contamination” [10]. Castle was equally dismissive of Muller in his reply claiming that Muller had to invoke modifier genes (residual inheritance) or quantitative factors for which he offered no evidence. “What a slender basis and what an absurd one from which to derive the ‘fundamental principles’ that Mendelian factors are constant! Do biologists take themselves seriously when they reason thus? Certainly no one else will long take them seriously” [11]. Castle took five years before he conceded that residual inheritance did account for the variation he studied in both spotted rabbits and hooded rats [12]. For Muller the slap in the face given to Castle was costly. Muller told me that Morgan had advised him not to publish the attack on Castle and this led to a rupture in their relation. It was also the beginning of Muller having a reputation as having a difficult personality. While most geneticists do not engage in or approve of polemical or ad hominem responses to critics, there are situations where such responses are applauded. An example of this was at the meetings of

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