UNIVERSITY OF CALIFORNIA, SAN DIEGO

Underdetermination and the Claims of Science

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy
in Philosophy

by

P.D. Magnus

Committee in charge:

Professor Paul M. Churchland
Professor Donald Rutherford
Professor Nancy Cartwright
Professor Naomi Oreskes
Professor Martin Sereno

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I completed my dissertation after digital technology had overtaken word processing, but before it overtook the submission and archiving of dissertations. I prepared it in LaTeX, processed it as a PDF, printed it on cotton paper, and submitted it in duplicate to the Office of Graduate Studies and Research. OGSR sent both copies to the library, and one copy was forwarded to University Microfilms, Inc. UMI photographed the pages so as to make a copy on microfilm, scanned the microfilm, and made the scans available as a PDF. It is a grainy, uneven scan. It is not searchable. UMI charges money for it, unless you request it from the UCSD network.

It is the 21st century, and we can do better. Here is the original PDF: The fonts are fonts, and they render as vectors. The contents are fully searchable. This is available free, and you are welcome to distribute it intact for noncommercial purposes.

There are, no doubt, errors in the text. There are certainly things I would do differently if I were writing it fresh. With the exception of the signature page, however, this is the dissertation exactly as I defended it in March 2003. (Committee members signed the actual physical copy, and so the signature page here has empty lines.)

The bibliography contains entries for two forthcoming papers. They have since appeared; here are the full citations:

'Underdetermination and the Problem of Identical Rivals.' Philosophy of Science, 70(5): 1256-1264. December 2003.

'Success, truth, and the Galilean Strategy.' British Journal for the Philosophy of Science, 54(3): 465-474. September 2003.

P.D. Magnus

Albany, New York

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The dissertation of P.D. Magnus is approved, and it is acceptable in quality and form for publication on microfilm:

Chair

University of California, San Diego

2003

For Cristyn. Words are insufficient on this matter,

so I'll let a smile and a nod passed between us suffice.

"Whenever you read a good book, it's like the author is right there, in the room talking to you, which is why I don't like to read good books."

—Jack Handy

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Preface

It is common to say that underdetermination gives us a reason to rethink science— to curtail its authority in some way. This underdetermination is sometimes taken just to be the problem that theories might have empirically equivalent rivals. It is sometimes taken just to be the Duhem-Quine problem that theories require auxiliary hypotheses in order to issue in testable predictions. It is sometimes taken just to be the problem of induction (either Hume's or Goodman's). And so one might suspect that 'underdetermination' is not one phenomenon at all, but rather a crypto-omnivorous catch-all for phenomena that mitigate the authority of science. The present project lays this worry to rest, I hope, by providing a framework within which all these worries can be seen as species of one genus. Within the framework, many familiar worries about underdetermination appear in a new light. Some underdetermination shows nothing interesting about science, and no underdetermination undercuts the authority of science *tout court*.

I began this project with modest expectations, with a fallibilist but non-sceptical conception of science— by which I mean that we might be wrong about any specific claim we make about the world, but that nevertheless scientific knowledge is possible. Some of what I say can be taken as motivation for this view; see especially the general epistemic considerations of Ch II. I don't think we need underdetermination to arrive at this view, however. It suggests itself if we look to the practice and history of science. And so when I say in what follows that underdetermination tells us nothing new about science, I mean that it tells us nothing new given that we were already fallibilists.

One may insist that 'x knows p' entails 'x could not be wrong about p.' Thus, fallibilism is tantamount to denying that we *know* anything, and 'fallibilist but non-sceptical' is a contradiction. If one did insist on this, then I think underdetermination would provide one with great head-aches. Similarly, if one thinks that science provides the unquestionable *truth*, then underdetermination should give one a sour stomach. And so on.

Acknowledgements

The present text is the result of many hours, a great number of them spent in coffee houses in the San Diego area. I have been especially thankful for the Grove (which was offered as an enticing example of the local scene when I visited in 1996), Ken Coffee (where, for some reason, I often end up calculating probabilities), Lestat's Coffee House (open 24 hours), Muir Woods (just across from the office), Peet's Coffee and Tea (a convenient mid-point in the walk from my apartment to the department), and Wired (a misleadingly named French café nearby).

I owe a great deal to friends of mine, who indulged me in conversations about all sorts of things. Among my fellow graduate students, I extend special thanks to my longtime officemate Ryan Hickerson; to fellow pragmatists Ilya Farber and David Smith; also to Mark Newman, Andrew Hamilton, and Luke Robinson. A special nod goes to Carl Sachs, who bet me a beer that I couldn't work the phrase 'mad knowledge of self' into my dissertation.

I owe a great deal to teachers of mine, who did me that weightiest favor of taking my ideas seriously. At TCU, Richard Galvin and Ted Klein encouraged me and ushered me on toward graduate school. At UCSD, I had the good fortune to study with a parade of first-class philosophers of science, including Jeff Bub, Craig Callender, Nancy Cartwright, Paul Churchland, Gerry Doppelt, Clark Glymour, Philip Kitcher, and Sandy Mitchell.

Other intellectual debts are more subtle. After a talk in Spring 1996, Janet Broughton impressed upon me the fact that Cartesian doubt is not a mere

will to doubt; I recognized the impact of this point on my reading of Descartes only after Ch II was more or less complete. Gideon Yaffe introduced me to the works of Thomas Reid while he was a visiting professor at UCSD in 1998–9; it is on Gideon's account, I think, that I see applications for Reid's epistemology everywhere. And so my thinking has been nudged along for the better by many influences so profound as to escape my notice.

The discussion of notational variants and what I call the problem of identical rivals (§1.2.3) was presented in an earlier form to the Southern California Philosophy Conference in October 2001 and at the Philosophy of Science Association meeting in November 2002 [Mag03]. The discussion of the Galilean Strategy §3.1.2 was originally written as a stand-alone paper [Mag], but also found a natural home here. The discussion of Helen Longino's work (§4.3) is descended from work presented at the University of Alabama, Birmingham in February 2001— Longino was in attendance and provided helpful comments.

VITA

Fall 2000	UCSD. M.A. in Philosophy
Spring 1996	Texas Christian University. B.A. in Philosophy and Physics, summa cum laude with University Honors

PUBLICATIONS

'The price of insisting that Quantum Mechanics is complete.' Accepted and forthcoming in the *British Journal for the Philosophy of Science*.

'Success, Truth, and the Galilean Strategy.' Accepted and forthcoming in the *British Journal for the Philosophy of Science*.

'Underdetermination and the Problem of Identical Rivals.' Accepted and forthcoming in *Philosophy of Science*, September 2003.

FIELDS OF STUDY

Philosophy

ABSTRACT OF THE DISSERTATION

Underdetermination and the Claims of Science

by

P.D. Magnus

Doctor of Philosophy in Philosophy

University of California, San Diego, 2002

Professor Paul M. Churchland, Chair

The underdetermination of theory by evidence is supposed to be a reason to rethink science. It is not. Many authors claim that underdetermination has momentous consequences for the status of scientific claims, but such claims are hidden in an umbra of obscurity and a penumbra of equivocation. So many various phenomena pass for 'underdetermination' that it's tempting to think that it is no unified phenomenon at all, so I begin by providing a framework within which all these worries can be seen as species of one genus: A claim of underdetermination involves (at least implicitly) a set of *rival theories*, a *standard* of responsible judgment, and a *scope* of circumstances in which responsible choice between the rivals is impossible.

Within this framework, I show that one variety of underdetermination motivated modern scepticism and thus is a familiar problem at the heart of epistemology. I survey arguments that infer from underdetermination to some reëvaluation of science: *top-down* arguments infer *a priori* from the ubiquity of underdetermination to some conclusion about science; *bottom-up* arguments infer from specific instances of underdetermination, to the claim that underdetermination is widespread, and then to some conclusion about science. The top-down arguments either fail to deliver underdetermination of any great significance or (as with modern scepticism) deliver some well-worn epistemic concern. The bottom-up arguments must rely on

cases. I consider several promising cases and find them to either be so specialized that they cannot underwrite conclusions about science in general or not be underdetermined at all. Neither top-down nor bottom-up arguments can motivate any deep reconsideration of science.

I

Framing the Problem

> "[If] science refuses to understand that there is something which it cannot understand, or better still, that there is something about which it clearly understands that it cannot understand it— then all is confusion. For it is the duty of human understanding to understand that there are things which it cannot understand, and what those things are."
>
> —Søren Kierkegaard, 1847 [Kie58, p. 177]

The claims of science enjoy a presumptive authority in our society. There is already something odd about saying this, of course, since *science* does not speak with its own mouth. The particular claims that are taken as authoritative are, on any particular occasion, made by some scientist or other. Understanding some claims as the claims of science presupposes that those scientists are proceeding in a responsible way such that something distinguishes what they say about the world from what dogmatic non-scientists say about it.

This is a familiar enough line of thought: There are limits to the rightful authority of science. Neither scientists nor the laity properly understand those limits. As a result, we are in a great muddle.

In the 20th century, such thoughts have been advanced under the banner of *underdetermination*. Invoking this shibboleth, the worry may be put in this way: The choice of scientific theories on the basis of evidence is underdetermined.

A successful scientific theory isn't better in some compelling sense than its losing rivals. Underdetermination should lead us to doubt the presumed authority of science.

This conclusion is often taken to be an accepted result of post-Kuhnian science studies. Putting the situation in bombastic terms, Philip Kitcher writes: "Because massive underdetermination of belief by 'objective' factors came to seem omnipresent, there opened up a vacuum into which social explanations of scientific behavior could be inserted. Instead of an ordered abode of reason, science came to figure as the smoke-filled backrooms of political brokering" [Kit93, p. 7]. Underdetermination made science seem like just another negotiated social arrangement.

Dale Jamieson writes that one major "source of indeterminism flows from the underdetermination of theory by data. ...While there may be grounds for preferring one of two empirically equivalent theories, there is no empirical fact of the matter about which theory is true..."[Jam96, p. 39]. Jamieson has an obscure way of putting the point, but it should be noted that he mentions underdetermination only as an aside. His central concern is elsewhere, and he does not dwell on "the underdetermination of theory by data" because he supposes that it is an established and understood phenomenon. Indeed, it has become quite common to presume that 'underdetermination' is some well defined, even if unappreciated, fact of our epistemic situation.

If there is to be an inference with underdetermination as its antecedent, underdetermination must be brought out of the shadows. It must be laid out cleanly and not be allowed to hide behind a rhetorical flourish. This chapter lays out a formula for underdetermination and explores variations in its parameters. Of course, bringing underdetermination into focus is not enough. The ultimate question is whether we should reconsider the authority of science on account of this underdetermination. It is too early to answer this ultimate question, however, because it has not yet even been asked clearly.

1.1 A working definition

Consider an epistemic agent— a scientist— who is trying to decide whether or not to believe a theory about the world. Say her choice is underdetermined if, *given the evidence she has and the other things she knows about the world, she is unable to responsibly decide whether or not to believe the theory.*

One might say immediately that this definition is too broad, because it is an ordinary enough thing to be unable to decide between rival theories. One reason there is enquiry at all is because, initially at least, there is no way to decide between rival accounts. The point of enquiry is to gather evidence and make determination possible. We might restrict the definition and say that underdetermination occurs where *no epistemic agent could ever make a decision between rival theories.* This is underdetermination with a philosophical sting, not mere ignorance to be redressed by further investigation. However, this narrower definition turns on the notion of decisions which could never be made and so, more generally, on the notion of what could never be; these notions, in turn, are open to many interpretations.

We might suppose that possibility here is *logical possibility.* Underdetermination so understood would obtain, for instance, between logically equivalent theories.[1] Yet it is not at all clear why logically equivalent theories should even be understood as rivals. Where theories are equivalent, we typically think of them as one and the same theory.

We might instead suppose that the possibility here is *natural* or *physical possibility*— what could actually occur given the laws that govern the universe. This assumes, of course, that there really are laws that govern the universe. Perhaps for the sake of generality, we should not make so strong an assumption. Further, some things may be allowed by the laws but remain for practical purposes impossible. Perhaps the deciding experiments would require the energy of ten-thousand suns or arrangements so unlikely that they are unapt to occur even once in the lifetime of the universe. Perhaps organizing the data would require

[1] It may be logically impossible for any agent to decide even when the theories are logically distinct.

a repository the size of Saturn and analyzing it would demand god-like computational powers. We may confine ourselves to *practical possibility*, but this would relativize the discussion to some level of technical prowess.

Worried about counterfactuals, we may insist that an agent can resolve a dispute only if she will in fact resolve it. This would save us from needing an account of possibility, but at the cost of making underdetermination depend on what will come to pass for us. This brings to the fore a different problem with the definition we are considering: Why should underdetermination obtain only when responsible decision is impossible *for any* querist? Although scientists should be concerned if no *conceivable* person could decide between two rival theories, they should be just as concerned if no member of their scientific community could ever make the decision.[2]

How we define underdetermination will significantly effect what is to follow. A narrow, strict definition of underdetermination will make it out to be a monolithic phenomenon. A more productive analysis will allow for underdetermination of various sorts, varying with the range of conditions across which no responsible decision can be made between rival theories and the range of querists for whom the decision is impossible. Call these the *scope* of the underdetermination. The widest scope takes in all logically possible circumstances and all logically possible scientists. Considering physically possible circumstances narrows the scope. Considering only practically possible circumstances narrows the scope further and allows for the eventuality that a decision might be underdetermined for one community but not for another— perhaps the latter community has quantum computers but the former does not.

We may thus represent a case of underdetermination by specifying a set of rival theories, a scope over which decision between rivals is underdetermined, and a standard for responsible theory choice. Varieties of underdetermination may

[2]It may be that epistemic virtue can be given a methodologically individualistic treatment, or it may be that there are irreducibly social dimensions. The point here is only that the possibility of determination only by someone far from the scientific community would be small comfort to scientists.

be characterized by differences in any of these elements.

1.2 Variations in the set of rivals

The general formula for underdetermination requires the specification of a set of rival theories, of the theories between which the querist might choose. Given the language of theory *choice*, one might suppose that certain constraints ought to be placed on what can count as a set of rival theories.

In the first place, one might insist that each of the theories in the set of rivals be one which the querist could seriously entertain and possibly choose. The Theory of Relativity was not a rival to Aristotelianism for Galileo, one might say, since entertaining the Theory of Relativity as a serious possibility was only possible after developments that occurred in the 19th century. Following William James, say that rivals are *live options* for you if “trained as you are, each hypothesis makes some appeal, however small, to your belief” [Jam48, §I]. This first proposed constraint can then be expressed as the requirement that each rival must be a live option.

In the second place, one might insist that the set of rivals be exhaustive—that the querist cannot help but choose one of them.[3] Without such a constraint, the set of rivals might be something so obscure as the set of unitarianism (‘God is an undivided one’) and trinitarianism (‘God is three in one’). Insofar as the ultimate nature of God is unknowable, the choice between these is underdetermined. The intuition is that there is something unfair about this case, since a querist may insist that there is no God. Again, a querist may choose to suspend belief. So the set of rivals should contain not only unitarianism and trinitarianism, but also atheism, agnosticism, pantheism, and whatever else.

In the third place, one might insist that the set of rivals be exclusive—that the querist can choose at most one of them. This raises difficult questions about theory identity and about how we can distinguish theories from one another.

[3] This is to insist that the choice be *forced*, to use James’ terminology.

1.2.1 Live options

Underdetermination arguments often aim to show that there are some unappreciated rivals to the theories we presently esteem. Some attempt to do this by providing an algorithm for generating such rivals.[4] The arguments are not predicated on the hope that scientists will find the algorithmically generated rivals to be live options. John Earman, commenting on such a spurious rival, reflects a common sentiment in saying "it is hard to get excited about this example" [Ear93, p. 31].

Perhaps 'underdetermination' should be reserved for cases worth getting excited about. If so, the rivalry between an actual theory and its algorithmically-generated, pedantic counterpart would not be underdetermined, since the generated variant is not a live option and hence is inadmissible as a rival. I might reply that such a constraint on what will count as underdetermined opens the door for individuals to believe whatever they want, since they might marshal their will toward killing off the other options and leaving only their preferred possibility. The suggestion may be amended, then, to allow a community of responsible querists to settle such matters in order to average over the willfulness of individuals. Laudan and Leplin make a suggestion rather like this one and say that the scientific community should be relied upon to distinguish real from toy rivals [LL93, p. 12].

Whatever may be said for arguments based on algorithms for generating toy rivals, they are commonly enough called 'underdetermination' arguments, and so a general account of underdetermination should have something to say about them. It may be that we find these sorts of cases uninteresting— we find them to be *cheap*— but that is to say that a case of underdetermination has a different significance if the set of rivals contains some esteemed theory and its toy rivals than if the set of rivals contains two or more live options. I have yet to give an argument for this differing significance, but stated as a conclusion about the significance of

[4] Van Fraassen [van80] and Kukla [Kuk93] are among authors who make this sort of argument. I argue in the next chapter that Descartes' arguments in the First Meditation are also of this sort.

different sorts of underdetermination it can only be formulated if we do not insist that sets of rivals must contain only live options.[5]

1.2.2 Exhaustive choices

In order to avoid odd choices like the one between unitarianism and trinitarianism, the suggestion is that the set of rivals must exhaust possible choices. This might be achieved by adding a 'None of the above' rival to every set of rivals. Note, however, that many of the logically possible choices will not be live options in the sense of the previous section. There may be particular scholars for whom unitarianism and trinitarianism are the only live options, and for them underdetermination for that set of rivals may really be a source of consternation. That choice does not confront a different querist, if atheism is a live choice for her. Relevant cases of underdetermination will usually be ones in which the set of rivals contains all the rivals which are live options for the querists in question, although that again is to distinguish between the significance of different cases and not to exclude some cases from 'underdetermination' proper.

There is another reason not to insist that 'real' underdetermination must involve mutually exhaustive rivals. The long-running contest between the wave and particle theories of light was a legitimate scientific dispute. Although we recognize in retrospect there were options besides wave and particle theories, an unspecified 'None of the above' was not and is not a serious option. An observation like the celebrated Poisson bright spot worked to confirm the wave theory over the particle theory, but how could it have worked to confirm or refute 'None of the above'? The catch-all hypothesis is underspecified, making it impossible to assess its relation to evidence.[6]

[5]Such an argument will have to wait for Ch III.

[6]Salmon, expressing this issue in the context of Bayesian confirmation theory, argues that it is impossible to represent the likelihood of evidence given the catch-all hypothesis [Sal90].

1.2.3 Exclusive choices: the problem of identical rivals

The third suggestion is that the set of rivals should be such that the agent can only choose one of them— this is suggested even by just calling it the set of *rivals*. This can be accomplished formally by taking the power set of the set of rivals, disposing of the inconsistent elements, and considering underdetermination with that set as the set of rivals. In practice, this formal trick wouldn't be enough.[7]

Take a simple example of two atomic theories. The first is the usual theory according to which 'electrons' are negatively-charged particles at the periphery of atoms and 'protons' are positively-charged particles in the core of atoms. The other maintains, contrarily, that 'electrons' are positively-charged particles in the core of atoms and that 'protons' are negatively-charged particles at the periphery of atoms. Suppose further that the latter theory attributes every feature to the 'electron' that the former attributes to the 'proton' and vice versa. The latter theory includes the claim, for instance, that the 'electron' has the same mass as the 'neutron.' How do these two fare as rivals?

It is tempting to say that these are not even distinct theories. The Russian or French translations of our usual atomic theory are nevertheless the same *theory* as the first theory we are considering. So, too, we might insist that the second theory is just another formulation of the first— not in Russian or French, but in its own obscurantist argot. If the two were merely translations of one another, then we wouldn't have distinct theories and *a fortiori* we would not have rival theories. If this were so, it would make no sense to ask whether the choice between the two was underdetermined; the answer would be trivially negative. If they were really distinct, however, then the choice between them is plausibly underdetermined. So, identifying underdetermination seems to turn on telling whether two theories are merely notational variants.[8]

[7] An early version of the material in this section section was presented at the Southern California Philosophy Conference in October 2001. A later version was presented at the Philosophy of Science Association meeting in November 2002 and will be published in the conference proceedings [Mag03].

[8] Horwich considers such cases and claims on grounds of common usage that theories like the second are false [Hor82]. Like the present suggestion, this avoids underdetermination between alleged rivals just

Put the worry this way: We can't reasonably say whether the choice between two rival theories is *underdetermined* if the two putative rivals might only be rival formulations of one theory. Identifying when putative rivals are merely alternative formulations of the same theory is not an easy thing. If we give it the somewhat paradoxical label *the problem of identical rivals*, then the worry is that resolving the problem of identical rivals is a necessary condition for a serious discussion of underdetermination.

Quine's solution

This worry motivates Quine, who considers cases like this electron-proton inversion and suggests, just as I have, that they be treated as two formulations of one theory [Qui75, p. 319–20]. He generalizes from these cases and proposes that "two formulations express the same theory if. . .there is a reconstrual of predicates that transforms the one theory into a logical equivalent of the other" [Qui75, p. 320].[9] A reconstrual of predicates is a mapping from the predicates of one language onto the open formulae of the other, such that each n-place predicate is mapped onto a formulae with n free variables. In our example, the reconstrual is straightforward: map the predicate *electron* onto the sentence 'x is a proton' and v.v.

Quine goes on to consider "a less trivial case," one which he attributes to Poincaré— more on Poincaré in a moment. The example involves two cosmologies: "Here we have one formulation of cosmology that represents space as infinite, and another formulation that represents space as finite but depicts all objects shrinking in proportion as they move away from the center" [Qui75, p. 322]. The latter cosmology is like the tiled surface in MC Escher's Circle Limit prints (figure 1.1). Quine insists that these two cosmologies are alternative formulations of the same theory just as our two 'atomic theories' were. He does not specify how this is

when we have a way to tell that alleged rivals are notational variants.

[9] Quine also requires that the two formulations must be empirically equivalent. Nothing here turns on that addition.

so, but instead asserts that the reconstrual, although “less simple,” “presents no serious challenge” [Qui75, p. 322]. Quine leaves this as an exercise for the reader, but the reader may well be puzzled.

There is a serious— perhaps insurmountable— challenge which suggests that Quine’s claim about these two cosmologies is simply false. The latter cosmology, we may expect, has a predicate ‘Point x is the center of the universe’ which is satisfied by exactly one point. How are we to reconstrue this in the idiom the first cosmology, an idiom in which no point enjoys such a unique status? If there is no such reconstrual, then the two cosmologies are distinct theories even by Quine’s own criterion.[10]

Moreover, the structure of the two cosmologies may be discernably different. It seems natural to suppose that figure 1.1 illustrates a cosmology like Quine’s second. All the angels and devils depicted in the Escher print are interchangeable with all the others; the smaller ones are smaller only on account of being further from the center. Yet on an infinite Euclidean plane without distortions, for any given hexagon there are six other hexagons that share a side and two vertices with it and no hexagons that share only a vertex with it (as in figure 1.2a). In the Escher print, a sextet of angels and devils combine to form a hexagonal figure. For each such hexagon there are six hexagons that share a side with it (just as for the Euclidean case), but *six* other hexagons share only a vertex (figure 1.2b). The angels and devils need only look to see what’s around them to determine which cosmology describes their universe. The two cases are discernably different.[11]

Although Quine’s example misfires, we can try to provide one more suited to his purposes. Let two cosmologies be given in this way:

$\mathbf{C}_1$ Spacetime has some geometry G, and everything in spacetime follows such-

[10]The fact that no observer in the second cosmology could determine which point satisfies this predicate is irrelevant. Saying the two theories are identical is stronger than saying that they practically indistinguishable.

[11]One might take this as a reason to think that figure 1.1 *does not* illustrate Quine’s second cosmology. Regardless, it underscores the point that these cosmologies are substantially more complicated than Quine admits.

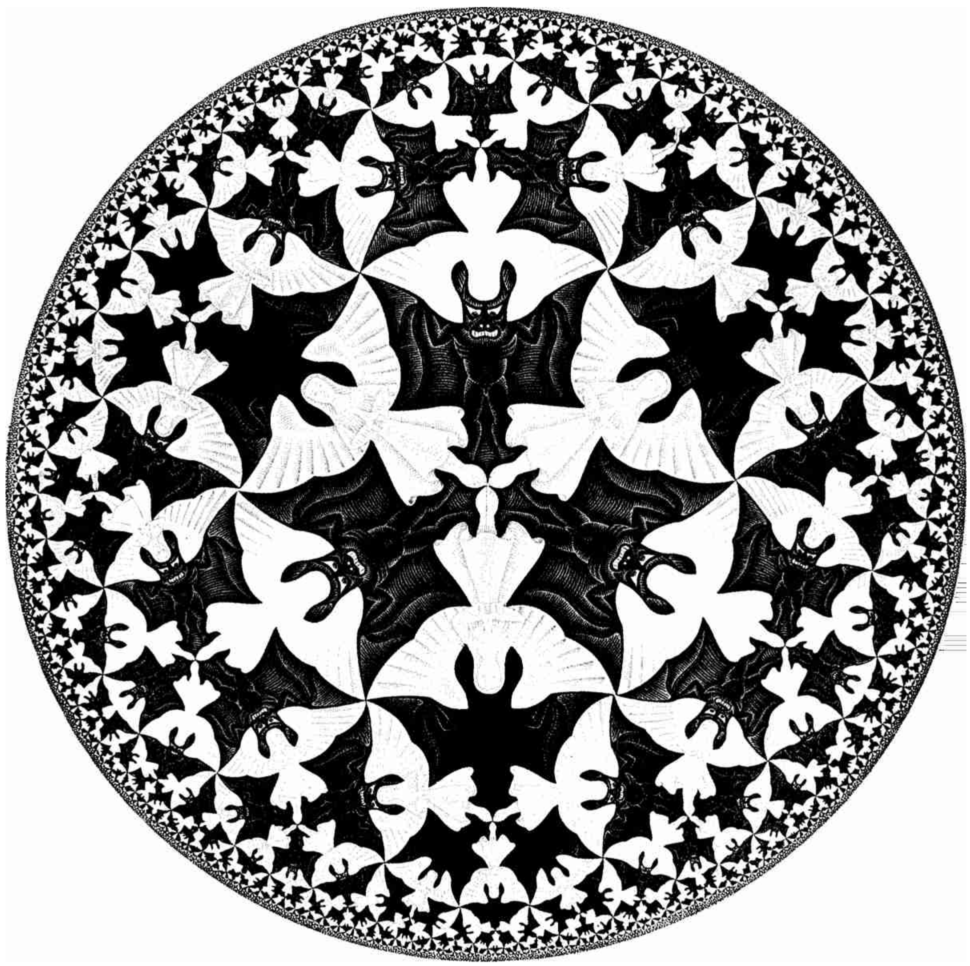

Figure 1.1: "Circle Limit IV." Finite space in which objects shrink as they move away from the center would be like this M.C. Escher illustration.

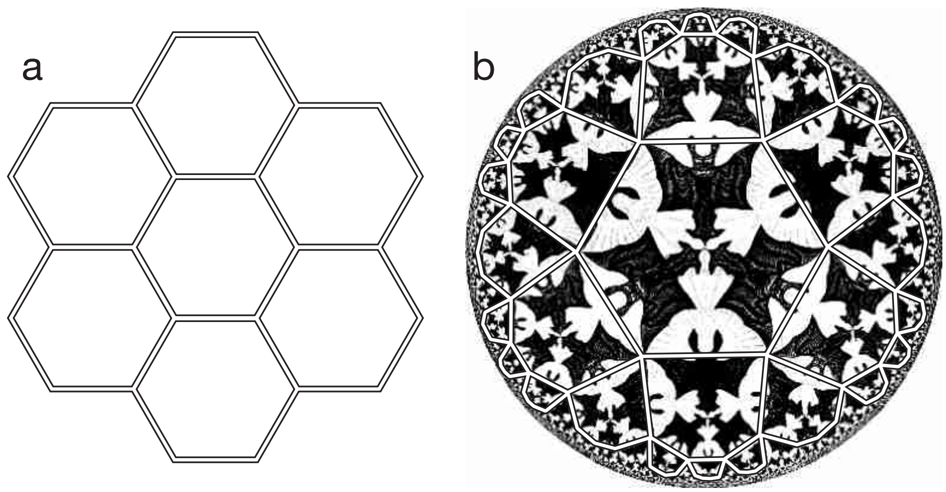

Figure 1.2: (a) An undistorted Euclidean hexagon. Six other hexagons share a side with it; zero share just a vertex. (b) Each hexagon in the Escher print shares just a vertex with six other hexagons.

and-so inertial trajectories.

C_2 Spacetime has some alternate geometry G^*, and universal forces act on everything in spacetime such that all trajectories conform to C_1.

Here we may have one cosmology that represents the curved spacetime of general relativity and another that represents a flat spacetime plus universal forces. It looks as if C_1 and C_2 are identical on Quine's criterion. We can take any claim about geometry in C_1 and map it onto a claim about *physical geometry* in C_2, and so on. Of course, this rivalry provides the basis for Reichenbach's argument that geometry is underdetermined by physical theory. Several authors, presuming the cosmologies to be distinct, have dared to argue that one is better confirmed that the other![12] We cannot even take them seriously if the cosmologies are not rival theories. Usefulness might distinguish one formulation from another, but confirmation will not. Thus, it seems that consequences of Quine's identity criterion are altogether too strong.[13]

[12]For Reichenbach's argument, see [Rei58], [Rei51, ch. 8], and §4.1.2. For representative replies, see [Gly80, ch. 9] and [Grü60].

[13]Similar conclusions are drawn by Mühlhölzer, who concludes that "Quine's criterion. . .blurs impor-

Quine does not say where Poincaré offers his example, but Poincaré surely does offer cases like it. He writes, for instance, of

> two universes which are the image one of the other. With each object P in the universe A, there corresponds, in the universe B, an object P^1 which is its image. The co-ordinates of this image P^1 are determinate functions of the object P; moreover, these functions may be of any kind whatever.... [Poi52, p. 98]

Poincaré maintains not that the claim 'We are in universe A' and the claim 'We are in universe B' are the *same* claim, formulated one in the language of A and the other in the language of B; rather, he says that "these two universes will be indistinguishable" [Poi52, p. 98]. He sees this as grounds for refusing to make any claims about absolute space whatsoever. Thus, where Poincaré insists that the choice of a theory about absolute geometry is underdetermined, Quine insists that it cannot be underdetermined because all the empirically adequate ones would— on final analysis— prove to be merely formulations of one theory.

The naturalist rejoinder

Quine's criterion would allow a scientist to dispose of the electron-proton inversion case, but she is unlikely to take it seriously without Quine's help. Since a scientist can dismiss that case without a formal justification, we might look to scientists and to the scientific community to determine which formulations represent distinct theories. This follows the advice of Larry Laudan and Jarrett Leplin, who counsel a policy of "deference to scientific judgment as to what constitutes a theory" [LL93, p. 13].[14]

It is one thing to look at scientific practice and attempt to abstract from it principles by which to guide our enquiry. We might use insights gleaned from

tant distinctions..." [Müh94, p. 123]. Quine's criterion would also collapse other interesting rivalries, such as solipsism vs. other minds (cf. §1.3.3) and multiple connection vs. massive reduplication (cf. §4.1.3).

[14] Laudan and Leplin make this suggestion as a way to determine which rival theories are serious rivals, so it may not be fair to attribute its consequences to them when it is applied to theory identity. Nevertheless, looking to the scientific community to resolve disputes about theory identity is a plausible enough naturalist move.

the bulk of science to shed light on some particular part of it. The proposal here is another thing entirely: that we should take our cue from particular scientific judgments. If we defer to science on each particular, we would not be able to dissent from any scientific judgement. We could adduce no normative principle besides 'Follow the scientific community in all things'— a principle trivially followed by the scientific community even where it stumbles or goes astray. If scepticism is the Scylla of epistemology, then quietism is its Charybdis. 'Believe what you believe' is of as little practical value to a deliberating agent as 'Believe nothing.'

The suggestion may be seen as something more than quietism insofar as it recommends for philosophers to defer to scientists. Advice to defer to experts may be of use to deliberating agents who are not themselves experts. This might be a blow to the egos of philosophers but perhaps it would be all well and good, if only scientists had expertise on the identity of theories beyond the ken of philosophers. However, scientists don't centrally concern themselves with criteria of identity or meaning. They employ and criticize particular theories, but they do not by and large pay attention to *theory as such*. Moreover, there have been theories in the history of science which at one time were considered rivals but which came to be seen as alternate formulations of a single theory. The lesson of such episodes is that deference to scientific judgment on these matters might lead us astray.

The case of matrix and wave mechanics

This point is best pressed home by considering a specific example. In 1926, two formulations of quantum mechanics were on offer: *matrix mechanics* which had been introduced by Werner Heisenberg and others the year before and *wave mechanics* formulated by Erwin Schrödinger. Schrödinger and Carl Eckart independently published results which claimed to show that matrix mechanics and wave mechanics were equivalent. Thus, matrix and wave mechanics were at most rival formulations of one common theory. It is usual to say both that the two were equivalent and that the papers by Schrödinger and Eckart provided valid reason for thinking so. A typical physics textbook claims that Schrödinger "showed that the matrix and wave mechanics formulations give identical results and differ only in their mathematical form" [TR93, p. 208]. Philosophers make similar claims, saying, for example, "The early formulations of the theory, by Heisenberg and Schrödinger, were, respectively, in terms of sequences and of functions; subsequently Schrödinger established that... the two formulations were equivalent." So, the two are treated by almost everyone as being merely "formulations of the [singular] theory" [Hug89, p. 45]. Such claims are so widespread as to count as common knowledge.[15] The connection with underdetermination has not gone unnoticed: Philip Kitcher uses the notion that the two are merely rival formulations to deny that the choice between them was underdetermined [Kit01a, p. 195] [Kit01b, p. 35], as do Wilson [Wil80, p. 217] and Sklar [Skl85d]. Yet, as Norwood Russell Hanson remarked, "the unguarded statement that Wave and Matrix Mechanics are equivalent physical theories is so unsound, historically and even conceptually, that a re-examination of the issue might still be tolerable" [Han63, p. 113].

F.A. Muller employs the structural view of theories in such a reëxamination and, like Hanson, concludes that Schrödinger's 1926 paper did not show what it is so often taken to show.[16] Even the usual account concedes that matrix and wave mechanics were associated with different *ontologies*— the former quite deliberately involved no commitment to unobserved states of particles, whereas the latter treated particles as quivering puddings of mass and charge.[17] This difference in ontology is not merely a matter of labels. Because waves are distributed in space, wave mechanics has the resources to express spatial relations. Apply this to a concrete case and consider a charged particle detector that occupies some specified region of space and an electron that turns up in it. Since waves disperse over time, wave mechanics predicts that the detector should not detect the entire

[15]Muller, who dubs the view the Equivalence Myth, provides a no doubt incomplete list of almost fifty sources that promulgate it [Mul97, p. 37].

[16]Muller cites Hanson as someone who "denies the equivalence, but unfortunately for all the wrong reasons" [Mul97, p. 37, fn. 4]. This is probably unfair, but I make no attempt here to resuscitate Hanson's reasoning.

[17]In Muller's phrase, "tiny jelly-like lumps of vibrating charged matter" [Mul97, p. 229].

charge of the electron. Only part of the electron wave will be in the detector, and only that part would be detected. Since there is no straightforward way to represent spatial coordinates in matrix mechanics, contrariwise, matrix mechanics does not yield this prediction. Muller [Mul97, p. 227] notes this prediction might have been tested, allowing the opportunity to distinguish empirically between wave and matrix mechanics. Rather than being mere window dressing, the differing ontologies reflect differences in expressive power and empirical upshot. With the further notion of wave collapse— the supposition that the wave becomes localized when it is observed— wave mechanics would not yield the critical prediction. Be that as it may, the notion of wave collapse had not been introduced at the time of Schrödinger's alleged equivalence proof.[18]

This historical episode holds an important telling point against the naturalist criterion for theory identity. Scientists can believe that two theories are merely rival formulations of one theory even when they have insufficient reason for doing so. This may be self-fulfilling; if further development of each rival is directed toward making the supposed identity more explicit, then successors of the two rivals may be formulations of some one theory even if the initial rivals were not. Surely, it is also possible for two rivals to be rival formulations of one theory even though scientists do not know that to be the case.

Taking stock

Muller's work using the *structural* conception of theories provides an example of how specific questions of theory equivalence may be resolved, so one might think that it shows how the problem of identical rivals may be solved *in general*. This structural conception (called variously the semantic or model-theoretic conception) requires specifying the theory as a class of set-theoretical structures (or models). Critics have charged that this formal requirement elides important fea-

[18]One may say that Schrödinger's theory of waves *qua* matter waves was simply falsified by subsequent developments.

tures of scientific theories,[19] and some advocates of the semantic conception argue for treating theories as 'models' in an informal, non-mathematical sense.[20] Where models are not or cannot be specified set-theoretically, Muller's approach can find no purchase. Thus relying on the structural conception here would be to replace one dispute with another.

Without attempting to decide between the structural view and its many rivals,[21] I remind the reader that theories are products of human craftsmanship. Not only do theories have histories, but *theory* itself has a history. Whatever it is now, it came to be that way and may come to be different. Philosophers can accompany an account of theory with a call to represent theories in that way,[22] but theory as scientists meet it is red in tooth and claw. Closer examination of episodes like the dissolving rivalry between wave and matrix mechanics might allow us to adduce principles of theory identity and non-identity— principles informed by and applicable in practice. Let me simply observe that questions of theory identity are unresolved and that their resolution would demand considerable further work.

Above, we encountered the worry that solving the problem of identical rivals was a precondition for talking seriously about underdetermination. Absent such a resolution, what can we say about underdetermination? Any two theory formulations which a scientist takes to be distinct can be considered as rivals for the purpose of asking if the choice between them is underdetermined. This enquiry might be fruitful in one of two ways. First, if the choice between rivals can be shown not to be underdetermined, then the theories must be distinct. Even if we don't know necessary and sufficient conditions for theory identity, we do know some sufficient conditions for *non-identity*— empirical inequivalence, for one. Second, if the alleged underdetermination can be shown to have no serious consequences,

[19]For instance, see [Car83, p. 159–61].

[20]Giere, for instance, argues for the "model-theoretic view" and allows models that are "prototypes or exemplars" [Gie94, p. 283, fn. 3].

[21]Formal work has also been done on the identity conditions of theories within state-space semantics [Chu98], but one is hard-pressed to see how the theories considered above could be reconstrued as patterns of activations in neural networks.

[22]As does [Sup68].

then nothing turns on whether the rivals are actually distinct. Even if the electron-proton inversion case involved distinct theories in some sense, it would be hard to get excited about the underdetermination between them.

It is possible, for all that has been said so far, that there will be some cases of underdetermination with considerable consequences. We might then be pushed to consider whether the apparent underdetermination obtains between distinct theories— pushed, that is, to consider the problem of identical rivals. In subsequent chapters, I aim to show that this possibility is not realized. For now, it's enough to note that the mere possibility should does not doom our enquiry into underdetermination.

I do not deny that a criterion of theory identity would be a nice thing to have. Problems of theory individuation, of which the problem of identical rivals is a special case, are interesting in their own right. Resolving them, however, can only come as the result of a careful examination of the history of science— an examination which must be left for some other time. I draw the modest conclusion that this open question need not give us much pause when we come to consider underdetermination.

1.3 Variations in scope

As explained above, we might organize variations in scope according to senses of possibility. These variations are the concern of the next section. In the subsequent section, I offer a different way that variation in scope may organized: Time. I then consider whether cases of very wide or very narrow scope are importantly different than other cases. Before moving on, there are two general remarks to be made about scope.

First, recall that scope is defined as the range of conditions across which no responsible decision can be made between rival theories and the range of querists for whom the decision is impossible. The range of querists may be constrained in terms of what querists need to know in order to make determination or in terms of what they would need to be able to do. In either case, the range of querists may be expressed as a range of conditions. Suppose, for example, that a theory choice is underdetermined for querists who lack some important background theory or who lack the know-how to build a complicated experimental apparatus. This might equivalently be expressed by saying that the theory choice is underdetermined across conditions in which the background theory is not antecedently known or in which the apparatus is unavailable. Scope for any case of underdetermination can thus always be given as a set of circumstances, without loss of generality.[23]

Second— for a fixed set of rival theories and a fixed standard— if the theory choice is underdetermined with respect to some scope, then it will also be underdetermined for any strictly narrower scope. Conversely, if it is not underdetermined for some scope, then it will not be underdetermined for any strictly wider scope.

This may seem counterintuitive, since one can easily imagine cases that might seem to contradict it. Consider two rival theories, call them Head and Heels. Suppose there are only two possible items of evidence that bear on the question, E_{Head} which supports Head over Heels to some degree and E_{Heels} which supports Heels over Head to the same degree. If a scientist knows both E_{Head} and E_{Heels}, her choice between Head and Heels is underdetermined. Yet if she knows only E_{Head}, her choice is determined in favor of Head. It looks as if the underdetermination is eliminated given a narrower scope. However, the case where she can decide is not one of narrower scope, but of less evidence. The scope for the case of underdetermination includes conditions in which she has either no evidence or both items of evidence. The circumstance of having only the one item of evidence and of her judgement coming up Head is not in the scope at all. Here, a circumstance can be expressed as a set of available evidence. The scope is then not a set of

[23] Background knowledge might instead be included in the standards for responsible choice; include among the standards that one may responsibly assume such-and-so theory.

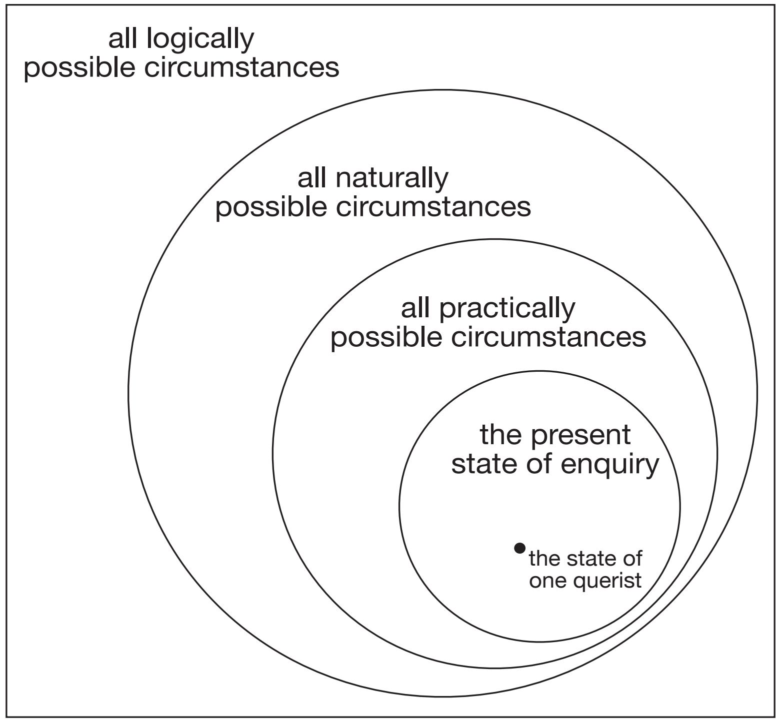

Figure 1.3: Scopes associated with various senses of possibility make for nested varieties of underdetermination.

evidence, but a *set of sets* of evidence.[24]

1.3.1 Variations in possibility

Many classes of underdetermination that have been of considerable concern to philosophers of science may be generated by specifying a sense of possibility. Considering cases which are underdetermined for a scope constrained by empirical possibility yields the kind of underdetermination that motivates Bas van Fraassen's arguments for constructive empiricism.[25] Varieties of scope generated in this way can be nested, as shown in figure 1.3.

Cases of maximal scope are those where rival theories would be favored

[24]Specifying a circumstance may involve more that just giving a set of evidence. As noted above, it may involve specifying capabilities of the scientific community. In general, it will involve specifying whatever features of a situation make a theory choice underdetermined for the agents who are in it.

[25]There is reason to be suspicious of the notion of 'empirical' or 'observational' possibility, but I save those for the discussion of empirical equivalence in Ch III.

to the same degree by any imaginable body of evidence. Cases that are underdetermined with a scope of all naturally possible circumstances may be harder to identify, since our identifying them requires that we have an accurate account of what is naturally possible. Such cases are interesting because they preclude saying that all theory choice is underdetermined; to have identified such a case, we must already have accepted a theory of sufficient power to specify what is naturally possible. Background theories can thus underwrite claims of underdetermination, but they also presume determination.

For any set of rival theories, there are background theories which— if scientists held them— would allow scientists to distinguish between the rivals. Consider the case where there are two rival theories T_1 and T_2 and scientists make some observation O. If they know $(T_1 \rightarrow O)$ and $(T_2 \rightarrow \neg O)$, then they can decide that T_1 and not T_2 is the better theory. Of course, suitable revision of the rival theories would repeat the underdetermination at the level of theory cum background theory. If the choice between T_1 and T_2 is underdetermined without the background theory, then the choice between $T_1 \& (T_1 \rightarrow O) \& (T_2 \rightarrow \neg O)$ and $T_2 \& (T_1 \rightarrow \neg O) \& (T_2 \rightarrow O)$ will be underdetermined regardless. If the underdetermination is averted on grounds that $(T_1 \rightarrow O)$ and $(T_1 \rightarrow \neg O)$ are themselves entailed by some background theory B, then repeat for the rivalry between $T_1 \& B$ and $T_2 \& B'$ for some doctored background theory B'— and so on until the relevant background theories are exhausted.

The implications of this argument demand careful attention, but one especially concerns us now.[26] A theory choice may be described in any number of ways. Described as a choice between T_1 and T_2, this choice is underdetermined for a scope which contains circumstances in which scientists have not learned the potent background theory. Described as a choice between T_1 plus the potent background and T_2, the choice may be underdetermined for a much wider scope. Therefore, the scope for which a theory choice is underdetermined depends on how the set

[26] I will have cause to revisit the argument in §3.3.2. References accompany that discussion.

of rivals is characterized (and also on the standard for acceptable determination.) So, whether a case of underdetermination is of maximal scope may depend on how it is described. Whether rivalry between theories is importantly underdetermined or momentarily undecided is a matter of degree. The distinction is best thought of as representing a continuum. At one end is inescapable underdetermination. At the other end is the normal situation of inquiry underway. In the middle are cases of varying tractability.

1.3.2 Variations in time

Scope might be constrained so as to include only certain times. It may be that some question about the future— for instance, what I will eat for lunch on my 80th birthday— is undecidable given present evidence. The choice between rivals— a sandwich, soup, nothing. . .— is underdetermined for a scope that includes only our present and past circumstances.[27] For a scope that includes sufficiently distant future circumstances, however, the choice is not underdetermined for the trivial reason that among the range of conditions contained in the scope are conditions under which querists could watch what comes to pass. On my 80th birthday, they should have little trouble resolving the issue.

Other variations with regard to time will have other consequences, but the general lesson drawn above still holds: removing times from the scope of a case of underdetermination will always leave a case of underdetermination; adding times to the scope of a case which is not underdetermined will never make the case underdetermined. Yet no general relationship exists between cases of underdetermination with scopes of specified possibility and cases with scopes specified by time. This can be shown by considering the possible permutations:

1. Cartesian evil demon scenarios are underdetermined for a scope that contains all logically possible circumstances and times, because no evidence could ever

[27]The example shouldn't be thought of as being about free will. The world is a complicated place, my 80th birthday is many years in the future, and lunches are not the concern of fundamental theories.

resolve them. (See Ch II.)

2. The choice between 'Emeralds are green' and 'Emeralds are grue$_{tomorrow}$' is underdetermined given a scope of all logically possible evidence from our past and present, because no evidence before the predicate's expiration date could resolve it.[28] Including future evidence, however, resolves the underdetermination.

3. Relativity, a natural constraint, entails underdetermination about the global structure of spacetime. The underdetermination holds for all time but not for all logically possible evidence. We can imagine evidence which would resolve the matter, but constrained by the laws of physics we could never in all of time gather such evidence. (See §4.1.4.)

4. The choice between theories of what I will eat for lunch on my 80th birthday is not underdetermined either for all times or for all logically possible evidence. Including sufficient parts of the future would allow for determination, as would the construction of logically possible but physically problematic chronoscopes.

This distinction mongering hasn't shown anything much about these examples. What it has shown is that variations of scope with respect to possibility and with respect to time are distinct.

So far, varying scope with respect to time has been treated as a matter of constraining which times observers would be allowed to watch from. Even if the choice between some rival theories might be underdetermined at *any* finite time, we may further wonder if determination would be possible given the totality of eventual evidence. For example, consider a group of geometers trying to determine the value of pi. Suppose they entertain all real numbers as rival theories. They quickly eliminate many of the rivals, narrowing the choice down to one between

[28] A thing is 'grue$_{tomorrow}$' if it has been green in the past and is green today, but will be blue tomorrow and henceforth.

numbers from 3.1415 to 3.1416. Given further enquiry, they can narrow this range further. Given any finite amount of enquiry whatsoever, there will still be an interval containing infinitely many rival theories. In the limit as the length of their enquiry approaches infinity, however, they decide on the one true theory. For a scope which contains all possible circumstances at all times, the choice is underdetermined. The underdetermination is only eliminated for a larger scope, one which contains a synoptic circumstance which sums over all of the times. This limit of enquiry is not possible for any actual querists, however, so these imagined mathematicians would be best advised to decide what degree of precision they need and adjust the set of rivals accordingly; for any finite degree of precision, the determination can be made in some finite time. The scientific community never reaches the end of science, so there is something odd about adding a circumstance which contains as evidence all other possible circumstances.

1.3.3 Maximal scope: agnosticism and fideism

Theory choices which are underdetermined with a maximal scope— ones for which responsible choice would be impossible in any logically possible circumstances at any time— demand special attention. They evoke two conflicting intuitions; call one *agnostic* and the other *fideist*.[29] The agnostic intuition is that, since the choice is forever beyond determination, one ought forever to suspend judgement about which of the rivals is true. The fideist intuition is that one might freely believe one of the rivals even now, since further enquiry will have no bearing on the question either way. The agnostic impulse is, in effect, to insist that the same standard for responsible choice that holds in cases of narrow scope underdetermination should hold even when it makes a theory choice undetermined with maximal scope; the only responsible thing to do in cases of maximal scope is to be indifferent between the rivals. Contrarily, the fideist suggests that where a theory choice is underdetermined with maximal scope given the usual standards

[29] In the discussion that follows, 'agnosticism' and 'fideism' are meant only to pick out views about how to respond to underdetermination— any religious overtones are incidental.

of responsible choice, one ought to suspend the usual standards in favor of the standard 'Do what you will.' On this alternate standard, the choice need not be underdetermined.[30]

Agnosticism and other minds

An example may help clarify these intuitions. Let the set of rivals contain one theory according to which only you the reader have a mind and another according to which you and most other human beings have minds. The rivalry between these two theories amounts to the familiar problem of other minds and is arguably underdetermined with maximal scope, since the two rivals would lead us to expect exactly the same course of events in the world. The underdetermination does not entail that the theory choice has no *practical* consequences, but rather that the consequences are not of the sort that can be used to test either theory. If you believe that you would have moral duties to people that you would not have to automatons, then there is a practical difference between the two. From one theory, it follows that you have a duty not to commit murder; from the other, it follows that murder is of a kind with smashing a television. Further experience will not contradict you in either case, but you may well act differently if you believe one rather than the other. How would you act if, taking the agnostic position, you remained indifferent between the two possibilities? How would you order your affairs if you lived with a real and persistent doubt as to whether other humans had mental lives like you have? It is tempting to think that you would become what the rest of us would call a sociopath. Sincere agnosticism about other minds, like solipsism, carries at least the hint of madness. Given your willingness to say that other people have experiences just as you do, doesn't it mean you are willing to relax your epistemic scruples on this matter?

If so, then the fideist wins the point. The agnostic may respond by arguing that the problem is not underdetermined. She may say: The madness of

[30]One may object that the will has no role in determining belief. I take this up in §1.6.2, below.

suspending judgement on this matter shows that the choice is not underdetermined even on ordinary standards, since any reasonable standard would allow you to choose the sane choice over the mad one.[31] Thus, the agnostic admits that there is an intuition regarding other minds but maintains that the intuition does not speak to the issue of underdetermination.

Cognitive habit

Moreover, the agnostic continues, fideism would do unintended harm. Good querists realize that reflective and deliberate application of standards is not enough. They try to develop habits of applying the standards of responsible theory choice; they cultivate these inclinations, so that they would feel uncomfortable accepting claims for which no good reasons can be adduced. They also place themselves in a social order that enforces the standards for responsible theory choice, they willingly put themselves in a situation where they would be chastised for capricious belief. The fideist only suspends the usual standard when a theory choice is underdetermined for maximal scope, to be sure, but he must thus work to weaken his and the community's habitual application of the usual standard. Suppose the decision between two theories is underdetermined for a maximal scope and that the fideist prefers to believe one rather than the other. If he ever has occasion to consider his choice, he will need to steel himself against his habituated unease; he will need to suppress the reaction which he cultivates in usual cases. Perhaps in reflective moments he will apply the usual standards as well as any querist. In more mundane moments, though, his habits will be weaker than those of the agnostics who spent no energy weakening theirs. There may well come a time when he will blunder where they might apply the usual standard by force of habit.

The fideist may reply to the agnostic's argument: It is an empirical question as to whether querists are best served by relying on brute habit. Perhaps

[31] Thomas Reid's arguments for the existence of the external world, given in the next chapter, may be applied here mutatis mutandis.

for the bulk of enquiry and for the bulk of querists, for normal scientists doing normal science, this will be the case. Those who are of stronger character can distinguish between cases, however. They will recognize the differences between cases underdetermined with maximal scope where they may believe as they will and cases underdetermined for narrower scope. Why should such stronger types be held back from exercising their strong character?

The agnostic replies in turn: It is doubtful that anyone has such a strong character, but that is— as you say— an empirical matter. Even if some scientists do have such a character, they should not exercise it. As it is said: "Be careful that the exercise of your freedom does not become a stumbling block to the weak."[32] Other scientists would either decide that they, too, could believe as they chose or be forced to admit that they had weak cognitive constitutions. If the former, then their habits would be weakened. If the latter, then they would be disheartened, and an unhealthy social division would be drawn among scientists. Thus if any scientists are such that their habits will be significantly eroded by fideism, then the whole community should embrace agnosticism.

The fideist replies, of course, but we can leave them to the dialogue. There are open questions of the degree to which habit formation is a good way to instill the usual standards of reasonable theory choice, the degree to which fideism would undercut these habits, and the degree to which fideism for a few would disrupt the community. These are questions which must be answered by methodological reflection and psychological data.

Fideism and narrower scope

The fideist intuition is strongest in cases, like the alleged underdetermination about other minds, where the scope is maximal. The fideist may further wish to suspend the usual standards of responsibility for choices which are underdetermined with a narrower scope— for instance, a scope of all naturally possible

[32] 1Corinthians, 8:9.

circumstances as defined by some well-confirmed background theory. This would exacerbate the problem of poor habits, since choices which are subject to the usual standards may closely resemble ones which are exempted from the standards.

Consider that Special Relativity entails that an observer cannot be affected by events outside her past light cone.[33] Supposing that she can only responsibly make theory choices on the basis of events to which she has some causal connection, specific claims about events outside her past light cone will be underdetermined for her with a scope constrained by what is possible given Special Relativity. Consider two events, call them Hop and Skip. Suppose that Hop was, from our observer's frame of reference, one thousand years in the past and six inches inside the surface of her past light cone and that Skip was, again from the observer's frame of reference, simultaneous with Hop but six inches outside the surface of her past light cone.

Following the fideism here on offer, she may believe whatever she likes about Skip but not so with Hop which is a mere foot away— even if Hop is some small event which would be practically impossible for her to discern. This is subject to the same worries about personal habit and social norms as fideism applied to cases of maximal scope, but perhaps she can will her belief about Skip while reminding herself that she has this freedom only because of Special Relativity, without being less apt to notice errors in reasoning about Hop, and without harming the social order. Insofar as she can do that, however, she is encouraged to treat Special Relativity as an inevitable and immutable constraint on possibility. If she seriously allowed for the possibility that it might be false, she would treat Skip as of a kind with Hop, both as events that she could not discern. Thus, fideist treatment of a particular claim about Skip would undercut her ability to be a fallibilist about the background theories that specify the relevant sense of natural possibility. Since she only needs to protect her willful judgement about Skip, she could allow for theory changes that would further constrain the relevant sense of

[33]The entailment presumes a little beyond just Relativity, but the complications are incidental to the argument here.

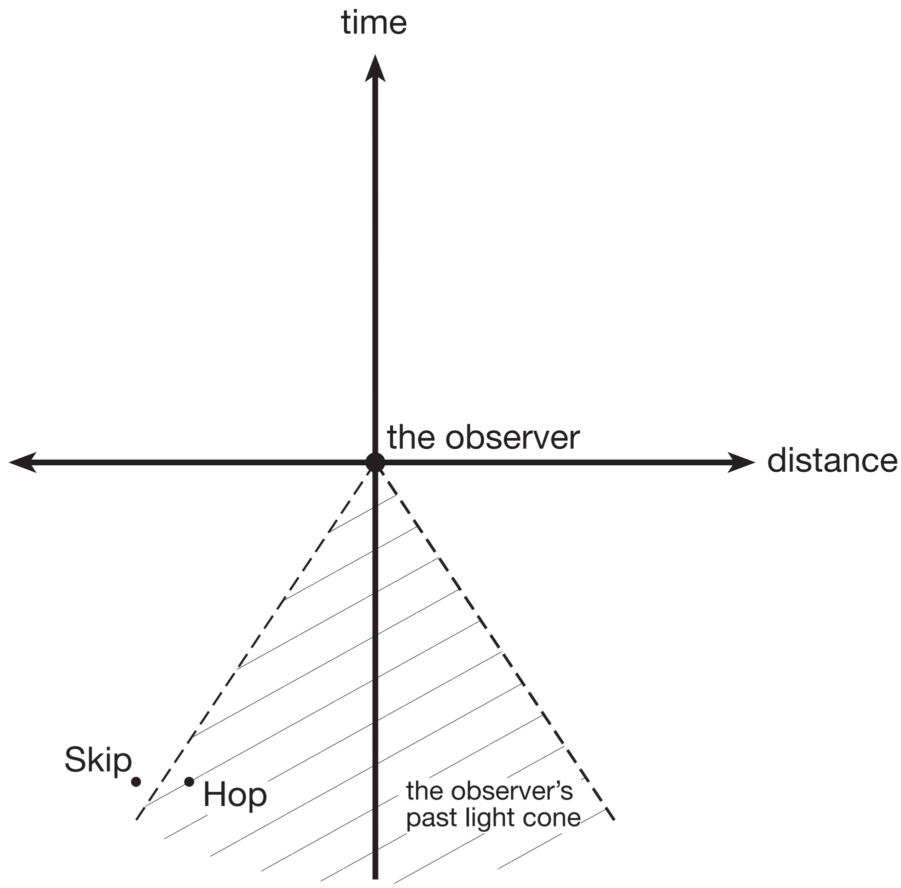

Figure 1.4: The observer may be effected now by Hop, but not by Skip, since the latter is outside the observer's past light cone

possibility. Since responsible change in theory may as easily open new possibilities as close them, however, this half-hearted fallibilism offers little comfort.

The fideist approach may be applied to cases of still narrower scope, but the problems only increase. Further problems arise and— for a theory choice underdetermined only with a scope so narrow that it excludes recognized, practical possibilities— agnosticism clearly wins out. Imagine, as an example of such a case, the question of whether there are serious side effects associated with an exotic drug. A large-scale, clinical trial would resolve the question, but it is underdetermined with a scope that includes only circumstances where the trial has not been performed.[34] If we have not performed the trial, we are not in a position to say whether the drug has side effects. Yet, it would be perverse to decide that it does have side effects if we merely find the drug distasteful. Here the fideist intuition has no sway. More generally, it would be perverse to will to believe regarding a matter that would be open to investigation if only we would will to investigate. The fideist suggestion was to *substitute* the standard 'Believe what you will' for the usual standard in some special cases, but invoking that standard in cases that might be resolved by ordinary investigation is— in effect— to insist that it should *be* the usual standard.

To summarize the action surrounding fideism: There is an intuition that in cases underdetermined with maximal scope it would be permissible to believe anything, since the course of events would never force one to alter the willed belief. Such a practice may have deleterious consequences for individuals and for the community, but whether it does is an open and at least partly empirical question. The only plausible fideism regards cases of very wide scope, so underdetermination with middling and narrower scope will require a different response regardless.

[34]The question could be decided without a clinical trial, for instance with a study of a population that took the drug recreationally. The scope of the underdetermination, then, must also exclude circumstances where such populations exist.

1.3.4 Very narrow scope

Where evidence is yet to be found but we have no reason to believe that we couldn't find it, underdetermination describes the normal state of science wherein some experiments are yet to be done and some data is yet to be collected. Our usual understanding of science in which there are open questions which may be answered by further study thus makes room for a certain kind of underdetermination: cases in which the scope includes our present circumstance but does not include some circumstance wherein we've done further study.

Legitimate controversies exhibit underdetermination of this kind. Consider the schematic example of two theories, a dominant theory Alt and a weaker rival Neu. In the beginning, scientists responsibly judge Alt to be superior to Neu. As new evidence is collected, however, Alt faces anomalies. Scientists begin to responsibly disagree as to whether Alt should be preserved or thrown over. More evidence is collected, new arguments are made, and Neu emerges victorious.[35]

The choice between Alt and Neu is underdetermined with a scope including circumstances in which the anomalies are known but in which the decisive evidence in favor of Neu is not known. Instances of this schema are commonplace in the history of science. In the Kuhnian idiom: One paradigm dominates, anomalies mount up, the paradigm enters crisis, and then the crisis is resolved by the shift to a new paradigm.[36] There are questions which seem settled now, which evidence can reopen, and which further evidence would settle again.

Narrow scope underdetermination of this kind is present wherever there is ignorance that might be overcome and wherever there is legitimate controversy. Yet who would ever deny that ignorance and controversy are real features of science? For fallibilists, this kind of underdetermination doesn't suggest any reëvaluation of the claims of science.

[35] The situation is somewhat more complicated if Neu is only formulated in response to the anomalies threatening Alt; cf. §4.2.2.

[36] Crises can also be resolved if new resources are developed within the prior paradigm. In the schematic case, Alt may emerge victorious if the anomalies can be resolved.

1.4 Variations in standards

A choice between theories is underdetermined for a particular community when its members are unable to responsibly decide which of the theories to accept.[37] In the simplest case, there is not enough of the right kind of evidence available to decide in favor of one theory or another. The theory choice is straightforwardly underdetermined by the standard of empirical adequacy. Imagine a more complicated case in which the community accepts as a standard that the simpler theory should be accepted, ceteris paribus. Let the two theories be Unum and Quorum. One member of the community chooses to believe Unum because it posits fewer entities than its rival Quorum. Another member of the community chooses to believe Quorum because, although it posits more entities than Unum, it describes the interactions of entities more elegantly than Unum does. The first querist applies the usual formulation of parsimony and insists that they should not posit entities needlessly. The second insists that the additional entities are not needless, since without them the dynamics of the theory would be needlessly complex. The standard of simplicity seems ambiguous between their interpretations of it, and so it seems plausible to say that the case is underdetermined given merely the standard of 'simplicity' even if it is not underdetermined given either the standard of parsimony or the standard of elegance.[38]

It might seem, then, as if there are two forms of underdetermination: the simpler form in which a well-articulated standard is insufficient to decide matters and another form of underdetermination which stems from the ambiguity of standards. Note, however, that the difference is one of degree and not one in kind, since intermediate cases of all sorts can be constructed. Imagine two theories Recto and Verso which both enjoy empirical success in some applications but which encounter anomalies in others; suppose that Recto succeeds most dramatically just where Verso fails, and vice versa. The criterion of empirical success is equivocal

[37] That is, when the choice is underdetermined for some scope that includes their present circumstances.
[38] This problem with simplicity arises for a concrete case in §4.1.3.

without being ambiguous. Everyone may agree that it speaks in favor of both theories, but insofar as the theories are incompatible they must admit that it is insufficient to decide which of the theories we should accept. The standards may be made more specific, of course. A standard which places greater importance on the applications which Recto successful executes will favor Recto univocally.[39]

In the abstract, it will always be possible to specify more discerning standards which would decide univocally between rivals. Let the rival theories be Chaulk and Cheese. The choice between them may be underdetermined for our preferred standards, but be that as it may it will certainly be underdetermined given only the standard encapsulated in the rule 'Believe either Chaulk or Cheese.' Contrarily, it will assuredly not be underdetermined for the standard 'Believe Chaulk, but shun Cheese.' Note, too, that the former standard leaves the choice underdetermined with maximal scope while the latter allows determination in any circumstances whatsoever.

This means that *any* theory choice is, in a sense, underdetermined in every circumstance, but also that the same theory choice is not, in another sense, underdetermined even given our present circumstance. This is no paradox, since the underdetermination present or absent is underdetermination relative to different standards. Yet one might still hope to discover which sense of 'underdetermined' is the *real* sense. The real sense, one might think, is the one that takes rationality as the relevant standard. This approach is fundamentally misguided. One sense of 'underdetermined' may be more important for us because it arises from standards which are important to us, but which standards are important to us may itself be a matter of debate. There is no absolute sense of rationality to be had.

There may thus be cases in which querists can not agree on a theory choice because they are applying different standards. This may be like the case of Unum and Quorum which was underdetermined because the two querists appealed to different conceptions of simplicity. It could be worse than that, perhaps, since the

[39] This theme returns in §4.3.

adherents of Unum and Quorum at least shared a commitment to an ambiguous common standard. We may imagine cases in which querists share no relevant standards at all. Although such confrontations are imaginable in some sense, actual querists always share an array of commitments and implicit standards. I leave this for now as a bold assertion. Using modern scepticism about the external world as a foil, I return to this claim in the next chapter to explore the role of standards in determining a case of underdetermination.

1.5 Top-down and Bottom-up arguments

The structure developed in this chapter organizes cases of underdetermination. We can already see the disparate phenomena that pass as *underdetermination* as varieties of a well-defined genus. It remains to be shown how underdetermination is supposed to yield a conclusion about science. The schema for *underdetermination* is not yet a schema for *underdetermination arguments*. A further distinction will be helpful.

Top-down arguments begin with ubiquitous underdetermination— either as a premise or an intermediate conclusion. This ubiquitous underdetermination is then argued to have some importance for our thinking about science. Paradigmatic underdetermination worries like the problem of empirically equivalent rivals and the problem of reliance on auxilliary hypotheses (the so-called Duhem-Quine problem) are top-down arguments. They and their kin will be the addressed in Ch III.

Bottom-up arguments begin from concrete cases of underdetermination— perhaps one, perhaps a few, perhaps a broad sample of cases. Since some cases are underdetermined, is there any reason why others should not be? Philosophers have often fixated on the underdetermination of the metrical or topological structure of space, with the suggestion that the lesson of geometry tells us something important about all of science. This and other bottom-up moves will the subject of Ch IV.

1.6 Three asides

1.6.1 Aside: underdetermination and ontology

The scheme developed here doesn't make an explicit place for disputes about scientific realism, *maugre* the many authors who take underdetermination to be an anti-realist weapon against realism.[40] The issue may pitch a tent in many camps, I suggest, but it is not at root an ontological problem. The worry of underdetermination is that we may be unable to responsibly decide between rival theories. The claim of realism is (to a first approximation) that theories we have responsibly decided on are true or approximately true. Thus the latter seems to presuppose the former; realism supposes we have decided on a theory, and thus that underdetermination worries can be answered. Even given a victorious theory, though, there is the subsequent question of whether its ontology is the true ontology. Here there are problems of determining what ontology accompanies the theory, and these might without too much injury to the language be called an underdetermination problem. Nevertheless, this *underdetermination of ontology by theory* is not the problem that I am concerned with addressing here.[41] Anti-realists may argue from the *underdetermination of theory by evidence* to the conclusion that we might responsibly have decided on different theories; they may argue from the *underdetermination of ontology by theory* to the conclusion that our best theory can be understood as saying different things about the world; but it would suffice for them to argue that even a univocal ontology is not one we should believe. So overcoming underdetermination is not sufficient for realism. It is not necessary either, if a sly account of approximate truth might save realism in the face of rampant underdetermination.[42] Determination is neither the *sine qua non* or the

[40] Though not all authors collapse these issues. Laudan, for instance, rejects claims of both underdetermination and realism.

[41] For the general claim that there is no function from claims about the world to ontologies, see [Qui69]; for an argument that this the situation in contemporary physics, see [Jon91]. The idea that ontological relativity might count as a 'underdetermination' was suggested to me by Jeffrey Barrett.

[42] The sly account need not be an irresponsible one. I have no pretheoretic notion of 'approximate truth'— it is a term of art and may bear exotic definition. There is a pretheoretic intuition that science gets something right about the world, and that might spur one toward realism.

cum qua of realism. There are connections, no doubt, but for the realist *everything* about science connects to realism. I may remark on the connection at times, but I will offer no further apology for denying it pride of place.

1.6.2 Aside: the role of decision and choice

The initial characterization of underdetermination makes reference to an agent's decision to accept one of the rival theories, by which I mean the same thing as the agent's making a theory choice. Furthermore, underdetermination is taken to be instanced in homely examples of belief choice as much as in rarified examples of scientific theory choice. One might object to this language of *choice* on the following grounds: Belief is not a matter of choice. A person believes whatever she believes, and it makes no sense to think of her as making an explicit choice in the matter. Argument may lead her to some conclusion; if she believes it, well and good; if she does not believe it, then there is no sense in saying she should choose to do so.[43]

There is a sense of 'belief' in which belief is not amenable to choice, and the argument above is spot on regarding belief in that sense. However, there is a perfectly ordinary sense of belief in which it misses the mark. A belief framed as a proposition is something that a querist may choose to accept or reject. She has a certain intuition, a hunch, an inclination, but nothing about those dispositions requires her to accept a particular sentence as cashing out that intuition. Further, to believe a claim in the sense of accepting it is more than just to say it or have a disposition to say it— to accept a claim is to take responsibility for it, to stand by it, to stake one's reputation on it. This is a commitment to action, in part, and is as much a choice as any decision to act. Throughout what follows, talk of theory choice and belief choice should be understood in this sense.

[43] Consonant with this objection, one might say that it is impossible to will to believe anything. Believing is a matter of conviction, which ipso facto means being convinced. Perhaps, then, the objector must presume against fideism.

1.6.3 Aside: the units of epistemology

Even if one allows that theory choice is something that actually occurs, it may be taken in any of several senses. Return to our imagined rivals Chaulk and Cheese. A scientist might take Cheese as a guide to further research, even staking the prospects of her career on how well it fares. Although this is consistent with her *believing* Cheese in a strong sense, her commitment might instead reflect the fact that she only has time enough to pursue one of the two rivals. She realizes that if she were to split her time then her chance of finding dramatic evidence relevant to *either* theory would be greatly reduced. If she were making funding decisions, perhaps she would divide resources evenly between Chaulk researchers and Cheese researchers. Her funding decision would depend on whether there were enough resources to effectively fund both research programmes; the choice for the community mirrors her own choice in this respect. If *theory choice* is taken to include practical choices made to maximize the return on limited resources, like our scientist's choice of Cheese, then responsible theory choice is compatible with the rivals' being equally plausible.

We should not take this too far. Scientists agreeing on a powerful background theory is not merely an austerity measure adopted in lean times. As Kuhn tried to show, it is the condition of normal science [Kuh70]. Science progresses best when there is agreement about a great many things. These grounds of agreement are, in Kuhn's account, paradigms. Although *paradigm* is notoriously a crypto-omnivorous category that is at once over-blown and obscure, there is something right in the Kuhnian insight. Exclusive selections of a theory, paradigm, or research programme over its rivals are a central feature of science. They may not, in general, be dismissed as economic conveniences.

Yet we ought not press this Kuhnian point too far, either. As Mill reminds us, for any strong opinion, "however true it may be, if it is not fully, frequently, and fearlessly discussed, it will be held as a dead dogma, not a living truth" [Mil74, pp. 96–97]. Considering scientific knowledge specifically, Mill writes:

> [O]n every subject on which difference of opinion is possible, the truth depends on a balance to be struck between two sets of conflicting reasons. Even in natural philosophy, there is always some other explanation possible of the same facts; some geocentric theory instead of heliocentric, some phlogiston instead of oxygen; and it has to be shown why that other theory cannot be the true one; and until this is shown, and until we know how it is shown, we do not understand the grounds of our opinion. [Mil74, p. 98][44]

Note that although Mill thinks that we should discuss our opinions, he thinks that we may nevertheless form opinions. He does not advocate accepting things only provisionally and for some projects. It is possible that scientists and perhaps even the scientific community should pursue certain theories single-mindedly, allowing that such choices are to be openly debated. There is, no doubt, a balance to be struck between Kuhnian and Millian concerns— but I will not attempt to strike it here.

Fully resolving this issue would require a developed enquiry into the *units of epistemology*, a catalog of ways in which scientists and the scientific community can relate to theory. It would detail the possible variations of believing a theory in relation to accepting it, assuming it, entertaining it, and so on. Providing such a thing would be a serious undertaking and would lead us far afield of underdetermination. This need for a descriptive epistemology lurks in the shadows behind what follows. Where I do not mention it, I have endeavoured to assure that the arguments do not turn on ambiguities. In some places, reflection on underdetermination will yield interesting constraints on the units of epistemology. I leave the matter until we arrive at those places.

[44]It's possible to read Mill in this passage as invoking worries about underdetermination. I will revisit Mill's account of science in §4.3.2.

II

Underdetermination and Aspirations of Certainty

> "I... remember having scarlet fever... and that was the first time I really hallucinated. I was in the bedroom, and I could hear my parents in the kitchen and the refrigerator was blowing up and killing them all. It's remained with me, as if I'm still in that room. I still have certain dreams that cling, which I'd swear are real because my senses and my whole body seem to have experienced them. That's always been the problem, not knowing what's real and what isn't."
>
> —Terry Gilliam [Chr99, p. 1]

In this chapter, I explore the connection between underdetermination and scepticism. This exploration has three related, but potentially conflicting aims. First, I provide a reading of Descartes' sceptical arguments in the *Meditations*. The sceptical scenarios, I suggest, are rivals that underwrite claims of underdetermination. Second, I consider Thomas Reid's reaction to Cartesian scepticism. In this regard, it is critical to understand how subsequent thinkers like Reid have understood Descartes' arguments. Third, I use Descartes and Reid to illustrate the contrast between two types of underdetermination. From one starting point, confronting underdetermination is a winner-take-all, do-or-die struggle for knowledge; from the other, we can begin to distinguish varieties of underdetermination and to recognize that different sorts of underdetermination call for different replies. There is a danger that the historical characters will become idealized when they are considered as abstract exemplars. Nevertheless, I hope to provide a plausible interpretation of Descartes and Reid— as far as I can— and I will be explicit where I self-consciously side-step historical subtleties.

In the *Meditations*,[1] Descartes appeals to underdetermination to motivate methodological scepticism.[2] If we begin in this way, the consequences for epistemology are dire. In the next two sections, I will survey the Cartesian arguments and their consequences. In the three sections following, I will employ Thomas Reid's reply to scepticism as a reply to Cartesian epistemology. I will then argue that the fate of Cartesian epistemology motivates a different sort of enquiry, one typified by Reid.

2.1 Cartesian underdetermination

Descartes has the objective of escaping mere opinion and building his beliefs on the foundation of certain knowledge. He hopes to find some way to responsibly separate mere belief from certain knowledge, but he quickly admits that there is no hope trying to scrutinize his beliefs one at a time. Rather, he hopes to problematize them collectively. Since most of his beliefs come from sensory experience, he attempts to discredit sensory evidence *tout court*.

Descartes begins in this way:

> Yet although the senses occasionally deceive us with respect to objects which are very small or in the distance, there are many other beliefs about which doubt is quite impossible, even though they are derived from the senses— for example, that I am here, sitting by the fire, wearing a winter dressing-gown, holding this piece of paper in my hands, and so on. Again, how could it be denied that these hands or this whole body are mine? [MFP, AT VII 18]

[1] Passages are from the translation in Cottingham, et al. [Des85] (cited as MFP) except where the translation is my own (cited as MFP†). Page references follow the Adam-Tannery edition.

[2] It is almost commonplace to say that Descartes' arguments amount to claims of underdetermination, but all but one author that I know of say this only in passing.A singular exception is Stanford [Sta01].

We may be deceived by the senses under particular circumstances, but there are some things which would be perceived reliably if anything could be. That is, some judgements made on the basis of sensory input seem so secure that to discredit them would be to discredit sensory input as a source of knowledge. Descartes suggests that the belief that he is sitting by the fire in his dressing gown writing the *Meditations* is one such belief. Indeed, if his senses could deceive him about this, then it seems they could deceive him about anything. Let us call this theory T_1:

(T_1) Descartes is sitting by the fire in his dressing gown writing some philosophical remarks.

Descartes immediately asks what reason he could have for denying this claim. He frames the question without further discussion, but it is a critical turn in the argument. He does not attempt to summon up doubt by a willful refusal to believe, but rather by adducing reasons not to believe. These reasons take a specific form, the form of explanations of his experience which do not suppose the room, the fireplace, or any of the objects referred to in T_1. His strategy is to propose rival theories[3]; he will subsequently argue that the choice between T_1 and its rivals is underdetermined and thus that his assertion of T_1 is *prima facie* unjustified.

Descartes first notes that lunatics assent to crazy things as readily as he assents to T_1. Perhaps, he suggests, he is one of

> the insane, whose brains have been so shaken by a persistent vapour of black bile that they firmly assert that they are kings when they are paupers, that they are dressed in purple clothes when they are naked, that their heads are made of clay, that they are gourds, or that they are made of glass. [MFP†, AT VII 19]

We may call this T_2:

(T_2) Descartes' brain is impaired in some way such that he assents to T_1.

[3]In a restrictive sense of 'theory' which allows only the sort that practicing scientists would readily entertain, these are not theories at all. I have argued for a less-restrictive sense in §1.2.1 above, and 'theory' is used here to highlight the epistemic analogy with scientific cases. If it is too objectionable, one might read 'account' throughout.

He quickly dismisses T_2, saying of the insane that “these men are mad, and I would seem no less mad if I were to take them as my exemplar” [MFP†, AT VII 19]. In saying this, Descartes rejects T_2 as a rival to T_1 on the grounds that he would be mad to accept it. Yet, the very proposal of T_2 is that Descartes is mad. There are, then, two possibilities. If Descartes has sufficient ground for rejecting T_2, then he has acknowledged a general imperative: Do not believe madness. This imperative might be used to defuse scepticism, and indeed it is so used by Thomas Reid. I will return to this point when I take up Reid’s response to the sceptic (§2.3). Alternatively, if Descartes lacks sufficient ground for rejecting T_2, then this scenario alone is sufficient to demonstrate that his belief in T_1 is underdetermined. Regardless, Descartes immediately offers another hypothesis, one which has received more attention.[4]

Say of lunatics what you will, Descartes suggests, “As if I were not a man who sleeps at night and regularly has all the same experiences while asleep as madmen do when awake— indeed sometimes even more improbable ones” [MFP, AT VII 19]. This we may call T_3:

(T_3) Descartes is asleep in bed and dreaming that T_1.

Descartes can find no immediate evidence that leads him directly to prefer T_1 over T_3; he adds, “I see plainly that there are never any sure signs by means of which being awake can be distinguished from being asleep” [MFP, AT VII 19]. From this underdetermination, Descartes concludes “that physics, astronomy, medicine, and all the other disciplines which depend on the study of composite things, are doubtful…” [MFP, AT VII 20]. On the basis of the dream argument, Descartes offers an answer to the question which ultimately interests us here: What is the status of scientific claims in light of concerns about underdetermination? Dubious, Descartes says.

[4] Although commentators such as Popkin have not recognized the argument from madness as a separate sceptical scenario [Pop60, pp. 199-200], this point is noted by contemporaries of Descartes (such as Bourdin, in the Seventh Objections [MFP, AT VII 457]) and recent scholars (such as Janet Broughton [Bro02, pp. 21–2]).

Figure 2.1: Was Descartes imagining that he was dreaming or dreaming that he was awake? How could he tell?

Pressing the point further, Descartes suggests that everything he seems to perceive might be presented to him by God or some evil demon. Call this T_4:[5]

(T_4) Descartes is deceived by some malevolent agent into believing that T_1.

This sort of scenario, Descartes thinks, is sufficient to cast doubt on empirical science and even mathematics. Although the *evil demon* has often been understood as a straight-forward sceptical argument, it is important to note that Descartes posits “some malicious demon of the utmost power and cunning” [MFP, AT VII 22–3] only *after* concluding that all his beliefs are open to doubt [AT VII 21–2]. Descartes' original scenario comes as a dilemma: If there were an omnipotent creator God, then it would be within God's power to deceive him. If there were not, then he is the product of some imperfect thing and so must be that much further from perfect knowledge. It is possible, then, to distinguish two sceptical scenarios: that Descartes is deceived by some powerful agency (T_4) and that Descartes is the product of imperfect processes that have left him erroneously believing things like T_1 (what Broughton calls “the ‘fate or chance’ argument” [Bro02, p. 22]). After considering this dilemma, Descartes introduces the evil demon as a heuristic to keep himself from sliding back into ungrounded belief. He does think the heuristic is sufficient for that task, and so the evil demon was later seen as a cornerstone of Cartesian scepticism.

On the basis of the sceptical scenarios, Descartes concludes,

> I. . . am finally compelled to admit that there is not one of my former beliefs about which a doubt may not properly be raised; and this is not a flippant or ill-considered conclusion, but is based on powerful and well thought-out reasons. So in future I must withhold my assent from these former beliefs just as carefully as I would from obvious falsehoods, if I want to discover any certainty. [MFP, AT VII 21–2]

This is a strong conclusion and turns on the implicit methodological premise that we should suspend belief in the face of any underdetermination.

[5] I have expressed T_4 in such a way as to include 20th-century brain-in-a-vat and Neo-in-the-Matrix scenarios along with deceiving Gods and evil demons.

Before turning to the consequences of methodological scepticism, we should specify the scope of Cartesian underdetermination. We who are not Descartes may easily decide between T_1 and T_3, for instance. We need only look at him and see if he is awake or asleep. The "I" of the *Meditations* is not strictly Descartes, of course, and the reader is invited to imagine similar rival theories. Consider the theory U_1:

(U_1) You are now reading a dissertation on the subject of underdetermination.

Now let U_2, U_3, and U_4 be defined just as T_2, T_3, and T_4 above, substituting yourself for Descartes and U_1 for T_1. You are as impotent to decide between U's as Descartes was to decide between T's. The scope of underdetermination for particular rival theories of this type is thus in some sense rather narrow. It holds only for a particular querist enjoying particular experiences. Nevertheless, given an agent, the arguments give us procedures for constructing rival theories such that underdetermination confronts that agent in the circumstances the agent actually faces.

2.2 From scepticism to quietism

Famously, Descartes scepticism is methodological— it's meant as an instrument for discerning what he should believe, rather than as real and abiding doubt. And so he claims to escape scepticism by the end of the *Meditations*. Each of T_1–T_4 entail Descartes' existence, thus although he cannot know which is true he can know that he exists. From this knowledge, by a train of reasoning, he claims to prove the existence of a good and perfect God. Since this God is no deceiver, Descartes explains,

> Despite the high degree of doubt and uncertainty involved here, the very fact that God is not a deceiver, and the consequent impossibility of there being any falsity in my opinions which cannot be corrected by some other faculty supplied by God, offers me a sure hope that I can attain the truth even in these matters. [MFP, AT VII 80]

To see how this defuses underdetermination, suppose Descartes assents to T_1. If T_1 is true, the sceptical worries were unfounded. Conversely, if T_1 is false then God must have provided Descartes with some way of discovering its falsehood. Since Descartes could determine T_1 to be false, its rivalry with T_3 (say) becomes the less potent form of underdetermination which can be resolved by mere persistence. The scope narrows to the point that he will be able to escape it. Obviously T_1 must be either true or false, and in either case the underdetermination is defused.

Descartes does not stop at defusing underdetermination; he further uses it to separate claims according to how reliably we may know them. Descartes hopes to have shown that one's knowledge that oneself exists and that there is a perfect God are not vulnerable to underdetermination in the same way as one's knowledge of other things and, further, that one's knowledge of the external world is underdetermined relative to sceptical scenarios (like the dream hypothesis) which are insufficient to undercut one's knowledge of mathematics. This allows Descartes to construct a hierarchy of claims. Cogito and Deus are known with immediate certainty, mathematics with less immediacy, and empirical science with still less certainty. In this light, we can understand Descartes' remark in the Synopsis that the arguments of the *Meditations* are not valuable because of what they prove—

> ...namely that there really is a world, and that human beings have bodies and so on— since no sane person has ever seriously doubted these things. The point is that in considering these arguments we come to realize that they are not as solid or as transparent as the arguments which lead us to knowledge of our own minds and of God, so that the latter are the most certain and evident of all possible objects of knowledge.... [MFP, AT VII 15–6]

It may seem from this remark as if Descartes is not about to take scepticism seriously, as if the underdetermination and radical doubt only serve as a heuristic for revealing the hierarchy of knowledge. Even if this is the better reading of Descartes, he has often been read as taking scepticism seriously. My concern here is with that perhaps imagined Descartes who has influenced so much subsequent philosophy.

Yet even in this passage where Descartes admits that the argument against underdetermination is not "as solid or as transparent" as arguments for claims higher in the resulting hierarchy, he does not suggest that the arguments are insufficient for the task. That is, he still supposes that the arguments do prove that the world exists, even if he feels no obligation to prove that conclusion. In the First Meditation, Descartes does not stop when he notes that no person doubts the existence of the world, but instead offers sceptical scenarios to motivate doubt. If these doubts are hard to take seriously, he says, it is because beliefs about the external world are like pleasant dreams from which we are loathe to be woken [MFP, AT VII 23]. Thus, the rhetoric in the body of the *Meditations* suggests that Descartes took scepticism seriously and believed he had an answer to it. Descartes appeals to putative underdetermination to argue for a hierarchy of things we can know— that much is clear— but I do not insist on the reading of Descartes on which underdetermination motivates serious scepticism against the reading on which scepticism is invoked as a foil but never treated as a serious threat. Resolving that question would require historical scholarship beyond my ken and would lead us away from the thread I hope to tease out.

Questions of what Descartes actually thought to one side, he has often been read as an earnest sceptic. Reading him in this way and balking at the epistemological centrality of a benevolent God, subsequent writers have charged Descartes with providing no way out of First Meditation underdetermination. If we refuse to believe anything that cannot be proven beyond all doubt, then we will in the end believe nothing. It is commonplace to insist that methodological scepticism taken seriously matures into real scepticism. Descartes' contemporaries, *nouveaux Pyrrhoniens* such as Mersenne and Gassendi,

> showed over and over again that the standard sceptical difficulties could be raised against the constructive achievements of Descartes, and, using the Cartesian method of doubt, everything that appeared after the *cogito* could be challenged. Descartes had either taken scepticism too seriously, or not seriously enough. He had either inadvertently joined their number, or he had not established his philosophy on a foundation

> so solid that it could not be shaken by some of the standard gambits from the arsenal of Sextus Empiricus. [Pop60, p. 213]

The sentiment voiced by Mersenne and Gassendi is echoed by contemporary writers as diverse as Philip Kitcher and Paul Ricoeur.[6] Kitcher writes: "Sceptics who insist that we begin from *no* assumptions are inviting us to play a mug's game. Descartes's lack of success in generating an account of nature that would survive all possible doubt was in no way the result of deficiencies of intellect or imagination" [Kit93, p. 135, italics in original]. Ricoeur puts the point this way: "…nothing resists the most fantastic hypothesis, at least as long as one remains within the problematic defined by the search for a certainty that would be an absolute guarantee against all doubt" [Ric92, p. 16]. On this common reading of Descartes, eventual scepticism is built into the Cartesian starting point.

Descartes' admirable aim is to establish something "in the sciences that would hereafter be firm and lasting" [MFP†, AT VII 17]. He has before given up beliefs that he had held firmly, so his confidence in a belief is insufficient to assure that it will last. The very possibility of giving up a belief threatens its persistence. Thus, he sets out to withhold assent from beliefs wherever possible, "from opinions which are not certain and indubitable just as carefully as I do from those which are patently false" [MFP, AT VII 18]. There is something odd about this starting point, something which can perhaps be made clearer by considering a similar project. Suppose I were to reason in this way:

> I have often fallen and scuffed myself in the past. Many of the places that I supposed were firm ground subsequently slipped or fell out from under me. From this moment on I refuse to walk any further. I will examine the matter and not move again until I decide on some step which I can make without any hesitation whatsoever. Where I can imagine a misstep, I will not step.

Unless I have reason to believe I am in a minefield, this methodological paralysis seems wholly unmotivated. My ordinary ways of stepping work well enough, oc-

[6] A great many philosophers between the time of Mersenne and the time of Kitcher have also interpreted Descartes in this way. I discuss Thomas Reid at length below and leave the reader to enumerate further examples.

casions when I have fallen notwithstanding. My ordinary ways of believing work well enough, too, so scepticism seems no more practical than paralysis.

This analogy is too quick. First, it makes too much of moderate success. My ordinary ways of believing may do well enough, but what is done well enough for daily life may not stand to stricter tests. Enquiry in general and science in particular are predicated on the assumption that ordinary ways of believing can be improved. It is important to note, however, that (unlike the Cartesian method) typical enquiry begins against a background of ordinary ways of believing and revises rather than replaces ordinary belief.

Second, the parody of reasoning is unfair to Descartes. He insists, "I cannot possibly go too far in my distrustful attitude. This is because the task now in hand does not involve action but merely the acquisition of knowledge" [MFP, AT VII 22]. Practical belief is not at issue, only theoretical belief. In the *Discourse on Method*[7], Descartes provides "moral rules" which a querist should observe after he has undertaken but before he has emerged from the method of doubt. Descartes' first maxim:

> . . .to obey the laws and customs of my country, holding constantly to the religion in which by God's grace I had been instructed from my childhood, and governing myself in all other matters according to the most moderate and least extreme opinions— the opinions commonly accepted in practice by the most sensible of those with whom I should have to live. For I had begun at this time to count my own opinions as worthless, because I wished to submit them all to examination, and so was sure I could do no better than follow those of the most sensible men. [DoM, p. 23]

Descartes will continue as an ordinary Frenchman would, even as he attempts to doubt everything an ordinary Frenchman believes. Thus, methodological scepticism can never ripen into real scepticism. If Descartes cannot escape methodological doubt, he will go on in the French fashion indefinitely.

[7] Henceforth, DoM. Numbering follows the pagination of the French original. Passages are from the translation in Cottingham, et al. [Des85]

One might worry, since the moral rule is meant to govern matters of conduct rather than matters of belief, that it is really beside the point.[8] The rule, one might think, reflects abiding doubts about moral knowledge rather than the doubts about theoretical knowledge that Descartes thinks he lays to rest in MFP. Yet this suggestion is at odds with Descartes' own declaration that the moral rules are a "provisional code" to guide him in the interim between adopting the method of doubt and emerging from it [DoM, p. 22]. If it was for moral matters that his method could not settle, then the code would not be *provisional*. Yet one may still note the rule governs action and not belief. This is true, but Descartes sees a connection between action and belief: Our actions reveal our beliefs more reliably than our own second-order beliefs. I'll return to this point in §2.4.2.

If we set out as Cartesians and conclude with Mersenne, Gassendi, Kitcher, Ricoeur, and who-all else that God cannot save us from scepticism, we do not wind up as sceptics. Instead, if we follow Descartes' advice, we wind up following the conventions into which we were raised. Yet we were following these conventions before we ever heard of Descartes! Methodological doubt, if undischarged, ripens into dogmatic quietism— and what use is an epistemology that leads only to quietism?

Since Descartes thought he could escape scepticism, he did not dwell on the question of where he would remain if he failed to do so. Nevertheless, his method prescribes provisional scepticism *within the context of the project* and provisional credulity *outside of it*. If a querist emerges from doubt, she replaces provisionally-believed popular opinion with a panoply of absolute truths. If she does not, then she lives as her fellows live and has no resources to critique the circumstances in which she finds herself.[9] Ordinary enquiry may lead to piecemeal revision of prior beliefs, but Cartesian enquiry is an all-or-nothing affair.

[8] I am indebted to Don Rutherford and Sam Rickless for making me see the weight of this worry.

[9] At least, no resources from her unfinished Cartesian enquiries.

2.3 Thomas Reid's appeal to Common Sense

If we are to steer between scepticism and quietism, we must escape the problematic of the First Meditation. This section begins by examining Thomas Reid's response to Descartes and the Cartesian system.[10] Reid identifies his target explicitly as "the Cartesian system" [Inq, ch. 7, p. 204]. A brief sketch of Reid's opening salvo: Reasoning in the Cartesian way allows us to formulate sceptical arguments that discredit sense perception, but reason is one of our faculties just as perception is. Each is a way in which we naturally form beliefs. Why should we trust the faculty of reason if we refuse to trust the faculty of perception? Reason cannot prove the reliability of our senses, but neither can we observe the reliability of reason by means of the senses. If we are to trust either, we ought to trust both.

By reason, we believe the consequent of a conditional given the conditional and its antecedent. By Common Sense, we believe that there are men in the street when we see them emerge from a coach. Each warrants beliefs in a certain way, and neither can do the work of the other. Reid uses the phrase 'Common Sense' to mean these faculties besides reason— our senses, our memory, and so on.

Reid insists that the belief in an external world is something he is led to as "the immediate effect of his constitution" [Inq, ch. 6 §20, p. 183]. He explains:

> The sceptic asks me, Why do you believe the existence of the external object which you perceive? This belief, sir, is none of my manufacture; it came from the mint of Nature; it bears her image and superscription; and, if it is not right, the fault is not mine: I even took it upon trust, and without suspicion. [Inq, ch. 6 §20, p. 183]

He trusts in his faculties, trusts that properly applied they will lead to the truth. His appeal to "the Almighty" [Inq, ch. 5 §7, p. 127; also ch. 6 §20, 184] may seem like Descartes' conclusion that we can trust our faculties because they are endowed upon us by a benevolent God the creator. Recall that for Descartes this trust is the conclusion of an argument meant to escape scepticism. Indeed, both

[10] Similar responses appear in Reid's *Inquiry into the Human Mind* of 1764 (henceforth Inq) and his *Essays on the Intellectual Powers* of 1785 (henceforth EIP). Page references follow Reid's *Philosophical Works* [Rei67].

Reid and Descartes hold that perception is a source of epistemic authority separate from reason. However, there are several important differences between them. For Reid, trust in our faculties comes at the beginning of enquiry. If we do not begin by placing some trust in our senses, he thinks, we will be impotent against the sceptic. Descartes supposes that conflicts between perceptions should be adjudicated by reason, but not the reverse. Reid insists that reason and perception are both to be trusted and should serve as correctives for one another.

2.3.1 Madness

Clearly with Descartes in mind, Reid says of the sceptic, "though in other respects he may be a very good man, as a man may be who believes he is made of glass; yet, surely he hath a soft place in his understanding, and hath been hurt by much thinking" [Inq, ch. 5 §7, p. 127]. He says elsewhere that while the sceptic is in ways like a madman, in other ways he does not differ from anyone else:

> A remarkable deviation from them [the principles of common sense], arising from a disorder in the constitution, is what we call *lunacy*; as when a man believes that he is made of glass. When a man suffers himself to be reasoned out of the principles of common sense, by metaphysical arguments, we may call this *metaphysical lunacy*; which differs from other species of the distemper in this, that it is not continued, but intermittent: it is apt to seize the patient in solitary and speculative moments; but, when he enters into society, Common Sense recovers her authority.[11] [Inq, ch. 7.4, p. 209]

We may call this argument by Reid the *argument from madness*. Schematically, it proceeds in this way:

1. Believing P would be mad.

2. Therefore, (one should believe) not-P.

[11] Reid makes this point in the context of considering Cartesian doubt explicitly: "Can any man prove that his consciousness may not deceive him? No man can; nor can we give a better reason for trusting it, than that every man, while his mind is sound, is determined, by the constitution of his nature, to give implicit belief to it, and to laugh at or pity the man who doubts its testimony" [Inq, ch. 1 §3, p. 100].

Figure 2.2: Reid argues that a serious sceptic would be as mad as a man who thought he was a gourd.

As I argued above, if Descartes is serious that the existence of madmen is no ground for doubt, then he is committed to the maxim that one ought not emulate madmen.[12] This maxim legitimizes the argument against madness and, Reid suggests, is enough to quash hyperbolic doubt. The Cartesian may freely admit that the existence of madmen does constitute grounds for doubt, but this means that if the method of doubt is to be motivated at all, we need nothing so grandiose as the evil demon scenario.

One may object: 'Madness' is familiar in a pejorative use, applied to views that we find uncongenial— views which we judge are not to be believed. As such, insofar as the argument is valid the 'one should believe' in the conclusion is more about social acceptability than about epistemic justification. It is more

[12] One might extrapolate a similar maxim from his statement in the Synopsis that the arguments are not interesting for what they prove, since no sane person would doubt the conclusion [MFP, AT VII 15–6, cited above].

like 'one should not chew with one's mouth open' than 'one should not believe a contradiction.' Even if we could make out the argument as one we would want to endorse, the sceptic is free to deny its validity. He need not pay any great price to deny inferences of this form. If the sceptic lives without incurring sanction from the community, then his is a benign form of madness and this argument would be insufficient to shake him from it.

2.3.2 Cognitive commitment

This observation points us toward a different strand of Reid's argument. He suggests that by accepting the authority of reason, the Cartesian accepts the authority of our natural faculties. If one is in the business of accepting the authority of natural faculties, and if one concedes that perception is one of the natural faculties, then it makes no sense to attempt radical doubt with respect to the perceivable world. The Cartesian might deny that accepting reason demands accepting the authority of other faculties, but giving reason this special status is notoriously difficult to motivate.[13]

Reid's argument here might be seen as an instance of a general argument form. Let's call it the *argument from cognitive commitment*. Schematically, we may express it in this way:

1. You accept judgments $A, B, C \ldots$ because they are justified by inferential form F.

2. The contentious claim P is underwritten by F.

3. You should accept P or reject $A, B, C \ldots$

Parity of reasoning arguments follow this same structure. As is often observed, however, one man's modus ponens is another man's reductio— the matter turns on

[13]Fallibility cannot be what raises reason above perception— we often make mistakes in reasoning, too. As Reid says, "Our senses, our memory, and our reason, are all limited and imperfect— this is the lot of humanity" [EIP, ess. 1 ch. 22, p.335]. Even Descartes notoriously seems to admit that T_4 should undermine our confidence in reason and inference.

whether P is verum or absurdum. If Reid's diagnosis is correct and the Cartesian sceptic accepts reason on the grounds that it is a natural faculty, then the sceptic is faced with a choice of either accepting the senses on the grounds that they too are a natural faculty or rejecting even reason.[14]

The argument from cognitive commitment doesn't issue in a categorical conclusion. It is not a direct proof and it lacks deductive certainty. Instead, it is like a relative consistency proof. In set theory, mathematicians prove that ZFC is consistent if ZF is consistent, but they cannot prove that ZFC is consistent *tout court*.[15] Most do in fact believe that ZF is consistent, but Gödel's incompleteness theorem shows that there can be no direct proof of its consistency. Similarly, Reid argues that we should trust our senses if we trust our reason, but that does not show that we should trust our reason. Reid does in fact trust both, but admits that there is no proof that we should trust either.

I argued at the end of §2.2 that Descartes' method, employed thoroughly, ends not in doubt but in quietism. Reid escapes doubt, perhaps, but does so by trusting his faculties. Yet if he trusted whatever he had been taught and exorcised any spectre of doubt, then he too would end up in quietism. If this were so, Reid would be no better a model than Descartes. Is this merely dogmatism, preserving a precious worldview by appeals to Common Sense? I think not. Common Sense, unlike unrepentant dogmatism, allows room for criticism. Our senses have a positive presumption, in that seeing is *grounds for* believing, but the presumption is defeasible. I'll spend the remainder of this chapter elaborating this middle way between faith and proof.

2.4 Three further replies to the sceptic

> "It has often been argued that absolute scepticism is self-contradictory; but this is a mistake. . . . [T]here are no such

[14]It is possible for the sceptic to claim that reason is to be accepted on different grounds than its status as a natural faculty, but what other rationale might he offer?

[15]ZFC is the theory formed by adding the axiom of choice to the axioms of Zermelo-Frankel (ZF) set theory.

BEINGS AS ABSOLUTE SCEPTICS."

—C.S. Peirce, 1869 [Pei92, p. 56]

As we have seen, Reid's appeal to Common Sense— construed either as an argument from madness or as an argument from cognitive commitment— is not a direct answer to the determined sceptic. He concedes: "Perhaps the sceptic will agree to distrust reason, rather than give any credit to perception" [Inq, ch. 6 §20, p. 183]. Although Reid can offer no utterly compelling reasons why the sceptic should not do this, he goes on to offer three reasons why he and "the sober part of mankind" would not follow the sceptic in doing so.

First, Reid insists that he is unable to disbelieve all that he perceives. Even the sceptic, "may struggle hard to disbelieve the informations of his senses, as a man does to swim against a torrent: but, ah! it is in vain.... For, after all, when his strength is spent in the fruitless attempt, he will be carried down the torrent with the common herd of believers" [Inq, ch. 6 §20, p. 184]. It is no use for the sceptic to insist we should doubt everything if it is impossible to do so. There are more recent arguments as well that scepticism is impossible. Indeed, I find scepticism to be a psychological trick beyond my ken. Nevertheless, some people claim to be able to suspend judgement about everything. From what has been said so far, who am I and who is Reid to argue with their self reports?

Second, Reid suggests that actually doubting the world, were such a thing possible, would only lead to disaster. Suppose, Reid says, "I resolve not to believe my senses. I break my nose against a post that comes in my way; I step into a dirty kennel; and, after twenty such wise rational actions, I am taken up and clapped in a madhouse" [Inq, ch. 6 §20, p. 184]. There is a commitment in practice to the existence of an external world that contains many of the snares and pitfalls in which realists believe.

Third, Reid notes that scepticism about the world can only arise after many years of living in the world; the doubt is only possible after a long history of trust. He puts the point this way: "I gave implicit belief to the informations of Nature by my senses, for a considerable part of my life, before I had learned so much logic as to be able to start a doubt concerning them" [Inq, ch. 6 §20, p. 184]. The track record of perception has been good, and without perception we would never have come so far as to be able to entertain the possibility of doubt. If we put trust in our critical skills, why not also in the circumstances which fostered those skills?

If the sceptic persists in doubting after such reasons, Reid thinks that there is no ultimate argument with which to force assent.

2.4.1 Impotence

In the first of the three further replies, Reid alleges that scepticism is in some sense impossible. This *argument from impotence* turns on a psychological claim. Suppose Reid is right that I am utterly incapable of denying the existence of an external world. This doesn't show that my belief in it is justified. Nevertheless, it does give me a reason to accept that belief.

Consider a parallel case: The fact that perpetual motion is impossible does not show that I *ought not* build a perpetual motion machine, in the sense that it would be wrong for me to do so. It seems plausible to say that neither right nor wrong attach to building such a machine. Nevertheless, this fact convinces me that I should not spend time attempting to invent perpetual motion machines, even though they would be very useful if only they were possible. The force of this 'ought' is both rhetorical and rational. If I come to be convinced that perpetual motion is impossible, I will also come to give up any research into it. Not only will this reason convince me, it is reasonable for me to be convinced.[16]

The existence of the external world may be thought of similarly. The fact that I cannot help but believe in an external world provides me with a reason not to attempt withholding assent. That said, it remains to be explained how I

[16] The argument may be answered by claiming that, whereas this is only a prudential reason, decisions about what to believe should be made on the basis of epistemic reasons; it may also be rejected for a host of other reasons. I develop one especially salient objection below and, so doing, leave others undeveloped.

can know which beliefs I cannot help but accept. Some people claim to be able to withhold assent from the belief in an external world. Perhaps such gifted sceptics are different from the rest of us, but how could we know? Other people at other times have claimed that they could not but believe other, more controversial things. Perhaps the devout interlocutor will say that it is impossible not to believe in God, or the mathematically-retrograde interlocutor will say that it is impossible to deny the truth of the parallel postulate. Not only would I insist that it is in my power to doubt *these* things, I would suggest that their assessment of their own abilities reflects only a lack of imagination or determination. It is open to the sceptic to give the same reply, insisting that Reid's belief that he cannot doubt the existence of the world reflects only a lack of imagination.

The argument from impotence fails, then, not for a lack of rhetorical or justificatory force. Instead, the problem is that it turns on a premise about some matter of fact. Worse, this matter of fact is of a sort that is difficult to establish and of a sort that— we observe easily enough— people are apt to get wrong. Any interlocutor may respond to the argument from impotence merely by denying the premise, and after they have done so there is little more to be said.

2.4.2 Practice

In the second of the three further replies, Reid insists that sincere scepticism would undercut practical engagement with the world and thus suggests that the so-called sceptic betrays a belief in the real world by managing his affairs just as common folk do. We have already seen a prepared Cartesian reply to this argument: Methodological doubt is about belief but not about action. Following Descartes' methods, one would navigate the world just as believers do even before emerging from doubt [DoM, p. 23]. Reid anticipates such a reply, however, insisting: "If a man pretends to be a sceptic with regard to the informations of sense, and yet prudently keeps out of harm's way as other men do, he must excuse my suspicion, that he either acts the hypocrite, or imposes upon himself" [Inq, ch. 6 §20, p. 184]. Surprisingly, Descartes makes a similar point. Discussing how to decide what his countrymen believe, he writes:

> I thought too that in order to discover what opinions they really held I had to attend to what they did rather than what they said. ...[M]any people do not know what they believe, since believing something and knowing that one believes it are different acts of thinking, and the one often occurs without the other. [DoM, p. 23]

We should judge people by their actions, Descartes suggests, because their actions most reveal what they believe. It is hard to see how he could assert this but deny Reid's suggestion that the ordinary practices of people who are able to navigate the world indicate that they know their way around in the world and that they believe that there is a world.

This *argument from practical commitment* issues in a conditional conclusion much as the argument from cognitive commitment does. The sceptic may challenge the form of the inference, of course, by arguing that behaving much as common folk do does not suppose believing as common folk do. Perhaps some sense could be made out of the sceptical lifestyle which does not presuppose an implicit belief in the external world, but it doesn't look as if even Descartes would allow for such a possibility. If a sceptic concedes that her practice implies certain beliefs, then she is left with a choice of abstaining from her practice or accepting the beliefs. The argument from practical commitment cannot force her choice, but it makes her pay a higher price if she remains a sceptic.[17]

2.4.3 Cognitive commitment— a variant

In the last of the three further replies, Reid observes that sceptics lived as ordinary folk for many years and their scepticism can only be motivated in light of things they had learned in the course of ordinary life. The very observations which motivate scepticism come from trusting memory and the senses. This variant of

[17] Although applied here against scepticism, the argument from practice is quite general; cf. the discussion of constructive empiricism in §3.1.3.

Figure 2.3: Reid argues that the sceptic, were he determined and consistent, would meet with tragedy.

the argument from cognitive commitment leaves the Cartesian sceptic in a bind. Descartes says that there are madmen, that he has dreamed, and so on. Without these observations, he cannot make the case for underdetermination. Without the underdetermination, why distrust the senses rather than trusting them? The arguments of the First Meditation are meant to make the case for methodological scepticism, not to presume it![18] Absent such an argument, we trust perception as *prima facie* warrant to believe in the things perceived. In appealing to our past experience with dreams, Descartes implicitly asks us to trust our memory and senses— but our belief as to whether we are awake or dreaming will only be underdetermined if we give up trusting our senses once the rival hypotheses are spelled out.

2.5 Standards and Common Sense

Reid's arguments will provide no comfort to the Cartesian, since Descartes is not merely trying to determine what he should believe. As both Kitcher and Ricoeur observe, it is the Cartesian standard which makes scepticism inescapable (citations in §2.2, above). Descartes wants to know what he can believe with certainty, what he can believe without risk of needing to revise his beliefs, what he can know infallibly, and what he can know to be true without any antecedent commitments. These desiderata are distinct, but it seems fair to say that Descartes pursues them all. It's essential to distinguish the Cartesian desiderata in an attempt to find which among them traps the Cartesian in scepticism. The fault of Cartesian epistemology, we will see, is that it trusts nothing.

[18] One might instead think that for Descartes methodological scepticism *is* presumed. The sceptical scenarios would then serve as exercises to help us shake off our obdurate belief in an external world, rather than as arguments to convince us that we should shake it off. Regardless of what Descartes' intention might have been, Reid and many later commentators see the sceptical scenarios as arguments for scepticism.

2.5.1 Certainty

Blaming Descartes' standard for the bind Cartesianism gets us into is not enough, because Descartes has a number of motivations and his standards might be explicated in several different ways. The problem is the insistence on *certainty* in some sense, but certainty is a difficult notion to pin down. Let us ask instead what distinguishes Descartes' standards from Reid's.

First try: Both Kitcher and Ricoeur suggest that Descartes' error was in searching for beliefs that could survive all doubt. Yet, as the discussion of the argument from impotence showed, even Reid thought that there were some claims which we could not doubt. Whether there are such claims is ultimately an empirical question about human psychology which might go either way, for all one can say *a priori*. Descartes might have gone astray insofar as he saw inability to doubt as the touchstone of truth, but that criterion might point towards or away from scepticism. It is not yet the rotten core of the Cartesian standard.

Second try: The Cartesian standard demands contributions that will "hereafter be firm and lasting" [MFP†, AT VII 17, cited above]. Yet Reid also thinks that some elements of science will remain in place indefinitely; he writes that mechanics, astronomy, and optics are "really sciences built upon laws of nature which universally obtain. What is discovered in them is no longer a matter of dispute: future ages may add to it; but, till the course of nature be changed, what is already established can never be overturned" [Inq, ch. 1 §3, pp. 99–100]. Rhetoric about parts of science that will remain unchanged reflects an overly strong conception of scientific progress, but that conception of progress does not entail scepticism. Just as we may have reason now to believe some proposition P, we might have reason now to believe "We will always have sufficient grounds to believe P." Call this latter claim B_P. Believing B_P doesn't mean we must ignore legitimate reasons to stop believing P, should they arise— and those reasons would be reasons to stop believing B_P, too. Whether we should believe B_P for any P is an empirical question.[19] Again, we must look further.

Another try: The Cartesian standard demands infallibility; claims must be such that they *could not possibly* be false. Reid insists, contrawise, "human judgements ought always to be formed with an humble sense of our fallibility in judging" [EIP, ess. 7 ch. 4, p 485]. It is now commonplace to observe that none of our judgements is infallible— that anything we do believe might, in the end, prove false. It is possible to infer fallible knowledge from fallible premises, but infallible knowledge requires either infallible premises or— better still— no premises at all. The aspect of the Cartesian standard that Reid rejects most centrally, I suggest, is the aim to secure knowledge that relies in no way on our prior commitments. There are certain things we must *trust*, Reid insists, and no good epistemology can begin from systematic mistrust.

2.5.2 Trust

There is a certain paranoia in the Cartesian standard.[20] The senses do sometimes mislead us— this is uncontroversial— but Descartes writes that "from time to time I have found that the senses deceive, and it is prudent never to trust completely those who have deceived us even once" [MFP, AT VII 18]. Yet trust is rarely, if ever, a matter of trusting completely; to trust our senses is merely to take (e.g.) seeing something as a *prima facie* ground for believing it is as we see it. 'Seeing is believing' is at best a ceteris paribus law. In this way, a modicum of unreliability is compatible with trust. Consider, as an analogy, that I trust my officemate, Ryan; if he says Q then I take that as grounds for believing Q. If he were systematically wrong about certain sorts of things, I would stop trusting him on those matters; I have learned not to trust his taste in movies, for instance.[21] Were he in error merely about some one claim or other, then I would not cease to trust

[19]Since B_P entails P, we should probably never be more confident of the former than we are of the latter.

[20]As Nick Jolley once suggested to me, "There is something neurotic about Cartesian empistemology."

[21]The subjectivity of taste is not the issue. I have learned not to believe him when he says that *I* will enjoy some movie or other.

him. I have had a longer acquaintance with my senses than I have had with Ryan, and I trust them even though they sometimes mislead me. Reid himself makes the analogy between our trust in reliable witnesses and our trust in our senses, between testimony in the literal sense and the testimony of the senses [Inq, ch. 6 §24, pp. 194–200]. The Cartesian standard is overly strict not merely because it demands certainty, but also because it forbids placing real *trust* in anything. Trust, both in observation and in other scientists, plays a critical rôle in scientific enquiry.[22]

Despite the shipwreck of Cartesianism, the history of epistemology is filled with projects organized around strict standards. Kant, for instance, aims to establish a system which will persist in an "unchangeable state" [Kan96, p. Bxxxviii]. Without such a system, Kant argues in a famous passage,

> there always remains this scandal for philosophy and human reason in general: that we have to accept merely on *faith* the existence of things outside us (even if they provide us with all the material we have for cognition, even those of our inner sense); and that, if it occurs to someone to doubt their existence, we have no satisfactory proof with which to oppose him. (p. Bxxxix fn., italics in original)

Reid offers a way of answering the sceptic which is neither a strict proof nor an appeal to *mere* faith. If we are to be satisfied with this, however, we must admit that where it leaves us is no scandal for philosophy.

Reid's argument begins with the observation that reason and perception are natural faculties and, as such, merit *prima facie* credence. Descartes denies this, or at least seems to deny it, when he adopts the method of doubt. Reid explains:

> If a sceptic should build his scepticism upon this foundation, that all our reasoning and judging powers are fallacious in their nature, or should resolve at least to withhold assent until it be proved that they are not, it would be impossible by argument to beat him out of this stronghold; and he must even be left to enjoy his scepticism.[23] [EIP, ess. 6 ch. 5.7, p. 447]

[22] See the work of Steve Shapin, e.g. [Sha94].

[23] Reid applies the point to Descartes in the passage immediately following the one quoted here. He quickly interweaves it with the argument from impotence, making it easy to mistake for another instance of that same argument.

The point here is not that scepticism is impossible. Rather, scepticism involves an unreasonable standard of evidence: Assent should be withheld from a belief until it can be proven infallibly. Reid admits to the sceptic that this standard of evidence, applied consistently, demands withholding assent from any belief whatsoever. What Reid suggests is an alternative standard of evidence: Beliefs formed on the basis of natural faculties such as reason, perception, memory, and so on should be given a positive presumption of truth. When we reason to a conclusion that we have no other grounds to reject, we accept the argument. When we see a cat sitting on a table and have no reason to suspect dreams or animatronics, we accept that there is a cat. If indeed these are our natural faculties, this is an obvious standard. As Reid puts it, we trust our faculties. How could we do otherwise? How else would we form beliefs besides the ways in which we form beliefs?

This argument, rather than being merely psychological or practical, is fundamentally rational. It involves a claim about how one should responsibly apportion belief and doubt. To make this clearer, consider how this argument may be deployed to resolve underdetermination. As you have the experiences you are having now, you might believe that you are reading my dissertation, that it is the torment of some evil demon, or whatever else; you may believe any of the theories $\{U_1, U_2, U_3, U_4\}$ as defined in §2.1. No subsequent experience could convince you beyond all possible doubt of U_1 rather than U_4.[24] So, the scope of this underdetermination takes in all evidence that you might ever collect. Reid accepts this exposition, in effect, but argues that Descartes has presumed an unfair standard of what should count as a responsible decision between rival theories.

Recall from the previous chapter that to specify a case of underdetermination we must specify a set of rival theories and a scope over which decision between the rivals is underdetermined, but also a standard for what would count as responsible decision. As Reid admits, it is always possible for a sceptic to insist on the strictest standard. In such a case, it will not be possible to dislodge the scep-

[24] Here discounting the argument from impotence.

tic with rational arguments alone— what counts as reasonable is just one of the things in dispute! By appealing to the sceptic's own commitments, both practical and cognitive, Reid tries to show that the sceptic ought not accept such a strict standard. The commitments serve as an *arational* starting point. It might be nice to make a stronger reply than this, since the sceptic is always free to struggle in an effort to throw off these prior commitments. What would a stronger reply be like? We could argue that the sceptic's struggle is doomed to failure, but this is a claim about the sceptic's incapacities. This claim itself relies on a matter of fact about which it is possible to be sceptical. We could instead argue that the sceptic should in some binding sense accept these commitments, but that proof will itself suppose a standard of proof. Not only are Reid's arguments workable tools, then, but we would be hard-pressed to find better ones.

With a standard by which we trust in neither any beliefs nor any processes of belief formation, underdetermination is ubiquitous and uniform. It may be formulated as a worry about madness, dreaming, or demonic deception, but the epistemic upshot is the same in any case. Only relative to a different standard will it be possible to say anything else about underdetermination. The standard need not be too liberal, it need only allow for appeal to some of our prior commitments.

Although they can be applied widely, Reid-style arguments will not always be applicable. It may be that there are no commitments shared by disputants that are sufficient to settle the dispute. This shouldn't be a reason for despair. As Reid shows, the commitments can be very general and need not be controversial of themselves. Thus, the strategy holds out hope for resolving cases of underdetermination in science. Scientists, as scientists, share a host of commitments. Indeed, as an agent engaged in satisfying her human needs, a New York cabby shares many commitments with an Azande tribesman. Although these considerations cannot *in principle* settle all disputes, they may go a long way toward settling *actual* disputes among actual people. They may be short-circuited if some people give up the pivotal commitments, but actual people would not do so with the same willingness as imagined sceptics; these things are commitments, after all, just because people are antecedently committed to them.

III

Top-Down Arguments

> "Mr. Palomar has not yet managed to understand. The explanations offered are all a bit dubious, conditioned by hypotheses, wavering among various alternatives; and this is only natural, since these are rumors that pass from mouth to mouth, while science, which should confirm or deny them, is apparently uncertain, approximate. Things being as they are, then, Mr. Palomar has decided to confine himself to watching, to establishing down to the slightest detail what little he sees, sticking to the immediate ideas that what he sees suggests."
>
> —*Mr. Palomar*, Italo Calvino [pp. 61–2]

This chapter is concerned with *top-down* arguments that begin with universal underdetermination and move to object lessons about science. Often, philosophers are presumed to have provided compelling arguments for underdetermination. An author may write that *some other* author has demonstrated that all theory choice is underdetermined, and the present author draws out epistemic consequences of this result. The critical moment of these arguments is the part all too often relegated to a reference in a footnote. What sort of inference justifies the claim that all theory choice is underdetermined? What features is the allegedly ubiquitous underdetermination going to have?

Several families of top-down arguments merit attention. The first alleges that every scientific theory has empirically equivalent rivals such that neither the theory nor the rivals may be justifiably believed. Cartesian sceptical arguments are members of this family, and another member appears vividly in debates over constructive empiricism. The Cartesian menace was met in the previous chapter; in §3.1, I will develop Reid's replies to Descartes as resources against the constructive empiricist. Then a more general discussion of empirical equivalence is in order (§3.2). A second family of top-down arguments appeals to the interpretative flexibility of theories in an aim to show that they can never be unproblematically confirmed or disconfirmed. These arguments are usually attributed to Duhem and Quine. The flexibility may come from reliance on auxiliary assumptions (§3.3) or from the possibility of meaning change (§3.4). A third family of argument rests on the possibility of denying recalcitrant observations (§3.5).

3.1 From underdetermination to empiricism

Contemporary *constructive empiricism* is motivated by a worry about underdetermination. Admittedly, the founding and most well-known constructive empiricist— Bas van Fraassen— rarely appeals to 'underdetermination' explicitly. Yet, other authors have noted his implicit reliance on underdetermination (e.g., Churchland [Chu85, p. 37] and Kukla [Kuk98, p. 59]), and van Fraassen himself does mention it in key places (viz., [van80, p. 59] and [van85, p. 248]). In the next section, I will sketch the constructive empiricist's use of underdetermination. In the two subsequent sections, I apply two of Thomas Reid's answers to the sceptic as answers to the constructive empiricist. The constructive empiricist's underdetermination is one species of the genus empirical equivalence; much of what is to be said about constructive empiricism will carry over directly to the discussion of empirical equivalence that follows after it.

3.1.1 Constructive empiricism

Scientific theories describe a world that has observable parts and unobservable parts. We can check a theory's account of the former by direct observation, but the latter are more esoteric. We might say, on the basis of our observation, that the theory is *empirically adequate*, that it correctly renders the observable part of the world. We might say instead that the theory is *true*, that the whole theory matches up to the world. The latter claim is much stronger than the former, so as a matter of elementary probability theory the latter is generally less probable. Thus, there is a risk involved in believing that the theory is true that is not involved in believing that it is empirically adequate.[1] Rationality does not define exactly how risk-tolerant we should be, so it does not fix whether we should believe the theory or believe only that it is empirically adequate. Van Fraassen is careful not to say that belief in unobservable entities is *irrational*, but rather he says that it is *not compelled by rationality*. Thus, he thinks, he is within the bounds of reason in refusing to believe in them.

The constructive empiricist *accepts* but does not *believe* scientific theories. Acceptance in this sense involves cognitive and practical components. Cognitively, it involves belief that theories are empirically adequate— that the theory holds of the entities that have been observed, and also that it will hold of entities observed in the future. Practically, it involves the commitment to use the theory as a guide for future research, to employ the resources of the theory in guiding action, and so on. Van Fraassen explains:

> While the only belief involved in acceptance... is the belief that the theory is empirically adequate, *more than belief is involved*. To accept a theory is to make a commitment, a commitment to the further confrontation of new phenomena within the framework of that theory, a commitment to a research programme, and a wager that all relevant phenomena can be accounted for without giving up that theory.... Commitments are not true or false; they are vindicated or not vindicated in the course of human history. [van80, p. 88]

[1]For this reason, Teller refers to van Fraassen's use of underdetermination as " 'the pointless epistemic risk' argument" [Tel01, p. 128].

If van Fraassen is right, standards of responsible belief underdetermine belief—the scope for this underdetermination includes all observationally possible circumstances. (Observational possibility is hard to define, but allow it for the nonce. I return to the issue below.) It is permissible to believe that a theory T is true, but similarly permissible to believe only that it is empirically adequate. The set of rivals thus at least contains 'T is true' and 'T is empirically adequate.' Note, as van Fraassen is well aware, that we can add the further rival: 'T has been empirically adequate so far.' The underdetermination remains between these three rivals. Rationality, van Fraassen thinks, permits but does not compel believing any of the three. Thus, if van Fraassen is correct, it is just as reasonable to believe only that the theory *has been* correct about observables as it is to believe that it *will be* correct. Thus, one might permissibly be a sceptic about all things except phenomena that have actually confronted us. Given that scepticism is permitted on this view, we may expect Thomas Reid's replies to the sceptic to apply here. Indeed, the arguments from cognitive commitment and practice can readily be pressed into service against constructive empiricism.[2]

The argument from cognitive commitment, recall, appeals to grounds of justification that the sceptical interlocutor already accepts, if only implicitly, and aims to show that those principles support the contentious beliefs. (See §2.3.2 in the previous chapter.) The rhetorical aim is to make the interlocutor choose between his scepticism and the inferential commitment. Against the constructive empiricist, the aim is to show that reasonable inference principles do lead to belief in unobservables and that standards of theory choice don't allow as much slack as the empiricist thinks they do. A general argument of this form has entered recent literature touted as the *Galilean Strategy.* I take this up in §3.1.2.

The argument from practice maintains that the interlocutor who acts *for all the world* as if he believed certain things should be taken as believing them. (See §2.4.2 in the previous chapter.) Indeed, if the empiricist makes every practical

[2]The arguments from madness and impotence might be applied, but would be vulnerable to the same objections here as when applied to Cartesian scepticism.

decision just as if a theory were true— really *true*— then one should be dubious of his alleged agnosticism. He must excuse our suspicion "that he either acts the hypocrite, or imposes upon himself" [Rei67, p. 184]. I treat this more carefully in §3.1.3.

3.1.2 Cognitive commitment and the Galilean Strategy

In *Real Realism: The Galilean Strategy*[3], Philip Kitcher offers a defense of realism from nearly half-a-dozen arguments against it. Against constructivists and empiricists, on matters both semantic and epistemic, he deploys the Galilean Strategy— a move to show that methods of settling questions about unobjectionable, observable matters should be relied on to settle questions about controversial, unobservable matters.[4] I approach the Strategy here as a response to constructive empiricism, but concerns explored below may be raised with respect to the rest.

Kitcher outlines an argument from empiricist premises to the rejection of methods that putatively inform us about unobservables: We should only rely on methods that we can check independently. We can only check matters that we can observe. So, we should only rely on methods insofar as they inform us about observables. Therefore, we should remain agnostic about conclusions regarding unobservables.[5] Cast in different terms, the argument is aimed to show that *beliefs* about unobservables are underdetermined in a way that beliefs about observables are not because *methods* for fixing beliefs about unobservables cannot be checked.

Kitcher notes that this argument has a long pedigree and sees Bas van Fraassen as its contemporary champion. [RR, p. 166 fn. 27] In a recent paper, van Fraassen writes the following:

> If you see a reflection of a tree in the water, you can also look at the tree and gather information about the geometric relations between the tree, the reflection, and your vantage point. The invariances in those

[3][Kit01a], henceforth RR. This section is, in its essentials, my [Mag].

[4]He also develops the Galilean Strategy in less detail elsewhere; cf. [Kit01b, ch. 2].

[5]Kitcher calls this argument EEA. He provides the argument in six steps, but nothing here turns on my truncating it as I have done. [RR, pp. 161–2]

> relations are precisely what warrant the assertion that the reflection is a picture of the tree. If you say similarly about the microscope's images that they are pictures of e.g. paramecia, then you are asserting that there are certain invariant geometric relations between the object, image, and vantage point. But now you are *postulating* that these relations hold, rather than *gathering information* about whether that is so. [van01, p. 160]

This is not yet an anti-realist conclusion. To show that we ought not believe in paramecia, one would need to show that we ought not postulate paramecia— something van Fraassen does not try to show. Rather than claiming that postulating paramecia is *irrational*, van Fraassen insists that it is not compelled by rationality. We may believe in paramecia or we may remain agnostic, as we choose. The former exposes us to the risk of being wrong when we might have avoided error, and the latter exposes us to the risk of not believing a truth that we might have believed. As William James observes, our obligations to gather truth and avoid error are continually in conflict.[6] Van Fraassen only insists that avoiding potential error by remaining agnostic about unobservables is permissible. Thus, Kitcher would need to do more than defeat the argument above in order to sway the constructive empiricist. He needs to provide a positive argument that empiricists should give up their agnosticism.

Indeed, Kitcher provides a positive argument. Examining the argumentative strategies that Galileo employed in convincing his contemporaries to believe what they could see in telescopes, he argues that they had good reason (as van Fraassen might put it) to postulate the moons of Jupiter, the rings of Saturn, and all the rest. Moreover, he claims that an analogue of Galileo's argument gives us good reason to accept other methods that inform us about unobservables.

[6] [Jam48, §VII]. Longino similarly contrasts the "knowledge-extending mission" of science with "its critical mission" [Lon90, p. 34].

The Galilean Strategy

In 1610, Galileo was faced with the problem of justifying the telescope as an instrument. One could see points of light when looking through the device toward Jupiter, but he had to show that the lights were moons and not some artifact of the telescope itself. He did this "by showing that the telescope would deliver conclusions that could be verified using methods that his contemporaries, including his critics, would accept" [RR, p. 173]. He could take it out on a balcony, point it at a distant building, and anyone could see detail through it that they could only make out from a lesser distance with their naked eyes; this readily showed that the telescope was good for discerning details of structures in Northern Italy. After such tests, Kitcher notes, none of Galileo's interlocutors worried that the telescope would not work for such applications in the vicinity of London or Amsterdam. Within the terrestrial realm, there was no reason to draw a distinction between these places. As Kitcher writes: "Galileo's central problem was to make the celestial-terrestrial distinction appear as irrelevant as the difference between London… and Venice" [RR, p. 174].

Kitcher analyzes Galileo's answer to this problem into two parts. First, Galileo exploited the vagueness of the boundary between the observable and the unobservable to show that the telescope was reliable beyond the bounds of what was straight-forwardly observable. Where only sharp-eyed observers could distinguish a fine detail unaided, both they and folks of ordinary acuity could make it out with the telescope. The deliverances of the telescope were thus shown to be continuous with the deliverances of plain vision— the unobservable was shown to be continuous with the observable. Importantly, this could be done for astronomical phenomena. Already, the boundary between the terrestrial and the celestial was softening. Second, Galileo argued directly against that boundary by cataloguing changes in the allegedly immutable heavens. Kitcher summarizes the action: "Combining these two arguments with his ability to distribute telescopes that would generate an increasingly more consistent set of astronomical observations, Galileo was able to convince his peers that there was no more basis for thinking that the instrument was unreliable in the heavens than for believing it inept in some as yet untried part of the earth" [RR, p. 174].

Kitcher is not centrally concerned with the telescope; he derives from Galileo's argument an argumentative schema that he dubs *the Galilean Strategy*. He generalizes in this way:

> Methods of justification, like Galileo's telescope, can only be validated by examining the conclusions about observables to which they lead. It does not follow that the only conclusions licensed by those methods are conclusions about observables— any more than Galileo's demonstrations on buildings and ships only show that the telescope is reliable in Venice. We need to consider whether there are good reasons for distinguishing a method's usage in its application to observables from its usage in application to unobservables. [RR, p. 175]

To distinguish it from the particular arguments made by Galileo, let's call the Galilean Strategy '$\mathcal{GS}$'. Take some method M that provides the correct answers for matters we can check independently. $\mathcal{GS}$ may be summarized by the following schema:

$\mathcal{GS}$1 M provides correct answers up to and along the vague boundary between matters we can check independently of M and ones that we cannot check.

$\mathcal{GS}$2 Prevailing reasons for thinking that the boundary might make a difference to the reliability of M are mistaken.

$\therefore$ M provides the correct answers for matters that we cannot check independently of M.

$\mathcal{GS}$ and the empiricist

There is an obvious way to employ $\mathcal{GS}$ against the constructive empiricist. To begin, Galileo's own arguments will do. Telescopes provide us a way of learning about the moons of Jupiter, the rings of Saturn, and many things more distant and exotic.[7] A trivial variant of Galileo's argument might motivate belief in entities visible through optical microscopes: paramecia, cells, cellular organelles, and so on. We can use magnifying glasses and microscopes to clearly see things that we could otherwise see only with careful scrutiny, we can use them to see features which only our sharp-eyed friends can make out, and so on. The empiricist may resist these cases— distant things seen through the telescope could be made observable to everyone merely by bringing them closer, but this is not possible with the microscope. The empiricist may look to Hacking, who notes that the move "from a magnifying glass to even a low powered microscope is the passage from what we might be able to observe with the eye unaided, to what we could not observe except with instruments" [Hac85, p. 135]. Nevertheless, there are intermediate cases for which we can confirm the things seen with the microscope. Hacking provides the example of microscopic metal grids used for reidentifying particular bodies on microscope slides. Grids of ordinary size are photographically reduced and metalized using techniques which operate also in the macroscopic realm. [Hac85, pp. 146–7] We can imagine making a series of grids, the largest clearly observable to the average person without any magnification and the smallest unobservable to even the keenest eyes. This series of cases would show that the microscope is reliable at and through the limits of what the average person can observe using only their unaided vision. ($\mathcal{GS}1$ is satisfied for the optical microscope.) There is no reason to believe that the operation of the microscope *changes* when we point it at things just beyond the acuity of our sharp-eyed friends. ($\mathcal{GS}2$ is satisfied.) Thus, we draw the Galilean inference: Things we see in the microscope are really there.

Van Fraassen urges agnosticism about the deliverance of microscopes, but concedes, "...I really don't mind very much if you reject this option for the optical microscope. I will be happy if you agree to it for the electron microscope. ...The

[7] Van Fraassen allows that the moons of Jupiter are observable, since an astronaut in the vicinity of Jupiter would be able to see them without a telescope. [van80, p. 16] This is problematic, as Kitcher notes. [Kit93, p. 152–3] Should Galileo's contemporaries have objected by noting the then speculative nature of space travel?

point of constructive empiricism is not lost if the line is drawn in a somewhat different way from the way I draw it. The point would be lost only if no such line drawing is considered relevant to our understanding of science" [van01, pp. 162–3]. So a constructive empiricist can agree, in light of $\mathcal{GS}$, that there are paramecia and distant moons. Yet the genie of postulation, once let out of the bottle, is not so easily put back in.

Once we believe in the features that we can see with an optical microscope, we can employ $\mathcal{GS}$ again. The gross features that can be discerned with an electron microscope can be discerned with an optical microscope, and we can check them against each other up to the limits of optical magnification.[8] ($\mathcal{GS}1$ is satisfied for the electron microscope.) There is no good reason to think that the electron microscope betrays us just beyond the limits of what we can check. ($\mathcal{GS}2$ is satisfied.) So we should believe in things we can see with electron microscopes. Similar strategies can be used to extend the boundary of the observable whenever a new instrument has overlapping applications with one already vindicated by $\mathcal{GS}$.[9]

The constructive empiricist may reply that the boundary between the observable and the unobservable, vague though it may be, is principled and that this principle gives us a reason to presume that the boundary between matters we can check and matters we cannot is relevant. Suppose that the principle is to acknowledge only entities and properties that are amenable to direct, unaided perception.[10] Believing in paramecia would violate this principle and an application of $\mathcal{GS}$ directs us to believe in paramecia, so the constructive empiricist raises a worry about $\mathcal{GS}$. Note that the premise $\mathcal{GS}2$ only asserts that prevailing arguments are insufficient to show that cases we cannot check would be different from cases that we can check. Why should the burden of proof lie that way? The constructive empiricist may insist that $\mathcal{GS}2$ is insufficient and that there must instead be some positive reason to suppose that cases we cannot check would be like cases that we

[8] Hacking provides an illustration of electron microscopy being checked in this way. [Hac85, p. 144]

[9] As Hacking notes, light microscopes and electron microscopes are both congeries of related instruments. Using $\mathcal{GS}$ to vindicate the whole motley would require a great deal more than I can say here.

[10] This is van Fraassen's preferred version of the distinction. [van80, p. 10]

can check. If we accept this demand— and I can see no compelling reason not to— $\mathcal{GS}$ requires an extra premise:

$\mathcal{GS}$3 There is some significant positive reason to think that the success of M on matters we can check generalizes to matters that we cannot check.

Whereas $\mathcal{GS}$2 obtains when we have no reason to think M will *fail* beyond the limit of the observable, $\mathcal{GS}$3 obtains only when we have some reason to think M will *succeed*.[11] To apply $\mathcal{GS}$3, we need to show that there is continuity among the various applications of the method M. We might do this by marshalling systematic, theoretical resources. When M is an instrument, though, we can begin with the homey observation that it is the same instrument used in the same way in both cases. A microscope is the same observable, material object when used to view the date on a penny and when used to look at paramecia. Even where different lenses are used, the lenses may be made from the same glass and ground in the same way. The very material of the instrument provides continuity between cases where it is used to look at observables and cases where it is used to look at unobservables.

As Kitcher notes, Galileo's defense of the telescope involved relevant formulations of $\mathcal{GS}$1 and $\mathcal{GS}$2, but also an effort "to distribute telescopes that would generate an increasingly more consistent set of astronomical observations" [RR, p. 174, cited above]. Demonstrating the consistency of the instrument was a way of showing that telescopic observations of observables and unobservables were due to similar causes and thus that $\mathcal{GS}$3 was satisfied. Whereas Kitcher offers this as a move in addition to $\mathcal{GS}$, it is plausibly seen as supporting a further premise of $\mathcal{GS}$.[12]

[11]The problem of induction might be invoked as a reason to deny $\mathcal{GS}$3 for any M, but both the realist and the constructive empiricist should resist such a move. The constructive empiricist must show that $\mathcal{GS}$3 fails especially at the boundary between the observable and the unobservable, but the problem of induction plagues the unobserv*ed* as much as the unobserv*able*.

[12]The fact that it was part of Galileo's strategy gives some reason to try and see it as part of the Galilean Strategy.

Success and truth

Kitcher does not apply $\mathcal{GS}$ in the way I developed in the previous section. Rather, he considers the realist inference from the success of a theory to the truth of that theory. A querist may entertain theories about matters which are *temporarily* unobservable to her; some theories will prove successful, others will not. Later, she can check for herself or confer with others to learn which theories were true and which false. She will find— Kitcher suggests— a strong, positive correlation between success and truth. Just as Galileo's interlocutors could view distant buildings through the telescope and later check the results, the querist notes which theories are successful and later checks to see that those are true. Taking inference from success to truth as M, $\mathcal{GS}1$ is satisfied. It takes this form:

$\mathcal{GS}$1k Inferring truth from success provides correct answers up to and along the vague boundary between the observable and the unobservable.

Of course, 'success' must be understood in a rather strict way. If the querist's only goal is to give up smoking, then the correlation with truth will not be robust. 'Smoking is bad for me' might facilitate success, but so would 'Evil aliens will smite me if I light up again.' Kitcher constrains the type of success under consideration in several ways. First, he considers only success at prediction and at guiding intervention. Second, success must be over a large domain of applications that require fine-grained identification. Third, success must be at error-intolerant tasks. Finally, success should not be secured by compensatory errors. [RR, p. 179] Each restriction bars a way that the success-to-truth inference can fail. Consider a situation in which most any strategy would lead to a successful outcome, in which actual effort would be required in order to fail. In such situations, false theories might still support successful prediction and intervention. These cases are excluded by insisting that the task must be error-intolerant. The other conditions similarly exclude potential counter-examples to the correlation between success and truth. Supposing that all the major counter-examples have been excluded,

$\mathcal{GS}$2 is satisfied for the inference from success *understood in this way* to truth.[13] We might rewrite it perspicuously in this way:

$\mathcal{GS}$2k Whatever reasons we may have for thinking the inference from success to truth would fail when applied to unobservables do not apply to the 'success' considered in $\mathcal{GS}$1k.

Kitcher concludes from the instantiation of $\mathcal{GS}$ that we can infer the truth or approximate truth of scientific theories from their success.

Kitcher's application of $\mathcal{GS}$ has affinities with familiar realist arguments that reach this same conclusion. Realists claim that the best explanation of the success of science is the approximate truth of scientific theories. Yet anti-realists reject inference to the best explanation. Realists reply that such abductive explanation is critical to science, even the bits of science that concern observables. Thus, one might argue that $\mathcal{GS}$1 is satisfied for inference to the best explanation. Yet, the anti-realist replies, abductive inference in science is to the best *causal* explanation. Truth is not the cause of a theory's success. Peter Lipton explains: "...while scientific explanations are typically causal, the truth explanation is not. It is 'logical': the truth of the theory entails the truth of its observed logical consequences, but it does not cause it" [Lip94, p. 93]. This gives us good reason to think that the inference to the best explanation that we can check is different from the inference to the best explantation that we cannot check, so $\mathcal{GS}$2 is not satisfied. As such, $\mathcal{GS}$ will not underwrite inference to the best explanation.[14]

Yet Kitcher does not employ $\mathcal{GS}$ to defend inference to the best explanation. Rather, he defends the inference from success to truth directly. Unlike degenerate debates about abductive warrant, his argument does not rely on an intermediate principle of inference that empiricists already deny. The empiricist

[13]If there are further counter-examples, further monster-barring can ensure that $\mathcal{GS}$2 is satisfied for *some* version of the realist inference.

[14]Realists also claim that if the theories of science were not at least approximately true, then the success of science would be a miracle. Even if $\mathcal{GS}$ could be employed to support inferences from 'P would be a miracle' to '$\neg$P', the realist would have no comfort. Empiricists like van Fraassen insist that false theories could be successful even absent divine intervention.

might deny the legitimacy of $\mathcal{GS}$, but it both has a straight-forward plausibility and is informed by venerable, scientific practice. An obvious objection to $\mathcal{GS}$ can be answered by strengthening its assumptions— adding $\mathcal{GS}3$. It takes this form:

$\mathcal{GS}$3k There is some significant positive reason to think that the reliability of success-to-truth inferences about observables generalizes to inferences about unobservables.

Supposing this three-premise version of $\mathcal{GS}$, the anti-realist can only eschew the success-to-truth rule by showing that one or more of the premises is not satisfied. It may be obvious that the empiricist's best target is $\mathcal{GS}3$. When telescopes and microscopes are pointed at observables or unobservables, they are the same material instrument; as I argued above, this provides *prima facie* reason to think $\mathcal{GS}3$ is satisfied. Yet in the case of successful theories, the theories are not instruments made of the same stuff as one another. They are not made of anything at all. Thus, the presumption of continuity of cases for the microscope cannot be extended to the success-to-truth inference.[15] Why should we suppose that successful theories, even in a narrowly-defined sense, form a unified class of phenomena? Without some positive argument that what holds of successful theories of one sort will hold of other successful theories, the empiricist may refuse to generalize and thus reject the application of $\mathcal{GS}$.

The realist may resist the burden of proof represented by $\mathcal{GS}3$. Just as the difference between the Earth and sky was as irrelevant to the operation of the telescope as the difference between Venice and London— Kitcher suggests— the difference between the observable and unobservable is irrelevant until proven relevant. Kitcher hopes to claim the high ground and set the presumption in favor of realism, but this begs the question against the empiricist. The Venice-London rhetoric does not show that $\mathcal{GS}3$ is the wrong standard, since of course the difference between Italy and England was considered irrelevant to optics for positive

[15]Insofar as theories are linguistic and instruments are causal devices, this parallels the objection that abductive inference to truth is not the same as abductive inference to causal explanation.

reasons and not merely because no one could say why it should be relevant.[16] Do we have comparable reasons to think that the success-veracity correlation will generalize? Kitcher says in summary:

> In a nutshell, realists think that everyday experience supports a correlation between success and truth. They deny that empiricists can simply stipulate the limits of reliability of this correlation. Rather, those limits are to be charted in light of our best overall views about the ways in which the world works. [RR, p. 178]

Yet if $\mathcal{GS}3$ is required for the application of $\mathcal{GS}$, the empiricist need not stipulate anything; reliability is to argued for rather than presumed, and it is the realist who must provide positive reasons for thinking the correlation will remain reliable. Kitcher has realist intuitions, but van Fraassen has empiricist intuitions. More will be needed than that. It will not do for the realist to say that whether the boundary of the observable is relevant or not is "to be charted in light of overall views" about the world, because the empiricist and realist will cleave to different views.

To conclude: $\mathcal{GS}$ is insufficient to support the realists' beloved connection between success and truth. Yet thankfully, the success-to-truth inference is not required to diminish the force of underdetermination. Careful application of $\mathcal{GS}$ provides grounds for believing in unobservables from the amoeba to the moons of Jupiter.

3.1.3 Practice: Does acceptance collapse into belief?

There is a strong intuition that a belief that makes *no difference* is more an affectation or pose than it is a genuine belief. This intuition is reflected in many modern discussions of scepticism.[17] Indeed, van Fraassen acknowledges the intuition. Reiterating the practical and theoretical commitments of constructive empiricism, he reflects,

[16] Galileo and his contemporaries believed that the laws of physics would be invariant across space. More simply, travelling around doesn't seem to effect the behavior of light.

[17] See the citations from Descartes and Reid in the previous chapter. Similar intuitions are expressed by Hume.

> Suppose that in addition to all this I say that I do not believe the theory to be true. Suppose that I am agnostic about whether it is true; it may, as far as I am concerned, be false in respects that do not affect its empirical adequacy. That may certainly sound a bit hollow; what is that reservation? Is it just a bit of lip service to a, you might say, pious agnosticism? [van01, p. 165]

Arthur Fine suggests that constructive empiricism is, at its heart, pragmatism.[18] This pragmatism sees science as aiming at reliability and "believing a theory reliable amounts to trusting it in all our practical and intellectual endeavors" [Fin01, p. 112]. On this reading, there is nothing at stake in belief beyond the reliability of our theories. Thus, the constructive empiricist's protestations not to *believe* theories amount to nothing. Not surprisingly, van Fraassen resists Fine's characterization. He calls for an account of the units of epistemology (a "descriptive epistemology") which would detail the possible doxastic states ranging from full belief, to commitment, to incredulity [van01, p. 165]. Absent such an account, he thinks,

> ...we can say this much. If two propositions are different to the extent that one could be true without the other, and we realize this, then it is possible to believe one without believing the other. If it is possible to distinguish between the observable and the non-observable, then it is possible to distinguish between empirical adequacy and truth. [van01, p. 166]

Of course, van Fraassen is right to insist that one might believe only that a theory is empirically adequate. The question, rather, is whether one can do that while comporting oneself in all respects *just as if* the theory were true. For this worry about "lip service to a... pious agnosticism" to be answered, the constructive empiricist must show what practical difference is made by his refusal to believe. This need not wait on a well-developed account of the units of epistemology, because it need not involve cataloging *all* the differences between belief and acceptance. What one wants is a suggestion of *even one* difference.

[18] Specifically, pragmatism after the fashion of John Dewey, where Dewey is to be understood as neither a relativist nor a realist.

As an historical aside, there is a case to be made that this use of Reid's argument from practice is not one that Reid himself would accept. Considering scientific speculation, he writes, "Let hypotheses be put to any of these uses as far as they can serve. Let them suggest experiment, or direct our inquiries: but let just induction alone govern our belief" [EIP, ess. 2 ch. 3, pp. 251].[19] Yet even here there seems to be a distinction between hypotheses guiding us *as far as they can serve* and hypotheses guiding us *tout court*. Thus, I think even Reid may concede that a claim that guides action *in all respects* should be counted as a belief.[20]

This suggests that the commitment that comes from accepting the theories is weaker than the commitment that comes from believing them because the former is more narrow than the latter in this way: Acceptance involves commitment to a theory as a research programme, but belief involves commitment to using the theory as a guide for action always and everywhere. The notion is that although belief and acceptance might look the same in the lab, we conduct our daily lives where the two come apart— outside of the lab and beyond the reach of research programmes. There are reasons to be dubious of this suggestion. For one, realists need not be committed to taking our best theories as guides for action *always and everywhere*. A realist may admit freely that some theories, although true, are too cumbersome to be used in practical applications. To use a well-heeled example, star charts are still produced as if the universe were geocentric; we know that it is not, but more precise charts would make navigation more rather than less difficult. Beyond its inherent implausibility, this suggestion would be a poor reading of van Fraassen. He writes:

> If I accept a theory then I believe that it is empirically adequate, and I also commit myself to seeing nature through that theory's eyes. Thus, in addition to that belief in the theory's empirical adequacy, there is a pragmatic aspect to acceptance. Nature is confronted and/or appreciated within that theoretical framework, the theory guides experimental

[19] The citation is from Reid's *Essays on the Intellectual Powers* of 1785 and the page reference from his *Philosophical Works* [Rei67].

[20] One might instead simply say that there is a tension between Reid on hypotheses and Reid against the sceptic. Even though there may be some tension, I think the issue is rather more subtle— I will return to Reid's treatment of scientific hypotheses in the next chapter (§4.3.2).

> design and new projects for observation, new theories are required to be compatible with it, and so forth. These assertions express commitment rather than belief, though there is obviously some sort of coherence connection between a commitment and opinion about its chances of being vindicated. The accepted theory is thus the guide both to theoretical and practical life. [van01, p. 164]

Perhaps I am reading this passage too strongly; perhaps there is still some practical difference between acceptance and belief.

Imagine the constructive empiricist points to a way that his life is different from the realist's. This difference might be a matter of mere preference, such that standards of scientific judgement would not decide the matter. The constructive empiricist faces a dilemma, however. If the difference is so small as to be of no consequence, then it may be insufficient to distinguish acceptance from belief. Conversely, if the difference is significant, then standards of reasonable judgement may well favor one over the other. Can the constructive empiricist resolve this dilemma? Without knowing what difference constructive empiricism would make to our lives, it is unclear how to answer that question. The burden is on the empiricist, I think, to show some difference that empiricism makes in his life. And then we shall see.

The units of epistemology

The argument above was meant to show that we need not have a full account of the units of epistemology to see whether constructive empiricism is defeated by the argument from practice and, thus, that van Fraassen's call for "descriptive epistemology" is an argumentative *non sequitur*. I have neither shown nor did I mean to show that an enquiry in the units of epistemology would lack value. Surely, as van Fraassen notes, belief is not merely a binary variable associated with propositions— not merely a matter of propositions being in or out of a belief box. There are several puzzles about what *choosing a theory from a set of rivals* might mean. One puzzle involves the difference between belief as binary (one believes or does not) and degree of belief as a continuous variable (one has confidence p such that $0 \leq p \leq 1$).

Another puzzle involves the difference between believing a claim and accepting it for practical purposes. It is certainly possible to accept a claim for some purposes without believing it— it may be expedient and *close enough* but not strictly true. It is also possible to believe a claim without employing it in putatively relevant contexts— the truth may be computationally intractable, insufficiently specific, or simply *too much trouble*. The example of a navigator's star charts illustrates both; that is, the charts are drawn *as if* the stars were on a sphere with the Earth at the middle by people who *believe* that the stars are in a complicated three-dimensional arrangement and that the Earth orbits the Sun. One begins to solve this puzzle by noting, first, that if I accept a claim only for some purposes then my reliance on it should be confined to pursuing those purposes regardless of the claim's expediency when I am pursuing other purposes and, second, that if I believe a claim then I will rely on it except when it is too cumbersome.Although working posits may displace beliefs in application, working posits are presumptively local while beliefs are presumptively global. Of course, this is at best the beginning of an answer. Evaluating whether an agent believes a claim seems to require assessing the truth of counter-factuals like this: Would the agent rely on the claim in such-and-so contexts if it were feasible to do so? Questions like these can be hard, perhaps impossible, to answer. Where the constraints on feasibility are formal, computational constraints, the antecedent of the counter-factual might be seen as entailing a contradiction.

The puzzle of how belief relates to practice *in the details* is an interesting and important one. The argument from practice relies not on the details, but only on a *gross feature* of this relation. Resolving the puzzle would require an account of the units of epistemology of the sort that van Fraassen demands. The argument from practice, rather than requiring such an account, puts a constraint on what it might look like. A developed account of the units of epistemology should not distinguish between cognitive commitments that could make *no possible* practical difference, regardless of whatever else the account says about the relation between cognition and practice. The demand for an enquiry in the units of epistemology, although legitimate, does not save constructive empiricism.

These remarks are at best preliminary. Since the topic here is underdetermination, I beg the reader's forgiveness for not tilting at *all* the windmills.

3.2 The road from empirical equivalence

The inference *to* constructive empiricism can be seen as just one of many arguments *from* empirical or observational equivalence. Indeed, the 'problem of underdetermination' is sometimes used interchangeably with the 'problem of empirically equivalent theories.'[21] The crux of underdetermination *qua* empirical equivalence may be expressed as a syllogism in the figure *Celarent*:

1. No theory with empirically equivalent rivals merits belief.

2. All theories have empirically equivalent rivals.

3. ∴ No theory merits belief.

The conclusion of this argument is the promise of underdetermination. The major premise concerns what merits belief, so let's call it the *belief assumption* and abbreviate it *EEbel*. The minor premise guarantees the existence of suitable rivals, so let's call it the *rival assumption* and abbreviate it *EEriv*. Rewriting the argument in a less scholastic form:

EEriv Every theory has indefinitely many empirically equivalent rivals.

EEbel There is no good reason to believe a theory over its empirically equivalent rivals.

[21] This short-sightedness is distinct from but usually accompanies the view that underdetermination is merely a play in the game between realists and anti-realists. I have said something about that other conflation already in §1.6.1.

∴ Therefore, there are never decisive reasons to believe any theory; that is, theory choice is always underdetermined.

This may readily be assimilated to the framework developed in Ch I. *EEriv* states that for any theory there are rival theories of a particular sort. This gives us a set of rivals for the underdetermination scenario. *EEbel* states that permissible standards of judgement are insufficient to distinguish between empirically equivalent theories. The conclusion brings these two together. The choice among the set of rivals in *EEriv* is (by *EEbel*) underdetermined. The implicit scope here is all observationally-possible circumstances— as we'll soon see, difficulties delineating observationally possible circumstances are legion. The ultimate conclusion that we may accept but not believe successful theories turns on a substantive principle about what we should do in the face of such underdetermination. It assumes that *agnosticism* is the appropriate response to underdetermination. (On agnosticism, see §1.3.3.)

The conclusion of the argument does not entail that an arbitrary pet theory can be held come what may, of course, since there may be preferable rivals which are empirically differentiable from the pet theory. Yet, the conclusion does entail the ubiquity of a certain sort of underdetermination. If belief in theories is unjustified, then it is not the appropriate honorific to confer upon our best theories. We may responsibly believe in the empirical adequacy of our best theories, though not in the theories themselves. Crudely put: Underdetermination is everywhere, therefore we should not believe any of the theories to which we commit ourselves.

Without pursuing niceties, I will suppose that the argument is either valid or can be made valid without substantial revision. I argue instead that the argument is unsound; its premises are, if not false, at least not true in any straightforward sense.[22]

[22] My general conclusions accord with Laudan and Leplin [LL91].

3.2.1 Cheap proofs

One might attempt to disprove *EEriv* on formal grounds. Consider a theory T and suppose that the set of T's empirically equivalent rivals is finitely specifiable. We could then replace T with the theory $E(T)$ which is true if either T or one of its empirically equivalent rivals in true.[23] The possibility of $E(T)$ shows that *EEriv* is not true for the general case, since $E(T)$ would have no empirically equivalent rivals. *EEriv* will also fail for any theory lacking non-empirical content. Regardless, most theories of interest *do* have non-empirical content. Similarly, it seems farfetched that the rivals to complicated, scientific theories will be finitely specifiable.

One might instead attempt to provide a formal proof of *EEriv* by providing an algorithm which, for any theory T that has non-empirical content, generates an empirically equivalent rival T'. A typical algorithm is one that defines T' to mean that everything appears *as if* T were true but nevertheless T is false. By definition, appearances would not decide between T and T'.[24] Some authors have objected to algorithms of this sort on the grounds that T' is *not really a theory* (e.g., [LL91, pp. 456–7] [LL93, p. 11]). Various criteria have been proposed for distinguishing real theories from pseudo-theories. Consider a couple: One proposed criterion is that real theories should be *non-parasitic*; T' is parasitic in the sense that it is defined in terms of T. However, we might express T' in a way such that it is non-parasitic. We need only substitute whatever details appear in T for the mention of T in the definition of T'. A second proposed criterion is that real theories are the sort of thing that scientists take seriously; scientists don't take possibilities like T' seriously. This criterion is poorly motivated. Without some explanation as to *why* scientists don't take T' seriously, we can't say whether they discount it for legitimately *scientific* reasons. The mere fact that some behavior is common among scientists should not be taken as a normative justification for that

[23] If the set of rivals were finite, then $E(T)$ would be the disjunction of the rivals.
[24] Kukla refers to this algorithm as "Van Fraassen's favorite" [Kuk98, p. 59].

behavior— scientists make mistakes.[25]

The attempt to debar T' as a pseudo-theory harkens back to the halcyon days of logical empiricism, when rivals could be dispatched by the charge that they were pseudo-scientific, so we might exhume other criteria to exclude variants of T' from consideration.[26] I suggest that we instead permit theories like T' as potential rivals, allowing that underdetermination of some sort may obtain between T and T'.[27] If *EEriv* is proven in this way, however, *EEbel* becomes implausible. Recall the case of Cartesian underdetermination from Ch II. It will suffice to contrast the theory that we live in houses on Earth, that we read books, and so on (call this T) with the theory that it merely appears as if this were so (T'). It is not clear that even Descartes would have taken T' seriously, since each of his sceptical rivals proposes some mechanism by which it comes to appear as if T were true: madness, dreams, or malign agency. Nevertheless, T and T' are in some sense empirically equivalent theories— if we cannot decide between them now, then no further empirical enquiry will drive a wedge between them. Yet we do have good reasons for believing in our lives on Earth rather than in some grand dissimulation. (Recall Reid's arguments from §§2.3–2.5.) If *EEbel* denies that, then it is false.

Even if we were genuinely worried by Cartesian concerns, the reduction of underdetermination to such a familiar problem can tell us something. It tells us that no revolutionary rethinking of science will follow from underdetermination of this kind. The reason is two-fold: First, Cartesian scepticism has been openly debated for centuries. It seems plausible to think that our present thinking about science already accomodates such classic worries. Second, Cartesian scepticism says nothing especially about *science*. All knowledge is on the chopping block. As Kyle Stanford suggests, "empirical equivalents have proved to be a Devil's bargain for advocates of underdetermination— providing convincing evidence of an underdetermination predicament only where they have transformed the problem

[25] See the example of wave and matrix mechanics discussed in §1.2.3.

[26] See also Kukla [Kuk98, pp. 66–80], who surveys other criteria and vigorously defends the theoretical legitimacy of rivals like T'.

[27] This reflects the conclusions of §1.2.

into one or another familiar philosophical puzzle" [Sta01, p. S11].[28]

3.2.2 Characterizing the empirical

Note that the notion of empirical equivalence either defines or is defined by a notion of empirical difference— two theories are empirically-differentiable if they make different claims about some observables. Thus, making sense of $EEriv$ requires specifying the boundary of what is observable.[29] The observable, as van Fraassen defines it, is what can be detected by unaided human perception. Theories are empirically equivalent if they agree on all the observables or, in the model-theoretic idiom, if they share an empirical substructure. There are problems even with determining the extension of 'observable' in this sense. Electrons are not observable, because neither you nor I can see, hear, or taste them. Concede, for the sake of discussion, that there is thus some empirically equivalent rival physics that does not posit electrons. As Paul Churchland [Chu85, pp. 43–4] notes, creatures with electron microscopes for eyes could see electrons and, one supposes, if *they* were constructive empiricists then they would believe in electrons. Well and good for them, van Fraassen replies, but we may "assume that we (the epistemic community) are all humans, and no one of us is really a person from Krypton, like the comics' Superman, who could see Lois Lane's pink underwear when she was fully dressed" [van85, p. 254].

The point is that these creatures are only troubling insofar as we imagine watching them without admitting them into the epistemic community. We imagine recognizing them as observers and scientists, yet we also imagine the indignity that, by the rules of constructive empiricism, they are allowed to believe in things in which we are not allowed to believe. Thus, we imagine them as legitimate members of the scientific community at the very moment that we consider them as outsiders to the scientific community— van Fraassen writes, "The example as given tempts

[28] I turn to Stanford's positive argument in §4.2.2.

[29] Noting this connection, Churchland identifies 'empirically equivalent' and 'empirical adequacy' as "cognate relative" terms [Chu85, p. 38].

us to confuse two cases" [van85, p. 256]. I think van Fraassen misdiagnoses the problem; any such confusion is inessential to the example. Although humans have not got electron microscopes for eyes now, engineers might develop such upgrades. Suppose, for the sake of discussion, that in the year 2025 I will have my left eye replaced with an eye that can see electrons. The constructive empiricist believes that atomic theory will be empirically adequate for all observations. It will be adequate only if there are *in fact* electrons because, beginning in 2025, a member of the community (viz., me) will make direct observations of electrons. Thus, it seems to follow that the constructive empiricist should believe in electrons.

Churchland's version of the thought experiment turns on an analogy between imagined outsiders with natural electron-eyes and we scientists with our electron microscopes. Van Fraassen objects that the analogy begs the question by supposing that we with our microscopes are alike "in all relevant respects" to the outsiders [van85, p. 257]. Since that similarity can only be judged on the basis of our scientific theories about microscopes and outsiders, the constructive empiricist who believes only that those theories are empirically adequate will say only that our situation and theirs is empirically indistinguishable. Thus, with our electron microscopes, "all the observable phenomena are as if we are observing" electrons [van85, p. 258]. Yet this response does not address the revised version of the thought experiment, since it would be perverse to tell to me in 2025, when I have my electron-eye, that I cannot really see with it and that it is only *as if* I could. If you could say that to me then, what stops us from saying that now to people whose keen vision is due to laser eye surgery?

The constructive empiricist is free to object that "we (the epistemic community) are all humans" and that *humans* cannot see electrons [van85, p. 254, cited above]. If I see electrons after 2025, then I am no longer (fully) human. This might be plausible, especially if we imagine the electron-eye as extracted from a genetically-engineered electric fish. Yet the relevant question is not whether I would still be *human* but whether I would still be part of the *epistemic community*.

I see no compelling reason to suppose that having an electron detector installed in one eye socket should make me ineligible for participation in the epistemic enterprise. In any case, the same point can be made without imagining surgical modification. Blind people who employ seeing-eye dogs, I suppose, are justified in considering them as sources of observations and not merely as instruments. Thus, we may imagine that in 2025 I will have a sensing-electron fish that extends the boundary of what I can observe.

The constructive empiricist may still reply: The mere *possibility* of these scenarios is insufficient to show anything. Empirical adequacy is not about what would obtain in exotic, counter-factual circumstances. It is about what, *in fact*, will happen in the course of history. As van Fraassen explains, "empirical adequacy concerns actual phenomena: what does happen, and not, what would happen under different circumstances" [van80, p. 60]. This reply lacks force because we are not spectators to history and because what will happen is sensitive to our choices. Whether I have an electron-eye in 2025 will depend in part on whether we try to develop the technology and whether we dedicate resources to the effort. The constructive empiricist believes only that atomic theory is empirically adequate; determining whether the empirical adequacy of atomic theory implies the existence of electrons requires determining whether electrons will in time be observable; the answer to this latter question depends on our choices; thus, whether the constructive empiricist should believe in electrons depends on our choices. Therefore, whether we should believe that electrons *exist*, for the constructive empiricist, will depend in part on how much we want to observe them. For any empirically adequate theory, we could try to revise the human organism or the scientific community in such a way that the entities in that theory would be observable.

The constructive empiricist already allows a considerable rôle for volition in belief choice; on van Fraassen's account, querists must decide how much risk to accept and thus whether to believe theories (very risky) or merely believe them to be empirically adequate (less risky). He thinks that querists must decide who counts as their epistemic community. Yet as I've tried to show, querists must also decide what technological program to pursue and in this way decide how the chosen epistemic community will develop. We ask the constructive empiricist, 'Do electrons exist?' He must reply 'It depends. How much do we want them to exist?' This glib response should not be misunderstood as a promise of constructivist wish-fulfillment. An entity would not be observable *merely* because we wanted it, but only if that motivated us to reengineer ourselves such that we could observe it. This does not show that empiricism is *untenable*, but it does show that it makes possible a kind of round-about wish fulfillment. This possibility makes empiricism rather more bizarre than the empiricists' rhetoric of epistemic modesty would suggest.

Unhappy with this situation, the empiricist might suggest a different principle by which to divide observables from unobservables. The most basic optical microscopes operate on principles similar to the human eye, but higher power optical microscopes and electron microscopes do not. One may think that observable entities are ones that can be detected using processes analogous to the ones used in direct, unaided perception. In a careful development of this criterion, Sara Vollmer concludes that "the observation of any entity that utilizes the physical principle of the scattering of a wave and the application of an inverse Fourier transform to form an image of the object can have the same epistemological status as the observation of any other entity made in this way" [Vol00, p. 365]. This does provide some boundary— individual electrons are unobservable on this criterion— but "whenever we observe by this principle of scattered waves, whether by telescope, optical microscope, electron microscope, or x-ray crystallography, we observe in a way that is of a kind with ordinary visual observation" [Vol00, p. 363]. Things visible through electron microscopes are van Fraassen's paradigm case of the unobservable; yet if the distinction is drawn in this way, they will count as observable. Moreover, whether unaided, human perception is confined to "the application of an inverse Fourier transform" depends on what human organisms are like; this, as I have argued, depends in part on our own choices.

Observability historicized

One may concede that observability includes all the detections we might make with instruments and that this is historically variable. Empirical equivalence could then be indexed to our *present* capabilities. Consider a present theory T and suppose that it is (now) empirically equivalent to a rival T'. Our choice between these theories is (now) underdetermined. In the future, our capabilities may change in ways that allow us to decide between them— the choice would cease to be underdetermined.[30] So the *scope* of the underdetermination includes our prior circumstances, present circumstances, and some but not all possible future circumstances.[31] Although in some future circumstance we may look back and see how the choice between T and T' should have been made, one intuition is that we should take no comfort in that possibility.

Numerous examples of this sort can be found in the history of science. In the early 19th century, it seemed impossible to discern the composition of the stars. Many theories of stellar composition would have been empirically equivalent. Yet with the development of spectrographic methods, the theories became nonequivalent and it was possible to determine what stars were made of. The situation led C.S. Peirce to declare,

> The history of science affords illustrations enough of the folly of saying that this, that, or the other can never be found out. Auguste Comte said that it was clearly impossible for man ever to learn anything of the chemical constitution of the fixed stars, but before his book had reached its readers the discovery which he had announced as impossible had been made.[32]

Even though scientists in 18th or early 19th century would have been wrong to think that the matter could never be found out in some timeless sense, they could

[30] Laudan and Leplin take empirical equivalence or nonequivalence to be a timeless relation between theories and thus conclude that the variability of what is observable completely undercuts empirical equivalence [LL91, pp. 452–3] [LL93, p. 9].

[31] Note that van Fraassen's empiricist scruples make him eschew modality [van80, p. 197]. Considering only our *actual* future, though, would reintroduce the problems of choice discussed above.

[32] [HW29, §6.556] as quoted in [Res98, p. 187]; see also [Pei97, p. 273]. Interestingly, Comte *celebrated* the alleged impossibility [Sch95, pp. 262–3].

not say how it would be found out or what the composition of stars would turn out to be. Should the scientists have despaired at the (transient) empirical equivalence? John Earman suggests that they should have,

> For it is cold comfort to tell the scientists who were in the former epistemic context that if their situation had been different then they would have been able to gather evidence that would decide among the [hypotheses]. [Ear93, p. 34]

Perhaps it would be a cold comfort, but only a self-deceived epistemic enterprise could find warm fuzzies everywhere. For any underdetermination of less-than-maximal scope it will be true that, if our situation were different, we could find evidence that would decide among the hypotheses. Consider two sorts of cases: (1) Where prior scientists faced such underdetermination, they could have tried to change their circumstances by developing new techniques or instruments.[33] It is an ordinary enough thing not to have the resources *just now* to settle a question, but scientists can apply themselves to acquiring and developing new resources. (2) Alternately, scientists may have had good, systematic reasons to think that no new techniques would resolve the underdetermination. That they had systematic reasons shows that the underdetermination was only so discouraging in the context of a great many background theories— it was one case of underdetermination among a great many successful determinations.[34] The case of stellar composition illustrates both. Scientists in a context where determining the composition of stars was impossible developed techniques that made it possible; before 1800, say, one might reasonably have said that the composition of stars would forever remain a mystery. Neither situation should give us chills.

3.2.3 Verdict on empirical equivalence

The argument from empirical equivalence fails because of a fundamental shortcoming: Explications of empirical equivalence that make *EEriv* plausible

[33]It may add little for us to tell them that their efforts will succeed, since they are now characters from our history. Regardless, it cannot be that distance that makes our reassurance cold comfort.

[34]More cases of this sort are discussed in the next chapter.

undercut *EEbel*.

From Ch II: The rivals may be provided as sceptical scenarios like dreams, demons, or whatall else, but this makes it nothing more than Cartesian scepticism. The previous chapter develops resources for answering Cartesian scepticism, but the argument from empirical equivalence misfires even if the sceptic wins out. Although the underdetermination argument is introduced to say something about science especially, Cartesian scepticism would sweep away all empirical claims.

From §3.1.2: *EEriv* might be established by defining empirical equivalence relative to the discriminating power of specific capacities— the limits of our natural senses, for instance. The limits of these capacities will be fuzzy, however, and by using the Galilean Strategy we can exploit this to justify instruments and methods that reach beyond those limits. The Galilean Strategy can give us good reason to believe one of the theories over its rivals, falsifying *EEbel*.

From §3.1.3: If the rivals are defined such that choosing one or the other can make no difference at all, then the argument from practical commitment defuses the alleged rivalry. *EEbel* is not actually falsified, since we are not given a reason to believe one rival over the other. Instead, the problem is ill-posed, and *EEbel* is shown to be irrelevent.

From §3.2.2: The notion of empirical equivalence (considered *sub specie aeternitatis*) depends on our choices in a problematic way. This may be avoided by pegging equivalence to a specific time and community, but at the cost of weakening the resulting underdetermination. Either way, *EEbel* is implausible.

Strictly speaking, these arguments do not show that there is no sense of 'empirical equivalence' that can satisfy both *EEriv* and *EEbel*. It is difficult to imagine what this sense might be, however, and the arguments exemplify general strategies for defeating candidates. Perhaps philosophers' infatuation with empirical equivalence is a hangover from logical empiricism. Disregarding the headache, it is neither the only nor the most interesting form of underdetermination.

3.3 The road from auxiliaries

The familiar Duhemian argument for underdetermination begins with the observation that experiments in modern science often require appeal to auxiliary assumptions for their probative force. For the sake of concreteness, consider the claim that the Earth is flat and the counter-claim that the Earth is round— less colloquially, that the Earth is an oblate spheroid. Call these claims T_F and T_R respectively. There have been many adherents of T_R, of course, and many attempts to demonstrate its superiority over T_F. Copernicus provides a typical argument:

> This [spherical] form of the sea is also discerned by sailors, seeing that land is visible from the top of the mast, even when it cannot be seen from the deck of the ship. And conversely if a light is held on the top of the mast, it appears to those on the shore to gradually descend as the ship moves away from land, until at last it disappears like the setting sun.[35]

The idea is simple enough. If the sea were flat, then an observer who could see a ship clearly should be able to see both the hull and the mast, as in figure 3.1a. Contrariwise, since the sea is curved, an observer may see the mast even at a distance at which the hull is not visible, as in figure 3.1b. The latter of these is observed, and the observation decides between these two depictions. Nevertheless, an implicit assumption is embodied in figure 3.1. Light is depicted as travelling in a straight line, but of course the rectilinear propagation of light is independent of T_F and T_R.

Without that assumption, the observation may not favor T_R. Suppose T_F is true— the Earth is flat— but that light sags slightly between the object and the observer, curving down toward the surface of the Earth. At a distance, the light from the hull of the ship may sag down into the water while the light from the mast reaches the observer. Thus, the observer sees the mast even as the hull has passed from view. This situation, depicted in figure 3.2, would yield the relevant

[35] Book I Ch II of *De Revolutionibus*. The translation is my own.

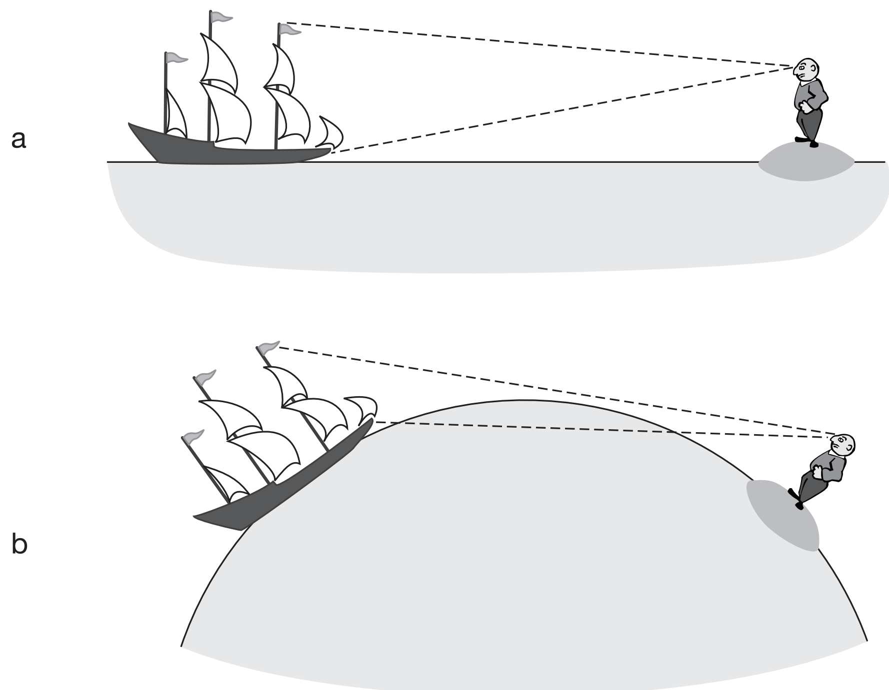

Figure 3.1: (a) If the Earth were flat, then an observer on the shore would see both the mast and prow of the ship if he could see either. (b) Since the Earth is round, the observer sees the mast even when the hull is occulted by water.

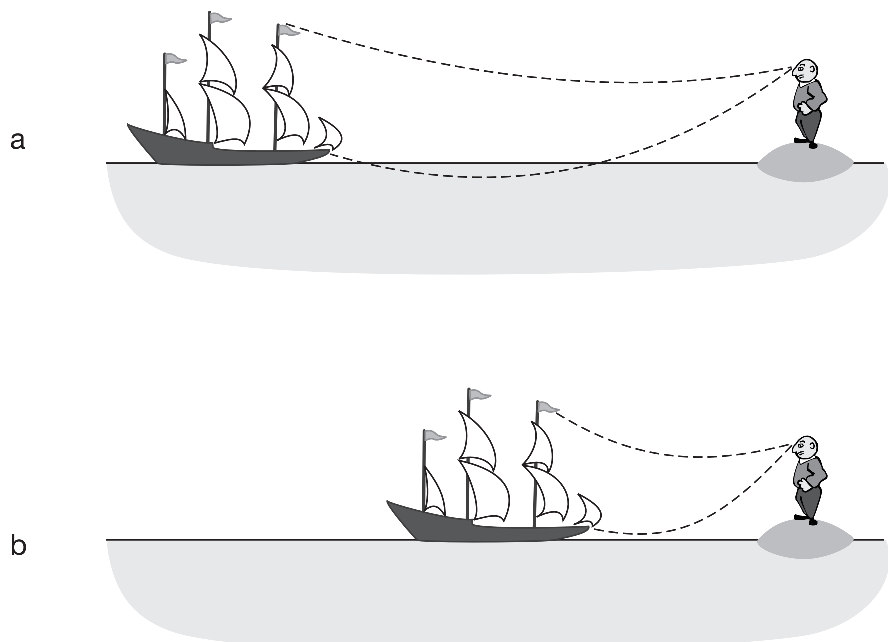

Figure 3.2: (a) Light beams sag between the ship and the observer, so the prow of the ship is occulted by water even as the mast is visible. (b) As the ship approaches, the observer can see both the prow and the mast.

observation.[36]

Call the assumption that light travels in a straight line T_L, and call the observation of the mast of the ship when the hull is out of sight O. O is offered as evidence of T_R over against T_F, but the best it can do is show that *if* light travels in a straight line *then* the Earth is round. One may conclude that this conditional is true, but not that T_R is true or that T_L is false.

Cases like this are used to underwrite what is sometimes called the Duhem-Quine (or DQ) Thesis that theories are not tested in isolation; as Quine puts it, they "face the tribunal of sense experience not individually but as a corporate body" [Qui53, p. 41]. The point may be stated as a lesson about underdetermination. The experiment was aimed to decide between T_R and T_F. This theory choice is underdetermined for a standard of judgement that denies you the assumption T_L; such a meager standard allows you only to conclude only $T_L \rightarrow T_R$. Worse still— since there is a great deal more to optics than the rectilinear propagation of light— the inference involves still other auxiliary assumptions T_M, T_N, and so on. If the DQ Thesis is correct, then the observation allows us only to conclude the rather uninteresting conditional $(T_L \& T_M \& T_N \& \cdots) \rightarrow T_R$.

So, O only yields T_R given an indefinite number of other assumptions, where the yield is understood as deductive entailment. We might have arrived at this conclusion directly. Let L be the set $T_L, T_M, T_N, \ldots$. By hypothesis, $(O \& L) \rightarrow T_R$, and there is no $M \subset L$ such that $(O \& M) \rightarrow T_R$. We observe O. These assumptions validly entail $L \rightarrow T_R$ but leave T_R indeterminate. Suspiciously, the conclusion follows without any consideration of the content of L, O, and T_R and without any reflection on methodology or confirmation.

The crux of the matter is whether standards of responsible judgement should lead you to assume L or treat it as being as much in question as T_R. One might argue that the right standards of judgement are timeless and unchanging.

[36]This example appears in Copi and Cohen's introductory logic, wherein the authors attribute it to C.L. Stevenson. They invoke it to show that no 'crucial experiment' can be deductively binding, but concede, "Within the framework of accepted scientific theory that we are not concerned to question, a hypothesis *can* be subjected to a crucial experiment" [CC90, p. 447].

T_L is a substantive, empirical principle and hence open to revision, and so we should not bind the timeless, canonical method to it. I am dubious as to whether method should be timeless, but it is enough to note that method might allow us to rely on 'well-confirmed background theories' *de dicto* without being committed to T_L *de re*. One might argue instead that the right standards should promise us certainty. T_L is open to revision, so conclusions drawn on the basis of it are *a fortiori* fallible. Since no certain knowledge is to be had, this will not do either.[37]

Whether we may rely on auxiliary hypotheses to decide between rival theories depends on their actual content and on our epistemic situation. Little more can be said in the abstract. The Duhemian, top-down argument seems to fail.

3.3.1 The Duhem in the Duhemian Argument

Although Quine is often cited as having established the force of underdetermination, in 'Two Dogmas of Empiricism' he writes that the "doctrine was well argued by Duhem" and offers it without much positive argument [Qui53, p. 41 fn. 17]. Admittedly, Duhem does seem to draw a rather strong conclusion. He writes in summary "that comparison is established necessarily between the *whole* of theory and the *whole* of experimental facts..." [Duh54, 208, italics in original].[38] It's important to note that this passage is a quick summary of his position, offered after it had been developed with greater care in prior sections. Moreover, Duhem did not see his holism as entailing any pernicious underdetermination. It does mean that theory choice cannot be a matter of deductive or logical certainty, but it leaves room for fallible theory choice. Duhem explains that "what impels the physicist to act thus is *not* logical necessity. It would be awkward and ill inspired for him to do otherwise, but it would not be doing something logically absurd..." [Duh54, p. 211]. Theory choice is not a matter of deduction, surely. It lacks even the plausible pretense of certainty. Duhemian concerns show us that, given a de-

[37] The demise of certainty is a modest lesson of the demise of Cartesianism; see §2.5.

[38] The critical turns in Duhem's argument occur in his Ch VI §§2–3, 8.

contextualized standard of judgement, underdetermination is rampant. If there are standards of good sense that allow querists in a context to decide between theories, as Duhem thought there were, then the underdetermination disappears when we consider choices relative to those standards. Duhem is, I concede, not always as clear on this point as he could be, and commentators have often recapitulated the ambiguity. For instance, Laudan treats what "is known to be true" and what can "carry logical weight" as issues of whether "a scientist is forced to relinquish" an hypothesis. If the responsible theory choice is the choice that a scientist is *forced* to make, underdetermination will be ubiquitous. Yet Laudan also allows for responsible choice in a less draconian sense; he concedes that an experiment he considers "would cause a rational person to cease to expound [the hypothesis]" and that giving it up "might be more prudent" than holding to it [Lau65, p. 299].

Duhem thinks that "good sense" should save the physicist from awkwardness and ill inspiration, but also that

> these reasons of good sense do not impose themselves with the same implacable rigor that the prescriptions of logic do. There is something vague and uncertain about them; they do not reveal themselves at the same time with the same degree of clarity to all minds. Hence the possibility of lengthy quarrels between the adherents of an old system and the partisans of a new doctrine, each camp claiming to have good sense on its side, each finding the reasons of the adversary inadequate. [Duh54, p. 217]

This reveals possibilities for underdetermination. On the cusp of controversies, the evidence will be insufficient to settle matters between rival camps— not because many scientists are undecided between rival views, but because good sense is vague enough to permit disagreement. Yet, new evidence is collected, old evidence is reconsidered, and each doctrine is run through its paces. In time, the question may be settled. There is not some instant in time before which the old theory is *the* reasonable choice and after which the contender is triumphant, but agreement may be secured by an array of new evidence along with the inconstant nudgings of good sense. Note, however, that this agreement may come about even though the theories in question still rely on auxiliaries, and reasonable disagreement may occur even when the interlocutors agree on the relevant auxiliaries. There is a kind of underdetermination that, as Duhem might say, follows from the vagueness of good sense, but it is neither ubiquitous nor established by scientists' reliance on background theory.

3.3.2 Turning the tables

In the last decade or so, the prevalence of background assumptions has underwritten arguments *against* underdetermination— principally in discussions following Laudan and Leplin [LL91].[39] Scientists utilize a host of auxiliary assumptions and collateral information in performing experiments, as is readily seen by considering examples like the one in the previous section. Suppose, then, that two theories L and L' make no predictions that would allow us to differentiate between them. This empirical equivalence might be taken to warrant a conclusion that the choice between L and L' is underdetermined. What would the scope of this underdetermination be? It would include our present circumstance, but we may imagine circumstances it would not include. Suppose we learned that $(L \rightarrow O)$ and $(L' \rightarrow \neg O)$ for some observable phenomenon O. The theories would not then be empirically equivalent, and our choice between them would not be underdetermined on that count.[40]

Of course, suitable revision of the rival theories would repeat the underdetermination at the level of theory cum background theory. We would be able to decide between L and L', but the choice between $L\&(L \rightarrow O)\&(L' \rightarrow \neg O)$ and $L'\&(L \rightarrow \neg O)\&(L' \rightarrow O)$ would remain underdetermined. Yet why do we believe $(L \rightarrow O)$ and $(L' \rightarrow \neg O)$? Surely not merely because they would defuse the underdetermination between L and L'! Say that we believe them because they are entailed (with some assumptions about initial conditions) by a well-

[39] Although the argument is not original to Laudan and Leplin, they have pressed it with the greatest vigor. Boyd considers it a "standard rebuttal" to empiricism, but thinks it can be effectively countered by shifting attention to Total Sciences [Boy82, pp. 650–1]. See also [Chu85, p. 38].

[40] I described this briefly and for different purposes in §1.3.1.

tested and widely-believed background theory X. We may attempt to cook up some alternate X' that would enjoy the same empirical support as X but entail $(L \rightarrow \neg O)\&(L' \rightarrow O)$, but there is no guarantee that this will be possible. The theory X may have systematic connections to the whole body of science, such that any X' sufficiently different to work here would introduce a panoply of anomalies.

Earman objects to Laudan and Leplin's argument in this way: Either the X appealed to is a contestable hypotheses like L and L' or it is not. If the former, then the rivals under consideration are no longer L and L'— the rivals are instead $(L\&X)$ and $(L'\&X)$. "The result is sidestepped…but that is changing the subject since what counts as the hypothesis has been changed." If the latter, then X is presumed. This would amount to dogmatism, since auxiliaries like X "must go beyond the empirical evidence…and thus their epistemic status will be just as open to question as that of the [hypotheses]." [Ear93, p. 35]

Earman may be right that this changes the subject, but that would *not* be legerdemain. A determinable theory choice may be substituted for an underdetermined one, a tractable problem for an insoluble one. The problem is soluble, though, just because X does not stand in the same need of justification as L or L'. The auxiliary goes beyond the evidence and so too is open to question— it is not in principle shielded from scrutiny.[41] Nevertheless, a community or an individual scientist may hold it fixed for the purpose of some investigation.

3.3.3 Total Science

We might ask about the choice not between theories but between packages of *Total Science*. A Total Science is the collected body of *all* scientific knowledge at a time.[42] Adding a theory T to a given Total Science $\mathcal{S}$ produces a new, different Total Science. Thinking of theories as sets of propositions, one might think of a Total Science as the union of all the theories known to science and think of the

[41] Going beyond evidence is actually beside the point, since querists may call the evidence itself into question as needed.

[42] Some authors call this a 'total theory.' This is at best misleading; as I argue below a Total Science is not a scientific theory in any ordinary sense.

combination as $\mathcal{S} \cup T$. Thinking of theories instead as sets of models, one might think of a Total Science as the intersection of known theories and think of the combination as $\mathcal{S} \cap T$. In order to remain neutral between these and more exotic possibilities, let $(\mathcal{S} \oplus T)$ stand for the resultant Total Science when theory T is added to Total Science $\mathcal{S}$.

Let the initial state of Science prior to any knowledge of L, L', or X be given by $\mathcal{S}_i$. The choice between $(\mathcal{S}_i \oplus L)$ and $(\mathcal{S}_i \oplus L')$ is, by assumption, underdetermined. *This* underdetermination cannot be resolved by appealing to X. Appeal to X is a non sequitur, since X is not part of the Total Science $\mathcal{S}_i$. If we learned X from some experiment, then we would transition to a different total science. The choice would be between $((\mathcal{S}_i \oplus X) \oplus L)$ and $((\mathcal{S}_i \oplus X) \oplus L')$. That choice would not be underdetermined, true, but that is a different choice between different Total Sciences. Perhaps the choice among $\{(\mathcal{S}_i \oplus L)$, $(\mathcal{S}_i \oplus L')$, $((\mathcal{S}_i \oplus X) \oplus L)$, $((\mathcal{S}_i \oplus X) \oplus L')\}$ is not underdetermined, but that too is beside the point. On this approach, strong conclusions are drawn from underdetermination that obtains between empirically equivalent Total Sciences.

Several authors appeal to Total Science in this way. Quine said, "The unit of empirical significance is the whole of science" [Qui53, p. 42]. Hoefer and Rosenberg, mindful of Quine, write that "the thesis of underdetermination of theory by evidence is about empirically adequate total science…" and conclude rightly "that Laudan and Leplin's arguments for the defeasibility of empirical equivalence have no application in the context of systems of the world"— that is, in the context of Total Sciences. [HR94, pp. 594, 598][43]

This can be seen as a reformulation of the Duhemian argument, one that cannot be answered merely by an appeal to fallibilism. Even allowing that it is legitimate to appeal to background theories, empirically equivalent Total Sciences cannot be distinguished by some empirical test because all of the available background theories are already included in each Total Science. In Ch I, I defined

[43] See also [Kuk98, pp. 63–66]

underdetermination obtaining for the choice among rival *theories*. Choices among Total Sciences fits awkwardly into this schema, but— I argue— this is to the credit of the schema.

There is something suspicious about the notion of Total Sciences. Querists never face choices between them. In actual enquiry, there is some matter in question and other matters presumed. It is possible, of course, that a querist should call some assumption into question. Nevertheless, there is no moment when everything is up for grabs. Underdetermination about Total Sciences can have no practical upshot whatsoever. What would a Total Science look like? It is no coincidence that philosophers' examples always take the the form of comparing the usual Total Science with a slight revision of itself; as with the example of flat space versus curved space with appropriate corrections,[44] we are given single theories as stand-ins for Total Sciences. We are invited to think that a Total Science is something we understand well enough. Since there is a body of scientific knowledge, then its makes sense to think of it collected at a time— right?[45]

Consider who we would ask or where we would look if we wished to know the state of Total Science. Perhaps Total Science includes only matters about which all scientists agree. Complete consensus is a rare thing, though, so this would count only the tiniest subset of what might plausibly be billed as scientific knowledge. If it includes matters about which there is disagreement, then determining the state of Total Science would be very difficult indeed. This may miss the point, since the underdetermination of Total Science by evidence is a lesson only about the situation of an individual querist. Even so, it is hard to know how the beliefs of a single scientist could be seen as a Total Science. The scientist may have some sense of which beliefs she believes *qua* scientist and which ones she believes *qua* citizen or consumer, but this will not be a sharp division. Moreover, what she believes

[44] The Quine/Poincaré example from §1.2.3. I take it up again in §4.1.2.

[45] Kukla writes that "a total science is nothing more or less than the conjunction of any 'partial' theory and *all* the auxiliary theories that we deem to be permissible. It does not matter which partial theory we begin with— the end result will be the same" [Kuk96, p. 143, my emphasis]. Yet there is no guarantee that this conjunction either exists or is well-defined.

qua scientist may be inconsistent; she believes the theory of relativity, but also quantum mechanics. It is impossible to sum up the beliefs of any actual querist as a Total Science— if she does not believe even one Total Science, why should it matter if she could not decide between several?

Again this may miss the point, since underdetermination of Total Science is meant to obtain between empirically adequate and equivalent Total Sciences. These Total Sciences might be thought of as sciences of the end times, sciences which have answered all questions as adequately as questions might be answered. Thinking in these terms can give no counsel to methodology. Hoefer and Rosenberg concede that "we can never be in a position to know a purportedly empirically adequate total theory is in fact a total theory or empirically adequate. But this epistemological truism does not undercut the conceptual point that two empirically adequate total theories would be nondefeasibly underdetermined by the evidence" [HR94, p. 595].[46] This conceptual point that some Total Sciences may be "nondefeasibly underdetermined" loses its force if no Total Sciences could ever be defeasibly determined. The underdetermination of Total Science for the abstract querist is at too far a remove from enquiry and methodology to be of interest to concrete querists like us.

Kukla treats underdetermination as an issue within the context of debates over realism. He deploys the notion of Total Science and concludes:

> Realists will undoubtedly wish to attack the notion of a total science. Admittedly, this notion has been severely underanalyzed by both friends and foes of [underdetermination]. But it remains to be seen whether its obscurities affect the role it plays in the argument for [underdetermination]. The prima facie case has been stated. The burden of proof is on realists to show why the total sciences version of the underdetermination argument fails. [Kuk98, p. 66]

This is unsatisfying in several regards. Underdetermination is not merely an issue between realists and anti-realists, nor have philosophers said so little that speaks to the issue of Total Science. For the last two decades at least, a growing number

[46] They attribute this "truism" to personal correspondence with Leplin.

of philosophers have argued for a "picture of science as radically fractured and disunified."[47] And these philosophers do not argue without precedent. Hacking points out that William Whewell, who coined the world 'scientist' as a moniker for a querist into nature, acknowledged the plurality of the sciences with titles like *The History of the Inductive Sciences* and *The Philosophy of the Inductive Sciences, Founded upon Their History* [Hac96, p. 37]. Yet in the latter of these works, Whewell writes that the aim of Science is to bring matters under general propositions "so as to form a large and systematic whole" [Whe89, p. 104]. There is a long history of thinking of the sciences as a plurality but also one of thinking that the sciences might make a Total Science. Without attempting to decide the matter for once and all, let me only cast serious doubt on Kukla's assessment of where the burden of proof lies. Hacking writes that, "The unity of science is rooted in an overarching metaphysical thought that expresses not a thesis but a sentiment. Since it is not exactly a doctrine, it lacks straightforward expression" [Hac96, p. 44]. It is only when we are in the grip of that sentiment that Total Science seems a plausible enough thing to constitute even a *prima facie* case.

Note further that the unity of science would not necessarily entail the existence of a final Total Science. As Paul Churchland suggests: "Just as there is no largest positive integer, it may be that there is no best theory. It may be that, for any theory whatsoever, there is always an even better theory, and so *ad infinitum*" [Chu85, p. 46]. If possible states of science stand in an ordering relation such that there is no supremum, then there would be no final sciences— not the two or more required for underdetermination.

3.3.4 Verdict on auxiliaries

Within the framework of Ch I, there are forms of underdetermination that result from the reliance on auxiliary hypotheses. Some of these are of narrow scope: During times of controversy, for instance, specific auxiliary hypotheses may

[47] [Dup96, p. 101] On the disunity of science, see also: Dupré [Dup83], Galison and Stump [GS96], and Cartwright [Car99].

come into dispute. Others suppose an unreasonable standard: Without trusting in some auxiliaries, we can never say more than what we observe directly.

3.4 The road from semantics

In considering the Copernican example (from §3.3), I have so far supposed that the rectilinear propagation of light, T_L, is an assumption of a logical kind with the hypotheses being tested— an assumption of more systematic importance than those hypotheses, true, but different only in degree. Duhem writes of its centrality:

> Was there... a clearer or more certain principle for thousands of years than this one: In a homogenous medium, light is propagated in a straight line? Not only did this hypothesis carry all former optics... whose elegant geometric deductions represented at will the enormous number of facts, but it had become, so to speak, *the physical definition of a straight line*. [Duh54, p. 212, my italics]

Yet, Duhem adds, it was questioned to the betterment of enquiry.[48] If we take the notion of a 'definition' seriously and not merely as a manner of speech, then we are left with a sort of underdetermination different from the ones we've considered so far.

Consider the moment in *gedanken* history when it was discovered that whales are warm-blooded.[49] Prior to this, the term 'fish' took in all manner of finned sea life, and common wisdom held the principle, F:

F All fish are cold-blooded.

The extension of 'fish' is a matter of definition while F is a commonly held theory about fish. Querists are faced with an anomaly: a warm-blooded fish. They could keep the definition of 'fish' and give up F. They could instead revise the definition and stand by F; they could specify paradigmatic fishiness in part by cold blood,

[48] T_L returned as an assumption and perhaps definition in General Relativity. Indeed, hay may be made as to whether T_L in a non-Euclidean spacetime is to be preferred over distorted light paths in a flat spacetime. I defer the matter until §4.1.2.

[49] This scenario is offered only to illustrate the confrontation between 'facts' and 'definitions.' The immediate point is conceptual rather than historical.

as we do. One course involves asserting $\neg F$ and the other involves asserting F; these mythical querists seem to face a choice between the rival theories F and $\neg F$. The choice between the two courses may be determined by good sense, as Duhem suggests regarding a similar example [Duh54, pp. 210–1], but that is not obviously so. Melville's Ishmael, reflecting on the science of *cetology*, writes

> ... that in some quarters it still remains a moot point whether a whale be a fish. In his *System of Nature*, A.D. 1776, Linnaeus declares, "I hereby separate the whales from the fish." But of my own knowledge, I know that down to the year 1850, sharks and shad, alewives and herring, against Linnaeus's express edict, were still found dividing the possession of the same seas with the Leviathan.
>
> The grounds upon which Linnaeus would fain have banished the whales from the waters, he states as follows: "On account of their warm bilocular heart, their lungs, their movable eyelids, their hollow ears. . . ." I submitted all this to my friends. . . and they united in the opinion that the reasons set forth were altogether insufficient. Charley profanely hinted they were humbug.
>
> Be it known that, waiving all argument, I take the good old fashioned ground that the whale is a fish, and call upon holy Jonah to back me. This fundamental thing settled, the next point is, in what internal respect does the whale differ from other fish. Above, Linnaeus has given you those items. But in brief, they are these: lungs and warm blood; whereas, all other fish are lungless and cold blooded. [Mel01, ch. 32]

So we imagine querists faced with whales. Their choice between Linnaean and Ishmaelean taxonomy— and thus between F and $\neg F$— is underdetermined with a scope that includes their present and future circumstances. (They could escape the underdetermination only by forgetting that whales are warm-blooded.) Because a suitable redefinition of some terms in F could make the sentence true (or false) however the world may be, this underdetermination is sometimes taken to entail that querists may responsibly believe (or disbelieve) any theory come what may. The querists need only adjust the meanings of the terms in the theory so that their belief (or disbelief) remains justified.

The weakness of this form of underdetermination may readily be seen in our mythical story of fish. The two paths open to the querists are not completely described as asserting or denying F. The first choice is to keep with the old use of 'fish' and deny the 'F.' The second is to self-consciously revise what is meant by 'fish' and to accept 'F.' Let Fish_1 stand for the sense of 'fish' in use among the querists prior to this crisis: finned creature of the sea. Let Fish_2 be the sense counseled by the second course: gill-breathing, egg-laying, lacking in hollow ears or moveable eyelids, and so on. We might better express the options open to the community as F_1 and F_2:

F_1 Some Fish_1 are warm-blooded.

F_2 All Fish_2 are cold-blooded.

Although F_1 is equivalent to one reading of $\neg F$ and F_2 is equivalent to one reading of F, F_1 and F_2 are not contradictory. We have defined underdetermination in such a way that the rivals need not be exclusive, and indeed it is best to see the two courses as genuine rivals of a sort. Good methodology advises against lexical bifurcations that might leave the querists' descendents with an indefinite number of terms Fish_n. Nevertheless, the logical compatibility of the rivals suggests that this underdetermination holds no profound lessons about *all* theory choice. F_1 and F_2 each represent a fact that the querists should acknowledge regardless of which rival course they take— especially since the extensions of 'finned creature of the sea' and 'gill-breathing, ... creature of the sea' remain the same on either course. It is tempting to say that the theories held by the querists in either case would be the same, even though their different jargon would lead them to express the theories differently. I stop short of succumbing to this temptation, mindful that I have no general identity criteria for theories.[50] It suffices to say that regardless of how nomenclature is arranged, any theory should allow for discourse about Fish_1 and Fish_2 under some labels. Such a theory would have advantages over any theory which had no taxonomic category corresponding to our category 'fish' (i.e., Fish_2) or no way of referring to finned sea life (i.e., Fish_1). Ishmael, insisting that whales

[50] See the discussion in §1.2.3.

are *fish* on the ground that they divide the sea with other fish and on the ground of common usage, insists that 'fish' should mean Fish_1. Yet he readily concedes that whales are different than other fish— that they are warm-blooded and so on— and so he has the resources to talk about Fish_2. The disagreement is about labels rather than about concepts.

Quine suggests that querists might save favorite beliefs by revising logical laws [Qui53, p. 43] and that this would be done by changing the meaning of logical terms. He later wrote that, in invoking Duhem, he is "not concerned even to avoid the trivial extreme of sustaining a law by changing a meaning..." [Qui76, p. 132]. His aim, rather, is to show only that there is no fundamental difference between meanings and facts. However that may be, on Quinean grounds there are criteria of theory identity, and the two options above offer the same theory.[51] Thus, for Quine, underdetermination due to semantic revision would not amount to *underdetermination of theories* at all.

The fact that the assertion 'F' can be preserved says neither much about holism nor about underdetermination. The former, because the whole language is never at issue— it is simply a question of what expressions pick out these *two* categories. The latter, because we may judge against theories that lack taxonomic categories present in their rivals. If the categories are frivolous, then our decision may go the other way. Regardless, we may decide.[52]

3.5 The road from voiding observation

The Copernican example from §3.3 can also be used to illustrate a different sort of underdetermination. Treating it merely as a hypothetico-deductive problem, theory T_F and auxiliaries T_L entail the negation of some observation O: i.e., $(T_F \& T_L) \rightarrow \neg O$. We stand on the shore and watch. Most of us are satisfied that we have observed O. Yet suppose there is one among us— call him Chester—

[51] See the discussion of Quine on theory identity in §1.2.3

[52] We should have as many categories as we need, but not more. Ockham's razor cuts both ways.

who is especially fond of T_F. He has heard what has been said so far, and he sees no point in trying to deny T_L. After a moment, he hits on the idea of denying O. Stepping away from the group, he admits that we and he have been in circumstances that *we* describe as observing O, but *he* is under no obligation to accept our reports as veridical.

In effect, he hopes that the choice between T_F and T_R is underdetermined in a way that we had not appreciated before. The scope of the underdetermination would contain any circumstances in which we could ever find ourselves, because he denies that any circumstance that *seems* to be an observation disconfirming T_F is actually such an observation. (The scope includes, Chester hopes, all naturally possible circumstances.) The standard of responsibility allows one to assume any auxiliary theories, provided they are not so strong that they determine the question *a priori*. Yet the standard is strange, because it permits our recalcitrant friend to deny that we have observed O, even when he has seen it with his own eyes. As we saw in Ch II, perverse mistrust of the senses leads to scepticism. Of course, Chester is always free to raise a particular doubt about what has happened; he may ask, "How can we be sure what we saw, through all the haze?" or "If that was a ship going out to sea, why did the image flicker like a projected movie?" If he claims not to have seen O but offers no such reason for doubt, we would suspect his eyesight. Suppose, then, that his powers of observation match ours when T_F is not at issue. Absent some reason for doubt, it seems, he should concede O.

Yet, Chester says, he does have a reason for doubt: T_F is true, he believes, but O (along with accepted auxiliary theories) entails $\neg T_F$. Since O is false, Chester argues, he has reason to doubt that any observations of O are veridical. Chester might offer this analogy: Stage magicians are able to perform many feats that we are tempted to describe as 'Making things appear and disappear.' Nevertheless, we have this background assumption that macroscopic, physical objects can only move from place to place by following a continuous path; they don't just *disappear* one place and *appear* another place. We don't take stage magic as a reason to seriously question this assumption, even if fellow audience members gawk and say that the pig has disappeared, that the lady has turned into a tiger, or whatall else.

There is an important disanalogy here, of course. There is a social convention of magic shows. Part of this convention is that the magician arranges circumstances such that the audience is deceived. We expect at magic shows that things will not be as they seem. Indeed, deception is essential to *magic* of this kind. Sword swallowers (who actually slide swords down their throats) and fire eaters (who actually extinguish flames with their mouths) are not considered magicians. Because of this convention, we have other background knowledge that makes us suspicious of the magic trick. Even if I do not know how the trick is done, our community has experts that do know— viz., magicians. And I do know the secrets of some similar legerdemain.

In these cases, it is important that the observation not be described in a way that begs the question against one of the rivals. At the magic show, we might describe our observation in any of several ways:

A_1 A woman disappeared in the sense that she could be seen and then moments later she was gone without having been seen to leave.

A_2 A woman was made to appear to vanish by some contrivance.

A_3 A woman traced out a discontinuous path in space.

Chester supposes we dismiss A_3 simply because we think that *nothing* traces out discontinuous paths in space. However, we do not dismiss the possibility of electron tunnelling on this basis. There must be something more going on here. We say that A_2 is a true description of the situation— it might count as *what we observed* under some circumstances— but if we want to consider both rivals we do not merely describe what we saw in a way that dismisses A_3. If we describe what we see in a conservative way (A_1), we may still infer A_2 on the basis of our knowledge about the ordinary world and our knowledge about magic shows.

Chester's attempt to void the Copernican observation can be handled in a similar way. We might describe the observation O in different ways:

O_1 When the ship was near to shore, we could see its hull and mast; when it was further away, its mast seemed lower and we could not see its hull at all.

O_2 The ships hull was occulted by the curvature of the Earth when it was far away.

If we meant the observation to test T_R, then we can not say that we directly observed O_2. Note, however, that there is no analog of A_3 here. Chester has no way of describing the observation that makes it congenial to his flat-earth dogma.[53] If he digs in his heels, we might revise O_1 to speak of the ship *appearing* to be near shore, and so on. Yet he must at some point admit that he observed something. Chester might then reply in a number of ways. First, he might simply accept O_1 but insist that T_F is well enough confirmed that this observation cannot overthrow it. This is fine, but he must at least admit that this is an anomaly and a problem for T_F. One anomaly need not be decisive, but it adds incremental weight against the theory. Second, he might say that there is no legitimate inference from O_1 to O_2. He would be forced to challenge the background assumptions that underwrite the inference, so we'd just have the familiar DQ problem. Third, he might dismiss the observation as a freak error. We may repeat it, however, on other days with different ships and from different beaches. None of these responses are sufficient to warrant Chester's merely throwing out observations of this kind.

The point about observation made here is illustrated in Trevor Pinch's study of solar neutrino experiments [Pin85]. Neutrinos only effect other particles in weak force interactions, so they are hard to detect. The solar neutrino detector Pinch discusses is a 100,000-gallon tank of dry-cleaning fluid located in a shaft deep beneath the Earth. The fluid contains an isotope of chlorine that can interact with neutrinos to produce an isotope of argon (Ar^{37}). The argon is collected, and a geiger

[53] An account in terms of sagging light beams or whatall else is precluded by his acceptance of T_L. If he reneges on this, then the case is just of the kind considered in §3.3.

counter is used to detect when it decays. The number of clicks on the geiger counter indicates the number of Ar^{37} atoms, which in turn indicates the number of neutrino interactions, which indicates finally the number of neutrinos passing through the detector. Pinch points out that observations made using this apparatus may be characterized in many ways: "(a) Splodges on a graph were observed. (b) Ar^{37} atoms were observed. (c) Solar neutrinos were observed" [Pin85, p. 9]. Scientists who built the detector described their observation as (c). Challengers argued that the observation should not be understood in that way, but could agree to (b) or at least (a). Specific auxilliary hypothesis came into question and could be explicitly debated.

Pinch introduces the notion of *externality* to organize the different reports. Of these three, (a) is of the least externality and (c) of the greatest. An observation increases in externality the more it relies on background theory— in Pinch's idiom, the more the evidential context must be specified. Just as I may report A_2 or O_2 as what I observed, the scientists report (c). If and when these reports are challenged, we withdraw to a report of less externality about which both we and the challenger can agree. As Pinch notes, this feature of observation amounts to the DQ Thesis [Pin85, p. 14]. Thus, doubting observations does not lead to a different kind of underdetermination. (Sorry, Chester.)

This chapter has explored top-down arguments that underdetermination is ubiquitous and *a fortiori* important. These arguments, I hope to have shown, either fail to prove that genuine underdetermination is ubiquitous or succeed in doing so only for uninteresting and *a fortiori* unimportant kinds of underdetermination. The fact that woeful underdetermination is not essential to our epistemic situation doesn't show that there isn't important underdetermination for all or most scientific problems, however. The next chapter turns to arguments that begin at the level of particular examples.

IV

Bottom-up Arguments

> "[O]VER-SIMPLIFICATION, SCHEMATIZATION, AND CONSTANT OBSESSIVE REPETITION OF THE SAME SMALL RANGE OF JEJUNE 'EXAMPLES' ARE... FAR TOO COMMON TO BE DISMISSED AS AN OCCASIONAL WEAKNESS OF PHILOSOPHERS."
>
> —J.L. Austin [Aus62, p. 3]

Whereas the arguments of the previous chapter concerned underdetermination in the abstract, other arguments begin with particular cases of underdetermination. Then, as Earman writes, "the production of a few concrete examples is enough to generate the worry that only a lack of imagination on our part prevents us from seeing comparable examples of underdetermination all over the map" [Ear93, p. 31]. These *bottom-up* arguments thus consist of two moments. The first involves showing that some cases of theory choice are underdetermined. The second involves generalizing from the case studies. Bottom-up arguments can go wrong at either moment. Some alleged cases of underdetermination are not underdetermined at all— more often, they are underdetermined but only for a narrow scope or unreasonably strict standard. Other arguments correctly identify interesting cases in which underdetermination follows from specific features of the rival theories, but where the specific case may lead us to expect underdetermination in its borough but not all over the the map. Thus, objections to bottom-up arguments may take two forms. One is to deny the alleged the underdetermination, leveraging plausible commitments where necessary. The other is to contain the underdetermination by showing that it arises out of special features of particular cases.

In §4.1, I consider several geometrical examples of alleged underdetermination that call for objections of both kinds. The geometrical cases typically offer single instances of underdetermination.

In §4.2, I turn to arguments that aim to reveal historical patterns of underdetermination in hopes of underwriting an induction from the history of science. A great deal depends, of course, on what sample one attempts to provide.

In §4.3, I consider attempts to exploit underdetermination in the cause of relativism. Exploring an empirical, moderate relativism, I discuss a trio of cases that might be seen to favor the relativist.

If I have succeeded, I will have shown by the end of the chapter that none of the cases here underwrite bottom-up arguments. The discussion might be extended *mutatis mutandis* to other cases, but there is no way of showing that *no* examples could succeed where these fail. That's life.

4.1 The road from geometry

Geometry, as a mathematical enterprise, is in one sense free from underdetermination. Gödel's incompleteness result entails something that might count as underdetermination, of course, but only about the truth of esoteric things like Gödel sentences. It is possible to prove in Euclidean geometry that the angles of a triangle add up to two right angles, and there is no further question about it for Euclidean geometry.

For a claim in the natural sciences, things are never so easy. There is always a question about whether Euclidean geometry describes anything interesting in the world. It is important, then, to distinguish between a *geometry* as a mathematical structure and the *actual geometry* of the universe. Claims about actual geometry, about space, and about spacetime are dogged by allegations of underdetermination.

4.1.1 Absolute velocity

Start with a simple example from Bas van Fraassen [van80, pp. 46–7]. Consider Newtonian mechanics with its absolute, Euclidean space; call this N. Now consider a point determined by the configuration of stuff in the universe— the center of mass for the whole universe, let's say. Let M_v be the hypothesis that this point is moving at velocity v. Now $(N\&M_0)$ is the claim that Newtonian mechanics holds and that the universe— as a composite— is at rest, while $(N\&M_7)$ agrees on the mechanics but entails that the universe is moving at seven feet per second. Newtonian mechanics is Galilean invariant (that is, the laws are preserved under uniform changes in velocity), so it looks as if there would be no observable difference between $(N\&M_0)$ and $(N\&M_7)$. The choice between them seems to be underdetermined.

We might hope to find evidence that would decide between the two. Suppose we had a device— a sort of übergyroscope— that pinged if it was at rest in absolute space but remained silent otherwise.[1] We need only move the device around and see when it pings. If it pings when it matches velocity with the universe, then M_0 wins out. Two obvious objections suggest themselves.

First, one may object that each $(N\&M_i)$ was meant to be a *total theory* in the sense that it could account for all phenomena; since N does not allow for übergyros, then such a counterfactual gizmo could not decide between them. Total sciences are dubious things, however; we already have reason to be suspicious of them.[2] Moreover, it would be a mistake to understand $(N\&M_0)$ as being total. N includes both the laws of mechanics and a collection of force laws— as Mark Wilson notes, "At no point during the reign of classical mechanics was this set of [force] laws ever fully established or agreed upon" [Wil80, p. 216]. Thus, the

[1] This übergyro is equivalent to hypothetical auxiliary theories entertained by Laudan and Leplin [LL91, p. 458]. These posit a new kind particle that arises with absolute motion.

[2] From the discussion in the preceding chapter, §3.3.3.

übergyro's operation might exploit some force law not included in the usual list.

Second, one may object that adherents of M_7 could accommodate this evidence by providing a different account of how the device works— they would say that it pings only when going in some particular direction at seven feet per second. This objection supposes that the account of how the device works could be varied without loss of credibility, but that might not be so. It depends on why we believed the gyro indicated something about the motion of absolute space in the first place, and of course the thought experiment is underspecified in this regard. The device, as described, is rather far-fetched. Regardless, it shows that the scope of this alleged underdetermination only takes in circumstances where we have got neither such a device nor an equivalently serviceable theory.

Even so, one must admit that M_0 and M_7 are underdetermined with respect to one another, given N. To describe the case fully: The rivals $(N \& M_0)$ and $(N \& M_7)$; the scope takes in all circumstances in which we do not have übergyros or their equivalent; the standard is (it seems) any plausible one. However, this scenario will not underwrite a bottom-up argument for the ubiquity of underdetermination. It relies on special features of the rivalry between M_0 and M_7. This can be made clear in two ways.

First, we might think about velocity as relational rather than absolute and thus insist that the alleged rivalry between M_0 and M_7 amounts to nothing. We have already seen Poincaré's relationalism in §1.2.3, but a relational account of velocity is open to us even if we maintain *contra Poincaré* that rotation and acceleration are absolute.[3] The choice between the relationalist analogs of these two theories is not underdetermined. We are invited to generalize from the rivalry between $(N \& M_0)$ and $(N \& M_7)$, invited to conclude that all or most theory choices are underdetermined. Yet we might just as easily generalize from the their relationalist analogs, concluding that there is little or no underdetermination.[4] The

[3]Earman [Ear89, ch. 2] provides a thorough discussion of these possibilities.

[4]One might even generalize from the two rivalries and conclude that about half of the theory choices in science are underdetermined. In short, there is no unobjectionable way to generalize from these cases.

original case does not underwrite any robust bottom-up argument.

Second— even if we insist on thinking in terms of absolute velocity— this underdetermination scenario relies on underdetermination *not* being ubiquitous. If we knew N, then we would know quite a lot about the world; our inability to decide between M_0 and M_7 would be a rather narrow inability. It would be a kernel of ignorance on an earful knowledge. In this way, the case would *undercut* bottom-up arguments.

4.1.2 Geometry and universal forces

In both M_0 and M_7, the actual geometry was assumed to be Euclidean. Imagine trying to test that assumption. You find two allegedly parallel lines and measure the distance between them. You then walk thirty meters along one of the lines, measure the distance between them again, and so on into infinity. If space were of constant curvature, there would be three possibilities: the distance is constant, the distance increases, or the distance decreases. These would correspond respectively to Euclidean, hyperbolic, and elliptical geometry.

This test can only be decisive, however, if your ability to measure length is unaffected by your tour of the universe. Suppose that the lines move closer together as you go along, but that your meter stick shrinks by a corresponding amount each time you move. You would measure the distance as being the same each time and conclude (wrongly) that the actual geometry is Euclidean.

Your situation would be like the situation of the monkeys in figure 4.1. The figure has an infinite number of inhabitants, and to them it appears as if they are on an infinite Euclidean plane. They exist in a finite space, however, because they shrink as they move away from the center.[5]

So regardless of whether you measure the distance as remaining constant, as increasing, or as decreasing, the actual geometry might have any constant

[5]Their situation is complicated, because their whole world is embedded in the Euclidean plane of the printed page. Thus they would rightly think that the actual space of their world is Euclidean, but for the wrong reasons, and they would mistakenly think that their world occupied infinite space.

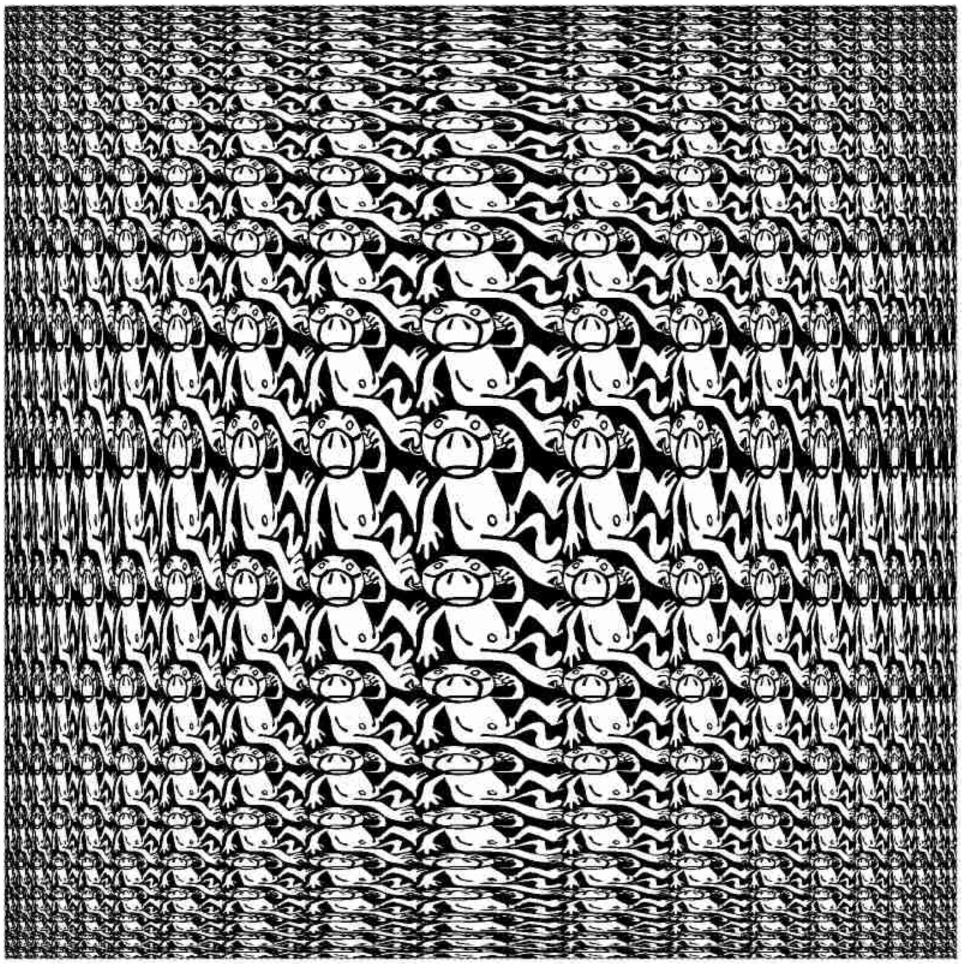

Figure 4.1: The monkeys grow smaller the further they are from the center. Since everything shrinks along with them, they don't notice the change. It seems to them as if there is an infinite grid of similarly-sized monkeys.

curvature— supposing that your meter stick changes appropriately in length as you move. You would not notice that it was changing in length, because you would be changing right along with it. Generalizing, we arrive at Hans Reichebach's *Theorem* θ:

> Given a geometry G' to which the measuring instruments conform, we can imagine a universal force F which affects the instruments in such a way that the actual geometry is an arbitrary geometry G, while the observed deviation from G is due to a universal deformation of the measuring instruments. [Rei58, p. 33]

There does not seem to be any evidence that could show the absence of universal forces. Appeal to simplicity is, as Reichenbach notes, equivocal. One may say that it is simpler to suppose that there are no universal forces, but some say that Euclidean geometry is the simplest geometry [Rei58, p. 34]. So, Theorem θ entails that actual geometry is underdetermined: The choice between 'Space has actual geometry G' and 'Space has actual geometry G'' is underdetermined with wide scope for any standard that does not allow us determine the effect of universal forces *a priori*.[6]

From actual geometry to physical geometry

Let's grant for the moment that this is a bona fide case of underdetermination. As you wander the universe with your meter stick, you will never be able to determine the actual geometry of space. However, we might ask whether the *measured distance* between the two lines varies as you move along them. Any deformation that effects both the lines, your meter stick, and you would be irrelevant. Thus, your observations would be sufficient to determine the *physical geometry* of space, the system of relations that obtains among objects even as they are moved around. The question of what physical geometry space has is not underdetermined in the way that the question of its actual geometry is.[7]

[6] Boyd calls this the paradigm example of underdetermination [Boy73, p. 5].

[7] The term 'physical geometry' is probably ambiguous. Hempel (for instance) uses the term to mean "the theory of the structure of physical space" [Hem49, p. 244] and to pick out what I have here called

One may object that a real meter stick will expand or contract as its temperature changes. If you infer the physical geometry of the universe from the behavior of transported meter sticks, then you will say that the physical geometry has variable curvature and depends on the local temperature. However, as Adolf Grünbaum explains, "Physical geometry is usually conceived as the system of metric relations exhibited by transported solid bodies *independently of their particular chemical composition*."[8] Thermal deformations depend on the composition of the thing deformed— for example, bimetallic strips bend because the two metals undergo different amounts of expansion or contraction. Thus, we need to abstract from thermal deformation to determine the physical geometry. This is possible because we have a theory that governs thermal contraction and expansion. It's possible to correct for them, using auxiliary theories from dynamics and thermodynamics. Hempel sees this reliance on auxiliaries as the crux of the issue, maintaining "that the test of a physical geometry G always presupposes a certain body P of non-geometrical physical hypotheses (including the physical theory of the instruments of measurement and observation used in the test), and that the so-called test of G actually bears on the combined theoretical system $G{\cdot}P$ rather than on G alone" [Hem49, p. 247]. Indeed, the reliance on auxiliary hypotheses is often alleged to engender underdetermination; this is the familiar Duhem-Quine Thesis. I argued in §3.3 that these worries can be answered. If the arguments there were insufficient, then underdetermination would tear down everything and the consideration of geometry would add nothing but a flourish. So let's set concerns about auxiliaries to one side and see if the underdetermination of geometry amounts to anything more.

One may object that this talk about physical geometry begs the question by supposing that there are no universal forces. Indeed, if there were no universal forces, then physical geometry and actual geometry would be the same. Yet you

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the actual geometry of space. I follow Reichenbach and Grünbaum in using the term to pick out concrete relations between bodies.

[8] [Grü60, p. 78, his emphasis], reprinted as [Grü76]; cf. [Rei58, p. 37].

need not *suppose* that there are no universal forces. Whatever the universal forces might be, they interact with the actual geometry to produce some net effect on measured distances. The physical geometry is thus the sum of actual geometry and universal forces.[9]

The underdetermination entailed by Theorem θ only holds if we ask about actual geometry; it is resolved if we ask instead about physical geometry. The underdetermination turns on special features of the rivals considered and, thus, it will not underwrite a bottom-up argument. There is no reason to think that scientific theory choice in general is more like the choice of a geometry than like the choice of a physical geometry. Why should we generalize from one rather than from the other?

The peculiarity of universal forces

What has been said so far would be enough to undercut any bottom-up argument based on Theorem θ. Even so, let's reopen the question of whether the actual geometry of space is underdetermined. Calling uniform deformations of meter sticks *universal forces* suggests that they would be phenomena like others already admitted in science. That is, it suggests that universal forces would be merely another entry on the list of forces— arranged alphabetically below electromagnetic and gravitational forces, perhaps. This use of the word 'forces', however, is misleading. Classical forces obey and are perhaps even defined by Newton's formula $F = ma$; the acceleration due to gravity or electromagnetism is inversely proportional to the mass of an object. The universal forces are presumed to affect all meter sticks and observers equally, regardless of their mass. As you tour the universe, the universal forces would need to shift around your flesh just to the degree they shift around your bones. They would warp massive meter sticks to the same degree as they warp flimsy ones. Thus, universal forces would not be forces in a strict sense. They would be *sui generis* deformations. It seems that "in the

[9]This point is made by Reichenbach, but developed in greater detail by Glymour [Gly80, ch. 9].

end, such 'forces' are no better than 'phantom effects' and we are left with just another skeptical fantasy" [Sta01, fn. 6 p. S6].[10]

This point will be clearest if we try to give a formal specification of what universal forces must be. We might try to describe the universal forces at work in figure 4.1. If we declare the point $(0, 0)$ to be the center of the monkey-space, then we can map points (x, y) in the actual geometry of the undistorted plane into points (x', y') in the monkey's physical geometry:[11]

$$x' = x(.75^x)$$

$$y' = y(.75^y)$$

Yet this doesn't tell us about universal forces. For that, we need to understand the *dynamics*. This could perhaps be accomplished by finding the distortion of velocities $\vec{v}$ into velocities $\vec{v}'$ and so on, but that would be a tricky business.

Consider instead a simpler example. Imagine a universe with one spatial dimension and deformations that operate in a narrow band so as to make objects on the right side twice the size of objects on the left. Now consider a rocket ship approaching the universal forces from the left. As the nose of the rocket passes through the universal forces, it must begin to go faster than its tail, such that it stretches relative to the back end. As the tail passes through the universal forces, it undergoes a similar acceleration such that the various parts of the rocket are all moving with uniform velocity when it has cleared the region of universal forces. See figure 4.2.

When the final length of the rocket is twice the initial length, the final velocity is twice the initial velocity. Each part of the rocket is accelerated to twice its prior velocity as it passes through the universal forces; the universal force at a point exerts an acceleration on things that depends on their velocity. No classical forces are velocity dependent.[12] They could not be without breaking classical

[10] This remark is from Kyle Stanford, who attributes the point to David Malament.

[11] (.75) is the rate of shrinkage. There is nothing important about the precise number; any constant fraction would do. The particular value was selected only so as to make figure 4.1 aesthetically pleasing.

[12] Except perhaps friction, but this is sometimes taken as a reason to think that friction is not a

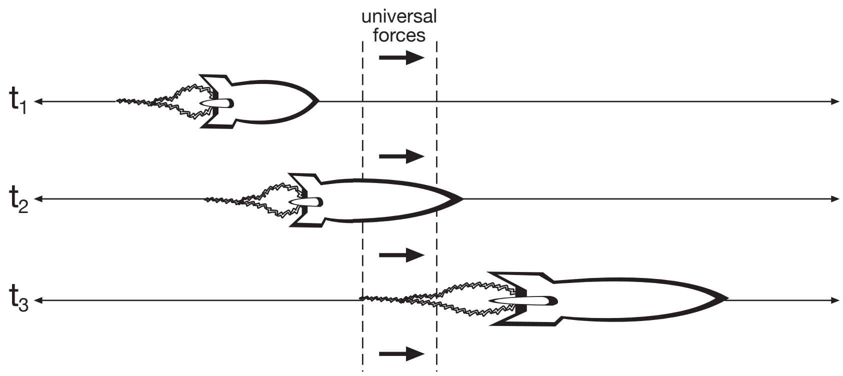

Figure 4.2: At t_1, the rocket approaches the region of universal forces from the left. At t_2, it has entered the region and its nose is moving faster than its tail. At t_3, it has left the region and the various parts of the rocket are once again moving with uniform velocity.

mechanics' Galilean invariance.

Of course, things passing through the universal forces don't always *double* in velocity. A rocket moving from right to left will have its velocity cut in half, as in figure 4.3. Since the effect of universal forces has both a magnitude and a direction, we can express the universal force at a point as a vector. As a particle passes through a point, the universal force at that point acts so as to increase any component of the particle's velocity in the direction of the universal force and to decrease any component in the opposite direction. For all space, we can represent the universal forces as a vector field.

So, as a formal matter, we can represent the universal forces at work in figure 4.1 as a vector field, as in figure 4.4. The vectors correspond to a velocity-dependent acceleration. Let $\vec{u}$ be the universal force at a point, let $\vec{v}$ be the velocity of a particle at the point, and let $proj(\vec{v}, \vec{u})$ be the projection of $\vec{v}$ onto $\vec{u}$. The acceleration due to universal forces is then given by

$$\vec{a} = \begin{cases} (|\vec{u}| - 1)\ proj(\vec{v}, \vec{u}) & \text{if } proj(\vec{v}, \vec{u}) \text{ is in the same direction as } \vec{u}, \\ (|\vec{u}|^{-1} - 1)\ proj(\vec{v}, \vec{u}) & \text{otherwise.} \end{cases}$$

fundamental force.

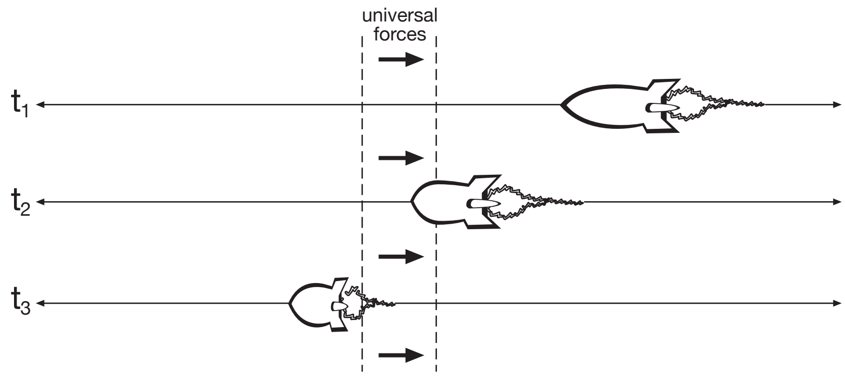

Figure 4.3: A rocket approaching the universal forces from the right has its velocity reduced. Since the nose slows down before the tail, the rocket's length is also reduced.

It may not be obvious that the equation should take this form, but the reader will perhaps rest content with applying it to the rocket ships in figure 4.2 and figure 4.3. Supposing the band of universal forces to be of unit length, then the magnitude of the universal force is 2. So the rocket moving from left to right undergoes an acceleration of $(2-1)v = v$ and acquires a final velocity of $2v$. The rocket moving from right to left undergoes an acceleration of $(\frac{1}{2}-1)v = -\frac{1}{2}v$ and acquires a final velocity of $\frac{1}{2}v$. These are the results we arrived at above in considering the case.

The important point is that the so-called universal force turns out to be an acceleration on every object that depends on the magnitude and direction of the object's velocity. They are unlike any other processes admitted in our science. Unless they were to have some further rôle in our physical theory, they would be nothing more that sceptical scenarios. Any reasonable standard of theory choice will allow us— absent further evidence of their existence— to deny that there are any universal forces.[13]

[13]Snider argues that universal forces in the sense discussed here only make sense in the context of classical mechanics anyway. She writes, "Considering space-time there is *no* analoque to self-identical rods which may be transported or which may undergo deformations" [Sni67, p. 65].

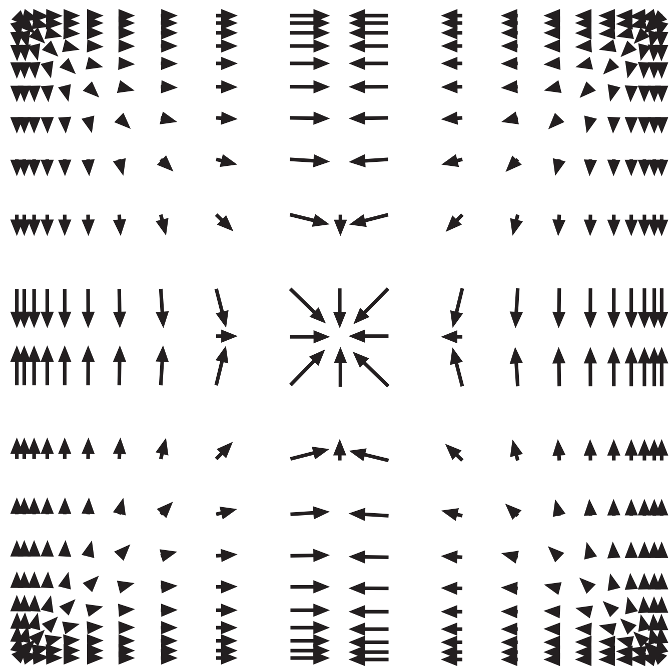

Figure 4.4: The universal forces at work in figure 4.1 can be respresented as a vector field.

4.1.3 Multiple connection rivals massive reduplication

As discussed in the previous section, underdetermination about metric structure may be avoided either by shifting our attention from actual geometry to physical geometry or by eschewing universal forces as *sui generis* monstrosities. Yet the topology of physical geometry remains underdetermined, as a simple thought experiment demonstrates.

Around the universe in 80 days

Imagine you were to board a rocket ship and fly in a straight line away from Earth. After some time, you find yourself approaching Earth— or so it seems. It's a blue-green planet orbiting a yellow sun, matching the planet you left behind to any discernible degree of detail. You might think you've flown in a circle, but you check your instruments and conclude that indeed you've gone in a straight line away from Earth. Is this planet Earth? How could you tell?

Let S_1 be the theory that space is a finite cube wherein opposite sides are identified, such that anything reaching the top side would emerge on the bottom, anything reaching the back would emerge at the front, and anything passing to the right side would emerge at the left.[14] If S_1 were true, then the planet would be Earth. Like the astronaut in figure 4.5, you'd have flown away from Earth and arrived back there.

Let S_2 be the theory that space is a finite volume with its contents repeated twice over. Space is connected as in S_1 but is larger, such that when you arrive at this blue-green planet you've made it half-way across the universe. If S_2 were true, you'd have arrived at the likeness of Earth and not at Earth itself. The situation would be like figure 4.6.

What could you do to decide between S_1 and S_2? You might retrace your path to Earth and ask if you'd been seen coming the other way— if they saw you from Earth, then your journey had taken you to Earth and you could conclude that S_1 was correct. Yet how could they be sure it was you that they saw? If S_2 were true, the other planet would be an exact likeness, so it too would have sent out a rocket ship. Your friends on Earth would be unable to tell whether it was you or an indistinguishable likeness they had seen.

It looks as if your choice between S_1 and S_2 might be underdetermined for any evidence. Of course, you might entertain theories $S_3, S_4, S_5, \ldots$, in each of which space is larger than in the last and there is one more planet sending out one more rocket. You may even entertain the limit case, S_ω, in which space is infinite and there are an infinite number of indistinguishable planets launching an infinite number of rocket ships.[15]

Let $\mathcal{S}$ stand for $\{S_n : 1 \leq n \leq \omega\}$. If your selection from $\{S_1, S_2\}$ is underdetermined, then your selection from $\mathcal{S}$ will similarly be underdetermined.

[14] This is equivalent to supposing that space is a 3-dimensional torus. You might worry instead that space is a Klein Bottle or that space is infinite in one or even two dimensions; such variants may be plugged into the discussion that follows *mutatis mutandis*.

[15] Transfinite theories of the form $S_{\omega+n}$ are ruled out; you can travel in either direction, so both the successor and the predecessor of each element must be defined.

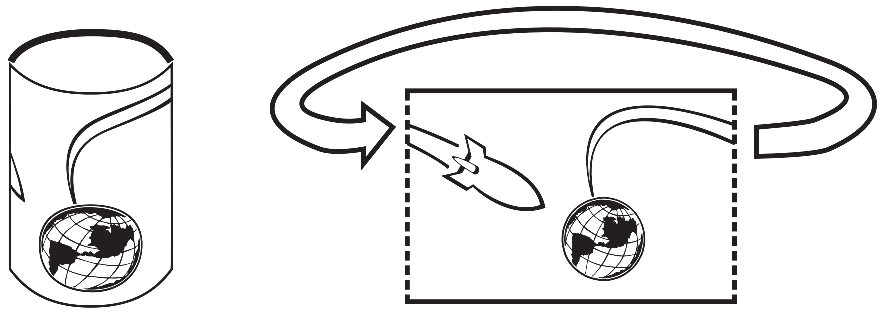

Figure 4.5: In finite space, the intrepid spaceman travels directly away from his planet only to arrive back home.

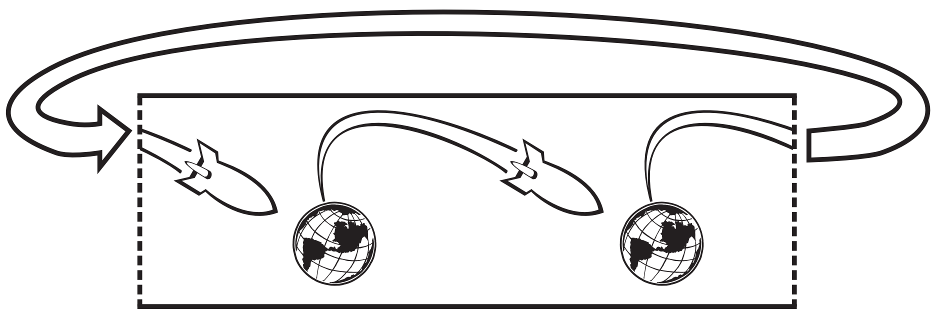

Figure 4.6: Space is larger but still finite. The intrepid spaceman travels directly away from his planet to arrive at an identical planet, while an astronaut leaving from the other planet travels to the first spaceman's home.

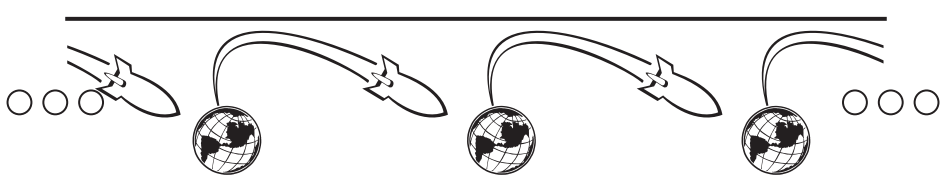

Figure 4.7: In infinite space, the intrepid spaceman travels to one of an infinite series of identical planets while an infinite number of other astronauts do the same.

Indexicality

In *Individuals*, P.F. Strawson famously worries about the possibility of one sector of the universe repeating another down to the last detail. He calls this possibility "massive reduplication" [Str59, p. 20]. If our S_i obtains for $i > 1$, then massive reduplication would be realized. We should, then, consider whether Strawson's discussion sheds any light on the rivalry between the members of $\mathcal{S}$. He writes "that we build up our single picture of the world, of particular things and events, untroubled by possibilities of massive reduplications, content, sometimes, with the roughest locations of the situations and objects we speak of.... This we do quite rationally, confident in a certain community of experiences and sources of instruction" [Str59, p. 28–9]. This seems to suggest that we might "quite rationally" accept S_1, but a moment's reflection will show that this is not so.

Strawson is initially worried about massive reduplication in the context of considering whether singular reference can be secured by means of descriptions. For any non-indexical description that we could know to hold of an object, he notes, we could not rule out the possibility that *some other* object also matches that description. Thus, massive reduplication arises as the worry that even a detailed description of a thing's environment might fail to individuate it if an indistinguishable *thing-cum-environment* exists elsewhere in the universe. Strawson resolves this worry by noting that we can employ indexical descriptions, picking out the thing as 'this' or 'that', its environment as 'here' or 'there', and so on. He writes that to answer the worry about massive reduplication, "it is sufficient to show how the situation of non-demonstrative identification may be linked with the situation of demonstrative identification" [Str59, p. 20]. It is not always possible to indexically specify an object, for instance if it is far away and out of sight. Nevertheless, it is possible to say *where* the thing is and to specify that location relative to *here*. We can can pick out a thing by specifying its location in some sector of space, and the question of how that specification picks out one individual "may be answered by relating that sector uniquely to the sector which speaker and hearer themselves currently occupy" [Str59, p. 20]. Thus, for Strawson, we can always pick out an object with indexicals because we can place it in a unified spatiotemporal system.

It is important to notice that Strawson's argument does not show that massive reduplication *does not obtain*, nor does Strawson claim to show that— Strawson shows, at most, that the possibility of massive reduplication should not trouble our ordinary practices of identifying individuals. Your situation after flying across the universe in your rocket ship is extraordinary, however, and may cause ordinary practice to break down. The morning before you leave on your journey, you know that you are in *your* house on *your* home planet, Earth— that Earth is the planet *here* and *now*. After your rocket journey, you arrive at a planet indistinguishable from your Earth. Imagine you land and go to a house indistinguishable from your house. Your key (which you brought with you) unlocks the door. You go inside. You climb into a bed like your bed in every detail and go to sleep.

Strawson argues that the possibility of reidentifying objects in this way is presumed by our conceptual scheme. For this reason, the sceptic about reidentification is horribly confused; he accepts our concepts in asking whether it is possible to *reidentify individuals*, but reneges on them in suggesting that reidentification is impossible; "He pretends to accept a conceptual scheme, but at the same time quietly rejects one of the conditions of its employment" [Str59, p. 35]. The rivalry between the members of $\mathcal{S}$ does raise doubts about your attempt to reidentify this new planet as the one you left, but none of the members of $\mathcal{S}$ demand rejection of our usual conceptual scheme. They all allow for unproblematic intra-planetary reference.

The fact that you identify your home planet with an indexical— as *your* home planet— doesn't help you resolve whether this planet you arrive at is your home planet, whether this is your house, or whether this is your bed. In the scenario we are imagining, indexical reference to things back on the planet you left is unproblematic. Strawson thinks that having a single, spatio-temporal framework is required for referencing particulars. Since each of member of $\mathcal{S}$ posits a single spatio-temporal framework, Strawsonian considerations do not distinguish between them. The members of $\mathcal{S}$ disagree with one another as to what properties the framework would have, and that is the crux of the issue.

So it looks as if you have no way of knowing whether the bed you sleep in after you arrive is *your* bed at all. Of course, the residents of the planet on which you are sleeping are in no better position to decide between the members of $\mathcal{S}$ than you are.[16] You have landed, gone into a house, and gone to sleep. If it is your house, then you have every right to do so. If it is not, then you are trespassing on the property of their heroic astronaut. Their heroic astronaut is on the next planet over sleeping in an identical bed, but what is that to them? If they believe $\neg S_1$, then they have grounds to arrest you.

If the problem is indeed underdetermined, then they will not have reasonable grounds to decide whether S_1 is true or not. They may adopt an agnostic position and refuse to affirm or deny any of the members of $\mathcal{S}$, or they may adopt a fideist position and believe one of the members of $\mathcal{S}$ on faith.[17] If the latter, they should welcome you if they are charmed by S_1 but arrest you otherwise. If the former, their choice is not so easy. Although they don't wish to *believe* any member of $\mathcal{S}$, they are forced to act toward you in some way or other. They might reason in this way: Since no considerations could favor a member of $\mathcal{S}$ over any of the others, then they should assume that the members of $\mathcal{S}$ are equiprobable.[18] They know that if S_1 is true, then you are their hero, but if some other member of $\mathcal{S}$ is true, then you should be arrested. S_1 is measure zero in $\mathcal{S}$, so they may safely ignore that possibility. You are arrested for trespassing in the night, and you are forced to sell your rocket ship to pay legal fees. Tragic, no?[19]

[16] If S_1 is true, then the residents are we Earthlings. Otherwise, not.

[17] Fideism and agnosticism are here meant in the sense developed in §1.3.3.

[18] This appeal to the principle of indifference would be irresponsible of them, I suppose.

[19] The tragedy is acute, since either you were jailed unfairly (if S_1 is true) or other poor astronauts are treated as roughly as you are (otherwise).

Simplicity itself

It seems that in order to avoid arrest, you must show that the choice between members of $\mathcal{S}$ is not underdetermined and that S_1 is to be preferred. You note that if S_1 describes the universe as having m objects in it, then S_n describes the universe as having $n \cdot m$ objects. Invoking Occam's Razor, you conclude that S_1 wins out. Yet the prosecutor may insist that Occam's Razor applies to *kinds* rather than to individuals and note that the ontological excess of S_ω consists of more *things* but no more kinds. He insists further that infinite space is sufficiently simpler than unbounded, finite space to justify believing that space is infinite whenever possible. Thus, he concludes, S_ω is to be preferred. Insofar as *simplicity* is an underanalyzed desideratum, it is unclear what the jury should make of these appeals.

Empirical equivalence

It may be tempting at this point to say that the members of $\mathcal{S}$ are all empirically adequate and that there is no way to decide between them. Whatever else might be said about empirical equivalence— and a great deal *was* said in the previous chapter— the members of $\mathcal{S}$ are *not* empirically equivalent. Suppose we consider theories empirically equivalent if they entail all the same observation sentences. Given S_1, you can truly say upon arriving to the planet, 'Here is Earth.' Given any other member of $\mathcal{S}$, you cannot make this observation. Thus the theories would not be equivalent. We might instead follow Quine [Qui75] and adopt a behaviorist conception of observation sentences, but on Quine's account the members of $\mathcal{S}$ are not distinct theories.[20]

Suppose instead we follow van Fraassen [van80], who considers theories to be sets of models or structures, and call theories empirically equivalent if they have the same observable sub-structures. Yet the planets in each of the members of $\mathcal{S}$ are observable, so each of the theories has different observational sub-structures.

[20]Regarding Quine and the problem of identical rivals, cf. §1.2.3.

S_1 has a solitary planet Earth, S_2 has a pair of distinct planets 'Earth', and so on. This consequence could only be avoided by specifying the members of $\mathcal{S}$ in a language without an identity predicate, but then the theories will be satisfied by all the same models— they would be the same theory and not genuine rivals.

So the problem of reduplication does not fit well into the rubric of empirical equivalence: If the rivals come out as distinct, they count as *empirically inequivalent*. So, the choice simply cannot come out as underdetermined— not because you could decide between the members of $\mathcal{S}$, but because the language of empirical equivalence is not up to the task of describing the case. Were you to make this rocket journey, you would find such an analysis to be frivolous logic chopping. The sense of underdetermination developed in Ch I can make sense of that underdetermination in this case, providing a strong reason to favor it over the usual story about empirical equivalence.

Observational cosmotopology

Abandoning the narrative for a moment, one might respond to this example by noting that it is purely hypothetical. *If* you travelled away from Earth and found an Earth-like planet *then* you would be unable to decide between the members of $\mathcal{S}$. The antecedent is rather fanciful, so we should not get too excited about the consequent. A bottom-up argument that relies on a complicated, counter-factual scenario shouldn't lead us to expect underdetermination all over. The argument goes wrong— one might say— not because the choice fails to be underdetermined, but because the underdetermination follows from features of the particular, fictional case.

This reply simply won't do. The example of your rocket journey is simpler than actual cosmology in several respects, of course, but similar difficulties may arise in the context of relativistic cosmology. You will never get in a rocket ship and travel across the universe, but spacetime might be multiply connected in detectable ways.[21]

At the end of the 19th century, Karl Schwarzschild suggested that we might look for distant images of our own galaxy [Sch00]. Suppose we looked out with our telescopes and saw images of the Milky Way repeated out into infinity—we might think either that a distant galaxy strongly resembles our Milky Way or that, because of the geometry of space, a galaxy that appears to be in the distance *is* our Milky Way. Schwarzschild explains:

> One could imagine that as a result of enormously extended astronomical experience, the entire Universe consists of countless identical copies of our Milky Way, that the infinite space can be partitioned into cubes each containing an exactly identical copy of our Milky Way. Would we really cling on to the assumption of infinitely many identical repetitions of the same world? In order to see how absurd this is consider the implication that we ourselves as observing subjects would have to be present in infinitely many copies. We would be much happier with the view that these repetitions are illusory, that in reality space has peculiar connection properties so that if we leave any one cube through a side, then we immediately reenter it through the opposite side. [Sch00, p. 2544]

He identifies an intuition that infinite repetition without identity is absurd. Interestingly, he thinks we would find finite space reassuring, since it would give us the prospect of having surveyed all of space just as we have surveyed all the Earth. Yet this reassurance carries no logical force, and the *absurdity* of infinite repetition is not a manifest contradiction. S_ω is consistent and as much in agreement with the imagined evidence as S_1. Nothing Schwarzschild says disarms the *prima facie* underdetermination between S_1 and S_ω. He speaks elsewhere in the essay of what is *true* or *real*, but here he speaks of our *happiness with a certain view*. This suggests fideism: Because the choice between S_1 and S_ω is underdetermined, we may believe whatever will make us happiest. Schwarzschild says nothing further to dispel the many worries one might have about this resolution to the problem.[22]

[21] Luminet, et al. provide an excellent informal introduction to these issues [LSW99].

[22] As we saw in §1.3.3, fideism might disrupt the scientific community or lead scientists to develop poor habits of thought.

Subsequent work has taken up Schwarzschild's suggestions, but attempts to identify multiple images of the Milky Way face considerable obstacles. Because the images that travel further would take longer to arrive, the images we could see now would portray the Milky Way at different times. Further, each successive image would be shifted and show the galaxy from a different angle. Attempts to reidentify quasars and galactic clusters have faced similar difficulties. Phenomena like gravitational lensing complicate matters further, because there would be multiple images of some objects even if space is simply connected. A recent review concludes that there is "little chance to recognize different images of a given object" [ULL00, p. 7].

Recent work has attempted to develop statistical tests to distinguish between observations of independent objects in simply connected space and repeated observations of the same objects in multiply connected space. The so-called *crystallographic* method analyzes catalogs of astronomical objects of a given type and plots the pairwise distances between them. For each multiply connected geometry, there is a characteristic distance between images of the same object. If the universe were a billion lightyears across, for instance, every object would repeat with a billion lightyears between repetitions. When the distances between all objects of that type were plotted on a histogram, the repetitions would create a spike in the graph at a billion lightyears.

Unfortunately, the crystallographic method relies on the catalog of astronomical objects. These catalogs are problematic in themselves, since the position of each object in real space must be inferred from angular position and redshift [ULL00, p. 7]. Inferring from redshift to distance requires making cosmological assumptions. Also, problems with gravitational lensing and the motion of objects remain, although one may hope that these effects are not so large as to wash out the repetition.[23]

Other methods aim to find evidence of multiple connection from the

[23]Hopefully, these effects would blunt rather than eliminate the spike in the histogram.

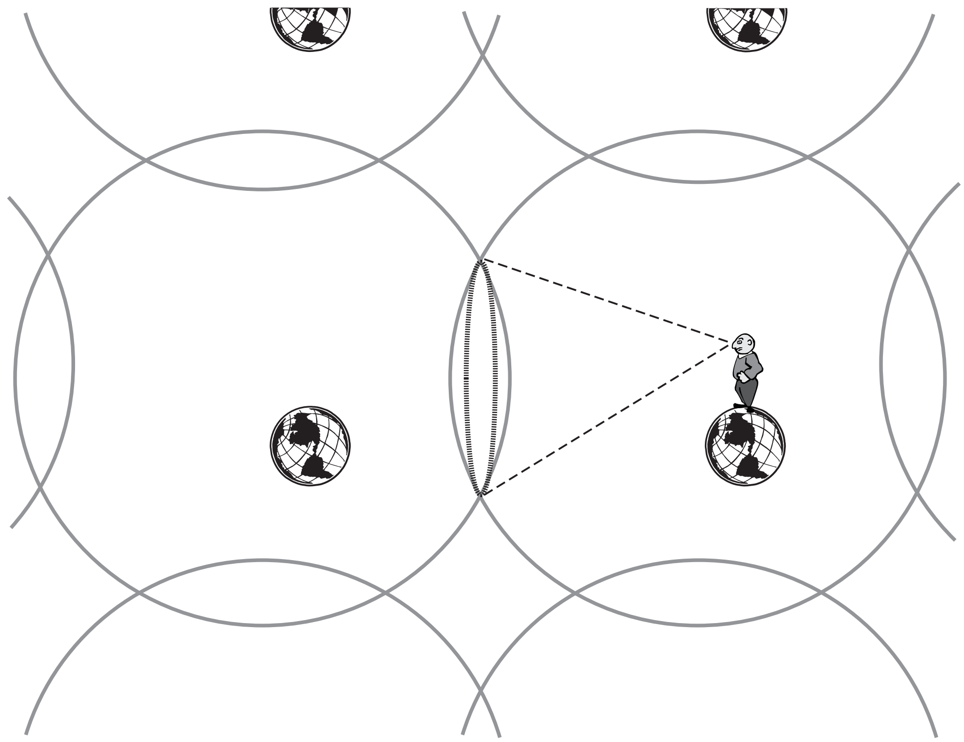

Figure 4.8: In multiply connected spacetime, the cosmic background radiation would overlap with itself. The phenomenon would appear as rings in the background to an observer on Earth.

record of the cosmic microwave background. The cosmic background has expanded from the birth of the universe at the speed of light and comprises the edge of what we can observe; analysis of it thus does not rely on problematic inferences from redshift to distance. If space were multiply connected, then the background surface would eventually cross itself. The sphere would overlap to form circles, as in figure 4.8 [CSS98] [Wee98]. Our observations of the cosmic background are still too imprecise to discern whether such overlapping is present [Ino01].

There is no denying that work being done in this area is ingenious, but it does nothing to speak to the issue of underdetermination. If the correlated pairs in our astronomical catalog exhibit certain features or if rings can be distinguished in the cosmic microwave background, physicists are prepared to conclude that the universe is multiply connected. In this, Schwarzschild correctly predicted what we would *happily* infer. Underdetermination scenarios like S_ω go unmentioned. Is this because contemporary physicists are fideists, as Schwarzschild seems to have been?

Non-demonstrative geometry and rules of repetition

We saw two responses above to Reichenbach's Theorem θ, either one of which would be sufficient to defeat bottom-up arguments based on it: Theories about physical geometry were not underdetermined, and universal forces proved to be more bizarre posits than they at first appeared. We can make similar responses to massive reduplication.

First, consider physical geometry stripped of any indexicals— call this *non-demonstrative geometry*. It would be a catalog of things and spatial relations: A planet of a certain local description is in such-and-so a configuration with respect to planets of identical local descriptions, and so on. By hypothesis, all members of $\mathcal{S}$ have the *same* non-demonstrative geometry. Just as Theorem θ was insufficient to make our choice of physical geometry underdetermined, the possibility of massive reduplication is insufficient to make our choice of non-demonstrative geometry underdetermined. We may not be terribly interested in non-demonstrative geometry, but that's beside the point. It's enough to show that the underdetermination of physical geometry on account of possible reduplication doesn't show that all theory choice is underdetermined. Why should we suppose that other scientific theory choices are more like the choice of a physical geometry than like the choice of a non-demonstrative geometry? This is rather like the question that concluded §4.1.2, so one might suspect that there will always be further underdetermination that forces a retreat to weaker and weaker theories: we retreat from actual geometry, to physical geometry, to non-demonstrative geometry, to something weaker still. Yet this retreat is prompted first by problems with metrical structure and then by problems with topological structure. There is no further kind of structure involved in specifying a geometry, and so there is no further structural argument that could force our retreat. It ends here.

Second, we note that a demonstrative geometry requires both a specification of the underlying geometry and a rule of repetition. Each member of $\mathcal{S}$ (except S_1) thus presumes a law-like connection between events on each of the Earths that preserves the reduplication: Each planet sends out an astronaut, each astronaut behaves in the same way, and so on. Since S_1 posits only one Earth, it does not require a rule of repetition. Note also that the underlying topology of space in S_1 and S_2 is the same; they're both toruses. Since S_2 requires this topology *and* a rule of repetition, S_2 is just logically stronger than S_1. Thus, S_1 will always be better confirmed.[24] Applying the same reasoning, S_1 is to be preferred over $S_3, S_4, \cdots$.

In this way, we can dispose of all the S_n's for $1 < n < \omega$. Since S_ω has a different topology than S_1, it remains in contention. This justifies Schwarzschild's intuitions that S_1 and S_ω are the only real contenders. How can the physicists' implicit preference for S_1 be motivated? There has, historically, been a presumption of infinite, simply connected space (the topology of S_ω). Since geometry has come to be an empirical matter, both simply connected and multiply connected space are contingent possibilities. The crucial difference isn't there. S_ω posits an infinite repetition of the entities posited in S_1, and the difference between S_ω and S_1 amounts to the difference between believing or not believing in infinite repetition. That is the crux of the matter; if scientists have good reasons for eschewing claims of infinite repetition, then they have good reasons for preferring S_1.[25]

The point is clearest if the universe is indeterministic. Suppose, for instance, that radioactive decay is a genuinely random phenomenon. You travel to a distant planet that might be Earth (if S_1 is true) or it might not (if S_ω is true). You make a series of observations of radioactive decay and share your results with the denizens of this planet. You then return to Earth— where an astronaut has come and performed similar experiments— to see how the experiments on Earth

[24]There may be reasons to prefer logically stronger theories in some cases (e.g., if they are predictively more accurate), but no such reasons are present here.

[25]Reichenbach concludes, similarly, that "the topological properties of space are closely related to the problem of causality; *we assume a topology of space that leads to normal causal laws*" [Rei58, p. 80, emphasis in original].

went. The results of your experiment always match the results of the experiment on Earth, so either they were the same experiment (and S_1 is true) or there is a law-like connection between the different experiments.[26] A law-like connection contradicts the assumption that the phenomena studied are genuinely random, so S_1 must be true.

S_ω's requirement of infinite repetition amounts to a causal constraint that the infinitely many copies of each thing will behave in the same way. This constraint would necessarily be deterministic— otherwise the resemblance between the infinitely many planets would break down over time. If we think that the universe is indeterministic, this rigid parallelism is a non-starter. Even if the universe is deterministic, however, the law of repetition is still unlike other claims that scientists accept. Like the universal force in Reichenbach's Theorem θ, a law of infinite repetition is a *sui generis* kludge.

Relativity requires that the laws of physics be Lorentz invariant— that is, they must hold the same way for you regardless of where you're going or how fast. The laws of physics aren't supposed to have a preferred reference frame. The conflict is this: Infinite repetition stipulates that what is happening here is happening in the same way just at this moment on all the other Earths. But according to relativity there is no general answer to what is happening 'just at this moment' at two space-like separated points; simultaneity is relative to reference frames. Thus, S_ω picks out a special reference frame, the frame in which repetition occurs.

A different way of seeing the point: Relativity is usually taken to prohibit superluminal influences— that is, it's impossible to send a message at faster than the speed of light. Yet, given S_ω, you can send a message instantaneously across space. Imagine you arrive at the next planet and want to send a message home. A radio message would take a very long time to cover that distance. So, instead, you write a message on a piece of paper and drop it on the ground. Because of

[26] Note that if the results do not match, then no member of $\mathcal{S}$ is true.

infinite repetition, you know that another astronaut has dropped the same message (although not the same sheet of paper) back on your planet Earth. Your message is sent.

Scientists thus have good reason to reject S_ω.

A few attempts at defending rules of repetition

One might try to defend S_ω by noting that there's no Lorentz invariant way to formulate quantum mechanics, either. Since physicists accept quantum mechanics, why not infinite repetition? The relation between relativity and quantum mechanics is a complicated subject,[27] the cases are very different. First, we have independent reasons for accepting quantum mechanics. It's been successful in many experimental domains. S_ω has no independent motivation. Second, there is no Lorentz invariant alternative to quantum mechanics. There is a Lorentz invariant alternative to S_ω; viz., S_1. Third, quantum mechanics does not allow for super-luminal messaging. According to S_ω, we send superluminal messages all the time. Finally, although quantum mechanics picks out a preferred reference frame metaphysically, it does not do so epistemically. There is no way we could learn which reference frame is preferred.[28] Determining the preferred reference frame in S_ω is trivial.

One might defend rules of repetition in a different way. In a deterministic universe, repetition need not be posited as a persistent causal law. Rather, it might obtain on account of special initial conditions— the contents of the universe were repeated i times over at the beginning. Yet, a peculiar initial condition of this kind is as odd a duck as a law of infinite repetition. Perhaps it is even the same duck— as Craig Callender suggests [Cal], special initial conditions of this kind are tantamount to a law.

[27]Maudlin provides a thorough discussion of these issues [Mau94].

[28]Although this is not true of all interpretations of quantum mechanics, when true it is considered a virtue.

4.1.4 Spacetime and clothesline arguments

The last section concerned rivalry between members of $\mathcal{S}$– the argument, if correct, shows that S_1 is to be preferred over other the members of $\mathcal{S}$. This set of rivals made sense in the story. The question was whether you had arrived at Earth or a dopplegänger Earth, because you had *ex hypothesi* travelled away from Earth and found a planet indistinguishable from Earth. Concentrating on this set of rivals would also make sense if astronomers observed rings in the cosmic background (as we above saw in §4.1.3).

You have not gone on a rocket journey, however, nor have astronomers made detailed enough observations of the cosmic background radiation to say whether rings are present or absent. It makes sense then, to consider the possibility that spacetime is simply-connected and that there are no law-like repetitions of the stuff in the universe. This would entail that if you can travel away from Earth in any direction, then you will neither come to another planet just like it nor will you come upon it again (without having gone in a circle). Call this possibility S_0. Given the state of present evidence, we should consider both S_1 and S_0 to be live options. The choice between them is underdetermined with a scope that includes at least our present situation.

If the universe were Newtonian, then it would be possible for us to escape the underdetermination. As Clark Glymour explains,

> Many topologically different Newtonian models can be distinguished empirically either by making global journeys through space or by observing systems that have made such journeys. The possibility of such journeys results not solely from the fact that Newtonian theory allows arbitrarily fast causal signals, for even very slow signals can make transits of the universe, given enough time. [Gly77, p. 50]

In a non-Newtonian universe, we have no such guarantee. Indeed, general relativity entails that there are some global features of spacetime that— were they to hold— would preclude our learning that they held.

David Malament [Mal77], extending results due to Glymour [Gly77], summarizes some of these properties. One such property is *causality*, which is said to hold if there are no closed, future-directed causal curves; that is, if causal loops are not possible. If causality is violated, then perhaps there is some path through space time which, if you followed it, would lead you to your own third birthday party.[29] Malament argues that "if causality is violated in a space-time, some observer will know about it; if on the other hand it is not violated, no observer will ever know for sure one way or the other." To put this in terms of underdetermination, let the two rivals be Loop and NoLoop:

Loop Causality is violated, and there are causal loops; and

NoLoop Causality is not violated, and there are not causal loops.

Malament's proof turns on showing, for a spacetime M in which NoLoop holds, there is another spacetime M' in which Loop holds such that no observer in M could tell that they weren't in M'.

The construction [Mal77, pp. 69–70] goes in this way: For each spacetime point x, define $P(x)$ as the *causal past* of x. $P(x)$ contains all the points from which signals could possibly have been sent to x; that is, all the points in the past lightcone of x. Now take a countable set of points $X = \{x_i\}$ such that the causal pasts of the member of X cover M; that is, $\cup\{P(x_i)\} = M$. We can then extend each $P(x_i)$ with some suitably chosen *spacetime filler*. Finally, we add a causal loop to the spacetime filler. Malament calls this a "clothesline construction", because each of the members of X is hung like laundry on the spacetime filler— see figure 4.9.

If NoLoop obtains, then each observer occupies some point in a causal spacetime M. Any observations possible from a point x must be of things in the causal past of x— Malament calls the set $P(x)$ the "observational past" of x. Yet for all $x \in M$, $P(x)$ is isomorphic to some region of the constructed 'clothesline space' M'. So any observations possible from x are compatible with being in M

[29]No paradox would threaten, because you could only go on to influence your younger self in this way if your older self did in fact influence you at your third birthday party.

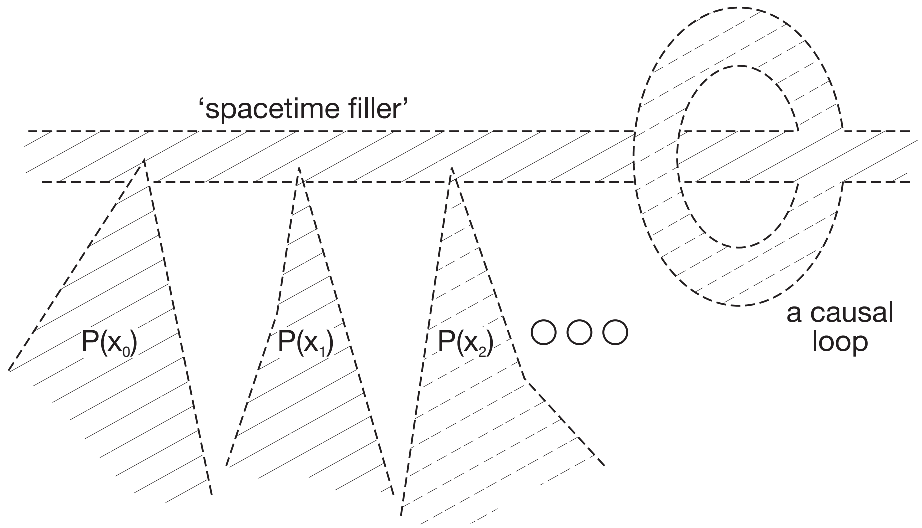

Figure 4.9: The construction of M', a space in which each of the possible past histories $P(x_i)$ can be embedded isometrically.

or being in M'; that is, they are compatible with NoLoop and with Loop. So an observer in M could not tell if she was in M or in M'; she could not tell which of NoLoop or Loop was true.

Conversely, if Loop obtains, then a causal loop exists in the causal past (and future) or some point x. An observer at x could, in principle, detect the causal loop and thus demonstrate that Loop obtains.

So, the choice among {NoLoop, Loop} is underdetermined with a scope that includes all circumstances in which NoLoop is true and some (but not all) circumstances in which Loop is true. Other properties of spacetime can be shown to be underdetermined by similar constructions; Malament claims that underdetermination of this sort "is so widespread in the class of spacetimes as to be of epidemic proportions" [Mal77, p. 69]. What conclusions can be drawn from this sort of underdetermination?

It may be that the underdetermination involves an overly strict standard for responsible theory choice. One may argue that there are non-observational factors that should decide between Loop and NoLoop. One might perhaps even argue that there is a sort of observation that reaches outside the observer's past lightcone. Nevertheless, these routes do not seem promising. In general relativity, questions of the geometric structure of spacetime are an empirical matter, and the invention of a supra-luminal chronoscope seems like a longshot.

Underdetermination of "epidemic proportions" might seem like good support for a bottom-up argument for the ubiquity of underdetermination. Yet the epidemic infects the class of possible spacetimes in the context of general relativity. One of two things may be the case: Either we may responsibly rely on general relativity or we may not. If we may, then general relativity is not underdetermined relative to its rivals. The case would thus *undercut* bottom-up arguments by providing a powerful example of a theory choice that is not underdetermined. If we may not rely on general relativity, then the clothesline contruction is just so much hemming and hawing— without the background theory, the construction shows nothing.

One might still think that the case does show something general. The background theory here entails that certain theory choices are underdetermined. If we consider the selection of background theories, then perhaps similar underdetermination will arise. Just as the existence or nonexistence of causal loops is underdetermined given general relativity, the truth or falsity of general relativity might be underdetermined given the background for that theory choice. Yet this worry is the familiar Duhem-Quine problem with auxiliary hypotheses that concerned us in §3.3. We worried there the rectilinear propagation of light (T_L), even though no further underdetermination is entailed by T_L. DQ worries can be raised whether or not general relativity leaves particular choices underdetermined.

4.1.5 Verdict on geometry

The chapter so far has considered geometrical features of space or spacetime that allegedly exhibit underdetermination. In some cases, the underdetermination arguably does not obtain at all (§§4.1.2, 4.1.3). The formal nature of

	This choice isn't underdetermined	Slight variant isn't underdetermined	Requires background theory
M_0 or M_7?		✓	✓
Universal forces?	✓	✓	
Massive reduplication?	✓	✓	
Causal loops?			✓

Figure 4.10: Specific cases from geometry fail to support bottom-up arguments.

geometry makes it especially recondite in certain respects, but that shows nothing about biology, psychophysics, geology, hydrostatics, or the rest of the sciences. Even were we to grant that the above cases were legitimately underdetermined, the underdetermination would prove too specialized to show anything about science in general. Where the underdetermination does not plague some related theory choice (§§4.1.1, 4.1.2, 4.1.3), there is no clear reason to generalize from one choice rather than from the other. In some cases, the very demonstration that there is underdetermination requires presuming a determined background theory (§§4.1.1, 4.1.4). These results are summarized in figure 4.10.

4.2 The road by induction

The geometrical bottom-up arguments discussed so far generalize from a single example or a carefully selected set of examples. One might instead attempt to survey the history of science, look at a sample of past theory choices, and generalize from them. This sort of induction, one might hope, would support the bottom-up move better than isolated case studies. One such argument is already saturated in the literature, the so-called *pessimistic induction* against realism. Another, the aptly named *new induction*, has been offered as a proof of "the kind of underdetermination that the history of science reveals to be a distinctive and genuine threat to even our best scientific theories" [Sta01, p. S12].

4.2.1 The old induction

Worrall [Wor89b] calls the pessimistic induction the main consideration against realism. Although the general argument goes back at least to Duhem and Poincaré, contemporary discussions typically cite Larry Laudan's 'Confutation of convergent realism.' Laudan writes:

> I daresay that for every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring. [Lau81a, p. 35]

The historical record is taken to show that most widely-believed past theories were successful but ultimately false, so we are encouraged to think that our present successful theories will turn out to be false as well. The fate of the pessimistic induction need not concern us, because the induction shows nothing about underdetermination.[30]

Consider some theory from the past, such that past scientists believed it but that we now reject it. This does not show that the belief in the theory was underdetermined. It is possible that, given the circumstances of earlier scientists, relevant considerations picked out the theory uniquely and that, given our circumstances, relevant considerations pick out one of its rivals. The choice need not be underdetermined in either circumstance. One may object that this overlooks the issue of truth: If the pessimistic induction is sound, then we have reason to believe that our present theories are false. Since they could be shown to be false,

[30] There are many objections to the pessimistic induction in the literature. Many authors have attempted to show that the historical record provides little ground for pessimism, e.g. Kitcher [Kit93] and Psillos [Psi99]. Peter Lewis has recently argued that the inference from a history of failures to pessimism relies on a statistical fallacy [Lew01]; I have (with Craig Callender) discussed Lewis' objection elsewhere [MC].

however, believing them *to be true* would be determined against and *a fortiori* not underdetermined. This mismatch between the pessimistic induction and the problem of underdetermination should not come as much surprise. Laudan, without an air of contradiction, argues both *pro* the pessmistic induction [Lau81a] and *contra* underdetermination [Lau90] [LL91].[31]

4.2.2 The new induction

Kyle Stanford, following suggestions in Duhem and Sklar, has called for a *new induction* [Sta01].[32] The historical record, Stanford thinks, is full of episodes that fit this schema: At some past time, scientists accepted a theory T that was superior to all its *acknowledged* rivals. Subsequently, a rival U was developed that came to replace T. The evidence that favored T over its acknowledged rivals also favored U. The choice between T and U was underdetermined, even as scientists assented to T— T was underdetermined relative to a rival that scientists hadn't yet imagined.

As Stanford notes, T and U would not be empirically equivalent; later scientists find evidentiary grounds to prefer U. The underdetermination is, to use a term Stanford takes from Sklar [Skl85b, p. 30], *transient.* We may put it this way: The *scope* of the underdetermination includes only a specific historical period. Outside of that period, standards favor one theory over the other. The *rivals* are the dominant theory in this period and a theory which had yet to be formulated. Thus, one of the rivals was not available to scientists within the scope of the underdetermination.[33] The *standard* is left implicit for now.

One might worry about Stanford's insistence on *equal* confirmation of T and U; if degree of confirmation were a function from evidence and a theory to the real numbers, then we should expect that two different theories would *never* be

[31] Although it appears that Laudan does endorse the pessimistic induction, his essay might instead be read as providing counter-examples to the realists' no-miracles argument. Read in that way, the essay is straight-forwardly about something besides underdetermination.

[32] I am indebted to Stanford for helpful comments on an earlier draft of this section.

[33] The unformulated rival would thus not have been a *live option* for the scientists; cf. §1.2.1.

equally well-confirmed.This objection, however, would be spurious. As Stanford notes, two theories may have different evidential successes and different anomalies. During periods where the old theory is weighed down by anomalies but the new theory is yet to score a panoply of successes, the choice between them is underdetermined. Such a period may be protracted, especially as conservative scientists work to extend the old theory and reformers refine the new one. Thus, it is implausible to insist that confirmation is on a knife edge. Two theories may be remain (nearly enough) equally well-confirmed over a period of time even as evidence changes.[34]

Stanford admits that he is "unable to do more... than suggest... the verdict of the historical record" [Sta01, p. S11], but he nevertheless provides a flurry of examples. He mentions mechanics, chemistry, embryology, thermodynamics, electromagnetic theories, theories of disease, theories of light, theories of inheritance, theories of evolution, "and so on in a seemingly endless array of theories, the evidence for each of which ultimately turned out to support one or more unimagined competitors just as well." He concludes: "Thus, the history of scientific inquiry offers a straightforward inductive rationale for thinking that there typically are alternatives to our best theories *equally* well-confirmed by the evidence, even when we are unable to conceive of them at the time" [Sta01, p. S9, my emphasis].

Suppose, as Stanford suggests, that we look to the history of science and find many cases that match the schema. We might conclude from this that our present theories are underdetermined against some as yet unimagined rivals.[35] Why does the fact that U is unformulated make any great difference? Imagine that T instead faces a rival S, one that scientists are mindful of at a time when T is reasonably preferred. New evidence is collected such that the choice between S and T is underdetermined, and when more is collected S is determined univocally. (Alternately, there is a theory ascendant in some related discipline such that S

[34] In what follows, for reasons of readability, I write 'as well-confirmed' without adding 'effectively' or 'nearly enough'.

[35] One may worry that instances of this schema being *common* is insufficient to support an inductive generalization. One would further need to know something about how observed instances relate to the population of past and present theories; cf. [Lew01] and [MC].

wins out on systematic grounds.) The case of T and S is the familiar scenario in which one theory dominates for a time, it is underdetermined relative to some rival during a period of controversy, and its rival wins out. What difference does it make that U is unimagined when T dominates but that S is already imagined? The asymmetry is that the choice between U and T *had always* been underdetermined, that reasonable standards would never have picked T over U. If U is actually developed when T begins to strain under the weight of anomalies, however, doesn't that suggest that the choice is only underdetermined once those anomalies are manifest? Plausibly so, and for that reason the new induction doesn't show that our present theories (the ones not in crisis, at any rate) are underdetermined relative to as yet unimagined rivals.

The underdetermination between the decline of a preceding theory and the ascension of a successor is not sufficient to support the new induction; that is the familiar underdetermination during times of legitimate (and often open) controversy. The problem is only acute if the successor is just as well-confirmed as the predecessor at all times before the successor is proposed. At the very least, the underdetermination must obtain well before the predecessor theory is in crisis. Is this ever plausibly the case? Take the example of classical and relativistic mechanics. There was clearly a period in the late 19th century— with irregularities in the perihelion of Mercury, the absence of ether drift, and so on— when classical mechanics faced anomalies unlike ones it had faced previously. Surely, there is some time before the actual introduction of special relativity when it would have been a viable contender. (This could only fail to be the case if special relativity was proposed at the first instant in history in which it would have been viable. Even if this was so, it would be far-fetched to imagine that such extreme timing characterized all or even most of Stanford's examples.) Yet to be an instance for the new induction, special relativity must have been as well-confirmed as classical mechanics before the anomalies developed. Is it reasonable to think that special relativity was well-confirmed in the late 18th century, even as Kant penned a metaphysical foundation for classical mechanics?

It's unclear how we should even go about answering this question of counter-factual history. For myself, I find it *ridiculous* to suggest that Kant should have given as much credence to Einstein as to Newton, imagining that he had entertained relativity but considered only the evidence available at his time. Intuitions may differ about this, and it may be that no amount of historical evidence will make the same intuition salient to all philosophers.

Regarding these cases, Stanford takes his cue from Sklar. Yet Sklar does not suggest, as Stanford does, that Einstein and Newton were ever on equal footing. Rather, he thinks that the history of science shows us how "the wealth of data previously taken to support Newtonian theory was, *when taken in conjunction with new data incompatible with the older theory*, equally supportive of novel theories incompatible with the Newtonian" [Skl85a, p. 149, my emphasis]. Sklar's point, then, is that present evidence may form *part* of the *body of evidence* that eventually favors a different theory than the one we favor now. This is true, but it does not follow that the present body of evidence favors the future theory.

For us fallibilists, the new induction yields only old news. We are prepared to admit that our best present theory might lose out to some future contender, and it seems only a small corollary that some future contenders might be in fighting shape even now to fight our present theories to a tie. It is hard to see how the new induction amounts to anything more than this.

Aside: Realism and the new induction

Perhaps I've missed the point of the new induction. As a weapon against realism, it need not show that selection from among imagined alternatives is irresponsible. It need only show that selection from among imagined alternatives is unlikely to get us the truth. If Kant's epistemic situation left him accepting Newtonian mechanics— either because he had not thought of relativity or because relativity would not have had as much warrant as Newtonianism— then we might think that he was simply in no position to know the truth. Yet contemporary realists will protest both that Kant did have warrant for accepting Newtonian mechanics and that Newtonian mechanics was *approximately true*. Just so, the realist says, we now have warrant for accepting relativity and it is both approximately true and *closer to the truth* than Newtonianism. Insofar as the realist can appeal to somesuch story about approximate truth, the new induction gives him nothing to worry about. If the story about approximate truth fails, the realism would fall apart— yet it would then fall apart whether or not the new induction was in play. For this reason, the new induction puts no new pressure on the realist. It can at most underscore the already acknowledged dependence of realism on an account of approximation.[36]

4.3 The road to relativism

> "You know what the truth is? It's some crazy thing my neighbor believes. If I want to make friends with him, I ask him what he believes. He tells me and I say 'Yeah, yeah—ain't it the truth?"'
>
> —Rabo Karabekian [Von73, p. 209]

The relativist begins by noting that scientific standards can themselves be contested. If theory choice were often or always underdetermined relative to uncontested standards, then the only way to decide between rival theories would be to adopt a contested standard. Without further constraint, one might choose any standard at all: Anything goes.

The relativist might hope to offer a general argument to show that underdetermination is rampant and that standards are unfettered in this way. A naïve top-down argument suggests itself:

For any set of rival theories, there is *some* standard such that the

[36] In order to say that relativity was a respectable option in the time of Einstein but not in the time of Kant, the realist must say that scientific respectability is contextual and historical. But savvy realists already acknowledge this; cf. [Boy82].

> choice between the rivals is underdetermined with arbitrary scope.[37] Thus there will *always* be an underdetermination scenario that shows how disputes over scientific standards could become central.

The premise of this argument is trivial to prove:

> For a set of rivals $\{\alpha, \ldots, \omega\}$ and a scope $\mathcal{C}$, let the standard be, "If you are in a circumstance in $\mathcal{C}$, remain agnostic; otherwise accept α." For all and only circumstances in $\mathcal{C}$, this standard is insufficient to distinguish between the rivals— i.e., the choice between them is underdetermined. QED

The proof, however, is ridiculous. The only thing to be said for this *ad hoc* standard is that it provides the example needed for the proof. Nothing recommends *adopting* it, and only a madman or a philosopher would be willing to do so. This should be no surprise— it should by this point be familiar that there are many cases of underdetermination which present no *actual* barrier to theory choice.[38] Thus the conclusion of the naïve argument does not follow. There is no a priori assurance that standards can be contested and that *credible* rival standards will be available. Actual examples will be needed to show that significant underdetermination actually obtains in science.

One might employ a variant of the naïve argument to outline a schematic underdetermination scenario and then supply examples to show that scientific episodes instantiate this schema. The remainder of this chapter concerns attempts to do this.

Moderate relativism

Gerald Doppelt argues that scientific standards are historical— in the sense that they change over time— and thus that scientific judgment is always relative to the standards of some particular scientific community. He calls this position *moderate relativism.* It is moderate in the sense that there always are standards that govern theory choice, even if those standards change over time and

[37] This point was made previously in §1.4.

[38] We have seen *inter alia* sceptical Cartesian scenarios (Ch II) and cases of meaning change (§3.4).

vary between communities. Doppelt originally offered moderate relativism as an interpretation of Kuhn [Dop78] and still sees it as a corollary of "Kuhn's most important claim" [Dop01, p. 160].[39] This is a characterization that Kuhn would have resisted (cf. [Kuh77]), but let that be as it may. What Doppelt took away from Kuhn was that

> which of the various good reasons for and against each of the rival paradigms is ultimately perceived as compelling (and thus determining for choice) itself depends on which paradigm's internal criteria are embraced. This latter choice is itself bounded by reasons but underdetermined by them: equally rational, scientific, and responsible practitioners of an established paradigm confronted by a revolutionary rival in a period of crisis make opposite choices at every point in the process through which the new paradigm gradually comes into dominance. [Dop78, p. 54]

Doppelt argues further that "every new paradigm— no matter how successful and well-established it becomes— involves *losses* as well as gains with respect to its predecessor(s) in terms of the kinds of data and problems it... can in fact handle" [Dop78, p. 44]. In this early work, Doppelt claims the choice of *paradigm* is underdetermined, but in subsequent work he drops that notoriously obscure term. He continues to maintain, however, that "in the course of its development, science changes... its conception of the very kinds of evidence, empirical data, theories, explanations, reasoning, and standards which are, and are not, *genuinely* scientific and conducive to *genuine* scientific knowledge" [Dop01, p. 160, emphasis in original]. Disputes between rival conceptions of science leave fewer standards shared by both sides. Given this small pool of shared standards, choice between the rival theories is underdetermined.[40] The choice between rival theories is *not* underdetermined given either set of contested standards; rather, each set of standards favors a different rival. Of course, these self-consciously historical arguments will stand or fall with an examination of the history of science.

[39] Doppelt has developed moderate relativism in a series of papers, usually attributing the central insight to Kuhn— e.g.: [Dop81] [Dop83] [Dop88]

[40] The scope of the underdetermination— the range of circumstances across which theory choice is underdetermined— is left unspecified for now.

Moderate relativism should not be confused with radical methodological anarchism. Doppelt does conclude that "the reasons which drive commitment to standards are practical and interest-laden" [Dop01, p. 162]. Nevertheless, he does not see this as underwriting the direct engagement of interests and values in deciding which theory to believe. It is not as if scientists may legitimately appeal to any shared values to justify the choice of one theory over another. According to the moderate relativist, scientists justify their choice of theory by providing evidence, explanations, and what-not that they deem legitimate in light of their conception of science. It is only in changing their conception of science that scientists invoke their extramural interests.

In the course of history, our conception of science changes and with it the standards that count as scientific. All manner of contingent and parochial interests can underwrite changes in the prevailing conception of science, says the moderate relativist, but at each moment the standards for what will count as *good* or *scientific* theory choice will be determined by the prevailing conception of science. Thus, moderate relativism entails value-freedom of a certain sort; given a conception of science, standards for explanation, and so forth, contextual values have no direct rôle to play when scientists choose between rival theories. Values come into play when standards of theory choice are themselves in flux— not to settle the choice between theories, but to settle the choice between rival conceptions of science.

If it were *always* permissible to dispute standards and if *anything* could count as a potential conception of science, then moderate relativism would collapse into anarchism. It would always be possible for parties to a scientific controversy to propose *ad hoc* standards that would permit contextual values to decide between theories directly. If a scientist finds intelligent design theory to be congenial to his values, he could stipulate a standard that allows explanation by design; in the Kuhnian idiom, he could take intelligent design to be *paradigmatic* of good science. The problem with allowing such stipulation may be highlighted by choosing an even more *outré* example: The scientist may as easily take as paradigmatic a cosmology on which the universe is a twelve-dimensional sculpture of Hello Kitty, if he found that congenial. Doppelt imposes no formal or explicit constraints on what counts as a legitimate conception of science; it's not clear how he could without giving up his moderate relativism. Doppelt says— as he must— that conceptions of science are constrained by "practical reasons, interests, and investments" that explain "how and why individual scientific practitioners became interested in and identified with a particular practice of inquiry and a particular community of inquirers bound together by relations of trust..." [Dop01, p. 162]. So it's not always permissible to invoke a novel conception of science, but the constraint on such invocation is not a formal one. The case that shifting conceptions of science make for substantive underdetermination— the case for moderate relativism— can thus only be completed by citing episodes from the history of science in which the relevant sort of underdetermination actually arose. Doppelt appeals to the rise and fall of phlogiston and æther as episodes of this kind. I'll consider those cases in a moment.

Social knowledge

Helen Longino argues that scientific knowledge must be understood not as the mere aggregate of responsible individual judgments, but instead as the outcome of a responsibly constituted scientific community. The warrant for scientific claims, she thinks, comes from the structure of the community that licenses the claim. Longino's position can be seen as a species of moderate relativism: Which factors figure in responsible theory choice is determined by the shared standards of the scientific community. The shared standards themselves, however, change in the course of history and cannot be motivated merely by epistemic considerations.

Longino appeals to underdetermination to motivate her position, discussing underdetermination of two kinds. One kind results from the reliance on background assumptions, the other from the flexibility of epistemic considerations.

The ubiquity of background assumptions

Longino first appeals to a version of the argument from auxiliaries, discussed in the previous chapter (§3.3). A particular fact, Longino notes, only stands as evidence for a theory in light of other assumptions. Since those assumptions may turn out to be false, the inference from the evidence to the theory may turn out to be unjustified. The background assumptions may themselves be investigated, but any evidence for them will only stand as evidence in light of other assumptions. These background assumptions may also be debated, and so a regress begins. So, once science moves beyond things which can be seen directly, theory choice is underdetermined. The regress of justification will be stopped by further background assumptions, and perhaps these further assumptions will rest on non-epistemic values. Examples are required to show that it does so in important cases.[41] We can see, then, why Longino concludes:

> The argument so far has established that contextual values, interests, and value-laden assumptions *can* constrain scientific practice.... This is not yet to show that contextual values are always or necessarily implicated in scientific reasoning.... [Lon90, p. 83]

The flexibility of epistemic considerations

Longino also argues: Although we can list off factors like empirical adequacy, consistency, breadth, simplicity, and fecundity, such a list is insufficient to tell us which theories to accept. There may still be disputes about what these mean and how they should be weighed. Reasonable scientists can disagree about which of two theories is simpler, for instance, and furthermore about whether fecundity should trump simplicity. Here, values enter the process: “The particular weighting and interpretation assigned these standards will vary in different social and historical contexts as a function of cognitive and social needs” [Lon90, p. 77].

[41] Elizabeth Anderson suggests that the theory of relativity, functionalist sociology, and marginal utility theory provide examples that would serve Longino's needs. She writes, “When the data run out, values legitimately step in to take up the ‘slack’ between observation and theory” [And95, p. 29]. Unfortunately, she offers no reason to believe that the slack in these cases was taken up by value-laden assumptions.

One might worry that this argument is altogether too weak. Perhaps the differing application of scientific values provides, as Kuhn suggests, "an indispensable means of spreading the risk which the introduction or support of novelty always entails" [Kuh77, p. 332]. In this way, the vagueness of these values is their strength. It allows for disagreement, true, but without disagreement either every scientist would assent to a new theory or everyone would remain with the received view. The community would face the false dilemma between anarchy and orthodoxy. A reasonable disagreement allows individuals to pursue separate lines of research. One line will only become the dominant view if it gathers so much evidence that most everyone agrees to it, even with varying concern for and understanding of the standards by which a theory is judged.[42] This concedes that theory choice is underdetermined, but the scope of that underdetermination includes only circumstances of legitimate controversy and of transition from one theory to another. When a single theory is dominant in the community, it meets even the vague standards of practitioners. The moderate relativist needs underdetermination of a broader scope— underdetermination that affects even theories accepted by the whole community. (Longino might find this reply congenial. Although she can be read as a moderate relativist, her central aim is to show that science is necessarily social. This reply defuses the underdetermination by appealing to the social organization of science, thus *admitting* that science is a collective enterprise.)

One might worry instead that this argument is altogether too strong. If values were never sufficient to decide theory choice, then Longino would be right to think that theory choice is underdetermined on the basis of epistemic values. However, the situation would be no better for social values.[43] They are values, after all, so we could not decide between theories by appeal to them. Yet Longino suggests that the social resolves underdetermination! She may be forced to say that we should not worry about rivalry between theories which equally satisfy all

[42]This line of argument, present in Kuhn, has been advanced more recently by Kitcher [Kit93, ch. 5], Haack [Haa98, pp. 107-8], and the present author [§3.3.1].

[43]In Longino's terminology, epistemic and social values are *constitutive* and *contextual* values respectively.

our values. If we can be untroubled by such rivalry, though, why should we be troubled by rivalry between theories which equally satisfy epistemic values? It must still be shown that underdetermination occurs in matters which we think science can settle.

Neither of these ways in which theory choice *might* be underdetermined are sufficient to show that it indeed *is* underdetermined in actual scientific practice. Longino's general considerations are thus best read not as top-down arguments, but rather as considerations to soften us up for a bottom-up argument. Longino, like Doppelt, needs substantive examples.

Underdetermination of standards

The moderate relativist wants to make a distinction between theories (which are chosen on the basis of standards) and standards (which are chosen on the basis of values). So one might try to answer the relativist in this way. Let the rival theories be Sylph and Gnome. Suppose that Sylph is favored by one conception of science (call it Sylph-science) and that Gnome is favored by another conception of science (call it Gnome-science). Let Mere-science be the minimal shared conception of science, one insufficient to allow a responsible choice between Sylph and Gnome. The choice between Sylph and Gnome is underdetermined given Mere-science, but determined given either Sylph-science or Gnome-science. So how do we choose between Sylph-science and Gnome-science? If this rivalry can be settled by metatheoretic standards, then the underdetermination between Sylph and Gnome can be resolved.

Unfortunately, the metatheoretic standards required by this answer are fantasies. If there are metatheoretic standards that justify a shift from Mere-science to Mere-science+Sylph-science, then the standards must either be part of Mere-science or part of Sylph-science— they must either be part of what is agreed upon or part of what is contested between the rival groups. If the former, then the choice between Sylph and Gnome was never underdetermined. If the latter, then the standards beg the question in favor of Sylph. If Sylph-science is allowed to establish its own bona fides, why shouldn't Gnome-science be allowed the same latitude? Twelve-dimensional sculptures of Hello Kitty cannot be far behind.

One might say in this case that that there is underdetermination between Sylph-science and Gnome-science, but these are standards rather than theories. We should distinguish this *underdetermination of standards (by what?)* from *underdetermination of theory by evidence*. The latter is the usual sort of underdetermination and the one at issue here.

4.3.1 The case of phlogiston

Doppelt, who attributes moderate relativism to Kuhn, maintains that the chemical revolution of the 18th century is the example in which Kuhn "offers the clearest and most powerful illustrative evidence of his incommensurability thesis" [Dop78, p. 81, fn. 13]. The new chemistry, developed by Lavoisier and Dalton, could account quantitatively for the weight relations and proportions in chemical reactions. Yet this success, on Kuhn's account, was accompanied by a loss of explanatory power. Prior chemistry had been able to explain why metals were metallic, why acids were acidic, and so on for qualitative features and changes. The old chemistry was successful according to the prior conception of science on which these qualitative properties were the primary *explananda*. The victory of the new chemistry thus relied on the ascension of a new conception of science. Doppelt concludes that "the two paradigms seek to explain different kinds of observational data, in response to different agendas of problems, and in accordance with different standards of success" [Dop78, p. 43].

Perhaps the only underdetermination at stake in the example is an underdetermination of standards: The question, one may think, is whether quantitative or qualitative explanations are to be privileged and nothing more. Yet Doppelt admits that adherents of the old paradigm could appreciate the victories of the new one. He writes that "the old chemistry could recognize the success of the new chemistry with the problem of weight-gain in combustion as a… good reason on the latter's behalf *because* the two shared this problem though they did not give it the same epistemological weight" [Dop78, p. 53, emphasis in original]. Thus, there was some continuity between the standards of the old and new chemistry. If we consider the standards which are shared by the two— for instance, that weight gain is a problem of *some* importance— then we may ask whether these were sufficient to decide between them. The problem, then, is one of underdetermination of theory by evidence. The theories are the old and new chemistry; the scope includes circumstances at the time of the chemical revolution; and the standard is the minimal, *shared* standard. So, was the case actually underdetermined?

Kuhn's claim, which Doppelt repeats, is that the new paradigm "ended by depriving chemistry of some actual and much potential explanatory power" [Kuh70, p. 107] [Dop78, p. 43]. What is the status of this *potential* explanatory power? Neither Kuhn nor Doppelt claim that it justifies a return to the old chemistry, nor do they attempt to show that the explanatory loss is what stopped diehards from adopting the new chemistry. Responding to Doppelt, Philip Kitcher writes that

> the debate between Lavoisier and his opponents does not involve phlogistonian claims to the effect that the new chemistry fails to explain what all metals have in common. … Phlogistonians such as Priestly, Kirwan, Cavendish, and Gren do offer many arguments, appealing to a diverse set of empirical findings, but the invocation of explanatory successes of the kinds that Kuhn and Doppelt see as critical to their case is absent. [Kit93, p. 276]

Indeed, the example will not do for the moderate relativist if none of Lavoisier's contemporaries saw qualitative explanation to be the winning virtue of the old chemistry. He needs an historical instance in which scientists were faced with overt underdetermination. Although Doppelt still lists the chemical revolution among cases that speak in favor of moderate relativism [Dop01, p. 166], he has not made an effort to show that Kuhnian considerations held actual sway with adherents of the old chemistry.

4.3.2 The case of æther theories

In recent work, Doppelt provides the example of 18th and 19th-century æther theories [Dop01, pp. 169–71]. In the 18th century, æther theories were deployed to explain electrical, gravitational, chemical, and mental phenomena. Scientists of the period postulated subtle fluids in almost every domain. Some, most notably Scottish natural philosophers such as Thomas Reid, resisted. In the 19th century, Fresnel's wave theory of light gained wide acceptance. It seems that the same scruples that made some scientists resist 18th-century æther theories should have made them resist the wave theory of light as well. These scruples reflect different standards of evidence and, Doppelt suggests, different conceptions of what counts as a permissible scientific theory.

As Doppelt presents it, the case involves a conflict between a Newtonian, inductivist conception of science retained by Reid and an ætherial, hypothetico-deductive conception advanced by Fresnel and others.[44] The two conceptions involve different standards which are, in themselves, equally "consistent, rational, and scientific" [Dop01, p. 170]. Reid and the Scottish contingent had good reasons, Doppelt thinks, for keeping to the old ways. There was no tension in their position that would have forced them to give up their trenchant inductivism. First, they did not count the explanations of the æther theories as legitimate and, thus, they saw no explanatory loss in refusing to accept them. Second, they shared with other scientists the view that explaining gravitational, mental, and chemical phenomena was a legitimate *aim*. They felt rather that the aim could yet be accomplished within the constraints of Newtonian methodology. Third, they had good reason to rule æther hypotheses out of court. Doppelt writes, "The method of hypothesis could not distinguish between what was regarded as the very paradigm of scientific knowledge— Newton's laws— and the very paradigm of non-science— the unsavory, *ad hoc*, empirically vacuous hypotheses of Cartesian physics, such as vortex theories, which could always be arbitrarily manipulated at will to save the

[44] Doppelt follows Laudan's [Lau81b, ch. 8] description of the case.

phenomena" [Dop01, p. 170, emphasis in original].

This case is meant to support moderate relativism in this way: The acceptability of æther theories was relative to the operative standards of what should count as scientific. There were rival conceptions of science which had different standards. For each conception of science, there were reasons in favor of adopting it. All scientists recognized as legitimate the aims of explaining phenomena and avoiding a return to vortex theories. Yet, which reasons scientists took to be persuasive depended on what was important to them.

Hartley's nervous æther

Reid's inductivism is at work in his discussion of David Hartley's vibratory theory of brain function [EIP, ess. 2 ch. 3, pp. 248–53].[45] Hartley proposed that a person's nerves are filled with some subtle fluid that vibrates in response to stimuli at the person's sensory periphery. Cognition, perception, and mentation consist— according to Hartley— in the vibrations of this fluid.

In responding to Hartley, Reid presents Newtonian methodology as demanding that a causal explanation must do two things: First, causes figuring in the explanation must be shown to exist; they must not be "barely conjectured" [EIP, p. 250]. Second, the causes must be sufficient to produce the effect that they are enlisted to explain. Laudan claims that "Reid's first condition amounted to the rule that the scientist is allowed to postulate *only those entities which are observable.* Ethers and other imperceptible fluids are thus, by their very nature, disqualified from legitimate scientific status" [Lau81b, p. 126, emphasis in original]. According to Doppelt and Laudan, Reid refused to even consider æther theories because they posited entities that could not be shown to exist and were, therefore, unscientific. If this were the case, we should expect Reid to apply the first Newtonian criterion and be done with Hartley. Indeed, Reid does denounce the method of hypothesis. He writes:

[45]The citations are from Reid's *Essays on the Intellectual Powers* of 1785; page references follow his *Philosophical Works* [Rei67].

> . . .Dr Hartley [says], 'supposing the existence of the æther, and of its properties, to be destitute of all direct evidence, still, if it serves to account for a great variety of phænomena, it will have an indirect evidence in its favour by this means.' There never was an hypothesis invented by an ingenious man which has not this evidence in its favour. The vortices of Des Cartes, the sylphs and gnomes of Mr Pope, serve to account for a great variety of phænomena. When a man has, with labour and ingenuity, wrought up an hypothesis into a system, he contracts a fondness for it, which is apt to warp the best judgement. [EIP, p. 250]

Yet Reid is not a hyper-empiricist. Despite Laudan's suggestion that the first condition countenances only observable causes, *interia* is Reid's paradigm case of a cause that can be shown to exist [EIP, ess. 2 ch. 6, pp. 260–2]— yet inertia is not observable in a strict sense. Reid was also willing to countenance Franklin's fluid theory of electricity and the particle theory of light. [Cal99, p. 21] Although Reid does denounce Hartley's æther as a spurious hypothesis, he thinks that different evidence might justify the claim that there is an æther. As he writes, "we ought to hold the existence of such an æther as a matter not established by proof, but to be examined into by experiments. . ." [EIP, p. 250].

Before suggesting that an æther is tantamount to a gnome, Reid considers Hartley's ground for claiming that there is an æther. Reid reconstructs Hartley's reasoning in this way: Sensations persist for a span of time. Since these sensations reside in a brain, they must be vibratory, "because no motion, besides a vibratory one, can reside in any part for a moment of time" [EIP, p. 250]. If the impression from a sound were transmitted through the nervous system as linear motion, then the auditory sensation would not persist— it would impact and pass on. Hartley further infers from vibratory motion to a vibratory medium, an æther. Reid resists this inference, of course. He writes:

> other kinds of motion, besides that of vibration, may have some continuance— such as rotation, bending or unbending of a spring, and perhaps others which we are unacquainted with; nor do we know whether it is motion that is produced in the nerves— it may be pressure, attraction, repulsion, or something we do not know. This, indeed, is the common refuge of all hypotheses, that we know no other way in which the phænomena may be produced. . . . [EIP p. 250]

Reid thus objects that Hartley infers an æther from vibrations and vibrations from the mere persistence of sensations— a phenomenon that is not itself vibratory.

Reid goes on to apply the second Newtonian criterion and ask if Hartley's vibrations explain the phenomena of consciousness. They do not, Reid argues, pointing to several inadequacies of Hartley's theory. First, we have good reason (according to Reid) to think that thought is not identical with any material process. Even if we find Reid's dualism objectionable, we should note that it is not motivated by inductivism— Reid saw it as a distinct reason to reject Hartley's account of the mind. Second, mental events exhibit many differences in kind. There are many kinds of thoughts. Even just among sensations: sounds, sights, smells, and the rest "differ totally in kind" [EIP p. 252]. Within each kind, there are further variations that are not mere differences in degree. Vibrations may vary with respect to speed or strength, but that is all. Reid concludes, then, that vibrations cannot account for the great variety of mental events. Third, sound is supposed to be heard when it causes a vibration in the ear, light seen when it causes a vibration in the eye, textures felt when they cause vibrations in the skin, and so on. Yet this explanation provides not reason why sound should not be seen if it causes a vibration in the eye, why texture should not be heard, and why light should not be felt.

Thus, Reid does not dismiss Hartley's theory primarily on the basis of an inductivist demarcation criterion. Instead, he makes objections that even have weight given a hypothetico-deductive conception of science: There is no independent reason to think that mental phenomena are vibratory or that the nervous system is a vibratory medium. Positting vibrations provides (at best) a poor explanation of mental phenomena. So (*contra* Doppelt) we should not see Reid's rejection of Hartley's æther as requiring the invocation of incommensurable standards.[46]

[46]Callergård reaches a similar conclusion about Reid. He writes that Reid should not be seen as "rejecting the ether because of methodological principles." Instead, "Reid simply points out that Hartley has not made a contribution to the body of scientific knowledge since he hasn't discovered or explained anything" [Cal99, p. 24].

As discussed above, Doppelt claims that Reid *needed* to reject æther hypotheses, lest Cartesian vortices be readmitted to scientific debate. Yet Cartesian vortices were not ruled out merely by inductivist scruple, either. Newton eschews hypotheses, but only after revealing explanatory inadequacies of the vortex hypothesis. The General Scholium in the *Principia* begins with a discussion of these inadequacies [New99, p. 939–40], and only later yields the dictum *Hypothesis non fingo*. Newton rejects Cartesian vortices because there is no evidence for them besides phenomena which they are insufficient to explain; Reid rejects Hartley's vibrations for the same reason.

Doppelt's discussion of æther theories concentrates on what Laudan calls the *first phase* of the debate, the period 1740–1810 in which Reid resisted theories like Hartley's. Reid is a sympathetic character for us in the 21st-century who consider neural and gravitational æthers to be frivolous. As Laudan tells the story, Reid was on the side of right in this debate. Yet, Laudan suggests, the standard Reid used to defeat frivolous, 18th-century hypotheses was too strong. Laudan writes of Reid,

> His very narrow observational construal of the ground for warrentedly asserting the existence of a thing left Reid completely unable to give an account of the success of the many deep-structural theories of his time. By demanding too much, his epistemology was altogether unable to come to grips with the contemporary theoretical sciences. [Lau81b, p. 127]

Unfortunately, it is unclear which theories he has in mind when he speaks of successful "deep-structural theories" of Reid's time. These cannot be the æther theories like Hartley's— as we have seen, Reid had serious doubts that these were even explanatorily successful. Laudan goes on to discuss Fresnel's wave theory of light and the optical æther. These were not of Reid's time, however, but of half a century later.

To recap the action so far: The moderate relativist is ill served by the case of Reid's rejection of Hartley's æther. Although Reid does, in some sense, have a different conception of science than Hartley, his conception does not peremptorily strike æther theories. Reid offers reasons to think that Hartley's theory is not successful, not even on its own terms. Reid needs a unilateral conception of science neither to defuse Hartley's theory nor to hold Cartesian bogeymen at bay.

Luminiferous æther

In 1819, A.J.Fresnel won the French Academy prize competition by showing how the assumption that light is a wave could account for all known diffraction phenomena. One of the prize judges— no less a luminary than S.D.Poisson— attempted to provide a *reductio* of Fresnel's account. Considering a small disc held in the path of light emanating from a pinhole, Fresnel's account entails that the center of the shadow should be as brightly lit as if the disc were absent. D.F.Arago, Fresnel's champion on the prize committee, performed the experiment. The *reductio* proved *ad verum*. Fresnel won the prize, and in the decades that followed the wave theory of light came to preëminence. Many scientists who accepted the wave theory accepted its postulated medium, the luminferous æther.

Laudan portrays this as launching the *second phase* of debates over the æther (1820–1850), another stage in the conflict between inductivist and hypothetico-deductive conceptions of science. According to Laudan, the rule of predesignation (the requirement that hypotheses make novel predictions) was the great innovation of 19th-century hypothetico-deductivism. William Whewell, defending the wave theory and the method of hypothesis, introduced the rule of predesignation so as to address worries about spurious hypotheses. Hypotheses must not only explain known phenomena, but must also yield novel predictions that are subsequently confirmed. Only then should we believe a theory.

Doppelt might do better to concentrate on this second phase. We recognize the wave theory, in retrospect, as a great scientific success. And so we might anticipate being less sympathetic to the 19th-century inductivist than to his 18th-century counterpart. Concretely, this means being less sympathetic to John Stuart

Mill than to Thomas Reid. Although Mill claims to take his cue from "the writings of the Scotch metaphysicians, and especially of Reid" [Mil74, p. 236] and Laudan includes Reid on the list of *classical empiricists* (along with Locke and Hume) [Lau81b, p. 113], there are striking differences between Reid's views and Mill's. Reid strongly resists Hume's sceptical conclusions about causation and the external world; Mill seems to embrace them. Reid believes that we have knowledge of external objects directly through perception; Mill believes that *objects* are nothing more than the permanent possibility of particular sensations. And so on. Thus, it would be naïve to read Mill's answer to the wave theory of light as if it were also Reid's. Yet attempting to say what Reid himself would have said about Fresnel's successful wave theory would inevitably involve considerable reconstruction, and I decline to speculate. (In print, at any rate.) So let's begin with Mill.

Mill resists both the wave theory of light and the method of hypothesis [Mil74, esp. bk. 3 ch. 14 §6]. Mill insists that, just as a false theory might explain known phenomena, a false theory might entail true novel predictions. Responding to the novel predictions of the wave theory, Mill writes:

> Such predictions and their fulfillment are, indeed, well calculated to impress the uninformed, whose faith in science rests solely on similar coincidences between its prophecies and what comes to pass. ... Though twenty such coincidences should occur, they would not prove the reality of the undulatory ether; it would not follow that the phenomena of light were the results of the laws of elastic fluids, but at most that they are governed by laws partially identical with these.... [Mil74, p. 356]

Whewell differs with Mill on just this point. He replies to the passage just cited, writing that "there is no doubt that the most scientific thinkers, far more than the ignorant vulgar, have allowed the coincidence of results predicted by theory with facts afterwards observed, to produce the strongest effects upon their conviction..." [Whe89, p. 294]. Whewell then appeals to the "undulatory theory of light" as an example. So this case might be seen to favor moderate relativism: If we think of science as an hypothetico-deductive enterprise and recognize the rule of predesignation, Fresnel's wave theory of light was to be accepted on account of the celebrated bright spot. If we cleave to inductivism, then Fresnel's wave theory was to be rejected on account of its postulating an unsavory æther.

The relativist story is weakened somewhat if the rule of predesignation did not play a central rôle in the acceptance of the wave theory. The fact that it did follows from the common view that the celebrated white spot was the crucial experiment of the wave theory and also from Whewell's description of the case. Yet, as John Worrall details, the prediction of the white spot was not what swayed the prize committee [Wor89a]. Instead, the commissioners seemed most impressed with Fresnel's ability to handle straightedge diffraction— a previously known phenomenon. Moreover, they were unconcerned to experimentally check for a central dark spot in light from a small circular opening— a prediction of Fresnel's theory that was as much unprecedented as the bright spot.[47] So, Worrall argues, predesignation was not the touchstone of scientific virtue for scientists accepting the wave theory. In this respect, the exchange between Whewell and Mill may be a poor indication of which factors actually swayed scientists.

Nevertheless, there is no denying that Whewell and Mill disagreed as to whether the evidence warranted belief in an æther— and that they did so because of differing methodological commitments. It would not be too pedantic, however, to distinguish between the *wave theory* of light and the *æther theory*. The former involves the claim that the behavior of light is described by Fresnel's wave equations. The latter is the claim that light is a disturbance that propagates in a subtle fluid. In a footnote to the passage cited above, Mill writes:

> What has most contributed to accredit the hypothesis of a physical medium for the conveyance of light, is the certain fact that light *travels* (which can not be proved of gravitation); that its communication is not instantaneous, but requires time; and that it is intercepted (which gravitation is not) by intervening objects. These are analogies between its phenomena and those of the mechanical motion of a solid or fluid substance. But we are not entitled to assume that mechanical motion is

[47] Indeed Whewell appeals not to constructive interference predictions like the bright spot, but to "the production of darkness by two luminous rays interfering in a special manner" [Whe89, p. 294].

> the only power in nature capable of exhibiting those attributes. [Mil74, fn. p. 356]

The argument for the existence of the luminiferous æther (as Mill reconstructs it) parallels Hartley's argument for the nervous æther (as Reid reconstructs it) [EIP, p. 250, cited above]. The phenomena are observed to have a certain features, the only known systems in which these features are instantiated are vibratory or mechanical systems, so the phenomena must be vibratory or mechanical in nature, and so there must be a medium in which the phenomena propagate. Mill and Reid object to this kind of eliminative inference. The fact that we know of no other systems that instantiate the relevant features may be due only to our lack of knowledge and imagination. Strictly speaking, Mill's argument undercuts the æther theory but not the wave theory. Mill seems to concede that light is governed by the wave equation (on p. 356). The claim that luminous phenomena obey wave equations may be tested directly by experiment and need not rely on eliminative inferences of the contested kind.

We are prone to think of the wave theory and not the æther as the great development of 19th-century optics. Since hypothetico-deductivism and inductivism both warranted accepting the wave theory, then the case seems not to favor the relativist. Yet the relativist may object that this reply presumes our present science— and in so doing presumes whatever values underwrite our present practice. The claim of incommensurability (the relativist might say) was never meant to entail that querists with differing commitments *could never agree on anything*, rather that there are *some things about which they could never agree*.[48] By excluding æther theories from consideration, inductivism embodied an importantly different methodology than hypothetico-deductivism.

Yet this would misrepresent the case. Mill allows the possibility that different kinds of evidence might be able to establish the æther theory. Commenting

[48] That is: For rival standards, there exist rival theories such that there are no circumstances in which both standards warrant belief in the same theory. This seems to be the sort of incommensurability that Doppelt commends.

on anomalies in the return of a specific comet, he allows that it may be due to the resistance of the æther. If such an effect could be observed in the motions of other comets and planets as well, Mill suggests, “the luminferous ether would have made a considerable advance toward the character of a *vera causa*” [Mil74, p. 355, italics in original]. Even Mill did not rule out æthers entirely.

4.3.3 The case of hormones and gender-linked behavior

The final example I will take up involves causal models of how hormone levels affect gender-linked behavior. Although the Doppelt does not consider this case, it is Longino’s central example of underdetermination [Lon90] [Lon02, pp. 126–7, 183, 199–200].

A recent news item heralding British research on the source of boyish behavior in young girls announces, “Forget nurture, the tendency for a girl to behave like a tomboy is all up to nature— specifically, the amount of testosterone a baby is exposed to during pregnancy.” Quoted in the story, a researcher says, “Because hormones influence basic processes of brain development, they also exert permanent influences on behaviour” [ABC02].[49] This research and the uncritical reporting of it presuppose that chemicals effect behavior by effecting brains. Differentiation of behavior between the sexes is taken to be the result of differing brain chemistry, and this in turn is presumed to result from differing levels of hormones. Whatever rôle environment plays in the differentiation of behavior is (according to the model) independent of hormone levels and brain development. Longino calls this the Linear-Hormonal (LH) model. It posits a causal structure like the one in figure 4.11.[50]

Two sorts of evidence are used in support of the LH model: animal and

[49] The study touted is [HGR$^+$02]. The assumption of the LH model, although exhibited in the researcher’s comments to the press, does not appear explicitly in the published study.

[50] This is Longino’s figure 2 [Lon90, p. 138]. The arrows should not be taken to imply immediate causation without an intermediate causes playing a rôle, but only to imply that the elements represented in the graph do not, according to the model, causally influence one another except where there are arrows. The graphs as I employ them here can be understood as directed, acyclic graphs in the manner of Spirtes, Glymour, and Scheines [SGS93] [Gly98].

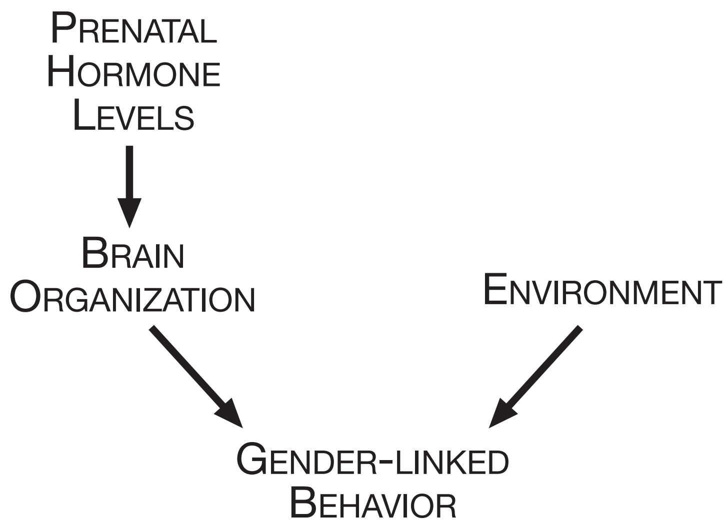

Figure 4.11: The LH model.

human studies. In the animal studies, hormone levels are manipulated directly and behavior is then observed. For example, testosterone correlates with fighting behavior in rats. In the human studies, groups with hormone disorders are observed and compared to control groups. For instance, girls with abnormally high levels of prenatal androgens are observed to exhibit more boy-like behavior than girls with normal levels. Longino responds separately to the two sorts of evidence.

Animals, she argues, are sufficiently unlike humans that the animal studies should not be assumed to describe how the hormones being studied affect human behavior. She makes several arguments for this conclusion: Hormones are known to cause different effects on different species, so the presumption should not be that some particular laboratory species responds just as humans do. Humans exhibit a degree of intentionality not seen in rats and have more complicated brains. Human behavior occurs in contexts more complicated than the laboratory situations that confront rats. Even monkeys, who lack many of the complications present in humans, have been shown to react differently to hormones than rats. [Lon90, p. 157] I find Longino's arguments here convincing, but they don't speak to the issue of moderate relativism. She argues that the animal evidence should not be used to underwrite conclusions about humans, but she does so without appealing in any obvious way to a rival conception of science. She does not argue from the premise that she is a feminist to the conclusion that humans are different than rats, but rather she argues against the analogy by pointing to disanalogies. Would the disanalogies be unconvincing to anyone working within the LH model? Wouldn't LH partisans be unreasonable if they were unconvinced?

The human studies, Longino believes, only stand as evidence for a particular hormonal connection if one begins with the LH model. Longino suggests as an alternative a selectionist model developed to explain human memory, learning, and self-awareness. On this model, she explains, "experience... and self-image... play a primary role in the *biological* explanation of the behavior-action of species with a highly developed cortex" [Lon90, p. 148].

Although this model "is not in use to explain any particular category of behavior" [Lon90, p. 143], Longino suggests that it may provide an alternate explanation for the results of human studies that observe a correlation between androgens and boy-like behavior. First, the girls with abnormally high levels of prenatal androgens were aware that they had such a condition. This medical history might make them feel self-conscious, feel unlike other girls, and feel uncertain of their femininity. These factors could result in behavior seen as more boy-like. Second, testosterone is known to affect muscle development. More rambunctious behavior by the girls might simply have been an expression of the consequent need for exercise. [Lon90, p. 150] Research employing the LH model cannot be criticized as bad science, Longino thinks, because constitutive values alone give no reason to prefer the selectionist model.[51] She writes:

> Both rest on explanatory models that involve metaphysical assumptions about causality and human action. Neither theoretical perspective can muster constitutively based arguments sufficient to exclude

[51] That values influence the actual scientists in this instance does not show that science ought to be value-laden. If the value-free view is correct, then dogged adherents of *either* theory are doing bad science. If one appeals only to constitutive considerations and constitutive considerations are insufficient to decide between the two theoretical perspectives, then the situation demands agnosticism. 'Perhaps both mechanisms are at work.'

> the other— thus both can continue to generate studies that are used to support microhypotheses about the etiology of particular forms of behavior that are consistent with one or the other broader model. [Lon90, p. 161]

Rather than deciding based on strictly scientific concerns, Longino thinks we should prefer the selectionist model because it accords with political ideals of autonomy and responsibility. Whereas the LH model connects behavior to a simple biological variable, the selectionist model emphasizes "the enabling rather than the limiting aspects of biology" [Lon90, p. 175-6].

Perhaps breadth of scope might lead us to favor the selectionist over the LH model. As Longino notes, the former derives from very broad considerations of human consciousness and action, while the latter promises to explain only the connection between hormones and sex-linked characteristics. However, as Longino also notes, for the selectionist account "gender role behavior is removed from the theoretical umbrella of the hormonal model that does explain other aspects of [sexual] differentiation" [Lon90, p. 161]. Breadth of scope differently understood might lead us to prefer the LH model, so this attempt to marshal constitutive considerations falls prey to underdetermination.[52]

Longino laments that "work on cognition shows no sign of reflection or analysis on the part of researchers. Correlation after correlation is produced with no attempt to understand just what it is that is being measured or its relation to associated phenomena" [Lon90, p. 167]. Indeed, she is right to insist that an observed correlation alone does not establish any particular causal connection. Nevertheless, a correlation is something to be explained— Longino admits as much by providing a rival explanation. She accepts, just as adherents of the LH model do, that the general causal structure is such that prenatal hormone levels are at one end of a causal chain that has gender-linked behavior at the other end. Longino offers only schematic and complicated depictions of the selectionist model, but she offers specific causal hypotheses:

[52]Underdetermination resulting from the flexibility of values, discussed above.

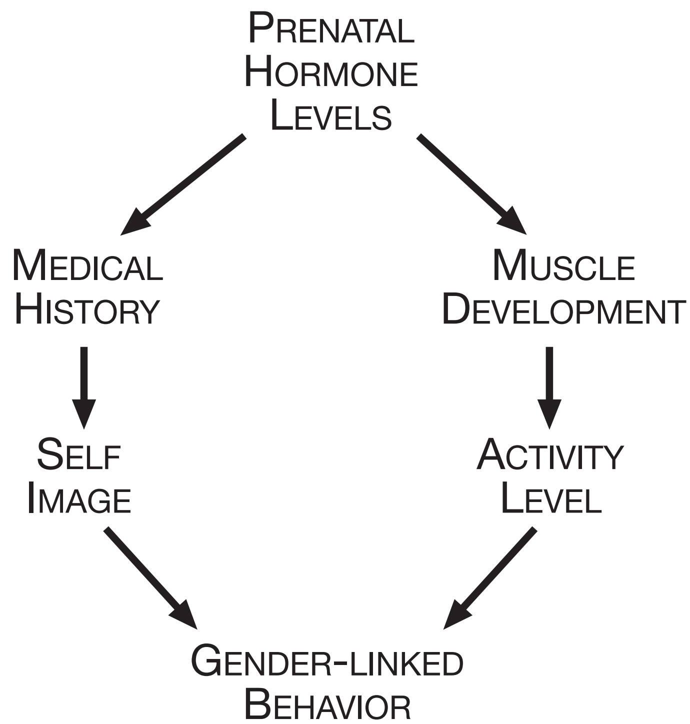

Figure 4.12: Longino's selectionist model.

> As for the... children who provide the strongest evidential base for the hormonal model, one can say that their behavior is a specific response to their situation as they perceive it. They are... persons who (1) have a medical history productive of greater self-consciousness and self-knowledge than is usual for young people, (2) are quite aware of their uniqueness, and (3) are very likely uncertain of their "femininity" and possibly of their femaleness as well. This combination of circumstances may account for their choosing as children behavior less restrictive in its implications for adult life than traditional girllike behavior is. Finally, such direct effects of testosterone as do exist could be on muscle development, with a consequent need for exercise. [Lon90, p. 150]

These are the specific hypotheses summarized above and in figure 4.12.

Longino makes much of the causal loops in the selectionist model. For instance, a child's psychological state both effects and is effected by her actions.[53] Yet there is no possibility of reciprocal causation between prenatal hormone levels and gender-linked behavior, since the former plays its rôle years before the latter

[53] Cf. her figures 6 and 7. [Lon90, pp. 147, 149]

occurs. If there is some causal connection, it must flow one way and not the other. The correlation might result from some other causal structure— e.g., if both were effects of a common cause— but, provided one is a causal ancestor of the other, it must be hormone levels causing childhood behavior and not *vice-versa*. The only question is, What is the mechanism? Longino suggests that hormone levels effect the girls' personal history in ways that effect, in turn, their self image. The girls then make different choices than other girls. Choices are causes of actions, of course, so this means that self image is taken to be one causal factor controlling behavior. There is an additional causal pathway from hormones to behavior, one that goes through muscle development. It is this causal scenario— and not the selectionist model *tout court*— that is represented in figure 4.12.

Is Longino right to think that the choice between the causal account represented in figure 4.11 and the account represented in figure 4.12 is underdetermined on the basis of evidence and that this determination could only be made on the basis of contextual values? It is true that the mere correlation between hormone levels and gender-linked behavior does not favor one over the other, but there are testable differences nonetheless. Notice that on the selectionist account (figure 4.12), hormones only influence behavior through intermediate causes of personal history and muscle development. Two children with identical histories and muscle development should thus exhibit the same degree of gender-linked behavior *regardless* of their prenatal hormone levels. Contrast this with the LH model (figure 4.11), in which there is a causal path from hormones to behavior which goes only through brain organization, such that gender-linked behavior should be strongly correlated with hormone levels even among children in identical environments with identical muscle development.

Imagine an experiment, then, in which one each out of pairs of identical twins is treated with hormones in utero. The twins are never told which was which. They are not even told that they are part of a medical study. As they grow up, careful records are kept of their muscle development, using some objective measure like muscle mass. When they reach school age, case workers judge whether the childrens' behavior is boyish or girlish. The question here would not be simply whether or not prenatal hormone levels correlate with behavior, but whether they do so conditional upon history and muscle development. If a correlation remains conditional on these other factors, then it could not be explained by a causal structure like figure 4.12; some other causal path would have to exist between hormone levels and behavior. Conversely, if no significant correlation remains after conditioning on history and muscle development, then a causal structure like figure 4.11 could be ruled out. Neither result would prove what the causal structure actually is *in an absolute sense*, but either would eliminate one of the two rivals. Controlling directly for personal history and conditioning on muscle development would eliminate the correlation between those variables and prenatal hormone levels. If a correlation remains between prenatal hormone levels, then there must be some other causal path between the two. Some variable X may intervene— perhaps brain organization. This situation is represented in figure 4.13.

There may be concerns about this experimental design; there are familiar worries about twin studies that there are never enough natural twins and that there are sample selection biases. We may imagine a more extreme version of the experiment in which twins are forcibly harvested and raised in a clinical facility to fully control for history, however, and some variation on this double-blind structure should yield robust results. The underdetermination yields no significant conclusion if further evidence can close the inferential gap. This is just the ordinary situation of enquiry underway. Cost might make such a study impractical, but more importantly moral considerations would make it reprehensible. What this shows, though, is that there are things we are not willing to do in the name of science. Empirical considerations could decide between the two theories, if we were willing to pay the price for the knowledge.[54]

[54]It's not clear that such an extreme study is necessary. The study mentioned above [HGR$^+$02] involved healthy children rather than children with hormone disorders. One of the factors that Longino appeals

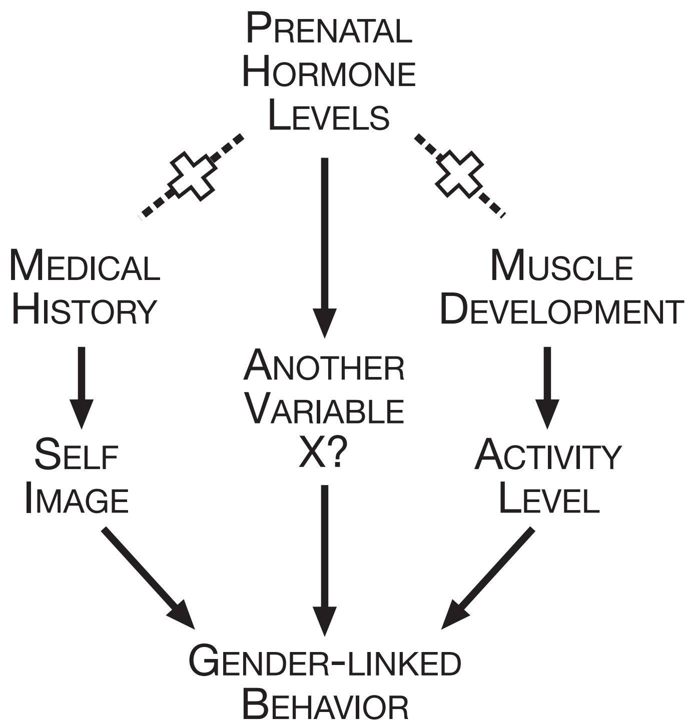

Figure 4.13: The experimental situation.

Of course, these empirical considerations do not settle the matter deductively. There are still assumptions that, as Longino says, "establish the evidential relevance of data to hypotheses" [Lon02, p. 126]. Yet the assumptions are not morally charged assumptions about humanity and agency. It is unclear how the assumptions involved in a suitably designed experiment would have a controversial sociopragmatic dimension.

4.3.4 Verdict on relativism

To summarize: The historical examples of phlogiston and various æthers are insufficient to establish moderate relativism, because conflicting standards did not obviously preclude agreement. The contemporary example of hormone/behavior research does not show that there are distinct, defensible, value-laden standards; it shows only that some present scientists are insufficiently critical. Nothing said

to (peculiar medical history) is absent.

here precludes the moderate relativist from offering further examples. The cause might be better served by looking at debates over methodology in natural history— debates in 19th-century geology and evolutionary biology, for instance. Whatever examples are offered, however, they must do more than show scientists disagreeing about methodology. Considering inductivism and the luminiferous æther, it's unclear what *values* were supposed to have motivated Mill's work. Mill seems to have been motivated more by empiricist *arguments* than by *values*. So instances in support of moderate relativism must not only involve some disagreement over methodology, but they must also exhibit the rôle of differing values in fostering such disagreement.

There is a mitigated sense in which moderate relativism is no doubt correct. Our conception of science is a historical product and thus is open to change over time. How it changes will be effected by what we do and see— effected, that is, by what projects we pursue. Yet this alone does not entail that different projects will yield incommensurable or even importantly different conceptions of science.

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