Letters

What is history of science, and who should speak for the past? In his review of my book, *The Molecular Vision of Life: Caltech, the Rockefeller Foundation, and the Rise of the New Biology,* in the spring issue of *Engineering & Science,* Robert Sinsheimer reaches a curious verdict. He writes, "Kay clearly belongs to the school of historical determinism that maintains the view that the course of scientific progress cannot be autonomous, but is always a response to cultural, usually political and economic, forces." I take this as praise! Historical determinism—the thesis that certain forces shape historical processes—is a fundamental premise of historical scholarship, and demonstrating that the development of science is a genuine historical process is one of the principal challenges to historians. For example, how do intellectual and technocratic elites shape, and how are they shaped by, social and political agendas?

That such a scholarly goal and its attainment constitute a first-order accomplishment in the history profession, while deemed subversive by many scientists, underscores the essential tension between the two professions. To be sure, this tension over who speaks for the past can be healthy and productive, providing it is governed by mutual scholarly respect and authentic interpretations of the arguments.

Thus I cannot fault Sinsheimer (or others from Caltech) for being scandalized given his reading of my interpretive framework. For he drew precisely the conclusions that I warn against in the lengthy introduction to my book: 1) that the Rockefeller Foundation had a hidden agenda of social control; 2) that individual scientists were manipulated and co-opted by the Rockefeller Foundation; and 3) that "human betterment" amounted to conspiracy or a Machiavellian plot. Indeed, as he rightly concludes, such lessons border on the ludicrous.

I do, however, fault Sinsheimer for misreading my thesis. As I make clear:

1) There was nothing covert about the Rockefeller Foundation's interests in social control; it was not a "hidden design." Quite the contrary, the trustees and officers explicitly and openly stated these goals in many of their documents, which I quote verbatim. I do not accuse or condemn but explain how their premises and specific formulations of social control were congruent with their commitment to the political and economic framework of pre-World War II America. (Articulations of social control in 18th-century France or 19th-century China looked quite different.)

2) Individual scientists were not manipulated and science was not co-opted by the Rockefeller Foundation or by Caltech trustees. Throughout the book I show how Millikan, Noyes, Morgan, Pauling, Delbrück, and Beadle "used" the Rockefeller Foundation as much as the Foundation "used" them. These were strong-willed, farsighted individuals who, as Rockefeller advisers, often told the officers how to plan. They were neither helpless pawns nor co-conspirators, as Sinsheimer portrays my account. I clearly say in my book (pages 8–11) that, being cut of the same cultural cloth, the managers of science, Rockefeller Foundation officers, and Caltech's trustees shared a *Weltanschauung*, yet I stress that this did not constitute an explicit agreement on all aspects of programs and policies. The complex problem in political theory of how intellectual elites fit into social agendas has been extensively studied, and it is on this body of knowledge that I base my analysis.

3) It does not take top-down coercion or a Machiavellian plot to get groups of people to cooperate. Any scientist with leadership experience must know that successful power sharing is predicated on compromises—some explicit, some tacit, sometimes unconscious. Scientists have always worked within bounded and negotiated autonomies. Today's constraints are different from those of the 1930s, but there have always been constraints on the course of science. Thus, rather than co-optation, I see the rise of molecular biology as a nuanced co-production of scientific knowledge by patrons and researchers.

That the Rockefeller Foundation had a shaping power in molecular biology is hardly news; there has been excellent scholarship on this topic. There are also outstanding works showing how institutions (including Caltech) and social trends have shaped (though not *determined*) the course of modern science. My work, which links social, institutional, and cognitive agendas, is *not* revisionist; it is squarely within the mainstream of history of science.

Sinsheimer laments that such detailed scholarship should have been placed in the service of a distorting, revisionist ideology. This is strange. Had I written a hagiography of molecular biology at Caltech under the aegis of the Rockefeller Foundation, would scientists view my account as ideology-free? Curiously, a history is pronounced ideological when it challenges the dominant version of the past. I did not come to this subject with an ideological bias. It was the archival documents, primary sources, and earlier historical works that shaped my interpretation.

I genuinely regret that, by the misreading of my thesis, the book has caused Sinsheimer and others dismay and that they feel affronted. It is important to keep in mind that this book is not primarily about individuals but about mechanisms, about how science as a system worked in a specific historical context. Individuals are certainly crucial elements in such a process, but surely the scientific whole is greater than the sum of its individual parts. (The social responsibility of scientists is another topic deserving separate discussion.) I have high regard for the science and scientists at Caltech in the period I have studied. My thesis is not aimed at individuals but at the social processes which, knowingly or not, they helped shape and were shaped by. Does Sinsheimer suggest that science at Caltech has escaped the forces of history?

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