Thought experiments since the Scientific Revolution

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Introduction

Only the untutored would say that science *began* in the seventeenth century, but this historical period did mark a difference in the way things were done. The most common view of the Scientific Revolution is that the investigation of nature became much more experimental, that careful observation became the standard practice. The other side of the experience vs reason dichotomy has its champions, too, however. None is so forceful as Alexandre Koyré who unabashedly declares that 'Good physics is made *a priori*' (1968, p. 88.) Koyré concurs that something quite different started to happen in the seventeenth century, but it was not the increased reliance on experimental observation. In fact, Koyré says, it was exactly the opposite, and he goes so far as to claim for Galileo 'the glory and the merit of having known how to dispense with experiments . . .' (1968, p. 75). The Scientific Revolution happened because people stopped looking and started thinking.

Perhaps more than anything else, the thought experiment is the main vehicle of the new way of doing science. This wonderful device for the investigation of nature is the subject of this essay. I will be examining the structure of thought experiments and their role in the development of science, both in the scientific revolution and in more recent times (since, after all, basic techniques have not changed one iota since Galileo).

The seventeenth and twentieth centuries, more than other times, are the home of the thought experiment. The scientific revolution was in full flower during the 'century of genius', and the reason for this had as much to do with the purely mental gymnastics of Galileo, Descartes, Newton, and Leibniz as it had to do with new experimental techniques and empirical results. In our own century, the trains and elevators of Einstein, and the various imaginary contraptions of Heisenberg, Bohr, and Schrödinger have had a similar impact in the contemporary revolutions of relativity and quantum mechanics. It might well be admitted by everyone that experi-

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ments performed in the mind have had some impact; most people have nice things to say about reason so long as reason isn't taken too seriously. But when it comes to detailed accounts of what thought experiments are and how they work, the paucity of discussion to be found is somewhat surprising.[1] There is remarkably little in the way of sustained comment, and the views of those few who have pronounced are not entirely satisfactory, though some are quite insightful. Koyré and Kuhn, for instance, are both quite interesting while Mach and especially Duhem are rather disappointing.

When we turn to the great philosopher/physicist/historians, Pierre Duhem and Ernst Mach, we expect to be enlightened on the subject; but this is not to be. Duhem is disappointingly critical. He considers thought experiments to be bogus and misleading. 'To invoke such a fictitious experiment,' he complains, 'is to offer an experiment to be done for an experiment done; this is justifying a principle not by means of facts observed but by means of facts whose existence is predicted . . . , an act of bad faith. But,' Duhem continues, 'there are worse things. Very often the fictitious experiment invoked is not only not realized but incapable of being realized; it presupposes the existence of bodies not encountered in nature and of physical properties which have never been observed.' (Duhem, 1954, p. 202.)

Though most often he is admirably subtle in his empiricism, here Duhem seems unreasonably simple-minded in his insistence on the observable. (Perhaps we should keep in mind that Duhem was no great friend of the scientific revolution, nor of Galileo, the greatest thought-experimenter.)

Extreme empiricism, however, doesn't get in the way of Mach's admiration for thought experiments. He holds the more common view that they are interesting and important. Calling them *gedankenexperimente*, he was perhaps the first to baptize the genre. His great philosophical/historical work, *The Science of Mechanics*, has several examples which are cited with high approval. But when Mach tackles the subject directly in his essay 'On Thought Experiment', we find only platitudes and hand waving. There is no analysis of what thought experiments are, or how they work.

On the other hand, the more recent studies of Alexandre Koyré and Thomas Kuhn are considerably more perceptive. Both see thought experiments as playing a pivotal part in their respective accounts of science. As I alluded to above, Koyré, who is a thoroughgoing Platonist, sees thought experiments as performing an *a priori* role. Kuhn sees thought experiments as often playing a crucial role in paradigm change. 'A crisis induced by the failure of expectation and followed by revolution is at the heart of the thought-experimental situations we have been examining. Conversely, thought experiment is one of the essential analytic tools which are deployed during crisis and which then help to promote basic conceptual reform.' (Kuhn, 1977, p. 263.) The studies of Koyré and Kuhn have much to recommend them and I shall be following their views in several respects, though they are not without their shortcomings which I'll mention below.

Before getting on with the main considerations, the topic should be circumscribed. I may not be able to define thought experiments, but I should say something about what they are *not*. First, they should be distinguished from *merely imagined* experiments. In some instances, when he says that a cannon ball and a musket ball fall with the same speed, Galileo is describing an imagined experiment. Galileo didn't perform it (at least, not according to Koyré) but he gives the results with an air suggesting that he did. The crucial thing is that he *could* have performed the experiment. When we have an experimental observation which could have been carried out, but wasn't, we usually have a merely imagined experiment.

On the other hand, a genuine thought experiment usually cannot be performed, in principle. It is either technologically, physically, or conceptually impossible. We have entered the realm of the genuine thought experiment when we consider frictionless planes and massless levers, or when we posit intelligent demons who are no bigger than a molecule, or when we require a degree of observational accuracy greater than anything practically possible, or when we rotate a bucket while the rest of the universe has vanished, or when we observe a light ray bend while standing in an elevator in a gravitational field so strong that, in consequence, we would really be just a puddle on the floor.

The merely imagined experiment is not the same sort of thing as what I am here calling a thought experiment. Koyré might be right in claiming that Galileo didn't perform either type of experiment, but we should still not run the two together (even though the boundary between them may be fuzzy).

There is another kind of experiment which takes place in thought, but isn't a genuine thought experiment in my sense either. In psycholinguistics, for example, we might be asked to consider whether a sentence, say, 'Colourless green ideas sleep furiously', is grammatical. We introspect to find the answer. Understandably, this is often called a thought experiment. But it strikes me as *not* a *thought* experiment at all. On the contrary, it seems a *real* experiment where thinking is the *object*, not the *method*, of the experiment. At any rate it appears to be quite different than the classic thought experiments in the history of physics.

Thought experiments in philosophy (especially in the philosophy of mind) may be closest to the classic examples from physics. Typically, one might imagine two people fused together into one, or one person splitting like an amoeba into two. Morals are then drawn about personal identity and survival. Such thought experiments have recently come under attack (Wilkes, forthcoming) which is slightly worrisome since the arguments against their use in philosophy might spill over and undermine the use of thought experiments in physics. However, I won't try to defend them here; I'll take it for granted that thought experiments have a legitimate use in physics and rather try to spell out how it is that they work.

If definition by example is what's called for, then here is a list of typical thought experiments that I'll focus on: Einstein's elevator, Newton's bucket, Maxwell's demon, Stevin on the inclined plane, Newton on orbits, Galileo on free-fall, E.P.R., Schrödinger's cat, Einstein, chasing a light beam, and Leibniz on *vis viva*. Of course, many others could be added. Before getting on to an analysis of these enticing things, and to put ourselves in the right frame of mind, it would be pleasant to have a leisurely look at some of these examples of thought experiments. Considerations of space, however, prohibit this. We'll see some of them later as illustrations, but for now I can only hope that the right images were conjured up by this short list.

A taxonomy

We may praise the inventors and savor their achievements, but can we say anything more about thought experiments? Are they just a diverse and curious collection of dazzling displays of mental gymnastics, each unique in its own way; or is there some sort of pattern? Some of the mystery can be taken out of the subject if we impose a classification scheme on things. The first main task of this paper will be to construct a taxonomy. But just before I do, I want to note something rather striking about thought experiments.

In normal situations there is a huge jump from the observed data to an explanatory theory. This doesn't seem to be the case in thought experiments at all. The leap is tiny, so small that one is tempted to say: 'Just look after the (thought-experimental) data and the theories will look after themselves'. I'll state this as my first claim:

> *Thesis I*: The burden of any thought experiment rests on the establishment (in the imagination) of a phenomenon. Once the phenomenon is established, the inference to a theory is fairly unproblematic; that is, the jump from data to theory is relatively small.

Why is the inferential jump such a small one? I'll try to give an answer to this question below, but for now I'll turn to the details of my taxonomy. The classification will be motivated in large part by Thesis I.

The scheme I propose is a fairly simple one. First, thought experiments break into what I'll call the *destructive* and the *constructive* kind. In turn, constructive thought experiments admit a further division into the *conclusive* and the *conjectural.*

My second thesis consists of this taxonomy; that is, *Thesis II* is simply

- Thought experiments
  - Destructive
  - Constructive
    - Conclusive
    - Conjectural

the claim that this is the right way to chop things up. A few examples to illustrate the scheme will also help to make it convincing.

As its name suggests, the *destructive* thought experiment is essentially an argument directed *against* a theory. It destroys or at least presents serious problems for a theory, usually by pointing out a shortcoming in the existing framework which may be anything from a minor tension to an outright inconsistency.

Einstein reports (1949, p. 53) that he was but in his teens when he discovered a problem with Maxwell's electrodynamics. What would it be like to travel beside the front of a beam of light? According to Maxwell's theory, light is a wave in the electromagnetic field. As the electric field changes, the magnetic field comes into being, and as the magnetic field changes the electric field comes into being. It is like leap frog, except the frogs exist only when leaping. To travel along with the front of the beam is to see a stationary oscillatory field. But such a field could not exist.

Another example of the destructive thought experiment is Schrödinger's 'cat paradox' (Schrödinger, 1983), but the result (a cat in the superposition state of being both living and dead) is not logically impossible, nor incompatible with the usual understanding of the theory. Rather, it shows the theory to have a very bizarre and highly counter-intuitive consequence.

A chief characteristic of all thought experiments is their *picturability*. Lots of theories are inconsistent, but a demonstration of their inconsistency wouldn't necessarily be a thought experiment. Quantum electrodynamics, for instance, implies the absurdity that the self-energy of the electron is infinite. A proof of this (i.e., a demonstration that a certain series is divergent) is not a thought experiment.

The other main category of my taxonomy is the *constructive* thought experiment. The idea here, as the name suggests, is that we get some sort of support for a theory. But the constructive immediately breaks down into two further types: the direct argument, on the one hand, and the occasion for speculation, on the other. Respectively, they are as I shall call them, the *conclusive* and the *conjectural.*

Simon Stevin wondered about some of the properties of the inclined plane. In particular, he wondered whether a chain on a frictionless plane (such as in the diagram below on the left) would remain in equilibrium, or, acting under gravity, slide down one side or the other (Mach, 1960).

After a quick glance at the diagram on the right, the answer is obvious. The chain must remain in equilibrium. The chain below is perfectly

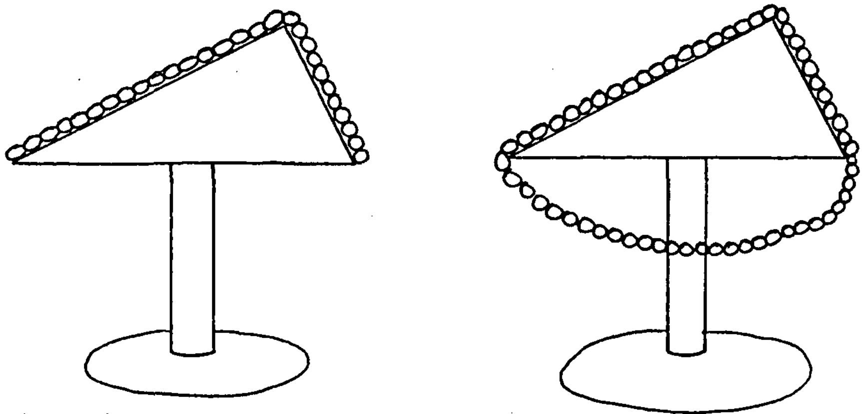

Diagram 1

symmetrical. If the links above started to move in either direction, then they would have to continue forever.

Stevin's reasoning is simple and ingenious. The answer is conclusive and entirely convincing. It does rest on one important assumption, however: There are no perpetual motion machines. Thought experiments often draw on auxiliary assumptions, and moreover, those assumptions might be false. Just as there are no *crucial experiments* in general, so, thought experiments are not crucial either. They are fallible and often rest on considerations which may prove wrong in the long run.

Newton's thinking about the moon's orbit presents us with another example of a conclusive thought experiment (Newton, 1934). He asks us to

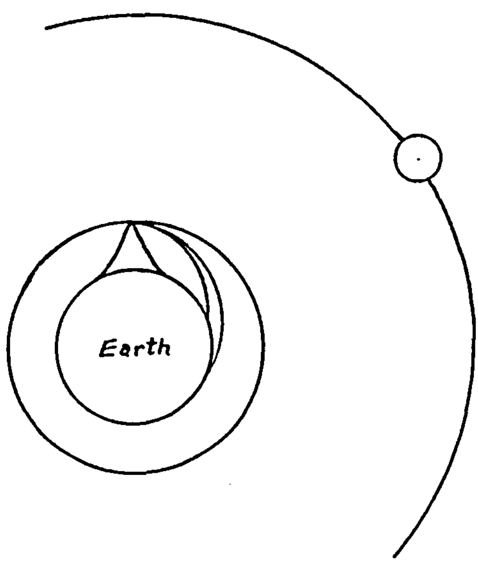

Diagram 2

imagine a cannon fired from a mountain top. The cannon ball falls toward the earth each time. But the more powder we put in, the farther it goes. We could conceivably carry this on to the limit when the cannon ball falls all the way around the earth and comes back to where it started from. Once we see this possibility for a projectile, we then see that the moon is not 'suspended' in the sky, but rather, is constantly falling to the earth in exactly the same way as the cannon ball.

Einstein's elevator provides a third illustration. According to the *principle of equivalence*, there is no difference between frames of reference; whether they are inertial or not, the laws of physics are the same in all (Einstein, *et al.*, 1952). Suppose then, an observer is inside an elevator sealed off from the outside so that the observer cannot tell whether in a gravitational field or accelerating. If accelerating, and if a light beam were to enter one side, then, due to the elevator's motion, the beam would appear to drop or curve down as it crossed the elevator. Consequently, it would have to do the same thing if the elevator was not accelerating, but was in a gravitational field. Therefore, gravity 'bends' light.

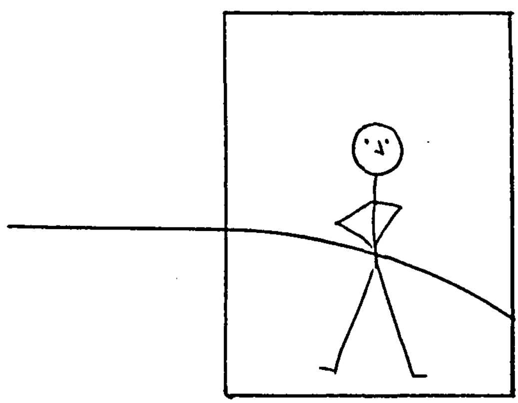

Diagram 3

The thought experiments of Stevin, Newton, and Einstein are examples of what I call conclusive thought experiments. Now to an example of a conjectural one.

The bucket experiment which is alleged to show the existence of absolute space is one of history's most celebrated and notorious thought experiments. Newton (1934) asks us to imagine the rest of the material universe gone and to focus our attention on a solitary bucket partly filled with water and suspended by a twisted rope. As the rope unwinds we notice distinct states of the bucket/water system.

In state I, before the bucket is released, there is no relative motion between the bucket and the water, and moreover, the surface of the water is level. In state II, just after the bucket has been released, the water and

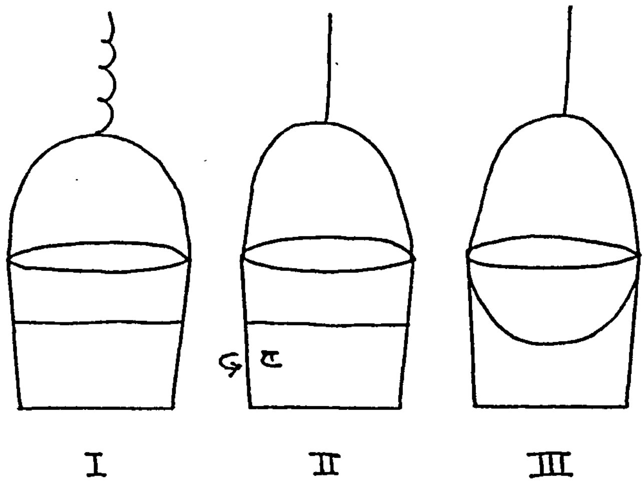

Diagram 4

the bucket are in relative motion. In the final state, after some time has passed, the water and the bucket are again at rest with respect to one another; but this time the surface of the water is not level.

The problem now is this: how do we account for the difference between states I and III? How might we explain this (thought-experimental) phenomenon? Newton's answer is very simple. The bucket and water are at *rest* with respect to *space itself* in state I and are *rotating* with respect to this absolute space in III. That (with the law of inertia) explains the difference in the surface of the water in the two cases.

Now to stress a most important point. Since it is not a question of fallibility or infallibility, it might well be wondered why I'm calling Newton's thought experiment 'conjectural' and Steven's 'conclusive'. Since thought experiments are all fallible, shouldn't they all be called conjectural and none of them termed conclusive?

There is a good reason for making the distinction that I do, and it is connected to my first thesis. Newton's bucket experiment has two distinct parts. The first part is the claim *that a certain phenomenon exists* (that is, exists in the thought-experimental situation). Given this phenomenon, we might agree that Newton has conclusively established the existence of absolute space in the second part of his overall argument. However, it is the *first* part of the thought experiment which makes it conjectural. It is also, not surprisingly, the part which has received the most criticism. Both Berkeley and Mach *denied* that if all the rest of the matter in the universe were somehow eliminated, the water would climb the walls of the bucket as described in state III. They denied the existence of the very thing Newton was trying to explain. This phenomenon, they claimed to the contrary, only occurs when the bucket and water are rotating with respect to the 'fixed' stars and, moreover, if we could somehow give the stars a push 'around' the bucket, then the water would climb the walls just as in state III. (This is Mach's Principle.) Thus, *contra* Newton, they maintain *all* motion is relative motion.

However, the crucial thing, I repeat, was not Newton's passage from the imagined observations to the theory of absolute space; rather, it was his positing of those observations in the first place. That's what makes it a conjectural thought experiment. Stevin and others had to *argue* for the phenomena in their respective cases.

Among other conjectural thought experiments is Maxwell's famous demon (Maxwell, 1871). The second law of thermodynamics implies that heat won't pass from a cold body to a hot one. In classical thermodynamics this law is quite strict; but in Maxwell's kinetic theory of heat there is a probability, though extremely small, of such an event happening. Some thought this a *reductio ad absurdum* of Maxwell's theory. To show how it is logically possible to violate the second law Maxwell imagined a tiny creature who controls a door between two chambers. Fast molecules from the cold box are let into the hot box, and slow molecules from the hot are allowed into the cold. Thus, there will be an increase in the average speed in the hot box and a decrease in the average speed of molecules in the cold. Since, on Maxwell's theory, heat is just average speed of the molecules, there has been a flow of heat from a cold body to a hot one.

The crucial aspect of the thought experiment is in positing or conjecturing the phenomena of the demon and the molecules moving with various velocities. Once this is given, Maxwell's conclusion is straightforward.

Mixed cases

I now come to the most interesting (which is to say, the most doubtful) part of my paper. There are a small number of thought experiments which are quite remarkable. They are simultaneously destructive and constructive. At one and the same time they can destroy the old theory and create a new one. Perhaps the most beautiful of these is Galileo's thought experiment on free fall which appears in the first day of his *Discorsi*. The theory it destroyed was the Aristotelian one which said that heavier bodies fall faster than lighter ones (i.e., $L < H$).

Galileo asks us to imagine a heavy ball attached by a string to a light ball (Galileo, 1974, p. 66 ff). What would happen if they were released together? Reasoning in the Aristotelian fashion leads to an absurdity. The ligher ball would slow up the heavy one, so the speed of the combined balls would be slower than the heavy ball alone (i.e., $H + L < H$). However, since the combined balls are heavier than the heavy ball alone, the combined object should fall faster than the heavy one (i.e., $H < H + L$).

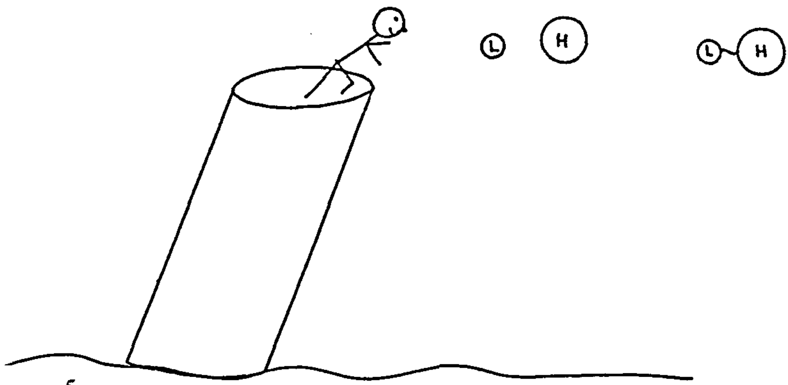

Diagram 5

We have a straightforward contradiction; the old theory is destroyed. Moreover, the new theory is established; the question of which falls faster is obviously resolved by having all objects fall at the same speed.

Here we have a transition from one theory to another which is quite remarkable. There has been *no* new empirical evidence. The old theory was rationally believed before the thought experiment, but was shown to be absurd by it. The thought experiment established rational belief in a new theory.

Koyré, as I mentioned above, thought good physics is done *a priori.* Of course, he overstated things, but a weaker claim is plausible.

> *Thesis III*: A thought experiment which is both destructive and conclusive provides the ground for an *a priori* transition from one theory to its successor.

Leibniz (1969) on *vis viva* and the well-known E.P.R. (Einstein, Podlosky and Rosen, 1936) thought experiment perhaps also exemplify Thesis III, though neither is so dramatic as Galileo's wonderful example.

Since *a priori* science is so out of fashion, let me stress how weak my thesis is. First, by '*a priori*' I mean 'independent of sensory experience', but I do *not* mean 'infallible'. Second, my *a priori* transitions do not in any way suggest we have arrived at a final theory. It is only an *a priori* step down the road to greater verisimilitude, problematic though this notion is. (Indeed, the examples I have chosen to illustrate are all probably false. The Galileo result has recently come under attack by Fishbach *et al.* (1986); Leibniz's result is a part of classical physics which has been completely overturned by relativity and quantum mechanics; and the E.P.R. argument for local hidden variables has floundered on the more recent Bell results.) Third and finally, thought experiments do not *alone* bring about scientific change. There is usually lots of regular empirical input as well. The *a priori* element is merely a contributing factor.

On the other hand, I don't want to overdo the disclaimers. What's going on in some thought experiments really is a case of genuine *a priori* theorizing and it has often been highly successful at that. Let me quickly go over some reasons for thinking so.

Why *a priori?*

First, there has been no new observational data. So the old theory is not tossed out and the new instituted on the basis of empirical observation. Aristotle's theory of free-fall, in particular, did not succumb to new observational findings made by Galileo (at least, not in this instance).

Second, it is not a case of seeing old empirical data in a new way. This is essentially Kuhn's thesis. In his (1977) he does not use the terminology of 'paradigms' and 'gestalt shifts' found in *The Structure of Scientific Revolutions*, but the ideas are the same. The thought experiment shows us a problem in the old framework, says Kuhn, and helps us to see the *old* data in a new way. (Most Kuhnian theses are disturbing to empiricists, but this one might prove relatively attractive since it solves the problem of how we can learn something new about nature without making any new observations.)

Though extremely perceptive in many ways, Kuhn's views on thought experiments are ultimately not persuasive. There are a number of reasons for this. To start with, there are several thought experiments which have nothing to do with the overthrow of an old theory (e.g., Stevin's). In paradigm change, on Kuhn's view, there is no new paradigm that is *uniquely* and *determinately* the one that must be adopted. Yet Galileo's theory seems the unique belief one ought to adopt after Aristotle's theory in the light of the damage done to it by the thought experiment. Moreover, even though Kuhn is generally right about the difficulties of comparing different paradigms, incommensurability problems do not seem to be present in the Galileo case. There has been no change of meaning in the terms 'light', 'heavy', and 'faster'. Galileo and his Aristotelian opponents are not talking past one another *during* the performing of the thought experiment. Indeed, it can only be performed because they are not talking past one another. Consequently, it seems incorrect to view thought experiments in the way Kuhn does, as bringing about paradigm change through gestalt shifts.

My third reason for thinking something *a priori* is happening in the Galileo case has to do with logical considerations. Galileo has not merely deduced his theory of free-fall from already given empirical premisses. Nor is his achievement to be trivialized by saying it follows from the contradiction in Aristotle's account. If that were all that is going on then Galileo could also have deduced 'The moon is made of green cheese', all of the quantum theory, and anything else he liked. Moreover, Galileo's theory is not a formal truth that one could have inferred from no premisses at all because it says nothing about the world. That is, it is not some sort of analytic truth. Rather, it is synthetic *a priori.*

Perhaps the best way to see these logical claims is just to note that all bodies need not fall with the same speed. They might fall at different rates due to their colour, or as has been recently argued (Fischbach *et al.*, 1986), due to their chemical composition.

Fourth, the transition from Aristotle to Galileo's theory is not just a case of making the simplest overall adjustment in the old theory. It may well be the case that the transition was the simplest, but that was not the reason for making it. Suppose the degree of rational belief in Aristotle's theory of fall is r, where $0 < r < 1$. After the thought experiment has been performed and the new theory adopted, the degree of rational belief in Galileo's theory is r′, where $0 < r < r' < 1$. That is, I make the historical claim that the degree of rational belief in Galileo's theory is *higher* that it was in Aristotle's. Appeals to the notion of smallest belief revision won't even begin to explain this fact.

My fifth (and by far my most speculative) reason for claiming an *a priori* element in some thought experiments has to do with recent developments in thinking about the laws of nature. A new account of laws has been proposed recently by Armstrong (1983), Dretske (1977), and Tooley (1977). It was designed to replace the regularity account, a view favoured by traditional empiricists, but one with many notable deficiencies.

On the new view, 'It is a law that all ravens are black' is analysed as: there is a contingent relation between the universals ravenhood and blackness such that the former necessitates the latter. This relation of necessitation in turn entails the regularity: $N(R,B) \rightarrow (x)(Rx \supset Bx)$. But the entailment does not go the other way: $(x)(Rx \supset Bx) \not\rightarrow N(R,B)$.

This is not the place to tout the virtues of the new view. It does, however, require a significant ontological commitment, namely, realism about universals. Undoubtedly, the empiricist-minded will balk at it, but I won't. This is the feature of the view which may fit in well with my account of thought experiments.

In the hands of Armstrong, Dretske, and Tooley, the new view of laws is entirely metaphysics. The universals *explain* the observed regularities. If we want to *know* what the laws of nature are we must look to the empirical regularities themselves for evidence. We see black ravens, never blackness or ravenhood. However, on the principle that we might as well hang for a sheep as for a lamb, why not make the universals do some epistemological work for us as well. If we do so, we can achieve a rather harmonious relation between this account of laws, on the one hand, and thought experiments, on the other. We simply posit that in *some* thought experiments we can 'grasp' the relevant law of nature.

The difference between my view and that of Armstrong *et al.* is similar to the difference between Gödel and Quine on the nature of abstract objects. Both are realists. But Quine holds that our knowledge of sets comes from ordinary sensory experience; that is, physics plus mathematics implies various empirical observations. We use these observations to test our whole network of belief; modification, in the light of this experience, might come anywhere in the network. Gödel, on the other hand, holds that we can directly perceive the sets themselves. It is not ordinary sense perception that gives us mathematical knowledge, but rather some kind of intellectual grasp, a seeing with the mind's eye.

Gödel, it seems to me, has the advantage here. First, mathematics has a history of its own. True, it is often inspired by developments in physics, but there have been no revolutions in mathematics due to new empirical observations. Second, there is a psychological fact to be accounted for. Some mathematical truths (i.e., $2 + 2 = 4$) feel certain, inevitable; yet typical propositions of science (i.e., 'The electron and proton have equal and opposite charges'; 'Grass is green') do not feel this way. Both of these facts strongly suggest that typical mathematics and typical natural science have quite different epistemologies.

What I wish to maintain is a Gödel version of the epistemology of the laws of nature in preference to the Quine version held by Armstrong *et al.* Gödel, of course, does not hold that we always see sets clearly. Far from it; our perception of these abstract objects is often fuzzy – witness the Russell paradox and others. But it is sometimes clear. Analogously, I certainly don't hold that we can grasp all the laws of nature. We can see at all, and fallibly at that, only in those very few special cases such as in Galileo's thought experiment.

Above, when stating my first thesis, I promised an explanation of it. Recall that this thesis was the claim that the jump from (thought-experimental) data to theory was a very small jump. There seemed to be no problem of an inductive leap; if we got the phenomenon right then the theory followed more or less automatically.

If the above account of laws is correct, the reason for the ease of inference becomes obvious. In the case of some thought experiments we are simply 'seeing' the universals themselves and not having to make an inference based on the sense perception of a finite number of instances of the universal and then having to generalize from that.

Let me say at once that none of the arguments I have given conclusively demonstrate that thought experiments yield *a priori* knowledge. All of these arguments might well be resisted and an alternative, empiricist-type account might be given. To anticipate, it might even go like this: Galileo discovered a logical contradiction in Aristotle. While he could not conclude from this alone that all bodies fall at the same rate, he could derive the conditional statement: 'If the rate of fall is due to weight alone then all bodies fall at the same rate'. We might then add the presumably already established empirical premiss that the rate of fall *is* due to weight alone. From these two premisses we can now deduce Galileo's conclusion that all bodies fall at the same rate.

No doubt, such a simple reconstruction of Galileo's thought experiment will have its attractions. It confines itself to either logical moves or to ordinary sensory evidence. There is nothing in it which could be taken as interestingly *a priori*, that is, as yielding synthetic *a priori* knowledge of nature. In short, it is an account of Galileo's thought experiment which empiricists, nominalists, and naturalists are sure to favour over mine. But which account should be preferred?

Among contemporary philosophers there is a strong prejudice against abstract objects and the *a priori.* The prevailing spirit is that such an abstract ontology or such a mode of cognition is to be accepted only if all other accounts fail (no matter how intrinsically implausible those other accounts may be).

There are, however, rival views to the prevailing empiricism. These are accounts of linguistics, ethics, mathematics, and modal logic which feel the need to posit universals and other abstract entities and do not shy away from 'intuitions' and other forms of intellectual grasping. My account of thought experiments is of a piece with them. The best explanation, of course, may not be the only one available. None of the reasons I gave for favouring my account of thought experiments is decisive, but they do, I think, tip the balance slightly in its favour.

Undeniably, the belief change induced by some thought experiments has been a rational change; perhaps it has also been rational in the strongest seventeenth-century sense.[2]

NOTES

1 However, I had barely written these words when I received a notice that a conference on the subject would occur in Pittsburgh, April 1986.

2 Versions of this paper were presented at Glendon College, Carlton University, and Dubrovnik. I am grateful for the many helpful comments, especially from B. Katz, E. Manukian, K. Okruhlik, A. Portoraro, S. Talmage and A. Urquhart.

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