# Response

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# Response

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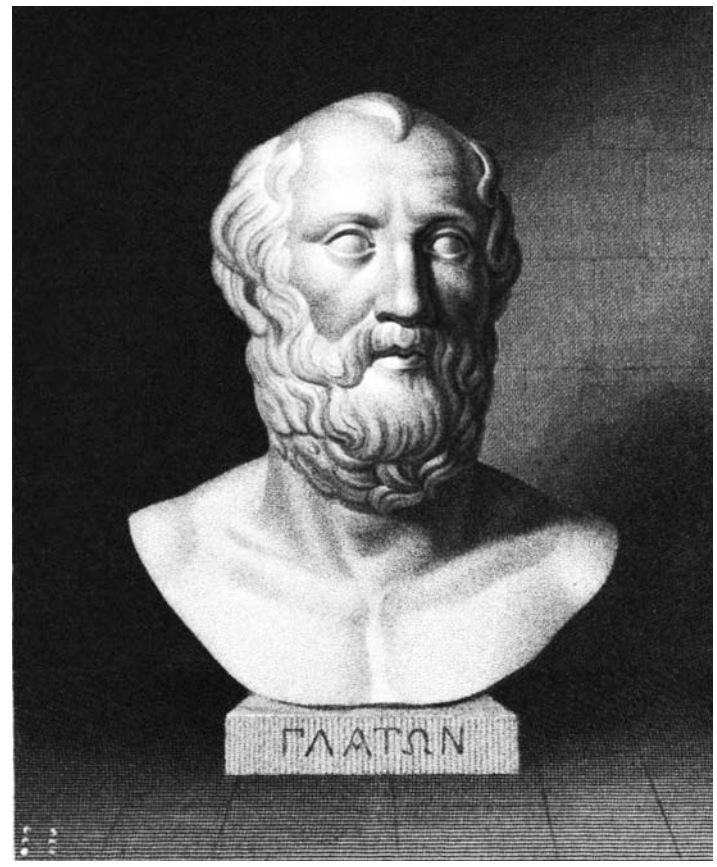
It was from Floris Cohen's magnificent book on the historiography of the Scientific Revolution that I first learned of Ben-David's *The Scientist's Role in Society*, where, buried in a mass of other information, was a gem: a clear and precise statement of the thesis that the development of science in the West was not a successful realization of something common to all scientific cultures, and at which they had all aimed, but an anomaly. Rather than trying to understand why other scientific cultures hadn't achieved what was achieved in the West in the Scientific Revolution, scholars should ask why the development of science in the West took a path quite different from every other earlier culture. The model in the other cases was a boom/bust one, in which interest in scientific questions is very much related to solving particular questions, and when these are dealt with, there is a decline of interest in science as such. By contrast, the development of Western science from the late 16th century onward manifests a cumulative growth: Why?



From *The Works of Plato, Volume II*, trans. by Henry Davis (London, 1854).

It is not surprising that, having realized the importance of the Ben-David thesis, Floris and I both set out to problematize the emergence of modern science, and both of us realized that this was a project that required a particularly large-scale perspective. I believe that both of us started thinking seriously about these questions in the mid-1990s, and it was ten years before the first volume of my own project, *The Emergence of a Scientific Culture* was ready, and fifteen years before Floris's volume, *How Modern Science Came Into the World,* appeared. Working with a large-scale historical perspective means honing the questions one needs to ask, and the resources one needs to draw on. This is what takes up much of the time and, despite our common starting point, it is not surprising that this long and difficult process, one that requires constant revision, did not have a single outcome, and that the way we honed the questions, and the resources that we drew upon, turned out to be significantly different.

This is perhaps most evident in the treatment of medieval natural philosophy. By contrast with Floris's account, in mine there is no single underlying story to be told. In the case of developments from the 13th century onward, there are two competing views: one holds that nothing occurred that was of any significance to the future development of science; the other sees the Scientific Revolution as in essential respects a continuation of medieval work in areas such as kinematics. Floris writes that I am definitely of the latter view, but I am not. In fact I think medieval theories in mechanics, for example, were a dead end, and I would not even put Copernicus at the source of modern natural philosophy because his heliocentrism remained completely unworkable: as I argue in *Emergence*, he was unable to make any sense of geocentrism in terms of the physical theory he was working with, and indeed the two were in conflict. Why then, does Floris take me to support a continuist account? Presumably because I argue that when 13th-century theologians began to think through theological questions in terms of Aristotelian metaphysics, they brought about a cultural shift from a contemplative view of nature to one in which our sole source of knowledge of the natural world lay in the use of our senses. This meant that those disciplines that we now think of as scientific were not only pushed to the forefront of understanding, but they also became the sole means of acquiring knowledge of the natural world (which included the treatment of the soul).

The key for me is not to uncover the underlying story, because I don't believe there is one, but to bring together two different sets of issues—the emergence of a scientific culture (13th century) and the development of a viable physical theory (17th century)—and explore how they interact. This exploration led me to develop an account of the *persona* of the natural philosopher, something that has no place in the kind of linear account that Floris offers. In my account changes in the self-image of the natural philosopher in the 16th and 17th centuries are crucial.

Indeed, I hold that my reading is more discontinuous than that offered by Floris in *How Modern Science Came Into the World*. There Floris traces various key developments in the Scientific Revolution back to germs of ideas, typically in classical and Hellenistic antiquity, which were unable to flourish in their own time (or for that matter in medieval attempts to revive them), but found a rich nurturing culture in the 17th century. I reject such an account on a number of grounds, not least historiographical, for it implies a kind of teleology that strikes me as questionable: as if everyone, from antiquity onward, were ultimately aiming at the same thing. I find this quite implausible. When resources that had been developed in antiquity were taken up, what we need to understand is why these were chosen from among the available options, and under what conditions. To answer this question we need to understand 17th-century science, not antiquity. As Koyré once pointed out, if we want to understand why Platonism was revived by Galileo, what we need to understand is not Plato but the kinds of conceptual issues facing Galileo and the resources available to him.

But what is fundamentally at issue here, to the extent to which we are dealing with questions in intellectual history, is a philosophical as well as a historiographical question: Is the history of science a history of truth and how we have arrived at it, or is it a history of the development of theories and their justifications? History of the former kind can only be genealogy, whereas history of the latter kind is, in my view, a genuinely fruitful form of inquiry, one that explores the different paths we might have taken, and questions whether the direction we have taken was the only possible one. Take the case of Newton on gravitation. Floris is quite correct that I don't stress that Newton got it right. That's not how I'm approaching the question. I do look in detail at how his account resolves problems, both at a technical level, and at the level of a general approach to mechanics. For example, I devote a lot of attention to Newton's rejection of the dominant statics model for dynamics, and his successful revival of the failed Galilean kinematic model, because this shows how a fundamental and difficult shift in understanding was needed before he could get off the ground. I also devote a lot of attention to the genuine difficulties Newton had accepting his own account of

gravitation and his search for a satisfactory account of its nature in a chemical matter theory, as well as to the genuine difficulties that continental natural philosophers had in accepting gravitation before 1730. In looking at the arguments and the problems, having the "right answer" in front of one is an impediment and, I believe, not conducive to increasing our understanding of these episodes. Having said this, I am not denying that there is a great deal to be learned from *How Modern Science Came Into the World*, but I do question some of its fundamental assumptions.

Dan Garber's comments raise the question of how far back we can trace science. In one sense this is a terminological issue: the intimate connection between science and technology, and the notion that science is something typically pursued by a scientist, that is, a professional researcher who has been trained in a particular way, are defining characteristics of modern science, and these are 19th-century developments. But, of course, there are substantial questions at issue. My view is not that Aristotle's natural philosophy lies at the origins of modern science (I don't believe one can learn anything from pursuing questions like that), but that the replacement of Platonism by Aristotelianism in the 13th century meant that the path to understanding the natural world was now via observation, not contemplation. This was a massive cultural shift (and one that was fiercely resisted) but in itself it doesn't yield "science." Dan focuses on the lack of quantitative and empirical testing before the late 17th century, taking these as the key features of some recognizably modern science. Such a claim wouldn't be incompatible with my project, although it's worth noting that in pursuing these questions of quantitative and empirical investigation, I have come to doubt any natural pairing of these features before the late 19th century. In *The Collapse of Mechanism* I show that the most sophisticated form of quantitative physical inquiry in the 18th century, namely rational mechanics, was an empirical dead end, closing off questions of evidence to such an extent that it became separated from the physical realm. By contrast, the exact opposite is the case for the cutting-edge empirical disciplines of the 18th century: in chemistry, quantitative considerations do not go beyond simple arithmetical ratios, and in physiology mathematical considerations are irrelevant. Once we have abandoned—as we must in studying the 18th century—a model of science based on the geometrically/algebraically formulated physical sciences, this is no longer puzzling or mysterious.

One thing that Floris Cohen and I definitely agree on is the need for big history. Neither of us think you can do this without having done a significant amount of detailed microhistory, and indeed both of us combine the two in our work. If you don't think explicitly about big history, you are condemned to making all kinds of assumptions that may be unfruitful, counterproductive, or just plain ignorant. It is something that every historian has to think about at some stage, and it distinguishes history from antiquarianism. My own route to these questions was through philosophy, and I discovered that the kinds of philosophical questions that I wanted to pursue—the legitimacy of a scientific culture, what it consists of, how it can come about—have an essentially historical dimension. Consequently, the history of science has been my route to dealing with philosophical questions about the nature of science, and as Peter Dear notes, this has left traces in my account, in that it is largely an intellectual history of science. As Peter also notes, the elaboration of a social and institutional dimension to the project would seem to offer considerable benefits, especially as it moves into the 19th century. I suspect this is true, though I think it is worth stressing that I am not writing a history of science in which one has to balance conceptual and technical considerations against social and institutional ones. Even though Peter is aware of this, it is worth reiterating that various forms of historical inquiry are the vehicles by which I pursue the project. The resources I draw upon depend very much on what questions I pose and how I pose them. Conceptual investigation will not throw much light on the differences between the uptake of Newton's theory of gravitation in England and France in the late 17th and early 18th centuries, for example, any more than social and institutional investigation will illuminate the problems caused by identifying inertia with equilibrium in statically-modeled accounts of dynamics.

More importantly, the problem is not so much a reductionist temptation to treat all issues in either conceptual or social terms, but rather that there is a danger that these kinds of approaches can be taken to exhaust the alternatives. It is crucial for the kind of project in which I am engaged that this is not assumed. New tools occasionally need to be forged, and the prime example in my work has been that of the *persona* of the natural philosopher. The changes to the standing, aspirations, perceived expertise, and sense of identity of the natural philosopher are not something that can be captured in social or institutional terms. The key is to devise an account of this autonomous realm and identify the resources one needs to explore it, without reducing these questions to psychology or sociology or reducing the content of theories or experimental practices to effects of a particular type of *persona*. Equally pressing questions arose in accounting for experimental traditions, which are often neglected by those concerned exclusively with the development of theories and treated in reductionist terms—e.g, in terms of gentlemanly witnessing—by sociologists of science.

Finally, let me say how grateful I am for the opportunity to have some of the larger historiographical issues aired. They are always there in the background, but it is rare for them to be considered explicitly.