Chapter 6
Theories About the Development of Science

'Anything goes.'
P. Feyerabend.

6.1 Introduction

The debate about the development of science is a debate about scientific rationality. The central question is whether there is any rationality in science, and if so, what that rationality could be. This question has long been discussed in one form or another.

Just after the First World War, this debate got a new focus in the work of the Vienna Circle. The starting point for this group was a general distrust of speculative metaphysics. Questions about reality's true nature, what lies beyond observable things – which were the traditional metaphysical questions – were considered meaningless.

Today, no one accepts the Vienna Circle's more specific doctrines; but even so, one may say that it had a large impact on the philosophical discussion about science. The analysis and critique of its fundamental points led to a fruitful development of concepts and methods within the philosophy of science. In this chapter I shall give an overview of four of the main positions on this topic: the Vienna Circle's program of logical positivism, Popper's falsificationism, Kuhn's theory of paradigms and scientific revolutions, and Lakatos' theory about research programs. I shall also give a short overview of Feyerabend's theoretical anarchism, and, finally, I shall propose a refinement of the concept of scientific rationality.

6.2 Logical Positivism

Beginning around 1920, a group of philosophers, sociologists, physicists and other academics met regularly in Vienna to discuss questions within science. This group, the Vienna Circle, focused their interest on the question of what is special about science. The natural sciences, especially physics, were taken as the ideal, with their

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great strides forward, while philosophy, and in particular metaphysics, trod much the same well-worn path it had been treading for over 2000 years. The Circle thought that one could establish a criterion that science usually fulfilled, but that speculative metaphysics did not: the *verifiability criterion*, which states that a sentence is meaningful if and only if one can describe a method to verify the truth of the sentence. (If it proved to be false, one would then have a method for showing that its negation is true.) It was claimed that many traditional metaphysical propositions, often the topic of philosophical discussion of the day, did not meet this criterion; and consequently, were strictly meaningless. In contrast, scientific propositions invariably did meet this criterion and were thus meaningful.

This criterion is a semantic criterion. It was an important part of a general theory of linguistic meaning to which the Vienna Circle subscribed. The main theses in this theory are that there are only two types of meaningful sentences; *analytical sentences*, which are true or false in virtue of the meaning of their words, and *synthetic sentences*, which are true if they are verified and false if their negations are verified. If a sentence cannot be placed in either of these categories, it is meaningless; they held this to be so even if it is syntactically well formed and where we believe we understand its meaning.

According to the Vienna Circle, how do we verify synthetic sentences? First, they thought that one could draw a sharp line between theoretical and observational sentences. Obviously, or so they thought, observational sentences are verified by making observations, the Vienna Circle saw no deep problem with this. However, the verification of theoretical sentences required a more worked out methodology.

The theoretical sentences in which the Vienna Circle had an interest were generally law-like sentences of the form ‘all objects of type A have property B’. It was thought that such sentences are verified via an inductive method. An inductive method is a method that gives the conditions under which it is correct to conclude a general sentence (e.g. all swans are white) from singular sentences (e.g. swan 1 is white, swan 2 is white, etc.). This conclusion of a general law from individual cases was deemed legitimate if the following conditions are fulfilled:

- One has observed a large number of individual cases of the presumed law.
- One has not observed any counterexamples.
- One has observed these cases under many different circumstances.

It is obvious that the theoretical laws accepted as such in the natural sciences normally fulfil these conditions. However, these conditions are not very precise. There are two obvious problems: (i) exactly how many observations are required to say that a law is verified, and (ii) since one cannot, or should not, worry about all types of circumstance, which circumstances are the relevant ones? For example, suppose that on a winter day one observes that all the swans in Stockholm’s waters are white. Can we now draw the conclusion that all swans are white? Before we do, it is reasonable to require that one investigate other places at different times of the year. On the other hand, it seems unreasonable to require that one take into account the weather or whether the king happened to be home at the Royal Castle as relevant circumstances. This is because we are convinced that these factors are not relevant to the colour that a bird's plumage takes. But how do we know? This knowledge is supposedly based on a general theory of causal connections in nature, according to which such factors cannot influence the colour of birds. However, this is theoretical knowledge that we have come by through other verifications, which, in turn, must also have met all three criteria above. Thus, we have gone round in a (Viennese) circle.

The Vienna Circle faced another difficulty in regards to some very general statements in science, such as the principle of the conservation of energy in closed systems. How does one verify such a statement? It is so general that it applies to absolutely every system, and so one begins to question whether it can be verified at all. But if such verification is impossible, then the principle of the conservation of energy must be a metaphysical sentence lacking meaning. This is not a reasonable conclusion. Thus, we are rather motivated to conclude that there is something wrong with verifiability as a criterion of meaningfulness.

The Vienna Circle, especially Rudolf Carnap,[1] reacted to this difficulty by changing the way they looked at theoretical sentences. Their view, developed in the 1930s, was that theoretical sentences are strictly meaningless, since they do not meet the verifiability criterion, and thus they have no truth-value. However, they are still used in science as logical tools for drawing conclusions about future observable events. The idea is that one first makes observations, formulates observational statements that are true and hence meaningful. Then, with help of correspondence rules and theoretical sentences (laws), one can logically infer new observational statements that describe situations not yet observed. Theoretical sentences in themselves do not claim anything. The theoretical terms we use do not refer to any unobserved objects. Theoretical sentences are mere instruments for prediction, which is why this view is called *instrumentalism*.

A necessary condition for holding this view is that one can maintain a very sharp distinction between theoretical and observational sentences, as they have completely different semantic properties. It is not enough to say that there is a vague line between theoretical and observational sentences, since this immediately raises the question as to whether sentences in the vague area have truth-values. This difficulty was one that the Vienna Circle could not satisfactorily solve.

Despite these glaring difficulties, this was 'the received view' up until the early 1960s. It is called logical positivism, or logical empiricism, and can be recognized as *verificationism in semantics, instrumentalism in regards to theories, and inductivism in regards to methodology*.

[1] Rudolf Carnap, (1891–1970), was born in Germany, but 1935 he moved to USA for political reasons. Carnap is the leading representative for logical positivism and heir to Frege, Russell and the early Wittgenstein.

6.3 Falsificationism

Karl Popper worked concurrently with many Vienna Circle members and sometimes participated in their meetings in the early 1920s. He agreed with some of their critical points in regards to metaphysical speculation and saw that it was important that one give a correct criterion for what can be called science. He was also critical of many popular theories of the time such as psychoanalysis and Marxism, whose supporters claimed were scientific theories. Popper argued that these supporters were never prepared to truly and unconditionally test their theories. However, Popper, as opposed to the members of the Vienna Circle, rejected any use of inductive reasoning in science. He argued that Hume had once and for all shown that the problem of induction was unsolvable. Hume's argument goes as follows. We often use inductive reasoning in science; we observe a number of events of type A each followed by an event of type B, then we generalise and conclude: all A-events are followed by B-events. In what way can this method be justified? It is obvious that this inference is not logically valid, so logical justification is out of the question. Then perhaps one might say the following: throughout the ages, we have found that inductive reasoning has quite often proved correct, even though one cannot claim any guarantee that the conclusions are true. Hence, this justifies further use of induction. But this argument is itself an example of induction; we have used induction in order to show that induction is a valid method, thereby assuming that which we set out to prove. Hence, we cannot justify induction using either our experience or logical arguments. But, then, what other method of justification is left? Nothing! For this reason, Hume argues, induction cannot be justified. Hume accepted that we humans naturally use inductive reasoning, in everyday life and in science, without justification, but Popper did not accept that. He held that science should be built upon rationally motivated modes of reasoning and hence we should reject all forms of induction.[2]

Popper's alternative was to build scientific reasoning on logically valid principles such as Modus Tollens. That is to say, if we infer an empirical consequence from a number of hypotheses and this consequence proves to be false, then we have a logically valid argument for the falsity of one or more of the hypotheses (See Chap. 3 and Appendix).

According to Popper, we are never justified in stating that we have proved some hypothesis, but we can say, under certain conditions, that we have conclusively falsified some hypothesis. This is the core of Popper's falsificationism. His methodology can be summarized in the following way:

- Put forth a hypothesis.
- Derive the consequences.
- Test the consequences and determine whether they are true of false.

[2] One should keep in mind that mathematical induction is quite another matter: it is an axiom for natural numbers.

- If any of the consequences are false, reject the hypothesis.
- If none of the consequences is false, then provisionally accept the hypothesis.

This is largely similar to the hypothetical-deductive method described in Chap. 3. However, there are some differences between Popper's view and the view I earlier expressed. In Chap. 3, I wrote, in agreement with most philosophers, that if the empirical consequences prove to be in agreement with experiment and observation, then one could say that the hypothesis has been strengthened. This strengthening can be interpreted as an increase of its believability, or equivalently, of the probability of it being true. This conclusion is inductive, since we argue from present knowledge to future unknown events. If the hypothesis is strengthened, then it is more probable than it was before the test that the next test will agree with the hypothesis. But Popper cannot claim that a hypothesis that has passed many tests has been strengthened, for then he would have to accept inductivism. Instead, he says that the hypothesis is *corroborated*. According to Popper, a theory is well corroborated if and only if it has survived many tests. It does not follow from the proposition that a hypothesis is well corroborated that the hypothesis is highly probable, or even that it is probable that the next attempt at falsifying it will fail.[3]

Popper also argues against all claims that a hypothesis is more probable after successfully passing a test with the following argument: Every general hypothesis (e.g. 'All swans are white') has a very large, and sometimes infinite, number of empirical consequences. The probability of a hypothesis is the quotient of a number of positive instances over the number of possible instances. If the number of possible cases is infinite, then it makes no difference how many observations and experiments with positive outcome one makes, because the quotient will always be zero. Thus, no finite number of tests can make a hypothesis more probable than if it had not been tested.

The question of how one 'discovers' good hypotheses cannot, and needs not, concern scientific methodology. Popper drew a sharp line between 'the context of discovery' and 'the context of justification'. According to him, there is no method for discovering or inventing new ideas since if there were, the results of such methods would not be genuinely new ideas or discoveries. What need justification are the conclusions drawn from such inspirations.

One problem with falsificationism is how it should treat the thesis that all observational statements are theory-dependent. This problem is related to the following: in order to say that one has falsified a hypothesis one must be able to say that the observation on which the falsification is based is irrefutable. However, if this observation, in turn, depends on the truth of certain theories, such as theories about our instruments or cognitive mechanisms involved, then one can ask whether it is the observational statement or the tested hypothesis that is false. But this means that we can never tell whether a hypothesis has been conclusively falsified.

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[3] One should be well aware that Popper here uses the word 'corroborate' in his own technical sense. The usual meaning of 'corroborate' is 'support', or 'confirm'.

Popper's answer to this objection was to accept that observations are theory-dependent, whilst maintaining that before we even begin to test our hypotheses we should state what results we are prepared to accept as refutations. Thus, we are to point out a number of so-called base propositions that should not be questioned in a test situation (e.g. that certain instruments are reliable). Popper means that someone who is not prepared to make this methodological decision cannot be said to have a rational, scientific perspective. Obviously, this decision is not made once and for all; base statements, like all other scientific statements, should be open for critical discussion.

In my opinion this stance is entirely reasonable. However, for Popper, there is still the problem that he cannot consistently claim that it is possible to conclusively falsify a hypothesis. What he can say is that, *given* that we choose to treat a number of statements–those necessary for deriving consequences from a hypothesis–as true, we must *hold* that a hypothesis whose observational consequences conflict with observations is false. This is quite different from saying that the hypothesis in question has *been* falsified, since this would mean that we are certain that we know that it is false, and not just that we take it to be false, conditional on holding other propositions true.

Furthermore, there can be reason to point out that even the recognition of the need to make a methodological choice undermines Popper's strong requirements on rationality. For, on what grounds should we make the methodological choice of base propositions? Take, for example, the choice to consider a certain instrument to be reliable. Why should this choice be rational? Popper can hardly claim that it is based upon experiences of previous experiments, as this would be to use induction. Neither can a purely logical argument suffice, since logic alone cannot justify propositions about empirical facts. But then there is no justification at all. Hence the choice of base statements is, according to Popper's own norms, just as unjustifiable as every inductive inference.

In my view, Popper's falsificationism (he himself called his position 'critical rationalism') is not any real alternative to inductivism, since either he rejects all induction, thus making it impossible to motivate his methodology, or he accepts certain inductive inferences, which is essentially a variant of inductivism.

I think that induction is and will be an important part of all scientific thinking that aims at something more than merely describing events that have already has occurred. Popper's treatment of rational thought as being on par with strict logical thought and his simultaneous requirement for strict rationality in science results in science being reduced to pure logic, which, of course, is absurd. Nevertheless, Popper's methodology contains many reasonable points. As the reader surely has noticed, I have placed significant weight on the hypothetical-deductive method, and Popper should be given the honour of being one those who have worked hardest to spread this particular scientific method.

6.4 Normal Science, Scientific Revolutions and Paradigm Shifts

In 1962, Thomas Kuhn[4] published a book, *The Structure of Scientific Revolutions*, which quickly arose great interest among philosophers and other scholars. In his book, Kuhn presents his famous theory on the development of science as consisting of periods of normal science interrupted by scientific revolutions. The main concept in this theory is that of a *paradigm*, which has been widely dispersed, and has since come to be used in ways that Kuhn never intended. Thus, we must keep in mind that Kuhn's concept of a paradigm has a somewhat different meaning than is nowadays attributed to it, but more on this later.

Kuhn described the development of a certain science in the following way. In the beginning there was chaos; no established results to start with, no clear methods to apply. One did not even have a particularly well-described field of research or a list of problems to be solved. All researchers started from scratch with a fundamental philosophical/conceptual discussion and, since no results were indisputable, there was no foundation on which to build meaningful development. This phase is what Kuhn calls the pre-scientific stage.

Sooner or later a breakthrough occurs. Someone succeeds at solving a problem and everyone considers this success to be a clear step forward. Others then try to apply the same concepts, methods and techniques to solve new problems. If these attempts are generally successful, then one has taken oneself out of the pre-scientific stage and into a stage of normal science. No longer do researchers have to start with conceptual investigations, but can begin by arguing from the concepts, methods and results that others have already established. One is able to continue to build on this common ground taking ever more steps forward. This period of normal science is characterized by Kuhn as researchers working within a *paradigm*, where a paradigm is a complex of results, concepts, norms and assumptions. In his book, Kuhn used the word paradigm in many different ways. In fact, one critic counted 21 different definitions! In response to this critique, Kuhn, in the appendix to the second edition of his book, replaced his concept of a paradigm with that of disciplinary matrix, consisting of the following four components:

- Symbolic generalizations
- Metaphysical assumptions
- Methodological norms
- Exemplars

The last component, exemplars, is close to what Kuhn originally apparently intended by the word 'paradigm', and it is now generally accepted that the word 'paradigm' in a narrow sense refers to exemplars of successful problem-solving.

[4] Thomas Kuhn, (1922–1996) was an American philosopher and historian of science.

An example may illustrate the meaning of these terms. Newton's *Principia* was immediately considered a breakthrough in mechanics, the science of bodily motion. It quickly became a paradigm for both mechanics and physics in general, and remained so for more than 200 years.

The symbolic generalizations in this normal science are Newton's laws. Among the fundamental metaphysical assumptions, we find Newton's assumptions about absolute space and absolute time. Another quite important metaphysical assumption is determinism: the assumption that Newton's laws, together with a set of initial conditions, determine all bodily motion.

Among the methodological norms, we can name the norm that hypotheses should be strengthened through systematic experimentation (though Newton probably had a somewhat different view in this case) and that physics should use mathematics in order to make numerically precise propositions about the motion of bodies.

There are many prime exemplars in *Principia*, such as Newton's analysis of lunar motion as a continual free-fall caused by the earth's gravitation and his analysis of the motion of the tides as an effect of gravitational forces from the moon and the sun.

Once a paradigm has been established in a discipline, it functions as a general conceptual framework. Researchers are more or less unconscious of the framework and its governing power. The paradigm contains the concepts and assumptions that researchers take to be so obvious that they never reflect upon them or consider their justification. The common reaction to a researcher who questions any part of the paradigm, by their colleagues, would be to question his or her ability or judgment rather than questioning the paradigm. This reaction is even more pronounced where the critic is not a researcher within the paradigm in question. Consequently, and this is Kuhn's point, as long as normal scientific activity is reasonably productive, the paradigm is never questioned.

Working within a normal science may appear to be a trivial and unimaginative activity, but such is not the case. Creativity and fantasy play a significant role, but that role is defined by the paradigm.

Sooner or later the researchers of a normal science run into some difficulties. Kuhn claims, quite reasonably, that unsolved problems always exist in all sciences, but as long as these problems are not too many, or too insistent, then they are not viewed as grounds for doubting the fundamental assumptions of the paradigm. However, if the number of unsolved problems–which Kuhn calls anomalies–become sufficiently serious and numerous, researchers in the field will begin to question the fundamental assumptions. One becomes aware of certain aspects of the paradigm in question, and in light of the accumulation of anomalies, one begins to question these aspects. The result is that the discipline enters a crisis. This, in turn, is a precondition for a scientific revolution and for throwing out one paradigm for another. The new paradigm may be the conceptual framework for a new period of normal science.

Kuhn's most controversial thesis is that different paradigms within a discipline are *incommensurable*, or incomparable. Kuhn has two arguments for this thesis.

The first is that different paradigms use different concepts in describing a research field. The meaning of these concepts is decided holistically: each concept's meaning depends upon its relation to other concepts in the paradigm. This view is called *semantic holism*. Semantic holism applies even to the concepts that we use to describe our observations, which means that all observations are theory-dependent. This, in turn, means that two propositions claimed within two different paradigms cannot in general be said to be about the same thing; and therefore, can never be said to be in conflict.

The second argument builds upon the fact that norms are part of paradigms. If one is to objectively compare two paradigms, then one must have some criteria or scale against which we measure concurrent paradigms' relative strengths and weaknesses. However, using such criteria is to apply methodological norms and such norms comprise significant parts of paradigms. If the methodological norms of the two paradigms are different, we cannot give an impartial verdict, for it is obvious that we have (partly) already chosen a paradigm when we choose which methodological norms are acceptable in comparing the two paradigms. One can metaphorically describe this situation as there not existing a neutral ground on which one can stand and impartially compare to concurrent paradigms' strengths and weaknesses.

However, if one cannot compare two paradigms, one established and the other new, then how do we choose between them? Apparently, one cannot make a rational decision based on a comparison of advantages and disadvantages from a neutral point of view. Yet, the scientific community, the active researchers within a discipline, do make a choice. According to Kuhn, this decision must be based on external factors such as 'political correctness', personal sympathies and antipathies, the possibility of getting funding, etc. The main point here is, given one's goal is to make scientific progress, there is no rational way to make a choice such that one optimizes the chances of attaining that goal, if Kuhn's incommensurability thesis is correct.

Kuhn's critics think this is absurd: if science is not the pattern of rational decision-making, then nothing is. Thus, there must be a rational methodology in science and Kuhn must be wrong.

Kuhn did not fully accept this consequence that paradigm choice is irrational. He maintained that his theory did not lead to irrationality. In my opinion however, maintaining that science is fully rational is nigh on impossible, if one accepts, as Kuhn does, that two different paradigms are incommensurable.

Fortunately, one can question the strength of the premises on which the incommensurability thesis is built. In my opinion, Kuhn has greatly exaggerated the extent and implications of semantic holism. For instance, Quine–who accepts a strong form of semantic holism–has not drawn the conclusion that it is impossible to compare and choose between theories. As discussed in Chap. 5, Quine thinks that there are theory-independent statements that, in a certain sense, constitute the foundation of science. Thus, he maintains that one can objectively compare theories at this level. Despite this difference, Quine agrees with Kuhn that holism implies that individual statements, or sentences, in a theory cannot be tested in isolation, but that one must test and compare entire theories.

Several philosophers, less holistically inclined than Kuhn and Quine, have used Frege's distinction between a term's meaning and reference. Using that distinction one may accept that a term has changed meaning, while maintaining that its reference remains, and this enables comparisons between paradigms.

Even the second argument is strongly exaggerated. Many of the paradigm shifts that Kuhn discusses in his book are not such that the scientific norms have changed to any significant extent. In most cases, when a scientific revolution and paradigm shift occur, the major norms remain unchanged in regards to, for example, the value of accurate predictions. This is why one often can compare new and old paradigms.

In summary, Kuhn's arguments for the most controversial thesis in his theory, the incommensurability of paradigms, are weak and provide insufficient support. Consequently, there is no basis for claiming that the conclusions drawn during scientific revolutions are as irrational as Kuhn's theory suggests.

Kuhn has in later publications[5] developed and modified his views about scientific change, incommensurability, paradigms and rationality, but these writings have been less influential.

6.5 Lakatos' Theory of Research Programmes

Imre Lakatos[6] succeeded Popper as professor of philosophy of science at the London School of Economics and was a follower of Popper in regards to his stance concerning questions about scientific development.

Lakatos called his theory 'methodology of scientific research programmes' MSRP for short. One can see this theory as an attempt at saving as much as possible of Popper's view of science whilst at the same time taking into consideration the objections raised against it. In some respects, Lakatos was also influenced by Kuhn's ideas on scientific revolutions, though he did not accept Kuhn's incommensurability thesis. Lakatos – like Kuhn, but unlike Popper – was strongly oriented towards the actual, historical development of science in developing his methodology.

Lakatos had two starting points from which he constructed his theory of research programmes. The first is the observation that certain large scientific breakthroughs are actually best characterized as verifications and not falsifications, as Popper had claimed. Characteristically, these verifications are such that two theories are compared using a decisive experiment. Lakatos illustrated his thesis with Eddington's observations of the deflection of light at the sun's edge, which was carried out during the solar eclipse of 1919. During a solar eclipse, one can see the light from

[5] Collected in his Kuhn (2000).

[6] Imre Lakatos (1922–1974) was born in Hungary, but fled to England in 1956.

stars that pass near the edge of the sun, on its way to earth. Einstein's general theory of relativity predicted that the sun would bend this light as it passed by the sun. The results of Eddington's observations agreed with relativity theory and conflicted with classical mechanics. According to Lakatos' interpretation, it was clear that this episode verified relativity theory while at the same time falsifying classical mechanics.

Lakatos' second starting point is that one neither can, nor should, evaluate a single theory in regards to a fixed point in time; rather, all that one can evaluate are tendencies in the development of a series of theories (i.e. a research programme). The deciding question is whether the research programme is progressive or degenerative. In order to determine which is the case one must adopt a historical perspective.

According to Lakatos, a research programme consists of a series of related theories. The common core of such a series is called the *hard core*. This concept is quite similar to Kuhn's concept of symbolic generalizations. Furthermore, there is a *protective belt*, which consists of assumptions about measuring instruments, observation conditions, background assumptions, etc. The purpose of the protective belt is to provide a buffer between the hard-core and recalcitrant experimental results; instead of saying that a result shows that the hard core is false, one says that some assumption in the protective belt is false. This principle is a heuristic principle, the negative heuristic, which Lakatos formulated as a categorical prohibition; do not use a Modus Tollens argument against the hard core! Finally, there is also a *positive heuristic*, which consists of guidelines for the construction of new theories in the research programme. Thus, a research programme has four components:

- The hard core
- The protective belt
- The negative heuristic
- The positive heuristic

Lakatos' basic methodological rule is the following: give up a degenerative research programme in favour of a progressive research programme. A degenerative research programme, according to Lakatos, is characterized by theory development that occurs primarily in response to empirical difficulties; one finds new alternatives, exceptions, or reinterprets results in order to explain why the original theory no longer agrees with the observations. A progressive research programme, on the other hand, is characterized by theory development that pre-empts experimental results. Theories in the research programme have inherent possibilities for further development, which able researchers take advantage of prior to experiments taking place to test these possibilities.

One cannot reasonably say that one should give up a research programme at the first sign of difficulty. That would be too rash. Therefore, Lakatos argued that the methodological rule of throwing out a degenerative research programme should be supplemented with the regulation that one should give a research programme that has come into a degenerative phase a certain amount of time to regain its force and once again become progressive. This immediately triggers the question, how long time? Lakatos gave no answer to this question, which leads one to concur with the following critique, as formulated by Feyerabend: Lakatos has not, in all practicality, given us any methodological rule to follow. Using Lakatos' rule, every conclusion can be defended as rational, which means that he has not succeeded in distinguishing between rational and irrational methods. Without breaking Lakatos' rules, one can both claim that it is rational to give up a research programme because it is degenerative and that one should stick with it because it is in a phase of temporary difficulty. Furthermore, there are well known historical examples that show that it can take several decades before one is able to overcome temporary difficulties. As a result, Feyerabend characterized Lakatos' methodology as 'verbal ornamentation'.

A response to this critique can be reconstructed from Lakatos' remarks and general positions (since Lakatos died in the midst of this debate he did not himself respond) in the following manner: The theory about research programs is not primarily intended as a methodological rulebook for on-going research. Instead, it is to be seen as a methodology for *rational reconstruction* of the history of science. When we look back at what has happened in the history of science, it is possible, according to Lakatos, to describe the development of events as a rational process, since the conclusions made in practice fit his model. When programmes were abandoned, it was because they had been in a degenerative phase for some time, and when programmes were not abandoned, it was because one was trying to overcome temporary difficulties. *With hindsight,* we can say that large parts of the history or science are rational processes. However, one cannot expect that individual researchers would be more rational than other people would. Researchers – like most people – indulge in wishful thinking, get caught up in impossible views, and let short-sighted career interests influence their conclusions, etc. However, this does not take away from the rational character of their *collective activity*.

Lakatos is, in contrast to most in the debate about scientific development, a methodological collectivist (see Sect. 9.1) in that he thinks one should explain each individual's decisions and beliefs in terms of objectively acting forces. The general idea of methodological collectivism was first proposed by Marx and in his theory these objectively acting forces are social in nature, (in Marx' theory they are the conditions for production) but when talking about scientific research one can interpret them as nature exerting a selective pressure on our perceptions, decisions and ideas. Individual researchers are not more rational than other people, but the research community is influenced, as a whole, by objective reality; and hence, the scientific community now and then changes research programme. Lakatos' concept of rationality should thus be used primarily at the collective level. The critique of Lakatos that he has not given any solid rules that an individual researcher can follow in choosing between research programmes misses the point, for this was never Lakatos' goal.

Let me stress once more that Lakatos has not explicitly expressed these points himself; rather, this is an interpretation of his position. It is Ian Hacking in particular (in his book *Representing and Intervening*) who has pushed this interpretation. I find it plausible since Lakatos, as a former communist, was strongly influenced by Hegel and Marx, and both of these thinkers were methodological collectivists.

6.6 Methodological Anarchism: Anything Goes

Paul Feyerabend[7] participated intensely in the debate about the scientific method and adhered to essential parts of Kuhn's view. In particular, he wholly accepted the incommensurability thesis. Though he went one step further than Kuhn in criticizing the popular philosophical position that what we know to be science employs a single identifiable method and the success of that method explains scientific progress. He has partly a descriptive and partly a normative critique of those, like Popper, who maintain that there is only one general methodology that qualifies as scientific. His first point concerns the history of science. Each proposal for a scientific method hitherto considered has, in practice, often been neglected or actively rejected by active researchers, and yet these researchers have managed to make progress. Feyerabend claims that if researchers were to accept any one of the philosophers' proposed methods, it would hinder scientific progress.

Feyerabend's way of looking at science is quite similar to the ultraliberal economist's view of capitalism and market economy. These economists claim that the most effective way to promote a rapid increase in wealth is to abolish all, or most, regulations and let competition in the market take care of itself. The products and services that best satisfy the customer's desires will be produced, and every attempt to direct the market will diminish its effectiveness.

Feyerabend argues that the same goes for research goals and methods. He even argues that the state should stop favouring activities traditionally called 'research'. One should allow all possible traditions of acquiring 'knowledge' such as Voodoo, astrology, crystal therapy, homeopathy and the like, to use a part of societies' resources for equal competition. This will speed up scientific development and increase the chances of dispersion of radically new insights. Finally, Feyerabend argues that there is no reason to accept the traditional view that knowledge, understood in the traditional way, leads to a happier life for human beings. In a truly liberal society, everyone has the right to decide his or her life's goal. So, what would the majority prioritize if they could choose? Perhaps they would not want to promote science? In short, the only methodological rule that Feyerabend accepts is that there is no binding methodological rule; *anything goes.*

Some of the Feyerabend's wording bears traces of the rhetorical figures and ideological debate that was prevalent, especially at universities, during the 1960s and 1970s. For my part, I find his analysis of the history of science quite relevant

[7] Paul Feyerabend (1924–1998) was born in Austria, but was mostly active in USA.

and pertinent. Without having investigated the issue more systematically, it strikes me as an accurate observation that science has made progress without consciously following any methodological rules. It is also clear that no hitherto proposed rules have been simultaneously generally accepted and practiced; and yet progress has been, and continues to be made. Furthermore, I think that philosophers should be extremely careful when prescribing how one should act in order to be a rational, genuine scientist.

However, if one accepts the view that progress occurs if we let theories and research projects compete in the 'market of ideas', then one needs an explanation of what is meant by 'progress'; why one effort result is considered progress, but not another. Feyerabend, like Kuhn, is faced with difficulties regarding the concept of progress due to his belief in the incommensurability thesis. The reason is that in order to claim that progress has occurred it does not suffice to say that a new theory is different from an older one; that the newer theory represent progress presumes that one has compared the two theories with respect to some criteria that the new theory better satisfies. If a comparison has been made, it means that the theories are not incommensurable. I would even go so far to claim that it is self-contradictory to simultaneously hold that progress has occurred when one theory is given over for another and that those theories (paradigms, or research programmes) are incommensurable.

Feyerabend's stance that scientific knowledge, traditionally understood, does not lead to happier life for people in general, I strongly dismiss; it strikes me as absurd to deny that common people in the developed countries live a better life nowadays than, say 200 years ago, and that this is due mainly to scientific achievements.

6.7 Summary of the Debate

This chapter gives a short overview of twentieth century currents in the study of scientific methodology. The main issue is, and has always been, how we should characterize scientific methods in such a way that one can use the characterization to exclude from science all those activities that do not properly belong to science (superstitions, religious arguments, pseudoscience, etc.), and include in science all those activities that do so belong (physics, biology, astronomy, etc.). This formulation suggests that there is a tension between the normative and descriptive goals of philosophy of science. According to my grasp of the situation, Popper's biggest mistake was that he was far too normative and did not sufficiently take into consideration how science is actually performed. This mistake allowed his critics to question the relevance of his view. Kuhn, on the other hand, made the opposite mistake. He concentrated far too much on describing the development of science, which lost him the possibility of saying anything about how science ought to be carried out in order for it to qualify as science. Philosophical theories about the development of science must balance both of these aspects in order for them to have any relevance.

Accordingly, I propose that the starting point should be the hypothetical-deductive method and the related approach to scientific theories. A generally held characteristic of what 'scientific' means could be the following:

> To put forth hypotheses, infer testable consequences of those hypotheses, and to be prepared to reassess one's hypotheses if the consequences do not agree with observation.

This formulation is quite similar to Popper's falsificationism. The difference is that Popper more precisely claimed that a scientist *should* abandon a hypothesis if the consequences conflict with observation, whereas I think that such a formulation is far too categorical. There have been many cases in the history of science where one has stood by a hypothesis even though it seemed to conflict with observation. This strategy has shown itself to be correct on occasion and has generally been considered both scientific and sensible. It is also easy to cite basic arguments for such a stance. One argument is that verified observational statements are not unshakable truths. On any occasion, there could have been something wrong with the measuring instruments, or there might have been some unknown interference in connection with the observation. Another way of expressing the same thing is to point out the possibility that some auxiliary assumptions may have been false. Therefore, I think that one cannot categorically state that one should abandon a hypothesis as soon as it comes in conflict with observation, but rather one must make a comprehensive comparison of the probability that something is wrong with the hypothesis and the probability that something is wrong with the observations, and then conclude which seems the most reasonable. (Popper later modified his view, holding that observational statements, which he called base statements, can and should be subjected to critical inspection, just like the rest of a theory.) Adherents of pseudoscience would now be able to claim the following:

> There is no clear difference between our methods and your so-called scientific methods. We also put forth hypotheses that lead to observable consequences, and you are just as dogmatic when you stand by your hypotheses in the face of recalcitrant observations as we are. In practice, the only differences are the principles we believe in. Your attempts to demarcate what is scientific (namely, your own methods) are merely a privileged group's attempt to hold on to its privileges, its grants and society's appreciation. In a liberal democracy one should allow all people to do what they want, as long as it does not hurt others. Since proponents of so-called scientific methods have not succeeded in showing that these methods lead, in the long run, to more secure progress and better results, it is entirely possible that we would be able to make significant progress if only it were allowed that more ways of looking at reality were given the resources to investigate their inherent possibilities. (This is essentially Feyerabend's argument.)

This critique of established science cannot be immediately rejected. Unfortunately, the first argument–that scientists have not succeeded in presenting a sufficiently clear criterion for what science is–is true. My approach above is not precise enough to withstand the critique. The second argument also has a certain air of truth about it. If one is in academia, one is dependent on access to research grants, and it is natural that one would try to reduce the competition by claiming that potential competitors do not fulfil the requirements of science.

Nevertheless, I am convinced that there is a sizable difference between science and pseudoscience. There are certainly difficult borderline cases, but there are also clear-cut examples on both sides of this line. This difference has to do with the way a research collective evaluates and revises its beliefs. It can very well be that an individual researcher breaks with scientific norms, but this is not reason enough to reject an entire activity as unscientific. The relevant level at which to decide this issue is not at the individual level, but at the collective level. Three reasons for this are (i) what counts as scientific results is not what an individual researcher or research group claim but what those who are familiar with the subject, the researchers at the forefront of their field, accept as a whole, (ii) at the collective level, the individual researchers' private opinions regarding politics, ethics, religion, and their career interests will cancel each other out when it comes to the collective decision, and (iii) when faced with a conflict between hypothesis and observation, it is not the individual researcher's judgment that determines which conclusion will be drawn, but that of the entire research collective. For example, in the case of a conflict between theory and experiment, other researchers repeat the experiment under different circumstances in order to try to determine if the observations are at fault, or if the observations are correct and it is the hypothesis that requires revision.

Hence, I would argue that one could harbour certain expectations regarding the rationality of science, even though individual researchers are just as irrational as everyone else. The crucial question is what the institutional organization looks like. It is required, among other things, that there are multiple independent research groups working within the same subject area, that these groups have the possibility of sharing results and that these results are published and subjected to debate and critique by others within the subject area. In other words, there are reasons to believe that it is possible to define a concept of scientific rationality, even if it is impossible to give a sharp delineation between science and pseudoscience.

6.8 The Rationality of Science: A Model

William Newton-Smith has proposed a model of scientific rationality in his book *Rationality of Science*. It is intended as an analysis of theoretically interesting cases where researchers have abandoned an old theory for a new one. This change of beliefs is, according to Newton-Smith, a rational process if it fulfils the following criteria:

1. The scientific community has a goal specified by the model with its scientific activity.
2. Between two competing theories T_1 and T_2, one of them, say T_2, is closer to this goal, given the available evidence.
3. The scientific community realizes that T_2 is closer to the goal than T_1.

4. The scientific community rejects T_1 in favour of T_2 on the basis of the realization that T_2 is closer to the goal than T_1.

This model is nothing but a variation of the model for action explanations given in Chap. 8, this time applied to collective decisions and collective beliefs; and as I have previously stated, there is every reason to discuss scientific rationality at this level.

I have, however, a minor objection to this model. One cannot say that all activities that fit into this model are scientific, because the goals are not specified. For example, if one were to plug in for the goal an increase in profitable activity, this model would apply to sales-oriented companies. (Theories would then become systems of assumptions about business plans, markets, etc., and the scientific community would become upper management.) What is specific to science is, I would say, the search for truth. (This is not explicit in Newton-Smith's view.) Many would try to fend off such a solemn formulation, preferring formulations such as to explain what happens, to predict what will happen, to find a theory that is empirically adequate (van Fraassen), or to find useful theories. In my opinion, all of these formulations strike me as unsatisfactory on the basis that they do not sufficiently distinguish science from other activities that should not reasonably be called science.

Point 3 above assumes that T_1 and T_2 are comparable with respect to how close they are to the goal. This is a notoriously difficult problem in the philosophy of science where one takes the goal to be truth. Popper tried to define closeness to the truth, *verisimilitude*, in terms of sets of true and false sentences in the theories in question. This attempt was a capital failure, and many of Popper's critics have taken this to indicate that Popper's philosophy is bankrupt. Regardless of whether or not this conclusion is justified, I believe that no one who wants to discuss scientific methods and progress can do so without an ability to judge the relative merits of competing theories. Of course, one can say, formally, that a theory, which contains a single false sentence, is false and should be abandoned. Consequently, as it is likely that all theories we can come up will be marred by some falsities, so all scientific theories will prove equally false and so equally worthy of rejection. This is not a reasonable stance, there are better and worse theories and a bad theory is better than no theory at all. Ilkka Niinilouto has discussed this issue in depth[8] and has defined a concept of truth-likeness in terms of quantitative differences between theoretical predictions and observations. This seems to be an intuitively reasonable way to go.

Finally, point 4 is a clear expression of an internalistic position. If it were the case that the relevant scientific community abandoned one theory in favour of another on the basis of other reasons than that it is closer to the goal (e.g. political, economic or religious reasons), then we would not say it was a rational step, even if the decision could afterwards be motivated by a high

[8] Ilkka Niiniluoto (1987). *Truthlikeness*. Dordrecht: Reidel.

probability of fulfilling the goal (e.g. acquiring large research grants for active researchers). This is entirely analogous to how we view an individual person's actions. If someone who reads in a horoscope that he/she could have success in financial dealings becomes inspired to buy a lotto ticket and wins millions, we would still hardly say that this action was rational even if the action, given the agent's individual goals, led to a high degree of goal fulfilment.

Exercise

Below is a fictional dialogue between Doctor Faustus, the medieval scholar who, according to a classic German legend, sold his soul to the devil for knowledge, and a present-day physics student. Perform a science-theoretical analysis of Faustus' argumentation!

Friction Dialogue

Participants:

Doctor Faust, a medieval scholar in league with the devil.

Pelle, a present-day physics student.

F You are always talking about friction. How do you know that it is friction that stops a ball from rolling and not demons?

P I do not believe in demons.

F I do.

P Anyway, I do not see how demons can make friction.

F They just stand in front of things and push to stop them from moving.

P I cannot see any demons even on the roughest of table.

F They are too small, also transparent.

P But there is more friction on rough surfaces.

F More demons.

P Oil helps.

F Oil drowns demons.

P But if you polish a table there is less friction and balls roll farther.

F You are wiping the demons off; there are fewer to push.

P A heavier ball experiences more friction.

F More demons push it; and it crushes their bones more.

P If I put a rough brick on the table I can push against friction with more and more force up to a limit, and the block stays still, with friction just balancing my push.

F Of course, the demons push just hard enough to stop you moving the brick; but there is a limit to their strength beyond which they collapse.

P But when I push hard enough and get the brick moving, there is a friction that drags the brick as it moves along.

F Yes, once they have collapsed the demons are crushed by the brick. It is their crackling bones that oppose the sliding.

P I cannot feel them.

F Run your finger along the table.

P

Friction follows definite laws. For example, experiment shows that a brick sliding along the table is dragged by friction with a force independent of velocity.

F Of course, same number of demons to crush, however fast you run over them.

P If I slide a brick along the table again and again, the same friction is the same each time. Demons would be crushed in the first trip.

F Yes, but they multiply incredibly fast.

P There are other laws of frictions: for example, the drag is proportional to the pressure holding the surfaces together.

F The Demons live in the pores of surface: more pressure makes more of them rush out to push and be crushed. Demons act in just the right way to push and drag with the forces you find in your experiments.

Adapted from Eric M Rogers*: Physics for the Inquiring Mind*.

Further Reading

Feyerabend, P. (1988). *Against method*. London: Verso.

Hacking, I. (1983). *Representing and intervening*. Cambridge: Cambridge University Press.

Kuhn, T. (1962). *The structure of scientific revolutions*. Chicago: Chicago University Press.

Kuhn, T. S. (2000). *The road since structure: philosophical essays, 1970–1993*. Chicago: University of Chicago Press.

Lakatos, I., & Musgrave, A. (Eds.). (1970). *Criticism and the growth of knowledge*. Cambridge: Cambridge University Press.

Newton-Smith, W. (1981). *Rationality of science*. London: Routledge.

Popper, K. (1992[1959]). *The logic of scientific discovery*. London: Routledge.