Chapter 13
The Robustness of Science and the Dance of Agency

Andrew Pickering

‘Robustness’ can mean many things. Wimsatt’s (1981) classic discussion of robustness in science has an epistemological slant: a scientific result or finding is robust to the extent that it is derivable in multiple and independent ways. Here I come at the problematic from an ontological angle (though epistemology will come into the story too), emphasising a certain robustness that one can associate with the materiality of scientific culture.

To set up the problematic of this essay I find it helpful to think about two stock images of science. One derives from a common-sense view: scientific knowledge is true knowledge of how the world is. On this view, science is not just robust; it is as solid as a rock, given by the world itself. Importantly, it is absolutely *other* to its producers and users. We humans do not have any choice in the matter: the acceleration due to gravity just is 32 feet per second squared. The other stock image of science is the inverse of this. This is the idea of science as a ‘mere social construction’—something put together by human beings to suit their interests or to fit in with their social structure or whatever. Here science appears, not as rock-solid, but as extremely soggy, as if any form of knowledge can be projected onto an indifferent and unresisting world. As Barry Barnes (1994) remarks, the world doesn’t care what we say about it, so we can say whatever we like. On this view, the otherness of science vanishes. All of the responsibility for specific knowledge claims rests on its human producers, and none on the world itself.

The tension set up by these two mirror-images of science creates the need for a concept like robustness. It is very difficult to put all the weight on the world in accounting for scientific beliefs. Empirical studies seem to point relentlessly to the conclusion that science really is a social construction. The only question that is left is whether it’s *merely* a social construction (Pickering 1990). And I take it that speaking of robustness is, then, a way of trying to articulate a sense in which the

A. Pickering (✉)
Department of Sociology and Philosophy, University of Exeter, Exeter EX4 4RJ, UK

Department of Sociology, Kyung Hee University, Seoul, Korea
e-mail: a.r.pickering@exeter.ac.uk

L. Soler et al. (eds.), *Characterizing the Robustness of Science*, Boston Studies in the Philosophy of Science 292, DOI 10.1007/978-94-007-2759-5_13,

'mere' disappears, of trying to get at the idea that the world really can take some of the credit for scientific beliefs, even while acknowledging that they are socially constructed.

That is the line I want to take here. Drawing primarily on the studies and ideas that I set out in *The Mangle of Practice* (1995a), I seek to clarify my own ontological way of grasping the robustness of science, and then ask how this speaks to some traditional philosophical problematics.

The practice turn in science studies (Pickering 1992) has proved destabilising for the philosophy of science, beginning a slide from epistemology to ontology. If the traditional philosophical concern is with what scientists know, the practice turn encourages us to pay attention to what scientists do, and it turns out that one thing that scientists do is to pay close attention to what the world does. So we move from an interest in the doings of scientists to an interest in how nature itself performs and in the coupling of the two. This interest in material performance and agency is by no means a central concern of mainstream English-speaking philosophy of science, but I am convinced it is the place to start in thinking about the robustness of science. If there is a certain nonhuman toughness about scientific knowledge, it is grounded in performative (not cognitive) relations with the material world. That is what I want to discuss first.

Just how do scientists intersect with the material world? Very often not directly with their objects of study; much more often with machines and instruments that generate data for downstream processing. So what does the interaction with machines and instruments look like?

In *The Mangle*, I argued that it takes the form of a *dance of agency* between the human and the nonhuman. In their research, scientists seem to oscillate between bursts of what Ludwik Fleck (1979) called phases of *activity* and *passivity*. In the active phase, scientists are genuine agents, setting up their apparatus this way or that. In the passive phase, they stand back and see what happens. And we can symmetrise the picture by saying that in the phase of human passivity nature is itself active, a genuine agent, doing whatever it will, quite independently of human goals and desires. And then the human agents resume the active role, reconfiguring their apparatus in the light of what they have just found out about how it performs. And then they stand back again and nature resumes the active role, and so on, back and forth—this is what I call the dance of agency. In Chapter 2 of *The Mangle* I dissected this process as best I could in the history of the bubble chamber as an instrument for detecting elementary particles. En route to his Nobel prize, Donald Glaser experimented with all sorts of material systems, putting them together and then literally standing back, with a movie camera in his hand, to record their performance; and then he redesigned and reconceptualised them in the light of that, and tried again to see what the new version would do, and so on.

Over the course of many iterations of this back and forth process, the material form and the material performance of his chambers changed beyond recognition—they were mangled, as I put it—and eventually Glaser arrived at a new instrument, the bubble chamber, that was indeed extremely useful in experimental physics.

So, this is all very simple, but still I think this sort of dance of agency is what we need to focus on if we want to appreciate the robustness of science, so let me continue the analysis. The first point to make is that the bubble chamber was undeniably a human construction. Glaser's active agency was constitutive of its production: he imagined the possibility of constructing a new kind of particle detector, and he put together and reconfigured all of its parts. But we can see at once that the chamber was not a mere social construction. The world may not care what we say about it, but it certainly cares what we do and vice versa. Glaser did not will the chamber into existence; his agency intertwined with material agency in a constitutive way in a dance that he could not control. He had to *find out* what matter will do when arranged this way or that, and this, I think, is the primary sense in which the world enters constitutively into science—and the primary sense in which science is a robust enterprise and not a mere construction.

And I can put this point perhaps more strongly. There was nothing robust about this dance of agency in itself. It was fluid and evolved open-endedly in time. But I think it nevertheless makes sense to speak of the robustness of its product, the bubble chamber. The important thing about the chamber was that it stood apart from Glaser and operated reliably on its own. It was, as I would say, a *free-standing machine* which manifested a sort of *practical duality* of the human and the non-human (Pickering 2009)—it was a material object that acted in the material world quite independently of Glaser or anyone else. Though I need to qualify this idea in a minute, it is worth appreciating the extent to which this does point to a sort of absolute toughness and inhumanity of science, a sense in which science produces and incorporates into itself an utterly inhuman material agency. If the word 'robustness' connects to a feeling that there is something admirable, wonderful, awesome, about science, I personally would locate that feeling in the achievement of such free-standing machines.

So, thinking about the dance of agency and its products quickly and easily gives us an ontological sense of the robustness of science, of how the world itself constitutively enters into science and why science is not a mere social construction. And I therefore want to make a couple of comments on what we have seen so far, before moving on.

One is simply to note that while we have gained an appreciation of the robustness of science I have not yet said anything about scientific knowledge. The dance of agency here was a performative dance not a cognitive one. The bubble chamber itself performed; it was not an idea; it did something in the world. I think philosophy of science, with its epistemological obsession, has spent a long time looking in the wrong place for robustness.

Second, I need to say something more about the otherness of the bubble chamber as a material device. On the one hand, the chamber was a free-standing machine that acted in the world independently of its human users. In that sense it really was other to humanity. On the other hand, we need to remember that the chamber only counted as a successful capture of material agency, as I called it, in a certain social field. At any other time in history it would have looked like a pile of useless junk;

only within a certain configuration of scientific culture did it count as a novel device for detecting elementary particles.

It is at this point that thoughts of *mere* social construction return, as if the state of scientific culture somehow conjured the chamber into existence (which is how Emile Durkheim understood the relation between the social and the technological). To fend off the temptation to think this way, however, we need only to remember that in this instance scientific culture was also reconfigured. In his dance of agency Glaser developed both a new instrument and a form of life that could accommodate it—in accelerator-based physics (rather than cosmic-ray physics, which is where Glaser started off), built and operated by large teams of physicists and engineers, and not by the lone researcher (which Glaser had been when he began his research). So the social did not, so to speak, call all the shots here, and we can hang onto the sense of the otherness of the chamber, as something constitutive of transformations of scientific culture, as something that transmits a certain material otherness to scientific culture, but we should not think of this otherness as absolute—the chamber did not force itself on some passive human world like an alien descending from Mars. We can admire machines and respect their robustness without having to factor out the human side of the dance of agency.

Now I want to widen the discussion. I said that to get hold of the robustness of science one should start with material practice, but that does not mean that scientific knowledge is not important and interesting and that one should not think about it. I have in the past worked through a couple of detailed studies of the production of experimental facts in science, one of Giacomo Morpurgo's quark-search experiments, Chapter 3 of *The Mangle*, the other of the discovery of the weak neutral current (Pickering 1984a, and see Chapter 10, this volume) Both manifest the features I would like to foreground here, but since the quark-search experiments have always been my touchstone for thinking about practice let me talk about them.

The early phases of Morpurgo's experiments were isomorphous with Glaser's. Morpurgo aimed to develop an apparatus that would reliably do something, a free-standing machine—in this case a gadget that would levitate particles of graphite in an electrical field. But the next phase added something new: now he tried to use the apparatus to measure the charges on the graphite particles, looking for the third-integral charges which would signal the presence of isolated quarks. And I find it striking how difficult this simple measurement turned out to be. Again one finds a sequence of active and passive moves in dances of agency, trying this configuration of the apparatus then that, seeing what readings came out of them, reconfiguring the apparatus and thinking about it again, and so on. With one early configuration, Morpurgo indeed found evidence for free quarks. Then he widened the separation of the plates that set up the electric field and the evidence went away. Then he redid the calculations in simple electrostatics that had suggested that the plates should be close together and concluded instead that they should be far apart. This was his first achievement of interactive stabilisation, a point at which the dance of agency extinguished itself in terms of a now precisely tuned machine plus a precisely tuned set of interpretive resources, a place at which his practice could rest and he could publish some results, an articulated fact—namely, the absence of free quarks on some specified amount of matter.

How should we think about this episode, which I take to be typical of empirical knowledge-production in science? The first point might be that any sense of robustness can easily vanish here. Certainly Morpurgo, like Glaser, had built a free-standing machine that performed on its own in the generation of facts. Matter got into the story that way. But the performance of this machine and the conceptual interpretations that Morpurgo wove around it appear to be tied together in a damagingly circular fashion. That this configuration of the machine rather than that was the right one could only be argued on the basis of an interpretive model of the machine, but the rightness of that model was not self-evident and was only guaranteed by the fact that the results obtained fitted in with yet another theoretical model, concerning the presence or absence of free quarks. It is important to see the force of this argument, I think, and it seems to point us back towards an understanding of scientific knowledge as a mere social construct.

How can we escape from this line of thought? As follows, I think. I once used a concept of ‘plasticity’ to analyse Morpurgo’s practice (Pickering 1989), a concept picked up by Ian Hacking (1992). The idea was that the material form of the apparatus and the conceptual form of Morpurgo’s understanding of it were not fixed—that they could be bent around and changed open-endedly until they somehow went together and reinforced one another. The trouble with this concept is that it goes very nicely with ideas of the circularity of knowledge production. All that scientists have to do, on this view, is mould the different elements of scientific culture so they fit together. What one cannot get at with this talk of plasticity is why the production of scientific facts is a difficult and uncertain business that can easily fail. And the question thus becomes: where does the plasticity metaphor go wrong?

The point to note is that, while scientists can certainly assemble cultural resources however they like, they cannot know how they will then perform. This gets us back to the dance of agency in an extended sense. Just as Morpurgo, like Glaser, could not know in advance how his material apparatus would perform when configured this way or that, nor could he know in advance where certain theoretical assumptions and approximations would lead him. It just turned out that when he started with one set of plausible assumptions in electrostatics he was led to conclude that the metal plates in his apparatus should be as close together as possible, and that with some modified assumptions he was led to conclude the opposite. So at the conceptual as well as the material level the plasticity metaphor fails, precisely in that while scientists can tinker with their resources however they like, they have genuinely to find out what the upshot of that will be.

So what we have in this instance is not mere social construction but a set of coupled findings-out—finding out where some theoretical calculations will lead, and finding out how an instrument will perform. Again we have to note that Morpurgo was not in control of either of these processes of finding out, nor of whether their products would fit together and *interactively stabilise* one another, as I called it. This was a chancy process, that could have failed. It is a non-trivial historical fact that the performance of the material instrument eventually hung together with one of Morpurgo's theoretical estimates; it did not have to turn out that way at all; they might not have done so; the experiment could have turned out quite differently, both materially and conceptually.

And here again, then, we can salvage a sense of the robustness of science, now of scientific knowledge. Scientific knowledge is not a mere construction, projected onto a passive nature by scientists. The material performance of instruments is indeed constitutive of the knowledge they produce, though prior scientific conceptualisations of the world are constitutive too, and this in an irrevocably intertwined fashion. At the same time, as I said before, this sense of robustness is not one of unsituated otherness. Our knowledge is *our* knowledge, conditioned by the culture it is made from, as we can see in this example, not something forced upon us by nature itself.

I have probably said enough about my overall picture of the robustness of science and how it arises in dances of agency, but I just want to add that I think the picture sketched out so far can be readily extended in all sorts of directions. I think, for example, that one can find dances of agency and interactive stabilisations in purely conceptual practice as well as in the more material strata of science. That was what I just suggested in connection with Morpurgo's interpretive models of his apparatus, and I argued it at length in Chapter 4 of *The Mangle*, taking as my example William Rowan Hamilton's 19th century construction of the mathematical system of quaternions. There I introduced a concept of *disciplinary agency* as a way of talking about what carried Hamilton along to unpredictable places in his development of various algebraic and geometrical formulations, and hence as a way of getting at the non-triviality, the robustness, of interactive stabilisations in purely conceptual systems. I also argued in *The Mangle* that the overall form of my analysis was *scale-invariant*, and that dances of agency punctuated by moments of interactive stabilisation can be found on the macro-historical scale as well as the micro—a claim I tried to exemplify in two later case studies: one of the history of organic chemistry and the synthetic dye industry in the 19th century, the other of coupled transformations of science, technology, society and warfare in and after WWII (Pickering 1995b, 2005). My argument remains, then, that the mangle is a sort of *theory of everything*—though not of the reductive sort beloved of particle theorists.

Now I want to examine the mangle and its associated conception of robustness from some more angles and in relation to various philosophical problematics.

We could start with the problematic of realism. As I said at the beginning, correspondence realism is, I suppose, the starting point for all this talk about robustness. The truth of nature, if scientists had it, would be the ultimate form of robustness. It might therefore be worthwhile examining further just where my analysis departs from realism.

The most obvious departure is that my story of robustness is, as I said before, not in the first instance about knowledge at all. It is about performance in the material world, about what I once called science's *machinic grip* on the world (Pickering 1995a). I find the endless proliferation of free-standing machines and instruments in science enormously impressive—that is where I look for an explanation of the feelings of robustness that science inspires; that is where the otherness of the world enters into science. We can, as I just indicated, draw articulated knowledge into the same picture, by recognising that knowledge production depends on achieving chancy and highly non-trivial alignments and interactive stabilisations of conceptual structures and material performances. One might, then, try to argue that such alignments point to correspondences between the knowledge produced and its objects in the world, but I cannot think why they should, and what interests me most is that my account somehow *defangs* realism and makes it a less pressing topic. If one has no other account of the robustness of science then realism seems very important, the only way to underwrite our intuitions about the otherness of science. But if one has an account like the one offered here, then maybe we could just forget about the entire topic of correspondence: who needs it? We can see and talk about the fact that science is an immensely formidable edifice, by no means a mere construct, without this implausible manoeuvre of picking on one bit of scientific culture—knowledge—and trying to persuade ourselves that it corresponds to the hidden order of the world.

And we could go further with this line of thought. Realism depends on an intuition of *uniqueness*: the world just is one way or the other; science either gets it right or wrong; it would be madness to say that science just gets it wrong; therefore we have to be realists. Ian Hacking, in his book, *The Social Construction of What?* (1999), got at this idea with his conception of 'sticking points'—the points at which scientists resist any kind of constructivist argument. As an example of such a sticking point, Hacking mentions Maxwell's laws of electromagnetism. He himself seems to think that if something like physics were to flourish anywhere in the universe it would eventually have to articulate something like Maxwell's laws; those laws would just impose themselves on the scientists; they are absolutely other to us.

How can I respond to this? I am inclined simply to disagree with Hacking and the physicists for whom he speaks, but I cannot see any way to settle the matter directly. What I can do is discuss the ontological visions that divide us. Hacking's sticking points make sense in a world that really is structured more or less as physicists now describe it. And if it is that sort of place, then probably we were indeed doomed to arrive at Maxwell's laws whether we liked it or not. But everything I have learned from looking into the history of science speaks to me of a world that is not that sort of place at all. It speaks to me of a world that is endlessly rich in endless ways, that can always surprise us in its performance. It is indeed a non-trivial fact that science can latch onto the world in the construction of free-standing machines but, as I have tried to argue, what counts as a free-standing machine depends on who we are and what sort of things we want to do. Within the culture of 1950s particle physics the world revealed itself to us in the shape of the bubble chamber. In the culture of the 1960s, it revealed itself to Morpurgo as having no free quarks. But I find it easy to imagine that different cultures could have elicited quite different machines and instruments and material performances from the world; and I can see no reason not to imagine that. Hacking's sticking points, from this perspective, are facts not about the material world but about the scientific imagination, and its inability to recognise the richness of the world that the history of science itself displays for us.

Another way to put the same point is to note that the analysis of practice I set out in *The Mangle* is an *evolutionary* one, precisely analogous to the evolution of species in a responsive environment. It is a story of continual open-ended searches through spaces of material, disciplinary and social agency and performance, in which specific historical trajectories are marked out by contingent and emergent productive alignments between these elements. This means that transformations of scientific culture in time are *path-dependent*; starting points matter and so do the contingencies that happen along the way. Different starting points and different contingencies should thus be expected to lead to different futures. Stephen J Gould's (1989) vision of biological evolution was that if we could rewind the clock and start the process again then the course of subsequent evolution would be different, leading to a quite different biological world from the one we have now. I think just the same could be said about the history of science. And just as there are no sticking points in biological evolution, I think there are none in the history and future of science.

This is, of course, to juxtapose two visions of what the world is like, and not, as I said, to settle the matter. But I could try to throw a bit more weight on my side of the scales, which will at least widen the field of discussion. I do not know much about the history of Maxwell's theory of electromagnetism, so I cannot argue about that, but I do know about the history of quark-search experiments, and what I know about them increases my aversion to the intuitions of uniqueness that underlie realist philosophy. I think here of the following.

I have discussed my analysis of Morpurgo's quark-search experiments as an explication of the robustness of scientific knowledge in its difficult and chancy alignment with the performance of machines and instruments. But in this case at least robustness evidently did not imply uniqueness. Over just the same period when Morpurgo was reporting his inability to find any evidence for the existence of free quarks on ever increasing quantities of matter, William Fairbank at Stanford was reporting that he could indeed find such evidence. Using apparatus that differed only in specifics from Morpurgo, Fairbank reported several findings of the fractional charges that pointed to free quarks, and a debate grew that encompassed more and more people.

What should we make of this? Two points strike me. The first is that Fairbank achieved just as much of a machinic grip on the world as Morpurgo. Everything I said about Morpurgo's experiments I could have said about Fairbank's. Fairbank's knowledge claims were just as robust in this sense as Morpurgo's. And the conclusion I draw from this is that one should not go overboard about robustness. There is something tough and admirable about articulated scientific knowledge—it is not a mere construction—but this controversy reminds us that it remains situated, relative to particular configurations of material and conceptual resources (Chapter 6 of Pickering 1995a).

Second, we can note that this controversy was eventually settled in practice: Morpurgo's results were taken to be true and Fairbank's false. But the question remains of how this settlement was achieved. Like all of the controversies I have examined in the history of science, there was, in fact, nothing striking or decisive about its ending. One can speak here of yet more manglings of the material, the conceptual and the social, but nothing qualitatively new emerged. Perhaps the best one can say is that the robustness of Morpurgo's results was socially amplified here, as an increasing number of physicists became involved and failed to find either more evidence of free quarks or any productive way of reconciling Fairbanks' results with his. But no-one, I think, claimed that this episode put Fairbanks' claims definitively to rest. At the most empirical level, a great deal of scrutiny of the material and conceptual bases of Morpurgo's and Fairbank's claims turned out not to lead to any solid ground, but came to hinge on details of the electrical behaviour of metal plates they used, something that no-one knew much about. I am inclined to say that the two sets of experiments were *incommensurable* in Kuhn (1970) and Feyerabend's (1975) sense: it proved impossible to find a common measure against which to adjudicate between them.[1]

And to conclude, I want to mention another example of incommensurability, now at the macro-rather than the micro-level. In my book *Constructing Quarks* (1984b), I claimed that one can find two incommensurable regimes in the history of particle physics which I called the old and the new physics. Without going into detail, these understood the world of elementary particles in two very different ways, in terms of very different theoretical models with little overlap. The old physics spoke in terms of constituent quarks and Regge poles; the new physics still spoke of quarks, but now as field theoretic entities, accompanied by a host of other such entities such as gluons and intermediate vector bosons. And the point of calling these historical formations incommensurable, as far as I was concerned, was that they latched onto the material world in different ways. They spoke to different fields of data that were generated by different fields of machines and instruments (colliders instead of accelerators, detectors tuned to 'hard scattering' phenomena rather than 'soft,' computer algorithms that further filtered the data one way or the other).

This macro-incommensurability, then, consisted in almost disjoint machinic grips on the world: the data produced by the machines and instruments of the old physics had almost no bearing on the theoretical concerns of the new physics and vice versa. As Kuhn (1970) put it, the old and the new physics *lived in different worlds*, here in a very down to earth sense. Both, I would say, were admirably robust in the terms sketched out here. There is no suggestion that either was a mere construct. And both, in this macro-example, were able to sustain the practice of large numbers of physicists—so simple social multiplication is clearly not decisive as far

[1] Franklin (1986) notes that at a late stage in the controversy, Luis Alvarez at Berkeley proposed that Fairbank carry out a blind version of his experiment, which is said to have produced random results. With the exception of one mention in a PhD dissertation this story made no appearance in the scientific literature and was not the subject of any technical discussion, so I have not pursued it further. Clearly Fairbank himself did not regard it as definitively setttling the matter: as the *New York Times* reported in an obituary on 3 October 1989: 'Although Dr. Fairbank retired two years ago as physics professor at Stanford University, he had been at work there the night before his death, trying to verify his report of 11 years ago concerning the existence of individual subatomic particles called quarks' (Sullivan 1989). For an account of a very public attempt by members of Alvarez' group to end another scientific controversy at much the same time, by discrediting a Berkeley colleague, see Pickering (1981). The leader of this attempt got so carried away that he accidentally destroyed the nuclear emulsion which was the key piece of evidence in a claim to have discovered a magnetic monpole.

as robustness is concerned. It is, of course, the case that the new physics largely displaced the old in the course of the 1970s, but again I could not find anything philosophically decisive about this process. In the end, the old physics remained viable but was starved of data as programming committees and politicians put their resources increasingly into the hardware and software of the new physics, leading to the overgrown big science we have today.

In summary: I think we can indeed specify the source of science's robustness in dances of agency, especially with the material world, and in the production of free-standing machines and instruments, but we should not overrate this robustness. Machines, instruments and knowledge are machines, instruments and knowledge only in relation to us, situated in a path-dependent fashion with respect to the cultural fields in which they are built, fields which themselves co-evolve in a chancy fashion with those machines, instruments and bodies of knowledge. To put robustness in its place, and to undermine the intuition of uniqueness that goes along with our taken-for-granted realism about science, I have tried to suggest that incommensurability is always bubbling up in science, at all scales from the micro to the macro, and that science has no magic recipe for getting round that—only more manglings and dances of agency.

Traditionally, philosophy of science has done all it can to reject, or simply ignore, the very idea of incommensurabilty. Personally, I think it makes philosophical thought much more interesting. One has to learn to imagine an indefinitely rich world which we can latch onto in an indefinite number of ways. I think we can do that, and still appreciate the robustness of science at the same time.

Acknowledgement This work was supported by the National Research Foundation of Korea Grant funded by the Korean Government (NRF-2010-330-B00169).

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