Constructivism for Philosophers (Be it a Remark on Realism)

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Bereft of the illusion of an epistemic vantage point external to science, what should be our commitment towards the categories, concepts and terms of that very science? Should we, despaired of the possibility to found these concepts on rock bottom, adopt empiricist skepticism? Or perhaps the inexistence of external foundations implies, rather, immunity for scientific ontology from epistemological criticism? Philosophy's "realism debate" died out without providing a satisfactory answer to the dilemma, which was taken over by the neighboring disciplines. The "symmetry principle" of the "Strong Programme" for the sociology of science-the requirement that truth and error receive the same kind of causal explanations-offered one bold metaphysical answer, under the guise of a methodological decree. Recently, however, it has been argued that this solution is not bold enough, that the social constructivists replaced the naïve presumption of an independent nature which adjudicates our beliefs with a mirror-image presumption of a sui generis society which furnishes these beliefs autonomously. The proper metaphysics for a foundationless epistemology, argues Bruno Latour, is one which grants nature and society, object and subject, equal roles in the success and failure of science and technology; one in which history of society merges with a history of things-in-themselves. The paper analyzes the philosophical and methodological motivations and ramifications of this extraordinary suggestion.

No Foundations

After taking central stage in the 1980s, the realism debate seems to have subsided in recent years without leaving much of a mark. Given the gravity of the issues at stake, a future historian of philosophy may find herself intrigued by this quick dissolution. The debate, after all, was an attempt to accommodate an extremely important realization: that we do not have

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an independent perspective from which to view and adjudicate our knowledge; no neutral language to talk about both nature and its representations. All the various arguments and positions in the debate, our imagined historian would explain, were attempts at answers to one fundamental question: bereft of the illusion of an epistemic vantage point external to science, what should be our commitment towards the categories, concepts and terms of that same science? Should we, despaired of the possibility to *found* these concepts on rock bottom, adopt empiricist skepticism? Or, perhaps the inexistence of external foundations implies, rather, immunity for scientific ontology from such epistemological criticism? The realism debate was quick to lose its vivacity, she might conclude, because, as many of its participants noticed (Rescher 1987, p. xi), it had turned "technical" before coming to grips with this basic dilemma. Philosophy, it seems, has adopted realism as its official stance on science while hardly noticing that it has chosen, *ipso facto,* the latter of the two alternatives and without reflecting upon the metaphysical and epistemological ramifications of this choice.

In particular, realism's quick ascendance to the throne of mainstream philosophy of science—driving the incumbent, logical positivism, out of its last strongholds—has obscured the fact that the title "realist" stood for two completely different philosophical personae. The one, represented by, e.g., Wright (1986), Harré (1986), Rescher (1987), Musgrave (in Nola 1988) and Putnam (1987), perceived the above dilemma as a reenactment of the old philosophical struggle with the skeptic, who has simply taken on a small array of new guises—anti-realist about this aspect of science or the other. The other persona, assumed by the likes of Rorty (1979), Hacking (1983), McDowell (1994) and Putnam of "The Meaning of Meaning," was engaged, to varying degrees of reflection and success, in a ground-breaking project of dismantling the very opposition in which the former type of "realist" was taking a side. Taking their key from previous assaults on the sets of dichotomies and hierarchies defining the Philosophical Kingdom of the battered positivist sovereign—Sellars on the "myth of the given," Quine on the dichotomy between "analytic" and "synthetic," Davidson on that between "scheme" and "content"—they set a devastating challenge to what Rorty called "the visual metaphors of knowledge," and Hacking, following Dewey, summarized as "the spectator theory of knowledge."

The realism debate subsided, but the absence of an Archimedean support for science has lost none of its epistemological significance, and that fundamental dilemma, left unresolved by philosophy, has come to haunt the neighboring disciplines of history and sociology of science. Indeed, the students of these disciplines did not originally experience the philosophical flight from foundationalism as a cause for concern but rather as a liberating breakthrough. The manifestos of the Edinburgh School in the mid-1970s were celebrations of this liberation.[1] Provoking as much angry opposition as enthusiastic application, the "social constructivism" evoked by the self-titled "Strong Programme for the Sociology of Science" became the liveliest and most fertile field for the study of science in the last quarter-century. It did so by holding on to both horns of the dilemma: insisting on its own scientific merit—thus upholding science's claim to unique epistemic status—while denying science (including the sociology of science itself) any privileged realm—any autonomous epistemic dominion where reasons rule over causes. In the name of the *scientific* values of empiricism, objectivity and generality, the Strong Programme demanded for itself the right (and assumed the responsibility) to provide causal accounts for the essential core of scientific knowledge as well as its paraphernalia (belying, in the process, the very distinction between core and periphery), for its content as well as its institutions, and most importantly for its true claims as well as its erroneous hypotheses and speculations. These accounts were to be sociological—scientific knowledge is a social phenomenon, argued Bloor, Barnes, and their disciples against the solipsistic instincts of most of modern epistemology. But it was the "symmetry principle"—the requirement that truth and error receive the same kind of causal explanations—that has turned the Sociology of Scientific Knowledge into a strong philosophical position—a genuine "Empirical Program of Relativism (EPOR)".[2]

2. Symmetry

To be a constructivist—social or otherwise—is to perceive the symmetry principle as reflecting a profound epistemological and metaphysical insight: that human knowledge is fundamentally a human product, constructed by human agency out of malleable, though recalcitrant natural ingredients. According to the constructivist credo, it is not unmediated Nature that distinguishes between true and false claims. Humans make the distinction, by applying historically changing and culturally dependent criteria. From this point of view there is clearly no place for two different types of historical, sociological or philosophical accounts of science; one—internal and rational—for its successes, and the other—external and causal—for failures.

This is a very powerful philosophical position, but it is not where the symmetry principle displays its true force. The demand for symmetrical

1. Perhaps the most exemplary ones are Bloor's (1976) and Barnes' (1977).
2. Collins (1981)

causal accounts of true and false science still presents an intriguing challenge to the philosophy of science precisely because it can be coached and supported in strictly methodological terms, committing, as it were, to *no* metaphysical creed but that implied directly by the "scientific method" itself.[3] One does not need to accept any assumptions regarding the nature of scientific truth in order to accept the symmetry principle; it is a straightforward application of the scientific edicts of causality, generality, parsimony and, especially, objectivity.

This is so because even a staunch believer in the existence of a province of scientific knowledge that gains its legitimacy directly from nature, an autarchic "realm of reasons"[4] unfettered by causes, will find it hard to insist that we know the boundaries of this domain. Even Lakatož himself would have had to concede, it seems, that we do not know the real pedigree of our beliefs: we do not know which of them were conceived and bred by reasons within the realm and which by causes outside it. The unperturbed Lakatožian would be right to point out that to accept a knowledge claim as scientific, let alone as true, is to grant the credibility of its lineage, and would also probably argue, against the constructivist, that this credibility means that the claim was reasoned rather than caused. But, he will surely admit, a station within the realm of reasons is always as tentative and provisional as any other attribute we assign to a scientific claim. The realization that we do not have an independent point on which to found the truth of our knowledge applies just as well to its rationality. All one can say, in that respect, about the most reliable and trustworthy pieces of current science, is precisely that: that they represent the best knowledge we have, probably the best we ever had, and perhaps the best we could hope for; or, similarly, that they are supported by the best, purest reasons we could come up with.

This, one should stress, is quite a lot. It should be enough to defeat the skeptic: if there is no firm, independent standing point from which to ascertain that our scientific convictions are reasoned and true, there can also be no independent position from which they can be shown as fundamentally wrong or irrational.[5] But the reliability and trustworthiness of these convictions is not nearly enough to censure the question of how

3. See, e.g., Golinsky 1998, pp. 6, 8.

4. This is the phrase developed by Sellars in his 1956, granted in a much more sophisticated way than can be discussed here.

5. This argument does not effect anyone's entitlement to offer external criticism of the morality, political standing, financial cost, cultural implications or any other aspect of science. Science's relative immunity, according to this line of thought, is limited to its epistemic prowess. That, again, is not due to science's own unshakeable foundations, but to the impossibility of any such foundations.

we did establish them, nor any well-supported answer to this question—notwithstanding that both question and answer may trespass the boundaries marked and declared by scientists, the legitimate inhabitants of the alleged domain of pure reasons. In other words: if our assumptions about the nature of scientific truth preclude the possibility that a scientific claim may be both caused and true (assumptions rejected by the social constructivist), we might not like to discover that some or all of our beliefs are an effect of "external" causes. Even if we were shown a convincing causal account of their emergence and acceptance, we may decide to reserve judgment about whether or not to keep holding to those beliefs. But we cannot preclude the possibility of such an account concerning any particular belief.

Thus, not knowing in advance which of our scientific convictions rightfully belongs in the touted realm of reasons, it is scientific objectivity itself that demands of the investigator of science to treat all of them alike. The historian, sociologist, or philosopher of science should put aside her own (probably favorite) opinion concerning the truth of the claims made by scientists. And unless she believes, against strong evidence to the contrary, that "truth prevails"—that the very truth of a claim, scientific or other, guarantees that it will ultimately be recognized as such—this demand will not strike her as a difficult one to meet. If she is interested in Einstein's path to relativity theory and the means by which he swayed his peers into accepting it, how could she benefit from her own knowledge that the theory was correct? Assuming, as we do, that she has no recourse to an external vantage point from which to examine both Einstein's hypotheses and their independent "truth of the matter," we must conclude that she based her conviction upon Einstein's own success in convincing himself and his peers. But this success is exactly what she attempts to account for; it cannot be used as part of the explanation. Hence, when one gives up the uplifting but ill-founded belief in revelation—in the mystical property of truth to declare itself to the unobstructed gaze of the human mind—symmetry becomes a simple consequence of scientific parsimony. If the truth of an hypothesis is not to be employed in the account of its emergence and acceptance, than there is no reason to field two essentially different kinds of explanations; one for true science and the other for false.

This is only one way to spell out the requirement of symmetry in explanation. It is somewhat less exciting than the epistemological version I summarized above, but it has one important advantage. It helps to illustrate that, in complete opposition to its prevalent "anti-science" image, social constructivism tends to behave very much like its great punching bag—good old logical positivism—purporting to be a metaphysics-free methodological critique. And in a vain much similar to the disillusionment suffered by that previous attempt at scientific philosophy, the confident methodological decrees of the Strong Programme, their structure thoroughly explored by their most competent upholders, gave way to painful metaphysical dilemmas. In fairness to the Strong Programme, it should be noted that the dilemmas were raised against the background of significant empirical success, which definitely redeems the methodological self-understanding. Moreover, these dilemmas were given their most pungent formulation by one of the scholars most responsible for its success—Bruno Latour.

3. Super Symmetry

Latour never shied away from metaphysical commitments, especially those implied by the constructivist approach to knowledge. All epistemological dichotomies, a-symmetries and hierarchies, he happily contends, are constructions. This is true, he specifies, of the superiority of Western science over any other mode of knowledge, and of course true of the distinction between "internal" reasons and "external" causes of belief, as the Strong Programme proficiently argued. This fact itself does not make a-symmetries like these any less "real"; science's superiority, one recalls, was forcefully upheld by the advocates of the Strong Programme, while the external-internal distinction, Latour had already shown in his *Laboratory Life,* is an important argumentation tool in the hands of scientists. The constructed nature of a-symmetries does mean, however, that one is not obliged to *adopt* any of them—they should rather be treated as a subject matter for analysis; "topicalized," in the internal lingo of the debate. In requiring exactly this, under the principle of symmetry, the Strong Programme has been a genuine intellectual revolution. Insisting that *both* truth *and* error are outcomes of social negotiation, the social constructivists demonstrated how the establishment of even this most basic dichotomy occurred differently and locally each time anew. They thus all but obliterated the most sanctified a-symmetry of traditional epistemology, namely, the custom of assigning truth to nature and error to society.

But the Edinburgh revolution cannot be the last, insists Latour. The social constructivists, he proclaims, stopped short of committing themselves to the historization of *all* a-symmetries. Assaulting the a-symmetrical preference to nature, they ended up replacing it with a similar preference to society. Insisting that the former is a construct, they found themselves accepting the latter as a *sui generis,* autarchic entity.[6] Wran-

6. A similar accusation is levelled at the philosopher most commonly associated—not necessarily to his liking—with social constructivism, Richard Rorty. Richard Bernstein

gling with the custom of assigning exclusively to Nature the positive role of begetting truth, and to society the negative role of introducing error, the social constructivists fell into the habit of allocating to society every active move in the production of knowledge and leaving Nature with only, at best, the passive role of recalcitrance. Finally, choosing society over Nature, but remaining within the boundaries of the dichotomy between the two, they again found themselves unwittingly mimicking their positivist arch-rivals: having to allow human agents the freedom to construct their knowledge according to social forces, they were inclined to watch as "[Nature] 'itself' drops out of the story" (*ibid.*). They did this by reconstituting the archaic notion of a neutral observation; a realm of consensus, where all observers agree upon the presence of a "redish powdery substance" (Bloor 1999, p. 93) in front of them.

Yet, there is no more basis for this new a-symmetry, which favors society to Nature, than for the old one, where Nature was preferred. The very dichotomy between Nature and society is a construct. The sharp distinction between subjects and objects, claims Latour, between human-societal and objective-natural, is but another artificial a-symmetry, constructed philosophically and politically in the seventeenth century—as beautifully shown in one of the classics of the school, *Leviathan and the Air Pump* (Shapin and Schaffer 1985). Therefore this dichotomy, and especially its boundaries, should not be taken for granted; there is no point in replacing naïve realism—the belief that Nature is "out there," independently of what humans make of it—with naïve sociologism—the belief that society is simply "in us," independently of what Nature enforces on it.

With that Latour calls upon the next revolution, establishing a "super-symmetry"[7] (my term) between subjects and objects in place of the local symmetry between truth and error. How can we do this? Well, by letting objects, as it were, "speak for themselves"; by allowing them to participate as equal partners in the stories of the successes and failures of science and technology—and, for that matter, society as well. The Copernican Revolution did not belong solely to Copernicus, Tycho, Kepler and Galileo. The planets, comets and super-novae had no less of a role in it, and the rapid social changes that followed the casting of Earth into the margins of heaven and setting it in triple motion should be ascribed to all those relevant agents—history of society cannot be separated from the history of things-in-themselves.

claims that if in Rorty's mind "social practices are the sort of thing that are *given,* and that all we need to do is to look and see what they are," then he "himself is guilty of a version of the 'Myth of the Given'" (Bernstein, 1985, p. 83).

7. Collins and Yearly mark the idea "hyper symmetry." See their 1992 and Collins 1994.

4. Latour's Dilemma

This is an exciting specter, and Latour attempts to carry it through in his remarkably wide-ranging work. He seems to get closest to his historiographic ideal in *Aramis* (Latour 1996). Yet the great allure of this position cannot disguise its immense difficulty, for which he has received sharp criticism by his erstwhile comrades, most notably Collins and Yearly (1992) and Bloor (1999).

Both the allure and the difficulties are well demonstrated in Latour's programmatic contribution to Daston (2000, pp. 247.269). Referring to an episode on which his expertise needs no further testimony, he turns our attention to the debate between Pouchet, the last "legitimate" champion of spontaneous generation, and Pasteur, one of the original two masters (Koch being the other) of germ theory. It will not do, explains Latour, to treat Pouchet as hopelessly pursuing an entity that has never existed anywhere, while Pasteur is playing hide and seek with *real* entities, which have always been everywhere. Such a "demarcating" attitude only masks the actual discrepancy between the two. The warm and fuzzy blanket of these seemingly-obvious categories—"real" vs. "unreal"—would completely blur the intricate differences in the theoretical, experimental, institutional, political and technical associations by which both men were trying to envelope their competing phenomena in order to bring them into stable and secure existence. Moreover; it will mask the hard labor which Pasteur had to put in, in order to extend the existence of germs from his laboratory towards the always and everywhere. But it would be only marginally better to look at the two as employing an array of human resources—theories, prejudices, political loyalties and bodily skills—to create consensus concerning "dramatically underdetermined matters of fact" (p. 264). This would mean that "matters of fact [are] playing no role at all in the controversy human agents have about them" (*ibid.*)—the exact mirror image of the discarded demarcation. Both approaches are radically a-symmetrical, pitting humans in their ever-changing society to objects in their never changing Nature.

Yet, what exactly does it mean to let "matters of fact [play] a role" in the closure of the dispute between Pasteur and Pouchet? How are we supposed to let "things-in-themselves" tell their own version of Pasteur's winning the day? Even the most rudimentary attempt to explore the significance of Latour's beautiful phrase is bound to come up against that primary philosophical insight with which we started: we know no other way of listening to "matters of fact" but through science. In order to achieve super-symmetry, it appears, in order to let objects fulfill historical roles similar to subjects, we must refer to science in the attempt to recount history—science's own history included. It may seem somewhat petty to waive the flag of *petitio principii* here; admittedly, the "recount" we are seeking is not an abstract argument, but a causal-historical narrative. But the difficulty this *petitio principii* signals is not merely logical. If, contrary to the old constructivists, we are to give Pasteur's germs their fair share in his success to fill our world with industrial yogurt and antibiotics, how are we to avoid prejudging his dispute with Pouchet in his favor? After all, that was exactly Pasteur's claim—namely, that the success of his prize-winning experiments was due to germs; that *germs* were responsible for fermentation and putrefaction.

The difficulty of Latour's position, the price paid for the next revolution, is steep, and is to be delivered in hard metaphysical currency. In order to secure the symmetry between things and people, between germs and Pasteur, it appears, Latour has to sacrifice the cherished and hard-earned symmetry of SSK—the one between truth and falsehood, between germs and spontaneous generation. If we *were* to grant Pasteur's germs with historical agency, then the requirement of symmetry would force us to ascribe the same agency to Pouchet's spontaneously generated eggs. It is hard to believe that even the most devout of constructivists would approve of granting agency to non-existing entities. Yet giving up on symmetry is renouncing the most significant philosophical achievement of the Strong Programme: the empirically supported claim that established science is a *contingent* creation, one of a variety of possible products of social negotiations. To wit: if the reasons for Pasteur's success are different from the causes for Pouchet's failure, if Pasteur won the dispute because he had germs on his side, then the die was cast in his favor from the outset; the conclusion of their dispute was predetermined by nature rather than contingent upon human labor.

Contingency is the strong metaphysical commitment behind the symmetry principle, a commitment disguised earlier by presenting symmetry as a metaphysics-free methodological ploy. Without contingency, there is no constructivism, social or other: if humans construct knowledge using natural materials, it must be no more necessary than any other human construct; any other artifact.[8] Yet from the point of view of *science*—the perspective that Latour's new demands appear to force upon us—this same knowledge looks anything *but* contingent. This is exactly how it should be; it is the business of scientists to make their claims and results appear as necessary and inevitable as they possibly can. It is the business

8. Hacking (1999) makes "contingency" one of his three marks of constructivism, together with nominalism and external explanations of stability. C.f. Ch. 3, pp. 63.99.

of historians, philosophers and sociologists to trace their contingency. This difference in epistemic commitments and interests can—but does not have to—develop into epistemological difference, and it did—as it should not have—develop into the political quagmire known as "the science wars," but it is a genuine difference even without such developments. It is hard to see how one can hold to both perspectives at once, how one can maintain "internal realism" simultaneously with "empirical relativism."

5. The Case of Newton's Optics

It would perhaps be better to understand and judge the dilemma brought about by Latour's critique against the backdrop of real historiographic debate, rather than his made-for-the-occasion examples. Competing historiographic narratives of one and the same episode are almost as hard to find as a replication of an experiment, but the significance of the debate is such that Alan Shapiro, a distinguished historian of Newtonian science, in an explicit attempt to lay bare the constructivist folly, wrote in 1996 a massive recount of the introduction and acceptance of Newton's optics—for which Simon Schaffer had suggested a detailed constructivist account of just a few years earlier (Shaffer 1989). The two papers, brilliant pieces of scholarship in their competing approaches, are exciting enough to compare as they stand. Reviewed from the perspective of Latour's dilemma, the dispute between them becomes almost unsettling.

Most of the basic chapters of the episode are not under contention. Sometime during the academic year 1666, while working on improving optical instruments, Isaac Newton, then an undergraduate student at Cambridge, concluded that the elongated spectrum, cast by a light ray refracted through a prism on a screen 20.22 feet removed, was not an artifact of an asymmetrically placed prism, but rather a genuine effect of the nature of light. A long series of experiments followed, and a remarkable "New Theory of Light and Colour" ensued. According to this theory, the white sunlight surrounding us is not simple, but rather a mixture of primitive rays, each characterized by a unique color and a unique index of refrangibility. Refraction did not modify light in creating the colors of the rainbow, but rather broke it down to its primitive constituents.

After presentation at a lecture course in Cambridge, the theory was submitted to the Royal Society of London in 1672, in a letter that cited only three experiments. The most celebrated of them—the so called *experimentum crucis*—involved a second refraction, to demonstrate that the first refraction did not, indeed, modify the characteristics of white light, but rather exposed the real and immutable properties of primary rays, properties which persevered through the second refraction.[9] The theory was first enthusiastically endorsed, but the resistance, which started to mount from both Britain and the continent, pushed Newton into angry withdrawal from public scientific life until the 1680s. The resistance, however, waned; by 1704, when Newton published his *Optics,* it was received almost unanimously, and the little debate that did arise was firmly settled in Newton's favor. "After 1726 or 1728 . . . to oppose [Newton's theory] was to initiate being removed from the mainstream of the scientific community," claims Shapiro (1996, p. 125). Schaffer disagrees on the dates: "The 1740s saw important . . . specific criticisms of some of Newton's apparent claims" (1989, p. 99), he points out, but he does agree that: "In popular texts such as Voltaire's *Elements of Sir Isaac Newton's Philosophy* (1738) and Algarotti's *Newtonianism for Ladies* (1737) it was claimed that those who had not succeeded in replicating Newton's trials 'had not been happy enough in the Choice of . . . prisms'" (pp. 91.2).

This is more or less where the agreement between the two historians ends. Their differences on the question of why and how Newton's theory was accepted, an afterthought issue for historians of previous generations, runs so deep that it colors every other aspect of their respective accounts. Why did Newton, for example, offer only three experiments in the paper submitted to the Royal Society? Was this deviation from the experimentalism pontificated by the Society a simple stylistic mistake, which caused him to lose points with its gentlemen members, as offered by Shapiro, or a sophisticated rhetorical ploy, intended to highlight his *experimentum crucis* and provide it with emblematic status, as Schaffer claims? And what did Newton intend the *experimentum crucis* to demonstrate? Was it the immutability of colors, as most of his contemporaries seem to have assumed, or the different and constant refrangibility of each colored ray, as Shapiro explains? Or did Newton himself, as Schaffer suggests, change his interpretation in order to defeat recalcitrant opponents? What, in general, was the degree of such recalcitrance? Was Newton's experimental "authority . . . necessarily unstable and contested" even well after his death and near-deification (Schaffer 1989, p. 100), or is it that "Schaffer, as a constructivist, focuses almost exclusively on controversy" since it allows him to "make it appear that Newton's theory was continually contested" while in fact "focusing on Newton's critics" is a vicious constructivist bias, which "tells us little about his supporters" and "easily

9. Whether the properties demonstrated were the unique colors or the unique indices of refrangibility was importantly disputed, and still is among the involved historians. I will return to this dispute momentarily.

distorts the historical picture" by "reducing the issue of acceptance to one of power and authority" (Shapiro 1996, pp. 60. 2)? And what did, in fact, determine the conclusion of Newton's debates, especially the ones with that group of English Jesuits from Liège? Did the scientific community arrive reasonably at the conclusion that being the last ones to still report failure to replicate Newton's not-too-complicated experiments, the Jesuits were simply incompetent (Shapiro)? Or did Newton succeeded in marginalizing the group, which stubbornly defied the success of his experiments and their interpretation, by controlling the rules of the debate, constantly changing the significance of the experimental set-up, dictating the interpretation of the results and de-legitimizing their claims (Schaffer)?

Until recently, any student of science with basic sympathy to constructivism could have easily pointed out the misunderstandings from which stems Shapiro's criticism of Schaffer. To begin with, Shapiro's notions of "power and authority" are limited to brute power and repressive authority, gathered by "conspirators" and distributed to "acolytes" (p. 60). Needless to say, no constructivist for whom the name "Foucault" rings remotely familiar would grant that these are the only relations holding between power and knowledge. More significant still, is that Shapiro constantly favors the winners; where Newton "explains," the Jesuits "insist" (p. 77); where Newton's critics "fail to replicate," his supporters "elide difficulties" (p. 94). And Shapiro's most significant failure, from the traditional constructivist perspective, is in seeing reasons where he should have seen effects; in finding explanations for Newton's success where he should have located the mysteries of that success. Thus, he explains with the Newtonians why Venetian glass was inadequate for replicating Newton's experiments, instead of accounting for the Newtonians' success in ascribing every failure in replication to the (low) quality of the equipment or the (lack of) skills of the experimenters. This, the constructivist would be quick to point out, is exactly the dilemma facing the experimenter: whether to attribute the failure of his experiment to the inadequacy of his equipment or to that of the inspected theory.[10] The eighteenth century scientific community could have taken the fact that Newton's experiments could not be replicated "with Venetian glass, long considered Europe's best" (p. 128) as a refutation of Newtonian optics, or it could have accepted Dereham and Desaguliers' arguments that the failure was due to bad prisms; it decided to do the latter. Instead of explaining why

10. This is the dillema Collins carefully inspects in his 1985 under the title "The Experimenter's Regress."

("topicalizing" the episode, in the common Edinburgh dialect), Shapiro adopts the Newtonians' arguments.

However, Latour's comments shatter the constructivists' confidence. To ascribe the "gradual acceptance of Newton's theory" solely to the negotiating skills of Newton and his allies, he explains, is almost as bad as ascribing it directly to Nature. If, in the name of symmetry, Newton's "insistence" on his interpretation of his results should receive the same treatment as the Liège group's "explanation" of theirs, then, *in the name of symmetry,* differently refrangible colored rays should get as much credit for establishing Newton's authority as he and his authority get in establishing their existence and significance. Could this be done without adjudicating the dispute by its results? This is the dilemma I named after Latour.

6. Back to Realism?

So, is Latour's dilemma not, after all, just another stance in the realism debate? It is, definitely, a worry very similar to the ones that sparked that debate, namely: How do we settle our loss of epistemological innocence with our acknowledgement of the indispensability of scientific ontology? What is the proper metaphysical commitment to a science that is both unique and contingent? One way to understand realism along the lines I sketched at the beginning, is to view it as an attempt to answer this challenge by falling on the ontological side: "when we say, and *mean,* that such-and-such is the case, we—and our meaning—do not stop anywhere short of the fact; but we mean: *this—is—so*" (Wittgenstein, *Philosophical Investigations* §95; cf. McDowell 1994, pp. 26.29). Epistemologically, this choice implied a thorough rejection of all attempts—positivist, instrumentalist and all their nuanced variations—to hold in *media res;* to believe science on a tentative basis while denying its categories the status they aspire to.[11] Such unabashed adoption of scientific ontology, it seems, is exactly what Latour requires to resolve his dilemma. If the use of non-scientific arguments supports the use of scientific ontology—if one can justify employing scientific concepts without referring to the reasons adduced by the scientists under investigation—then Latour should be allowed to bring these concepts into his accounts of science. Might realism, the destructor of the previous "methodological philosophy," logical positivism, come to the rescue of the current one, social constructivism?

11. It is important to recall that although the title "realism" for this position is relatively new, the position and arguments for it are not. One early version of them is Galileo's rejection of Cardinal Belarmine's suggestion that he (Galileo) should adopt what we would call an instrumentalist approach towards Copernicanism. See "Galileo's Considerations on the Copernican Opinion" in Finocchiaro 1989, pp. 70.86.

Certainly, not every self-styled realist would conceive of constructivism after Latour as requiring—or even deserving—a rescue. I introduced realistic thought as consisting of two strands: the anti-skeptic and the anti-representational. For thinkers of the former ilk, Latour's move did not appear to suggest any dilemma. Rather, they viewed it as a welcome sobering-up; a commendable retreat from fanciful constructive epistemology and a return to the good old "idea that experiment and debate allow science to home in on the true mechanisms behind the appearances" (Papineau 1995, p. 491).[12] The worry of old constructivist avant-garde, best voiced by Bloor (1999), that Latour's further revolution is nothing but a counter-revolution, echoes the realist hope that Latour "is inching his way towards common sense" (Papineau 1995), instigating Latour's rant that: "the acquiescence of the two archenemies, social constructivists and realists, to the very same metaphysics for opposed reasons has always been for me a source of some merriment" (Latour 2000, p. 264). To those who believe that "beliefs should be caused by the facts they are about" (Papineau 1987, p. xiv), Latour may seem to present no dilemma, as the principle of symmetry presented no achievement, and its abandonment is therefore no loss.

But the realist sigh of relief is premature. The anti-relativist realist cannot, to be sure, tolerate the relativism that seems to stem from the strong metaphysical reading of the symmetry principle offered by Latour. She *does,* however, have a vested interest in the original, methodological version of the principle, for reasons akin to the ones with which I introduced this version. Anti-relativism becomes scientific realism once the trust in scientific criteria, procedures, techniques etc.—the (anti-relativist) belief in their efficacy to pick true scientific statements from false ones—is supported by the (realist) conviction in the objectivity of scientific statements—in their gaining their subject matter and their truth from the objects of which they are about.[13] When the realist urges us to trust the objectivity of science, she vouches her trust in the hope that these criteria, procedures etc. are able to sort through scientific statements to the objects that give them their truth and meaning. If the criteria and procedures contain biases, if they prejudge scientific hypotheses, then they are *ipso facto* not objective—they do not allow the objects to adjudicate the truth of statements. The symmetry principle is nothing but a demand for objectivity in this very sense, applied to the study of science itself—a demand

12. This citation is from Papineau's review of Pickering (1995), which is an attempt—far less successful, to my mind—in the same direction as Latour's.

13. Anti-relativism can of course be supported in many other ways as well, e.g., by the belief in a benevolent God or in evolutionarily-tested categories.

not to prejudge our hypotheses about the coming to being of scientific statements by our knowledge of their truth. This demand is aided by another fundamental element of the realist creed, namely that "defeat always *is* a possibility where criteria are concerned [a]nd it will be in the lap of the gods whether it occurs in any particular case" (Wright 1987, p. 279). This fallibilism follows immediately from that most realistic of principles, namely that truth transcends all evidence, and it means that we always have to allow that we have been wrong to accept that any particular scientific claim has "a 'genuinely factual' subject-matter" (Wright 1987, p. 7). Thus, even if we are certain of the truth of a specific scientific assertion and the falsehood of its rival, it is *realism* that commands us to treat them symmetrically. It is realism that requires that we let the hypotheses about the discovery and justification of true as well as false claims to scientific knowledge be decided by the objects of inquiry, whether historical or sociological, without these hypotheses being prejudged by the truth or falsehood of the claims.

Realism requires symmetry, and for the realist, the difficulties arising from this requirement should be a cause for concern rather than glee. If, as Bloor fiercely contends, his position is a realist, naturalist and materialist one (e.g., 1999, pp. 87.91), then there is no apparent reason why the arguments which Latour directs against this position could not be generalized to pertain to more conventional versions of realism. The difficulties raised by Latour concerning the Edinburgh way of interpreting and applying the symmetry principle—namely the unwitting consequent shift towards idealism—are difficulties shared by the realist. This is, indeed, the major fault that Hillary Putnam finds in her position: "so far as the commonsense world is concerned," he concludes, "the effect of what is called "realism" in philosophy is to deny objective reality, to make it all simply *thought*" (1987, p. 12).[14] The social idealism with which Latour charges the Edinburgh school is different from the idealism to which Putnam refers, but it is not different enough to avert the suspicion that if Latour's criticism of the Strong Programme is a move within the realism debate, its significance resides in pointing at an internal inconsistency within the anti-skeptic realist position.

Even more troubling, from the perspective of anti-skeptic realism, is the dilemma emerging from Latour's attempt to replace the idealism he recovers with realist intuitions—namely; that the success of this attempt

14. In fact, Putnam's conclusion is weaker than allowed by his argument, which demonstrates that quite a few scientific properties beyond "the commonsense world" become a product of "thought" when viewed from the perspective of the brand of scientific realism he tags, after Husserl, "objectivism."

apparently comes at the cost of the original symmetry. This is because Latour's dilemma is highly reminiscent of a familiar, nagging tension in this version of realism: anti-skepticism is based on affirming and acclaiming the success of contemporary science. But this success is predicated on the failure of its predecessors, and the failure suggests that the success is temporary and tentative, and thus no weapon against the skeptic. In Latour's case, it is the apparent discrepancy between the two realist interpretations of symmetry—Bloor's and Latour's—which presents realism as deconstructing itself. In anti-skepticism, it is the discrepancy between the realist interpretation of contemporary success and the unflattering "meta-inductive" conclusion drawn from past failure. Again, the intellectual motivations are very different, but the resultant worry is the same; realists of the anti-skeptic camp should have an interest in a solution to Latour's dilemma, and their failure to as much as address it can rightly be perceived by Latour and his disciples as another evidence for the poverty of their approach.

7. A Possible Realist Resolution

But there is another brand of realism. Its subscribers are not always given this title, since they do not usually make firm proclamations in favor of mind-independent reality, truth-likeness of theories or unknowably-true statements. This is not because they believe in the opposite doctrines, but because they find it hard to express themselves in terms of a gap between mind and object. This defiance makes those thinkers—some of whom I mentioned above—less likely to take a position within the "realism debate" as shaped in the 1980s, but I think it does warrant labeling them "realists." This is not the place to review the various attitudes that they might develop towards Latour's dilemma, but, by way of example, I will try to distill such a possible position from one of their own to whom the term "constructivist" can be applied with least violence—Ian Hacking.

In his recent *Social Construction of What?* (1999) Hacking offers a less-than-favorable, if fair, critique of social constructivism in general and its epistemological brand in particular (cf. his chapter 3, pp. 63.-99), and expresses surprise that his earlier *Rewriting the Soul* (Hacking, 1995) was labeled "a classic of social constructionism" (1999, p. viii). Yet, in his still earlier *Representing and Intervening* (Hacking 1983), he takes a leaf from the constructist analyses of the preceding decade: "Traditionally scientists are said to explain phenomena that they discover in nature. I say that often they create the phenomena that then become the centerpieces of theory" (Hacking 1983, p. 220).

Hacking's adoption of this stance—now a constructivist commonplace, then still a small philosophical rebellion—is significant for our purposes here especially because it was formulated as an explicitly realist, anti-positivist argument (the immediate target was van Fraassen 1980). I argue in another place (2002, pp. 63.81) that Hacking's fusion of constructivism and realism, captured nicely by his slogan "*if you can spray them then they are real*" (Hacking 1983, p. 23), fails exactly where the framework of the realism debate forces him to inadvertently revert to (what he himself contemptuously names, after Dewey) "the spectator theory of Knowledge" (p. 130). This very shortcoming is rather an advantage here; it allows us to investigate how far one can proceed in solving Latour's dilemma without succumbing to Latour's extraordinary demand that we completely abandon the distinction between Nature and our knowledge *about* this Nature.

A possible, admittedly indirect, resolution stems from Hacking's thoroughly constructive analysis of microscopic observation: "you learn to see through a microscope by doing, not just by looking" (p. 189). This, by Hacking's admission, is a reinstatement of Berkeley's "Theory of Vision": "We see the tiny glass needle—a tool that we have ourselves crafted under the microscope—jerk through the cell wall. We see the lipid oozing out of the end of the needle as we gently turn the screw on a large, thoroughly macroscopic plunger . . . John Dewey's jeers at the 'spectator theory of knowledge' are equally germane for the spectator theory of microscopy" (p. 190).

This analysis is an important achievement for constructivism because it applies the idea that "scientific knowledge is a human creation, made with available material and cultural resources" (Golinsky 1998, p. 6) directly to observation. Observation, needless to mention, has always been epistemology's leading metaphor, and within the empiricist tradition was always assumed to be the fundamental level of knowledge acquisition. But if knowledge is *produced* at its most basic and primitive level—that of direct observation—then there is no more reason to worry about scientific concepts than about everyday ones.

This seems to be the non-scientific support for the use of scientific ontology that Latour requires in order to justify using scientific vocabulary in explaining science. Hacking's line of thought does not come close to insuring that scientific concepts touch "things-in-themselves," but it does suggest that no other way of engaging with these "things" is doing a better job. In other words: if we cannot be assured that in using scientific vocabulary we are actually allowing "things-in-themselves" to participate in "causal accounts" of science, at least we are advised that we have no real choice. There is nothing in hands and microscopes, Hacking tells us, which relates to nature in a more direct or a less problematic way than the vocabulary of the participants, and if this vocabulary happened also to be ours, then so be it. We have, we realized, only one science, and there is no external "epistemological" point of view from which to judge its epistemic claims.

8. Tentative Conclusion

This authorization to follow Latour and remain an upright constructivist may strike one as unsatisfactory specifically because of its skeptical overtones, but it does highlight an important aspect of Latour's dilemma. Hacking's brand of constructivism is anything but social. Entrenched as it is in the traditional epistemology it sets out to challenge, it is *personal* knowledge that *Representing and Intervening* is commonly arguing about, and its examples and analyses are characteristically individualistic in tone.[15] The examples deal with the solitary observer, the single experimenter, the lone expert; rarely are the large systems of "big science" favored by constructivists, or even the whole laboratory explored by Latour, even mentioned. This is a problematic approach not simply because it delegates the public aspect of science to someone else. Science, the social constructivists taught, is public *in essence.* Theories, experiments, mathematical demonstrations—all these claim and gain their epistemic authority in the public realm, and cannot be reduced to the knowledge held privately by individual scientists.[16] But like the previous failure I noted in Hacking's critique of epistemology, this one also has a clear advantage in our context: by applying constructivism strictly and directly to the individual, Hacking avoids the two pitfalls of social constructivism, which Latour pointedly marked out: the assumption of a *sui generis* society; and the assumption of free-for-all data.

From the social constructivist point of view, individualism is too steep a price to pay, even if one gives heed to Latour's complaint. The social character of knowledge in general and science in particular; the principle that "knowledge [is] whatever is collectively endorsed" and "knowledge is better equated with culture than with experience" (Bloor 1976, pp. 3, 12) is too basic a principle for the Strong Programme. It is more deeply entrenched, in fact, than the constructive principle—that knowledge is a human product. Yet it is exactly this individualism that allows Hacking to bring in the notion that knowledge is produced "all the way down"—to direct observation—just as the need "to let society in" forced Bloor to assume a level of agreed-by-all observation, one in which all individualist constituents of the constructive epistemology—skills, expertise, command of instruments—are neutralized, and elementary consensus can be established (see above).

15. As cited above, *Rewriting the Soul* (Hacking 1995) is markedly different in that respect.

16. The most elaborate case for this claim is made by Shapin (1994).

This line of reasoning suggests that Latour's dilemma may be founded on an unexpected conflict between "social" and "constructivism" in their original coupling. The (not necessarily social) constructivist claim that the agency involved in creating knowledge is human, rather than Nature's, is a direct assault *against* the dichotomy between the knowing human and the known Nature. The claim also aims against the sharp distinction between individual and society. To wit, the traditional solipsistic puzzles proceed from assuming Man's detachment from objects to worrying about his loneliness amongst his fellow humans, and constructivism eschews both assumption and worry. The social (but in fact not necessarily) constructivist claim that knowledge is a social entity, on the other hand, creates a strong stake in preserving and strengthening the individual-society dichotomy. Thus, by the same token, it provides a prop *for* the knower-Nature one. The dichotomy between "individual experience" on the one hand, and the "collective vision or visions of reality" which "society furnishes" on the other (Bloor 1976, p. 12) is dependent upon "sustaining the distinction between subject and object, . . . driving a wedge between nature itself and the descriptions of it" (Bloor 1999, p. 94). Since for the adherents of the Strong Programme it was society, with its groups, institutions, interests and practices, which was to provide our knowledge with structure—"stability [of inductive generalizations] is the stability of forms of life or taken-for-granted-practices" (Collins 1985, p. 18)—the "wedge" was necessary; the individual had to be posited as stranger in her own world.

This leads to a somewhat different interpretation of the social-constructive predicament than the one offered by Latour himself. It is not that the Strong Programme lost its verve and courage when confronted with the final application of the symmetry principle, viz., when it had to come to terms with the historicity of the distinction between subjects and objects. It is, rather, that in spite of symmetry being perhaps their greatest claim to fame, the commitment of the Programme's adherents to this principle—which all but embodies constructivism—was less than complete to begin with. As long as the relations between social, individual and Nature were assumed to be what they were, "constructivism" had to be compromised, if it were to be "social."[17]

One may be lead to conclude that there is, in fact, no real dilemma; that the insistence of constructivists like Bloor (1999) to *not* use science's accounts of Nature in their own accounts of science reflects only the inability to fully incorporate their own constructive principles. In particular,

17. One can read Latour's arguments for favoring anthropology over sociology as proceeding along the same line. Cf. his 1993.

they seem to unwittingly share with all empiricists since Bacon the instinctive conviction that *knowledge cannot be both real and constructed,* despite Bloor's excellent arguments why this conviction is supported by nothing but instinct (1976, pp. 5..19).[18] Of course, where their old rivals were careful to steer clear of all idols—all human interventions—the constructivists opted to celebrate those interventions. But by actually shunning science, by refusing to employ its results—their vehement avowal of its method notwithstanding—they are in practice, if not in rhetoric, refusing to accept its claim on truth. They show themselves to accept the same empiricist exclusive disjunction: either science is real or it is constructed, but not both. Without that in-built suspicion of their own constructivism, without this requirement to delineate a space for "the social," so the claim would go, there would be no Latour's dilemma; it would not seem like we are prejudging the historical process of acquiring knowledge about nature by applying our current knowledge of nature in the historical account.

Like the previous suggestion, this resolution strikes one as unsatisfactory. If the basic structures available for the historical account of science remain unchanged, the complaints of traditional social constructivists against Latour's suggestions seem to remain valid, regardless of all philosophical niceties: either knowledge is the outcome of the process or its motor; either the agency is with the human inquirers or with the Nature inquired; either science has a profane history of human interpretation or a sacred one of Nature's revelation. If one wishes to keep constructivism but avoid social-idealism, to re-introduce realism but avoid Whigism, to establish super-symmetry without dismantling symmetry, so it seems, these basic narrative structures should be radically altered, and with them the relations assumed between their main actors—Nature, society and individual.

Latour attempts to do just this.

9. Latour's Solution

Beyond Latour's sometimes heavy metaphorics, which is at least partly responsible for the vehemence in which he is opposed, lies a bold and simple solution: to assign historicity directly to things. Instead of attempting to guarantee the temporal, contingent status of germs by pitting the historically situated Pasteur against the eternally entrenched Nature, Latour

18. Regrettably, in his (1999) Hacking appears to succumbs to this habit as well. Though he says, on p. 68, that epistemological constructivism "is very different from doubting the truth or applicability of any propositions widely held in the natural sciences," the whole tenor of his analysis of constructivism in general is as a type of conspiration-exposing relativism.

suggests, we should affix the sign of time on germs' own sleeve. The worry was that, by letting scientific objects participate in shaping (the outcome of the very historical process that brought about) their own existence and character, we are giving in to the myth of their being a part of a never-changing Nature awaiting discovery. But this is a misplaced worry, stemming from the same dichotomy that Latour explicitly rejects. He does not suggest a new distribution of credit—for germs or light rays—between society on the one hand and nature on the other. Super symmetry means that neither end is a primitive, originary source of agency, but rather that both ends are idealized abstractions of *the real things*—germs and differently refrangible rays—which are both historically situated *and* "out there."

Germs, Latour teaches, do not have to remain passive in order to save their contingency and historicity. Yes, they *did* help Pasteur in his dispute with Pouchet—but they could not have done so before 1857. Until 1854 germs hardly existed, although in 1861, after Pasteur won the Académie's prize for his *Memoire,* they became his main allies. By then their existence had stabilized enough, thanks, largely, to Pasteur's deployment of his experimental, instrumental, rhetorical, cultural and political skills. And, indeed, Pouchet's eggs can also claim credit for his courageous standing; without them, he would have lost the dispute back in 1859. True, by 1864, when the Académie ruled in favor of Pasteur and against Pouchet, they were no longer in a position to help—they were growing extinct. The fabric of experimental, instrumental, rhetorical, cultural and political connections upholding them was becoming undone. This was partly due, of course, to the work of Pasteur.

So dare one say that when Pasteur was sick before 1854, it was due to whatever mysterious reasons, but when Pouchet caught the flu after 1864, he was being infected by vicious microorganisms? Why not? One can of course retort to the more intuitive idea that the young Pasteur was also suffering from the long reach of his yet-to-be-discovered germs; sometimes we extend their efficacy into the much more remote past, as when we apply tuberculosis to the mummy of Ramses II (Latour 2000, pp. 247…251). But we would be better advised to remember that that is exactly what *we* are *doing,* namely, extending and applying, and that this extension and that application are not automatic. Science is most human, most constructed, when it makes its most general and furthest reaching claims; it is there that it resorts to the most complex instruments, most heterogeneous technologies, least rigorous mathematics

How would this approach adjudicate the dispute between Shapiro and Schaffer? Did the Liège group lose their bout with Newton because of their experimental incompetence or was this incompetence the outcome of

Newton's careful efforts to marginalize and discredit their claims? The answer, if we follow Latour, is wholly dependent on the point in time about which the question is asked. By 1678, and definitely after 1704, the Jesuits of Liège were simply incompetent. By then Nature has been shaped to yield Newton's results when properly observed. Similar claims can be made concerning Venetian glass: by 1730 it was much too crude to allow Nature to fully expose itself. Had Hooke and his interests in the colors of thin films won the day back in 1672, this greenish, veined glass might have been necessary equipment for any optician, but by 1704 this was no longer an option. Hooke was dead, and the success of Newton's reflecting telescope back in 1672 set him on a track for the *Principia,* the *Optics,* the presidency of the Royal Society and enough fame and prestige all over Europe to be calling all the shots in all scientific debates. Did Newton have a hand in this change? Most definitely. But neither was he, nor the Royal Society, nor the rest of the London-Cambridge-Liège axis, impervious to the change that began in 1666 when he removed the screen to 20-some feet from the symmetrically placed prism. The process which broke light into primitive colored rays, each equipped with its own index of irrefrangibility, had engulfed all: Nature, society and Newton himself.

10. Conclusions

It is one thing to sympathize with the historiographical and epistemological motivations that Latour discharges by suggesting that *the real things* are hybrids of natural law and social order. It is a wholly different matter to adopt this audacious suggestion. The neat solution of the Shapiro-Schaffer dispute suggests that, from the historiographical point of view, Latour's totemism, his fusion of nature and society, may be a practical methodological approach, even if its successful application still requires some further exercise (Latour's most daring attempt in this direction—his aforementioned *Aramis,* is, to my judgment, only a partial success). It is yet a much more difficult question whether it is also a viable metaphysical position. Playing around with the subject-object dichotomy is a dangerous game, and it remains to be seen how Latour is going to survive it. However, I would like to point out by way of conclusion that the dangers he is facing do not come from any of the expected directions.

Perhaps the most expected one is the allegation of historicism. The instinctive apprehension instigated while reading Latour is that his totemism is nothing but reification of the historical process. In an attempt to avoid naïve realism on the one hand and naïve sociologism on the other, the feeling emerges, Latour falls into naïve historicism; not wanting to assign either Nature or society the responsibility for the creation of things, he assigns it to history.

This is a false allegation. What saves Latour from becoming an SSK Hegelian is not only his irony and light-hearted skepticism that defies the self-righteous systematicity of old historicism. More significant is that his offer to assign historicity to things does not reconstitute history as the ultimate substratum of necessity. Just the opposite: Latour's historization of natural things is an attempt to provide a space for the *contingency* of human knowledge, without falling into the trap of burdening the human knowing subject with more agency than it can or should bear. Contingency, I argued above, is the most fundamental feature of constructivism. It is its contingency that makes science *historical;* a development within human history, rather than a gradual manifestation of reason independent of this history. Thus constructivism, in general, is historical rather than historicist, and Latour's totemism in particular accentuates this point. The disputes between Newton and the Jesuits and between Pasteur and Pouchet, Latour points out, could have just as well gone the other way, and the destiny of rays and germs could have been completely different.

If the charge of Hegelianism turned out to be fairly easy to fend off, one may expect Latour to find the challenge from the realist camp much more devastating. In fact, from the realist point of view, there is one good reason and one bad reason to object to Latour's totemic metaphysics. The bad reason is the seemingly more obvious one, namely, that by adding human history to the make-up of natural things we are abrogating the first principle of realism, viz.: "humankind confronts an objective world, something almost entirely not of our making" (Wright 1986, p. 1). To the degree that such a complaint reflects a pious concern for the independence of the "objective world" from human machinations, it is badly misdirected. Latour's main motivation, one should recall, is *anti*-idealist; he challenges social constructivism to find an aperture through which *things* can enter into *human* history and "*make a difference*" (1999, p. 117, italics in original)—not *vice versa.* Indeed, for that to be achieved without symmetry being compromised—without assuming that things control human history by simply revealing themselves at their heart's desire—we heed to have subjects and objects share one causal structure. In relating human history to natural history—in rejecting idealism—we indeed eschew the total independence of objects from subjects—things cannot be completely indifferent to humans if they are to be causally connected—but this is a far cry from subjecting things to human agency. Latour's world is still "almost entirely not of our making," in spite of the utterly non-standard way in which it is granted this independence. If anything, it is a more "objective world" than usual, as the *human* part of this world appears less "of our making" than we used to think.

This argument will probably strike the realist as utterly unsatisfactory. Latour's way of assuring that the world is "not of our making" is too foreign to commonsense realism to be made palatable with one neat turn of phrase. The intuitive rejection could probably be wrapped with a solid counter-argument, but more interesting still would be to follow the light it sheds on the intellectual instincts behind realism, especially in its anti-skeptic mode. Since Latour does not subject the objective world to the human mind, it appears that the aspect of his proposal that the realist finds so troubling is rather the subjugation of the human mind to the objective world. This suggests the (somehow not completely surprising) possibility that realism was less interested in protecting the objective world against the intervention of its human inhabitants than in preserving human independence, or rather estrangement from that world. This is an intriguing suggestion, partly because such estrangement pits realism against its classic ally—materialism, the belief that everything, *humans included,* is made of one basic substance—matter. There is no *prima facie* reason why anti-materialist realism is not a tenable metaphysical position, but it is probably not one that your run-of-the-mill realist would have expected to find himself holding.

This line of reasoning takes us beyond the scope of this paper. The entanglement of humans and things, I claimed, was the obvious but mistaken reason for a realist to reject Latour's ideas. The less obvious, but much more difficult challenge to the realist wishing to adopt these ideas is their incongruence with the principle with which we have started, viz., that science, though it is historical and contingent, is unique. There is no outside perspective from which the objects of science can be viewed and science's account of them questioned.[19] Yet that is exactly what Latour seems to offer: a claim about the makeup of these objects, supported by non-scientific arguments, which stands in complete opposition to the claims made by science. The uniformity of laws of nature over time and space is perhaps the most basic metaphysical *cum* methodological assumption of science since the early seventeenth century, and it pervades all of science's theoretical and practical work. One may of course decide whether to believe statements based on such assumptions or not, but one cannot purport to be a realist, especially of the anti-representational sort, if one chooses to believe science while rejecting the status it assigns its objects.

19. According to Michael Friedman (1999, esp. pp. 2..11), a very similar realization was at the heart of the scientism of early logical positivism. As Friedman acknowledges, his interpretation is not uncontested (fn. 3, p. 3), but if he is correct, it underscores the similarity between logical positivism and social constructivism discussed above.

This is a very strong argument, but it does not entail, I think, the defeat of constructivism a-la Latour. It is, rather, an *aporia* arrived at following a realist train of thought. Realism, as an attempt to bridge the wall between knowing subject and known Nature, faces the constant embarrassment of finding itself fortifying that wall—the previous argument was just another instance of this phenomenon. If realism were to provide an alternative to oppositional metaphysics and its corresponding visualistic epistemology, it would have to start "from the middle"—from things as we know them. Alas, we know them historically, and as they are part of our history, we are, *ipso facto,* part of theirs. There are no standing grounds from which to view the relations between humans and reality "from sideways on" (McDowell 1994, p. 34), but if the planets had a different effect on European society before and after Copernicus, if germs effected French economy differently after Pasteur, and if we already fully digested and assimilated the understanding that the difference is not well-grasped by the simplistic notion of "discovery," than we are forced to look for this middle kingdom, where human history and natural history meet. That this kingdom is not a place we feel comfortable in is not Latour's fault.

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