ESTONIAN STUDIES IN THE HISTORY AND PHILOSOPHY OF SCIENCE

Edited by

REIN VIHALEMM

University of Tartu, Estonia and
the Philosophy and Methodology of Science Coordination Centre of the Baltic Association for the History and Philosophy of Science

KLUWER ACADEMIC PUBLISHERS
DORDRECHT / BOSTON / LONDON

A C.I.P. Catalogue record for this book is available from the Library of Congress.

ISBN 0-7923-7189-5

Published by Kluwer Academic Publishers,
P.O. Box 17, 3300 AA Dordrecht, The Netherlands.

Sold and distributed in North, Central and South America
by Kluwer Academic Publishers,
101 Philip Drive, Norwell, MA 02061, U.S.A.

In all other countries, sold and distributed
by Kluwer Academic Publishers,
P.O. Box 322, 3300 AH Dordrecht, The Netherlands.

Printed on acid-free paper

Printed in the Netherlands

TABLE OF CONTENTS

PART III / STUDIES IN THE PHILOSOPHY OF PHYSICS

PART IV / STUDIES IN THE METHODOLOGY AND PHILOSOPHY OF SPECIAL FIELDS OF RESEARCH

REIN VIHALEMM

INTRODUCTION: ESTONIAN SCIENCE STUDIES

For the first time, a collection of studies by Estonian authors in the field of philosophy of science, including the history and policy of science in Estonia, is available in English.

In 1940 Estonia was incorporated into the Soviet Union, and after World War II Russian became the dominant language of science and philosophy in Estonia. Soviet-style Marxism was made compulsory in philosophy. Direct links with Western philosophy were blocked. However, even in those abnormal conditions, as we will see, philosophy, particularly philosophy of science, continued developing.

Since Estonia regained independence in 1991, the situation has gradually returned to normal. Knowledge of Western publications and direct links with scholars from other countries have become a matter of course. One of the reasons why works by Estonian philosophers have appeared relatively seldom in international publications is the necessity to create original philosophical literature in Estonian and to translate foreign authors. The present collection attempts to be an essential step in acquainting the international reader with the Estonian contribution to the philosophy and history of science. The book comprises mainly of new works, although it also includes articles that contain parts or reviewed ideas from earlier publications in Estonian and Russian. The authors — philosophers and scientists — represent every generation from emeritus professors to young researchers who have already received their education according to international standards and have acquired contemporary methods of analytic reasoning and techniques of investigation.

The collection consists of four parts. Part I "Studies in the History and Policy of Science in Estonia" enables the reader to learn something of general interest concerning the history of science in this country and about Estonia's current science policy. The history of science and philosophy in Estonia dates back to 1632 when a university was established in Tartu, then Dorpat (called, after its founder, the Swedish King Gustavus II Adolphus, *Academia Gustaviana* during the first period of its history). In its essence, for three centuries this history coincides with the history of Tartu University. It reflects the demands of these commonwealths to which Estonia happened to belong (1632–1710 a Swedish university, 1802–1918 a Russian university). The relations with Germany had their roots in the 13th century when the German crusaders conquered and Christianised Estonia. Within the Russian Empire,

Husserl, E. 1930 'Nachwort zu meinen "Ideen zu einer reinen Phänomenologie und phänomenologischen Philosophie"'. Jahrbuch für Philosophie und phänomenologische Forschung, 11: 549–570. Halle (Saale): Max Niemeyer Verlag.
Husserl, E. 1950 *Cartesianische Meditationen und Pariser Vorträge.* (Edmund Husserl. Gesammelte Werke (HUSSERLIANA) I.) Hrsg. von Stephan Strasser. Den Haag: Martinus Nijhoff Publishers B. V.
Husserl, E. 1960 *Cartesian Meditations. An Introduction to Phenomenology.* Translated by Dorion Cairns. The Hague: Martinus Nijhoff.
Husserl, E. 1967 *The Paris Lectures. Translated by Peter Koestenbaum.* With an Introductory Essay. Second Edition. The Hague: Martinus Nijhoff.
Husserl, E. 1976 *Grundprobleme der Phänomenologie 1910/11.* Herausgegeben von Iso Kern. Den Haag: Martinus Nijhoff.
Husserl, E. 1987 *Cartesianische Meditationen. Eine Einleitung in die Phänomenologie.* (Philosophische Bibliothek, 291.) Herausgegeben, eingeleitet und mit Registern versehen von Elisabeth Ströker. Hamburg: Felix Meiner Verlag.
Kuhn H. 1973 'Rezension von: Edmund Husserl, Méditations Cartésiennes. Introduction à la phénoménologie. 1931 (1933)'. *Husserl. (Wege der Forschung)*, XL: 174–187. Herausgegeben von Hermann Noack. Darmstadt: Wissenschaftliche Buchgesellschaft.
Matjus, Ü. 1973 'On the history of the problem of intentionality in philosophy'. *Papers on Philosophy XIV. Transactions of Tartu State University*, 324: 188–212 [in Russian].
Matjus, Ü. 1985 'E. Husserl's understanding of the problem of existence from the viewpoint of the theory of categorial contemplation'. *Social determination of cognition.* Abstracts of conference reports, 19–20 September, 250–253 [in Russian]. Tartu.
Matjus, Ü. 1988 'Problem of existence of "material things" in E. Husserl's phenomenology'. *Papers on Philosophy XXXIV. Transactions of Tartu State University*, 829: 83–101 [in Russian]. Tartu.
Matjus, Ü. 1993 'Edmund Husserl on the paths of Descartes'. *Akadeemia*, 8: 1571–1603 [in Estonian].
Röd, W. 1982 *Descartes: Die Genese des Cartesianischen Rationalismus*, 96–101. Zweite, völlig überarbeitete und erweiterte Auflage. München: Verlag C. H. Beck.
de Waelhens, A. 1973 'Descartes und das phänomenologische Denken (1938)'. *Husserl. (Wege der Forschung)*, XL: 188–209. Herausgegeben von Hermann Noack. Darmstadt: Wissenschaftliche Buchgesellschaft.

ENDLA LÕHKIVI

HERMAN BOERHAAVE – *COMMUNIS EUROPAE PRAECEPTOR*:
Internal vs. External in the History of Science

1. INTRODUCTION

Hermann Boerhaave (1668–1738), according to the often-quoted expression of his pupil Albrecht von Haller, *Communis Europae Praeceptor*, the teacher of all Europe, has not received the attention from the historians of science that he should deserve. In the early 18th century, he attracted students from almost all over the world. To mention only a few, among his students were Linné from Uppsala, Haller from Göttingen, Cullen, Monro, and Sinclair from Edinburgh and he had contacts with Russian and Chinese scholars, etc.[1] What was the basis of his fame? One cannot connect any scientific discovery to his name. At the same time, we know that his pattern of scientific work, his *method* spread all over Europe. A number of historians of science find that it was Boerhaave who made the Newtonian turn, i.e., the Scientific Revolution, in chemistry, as well as in biology and medicine. If so, why is Boerhaave then almost unknown at the end of the 20th century?

In this essay I shall restrict myself to Boerhaave's chemistry, although his scientific pursuits also concerned biology and medicine. The main issue of my study, however, is not the history of chemistry, rather it concerns some meta-level historiographical questions.

Traditionally, there are two alternative positions seen in respect of the Scientific Revolution, one called internalism and the other externalism. For a long time, the internalist model served as an "officially" accepted historiographical position, whereas externalism was taken to be a position of Marxist, leftist and other radically minded marginal historians. Nowadays, the general attitude has changed. Internalism has become a view of the past that almost no-one in the history of science field accepts any more, whereas externalism has advanced and become a widely approved position, although it might seem odd to talk about *generality* or *universality* of externalism, because externalism in itself rejects any "big pictures".[2]

According to *internalist* rational reconstructions, the Scientific Revolution must be seen as a logical consequence of the earlier, pre-scientific imaginations of alchemy, iatrochemistry, etc., even if the revolution itself consists in the break with old tradition, it is the break within the content – in the internalist approach only the content matters. The problem with internalism is that it

R. Vihalemm (ed.), Estonian Studies in the History and Philosophy of Science, 139–150.

assumes that sciences follow a mysterious internal rationality of development. Thus, chemistry was seen to become a science only when "rational" and "progressive" Newtonian metaphysics was accepted by its practitioners. It is quite obvious how arbitrary and presentist this idea is — it imposes contemporary standards of rationality as universal upon history. As result of this kind of historiography, only the achievements, discoveries and inventions which support the supposed line of progress remain visible in the story of science. Newton and mechanicism, of course, are seen as a stage of the universalist history of science. In this connection, it even may be surprising that Boerhaave has acquired such a marginal position. The reason for that consists in the causes *why* Boerhaave introduced mechanicism into chemistry. In his natural sciences, mechanism served just as an instrument for practical purposes. Seen in the light of the dichotomy internal *vs.* external, the causes of acceptance of a theoretical belief must be seen as external. Even within internalism it is obvious that some external factors cannot be neglected. However, it is not just a matter of including the external issues. The internalist approach cannot be improved just by adding the external influences, although many internalist philosophers, historians and sociologists of science have proposed such an idea.[3] The external issues will still remain secondary, additional and unimportant in the whole rational reconstruction.

For this reason, one could suggest to choose another theoretical point of departure, a one which enables us to consider the external circumstances seriously, and that could be called externalism. Nevertheless, such a conclusion in itself is not satisfactory either. The problem with externalism, constructed as a counterpart of internalism, is that it preserves the whig history scheme, or internalism as such. Radical externalism would involve explanation of scientific knowledge in terms of social, economic, political, ideological, etc. causes. That means a social, economic, or political, etc. reduction.[4] Since the externalist model will thus view just the social, ideological or political, i.e., contextual aspect of scientific knowledge, it may seem necessary to save also the internal history in parallel relating to the real content of science. Robert Young, a proponent of a form of externalism, which he calls contextualism or relativism, admits that

> It is therefore very difficult indeed to refrain from treating the materials in terms of the model of 'internal' and 'external' factors, science and society. (Young, 1973: 376)

Therefore, the label externalism turns out to cause a serious confusion. Those who regard themselves as externalists are certainly interested in the contents of scientific knowledge as well as its context. Externalists point out the significance of the connection between content and context. Thus they do not just deal with the external aspect of science, as one could assume on the ground of the dichotomy: internalism *vs.* externalism, they deal with science in its context. Externalism is opposed to any "big pictures" of the progressive "edge of objectivity".[5] According to a consistent externalist view, there is no Scientific Revolution as such. In the history of sciences, we only find particular scholars, researchers, teachers involved in their particular practical activities, solving problems, constructing new devices, doing experiments. Scientific Revolutions as well as the progressive and degenerative research programmes, rational models of research traditions, etc. belong to the "big pictures" of internalist history.

The difficulties, however, just begin here, because the distinction between the internal and external factors is practically a very complicated one, the content and the context of scientific knowledge appear to be inter-related. For instance, there has been a lot of discussion around the possible classification of scientists' metaphysical, religious and aesthetic views, whether these should be seen as internal or as external ones (McMullin, 1987: 58–64)? A number of historians consider ideological and metaphysical views as internal whereas the others refer to these as external to scientific knowledge. Since I prefer to leave this huge historical discussion aside in my short essay, I suggest to replace the traditional and still problematic dichotomy of internal vs. external with another, that of intrinsic and extrinsic, originally proposed by John Christie and Jan Golinski (1982). The new dichotomy is constructed with intention of historical analysis and explanation. For such an explanation, one should choose between the two alternative approaches: either to consider both internal and external issues closely related to a particular discipline at a particular time and in a particular location, e.g., chemistry in Leyden in the 1720s, that is *intrinsic* approach; or to consider the scientific ideas in the light of some non-scientific, i.e., general metaphysical, epistemological, ideological, etc. framework — that is *extrinsic* approach.

When following the intrinsic approach after Golinski and Christie, the reasons, that caused Boerhaave to accept Newtonianism, can be easily explained by the practical needs of scientific activity: knowledge needs to be organised for teaching and communication. For Boerhaave, mechanicism served as a conceptual tool in knowledge transfer. Similarly the big changes in the sciences in general, related to mechanicism, the new worldview, change of scientific language, can be explained by practical purposes of communication and teaching sciences, rather than by metaphysical necessity.

In the internalist tradition, Boerhaave has been seen as a Newtonian scholar who was close to revolutionary turnabout in chemistry. It is still important to emphasise that he was not only a great Newtonian scholar who prepared ground for Lavoisier's work in chemistry. He was a practising surgeon, chemist, biologist and physiologist, who was occupied with several practical questions, how to treat his patients, how to classify diseases, how to prepare drugs, how to understand a human body and processes therein, how to classify herbs which he needed for preparing drugs, and finally, how to pass all this practical knowledge that he possessed to the next generation of scholars.

In the following section, I will analyse the historiographical questions at somewhat greater length, in the third section, Boerhaave's chemical activities will be considered in the light of the hisoriographical conclusions. In the fourth concluding section, I will return to the questions posed at the outset.

2. SOME HISTORIOGRAPHICAL CONSIDERATIONS: Internalist — externalist distinction vs. intrinsic — extrinsic

According to a widespread opinion among historians of science, the content of 17–18th century science was mainly determined by religious, economic and social factors. (Nilsson, 1984: 107) According to another opinion, development of science is supposed to be explained on the basis of the science itself, on that of observations and experiments more than theories and hypotheses, without reference to the historical context. (Nilsson, 1984: 110) The first is the so-called *externalist* and the second the *internalist* position. Since the distinction is a problematical one, as we could see in the introduction — it often is namely the internalist position that assumes religious or metaphysical factors to determine the content, etc., — I hereby invoke another dichotomy, as suggested by J. R. R. Christie and J. V. Golinski, namely that of *intrinsic* and *extrinsic*. They describe their intrinsic approach as follows:

> The analytical focus we urge is concerned with the question of the nature of chemistry as an historical practice. This focus is interested in the whole range of social and cultural conditions governing both practical chemistry and chemical discourse, but it is the human activities of practising and talking about chemistry which are central, and around which broader themes are articulated. We would like to describe this perspective as an 'intrinsic' one. Against it we would set a class of approaches which could be described as 'extrinsic'. Such approaches shift the focus of analysis away from chemical practice to non-chemical fields of discourse. Assuming the nature of chemistry as fundamentally unproblematic, the extrinsic approach tends to construct chemistry in terms which give great emphasis to the influence thereon of activities such as speculative natural philosophy, matter-theory, epistemology, methodology and theology (Christie and Golinski, 1982: 235–236).

Without rejecting the influence of metaphysics, theology, etc. on chemistry, they, in contrast to the extrinsic approach, do not presuppose unidirectional determination of chemistry by external attitudes. An intrinsic approach insists upon the problematic nature of the relationship between such factors and the practice of chemistry.

According to Christie and Golinski, internalism includes those factors that have been designated above as extrinsic to chemistry, whereas it denies the social, economic, cultural characteristics in the description of scientific process,

focusing only on the "pure products of intellect." The intrinsic approach, by contrast, demands sensitivity towards the precise location of intellectual production, for the location might have certain effect on the discipline, thus, e.g., theology and epistemology are suitable for explanation of chemical activities in certain circumstances, relevance of these explanations is historically variable. In their article, Christie and Golinski demonstrate how intrinsic historiography might be made to work on the example of the 17–18th century chemistry as a practical and a didactic discipline.

In a later writing, Golinski (1993) points out that the misleading dichotomy brings one to a theory of postponed revolution in chemistry and its consequences such as in Herbert Butterfield's account.[7] According to Butterfield, chemistry as a science suffered remarkably because of postponed revolution, i.e., there were no achievements in the 17th century chemistry, and scientific conceptual framework was lacking before Lavoisier's contribution to chemistry in the late 18th century. For Golinski, the view of historians, who attach rationality to 17th century chemistry only to the extent it has taken over mechanistic philosophy and language, is equally fallacious. In their opinion, the criterion of maturity of science was its mechanicism, and the first scientific chemists accordingly were Nicholas Lemery (1645–1715) and Robert Boyle (1627–1691), whose works contain mechanistic elements. Golinski finds that:

> Such a historiography simplifies the relations between the chemistry and natural philosophy of the seventeenth century by assigning to chemical practice a position of subservience and passivity with respect to theoretical developments in contemporary metaphysics. (Golinski, 1993: 368)

Some studies on 17th century chemistry say that the mechanistic philosophy was accepted by chemists only because of psychological and epistemological considerations, the language mechanics offered was a privileged one, clear and easily applicable in chemistry and in other fields of natural science as well. However, such an attitude lacks historical specificity, as Golinski argues, the transition could happen in any place at any time.

According to Golinski both the French historian Hélène Metzger and Butterfield take the criterion of a mature science to be the existence of a logical structure of concepts, founded upon a metaphysical theory of matter. Such a structure is assumed to be a mental entity, psychologically connected with immediate experience. Also, such a structure would be separable from its historical and material manifestations, words, texts, practices. Consequently, the mechanical language of chemistry was regarded simply as representing the metaphysical structure.

Golinski sees two connected assumptions to exist in this historiography, first, that chemistry is taken to be essentially dependent on a philosophy of matter, and second, that the language in which chemistry represents phenomena is seen to be unproblematic. Chemical texts just reflect the philosophical theory that rest on the background and correspond with reality. In Metzger's opinion, the mechanistic attitude enables us to abandon the allegorical and metaphorical style of earlier chemical discourse. An interesting question arises, how actually did the alchemical obscure languages function? To what extent and how did alchemists understand each other? And why did chemists suddenly come to accept the mechanic concepts instead of the old alchemical language? Did they just decide to start to talk more clearly?

According Maurice Crosland (1963), a historian of chemistry, it is plausible that the need for reorganisation of the language appeared within the alchemical symbolic tradition itself. Crosland refers to Lemery who had been rather critical about his contemporary chemists' language, thus in his days already (in the end of 17th century) the obscure language of alchemy was out of date.

Lemery's own mechanistic rationalisations of the texts had a direct practical purpose: his pharmaceutical prescriptions needed to be widely understandable. So, we are justified to ask together with Christie and Golinski (1982: 245): did the mechanistic rationalisations serve, in some sense, as legitimisation for his chemistry, or were they a result of the search for clear and unambiguous descriptions of chemical processes?

In Golinski's recent study we find a reference to another example of intrinsic approach in the history of chemistry. This is Owen Hannaway's work on the history of early modern chemistry. Hannaway proposed to investigate the origins of chemistry at the beginning of the seventeenth century as a didactic discipline. In his opinion, the formation of chemistry, as a textual tradition, predated the widespread acceptance of mechanical philosophy.[8] The main task was to distinguish between the activities called chemistry and those of alchemy. Hannaway characterises chemistry and alchemy in terms of different attitudes to modes of argument and communication, and for the very possibility of learned discourse. Chemistry is a didactically oriented discipline, committed to the values of open and clear communication, whereas alchemy is an object of conspiracy. Hannaway equates the birth of chemistry as a didactic art with the appearance of Andreas Libavius' *Alchemia* (1597) and sees it as formed in opposition to the Paracelsian-Hermetic school, exemplified in Oswald Croll's *Basilica Chymica* (1609). Croll's (alchemical) epistemology was individualistic: all the knowledge is as a result of interaction between a man as microcosm, the centre and subject of creation, and macrocosm. The interaction or externalisation of knowledge is possible only via a sympathetic attraction between these two: micro- and macrocosm. Knowledge was conferred by divine grace, rather than by reason, it could not be read, either from nature or from books. For Libavius, chemistry was supposed to be open, co-operative, and hence cumulative. His famous *Alchemia* was an attempt to embody chemical doctrine in a form that made it as communicable as possible.

Golinski sees Hannaway's approach as opposing that of Metzger and Crosland, according to whom, we may say that the language of chemistry changed because chemists decided to start talking more clearly, using mechanistic philosophy and corresponding terms for the purpose. Chemists even could have made such a decision, but the reason of that certainly lay in communicative needs. Knowledge can only be acquired from other people, e.g., from the texts written by other people, if one can read them, and at special institutions, arranged for the purpose of the spread of knowledge. The institutions of learning and teaching, for example universities, can exist only as long as there is a demand for academic knowledge.

According to Golinski, the social basis for the new model of chemical communication had to be constructed historically. The 17–18th century chemistry might be characterised by an expanding market for printed chemical texts, whereas besides that there also was a kind of restricted communication between chemists: chemical and other technological secrets were exchanged between one another, to the advantage of both participants. The new kind of communication offered opportunities for power, and new careers, which concerned institutions, e.g., universities. Crosland (1963: 370) notes that there were close connections between chemical theory and the industries producing porcelain, dyes and gunpowder.

The 'oxygen-phlogiston revolution' at the end of the 18th century, often considered as the revolution establishing scientific paradigm in chemistry, a revolution in the internalist sense, appears to be problematic when seen from a different historiographical perspective. Christie and Golinski (1982: 259) find that there was nothing that Lavoisier said about oxygen and phlogiston that Edinburgh chemists had not said earlier. Lavoisier was not 'the final chapter of the influence of Newtonian matter-theory' regarding eighteenth century chemistry (Christie and Golinski, 1982: 258). Different chemical communities were occupied with different theoretical and empirical issues, thus, 'the revolution' was variable. For Hannaway, e.g., Lavoisier's revolution consists in the decision to embody the new chemistry (new nomenclature) in an elementary textbook which was a realisation of the power of the word for chemistry (Christie and Golinski, 1982: 260–61). Consequently, Boerhaave or his students from Edinburgh, could be regarded as revolutionary in chemistry as well, especially in the light of Lindeboom's statement that Boerhaave was considered as the first scientific chemist by his contemporary scientists.

3. BOERHAAVE AS A CHEMIST

According to Lindeboom, Boerhaave was an iatrophysicist who was interested in chemistry. However, it was Boerhaave who had brought chemical practice into the university. Thus, he himself had introduced a certain criterion for the distinction between science and art: since then, sciences belonged to institutions like universities, arts such as alchemy, iatrochemistry, and botany were developed outside of the academic institutions, in drug stores, etc. Boerhaave, himself, uses both the term "Science," and the term "Art" about (even university) chemistry. Some historians interpret the emphasis on "Art" as a rhetorical intention to convince his audience of the need to develop chemistry as a "Science," the others, such as Lindeboom, point out that in Boerhaave's time, the terms "Art" and "Science" were synonymous. Otherwise, Boerhaave would have preferred "Science" because what he meant, was *science*. Christie and Golinski refer to the unauthorised publication of Boerhaave's lectures, from the year 1727, where Boerhaave had compared chemistry with the art of sculpting. Both *artists* intend particular effects in the material world, both require material tools, and a principle of effective knowledge. Effective knowledge for Boerhaave meant communicable knowledge. (Christie and Golinski, 1982: 248)[9]. For both *artists* the concept of 'instrument' is a central one: all the Aristotelian elements were defined as instruments, having a similar function in relation to the concept of art. Instrument is needed to attain the intended aim of the artist, so "fire" or any other "instrument" all equally served didactic aims.

Not only chemistry, but also the other sciences, such as geometry, botany, etc. were taken to be arts. It can be understood as a popular metaphorical explanation of the sciences by a demonstration of their the practical connections. The turn from purely scholastic writing to experimental science capable of solving practical problems was, perhaps, the main revolutionary change in the academic tradition. Theory became related to practice. It is not then surprising that in the period of transition the terms "Sciences" and "Arts" were confused. Even the greatest scientists dealt with the alchemical experiments: Newton, Boyle, as well as Boerhaave who boiled mercury during a period of almost sixteen years until a careless student broke the vessel. Being himself critical about alchemy and iatrochemistry, he admitted that important facts could be found by alchemists' observations.

According to Boerhaave, the movement in chemistry from art (*techne*) to science was possible because chemistry was capable of correcting her mistakes — to abandon the magical and mystical basis of alchemy, the dream of gold-making, exaggeration with the idea of effervescence and the wrong interpretation of fire as an immaterial substance. Therefore he assured in his inaugural speech *Discourse on Chemistry Purging Itself of Its Own Errors*, in 1718, that

> while acknowledging that Science is strewn with the chemists errors, I shall try to prove that these same errors have been most successfully wiped out, solely by the efforts of these same chemists. (Boerhaave's Orations, 1983: 194)[10]

The principal mistake, from his point of view, was uncritical application of

chemical ideas to medicine, and consideration of physiological processes as similar to those of chemistry. In the speech he criticised van Helmont, Paracelsus and his teacher Sylvius for these kinds of errors.

In his textbook *Elementa Chemiae*, Boerhaave defines chemistry as

> an Art that teaches us how to perform certain physical operations, by which bodies that are discernible by the senses, or that may be rendered so, and that are capable of being contained in vessels, may be thence produced, and the causes of those effects understood by the effects themselves, to the manifold improvement of various Arts. (Lindeboom, 1968: 328)

Boerhaave's chemistry was a branch of Newton's physics. Its various phenomena could generally be explained simply in terms of motion. When water dissolved salts, this was simply by the interior motion of its particles. These particles were in fact atoms — the solid, massy, hard, impenetrable, movable particles of Newton. Boerhaave speculated on the different sizes of atoms, which would serve to explain certain chemical reactions. He believed that mathematics could be usefully applied to chemistry, his approach was more quantitative than usual at that time, e.g. it was not every chemist who when referring to the solubility of a salt would mention temperature. (Crosland, 1963: 393–397)

Boerhaave's most characteristic quality was exactness, he had formulated seven rules for doing experiments, and he followed these rules strictly. His writings are systematic, clear and accurate. His main textbook in chemistry *Elementa Chemiae* covers systematically all the chemical knowledge in Europe at that time, and was used in many universities as a textbook for many years.[11]

Boerhaave introduced the microscope into chemical research.

Because of his practical interests and with purpose of the treatment of his patients, Boerhaave examined carefully the nature of milk, eggs, cream and other organic products. Only, his physical method and critical attitude towards *iatrochemists'* studies on effervescence did not allow him to realise the importance of biochemistry within its whole scale.

Data from experiments, which he had completed, spread widely among other scientists, and were approved.

Thus, he did everything to develop chemistry as a science, as he had promised to do in his *Discourse on Chemistry Purging itself of its Own Errors*, 1718. First of all, he had taught the new generation to understand the importance of chemistry. With great pathos he turned to his students in the end of the *Discourse on ...* (*Boerhaave's Orations*, 1983: 212–213) and expressed his gratitude to them whose demand had forced him to resume teaching and working on chemistry year after year.

4. CONCLUSIONS

At the beginning of the essay, I asked why was Boerhaave so famous all over the Europe in the 18th century? And why is he almost forgotten by the end of 20th century?

Boerhaave was famous because of his *practical utopia*: didactically orientated research in the sciences with practical aims of medicine. He was even somewhat eclectic, joining together different theories and traditions, but joining only to the extent the theories enabled communication and co-operation to solve some practical problem. Aant Elzinga and Andrew Jamison (1984) find that his fame was based on the school he created and which continued his tradition that might be called practical utopia, pedagogical utopia, oriented to systematisation and organisation of scientific training at the university. He had a catalytic influence on the creation of, e.g., the Edinburgh school of medicine and chemistry, and therefore also impact on British 18th century medicine and chemistry generally.

He did not make any scientific discovery. His only invention was a greenhouse heater (Cunningham, 1986: 41). But as Elzinga and Jamison indicate, in early 18th century science:

> It is the social meaning, generalist ambitions, and external service orientation that dominate. (Elzinga and Jamison, 1984: 162)

Boerhaave's *practical utopia* suited well the social environment of the early eighteenth century. Leyden in the Republic of Netherlands was a real citadel of tolerance because the University of Leyden was open to all students, irrespective of nationality or creed. This was different from the Oxford and Cambridge Universities, which admitted only members of the Anglican Church, whereas many other universities in Europe were under Catholic control. State and university authorities gave support to his chemical and medical studies. There were plenty of donations to his laboratory. It is because

> the emphasis lay more on social utility and meaning than on scientific growth in any narrow sense. (Elzinga and Jamison, 1984: 161).

Crosland (1963: 370) indicates that Boerhaave's textbook and even notes from his lectures easily found readers. There was a market for the systematised representation of chemical knowledge. It was certainly related to the increasing need for applicable knowledge, necessary, for example, for the manufacture of porcelain, gunpowder, dyes, etc. Chemistry had become a popular science by the second half of the 18th century, and chemistry had been firmly established as one of the physical sciences — namely with the accent on the physical sciences. Here the answer to the second question becomes transparent. Boerhaave applied Newtonian mechanics and its extension, the theory of affinity to a new area of research — chemistry, whereas chemistry itself remained secondary or

an assistant science in comparison to physics. It was science to the extent it contained physics. On the other hand, chemistry served medicine. Chemistry described few phenomena unobtainable to the other sciences. Chemistry turned to be a science in comparison to alchemy or iatrochemistry because it became independent from the main mistake of the latter, magic and mystic explanations criticised in Boerhaave's famous Oration, *Discourse on Chemistry ...* . This can be regarded as a credit to Boerhaave. Also, his influence on Scottish and French schools is remarkable. But he did not complete the change in the paradigm, that was left for Lavoisier, who linked French pneumo-chemistry together with Newtonian tradition. It must be mentioned that Lavoisier could be seen as one of the pupils — as a reader of the textbook — of Hermann Boerhaave, *Communis Europae Praeceptor*.

Unfortunately, the history of science textbooks often overlook great teachers and applicants of scientific findings whose influence on the discoveries and discoverers cannot be over-estimated. Perhaps the great teachers of the past will receive more attention when the intrinsic model of history of science gathers strength and comes to replace the internalist "big pictures" of great discoveries which still tend to be favoured, if not in research environment, still in popular publications and textbooks.

University of Tartu

NOTES

1 See for instance (Brock, 1992: 37, 77, 108, 133), (Butterfield, 1949/1980: 205). According to G. A. Lindeboom (1968), about one third of his students came from English-speaking countries, where, at that time, no good medical education could be obtained, large number of students came from German-speaking countries, so, relatively to the other national groups only minority were Dutch. See his *Herman Boerhaave: The Man and his Work* (1968), p. 363.

2 See a recent work of Jan Golinski (1998) where he describes the main historiographical changes in the last few decades.

3 See for example Lakatos (1971), Laudan (1977 and 1981), Merton (1973).

4 Such an attempt has been made by some sociologists of scientific knowledge in the 80's. See especially H. M. Collins (1981). The social constructivism preferred pure social explanation to rational.

5 See Gillispie (1960), *The Edge of Objectivity: An Essay on the History of Scientific Ideas*

6 The appointment as a university lecturer involved right to deliver private lectures (Lindeboom, 1968).

7 See H. Butterfield (1949/1980), Ch. XI, "The Postponed Scientific Revolution in Chemistry," pp. 191–209.

8 Referred via Golinski (1993: 372).

9 Didactic practice as an aim was emphasised only in the first unauthorised issue of Boerhaave's *New Method of Chemistry 1727* known through a translation of Peter Shaw, based on notes from Boerhaave's lectures. The didactic aspect was decreased in the *Elementa Chemiae* 1735

published by Boerhaave himself.

[10] "Discourse on Chemistry Purging Itself of Its Own Errors," in *Boerhaave's Orations* (1983).

[11] The University of Tartu was among the others, there are several copies of Boerhaave's textbook in French and Latin available in the *Bibliotheca Universitatis Tartuensis* from the 18th and 19th centuries.

REFERENCES

Boerhaave's Orations 1983. Translated with introductions and notes by E. Kegel-Brinkgreve and A. M. Luyendijk-Elshout, Leiden: E. J. Brill/Leiden University Press.

Brock, W. H. 1992 *The Fontana History of Chemistry*. London: Fontana Press.

Butterfield, H. 1949/1980 *The Origins of Modern Science 1300–1800*. London: Bell & Hyman.

Christie, J. R. R. and Golinski J. V. 1982 'The Spreading of the Word: New Directions in the Historiography of Chemistry 1600–1800'. *History of Science*, 20: 235–266.

Collins, H. M. 1981 'What is TRASP? The Radical Programme as a Methodological Imperative' *Philosophy of Social Science*, 11: 215–224.

Crosland, M. 1963 'The Development of chemistry in the eighteenth century'. *Studies on Voltaire and the Eighteenth Century*, XXIV: 369–441.

Cunningham, A. 1986 'Medicine to calm the mind: Boerhaave's system, and why it was adopted in Edinburgh'. In A. Cunningham and R. French (eds.) *The Medical Enlightenment of the Eighteenth Century*, 41–66.

Elzinga, A. and Jamison, A. 1984 'Making Dreams Come True: An Essay on the Role of Practical Utopias in Science'. In E. Mendelsohn and H. Nowotny (eds.) *Nineteen Eighty-Four: Science between Utopia and Dystopia. Sociology of the Sciences*, VIII: 147–172.

Gillispie, C. C. 1960 *The Edge of Objectivity: An Essay on the History of Scientific Ideas*. Princeton, NJ: Princeton University Press.

Golinski, J. V. 1993 'Chemistry in the Scientific Revolution: Problems of language and communication'. In D. C. Lindberg and R. S. Westman (eds.) *Reappraisals of the Scientific Revolution*, 367–396. Cambridge/New York/Port Chester: Cambridge University Press.

Golinski, J. V. 1998 *Making Natural Knowledge: Constructivism and the History of Science*. Cambridge: Cambridge University Press.

Lakatos, I. 1971 'History of Science and Its Rational Reconstructions'. In R. C. Buck and R. S. Cohen (eds.) *PSA 1970 In Memory of Rudolf Carnap. Proceedings of the 1970 Biennial Meeting. Philosophy of Science Association, Boston Studies in the Philosophy of Science*, VIII: 91–136, Dordrecht: Reidel.

Laudan, L. 1977 *Progress and Its Problems. Towards a Theory of Scientific Growth*. Berkeley/Los Angeles/London: University of California Press.

Laudan, L. 1981 'The Pseudo-Science of Science?' *Philosophy of Social Science*, 11: 173–98.

Lindeboom, G. A. 1968 *Herman Boerhaave. The Man and his Work*. London: Methuen & Co Ltd.

McMullin, E. 1987 'Scientific controversy and its termination'. In H. Tristram Engelhardt, Jr. and A. L. Caplan (eds.) *Scientific controversies. Case studies in the resolution and closure of disputes in science and technology*, 49–91. Cambridge/London/New York: Cambridge University Press.

Nilsson, I. 1984 'Vetenskapshistoria. Att studera vetenskapens föröndring'. I J. Bärmark (red.) *Forskning om forskning eller Konsten att beskriva en elefant*, 105–141. Lund: Natur och Kultur.

Smith, R. 1997 *The Fontana History of the Human Sciences*. London: Fontana Press.

Young, R. 1973 'The Historiographic and Ideological Contexts of the Nineteenth Century Debate on Man's Place in Nature'. In M. Teich and R. Young (eds.) *Changing Perspectives in the History of Science. Essays in Honour of Joseph Needham*, 344–438. London: Heinemann.

LEO NÄPINEN

THE PROBLEM OF THE RELATIONSHIP BETWEEN HUMAN AND PHYSICAL REALITIES IN ILYA PRIGOGINE'S PARADIGM OF SELF-ORGANISATION

1. INTRODUCTION

The relationship mentioned in the title began to pose a problem in connection with the emergence of exact sciences in modern times. Until most recent times, the ideal of exact sciences has been "pure," that is to say, subject-free, objectivity, i.e. the description of the world in the form of objects, not human activities. Everything connected with humans and their everyday life — free will, chance, irreversibility, complexity, quality, inexactitude, unpredictability, etc. — have been considered factors which disturb scientific objectivity. Thus, somehow we have had to deal with two worlds (realities) standing apart from each other. In literature, this problem has been called the problem of two cultures: one of them is scientific or technological and the other — the remaining cultural tradition. The former, unlike the latter, does not recognise any indeterminacy in reality. It finds that the physical world can be captured only by means of purely mathematical thinking. In western philosophy, this problem dates back to Descartes. Descartes' treatment of the world remained mechanical and mathematical. He doubted nearly everything except knowledge acquired through mathematics. He excluded indeterminacy from scientific rationality. The opposition existing between the two cultures, (scientific-technological and social-humanitarian), has also been stressed by Kant, Heidegger, Koyré, and others.

Such treatment of the world, which excludes humans, has also been preserved in the theories of exact science of the 20th century. For example, Einstein's general theory of relativity can be regarded as a form of Descartes' extent (spatiality) elaborated in detail.

Still, in recent years the gap between the two worlds (the physical universe and human everyday life and experience) and correspondingly the two types of sciences (mathematical natural science, and humanities and social sciences) has begun to disappear. Signs of this can be noticed in the theoretical studies of Ilya Prigogine and his colleagues on self organisation of the physical world (Prigogine, 1980; Prigogine and Stengers, 1977; 1984, 1994). The main aim of the present article is to observe how, and to what extent, Prigogine has managed, on the theoretical level, to unite the "two realities" (and correspondingly the