9 The Politics of Testing in Modern, "Big" Science: The Strange Case of Solar Neutrinos

This chapter is based on an important, indeed classic book in the history and sociology of science, Trevor Pinch's, Confronting Nature: The Sociology of Solar Neutrino Detection. Pinch's evidence and argument have been relied upon here and modified to fit the flow and tenor of this subject .

During the late 1950s and early 1960s Raymond Davis, a leading chemist at Long Island's Brookhaven National Laboratory lobbied the scientific community and the United States Government for funding to perform a certain experiment. Davis wanted 100,000 gallons of perchloroethylene (which is dry-cleaning fluid) to be placed in a swimming pool, one mile deep in an abandoned mine. According to Davis the mile deep swimming pool of dry-cleaning fluid would act as a kind of telescope. This telescope would have been different from an optical telescope that uses light, or a radio telescope that uses radio signals. His telescope was intended to study cosmic objects which no-one had actually detected before through any sort of instrument or experiment. These cosmic objects were Solar Neutrinos.

Neutrinos are one of the oldest members of the twentieth century family of sub-atomic particles, their existence had been theoretically predicted during the 1930s.They are objects which have to exist if equations of certain nuclear reactions are to follow a certain form. Neutrinos were predicted to be massless and chargeless, which makes them hard to detect. Their presence was meant to balance out the momentum and energy conservations in certain nuclear reactions. The general reliability of the equations in question depended in large part upon people generally believing that Neutrinos did in fact exist.

Raymond Davis wanted to observe and capture Solar Neutrinos--neutrinos produced by the thermal nuclear fusion reactions occuring in the core of the Sun. These fusion reactions, it was thought should produce a large number of neutrinos. However, there were disputes amongst physicists and astro-physicists about which precise fusion pathways occurred in the Sun. There are a number of possibilities, and which paths are followed and to what degree is an arguable matter. Davis thought that if we were able to observe Solar Neutrinos we might learn more about which fusion pathways were followed inside the Sun. This, we could say, was Davis's 'sales pitch' with which he tried to interest the funding bodies and the astrophysics community in his ideas. Interestingly, no-one had ever proposed such a test before: no-one in the astrophysics community, theoretical physics community or astronomical community, and curiously, Raymond Davis was not an physicist, astrophysicist, or an astronomer, but a chemist.

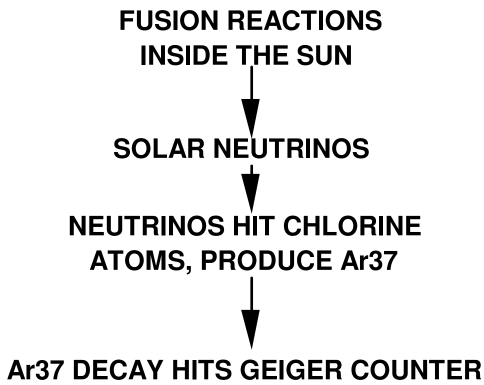
What, we may ask was this interloper up to? Why was he proposing something that no-one in the relevant community had ever thought of doing? Davis wanted this telescope because he wanted to do his own research in chemistry. He wanted a source of Solar Neutrinos which he could use in his research into radio-active argon and chlorine. When Davis first thought of experimenting with neutrinos, he learnt very quickly that earth-based nuclear reactors did not produce the right kind of neutrinos to allow him to conduct his experiments. He concluded that the Sun's neutrinos were needed. Davis could not proceed on his own research path because he really needed a piece of apparatus (the Sun) that is not available on Earth; and in order to conduct his experiment **he had to sell his idea to the astrophysicists as something which is going to test one of their pet theories.**

After a great deal of trouble Davis succeeded over a period of years in interesting three key groups in the possibility of this test. First of all he convinced a few astrophysisists, notably a young and wet-behind-the-ears John Bahcall whose job was to derive the predictions for the test. However, it was not enough for Davis to interest people like Bahcall, for they did not possess the necessary research funding to perform the experiment. It was necessary to get the United States Government and the Atomic Energy Commission involved in their proposed test to secure sufficient funding for their telescopic experiments. The Government eventually became interested in the project because it had been led to believe that this fairly esoteric form of pure research might, in the future, have some practical benefits, because solar neutrinos are broadly relevant to questions of nuclear power and energy and hence to questions of future energy and defence issues. Without this conviction on the part of certain science bureaucrats this experiment would not have been carried out. We have to bear in mind that in modern science very little happens without this preliminary political lobbying and this is where a large number of the issues in Science and Technology Policy reside.

The idea for this test of solar physics did not grow out of applying any rule of scientific method whether traditional or Popperian. No astrophysicist had thought this test up; it was an outside person who had to lobby various sections of the relevant communities to enable the test to go ahead. Despite Popper's claims, astrophysicists were not desperate to perform a 'crucial test' of their theory. The decision to build the telescope was in essence a result of the interaction between the scientific and political communities who could see mutual benefits arising from the project. Hence the conclusion must be that the experiment was more a result of political history, than of applying rules of scientific method.

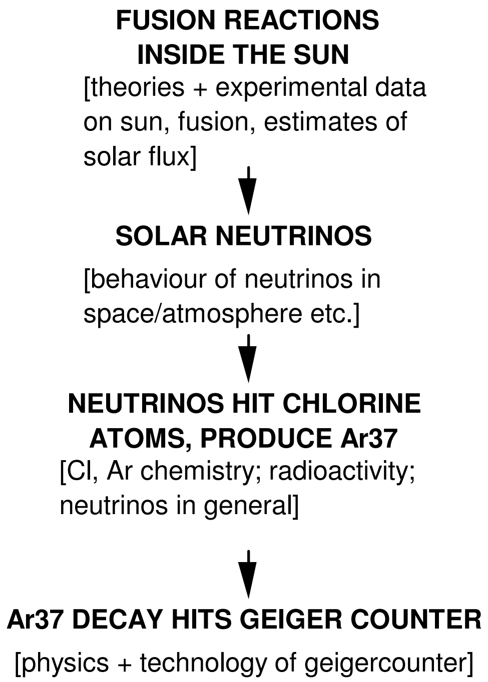
The general idea of what was supposed to happen in this experiment [Fig. 1] is that fusion reactions occur inside the Sun and these reactions produce Solar Neutrinos. There is a tank of dry-cleaning fluid deep inside the earth in which the emitted Neutrinos hit the chlorine atoms in the fluid and produce radio-active argon 37. The radio-active argon 37 will decay and that decay can be registered by a geiger counter. When the geiger counter clicks, that is an argon 37 decay produced by a chlorine atom which was affected by a Neutrino which was produced by fusion in the Sun.

FIGURE 1



Obviously, each of these steps would itself depend on a deep background of accepted theory, facts, and judgments which in turn form the basis for interpreting the new facts and theories. [Fig. 2]

FIGURE 2
Theories, data, assumptions at each step



For example, the question of fusion reactions in the Sun: we have theories about the nature of the Sun; we have experimental observational data about the Sun; we also have experimental data about fusion reactions on Earth. Some of that information from the fusion reactions observed on the Earth has to be put into our theory of the Sun. There also have to be estimates of solar flux, that is an estimate of how many neutrinos the Sun produces.

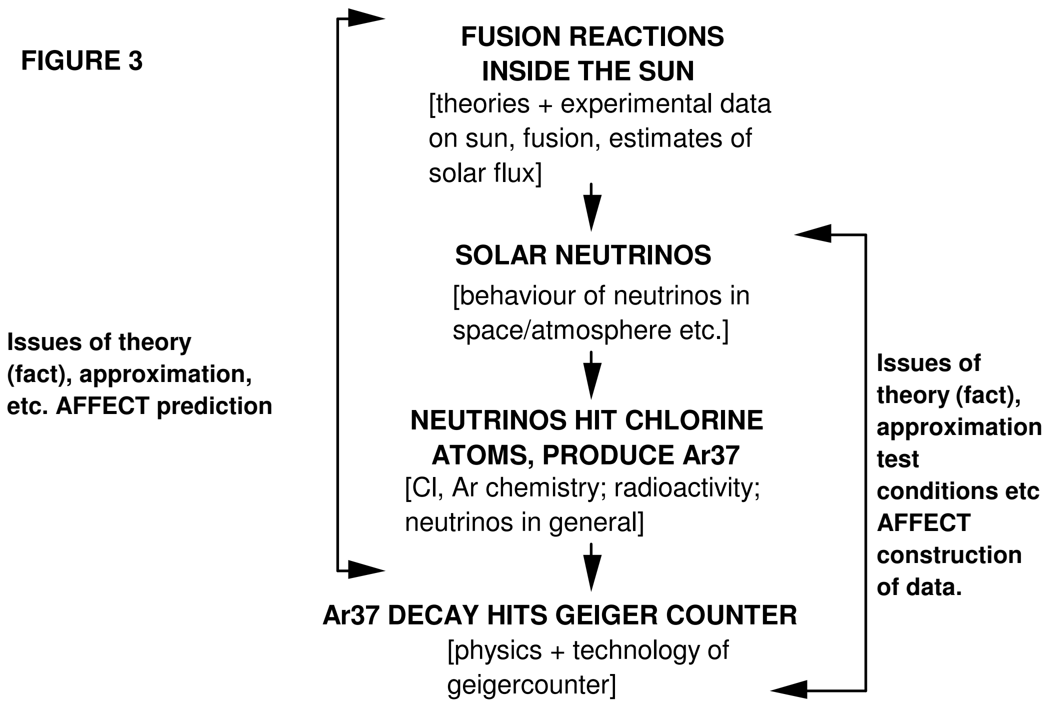
As for solar neutrinos we have to have some theory/idea about how they travel in space, how they travel through the atmosphere, and how they travel through the crust of the Earth. We have to know something about their behaviour, embedded in theory and fact, and we have to have bodies of knowledge in chemistry, radio chemistry and a theory of neutrinos.

Finally in this chain of events the decay products of argon 37 hit the geiger counter, and here we have the physics and technology of geiger counters, which of course is well established, highly factual and not likely to be questioned during the course of these experiments. Geiger counters are industrially produced commodities and no-one is going to challenge the authority of this hardware. Debate about the meaning and results of the experiment will congregate elsewhere in the chain of theory-data-assumption lurking behind the experiment.

Now, what we need is a prediction about the number of neutrinos that will hit the chlorine atoms per second, per day, at the bottom of the tank. All of the material in the 'prediction bracket' of Fig. 3 had to be manipulated to produce just such a prediction. John Bahcall, the astrophysicist had to produce the prediction. He needed to take the results of experiments on Earth concerning nuclear cross sections, that is the types and kinds of reactions occurring, and plug them into the theories of the Sun. He then made some mathematical approximations and crunched some numbers.

Therefore, Bahcall had theories of the Sun, astrophysics, test data from Earth; and mathematical approximations at various stages of the derivation. From all of this came his prediction, which had to be modified a little by what Davis had to say about the behaviour of chlorine, argon and neutrinos in the tank (in the 'issues of...test conditions brackets in Figure 3). By the time we had finished with all these things, Bahcall predicted that on the surface of the Earth we should be getting about 54 billion neutrinos per square centimetre per second and down in the bottom of the tank we should be getting between 4 and 10 reactions with the chlorine atoms per day.

FIGURE 3



Between 1964 and 1967 they built the apparatus (the tank), they got the geiger counter working, and they started getting some results--test data. Davis' produced some initial data from his early runs, however, certain trials were thrown out for various reasons ie: the geiger counter wasn't working correctly etc. From the test data gathered there were very few geiger counts--there were a lot less argon decays than the predictions suggested. There was a big 'gap' between the data Davis had collected and Bachall's original prediction.

In his first publication about this Davis said that there were a lot less decays than predicted and that therefore he felt that there was a 'contradiction' between the data and the prediction. Davis was talking the language of the Philosophy of Science, he was saying that the gap was bigger than they had anticipated. Remember, experimental science is about 'interpreting the gap' and Davis stated that his experiments were sound. We would think that Davis would then go on and criticise nuclear or astro-physics, from which the prediction side was derived, but he does not criticise either astrophysics or Bahcall. There were probably good reasons for this: (a) He was not an astrophysicist and he knew he had no sound basis on which to say that the astrophysicists were wrong. (b) He was also not going to undermine the credibility of his own collaborator.

Now, Bahcall heard about these results **before** he published his own interpretation in astrophysics journals (whereas, Davis published in chemistry journals), **and before publishing he changed his original prediction**. In other words, Bahcall changed his prediction during the publication of his interpretation, because he knew that his prediction was now 'wrong' or incommensurable with the available data. Bahcall therefore rejigged the derivation to push his prediction down, so that there was a better agreement, or less 'gap' between the data that Davis had and the new prediction. Bahcall did not change the prediction by cheating, he was not a fraud, he just changed a few approximations.

In the first calculation, Bachall, made certain judgements, now he changed those judgements and manipulated some of the parameters to produce a lower prediction. This was not straight out bias or fraud. It was the behaviour of a scientist who is a human being, as opposed to some kind of calculating machine, who has to act in a human, social negotiating situation called 'making out the result of the test', or 'interpreting the gap'. There were good reasons for him to make these second assumptions, as well as good reasons for making the first assumptions. There is no method or God-given revelation of why he should make one set of approximations over another. What Bahcall wanted to do was stick close to the emerging data, rather than stick with his original prediction and have either to confess a bad prediction or accuse the experiment of being wrong (the inverse of Davis's situation!). Why? There was a social, psychological and professional reason why -- this was Bahcall's first big project and he did not want to be out on a limb and accused, if not by Davis, by his colleagues of having botched the prediction. Bahcall preserved his standing, by recalibrating his prediction.

Now what do other astrophysicists say about this. Some say Davis is right and agree that the gap is 'too big'. They criticise Bahcall about his predictions and they decide to work on the predictions made by astrophysics. In effect they are saying in public what Bahcall is trying to hide. Other astrophysicists say that the gap is small enough, the result is 'good enough'. What these people are really saying is that they do not care much about pursuing this experiment or its conclusions, astro-physical theory is 'ok' and so is Davis' new telescope. So we have differing responses grounded in different social, psychological and professional settings and aims in the professional community--as I hinted was the case with Lavoisier, Priestley and with Galileo in earlier chapters.

The story goes on and on, but I will just say that after Bahcall had become a tenured professor he decided that there was indeed a 'contradiction' between the data and the prediction in the experiment. Remember that his first move was to close the gap. When he was a more mature practitioner he argued that there indeed was a problem, in fact he intended to work on closing that 'too big' gap because he could win more professional merit by closing it. In one year and one social place a scientist can hide the discrepancy between the data and the prediction and in another year and another social place he can acknowledge it was a problem and decide to work on closing the gap.

In the ten years from 1968 to 1978, 134 articles were published on this experiment with 40 different assumptions about the predictions or the data. The predictions kept going up and down; the data kept being rejigged and to the present day no consensus has been reached about the prediction, the data, the gap or its meaning. This is an experiment without a socially negotiated outcome. And a socially negotiated outcome is the only outcome an experiment can have. God does not close the debate; applying a method does not close the debate, and remember above all, "The data do not speak for themselves".

Let's just remind ourselves what people could manipulate if they wanted to rejig the predictions or rejig the data. This is certainly what went on in the 134 articles over 10 years [Figure 3]. We can rejig the prediction at the level of the theory of the Sun; at the level of our nuclear cross sections measured on Earth; at the level of our theory of the behaviour of neutrinos in space, the atmosphere or on Earth; at the level of radio chemistry. These are issues of theory, fact, approximation etc. which all affect the prediction. We can also reinterpret the data by rejigging all of the above. One could also go as far as to suggest some rejigging of our understanding of the geiger counter, although I will put it to you that no-one did nor were they likely to for it was the one firm rock of 'fact' in the whole experiment, since it would be too expensive for science to start questioning data from that source.

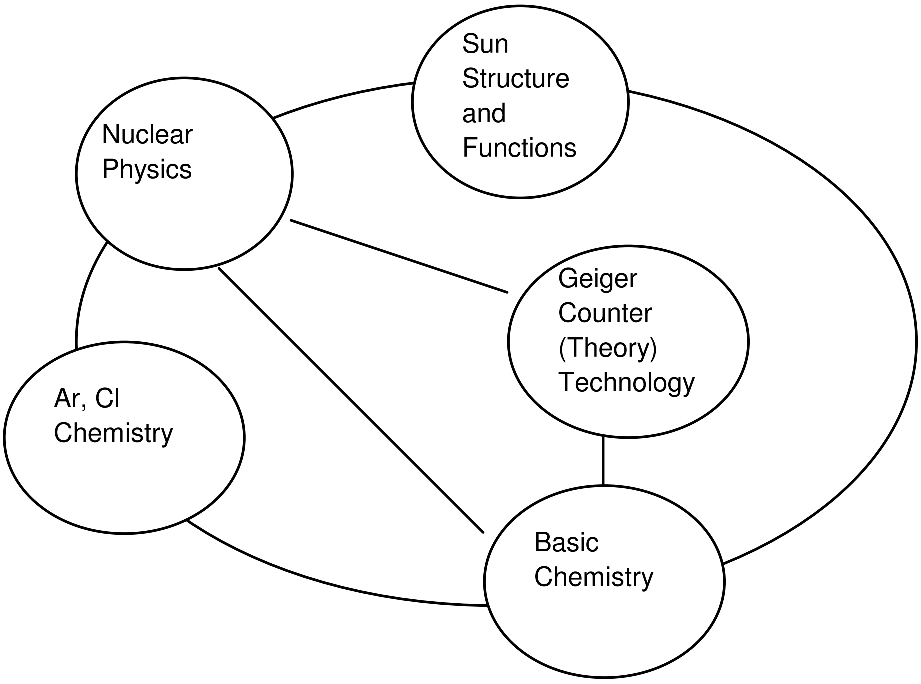
Notice the overlap in the brackets on each side in figure 3. Issues in the overlap area can be rejigged to affect the data or the prediction. It all depends whether the prediction is approached from a background in astro-physics or from a background in Radio chemistry. Notice the former is the realm of astrophysics--a professional sub-group and the latter is the realm of radio chemistry--another professional sub-group. Part of this story is obviously the inability of the two professional sub-groups to agree on the gap or its meaning. Some summary observations: in simple terms this story is a prime example of exactly the same thing that happened to Galileo and his friends--negotiations over what the prediction was, what the results were and exactly what they meant.

Now let's go a bit beyond Pinch's analysis and look at the stakes involved in closure or non-closure of this negotiation, and in particular how knowledge, facts and even artifacts might change depending upon the particular form taken by a closure.

The first point that I would like to make is that the attempt to build a neutrino telescope occurred at a junction or node in a network of relationships of already accepted (in 1967) theories and facts. [Fig. 4] Theories and facts in nuclear physics; in radio chemistry and basic chemistry; in geiger counter theory and technology. Right in the middle is where Davis wanted to insert a neutrino telescope, he wanted to place there a new apparatus, a new commodity. We know that no decision was ever reached by the relevant scientific community on the meaning of the gap. But, let us look at the options involved.

FIGURE 4

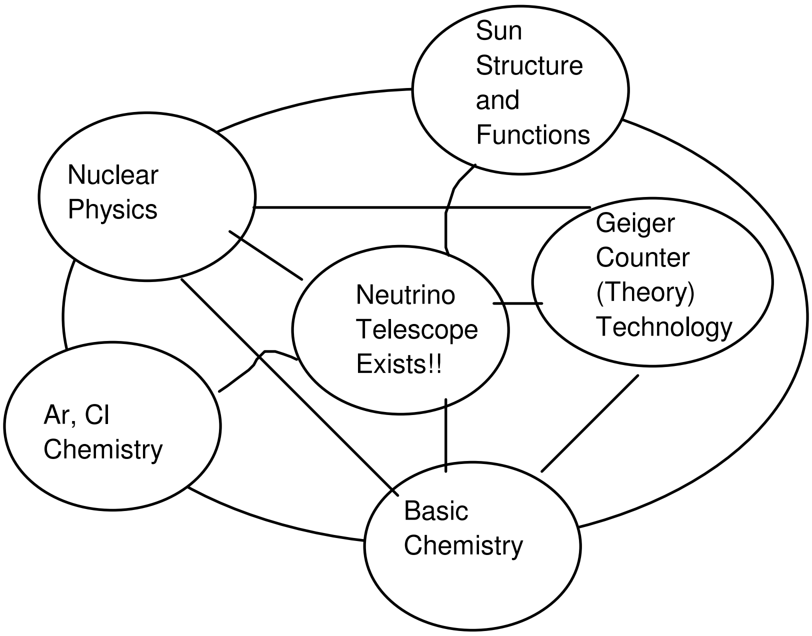
1) Before attempt to build telescope:
Relations of bodies of knowledge - fact - practice



The first option is this: [fig.5] now we have a network; nuclear physics, chemistry, geiger counter functions and we have the neutrino telescope. Let us pretend that the test had been conducted originally with Bahcall's first prediction and Davis' first data, and that everyone had decided that the gap was 'sufficiently small' for the test to be ruled O.K. In this hypothetical situation what would the consequence of that social decision have been? The consequences would have been (a) such a thing as a neutrino telescope exists and works; (b) all related knowledge would have been reinforced and not changed much at all. Correspondingly, there would have been no threat to nuclear physicists, to radio chemistry, or to geiger counters. We would probably have learned that certain solar reaction paths are favoured over others, so we would have learned more 'information' about that science. This is working on the assumption that everyone agreed that the gap in the first test was 'small enough'. **What we must understand is that what we are looking at is a possible social outcome (everyone agrees) and we are looking at the consequences for knowledge of that agreement.**

FIGURE 5

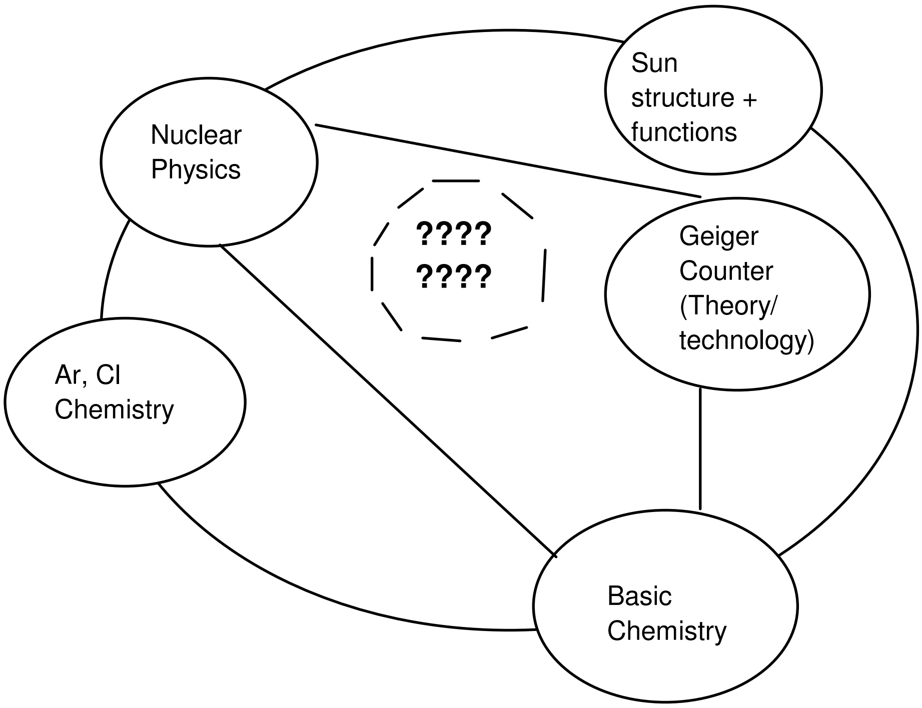
2) If 'gap' in telescope test is 'small enough'
  a) telescope works
  b) all related knowledge reinforced
  c) particular solar reaction paths reinforced



Now what if the exact opposite had been firmly decided by everyone: that the gap was not small enough? [fig. 6] Assume also that no agreement could be reached on any other changes of knowledge (involved in data production or prediction production) to make the gap smaller. What then? The result of that social negotiation would have been that **there is no such thing as a neutrino telescope**: it was an illusion, a mistake and no more of them should be built in that form. This would be the outcome if no-one agreed on ways of changing the knowledge. None of these decisions are reached by applying some scientific method that magically puts us in touch with the facts. What we are talking about is negotiations and judgements made in the relevant communities and whether there is consensus or not, and how agreements may change knowledges, artefacts and knowledge--artefact networks.

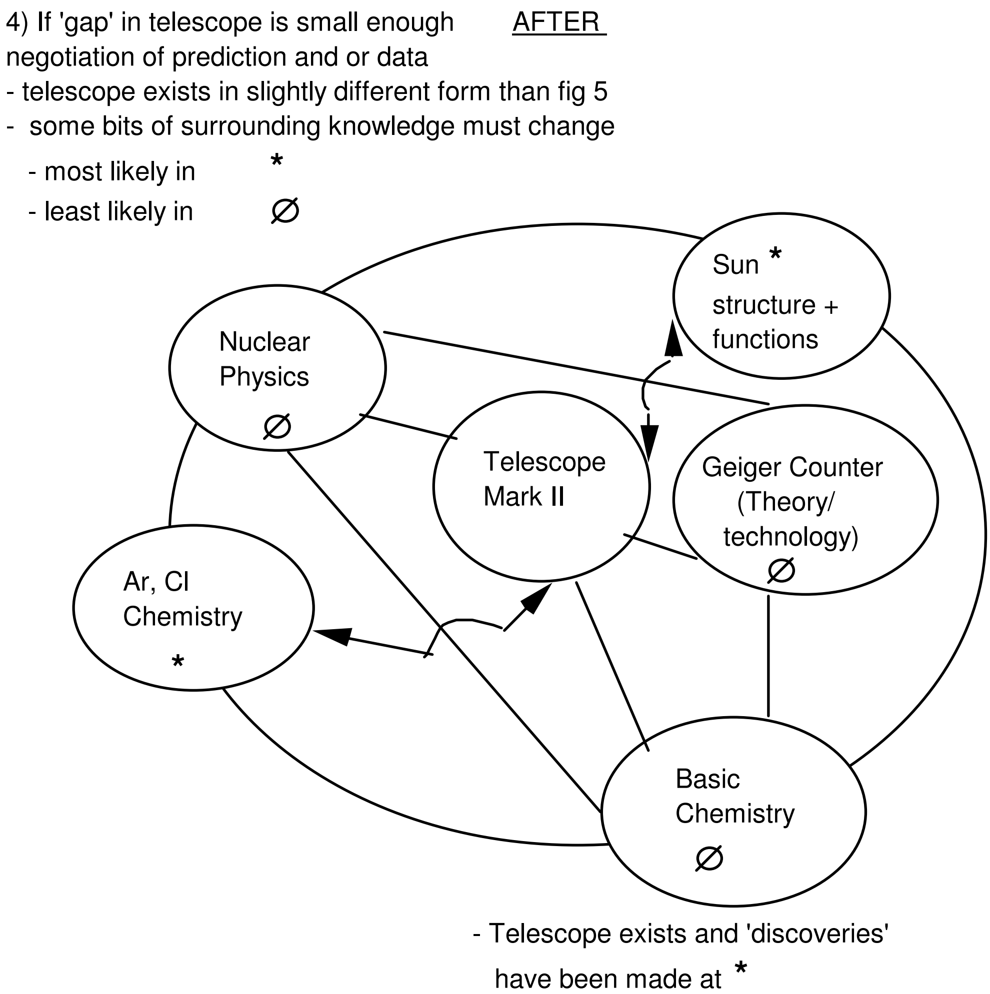
FIGURE 6

3) If 'gap' in telescope is not 'small enough'
- the telescope does not exist
- all other knowledge <u>constant</u>



Finally, in [Fig. 7] what would have happened if after a lot of negotiation everybody decided the gap was small enough? First of all the telescope would exist in a slightly different form to Fig.5 where we just accept the initial results as being real. **But now we are accepting the telescope after some negotiations about the prediction and the data. Some bits of surrounding knowledge must have changed. This is what the negotiation was about.** What were they able to adjust? Probably not the geiger counter; probably not fundamental physics--it is not going to be changed; the most likely places where some change is likely to occur is in the theory of Sun-structure and function or in radio chemistry, which could undergo a change of theory in order to get the data and prediction into a more agreeable relationship. Now we can have a neutrino telescope with slight changes at places marked with an asterisk. The conclusion in this scenario is that the telescope exists and that 'discoveries' have been made at the places marked. We made new 'discoveries' of our understanding of the sun and/or new 'discoveries' in radio chemistry. (Remember that discoveries are socially negotiated and accepted changes of theories and procedures) What would be called 'discoveries' are actually agreed modifications of theory-practice (Chapter 5), arising in this case from negotiations about how to narrow the gap and so allow a solar neutrino telescope to exist.

FIGURE 7



How would any of these scenarios have come to pass? It would not happen by deploying some scientific method. The existence or non-existence of the telescope or its results, are all the result of political processes of negotiation and interpretation by the relevant scientific communities. Different people, different groups argue and negotiate from somewhat different positions or infrastructures. T**here is no way that such social processes can be stopped or avoided; in fact there is no way that it should be stopped because that is what science is--that is how the business of is conducted as experiments are performed, apparatuses 'invented' and 'discoveries' made!**

Post-script: Recall Popper insisted that tests should be taken as definitive and that if our hypothesis failed the test, we should discard the theory straight away. Popper's tale of the scientific method is very telling for it never does or can happen that way. **People never instantaneously judge the tests as invalid, for negotiation over the gap and meaning of the gap is not just an option but, it is a necessity, because gaps do not speak for themselves. People and groups from various social locations in the scientific field try to speak for the gaps. The whole challenge in the History, Sociology and Politics of Science is to understand how, why and to what purpose different groups speak and negotiate the way they do.**