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Boundaries, Contexts, and Power

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From Relativism to Contingentism

Although routine bashing of whiggish history of science has been among the favorite sports of science historians since Butterfield, we may be now witnessing something of a return of the repressed. Reviewers of recent works in the history and sociology of scientific knowledge have called attention to the emergence of "whiggish social studies of science," while some historians of science display their presentist frame of reference in their printed work and discuss it more explicitly in informal settings.[1] In the last decade or so, other historians and philosophers of science had already discussed "presentism" or "present-centered history" as appropriate approaches to the study of past science.[2]

The relativist historians' "neo-whiggism" seems methodologically equivalent to the sociologists' frequent bracketing-off of what they take to be the dangers of so-called reflexivity. Many historians of science and sociologists of scientific knowledge who take a relativist stance in examining the belief systems and practices of the scientists they study end up writing their interpretations from a more or less unacknowledged nonrelativist frame of reference.

Differently from philosophical analyses of reflexivity that approach the issue in terms of its methodological implications, I want to consider the emergence of "neo-whiggism" and the sociologists' frequent bracketing-off of reflexivity as possible indicators of the current socioinstitutional predicament of science studies and of its practitioners' cultural and professional identities. I believe that, if properly contextualized, the issues raised by the reflexivity debate in sociology and by the limits of the historians' ability to understand the past "on its own terms" do not need to deepen the anxieties of relativist historians and sociologists about the cognitive status of their disciplines. Instead, the reflexivity debate may provide a useful starting point for avoiding the deadlock that has characterized the rationality/relativism debate in recent years.

Interpreters in Hiding?

The last few years have witnessed the emergence of a range of reflections about the interpretive limits of the social sciences—history and ethnography in particular.[3] However, these debates do not seem to have yet influenced mainstream historiographical or anthropological practices. Quite to the contrary, they have been often ignored or cordoned off by historians and anthropologists in an attempt to prevent the double-edged sword of relativism from being turned against their own work. With some notable exceptions, science studies too have avoided or tried to control similar methodological issues.[4] As Malcolm Ashmore shows, sociologists of scientific knowledge usually tend to dismiss, exorcise, or simply pay lip service to the implications of reflexivity.[5] Similarly, judging from the very limited debate on reflexivity within history of science, its practitioners (myself included) keep plying their craft without losing much sleep over that issue.[6] As we proudly repeat the methodological dictum of our discipline ("You shall interpret the past on its own terms") we do not seem to spend much time analyzing the major tensions barely hidden underneath such an optimistic professional credo.

Because of its methodological sophistication, some of the recent work on scientific discoveries offers a good example of the historians' routine effacing of their own frame of reference. These studies argue that the scientists' historical narratives about their discoveries are rooted in and help stabilize the very closure of the debates through which those discoveries had become legitimized (and then passed into history) as such. In short, those narratives are constitutive of (and constituted by) the scientific facts they are about. However, while the historians contextualize the scientists' own accounts of discoveries by relating them to the dynamics of the debates and of the structure of the communities or networks involved in them, the historians' narratives are not usually presented as being affected by comparable processes.[7]

In history of science, this routinized avoidance or boxing of reflexivity is particularly intriguing because of the higher epistemological status that history of science was thought to have gained at least since Kuhn's *Structure*. If science is now often seen as a process whose dynamics can be understood through its history (or, more generally, by looking at it in "action"), then one would have expected that the features and limitations of historical interpretations of science should have received extensive attention by historians. Instead, these issues have been addressed almost exclusively by philosophers like Kuhn and Feyerabend, who, because of the historical nature of their theories of scientific change, could not avoid confronting the problem.[8] Although reflexivity becomes an issue whenever one recognizes cultural differences between his or her own culture and that being studied, the problem emerges more explicitly when—as argued by Kuhn and Feyerabend—one may encounter incommensurability between the belief systems of the various historical actors or between those of the historical actors and the historian studying them.

Both Kuhn and Feyerabend have argued that it is possible to circumvent incommensurability and reconstruct alien worldviews either by becoming bilingual (Kuhn) or by enriching one's language (Feyerabend). However, I believe that their assessment of the implementability of their interpretive guidelines has been overly optimistic. For instance, it may be telling that Kuhn did not present a sustained analysis of the implications of incommensurability for the writing of the history of science, but introduced the issue by means of an autobiographical reminiscence.[9] The tale of young Kuhn coming to terms with the incommensurability between Aristotelian and Newtonian physics through a sudden "conversion" was offered as a living and true exemplar of the general process being discussed. Quite fitting with Kuhn's own view of scientific change, his conversion narrative did not present explicit rules about how to do history of science in an environment of incommensurability, but seemed to suggest that what one needs is an unverbalizable skill—the historian's "tacit knowledge." However, the vividness of Kuhn's exemplary conversion tale may hide some of the sociocultural conditions that frame the possibility of experiencing such a gestalt switch.[10]

Therefore, while the "neo-whig" relativist historians tend to gloss over their presentist point of view by making it "tacit," Kuhn invoked some kind of "tacit knowledge" to argue for the possibility of a nonpresentist and fully contextualized understanding of past science. As I hope to show, we may not need (or want) to rely on such opaque categories or stances.

Locating Historians of Science

In the 1960's, Kuhn's and Feyerabend's appreciation of the otherness and the legitimacy of old scientific worldviews reflected an innovative stance within history and philosophy of science. Today, especially in the wake of "multiculturalism" and its reception in academia, beliefs in the specificity and legitimacy of the "other's" culture and in the necessity of understanding it on its own terms are quite commonly held (or at least professed) by academics in the humanities and social sciences.[11] Such beliefs convey the comforting feeling that, if approached with a relativistic attitude, the interpretation of "the other" (historical, ethnic, cultural, etc.) is going to be "fair" and that dialogue (or at least consent) among different cultures may be achieved—at least in principle.

By avoiding any privileged point of view, academics suggest that relativism would keep clear from the dangers of hegemonic discourse. While relativism provides important tools to denaturalize claims and beliefs and to expose the role that power, domination, and other unpleasant dynamics may have played in their establishment and maintenance, relativist academics are often believed to be immune from these dangers because of the reflexivity they develop by practicing relativism. A relativist may expose the workings of power but should be able not to reproduce them in his or her historical interpretations. As epitomized by the transition from the "melting pot" to the "salad bowl" as metaphors for national identity in the United States, relativism's methodological and political "civility" has been a crucial tool for promoting respect for other cultures within academia and without.

However, the potential for cultural criticism is not an essential quality that belongs naturally to certain methodologies while being *a priori* alien to others. Methodologies are resources that yield different results when they are deployed in different cultural, political, and institutional environments. For instance, while relativism can be a powerful critical tool when deployed by a marginalized group against a hegemonic discourse, it may yield very different results when used by the status quo in a "democratic" environment as a way to delegitimize minority claims.[12] Symmetrically, while rationalism has been politically critical in the past—as when it was used in an attempt to delegitimize societies structured around birth-related privileges—it can, in other contexts, lend legitimacy to hegemonic master narratives. In short, both rationality and relativism may, when employed in suitable environments, lend legitimacy to existing forms of power. The crucial difference lies mostly in the different ways they do so.

As Donna Haraway has recently argued, rationalists speak as if they were "nowhere while claiming to see comprehensively."[13] Instead, she sees relativism as "a way of being nowhere while claiming to be everywhere equally." As she puts it, both stances are "god tricks": one claims not to be speaking from a specific, identifiable place and pretends, instead, to be able to evaluate the matter *globally*—either by being *everywhere* or by seeing *everything*.[14] Haraway's proposal for avoiding both "god tricks" is to think in terms of situated, partial knowledges. While her proposal is itself situated, as it reflects her perception of the predicament of feminist science studies, I believe that the proposed shift to situated (rather than simply *local*) knowledge is one that is relevant to other constituencies and that it may provide an important starting point to overcome the epistemological (and political) shortcomings of both relativism and rationalism.

As I understand it, the main difference between local and situated knowledge is that the latter is not presented as a knowledge that a given group simply "happened" to develop. Rather, situated knowledge is something that is produced through being located in a certain position that allows for a *specifically* partial perspective. Consequently, although such knowledge may well be *partial*, it would not be *arbitrary*. Moreover, being partial is no sin, because the belief that accurate knowledge can be produced only through global perspectives (seeing *every*thing or being *every*where) is shown to be maintained only through the *ad hoc* introduction of god tricks. These are aporias resulting from the assumption of the possibility of *global* knowledge (in either its logocentric or its relativistic brand). Therefore, the shift to partial knowledge proposed by Haraway is not a gesture of epistemological retreat but rather the result of the awareness of how knowledge is *necessarily* produced through partial perspectives.

Trying to apply some of these considerations to the current methodological practices of historians of science, I see the emergence of "neo-whiggism" in history of science (or the avoidance of reflexivity in sociology) as an important (though implicit) admission that, in the end, our god tricks do not work. Relativist historians of science cannot be "nowhere while claiming to be everywhere equally." Quite to the contrary: we are anchored in (or stuck with) our present, and our location imposes specific limits to our understanding of the historical "other." In short, despite frequent reassuring-sounding claims to the contrary, we cannot understand the past on its own terms. As indicated by the implicit assumption of a presentist point of view in some of the recent historiography, our allegedly global respect of the historical "other" is bound to break down.

This, I think, is no simple failure of "methodological nerve." By boxing the reflexivity question or by writing implicitly "neo-whig" history we are doing more than simply effacing what we may perceive as a problematic feature of the relativistic framework. Rather, such a defense of relativism might also reflect an attempt (though not necessarily a conscious one) to defend a discourse that, after a "subversive" past, has recently developed symbiotic ties with the university, which, especially in the United States, has become interested in representing itself as an institution producing pluralistic, fair, and nondogmatic culture. If in the past being a relativist put one at risk of being represented as a science hater anarchonihilist, now the same stance may allow one to fashion himself or herself as an interpreter whose relativistic impartiality legitimizes him or her as a reliable interpreter of all the many tensions, struggles, and negotiations that characterize the workings of science and society.[15]

In short, by being relativists, we may be also defending our identity as university-based social "scientists"—a new kind of expert for a new kind of socioinstitutional environment and cultural agenda. For example, on 3 June 1992, in a radio program about the Los Angeles "riots" on KCRW (a Santa Monica–based, NPR-affiliated radio station), a relativist academic was introduced as "an expert in cultural diversity."[16] When placed in the contemporary academic context, relativism provides a venue for representing oneself as an "objective" interpreter—that is, as somebody whose comprehensive perspective does not derive from one's panoptical (and therefore hegemonic) vision, but from "being everywhere" and therefore "understanding everybody." We are no longer after who is right or wrong: now we can understand science as a process or as a form of "action," something that is neither good nor bad but simply *is*.

When practiced by academics, relativism tends to transform the institutional locatedness of their knowledge into a "politically correct" method—one that gives credibility to the interpreter while legitimizing the host institution as a place where nonhegemonic, nondogmatic, other-friendly discourses are developed. Through these discourses, the university contributes to the mythologies of consent, fairness, and respect of difference on which modern participatory democracies are deemed to be based. By this I am not trying to suggest that relativism is politically problematic in a general sense and that, consequently, it does not have anything else to contribute to cultural and political debates.[17] As we know, the respect of other cultures is far from being something we can take for granted. Moreover, here I am not talking about the uses of relativism by marginalized or oppressed groups, but rather those produced by constituencies operating in allegedly legitimate and legitimizing institutions such as universities.

To summarize, I suggest that by "locating" the historians and sociologists of science (and making explicit the partiality of their perspective on scientific change and practices) we may be able to address three important and related issues. One is that the hesitations about acknowledging "neo-whiggism" and the boxing of reflexivity are strategies aimed at covering a problem that does not exist—a problem that is caused only by our insistence at playing god tricks. The second is that, by dropping the relativists' god trick of "being nowhere while claiming to be everywhere equally" we may also be able to avoid many of the epistemological problems for which relativism is attacked by rationalists. Third, playing relativistic god tricks is not just a harmless self-deception. When played by relativist academic historians and social scientists, god tricks help legitimize the university as the institution where "scientific" social knowledge is produced. In short, making explicit the historians' and sociologists' location is both epistemologically rewarding and politically critical.

From Relativism to Contingentism

I will now try to develop a localizing critique of relativism in science studies—a view that I would call "contingentism." Paradoxically, such a critique may be developed from an analysis of the role of incommensurability—a notion that is usually taken as the very emblem of relativism.

I have recently proposed a genealogical interpretation of the phenomenon of incommensurability based on an analysis of a debate between Galileo and a group of Aristotelian philosophers.[18] Instead of taking a *synchronic* view of the incommensurability sometimes existing between competing scientific paradigms, I have suggested a genealogical perspective aimed at identifying some of the processes responsible for the *diachronic* emergence of incommensurability. While a synchronic approach is bound to present a scenario in which different groups and cultures hold different and possibly untranslatable belief systems, a genealogical view of scientific change gives visibility to the *mechanisms* linking the production and maintenance of groups' identities to the knowledge they produce over time. The adoption of a diachronic view makes it possible to perceive the phenomenon of incommensurability not simply as a problem but rather as a key to understanding how new paradigms or worldviews develop out of or away from old ones during a nondirected process of scientific change in which different groups or cultures fashion new identities for themselves.

Moving from a preliminary analogy between Darwin's population-based notion of species and Kuhn's community-based view of paradigm, I suggested an analogy between incommensurability and sterility. Just as a variety's inability to breed back with the original species marks the beginning of a new species, the inability to communicate between an emerging paradigm and the previous one (that is, of "breeding cognitively") may be seen as the sign of the establishment of a new "scientific species." However, as I hope will become clear later, my position is quite distinct from what is commonly known as evolutionary epistemology.[19]

It is through this perspective that incommensurability becomes more than just an obstacle to communication among competing groups of scientists and begins to appear also as a necessary component of the process of scientific change. In a sense, incommensurability represents a cognitive cost that sometimes cannot be avoided for scientific change to take place. If scientific knowledge is the collective product of a group of interacting scientists, such a group needs to remain cohesive and committed to the articulation of its paradigm.[20] The maintenance of the group's cohesion depends also on the members' sharing a professional and disciplinary identity, and having an identity is connected to representing oneself as different from others—a process that involves various ways of "keeping the other at a distance." In the case of science, the maintenance of group cohesion is often connected to scientists of competing groups' talking past each other or boxing adversaries' claims within one's linguistic and conceptual categories—a behavior that is very likely to produce misreadings. Consequently, the possible breakdown of communication between competing scientists cannot be seen as merely unfruitful. The hypothetical scenario resulting from everybody's willingness to learn every other group's worldview might not be characterized by a perfectly ecumenical and rational science, but rather by the absence of different groups, disciplines, and paradigms, and, consequently, by the termination of cognitive activity itself.

Seen from this angle, incommensurability ceases to be the unfortunate result of cohesion-keeping processes that happen to reduce one group's willingness to dialogue with another. Instead, these noncommunicative behaviors function as a sort of containing belt that makes cognition possible by keeping its actors together and committed to the articulation of their lexical structure. In the long run, such an articulation may produce a new lexical structure that may be incommensurable with that of the original group.[21]

An interesting reframing of the process of theory-choice results from this genealogical interpretation of incommensurability. By shifting our attention away from the process of choosing between competing theories to the processes that allow a group to maintain cohesion, build a common sociodisciplinary identity, and articulate a worldview, this view suggests that competing groups do not need to engage in a fully constructive dialogue in order to produce science. To be dropped, a claim does not need to be falsified (nor a research program superseded). Like species that die off not because they are directly eliminated by others but because they no longer fit the environment, paradigms can come to an end not because they are replaced or refuted by others but because they no longer fit that ecological niche—that is, the reward system of science and the socioinstitutional context in which they are located. However, as I will argue in a minute, the "extinction" of a given worldview does not mean that it is unsatisfactory by any absolute standard. Local contingencies (rather than the hidden hand of rationality) have a lot to do with it.

Different groups develop and hold different representations of the world, but it is not at all clear why they would always need to convince the competitors that theirs is best. The success of a representation is not necessarily achieved by having it chosen and adopted by all competing groups. Rather, it may simply be that a paradigm or set of practices appears to have been adopted by everybody simply because the groups that did not adopt it became professionally extinct. In short, intergroup justification of beliefs is not generally necessary. Somebody belonging to a given scientific group does not necessarily need to justify his or her beliefs to members of other groups. Simply, those beliefs are the only ones he or she has. In a sense, to ask people why they believe what they believe is a bit like asking them why they look the way they look.

This genealogical view of scientific change helps reframe relativistic pictures of science both by stressing the diachronic (and localizing) processes that limit a group's knowledge options and by acknowledging the role of the "out-there" in the cognitive process. In fact, a scientific tribe ontologizes its worldview not simply because it does not have access to alternatives, but also because such a worldview embodies (by the very fact of having survived) the result of the successful interaction between that tribe and the environment (both natural and social) with which it happened to interact. However, there is still some degree of arbitrariness (for lack of a better term) in the tribe's knowledge, in the sense that the tribe's cultural genealogy could have taken different paths and could have led to much different cognitive and sociocultural scenarios.

Consequently, although the knowledge of that tribe reflects a "success story" in the sense that it indicates that the tribe has survived a more or less long interaction with its environment, the quality of this knowledge cannot be assessed according to any general and external parameter. Although one may say that a tribe's knowledge is "absolutely good," that statement has to be understood as meaning that the tribe that held that knowledge managed to survive, and that we have no external point of reference by which we may evaluate the degree of its quality. The way in which it is "absolute" is by default only. Although the dynamics of this genealogical process indicate that other scenarios may have come about, those are scenarios that, in practice, we cannot reconstruct.[22] In short, this perspective suggests that the "out-there" had and has an essential input in the knowledge of the tribe, but that, at the same time, one cannot apply categories such as truth or progress to this process except in a very specific sense.

The notion that is central to this view of scientific change is the fit between a given group and its worldview. However, the notion of fit is not meant to refer to the closeness between the physical world and the group's representation of it. Once viewed in a genealogical framework, the problem of evaluating closeness (or hazy notions such as simplicity or elegance) becomes something of a red herring. A worldview's being good (in the sense of having contributed to the group's socioprofessional survival) does not imply that one's representation of the world was necessarily close to it. Although a good representation is one that fits the environment, fit does not need to be thought of in a *mimetic* sense. We do not need to think of representations of the world as good or bad copies of it, but simply as contingently effective or ineffective—that is, as making it possible (or impossible) for a given group or culture to survive as such.

Evidently, this does not suggest that, as far as representations of the world are concerned, "anything goes" in any given context, but rather that, in different contexts, worldviews may "go" for different reasons. That a tribe holding a certain view survived socioprofessionally implies that something "went" at some point. As we know, not all worldviews would have been effective in allowing a group to survive in a given socionatural context, or in a given sequence of such contexts. However, because both tribes and socionatural contexts change both historically and geographically, I see no point in assuming that the interaction between representations and environments must lead to their survival or extinction *always for the same reasons.*[23]

This is something similar to saying that at one time and place one species may have become extinct because it could not survive a change in the environment's temperature, whereas in a different place or period another species may have been exterminated by the arrival or development of a disease previously unknown in that ecological niche. However, had they survived those unexpected changes, these species might have turned out to be doing quite well in today's environment. In short, representations *do* interact with the "out-there" (as shown by the fact that some of them survive though others become extinct), but such effects cannot necessarily be traced back to the same cause. Nor can we say that those causes were rational. At the same time, the locality of the parameters of fit should not be read as implying that science is arbitrary. The contingency framing scientific change is crucial to its "adaptation" and eventual possible survival.

Consequently, I do not think that the fit between the environment and its representation can be conclusively evaluated through *a priori* rules such as "rationality."[24] "Fit" can be detected only *a posteriori*—accordingly as a tribe did or did not survive. Nor do I think it is correct to talk about degrees of fit. Fit is, so to speak, a binary category. Either the culture fit (and survived) or it did not fit (and became extinct). For similar reasons, I think it improper to say that a representation fit*s* a given socionatural environment.[25] True, a paradigm's being still around suggests that, in *some* way, it does *not not fit* the environment. However, the only certainty about fit is necessarily an *a posteriori* and *negative* one. All we can say with certainty about fit is that the cultures or groups that are no longer around happened *not* to have fit the environment at *some* point and in *some* way.

Finally, it is worth pointing to the fact that, by considering scientific change in terms of paradigm survival rather than paradigm choice, we do not need to draw the line between society and nature. I am not saying that the natural should be subsumed under the social or vice versa, but simply that such a distinction is not useful once one looks at scientific change with a genealogical perspective. What makes a worldview extinct is the socionatural environment, and not nature or society taken separately.[26]

This genealogical view of scientific change avoids, I think, the charges of arbitrariness usually leveled against relativistic views of science by showing the processes through which a given group may adopt certain beliefs because of the impossibility (at that location and time) of holding alternative views and yet remaining members of that same tribe. At the same time, it offers a picture of the development of knowledge as a process of full interaction with the contingencies (and the play of chance) that a scientific tribe may encounter along the way. In short, the "out-there" does enter the picture, but in ways that cannot be reduced to the rules of Rationality.

Understanding these mechanisms helps a shift of focus from *local* to *located* (or situated) knowledge in ways that are, I think, congruent with Haraway's proposal. As I hope to show, this shift is useful not only in order to bypass a deadlocked debate on rationality and relativism, but also in order to locate (and acknowledge the limits of) our possible interpretations of past (or other) scientific cultures given our cultural and institutional position.

Once we consider the historiographical implications of this genealogical perspective we may realize that neo-whig history of science becomes a quite appropriate methodological option. Fortunately, what I have kept referring to as neo-whig history is really not so whiggish after all. Rather, it can be best described as presentist history. Differently from whig historians, presentists do not write history as leading to the present in order to legitimize it. Although it is written from the present, presentist history acknowledges that the present from which it is written might have turned out quite different from what it happens to be.

As historians of science, we are not only located in the present but we are also tied to the current state of scientific knowledge and to our institutional predicament and culture as our ultimate frames of reference for historical interpretations. However, as discussed before, today's scientific knowledge cannot be said to be the best possible in any general sense. It is good only in the (important) sense that it made it to the present. Therefore, while the present state of science is the only system of reference we have, this system might have turned out quite different. This present is a *fact* simply because it is the *only artifact* we happened to get.

Consequently, it is not whiggish to say So-and-So was right if all we mean by that is that So-and-So's scientific tribe happened to be successful and that *we happen to belong to a culture that is genealogically connected to that of that tribe.* True, So-and-So survived also because his or her worldview was "good." However, as mentioned before, the meaning of "good" needs to be understood within this genealogical framework. Had the socionatural environment been different, things might have gone differently, and we might have been writing not only histories of a different science, but also from a very different point of view.

However, we cannot (re)construct how things might have gone—we do not have alternative points of reference beyond *this* present. Given these epistemological limits, all we can do is not to look for other ways in which science might have evolved (that would be another god trick), but to study the *mechanisms regulating the process* through which it became what it is now, and to understand how this very process frames our interpretation of the evidence about the genealogy of modern science.[27] While it is appropriate and rewarding to study science as a process, we need to understand that we do not have any external point from which to view that process.

Of course, the present is not one but many (depending on who we are and where we are situated in it). Nor do we need to be happy with or accept the present as we have it. As exemplified by Haraway's mixing of history and philosophy of science with science fiction, it is quite possible to write in this present while proposing categories, images, and metaphors that may be useful in changing it. My goal here was not to present a paralyzing picture of our predicament but rather to point at the processes through which we academic social scientists may contribute to normalization (through contextualizing and historicizing) by overlooking the processes through which we ourselves are historicized and situated.

Contingentism and Canon Formation

This takes me to Simon Schaffer's "Contextualizing the Canon"—a response to "contingentism" he presents in this volume. Schaffer too finds it problematic that relativist historians of science do not treat their claims as being produced through the same processes of knowledge construction they describe in the natural sciences. He too wants to correct this asymmetry *and* avoid reflexive regressus by focusing on the genealogical processes that locate the historians and their claims. However, his genealogical approach is different from mine.

Schaffer sees the canon of a discipline as "the corpus of exemplary texts that provide a standard of that discipline. The canon of the human sciences provides resources for any currently possible historiography of the natural sciences." I read Schaffer as presenting the formation of the canon in a given social or human science as a process analogous to what sociologists of science have called the "closure" of scientific controversies. In the natural sciences people canonize experiments and instruments, whereas in the human sciences people canonize books. In both cases, the closure or canonization provides the practitioners of a discipline with a temporarily stable frame of reference against which to evaluate their claims. According to Schaffer, reference to canonical texts puts an end to the historians' "regress"—at least temporarily. Canonizing is like black-boxing.

However, as Schaffer argues through a number of examples, among the things relativist historians are best at doing are precisely the contextualization, historicization, and relativization of canonical texts. In these, they typically move backward from the time at which the canon is established and reconstruct the precanonical context—the array of contending candidates that were eventually silenced (at least temporarily) by the text that happened to be successfully canonized. This he calls the "teleological" (and I would say "presentist") dimension of relativist history. Therefore, while the documentation of a canon is a move toward removing the asymmetry between the historians' views of the construction of past scientific knowledge and their own historical narrative about it, that step is not sufficient to solve the problem. True, the canon may provide historians with a relatively stable frame of reference, but such a stability is not immune to the relativist historians' contextualizing skills. That relativist historians routinely contextualize other canons *but not their own* shows that, by itself, a canon is not sufficient to put the historians' regressus fully under control.

What we need to do, Schaffer argues, is to supplement the documentation of a canon with "a social history of canonization." It is not enough to contextualize, historicize, or relativize a canon;

one needs to show the processes through which this takes place—processes that should be applicable to one's own canon as well. The process of canon formation plays in Schaffer's scheme a role comparable to the process of genealogy of cultures in the framework I propose.

My first query is about how a social history of canon formation would look. While such history may provide us with a number of examples, it does not necessarily follow that we would be able to *abstract* the *process of canonization* from those discrete instances. To put it differently, what is the difference between documenting the canon and writing the social history of canonization proposed by Schaffer? While this distinction is crucial to the workings of his two-piece approach (because it is this distinction that would stop the historians' regressus), there seems to be a tension in the separation of these two components—a tension that has all the features of an "aporia." In fact, works like *Leviathan and the Air-Pump* or McKeon's and Ashcraft's contextualization of the canonization of the novel or of Locke's *Two Treatises* are *both* case studies *and* analyses of the process of canon formation, and yet their analyses do not seem to be free from regressus. Schaffer assumes that we can actually do something more than this, that we can have access to some sort of "meta-" point of view about canon formation. But is that really possible? And, if not, what would be left of his proposal?

I also have a corollary question about Schaffer's exclusion of science studies from the range of examples he provides about canon formations and contextualizations. This, I assume, results from Schaffer's belief that a discipline cannot undo its canon without undoing itself as a discipline. It seems that Schaffer would be right in not being too disturbed by this local asymmetry if the historians could actually gain sight of the *process* of canon formation through its social history. Had they access to that meta-level, the historians not only would know that canons are only contingently stable but would also understand how canons (including their own) emerge and disappear. They would be aware of operating in a domain of canon change but would also understand the dynamics of that change. However, as mentioned above, it is not clear whether canon formation would actually be graspable through its social history, or whether the understanding of that process we gain from studies like *Leviathan and the Air-Pump* would end the regressus. In short, is Schaffer's proposal really adequate to avoid the limits of contextualism and historicism he is addressing?

By contrast, the genealogical process of *situating* I have sketched above tries to locate the practitioners of science studies in ways that are both more rigid and more flexible than Schaffer's proposal. The localization I propose is more rigid because it points to a few constitutive (and therefore unavoidable) constraints of the historians' knowledge. My central claims are that presentism is inescapable and that the necessary character of presentism is sufficient to stop the historians' regressus. But while my claim about the necessity of presentism is categorical, the view I propose does not impose constraints on whatever canons (including none) people may want to adopt.

That my model does not talk about canons is not accidental. Being located is not reducible to having a canon, and I do not believe that, as Schaffer puts it, "the canon of the human sciences provides resources for any currently possible historiography of the natural sciences." Being situated in a certain position frames one's perception of the resources one *can* use to legitimize his or her interpretation of science, and the canon of the human sciences (whatever that may be) may (or may not) be seen as a resource. What about interpretations of science that mobilize science fiction as much as more "canonical" science-studies literature? Further, while I do agree with Schaffer that (in certain disciplines and in certain historicoinstitutional contexts) canons did and do play the role he attributes to them, I am much less convinced about both the viability and the desirability of a canon in science studies today. Not only does science studies seem to have no unified canon, but—to continue to play on the analogy between canon and closure—we may not even have (or want to have) a "core set" (to use Collins's terminology) that would make closure or canonization possible. Given the interdisciplinarity of our field (history, philosophy, sociology, anthropology, gender and cultural studies) and the porousness of many of these communities and discourses, it seems very unlikely that we will ever be able to get to a canon even if we wish to do so.[28] More important, is "canon" something that is really worth defending (though in a "reflexive" manner) or—as I have tried to propose—should we rather try to think about alternative ways in which regressus within relativist science studies can be avoided?

Conclusions

Contingentism argues that there is no need to be apologetic about relativism, to defend it, and to pretend that we can be everywhere. Contingentism provides, I think, a better link between the process of scientific change and its historical or socioanthropological interpretation. In doing so, it bypasses some of the deadlocks of the rationality/relativism debate, and, by locating the actors (both scientists and science-studies practitioners) and framing their interpretive options, it avoids the pitfalls of relativism and its possible problematic political uses resulting from playing experts—though of a "politically correct" brand.

In fact, the exposure of the aporias of relativism provides tools for criticizing the ways academic discourse uses those aporias as opaque spots in which the power dimensions of that discourse can remain hidden. Contingentism indicates that, despite academic mythologies of politeness, we cannot interpret the past (or the "other") on its own terms. Interpretations of the "other" are bound to be partial; that is, partiality is not a failure but a necessity. All we can do is be aware of the ways in which they are *necessarily* partial—that is, of how these partial understandings of the "other" (not unlike incommensurability) allow for the development of different cultures (scientific or not).

Contingentism does not present a critique of relativism in an attempt to go back to rationalism. Rather, it indicates that some of the problems of relativism derive from confronting cultural difference with an incomplete understanding of the processes framing the genealogy of those differences. Understanding those processes supports the relativists' critique of rationalism while avoiding some of its epistemological and political problems. In fact, by pointing at the genealogical limitations on our ability to understand other scientific cultures, it helps us understand the processes that frame scientific change and the ways in which we and our discourses about science are situated by those same processes.

Biagioli, From Relativism to Contingentism

1. David Oldroyd, "Why Not a Whiggish Social Studies of Science?" *Social Epistemology* 3 (1989): 355–60 (a review of *Leviathan and the Air-Pump*); George Bowker and Bruno Latour, "A Booming Discipline Short of Discipline: (Social) Studies of Science in France," *Social Studies of Science* 17 (1987): 724–26.

In "Scientific Discoveries and the End of Natural Philosophy," *Social Studies of Science* 16 (1986): 397, Simon Schaffer argues: "We can say that the research of the early nineteenth century produced the discovery of photosynthesis in the late 1770s. Without some form of *teleology*, there is no reconciliation available. *It seems simultaneously unnecessary, ill-mannered, and impossible to find a mark of discovery separate and superior to the locally generated rules of communities of natural philosophers"* (emphasis mine). However, nothing in Schaffer's analysis indicates that historians of science are exempt from this "teleogy" (definitely a whiggish category) when they reproduce that discovery historiographically.

My "The Anthropology of Incommensurability" (*Studies in History and Philosophy of Science* 21 [1990]: 183–209) displays the same aporia. In fact, while describing the debate on buoyancy as undecidable, I also said that Galileo was right without explaining how the two apparently opposed stances could be reconciled. Some colleagues have read those claims about Galileo's being "right" as signs of my hidden whiggish agenda. Although I insisted on denying charges of whiggism, I had to admit that I had not made explicit my system of reference.

During a TECH-KNOW Workshop at UCLA in November 1989, a number of participants discussed reconsidering the taboo about whig history of science.

2. David Hull, "In Defense of Presentism," *History and Theory* 18 (1979): 1–15; and A. Wilson and T. G. Ashplant, "Whig History and Present-Centred History," *Historical Journal* 31 (1988): 1–16; idem, "Present-Centred History and the Problem of Historical Knowledge," *Historical Journal* 31 (1988): 253–74. See also an earlier piece by George W. Stocking, Jr., "On the Limits of 'Presentism' and 'Historicism' in the Historiography of the Behavioral Sciences," *Journal of the History of the Behavioral Sciences* 1 (1965): 211–18.

3. Some anthropologists have begun to look at the way ethnographic records are produced as *texts* and the ways in which the *dialogue* between the ethnographer and the informant is usually effaced in the final narrative in order to create a sense of scientific distance between the ethnographer and the foreign culture being described: George E. Marcus and D. Cushman, "Ethnographies as Texts," *Annual Review of Anthropology* 11 (1982): 25–69; George W. Stocking, Jr., ed., *Observers Observed* (Madison: University of Wisconsin Press, 1983); James Clifford and George E. Marcus, eds., *Writing Culture* (Berkeley and Los Angeles: University of California Press, 1986); Renato Rosaldo, *Culture and Truth* (Boston: Beacon Press, 1989). Similarly, in the wake of Hayden White's argument that modern historiographical discourse is inexorably emplotted by literary tropes, a number of authors have analyzed the limits and "hidden agendas" of historical interpretations: Hayden White, *Metahistory* (Baltimore: The Johns Hopkins University Press, 1973); Michel De Certeau, *L'écriture de l'histoire* (Paris: Gallimard, 1975); idem, "Writing vs. Time: History and Anthropology in the Works of Lafitau," *Yale French Studies* 59 (1980): 37–64; Sande Cohen, *Historical Culture* (Berkeley and Los Angeles: University of California Press, 1986); Michael Taussig, "History as Sorcery," *Representations* 7 (1984): 87–109.

4. The main exception is Steve Woolgar, who has repeatedly called for a reflexive sociology of science. His most articulated statement is perhaps "Reflexivity Is the Ethnographer of the Text," in Steve Woolgar, ed., *Knowledge and Reflexivity* (London: Sage, 1988), 14–34. The ethnomethodologists have also stressed the role of reflexivity in their method. However, the meaning they attribute to reflexivity is quite different from the one adopted by most participants in the reflexivity debate. To the ethnomethodologists, reflexivity is not a "problem" or an issue that would question or undermine the scientificity of the social sciences. Quite to the contrary, they see it as a necessary component of their "scientific" methodology.

5. For a good summary of the positions on reflexivity by a number of sociologists of science, see Malcolm Ashmore, *The Reflexive Thesis* (Chicago: University of Chicago Press, 1990), 26–86. On reflexivity within the sociology of scientific knowledge, see also Woolgar, ed., *Knowledge and Reflexivity*. For reflexivity as the predicament of postmodern philosophy, see Hilary Lawson, *Reflexivity* (La Salle: Open Court, 1985).

6. Although Yehuda Elkana has been discussing "reflexivity" extensively, the meaning he attributes to that term is quite different from the one it has assumed in the contemporary debate on reflexivity in the social sciences.

7. Augustine Brannigan, *The Social Basis of Scientific Discovery* (Cam-

bridge: Cambridge University Press, 1981), is a good example of this trend. See Simon Schaffer's "Scientific Discoveries and the End of Natural Philosophy," *Social Studies of Science* 16 (1986): 387–420, for a review of some of the more recent literature on scientific discoveries.

8. Feyerabend and Kuhn have used history as a tool for philosophical critique, that is, to attack the separation between the contexts of discovery and justification. At the same time, their introduction of the notion of incommensurability has given their critics a good resource to undermine their statements by pointing to the fact that incommensurability would jeopardize their ability to interpret past science and, therefore, to use history to undermine received rationalistic philosophies of science: see Hilary Putnam, *Reason, Truth and History* (Cambridge: Cambridge University Press, 1981), 113–19. Feyerabend's response to Putnam is reprinted in his *Farewell to Reason* (London: Verso, 1987), 265–72. The anthropologist Dan Sperber has presented a critique of relativist anthropology that is structually analogous to Putnam's argument against relativist philosophy of science: "Apparently Irrational Beliefs," in M. Hollis and S. Lukes, eds., *Rationality and Relativism* (Cambridge, Mass.: MIT Press, 1982), 180.

9. Thomas S. Kuhn, *The Essential Tension* (Chicago: University of Chicago Press, 1978), xi–xii; idem., "What Are Scientific Revolutions?" in Lorenz Kruger, Lorraine J. Daston, and Michael Heidelberger, eds., *The Probabilistic Revolution* (Cambridge, Mass.: MIT Press, 1987), vol. 1, p. 9.

10. This point cannot be fully articulated here, but will become clearer in the later part of the essay. See also note n. 27 below.

11. Of course, not all academic social scientists and humanists are relativists, though the vast majority of the younger ones are. Academic relativism comes in all forms and shapes. For instance, many academics are relativists only when they discuss social, cultural, or political issues but often display a less tolerant "Enlightenment gestalt" and a sanguine veneration for Rationality in their discussions of the production and acceptance of scientific knowledge. However, as shown by recent reforms in the teaching of Western Civilization (as at Stanford and Berkeley), it seems that cultural relativism has become accepted (actually or rhetorically) by many academic institutions in the United States—especially those having an ethnically and culturally diverse student population.

12. For instance, relativism helps to represent the claims of minorities as local and bound to the specific views and needs of their subcultures even when, instead, they address issues of a much more general currency. In short, minorities are nominally entitled to have their cultures and claims, but they are also kept in check by the representation of their claims as those of a "special-interest group."

13. Donna Haraway, "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective," *Feminist Studies* 14 (1988): 575–99; reprinted in revised form in Donna Haraway, *Simians, Cyborgs, and Women* (London: Routledge, Chapman and Hall, 1991), 183–201.

14. Haraway, "Situated Knowledges" 284. Haraway's claims echo Renato Rosaldo's critique of North American social scientists who "pretend to speak either from a position of omniscience or from no position at all": *Culture and Truth*, 204.

15. Things have changed since 1990, when this essay was initially written. As shown by Paul R. Gross and Norman Levitt, *Higher Superstition: The Academic Left and Its Quarrels with Science* (Baltimore: Johns Hopkins University Press, 1994), we may be witnessing the beginning of an attack on relativistic science studies as part of a reaction against postmodernism (construed by its critics in extremely broad terms). I believe these reactions reflect precisely the widespread success of relativistic approaches within the humanities and social sciences.

16. During the "Which Way L.A.?" program.

17. In particular, I do not endorse the rationalists' critique of relativism, and share most of Clifford Geertz's argument in "Anti Anti-Relativism," *American Anthropologist* 88 (1984): 263–78.

18. Biagioli, "The Anthropology of Incommensurability." This article has been revised and expanded into chaps. 3 and 4 of *Galileo Courtier* (Chicago: University of Chicago Press, 1993).

19. The view of scientific change presented in my "Anthropology of Incommensurability" is quite different from what goes under the label of "evolutionary epistemology"—an approach (first proposed by Donald Campbell) based on a Popperian interpretation of Darwin. "Evolutionary epistemology" draws a close analogy between Darwinian natural selection and Popperian falsification and ends up seeing natural selection as a "rational" process of "error elimination." As the rest of the article shows, I do not share this Popperian reading of Darwin and, in particular, do not see "natural selection" as a "rational" process, and do not use categories such as progress or "directed evolution." On evolutionary epistemology, see Donald T. Campbell, "Evolutionary Epistemology," in Paul A. Schilpp, ed., *The Philosophy of Karl Popper* (La Salle: Open Court, 1974), vol. 1, pp. 413–63; and Kai Hahlweg and C. A. Hooker, eds., *Issues in Evolutionary Epistemology* (Albany: SUNY Press, 1989). A much more interesting (and empirically articulated) evolutionary view of science available is found in David Hull's *Science as a Process* (Chicago: University of Chicago Press, 1988).

20. Although in this essay I keep talking about "groups" and "tribes,"

I do so for the sake of simplicity. My claims can, I think, be transferred to sets of historical actors who are connected through networks rather than by sharing the same physicogeographical niche. Also, later in the essay I talk about scientific species, tribes, theories, and paradigms. Although the nomenclature may not be apparently consistent, what I refer to through all these terms is a set of related scientists holding a certain theory or set of theories. So when I talk about tribes and scientific species, I also mean their theories. Vice versa, when I mention theories or paradigms, I also mean to refer to the people that hold them.

21. Although incommensurability should not be confused with the various noncommunicative behaviors we may observe during scientific controversies, it is, I think, connected to them. At an early stage of the process, it is quite probable that the subgroup or "variety's" linguistic grid may be still largely commensurable with that of the original group. However, the subgroup's cohesion and socioprofessional identity, maintained through these strategies of noncommunication, would commit its members to develop their new worldview and lexical structure so that, eventually, it may become linguistically incommensurable with the old one. Linguistic "sterility" between the subgroup and the rest would then intervene, indicating that the scientific "variety" had turned into a "species."

22. The view on the evolutionary process I am proposing here is closer to that of Stephen J. Gould, as articulated in *Wonderful Life: The Burgess Shale and the Nature of History* (New York: Norton, 1989), than to many "progressivist" interpretations of Darwin's views.

23. Because of this, I strongly disagree with the evolutionary epistemologists' association of natural selection and Popperian falsification.

24. Actually, one may try to think of "rationality" as a result rather than a cause of knowledge production: that is, as a set of protocols that, at some point, were developed by a culture looking back at the process through which it had produced its own knowledge. In short, rationality may be perceived as a set of guidelines (rather than prescriptions) developed *a posteriori*. "Rationality" may be simply seen as a method—one that, at some point and to some people, seemed to describe how knowledge had been successfully produced.

25. The "binarity" of "fit" is one reason why I think it wrong-headed to try to assess the (degree of) "closeness" between a theory and "reality."

26. However, this does not suggest that the distinction between internal and external features of science is meaningless. In fact, here I am simply talking about "socionatural selection" and I am not discussing the processes of "scientific variation."

27. In trying to convey the basic core of the argument, I have glossed

over a number of important issues. One of them is the role of bilingualism in the interpretation of old, alien, and possibly incommensurable worldviews. In "The Anthropology of Incommensurability," I discussed the ways in which processes of identity preservation tend to prevent members of a scientific tribe from becoming bilingual—that is, from learning the language of the "other." I believe these considerations apply to historians as well.

The "exemplary" historian of science described by Kuhn or Feyerabend is somebody who can become bilingual. However, neither Kuhn nor Feyerabend has analyzed the conditions regulating historians' access to bilingualism. I would argue that when historians encounter very alien systems of belief in the process of doing history, they respond to that encounter in ways that reflect their socioprofessional identity, that is, they may or may not decide to try to become bilingual. However, historians' socioprofessional identity is quite different from scientists', and this allows historians to become bilingual more easily. In fact, because historians' socioprofessional identity does not need to be linked to the theories of the scientists they are studying, they may not feel threatened (as the past scientists may have) in learning the language of the "other." In short, the academic historians' option to become bilingual is not merely a result of his or her "objective distance" from the events he or she studies. More simply, it derives from the historian's having a socioprofessional identity that is centered on beliefs that are different from those held by the scientists. In short, a historian's potential ability to become bilingual is not a matter of *distance* but of *difference*. It is not that the historian is "objective" by virtue of not having high stakes, but simply that the stakes may lie elsewhere.

However, even bilingualism does not avoid the incommensurability between the historian's culture and the past scientist's theories. Bilingualism allows the historian to detect incommensurability, but not to solve it. Consequently, the "defeated" scientific theory does not have much chance to be fully understood. In fact, finding himself or herself in a situation of undecidability, the bilingual historian would "lean" on the side to which he or she is genealogically connected. This is so for at least two reasons. First, the historian's identity is more or less (but inextricably) tied to that of the "winner." Second, the "other" theory stands there isolated, without a comprehensive picture of what it could have turned into. In short, because of the way scientific change takes place, we are bound to some extent to reinforce the memory of the winners even when we think that we are "really" understanding the "other."

28. An eloquent statement against the institution of a canon in science studies (and the dangers that that move may entail) is Sharon Traweek,

"Border Crossings: Narrative Strategies in Science Studies and among Physicists in Tsukuba Science City, Japan," in Andrew Pickering, ed., *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992), 429–65.