nnatural Acts: Theorizing the Performative

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partitioning of performance into obligatory appearances and strict allowances is a complex social code assumed to be "natural" until ent notions of performativity unmasked its operations. Performance titions, strictly enforced within traditional conceptions of the arts, eground the gestures of the dancer, but ignore those of the orchestra yer, assign significance to the elocution of the actor, but not to the erances of the audience. The critical notion of performativity both eals these partitions as unnatural and opens the way for the con- eration of all cultural intercourse as performance. It also exposes the npulsory nature of some orders of performance. The oppressive uirements of systems that organize gender and sexual practices mark o may wear the dress and who may perform the kiss. Further, the nion of the dress and the colorizing of the skin that dons it are disci- ned by systems of class and "race." These cultural performances are ical sites for study.

The series Unnatural Acts encourages further interrogations of all va- ies of performance both in the traditional sense of the term and from broader perspective provided by performativity.

Choreographing HISTORY

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Tacit Knowledge, Courtliness, and the Scientist's Body

Traditionally, history and philosophy of science represented themselves as disciplines dealing with the products of minds rather than bodies. Slowly, this picture began to change as a result of the work of Kuhn and Feyerabend. Kuhn hinged his then-radical view of scientific change on "tacit knowledge"—a notion he borrowed and articulated from Polanyi and Wittgenstein.[1] Philosophers of science of the logical empiricist tradition had presented the link between the scientists' linguistic categories and the physical world as established by operationally explicit "correspondence rules." Kuhn, however, began to argue that the understanding of how scientists connect observations to linguistic categories cannot be reduced to explicit rules, but rather needs to be seen as the tacit result of their training and the "gestalt" they developed through that process.

Following a different path, Feyerabend linked radical scientific change to the development of new "natural interpretations." For instance, looking at Galileo's eventually successful attempt to legitimize the telescope as a producer of evidence contradicting the traditional Aristotelian cosmology, Feyerabend argued that the acceptance of these observations was not rooted in Galileo's ability to put forward a detailed description of the telescope's image formation through the refraction of light rays. Rather, Galileo presented an alternative "philosophical package" (and a new notion of evidence) and found a new audience for it.[2] In short, one had to be willing to accept a specific tacit knowledge (Galileo's "way of seeing") in order to trust his instrument and observations.[3] The acceptance of telescopic evidence as unproblematic was what Feyerabend called a "natural interpretation."

I would argue that it has been through Kuhn's emphasis on tacit knowledge or Feyerabend's introduction of the notion of natural interpretations that recent science studies have slowly made room for the role of the body in the production of scientific knowledge. In their initial formulation, Kuhn's and Feyerabend's categories were still more about minds than bodies. Yet, by showing that the connection between things and words was not a transparent matter of correspondence rules but relied on the training of one's perception, they began to

"embody" the mind of the scientist—though this embodiment was initially limited to the perceptual apparatus. As we will see in a moment, from Kuhn's and Feyerabend's initial emphasis on mental embodiments in terms of perceptual apparatuses and their training, historians and sociologists of science have moved toward the consideration of the experimenters' bodies and their role in the replication of experiments. By doing so, science studies have eventually encountered the problem of mapping the disciplining of the body through which tacit knowledge is acquired—a problem they share with several of this volume's contributors who focus on other forms of bodily performances.

Let us sketch this trajectory. According to Kuhn, a science student learns how to apply theories by being exposed to specific paradigmatic exemplars, that is, by being taught how to reduce different events to a specific class of phenomena—one for which the student has been given a paradigmatic solution. Through these exemplars, the students gains competence in the paradigm's linguistic categories. In short, a scientist's ability to see a specific event as belonging to a certain category of processes results from she or he having been *shown* examples, *directed* to equivalent cases, etc. It is not "natural" to assume that the laws that account for the orbits of planets work also for the trajectory of a stone thrown by a slingshot on earth. Aristotelian philosophers did not think so. That different phenomena are perceived as belonging to the same category does not derive from them being "naturally identical" but from a specific training that makes the scientist ignore what is different about them and focus, instead, on some similarities—similarities that are emblematic of that specific paradigm.[4] The perception of the identity of different phenomena is not the result of nature per se but of how nature is taxonomized by that paradigm.

To summarize, not rules but *ostensions* (something that escapes complete formalization) teach a graduate student how to become a scientist. To put it differently, it is through *practices* that students are taught how to model specific natural processes by perceiving Wittgensteinian "family resemblances" between them and the paradigmatic case study they have been exposed to during their training.[5] Finally, following Wittgenstein's argument against the existence of "private languages," Kuhn has argued that scientists cannot make explicit or fully verbalize the rules according to which they operate. In short, tacit knowledge is presented as necessarily escaping full mapping while, at the same time, being crucially important to the production of scientific knowledge. As one may expect, Kuhn, Feyerabend, and their followers have been often charged with relativism (or even irrationalism) because of this feature of their views of scientific change.

More recently, the scope of tacit knowledge has been expanded by the shift of focus in science studies from theories to *practices* and *experiments*—a shift associated also with the adoption of ethnographic methodologies within science studies.[6] In fact, the learning of paradigmatic examples (the training into perceiving family resemblances) does not occur simply through problems and exercises to be solved *on paper* but is often rooted in *laboratory practices* involving the student's ability to manipulate experimental apparatus—to "tune" it so that the "correct" outcome of an experiment will result.[7] In a sense, the connection

between a theory and the "out there" is learned by the student as she or he learns how to *move around* and operate in a laboratory (or in the field) and to tinker with instruments. Tacit knowledge is of the body as much as of the mind.

As shown by Collins's work on the role of instruments and laboratory practices in the construction of scientific knowledge, the replication of experiments often depends on people moving from the original laboratory to the one where the experiment is going to be replicated.[8] The accurate verbal description of the original experimental protocol and experimental apparatus is generally insufficient to allow others to replicate it—especially when that experiment has not yet been accepted and canonized by the community.[9] For instance, Collins has shown that the construction of the TEA laser (initially developed in Canada in the late 1960s) was not achieved in other laboratories by scientists who just read published descriptions of the device. Rather, the successful replicators were those who visited the originating laboratory and had repeated contacts with the original developers.

Also, he noticed that "scientists did not know whether they had the relevant expertise to build a laser until they tried it."[10] The replicators' skills were opaque to their own holders. It was not by putting in writing what they knew about it that they could realize whether they were "ready" to replicate it. Instead, that could be done only by actualizing their skills by constructing the device and see if it worked. Collins's thesis does not hold only for recent science. It has been corroborated with evidence from earlier periods by Shapin and Schaffer, who have argued that, in the 1660s, the successful replication of Boyle's experiments with the air pump was rooted in the replicants' direct witnessing of Boyle's pump and not just in their access to Boyle's published descriptions of his apparatus and experimental protocols.[11] All these case studies show that the knowledge necessary for the successful replication traveled with bodies and not only with texts.[12]

These different case studies indicate that it is not simply that an experiment is "right" because it can be replicated, but it also becomes replicated by being accepted as "right." More precisely, a disputed experiment becomes undisputed (that is, replicated, canonized, blackboxed) when people begin to accept the experiment itself, the apparatus with which it has been performed, and the bodily skills of the original experimenters *as the term of calibration for their own replications*. In a sense, one has to accept an experiment before one can reproduce it. Of course, the power and credibility of the person or team that proposes an experiment has much to do with its acceptance and replication by others. Similar power dynamics are to be found in the case of the physics graduate students described by Kuhn who accept the ostensions provided by their teachers or textbooks (and the tacit knowledge that goes with them), also as a result of the authority they attribute to those sources.

In short, these studies of the closure of scientific disputes through the replication of blackboxing of an experiment and its apparatus hinges on the notion of skill, a body-related version of tacit knowledge. In a sense, one party's ability to have one's experiment blackboxed depends on that party's ability (or power) to have its skill accepted as the term of reference for the experiment's replication rather than by having it fully spelled out in terms of explicit rules of behavior. If one expected experiments to be replicated by having their proponents spell out

fully the experimental protocols, one would get in a deadlock; skill (like tacit knowledge) is something that cannot be made fully explicit. This is what Collins has called the "experimenter's regress."[13] To avoid it, one has to trust the experimenter's body and its skill, its bodily tacit knowledge. Once one's skill is accepted, it becomes part of the "bodily canon" of the branch of science to which that experiment belongs, very much like a certain form of tacit knowledge (in the Kuhnian or Feyerabendian sense) becomes the unspoken foundation of a given paradigm or "natural interpretation."

More recently, Collins has explicitly articulated the cognitive dimensions of skill and tacit knowledge by tracing their implications into the debate over the cognitive limits of artificial intelligence. One of the most powerful arguments about the limits of artificial intelligence was developed by Hubert Dreyfuss, who drew on the work of Wittgenstein.[14] Wittgenstein had argued that a member of a given language game could not fully verbalize the rules underlying that game. As we have seen, this argument was also at the base of the notion of tacit knowledge and skill as used by Kuhn, Feyerabend, and Collins. Dreyfuss moved from Wittgenstein's argument and claimed that one of the structural limits of artificial intelligence (and in particular of so-called expert systems) was that it relied on the codification of human cognitive skills into a computer program. Because the human expert could not produce a complete codification of the rules according to which he or she operated, the computer was bound to run an "incomplete" program. Wittgenstein's thesis provided Dreyfuss with an a priori argument about the computer's inability to simulate a human expert's "tacit knowledge" by showing that the transfer of expert knowledge from humans to computers was bound to be incomplete. One could say that the computer could not be as "smart" as a human not only because of its limits in simulating the behavior of the human "mind," but also because it could not replicate the expert's "bodily knowledge."

Moving from this scenario, Collins has argued that the roots of the limits of artificial intelligence are sociological as much as philosophical. Basically, what the computer can reproduce are those forms of human cognition that have been *socially* blackboxed.[15] Once a human cognitive activity has become "solidified" or "canonized" because of social pressure and reinforcement, then the skills required for its performance become increasingly standardized and transferable both to a wider sector of social actors and to a machine. We have seen that after the closure of a scientific dispute, the experimental apparatus involved in that dispute becomes increasingly reproducible. Eventually, it becomes blackboxed to the point of incorporating much of the skill necessary for its "proper" operation. It becomes "user friendly". Collins's argument about artificial intelligence hinges on his treating a computer as a scientific instrument being operated after the closure of a dispute. Therefore, when it works in simulating human cognition, the computer does so not so much because it has a "smart" architecture or a powerful processor but because it incorporates social practices and bodily skills that, by that time, have become "unproblematic". Paradoxically, it is a fully socialized nature that can be "replicated" by a computer.

To summarize, as beliefs in simple correspondence rules have left the stage, the considerations about the body and its training have entered debates in science

studies and philosophy of the cognitive sciences. However, this is true more in principle than in practice. In fact, by looking at recent science studies literature (which tends to concentrate on recent or contemporary science) one is struck by how little "body talk" is to be found there. True, we have much interesting work on the scientific construction and representation of the body (Foucault, Laqueur, Schiebinger, Jordanova, etc.), but we find very little on the disciplining of the *scientist's body* and on the cognitive implications of this process.[16] While the spotlight is no longer on the metaphysical foundations or formal aspects of scientific theories but on body-centered or body-dependent categories like practices, instruments, experiments and laboratories, the research still tends to emphasize the mentalistic rather than bodily dimensions of tacit knowledge. The inherent elusiveness and opacity of skill is creating formidable problems to its study.

This neglect is probably related both to the ephemeral nature of the traces left by the body's skill and to text-oriented or quantitative interpretive tools typical of historians, sociologists, and philosophers of science. While it is impossible to trace changes in the scientists' mental categories and infer shifts in their tacit knowledge by taking a diachronic look at scientific literature, it is much more difficult to write a history of the changing skills that went into replications of experiments and in the production and calibration of experimental apparatus. While scientific articles (and sometimes manuscript papers) are often accessible, a history of instrument-building, replications of experiments, and the shifting tacit knowledge that went with them tends to become an often impossible archeological feat because it is usually impossible to go back and find or operate the original instruments in their original setting and setup.

These empirical problems may be somewhat less severe when we study a period in which the scientists' protocols of argumentation and behaviors had not yet conspicuously differentiated themselves from surrounding cultural practices. After the scientists' professional culture became specialized (and therefore local), a historian faces a situation in which the evidence about and traces of the scientists' skills are limited to the artifacts and texts of that community. Instead, if we study early modern scientists active within the culture of the princely court or of gentlemanly circles, we can utilize a much wider range of evidence about the bodily skills—skills that were integral to their self-fashioning as natural philosophers and that framed their argumentative and experimental performances.

Together with other historians of science, I have begun to argue that the legitimation of the new natural philosophy in the seventeenth century was rooted in its practitioners' adoption of forms of argumentation and presentation of the self consonant with courtly etiquette and gentlemanly codes of behavior.[17] Because of this homology between the bodily and discursive culture of the court and that of natural philosophy, and because of the distinctively performative character of science in this period (experiments were often seen as spectacles and philosophical disputes tended to be perceived as courtly theatrical events), we can draw from the many etiquette handbooks and court treatises to reconstruct some aspects of the scientists' "performances" and the tacit knowledge in which they were rooted.

There is a point that needs to be stressed to understand how this literature can be brought to bear on the reconstruction of the scientists' skills: the homology between courtly discourse and presentation of the self. Court literature shows that one's "gracious conversation" was the oral analog of one's elegant presentation of the self.[18] Both expressions were instances of one's "courtliness" and were evaluated according to similar codes. For instance, being badly dressed or behaving awkwardly was read as a sign of uncourtliness, and one who was badly dressed or displayed an unelegant demeanor was expected to be a pedant, a bore, and a coarse speaker. An elegant demeanor and an elegant conversation were signs of one's "virtue" (the essence of courtliness). This "virtue" (also referred to as *sprezzatura,* that is, nonchalance) was, like tacit knowledge, something that was not fully reducible to a set of explicit rules of behavior. While etiquette books provided useful guidelines, one needed to have some natural gift, some *je ne sais quoi* to allow him or her to become a "natural" courtier.

As shown by Castiglione's *Book of the Courtier,* one could not fully describe what *sprezzatura* or virtue was about. For instance, Castiglione could not (or chose not to) offer a *definition* of courtliness but could only present a fictional conversation at court in which the participants agreed to play the game of "forming in words a perfect Courtier."[19] His was not a treatise that spelled out the courtier's tacit knowledge but rather a fictional description of a parlor game about the ideal *representation* of the courtier. However, that Castiglione did not present his work as a textbook of etiquette was not just the result of the elusive nature of the courtier's tacit knowledge. That court *sprezzatura* could not be spelled out was crucial to its social effectiveness. Its elusiveness allowed it to be presented as a mysterious quality not unlike nobility: Either you had "it" or you did not. As with Collins's argument about how the elusiveness of tacit knowledge created a space in which power could play and eventually bring a scientific dispute to closure, the opacity of *sprezzatura* was central to the prince's self-representation as somebody who could "certify" true courtliness. But being a perfect courtier meant to be a perfect, "natural" subject—somebody who happened to have the right subject-ivity. In fact, to have the right type of subjectivity meant to have the "right attitude" about being a subject of the prince.

Therefore, the opacity of *sprezzatura* allowed the prince to set the standard for the perfect behavior of the subject (one that would confirm his identity as prince) by giving "ostensions," not rules. The prince alone could "calibrate" his courtiers. To spell "out" those rules would have been equivalent to giving "away" the definition of sovereignty—something the prince could not and did not want to do. Thanks to the opacity of *sprezzatura*, the prince could represent himself as powerful without really saying why he was so. In a strong sense, the opacity of *sprezzatura* allowed for the representation of sovereignty as a "mystery of state."[20]

In short, behaving and speaking in a courtly fashion was not just a "professional skill" but rather the demonstration that one was a person worthy of that title—a legitimate subject of power. Non-courtiers were, in fact, not seen as "persons" but rather as members of the "masses." They too were subjects, but did not participate in the construction of the power-image of the prince. Courtly

nonchalance was not a matter of rhetorical style, but it was a way of constructing oneself or, more precisely, of representing oneself as having the only acceptable type of subjectivity. This was a prerequisite to having one's claims taken seriously. While, in later science, credibility became connected to professional training, institutional affiliation, and peers' recognition, the natural philosopher operating within the court or aristocratic culture constructed her or his credibility by displaying "virtue." Therefore, to look at these non-verbalizable modes of presentation of the self is not just to study the scientists' "literary style" but the discoursive "I do not know what" that provided the *conditions of possibility* (in the Foucauldian sense) of their claims about the natural world.

The pervasive and constitutive role of *sprezzatura* and gentlemanly politeness in seventeenth-century natural philosophy emerges from a variety of sources. For instance, Shapin and Schaffer have argued that Boyle's experimental philosophy was predicated on gentlemanly etiquette. The experimental philosophers' ban on the pursuit of final cause and their commitment to a scientific discourse that interpreted empirical evidence without invoking comprehensive philosophical systems (like Aristotle's) aimed at avoiding dogmatism that could lead to bitter, honor-tainting disputes. This concern for the maintenance of proper gentlemanly interaction among the certifiers of scientific claims was codified in their notion of "matter of fact."

A matter of fact was not just a fact. It was a specific claim constructed through an appropriate "etiquette of inquiry." By being a local claim unconnected to dogmatic philosophical systems it did not threaten the honor and status of the gentlemen involved in its construction and therefore allowed them to certify it. It was "true" precisely because it fit gentlemanly etiquette. I would say that the new notion of evidence introduced by experimental philosophy was a form of "solidified politeness" in the sense that the "solidification" of a matter of fact resulted from its unthreatening features, which allowed the gentlemen to argue politely and, by doing so, eventually "close" it.[21] In a sense, the turning of a claim into a "matter of fact" rested on the blackboxing of the gentlemen's tacit knowledge as gentlemen. A claim would become a matter of fact only through the gentlemen acting like gentlemen around it. A matter of fact was a "gentlemanly disciplined" form of evidence.

Galileo provides further examples of how courtly *sprezzatura* was central to the acceptance of his scientific claims and practices. As we have seen, courtiers differentiated themselves from the masses by possessing virtue or *sprezzatura*. Such a virtue was routinely opposed to "pedantry," which was seen as the sign of one's technical and therefore low-class background. To be a pedant meant to have dogmatic opinions, to be unable to argue as a "free-thinker," to be slave to a philosophical system, to be unable to "play." In short, somebody who sought final causes or was prisoner to a philosophical system did not display the "intellectual nonchalance" required of a courtly cultural performer. While some of these issues informed Boyle's development of a noncontentious and polite experimental philosophy, they were used by Galileo in a more aggressive fashion. His recurrent attacks on the Aristotelian philosophers and the Jesuits rested on the

social trope of the pedant—a trope that gained its power from its role within court culture.[22] He was not just a "modern" fighting the "ancients." His attack on ancient authorities was not just in the name of the "modern" science but also in the name of court culture.

The new natural philosophy he was elaborating was one predicated on courtly *sprezzatura*. The ancients (and those who saw them as authorities) were not just wrong, they were "uncool." Galileo's adoption of a nonchalance-based style was crucial because, as in the case of his attempted legitimation of Copernican astronomy, he often could not offer conclusive proofs for his specific claims nor could he justify philosophically the superiority of his method over that of the "ancients." Consequently, to some extent, Galileo's success rested on his supporters' buying into his "language game," that is, into *believing* in his agenda (if not in all his specific claims) rather than in asking for metaphysical arguments about the soundness of his method. In a strong sense, the "tacit knowledge" of the courtiers became the "tacit knowledge" in which Galileo's "form of scientific life" was rooted.

The reception of his astronomical discoveries provides an example of this process. As we have seen, Feyerabend has argued that the acceptance of Galileo's telescopic evidence signified the adoption of a different tacit knowledge. I would add to this that courtly *sprezzatura* played a crucial role in the process. Courtiers and gentlemen were likely to accept Galileo's instrument and the evidence it provided also because their culture may have prevented them from asking the questions that were likely to problematize the credibility of the telescope. As mentioned, Galileo could not provide a satisfactory explanation of the process of image-formation through the telescope. But the exhaustive description of the optical processes underlying the behavior of the instrument that philosophers and mathematicians would have expected from Galileo was not something that courtiers were likely to appreciate. That type of discourse was of mathematicians and philosophers, not of courtiers. They tended to perceive that discourse as technical and therefore "pedantic." That Galileo could not provide such an exhaustive set of "correspondence rules" did not seem to be a serious handicap for courtiers. Actually, they were not culturally disposed to even ask for that. What Galileo was offering them through the telescope was not just a "marvel" (something that fit the courtiers' taste) but also a notion of evidence that was not pedantic. Galileo's was "cool evidence." If the closure of Boyle's "matters of fact" rested on gentlemanly (unspeakable) politeness, what allowed for the closure of the dispute over Galileo's telescope was also the tacit knowledge of the courtiers—their "virtue" and *sprezzatura*.[23]

It may seem that what I am discussing in the case of the courtiers is something different from the tacit knowledge and skill encountered in Wittgenstein, Kuhn, Feyerabend, and Collins. The answer is both yes and no. Of course, in the case studies presented by Collins and his followers, we find a very circumscribed form of skill connected to the replication of an experiment or to the reproduction of a piece of apparatus while, in the case of the court, the skills entailed by the new natural philosophy appear "softer" and usually connected to forms of argumentation rather than to "hard" practices like tinkering with experimental

apparatus. However, underneath these important differences we find that courtly demeanor and gentlemanly mannerism framed the conditions of possibility of Galileo's and Boyle's "forms of life" in ways that are quite similar to the way in which modern paradigms, natural interpretations, or the replication of experiments rest on tacit knowledge.

The differences among these forms of tacit knowledge result not only from the increasing specialization of scientific culture, but also from the different regimes of power in which seventeenth-century natural philosophers and modern scientists operated. For instance, Collins has argued that power plays an important role in the closure of scientific disputes and in the consequent blackboxing of specific claims. The tacit knowledge that gets blackboxed is the one that fits (and reshapes) the power field of that scientific community. Collins's work (like that of Kuhn) rests on the assumption of the scientific community having some degree of separatedness (though not necessarily of isolation) from society at large.[24] The language game they consider is that of a quite specialized "tribe"—one that has developed peculiar forms of tacit knowledge. Instead, in the case of Boyle and especially Galileo, the field of power in which their discourses developed was still quite undifferentiated from (or at least porous to) the broader regime of power that framed "high" culture in general. For instance, Boyle's "form of life" was directly framed by the political discourse of seventeenth-century England, while Galileo's courtly scientific discourse was largely framed by the discourse and power regime of political absolutism as expressed by court culture.[25] Therefore, the different forms of tacit knowledge and subjectivity we encounter in the seventeenth and twentieth centuries are genealogically connected by a long process of professional differentiation and disciplining.

In recent years, the disciplining of subjectivity has been seen as emblematic of the development of modernity. Although the main elements of this view can be traced back at least to Weber, it is in the work of Elias and Foucault that we find a fuller development of this perspective. However, Foucault has claimed that such a disciplining of subjectivity did not apply to the mathematical sciences. In a sense, his concern with the development of the life, medical, and "human" sciences focused on their role in the disciplining of the subjectivity of those who are *subjected* to power rather than on the way in which the holders of this knowledge were disciplined themselves by the same process. Not the scientist but the insane, the criminal, and the medical patient are the center of Foucault's attention. I would argue that it is because of his lack of emphasis on the scientist's body that Foucault has found himself unable to see how disciplining was crucial to the development of the modern physical sciences.

Although the work of Elias does not deal directly with the production of scientific knowledge, some of his reflections may help correct this blind spot in Foucault's model. Elias offered a comprehensive view of the development of modernity by relating it to the disciplining of feelings and behaviors that resulted from what he saw as the increasing interconnectedness among the members of society. This pattern was exemplified by the long transition from the feudal system, characterized by loosely connected local lords and kings, to the highly centralized court of an absolutist prince like Louis XIV (an institution he saw as

the forerunner of the modern state). From this evidence, Elias argued that the development of increasingly polite manners and intricate court etiquette resulted from the need to have many powerful people (the king and the court aristocrats) interact at increasingly close quarters.

However, it is not that undisciplined behaviors would have been simply "unpleasant": They would have disrupted the collective process through which court society held together and framed the construction of identity of its members.[26] This, I think, is homologous to how impolite manners would have disrupted the certification of "matters of fact" by disrupting the gentlemanly identity of those involved in that process. Elias argued that the sense of individual identity experienced by courtiers was a social construction: To be an individual meant to be a highly socialized being—something that could be achieved only in a closely interconnected society. In a sense, Elias provides an important complement to Collins. While Collins has argued that the "hardness" of claims about nature results from their being very social, Elias indicated that the "hardening" (or blackboxing) of one's identity was rooted in its being tightly constructed through intense and continuous interaction with other people on whose confirmation that sense of identity rested.[27] Moreover, what was being produced through these interactions was not just individual identities but the prince's power as well. Power was not just the external cause of the courtiers' self-fashioning but also its result.

Elias's work suggests how the processes of the "blackboxing" of subjectivity and of claims about nature are produced through homologous processes through which power and status (and therefore credibility) were constructed. Self-fashioning, world-fashioning, and the construction of authoritative discourse were all components of an interconnected process. Consequently, I would suggest that the emergence of modern science may also reflect a disciplining process that has led to a very specific form of bodily and mental "etiquette" within the context of an increasingly interconnected and authoritative scientific community. It is the specificity of this "etiquette" and of the related regime of power and credibility that may account for the specific characteristics of scientific knowledge.[28]

In a sense, a scientist's manipulation of an instrument to have it yield meaningful results may not be unlike the bodily skills expected of a courtier to achieve the appropriate presentation of the self. Similarly, a scientist's reading of a given experimental report as confirming or refuting a given theory (one in which her or his professional identity and career may be at stake) may be commensurable to the processes by which a courtier reads the signs of favor, interest, or coldness as they are conveyed by the demeanor of her or his fellow courtiers. Both perceptions result from specific forms of disciplined subjectivity and bodies rooted in specific regimes of power. Both cases exemplify a culturally constructed behavioral response (one that cannot be reduced to unambiguous algorithms) that relates one's subjectivity to the signs encountered in his or her power-laden socio-professional environment. While the regimes of power and the related social structures changed remarkably from the seventeenth century to today, the processes that linked self-fashioning to world-fashioning are comparable. In short, courtly *sprezzatura* (the result of court disciplining) and modern scientists' tacit knowledge (the result of professional disciplining) result from, I believe, analogous processes.

I would like to conclude by going back to the problem of mapping the tacit knowledge in which claims about nature and the replication of experiments are rooted. Previously, I argued that this task may be easier in the seventeenth century than in the twentieth because of the relative lack of differentiation between the tacit knowledge of the natural philosophers and that of gentlemen and courtiers, and because of the wealth of texts on courtly and gentlemanly behavior. While I would still maintain that point, there is a caveat that needs to be addressed. In particular, the differences between these two historical periods rest on the quantity rather than the quality of evidence available about them.

As we have seen, the verbal description of experimental apparatus and protocols has been shown to be insufficient for the replication of a claim before closure is achieved. Similarly, we find frequent remarks in court literature that how-to books of court etiquette were poor vulgarizations of "real" courtliness and that one who relied on them exclusively could not achieve real *sprezzatura*.[29] However, this is the point of view of those who needed to defend the uniqueness of their social identity by declaring it irreproducible by the masses. Quite probably, etiquette textbooks were useful (at least to some extent) to those who wanted to fashion themselves as courtiers in the same way that, after closure, a textbook becomes a powerful resource for the professional initiation of a "novice." In short, in both cases the problem is not with the essential limit of texts in conveying bodily knowledge, but rather in how blackboxed that bodily knowledge was by the time it was inscribed in a text. Etiquette textbooks were probably reliable guides about "mainstream" courtliness, though they may have been insufficient to convey the "cutting edge" in courtly behavior. As we have seen, similar considerations apply to the ability of scientific textbooks to provide the proper "ostensions."

Consequently, while etiquette textbooks allow us to get a more detailed picture of the tacit knowledge of, say, Boyle or Galileo, the basic interpretive problem is left unsolved. Historians of tacit knowledge (whatever the period they may be investigating) can study tacit knowledge after it has been reified either in a text or in a device. This is not simply to say that the observers' knowledge is always a posteriori. If that knowledge has not been blackboxed, a posteriority cannot guarantee access to it. Also, as we have seen, being a participant does not allow one to spell out his or her tacit knowledge either: One can perform it, but full verbalization is bound to remain another matter.

NOTES

1. Polanyi, *Personal Knowledge*; Wittgenstein, *Philosophical Investigations*.
2. The law of refraction had not yet been formulated and, moreover, Galileo did not seem to be familiar with the leading text on optics, Kepler's 1604 *Ad Vitellionem paralipomena*.

3. Feyerabend, *Against Method*.

4. Actually, the notion of tacit knowledge problematizes the very concept of similarity. In fact, once one argues that the perception of similarities is the result of training, the very notion of similarity rooted in inherent features of nature becomes problematic.

5. Kuhn, "Second Thoughts on Paradigms," pp. 293–319.

6. See, for instance, Latour and Woolgar, *Laboratory Life*; Knorr-Cetina, *The Manufacture of Knowledge*; Latour, *Science in Action*; Lynch, *Art and Artifact*; Traweek, *Beamtimes and Lifetimes*; Shapin and Schaffer, *Leviathan and the Air-Pump*; Galison, *How Experiments End*.

7. Kuhn, "The Function of Measurement," pp. 178–224. See also, Atkinson and Delamont, "Mock-Ups and Cock-Ups," pp. 87–108.

8. Collins, "The TEA Set," pp. 165–86; "The Seven Sexes," pp. 205–224; *Knowledge and Controversy*, pp. 3–158; Gooding, *The Uses of Experiment*.

9. Collins, "The Seven Sexes," pp. 207–208; Shapin and Schaffer, *Leviathan and the Air-Pump*, chaps. 5–6.

10. Collins, *Changing Order*, p. 56.

11. Shapin and Schaffer, *Leviathan and the Air-Pump*, chap. 6, "Replication and Its Troubles," pp. 225–82.

12. As a counterexample, Schaffer has looked at a case in which the original experimenter was not forthcoming in sharing information about his experimental protocols and apparatus but offered "generic" descriptions and did not allow for a public witnessing of his experiments. This was the case of Newton's prism-based work on the decomposition of white light into its constitutive colors. Because of Newton's reluctance to transfer his "skills," the debate did not reach quick closure but eventually forced Newton to withdraw from it to avoid accusations of having produced experimental artifacts. Closure was achieved only sometime later when Newton managed to "canonize" his experiment through a different path, that is, by deploying his newly acquired power as president of the Royal Society. See Schaffer, "Glass Works"; Gooding, *The Uses of Experiment*, pp. 67–104.

13. Collins, *Changing Order*, pp. 129–57.

14. Dreyfuss, *What Computers Can't Do*.

15. Collins, *Artificial Experts*.

16. A partial exception to this is Simon Schaffer's intriguing "Self Evidence," pp. 327–62. However, after addressing very clearly the connection between the problem of the disciplining of the scientists' bodies and that of other subjects of power (along the lines provided by Elias), Schaffer's narrative does not analyze this connection in the seventeenth and eighteenth centuries but focuses on what he takes to be a crucial turning point in the early nineteenth century: the systematic transfer of tacit knowledge from scientists to instruments. In short, he does not focus on the disciplining itself but on its problems and their eventual "solution." On the disciplining of the practitioners' bodies, see also Graham Gooday's piece, forthcoming in *Technology and Culture*, on the laboratory-based training practices of late-nineteenth-century English physicists, and the special issue of *Social Epistemology* on "The Historical Ethnography of Scientific Rituals."

17. Shapin, "The House of Experiment"; "A Scholar and a Gentleman," pp. 279–327; Biagioli, "Scientific Revolution," pp. 11–54; "Absolutism, the Modern State, and the Development of Scientific Manners"; Daston, "Baconian Facts," pp. 337–63.

18. Arico, "Retorica barocca come comportamento," pp. 338–56.

19. Castiglione, *The Book of the Courtier*, p. 25.

20. On this issue, see Marin, *Portrait of the King*; Kantorowicz, *The King's Two Bodies*; and Biagioli, *Galileo Courtier*, chap. 1.

21. Biagioli, "Scientific Revolution," pp. 32–39.

22. Biagioli, *Galileo Courtier*, chaps. 2, 5.

23. Biagioli, *Galileo Courtier*, pp. 90–101.

24. Collins's notion of "core set" is crucial here. See his *Changing Order*, pp. 142–67.

25. On this aspect of Boyle's work, see Shapin and Schaffer, *Leviathan and the Air-Pump*. On Galileo's science and the discourse of absolutism, see Biagioli, *Galileo Courtier*.

26. For a concise description of Elias's thesis, see Roger Chartier, "Social Figuration and Habitus," pp. 71–94.

27. What I am saying about Collins applies to Latour's model as well.

28. Because someone may think of this as a Latourian statement, let me point to a crucial difference. Bruno Latour, in *Science in Action*, has tried to describe the difference between modern Western science and traditional or non-Western forms of knowledge as something along the lines of Jack Goody's argument in *The Domestication of the Savage Mind*. That is, he has stressed the different devices (writing, printing, imaging systems, experimental apparatus, and laboratories) that broadly demarcate these two sets of cultures. The differences in these two types of knowledge are largely the result of the different ways people operate and think in environments populated by very different sets of knowledge-making devices. However, because neither the self nor the body play a relevant role in Latour's picture, he has not considered the "disciplining" of the scientist's body as playing any role in the workings of science. Technological developments *per se*, not the disciplining of the body (and mind) in a framework characterized by those developments, is Latour's key to understanding the workings of science.

29. See, for instance, Whigham, *Ambition and Privilege*.