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Whatever happened to history of science? How scholarship became politicized story-telling

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WHATEVER HAPPENED TO
HISTORY OF SCIENCE?
HOW SCHOLARSHIP
BECAME POLITICIZED
STORY-TELLING

BY JOHN STADDON

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CENTER FOR THE
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Whatever Happened to History of Science?

How scholarship became politicized story-telling

John Staddon

*It is well-known that history of science is much hampered by the double obstacle that most scientists have no sense of history, while most historians are ignorant not only of the facts, but of the very spirit of science*¹.

History of science is a relatively new field. The original emphasis, in the works of George Sarton, Gerald Holton, and Thomas Kuhn, for example, was on natural science — physics chemistry, biology and medicine. But in recent decades attention has increasingly shifted to the social sciences: economics and psychology. Some sociologists sought to become historians of science; some anthropologists have sought to treat scientists as they used to treat the Yanomamö and the Tikopia. History of science has become more personal: biographies and biographical sketches abound, and social influences are promoted over the factual force of scientific findings. Political issues such as feminism play an increasing role. A colorful French *nouveau philosophe*, Bruno Latour, has become a leading figure in the new sociological field of *science studies* in which social (political incentives) and personal (expertise, gender) factors play an increasing role. Science is on its way to becoming a social construct.

In this piece I look at just one small corner of science history touching on biology, psychology and political science. I tend to agree with Paul Feyerabend's *Against Method* to the extent that if there is no algorithmic *scientific method*, then science is best understood through examples². The same is surely true of history of science: there is no template. In this article I discuss five books to illustrate a three trends: a retreat from political neutrality; downplaying the *truth* (however assessed) of scientific claims as a reason for their acceptance; and the desire for a good story, preferably a capitalist plot.

The First Cause

Historians have always been subjects of criticism: bias, omissions, inaccuracies, fabrications, insinuations and just a bad attitude have been charges aimed at even the most eminent. Edward

¹ Cohen R.S., Stachel J.J. (1979) Science in History (Review of J. D. Bernal's *Science in History* [Watts & Co., London, 1954. xxiv + 967 pp.]) [1956f]. In: Cohen R.S., Stachel J.J. (eds) *Selected Papers of Léon Rosenfeld. Boston Studies in the Philosophy of Science*, vol 21. Springer, Dordrecht

² Staddon, John (2017) *Scientific Method: How science works, fails to work or pretends to work*. Taylor and Francis.

Gibbon, author of the incomparable *History of the Decline and Fall of the Roman Empire* (1782) was not immune. He was accused of disrespecting Christianity, for example in this passage:

Our curiosity is naturally prompted to inquire by what means the Christian faith obtained so remarkable a victory over the established religions of the earth. To this inquiry, *an obvious but satisfactory answer may be returned; that it was owing to the convincing evidence of the doctrine itself*, and to the ruling providence of its great Author. But as truth and reason seldom find so favorable a reception in the world, and as the wisdom of Providence frequently condescends to use the passions of the human heart, and the general circumstances of mankind, as instruments to execute its purpose, we may still be permitted, though with *becoming submission*, to ask, not indeed what were the first, but what were the secondary causes of the rapid growth of the Christian church...

Christian apologist William Paley is the author of the famous proof of design: “if you find a [watch on the ground](#), you infer a designer” (hence, “new atheist” Richard Dawkins’ bestseller refutation “The Blind Watchmaker”). The editor of an 1845 [edition](#) of Gibbon’s six volume masterpiece is not fooled by Gibbon’s disclaimers and quotes Paley:

Paley, with his intuitive sagacity, saw through the difficulty of answering Gibbon by the ordinary arts of controversy; his emphatic sentence “*Who can refute a sneer?*” contains as much truth as point...[emphases added]

A modern reader will surely see Gibbon’s acquiescence to the truth of Christianity as graceful, though possibly insincere. We are less likely than editor Rev. Milman to take offense at lèse-majesté directed at the Christian faith. But Gibbon had a point about *proof*. Philosopher David Hume perhaps convinced him that the facts of science are provable but the facts of religion for the most part are not³. They are matters of faith, not science. There is no *scientific* evidence for the truth of Christianity. So Gibbon was perfectly justified in looking for causes other than “convincing evidence of the doctrine” for the rise of Christianity and the fall of Rome.

History of science is different. Science’s whole existence depends ultimately upon *convincing evidence of the doctrine itself*. We believe in the facts of science because they have passed our tests. These proofs surely are, in Gibbon’s words, the *first causes* for the rapid growth of science. It follows that a pre-requisite for any historian of science is *to understand the science*.

³ Staddon, J. E. R. (2013) Faith, Fact, and Behaviorism. *Behavior Analyst*, 36(2), 229-238. See also [Science and morals](#).

Understanding science is essential for an accurate account of its history. In this respect, some contemporary historians of science are not doing well.

Darwin and (Political) History of Science

But I begin with Paley's "sneer". Sneers have been directed at Charles Darwin in influential modern histories. For example, there is some controversy about Darwin's reaction to Alfred Russel Wallace's scooping his discovery of evolution by natural selection. In 1858, Wallace, a young naturalist who had corresponded previously with Darwin, sent him a short paper. He asked Darwin to send the piece to Darwin's friend and mentor, geologist Charles Lyell. Wallace's paper reached the same conclusion Darwin had been buttressing with tireless research for the previous twenty years. Darwin was devastated. Some years earlier he had shown an essay describing natural selection to several colleagues, so there is no doubt that he had the idea first. He had also amassed a pile of data and arguments to support it and refute many possible objections.

Darwin had delayed publication for several reasons. Possibly he thought the anti-religious implications of evolution too inflammatory to publish without overwhelming support — Robert Bridges in his popular (but anonymous) *Vestiges of the Natural History of Creation* (1844) had achieved the kind of notoriety that was anathema to Darwin. More likely he was simply conscientious; he wanted to get it right before going public. In the end his friends Lyell and Joseph Hooker arranged for a joint paper to be presented that same year to the Linnaean society, although neither author was present — Darwin, sickly at home in Downe, Wallace still in the South Seas.

Wallace did not mention publication in his letter, but this is what Darwin wrote to Lyell: He would "of course, at once write and offer to send [Wallace's paper] to any journal". Yet "all my originality, whatever it may amount to, will be smashed....I should be *extremely* glad now to publish a sketch of my general views in about dozen pages or so. But cannot persuade myself that I can do so honourably..."⁴ These are not the words of an arrogant or dishonest man. In after years, Wallace was perfectly happy at the way Darwin had treated him and named his book on the topic *Darwinism*.

Yet, here is the sneer. We learn from Desmond and Moore's 808 page *Darwin: The life of a tormented evolutionist* (1991) that "Irony and ambiguity shrouded Darwin as no other eminent Victorian" (an allusion, of course, to Bloomsbury groupie Lytton Strachey's brilliantly

⁴ Quoted in Janet Browne's excellent *Darwin: The power of place*. Alfred Knopf, 2002.

destructive caricatures of Victorian heroes such as Florence Nightingale and General Gordon). Desmond and Moore go on:

[Darwin] hunted with the clergy and ran with radical hounds; he was a paternalist full of *noblesse oblige* [a bad thing, apparently], sensitive, mollycoddled [despite heroic actions during the *Beagle* voyage that got a channel named after him], cut off from wage-labour and competition [could he have done so much science otherwise?], who unleashed a bloody struggle for existence [blaming the messenger?]; a hard-core scientist addicted to quackery, who strapped ‘electric chains’ to his stomach and settled for weeks at fashionable hydropathic spas [he tried remedies for a medical condition that had no cure at that time⁵]...

Polite inquiries of a scientific opponent, the American Alexander Agassiz, are labeled “pickpocketing”. Describing Darwin’s interaction with pigeon-fanciers, they write: “His kindly paternalism gave their backyard hobby a certain cachet. But to the end he remained impeturbably a gent among working fanciers.” Moreover, Darwin *dissimulated*. To his cousin Fox on the mutability of species Darwin wrote⁴ “I mean with my utmost power to give all arguments & facts on both sides.” D & M know better: “Balance and doubt were a public mask. Despite appearances, he knew exactly what he was doing”. These eminent historians of science discern dishonorable motives even in Darwin’s most transparently decent actions. It seems to be his upper-middle-class origins they particularly dislike, irrelevant though these should be to his scientific achievements.

Wallace and Darwin viewed natural selection a bit differently. According to D & M Darwin provided what his supporter T. H. Huxley wanted “a new competitive, *capitalist* sanction in place of Anglican Oxbridge paternalism.” A “Malthusian, capitalist, competitive mechanism ... unlike any rival evolutionary theory.” [emphasis added] Darwinian evolution was a metaphor for industrial capitalism. “Nature was a self-improving ‘workshop’, evolution the dynamic economy of life. The creation of wealth and the production of species obeyed similar laws.” Darwin’s views are hardly surprising, they suggest, as “[He] was a heavy investor in industry” not to mention family connections to the industrial Wedgwoods. Never mind; true or false, Darwinian natural selection is just capitalism by another name.

Wallace, on the other hand, was self-employed, more interested in cooperation than competition and a “self-taught socialist.” “Wallace’s naturally selected group morality was leading society in

⁵ Possibly lactose intolerance: Campbell, A. K. & Matthews, S. B. (2005) Darwin’s illness revealed. [Postgraduate Medical Journal](#). (Thanks to John Malone for the reference.)

a very un-Darwinian direction [not that Darwin ever speculated about society].” Wallace also believed that humanity, by virtue of language and intellect, was special. Wallace believed in the inevitability of evolutionary progress. Darwin did not; contemporary evolutionary biology also does not.

Needless to say, D & M do not think that Darwin treated Wallace well. He should have seen from their earliest correspondence that Wallace was on to natural selection: “He did not really catch Wallace’s drift”. Darwin should just have bowed out, apparently. This gross misapprehension has made it into popular writing, even of usually accurate and always entertaining Tom Wolfe⁶.

The authors seem to trace Darwin’s ideas not to his own genius and the mountains of data he gathered, but to his class, his upbringing, and his position in society. How much more likely is it that their own biases arise from a like cause! Quite possibly the authors’ sympathy for Wallace follows from their own political preferences. Previous coverage of Darwin “served a purpose a century ago in securing Darwin’s immortality...But today’s needs are different...We want to understand how his theories and strategies were embedded in a reforming Whig society.” Desmond and Moore are determined to tie Darwin’s behavior to social issues rather than to his lifelong curiosity and love of nature, puzzling over rocks and atolls, barnacles and beetles. In a subsequent book, *Darwin’s Sacred Cause*, they link his theory to his abhorrence of slavery — an intriguing connection since at least one other, admittedly odd, book makes Darwin a racist⁷! The man’s intellectual independence, his ability to ponder problems invisible to many others, is lost in D & M’s account. Society, social movements, social agitations, *social* is the thing. Yet, all admit that Darwin was for most of his life a recluse. He communicated largely by letter and largely about biology. He wrote almost nothing about society. He was neither a sociologist nor a political scientist; he was above all a biologist. His inspiration was from nature, not society. This is a conclusion that Desmond and Moore seem unwilling even to entertain, much less accept.

Desmond and Moore’s book is long and copiously referenced. They (unlike some others I will mention) obviously understand the science. But they give little weight to it. Instead their view of the man and his achievements is suffused with a sneering politically tinged tone that disparages Darwin and devalues his scientific contribution. Darwin was just a predictable product of his

⁶ Tom Wolfe (2016) *The Kingdom of Speech*. (Little, Brown), see also An [Open Letter to Tom Wolfe](#).

⁷ Leon Zitzer (2017) *A short but full book on Darwin’s racism* (Universe), which is a summary of a much longer book accusing Darwin of racism.

time and place. No room for talent or even luck. No room for the verifiable truth of the man's ideas. The book is not a history of science so much as science as politics by other means⁸.

First, *understand*

Nancy MacLean is the William H. Chafe Professor of History and Public Policy at Duke



University. She has written a book whose central figure is a soft-spoken Southern economist, winner of the 1986 Nobel prize in Economics, James McGill Buchanan. One critic of the book is Michael Munger, a Duke colleague and expert in *public choice*, Buchanan's field. Munger calls it a "remarkable book"⁹, albeit "speculative historical fiction." A strong charge, but amply documented in Munger's long review¹⁰.

*Democracy in Chains*¹¹ is about politics, about threats to American democracy. It is written in a lively, who-dunnit fashion as a sort of spy story. James Buchanan is the evil conspirator at the heart of the plot. Buchanan died in 2013, not long before MacLean took up her work. Conveniently, as the timing meant that MacLean could freely

attribute motives to him without danger of rebuttal; and she found a trove of Buchanan's papers, then un-curved and un-censored, in a house at George Mason University.

From this "unlisted archive" she has contrived a sinister plot. Buchanan, it turns out, aided and abetted by "shadowy billionaires", most notably the Koch brothers, wanted to destroy democracy. She can now reveal "the utterly chilling story of the ideological origins of the single

⁸ A view *recommended* as a proper view of science by philosophe Bruno Latour. Latour received the Bernal Prize in 1992 (more on Bernal in a moment).

⁹ Possibly an echo of Winston Churchill's comment when he first saw his portrait, painted by Graham Sutherland and commissioned by the British House of Commons to celebrate his 80th birthday: "A remarkable example of modern art!" He hated the picture and it was reportedly burned by his wife after his death.

¹⁰ Michael C. Munger (2017) [On the origins and goals of public choice: Constitutional conspiracy?](#) *The Independent Review*, 22(3), a [definitive 21-page critique](#) of the science and history in the book

¹¹ Nancy MacLean *Democracy in Chains: The deep history of the radical right's stealth plan for America*. Penguin 2017.

most powerful and least understood threat to democracy today: the attempt by the billionaire-backed radical right to undo democratic governance”:

Buchanan ... argued that representative government had shown that it would destroy capitalism by fleecing the propertied class—unless constitutional reform ensured economic liberty, no matter what most voters wanted.

This single sentence captures MacLean’s innocence of political economy. She doesn’t understand the science.

“Representative government” — democracy — means rule by the people. But just what people? It has been known since at least the 18th century that if the franchise is broad, including many poor people as well as a few rich ones, then it has a potentially fatal flaw:

A democracy is always temporary in nature.... A democracy will continue to exist up until the time that voters discover that they can vote themselves generous gifts from the public treasury... From that moment on, the majority always votes for the candidates who promise the most benefits from the public treasury, with the result that every democracy will finally collapse due to loose fiscal policy, which is always followed by a dictatorship.

The source of this quote is uncertain¹² but its truth is undeniable. A broad democracy without checks and balances to protect wealthy minorities from expropriation by the state is an unstable arrangement.

American and British democracies two or three hundred years ago were not, of course “broad”. For much of the time, only those with skin in the game — men, owners of land or payers of tax — were eligible to vote. (The system was then subject not to the Tytler dilemma, but its opposite: exploitation of the poor by the rich.) Now, however, the US has a very broad democracy: there are no sex or property qualifications and voting age (18) is below the legal drinking age (21) — and a few politicians want to lower it even further. Without checks and balances to limit total dominance by the majority, the system would indeed be unstable.

There are of course many such checks in modern America, in the Constitution and after. MacLean describes a few and deplores all of them. But that some such restraints are necessary is

¹² Scottish legal figure Alexander Tytler (1747-1813) is the most popular source. James Madison saw a similar problem.

unarguable. Even Adam Smith saw the danger of monopolies. In MacLean's majoritarian version of democracy, the majority *is* a monopoly, with absolute power. She is surely aware of Lord Acton's dictum about absolute power. In fact, as Munger points out, she doesn't even recognize this elementary problem. Public policy professor Maclean seems not to understand one of the most basic facts about democracy.

It is always possible, given a large enough collection of facts, to select from them just those that support a desired story. This is what Desmond and Moore did in their *Darwin*. They ignored the data, the fossils, the specimens, Darwin's many curious speculations about what he was finding. What was left was the social milieu in which Darwin found himself. *Aha! that* must be what drove him to his atheistical, capitalist, solution they conclude. Well, no: what they left out is much more important than what they retained.

Nancy MacLean's book is in this same tradition. Buchanan in his many writings and talks always emphasized at the outset that *ideas* were supremely important to him. His overriding motivation was to understand/explain (his usage) how individuals make choices and how different social arrangements affect their choices: "we shall construct, in an admittedly preliminary and perhaps naïve fashion, a theory of collective choice."¹³ Yes, he was a libertarian, or a *classical liberal*, as he preferred to call himself. Yes, he would no doubt have liked society to be organized differently. But no, these desires were not the main source of his work. He admitted in one interview that a socialist society would be perfectly acceptable to him if it was the result of genuine consensus. Yet Buchanan's supposed political preferences absolutely dominate MacLean's account. Buchanan is transformed from a thoughtful intellectual to a sinister hatcher of plots.

Michael Munger's detailed critique points out the many places where MacLean has found conspiracy when Buchanan was simply advancing an intellectual movement. For comparison, he quotes MacLean's Marxist colleague Fredric Jameson¹⁴, who has for many years was quite open about his aim to create a "Marxist intelligentsia". Buchanan's half-serious aim was to create "an effective counterintelligentsia". The briefest of glances at the political preferences of social-science faculty in American universities shows that Jameson has come a lot closer to achieving

¹³ In Chapter 1, The conceptual framework, of Buchanan and Gordon Tullock's landmark work *The Calculus of Consent*, Liberty Fund (1962/1999).

¹⁴ Eminent postmodern philosopher in Duke's Literature program; also 1997 winner of Dennis Dutton's famed *International [Bad Writing Contest](#)*.

his goal than Buchanan. So much for the “far-flung...intricately connected institutions funded by the Koch brothers and their now large network of fellow wealthy donors.”¹⁵

In his Nobel lecture, Buchanan points out the eighteenth-century discovery of Adam Smith’s “invisible hand”, the way that individual, self-interested choices (can) lead to collective good. Public Choice Theory, a product of work by Buchanan and his long-time collaborator Gordon Tullock, attends to “the processes through which individual choices are exercised.” Basic economic principles can in this way be extended to a variety of institutional settings. This ultimately voluntary basis for political agreement runs counter to the postmodern/Marxist emphasis on politics as power. Instead of being either oppressors or oppressed, passive or active, objects of power, Public Choice sees individuals in a democracy as voluntarily obeying rules arrived at through an agreed process. It is an attempt to extend Smith’s insight to collective action. Since claims about unfair power relations are at the heart of progressive politics, MacLean is perhaps unwilling to consider an approach that tries to understand democracy as the outcome of voluntary action by free individuals.

MacLean’s book has received considerable acclaim. The first few pages are devoted to no less than twenty-seven enthusiastic endorsements, ranging from Bloomberg, the *New York Times Book Review*, the *Nation*, the UK *Independent*, and Oprah to something called AlterNet. Why this enthusiasm from so many left-leaning sources? The answer is simple. First, the book is readable, well-written and with a great plot. But more important it ticks every box of the progressive checklist, taking approved positions on every one: capitalism, white men, selected billionaires, the ‘radical’ right, vouchers and public schools, Augusto Pinochet, Scott Walker, the Mont Pelerin Society, anthropogenic global warming, Southern racism and racial segregation. “Dog whistles” and “coded meanings” abound, words mean what the author says they mean: individual liberty = capitalism, special interests = black people, criticism of ‘big government’ = racial anxiety; the book is full of translations of innocent words into incriminating ones. If you are of the appropriate political persuasion, what’s not to like?

In fact, the book is footnoted docudrama of the “Based on Real Events” type, masquerading as disinterested research. It is embarrassing that such a product is apparently valued as a scholarly contribution by Duke’s History and Public Policy Departments.

¹⁵ For more on the Kochs’ non-influence on Buchanan: in addition to the Munger review, see Brian Doherty What Nancy MacLean Gets Wrong About James Buchanan. (2017) *Reason*, 7/20/2017.

Ideology and History of Science

A political bias need not damage historical writing if it is presented honestly. John Desmond “sage” Bernal (1901-1971) was a polymath Irish X-ray crystallographer and a committed communist. He was a prolific writer as well as an influential scientist, who played a peripheral role in the Watson-Crick discovery of DNA as a senior colleague and supporter of Rosalind Franklin¹⁶ at Birkbeck College in London. Bernal wrote a 1039-page four-volume book *Science in History*¹⁷ which covers the history of science, very broadly defined, in a rather back-and-forth fashion (topics recur, widely separated) from the Stone Age to the twentieth century. The book is generally well-regarded, even by the critics quoted at the beginning of this article. Nevertheless, it has been in the vanguard of a distressing trend.

Bernal certainly understood science and saw its value and its effect on society. But he saw also, perhaps too vividly, the reciprocal effect of society on science. In the Preface he writes:

In the last 30 years, largely owing to the impact of Marxist thought, the idea is grown that not only the means used by natural scientists in their researches but also the *very guiding ideas of their theoretical approach* are conditioned by the events and pressures of society [emphasis added].

Bernal emphasized the science → society link in the early history, but tended to favor the reverse for more recent times. The link between society and scientific ideas is, as we have seen, the *leitmotif* of the Desmond and Moore book. Desmond and Moore clearly write in the Bernal tradition, but without declaring their Marxist leanings.

Proceeding from what he thought of as the axioms, undoubted truths, of Marx and Engels, Bernal goes on to pronounce on various aspects of science and society. For example, “In the capitalist world the major feature of the 20th century has been the rapid growth to complete dominance of large combines, trusts or cartels, partly commercial, partly industrial.” Yes, unchecked capitalism produces monopolies. We see it now not so much in heavy industry as in big tech — Facebook, Twitter, Amazon and Alphabet — “natural” monopolies produced by huge positive feedback to scale, advantages not of production but in augmented demand. The most popular service will be most attractive to new users. Monopolies have been restrained in the US since the Sherman

¹⁶ Franklin’s [X-ray data](#) were ‘stolen’ by James Watson and formed part of the basis for his discovery, with Francis Crick, of the helical structure of DNA, all described by Watson in his best-seller *The Double Helix* (1968).

¹⁷ John Desmond Bernal *Science in History*, 3rd edition, Watts & Co., London, 1965. See also K. C. Holmes (2006) The life of a sage (review), [Nature](#), 440, 149-150.

Anti-Trust act of 1890, but the old act has proved inadequate to deal with 21st-century technology.

Of course communism doesn't give rise to monopolies; it doesn't need to because it is itself a monopoly. The state runs everything, which Bernal applauds. He was apparently unaware of Friedrich Hayek's influential 1944 *Road to Serfdom* written while Hayek was working at the London School of Economics, just a couple of miles from Birkbeck where Bernal was a professor. Hayek convincingly pointed out the difficult and perhaps insoluble information problems confronted by central state planning.

Marxism is intellectually imperialist: it covers every human activity. Bernal's belief in Marxism led him to consider social science a real science and to include in it everything from history to law. As for experimentation, which is the essence of science, the proto-communist societies of Soviet Russia, China and Eastern Europe seemed to him just as good as the laboratory studies of Michael Faraday or Claude Bernard. This extraordinary delusion, a product of the devotion to Marxism and the Soviet 'experiment', led him to support the destructive pseudo-science of Trofim Lysenko. Lysenko's [failed](#), Stalin-backed, agricultural policy led to the death of millions. Dissenting scientists lost their jobs or were imprisoned or even in some cases executed by the Stalinist regime. Bernal remained a fan anyway.

Nevertheless. *Science in History* is a fascinating read, full of interesting facts and speculations. Bernal understands Darwin even as he deplores one kind of Darwinism:

The simple tracing of evolutionary relationships between organisms and the building of elaborate family trees distracted naturalists from the study of the actual lives of the inner workings of animals and plants...For this no one could blame Darwin himself, who was, as his detailed researches on such varied topics as earthworms, carnivorous plants, and the expression of the emotions show, one of the pioneers of experimental biology.

Bernal blames eugenics not on Darwin but on the brilliant Francis Galton: "It was with the highest of intentions that Francis Galton, Darwin's cousin, set about studying the heredity of men of exceptional ability in Britain." Scientists share some blame for the horrors that followed, says Bernal "[scientists'] fear of entangling themselves in politics meant that they left the social application of their own ideas to other people, and made no effective protest against the perversion of the products of their own researches." Thus, James-Bond-like, scientists are handed a license if not to kill, at least to *politic*. Others might see Lysenko as a textbook example of the dangers of politicized science, but his devotion to Soviet Marxism made this

impossible for Bernal. Ideological commitment it is not a basis for truthful history — of science or anything else.

History of Science as Journalism

Naomi Oreskes and Erik Conway's *Merchants of Doubt*¹⁸, is both a history of research on a number of hot-button topics — anthropogenic global warming (AGW), the ozone hole, acid rain, passive tobacco smoke — and an attack on dissenters such as global warming “deniers.” It is a “riveting piece of investigative reporting” says a *Guardian* [reviewer](#), who goes on to say “The far right in America, in its quest to ensure the perpetuation of the free market, is now hell-bent on destroying the cause of environmentalism.”

Fair enough. The book is journalism: to be judged on how attention-grabbing it is, with accuracy and comprehensiveness as subordinate virtues. Like much journalism it embraces the genetic fallacy not as a fallacy but as a useful rhetorical tool: Never mind what he says, who is he and why does he say it? Who's paying? It assumes, as many journalists do, that peer review is a guarantee of truth, rather than a weak barrier against obvious mistakes — and sometimes a suppressor of dissent. The book's coverage is partial: the most prominent AGW “deniers” are elderly senior scientists. Not included are younger, more active scientists¹⁹ less likely to be dismissed as ‘past it’.

As I have pointed out elsewhere²⁰ the book's case against passive smoking is flawed: an important study is missed, some studies are misrepresented and the book seems oblivious to the scientific difficulties involved in studying potentially dangerous effects that are likely to affect only a small fraction of the population and only after long delay. Nevertheless, it is in many ways a brilliant and persuasive book.

The case should end there, but it does not, because Naomi Oreskes is not a journalist but an academic historian of science. Is scholarly history of science now become nothing more than political journalism— with footnotes? Probably, as Oreskes discussing her most recent book²¹ happily promotes shared values as a way to get a scientific message [across](#): “[S]cientists need to

¹⁸ Naomi Oreskes, Erik M. Conway *Merchants of Doubt* Bloomsbury, 2010; there is also a [movie](#).

¹⁹ Included: Frederick Seitz, Fred Singer, Richard Lindzen, and a relatively youthful Bjorn Lomborg; omitted: Will Happer, Nicola Scafetta, Willie Soon (except as a junior author of a single cited paper).

²⁰ Staddon, J. (2020) Facts vs. passion: The debate over science-based regulation. *Academic Questions*, 33, 101-110.

²¹ Naomi Oreskes *Why Trust Science?* Princeton University Press, 2019.

talk about the values that motivate them and shape the science they do...”²² Bernal would agree. I am less sure that he would agree with the book’s conclusion that “Many scholars in the history and philosophy of science and science studies have, however, recently converged on a new view ...of scientific knowledge as fundamentally consensual.” Taking a lead from feminist philosophy, Oreskes comes perilously close to the view that if the experts agree, [we must accept](#) it as truth. Those that disagree can be dismissed as special interests or “cranks”, a position somewhat at odds with her advocacy of “diversity”. She is aware of the contradiction and offers this resolution: “Respecting professional diversity and lay expertise is also a different matter from heeding “dissent” from people with no credible claim to expertise...” In any case, the diversity Oreskes wants is not viewpoint diversity but demographic diversity. But, no problem, one implies the other: “demographic diversity is a proxy for perspectival diversity...” Well, it isn’t: not infrequently members of a group that is diverse in Oreskes’ sense will be [less free](#) with their in opinions than members of a more homogeneous group, out of fear of giving offense. And what Solomon will be the judge of whether a dissenter be a ‘crank’ or not?

Behaviorism

Like Oreskes, Nancy MacLean writes as an imaginative journalist not an historian. But these two are far from alone. A final example is a colleague of Oreskes’, Rebecca Lemov, Professor of History of Science at Harvard. She has written a book about behavioral control: *World as Laboratory*.²³ WAL conjures up a sort of plot, albeit not focused on a single individual, and less entertaining than MacLean’s ‘exposé’ of James Buchanan. The plotters are *behaviorists*, a group into which Lemov bundles every kind of psychology, from rats in mazes to Timothy Leary, from intelligence tests to the SAT, not to mention brainwashing and a vast anthropological database at Yale University. Sigmund Freud also gets a spot, as one inspiration for the Institute of Human Relations at Yale in the 1930s.

A sort of behaviorist myself²⁴, I found Lemov’s version of behaviorism unrecognizable. She includes behavioral/human/social engineering as parts of behaviorism which, historically, they were not. She also has almost nothing to say about B. F. Skinner, citing only his first book, *Behavior of Organisms* (1938). It was Skinner who followed most closely the prescriptions of John Broadus Watson, the originator of behaviorism, not Yale’s Clark Hull, who was much more interested in simple mathematical models of rat learning than in the control of human behavior

²² Well, now they do. The neutrality Rubicon was officially crossed when the respected journal *Scientific American* [officially endorsed](#) Democrat presidential candidate Joe Biden in 2020; *Science* and *Nature* may well follow.

²³ Rebecca Lemov *World as Laboratory: Mice, mazes and men*. Hill and Wang, e-book 2011; there is also a [movie](#).

²⁴ Staddon, John (2014) *The New Behaviorism* (2nd edition) Philadelphia, PA: Psychology Press.

writ large. The animal-laboratory wing of behaviorism had no interest at all in the “conviction...that the insights of Freud could be merged with the science of behaviorism.” Skinner’s radical behaviorism (wrongly labeled ‘operant conditioning’, which is a process not a philosophy) was indeed concerned with control. Skinner himself wrote about his ideas for society in several books and articles, most notably *Beyond Freedom and Dignity* (1971) and his 1948 novel *Walden II*. On the other hand, Lemov links her version of behaviorism to a number of bad practices such as brainwashing (encouraged by the US government and abetted by various foundations) even though the theoretically oriented Hullians were little interested in immediate human application. I’m aware of no such sinister connections to Skinner or even to Hull.

Lemov sometimes also misses the point. The large anthropological database, named the Human Relations Area Files in 1948, at Yale, was to be used for “statistical and correlational methods of scientific enquiry.” Lemov admits that the desire for a kind of natural history of cultures dates from the nineteenth century and with good reason. Just as the “botanizing” of birdwatchers and beetle collectors provided the database that allowed Linnaeus to classify and Darwin to understand the evolution of animals, so a database of cultures is a reasonable beginning for the science of man. Nevertheless, Lemov finds this endeavor to be hubristic if not sinister, because “[the data were] easily transformed by the impetus of World War II into an intelligence source and collator, an administrative device for tracking displaced or interned people, an aid to military occupation, and eventually a cold war strategy tool to use in locations at home and far from home.” Knives can be used for killing as well as carving; this kind of flexibility seems to be a problem for Lemov.

The organizing theme for WAL is the *control* of human behavior, although Lemov puts it very un-behavioristically as being able to “quantify and control the internal arena of the personal self”, whatever that is. She doesn’t like the idea of scientifically controlling behavior, even though “the running of a modern society would [then] require less brute external force....putting this idea into practice would make it possible to regulate human beings in tune with the needs, demands, desires, and models of the social order...” The problem seems to be that the method “leav[es] no room for what sentimentalists would call the soul...” What she is really objecting to is “the general idea of a science of human behavior [that] had bewitched people for a long time.”

In other words, what we have here is a historian of science who rejects the very idea of a science of human behavior. Lemov seems to object to three things:

1. that behaviorists are cruel to animals,
2. that they want to control behavior,
3. that they view organisms as machines.

How cruel are behaviorists? In fact most Skinnerian behaviorists do single-subject experiments that are not invasive. The experiments are rarely painful, the animals are hungry, but no more so than they would be in the wild, and they re-use the same animal repeatedly. These experiments are much less cruel, to many fewer animals, than the majority of animal studies in biomedicine. Medical pioneers like Vesalius and Pasteur were a great deal tougher on animals than any behaviorist.

To point two, Lemov traces the idea of control to John Broadus Watson (1878-1958), who famously wrote: “Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior.” Watson, who early on did some ethological research with seabirds, went on to claim that “The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brute²⁵”, hence all those animal experiments of which Lemov disapproves.

Science can be ultimately justified by the control over nature: “knowledge is power” and so forth. To this charge the behaviorists, like all scientists, must plead guilty — although the Hullians followed Watson’s “control” imperative less faithfully than the Skinnerians.

To point 3, the organism-as-machine: Lemov’s book reviews some of the history of behavioral control beginning with the biologist Jacques Loeb (1859-1924), who is famous in psychology for his studies of simple orienting mechanisms in plants and lower organisms²⁶. Loeb was committed to the idea of animals as machines, to an “engineering standpoint”, as WAL puts it. Lemov is not a fan, even though a belief in the lawfulness of biochemistry was essential to the development of molecular biology. Conversely, a science of the soul or even of consciousness is almost certainly a lost cause. Yet that seems to be the path that Lemov favors,

Lemov rejects the fundamental tenets of behaviorism even as she insists that modern society actually lives by them: “the science of behaviorism was made to factor out such things as emotional states, innerness, subjectivity, and the unconscious and reduce activity to a series of blind mechanisms.” First, “blind mechanism” is surely just a pejorative way of saying that science, all science not just behavioral science, must begin with the assumption that its subject matter is lawful, deterministic. Second, “innerness” “subjectivity” and the like are part of behaviorism to the extent they can be observed by third parties. I — “science” — cannot know the “innerness” and “subjectivity” prompted in Lemov by the Mona Lisa, but I can see if she

²⁵ John B. Watson (1913) Psychology as the Behaviorist Views it. *Psychological Review*, 20, 158-177.

²⁶ The standard work on the topic from that time is G. Fraenkel & D. L. Gunn: *The Orientation of Animals: Kineses, taxes and compass reactions*. Dover (1940/1961).

judges the Leonardo more similar to La Bella Principessa than to Picasso's Weeping Woman. That is all that behaviorism means now and it is accepted by most of behavioral science²⁷. Moreover, many behaviorists, including (surprisingly) B. F. Skinner, acknowledge the reality of an unconscious²⁸, if not Freud's version. And third, Prof. Lemov and I remain perfectly free to experience as much "innerness" and "subjectivity" as we like: life is not science.

The main problem with the book is that it forgoes accuracy in favor of an intriguing political narrative: that modern capitalist society is run like a lab experiment propelled by billions of dollars funneled into laboratories that were engineering behavioristic methods of social control. In service of this story, Lemov seriously distorts the history of behavioristic psychology. She places much emphasis on Clark Hull (1884-1952) at Yale, calling his research program in search of quantitative principles of learning "ridiculous". Hull and his followers found a few things and failed in many respects. But science is trial-and-error; failure is an essential part of it. Lemov's failure to understand that basic fact shows that she really does not understand "the spirit of science."

Clark Hull was influential, but the context is missing. Hull was not the most important behaviorist and the line of work he started led not so much to behavior engineering as to *cognitive psychology*, helped along the way by a U. C. Berkeley behaviorist Edward Chace Tolman (1886-1959), who once claimed that everything about human psychology could be learned from the behavior of a rat at a choice point.

The context is a field called *experimental psychology*, represented at Harvard in the 1950s and early '60s by the Department of Psychology, which was housed in the basement of Memorial Hall with just a half dozen faculty. The rest of psychology was in the much larger Department of Social Relations in Emerson Hall. Experimental psychology was inspired by 19th century German psychologists interested in sensation, perception and memory; in the US and UK, the study of learning soon became a major component. A grand survey of the field is Charles Osgood's massive 1953 *Method and Theory in Experimental Psychology*; its history is covered in two books²⁹ by Harvard psychologist Edwin G Boring (neither cited in WAL).

Experimental psychology no longer exists as a separate entity. Following a national trend, the two Harvard departments were merged into a single Department of Psychology in 1972, housed

²⁷ Henry Roediger (2004) What happened to behaviorism *APS Observer*, March 1.

²⁸ Staddon, J. (2017) Theoretical behaviorism. *Behavior and Philosophy*, 45, 27-44.

²⁹ Edwin G. Boring *A history of experimental psychology* (2nd edition). Appleton-Century Crofts, 1929/1950; *Sensation and perception in the history of experimental psychology*, Appleton-Century Crofts, 1942.

in the multi-storey William James Hall (which keeps diverging sub-disciplines safely segregated, four fire-doors apart, on separate floors).

The two dominant behavioristic movements were headed by Clark Hull at Yale with his younger colleague Kenneth Spence at the University of Iowa, and B. F. Skinner and friend and fellow Harvard student Fred Keller at Columbia. It was Skinner who most closely followed Watson's lead, working with rats and later pigeons as experimental subjects (bizarrely, Lemov has Skinner "doing his dissertation on ants"³⁰!). Skinner, who is hardly mentioned in Lemov's book, was indeed interested in the control of behavior and developed powerful techniques, based on the principle of reinforcement (reward). The methods of operant conditioning led to new discoveries such as reinforcement schedules and to considerable advances in our understanding of how reward and punishment affect not just rats and pigeons but human beings. Almost all Skinner's experiments were with animals because they could be raised and tested in a controlled environment so that the causes of the measured behavior could be identified with certainty, something that is simply not possible with human subjects.

Skinner had little interest in behavioral biology and did indeed consider his pigeons as models for humans, although it is a gross exaggeration to say, as Lemov says of Watson, "the activities of animals under experimental conditions were equivalent to human activities under all conditions." But many of his followers as well as Hull's looked at pigeons and rats as systems from which laws and processes might be inferred that might then have some human applicability. A better analogy would be the step from studying simple inorganic chemistry to the much more complex chemistry of organic compounds. No one expected salt to be a model for a protein, but much could be learned about proteins by first studying salts. It is simply not true that "[behavioral] scientists willingly modeled human society on rat behavior in...a bald-faced way." (In fact modern medicine is probably more guilty than behavioristic psychology of taking 'animal models' literally.)

Lemov's book has a few errors which suggest serious limitations on the author's grasp of basic science. For example, writing about an experiment of Hull's she writes: "current ranged in intensity from 3.3 amperes to 7.6 volts..." which is like saying "the samples weighed from three pounds to four inches." She speaks dismissively of early research on "the curious abilities of certain talented cats or foot-stomping horses." Perhaps she is referring to the famous "[Clever](#)

³⁰ Skinner's dissertation was on *The concept of the reflex in the description of behavior* (1930), with physiologist William Crozier.

[Hans](#)” experiment which showed that a circus-trick horse that seemed to count and add was in fact responding to unconscious cues from its trainer, a far-from-trivial piece of detective work.

In a particularly egregious passage Lemov writes “the coauthor of the famous Bell Curve, Richard Herrnstein, who trained as a behaviorist at Harvard, could not have made his controversial argument about how race in human beings is linked to IQ without doing extensive work on pigeon behavior in the laboratory.” Having worked in Herrnstein’s pigeon lab³¹ from 1962-64, I can say without equivocation that this is absolute nonsense. Herrnstein was Skinner’s student. IQ is part of the study of individual differences. Skinner’s message was the essential *uniformity* of humans — and animals. A famous graph of his³² shows the performance on what is called a multiple schedule, in which successive stimuli (colored disks for a pigeon) signal payoff according (in these data) either to time or number of responses. The three graphs show indistinguishable data from three species, pigeon, monkey and human all trained on the same schedule. Skinner proudly points out “the curves are quite similar.” Yes, they are and the uniform effects of reinforcement schedules, independently of the individual or even, up to a point, the species, was a major theme of Skinner’s work as well as Herrnstein’s at that time. Herrnstein’s later interest in individual differences, in connection with IQ and crime, has no discernible connection to his work on the operant conditioning of pigeons.

Herrnstein’s first essay into Bell Curve territory of which I am aware was an article in *The Atlantic* followed by his book *I.Q. in the Meritocracy*, both in 1971. A more likely source of this interest is not Skinner or pigeons, but a *zeitgeist* dominated by psychophysicist S.S. “Smitty” Stevens, a senior figure in Memorial Hall. Smitty had a side interest in *somatotyping*, the flawed attempt by W. H. Sheldon to assess personality by three measurements of body type. There was some casual discussion of body type and IQ in the basement of Mem Hall at that time; it was never a theme in any lab, as far as I am aware. But Herrnstein conceivably picked up an interest in individual differences from Stevens.

World as Laboratory is a disappointing book given the author’s Harvard position and the interest of the topics it covers. It parodies behavioral science which is not (as one reviewer put it) “the theory that there is a predictable pattern between stimulus and response in the human brain”. Many, if not all, behaviorists were looking for the *process* that mediates between stimulus and

³¹ Lemov might have consulted a special issue of *The Journal of the Experimental Analysis of Behavior*, Volume 77, Issue 3 Pages: 211-392, May 2002, devoted to reminiscences about the Skinner-Herrnstein pigeon lab at Harvard, to get a more accurate idea of the atmosphere in which Herrnstein worked.

³² Skinner, B. F. A case history in scientific method. *American Psychologist*, Vol 11(5), May 1956, 221-233, Figure 14.

response, not some magical omnipotent stimulus. Indeed, this is the aim of all scientific psychology, although schools differ on what processes are important. The book elides the context of behavioral psychology and betrays some ignorance of basic science as well as a skimpy knowledge of experimental and comparative psychology. It is only partially successful as a work of journalism; as a work of scholarship it is an embarrassment.

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History of science is important because it tells non-scientists, especially students who might become scientists, what science is. If historians of science present a distorted picture, the effect can be damaging to the future of science, on which modern civilization depends.

History of science as an academic field has been infected by two fashionable viruses. Politics: the work has to signal clearly that it is on the right (i.e., *left*) side. Narrative: It must tell some kind of story. Never mind objectivity, it should have a good yarn. So from Desmond Bernal, who was indeed politically biased but was up-front about it, understood science and didn't try to make a novel out of it, we pass through Desmond and Moore's covert Marxism and knowing re-interpretation of Darwin's language to, finally, Oreskes and Conway, MacLean and Lemov, writing history so as to entertain and indict capitalism, at the expense of objectivity and a real understanding of science.

It is surely time to restore the distinction between scholarship that strives to be objective and comprehends the science it is writing about and other kinds of historical writing. Honest fiction, such as Hilary Mantel's *Cromwell* books or Dorothy Dunnett's many works of historical fiction, is one thing; fake narrative masquerading as objective truth, subsidized by academic salaries, like the books I have discussed, is quite another. If these folk want to tell tales, let them compete in the fiction market. If they want to write politicized journalism, then find a job with the *New York Times*. But if you want to claim to be an historical scholar, then facts first and stories, if there be stories, much later. Above all, understand the science about which you write.

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