

## **Ontologies without Metaphysics: Latour, Harman and the Philosophy of Things<sup>1</sup>**

*Jay Foster*

I have a crazy love of things. I like pliers and scissors. I love cups, rings, and bowls— Not to speak, of course, of hats. I love all things not just the grandest, also the infinitely small. . . . Many things conspired to tell me the whole story. Not only did they touch me, Or my hand touched them: They were so close that they were a part of my being, They were so alive with me that they lived half my life and will die half my death.<sup>2</sup>

Pablo Neruda

### **Common Things**

Like Pablo Neruda, Bruno Latour and Graham Harman have a philosophical affection for common things. Both take themselves to be materialists of a kind, though neither is sanguine about pursuing a materialist program in which the world is reduced to atoms, corpuscles, electrons, quarks or other *materia minima*. Instead, Latour and Harman are materialists who are philosophically curious about the collections of common, material things that make up the world in which we live. Both are attentive to those things that analytic philosophers tend to call “middle-sized objects.” Although intuitively appealing, the mid-sized object has always been a philosophical conceit. It isn’t clear what qualifies as mid-sized on the spectrum between the super-small (quarks, electrons, neutrinos and photons) and the super-large (take the star R136a1 which is 265 times larger than the Earth’s sun). Philosophers at the beginning of the analytic tradition— like Russell, Moore and Wittgenstein—explicitly included tables, chairs and hands in the class of mid-sized objects. This short list was, presumably, representative rather than exhaustive. In more recent analytic writing, the defining feature of a mid-sized object is that it can be picked out using unaided human sensory capacities. It isn’t clear whether spectacles should count as sensory aids. If glasses are sensory aids, then what falls under the definition of mid-sized would seem to vary considerably from person to person. If glasses are

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<sup>1</sup> The thoughtful and helpful comments of Bryan Boddy and Daryn Lehoux are gratefully acknowledged. Thanks to Noah Foster and Jessica Webb for their help, kindness and patience.

<sup>2</sup> Pablo Neruda, “Ode to Common Things,” in Ferris Cook, ed., *Odes to Common Things*, trans. Ken Krabbenhoft (New York: Bulfinch Press, 1994), 11-17.

not aids, then it isn't clear why the objects resolved by telescopes, microscopes and MRI should not qualify as mid-sized objects.<sup>3</sup> Despite these unresolved questions the intuition that frames the concept of the mid-sized object is not optical resolution but that the mid-sized comprises those familiar objects encountered on a day-to-day basis in common human living. For Latour and Harman, if not for analytic philosophers, the list of mid-sized objects is a list of common things. It includes not just tables and chairs, but also Neruda's refulgent list: pliers, scissors, cups, rings, bowls, thimbles, nails, clocks and compasses. Given Harman's self-declared heterodox Heideggerianism, hammers should probably be added to that list.

At least since Bertrand Russell's *Philosophy of Logical Atomism* (1918), it has been one of the main tasks of philosophy to give an account of how tables and chairs are differentiated in the plenum of atoms (broadly conceived) that is the world. The puzzle of logical atomism may be thought of as an analogue of physical atomism: if the world is just a continuum of atoms then what makes the atoms of the table so obviously distinct from the floor and the surrounding air? In a world with some uniform and simple substrate, what if anything, gives nature its joints? Russell, circa 1918, gave a remarkably skeptical answer to these questions. Neither "Socrates" nor "Piccadilly" nor "*The Twelfth Night*" name particular existing things. For the early Russell, as for the early Ludwig Wittgenstein, "the world is the totality of facts, not of things." Facts, not things, *are*. In contrast, Latour and Harman offer thing-philosophies. For Latour, if not Harman, it is things that bring facts into the world, not the other way around. Latour has sometimes identified his thing philosophy as "actor-network theory" (ANT), though he warns enigmatically, "there are four things that do not work with actor-network theory; the word "actor," the word "network," the word "theory" and the hyphen!"<sup>4</sup> Less often, but I think more felicitously, Latour has called his work "a philosophy of techniques."<sup>5</sup> In *The Prince of Networks* (2009), Harman offers the first attempt at a comprehensive synthesis of Latour's *oeuvre* that reads Latour's actor-network theory as dovetailing with Harman's own "object-oriented philosophy" (OOP). As Harman says, "The world is a stage filled with actors; philosophy is object-oriented philosophy."<sup>6</sup>

Perhaps it is the case that Latour's philosophy of techniques can be profitably aligned with Harman's object-oriented philosophy. Perhaps ANT and OOP can help each other, even if it were to turn out that they can't be precisely fitted together. It is certainly the case that Latour and Harman share a deep

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<sup>3</sup> Grover Maxwell, "The Ontological Status of Theoretical Entities," in Herbert Feigl and Grover Maxwell, eds., *Scientific Explanation, Space and Time* (Minneapolis: University of Minnesota Press, 1962), 3-15.

<sup>4</sup> Bruno Latour, "On Recalling ANT," in John Law and John Hassard, eds., *Actor Network Theory and After* (Oxford: Blackwell Publishing 1999, 2004), 15.

<sup>5</sup> Bruno Latour, "A Door Must Be Either Open or Shut: A Little Philosophy of Techniques," in Andrew Feenberg and Alastair Hannay, eds., *Technology and the Politics of Knowledge* (Bloomington: Indiana University Press, 1995), 272-281.

<sup>6</sup> Graham Harman, *The Prince of Networks: Bruno Latour and Metaphysics* (Melbourne: Re.Press, 2009), 16.

affection for things. Things have been enjoying something of a renaissance in cultural theory as well as philosophy of science. Bill Brown claims to be the architect of a new branch of critical theory called “thing theory.” Jane Bennett has produced what she calls a “political ecology of things.” Davis Baird develops an account of “thing knowledge” in which things, rather than propositions or statements, are bearers of justified true beliefs. Lorraine Daston has edited a volume of essays that offer biographies of scientific objects.<sup>7</sup> However, none of this recent work figures in Harman’s object-oriented philosophy. Even somewhat older resources about things receive nary a mention. Michel de Certeau’s philosophy of the everyday gets only a passing glance. Nowhere to be found is Mihaly Csikszentmihalyi and Eugene Rochberg-Halton’s oft-republished analysis of the relationship between domestic objects and the self.<sup>8</sup> Given these available resources, why does Harman see Latour’s philosophy as fitting so closely with object-oriented philosophy? Why Latour? Why no one else except perhaps Alfred N. Whitehead and Martin Heidegger? Harman is entirely open about his specific interest in Latour. His interest is purely metaphysical. Harman thinks that Latour’s work is a veritable goldmine for any future metaphysics. “When the centaur of classical metaphysics is mated with the cheetah of actor-network theory, their offspring is not some hellish monstrosity, but a thoroughbred colt able to carry us a century or more.”<sup>9</sup>

Harman’s object-oriented philosophy comes packaged with no light metaphysics. It is metaphysics in which objects are irresistible, unfathomable posits that are never fully revealed by their relations. *The Prince of Networks* emphasizes the features of Latour’s account that re-enforce this account of objects. Yet, an unusual feature of Harman’s discussion of Latour is that it proceeds as if Latour is already and always was contributing to a systematic philosophy. This is unusual because the locus of Latour’s work is not metaphysics, and for the most part, it is not even philosophy of science. As we shall see in the third section of the paper, his major contributions have been to a comparatively new field of study called “Science and Technology Studies” (STS), which he openly describes as “a booming discipline short of discipline.” That discipline began as the study of physics, chemistry, biology, zoology, primatology and other natural sciences using approaches from history, philosophy, sociology, anthropology and economics. The field began by examining the production and dissemination of knowledge in the natural sciences, but as it matured it went on to study the interconnections between different areas of study. The discipline was characterized by the late Stephen

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<sup>7</sup> Bill Brown, *A Sense of Things: The Object Matter of American Literature* (Chicago: University of Chicago Press, 2003). Jane Bennett, *Vibrant Matter: A Political Ecology of Things* (Durham: Duke University Press, 2010). Lorraine Daston, ed., *Biographies of Scientific Objects* (Chicago: University of Chicago Press, 2000).

<sup>8</sup> Mihaly Csikszentmihalyi and Eugene Rochberg-Halton, *The Meaning of Things: Domestic Symbols and the Self* (Cambridge: Cambridge University Press, 1981). Michel de Certeau, *The Practice of Everyday Life*, trans. Steven Rendell (Berkeley: University of California Press, 1984).

<sup>9</sup> Harman, *Prince of Networks*, 5.

Straker with the maxim, “studies studies studies studies.”<sup>10</sup>(It is left as an exercise for the reader to sort out the nouns, verbs and adjectives.) The field is, in Latour’s view, short of discipline not because it lacks rigor, thoughtfulness or results, but in a fashion reminiscent of Pico della Mirandola, because it substitutes syncretism for specific disciplinary commitments. As Latour acknowledges, how this syncretism was to be understood was a major point of departure from Pierre Bourdieu’s sociology and the Annales School’s historicism and also, as we shall see in the following, the sociology of scientific knowledge.

In *The Prince of Networks*, Harman orients Latour towards philosophical metaphysics, even though Latour’s main contributions have been to this strange new discipline called STS. Harman accomplishes this re-orientation not by arguing that Latour’s position might be *complemented* by the metaphysics of object-oriented philosophy. Latour would almost certainly agree with that point. Rather, Harman makes the much stronger claim that Latour’s work needs a metaphysics in order to be *complete or coherent*. Unsurprisingly, much depends on how the term “metaphysics” is understood. Consider for a moment E.A. Burt’s classic *The Metaphysical Foundations of Modern Science* (1931). Burt’s metaphysics for modern science is “a world of atoms . . . equipped with none but mathematic characteristics and moving according to laws fully statable in mathematical form.”<sup>11</sup> While Burt acknowledges the efficacy of this metaphysics, he also claimed that it overlooked “the big problems involved in the new doctrine of causality, and the inherent ambiguities in the tentative, compromising and rationally inconstruable form of the Cartesian dualism that had been dragged along like a tribal deity.”<sup>12</sup> As Lorraine Daston has observed, Burt’s analysis of science embraces at least three different takes on its metaphysics. He rejects the teleology of Aristotelian metaphysics. He accepts the atomic metaphysics of mathematical physics. And, he adds the caveat that this modern physics includes a number of claims that are metaphysical by virtue of being unsupported by logic or experience.<sup>13</sup> The simple lesson to be learned here is that the phrase “the metaphysics of science” can be invoked to signal the acceptance or rejection of a number of positions. As already noted, both Latour and Harman reject various forms of atomism, and they do so for reasons much like Burt’s. As we shall see in the first section of the paper, unlike Harman, Latour’s solution is not to trade one metaphysics of fundamentals for another. He does not reject atomism in order to adopt the view that there are unfathomable

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<sup>10</sup> Stephen Straker (1942-2004) was a Canadian pioneer in the field of STS who worked in the history and philosophy of science and was the progenitor of the STS program at the University of British Columbia. The program initially bore his name, but its name has since been administratively reformed so that it is suitably aseptic. Straker’s characterization of STS is reported by Katharine C. Wright, a former student of Straker.

<sup>11</sup> E.A. Burt, *The Metaphysical Foundations of Modern Science* (New York: Dover Publications, 1932, 2003), 303.

<sup>12</sup> Burt, *Metaphysical Foundations*, 301.

<sup>13</sup> Lorraine Daston, “History of Science in an Elegiac Mode: E.A. Burt’s Metaphysical Foundations of Modern Physical Science Revisited,” *Isis* 82 (1991): 522-531.

objects and manifest relations. Unlike Burt, Latour resists the Machian impulse to declare as irrational anything not explicable strictly in terms of logic and experience. What is interesting, and I think unique, about Latour is that he retains elements of classical positivism without demanding the elimination of metaphysics.

Harman has quipped that, “Latour is the only philosopher in history to have gone through his early and late phases simultaneously.”<sup>14</sup> I think that this observation has the merit of being roughly correct as well as funny, though for different very reasons than Harman’s. On the reading offered here, Latour’s philosophy of techniques appeared in its mature form in his earliest book *Laboratory Life* (1979). His later work has been an effort to write a prolegomena to that early work. The hallmark of Latour’s mature position is the claim that various specific techniques bring together things, comprised of human and non-human objects, and make and justify the places in which we live and act. Techniques are best thought of as practices that bring things together into recognizable arrangements and simultaneously provide justification for those arrangements. The prolegomena for this mature position has taken so many books and papers to write because Latour is still struggling to develop an adequate neutral monist language that can give rich expression to the philosophy of techniques. To support this view of Latour, this paper proceeds in three sections. The first section draws mainly on a recent debate between Harman and Latour at the London School of Economics to show that Latour is surprisingly indifferent to Harman’s argument that his position demands a metaphysics akin to that of object-oriented philosophy. The second section of the paper returns to *Laboratory Life* (1985) to reconstruct the features of that argument that I think are representative of Latour’s mature position. The third section of the paper situates Latour’s subsequent work in the context of disputes in Science and Technology Studies, particularly the arguments about “social construction.” I show why Latour rejected “social constructivism” in favor of simple “constructivism,” and argue that this rejection underscores the way in which Latour’s techniques include standards of ratiocination as well as material manipulation and intervention. I maintain that, once this encompassing view of technique is adopted, Latour is committed not to a speculative metaphysics of hidden, underlying principles but an explicative or hermeneutic metaphysics that Latour calls, borrowing from Michel Serres, a pragmatogony. The combination of techniques and pragmatogonies, I suggest in the conclusion, leaves us with ontologies without metaphysics.

### ***Harmonices Mundis v.s. Ars Technica***

On 5 February 2008, Latour and Harman met at the London School of Economics to discuss Harman’s book *The Prince of Networks*. The book offers a

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<sup>14</sup> Bruno Latour, Graham Harman, Peter Erdélyi, *The Prince and the Wolf: Latour and Harman at the LSE* (Winchester, UK: Zero Books, 2011), 52.

detailed analysis of the philosophical upshot of Latour's actor-network theory. An edited version of the LSE discussion is published in a slender volume titled, *The Prince and the Wolf* (2011). The meeting between Latour and Harman is described as a "debate" but is probably better described as a continued discussion. The meeting was structured but informal, diverse topics were thoughtfully discussed, and the conversation was affable and good humored from beginning to end. Latour and Harman had met before, in Paris, Cairo, and Amsterdam. The LSE meeting seems to pick up where those conversations left off. The title of the discussion comes from a casual remark made by Latour: "I'm like a dog following its prey, and the prey arrives in the middle of a band of wolves which are called professional philosophers . . . My intention was not to fall in with wolves and to have to answer all of these guys while trying to catch my prey."<sup>15</sup> Latour is an accidental philosopher whose intellectual "home," as he calls it, is in science studies.<sup>16</sup> What Harman finds attractive about Latour (he offers no comment about any other contribution to science studies) is the emphasis on the "irreducible singularity" of things which Latour expresses in his so-called "principle of irreducibility." That principle, sometimes simply called "irreduction," asserts that: "Nothing [no thing] is, by itself, either reducible or irreducible to anything else."<sup>17</sup> Harman sees a natural fit between the principle of irreduction and his position that objects are "unified entities with specific qualities that are autonomous from us and from each other." The aim of Harman's object-oriented philosophy is to ensure that, "things are granted a depth beyond any specific form—deeper than all flowers, coins, and wood."<sup>18</sup>

While Latour and Harman agree that things have an irreducible singularity, past this point their positions rapidly diverge. What is evident in the remainder of the LSE debate is that Latour and Harman both love things but they disagree profoundly about metaphysics. They do not agree about its role, and they do not agree about its place in philosophy. The promptness of that divergence is possibly a consequence of vastly different influences. Harman counts Martin Heidegger among his principal influences, though he claims to offer a radical reading of Heidegger. Harman tells us that Heidegger's tool-analysis is not "a theory of language and human praxis, nor a phenomenology of a small number of useful devices called „tools.‘'" Instead, "Heidegger's account of equipment gives birth to an ontology of *objects themselves*."<sup>19</sup> Latour and Harman may have discussed Heidegger in private conversation but no part of that conversation appears in *The Prince and the Wolf*. The reasons for that might be that Latour almost never has anything good to say about Heidegger. He finds it

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<sup>15</sup> Latour, et al., *Prince and Wolf*, 41. Latour's "prey" is a comprehensive understanding of modern science along with an account of what it might be to have a comprehensive understanding.

<sup>16</sup> Ibid., 81.

<sup>17</sup> Bruno Latour, "Irreductions," in *The Pasteurization of France*, trans. Alan Sheridan and John Law (Cambridge, Mass.: Harvard University Press, 1984, 1988), 158.

<sup>18</sup> Graham Harman, "On the Undermining of Objects," in Levi Bryant, Nick Srnicek and Graham Harman, eds., *The Speculative Turn: Continental Materialism and Realism* (Melbourne: Re.Press, 2011), 22.

<sup>19</sup> Harman, "On the Undermining of Objects," 22.

“baffling that people would take Heidegger’s „philosophy of technology“ seriously.”<sup>20</sup> Heidegger’s account of tools “has the somber and powerful appeal of all tales of decadence” but, in the end, it is “mistaken.” Latour’s stance is that it is impossible to speak of “any sort of mastery in our relations with nonhumans, including their mastery over us.”<sup>21</sup>

Despite Latour’s explicit rejection of Heidegger, one of Harman’s strategies for bringing Latour’s philosophy closer to his own is to highlight the ways that Latour fits with his own heterodox Heideggerianism. In the *Prince of Networks*, Harman claims that, from the perspective of object-oriented philosophy, there are important similarities between Latour and Heidegger:

Just as Latour teaches, there are countless actors of different sizes and types, constantly dueling and negotiating with each other. But objects are not defined by their relations: instead they are what enter into relations in the first place, and their allies can never fully mine their ores. In Heideggerian terms, objects enter relations but withdraw from them as well; objects are built of components but exceed those components. Things exist not in relation, but in a strange sort of vacuum from which they only partly emerge into relations.<sup>22</sup>

To buttress the claim that OOP sits Janus-faced between Heidegger and Latour, Harman highlights the parallels between “the strange sort of vacuum” in which Heideggerian things exist and the concept of “plasma” that appears in Latour. Most STS commentators would probably not count “plasma” among the essential Latour-isms, if they had noticed the concept at all. The concept of plasma makes an appearance at the very end of *Reassembling the Social* (2005), Latour’s excessively programmatic reappraisal of the social sciences. There we are instructed that plasma is “unformatted phenomena” that are “not yet measured, not yet socialized, not yet engaged in metrological chains, and not yet covered, surveyed, mobilized or subjectified.”<sup>23</sup>

Harman draws attention to plasma because it points to a couple of problems in Latour’s argument. The first problem is that if Latour thinks there are wild, formless phenomena that have yet to be domesticated, then Latour should also think that there is “some reserve or reservoir” hidden away within things that is the well-spring of all that unformatted phenomena.<sup>24</sup> What else could be the source of all that new and crazy phenomena? The second problem is that if Latour is claiming that phenomena are given their entire form by being studied, then this would imply that objects get their forms from relations,

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<sup>20</sup> Bruno Latour, “Can We Get Our Materialism Back, Please?” *Isis* 98 (2007), 140.

<sup>21</sup> Bruno Latour, *Pandora’s Hope: Essays on the Reality of Science Studies* (Cambridge, Mass.: Harvard University Press, 1999), 176 and 211-203.

<sup>22</sup> Harman, *Prince of Networks*, 132.

<sup>23</sup> Bruno Latour, *Reassembling the Social: An Introduction to Actor-Network Theory* (Oxford: Oxford University Press, 2005), 244.

<sup>24</sup> Latour, *Reassembling the Social*, 244. Latour, cited in Harman, *Prince of Networks*, 133.

specifically, the relation of being an object of study. That would mean that Latour isn't a true thing philosopher after all, because things are inextricably entwined with their relations, rather than being fully-fledged, self-sufficient objects. The force of Harman's comments on plasma is that, on pain of self-contradiction, Latour *must* have a metaphysics, despite any apparent protestations to the contrary.<sup>25</sup> Harman's hope is that object-oriented philosophy provides Latour with all the metaphysics he needs. The position of OOP is that metaphysics gives us a view of the "furniture of the universe" but that it cannot do so "absolutely and in final form." The reason that metaphysics can give us no final picture is that the universe's furniture, things or objects, have many qualities or properties that are not manifest. Things do take up relations with people and other things, but they also have a kind of inner essence that is fully actualized (and not merely potential) but never fully revealed. Harman denies that this position is not foundational, since that would imply "some sort of enduring foundation that you're going build everything on."<sup>26</sup> Foundations, if they even exist, are elusive since the world is never fully revealed. How then is the metaphysician to proceed? Harman proposes that reality, not foundations, is the proper goal of metaphysics, and reality consists of things. The wrinkle in that straightforward approach is that the reality of things is unknown because things are never fully known by what we glean by observing their relations. That's an insuperable problem, Harman concedes, but nevertheless, reality must be there since there must be something doing the relating that we observe. Latour offers a playful précis of Harman's metaphysics: "It's because things are beyond relations that they have relations." Harman expresses no dissatisfaction with this characterization.<sup>27</sup>

There is much that Latour likes about Harman's description of his philosophy, notably the characterization of his position as the first and, as yet, only "secular occasionalism."<sup>28</sup> That acknowledged, Latour is deeply resistant to the claim that a specific metaphysics is indispensable to his position. In the course of the discussion at the LSE, he does not flinch when an interlocutor suggests that he has an ontology, but on the question of his metaphysics, he only comments that, "if metaphysics is interesting, it is as a method: as travel, as a way of getting new insights."<sup>29</sup> In response to Harman's criticisms, Latour politely avoids mentioning that in *Reassembling the Social* he did not claim that plasma was unformatted, but only that plasma had no *social* form. Given Latour's position, which we shall further develop in the next two sections of this paper, it is perfectly plausible that plasma has *form* without having any specified social form or, for that matter, specified material form. Further, while Harman interprets plasma to be some kind of unformed world of goo, Latour conceives plasma in smaller more local terms. He tells us that plasma is "what allows for

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<sup>25</sup> Harman, *Prince of Networks*, 134. Latour, et al., *Prince and Wolf*, 61.

<sup>26</sup> Latour et al., *Prince and Wolf*, 57 and 68.

<sup>27</sup> Ibid., 62.

<sup>28</sup> Harman, *Prince of Networks*, 159 and 228. Latour, et al., *Prince and Wolf*, 112.

<sup>29</sup> Latour, et al., *Prince and Wolf*, 89 and 59.



the sustenance of a very small pocket of existence in which we are.”<sup>30</sup> Latour’s concern is not to explain existence *into* (whatever that might involve). His project is to explain how the small pockets of existence are sometimes reconfigured or transformed into new modes of existence. For example: how is it that things called ferments were once unequivocally caused by spontaneous generation but now precisely the same things are just as unequivocally caused by germs? How did the same object trade one property for another, completely incompatible, property? Or, to foreshadow the example of the next section, how is it that “souls” were re-written as “brains” and, in the process, how did the seat of the soul move from the pineal gland to the hypothalamus?

Given that Latour and Harman have very different philosophical projects, I suspect that Latour’s interest in object-oriented philosophy probably does not stem from a concern that it provides a metaphysics that ANT lacks but needs. Instead, his interest is that OOP is a thing-philosophy that faces some of the same quandaries as ANT, another thing-philosophy. That might be an uncharitable reading (I do not intend it to be), but it explains why Latour tackles Harman’s criticisms of plasma indirectly. Latour’s reply, such as it is, is to entangle OOP in precisely the same problem that Harman identifies for ANT. Latour points out that if objects themselves are “beyond all relations,” then all we have to go on to understand them is their relations. As he says, “it’s because things are irreducible that the relations have now center stage.”<sup>31</sup> Latour does not deny that things have properties that are not manifest, and might never be manifest, but he is just enough of a positivist to ask: if a thing is completely unrelated to us, if it does not manifest itself in some way, then how we even *know* that it is a thing? Things may have cryptic, hidden qualities nestled in the core of their being, but we only come to know the way things are by way of relations. Now, Harman finds this question muddled since it asks a question that is simultaneously about epistemology and metaphysics. Harman declares himself to be a “traditional” philosopher insofar as he thinks that metaphysical issues are “separate” from epistemological issues. Latour does not entertain this traditional distinction between metaphysics and epistemology, or ontology and epistemology. Questions about how scientists know objects via acquaintance with some but not all their properties are an indispensable part of Latour’s work. Latour does agree with Harman this far: scientific acquaintance with an object *does not* involve discovering its nature or essence. Even if there are such things as essences, it is not clear that scientific methods make them manifest. With access to the inner nature of objects barred, Latour answers his question about the relationship between knowledge and objects by developing a detailed account of the techniques that connect statements with things.

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<sup>30</sup> Ibid., 84.

<sup>31</sup> Ibid., 63.

## Laboratory Life

In early October 1975, Latour joined Roger Guillemin's laboratory at the Salk Institute for Biological Studies in San Diego California as a participant-observer. We might now think of him as an embedded anthropologist. Latour's co-author, Steve Woolgar, described Latour's condition in 1975 as follows: "His knowledge of science was non-existent; his mastery of English was very poor; and he was completely unaware of the existence of the social studies of science . . . he was thus in the classic position of the ethnographer sent to a completely foreign environment."<sup>32</sup> Woolgar overstates the situation to give the impression that Latour was a kind of *tabula rasa*, on an anthropological expedition to squat in the mud-huts of science. It is not clear what it might mean to have knowledge of "science." (How much does a specialist in, say, cladistics know about quantum gravity anyway?) Latour's English probably wasn't all that bad, compared with, say, my French. And, there is a good reason that the young Latour may have been unaware of Anglo-American STS. The major works in the field had yet to be published. David Bloor's *Knowledge and Social Imagery* would not appear until the following year. This does not mean that Latour was without influential companions when he entered Guillemin's lab; Gaston Bachelard, Michel Serres, Louis Althusser, Michel Foucault, Roland Barthes and Pierre Bordieu all went into the lab with him. Of course, it was dumb luck, plain and simple, that the science Latour was studying would win a Nobel prize just two years later. What Latour was studying was not defective or maverick science but exemplary science in action.

In 1977, the Nobel Prize in Physiology or Medicine was awarded to Roger Guillemin, Andrew V. Schally and Rosalyn Yalow. The recipients had all contributed to tracing the chemical function of hormones in the blood. Hormones are difficult to work with since many of them appear in the blood and tissues in trace quantities that are below the detection threshold of conventional chemical assays. Yalow developed techniques for detecting hormones in blood at concentrations of one thousand billionths of a gram per milliliter of blood. She received the Nobel prize for further work, alongside Berson, that demonstrated that minute quantities of some protein hormones could stimulate the production of antibodies, and (as if that were not enough) for developing a general method of using antibodies to measure the concentrations of antigens (like hormone levels in the blood). This technique is sometimes called the Yalow-Berson method or radioimmunoassay (RIA). Berson died in 1972 and Nobels are only bestowed upon the living, so Yalow was the sole recipient of half the prize. The other half was awarded to Guillemin and Schally who, unlike the co-operative duo Yalow and Berson, led competing (and sometimes antagonistic) teams of laboratory scientists who happened to reach the same goal at about the same time. They jointly received half the prize for being, "the first to isolate several of

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<sup>32</sup> Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton: Princeton University Press, 1986), 273.

the communicating chemical links between the brain and the pituitary, and they also determined their structure and succeeded in synthesizing them.” Rolf Luft, the cell-physiologist who was then Chair of the Nobel Committee for Physiology or Medicine, described Guillemin and Schally’s work as the discovery of “a substantial part of the link between body and soul” and “part of the link between the body (soma) and the soul (brain).”<sup>33</sup> What had been discovered that warranted such an enigmatic, if not outrageous, description?

Descartes speculated that the pineal gland was the seat of the soul, but arguably the late-modern soul is seated in the hypothalamus. The hypothalamus is an almond-sized gland that sits at the base of the forebrain of vertebrates just above the brain stem. The hypothalamus is unique because it receives stimuli via nerve connections from the nervous system, and it responds to those stimuli by releasing hormones that change the behavior of other glands and organs. In other words, the hypothalamus is the bridge between the electrical signaling system of the central nervous system and the chemical signaling system of the endocrine system. The hypothalamus is responsible for a variety of bodily functions— including those that maintain homeostasis, like heart rate, temperature and fluid balance—but it exerts most of its hormonal controls by proxy through the pituitary gland, a pea-sized gland that protrudes from the bottom of the hypothalamus. Which hormones are released by the pituitary, and in what concentration they are released, is controlled by hormones secreted from the hypothalamus directly into the pituitary via the hypophyseal portal system, small ducts running from the hypothalamus directly into the anterior pituitary. The pituitary is commonly understood to control growth and development, but it also controls a number of hormones that regulate a variety of bodily functions, among them adrenocorticotropic hormone (ACTH), antidiuretic hormone (ADH), follicle stimulating hormone (FSH), and thyroid stimulating hormone (TSH). As Schally reports, the hypothalamus-pituitary relationship was a matter of conjecture when he began his work in 1954. “Despite a strong circumstantial case favoring hypothalamic control of the pituitary, the proposition would remain speculative until direct evidence for the existence of specific hypothalamic neurotransmitters controlling release of pituitary hormones could be demonstrated.”<sup>34</sup>

A significant project completed by Guillemin and Schally in the late 1960s and early 1970s was to isolate and identify the structure of the molecule by which the hypothalamus signaled the pituitary gland to release a further molecule called thyrotropin that controlled the functions of the thyroid gland. The molecule by which the hypothalamus signaled the pituitary gland to release thyrotropin is called, perhaps unsurprisingly, Thyrotropin Releasing Hormone

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<sup>33</sup>Rolf Luft, “Award Ceremony Speech,” Nobel Prize in Physiology or Medicine 1977. Retrieved 5 July 2011. [http://nobelprize.org/nobel\\_prizes/medicine/laureates/1977/presentation-speech.html](http://nobelprize.org/nobel_prizes/medicine/laureates/1977/presentation-speech.html).

<sup>34</sup>Andrew V. Schally, “Aspects of Hypothalamic Regulation of the Pituitary Gland with Major Emphasis on its Implications for the Control of Reproductive Processes,” *Nobel Lecture*, 8 (1977): 406. Accessed 5 July, 2011. [http://nobelprize.org/nobel\\_prizes/medicine/laureates/1977/schally-lecture.html](http://nobelprize.org/nobel_prizes/medicine/laureates/1977/schally-lecture.html).

(TRH). TRH was Schally's usual name for the hormone; Guillemin generally just called it Thyrotropin Releasing Factor (TRF). Together the hypothalamus and pituitary control not only autonomic and endocrine functions; they are also involved in the physical expression of emotions. As Loft observed in his Nobel address, "When American soldiers were sent to the European war scene, thousands of female companions who were left behind stopped menstruation. They were completely healthy, but the emotional stress had an influence on certain body functions, causing these functions to cease."<sup>35</sup> In addition to stress, shivering, fight-or-flight reactions and a number of other psycho-physical reactions are all hypothalamic responses to stimuli from the central nervous system. It was in just this sense that Guillemin and Schally were said to have discovered the link between body and soul, between bodily function and brain-nerve stimulus. Even if a distinction were to be drawn between mind and brain, after their work it was possible to trace the neuro chemical relations by which the mind ultimately affected the body. And, of course, this tracing project had not been completed *holus bolus*, since only a few brain-body linkages had been examined in detail.

As Guillemin put it in his Nobel lecture, "The isolation and characterization of TRF was the result of an enormous effort. . . . It was of such heuristic significance, that I can say that neuroendocrinology became an established science on that event."<sup>36</sup> Two features of Guillemin's statement are worth highlighting. First, the discovery of TRH (along with the RIA technique) marked the establishment of a new *research program* in the emerging area of neuroscience, specifically neuroendocrinology. That research program would involve tracing the brain-body neurochemical messaging system in increasing detail. Thus, the isolation and identification of TRH not only showed how psychological states in the brain could affect the body (and vice versa), it was what Thomas Kuhn might have called an "exemplar" and Imre Lakatos would have called "a positive research program." TRH was a concrete result that promised further fruitful research that would lead to the production of new and novel facts. Second—and this is the feature of the event with which Latour grapples—he declares that the discovery of TRH was *a heuristic event*. The basic point here is that the techniques and practices developed in the identification of TRH would serve as an effective means of furthering the research program. This approach to research would seem to be good sense, but accepting that alone is not also sufficient reason to think that heuristics are truth-apt. Recall Edgar Zilsel's poignant observation that "even vague and dubious assertions can render good services to empirical research as a heuristic stimulus."<sup>37</sup>

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<sup>35</sup> Luft, "Award Ceremony."

<sup>36</sup> Roger Guillemin, "Peptides in the Brain: The New Endocrinology of the Neuron," *Nobel Lecture* 8 (1977): 366. Accessed 5 July 2011.  
[http://nobelprize.org/nobel\\_prizes/medicine/laureates/1977/guillemin-lecture.html](http://nobelprize.org/nobel_prizes/medicine/laureates/1977/guillemin-lecture.html).

<sup>37</sup> Edgar Zilsel, "Problems of Empiricism," in D. Raven, et al., eds., *The Social Origins of Modern Science* (Springer, Kluwer, 2000), 177.

When Latour entered Guillemin's laboratory in 1975, most of the work on TRH was complete. His anthropological stance was, at least in some part, feigned. It was a device that allowed him to answer the question, "How was the fact of TRH established?" without framing the answer in terms of another philosophical account of the scientific method. For Latour, what was striking about the lab work being done at the Salk Institute was how an object of knowledge and study, like TRH, was at once a number of statements that could be published in a journal and, simultaneously, a thing in the world. Lab talk and lab work seemed to move seamlessly between language and the world, as if there were no representational issues to be overcome. Before 1966-ish, TRH or TRF (take your pick) was a vague placeholder—it was some (we know not yet what) factor or hormone that (if it exists) is the chemical by which the hypothalamus causes the pituitary to release thyrotropin. After about 1966, TRH became an opaque something or other; it was not even clear that it was a peptide, and it might have turned into a chimerical artefact.<sup>38</sup> If science proceeded according to philosophical ideals, then Guillemin and Schally would have proceeded by confirming or falsifying hypotheses about TRH. Perhaps science does proceed in just this fashion, if viewed from a distance through a philosophical lens. But, from Latour's close-up anthropological stance, describing science in this way is a little like reading Tolstoy's *War and Peace* and then describing it as being about Russia.<sup>39</sup> On Latour's richer account, a laboratory does not perform a single operation on statements, like confirming or falsifying, rather "a laboratory is constantly performing operations on statements; adding modalities, citing, enhancing, diminishing, borrowing, and proposing new combinations."<sup>40</sup> While labs transform statements, they do so in specific relationships with things and techniques.

As a thing in the world, TRH, like other hormones, occurs in very scant quantities. Any single hypothalamus only contains about 20 nanograms of the stuff. By 1966, Schally's researchers extracted a whopping 2.8 milligrams of TRH from roughly 100,000 porcine hypothalami, and by 1968, Guillemin's lab isolated a comparatively modest 1 milligram of TRH from about 300,000 ovine hypothalami. From the birds-eye view of a pseudo-anthropologist, the Salk Lab appeared to be a sophisticated abattoir—a production line that gobbled up pig brains and used a variety of resources to transform the brains into articles in science journals. This view of the lab annoys Davis Baird so much that he bluntly describes it as a "travesty."<sup>41</sup> However, the point of this caricature of the lab was not *reductio ad absurdum*. Following Bachelard's *Le matérialisme rationnel* (1953), Latour was pointing out that TRH could not be said to exist, much less identified, without the purification techniques that separated it from the tons of

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<sup>38</sup> Latour and Woolgar, *Laboratory Life*, 134.

<sup>39</sup> With thanks to Woody Allen.

<sup>40</sup> Latour and Woolgar, *Laboratory Life*, 86-87.

<sup>41</sup> Davis Baird, "Thing Knowledge: Outline of a Materialist Theory of Knowledge," in Hans Radder, ed., *The Philosophy of Scientific Experimentation* (Pittsburgh: University of Pittsburgh Press, 2003), 42.

pig and sheep brains and the bioassay techniques that began to approximate its structure. “A fraction only exists by virtue of the process of discrimination.”<sup>42</sup> The “phenomenon-techniques” (to use Bachelard’s phrase) that discriminate TRH are the processes used for its isolation (Sephadex gel filtration, phenol extraction, CMC chromatography and free-flow electrophoresis) and the processes used for its approximate identification (a variety of spectroscopic techniques, including infrared, ultraviolet and nuclear magnetic resonance (NMR) spectroscopy). TRH is inseparable from these techniques of isolation and identification that make it manifest. Its reality, its presence in the world as TRH, depends on those techniques. Without these techniques, TRH is, like pixie dust and Cartesian vortices, pure speculation. As Latour puts it, “the solidity of this object, which prevented it from becoming subjective or artefactual, was constituted by steady accumulation of techniques.”<sup>43</sup>

A crucial part of the TRH story is that 2.8 mg of TRH was too small a quantity with which to identify the precise structure of TRH by the analytical techniques available in the late 1960s and early 1970s. Information about TRH could be gleaned from NMR spectra but the minute quantities of material available meant that this technique was stretched to its detection threshold, and as a result, the information obtained was unspecific. (This issue among others prompted the National Institute of Science [NIS], concerned about the project’s prospects of success, to begin a review of its funding for the research.) Prior to 1968, NMR and enzyme analysis had produced thoroughly ambiguous results: TRH may or may not be a polypeptide; if it were a polypeptide it might be a simple polypeptide; or it might not be a peptide at all. If the structure of TRH were to be positively identified, then it entailed a new strategy for identification or returning to the tedious, time-consuming process of mashing chunks of pig brain. As Schally remarked in an interview with Latour: “The key factor is not the money, it’s the will . . . the brutal force of putting in 60 hours a week for a year to get one million fragments.”<sup>44</sup> Faced with this gut-wrenching task, naturally a decision was made to pursue a new strategy. As Guillemin quietly put it, “the characterization of the molecular structure of TRH was achieved in an unconventional manner.”<sup>45</sup>

The new and unorthodox strategy was to synthesize a compound with a known structure that had all the same biological properties as the TRH extracted from hypothalami. If the synthetic molecule was indistinguishable from hypothalamic TRH, then it could be said that hypothalamic TRH had the same structure as the synthetic compound. Of course, this strategy is not unproblematic—criteria of indistinguishability must be supplied. Biological activity alone might not be enough since it is possible that another compound could behave in many of the same ways as TRH but not be *precisely* TRH. Structural analysis was hampered by the shortage of TRH. Chromatographic

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<sup>42</sup> Latour and Woolgar, *Laboratory Life*, 64.

<sup>43</sup> Latour and Woolgar, *Laboratory Life*, 127.

<sup>44</sup> Schally cited in Latour and Woolgar, *Laboratory Life*, 118.

<sup>45</sup> Guillemin, in “Peptides in the Brain,” 366.

analysis of both TRH and the synthetic molecule suggested they were identical, though the Schally team that conducted analysis thought this result to be more conclusive than the Guillemin team did. The Guillemin team thought that only a mass spectrograph analysis would be conclusive. Mass spectroscopy requires the sample to be volatilized in analysis, and both TRH and the synthetic molecule were resistant to volatilization. Sometime in September 1969, Roger Burgus developed a new technique to volatilize TRH and completed a mass spectroscopic analysis of TRH and the synthetic molecule. A spectrum was obtained “that no one in the field could interpret as being significantly different from that for the synthetic material.”<sup>46</sup> The structure of TRH had been discovered.

Later, Burgus and Guillemin commented that, “this is the first instance of the structure of natural product being determined on the basis of its similarity with a synthetic material.” This unconventional strategy was the new heuristic that Guillemin mentioned in his Nobel lecture. An inferential precedent had been set and a new standard of acceptable argumentation had been established. The Cartesian might scruple—hats and coats might cover automatic machines rather than men—but here the criterion invoked owes more to Lavoisier than Descartes. Lavoisier, exasperated with philosophical pieties, declared an element not to be “simple, indivisible atoms of which matter is composed” but instead “the last point which analysis is capable of reaching.”<sup>47</sup> For a moment Latour adopts Bachelard’s voice and points out, “Aristotle defined a substance as something more than its attribute. In chemistry, however, a substance can be so completely reduced to its attribute that an exactly similar substance can be obtained *de novo*.” Latour continues, apparently following Bachelard: “If the exact structure [of TRH] could be obtained, some of the solidity of chemistry and molecular biology could be injected into endocrinology.”<sup>48</sup> Guillemin may have been expressing just this kind of sensibility when he stated that the discovery of TRH made neuroendocrinology an established science. Although Latour seems to acknowledge that in chemistry and physics an object can be reduced to little more than its attributes, his argument does not rest with either Bachelard or Guillemin. Latour is not quite so reductionist.

With Lavoisier, Latour’s claim is that TRH *was not* discovered by reducing it to its metaphysical substrate, but by its position at the last point that analysis is capable of reaching. The structure of TRH was discovered by building up analytical techniques and aligning those techniques with assertions about TRH. For Latour, the discovery of TRH was not the discovery of a fact or the truth of the statement “TRH is tropic tripeptide hormone with the structure pyrtoGlu-His-Pro-NH<sub>2</sub>.” For Latour, the discovery of TRH is the construction of the practices and techniques that link the statement “TRH is a tropic tripeptide

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<sup>46</sup> Latour and Woolgar, *Laboratory Life*, 149.

<sup>47</sup> Antoine Lavoisier, *Elements of Chemistry*, trans. Robert Kerr (New York: Dover Publications, 1965), xxiv.

<sup>48</sup> Latour and Woolgar, *Laboratory Life*, 143. The reference here is to Bachelard’s *Le nouvel esprit scientifique* (1934).

hormone with the structure pyrtoGlu-His-Pro-NH<sub>2</sub>” all the way back to pig hypothalami. Scientific facts are thus constituted by the laboratory work that ties statements in journal articles and textbooks back to their material basis among things in the world. What makes TRH a fortuitous anthropological study for Latour is not that it was research that contributed to Guillemin and Schally sharing a Nobel. Rather, discovery of TRH involved the invention and adoption of a new laboratory practice of identification that itself entrenched a ratiocinative standard of identity as an explicit standard of disciplinary practice. According to that new disciplinary standard, if it quacks like a duck, looks like a duck and is indistinguishable from a duck *by all available technique*, then it is a duck. From this point on, at least in neuroendrinology, Cartesian worries about Vaucanson’s duck have no rational standing.

### **The Epiphenomena of Techniques**

When the second edition of *Laboratory Life* was published in 1985, it came with a new subtitle. In the intervening six years, some of the major contributions to STS had reached maturity and had begun to appear in book form, notably Harry Collins’ *Changing Order* (1983), Andrew Pickering’s *Constructing Quarks* (1984), and Steven Shapin and Simon Schaffer’s *Leviathan and the Air-Pump* (1985). (Shapin and Shaffer would later receive the prestigious Erasmus prize for this book.) Another three years would see the publication of Steve Fuller’s *Social Epistemology* (1988) and *The Social Construction of Technological Systems* (1987) edited by Wiebe Bijker, Thomas Hughes and Trevor Pinch. The intervening six years had been sufficient time to bring methodological distinctions between different camps of STS into sharper relief. By 1985, Latour (perhaps more than Woolgar) was concerned to distance himself from both the Edinburgh “strong program” in the sociology of science (organized mainly around the work of David Bloor, Barry Barnes and John Henry) and the Bath school of the sociology of science (organized mainly around the work of Harry Collins and Trevor Pinch). Notably what was struck from the sub-title of *Laboratory Life* was a single word, “social.” The 1979 edition of *Lab Life* was subtitled, “The Social Construction of Scientific Facts,” whereas the 1985 edition of *Lab Life* was called “The Construction of Scientific Facts.” The preface to the second edition warned that, “readers tempted to conclude that the main body of the text replicates that of the original are advised to consult Borges,” an allusion to Jorge Luis Borges tale of “Pierre Menard, author of *The Quixote*.”<sup>49</sup>

Prompting the apparently negligible change of title was a major disagreement among STSers. In 1976, David Bloor boldly claimed that the sociology of scientific knowledge (SSK) would explain the “very content and

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<sup>49</sup> Latour and Woolgar, *Laboratory Life*, 7. In that fanciful Borgesian tale, Menard studies the context of *Don Quixote* in such excruciating detail that he is able reproduce the text, word for word in the seventeenth-century Spanish.



nature of scientific knowledge.”<sup>50</sup> Science studiers from the Edinburgh and Bath schools of SSK were in general agreement that the entire *content* of science could be given a sociological explanation. Their argument for this position, in rough outline, had two parts. First, they argued that facts and reasons are always ambiguous. Second, they claimed that since there were no unambiguous facts and reasons it followed that scientific theories, methods and acceptable results are social conventions. A few nanoseconds after this relativist big-bang,<sup>51</sup> science studiers realized that these principles were just as applicable to the work of sociologists of science as they were to scientists. A roiling debate surfaced about how to cope with this issue of “reflexivity.” Edinburgh school sociologists reveled in the narcissistic delights of self-study, while Bath school sociologists took flight to a fully-fledged relativism.<sup>52</sup> In *Lab Life*, Latour and Woolgar had been part of the chorus of the sociology of scientific knowledge. For example, they were willing to assert, “sociologists of knowledge have convincingly argued the case for the social fabrication of science.”<sup>53</sup> But, by 1985, Latour if not Woolgar had begun to re-appraise his position. By striking the word “social” from the title of *Lab Life*, Latour distanced himself the claim that “social” accounts of the content of scientific knowledge had any unique explanatory efficacy. As Latour later expressed his reservation: “„Society“ has to be composed, made up, constructed, established, maintained and assembled. It is no longer to be taken as the hidden source of causality which could be mobilized so as to account for the existence and stability of some other action or behavior.”<sup>54</sup>

Although the world “social” was struck from the title, Latour retained and continues to use the term “construction.” Outside STS, from the standpoint of Anglo-American philosophy, talk of the “construction of facts” was no less alarming than talk of “the social construction of facts.”<sup>55</sup> Talk about the “construction of facts” or the “social construction of facts” is often surrounded by an aura of dramatic unmasking. Facts, which appear to be inevitable and objective, are unmasked and shown to be the contingent outcome of capital, gender, power or consensus among relevant elites.<sup>56</sup> It is a plot-line much like an

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<sup>50</sup> David Bloor, *Knowledge and Social Imagery* (Chicago: University of Chicago Press, 1976, 1991), 1.

<sup>51</sup> This delightful description owes its origin to Stephen Shapin, at least according to Harry Collins. See Harry Collins and Steven Yearley, *Epistemological Chicken*, in Andrew Pickering, ed., *Science and Practice and Culture* (Chicago: University of Chicago Press, 1992), 301-326.

<sup>52</sup> There are more nuanced divisions to this methodological dispute than this account suggests, though it is perfectly adequate for the purposes of this paper. The whole debate is nicely summarized in a series of papers by Michael Lynch, David Bloor, Harry Collins, Steven Yearley, Michael Callon, Bruno Latour that appear in Part 2 of Andrew Pickering, ed., *Science and Practice and Culture*, 215-390.

<sup>53</sup> Latour and Woolgar, *Laboratory Life*, 175.

<sup>54</sup> Bruno Latour, “When Things Strike Back: A Possible Contribution of „Science Studies“ to the Social Sciences,” *British Journal of Sociology* 51 (2000): 113.

<sup>55</sup> See, for example, Paul Boghossian, *Fear of Knowledge: Against Relativism and Constructivism* (Oxford: Oxford University Press, 2007).

<sup>56</sup> The apt term “unmasking” is borrowed from Ian Hacking, *The Social Construction of What?* (Cambridge, Mass.: Harvard University Press, 1999), 53.

episode of *Scooby-Doo*. The specific kind of social constructivism peddled in SSK rankles analytic philosophers and many scientists not simply because it unmask *some* scientific facts as constructions. That kind of weeding work could be thought of as a useful social or intellectual service. The sticking point is that SSK takes as a methodological precept the principle that *all* scientific facts great and small can be unmasked and shown to be the outcome of contingent historical circumstances and negotiation among scientists, not an outcome of the logical evaluation of evidence. The stance Latour developed around 1985, and retroactively super-imposed on *Lab Life*, is radically different from the usual SSK position—it denies that either “nature” or “society” by themselves can be the *explicans* of facts. Latour’s modified stance continues to deny the view that facts are primitive, and so it still departs from the Russell-Wittgenstein position that the world consists of facts, not things. The modified position continues to maintain that facts are “constructed” or “fabricated” by the practices and techniques that produce and maintain the relationship between statements and features of the actual world.

What exactly does it mean to say that facts are constructed or fabricated by the practices and techniques that make and maintain a relationship between statements and things? Let’s try to answer this question by returning to the case of TRH. On Latour’s account, TRH is not a mysterious, hidden fact or mystery that was uncovered or revealed by Guillemin and Schally. Rather, the fact of TRH is made and maintained by specific laboratory practices and techniques. There is nothing mysterious about these practices, and they can be instantiated anywhere; the same “peptide structure discovered in California works in the smallest hospital in Saudi Arabia.”<sup>57</sup> When Latour asserts that TRH *works* in any place with sufficient technical capacity, he is again appealing to the justificatory force of technique. The statement “TRH is a tropic tripeptide hormone with the structure pyrtoGlu-His-Pro-NH<sub>2</sub>” is a fact because that statement can be linked through traceable, reproducible steps of laboratory technique and inferential practices back to the hypothalami of pigs, sheep, cows, rats and other vertebrates. For Latour, facts are the complete spectrum of techniques that align statements and things. The fact of TRH is constituted by: the extraction of secretions from hypothalami; the refinement of that extract by gel filtration, phenol extraction and so forth; its approximate identification by spectroscopy; the synthesis of a molecule isomorphic to the extracted TRH; the mass spectroscopic analysis of the extracted and synthetic TRH (along with a new means for its volatilization); and, last but not least, the ratiocinative principle that behaviorally indistinguishable molecules have the same structure. At one end of this spectrum of techniques are meaty, fleshy objects—100,000 or more vertebrate hypothalami. At the other end of the spectrum is the statement in an article or textbook: “TRH is tropic tripeptide hormone with the structure pyrtoGlu-His-Pro-NH<sub>2</sub>.” For Latour, the *fact* is the whole spectrum of linkages between hypothalami and the statement. In modern laboratory science, the facts

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<sup>57</sup> Latour and Woolgar, *Laboratory Life*, 183.

of the matter are constituted or constructed by the linkages created by practice and technique that hook up things in the world with statements.

In *Lab Life* and *Science in Action* (1987), Latour is noticeably obsessed with “inscription machines.” The purpose of almost every laboratory device and practice is to transform material things into a form that could be published in a journal article. Detractors from the Bath school of SSK sardonically commented that in Latour’s hands science had become “just writing.”<sup>58</sup> Ian Hacking remarked more charitably that Latour’s youthful fixation on writing was “a bracing reminder of that glorious Parisian world of long ago, the late sixties, when inscriptions were the reality and text was substance.”<sup>59</sup> In Latour’s later work, the emphasis on the production of inscriptions all but vanishes. In its place appears a new emphasis on “mediation.” Also in the later work, the terms “construction” and “fabrication” fade from view and the term “constitution” is used with much greater frequency. At first glance, that seems like a mighty shift in position, but for the most part, Latour’s later views bear an eerie resemblance to his early analysis of TRH research in the Salk laboratory. Retained is the important idea that facts are not unified, singular posits but traceable linkages between statements and things. New is a much more refined account of the linkages that constitute facts. In *Lab Life*, laboratory techniques winnow away things in the world, gradually replacing features of the thing with inscriptions about the thing. This leaves open a potential misreading of Latour’s position that takes “things” to be natural and statements to be “social” and so misconstrues facts as bridges between the natural and the social.<sup>60</sup> Later work diminishes the possibility of such a misreading by making both “nature” and “society” themselves epiphenomena of technique.

As shown in the previous section, *Lab Life* invoked Bachelard’s phrase “phenomenon-techniques” as a means of explaining how laboratory techniques produce a phenomenon. Latour extends this position by claiming that the isolation and identification of an entity is only possible given such techniques, and so it is perfectly reasonable to say that the entity only exists by virtue of these techniques’s processes of discrimination. For the early Latour, “it is not simply that phenomena depend on certain material instrumentation; rather the phenomena are thoroughly constituted by the material setting of the laboratory.”<sup>61</sup> Phenomena-techniques reappear in Latour’s later work. For example, in the essay “Circulating Reference” (1999), he tells us: “Phenomena . .

. . are not found at the meeting point between things and the forms of the human mind; phenomena are what circulates all along the reversible chain of transformations, at each step losing some properties to gain others that render

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<sup>58</sup> Collins and Yearley, “Epistemological Chicken,” 379-380.

<sup>59</sup> Hacking, “The Self-Vindication of the Laboratory Sciences,” 35.

<sup>60</sup> Just such a misreading aligns Latour with the sociology of knowledge. For the most part, STSers do not engage in this misreading and recognize that Latour has almost no sympathy with SSK. See, for example, David Bloor, “Anti-Latour,” *Studies in the History and Philosophy of Science* 30 (1999): 81-112.

<sup>61</sup> Latour and Woolgar, *Laboratory Life*, 64.

them compatible with already-established centers of calculation.”<sup>62</sup> This is certainly reminiscent of the old Latour. But, in the old Latour every phenomenon-technique was an “inscription machine” that, to some extent, de-materialized things and transformed them into statements. The work of technique was to convert natural things into social text. But, for the new Latour, both “nature” and “society” are themselves “constituted” by phenomena-techniques, so it is now verboten to given an account of the work of techniques as converting things into text or, more generally, nature into society. Again—and this point cannot be stressed strongly enough—for Latour, the division of phenomena into the categories of “the natural” and “the social” cannot be taken for granted. Any fundamental analysis must show how nature and society, as well as other problematic categories—like person and thing, soul and body—are an outcome of other kinds of processes.

Latour’s unique form of hyperbolic doubt about the categories of analysis has, like so many of his ideas, its distant roots in *Lab Life*. In *Lab Life* a distinction was often drawn between that which is known by a strictly logical method or “demonstration” and that which is known by some other kind of persuasive strategy or “rhetoric.”<sup>63</sup> Against those who maintain that science ought to proceed logically, Latour replies that there is, “an essential contradiction between the use by scientists of procedures which are logical (but sterile) and yet fruitful (but logically incorrect).”<sup>64</sup> As the case of TRH shows, scientists had no qualms about creating an altogether new ratiocinative standard according to which a hormone may be said to have an identical structure with a synthetic molecule that exhibits all the same relevant causal behaviors. That new standard is not irrational by any stretch of the imagination, and it certainly was fruitful, but it also isn’t textbook deductive, demonstrative logic at work. Even in *Lab Life*, Latour was beginning to push this point to its rational terminus: “The list of possible alternatives by which we can evaluate the logic of a deduction is sociologically (rather than logically) determined.”<sup>65</sup> The late Latour might accept the upshot of this claim—that there are always decisions to be made about which practices to accept as constitutive of rational explanation—with the additional proviso that these decisions cannot be adequately explained “sociologically” because “the social” itself is an outcome of decisions about canons of rationality. By the same argument, decisions about canons of rational explanation cannot be

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<sup>62</sup> Latour, *Pandora’s Hope*, 71-72.

<sup>63</sup> This observation is not unique to Latour in the STS literature. Shapin and Schaffer raise a similar issue in *Leviathan and the Air Pump*. It took time and argument to convince philosophers that experimental demonstrations demonstrated anything at all since they didn’t have the form of logical demonstration. See Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life* (Princeton: Princeton University Press, 1982), 110-154.

<sup>64</sup> Latour and Woolgar, *Laboratory Life*, 174.

<sup>65</sup> Latour and Woolgar, *Laboratory Life*, 136.

explained by appeals to “nature” since nature is also an outcome of decisions about which methods of study are rational.<sup>66</sup>

Latour’s later position is not quite a skeptical position since a criterion for decision can always be hammered out if it is not ready to hand. That stance flirts with relativism in a more obvious way than *Lab Life*, since the early work emphasized the social production of facts about nature. But now distinctions between society and nature, persons and things have been abandoned, so the movement of Latour’s early account is suddenly superficial rather than fundamental. Impishly, Latour admits that he once used the phrase “socially constructed” but adds, “I recanted immediately, since I meant something entirely different from what sociologists and their adversaries mean by social.”<sup>67</sup> The extent of Latour’s attempt to eschew almost all conceptual and analytical presuppositions is manifest in the abandonment of the term “fact” in favor of “factishes.” That utterly unforgivable neologism is the monstrous offspring of the hybridization of “fact” with “fetish.” Fetishism, we are told, is the accusation that “believers have projected onto a meaningless object their own beliefs and desires.” Facts are much like fetishes (so defined) because “they have a common element of fabrication.”<sup>68</sup> If these claims are taken as—is with no further attempt at examination or interpretation, then it would appear that we are left with an uber-relativism in which everything is fabricated, even the means of fabrication. If that is the most plausible reading of Latour, then how could we even begin to take seriously his strange assertion that he is, “interested only in retracing our steps back to the moment when the very distinction between content and context has not yet been made”?<sup>69</sup> With what tools, instruments, machines, devices or inference rules could we begin to do this work of retracing? If we make our analytical tools as we go along, as Latour surely intends us to do, then is it not probable that, after all that fabricating, it is those very tools that that have produced the mythical moment when and where social context and natural content have not yet been distinguished?

But if Bruno Latour hasn’t completely lost his mind, then there may be another Latour to salvage from regress, endless *aporia* and potential contradiction. Such a re-reading of Latour could return to *Lab Life* (1985 not 1979) and put epistemological justification and techniques front and center and side by side. This reading would to a great extent gloss over the later archaeological expedition to discover the lost world without persons and things and nature and society. The re-reading would treat that excursion as just another quasi-mythical pragmatogony. Latour borrows this term from Michel Serres’ *Statues* (1987), and it combines the Greek *pragma* (thing) and *gonos* (the

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<sup>66</sup> Bruno Latour, “One More Turn After the Social Turn: Easing Science Studies into the Non-Modern World” in Ernan McMullin, ed., *The Social Dimensions of Science* (Notre Dame: Notre Dame University Press, 1992), 276-289.

<sup>67</sup> Latour, *Pandora’s Hope*, 197.

<sup>68</sup> Latour, *Pandora’s Hope*, 306. Thankfully, Latour acknowledges that the factish is “a complete failure as an invented term,” Latour, et al., *Prince and Wolf*, 102.

<sup>69</sup> Latour, *Pasteurization of France*, 252.

created). Latour reads Serres as saying that, “A pragmatogony is a slow movement that leads through a series of substitutions from a purely social and collective definition of the „thing“ to a definition that requires more and more „objects“ to hold it together. You start with a collective; you end with a collective *plus* a nature, *plus* a technique.”<sup>70</sup> To be clear, the work of the pragmatogony, however frustratingly, hazy and ill-defined the notion may be, is not to offer another story about *homo faber* in which primitive humans fabricate the tools and other objects with which they build a social world that is supervenient on the natural world. Instead, a pragmatogony seems to tell a story about how things, human and non-human, came together in such a way that a specific, local technique became possible. Arguably, the story of TRH is a pragmatogony, since it is the far-from-definitive story of the way in which hypothalami, lab researchers, the Salk Institution, mass spectrometers, research journals, the NSI and filtration gels collected together to constitute the fact of TRH. Later, Latour reflects, “all the interesting realities are no longer captured by the two extremes [nature and society], but are to be found in the substitution, cross-over, translations, through which actants shift their competences.”<sup>71</sup>

Latour’s larger pragmatogony of a primeval conceptual world (if you like, a conceptual Pangaea before its separation into the Gondwana of nature and the Laurasia of society) may be read as a kind of *reductio*, a demonstration of how unfathomable, alien and un-navigable the world is without the familiar techniques by which a semblance of order is secured. Throughout Latour’s mature work, techniques continue to occupy a central place in his inquiry. For instance, he continues to talk about the “non-humans mixed into our collective life through laboratory practice.”<sup>72</sup> He also argues that technique should be conceived in terms that are neither idealist nor materialist. Technique is characterized as more than an idealist projection of human beliefs and desires when Latour says, “techniques are not fetishes, they are unpredictable, not means but mediators, means and ends at the same time.”<sup>73</sup> Further, technique is detached from materialism when Latour declares that, “in artifacts and technologies we do not find the efficacy and stubbornness of matter . . . it is full of engineers and chancellors and lawmakers, commingling their wills and their story lines with those of gravel, concrete paint, and standard calculations.”<sup>74</sup> Elsewhere, Latour simply tells us that techniques are the liminal places where things like tools, instruments, machines, and molecules meet institutions, lab groups, economic scarcity and styles of reasoning. In the midst of any given technique, there is no clear demarcation between the social and the natural, the human and the non-

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<sup>70</sup> Bruno Latour, “The Force and the Reason of Experiment,” in Homer E. Le Grand, ed., *Experimental Inquiries: Historical, Philosophical and Social Studies of Experimentation in Science* (Dordrecht: Kluwer, 1990), 74.

<sup>71</sup> Bruno Latour, “Postmodern? No, Simply Amodern! Steps Toward an Anthropology of Science,” *Studies in the History and Philosophy of Science*, 21 (1990): 170-171. “Actant” is Latour’s neutral monist term for human and non-human things.

<sup>72</sup> Latour, *Pandora’s Hope*, 3.

<sup>73</sup> Latour, *Pandora’s Hope*, 197.

<sup>74</sup> Latour, *Pandora’s Hope*, 190.

human, since “we cannot separate forms of organization from technical practices.”<sup>75</sup> Where and when human and non-human things (or things and non-thing objects) come together in an efficacious technique, there is from that formation organization rather than mere noise or plasma. The emergence of that moment of organization, however it might be portrayed in a myth about its emergence, is indistinguishable from the technique. There is, in short, nothing hidden or cryptic beyond techniques that is to be discovered or decoded except endless myths and pragmatogonies about the ways in which those techniques began, just so.

### **The *Res-publica* of Latour**

Latour is not overly fond of Anglo-American philosophy of science, just as he is probably not overly fond of Californian wine. With jovial acidity he remarks, “If there is one thing that has made philosophy of science so lame, it is to have used mats and cats, mugs and dogs, in order to discover the right frame of mind to decide how we know with accuracy objects such as black holes and fossils, quarks and neutrinos.”<sup>76</sup> He continues this thought in his debate with Harman with the remark, “Complicated objects” are interesting because “we cannot easily do for them what we do with hammers and with ordinary objects . . . science and technology are easier to study because they are a complication and novelty.”<sup>77</sup> This is not to deny that, say, the simple hammer will turn out to be a complicated object if it is given due attention. There are any number of hammers specially constructed to fit them to specific hammering tasks: framing hammers, geological hammers, ballpeen hammers, brass hammers, sledgehammers, jack hammers and short-swing hammers. The list could go on. The nails driven by some of these hammers are similarly various, and while once they were completely specified by length, weight and price, they are now further articulated by ASTM F1667, the “Standard Specification for Driven Fasteners.” There might yet be a Latourian analysis of the hammer, but thus far, bigger meatier things, like TRH, mass transit systems and Pasteurization, have been attention-grabbing because they are the result of techniques that invoke so many dendritic connections between humans and things.

Contra Russell and Wittgentstein, Latour does not hold that the world is made up of facts. For Latour, the world is very much made up of things. What those things are, and what those things can do, is neither mysterious nor obvious. There are facts about things to be learned, but those facts about things are made

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<sup>75</sup> Bruno Latour, “Pragmatogonies: A Mythical Account of Humans and Nonhumans Swap Properties,” *American Behavioral Scientist* 37 (1994): 803. A less specific account is offered in Bruno Latour, *We Have Never Been Modern*, trans. Catherine Porter (Cambridge, Mass.: Harvard University Press, 1993), 82-85.

<sup>76</sup> Bruno Latour, “A Textbook Case Revisited—Knowledge as a Mode of Existence,” in Edward J. Hackett, Olga Amserdamsak, Michael Lynch and Judy Wajcman, eds., *The Handbook of Science and Technology Studies*, 3<sup>rd</sup> ed. (Cambridge, Mass.: MIT Press), 91.

<sup>77</sup> Latour, et al., *Prince and Wolf*, 96.

not found. Laboratories are places where scientific facts are made. In labs, scientific facts are constructed which is to say that they are built-up with techniques and practices. Saying that facts are constructed or constituted need not entail anti-realism or skepticism about facts. Computers, teapots and nails are all constructed and they are all perfectly real. A fact's quality or state of being a fact—what is sometimes unhappily called its facticity—depends not on any specific configuration of matter and form, but on the techniques that sustain it. Thus, Guillemin and Schally did not receive a Nobel prize for their miraculous discovery of the fact of TRH. They did not, so to speak, discover TRH by painstakingly dusting away sand to reveal the hidden fact of TRH—they were, after all, endocrinologists not archeologists. They shared a Nobel for developing the array of techniques by which it is subsequently possible to state that it is a fact that “TRH is a tropic tripeptide hormone with the structure pyrtoGlu-His-Pro-NH<sub>2</sub>.” Many of those techniques did not exist before the work of Guillemin and Schally, most involved the procurement equipment and reagents from laboratory supply companies, all involved highly trained laboratory personnel to deploy the equipment and produce the results, and at least one required convincing others of the surety of a new ratiocinative standard. The fact of TRH depends on the stability of all of these techniques that involve networks or associations among humans and things. To use a language that Latour later rejects, the stability of facts depends on relations among social institutions and arrangements, human-made things and things found in the world. In Latour's later glossy ANT language, facts are networks of human and non-human actants. In an infamous and oft-quoted passage from *Lab Life*, Latour rejects the Russell-Wittgenstein view of facts and replaces it with an account that underscores the role of techniques: “Facts and artifacts do not correspond respectively to true and false statements. Rather, statements lie along a continuum according to the extent to which they refer to their conditions of construction. Up to a certain point on this continuum, the inclusion of reference to the conditions of its construction is necessary for the purposes of persuasion.” The point here is that the term “TRH” refers to a thing in the world, but it does not do so because there is any single principle of facticity that makes it refer. The fact of TRH is not ostensibly established simply by pointing to a test-tube containing a few milligrams of barely visible stuff and saying, “There it is! That's TRH. It's pyrtoGlu-His-Pro-NH<sub>2</sub>.” Nor is there much to be gleaned by asserting that “TRH is tropic tripeptide hormone with the structure pyrtoGlu-His-Pro-NH<sub>2</sub>” is true if and only if TRH is tropic tripeptide hormone with the structure pyrtoGlu-His-Pro-NH<sub>2</sub>. The trouble with the ostensive and disquotational accounts of facts, in Latour's view, is they mistake the relationship between facts and techniques. It is not that we accept only those techniques that generate true statements; rather, we take to be true those statements generated by the techniques we accept. Or, to express the same idea in a different way, both accounts tend attempt to explain *why* TRH is fact without mentioning *how* it became a fact. For Latour, this kind of separation of the context of justification (giving reasons *why* facts are true) and the context of discovery (*how* they were constructed in the first place) tends to hide the



justificatory work done by technique in laboratories and elsewhere. Many, and perhaps most, effective lab techniques are not truth-apt in the sense of involving formal, logical demonstrations. Rather they provide experimental demonstrations by manipulating things to produce consistent and regular phenomena. If lab technique is occluded from the context of justification, then what follows is the mistaken view that there is a gap between the world and language or mind that must be bridged by a philosophical theory of correspondence.<sup>78</sup>

If we go to Wikipedia, or some other heir of Diderot, and lookup TRH, we will find a statement something like the following: “TRH is a tropic tripeptide hormone with the structure pyrtoGlu-His-Pro-NH<sub>2</sub>.” Before 1969, no encyclopedia included any such statement about TRH. Today no encyclopedia could claim to be complete without TRH. Of course, what appears in encyclopedias are statements about TRH, not TRH the thing itself. This is nothing more or less than the problem that beset early modern philosophy—an idea in the mind can resemble an external object in every respect except its existence. Various solutions to the problem have been proposed, none of them entirely satisfying. Since Frege, a widely accepted way of slipping around the problem has been to claim that existence is a property of properties not a property of things, and fortuitously, properties are always already concepts apprehensible by the mind. The Fregean solution comes at a high cost to common things and common sense. In *Philosophy and the Mirror of Nature* (1979), Richard Rorty infamously declared the modern problem to be insoluble. Unlike Rorty, Latour does not think that philosophy has met its Kobayashi Maru, and unlike later-day Fregeans, Latour is unwilling to dispense with things in favor of facts. His philosophy of techniques keeps the world and statements, things and humans, bound together so the question of correspondence does not arise. Like Odysseus, Latour does not make his passage without a sacrifice. What Latour sacrifices to the sea-serpent in order to navigate his North-West passage is precisely Harman’s brand of metaphysics.

If there is a philosopher who likes things more than Bruno Latour, it is Graham Harman. Harman’s world is populated by a blooming-buzzing confusion of things. Things burble through Harman’s writing just like they cascade out of bulging closets in Don DeLillo’s novels. There are “windmills, sunflowers, propane tanks, and Thailand” and “apples, vaccines, subway trains, and radio towers,” not to mention “chefs, biologists, aeronautical engineers and seducers.”<sup>79</sup> And, of course, there are hammers. These things are known by virtue of their relations with us—their visible outline, taste, color, smell and what they do in our world. But these manifest relations are the surfaces of things, mere contact points with us and other things. For Harman, the pressing question of philosophy is how there are any contact points at all among things and between humans and things. The answer, claims Harman, is a thing has a hidden nature tucked away, deep down in itself, an inner reserve or reservoir that exceeds all

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<sup>78</sup> Latour, *Pandora’s Hope*, 69.

<sup>79</sup> Harman, *Prince of Networks*, 5, 13 and 14.

relations. Only this metaphysical reserve can explain the obvious fact that there are any relations at all. Latour is genuinely puzzled by Harman's position, though he is perfectly willing to acknowledge that, "causes, stones and facts never occupy the position of the thing-in-itself."<sup>80</sup> Latour says there has never been a philosopher who thinks there is nothing to things beyond relations. Even Hume, continues Latour, thinks there is something beyond relations; he just does not know how to get there with the philosophical tools available to him. Whereas Harman claims that the fact of relations shows there must be something beyond relations, Latour observes that such a beyond is inaccessible. Things may well be more than their manifest qualities, but they are only *known* to us by way of relations. In a similar fashion, the great experimental philosopher Lavoisier replied to a different kind of metaphysician many years ago: "If, by the term elements, we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable that we know nothing at all about them."<sup>81</sup>

Following Serres at arms-length, Latour does not deny the possibility of things having essences beyond relations, but this "beyond" is not first philosophy. The beyond is part of the mythical, poetic and narrative background that is continually drawn upon to reshape established techniques, and to motivate that reshaping. This kind of metaphysics is loved only for its "practical consequences." Such a metaphysics is not explanatory but inspirational. It is retained for its ability to take inquiry in new and surprising directions, as James Lovelock did when he asked whether the Earth is an organism that maintains homeostasis and drew on the myth of Gaia to inform his answer.<sup>82</sup> Thus, while Harman and Latour both offer a philosophy of ordinary objects that is a materialist philosophy without materialism, ANT and OOP have very different relations with their respective metaphysics. If ANT has a metaphysics at all, it is a weak metaphysics with a post-Humean flavor. There is no science of being *qua* being, and in its stead is only an anthropology of the techniques by which persons and things are collectively articulated. The metaphysical work, such as it is, is the anthropological work of giving a description of the processes by which specific categories of being came into view, and on occasion, passed away. In other words, Latour's thing philosophy is a philosophy of techniques that leads to an ontology without metaphysics. If pressed to give a further account of the origin of things, the Latourian has little recourse but to offer a mytho-poetic pragmatogony. If pressed to give a further account of the origin of techniques beyond an anthropological description, the Latourian has little recourse but to say, "This is simply what I do."

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<sup>80</sup> Latour, *We Have Never Been Modern*, 83.

<sup>81</sup> Antoine Lavoisier, *Elements of Chemistry*, trans. Robert Kerr (New York: Dover Publications, 1965), xxiv.

<sup>82</sup> Latour, et al., *Prince and Wolf*, 118.