Dear Ronald,

Thanks for reading my article and taking the time to comment, but you seem to have misunderstood my argument. Obviously, that must be my fault for not expressing it more clearly, or perhaps I was addressing it to too narrow an audience (it was aimed at professional historians of science, but then it was in a specialised academic journal).

I never said that mutation theory "unduly influenced" science, nor could I, since that would imply that I think there's a "proper" amount of influence a theory "ought" to have. As a historian, I am simply trying to describe what I think happened by offering an argument that (I hope) provides a plausible interpretation of the evidence. Part of that evidence is that particular ideas have had a particular influence on various people, so there cannot (from my, descriptive, perspective) be an "undue" influence.

Perhaps the problem is that you seem to think that I'm trying to make science policy, or describe how it ought to be made (I have no expertise in either, but — while I'm interested in both — neither was at issue in this article). I can't otherwise explain your, frankly perplexing, claim that my "conclusion presupposes that the social body would be better served if 'scientific advances' (Endersby mentions GMOs and the MMR vaccine) met with less public resistance". (It's not important, but just for the record: I'm an opponent of GMOs, for reasons I explain in the conclusion to my book, "A Guinea Pig's History of Biology"; and, both my children have had the MMR vaccine, after I'd read some of the debates around it and decided there was no evidence of risk.)

What I said in the article was that cases like these demonstrate the way that: "even a strong consensus within the scientific community may not be sufficient to quell public scepticism". This was intended simply to *describe* the ways in which the public interprets and makes use of scientific ideas: not to suggest they are either right or wrong. My whole point was that the public cannot be "wrong", as far as a historian is concerned. If the public thinks "evolution" means "progress" then that is what evolution means, *to the public*, however much most scientists would disagree. My quarrel (such as it is) is not with either scientists or the public, but with those of my fellow historians who tend to assume that the "meaning" of a scientific theory (which, for me, is its historical and cultural significance) is something that is determined entirely by scientists (or, to put that in academic jargon, with the residual internalism that still affects much C20 history of biology). The case of mutation theory shows two things: that the public saw all kinds of things in the theory that de Vries never imagined (but that neither he nor the other scientists could control); and, that the public's interest in the theory persisted long after the scientific community's. So, various publics were participants in the historical process of deciding what the mutation theory meant.

Having said all that, I found your textbook history of de Vries absolutely fascinating; it confirms some of what I thought, while suggesting some fruitful avenues for further research (this article is the first part of what will eventually be a book, if I ever get time to work on it). Clearly, the textbook writers view of de Vries would be another interesting way to look at the ways in which the mutation theory's meaning was created, sustained and (eventually) destroyed. From your conclusion, I would think that we actually have a very similar view of this history in many ways, but clearly my throwaway remarks about MMR, etc, mislead you as to the main purpose of the argument. Which, as I said at the outset, must be my fault for not clarifying the argument sufficiently.

I'm looking forward to reading your Ella Thea Smith article when I get a moment; sounds fascinating.

Very best wishes, Jim

_____
Dr Jim Endersby
Reader in the History of Science, Director of Teaching and Learning
School of History, Art History and Philosophy, University of Sussex
Book reviews editor, Annals of Science

Jim,

I am most grateful for your considered and thorough comments. Upon review, I see I did get a good bit wrong. I must apologize for suggesting that your article was in any way an attempt to assist advocates of science-based social policies by educating them on how the noise generated by the popular press around folk-science interpretations of data may "unduly" influence their work.

You don't say that anywhere in your article, and it's not your point.

I have to admit my high horse was whipped and my senses dulled by a couple of your throwaway comments. First, by the suggestion that evolution means progress to the public, but not to the specialist. Though perhaps true now, at least relative to organic evolution, it was hardly the case in the early 1960s (guys like Muller, Glass, Moore and others were progressionists through and through, and they very consciously used the BSCS texts as a platform to promote their ideology). And second, by the suggestion that traditionalists, relative to theories of evolution, tended to confine their comments to specialized publications for fear of providing material support to evolution's proto-creationist enemies. That is the conventional view. But a quick review of the Ella Thea Smith article would help explain why I think this "myth" makes for bad history.

Anyway, thanks again for the comments. I look forward to your book! And yes, I think we probably do have similar views of the history here. It'd be fun to compare points of view over a pint some day. If I'm ever Sussex way …

Best,

Ron