Astronomy and Experimentation

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Abstract
In this paper I contest Ian Hacking's claim that astronomers do not experiment. Riding on this thesis is a re-evaluation of his view that astronomers are less justified than other natural scientists in believing in the existence of the objects they study, and that astronomers are not proper natural scientists at all. The defense of my position depends upon carefully examining what, exactly, is being manipulated in an experiment, and the role of experimental effects for Hacking's experimental realism. I argue that Hacking's experimental realism is not adequately defended, and even if we accept it in good grace, the case can be still made that astronomers experiment by Hacking's account.

Keywords: astronomy, experiment, realism, Hacking

1. Introduction

In *Representing and Intervening*, Ian Hacking argues that experimentation is the best defense for our beliefs in unobservable entities' existence (1983, 274). Attendant with that view is a skepticism toward the existence of unobservable astronomical entities such as black holes (1983, 275). Experimentation gives us best defense for entities, even if they are observable, and failure to experiment likewise grounds caution toward such astronomical objects as gravitational lenses (Hacking 1989). Hacking also uses experimentation to demarcate the natural sciences (chemistry, physics, molecular biology) from other scientific endeavors. He does not appear unsympathetic to scientific disciplines outside physics and chemistry, although the status of their findings is diminished because of the inability to experiment there, where conducting experiments looks to be rather an all-or-nothing affair. This essay examines, on one hand, whether experimentation delivers for the natural sciences the result Hacking intends. On another hand, this paper pushes for the idea that Hacking's view on experimentation is quite flexible. That is why he seems to hold that experimentation both does and does not occur in astronomy. We will consider case studies from the history of astronomy to see this play out.

2. Experimentation and the Natural Sciences

Hacking observes that after the 17th century's scientific revolution, interfering with and manipulating the world gained importance as never before in the history of science (1983, 149). Pure theory and passive, non-invasive observation were no longer held to be up to the task of plumbing nature for its fundamental, hidden truths.[1]

This development in the history of science has been masked, however, because the history and philosophy of science have traditionally prioritized the theoretical side of science, probably because historians and philosophers prioritized theory themselves. Experimental results received attention insofar as they were shown to support or disconfirm a theory. But generally speaking, historians and philosophers were largely inattentive to experimental life and naturally the picture of science that emerged through their writings was lopsided. It is in no small part thanks to Hacking's work that non-continental philosophers of science turned to look at experimentation more seriously.[2]

There is more to gain than narrative completeness by paying appropriate attention to experimentation. There are long-standing problems in philosophy, and philosophy of science, orbiting the subject of realism. One might think that scientific success under the auspices of a particular theory signals the likelihood that the objects (quarks, electrons, oxygen, etc.) the theory posits are real. But spelling out how it is that symbolic elements of theories match up with parts of the world, or how a theory may be said to represent the world, has been fraught with troubles.[3] The linkage between the realms of the intentional and physical is difficult to describe or explain, or justify.

Hacking promotes revitalizing realism by turning to the (then) relatively unexplored world of experimentation, of action. If we're doing an experiment, then we have good reason to believe that the objects involved in the experiment exist. We are justified in believing these objects are real – a particularly pressing concern when they are not observable – when we have characterized their causal properties successfully enough to use them in an experiment.

One important feature that goes into making an activity experimental is control. The term 'control' connotes the idea of imposing one's will upon something else, particularly in order to force that thing to act in a way that it wouldn't if left on its own. Everyday examples that come to mind are controlling one's dog to keep it from jumping on someone, or controlling one's temper so that one doesn't it get the upper hand in thoughts and behavior. This sensibility is expressed in the quote Hacking chooses from Bacon, "The secrets of nature reveal themselves more readily under the vexation of art than when they go their own way" (1983, 246). It is a sensibility about control that is present in Hacking's views as well: when we experiment with electrons, we build devices that make the electrons "sit up and behave" (1984, 157).

If we understand the causal nature of experimental entities well enough, then we can successfully construct apparatuses that make use of, or control, those entities' causal powers in ways that produce well-defined effects.[4] Hacking writes,

> The more we come to understand some of the causal powers of electrons, the more we can build devices that achieve well-understood effects in other parts of nature. By the time that we can use the electron to manipulate other parts of nature in a systematic way, the electron has ceased to be something hypothetical, something inferred. It has ceased to be a theoretical and has become experimental. (1983, 262)

In short, the experimental entity becomes part of the experimental apparatus. The entity, now ready at-hand to perform in a prescribed way, has the status of a tool:

> Once you have the right experimental idea you know in advance roughly how to try to build the device, because you know that this is the way to get the electrons to behave in such and such a way. Electrons are no longer ways of organizing our thoughts or saving the phenomena that have been observed. They are ways of creating phenomena in some other domain of nature. Electrons are tools. (1983, 263)

In the main, then, to perform an experiment – and in particular, experimenting using unobservable (inferred) entities or objects – means to design, build and use an apparatus with an understanding of those entities' causal powers. The entities' causal powers together with the rest of the experimental apparatus make for a novel combination. Thus the purported entity responds in new ways by the occurrence of causal powers that the rest of the apparatus is designed to elicit and control or channel. In other words, the experimental object's powers are enjoined to occur in ways that, left on their own outside the experiment, they would not. These causal powers, appropriately manipulated, are used to interact with another part of nature so that a well-definable effect is created.

The successful creation of an effect, or what Hacking also calls a phenomenon, is an experimental output that is controlled, or maintained and stabilized, long enough that it can be measured accurately. When you are in a situation where an object, even if you can't see it, has causal powers that enable you to produce reliably a stable effect, then you are in a much better position to assert that the invisible object exists. At least one reading of a linking justification goes: we couldn't have manipulated these causal powers as well as we did, unless there was an entity there to which those causal powers belong. With this experiment, these specific causal powers were successfully manipulated. Thus there exists something with the causal powers enabling that to occur. This is a different justification for an existence belief than postulating an object – say, a celestial sphere – in order to make sense out of events that occur without our willful designs, such as the movement of the stars and planets.

> The best kind of evidence for the reality of a postulated or inferred entity is that we can begin to measure it or otherwise understand its causal powers. The best evidence, in turn, that we have this kind of understanding is that we can set out, from scratch, to build machines that will work fairly reliably, taking advantage of this or that causal nexus. Hence, engineering, not theorizing, is the best proof of scientific realism about entities. (1983, 274)

3. Why astronomy is not a natural science

Now we are in a better place to approach why Hacking has difficulty believing astronomers experiment, and why the justification for believing in astronomical objects is comparatively shaky. "Cavendish and Fizeau worked entirely in the laboratory with artificial instruments. You can't fool around with the moons of Jupiter, or Mt Chimborazo. This is connected with what I called the creation of phenomena. One is able to produce, in laboratory conditions, a stable numerical phenomenon over which one has remarkable control." (1983, 237).[5] Objects we cannot move about, like planets or mountains, we cannot use to produce stable effects elsewhere in nature. It's by using objects as tools that we have good reason to believe claims made about them, especially when existence claims are for invisible things.[6]

The sciences – what Hacking calls the natural sciences – are able to make such claims for the objects they study through experimentation includes physics, chemistry, and molecular biology (1999, 64).[7] Even other sciences that aren't physics or chemistry fare better on an experimental evaluation than astronomy does:

> The remarkable fact about recent physical science is that it creates a new, collective, human artifact, by giving full range to three fundamental human interests, speculation, calculation, and experiment. By engaging in collaboration between the three, it enriches each in a way that would be impossible otherwise.
>
> Hence we can diagnose doubts some of us share about the social sciences. … There is no end of 'experimentation' but it as yet elicits almost no stable phenomena. There is plenty of speculation. There is even plenty of

> mathematical psychology or mathematical economics, pure sciences which have nothing much to do with either speculation or experiment. Far be it from me to offer an evaluation of this state of affairs. Maybe all these people are creating a new kind of human activity. But many of us experience a sort of nostalgia, a feeling of sadness, when we survey social science. Perhaps this is because it lacks what is so great about fairly recent physical science. Social scientists don't lack experiment; they don't lack calculation; they don't lack speculation; they lack a collaboration of the three. (1983, 248-49)

It looks as though Hacking holds that at least the social sciences experiment, but the meaning of this passage is not straightforward. On one reading, he looks to endorse the claim that social scientists experiment; they only lack the right combination of that with other skills. On a second reading, the social scientists "kind of" experiment, but not very well since they have been unsuccessful in creating stable phenomena. On a third reading, still holding that stable phenomena are key to experimentation, the social scientists aren't really experimenting at all because they do not create stable phenomena. Either of the latter two readings would make sense of the scare quotes he uses around the word experiment, and either is elsewhere endorsed by him: "In my opinion, one of the fundamental ways in which laboratory science and ethics separate is that the former engages in what I have called the creation of phenomena: the purifying and maintaining of phenomena that do not exist in a pure state anywhere in the universe" (1988, 152). Thus I read Hacking's position to be that experimentation essentially involves the creation of phenomena, and in scientists' capacity to do so, they are engaged in the laboratory sciences, where "laboratory sciences" reads quite easily for what Hacking means by "natural sciences."

But while the status of the social sciences may be sketchy, it is clear that Hacking does not view the natural sciences to include astronomy. At the end of *Representing and Intervening,* he mentions that he is skeptical about the existence of black holes because our beliefs about black holes are purely theoretical, but not experimental (1983, 274-75). He continues in a later writing that with the exception of the moon and some planets, "galactic experimentation is science fiction, while extragalactic experimentation is a bad joke" (1989, 559). To put the matter plainer still:

> The technology of astronomy and astrophysics has changed radically since ancient times, but its method remains exactly the same. Observe the heavenly bodies. Construct models of the (macro)cosmos. Try to bring observation and models into line. In contrast: the methods of the natural sciences have undergone a profound transformation, chiefly in the seventeenth century. Or one might say: the natural sciences came into being then and thereafter, while astronomy is not a natural science at all. (1989, 577)

4. A stronger read on experimental realism

Yet it doesn't automatically follow that we should be cautious about the truth of claims made about planets or stars or black holes. Hacking's idea is that if an entity's causal powers are used in an experiment, then we have good reason to believe in the existence of the object to which those causal powers belong. This wording of the experimental thesis is a little convoluted, but deliberately so, and here is why.

An initial reading of Hacking may lead to the message that if we use an object as a tool, then that object exists. This is not a felicitous reading of his experimental realism. Once we affirm the antecedent, "We've used this entity as a tool," then the entity's existence is tautologously given.[8] But then experimentation doesn't give us a reason to believe an entity exists; experimentation instead presupposes the entity's existence.

If experimentation gives us a reason to believe an entity is real, then there is something about the process that supports our belief in its existence. That process, as Hacking describes it, is one of taking advantage of causal nexuses, understanding causal powers, well enough to build appropriate devices that work with those causal forces to produce stable effects. Then, and only then, are we entitled to say we have reason to believe in an entity whose causal factors made the effect happen.

Thus now we are at a stronger reading than before of the justification between experimentation and realism. Earlier I worded the justification: we couldn't have manipulated these causal powers as well as we did, unless there was an object there to which those causal powers belong. But on that reading, even if it's false that we use an object's causal powers as tools, we are not logically compelled to doubt existence claims about the object. Maybe tool-usage is grounds for believing that the object used as a tool is real. But if one doubts the existence claim, the reason cannot be because it was not used as a tool in an experiment.

If the failure to use astronomical entities' causal powers as tools in experiments is a reason to be cautious about the empirical claims we make about them, then there are two justifications. First, perhaps the doubt comes from somewhere other than the absence of experiment. Maybe Hacking has other reasons for being skeptical about black holes. But that doesn't look like Hacking's take; it is manifest from the selections given above that he finds astronomical practice to be non-experimental and that's the reason why beliefs about astronomical objects should be taken with caution.[9]

Another pathway is to maintain an additional proposition: that if we have good reason to believe in the existence of an object (particularly an unobservable one), then we have manipulated its causal powers as tools in experimentation. Its contrapositive might make the appropriateness of this move clearer: if we do not use a thing's causal powers as tools in experimentation, then we do not have good reason to believe in the object itself. Now we have the inferential apparatus to reach the skepticism about astronomical unobservables that Hacking endorses when conjoined with the assertion that astronomers do not experiment.[10]

5. The Creation of Phenomena

I'd like to switch now away from experimental entities to the phenomena they help to create, and the phenomena's importance. On one hand, their significance may be seen to subsist in their usefulness: their stability and reliability distinguishes real experimentation from pseudo-experimentation.[11] Yet instrumental effects – events that trigger data-readings that are contributed by the experimental rig, as opposed to the phenomena the devices are built to detect – can also be stable and reliable, but they are not what lend credence to an experimental result.[12] Of course instrumentalists and scientists seek to find if their devices produce reliable and stable effects for the purpose of being able to eliminate, suppress or otherwise account for their contribution to the collected data. But if stability and regularity are the only criteria for experimental effects, there's no obvious sign on a bit of data announcing it as due more to an experimental effect than to an undocumented instrumental contribution. We'll see this conundrum come up in the science case discussed later.

Arguably, distinguishing useful or meaningful data from noise comes with a concomitant sense that the meaningful data is indicative of something extra-instrumental, something real: the experimental effect. If that is the case, then we have a difficulty with Hacking's account, for by it we are not entitled to claim than an experimental effect is real. "Experimenting on an entity does not commit you to believing that it exists. Only *manipulating* an entity, in order to experiment on something else, need do that" (1983, 263, emphasis Hacking's). Yet it is eminently plausible to think that unless we believe in the reality of the effect – and here it is not meant the data reading (by whatever means of recording), but something that the data signify – then an experimental effect would lend little support to believe in the reality of the entity used to call it into existence. Curiously, any belief a person might have in the reality of experimental effects does not come from Hacking's experimental realism. If the point made here has merit, then it contains injurious consequences for Hacking's position, in that a sense of realism, independent of his experimental realism, seems to be needed to defend his position.[13]

We can read Hacking as being of a mind embracing more than a practical attitude toward experimental phenomena. Consider the passage from Hacking below. It is a condensed selection that I will spend some time unpacking.

> [T]he phenomena of modern physics are manufactured. The phenomena about the species – say one that a pride of lions hunts by having the male roar and sit at home base while the females chase after and kill scared gazelle – are anecdotes. But the phenomena of physics – the Faraday effect, the Hall effect, the Josephson effect – are keys that unlock the universe. People made the keys – and perhaps the locks in which they turn. (1983, 228)

Experiments are the means by which we are enabled to peel back surface appearances and make manifest what was previously hidden: the stable phenomena our scientists create. It's no surprise that Hacking approvingly references Francis Bacon's dictum, "The secrets of nature reveal themselves more readily under the vexation of art than when they go their own way."

However, we should sit a while longer on the poetry of his statement, "[T]he phenomena of physics – the Faraday effect, the Hall effect, the Josephson effect – are keys that unlock the universe. People made the keys – and perhaps the locks in which they turn." The "keys" – the experimental effects – are manufactured in the scientists' lab, and so too, perhaps, the problems that they help solve. "Traditionally scientists are said to explain phenomena that they discover in nature. I say that often they create the phenomena which then become the centerpiece of theory" (1983, 220).

Hacking describes experimental effects as powerful tools, as important "keys." But according to his view, these same things are dubiously real, unless they have also been used as tools elsewhere in experimentation. A questionably real key does not connect solidly with being a powerful tool, as a tool must be if it has the potential to unlock something as extravagant as the entire universe. Perhaps Hacking is guilty of a little hyperbole, but even if we tone down the sweeping scale of his assertion, the same difficulty persists: a tool whose existence is iffy is not demonstrably useful. If we would commit ourselves to the power or usefulness of given and purportedly real experimental effects (beyond justifying a belief in the past entities that produced them), we might conceive basing our commitment *not* on their actual use as instruments (as experimental realism would have it) but on other reasons instead, such as our careful measurements of them. Which means there might be room enough left to believe in the existence of an unobservable entity that is not exhausted by instrumentality.

For the sake of argument, let us consider that the experimental effects in question are ones that have had their reality solidified by having been used as experimental tools upon something else. Let us also return to our familiar quote: "[T]he phenomena of physics – the Faraday effect, the Hall effect, the Josephson effect – are keys that unlock the universe." If it's true that physics and chemistry cover such a natural scale, and no doubt many believe they do, this is a powerful reason for why sciences such as physics and chemistry are the preeminent natural sciences.

The trouble is that Hacking's given us reason to doubt that scope. Doubt might already be lurking in the minds of readers upon reading the first sentence of the passage from Hacking above: "[T]he phenomena of modern physics are manufactured." But this point should be unsurprising, given what has been discussed already about the control elements in experimentation, and how experiments are executed in order to achieve results that would not ordinarily occur.

> Does not a current passing through a conductor, at right angles to a magnetic field, produce a potential anywhere in nature? Yes and no. If anywhere in nature there is such an arrangement, with no intervening causes, then the Hall effect occurs. But nowhere outside the laboratory is there such a pure arrangement. (1983, 226, also elaborated in 1988, 152)

One might want to make the case that the experimental entities and the phenomena they produce are superior to what occurs in a natural state because they are manufactured into a "pure" state. But if it's true, as Hacking indicates it is, that these events have little match with affairs outside the laboratory, then it's prudent to pause before accepting that laboratory findings inform us about the natural world. "Most of the phenomena, effects, and events created by the experimenter are like plutonium: they do not exist in nature except possibly on vanishingly rare occasions" (1984, 155). The issue becomes more pressing, the more iterations that occur of the experimental cycle.

The scenario envisaged is one where an effect is created, which is then used as an experimental tool that creates another effect, which is used as an experimental tool, and so on. For instance, this is the picture invoked by Hacking's discussion of using electrons to create an experimental effect – neutral bosons. By that success we are entitled to believe in the real existence of electrons, but not that of neutral bosons. When neutral bosons are later manipulated to experiment on something else, then the neutral bosons gain "real" status (Hacking 1984, 167-68). But by this scenario, each step gives us something increasingly removed from the world outside the lab. Calling such investigations into the world part of the natural sciences seems to be a terrific misnomer.[14]

6. The inference from effects to their cause

Before turning to discuss a case in point of Hacking's view, I'll pause over one final point about it. The focus for his experimental realism is on experimental entities: theoretical, hypothetical, invisible or unobservable kinds of objects. As discussed already, the reason to believe in those objects' existence isn't by what we do with an object per se, but how we work with causal processes, the success of which entitles us to say that an object exists.

Scientists and engineers use their understanding of these objects' causal powers to design their experimental devices and then hopefully move those powers to good effect. "There is … a family of causal properties in terms of which gifted experimenters describe and deploy electrons in order to investigate something else" (1983, 272). But unless a person is prepared to assert that an object is nothing but the sum of its causal powers, there is a metaphysical gap.[15]

If there is something about the agent or entity that makes sense out of why it executes *these* kinds of activities rather than *those*, then a meaningful attribution of causal responsibility can come about and all the pieces fall into place. We don't see in Hacking any general structure for a story linking unobservable, experimental entities with their causal powers, and it might be untoward to ask Hacking to give such a narrative at the primitive level of reality he discusses. But I suggest its absence is one reason why a person might be prevented from taking his pathway to scientific realism all the way to the end of the line.

This difficulty, if it is one, is exacerbated by cases where an experimental entity manifests on the whole mutually inconsistent causal properties. Consider when Hacking writes

> Various properties are confidently ascribed to electrons, but most of the confident properties are expressed in numerous different theories or models about which an experimenter can be rather agnostic. Even people in a team, who work on different parts of the same large experiment, may hold different and mutually incompatible accounts of electrons. That is because different parts of the experiment will make different uses of electrons. (1983, 263-64)

So it doesn't seem Hacking can really be of the mind that an object is the sum total of its causal powers. That would render objects being rather incoherent and logically impossible, and as much as Hacking works to diminish the authority of theory, I'm not confident he'd go so far as to be done with consistency. This quote puts across the same message as the story of the blind monks and the elephant: there is an object capable of appearing, or exhibiting its causal powers, differently depending upon the angle we take upon it. But we are as dark as ever about what such objects are, and how they exist.

In the end, then, what we are left with is uncertainty about the connection between the successful manipulation of causal powers and the object we are interested in believing is real that manifests those causal powers. There is also uncertainty about the metaphysical status of experimental effects, and how they effectively function to support a belief in the existence of the entity used to produce them. Both these concerns go toward a general uncertainty about the tenability of Hacking's experimental realism. That uncertainty grows further still when we look at cases from the history of radio astronomy. On one hand, Hacking quite vigorously rejects the prospect of experimentation occurring there. Yet he is not, on experimental grounds, completely negative of the work done there. And as I explore an additional case from the history of astronomy, we will see that Hacking's account of experimentation works quite comfortably there.

7. Case in point: the cosmic microwave background discovery

Most of this paper has been conducted in terms of high generality. Let us look now to a particular example from the history of science that Hacking discusses as a prime example of experimental activity, a "sheer dedication to an experimental freak" (1983, 159).

The detection of the cosmic microwave background was first announced by Arno Penzias and Robert Wilson in 1965, even though they were never looking for it. The experiment Penzias and Wilson were working on, as Hacking describes, was a follow-up of studies began in the 1930s when Karl Jansky found unexpected sources of radio noise in his work in radio communications and inadvertently became the father of radio astronomy. The noises were gradually matched with known, observable, objects such as one particularly steady hiss with the galactic center.[16] Some 30 years later, Penzias and Wilson

> [a]dapted a radiotelescope to study this phenomenon. They expected to detect energy sources and that they did. But they were also very diligent. They found a small amount of energy which seemed to be everywhere in space, uniformly distributed. (1983, 159)

Hacking's account could have one think that Penzias and Wilson sought to do measurements to match radio sources to familiar celestial objects, and that they realized that their measurements showed them ubiquitous radiation throughout intergalactic space. Neither one of these was the case (Wilson 1983, 187; Penzias 1972, 30). They were using a radio telescope to perform different measurements entirely, the exact nature of which need not derail us here except to point out that it was of particular importance for them to make the most sensitive measurements possible.[17] Thus they sought to identify and remove, or at least be able to account for, every bit of emission their receiver measured.

For radio astronomers, everything is a source of radio radiation, because anything that is warmer than absolute zero (0 Kelvin, or 0K) emits energy in the radio wavelength regime. Some sources of unwanted radiation can be suppressed: reducing the temperature of the receiver for instance, will do this. The device itself emits radio-wavelength energy, which it subsequently picks up in its reading. The lower the receiver's temperature, the less energy it emits and so its contribution to the overall reception can be, to a degree, eliminated.

The receiver, however, cannot be cooled to absolute zero, and neither can the entire telescope apparatus nor the ground all around the apparatus that reflects radiation back up on the telescope. They will always contribute something to its reading which will need to be accounted for. So painstaking testing is done prior to use to measure how the device performs at specific temperatures, so their contributions to a reading can be subtracted in order to gain a more accurate reading of the temperature, or energy, of the source. We'll take a highly simplified case, mainly for sake of making the shape of the scenario clear. Let us assume that the telescope is pointed to a region of the sky where nothing is believed to exist, and that intervening atmospheric and surrounding environmental contributions are accounted for, and that outer space is perfectly cold. If the receiver picks up a signal, then when the noise of the telescope is subtracted the result should be zero.

This is the kind of noise-tracking Penzias and Wilson were doing prior to their measurements. And to their disappointment, they found a persistent excess of roughly 4K. They searched exhaustively for an instrumental source of the emission, including the forceful eviction of pigeons insistent upon nesting inside the horn of the telescope. They combed the local area for possible stray radiation leaking over from New York City, 50 miles away. They measured during the day; they measured during the night. Penzias and Wilson took readings for an entire year and although they had honed down the measured excess to 3K they still couldn't explain it. They were ready, as a predecessor had been before who used the same equipment, to chalk up the excess to an unaccountable source of instrumental noise. But it threatened their ability to perform the tests they wished, so they were understandably disappointed (Wilson 1983, 194; Penzias 1972, 32).

Bernard Burke, a professional acquaintance, provided them with a link to solve their conundrum. Burke knew that a group at Princeton University – Robert Dicke, James Peebles, David

Wilkinson, and P.G. Roll – was working on testing Dicke's prediction of a ubiquitous low-energy radiation, at roughly 3K, left over from a big, banging start to our universe's existence (Wilson 1983, 194-95; Wilkinson and Peebles 1983, 177; Penzias 1972, 34). Mind, while Big Bang cosmology seems commonly apparent now, at the time under discussion there wasn't much solid in the way of empirical confirmation, and cosmology was even more conjectural than it is today.

In a phone conversation with Penzias, Burke suggested he contact Dicke. Dicke, upon receiving Penzias' call, knew that they were "scooped" (Wilkinson and Peebles 2000, 138). The Princeton group's hope for a first detection was over. The discovery, all the while unrecognized as such by Penzias and Wilson, had already been made.

Hacking admires the care and diligence Penzias and Wilson exercised in making their serendipitous discovery. He also quite appears to believe in the existence of the cosmic microwave background radiation. At least, he gives no indication that he is as doubtful to its existence as he is to that of black holes or gravitational lenses. He writes:

> It is sometimes said that in astronomy we do not experiment; we can only observe. It is true that we cannot interfere very much in the distant regions of space, but the skills employed by Penzias and Wilson were identical to those used by laboratory experimenters. (1983, 160)

It is sometimes said indeed – by Hacking himself, for instance. And if it is the case that conscientious astronomers such as Penzias and Wilson are of a kind with laboratory experimenters, and if the findings of Penzias and Wilson are not disreputable, then a glimmer at least shows of the possibility of a gateway along experimental lines between astronomy and the natural sciences as Hacking conceives of them.

8. The first detections of interstellar CH

Let me pull one more example from the history of radio astronomy to offer another leg to support my final conclusions on experimentation and astronomy: the detection of interstellar CH. It provides an example of where laboratory chemistry fails us but astronomy enables us in finding whether certain kinds of molecules exist.

This example comes from a time in radio astronomy when molecular spectroscopy was quite the new thing. Not so long ago it was thought that optically dark interstellar regions were pretty empty and uninteresting in comparison to luminous stars and galaxies. Interstellar space proved to have much more to show once we had the instruments to peer into it in ways that we hadn't before.

For those of us who had and remember high school or college physics labs, there was probably the time that we used a diffraction grating to look at the spectra of excited elements, found where their emission lines where and proceeded to identify them by their spectral "fingerprints," since the spectroscopic characteristics of each element are unique. Optical astronomers are able to identify the constitution of chemical elements by their spectra, and spectra also shows in the fantastic color images we have of astronomical objects. Images of the Orion Nebula, for instance, glow with a particular shade of pink that indicates the cloud contains ionized hydrogen that is excited by hot young stars within the nebula. Spectroscopy allows us to identify the hydrogen there. It is no exaggeration to say that without spectroscopy, astronomy as we know it would be impossible.

The analysis of spectra occurs in other wavelength regimes as well, of course. Its introduction to the astronomical scene has occurred only recently (considering the length of time astronomy has been studied on the whole) pending the technological development needed to pursue it, and the theoretical (or pre-theoretical) readiness to expect any gain in using non-optical wavelength techniques. One way of establishing the capacity of new, non-optical techniques and measurements to reliably reveal something novel about the universe was to bridge non-optical detections to optical ones. This is why the detection of interstellar CH was so significant.

In the 1930s Theodore Dunham, Jr. and Walter Adams used optical spectroscopy on Mt. Wilson to identify interstellar CN, CH, and CH^+ (Rank et al. 1971, p. 1085). In 1953 Iosif Shklovsky predicted that CH would produce spectral lines in the radio regime, but by 1973 this was not yet detected in radio although CN was (Rydbeck et al. 1973, p. 466). Finding CH in radio astronomy held out good rewards for its finder as a kind of “missing link” between established spectral line research in optical and new spectral line research at radio wavelengths.

Slowing down the discovery was uncertainty about exactly where CH’s spectral lines should occur. Empirical measurements were unsuccessful because CH is a highly radical compound, and could not be investigated in a laboratory (Turner and Zuckerman 1974, p. L59). But although the space between stars is not a complete void, the neighborhood is empty enough that radicals like CH have a good chance at persisting without interference and thus be detectable, if we know where and how to look. Predicting CH’s performance in radio therefore had to be done on a purely theoretical basis..

Attempts were made in 1966 at Berkeley’s Hat Creek Observatory to predict and detect the radio wavelengths of CH, but they were unsuccessful. Australian astronomers met a similar fate (Barry Turner, personal communication). Two other tries were made in Sweden by O.E.H. Rydbeck and J. Elldér at Onsala, one in 1968 and another in 1971, also without success (Rydbeck et al. 1973, p. 466).

A new prediction about CH’s radio frequency was published in 1971 (Baird and Bredohl 1971), and Barry Turner and Bill Zuckerman applied for telescope time to follow up the calculation. Although it took more than one trip to the telescope, for want of a sufficient receiver, believable results were finally achieved and their discovery paper made it into press a few short weeks after the Onsala group announced similar findings first (Turner and Zuckerman 1974, Rydbeck et al. 1973).

9. Conclusions

There are two basic points in this conclusion. The first is that Hacking’s view on experimental realism is too restrictive on several counts. The second is that even if we are receptive to Hacking’s experimental realism, in playing it through we find that astronomers do in fact experiment on the entities they study.

To the first point, first. I maintain that Hacking’s experimental realism is too restrictive for the sciences, for three reasons. To start with perhaps the least philosophical reason, I propose that we take Hacking’s very practical point of view seriously practically. The sciences that best warrant us for being realists about the entities we investigate are, as already mentioned, the sciences of things over which we have control such that we can without transgressing any moral boundaries, manipulate them as tools. Thus this leaves out the social sciences, the earth science, meteorology, possibly the mind sciences, and probably more. It is not of primary concern to give a list of all the fields of study that do not measure up. But instead, I wish to note only that the list of exclusions is not small, and, taking his view with practical sobriety consider what that might mean if we endorsed it as a political reality and not only a philosophical possibility. Where there is ever precious little funding for the sciences, I see no obvious reason why it shouldn't go predominantly to those whose objects of study we can best be realists about. The federal support of a science like astronomy would rather seem as giving taxpayer money away to paranormal research since that enterprise, like astronomy (for instance) arguably does not measure up on experimental grounds. Perhaps we ought dampen studies in these other fields as well. That would be reasonable if we ought to be skeptical about the status of their domains, and, if my speculation on funding has any merit, there wouldn't be many jobs to be found in them, anyway.

On another hand, a person in support of Hacking's view might respond that it is not so eliminative of other sciences' merits because the category 'natural science' does not include them. Properly, the natural sciences are distinguished from natural history, earth science, social science, and so on. What Hacking has to say about experimental realism has properly application within the natural sciences, but outside of that there may be room still to make sense of being a good scientist without appeal to experimentation.

That may be. But I would bring out my second reason for holding that experimental realism is too restrictive with a domain pared down to physics, chemistry, and molecular biology. It looks like 'natural science' is a term that applies – to put it grossly – to what can be put into a test tube or spun around through a cyclotron, then unless we are thorough reductionists, it's a distortion of the language to say that the natural sciences are what we are left with. Furthermore, the need to have the actual ability to lay technological control over the matters at hand gives what's left of the natural sciences less to work with than one might think. Hacking's experimental realism is best suited to defend the existence of those objects – like molecules – that can exist in labs. Other similar objects, distinguished only by their incapacity to exist in a lab, are excluded. If experimental realism can enable us to plumb the secrets of the universe, it is a universe only as it can manifest itself in our laboratories.

Thirdly, the status of what experimental findings we achieve in the lab has already been called into question in terms of their representativeness of what's in the outside world. Whether those findings are representative or not, added now is the understanding that what we are capable of probing in a lab is more limited still, and the capacity of the natural sciences to unlock the secrets of the universe looks to be overly optimistic indeed.

My second thread of contention concerns that by Hacking's account, contrary to his apparent position on the subject, astronomers do experiment and have as good reasons to be realists about the objects they study as any other natural scientist.

Part of the incentive to see astronomers in a more favorable experimental light is provided by Hacking himself in the space he devotes to discussing the first bona fide cosmic microwave background detection. To be sure, in his discussion he doesn't exactly call this an *experiment*, although he does write that Penzias and Wilson used skills identical to those of laboratory experimenters. Yet with so many unquestionably laboratory-based experiments to pick from to exemplify laboratory skills, Hacking chose a moment from the history of the most imprecise of sky sciences there is. That the 3K detection was selected, and presented as approvingly as it was, indicates a positive attitude on Hacking's part toward the work Penzias and Wilson did. Even if their testing wasn't a proper experiment as Hacking develops the term, it wasn't utterly without experimental merit. That – in addition to the possibility he extends to the social sciences employing experimentation, discussed earlier in this paper – indicates that the difference between experimental and non-experimental, or natural and other, sciences is more continuous than dichotomous. He later writes,

> I hope that the image of the future will be one of the experimenter collaborating with nature rather than mastering it. This tendency will assuredly be augmented by the fact that the most imaginative sciences of the present decade are biological and astrophysical, one a life science and one scarcely a laboratory science at all. It may be that the self-conception of the experimental sciences will displace the role model set by physics. (1988, 154)

We can start to work this image of Hacking's later writings from the work done so far on his seeds already sown, but it takes pressing a bit harder on the idea that experimentation requires the manipulation of causal powers, yielding grounds for believing in a background object upon the production of reliable phenomena. Astronomers' objects of study have causal powers – electromagnetic radiation of various kinds – and they are channeled through devices, built with an understanding of those causal powers in mind, to produce reliable phenomena, such as a response in a receiver or a CCD, whose response is recorded as data.[18]

There might seem to be a mismatch between my example from astronomy and the kinds of experimental effects Hacking describes. I can imagine one responding that they are different because it is conceivable in the subatomic, but not for the astronomical, to imagine for any experimental effect producing reliable data, there is the possibility down the road that *it* could in turn be used as an experimental object. In other words, Hacking's kind of experimental effect has the potential for a sort of thing-ness with a future open to other possibilities that a receiver response does not have.

That objection doesn't change anything. The existence of Hacking's experimental effect, as with the behavior of a receiver, is utterly entangled with the technology that makes it manifest, so it isn't as though the former has a kind of technological independence that responding receiver lacks. And even if, in what Hacking calls the natural sciences, effects can be produced from effects that were produced from other effects, the strength of this chain of interactions is only as strong as any one of its links. And any one of its links involves the same process performed by astronomers: manipulating a proposed object's causal powers to produce a stable experimental effect.

So if Hacking firmly holds that astronomers do not experiment yet I make the case on Hacking's grounds that they do, then something seems amiss. Perhaps Hacking's account is inconsistent, although I do not see a specific place where it is obviously so. Or perhaps I have stubbornly overlooked something about astronomical objects that makes them inappropriate candidates for experimental objects – I suspect candidates may be issues concerning their size and/or their distance from us in time and space, and our corresponding inability to contain them. But in focusing on Hacking's own reasons for realism as centering upon whether and how we are able to use another object's causal powers, then it seems fair to leave considerations of where the object is and its size aside, so long as its causal powers are accessible.[19] Or finally, one might raise the objection that in moving away from discussing objects, I've switched the topic to a possibly more notoriously difficult matter of causation. That may be. But if making that switch is sensible, then taking on the collateral complications is a matter for another time.

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Endnotes

1 A view one finds enthusiastically endorsed by Lipkin (2000) where he writes, for instance, "the best physics I have known was done by experimenters who ignored theorists completely and used their own intuitions to explore new domains where no one had looked before. No theorists had told them where and how to look" (p. 74).

2 A point covered by Ihde (1991) . There he discusses the European continent's tradition of philosophical interest in technology and its influence on perception and culture, and the relatively recent growth of interest in technology among Anglo-American philosophers of science, and the increasing bridgement between the two schools of thought. See also Harré (2003).

3 See for instance Laudan (1984), Fine (1984).

4 Bogen and Woodward (1988) make an excellent case for distinguishing, among experimental effects, data (which is one outcome of an experiment) and phenomena, and I think that they are right in pointing out that authors such as Hacking, often fail to make clear the difference (see in particular p. 306, footnote 6). Although I'll follow Hacking in using "experimental effect" to include data and phenomena, as they are out experimental outcomes, I'll do my best to make clear in context which one I'm talking about.

5 Assuredly there is something not quite as conceptually tight as one might like here. We cannot manipulate the moons of Jupiter, but Hacking does write elsewhere that we are able to experiment with the moon and the planets which are close (1989: 559), which sounds quite unlikely, as it seems as improbable to move them about as it is to manipulate the moons of Jupiter. Perhaps the sense of "experimenting with the moon" is revealed more fully on the next page, where he continues that we experiment on rocks from the moon (p. 560), which of course is not quite the same thing as experimenting with the moon.

6 The aspect of containment is often implied as needed in the performance of experiments in Hacking (such that failure to contain is why we cannot fool around with moons or mountains). But containment is not championed as vigorously as the importance of constraining causal powers and producing experimental effects. Thus in this regard Hacking seems to diverge from his Baconian inspiration. See, for instance, Carolyn Merchant's (2008) discussion of Bacon and experimentation, particularly p. 753 ff.

7 Although for interest, compare this position to the physicist Felix Auerbach, quoted in Sibum (2004):
"Experimental physics does not – as the term already suggests – practice observation of nature like other natural sciences; it deploys artificial experiments which are performed just for a specific purpose. Strictly speaking, physics with regard to its method is not a natural science like astronomy, geology, botany, etc.; it does not deal with natural phenomena but artificial phenomena produced by intentional acts of the researcher…" (p. 61).
Thus Sibum is in agreement with Hacking on the artificiality of experimental effects but does not agree in considering (experimental) physics a natural science. Another author to the same point is Janich (1978), discussed in Radder (2003), p. 154.

8 A reading of Hacking's view found, for instance, in Kroes (1994): "They [unobservable or theoretical entities] become real as soon as they can be manipulated to produce new phenomena" (p. 434; emphasis Hacking's). It is

indicated also in Hacking when he writes "The argument – it could be called the 'experimental argument for realism' – is not that we infer the reality of electrons from our success. We do not make the instruments and then infer the reality of electrons, as when we test a hypothesis and then believe it because it passed the test. That gets the time-order wrong." (Hacking (1984) p 161).

9 An unwarranted conclusion, implausible and undesirable for the field of astronomy, according to Morrison (1990: 13), but a consequence that a view such as Hacking's cannot sensibly avoid. It is a view that seems endorsed by Patrick Heelan, however, when he writes "As for black holes, quarks, and such like, about which physicists are presently undecided, one will say that laboratory physicists will decide eventually whether these are … scientific phenomena" (1988, p. 523). Perhaps by creating a black hole in the laboratory? But then, as my colleague Bill Vacca points, out, that arguably would no longer be the work of an astronomer, but of a physicist.

10 To put this point in the eminently simplified terms that I heard once in a graduate seminar, Hacking's position is "If you can spray them, they're real." My point here is that Hacking's position is also: "If they're real, you can spray them." It is an implicit grasp of this point that is, it seems, behind the dissatisfaction astronomers I've talked to have expressed with Hacking's view, in cases where the response has been on the order of, "There are other reasons for believing something is real besides manipulating it in an experiment," or, "Something can be (sensibly believed to be) real without its being manipulated."

11 This echoes what Baird (2003) calls of material truth, where the authenticity of an instrument's function is tied in with the regularity and dependability of the phenomena it produces (p. 57).

12 Or even if they are not stable and reliable, experimental effects and what they portend can still be rather eye-catching. A case in point is Wing, Peimbert, and Spinrad (1967) who pursued the detection of potassium flares in astronomical optical spectroscopy which could have been identified with the characteristics of stars. Researchers followed up with several observations of stars, and then too tested the response of the spectrograph when a variety of matches were struck in its vicinity. And "[a]lthough the evidence is inconclusive, we feel that the match hypothesis presents the most simplest and most likely explanation of the spectroscopic observations" (p. 362). In other words, the data that revealed the presence of potassium tracked smokers in the observatory, not the characteristics of stars! Thanks to Bill Vacca for bringing this moment in the history of astronomy to attention.

13 A similar message occurs in Baird (1988): "Each successful instrument presents a phenomenon; each provides us with one connection with the real world. But, the very means that promotes our success at creating new instruments constrains what we see of the real world. In the first place, this is true because we demand a phenomenon of an instrument; this is what gives us confidence that the instrument is working properly – making contact with the real world" (p. 171). Thus, the phenomena that experimental practice create are important in justifying belief that the experiment works, and the experiment works in that it connects up with what is real in the world. Yet later in the same paragraph Baird continues, "[T]he sample of phenomena we access with our instruments is a convenience sample; there is no reason to suppose it is representative of all the phenomena nature has to offer. Convenience samples are biased, and inductions based on biased samples are not to be trusted. We should be shy to take any claim a theory makes that substantially generalizes away from our instrumental practice too realistically" (ibid., emphasis Baird's). Thus here, too, we have an awkwardness similar to what we see in Hacking: a tension between holding both that experimental phenomena are metaphysically and practically important, while somehow also not being altogether substantial, or natural, or representative of the world outside the lab.

14 This is an issue taken up by Kroes (1994), where he distinguishes between a strong and weak version of the phrase "to create phenomena." The strong version holds that experimenters create both the occurrence of a phenomenon, as well as its specific features. The weak version maintains that experimenters create the conditions so that a phenomenon can occur, but do not control its features (a process sounding not identical but very similar to Lelas' (1993) Heideggarian parallel between the experimenter and the midwife, where both remove obstacles and allow what's out there to come into spontaneous being). While by the letter, Hacking sounds as though he endorses the strong version, Kroes argues that the most sensible interpretation of Hacking is to take the creation thesis weakly. Under this reading, the phenomena created through experimentation do not sound so contrived and unconnected from the natural world. Thus experimenting stands to produce results that token phenomena outside the lab in the natural world, would that the conditions of the experiment obtain there. This reading of Hacking is warranted, yet I do not think it helps in connecting up laboratory results with the natural world as well as Kroes would like. A tie-in still lacks between the peculiar conditions inside the lab with natural events outside the lab where, as Hacking views it, laboratory conditions don't obtain. I imagine, for instance, a demented psychological experiment where experimenters lock their subjects up alone in dark rooms for weeks at a time with just enough food and water to keep them alive. At the end of the run, the experimenters announce their rigorous findings that people kept under such conditions exhibit characteristics A, B and C. What would these findings tell us about the behavior of human beings outside this lab? Not obviously anything, and the suggestion that the experimental phenomena would manifest, if those laboratory conditions maintained in the non-experimental world doesn't straightforwardly help, particularly if the presumption is that those conditions aren't likely to occur outside the lab. But where commonality can be found between the situation inside the lab and outside it, as in for instance between the assertion of authority and the Milgram experiments, or dominance and submissiveness and the Stanford prison

experiment, the situation is different; the connection between the inside and outside the lab are easier to draw. That's because events inside the experiment are not contrivances, but rather exaggerations of natural phenomena.

15 At least some of us have seen enough episodes of Law and Order to know that while everyone can be agreed as to the causal powers that produced, say, a homicide, that does not necessarily point out the correct agent responsible for the act. And of course we all know from our Hume, that finding a necessary connection between cause and effect is a no-win situation; but a necessary connection is not required. The absence of linkages between working with causal processes and the entities responsible for those processes is a point also made in Morrison (1990) pp. 6-13. On this same concern Harré (2003) and Woodward (2003) touch. Harré writes that we are entitled to draw connections from experimental practice to the natural world by "back inference," particularly where there are similarities between the two owing to an experimental model's being analogous to the natural world, as a domesticated animal or plant is analogous to its wild-type progenitors. Because of the similarity between model and natural world, experimental effects found via the model can be inferred back to the natural world. However, not all experiments straightforwardly allow of back inference. Such experiments Harré classifies as world-apparatus complexes, where the apparatus, unlike an isolatable model, interacts with a part of nature. This fusion gives rise to what he calls Bohrian artifacts. "Properly manipulated, they [the world-apparatus complex] bring into existence phenomena that do not exist as such in the wild – that is, in nature. In general, there is no material structure in nature like the apparatus" (p. 28). This kind of experimental process sounds much more like the kind of image Hacking has in mind, and of its ability to reveal truths about the natural world, Harré says its power is limited. We are shown evidence of the disposition of nature to react in certain ways, relative to the constraints imposed by the experimental apparatus. But "[a]t the deepest layers of scientific work there are no transparent windows on the world as it would be did the apparatus not exist" (p. 38). It may be in part because of this very difficulty of reading off experimental results what the objects are in question that exist to produce the effects, that Woodward takes the direction of experimental realism about causal relations. "[W]hat is worth taking most literally or realistically about [theories] are not their claims about what exists but rather their claims about relational structures and patterns and particularly their claims about how changing one quantity, property, or feature will change some other quantity. On this view, the causal or explanatory adequacy of a theory will have more to do with getting such relational features right than with getting the fundamental ontology right" (p. 114). Also touching on the topic is Morgan (2003), where she discusses that experimentation and its results are representative of the natural world insofar as the objects and events in the experimental process are similar to those in the natural world. But since the issue at hand in this essay concerns experimental events that are not similar to those in the natural world, Morgan's analysis does not assist here.

16 Technically, I don't believe it was conceived at the time that the galactic center was as much an object (as current astronomy tells us a black hole is at our galaxy's center) as a location, but I digress.

17 But for those who desire more information, here is Wilson to tell the story:

"My interest in the background measuring ability of the 20-foot horn-reflector resulted from my doctoral thesis work with J. G. Bolton at Caltech. We made a map of the 31 cm radiation from the Milky Way and studied the discrete sources and the diffuse gas within it. In mapping the Milky Way we pointed the antenna to the west side of it and used the earth's rotation to scan the antenna across it. This kept constant all the local noise, including radiation that the antenna picked up from the earth. I used the regions on either side of the Milky Way (where the brightness was constant) as the zero reference. Since we are inside the galaxy, it is impossible to point completely away from it. Our mapping plan was adequate for that project, but the unknown zero level was not very satisfying. Previous low frequency measurements had indicated that there is a large, radio-emitting halo around our galaxy which I could not measure by that technique. The 20-foot horn-reflector, however, was an ideal instrument for measuring this weak halo radiation at shorter wavelengths. One of my intentions when I came to Bell Labs in 1963 was to make such a measurement. " (Wilson 1980: 600)

18 On Heidelberger's (2003) account, scientific instruments can carry (at least) two functions: constructive and productive. Under their productive function, instruments produce phenomena, and among the instruments here Heidelberger includes telescopes. His constructive function sounds more like what Hacking sees experimental tools performing: isolating phenomena in their pure form, or manipulating phenomena to behave in certain ways (pp. 146-47). In either function, phenomena are produced, and on his reading then, contrary to Hacking's, astronomers to produce phenomena.

19 A similar point is made in Tiles (1993) p. 471. As he writes, of course astronomers' studies do not affect the course of astronomical objects in the least. However, astronomers do study the electromagnetic radiation emitted from astronomical objects and that radiation cannot be so quickly separated off as something distinct from the emitting objects themselves. Thus, being able to manipulate the former stands to matter, to some degree, in manipulating the latter. The difficulty I have with Tiles' points is some conflation between radiation and data, and astronomical object and phenomenon. The electromagnetic radiation received from distant objects sounds to be categorized by Tiles as both data, and part of the astronomical object. The astronomical object itself, which should in Hacking's terminology be dubbed the experimental entity (or tool) Tiles calls a phenomenon that, again by Hacking's terminology, pertains to an experimental effect. Then too there is here the difficulty discussed earlier pertaining to

the relationship between an entity and its causal powers, such that it is not exactly straightforward how the manipulation of the latter is a manipulation of the former.