CRITICAL RATIONALISM IN THE TEST TUBE?*

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SUMMARY. Popper's critical rationalism is widely accepted under scientists and philosophers of science as a proper method for the reconstruction of scientific theories. On occasion of the application of the Popperian ideas for the reconstruction of chemistry by Akeroyd the flaws of the critical rationalist approach are criticised and a methodical alternative is proposed, involving the operational definition of scientific terms.

Key words: chemistry, critical rationalism, methodical reconstruction, normative epistemology, operational definition

1. Introduction

In a series of articles published during the last ten years F. M. Akeroyd has tried to apply the Popperian methodology of the *Logic of Scientific Discovery* to the development of chemical theories. In his efforts he discusses some milestones in the historical development of this science as the ideas of Lavoisier and Fourcroy on the chemical constitution of acids, the electrochemical theory of the chemical bond of Berzelius or the work of Hopkins on vitamins. Akeroyd seems to be convinced that the Popperian methodology of "conjecture and refutation" or – as he calls it – the hypothetico-deductive method is not only appropriate for the reconstruction of these events but that it is the only approach that describes *truly* the process of "making science". In this point Akeroyd contradicts Lakatos[1] who claimed that scientific theories are not abandoned by the first contradictory observation, but that within a so called *research programme* some theories that constitute its "hard core" are defended by altering auxiliary "low range" theories and hypotheses that build a "protecting wall" around it. Akeroyd challenges the Lakatosian view literally using the historical cases he discusses as *empirical* evidence that Popper is right and Lakatos wrong.

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I do not intend here to take the part of either Popper or Lakatos in this argument in the sense that I am not going to excavate more historical examples that would support the one or the other view. My aim is to show that, while there is no deny that in natural science *many* questions are settled empirically, the reduction of the scientific enterprise to falsificationism is a shortcoming because it does not answer the question about the conditions for successful scientific action.

For this reason I will first give a brief recapitulation of the Popperian thesis of falsificationism, then I will examine it from a methodical point of view and then I will try to reinterpret the most celebrated historical example used by Akeroyd – Lavoisier's and Fourcroy's "theory" of acids (Akeroyd 1986, 1990, 1993 and 1996) – in order to show that we can understand history of science only on the background of a methodically reconstructed, normative scientific language.

2. POPPER'S *LOGIC OF SCIENTIFIC DISCOVERY*

Popper rejected the inductivism that in his view was held by the members of the "circle of Vienna" as a method for the acquirement of scientific knowledge because any acceptance of the principle of induction would lead to an infinite regress except one succeeded in giving some *a priori* reasons for its validity (Popper 1989, 4 [§1]). Popper stated instead that the only way leading to scientific progress is the postulation of general theories, i.e. sentences of the structure „All x are y" (Popper 1989, 35 [§15]), from which empirically falsifiable "basic sentences", i.e. sentences with the structure „At space-time point x there is an object y" or „At space-time point x a process z takes place" (Popper 1989, 68 [§28]), can be deduced. According to the logical *modus tollens*:

> [i]f p is a sentence deduced from a system of sentences t, that may consist of a theory and frame conditions [...], then we can symbolise the deductibility (analytical implicability) between t and p as $t \rightarrow p$, to be read as: „t implicates p". We assume that p is „false" – which we denote as $\bar{p}$, to be read as: „non-p". Because of the deduction relationship $t \rightarrow p$ and the assumption $\bar{p}$ we are allowed to conclude that $\bar{t}$, i.e. we also allowed to regard t as falsified. [...] [W]e can write $[(t \rightarrow p) \cdot \bar{p}] \rightarrow \bar{t}$; or in words: If p is deductible from t and p is false, then t is also false (Popper 1989, 45 [§18]).

The greater the class of basic sentences that are "forbidden" by a given theory, the greater the falsifibility of it and consequently its "power" (Popper 1989, 78 [§31]).

It must be stressed that Popper understands every assumption, hypothesis or conjecture as a more or less complex "theory" (Popper 1989, 54 [§22, footnote 1]):

Theory is the net that we throw in order to "catch the world" – to rationalize it, to explain it, to master it. We work on it, in order to make its mesh always finer (Popper 1989, 31 [§12]).

This is because every single statement and every predication like „This is a glass of water" contains "universals", i.e. words that denominate more than one thing[2]):

When we say „all swans are white" then we must agree that the predicated property „white" is observable; so one could say that a single sentence like „this swan here is white" rests on observation. However, this sentence transcends experience – not because of the world „white" but because of the word „swan". When we call something a „swan", we ascribe properties to it, that reach far beyond pure experience – approximately so far, as the statement that the object given consists of „corpuscles".

Not only the abstract, explaining theories transcend experience, but also the most common single sentences. Because even the common single sentences are always interpretations of the „fact" in the light of theories. (And this holds also for the particular „facts". They contain universals, and there where universals apply, there is always regular behaviour) (Popper 1989, 378 [* X]).

A Popperian scientist is then like the mouse in a Skinner-Box. Trapped in the labyrinth of "theories" but convinced that there is only one way out, he decides that the best procedure to find it, is to mark every dead end. It is obvious that this method can only function under the assumption that the found dead ends are due to the construction of the labyrinth and not created by the scientist itself. Consequently Popper addresses himself as a "robust realist" (Popper 1989, 76 [amendment 1968]).

3. THE FLAWS OF CRITICAL RATIONALISM

Popper's ideas are on a first view admittedly very attractive to the "front scientist" and to the philosopher of science. They seem to cover the field of laboratory activities since there "theories" are put under experimental scrutiny and then they are accepted or rejected. But can this apparent adequacy stand a closer examination of its logical foundations? Let us consider one of Popper's own examples the sentence „All swans are white". Under which conditions becomes this sentence true or false? Obviously the answer depends primarily on our *understanding* of this sentence. Before we can proceed to any deduction of "basic sentences" and their empirical test we have to decide if this is an empirically testable sentence at all. Popper claims that this sentence is a general law because it contains the "universals" 'swan' and 'white'. Undoubtedly laws *must* contain "universals" and not "individuals" (i.e. proper names for individual things persons, objects etc.). If the opposite were accepted, then the validity of laws would be not universal and general. On the other hand, not every sentence containing "universals" is a law, there are also other possibilities. Let us for instance replace the word 'swan' by the word 'leopard' and 'white' by 'spotted'. Then we obtain a formally similar sentence stating that „All leopards are spotted". Is this sentence falsifable? Obviously not, because 'leopard' is a denomination for a spotted panther. „All leopards are spotted" is a reformulated definition – an empirically not testable but true sentence –, no lesser scientific than „All swans are white". From this example we can see that in order for a sentence to become an empirical "law", it must fulfil some conditions concerning the meaning of the expressions it contains. With other words, the terms in such a sentence must be defined *prior* to its formulation, so that one can distinguish by means of these definitions alone if a given sentence is intended to be an empirical statement or merely a definition. Popper has intuitively seen this problem and stated that the required definitions occur in the light of other theories. So one has to have a theory about 'swans' or 'leopards' and one about colours in order to make the proper combinations between these terms. But then Popper gets into an infinite regress of theories, because if the use of the words 'swan', 'leopard', 'white' and 'spotted' is theory-guided, then these theories must be at least succesful. Again, this can be decided only by theory-laden observation etc., etc. The net thrown to catch the world turns to be a cobweb to which Popper and his followers are helplessly stuck. The only way to avoid the kiss of the lurking spider of metaphysics is to cut through this cobweb and reach the salvaging ground of unequivocal terminology.

Let us resume: In order for a sentence of the form „All *x* are *y*" to be empirically testable the terms *x* and *y* have to be introduced independently of the context in which the assertion is made. 'Swan' and 'white' must be defined in such a manner that the colour of an animal is not decisive for it to belong to the biological species 'swan' and the use of the word 'white' does not determine in advance that the object of the predication is an animal. In the case of colour terms the solution of the problem is relatively simple: They are introduced by example. We point to surrounding things and state that their colour is 'white'. We may also point to some counterexamples that are 'green', 'black' or 'spotted'. After some of such predications the meaning of 'white' is sufficiently well established, so that the members of a community use it practically without errors.

In the case of 'swan' the situation is slightly more complex. Certainly the term can be introduced also by example and counterexample, a procedure which is anyway sufficient for the daily life at a middle European pond. But its sufficiency ceases when we follow the aim to establish a system of relationships between animals, perhaps in order to decide which kind of them has to be preserved, or because we are trying to help breeders in improving their traditional methods. In this case we need a different criterion than cultural arbitrariness, we need a criterion that gives unequivocal results respective to the classification of birds in "swans" and "non-swans". Such a criterion is the *interbreeding*: Cross two animals and if they produce fertile offspring, then they belong to the same *biological species*. This kind of definition is an *operational definition*. It does not depend on any conjecture or hypothesis since the criterion of interbreeding is *normative* and *not* empirical. It enables the biologist to distinguish between the different animal species and with some auxiliary conditions it enables him to introduce the species *swan*. The sentence „All swans are white" becomes so a testable hypothesis (and a false one) because there are animals that can be identified as "swans" by the theory-free citerion of interbreeding and those animals are of different colour than our European pond-swans. Contrary the sentence „All leopards are spotted" is an analytically (i.e. per definitionem) true sentence because 'leopard' *is defined* as 'spotted panther'. Every animal that interbreeds with a panther and bears color spots on his fur *has to* be called a leopard.

From the above we can conclude that science does not proceed by theory-laden observational falsification of theories as Popper claims, but that it firstly produces an operationally defined basis of normative terminological distinctions with the aid of which it produces and examines phenomena in order to learn how to solve practical problems. This empirically obtained aim-oriented knowledge is the content of "laws" and "theories".

4. Lavoisier's and Fourcroy's statements about acids – theory or merely a definitory problem?

The normative basis of a science is not only necessary for the mastering of its daily problems but it is also inevitable for the understanding of its historical development. The question raised by Akeroyd, if the fate of Lavoisier's and Fourcroy's statements about acids was a case of empirical rejection of a theory or merely a problem of missing definitions can be answered only on the background of the operational definitions of the chemical terms involved in the discussion. We have then firstly to answer the question „What is an acid?" by giving operational criteria that enable the classification of a given substance as "acid". Secondly we have to compare our definition with the respective definitions of Lavoisier, Fourcroy and Davy (who according to Akeroyd refuted the "theories" of the latter[3]) in order to find out if there was a progressive (in respect to our terminology) definitory shift, or if their definitions were "modern", so that there was actually a case of experimental testing.

In pre-theoretical contexts "acids" are normally defined as substances tasting sour, turning blue litmus paper to red, dissolving metals under emanation of a combustible gas called hydrogen, and combining with an other class of substances called "bases" to form "salts". A closer look at this definition reveals some gaps: Not every substance we call today an "acid" reacts with metals, not every acid emanates with metals a combusible gas, not every acid turns blue litmus paper to red. What is worse, many "acids" can be prepared in solid or gaseous state. From this we can conclude that the above given "definition" is no definition at all but merely a list of some properties shared by some substances. If we are going to call every substance displaying one or more of these properties as "acid", then "acid" becomes a merely categorial term with limited practical significance. But we need an operational definition of acids, a criterion we can apply every time we produce a substance hitherto unknown. A hint for such a ceriterion is the examination of our actions when testing a substance for its "acidity". Normally we do not take an amount of substance from a flask with the label *Concentrated Nitric Acid* or *Cristalline Citric Acid* and try its taste or its behaviour against litmus paper. We dissolve these "acids" in water and then we test if the properties of water have been *altered*. The list of the above mentioned "acid properties" are then not primary properties of substances called acids, but phenomena accompanying the dissolution of such a species in water. The common property of acids is not their sour taste, colour changing capacity etc., but their ability to *alter* the properties of water. Acids are then operationally defined as substances that dissolved in water change its taste to sour, give to it (in certain concentrations) the ability to dissolve some metals under emanation of hydrogen, to change the blue colour of litmus, increase its conductivity to electric current and also alter other essential properties[4] of this particular substance. Prerequisite for the identification of acids is at this pre-theoretical level the supply of chemically pure water with standardised essential properties, e.g. neutral taste, no effect on the colour of litmus, no effect on the same metals, very low electric conductivity etc.

Let us now take a look on Fourcroy's definition of acids. In his *Philosophy of Chemistry*, the English translation of which was published 1796 he states that

[a]ll acid resemble one another in taste; in their manner of giving a red colour to vegetable substances; in their common tendency to combine with earths, alkalis, and metallic oxides; and in the property of attracting and being highly attractable (Fourcroy 1796, 43).

This definition resembles our first "definition" that was rejected for reasons of inadequacy. Fourcroy gives only a list of phenomena evoked *by* acids in solution but he has no operational or explicit definition of this term. Consequently his assertion that

> [...] every acid contains oxigene, and loses its acidity, in proportion as it is deprived of this principle: Acids are therefore to be regarded as burnt or oxigenated bodies [...] (Fourcroy 1796, 43).

is – taken as a "law" – a non-scientific statement because it contains an undefined term and because – even if we accept a "tacit" definition of acid – it did not cope with phenomena already known to Lavoisier, Fourcroy and their colleagues, namely that water and many metal oxides do not display "acid" behaviour although they contain oxygen. The only scientific acceptable interpretation of this sentence is that of a "prescription" guiding the efforts for the synthesis of substances that fulfil the operational criterion of acidity. Fourcroy's statement is then scientific not because it is empirically testable nor because it is a hypothesis in Popperian sense, but because it belongs to a different category of scientific statements, the *technical norms*[5]. Lavoisier, Fourcroy and their successors formulated it because they *wanted to elaborate* a method for the production of acids – in our sense. This norm is still valid today: Just combine some specified elements with the element oxygen; the products dissolved in water will display acid properties. Davy[6], on the other hand, did not refute any former theory, he just demonstrated another method for the production of acids, i.e. he formulated an other norm: Combine a very specific sort of elements (the "halogenes") with hydrogen and dissolve the products in water. It lasted several decades until chemists succeeded in establishing further technical norms for the production of acids a process that was strongly supported by the increasing theoretisation and axiomatisation of chemistry.

THE CASE POPPER VS. LAKATOS: A SALOMONIAN VERDICT

At the end of these reflections on the methodology of chemistry we should return for a last time to the controversy between Popper and Lakatos. We have seen that the Popperian view is insufficient for the reconstruction (not only) of chemical theories because it neglects the normative character of scientific statements and the practice-supporting aims of science itself, stylising a methodically secondary aspect of scientific enterprise – the empirical testing of prognoses deduced from axiomatic theories – to the very moment of scientific enterprise. Shall this mean that the argument beween Popperians and Lakatosians is decided in favour of the latter?

Taking into consideration that Lakatos' "hard cored" research-programme-view includes normative elements the answer could be yes. But the fact that Lakatos bases his conclusions also solely on a descriptivistic analysis using rather sociological than methodical arguments reveals the limits of his approach. Only an overcoming of the descriptivistic analytic philosophy of science in favour of a normative epistemology open for discussions about the aims of science and the means for reaching them will enable the full understanding of the language of chemistry and reveal the true magnitude of the work of its pioneers.

NOTES

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1 Cf. Lakatos 1968.

2 Popper's "universals" can be partially reconstructed as the *predicators* of Methodical Constructivism. For an introduction to the methodical contructivist terminology see f.e. Lorenzen 1984, Lorenzen 1987, Janich 1994 and Hartmann 1990.

3 Akeroyd has overseen that Lavoisier's opinions about acids have been challenged as early as 1785 by Berthollet who although being a proponent of Lavoisier's new system of chemistry noticed that some substances that showed acid properties did not contain oxygen. Berthollet continued his critisicm for the next decade with almost no success since his contemporaries were so enthusiastic about Lavoisier's ideas that they neglected many inconsistencies inherent to them. A good presentation of the oxygen-acid debate is given by Le Grand 1977/78.

4 The term 'essential property' (German: substantielle Eigenschaft) is part of a methodically reconstructed language of chemistry (Psarros 1995). For an introduction to the program of a constructivistic foundation of chemistry (*Protochemistry*) see Janich 1994.

5 Cf. Janich 1973, Janich 1985 and Hartmann 1993.

6 And also Berthollet some ten years before him by preparing prussic acid and hydrogen sulfide (Le Grand 1977/78).

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