The Problem with the Problem of Induction

The problem of induction notoriously concludes that induction is an illegitimate form of reasoning. But there is a fundamental problem with this problem. For when we examine exactly how the problem of induction concludes this, we find that its own argument invariably presupposes the legitimacy of induction in one way or another. Consequently, one may pose the problem of induction only by presupposing what it aims to deny. In short, then, the problem with the problem of induction is that it contradicts itself. In order to see exactly how it does this, let us begin by reviewing it and how it emerges.

This classic problem emerges naturally when we assume a skeptical posture and wonder how we might justify the very process of inductive reasoning. Traditionally, we may justify something either inductively or deductively. Now, obviously, we cannot justify induction inductively, for this would be to argue in a circle. But can we justify it deductively? It seems not. After all, a deductively valid argument is one whose conclusion *must be true* whenever its premises are true. But whenever the premises of an inductive argument are true, nonetheless its conclusion *may be false*. In order to see this important difference most clearly, let us eschew the textbook examples and consider an actual example of induction from the history of science.

In the early seventeenth century, Galileo and his Aristotelian contemporaries disagreed about whether the earth moves. One observation crucial to their disagreement concerned stellar parallax. If the earth moved in a giant annual orbit around the sun, as Galileo claimed, all astronomers training their telescopes on any particular star should have observed a parallax between their observations of it at different times of the year. For the diameter which Galileo believed the earth traversed in six months seemed big enough to require an observation of significant parallax. As it turned out, however,

astronomers of the time observed no parallax at all. That is to say, they observed no parallax in the first year they looked for one; then, trying again, they observed no parallax in the next year; and so on. After these several years of trying in vain to observe a parallax, though, the Aristotelians induced from this finite number of failures a universal conclusion that claimed to apply to all attempts to observe stellar parallax. They would all fail, the Aristotelians concluded, because there was simply no parallax to be observed, and thus the earth did not move.

Their inductive argument was deductively invalid, however, because the truth of its premises did not *necessitate* the truth of its conclusion. For although it is true that seventeenth century astronomers failed to observe a parallax on all the occasions they looked for one, it nonetheless remained *possible* that they might observe one later. Or so an anachronistic advocate of the problem of induction could have claimed. In other words, he could then have claimed that although the Aristotelian astronomers' premises are all true, nonetheless their conclusion *may be false*. And this, he could have added, is the hallmark of a deductively invalid argument. But in our own time we must notice the presupposition of his argument. As we now say, it is *modal*: it asserts a lack of *necessity*, or a *possibility*. Sensitive as we are to the problems such arguments pose, though, we must nowadays ask what could justify such a modal claim. To such a question, at least four answers seem available, although the fourth is by far the most common. As it turns out, close examination of the first three answers will help prepare us to later examine the fourth. Let us therefore begin with these less common answers.

First, this early advocate of the problem of induction could easily justify his modal claim by an observation. After all, an observation of stellar parallax would easily prove that the Aristotelian astronomers' conclusion *may be false*; for such an observation would show this conclusion to be, furthermore, *actually false*. In the history of science, as a matter of fact, such an observation was made, but not until the nineteenth century. For only by then were telescopes powerful enough to reveal the tiny parallax of something so

distant as a star. In the nineteenth century, therefore, another, later advocate of the problem of induction could have easily justified his predecessor's modal claim. Or so it seems.

For in order to be consistent, the more recent advocate should have similarly applied his predecessor's earlier skepticism about induction to the new observations of stellar parallax in the nineteenth century, arguing now that although the phenomenon had been observed during several years, to boldly conclude that it would therefore be observed in all other years would be deductively invalid. After all, he should say, the finite number of recent observations of stellar parallax does not *necessitate* the universal conclusion that stellar parallax will be always observable. For this conclusion nonetheless *may be false*.

Notice, however, that if he renews this skepticism, he thereby subverts this first justification of his predecessor's modal claim. For according to the first answer, the validity of the new observation of stellar parallax was supposed to bolster his predecessor's earlier skepticism. So long as the validity of the new observation remains doubtful, then, his predecessor's earlier skepticism will likewise remain unjustified. In order to bolster that earlier skepticism, therefore, he must presuppose the legitimacy of this later induction. But this will not do. For by presupposing the legitimacy of this later induction, he subverts the problem of induction which he has sought ultimately to advocate.

And yet, if he does not presuppose the legitimacy of this later induction, remaining faithfully skeptical about it, he now has the same problem as his predecessor: how to justify his own modal claim? He cannot hope for the discovery of another sort of observation by another generation of scientists, as this first answer would suggest, for then the same problem will only arise all over again. Indeed, the process could continue *ad infinitum*. Following this first answer, he must choose between setting off on an infinite regress and presupposing the legitimacy of induction. Neither will do. The

advocate of the problem of induction needs a justification of his modal claim that does not require an actual observation. And the next answer attempts to provide just such a justification.

Second, then, the skeptic might try to justify his modal claim by pointing out that many inductions have proven false in the past, so that we have good reason to expect any other induction will also prove false. But this is evidently a bad answer, for two reasons.

First of all, it contradicts itself when it tries to *induce* from the falsity of many inductions the conclusion that all other inductions may be false. For if we were to follow its super-induction, and suspect all inductions accordingly, we should likewise suspect the super-induction itself. And yet, suspecting this super-induction, it seems we should not follow it. Were we to ignore it, then, and trust once again in induction, we should nevertheless remember that many inductions have indeed proven false in the past, and might thus reasonably induce from these failures a global suspicion of induction. With this renewed suspicion of induction, though, we would find ourselves again believing the super-inductive principle, and thus again setting off on an infinite regress. This is therefore the first reason why this second answer is a bad one: its super-inductive principle is self-contradictory. (It might seem odd to some readers that something should both contradict itself and also produce an infinite regress, but the liar paradox--"This sentence is false"--also does this, and for the same reason: self-reference.)

But this second answer is also bad for another, more fundamental reason. Even if this super-inductive principle were not a logical singularity, and paradoxical in its own right, the very fact that it is an *inductive* principle means that someone who appeals to it must presuppose the legitimacy of induction. Setting aside the self-contradiction of the super-inductive principle itself, then, its use to bolster the problem of induction would also be self-contradictory. In sum, following this second answer, the skeptic has no choice but to contradict himself, once in the statement of his super-inductive justification, and again in his ultimate advocacy of the problem of induction. The next answer thus

tries to escape these contradictions by returning to our example from the history of science.

Third, the skeptic might try to justify his modal claim by appealing to other theoretical considerations. In our historical example, for instance, the early skeptic would likely have been a Copernican. Indeed, he might have been Galileo himself. After all, when Aristotelians induced that there was no stellar parallax (and thus that the earth did not move) from finite previous failures to observe a stellar parallax, Galileo himself actually responded that the stars were too far away for this phenomenon to be observed, at least with the telescopes of the time. In a way, then, Galileo advocated a version of the problem of induction. For he argued that previous failures to observe parallax did not *necessitate* the Aristotelian conclusion that there was no parallax; the Aristotelian conclusion, he objected, *may be false*. It may be false, that is, because the stars may be so far away that parallax cannot be observed with primitive telescopes.

Notice, though, that were the early skeptic to have justified his modal claim in this way, he would thereby have been appealing to other theoretical considerations, notably the distance of the stars. In the seventeenth century, as also now (though less obviously), this was a theoretical consideration. Galileo believed it based upon his firm belief in the Copernican theory, and especially its central claim, that the earth moves. After all, he reasoned, if the earth moves in a giant annual orbit, and parallax is so far unobserved, we must conclude that the stars are far away, with the result that the parallax will be tiny, so tiny that present telescopes are unable to discriminate it. But this is a paradigmatically theoretical argument, using as it does the theoretical premise *the earth moves*. And this premise was, and still is (though less obviously), a paradigmatically theoretical premise, since its justification requires a number of scientific observations. For Galileo himself, these observations included spots on the moon, comets, and the phases of Venus, among others. And these many observations were not haphazard--they

were all linked by a scientific theory, Copernicanism. According to this third answer, then, the skeptic could have justified his modal claim by appealing to this theory.

This third answer fails, however, for the same reason as the first, and just as badly. For if the skeptic were to justify his modal claim by appeal to other theoretical considerations, he must remember to renew the same skepticism with regard to them as well. That is to say, if he argues that the Aristotelian induction about stellar parallax *may be false*, and justifies this modal claim by appealing to the theoretical premise that the earth moves, to be consistent he should renew his skepticism here and ask how he knows this theoretical premise. Naturally, any proof to this effect will use induction.

Galileo himself, as we have seen, tried to prove that the earth moves by observing on many occasions that Venus exhibited phases, just like the moon. For he believed that these phases were explicable only on the hypothesis that the earth moves. As it turned out, he was mistaken: Venus does indeed exhibit phases, but this phenomenon was explicable also on the competing hypothesis of Tycho Brahe, according to which the earth remained stationary. At any rate, Galileo's mistake is irrelevant to our purposes here. What matters is that a stalwart skeptic could have raised a fundamental problem for this or any other theoretical consideration that Galileo might have raised to justify his modal claim. It should by now be familiar: although Galileo may very well have observed the phases of Venus on several occasions, these observations could not *necessitate* the conclusion that Venus has phases; for it is always still *possible* that this conclusion is *false*. Our stalwart skeptic, then, must emphasize this gap and thereby subvert his own theoretical justification for the original modal claim required by his original use of the problem of induction.

In desperation, he might try to close this gap by appealing to some other theoretical consideration, but in order to be consistent this too should be subjected to his original skepticism. Now generally, as we know, all theoretical considerations try to move from a finite list of observations to a universal conclusion, one which aspires to

apply to an infinite number of circumstances. But the skeptic must always doubt such a move, so that any effort to make it more sure, by whatever new theoretical consideration--even when he himself tries to do so, even when he does so in order to justify the modal claim needed for his original advocacy of the problem of induction--should only provoke redoubled skepticism. In this way, if any other theoretical consideration is raised to block this redoubled skepticism, here too he must raise the same problem of induction. And this could continue *ad infinitum*. So his vigorous skepticism sends him off on an infinite regress, ironically, to justify his original skepticism.

In sum, then, if he wishes to justify the modal claim of his original skepticism, he must therefore choose between setting off on an infinite regress or once ignoring the gap between finite observation and infinite universal conclusion, thereby presupposing the legitimacy of induction. Neither option will do. So the skeptic needs a justification of his modal claim that does not require any theoretical consideration at all. The next and final answer attempts to provide just such a justification.

Fourth, the skeptic might try to justify his modal claim by invoking the imagination. Frankly, this is the most common answer, and thus the most common form of the problem of induction. Indeed, it is so common that we might have ignored the others and treated it alone. And yet, as we shall see, our treatment of the first three answers has not been wasted time. They have helped us prepare for this one. For as we shall see, the same failures which they exhibit obviously, this fourth answer disguises but nevertheless contains.

According to this fourth answer, the skeptic can justify his modal claim--that is to say, his claim that the conclusion of any given induction *may be false*--by asserting that we can *imagine* a scenario in which it *is false*. Here he agrees with many recent philosophers, who assume that imaginability tests possibility. Like them, he assumes that something is possible if it is imaginable. And yet this conditional, and the epistemology it expresses, are rather vague. Our astronomical example will help to clarify it. Galileo

could have justified his claim that the Aristotelian conclusion *may be false* by asserting that he, and presumably others too, could *imagine* a scenario in which it *is false*. Such an act of imagination would involve, as we have seen, imagining a scenario in which the stars are far away--so far away that parallax here on earth is tiny, and thus impossible to observe, at least with primitive telescopes.

As a matter of historical fact, Galileo did imagine such a scenario. But what did this act involve? In order to speculate, let us briefly consider what *we* do when we imagine that the stars are far away. Of course, we know that different stars are vastly different distances from us--though they're all pretty far. Let us simplify, then, and consider what we do when we imagine a star that is, say, a billion miles from earth. [*Pause*] We certainly do not entertain a mental picture of this distance. For when we entertain a mental picture, the most we can imagine, it seems, is about the distance to the horizon--a few miles, at most a few dozen.

[*Comic relief*: Thomas Jefferson built a beautiful rotunda at the summit of his University. He made this the library and fastened magnificent windows around it, so that standing amid the books students could look to the horizon and see the rolling hills of Virginia. He did this, he said, to remind its students of "the illimitable power of the human mind". Now as it turns out, the power of the human mind reaches its limit by the time it settles on Monticello, about twenty miles away. But we should forgive Jefferson his optimism; the Enlightenment was a giddy time.]

Therefore, when Galileo or anyone else imagines a star far away--a billion miles away, or even significantly less than that--he cannot do so with a mental picture only, and certainly not an accurate one. What, then, did he do? He no doubt entertained a mental picture of some sort. For when we imagine a distant star we entertain some mental picture or other: for instance, a glowing globe, the blackness of space, a great distance between us and the globe, perhaps twenty miles at most. More importantly, though, we attach to this picture an interpretation, a story. In our story, the distance between us and

the globe is a billion miles; in Galileo's story, it was far--far enough to make stellar parallax unobservable with primitive telescopes. Most important of all, though, when we attach this story to our mental picture, we attach it as a true story, an actual story. Needless to say, we firmly believe that the stars are billions of miles away. Similarly, when Galileo attached his story to his mental picture, he attached it also as a true story, an actual story. After all, he also firmly believed that the stars were far away. So this is how we both imagine *as actual* such a scenario. According to this epistemic test, finally, it is our imagination of this scenario *as actual* that is supposed to prove that such a scenario is actually possible.

But what about the Aristotelians of the seventeenth century? They may have entertained a mental picture just like ours; moreover, they may have similarly attached a story to this picture according to which the stars are far away. By contrast, however, when they attached this story, they must have attached it as a false story, even a ludicrous one. For how could they have done otherwise?--A story in which the stars are far away fit poorly with the scientific theory to which they subscribed.

According to their story, briefly, all stars occupied the most distant heavenly sphere. Between this sphere and the earth, which remained stationary at the center of the universe, lay the other heavenly spheres, each of which contained one of the other heavenly bodies--including the moon, the sun, and the known planets. These spheres, and thus the bodies embedded in them, revolved around the earth once a day--accounting thereby for the observed diurnal motion of the heavens. With such a model, there was no reason to believe that the stars, and their sphere, should be any farther away from the next furthest sphere, that of Saturn, than that sphere itself was away from the next furthest sphere, that of Jupiter. Moreover, there was quite a good reason not to believe this. Since they supposed these spheres revolved completely once a day, a sphere as far away as Galileo claimed the stars were, would have to move at an incredible speed to achieve its full revolution. (To be exact, when Galileo retorted by protesting that the stars were not

embedded in any sphere and did not need to move at any speed at all, since it was the earth and not the heavens that moved, the Aristotelians asked him why the earth did not leave its inhabitants behind in its giant sweep around the sun, or why an object tossed in the air did not land many miles to the west instead of falling straight down, as it does. Galileo had no answer to these and related questions; consequently, he dedicated the rest of his researches to providing one.) At the time, then, a proposal that the stars were far away was ludicrous to Aristotelians. And reasonably so.

Could they therefore have imagined it? As it turns out: in one sense, no; in another, yes. For there are two importantly different senses of imagination: first of all, as we have seen, we can imagine something *as actual*; but, secondly, we can also imagine something *as possible*. In the former sense of imagination, as we have also seen, the Aristotelians *could not* have imagined a scenario in which the stars are far away. After all, they had good reasons for believing such a scenario false--that is to say, not-actual. In the latter sense of imagination, by contrast, they *could* have imagined a scenario in which the stars are far away. After all, it is quite easy to imagine something as possible. Let us examine these alternatives in detail, and in reverse order.

The Aristotelians could easily have imagined a scenario in which the stars are far away, so long as we mean the latter sense: *imagination as possible*. But this sense will not suffice to prove an actual possibility, which is what is required by the fourth answer to our question about the skeptic's modal claim. For if the Aristotelians imagined some scenario in which it was possible that the stars are far away, while remaining faithful to the epistemic test of possibility, the way for them to do this would have been to imagine a scenario in which they are imagining some other scenario in which the stars are far away. However, this nested imagination proposes a familiar dilemma. For the second act of imagination must itself be either *imagination as actual* or *imagination as possible*. If it is imagination *as possible*, the Aristotelians must therefore imagine a first scenario in which they are imagining a second scenario in which they are, in turn, imagining a third

scenario--one in which the stars are far away. So long as they continued to imagine *as possible*, this process would have to continue *ad infinitum*.

What about the alternative: *imagination as actual*? This seems more promising. For remember, the fourth answer to our question about the skeptic's modal claim required him to imagine this scenario *as actual* if his imagination was to have proven that it is actually possible that the stars are far away (thus eventually vindicating his skepticism about induction). Focusing on the Aristotelians' act of imagining, then, and wondering whether they could have done this imagining *as actual*, and thus proved it to be actually possible that the stars are far away, we see that neither their mental picture nor their story could have done this. Their mental picture could not have done this, since the most it could have proven was that it was actually possible that a glowing globe might exist twenty miles away from their bodies. But neither could their story have done this. For according to their story, as we have seen, this vast distance of the stars was not only false but ludicrous.

In sum, although the Aristotelians could have imagined *as possible* that the stars are far away, this would have been insufficient to prove that it is actually possible that the stars are far away. Furthermore, they could not have imagined *as actual* that the stars are far away--the sense sufficient to prove that it is actually possible that the stars are far away--because their theoretical perspective precluded them from doing so.

Let us therefore return to Galileo, someone who struggled to liberate himself from that theoretical perspective, and see whether his imagination of the same scenario could nonetheless have proved the same possibility. He, after all, had no scruples imagining *as actual* a scenario in which the stars are far away. But what did this prove? As it turns out, nothing. For like the Aristotelians, Galileo's act of imagination had two components: a mental picture and a story.

His mental picture was the same as that of the Aristotelians: a glowing globe, hovering in the blackness of space, about twenty miles from his body. Therefore, like

them, his mental picture proved nothing more than this prosaic possibility. Neither did his story prove anything, even though it differed from that of the Aristotelians. For if it proved that the stars were far away, whether possibly or actually, it did so only on the merits of his astronomical treatises. This story was nothing more than a personal version of those publicly available theories. Therefore, whatever it proved, it did so by relying on them. Consequently, if they proved anything at all, they did so by themselves, with no help from this sort of imagination.

If this theory of scientific imagination is correct, then, and scientific imagination does indeed function by attaching theoretical stories to prosaic mental pictures, imaginability cannot be a test of possibility, at least not in science. The belief that it can be such a test has probably arisen from the tacit belief among many philosophers that the imagination is a pure faculty, one immune to theoretical considerations, and thus a neutral ground upon which rival theories may compete. However, as we have seen, scientific imagination is far from theory-independent. When Galileo imagined distant stars he did so from the perspective of his own fledgling theory; when Aristotelians imagined distant stars they also did so from the perspective of their own venerable theory. Scientific imagination is therefore theory-dependent, because it involves attaching particular theoretical stories to mental pictures, so that these pictures might mean more than their prosaic equivalents. In this respect at least it is like observation, which recent philosophy of science has decisively shown to be theory-dependent. And this should not surprise us. For if the imagination is, as it seems to be, a sort of faculty of mental observation, we should not be surprised to find it exhibiting some of the same features as sensory observation.

But, in addition, if scientific imagination is theory-dependent, a familiar problem arises for the problem of induction upon this fourth answer. In order to elicit this familiar problem, let us first recall where we are in the dialectic of this paper.

We began by reviewing the problem of induction, noticing how it makes a modal claim: for any instance of induction, even when its premises are true, nonetheless its conclusion *may be false*. Wondering how this modal claim was justified, we next surveyed three failed answers to this question. The first answer asserted that the modal claim could be justified by an actual observation. It failed because it forced a choice between setting off on an infinite regress or presupposing the legitimacy of induction under the guise of an actual observation. The second answer asserted that the modal claim could be justified by considering the many known failures of inductive reasoning and using the super-inductive principle which they may warrant. This principle failed because it was self-contradictory and led to an infinite regress, while this second answer as a whole failed because, like the first, it presupposed the legitimacy of induction. The third answer asserted that the modal claim could be justified by appeal to other theoretical considerations. It failed because it too forced a choice between setting off on an infinite regress or presupposing the legitimacy of induction, this time under the guise of these other theoretical considerations.

Examination of these answers and their failures has prepared us well to notice the failure of the fourth answer. It asserts that the modal claim is justified by an appeal to the imagination. However, if scientific imagination is theory-dependent, as we have seen, then just like the third answer, this fourth answer fails because it forces a familiar choice between setting off on an infinite regress or presupposing the legitimacy of induction--this time, finally, under the guise of a putatively pure and neutral imagination which is in fact stained and biased by theory.

After all, if the skeptic tries to justify his modal claim by asserting that the original Aristotelian induction (that stellar parallax does not exist) *may be false* just because we can *imagine* a scenario in which it *is false*, this act of imagination must invoke theoretical considerations. For as we have seen, in order to imagine a scenario in which the Aristotelian induction is false we must imagine a scenario in which stellar

parallax does exist but is so tiny as to be unobserved by primitive telescopes. The most convenient way in which to do this, as Galileo himself discovered, is to imagine a scenario in which the stars are far away. And the way we do this, if this theory of scientific imagination is correct, is to entertain a mental picture of a star at the maximum mental distance, perhaps twenty miles, and then attach to it an interpretation, a story, according to which the star is far, far away. Now if this story makes sense, rather than being ludicrous, the skeptic has implicitly invoked theoretical considerations--the theoretical considerations of Copernicanism, which we discussed in our examination of the third answer.

However, in order to be consistent, as we then saw, he must renew here his skepticism about induction and faithfully subject Copernicanism to the same rigorous standards, just as he earlier subjected the Aristotelian induction. But as we have also seen, Copernicanism, like any other set of theoretical considerations, will exhibit a familiar gap which should provoke the same skeptical judgment: even when its premises are true, nonetheless its conclusions *may be false*. Like the failure of the third answer, therefore, if the skeptic wishes to justify the modal claim of his original skepticism in this way, he must similarly choose *between*, on the one hand, setting off on an infinite regress of theoretical considerations to bolster theoretical considerations to bolster theoretical considerations, etc.--all to justify his original modal claim; *or*, on the other hand, ignoring one time this troublesome gap between finite observation and infinite universal conclusion, thereby presupposing the legitimacy of induction. Neither option will do, of course. Thus, the fourth answer fails for precisely the same reason as the third.

And yet, as we have seen, all four answers failed for roughly the same reason. Each one either presupposed the legitimacy of induction or left the skeptic in a dilemma between this presupposition and some other, even less desirable option. In conclusion, then, each answer subverts the problem of induction by contradicting it. For when the

problem of induction must presuppose the legitimacy of induction, it thereby contradicts itself, and reveals itself as no problem at all.