10 Popper's Attempt to Save Scientific Method

In this Chapter we look at Sir Karl Popper racing to the rescue, trying to save scientific method as a believable story, trying to make a story that really can explain how scientists work and achieve scientific knowledge. Let me remind you of what we are doing in this Section. We are looking at some ideas of scientific method to see whether any of these ideas about scientific method are convincing, and can give us some explanation or understanding of the historical cases we will be looking at.

Consider again the traditional story of scientific method (fig. 1): We first generalise or induce the facts to form a draft law called an hypothesis, and then you test your hypothesis. If your test of your prediction drawn from your hypothesis matches the 'facts', then you're even more sure that you have a true, or highly probable, hypothesis, a law. We have also discussed a little bit about the history of these ideas, and I suggested that what we have learned about the ‘theory loading of facts’ really does a great deal of damage to this view: if you go out to generalise, or to look, you're only going to be able to see or to generalise within your own framework, within your own gridwork. The types of facts that you are going to be able to generalise about are, as it were, pre-given. Another point is, that theories are not just summaries of hard fact; theories are culturally shaped, shaped by background assumptions, or what we shall in the next Chapter call "metaphysics", so there's always more in a theory than just a collection of nuggety, hard objective, facts (assuming there such things are available to us poor humans).

So there are difficulties. And these difficulties have been recognised to a certain extent, in this century. Some people, like some historians of science who influence the kind of work that we do here, drew the conclusion that scientific method doesn't work, that it is a nice story that is told, perhaps for rhetorical or political purposes, but that the real nature of science, (how scientific knowledge is generated and negotiated), must occur in some other manner. But you should understand that in the philosophy profession, in the business of doing philosophy, it isn't necessarily going to be a happy conclusion that scientific method is out of business. Philosophers of science want to stay in business. And many philosophers of science, to a large extent starting with Karl Popper about 60 years ago, have taken the view that, the old idea of method is not workable, but that maybe we can do better. Maybe we haven't had a clear view of what scientific method is. If we could get our story more accurate and more up to date, then we could still believe that there is a scientific method, that it really works, and that it really explains what happens in science and what happens in the history of science. So it's a search for a replacement, for a modification of the old theory.

The most important and symptomatic and influential alternative new picture of scientific method in our century has been presented by Karl Popper, who originally wrote up his views in 1934 when he was in his early 30's, in German, in a book whose proper title should be "The Logic of Scientific Research". That's what the German title means. But in a terrible mistake, when it was translated into English in 1958, it was published as *The Logic of Scientific Discovery* which was a complete misconstrual of the title. And if you read this Chapter carefully you might be able to detect why it is such a mistake to believe that Popper is telling us how to discover things. He doesn't even believe that there is a method of discovery.

Popper studied philosophy, physics, and logic in Vienna in the 1920's. Even though the Austrian empire had been dismantled in 1918 after World War I, Vienna was still one of the intellectual centres of Europe, and a very important place for doing physics, philosophy, logic and psychiatry, (Freud, for example and his followers worked there). Sitting in this kind of hothouse intellectual atmosphere, was as a young Popper, very aware of a number of difficulties with the traditional view of scientific method. So, he set out as an ambitious, and very intelligent fellow, to solve these problems and to give an alternative vision of scientific method.

Let's consider some of the problems about method that he was facing. The first is the one that we addressed in Chapter 4, and again in the previous Chapter. That is, Popper was aware of the existence of the 'theory loading of perception' and the 'theory loading of facts'. In fact, he was one of the first people to discuss this in philosophy. He says some amazingly radical things about theory loading of perception in the '*Logic of Scientific Discovery*'. He knew that theory loading of facts really does cast some doubt on the idea that you just go out and look and generalise and invent a new law. Popper realised that you are going to go out and see what you are already 'gridded up' to see.

Popper was also aware that the history of science revealed some curious things about 'discovery'. What, after all, is discovery? In the old story of method, discovery is what happens on the left hand side of the diagram (fig. 1) This is where you discover a new law, or discover a new hypothesis. You go out and you look carefully, and you generalise, and you arrive at, or 'discover' a law or a hypothesis that nobody else has had before. But Popper knew that there were many episodes in the history of science that don't really look as though the scientist went out and just generalised. There are many famous examples in the history of science (some perhaps a bit fictional) where a great scientist didn't generalise, didn't go out and look at a lot of facts and generalise and discover a new law; instead, the new law or the new theory just kind of came to him in a creative flash or an imaginative leap, or even when he was asleep or dreaming.

For example, the apple that hits Newton on the head and led him to exclaim, "Oh! I see it now; it's the theory of universal gravitation!" No, it didn't happen that way, but the fact that stories like that pass around show that discovery was not always understood to be a plodding, generalisation of the facts. Another example is Archimedes, the ancient mathematician, killed by the Romans in 212 BC. Archimedes discovered the principle of buoyancy, that you are supported in water by a weight of water equal to the weight of the volume of water that you displace. And he discovered this, as you remember, as he was getting into his bath, and the bath overflowed and he said, now I've made a discovery! (Actually rumour has it that he leapt out of the bath and ran naked down the street shouting "Eureka,... I have found it!, I have found it!!"). It didn't happen like that, of course, but again in the story he wasn't generalising facts, he was having some sort of imaginative insight. Another example is the six sided figure, the Benzine ring, so-called, in chemistry, discovered in the 19th century. The corners are carbon atoms and it's very hard to imagine how six of them are bonded together in a molecule, unless you hit on this ring, (which says funny things about the bonding, we won't go into that). But this structure was seen or discovered as an idea, as an hypothesis, by a chemist named Kekule, in a dream.

So discovery doesn't always look like generalisation of facts. Generalisation of facts is in trouble anyway because of theory loading, you are just generalising within your framework anyway.

The final thing that bothered Popper, and it bothered a lot of people in physics and philosophy of science at the time, was the idea that apparently, every once in a while, there are massive theoretical revolutions in science, especially in physics. It seemed that sometimes, the brick wall of proven fact and theory (if we can go back to the brick wall metaphor which we have of the old method), has large portions just ripped down, or a huge hole is poked in it, and some different bricks, some different style of bricks, different bunches of different facts are put in place. Scientific progress is not always putting one brick on top of another. Sometimes there's a big change of structure.

The example that bothered Popper, was the problem of Einstein's physics. Einstein's special and general theories of relativity, (especially the general theory of relativity) which gained great prominence after 1919 when a certain test was performed. Now no test ever 'proves' anything in an absolutely definitive way: but, the physics community decided that Einstein had the better result, as opposed to Newton. So, Einstein's theories were gaining credibility. But, if you look closely Einstein's theories were not just more bricks in the old Newtonian wall. Einstein's theory meant ripping out part of the wall and putting in some new bricks. For example, Einstein uses words like space, time, mass, and Newton uses words like space, time and mass, but these words mean quite different things in the two different theories. They are almost two different grids: Newtonian time isn't Einsteinian time. Newtonian space isn't Einsteinian space. This is not just a question of piling brick on brick. There had been a revolution. The style and make-up of the wall had changed. Quite a few philosophers of science had noticed this and were worried about it.

Now the question was, why does this happen, how can this happen? You can't do it with the old story of method, because in the old story it's always proven facts, gotten into bricks, set down, and serving as the basis for laying more facts, more bricks, as you build up the brick wall, slowly and surely. In this story you just do not get wholesale 'revolutions' of the brick wall.

Now Popper, was a very clever fellow. Not only technically, but in terms of seeing how one could become famous in the philosophy of science. Obviously one could become famous in the philosophy of science by saving scientific method and answering all of these questions. This is how disciplines work; problems develop and people make bids to get credit and fame by claiming to solve them.

So let's look at his answer as published in *Logic of Scientific Discovery*. A lot of what I am going to say now is going to sound peculiar, bizarre, paradoxical, but it is a set of answers to these problems and very interesting and brilliant ones at that. I actually think he is completely wrong-headed and still misses what goes on in science, (and has produced another fairy story), but it is certainly, even from my perspective, a very clever fairy story and worth looking at closely.

The first thing Popper says is, "I'm not interested in a method of discovery". (This is what makes the translation of this book title so odd.) He says in effect:

> as a methodologist I cannot tell you anything about the process of scientific discovery. I cannot tell you, [in other words], how to use a method to invent or discover laws and theories. There is no method for inventing or discovering laws and theories.

So he takes those problems and those criticisms on board, wholesale. If you like, he throws the whole left hand side of the traditional method story, out the window. (Too bad he didn't throw out the other half, as well. He probably left it to us.) "Discovery is an issue to be elucidated by the researches of historians and anthropologists and psychologists, it is not a matter that can be reduced to a method". Right? How did Newton discover gravitation? It's a psychological and historical issue. How did

Archimedes discover buoyancy? It is a psychological and historical issue. Not a matter of applying a method.

So that's the first thing, he doesn't care how you've got a theory or law. He doesn't care how you've discovered it. He only cares about one thing. Can your theory or law be put to the test. Is it testable.

Testable means, does your theory or law make predictions that can be tested. And now comes the really bizarre part. Popper's fundamental point at this juncture is that when you go out to test the predictions made by your theory / law, you are not interested in positive evidence, not interested in evidence that shows your prediction to be true. In fact, ethically speaking, you shouldn't really waste very much time looking for positive evidence that supports your predictions. Rather, your whole heart and soul professionally and ethically, *should be committed to the search for negative evidence*. You should search for evidence that shows that your predictions are false. Hence the name of his methodology. Falsificationism.

In fact, this is his definition of a scientific theory. A theory is scientific if it makes predictions that are capable of being found to be false. A theory is scientific if it is falsifiable. Not if it **is false**; I mean **it might prove to be false** later. You have to conjecture or make up a 'scientific' statement, a law, a theory, you have to say something that when put to the test, might be found to be false. So if I say for example, as my theory of the weather tomorrow that "it will rain, or possibly not rain", then I have not made a 'scientific' statement according to Popper, because I'm going to be proved right no matter what happens. (it's either going to rain or not rain.) My claim that it's going to rain or not rain is fairly broad, and fairly empty. But if I say something like, "tomorrow it's going to rain between a half a centimetre and a centimetre", then I've said something which might turn out to be true, it has a possibility of being false. I've committed myself to a falsifiable statement. We don't know whether it's false yet. But it's falsifiable.

So, according to Popper, you have got to be up-front, you've got to make up laws or theories that are capable of being false, and you've got to go out and try to see if they are. This, according to Popper, is how we learn things. Every time a law or a theory is shown to be false we learn something solid. We learn that the theory or law that we just had is false. I said it's paradoxical. There's a little bit more to it, than this, but we'll come back to this in a moment. Let's first look at the question of positive evidence. Why is he not interested in positive evidence? He has three main reasons.

First of all. It's too easy to gather together lots of positive evidence. Anybody with a theory and therefore with a perceptual grid, is going to find it very easy to see and report things that support his or her own theory. For example, if I were an Aristotelian, and I wanted to support my theory, I could go out and I could spend my next forty two years dropping heavy things, and saying, "Oh, heavy bodies fall down." "Oh, there they go again." "Oh, more confirmation." "Ah, the positive evidence is really building up here." And you see, you're probably laughing! You're not convinced. You don't think that Aristotle's theory is any more true after I've dropped a body a trillion times than after I've dropped it once. There's something dodgy about this. Just piling up the positive evidence. And part of its dodginess is ethical. Popper sees science as an enterprise that has to be pursued in the right moral framework, and it's just possibly corrupt, and certainly trivial, to look for positive evidence.

The second point is a point in logic that was not at all new to Popper, but which he stressed. The point is that no amount of positive evidence proves a law to be certain, to be absolutely true. You can pile up that positive evidence, but it doesn't ever make your claim absolutely certain. But, if you find *just one* piece of negative evidence, so that you can conclude your law is false, you do know for certain that its false! That is, one piece of evidence can render a claim false, for certain. But no amount of positive evidence can render a claim true, absolutely true, absolutely certain. So there's an asymmetry there.

Popper's third point is really again a philosophical one and an ethical one: the search for positive evidence leads us away from the areas where we can actually learn something. Do I really learn anything repeating this test on 'falling bodies'? No, I don't really learn anything by sticking with this, but when I look for negative evidence and I make the conclusion, in the light of negative evidence, that my law is false, I really know something. I know it's false.

Now, the question is, what now does the history of science look like, assuming that scientists behave this way? Are we supposed to believe that scientists go around trying to get rid of their theories, and if they do, how is there any progress in science? Well Popper has an answer for this. He has an answer about how the history of science unfolds. And this is where it gets very interesting for us, because this is a possible story about the history of science. This is Popper's story: (fig 2)

Once upon a time there was a scientist named Fred Bloggs. He made up a theory, call it Theory 1 And remember, using scientific method, we can't tell how to discover or make up a theory. He just made it up. And it was a scientific theory because it was testable and falsifiable. So Fred Bloggs manipulated this theory so that it made a prediction. Prediction Number One. Then Fred Bloggs went out and tested it. He did an experiment, he made an observation. He tested his prediction, to find out whether the prediction was true or false. And of course he was hoping that it would be false. Because then he'd really know something. Unfortunately he found that Prediction One was confirmed. What he observed in his experiment confirmed his prediction. So he manipulated his theory a little bit more and he came up with another, different prediction. He went out and did an experiment and unfortunately found that that prediction was okay. The facts of the experiment supported it. Then he manipulated his theory a little bit more to make a third new prediction. Not an old prediction again, but a new prediction. And this time, he found, let's call it a question mark. He did an experiment, and he observed the results of the experiment, and the experiment did not agree with his prediction so his theory was falsified. His theory had produced a prediction that was incorrect. Now, what must he do? He must not make excuses, he must not pretend it didn't happen. He must not say, "let's do it over again", he must not say "let's try something let's forget about it"; he must say, "my theory is falsified, I throw away my theory." Goodbye. That's it. No procrastinating, no delay, no double dealing, no excuses, no rhetoric. Throw it away. Now we really know something. Theory One was false.

But where is the progress of science? Where do we go from here?. Well according to Popper, if we're lucky (and in the history of physics and astronomy we have been lucky) around about the time that Theory One is proved to be false, some other smart guy will perhaps dream up a theory, Theory Two, which is 'better' than Theory One. 'Better.' What does better mean? Better means, Theory Two makes exactly the same predictions as theory one did, where Theory One was successful. So Theory Two makes prediction one, and we know that prediction one was okay. It makes prediction two, and prediction two was okay. And now we come to the crunch. Theory One failed at the third hurdle. Its third prediction was false. Well, Theory Two must explain that. In other words, Theory Two must predict correctly, the outcome of this third experiment. The experiment that tripped up Theory One. So it must make some, let's call it, prediction ‘three prime’, which is going to be the outcome of this experiment. Now Theory Two is better than Theory One. It explains what Theory One explained previously *and* it explains the point about which Theory One failed. And presumably Theory Two will go on. It will make prediction four, which will be confirmed, prediction five, which will be confirmed, and then maybe somewhere, it will make prediction X, and prediction X will not be supported by observational and experimental evidence. Prediction X will be wrong. And what do we do now? Exactly the same thing. Goodbye, Theory Two and we hope that someone will now think up a better theory, Theory Three. Which will do what? It will explain everything that Theory Two successfully explained, and explain the point at which Theory Two failed. And on and on and on.

So, perhaps Theory 1 is Aristotle, perhaps Theory 2 is Newton, perhaps Theory 3 is Einstein. And what do we have? We have ‘progress’, because this series of confirmed predictions gets longer and longer, and we have revolutions, when we went from Theory One to Theory Two. We completely rejected Theory One and in a certain sense, the view of things that went along with Theory One, when we threw it out and adopted a different theory. This uses a different perspective and can be seen as a theoretical revolution. So, according to Popper, we have now dealt with theory loading, we have understood discovery as a creative act, we have accounted for the existence of revolutions in science, and we have allowed for progress in science. Pretty good. He was indeed a smart fellow in the philosophy of science business...But completely wrong-headed and unconvincing, as I’ll now argue for three reasons.

First reason. When you look at the great theories, like Copernicus or Newton or Einstein or Darwin, in the history of science, you find that at the moment of their very birth, each one was faced with apparently crushing, falsifying evidence. (Remember how the evidence from current theory was stacked against Copernicus?) If scientists really followed Popper's method, each of those theories would have been thrown out the moment it was published or put forward, because there was apparently very strong falsifying evidence for each.

Let me use an example from Copernicanism, but we can do this with Newton, we can do it with Einstein, we can do it with Watson and Crick and DNA, we can do it with any of them. With Copernicus, the theory was published in 1543 and from the stand-point of the belief system of the day, the theory is faced with immediate falsifying evidence. We are familiar with some of this. On a rough guess, if the earth is spinning, during the time which this chalk takes to fall to the ground, which is what, maybe a second, a little bit less, the earth should spin a couple of hundred metres, three or four hundred feet and surely, if the earth was spinning, and we dropped a piece of chalk, the earth would spin while the chalk was falling and the chalk would fall far to the west of where I dropped it, because the earth is spinning west to east. Crucial experiment. Copernicus predicts that things will fall to the west. Does he really? No. But the theory seems to predict that things will fall to the west; they don't. (In fact, they do a little bit ... but not enough to see.) It's falsified.

So what do we do? If Copernicus had been called Popper he would have slit his throat at that point. Or, slit his theory's throat. What did Copernicus do in this situation? Did Copernicus behave in the way that Popper said scientists are supposed to behave? (And by the way, there were other similar problems like parallax, and other things mentioned in Chapter 7.) Well, Copernicus's actual response in this situation was, well, whatever the Polish for "tough" is. Tough luck, I do not care . That's the response: I will ignore this problem, something will turn up. And something did turn up. Something turned up 89 years later. Now the question is, whether Popper's method means that we are allowed to wait for 89 years to see whether a falsification is not a falsification. All of a sudden this method is not looking like a method; it's looking like a fairy tale.

Anyway, something did turn up for Copernicus' theory, and this will lead me to my second point. Galileo turned up in 1632. And Galileo said, I just invented a new theory. It's called the theory of inertia. Now if you believe my theory, says Galileo, then you will not be worried by this experiment. You will understand that this experiment of dropping the chalk does not prove Copernicus wrong. It doesn't necessarily prove he's right, but it certainly doesn't prove that he is wrong.

Galileo's notion of inertia was not strictly Newton's principle of inertia of 50 years later. Galileo's kind of rough and ready theory of inertia works something like this. Imagine you're inside a system of bodies, you're part of the system. Imagine, perhaps, you're on a train. There's no friction of the wheels. There's no noise. There is no wind outside. The windows are blacked out. You probably wouldn't be able to tell, from inside the train, whether the train is moving or not. [Newton would say, you'd certainly know it was moving if it suddenly stopped or accelerated!] But leaving that aside, you wouldn't be able to tell whether or not the train was moving, from the inside, but you'd certainly be able to see things moving, within the train. People walking up and down, people doing things, people dropping things straight down...whatever.

In other words, when inside such a system you cannot tell whether the system as a whole is moving or not, based on movements of bodies within the system relative to each other.

Galileo says the uniformly spinning earth is an inertial system. [It's not, according to Newton.--Galileo like Copernicus said a lot that later became wrong.] Anyway, Galileo says it's an inertial system :We're all moving along all in the same system-- all in the same boat, as it were And so, we're moving along with the earth; the atmosphere is moving along with the earth; the book we drop is moving along with the earth; and when we drop the book; the earth doesn't spin underneath the book; the book keeps spinning with the earth. Hence we don't observe any sideways deflection of the falling book.

This is a revised interpretation of the falling body test. Galileo doesn't change the test result, he uses a extra, new theory to reinterpret the meaning of the result! It does not prove that the earth spins. It says, it's possible that the test result is consistent with the earth spinning. It's possible that the earth spins and this same test result occurs before our eyes--it drops straight down. (fig 3)

This leads me to my second point against Popper's method. No test is conclusive, because tests are conducted and interpreted in the light of theories and the theory of a test can be contested. Galileo has just challenged the interpretation of this test.

Galileo has said,

" You're assuming Aristotle's world-view. You're assuming the earth doesn't spin; but that if it did, the falling book wouldn't spin with it. Unattached to the spining earth, book would get left behind I, Galileo, have suggested an alternative interpretation. Think of this as a test carried out in an inertial system...Which might be moving; or it might not be moving You'll get the same test result--the book falls straight down. "

Galileo has blunted the force of the presumed the falsification. Copernicus' theory isn't necessarily falsified by the experiment, because Galileo offers another theory for interpreting the experiment.

But, let's be clear, Popper's method doesn't allow this sort of tactic. You can't perform an experiment, see your theory falsified, and then dream up a new theory that will take away the sting of the test. Popper considers this highly dishonest and unmethodological, but it seems to be part of the social, political the intellectual dynamics of science.

By the way, the fact that the theory behind a test can be questioned is the reason I said earlier that all great theories, at the moments of their births were apparently faced with falsifying evidence. No evidence ever simply and unequivocally constitutes a falsification of a theory, because the evidence itself is a function of other theories, which may be challenged. The negative evidence confronting Copernicus was of course grounded in theories, mainly those of Aristotle. Galileo challenged those background theories, but not with new facts, rather with altenative theories for interpreting the test.

Popper is caught in a real bind here. If he denies that the theories behind tests can be challenged, he must admit that most great theories should have died at birth. But if he admits the negotiability of the outcomes of tests, then he saves the great theores at the cost of giving up sharp, unequivocal falsificiations--that is, at the cost of giving up his method.

People are not going to follow Popper's method. They are going to defend their theories and one of the best ways to defend them is to challenge the basis of an experiment that supposedly threatens your own theory. We see this all the time in environmental disputes, in technical disputes.

Now the final example. This is the third point against Popper, and this one is called, ‘test data do not speak for themselves’. Popper's approach here seems to assume that you do a test and you get a definitive, objective result delivered to you directly by the results or data. The test speaks to us. You do a test and the test says, “Hello I'm a test. I consist of resultant data and I am telling you that your prediction is wrong, you prediction is false.” Or alternatively, “Hello, we are test data, your prediction is okay.” But, think about it, data do not speak for themselves. People speak for data, and the reason people speak for data is when you have a theory, and you make a prediction, and you do a test, and you get data, there is always a gap, a quantitative gap of some form, between the predicted numbers and the data numbers. There always is. That's why the data can't speak to you. You have to speak for the gap. You have to come in, as a scientist, and say, I believe the gap is ‘too big’ here, that we can conclude that the prediction is false. Or, I believe that the data are ‘close enough’ to the prediction. (fig. 8) There is no rule or method or model for deciding when a gap is ‘so big’ that the prediction is ‘wrong’, or when a gap is ‘so small’ that the prediction is ‘true’.

Let me explain this third case in more detail: When you make a test you are actually testing a prediction which arises from the theory or hypothesis. (fig. 4) A prediction is a statement about what will happen which is derived from your hypothesis. Usually this means some kind of mathematical or logical manipulation of the hypothesis to get out some observable prediction. You often have to do other things to get a prediction out of an hypothesis, such as making unexamined or half-examined guesses and assumptions. (fig. 4a) Usually you cannot directly derive your prediction from your hypothesis because you have to feed other things into the derivation, such as guesses, assumptions, information, other 'accepted facts'.

What your experimental set-up gives you is usually called 'data' or 'test results'. What we are looking at in a test is the relationship between data and the prediction. If the data do not 'match the prediction', we have strong grounds for getting rid of our hypothesis or questioning our theory. If the data 'agree with the prediction' then we feel happy and more confident about our hypothesis. We have seen this in high school science tests. However, this explanation is too simple and as soon as it gets a little more complicated the situation becomes much more interesting and much more political and muchmore damaging for Popper's theory of method.

In order to explore this in the concrete let us consider one of Galileo's famous tests (fig. 5). In 1638 Galileo published a book about physics and although a lot of what he said would not be accepted in your modern physics textbook, it is very much the first book of what we call today Classical or Newtonian physics. One of the most important parts of Galileo's physics was the mathematical Law of Falling Bodies. This is the mathematical description of how bodies fall under ideal conditions when air resistance and friction are neglected. This sounds elementary but it was the first hypothesis that was established as a law in Classical Physics.

Galileo was testing the claim that if a body is dropped it will travel in the following way: the distance travelled from the start will be proportional to the square of the elapsed time of falling: If the time of fall is doubled it will travel four times as far; if the time of fall is trebled then it will travel nine times as far. The distance [d] fallen from rest (time zero) is proportional to the square of the time [t] of fall (or, if the proportionality constant k=1, $d=t^2$). Galileo derived this from his more basic hypothesis which was velocity is proportional to the time of fall: when a body falls under these ideal conditions its speed increases with its time of fall. This basic hypothesis was less immediately testable than his derived hypothesis or prediction of $d=t^2$ (Galileo did not derive his testable hypothesis quite the way we would today for we would use calculus of which he had no knowledge; he used Medieval and Greek mathematics.)

Galileo states he did experiments but many people have doubted this claim for there was little detail of them in his book. But we know from some of his manuscripts that he actually did perform experiments and how he conducted them. To test $d=t^2$, Galileo built an inclined plane, intending to roll the best steel balls that he could find in the 1600s down it. Presumably these balls were not manufactured out of the highest grade steel in outer space, therefore, they were not perfectly spherical, but they were produced by the best Venetian or Florentine technology.

Galileo intended to roll the steel balls down the inclined plane and to measure off distances. He compared the distances rolled because he regarded this 'rolling' as essentially 'falling' and he also determined the time, by using a water clock. After one opens up the orifice at time zero, water falls through, during the time the ball 'falls' the set distance for the trial, and as the distance is completed, you 'instantaneously' close the orifice. A certain amount of water is available to be weighed and the amount of water is proportional to the length of time the orifice has been opened, that is, proportional to the time of fall. (fig. 5)

To a first approximation this is true, but not according to modern physics or even to the later physics of 1700: for the amount of water caught is not proportional to the amount of time the orifice has been open, because as the level of water in the top goes down, the rate of flow through the hole changes. In fact, it changes continuously as a function of the decreasing height of the head of the water. You need calculus to establish a better calibration of this clock, but Galileo did not know this and to a first approximation probably would not have cared. For Galileo, the clock is calibrated as follows: the amount of water caught is proportional to the time elapsed. He is utilising a simplified assumption (theory) about how the clock works (fig. 6).

What this shows is that when you want to perform an experiment there is often a lot of theory and assumption in the background of the performance. How can Galileo perform this experiment unless he assumes that his water clock is accurate, and so has to make specific assumptions about the theory of this clock. His experiment is dependent upon his clock and ultimately on his theory of the clock. If we change the theory of the clock and change the calibration of this experiment, the results of the experiment are changed to a certain degree. Therefore, we can say that the water clock is a theory-loaded or assumption-loaded piece of hardware which is necessary to his experiment. The idea of theory-loaded experimental hardware is crucial to understanding the nature and history of experimental sciences.

There are other things involved in this experiment that are essentially auxiliary pieces of hardware that are theory-loaded or assumption-loaded. Galileo states his inclined plane is straight but what is ‘straight’: in geometry it is the shortest distance between two points. Galileo may have said that (a) he did not bother too much about this definition of “straight” or (b) that the path of a light ray is “straight”. Of course physics in the 1700s following Newton would later state that light can bend a little in the presence of a gravitational field, but for Galileo, a light ray travels in ‘straight lines’ in a homogeneous medium. (fig. 6)

So, Galileo made some assumptions about his hardware which were necessary for the construction and conduct of his experiment. Some of his assumptions were theory-loaded and others were plucked out of the air because you have to make some assumptions. (For example, Galileo is also broadly assuming that air resistance friction is ‘not too important’ in this experiment although it affects every trial he conducts.) Galileo’s inclined plane, sphere and clock constitute a whole cluster of theory-loaded hardwares, wrapped up with background assumptions and auxiliary theories of the conditions of his experiments. (fig. 6) Galileo cannot step into his laboratory and conduct the test unless he has committed himself to certain assumptions and theories. Galileo’s incline plane experiments are not acts of nature, for his experimental set-up is a piece of culture. Galileo's apparatus is a piece of human technology whereby you might say his assumptions and theoretical commitments and even his aims are manifested as hardware and as modes of practice.

We are now ready to conduct a test (fig. 7) Here we have some typical data which shows the type of accuracy that Galileo would have found in his own experiments. We have distances laid out in squares; we have predicted times which assumes everything has been calibrated down to unit measures; square the predicted times and you gain a match up: d is proportional to square of time. The first three columns contain the predictions, the remaining columns the data we have obtained in trials. In the ‘times’ and 'times square' columns there are data similar to that which Galileo achieved. To one unit rolled it is close to one unit of time; for four units rolled it is close to 2 units of time; for nine units rolled it is close to 3 units of time. Some are up and some are down for they tend to be 10 to 15% off the predictions.

Galileo states in his book that he performed the experiment and that the data and the predictions match up well enough and so this confirms the truth of his hypothesis. Note, however, something interesting. There is a gap between his predictions and the data, but this is always the case in any experiment. It does not matter what the theory or what super computer crunched out the data, or what hardware was used for there is always a gap between your test data and your predictions for they never match perfectly. If they did match we would be living in some sort of mathematical wonderland. **There is always a gap**. (fig. 8)

The next thing to notice is the size of the gap, which depends upon further assumptions and interpretations. If we look at Galileo's data then we see that in the first case the data is about 19% off the prediction. It is possible that it could be decided this was a 'bad trial' and we could then discount this data, which would then reduce the gap to 10 or 11% for the overall trials. It is also possible to institute a 'rule' that states the trials that we thought were better should be given some more 'weight' and the trials that we thought were 'tainted' (for whatever reason) should be given less weight. We could introduce some complex equations about how to weight the different trials. In the end this amounts to what I call adjusting or negotiating the size of the gap.

Thus, the first thing to remember is that there is always a gap and the second thing is that the size of the gap depends upon decisions and judgments about how to manipulate the data. By saying this I am not accusing scientists of dishonesty or bias because data is so manipulated; I am saying that scientists have always got to make decisions; there may genuinely be a bad trial; or a genuine misfire on the water clock which of course means that as an experimenter you would say that particular trial does not count. If someone suspected you of being a dishonest scientist they would say that you manipulated the data (cooked the data) but I put it to you that every scientist (in fact anyone who has ever walked into a lab) knows that you have to do this. There is always a gap between the prediction and the data which depends on data handling techniques and decisions. You might ask who makes these decisions: well, the person performing the experiments and the other experts reading about the experiments in journals for it is a big, social decision-making process.

The third point, which is the most important, is that however big the gap is after making your data handling decisions, **the gap still has no intrinsic meaning**. Galileo states that the gap is 'small enough' for the data to match his predictions. But, using a metaphor, does the gap "speak"? This is really the moral of this story: does the gap speak for itself? Do the data jump from the paper or graph paper and say "we are 'close enough' to the predictions for the predictions to count as confirmed". Obviously not. You can program your computer so that when the gap is below a certain small size the computer can say that "the predictions is confirmed because the data is 'close enough' to count". But, you are the person who programmed the computer to allow it to make that "decision" and your colleagues may dispute your decision to program the computer to say what constitutes a "small enough gap". The data do not speak for themselves.

So who speaks for data? Nature! Divine Illumination! Guess work! No, people speak for data and say what the data means; but only those people in the relevant scientific community, who have been 'licensed' to argue about the particular kind of data, can speak for it. Those people in the relevant scientific community will argue and wrangle and compete and struggle over what the gap means in this case. This renders the study of experimental science much closer to the study of politics than you could imagine. The results of tests do not speak for themselves, for people speak the meanings. Popper’s image of a simple, 'nature-given' verdict of a ‘test’ is an illusion, a dangerously misleading illusion about the history and social character of science.

Of course the question then is, who gets to speak and who exerts the predominant influences in speaking. It was always possible for friends or competitors of Galileo to put other ‘words’ in the mouth of the data and of the gap. It is always possible for other friends or competitors to contest the meaning and significance of the size of the gap.

Some of Galileo’s best friends and scientific allies chose to interpret this experiment (the meaning of the gap) differently. Some of his friends in Paris repeated his experiments and gained data similar in gap-size to the experiment he had done, but they refused to accept the gap as sufficiently narrow to show the data supported the prediction. It is important to understand why his friends would reject his gap as adequately small. These people in Paris were the closest friends that Galileo had. In 1633 Galileo had been condemned and put under house arrest by the Catholic Church for teaching that the Copernican sun-centred system of the universe was true. Galileo then completed his work on physics and published his book without using the Copernican hypothesis, without actually stating it. His Parisian friends were also Copernicans, who supported Galileo and were horrified that he was placed on trial in 1633; they are on the same scientific side as Galileo and yet did not agree with his experiments.

As stated, people have to give meanings to the gap and there are only certain basic things they can do when they are involved in a debate about the meaning of a gap between prediction and data in the experiment. You can claim that the available results have a small enough gap to confirm the predictions, or you can say that the gap is too large and the predictions are not confirmed by the data. People who say that the gap is ‘small enough’ usually have some other agenda; they basically agree with the research and want it to be established as a basis for going on with some other branch of research. This is what Galileo was doing with his inclined plane experiments, stating that his experiment can be used as the basis for research in other areas.

Usually when people take the other line, saying the gap is ‘too big’, they have an agenda which, for whatever reason, aims to stop the line of inquiry involved in the experiment. Sometimes people will look at the gap and think it is a little too big and that therefore, further research is necessary to make the gap smaller. This further research aimed at ‘closing the gap’ can focus on the derivation of the prediction--it can be rejigged, other assumptions and auxiliary facts used; or it can focus on the data. They may do research on how the experimental apparatus functions, in this case, try to improve the theory of the water clock to improve the data so the gap closes; or perhaps pay more attention to air resistance with a theory that would allow an adjustment of the data and close so the gap. The size and meaning of the gap is open to discussion, and further lines of research may be advocated as strategies for attempting to close the gap from the data (experiment) or prediction (derivation) direction.

In the Galileo case, he wants the gap to be small enough so that his larger project of establishing the Copernican world system will be accepted. Galileo wants his physics to be true, and the most important way to do this is to get his test results accepted as basic principles. Galileo wants his physics to work so that it can support his astronomy.

His Parisian friends are more cautious in their acknowledged acceptance of the Copernican system. They are not known as Copernicans except in a select circle; therefore, they will not willingly follow Galileo's theories and possibly arouse the wrath of the Church or the scientific community. Hence, their recommendation that Galileo does some more work on closing the gap of these experiments. And, there was perhaps another thing on their agenda: there might be glory in these experiments for themselves, if they can first claim the gap is too big, and then succeed in 'closing' it themselves through their own researches--a lot of science proceeds in this manner. It's as though this is an issue in party politics -- not one party against another--but in the party room. People are jockeying for positions in their scientific and public careers.

A test and its results are an occasion for negotiation, argument, debate and further research in the relevant research community. It is a debate about the size of the gap, and the manner of narrowing it if it is judged too large. (Or perhaps, if the gap is judged too large, it can be a debate about rejecting the entire experiment, the entire derivation or both.)

No test is definitive in and of itself. No test speaks for itself. A test is only definitive if a predominant part of the relevant community of researchers reaches an agreement about the size and meaning of the gap. A predominant part does not mean 51%, but it means the people who impose their will on a sufficiently wide domain within the scientific community through a process of argument, struggle, and negotiation.

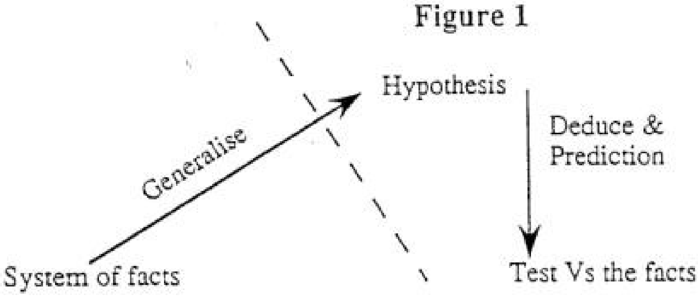
I would say that this view of testing and experiment is fundamental to understanding science as a social and political enterprise. At the heart of science there is this kind of small-scale politics, community struggle. So when experts spout their results of this or that investigation or test at us, we are now able to read through all the rhetoric and understand that what are put forward as the test 'results' are themselves not some lessons taught by nature or lessons taught by some super objective reality, but rather the outcomes of small-scale social processes of negotiation and contestation amongst those very same experts. If accepted test results are 'facts'; that is, widely accepted verbal/symbolic reports about natural states of affairs, then those 'facts' are also social constructs, the products of processes of interpretation, judgment, negotiation and conflict in the expert communities licensed to issue such reports.

So Popper misses the point here, and that is, every time there is a test, the relevant experimenters, the relevant scientists, are going to negotiate and argue with each other about the meaning of the test, especially the size and meaning of the inevitable 'gap'; and honest men and women can differ about the outcome of the test. Since they can, there is no method.

What's Popper telling us in the history and sociology of science today? He's not telling us anything that we need to know. What we really need to know is, how do professional scientists argue about and negotiate what they are going to make out of the outcome of the test. To tell us, "Do a test and if you pass, it's okay, and if you fail, throw away your theory" is to tell us nothing. Because that isn't what they are doing. They might say, "I'm a Popperian and Karl Popper says, you must do it this way", but that's just a story, a way of convincing other scientists to accept your reading of the size and meaning of a 'gap'. There are other story tellers, and they can hurl stories at each other, in this argument.

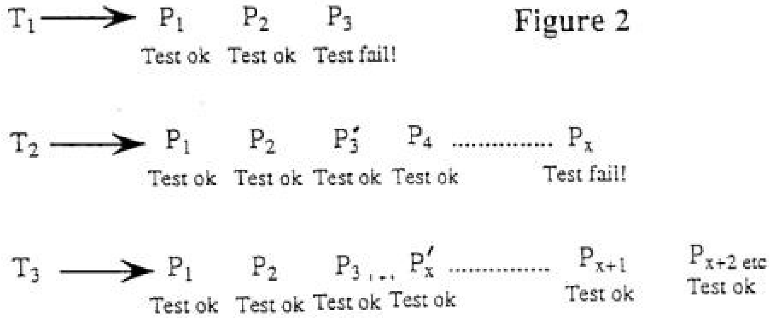
So, Popper has got problems, and that means that what we must do now, is go back in Section 4 to a little more of the detail of our story of Copernicanism, and look at the dissemination and negotiation and debate about Copernicanism, and get a picture of what was really happening, not being deluded by stories like this about what the players should or could have done. But first we need one last piece of conceptual apparatus for the history of science.

Figure 1

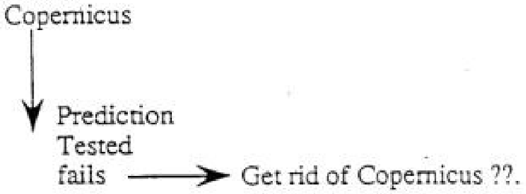


Popper rejects the left-hand side - 'Discovering hypothesis' - from the area of 'Method'

Figure 2



Poppers View



Alternative View: Galileo's tactic---Reinterpret the test and Its theory

Figure 3

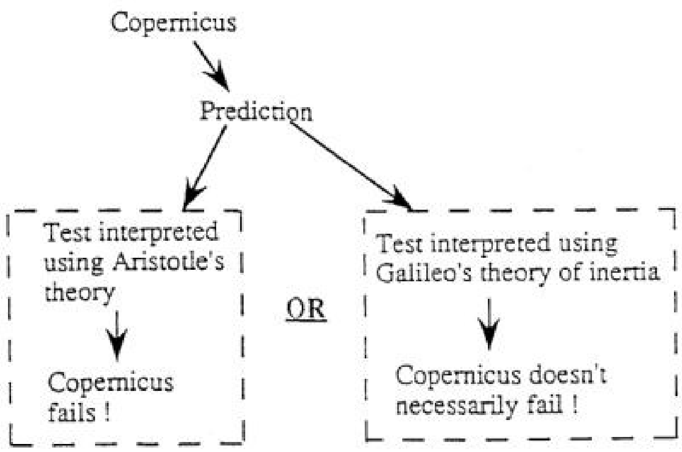


Figure 4

Theory or Hypothesis

↓

Derive testable prediction

Figure 4a

Theory or Hypothesis

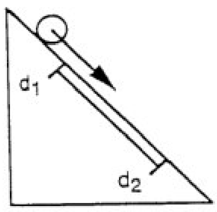
Auxilliary theories, data, assumptions →

← Simplification, E.G. Mathematical ones to permit derivation of prediction

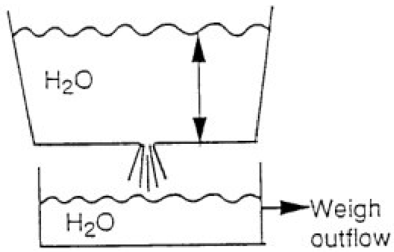
↓

Prediction

Figure 5



Inclined plane



Water 'Clock'

Galileo's law

Velocity α Time of fall from rest

↓ Derive prediction

Distance α $(\text{Time})^2$

Figure 6

Galileo's assumptions

(1) The inclined plane is stright and smooth
(2) Clock works in a linear fashion, Flow=Time
(3) Rolling ball is no problem
(4) Air resistance is no problem
(5) Distance measurement is accurate 'enough'

Figure 8

Theory

↓

Prediction
gap
data

Size ? Meaning ?

↑

Test (Experimental apparatus, procedures and assumptions

FIGURE 7

DATA DO NOT SPEAK FOR THEMSELVES-- EXPERTS STRUGGLE AND NEGOTIATE TO SPEAK THEIR MEANING

GALILEO'S (1638) LAW OF FREELY FALLING BODIES:

Distance fallen from rest is proportional to the square of the elapsed time of fall:

$$D = kT^2$$

Some Galileo-like data--what do they say?

	Predicted Times:		Test Data:		GAP
Distance	T	T^2	T	T^2	Diff.
0	0	0	0.00	0.00	0%
1	1	1	.90	.81	-19%
4	2	4	2.21	4.88	+22%
9	3	9	2.87	8.24	-8%
16	4	16	3.93	15.44	-4%

Average gap counting all trials equally: 13%
Average gap discounting the 'dodgy' trial at d=4 :10%
Average gap taking as 'adequate' only the 'good' trials at 'long' distances: 6%

IS GALILEO'S LAW CONFIRMED/FALSIFIED.?
IS THE DATA 'GOOD ENUF'/'NOT GOOD ENUF'?
WHO HAS THE AUTHORITY/POWER TO DECIDE?
HOW DO THEY DECIDE?
HOW DO THEY ENFORCE/SELL THEIR DECISION?