

## Book Review

### The Art and Politics of Science

Harold Varmus

New York/London: WW. Norton & Company

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Autobiographic throughout, *The Art and Politics of Science* is not an autobiography, and does not claim to be one. Neither is it a theoretical reflection on the relationship of science and politics. It is explicitly subjective, an always-personal account of what it means to do science. It tells us something about the nature of laboratory science, and of how politics is involved in the doing of science - for example through allocating funds and legislating research. The book does not make any claims of general validity.

Its author is one of medicine's Nobel laureates as the book's front cover immediately reminds us, and as head of the NIH he was a key figure of US science politics under the Clinton administration. Looking back at these two areas of his career, and at their interweaving, Harold Varmus also likes to tell us a little about his family origins as well as his hobbies: the back cover presents us with a black-and-white photograph of a happily worn-out and smiling Varmus in full cycling gear. And indeed, he was involved in some fast rides throughout his career. He seems to have enjoyed most of them: his reflections exude contentment.

In the introduction, Varmus refers to C.P. Snow's 1959 Rede lecture, in which Snow proposed the concept of the two cultures: the arts and the sciences as two worlds apart, separated by a gulf of language and ideas. Against the background of his own life in science and science politics, Varmus perceives Snow's proposition as too simplistic. In *The Art and Politics of Science* he does not attempt to refute Snow by offering his own account of how science and politics relate to each other, or by explicitly reflecting on how political conditions and rules shaped the science he was involved in. Yet his recollection of his own dealings at the bench and in front of congressional committees – often supported with quotations from transcripts – provides insights into how science and politics negotiate, contract and at times converge.

In parts one (“Becoming a Scientist”) and two (“Doing Science”) Varmus recollects in detail the science he was involved in which led to a Nobel Prize in medicine in 1989. He and his team focused on RNA tumour viruses; they wanted to find out about virus multiplication (how does RNA get translated into DNA) and the cellular mechanisms that lead to cancerous cell growth. Here, they dealt with questions of gene expression as well as genetic change during evolution. The team “discovered” the cellular origin of RNA

tumour virus oncogenes, i.e. genes that have the potential to cause cancer – a result with significance for the classification of cancers and for targeting by drugs and antibodies.

The two parts give us an insider's perspective on what it means to “do science” in the contemporary global context. Like many successful scientists, Varmus directed laboratory research within a large international team, hence, his preference for an inclusive “we” instead of the exclusive “I” when talking about his laboratory work. He taught classes, wrote papers and delivered talks, attended meetings and applied for grants. He had, in short, a typical science career, the outcomes of which – also typically – never developed in a linear fashion.

It is to Varmus' credit – and to the benefit of readers with a theoretical interest in the nature of science research – that he reflects on epistemological aspects of science: the various collaborations within and between research teams, the meandering movements, the to-ings and fro-ings of scientists in search of new insights, the role of luck and coincidence in this search, the nature of scientific “discovery” (more on that below).

But what made an excellent scientist turn to politics? Parts three (“A Political Scientist”) and four (“Continuing Controversies”) are devoted to Varmus' account of his involvement in the politics (and ethics) of science. When negotiating the naming of the AIDS virus in the 1980s he became involved in talks and committee work. After being awarded the Nobel Prize in 1989, he was “increasingly asked to voice opinions, to make speeches, and to join and even lead groups engaged in the politics of science. This new level of engagement with the forces that shape scientific life ... led ... to the directorship of the National Institutes of Health” – the largest supporter of biomedical research in the world.

Varmus claims to have had an “apolitical past”. By that he means, it seems, mainly that he took the funding he received for granted: he did not see it as a political act that included a variety of decisions made by a variety of players, including the government on the one hand, and the scientific community on the other. But this is, by and large, what science politics is for Varmus: a dialogue between the science community and the State with the concrete aim of allocating funds. “The inevitable point of departure must be money.” The partners in such dialogue need to stake out their claims: curiosity, a spirit of exploration, health benefits, economic benefits, stature of a nation – to name but the most obvious. It also requires ‘literate’ partners, who are able to read, speak and understand the other's language. *The Art and Politics of Science* is perhaps at its best when Varmus describes the process of how budgets and corresponding spending programmes are set up.

There is, however, a second strand to science politics, namely that which is concerned with the morality of research. It finds its expression in legislation governing issues such as embryo research, stem cell research or human cloning. Here, Varmus details his own comments made in the context of the annual NIH's appropriations cycle in 1998, the year

Dolly was cloned, and his (unsuccessful) dealings with both Clinton and Al Gore prior to the legislative ban on the cloning of human beings (he was against it).

And there is a third strand to science politics, which is only touched upon in chapter nine of *The Art and Politics of Science*. Recalling briefly the controversy that arose around the naming of the AIDS virus in 1985, Varmus deals with what I would call the political nature of scientific discovery. I will mention three aspects here: (i) discovering as a process vs. discovery as a point in time; (ii) the subject who discovers; (iii) the discovered object.

How can we talk about a discovery as a point in time when the scientist's search is not linear, but meandering, as Varmus describes in the first parts of his book? Who counts as "discoverer" when science takes place in teams and across nations and continents? According to scientific convention, only the discoverer is allowed to name a newly discovered entity. In the case of the AIDS virus, the claimants to fame were Robert Gallo, an American scientist at the NIH and Luc Montagnier, a Frenchman at the Pasteur Institute. And where were the teams who worked at the bench with them?

Why does it matter anyway who discovered, and therefore who names? Varmus rightly points out that "acceptance of a name for the virus by the scientific community could affect the outcome of debates over patents and distribution of royalties, and the decision could influence the award of important prizes, with implications for national stature". Members of governments and national media became involved. They worried about the proceeds from commercialization of tests kits and any future therapies or vaccines, should these be found.

In the AIDS controversy, the different names under consideration reflected *different views about the nature of the virus*. The differences were significant for the views of the virus as an agent of disease. That controversy arises around the naming of a discovered object discloses how we are unable simply to *read* the nature of a thing *off nature* and how our understanding of natural entities is always also the outcome of an argumentative process, a political process.

As described by Varmus, this is a process in which arguments go on between the discoverers, other scientists and governmental representatives. Negotiations take place until "an agreement is brokered" and a "consensus choice" is announced. In the case of the AIDS virus, the outcome "has worked well for many purposes – scientific, political, economic, and medical".

This begs further questions: if this had not been so, would a new process of naming have taken place? Will it take place, should the name suddenly prove not to work well anymore? And how does such a brokered outcome reflect back on the initial question of the nature of the discovered entity? How can we say we discover a thing when apparently

different people perceive the nature of the same thing differently, and when it is only at the end of a process of naming that we are able to go back and refer to the entity as *the one discovered* thing?

Similarly, we could probe Varmus in other contexts, too. Open questions remain or are initiated. Yet it is because of his ability to make us ask further questions that his detailed and honest account of what happens at the intersection of science and politics is a most recommendable read, not only to a research community interested in the relationship between science and politics, but also to audiences with an interest in the morality of science and how the moral nature of science research is discussed and expressed in legislation. Last but not least, the author's ongoing, enthusiastic commitment for open access publishing (essentially a struggle against large, for-profit publishing companies) should recommend his book to readers of journals such as *Genomics, Society and Policy*.

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