Science and non-science: the search for a demarcation criterion in the 20th century

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Abstract

Contemporary philosophy of science has struggled considerably over an apparently simple question: how can one distinguish scientific from non-scientific thinking (knowledge, research and so on)? What are the characteristics of science which make it a unique type of activity? A sub-theme is also explored: which disciplines or fields of study may be regarded as "science"? These questions constitute the quest for the so-called "demarcation criterion between science and non-science". The responses provided by some of the most prominent philosophers of science of the 20th century are evaluated, with the conclusion that they have not supplied a solid dmarcation criterion. In the final part of the article, however, I briefly outline the criterion elaborated by the South African scholar, DFM Strauss, in the context of the Dooyeweerdian tradition. I suggest that this criterion constitutes a plausible response to the demarcation problem.

Opsomming

Wetenskap en nie-wetenskap: die soeke na 'n afgrensing-kriterium in die 20ste eeu

In die kontemporêre wetenskapsfilosofie word 'n stryd gevoer oor die oënskynlik eenvoudige vraag: Hoe kan wetenskaplike denke

van nie-wetenskaplike denke (kennis, navorsing en so meer) onderskei word? Wat is die eienskappe van wetenskap wat dit 'n unieke soort aktiwiteit maak? Die subtema hiervan is: Watter dissiplines of studievelde mag as "wetenskap" beskou word en watter nie? Hierdie vrae konstitueer die soeke na die sogenaamde "afgrensing-kriterium tussen wetenskap en nie-wetenskap". In hierdie artikel word die antwoorde van sommige van die mees prominente wetenskapsfilosowe van die 20ste eeu op hierdie vraag geëvalueer, met die konklusie dat hulle nie 'n soliede afgrensing-kriterium daargestel het nie. Ten spyte hiervan, skets ek egter kortliks in die slotdeel van hierdie artikel die kriterium wat uitgewerk is in die konteks van die Dooyeweerdiaanse tradisie deur die Suid-Afrikaanse filosoof, D.F.M. Strauss. Ek stel voor dat hierdie kriterium 'n aanneemlike antwoord op die afgrensing-probleem bied.

1. Introduction/background

During the 20th century (most) philosophers of science and (some) scientists have gradually discovered the inevitable presence of all sorts of presuppositions, expectations (Popper), premises (Polanyi) around and in science. The ideal of unbiased objectivity has become more modest, and many types of extra-scientific factors are said to play a fundamental role in shaping science, factors like socio-economic conditions (Habermas), power-struggles (Foucault), psychological traits (Kuhn) and so on.

Does it mean that today the difference between scientific and non-scientific (thinking, knowledge) is not much clear? Most of the times, the intuition (in some cases the conviction) that there is a difference between the two is still there, but it seems quite difficult to explain it. In the meantime, within the academia "scientific status" is claimed by a broad range of "subjects" (often for funding purposes) while faculties offer courses on topics for which labels like "surrogate sciences" have been coined (Storkey, 1986:110-116).

Contemporary philosophy of science has struggled considerably over the apparently simple question: what is science? In what is it different from other types of thinking or knowing? Are there unique characteristics of scientific theorizing? These questions point towards the desirability of what is commonly known as a "demarcation criterion between science and non-science". The same questions constitute the research-problem of the fol-

lowing pages. This article provides a historical survey of some of the most authoritative answers offered during the 20th century. The survey shows that there was little agreement on this topic and that the various proposals by Popper, Kuhn, Feyerabend and others, although containing valid insights, did not fully hold water.

However, I will also briefly present a demarcation criterion which I consider more plausible. The latter has been proposed within the philosophical tradition of Dooyeweerd, Vollenhoven *et al.,* a tradition also known as the school of reformational philosophy. I will present this criterion in the version elaborated by D.F.M. Strauss, a well-known South African philosopher working in this relatively small school of Christian philosophy. This seems to me, at present, the best proposal available on the demarcation problem.

This survey has not the purpose of demonstrating that science is somehow more reliable than other forms of knowledge or vice versa. The goal is simply showing that it is possible to find a valid demarcation criterion, which should in turn be a valuable tool in several contexts and discussions.

Our exploration begins with a movement whose roots are in the 19th century.

2. The positivist background

The positivist criterion to identify the unique character of science is "verification": science deals with results, theories or experiments which can be verified. In the long run, admittedly, the criterion went through a process in which (at least in some cases) more "modest" positions were endorsed. In Carnap's *Testability and meaning*, for example, verification was gradually "reduced" to confirmation (Carnap, 1936; 1937). Later on, confirmation was gradually toned down to probability (Carnap, 1951).

In the positivist tradition the natural sciences are not necessarily the only sciences (Comte regarded e.g. sociology and ethics as sciences), but the humanities are supposed to endorse the method of the natural sciences. In other words, (some of) the humanities can be "scientific" provided they use the method of the natural sciences, which is valid in all fields and for all subject matters. In this respect, Comte's definition of sociology as "social physics" is indeed revealing.

The positivist approach has also "inspired" a few popular solutions to the demarcation problem. They are not often defended in philosophical debates but they seem to be popular among academics working in the natural sciences (and some of the humanities). The first criterion says that

we have science wherever we have experiment. The second one says, a bit more generically, that we have science wherever the scientific method is applied.

In general, these criteria betray an empiricist inclination and often aim at granting scientific status only to the natural sciences. However, the supporters of "experiment" often do not realise that there are many types of experiments: thought-experiments (see Galileo), psychological experiments (e.g. on perception), sociological experiments and so on. The aim of identifying science with natural science, therefore, is not easily achieved through this criterion. Furthermore, one needs to ask: do experiments constitute a unique characteristic of science? Indeed there are non-scientific types of experiments. One may reply that science is characterised by *scientific* experiments. But as long as we don't know what "scientific" means, we are caught in a vicious circle.

This seems to point towards the necessity of adopting "the right method" and so we come to our second criterion: we have science whenever the scientific method is applied. On this point, however, one may ask the question: "should science be determined by method or should methods be determined by the aims and objects of scientific research?" (Strauss, 2001:28). The supporters of this approach often exclude in advance that there may be a variety of scientific methods fitting a variety of scientific disciplines, objects, areas of research and goals. This approach, therefore, doesn't seem to be much adequate. On a more philosophical level the above discussion refers back to the positivist program of extending the method of the natural sciences to all sciences and to the objections raised by Dilthey, Rickert and Weber.

3. Popper

3.1 Falsification and deduction

The issue of demarcation stands for Popper at the centre of philosophy of science. It was precisely by working on this problem that he could develop his understanding of science in other directions as well (Popper, 1963:42 ff.).

Before discussing Popper's view we should have a look at his rejection of the verification criterion. On this point Popper argues that it is impossible to know when a theory is "true" in a final sense. In science we approximate the truth, we come closer to it, but we can never be sure that we have reached the final step. Most of the theories proposed in science before the 19th century are nowadays rejected. This shows that we keep improving

our knowledge and that, in every field of research, it is impossible to exclude that a better theory may be proposed tomorrow.

There is another reason why verification doesn't work. No matter how many verifying instances we may encounter in our observation or study of a certain phenomenon, they will never fully exclude the possibility of encountering disproving instances. The classical example refers to the existence of black swans. All observation prior to the 18th century confirmed (verified) that "all swans are white". Yet at a certain stage a black variety of swans was found in Australia by European explorers. This single observation refuted thousands of verifications. Therefore scientists should not try to verify but to falsify their theories, and this is done through mutual criticism. Theories which resist falsification can be considered "corroborated", reliable or verisimilar. Verification is not a scientific approach, from which we also learn that induction is not a scientific approach.

From a logical point of view, verification amounts to proposing a syllogism like: if my cat doesn't come home tonight it means that he is dead. The cat doesn't come home, therefore (I verify that) he is dead. But of course the cat may be chasing mice, or following a she-friend, or whatever. No, says Popper, the correct method is to say: if the cat comes home it means that he is NOT dead. The correct path is trying to disprove something.

Summarising, Popper distinguishes science from non-science on the basis of (the possibility of) *falsification*. Theories are scientific if they can be falsified, if they are open to refutation. Non-scientific conjectures, theories, views cannot be refuted. Popper's classical example is the difference between astronomy and astrology (Popper, 1963:37). One may prove wrong astronomical theories, predictions or hypotheses. But when it comes to astrology, no refutation can truly be applied: it will always be possible to propose "ad hoc" adjustments, to appeal to the complexities of the matter, to say that the prediction failed for a specific person but it remained valid for some others.

In this context, Popper (1963:37) discusses Marxist political theory and Freudian psychology as well, and he concludes that they are non-scientific. Now, for Popper, non-scientific does not automatically mean nonsensical, and here is another difference with previous positivist positions. As far as scientific theories are concerned, however, they must be falsifiable and the best scientific theories are the most open to refutation, the most "vulnerable" to disproof.

3.2 Science, universals and laws

So how many sciences would Popper recognise? Well, all those in which falsification can be applied. But it seems that here another element is considered necessary. Both communal criticism and (eventual) refutation require stable, universal and un-changing laws. A theory cannot be disproved on the basis of occasional and changing phenomena. What type of "laws" are these?

Popper creates a link between laws and universals. According to him (1963:118) "all universals are dispositional". Universal terms indicate that the thing named by the term shows a certain behaviour under certain conditions. Such behaviour is defined as "law-like behaviour" (Popper, 1963:278) and here we have the link between laws and universals. The laws of nature exist and are universal in scope. However, for Popper (1961:5) a synonym for "laws of nature" is "physical laws". In fact, Popper denies that there are historical or social laws but only "trends and patterns" (1961:115) and the only laws of nature that he recognises are physical laws.

In the social or cultural world, we may find patterns or regularities, but not the sort of law-like behaviour which is found in the natural world. This is the reason why, I argue, Popper is inclined to attribute the status of science mainly to the natural sciences, especially to physics. This is also why he (1970:57-58) labels psychology, sociology or theology as "spurious" sciences, ridden by all sorts of fashions and dogmas.

While the universal laws are for Popper the object of study of the natural sciences the other sciences are supposed to focus on individual types of events, while *presupposing* the natural (i.e. physical) laws (Popper, 1961:144; 1963:341). Here Popper offers the example of the historian who, by exploring the causes of Giordano Bruno's death on the stake, focuses on *individual* events and processes while presupposing the universal law that "all living things die when exposed to intense heat" (Popper 1961:145). (Of course, the historian does not need to pay much attention to such universal laws).

Popper restricts genuine universals to the world explored by the natural sciences. It is not easy to see why he does this: if we say that certain objects are breakable under certain conditions why would it be incorrect to say that people have a disposition to behave in a certain way under certain conditions? Why would physical dispositions be more real than social dispositions? And why should historians pay attention to the individual

Julius Caesar or to the unique English Revolution? Is it not because (only insofar as) they belong to culturally relevant *categories* like those of "dictators", or revolutions?

Another crucial challenge to the criterion of falsification is discussed by Kuhn. Popper argues that it is impossible to attain complete verification, but is it possible to have complete falsification? Popper himself admits that it is not possible: is this not a fatal blow to the whole criterion? (Kuhn, 1970a:15). An alternative Popperian approach, the one forged by Lakatos (1979:134 ff.), will be briefly discussed later, in the section (6) reserved to Feyerabend. In the next section we should pay some attention to Polanyi's approach to demarcation.

4. Polanyi

4.1 Demarcation and tacit knowing

It is not immediately clear what constitutes the demarcation between scientific and non-scientific for Polanyi. At first sight his approach seems to be more suitable to indicate the continuity between the two, rather than the difference. I'm referring for example to the passages where Polanyi says that the scientist must always start from some kind of pre-scientific interest, knowledge or concern for what is to be investigated. For example he remarks: "the existence of animals was not discovered by zoologists, nor of plants by botanists" (Polanyi, 1958:139). Yet this continuity does not constitute the whole picture: he also suggests a criterion to distinguish between scientific and non-scientific.

It is the distinction between focal and subsidiary awareness that informs his demarcation criterion. Scientific study is focal and integrates the (pre-scientific) awareness of particulars apprehended in a subsidiary way.

Every act of knowing consists of two dimensions or components: the implicit (or tacit) and the explicit. Let us start by describing the tacit component.

Polanyi (1966:3-14) recognises four types of tacit knowing: functional, phenomenal, semantic and ontological. Functional tacit knowing is related to knowing and applying skills. Phenomenal tacit knowing deals with the process of observation. The semantic aspect of tacit knowing deals with meaning or significance and ontological knowing involves understanding a complete entity. Each kind of tacit knowing, therefore, has crucial implications for the scientific activity. When scientists conduct experiments and manipulate instruments, they use skills involving a tacit component.

Observation has a tacit component. Trying to find the meaning of experiments and observations implies a tacit dimension. Finally, we can only understand an entity by relying on subsidiary particulars constituting the tacit element of the act of knowing. In short, all the major steps of the scientific enquiry are constantly tied up with implicit, subsidiary, pre-scientific knowledge.

What about the explicit component? While the implicit (tacit) component remains unnoticed, the explicit one is consciously perceived and becomes the focus of our attention. A person relies on the tacit component to understand the explicit component (Polanyi, 1966:9-10). When we read a sentence, for example, understanding of the meaning of the single words is tacitly assumed and constitutes a series of clues to the meaning of the whole sentence, which is the focus of our explicit attention.

In Polanyi's works the term science still refers primarily to the natural sciences. Philosophy of science is thus especially related to the problems and history of the natural sciences, but Polanyi also offers lengthy discussions of mathematics, the humanities and even technology. The humanities acquire more emphasis and focus in Polanyi's approach than in Popper's.

4.2 Critical remarks

Summing up, the distinction between focal and subsidiary awareness informs the demarcation criterion between science and non-science. For Polanyi (1974:150-151) however, pre-scientific knowledge is not only tacitly bound up with science. It also provides, on its own level, an informal and tacit integration of the parts of an entity that science may study (Polanyi, 1974:151). In other words, while the formal focusing of science is integrated with a pre-scientific subsidiary awareness, one finds on this subsidiary level further centres of tacit integration. This may create a problem: if focus and integration are to be found both in scientific and pre-scientific acts of knowing, can they still constitute a demarcation criterion?

In addition, the fact that science is regarded as focusing on "wholes" or entities creates a problem, as this seems to be rather a characteristic of non-scientific knowledge. That scientific knowledge does not focus on individual entities was known since the times of Aristotle (1961:981a, 30 and 1003a, 15). On what type of "entities" should then science focus? On this point I am not sure that Polanyi has provided a clear answer. The most famous of his students has followed a different road, in critical dialogue with Popper.

5. Kuhn

5.1 Puzzle-solving as criterion

On the problem of demarcation between science and non-science, Kuhn (1970a:6) differs from Popper and argues that the critical attitude is typical only of the revolutionary moments of scientific research. If we want to define science, we must keep in mind normal science (Kuhn, 1970a:6) and in that case the critical attitude is not the main ingredient. On the contrary, "it is precisely the abandonment of critical discourse that marks the transition to science" (1970a:6). What is essential to normal science, in Kuhn's opinion, is rather "puzzle-solving", which is "more fundamental" than Popper's testing (Kuhn, 1970a:7) and constitutes also the demarcation line between scientific and non-scientific (1970a:9). This criterion finally helps understanding why astrology is not a science: while anomalies in astronomy potentially lead to all sorts of puzzles, in astrology there are no puzzles. While the occurrence of failures can be "explained", particular failures do not lead to puzzles and cannot be used in an attempt to revise the astrological tradition (Kuhn, 1970a:9).

The phrase "puzzle-solving" should not be taken as pejorative. Puzzle-solving is an activity aiming at solving scientific problems generated within a certain paradigm. The puzzle-metaphor appeals to the fact that in normal science both the boundaries of the problems to be pursued and the outcomes of research are delineated in advance (Kuhn, 1963:361-362; 1970:35-36).

Curiously, Kuhn's solution of the demarcation issue seems to privilege the normal phase of science, leaving revolutionary science out of the picture. In some cases he tries to include the revolutionary aspects of science in his definition, for example when he speaks of both puzzle-solving and invention (in dialectical tension) as characterising science (Kuhn, 1963:368-369). But in general Kuhn is inclined to find the essence of science in normal science and more precisely in puzzle-solving (Kuhn,1970a:6 ff.). In fact, at least in Kuhn's initial understanding, science is born as normal science, after abandoning the pre-paradigm period characterised by creativity and proliferation of theories. In addition, normal science is by far the most common occupation of the scientific community and revolutionary moments are quite rare.

The problem with puzzle-solving, as a demarcation criterion, is that it may characterise not only science but ordinary life as well. Feyerabend (1970:200) rightly observes that even a group of criminals might very well

be involved in solving "puzzles", in order to realise their purposes. The same critique, it seems to me, is applicable to Popper's criterion of demarcation: the critical attitude and the attempts at falsification do not occur only in science but also in parliaments, in business, in courts and in everyday life in general. Strauss (2001:29) observes that the verification criterion is vulnerable to the same type of critique.

5.2 Which sciences are scientific?

Kuhn seems to value the contribution of the social and human sciences more than Popper. In particular, history (of science) acquires great importance. As the psychological and social aspects of the scientific community become more relevant for Kuhn, it might be argued that this is an implicit recognition that sociology and psychology may help clarifying the mechanisms of the scientific research. The history of the (natural) sciences is the basis of Kuhn's system of thought. Philosophy also acquires a new legitimacy and theology which in his view is not more dogmatic than the natural sciences (Kuhn, 1963:147ff.).

Kuhn reverses a time-honoured prejudice concerning the open mindedness and objectivity of the natural scientist as opposed to the dogmatism of the scholar involved in the humanities. In his view, the type of education imposed on students of the natural sciences is likely to cause dogmatism and a narrow-minded propensity for puzzle-solving. The human sciences on the contrary, show a rich variety of paradigms which probably keeps the mentality of the scholar more open (Kuhn, 1963:350-351). All this could only provoke the irritation of Popper (1970:57-58).

Kuhn's revaluation of the humanities is to a large extent real (cf. Kuhn, 2000:216-223). And yet the natural sciences maintain some kind of "superiority" in Kuhn's system of thought. For example, philosophy of science remains for him the philosophy of the natural sciences. At the end of the *Postscript* the natural sciences are repeatedly called "the sciences" while there are disciplines which are indicated simply as "activities" or "fields" (Kuhn, 1970:208-210). The fact that Kuhn (1970b:144 ff.) defines philosophy and the "arts" as "proto-sciences" betrays the conviction that (at least in their present condition) the humanities miss something, or have been left behind in the course of historical development. This "something" is obviously the presence of a first paradigm gaining the consensus of all schools and marking the transition to "mature" science.

Kuhn's criterion, however, was to be challenged very soon.

6. Feyerabend

6.1 In dialogue with Kuhn and Lakatos

As mentioned above, according to Feyerabend (1970:200) Kuhn's criterion of demarcation between science and non-science is un-plausible. Puzzle-solving can be practiced even by a gang of robbers (in some cases they might even change their strategies in "revolutionary" ways)!

Concerning Lakatos' criterion of demarcation, Feyerabend argues that it is only slightly different from Popper's criterion (i.e. falsification). The only difference, according to Feyerabend (1970:215) is that "Lakatos gives a theory time, he permits it to develop (...) and he judges it only in the long run". Lakatos wants to see whether the theory is part of a progressive or degenerating research program. Nevertheless, the "critical standards" he introduces make sense only if applied within a time-limit. But once the time-limit is introduced, unfortunately, we go back to naive falsificationism and all its problems re-appear "with only minor modification" (Feyerabend, 1970:215). At this point one can do "one of the following two things", says Feyerabend. One can abandon Lakatos' "critical standards" or retain them as "verbal ornament".

What about Feyerabend's own demarcation criterion? He seems to propose a sort of new idea when he speaks of the "interaction between tenacity and proliferation" (Feyerabend, 1970:211), the germinal formulation of which he attributes to his friend and colleague Imre Lakatos. For Feyerabend science implies tenacity in defending older views and theories, and the proliferation of new theories as well. The two ideas are not set in conflict (as in Kuhn) but dialectically united to account for the real practice of science. Feyerabend also avoids dividing science according to the dichotomy proposed by Kuhn's division between normal or revolutionary science. The two categories become two phases within the same science. In other words the "philosophical" and the "normal" phases of science co-exist in every segment of research (Feyerabend, 1970:212).

The main question, in this regard, is to know whether it is a real demarcation criterion or only a "picture of science" (cf. 1970:211). If it is a criterion it does not really tell us what is unique in science, or how science is different from non-science and it does not necessarily exclude non-scientific activities. In fact, both tenacity and proliferation can be observed e.g. in parliaments and in church or family life as well. I even suspect that perhaps this was done on purpose by Feyerabend: although he provided some sort of demarcation, in a context of high academic discussion, he did

not believe that a real distinction exists between scientific and non-scientific, between science and life (Feyerabend, 1975:19).

6.2 Against demarcation?

Feyerabend has no problem in granting scientific status to a large variety of disciplines, but this is not the main issue. It is demarcation itself, it seems to me, which cannot be allowed permanent residence in his system. He complains that a rigid border is set up between the sciences: physics is distinguished from theology and from philosophy (e.g. 1975:19). Then science is distinguished from life and from belief. “The separation of science and non-science” he writes in one instance, “is not only artificial but also detrimental to the advancement of knowledge” (Feyerabend, 1975:306). According to Feyerabend it is not yet understood that science is also a kind of belief, a demanding practice sometimes, but still a belief that cannot be completely justified.

Feyerabend considers science as a belief system, while he can also speak of the “scientific content of some myths” (1975:49, fn. 7). Belief in science should be treated like any other belief. There are other options available: one could prefer believing in voodoo or in ancient myths. Science should not be compulsory for our education and there should be a “separation of state and science” (Feyerabend, 1975:301), as there already is a separation between state and church (1978:106). Feyerabend complains, for example, that beliefs like “the earth rotates around the sun” are regarded as absolute truth, instead of saying “some believe that the earth rotates around the sun”! (Feyerabend, 1975:301).

In his work there remains a sense, however, in which myth and scientific elaborations can be distinguished. Scientific elaborations can be seen as articulations of a basic myth. Some myths are elaborated into all types of theories. Others are never elaborated, although the possibility remains always available. In any case, science is not more rational than other activities. The supposed objectivity and rationality of science are unmasked as pretensions the moment we analyse the concrete ways in which science has progressed through history. These ways do not exclude *ad hoc* adjustments, propaganda and so forth. Science is like any other human affair: it progresses without order, it needs to resist common sense and to proceed counter-inductively.

In this view, it is possible to compare beliefs with beliefs, and scientific theories with scientific theories. But one can also compare myths with

science, or scientific with non-scientific activities. For example, asks Feyerabend (1975:49-50, esp. fn. 7 and 8), is modern science "superior" to ancient achievements like the building of pyramids or the astronomic observations of the Maya? Our trust in science is often based on the conviction that it has achieved incredible practical results. Well, there are many primitive and ancient achievements as well, sometimes more modest (technically speaking) but much more gratifying from a psychological point of view (Feyerabend, 1975:306). They satisfied both physical and social needs, which modern science often fails to do.

With Feyerabend, in a sense, we have come "full-circle". We have started from the confident views of positivism and we have reached a position where the demarcation problem seems to become obsolete.

7. Other postmodern voices

In more recent times the question "what is so unique about science?" has not received extraordinary attention. One may suspect that a certain disillusion has crept in. The feeling is well illustrated in the words of Laudan: "the fact that 2400 years of searching for a demarcation criterion has left us empty-handed raises a presumption that the object of the quest is non-existent" (Laudan, 1980:275).

Nevertheless, another attempt was provided by Lyotard (1984:8,9,43) who, referring to Wittgenstein, characterised science as a particular type of language game. The problem with this approach is that it assumes that science is fundamentally qualified by language without spending much energy in the attempt to prove the point. The idea that scientific theorising is essentially a linguistic type of activity seems to me quite problematic. This solution overlooks the difference between rationality and signification and does not consider the possibility that scientific theories, axioms or statements may rather be regarded as logically qualified artefacts (cf. Stafleu, 1981; 1982).

The whole approach seems to rest on the linguistic turn emerging at the end of the 19th century and positing the language (i.e. sign-) aspect of reality as the most fundamental one. In this context, the main problem remains to know why the language mode should be given such a privileged position. As a matter of fact, other modes can be elevated to the same position. In contemporary philosophy of science, for example, we went already through a logicist turn (logical positivism and Russell), a historical turn (Kuhn and the "historical school"), a sociological turn (e.g. Brown and Collins) and so on (see Botha, 1994).

8. D.F.M. Strauss

At this point I would like to mention the contribution by Danie Strauss, a South African representative of the Dooyeweerdian school of philosophy. Admittedly, his contribution is built on a whole tradition and it would be important to trace the roots of his demarcation criterion in the history of reformational philosophy. At the same time I believe Strauss' systematising of the demarcation issue is one of the most accurate in this school of thought and we can therefore focus on it directly. Unfortunately, I can only do so in the limited space still available, leaving to the reader the task of digging deeper into this approach.

According to Strauss, what distinguishes scientific from non-scientific thinking is the type of *focus*. Naive thinking focuses on concrete things, events and so on (the "what"). Scientific thinking, on the contrary, focuses on concrete reality from a certain perspective, a certain modal aspect (the "how"). What are modal aspects? Concrete reality can be observed through fifteen aspects (see e.g. Dooyeweerd, 1984, 2:1-318) which are modes of reality and modes of our experience at the same time. Now, the scientist or scholar is busy with physical science when looking at reality through the physical aspect. The mathematician is busy with scientific theorising when he looks at reality via the numerical aspect. The same is true for the biologist or the legal scholar. Looking at reality through one (in some cases more than one) modal aspect, implies that a specific dimension of phenomena, data and laws are analysed. Analysis includes both abstracting a certain "object" of study and disregarding the rest. Analysis is the "core-meaning" of the logical aspect or mode of reality and experience.

Let us return for a moment to non-scientific thought. The latter is also a form of knowledge and is also rational, but it focuses on concrete events, properties and things. In this process we don't make abstractions along modal borders. Does it mean that abstraction is totally absent from naive knowledge? Clouser (2005:64) observes that when we are for example looking for a green book on a shelf, we focus on the colour while searching among the books. In this respect Clouser speaks of different levels of abstraction, more precisely of "low and high abstraction". The problem is: if abstraction is not a unique characteristic of science, can it still function as demarcation criterion? One might still posit "high abstraction" as the unique characteristic of science, but what is precisely the difference between high and low abstraction?

Strauss, however, has identified, I believe, the correct criterion. Scientific and non-scientific thinking adopt two different types of abstraction: entity/ary abstraction (in the case of naive thinking) and *modal abstraction* in the case of scientific thinking (Strauss, 2009:145; 2001:29-30).

Abstraction in itself is not typical of science. Strauss (2009:15) gives the example of a child "abstracting" the different characteristics of a bird (beak, tail) and later on identifying different types of birds on that basis. This type of abstraction "lifts up" certain entities (wings, feathers, etcetera). We can therefore call it entitary abstraction. Science, on the other hand is busy with *modal abstraction* in the sense that it looks at animals, plants or anything else via modal aspects. We can look at animals, for example, via the biotic aspect, or the historical, or the juridical aspect.

This criterion has some consequences for the question: which disciplines are scientific? Finally (I would say) in this approach an old prejudice concerning the superior scientific status of the natural sciences with regard to the humanities is abolished. It might be true that the natural sciences deal with "laws" and other sciences deal with "norms" (Strauss, 2001:33 ff.). It might be true that norms can be transgressed to a much larger extent than laws. Norms like justice require a "positivisation" by human beings, while "natural" laws are valid independently of human intervention. Nevertheless, all modal aspects serve as points of entry for scientific explorations of the world in which we live, and as a consequence the disciplines using those points of entry are to be regarded as having scientific status. This is equally true of mathematics, physics, history, law or theology.

9. Conclusive remarks

Answers to the question "what is science?" require an elaborate background in ontology and epistemology. It may sound surprising that a relatively small school of philosophy has provided such a valuable answer to an issue which has vexed Western philosophy such a long time. I am mentioning the whole school because, although Strauss deserves recognition for his specific contribution, he has of course profited from a long tradition of reflection on this issue and on that basis he has refined and sharpened a clear-cut response. In a next article (Coletto, 2011) I will explore the long process leading to the elaboration of a demarcation criterion in reformational philosophy.

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