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CHEMISTRY AND A THEORETICAL MODEL OF SCIENCE: ON THE OCCASION OF A RECENT DEBATE WITH THE CHRISTIES

ABSTRACT. In the philosophy of chemistry a view is developed according to which laws of nature and scientific theories are peculiar in chemistry. This view was criticized in an earlier issue of the *Foundations of Chemistry* (Vihalemm, *Foundation of Chemistry* 5(1): 7–22, 2003) referring to an essay by Maureen and John Christie (Christie and Christie, in N. Bushan and S. Rosenfeld (Eds.), *Of Minds and Molecules: New Philosophical Perspectives on Chemistry*. Oxford University Press, New York, 2000, pp. 34–50). This criticism was responded by the Christies (Christie and Christie, *Foundations of Chemistry* 5(2): 165–177, 2003). In the present article the debate is continued. The main issues which need to be elucidated in order to carry the analysis forward are pointed out and discussed. The relevance of a theoretical model of science for the philosophy of chemistry is stressed.

In an earlier issue of this journal (Vihalemm, 2003), I criticized Maureen and John Christie's view (Christie and Christie, 2000) that laws of nature and scientific theories are peculiar in chemistry. The Christies wrote a review on my article (Christie and Christie, 2003). In that review they write: "There are many areas where the debate can carry the analysis forward; there are others where we may simply have to agree to disagree. Some of our differences can be reduced to semantic issues, but there remains a large area of substantial disagreement." (Christie and Christie, 2003, p. 165) So, there are a number of issues which need to be elucidated. In the present response to the Christies I would like to point out and discuss the main theses and arguments which have been rejected or misunderstood by my opponents.

1. THE POINT OF THE CONTROVERSY

First of all, defending their earlier position, the Christies argue against the principal thesis of mine, which is a counter-thesis to

Foundations of Chemistry (2005) 7: 171–182

DOI 10.1007/s10698-005-0959-y

their main claim in the essay *"Laws" and "Theories" in Chemistry Do Not Obey the Rules* (Christie and Christie, 2000). My thesis is: granted that we are speaking about chemistry as a science, scientific theories and laws of nature are not peculiar in chemistry. As opposed to this, my opponents write in the concluding section of their paper:

> The statements and models that form part of the corpus of chemical theory in providing explanations of chemical phenomena are peculiar. The statements are often imprecise or exceptioned or both. Theoretical models are simplistic and diverse. They are often mutually inconsistent. The attention paid to their grounding in physical theory is more ritualistic than rigorous. In framing explanations chemists feel quite free to "mix and match" their models. (Christie and Christie, 2003, p. 172)

While I am ready to agree that this statement describes adequately the situation in chemistry, I don't understand how this is relevant to my argumentation. My opponents seem not to have noticed that I have argued about chemistry *as a science*, not about chemistry in general. It is right, however, that in my paper in *Foundations of Chemistry* reviewed by the Christies I have not stressed (unlike my several other papers[1]) that speaking about chemistry as a science does not mean that chemistry 100% can be regarded as a science. In that paper the dual character of chemistry was not discussed. Nevertheless, I did stress that my presumption was that laws of nature and scientific theories, as categories of philosophy of science, cannot vary in their nature from one discipline to another, granted that these disciplines are considered as sciences.

My idea was to proceed from a theoretical model of science, which can be elaborated on the basis of physics. But I stressed, particularly, that a theoretical model of science cannot be identified with physics. I have not regarded physics simply as a model of science, but only as a theoretical starting-point for the construction of a theoretical model covering any science. In elaborating such a model, chemistry is important as well. That was exactly my point when scrutinizing Mendeleev's periodic law. In order to understand what is the real nature of physical laws and theories – not merely as laws and theories in physics, but also as instances of laws of nature and scientific theories in general – it is almost inevitable that non-physical cases should be analysed, too.

My opponents reject the idea that physics should, in the sense that I intend, i.e., as the basis of a theoretical model of science (called φ-*science*), be regarded as a model of science. Along with this rejection, they also disagree that laws of nature and scientific theories should be understood in the light of that model. Unfortunately, again, there seem to be too many misunderstandings in their argumentation as concerns my ideas. I would like to comment briefly on their main arguments against the approach which tries to elaborate and employ a theoretical model of science in the philosophy of chemistry.

2. A THEORETICAL MODEL OF SCIENCE IN THE PHILOSOPHY OF CHEMISTRY

According to the Christies, the attempt to elaborate a theoretical model of science and a *law of nature* and *scientific theory* as theoretical concepts "is really only semantic point, though. Perhaps it is wise for philosophers of science to adopt definitions ... that are universal and discipline invariant." (Christie and Christie, 2003, p. 165) "The difficulty", as my opponents continue, "... is that the definitions become ideals, which are not realized in actual scientific practice" (Christie and Christie, 2003, p. 166). And:

> We would not want to concede that all discipline areas should aspire to a single ideal in terms of the logical structures of their explanatory frameworks – an ideal most closely approached by classical physics. [...] Nor, in our view, should scientific disciplines be measured against a pre-determined yardstick that was primarily designed with classical physics in mind ... (Christie and Christie, 2003, p. 166)

In the first place, I am convinced that semantic points are also important in philosophical discussions, in case they are not reduced to futile debates concerning the use of different terminology. In the present case, the problem lies in a false interpretation of a theoretical model as an idealization and its role in research work. Idealization in science does not mean an unattainable ideal, though it does mean an ideal that has been taken as an example. (Idealization functions as a tool in philosophy as well; for example, my theoretical model of science, φ-*science*, is also an idealization). Theoretical models as idealizations are in actual scientific practice known as concepts of very realistic content. On the other hand, the precondition for the philosophical comprehension of the actual content and importance of theoretical models, or idealizations, is familiarity with the conception of the practical nature of science.[2] For instance, empiricist, naïve realist, or instrumentalist approaches which have been used in philosophy up to the present time, are, to my mind, insufficient to provide a reliable interpretation of scientific-theoretical knowledge. It also seems to me that the Christies' misunderstandings concerning my approach can in part be ascribed to the fact that their philosophical position is not very clear (their position can be characterized as, so-to-say, scientists' common-sense empiricism and realism).

As a result, the Christies consider it unacceptable that I rely on practical realism, or that I hold a view which stresses the practical nature of science and admits that discoveries in science are made by construction. According to their interpretation, it is not a realist worldview. They write: "But our view of science differs from Vihalemm's. It arises out of a realist worldview. We see science more as an enterprise of discovery than as an enterprise of invention and construction". (Christie and Christie, 2003, p. 171).

They illustrate their interpretation with an example, which I would like to present here for further analysis:

> Imagine a strict falsificationist stuck in a maze. She *constructs* a model in her mind about which turnings to take, tests it empirically, and modifies her model accordingly, until eventually she succeeds. This is clearly an enterprise of *construction*. But the outcome of this process is that she *discovers* a way out of the maze. We would not say that she constructs a way out of the maze. That would require a cold chisel or a chainsaw! (Christie and Christie, 2003, p. 173, fn. 4)

This metaphor of a maze or labyrinth is interesting, but slightly misleading as an analogue of scientific research. The metaphor is more appropriate for characterizing the classifying-historico-descriptive type of research (or *natural history*) than science (in the sense of φ-*science*). It presupposes that the research task is 'given' by 'the external world as it is.' As regards scientific research, the metaphor of a road-builder would be more appropriate. The field of scientific research cannot be compared with the discovering of a way out of a maze: it is more like the designing or constructing of roads; in the course of this designing or constructing it becomes evident which constructions are realizable and which are not. The similarity with the maze-metaphor lies only in the aspect that the main goal of science is the discovery of laws of nature through construction practice. That is, since the researchers' activities are restricted by the laws of nature, a researcher moves like in a maze; the scientist has to find a way that does not contradict the laws of nature. In other words, he/she has to discover these laws.

On the other hand, the maze-metaphor can indeed be used for characterizing chemistry – not for characterizing chemistry as a science but as a research field with a dual character. A chemist has to act as if in a maze, not only as a 'pure scientist', i.e., not only in the sense that his activities are restricted by the laws of nature. The chemist's activity is restricted by the chemical substances as well: he has to identify, classify, etc. the chemical substances, their properties and transformations regardless of the fact whether and to what extent this is possible on the basis of the laws of nature. So, if a maze where a 'pure scientist' is stuck in represents only the laws of nature, then the chemist's maze has two types of restrictions: in addition to the laws of nature constraints are also set by chemical substances.[3] Of course, a chemist as a 'pure scientist' can say that he is interested in substances only insofar as they can be treated in accordance with the laws of nature. But in that case a great part of chemistry will be left out altogether; and such kind of 'pure scientist' in chemistry (and chemistry as purely φ-*science*) presupposes as its basis the socio-historically 'given' chemistry as a whole.[4]

Strangely, some of the reproaches of my opponents concern views which I, similarly to them, have rejected. My conception of science does not include the view that "there is a hierarchy of more mature and less mature discipline areas in science" and the view that "all discipline areas should aspire to a single ideal ... most closely approached by classical physics" (Christie and Christie, 2003, p. 166).

On the contrary, the narrower conception of science, based on the theoretical model which I have proposed, is actually meant as a criticism of the aforementioned attitude. Such a misunderstanding can easily arise if "science" is taken in the broad sense, i.e., it is not understood as a quite narrow, specific type of research and knowledge, but as the one and the only way of producing reliable knowledge – which means that the label 'science' is like an admired quality brand. Such an attitude is also evident in the discussions of the Christies, who do not agree with the view that a discipline is a science only insofar as it has the features which are characteristic of physics; thus, they consider it essential to emphasize that although chemistry is different from physics, this does not make "chemistry in any way a lesser branch of science than physics" (Christie and Christie, 2003, p. 167).

3. WHY BOTHER ABOUT A THEORETICAL CONCEPT OF SCIENCE?

From the point of view of philosophy of science, there is an unacceptable attitude, in my opinion, in the discussions of my opponents. If the concept of science appears to be applicable uncritically, simply on so-to-say 'sociological grounds' – i.e., regarding as science all disciplines which are 'normally' called sciences – and when the conception of science derived on the basis of physics is criticized and extended on this basis, then, as a result, we obtain a pluralistic picture of science. Then the question arises, what can this picture be used for and whether such a treatment has any actual content except the wish that the peculiarities of all disciplines developed under the name of science could be recognized as sciences and there would be no complications in accepting them as sciences?

I think that the conception of the laws of nature, for example, cannot be simply extended in such a way that, having noticed that the laws of nature, as traditionally defined, seem to serve physics very well, but have some peculiarities in chemistry, it is then decided that these peculiarities should be taken into account in the concept of a law of nature. So, according to the Christies, the laws of nature are not always precise and unexceptionable; they can also be approximate, or allowing exceptions, or both. It seems to me that, according to this attitude, there may exist laws of nature that actually are not laws of nature!

Here, it would be more reasonable to accept that chemistry has a dual character, and to treat the laws of nature in this context by using the theoretical model of science (according to which a law of nature as a general concept of science is independent of the peculiarities of the research field).

I have used this latter approach in my analysis of Mendeleev's Periodic Law. The nature of the Periodic Law remains rationally incomprehensible to my opponents, as they have written:

> We have said in our article, "The [periodic] law, while vague and inexpressible as a proposition, was quite definite in its entailments." [...] It is our view that the Periodic Law, somewhat paradoxically, is an exact law. Although its expression in a sentence is necessarily imprecise, it is not inexact in the sense of approximation, nor in the sense of allowing exceptions. (Christie and Christie, 2003,p. 171)

The Christies estimate my view of science, which they do not share, as "In some ways ... strangely reminiscent of Platonic dualism. The enterprise of scientific theorizing produces an idealized description – one that, for a number of very practical reasons, is imperfectly realized in the external world of nature." (Christie and Christie, 2003, p. 171) Here, it is probably the scientific idealizations which they have in mind. These, however, as stressed earlier, do not belong to the Platonic world of ideas imperfectly realized in the external world; from the point of view of a practical realist, they belong to the real world – more precisely, to an aspect of it which is 'given' in the form of scientific practice; therefore one cannot speak of two distinct worlds here.

Let me give an example of how idealizations exist in the real world, not in the Platonic world, or in the mind only. The concept of a chemical element might also serve as an example of an idealized object, being constructed as *a place in the periodic system*, as I have treated it in the above-mentioned work of mine. However, in case of this example it may not be entirely clear that an idealized object is considered; so let us provide a better example of an idealized object which is familiar to everybody. Very often, a geometrical point is regarded as an idealized object. The real nature of this concept is often misunderstood when it is described simply as an object that has no extension (although this is true!) and that does not exist in the real world. In fact, the geometrical point is defined through certain relationships in geometrical theory. This means that any object of real life can be regarded as a geometrical point, if it has those relations to some other objects through which the geometrical point is defined. For instance, for a steersman on a ship the North Star serves exactly as a certain geometrical point in determining the course of the ship.[5]

To sum up this section, I would like to return to the relationship of the theoretical model of science and physics, derived on the basis of the idealization principle, in the context of the second part of the Christies' argument that "differences in character between laws and theories in chemistry and those of classical physics 'are due to the different complexity of systems studied by physics and chemistry.'" (Christie and Christie, 2003, p. 165)

As mentioned before, my point of view is that the success of physics, due to which it is regarded as a paradigm of science, cannot be ascribed to the character of the natural phenomena studied by physics. The characteristics of a science are not determined by nature. (Recall the analysis of the maze-metaphor above.) Here, once again, it would be practical to acknowledge the difference between chemistry in general and chemistry as a science. It is far from being a semantic question, which can be reduced to mere terminology. If we accept the dual character of chemistry, we can view the issue of complexity from another angle, which has some advantages; namely, we are not trying to compare incommensurable concepts – science and natural history. Such a comparison can be regarded as a category mistake.

Using the maze-metaphor above, the situation can be explained clearly and briefly as follows: When the 'maze of physics' represents only general and fundamental laws of nature, then the 'maze of chemistry' has a dual character; in addition to chemistry as a science, which, like physics, is limited by the laws of nature (although not only by general and fundamental laws), restrictions are also imposed on chemistry, as natural history, by chemical substances. It is clear, that if we are really interested in the character of the laws of nature and scientific theories in physics and in chemistry, we do have to study them, i.e., we must treat chemistry also as a science, leaving aside the other components that in their type are different, while belonging to a different category.

In the light of this view, the Christies' claim that "attempts to ground chemical explanation fully in fundamental physical laws and theory will always fail in practice. We argue for an essential intractability of fundamental physical accounts of chemical systems. We see this as the source of the unusual character of theoretical accounts in chemistry ..." (Christie and Christie, 2003, p. 168) should also be analysed. The question is what (if anything at all) will remain from this description by the Christies of the relationship between the fundamental physical laws and theory, on the one hand, and chemistry, on the other, if we regard both chemistry and physics as sciences not in a nonspecified sense of the word 'science,' but from the point of view of a theoretical model of science, i.e., if we analyse both from these respects and degrees in which they can be identified as φ-sciences? I mean that from the theoretical or philosophical point of view it is reasonable to avoid the assumption of their (fundamental physics and chemistry) differences that should be ascribed actually to incommensurability or category mistake in relating different kinds of disciplines – φ-science and a discipline having a dual character – in spite of the fact that these disciplines are both 'normally' called sciences.

NOTES

1. I have argued in my earlier papers that the nonreducibility of chemistry to physics can be understood as a manifestation of a certain type of incommensurability: the concepts, models and laws used in the paradigm of chemistry cannot be derived from those of physics. But this does not mean that chemistry and physics as sciences are incommensurable on the methodological level as well. On the contrary, it is possible to speak about the methodological identity of chemistry (as a science!) and physics. To understand this, we should regard physics as the theoretical model of any science, an idealization (which may be called "φ-science"). Too often, however, the categories that on the methodological level are identical for any science (as φ-science) are identified with the corresponding physical concepts, without taking into account that their concrete content in physics and chemistry does not coincide. Besides, it is important to realize that chemistry as a field of inquiry has a dual character: its position is intermediate between science (in the narrow sense, i.e., as φ-science) and natural history (i.e., classifying-historico-descriptive type of knowledge and research). And it should be emphasized that the latter cannot be regarded as an inferior, less reliable or undeveloped type of knowledge and research. For chemistry as a φ-science see, e.g. (Vihalemm, 1999, 2001, 2004).
2. The idea of the practical nature of science was in fact one of the cornerstones of Kuhn's account, as recently stressed especially by Joseph Rouse (Rouse, 1996, 2002, 2003). Of course, this kind of philosophy of science, founded on Kuhn's interpretation, in the sense of stressing the practical nature of science, has been developed earlier (also in philosophy of chemistry and by myself). It is of course impossible to discuss in detail the conception of the practical nature of science in this paper. *Practical realism* is, broadly speaking, an alternative to traditional (naïve or metaphysical) realism, internal realism, instrumentalism (and pragmatism, more generally), and social constructivism. For example, Giere's *constructive realism* (Giere, 1988) or Chalmers's *non-representative realism* (Chalmers, 1982) are to my mind close to what one might call versions of *practical realism* in the philosophy of science, i.e., accounts which stress the practical nature of science. The central ideas of this conception are, as I see it, that (1) science does not represent the world 'as it really is' from a God's Eye position; (2) the fact that the world is not accessible independently of theories – or paradigms, more precisely speaking – developed by scientists does not mean that Putnam's internal realism is acceptable; (3) science as a theoretical activity is only one aspect of it (of science) as a practical activity whose main form is scientific experiment which in its turn takes place in the real world itself, being a theory-guided constructive, manipulative, material interference with nature; (4) science as practice is also a social-historical activity

which means, among other things, that scientific practice includes a normative aspect, too, and that means, in its turn, that the world as it is actually accessible to science is not free of norms either; (5) though not naïve or metaphysical, it is certainly realism as it claims that what is 'given' in the form of scientific practice is an aspect of the real world.

3. As concerns physics, the maze where a physicist is stuck in represents only laws of nature, since there are no physical phenomena 'given' by 'the external world as it is' (although according to the common view, mainly formed on the basis of general education, it looks like this). Physics is defined through general quantitative laws of nature. That is why physics serves as the basis for elaborating a theoretical model of science: by analogy to physics, science in general is defined through laws of nature.
4. The difference between pure science (i.e., φ-*science*), chemistry in general, and chemistry as a science can be expressed by the following slogans: there is no chemistry without substances; there is no science without laws of nature; there is no chemistry as a science without laws of nature about substances.
5. I borrowed this example from (Gryaznov, 1982, p. 61–65).

REFERENCES

A.F. Chalmers. *What Is This Thing Called Science? An Assessment of the Nature and Status of Science and Its Methods*, 2nd edn. Milton Keynes and Philadelphia: Open University Press, 1982.

J.R. Christie and M. Christie. Chemical Laws and Theories: A Response to Vihalemm. *Foundations of Chemistry 5*(2): 165–177, 2003.

M. Christie and J.R. Christie. Laws and Theories in Chemistry Do Not Obey the Rules. In N. Bushan and S. Rosenfeld (Eds.), *Of Minds and Molecules: New Philosophical Perspectives on Chemistry*, pp. 34–50. New York: Oxford University Press, 2000.

R.N. Giere. *Explaining Science: A Cognitive Approach*, Chicago and London: The University of Chicago Press, 1988.

B.S. Gryaznov. *Logic, Rationality, Creativity*. Moscow: Nauka, [in Russian] 1982.

J. Rouse. *Engaging Science: How to Understand Its Practices Philosophically*, Ithaca and London: Cornell University Press, 1996.

J. Rouse. *How Scientific Practices Matter: Reclaiming Philosophical Naturalism*, Chicago and London: The University of Chicago Press, 2002.

J. Rouse. Kuhn's Philosophy of Scientific Practice. In T. Nickles (Ed.), *Thomas Kuhn*, pp. 101–121. Cambridge: Cambridge University Press, 2003.

R. Vihalemm. Can Chemistry Be Handled As Its Own Type of Science? In N. Psarros and K. Gavroglu (Eds.), *Ars Mutandi – Issues in Philosophy and*

History of Chemistry, pp. 83–88. Leipzig: Leipziger Universitätsverlag, 1999.

R. Vihalemm. Chemistry As an Interesting Subject for the Philosophy of Science. In R. Vihalemm (Ed.), *Estonian Studies in the History and Philosophy of Science. (Boston Studies in the Philosophy of Science, Vol. 219)*, pp. 185–200. Dordrecht/Boston/London: Kluwer Academic Publishers, 2001.

R. Vihalemm. Are Laws of Nature and Scientific Theories Peculiar in Chemistry? Scrutinizing Mendeleev's Discovery. *Foundations of Chemistry* *5*(1): 7–22, 2003.

R. Vihalemm. The Problem of the Unity of Science and Chemistry. In D. Sobczyńska, P. Zeidler and E. Zielonacka-Lis (Eds.), *Chemistry in the Philosophical Melting Pot. (Dia-Logos: Studies in Philosophy and Social Sciences, Vol. 5)*, pp. 39–58. Frankfurt am Main: Peter Lang Europäischer Verlag der Wissenschaften, 2004.

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