Chapter 5

The Quine-Duhem Thesis and Implications for Scientific Method

In the previous chapters, we looked at worldviews, truth, facts, and reasoning, as well as a number of issues related to these topics. As we will see in this chapter, many of those issues have close ties to what is often referred to as the Quine-Duhem thesis (or, sometimes, the Duhem-Quine thesis, to reflect the fact that Duhem preceded Quine). The Quine-Duhem thesis is one of the better-known views in modern philosophy of science, and so for this reason alone is worth looking into. But in addition, the Quine-Duhem thesis will give us an opportunity to better see the ways in which the issues discussed in previous chapters are intertwined, and moreover, this discussion will help set the stage for later chapters, where we will see how these intertwined issues play out in examples from the history of science.

Another point worth noting is that the topics discussed in previous chapters, and the central issues involved in the Quine-Duhem thesis, have implications for various views on scientific method. Toward the end of the chapter, then, we will consider various proposals that have been suggested concerning scientific method. This section on scientific method will do double duty: first, it will allow us to see, historically, some of the views presented on the proper way to conduct science. This includes the standard view from within the Aristotelian worldview on scientific method, and as we will see, the Aristotelian approach to science differed substantially from what is typically taken now as the proper sort of approach. And second, as with the section on the Quine-Duhem thesis, discussing scientific method will give us a chance to get on the table issues surrounding methodology in science, with an eye toward setting the stage for later chapters, where we will see the (often surprising) methods used in cases from the history of science.

With these introductory points in mind, we will begin with a look at the Quine-Duhem thesis.

The Quine-Duhem Thesis

The Quine-Duhem thesis is a well-known view in the philosophy of science, and one involving a number of intertwined and controversial issues. In this section, we will look at three of the key ideas associated with this thesis, namely, the idea that (to borrow a phrase from Quine) our beliefs face the “tribunal of experience” not singly, but in a body; the claim that there can typically be no “crucial experiments” to decide which of two competing theories is correct; and the notion of “underdetermination,” that is, the idea that the available data typically does not pick out a unique theory as being correct.

First, a brief introduction to the principal players. Pierre Duhem (1861 - 1916) was a well-respected French physicist, and one who had substantial interests in broader questions, including questions involved in the testing of scientific hypotheses and theories. Williard Quine (1908 - 2000) was one of the most influential philosophers of the 20th century, and he had a lifelong interest in issues surrounding the philosophy of science. (To give an idea of Quine's emphasis on the philosophy of science, consider his often-quoted remark that philosophy of science is philosophy enough.)

To begin our discussion of the first of the key ideas associated with the Quine-Duhem thesis, recall (from the previous chapter) the options available when faced with disconfirming evidence for a theory. In particular, recall that, when faced with disconfirming evidence, there are almost always crucial (but usually unstated) auxiliary hypotheses involved. As we saw in the previous chapter, it is always possible to reject an auxiliary hypothesis rather than rejecting the main view.

Given the role played by auxiliary hypotheses, when we perform an experiment, presumably to test a particular hypothesis, we are not really testing just the individual hypothesis. Rather, in an important sense the test is more of a test of the main hypothesis plus the accompanying auxiliary hypotheses. So what we are typically testing is really a *body* of claims, any one of which can be rejected or modified in the face of disconfirming evidence. And this is one of the key elements of the Quine-Duhem thesis–that is, the key idea is that a hypothesis typically cannot be tested in isolation. Rather, what is tested is an entire group of claims, any of which is available for rejection or modification should the experimental results not be as expected. And this is the key idea behind Quine's phrase noted above, that our beliefs face the tribunal of experience not individually, but as a body.

The emphasis here on bodies of claims brings to mind our discussion of worldviews from Chapter 1. And indeed, this aspect of the Quine-Duhem thesis is closely tied to the notion of worldviews. To see this, recall our discussion of interconnected systems of beliefs from Chapter 1, in which we discussed such collections of beliefs by an analogy with jigsaw puzzles. Quine tended to speak of such collections of beliefs as "webs of beliefs," suggesting an analogy with a spider's web. In a spider's web, changes in the outer regions of a web affect the more central regions in only minor ways. Likewise, beliefs towards the outer edge of a "web of beliefs" can be modified with only minor alterations of more central beliefs (such beliefs would be the peripheral beliefs we discussed in Chapter 1). In contrast, changes in the central regions of a web will cause changes throughout the web, and in a similar way, modifications to central beliefs (the core beliefs) will cause changes throughout one's web of beliefs.

We noted above that, according to the Quine-Duhem thesis, tests of a hypothesis are typically not tests of the individual hypothesis, but rather, are tests of groups or collections of beliefs. How large of a group of beliefs are we speaking of here? For example, if we design an experiment to test a hypothesis, how large of a collection of beliefs are we really testing? Are we testing just a relatively small subset of our overall collection (or jigsaw puzzle) of beliefs, or more radically, is every experiment and test we perform in some sense really a test of our entire jigsaw puzzle (or web of beliefs, or worldview)?

There is no consensus on the answers to these questions. Quine at times defended the more radical view, maintaining that it is one's entire web of beliefs–that is, our entire interconnected collection of beliefs–that face the tribunal of experience as a whole. And faced with evidence that runs counter to views we hold, no belief, even a core belief, is immune from revision. We would of course typically be more willing to modify beliefs that are more toward the periphery, but Quine's point is that in principle any belief is subject to revision. Tests are tests of the whole package. Duhem, on the other hand, was a bit more reserved on this point, in that on his view, although tests might involve large collections of beliefs, it is not typically our entire collection of beliefs–our entire worldview–that is put to the test.

In spite of the differences in the details between Quine's and Duhem's views, there is a general agreement that tests are not typically tests of a hypothesis in isolation, but rather, such tests are typically tests of large bodies of beliefs. And as noted, this is generally taken as a key component of the Quine-Duhem thesis.

Another aspect of the Quine-Duhem thesis, and one closely related to what we have just been discussing, involves the notion of “crucial experiments” in science. The idea of a crucial experiment goes back at least to Francis Bacon (1561 - 1626), the idea being that, when faced with two competing theories, it should be possible to design a crucial experiment for which the two theories give conflicting predictions. Ideally, since the predictions of the competing theories conflict, such an experiment should show at least one of the theories to be mistaken. Because of the issues involved in confirmation reasoning discussed in the previous chapter (mainly that confirming evidence can at best support a theory, but not show definitely that the theory is correct), such an experiment would not show that the theory making the correct prediction was definitely the correct theory. Nonetheless, the key idea is that a crucial experiment, even if it could not show one of the competing theories to be definitely correct, could at least serve to rule out one of the competing theories.

However, if tests are typically tests of collections of beliefs, and if when faced with disconfirming evidence it is always an option to reject an auxiliary hypothesis rather than the main theory, then it seems that crucial experiments will typically not be possible. The reason should be clear: in any such experiment designed to show that at least one of the two competing theories gives a wrong prediction, whichever theory seems to make the wrong prediction can still be kept, simply by rejecting one of the auxiliary hypotheses. And again, as we noted in the previous chapter, it is often perfectly reasonable to reject an auxiliary hypothesis rather than the main theory.

It is worth noting that this skepticism about the possibility of crucial experiments can be understood in a variety of ways, some much stronger and more controversial than others. There is little question that in some cases, results of experiments for which competing theories give conflicting predictions can be accommodated within both of the conflicting theories. For example, the lack of observed neutrons during early cold fusion experiments was clearly compatible with the usual theories about fusion, but as we saw in the previous chapter, this result could also be accommodated within cold fusion theory by rejecting one of the auxiliary hypotheses involved. If we take the Quine-Duhem skepticism about crucial experiments in a relatively weak sense, as claiming only that competing theories can often both accommodate the results of an alleged crucial experiment, then the claim is reasonably uncontroversial. There are numerous examples from the history of science (the cold fusion example above being just one) that support this weaker version of the claim.

Another reading of this aspect of the Quine-Duhem thesis, in which this part of the thesis is construed as claiming that *any* experimental result whatsoever can be accommodated within *any* theory whatsoever, is a much stronger and much more controversial version of the thesis. It is much more difficult to find clear examples from the history of science to support this stronger claim. However, Quine did sometimes speak this way, and not surprisingly, there is far less consensus on this stronger claim. In short, while there is general consensus that a key part of the Quine-Duhem thesis involves a certain skepticism about the idea of crucial experiments, it is worth noting that there is less consensus on how strongly this claim should be interpreted.

One other often-discussed issue in the philosophy of science is worth noting at this point. This is an issue that likewise has close ties to the topics just discussed, and revolves around what is generally termed the *underdetermination* of theories. Recall again from the discussion above that theories can generally be preserved in the face of disconfirming evidence, and that generally it will be difficult if not impossible to design a crucial experiment to decide between competing theories. Add to this our discussion of confirming evidence from the preceding chapter, especially where we noted that, given the inductive nature of confirming evidence, such evidence can at best support a theory, but never demonstrate conclusively that a theory is correct. Putting all these factors together, we arrive at the view that the available data, including of course the outcomes of relevant experiments, can never fully determine that a particular theory is the correct theory, and that the competing theories are incorrect. In other words, a variety of competing theories will often be compatible with the available evidence. This is often summarized by saying that theories are *underdetermined* by the available data.

It is worth noting that, as with the aspects of the Quine-Duhem thesis we discussed above, the notion of underdetermination can be read in a variety of ways, some stronger and more controversial than others. There is little question that at times, the available data does not uniquely point toward one of two or more competing theories. To again use the cold fusion example, in the late 1980s the data simply did not point cleanly toward either the cold fusion theory or the existing "hot" fusion theory (that is, the usual view of fusion as requiring extremely high temperatures). That is, both the cold fusion and hot fusion theories were compatible with the available data. Understood in this relatively mild way, there is little question that theories are underdetermined in this sense.

At the other end of the spectrum, in contrast to this mild sense of underdetermination, it is not uncommon to see discussions involving a much more radical notion of underdetermination. On this much more radical view of underdetermination, scientific theories and scientific knowledge are viewed as "social constructs," more or less inventions of the relevant community. According to this view, scientific theories are seen as more closely tied to, and reflections of, social conditions rather than being tied to, and reflections of, the physical world. On this much more radical and controversial notion of underdetermination, there is no more a uniquely determined and objectively correct scientific theory than there is a uniquely determined and objectively correct set of table manners. On this view, table manners and scientific theories are both reflections of society, and one theory cannot be said to be the uniquely correct theory in any deep or objective sense of the word "correct."

In short, although there is agreement that the underdetermination of theories is a key aspect of the Quine-Duhem thesis, the notion of underdetermination is construed in a variety of ways. And as noted, some of these ways are substantially stronger and more controversial than others.

In summary, consider again the key issues associated with the Quine-Duhem thesis–the underdetermination of theories, the idea that hypotheses are typically not tested in isolation, and the notion that crucial experiments to decide conclusively between competing theories are typically not possible. All of these are reasonably uncontroversial, at least when construed in a somewhat mild way. How broadly such claims should be construed, and whether such broader claims can be supported by actual cases, is much more controversial. Watch for these sorts of issues as we discuss, in Part Two, historical cases such as that involving the dispute over the Earth-centered and sun-centered views of the universe. As we will see, such disputes involve a surprisingly wide range of issues, including those central to the Quine-Duhem thesis.

Implications for Scientific Method

As noted in the introduction, the issues discussed in previous chapters, as well as the issues discussed immediately above concerning the Quine-Duhem thesis, have some interesting implications for views on scientific method. Before closing this chapter, then, we will look briefly at a variety of proposals that have been made concerning the proper way to conduct science. This will allow us a chance to see the view on scientific method dominant within the

Aristotelian worldview (and especially note how much it differs from the way scientific method is more typically viewed today), as well as help set the stage for our discussion, in Part Two, of cases from the history of science.

At some point in your education, you may well have been taught what is commonly termed “the scientific method.” Although the exact formulation of this method varies somewhat from book to book, and from school to school, in general terms this method is generally presented as one that involves (i) gathering the relevant facts, (ii) generating hypotheses to explain those facts, and (iii) testing the hypotheses, typically by performing experiments that either confirm or disconfirm (using something like the patterns of confirmation and disconfirmation reasoning discussed earlier) the hypotheses.

Given our earlier discussions, especially concerning the nature of facts from Chapter 3, of confirmation and disconfirmation reasoning in the previous chapter, and of issues associated with the Quine-Duhem thesis discussed above, we might reasonably wonder whether the method just outlined is as straightforward as it is often presented to be. In what follows, we will look at some of the methods that have been proposed for doing science, and we will also explore some of the issues surrounding such methods. We will by no means be surveying every proposed scientific method, but we will look at enough to get a good idea of some of the factors complicating any attempt to give a single, definitive method for doing science. Let’s begin with a look at some of Aristotle’s ideas on the subject

Aristotle’s “Axiomatic” Approach

Within the Aristotelian worldview, science was generally viewed as geared toward generating knowledge that was certain. That is, it was generally thought that scientific knowledge had to be necessarily true, and not merely probable. If we ask how we might arrive at such necessarily true knowledge, there seems only one possible approach, and that is to use deductive reasoning that is based on necessarily true basic principles. If such necessarily true basic principles could be found, and if the reasoning used is deductive, then the conclusions (that is, the scientific knowledge) would “inherit” the certainty of the basic principles, so to speak, and we would thus arrive at necessarily true scientific knowledge.

Such approaches are often termed “axiomatic” approaches–that is, these are approaches based on deductive reasoning from basic principles that are in some sense certain or necessarily true. Aristotle was an advocate of such an approach, and during the period in which the Aristotelian worldview was dominant, the Aristotelian approach to scientific knowledge was generally viewed as the correct approach. Looking at Aristotle’s approach, then, will give us an idea of the sort of scientific method dominant for much of (at least recorded) western history, and also give a good sense of the fundamental problems that will be faced by any attempt to generate necessarily-true scientific knowledge.

Aristotle viewed logic as a tool to be used in investigations, including (but not limited to) scientific investigations. In fact, for Aristotle, providing a scientific explanation was essentially a matter of providing a certain sort of logical argument. We typically do not view scientific explanations and logical arguments as being all that similar, but in fact they are closely related. To illustrate this, consider the following example. (The example is chosen for ease of explanation, and since it uses notions discovered well after the time of Aristotle, it is not one that Aristotle himself would have or could have given.)

Suppose you are curious as to why copper conducts electricity. Suppose someone explains that copper contains free electrons, and that things with free electrons conduct electricity, and that’s why copper conducts electricity. Note how closely related this explanation is to the following argument:

All copper contains free electrons.

All things containing free electrons conduct electricity.

so All copper conducts electricity.

In fact, aside from the style of presentation, there is little difference between the explanation given in the preceding paragraph, and the argument given immediately above.

Arguments of the sort just given, consisting of two premises and a conclusion, are termed *syllogisms*. For Aristotle, a proper scientific explanation consisted of a *demonstration*, which in essence was a chain of syllogisms in which the conclusion of the final syllogism is the item that is being explained. (I should note that, strictly speaking, Aristotelian syllogisms are two premise arguments meeting certain conditions as to the form and arrangement of the statements involved. Likewise, strictly speaking there are more conditions on a demonstration than those just mentioned. However, these additional details need not concern us here.)

As noted, for Aristotle, scientific knowledge had to be certain knowledge. Or to put it another way, the conclusion of the final syllogism in the chain had to be necessarily true. Note how this differs importantly from the modern conception of scientific knowledge. Science is now generally viewed as producing theories that are probably correct, but we do not expect (nor do we think it possible) for science to guarantee that theories are correct. Not so for Aristotle, nor for the typical view of scientific knowledge up until the 1600s. Scientific knowledge was to be certain knowledge, and the certainty was tied importantly to its being derived deductively.

But how might such a deduction guarantee that the conclusion is not merely true, but necessarily true? As noted, there is only one way, and that is to use premises that are themselves necessarily true, so that the conclusion inherits, so to speak, the certainty of the premises.

But this raises the question as to where the necessity of the premises comes from. One solution would be to derive such premises, via other syllogisms higher in the chain of syllogisms, from other premises that were themselves necessarily true. And indeed, this is the way Aristotle envisions a full scientific explanation as proceeding. That is, in the chain of syllogisms, the final syllogism will consist of a conclusion that is necessarily true because it has been derived from premises that are necessarily true. And these premises typically will themselves be conclusions of syllogisms earlier in the chain, in which the premises of these earlier syllogisms are necessarily true.

Of course, the chain of syllogisms cannot go on forever, so at some point there must be some premises that are necessarily true, but that have not themselves been derived from earlier syllogisms. These starting points, the premises that are necessarily true in and of themselves, are typically termed *first principles*. First principles are envisioned as basic, necessarily-true facts about the world. But how does one recognize first principles, and in particular, how does one know a first principle is necessarily true? An analogy with geometry might be helpful.

Consider the axiom of Euclidean geometry that given a line on a plane, and a point on the plane not on that line, one and only one line can be drawn on the plane through the point parallel to the given line. This axiom is illustrated in the following figure.

<Insert Figure 5.1 here.>

In the figure, the paper represents the plane, the solid line at the top represents the given line, the point is the point on the plane, and the dotted line represents the one line that can be drawn through the point parallel to the given line. This axiom cannot be proven in Euclidean geometry, and thus it (or an axiom equivalent to the way it is phrased here) is taken as a basic, unproved starting point (that is, an axiom or postulate) of Euclidean geometry. Although the claim cannot be proven, it seems that if one has adequate education, intelligence, and an understanding of the terms involved, then one can simply "see" that this axiom must be true. (Incidentally, the discovery, in the 1800s, of non-Euclidean geometries casts serious doubt on whether it makes sense to talk about such axioms being "true" in any meaningful sense.)

In a way somewhat similar to the way that we presumably "see" the truth of axioms such as the one illustrated above, if one has adequate education, intelligence, training, and a certain amount of scientific savvy, so to speak, then according to Aristotle, one will simply "see" that certain basic facts about the world are not only true, but are necessarily true. And that, in outline, is how one comes to see first principles.

What you can probably "see" more clearly at this point is that this approach is simply not going to work. The basic problem lies with the first principles. Consider again our discussions from our earlier chapters on worldviews, truth, and empirical facts and philosophical/conceptual "facts." Given what we saw in those chapters, it is highly unlikely that there will ever be any such agreement on what constitutes basic facts, much less what might constitute basic facts that have to be necessarily true. And so the basic problem with the sort of deductive approach envisioned within Aristotle's method lies with the very starting points of that approach.

As noted, Aristotle envisioned science as resulting in theories and claims that were not merely probable, but rather were certain. This sort of axiomatic approach, based on necessarily true first principles, seems the only way such certain scientific knowledge might be achieved. And as you might guess, the problem noted above, that is, the problem of finding agreed-upon, necessarily true starting points, is going to be a general problem for all such approaches. Largely for this reason, there is now general consensus that scientific claims and theories cannot be guaranteed to be correct. And as we discussed in the previous chapter, this is not a defect of science, but rather simply a consequence of the inductive nature of much of scientific reasoning. However, before moving on to consider other approaches, one other axiomatic approach, that of Descartes, is worth considering briefly.

Descartes' Axiomatic Approach

We discussed Descartes at the end of Chapter 2, where we saw that Descartes was interested in finding necessarily true beliefs to serve as a foundation on which to build a structure of certain knowledge. In many ways, Descartes' view of the proper way to conduct science was similar to that of Aristotle (though Descartes did not restrict himself to the purely syllogistic method taken by Aristotle). In particular, Descartes too was interested in using deductive reasoning to derive certain knowledge from necessarily true starting points.

Also as with Aristotle, much the same problem arose when Descartes attempted to find agreed-upon starting points. When dealing with starting points that concern matters about the world, there simply do not seem to be any agreed-upon basic principles about the world that we can know with certainty. And so with respect to basic starting points concerning the world, Descartes' approach is going to run into essentially the same problem as Aristotle's.

But as we saw in Chapter 2, at one point Descartes considered his own mind in his search for necessarily true starting points. And as we saw in that chapter, there is a case to be made that his "I am, I exist" is necessarily true. So Descartes may have found at least one (generally) agreed-upon, necessarily true belief to serve as a starting point.

However, as we also discussed at the end of Chapter 2, the basic problem with this is simply that it is not enough of a foundation. In short, when searching for necessarily-true starting points concerning the world, Descartes had the same problem as Aristotle, namely, there seem to be no such agreed-upon, necessarily true starting points. And although there is probably more agreement that one can find at least some certainty in the proposition that one exists (at least as a thinking thing), this proves too slim a foundation on which to build.

Popper's Falsificationism

Karl Popper (1902 - 1994) is the best-known advocate of what is generally termed the falsificationist approach. Popper himself did not take falsificationism as being a definitive scientific method; in fact, he did not think there was a single, definitive scientific method. He did, though, view falsification as a key element of science, and a key criterion by which to distinguish scientific theories from non-scientific theories. In what follows, we will look at an outline of Popper's views.

In general, Popper argued that science should emphasize the attempted refutation of theories rather than emphasizing the confirmation of theories. According to Popper, it is too easy to find confirming evidence for many theories. To use one of Popper's examples, consider Freudian psychoanalysis. According to Popper, "predictions" made by this theory are general enough that almost any events could be interpreted as instances of confirmation. And thus, according to Popper, confirming evidence for such theories is simply not of much interest.

In contrast, consider Einstein's theory of relativity. As we noted early in Chapter 4, Einstein's relativity theory predicted that starlight should be bent when passing near a massive body such as the sun. And since such bending of starlight, if it indeed occurred, could be observed during a solar eclipse, Einstein's theory made a specific, dramatic prediction not made by any competing theory. In this way, Einstein's theory was taking a substantial and clear risk of being shown to be incorrect.

In a certain sense, for Popper, the riskier a theory is, the more scientific it is. For example, for the reasons just mentioned (that is, that Einstein's theory made a specific and dramatic prediction and hence was at risk of quickly being shown to be wrong), Einstein's theory is a much better example of a scientific theory than is, say, Freudian psychoanalysis. And in general, for Popper this was the hallmark of good science–science should emphasize falsification over confirmation, and should strive for at-risk theories.

As noted, Popper did not place great emphasis on confirming evidence. For Popper, what characterized a successful scientific theory was not that it had great amounts of confirming evidence; rather, what characterized a successful scientific theory was that it had survived repeated attempts to refute it via the testing of specific, dramatic predictions. And this sort of "falsificationist" approach, that is, the emphasis on trying to falsify theories rather than confirm them, is central to Popper's view.

This is a fairly brief outline of Popper's views, but it should be sufficient to give a sense of the approach he favored. And as you might guess, the issues we discussed earlier concerning disconfirmation reasoning, as well as the issues noted in the discussion of the Quine-Duhem thesis, are especially relevant here. As we noted earlier, seeming cases of disconfirmation are rarely if ever as simple as they appear. Rather, where a prediction made by a theory does not come out as expected, it is always an option, and indeed it is often more reasonable, to reject an auxiliary hypothesis rather than the main theory. In short, although there is no question that cases of disconfirming evidence play important roles in science, the issues surrounding such evidence are sufficiently complex as to make it unlikely that disconfirmation–that is, falsification–can serve as the central feature of science.

The Hypothetico-Deductive Method

One often sees reference to what has come to be called the “hypothetico-deductive” method, and given how prominent such references are, the hypothetico-deductive method is certainly worth discussing here. But our discussion can be brief, since at bottom the hypothetico-deductive method does not involve much beyond issues we have already discussed.

The basic idea behind the hypothetico-deductive method is that from a hypothesis or set of hypotheses (or theories, broadly speaking) one deduces observational consequences, and then tests to see if those observational consequences are observed. If so, then for the reasons discussed earlier in the discussion of confirmation reasoning, this is taken as support for the hypothesis. If the observational consequences are not observed, then again for the reasons discussed earlier in the context of disconfirmation reasoning, this is taken as evidence against the hypothesis.

A quick note: the hypothetico-deductive method is not generally concerned with how the hypotheses themselves are generated, but rather, the method is concerned with the justification or confirmation of hypotheses. In the philosophy of science, this distinction (between how hypotheses are generated versus how hypotheses are justified or confirmed) is usually described as the difference between the *context of discovery* versus the *context of justification*. The context of discovery is generally regarded as the more complex of the two, and as we will see in later chapters, the ways in which actual hypotheses or theories are developed are surprisingly varied and complex. However, as we are seeing, even the context of justification–that is, roughly speaking how we go about justifying or confirming hypotheses or theories–is plenty complex by itself.

There is no question that confirmation and disconfirmation reasoning play important roles in science. Given the close ties between these patterns of reasoning and the hypothetico-deductive method, it is safe to say that this method plays an important role in science. However, consider again the issues we have discussed–the inductive nature of confirmation reasoning, the possibility of rejecting auxiliary hypotheses in the face of disconfirming evidence, the underdetermination of theories, the difficulty if not impossibility of designing crucial experiments, the notion that hypotheses are tested in groups rather than singly, and so on. The view that science proceeds by a relatively simple process of generating predictions from hypotheses and then accepting or rejecting hypotheses depending on whether the prediction is observed seems, given what we have discussed, to be at best an overly simplistic account of science.

Again, there is no question that the hypothetico-deductive method–that is, essentially confirmation and disconfirmation reasoning–plays an important role in science. But given the issues explored earlier, although the hypothetico-deductive method is *a* method used in science, it would be misleading to call it *the* scientific method.

Concluding Comments

The Quine-Duhem thesis, and issues surrounding the topic of scientific method, illustrate some of the ways that issues in science and the philosophy of science are intertwined, and intertwined in complex ways. As noted at the outset of this chapter, our main goal was to get these issues on the table, so that we will be in a position to appreciate the way such issues come into play in cases from the history of science. We will be turning to such cases in Part Two; however, before doing so we need to consider a few more fundamental issues. We will turn next to some puzzling issues involving inductive reasoning.

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<Below are the captions for the figures in this chapter.>

Figure 5.1: Illustration of Euclidean Axiom