Method and control: naturalizing scientific culture in Bacon's *Novum organum*

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It has been widely noted that rules for scientific method fail to produce results consistent with those rules. Daniel Garber goes further by showing not only that there is a gap between Francis Bacon's methodological rules, outlined in the *Novum organum*, and his natural philosophical conclusions, but that his conception of natural forms informs the method in the first place. What needs further examination is *why* Bacon's application of his method manifestly violates his rules. Garber appeals to the spirit of Bacon's method, rather its letter, which allows him to reconcile an appreciation of Bacon's impact on modern science with a contextualist approach to the history of philosophy. A better approach looks at the larger significance of mythological accounts of scientific method, that understand seventeenth-century methodological doctrines as ideologies naturalizing scientific culture and outlining news ambitions for the control of nature. By examining Bacon's followers in the Royal Society, we can see how Bacon's "temporary" use of hypotheses helped secure support with the promise of future utility. The history of philosophy of science should focus on the conditions leading to emergence of certain kinds of distinctively modern discourses, practices, and ambitions going beyond the internal history of science.

Keywords: Francis Bacon, scientific method, Royal Society, ideology

Метод и контроль: натурализация научной культуры в «Новом Органоне» Бэкона

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Часто отмечается, что применение правил научного метода не всегда приводит к результату, соответствующему этим правилам. Дэниел Гарбер идет дальше, показывая не только то, что существует разрыв между методологическими правилами Фрэнсиса Бэкона, изложенными в «Новом Органоне», и его натурфилософскими выводами, но и то, что его концепция природных форм сама служит основой метода. Здесь важно понять, почему бэконовский метод так явно нарушает его собственные требования о беспредпосылочности. Гарбер апеллирует к духу метода Бэкона, а не к его букве, что позволяет ему примирить оценку влияния Бэкона на современную науку с контекстуалистским подходом к истории философии. Но будет еще эффективнее принять во внимание мифологические трактовки научного метода, которые понимают методологические доктрины XVII в. как идеологии, натурализующие научную культуру и выражающие новые амбиции по контролю над природой. Обращаясь к анализу последователей Бэкона в Royal Society, мы можем увидеть, как "временное" использование гипотез Бэкона помогало заручиться поддержкой в перспекти-

ве будущей пользы. История философии науки должна сосредоточиться на условиях, ведущих к появлению характерных современных дискурсов, практик и амбиций, выходящих за рамки внутренней истории науки.

Ключевые слова: Фрэнсис Бэкон, научный метод, Королевское общество, идеология

Daniel Garber [2021] presents us with a clear-cut demonstration of a now widely accepted conclusion about scientific method: scientific method doesn't do what it claims to do in "producing" scientific results from a simple set of rules of inquiry. This is particularly evident in Bacon's case because he was at pains to emphasize that his method outlined in the *Novum organum* was intended to be a fully general account of scientific procedure, applicable to any field whatsoever without making any assumptions about nature in the process.

The strength of Garber's paper is in showing quite clearly the physical assumptions that inform the methodology in the first place, rather than simply noticing a *gap* between what Bacon says in his methodological writings and what he says in his natural philosophical writings. Rather, the articulation of the method itself depends upon smuggling in a very substantial – and peculiar – ontology. The key animating assumption is that humans could gain control over natural processes by emulating the way natural bodies are produced as the result of the imposition of several distinct forms on a given substance. Those natural bodies are nothing but the combination of simple natures produced by certain kinds of actions.

Bacon's "inductive" method makes use of various tables to determine the presence and absence of various simple natures that make up what we would call the properties of any natural body. Bacon seems to assume that natural objects are nothing but the concatenation of these simple natures, each of which is produced by some common action that can be replicated by us to produce it at will in any body whatsoever by imposing the form corresponding to the same nature. In the case of heat, this means that we can produce heat if we produce an expansion that we then try to confine somehow, leading to an uneven expansion of matter that's equivalent to what we see in hot bodies, something that is lacking in cool ones. The corollary is that this is not at all the modern kinetic theory of heat and that the motion of bodies is understood as closer to animal appetites than Cartesian local motion.

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Where Garber remains puzzled, and where his analysis remains incomplete, is in understanding *why* Bacon carries out his methodological investigations in such an inconsistent way. Why was he not aware that his natural philosophy not only did not follow from his method, but that

the method itself was partially constituted by these taken-for-granted assumptions about the fundamental furniture of the world? There is a hint of an answer in his conclusion "that Bacon himself was probably as uncertain about how to think about these apparently necessary assumptions [of his ostensibly assumption-free method] as his later commentators are". If *we* cannot come to agreement about how to reconcile his methodology with his science, despite the careful sifting of texts and a much better sense of how scientists actually conduct their work, how can we expect to hold Bacon accountable for any inconsistency between his aims and his results?

Garber's solution is telegraphed in his title, referring to Bacon's "metaphysical method". Bacon would take his method to be free of metaphysical assumptions, while modern philosophers would reject the possibility. To make this point, Garber takes on board Popper's argument that observations are always directed, in some sense, by the interests that motivate our inquiry, an insight certainly not unique to Popper. Indeed, the view that observations are conventional or theory-laden has been widely accepted in twentieth-century philosophy of science, familiar from philosophers of science such as Pierre Duhem, Otto Neurath, Willard Quine, Imre Lakatos, and Paul Feyerabend, just to name a few.

Bacon, presumably, would not agree, but Garber shows us that he was wrong, in effect downsizing Bacon's own expectations for his method. Garber makes this point even though it takes him out of his preferred subject area of history of philosophy and textual exegesis to "philosophy proper". At the same time, Garber seems to resist the conclusion that method talk is nothing but a form of self-delusion, what John Schuster calls a "myth", following the structuralist analysis of Roland Barthes, or that method should "not be treated solely as a set of formal statements about how to produce knowledge, and not at all as a determinant of intellectual practice", as claimed by Steve Shapin and Simon Schaffer [Schuster, 1984; Shapin, Schaffer, 1985, p. 14].

Instead, Garber recovers "the spirit of Bacon's project" by attributing increasing control of nature since the time of Bacon to the instrumental effects of adopting Bacon's method, or something like it, despite its failure in any literal sense. Thus, in my reading, Garber seems pushed to his conclusion about Bacon's "metaphysical" method, a conclusion that would *not* fit with Bacon's self-understanding, as a necessary violation of historicist strictures, because doing otherwise would do too much violence to interpretation of his texts elsewhere. It is the smallest change in Bacon's self-understanding of his texts that can make sense of the integrity of the whole of his work.

Bacon's failure to literally enact his method is then rescued by attributing success to the spirit of Bacon's law, rather than its letter. This has the virtue of preserving his intent in a deeper sense and rescuing Bacon's contribution to modern science. We now, like Bacon, follow

the spirit of his method, just not the letter, as we clearly have been able to impose ourselves on the constitution of matter to some extent, while carrying out some kind of empirical method.

To the extent that this accurately captures Garber's own self-understanding, then his approach to the history of philosophy of science can be seen as modeled on a loose constitutionalist interpretation of the "living constitution" of science, rather than a literal, "originalist" view[1]. Bacon's method is not a strict and efficacious method for modern scientists to follow, but it is also not just a set of writings of purely antiquarian interest unconnected to modern science. In opposing Whiggish approaches to the history of science, while still maintaining the intellectual coherence of the field of history of philosophy of science, Garber must walk a fine line between crediting method talk with efficacy and understanding historical contributions in their own context.

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My own suggestion is that this approach is not quite the right "method" for understanding the significance of methodological talk. To understand what Bacon was up to in detailing rules of method that he did not strictly follow, it is necessary to think about what else is informing his construction of a general method. On the one hand, methods are objects of dispute between competing schools of natural philosophers in the seventeenth-century. In this manner, natural philosophers not only distinguish themselves from scholastic philosophy, but compete over what kind of inquiry will replace it. On the other hand, methodological narratives are invoked to form a *post hoc* justification of the research process, or in this case, to identify the fundamental forms of nature.

We can separate three different levels of discourse on method informing different approaches to the topic. First, general, abstract accounts of proper method are used to justify or legitimate scientific communities. Shapin and Schaffer develop this kind of account in explaining how the Royal Society's emphasis on the importance of matters of fact was

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[1] Of particular relevance here is the argument by Ackerman [2007, pp. 1738–1751] that a literal interpretation of the U.S. Constitution would distort, or even invert, the real significance of Constitutional amendments, while missing the shift in operational governance from an amendment process predicated on federalism to a national, interpretive process shaped by the appointment of partisan judges and the passing of landmark legislation by Congress. Moreover, the originalists, who ostensibly support a literal interpretation of the Constitution, actually look to reverse the written text through a focus on "judicial revolution" [1742] rather than amendment. The initial establishment of the Constitution also depended upon implementing a ratification process different from that required by the Articles of Confederation, which would have blocked its implementation.

developed to sideline Hobbesian dogmatism and shore up support in a delicate political context. Michael Mulkay set the pattern for this approach in rejecting Robert Merton's norms of science as binding on scientists; instead, appeal to norms constitutes an ideology that informs written communication, enabling continued support from the wider society [Shapin, Schaffer, 1985; Mulkay, 1976].

Second, method can be understood as a set of rules designed to generate or produce scientific findings. Scholars focused on this level tend to assume that this level is capable of working more or less as its proponents claim, though there may be disagreements about which method is the correct one. Alternatively, a capacious, but relatively non-specific, method like the hypothetico-deductive method can be seen as tacit in most early methods [Oldroyd, 1972], such that what seventeenth-century writers on method were doing was discovering how science makes discoveries [Jardine, 1984]. With the applications of modern historical methods to the history of philosophy of science, focus has shifted to the differences among early philosophers of science, and the role of intellectual and cultural context in shaping their views. The connection to a larger trajectory of method becomes less clear, except perhaps in terms of a "changing logic of scientific discovery" that is much closer to the third level of practice [Lakatos, 1978; Kadvany, 2001, pp. 393–395].

Third, one can use method in a mundane sense to refer to the actual level of concrete practice, using thick descriptions to show how scientists actually carried out their research. This "turn to practice" in both philosophy and sociology often comes with the assumption that no distinct level of method as a set of rules, apart from situated practice, exists or deserves attention [Wittgenstein, 1958; Turner, 1994; Lynch, 2021, chs. 4–5]. Influential approaches in Science and Technology Studies take this as a point of departure. After Mulkay dispatched Mertonian norms as binding on the actual behavior of scientists, any understanding of scientific change must look closely at science in the making, whether through laboratory ethnographies or close archival study of specific discoveries.

These levels should not remain separate, however, but are threaded through each other, such that methodological redescriptions of practice can shape practice itself, or influence the larger goals of scientific inquiry that methodologists seek to defend. Paying attention to this kind of narrative gloss on practice can inform the question of why Bacon connects method to his natural philosophy, or is committed to a general method, in the first place. If Bacon's ontological assumptions about nature inform his method, rather than emerging from a content-neutral method, as Garber shows, this should inform our understanding of the larger significance of his project.

Schuster borrows Roland Barthes' term "myth" to make sense of Descartes' method, "not [as] a colloquial term of abuse", but a "second-order sign" that *naturalizes* social meanings in line with dominant societal

ideologies [Schuster, 2013, pp. 270–271; Barthes, 1973]. While Barthes' analysis focuses on how media images naturalize twentieth-century consumer culture, Schuster sees the emergence of abstract, general methods in the seventeenth-century as naturalizing new scientific approaches and *aims*.

Bacon's account of natural objects as capable of being imprinted by human action to create any kind of desired effect reflects new aims of human manipulation of nature, so it is natural that this kind of assumption would be smuggled into the account of the method that promises such control. In turn, that promise of control, and preliminary instances of it ("prerogative instances"), could be used to gain support for a Solomon's House, an aspiration the Royal Society carried forward.

Schuster argues that the seventeenth century's systematic, general methodological doctrines were significant as "perhaps the first example of a characteristically modern Western myth". Thus, despite the great difference in subject matter and time period treated by Barthes and Schuster, Schuster considers seventeenth-century methodological discourse as a significantly similar modern ideology. The ideology depends for its effectiveness on disguising the role of choice, training, and background assumptions in the conclusions reached by science, something done so effectively that twentieth-century philosophy of science had to rediscover it. Methodologies are "discourses so structured that they necessarily lie about their own powers and capabilities in the interest of turning *culture* (how the natural sciences are actually practiced) into *nature* (a simple outgrowth of human rationality and nature's amenability to it)" [Schuster, 2013, pp. 270–71].

Similar to Garber's presentation of Bacon, Schuster argues that Descartes worked out his basic ontological and methodological commitments, which he then used to "constrain and condition the formulation of specific corpuscular-mechanical models". Rather than the models being produced by the clear application of rules that generated them, Descartes carried out an "on the spot interpretation and 'negotiation,' if only with himself!" He sought out evidence to support his interpretation, "carrying out a "voicing over" or monitoring of practice" [Schuster, 2013, pp. 276, 279]. The methods and theories could then become institutionalized through embedding such monitoring in scientific pedagogy and institutional routines. The sociologist of science John Law argues for something similar in calling for "the study of method in practice", showing "how methods are staged" to demonstrate their effectiveness. [Law, 2017, pp. 31, 39].

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I have followed a similar interpretative method in looking at how Bacon's methodological writings have functioned in the work of his followers in the Royal Society. Garber puzzles over my conclusions, suggesting that I may have partially recognized that Bacon's method incorporated

substantive physical assumptions. Bacon seems aware that his attempts to implement his method in a "legitimate, chaste, and severe" way cannot be accomplished until he is supplied with sufficient institutional support to carry out the method proper.

An affirmative "liberty" or "indulgence" allows him to present to the reader a picture of the kind of goal he has in mind, a hypothetical "prerogative instance". Bacon must temporarily suspend his "rules and methods of interpretation," to be replaced by the "ordinary use of the understanding in inquiring and discovering". These "wayside inns" for the weary traveler are not taken to invalidate the method, but rather indicate the kind of explanations that will be possible once his plan for a Solomon's House is fully implemented [Lynch, 2001, pp. 10–12][2].

My discussion here is designed to show that Bacon's "inductive" method is not incompatible with the "temporary" employment of hypotheses that would otherwise be rejected. Garber takes the point further by showing how the particular physical hypotheses adopted underwrite Bacon's methodological assumption that knowledge of natural forms can lead to human ability to produce the forms at will. In other words, Bacon is constructing his picture of nature to support the maker's knowledge tradition [Perez-Ramos, 1988].

Bacon's "temporary" measure makes "hypothetical" reasoning legitimate until a community of like-minded natural philosophers can be supported and trained to carry out the method. Interest in Bacon's method grew in the 1640s and 1650s among political and religious reformers, which should tell us something about the appeal of a concept of methodological reform of knowledge at the time. What is it about the method that helps put into motion a diverse range of people looking to make the reform of knowledge central to solving the political and religious crises of the day? When the Royal Society establishes itself under the charter of Charles II, some of these reformers can come in from the cold to take part in an explicitly Baconian enterprise, though one that must manage competing interpretations of the program.

The element of Bacon's methodology that unites the new institution is a formal rejection of hypothesis for a focus on matters of fact and a valorization of objects over words. While this suffices for distinguishing the Royal Society from competitors, it picks up only one aspect of Bacon's method and smuggles in hypotheses like those associated loosely with the mechanical philosophy. Moreover, its Fellows take different interpretations of this injunction to focus on things themselves.

These "things" incorporate quite specific ontological assumptions, so are no more neutral in their approach than was Bacon. The common language disguised the difference in emphasis. Objects can be the ordinary,

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2   Note that in Lynch [2001], I quoted from the translation in [Bacon, 1860], which differs somewhat from the translation quoted by Garber.

visible objects described in the natural history reports of the *Philosophical Transactions*, the technological artifacts manipulated by artisans that model control over nature, or the underlying forms that once discovered can provide an alphabet of nature allowing us to produce any effect at will. More is going on here than a simple effort to discover new phenomena or bring the technological arts and natural philosophy together. Bacon articulates an ideology of expanded human control coupled with a self-effacing view of method that treats this control as a simple extension of nature.

At the same time, Royal Society Fellows must make do with temporary hypotheses of their own, whether Robert Hooke's postulation of a more "active" congruity of matter, hybridized with language of the mechanical philosophy, or William Petty's use of "good suppositions" in estimating population or wealth rather than "demonstrated truths" that would follow from the proper collection of economic statistics [Lynch, 2001, pp. 107–115, 202–230; Henry, 1986; Schaffer, 1987; McGuire, 1970; Petty, 1683, p. 11]. The drive for method is aspirational, with a particular "hypothetical" account of the object of study developed at the same time to inform the kind of operational control that was sought.

One possible interpretation of Garber's paper is that he agrees that Bacon developed a distinctive ideology of control of nature, demonstrated through a methodological doctrine that incorporated a view of nature as amenable to that kind of control, in turn inspiring others to do the same without strictly following his method – because he also could not follow it. In that case, the history of philosophy of science becomes part of a larger project to understand the conditions leading to emergence of certain kinds of distinctively modern discourses, practices, and ambitions going beyond the internal history of science. The alternative interpretation of Garber's paper would be to take for granted the possible effectiveness of methods in generating scientific practice, and evaluate the extent to which Bacon succeeded or failed in shaping science, and in the right kinds of ways. This interpretation would have a much harder time explaining why Bacon was unable to see the limitations of his method, even while we would overlook the significance of his project for a wider history.

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