# What Kind of Scientist is a Physical Chemist or a Biochemist? Reflections on Scientific Identity and Institutionalisation in Science

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These thoughts are the result of a need to reflect on the concepts of "institutionalisation in science" and "scientific identity", which the author's earlier study of the development of science in Sweden (mostly on chemistry) raised.

A suitable starting point is the well known member of "das wilde Heer der Ionier" ("the wild horde of the Ionians"), Svante Arrhenius. He received the Nobel Prize in chemistry, was Professor of Physics, and Director of the Nobel Institute for Physical Chemistry in Stockholm. In Swedish encyclopaedias he has not only been called, chemist, physicist, physical chemist, but also scientist, natural scientist etc. So, what was his disciplinary identity? Is this at all a meaningful question, in his case and in the history of science in general? Is it meaningful to ask, what are the differences between "molecular biology" and "microbiology", or between "physiology and medical physics" and "pharmaceutical biochemistry", or between "scientific biochemistry" and "bioorganic chemistry", and thereafter use these concepts to describe what was going on in the history of science? And if it is not, which seems to be the implicit answer to the question posed, why are they used at all?

As the example of Arrhenius shows, "discipline" is a problematic concept. Frontiers between disciplines are always vague and changing, today as well as in historical times. What is called physics today is not what it was called yesterday, and the same goes for physical chemistry or biochemistry. This is well known, but people still write books, articles, arrange conferences on the question of disciplinary identity and on the histories of disciplines. On the scholarly map one can place oneself with the help of disciplines. For example, "I am a historian of chemistry /physics/biology/geochemistry". Earlier writings in history of science have usually used the concept of discipline unreflectingly: we have "History of physiological chemistry/biochemistry" etc. It seems as if we can't do without disciplinary titles.

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The same seems to be valid for other vague terms often connected to it, such as research schools, research traditions, and not least institutionalisation and specialisation.

These reflections on discipline, institutionalisation and identity are also a modest attempt to create some kind of tool by which we could discuss the emergence of new disciplines, their institutionalisation, and how that could be related to ideas on the scientist's disciplinary identity. The hope is at least that these reflections will allow the arrangement of what we already know in a little different way.

## The Organisation of Science

Some kind of organisation, sometimes pejoratively called bureaucracy, is needed for any science at all to be done. There has to be a structure, by which research is organised, money allocated, and teaching carried out. The basic unit in such an organisation is usually a University institution, an administrative unit which nearly always carries a disciplinary name.

In the following, such an administrative unit will be considered. It is a unit, which has to be officially approved and the Director should have the title of Professor, or the equivalent. It should have economical support from outside, its own budget, and not the least, a sign above the door, put there by someone else than those working at the institution. At Universities administrative units take part in the decision making process. They are, in general, respected members of the scientific community. The advantage for science is obvious, it gives a stronger position to the scientist from which he or she could buttress the kind of science they think is important. But, however necessary, research is not only a question of organisation.

What is called institutionalisation herein, that is, the creation of an independent institution of the kind just described, has its prerequisite in the emergence of new research areas. Institutionalisation is the whole process from the first budding signs of such a new research area to the complete and approved institution accepted by the scientific community. This process it is suggested consists of two main parts. The first part is the *inner institutionalisation*, which means the emergence of a new research field, vaguely characterised by a common goal, and common practices and theories around a central question. It should be distinguishable from other kinds of research, and slowly identified as a separate and more or less specific field. The second part is called *outer institutionalisation* which means the establishment of formal institutions, positions, laboratories, and journals, all dedicated to the new research field.

The two phases of institutionalisation are of course related to each other. The rise of new institutions cannot be explained by the use of only *inner-* or *outer- institutionalisation*, or by letting one of them become the only cause. Both factors are needed and they interplay constantly during the whole process of institutionalisation, but with the basic relation that *inner institutionalisation* precedes *outer-institutionalisation* chronologically. There must be in existence a fairly well defined research area before a formal institution can be established. A new institution does not, of course, start by someone inventing a disciplinary name such as "physical chemistry", or "biochemistry", and thereafter decide what kind of scientific research to do, and as the next step asks for money to build an institution, to create positions, etc. There has to be something which can be institutionalised. First comes a scientific content, thereafter an institution. This might seem to be a rather old-fashioned internal way of looking at the emergence of new disciplines and institutions. But it only seems old-fashioned. First: that the *inner institutionalisation* in time precedes the *outer* one, does not mean that it determines it. Second: internalism cannot explain why only certain areas become institutionalised, and others do not. Third: the development of the cognitive content of a new research area still depends on cultural, social, economic and other factors. Fourth: for a new discipline to be established a lot of lobbying and fund-raising is necessary, activities that are not of a scientific nature.

There are two very obvious consequences of separating inner and outer institutionalisation. The first is so obvious that there is a risk that we neglect it. When studying institutionalisation in science, it is not enough to count, Chairs, positions, journals etc. A sociological approach of that kind is necessary but it is not sufficient. One has to study what the scientists are doing in their laboratory, during the phase when direct political pressure is relatively weak.

The second consequence is just as obvious: namely that institutionalisation of new disciplines take time. Even if this is known, it is often hidden behind words which imply sudden changes, such as, "break-through", "revolution", "a new era" and the like. The symbolic act of "cutting the ribbon" is a confirmation that a new discipline and/or a new institution has come into existence, not a sign that institutionalisation has begun. The cut ribbon rather initiates the beginning of a second stage of institutional development, the enthusiastic time, during which a new research field, which already has passed *inner institutionalisation* is given the administrative possibilities to mature into a full-fledged science.

## The Institutionalisation of Physical Chemistry

The following two sections are an attempt to apply the above ideas on the institutionalisation of physical chemistry and more specifically, of biochemistry in Sweden in the beginning of the $20^{th}$ century.

When did physical chemistry begin? The traditional view is it started with "das wilde Heer der Ionier", Ostwald's *Institut für physikalische Chemie* in Leipzig and *Journal für physikalische Chemie*, 1887. All these events are certainly historically important. But the line between physics and chemistry has always been blurred, and the use of physical methods in chemistry is an old phenomenon. For example, Johan Gottschalk Wallerius published in 1759-1768 the impresive *Chemia Physica* (in Latin but also translated into German). Does that makes him a physical chemist? Did English chemists during the eighteenth century, for example Joseph Black, do physical chemistry when applying Newtonian physics to chemistry? And Lavoisier, who called himself experimental physicist, was he doing physical chemistry? The present author considers it would be a misleading use of words to call Lavoisier a physical chemist, and has doubts that any historical analysis would profit from so doing. To try to answer questions of that kind appears to be more like scholastics, where a definite meaning of every word is taken for granted, than nowadays scholarly works.

But it is perhaps more than just a scholastic question when one learns that the Swedish Docent Otto Pettersson in 1860's lectured in Uppsala on "physical chemistry", especially since he was one of the few scientists in Sweden who, twenty years later, supported Arrhenius when the latter published his theory of dissociation. How much physical chemistry was around when Arrhenius and the others appeared on the scene? To answer such questions by defining physical chemistry seems meaningless. A better understanding of the relation between Pettersson and Arrhenius can be reached by examining their sciences and the links between them: what did Pettersson do, what did Arrhenius do? To quarrel about what we shall call it or describe what they "really" were doing seems to lead nowhere.

In 1884 Arrhenius defended his thesis and many stories have been told about the event, usually with Arrhenius as the prime witness. His thesis was only just approved by the members of the committee, and it did not receive a high grade, but as the myth-makers like to say, twenty years later it gave him the Nobel Prize. Regardless this and the many other picturesque details around his dissertation, it is true that Arrhenius experienced problems in Uppsala. The fundamental reason for this conflict was that the field of research that he had chosen, electrical properties of electrolytes, did not exist in Uppsala, and did not fit into the existing research traditions. Arrhenius rather drew on the well developed research tradition in Germany, in which Kohlrausch, Hittorf and others, had long before studied the electrical properties of electrolytes, and to whom Arrhenius frequently referred. However, there were virtually no references to Swedish scientists in Arrhenius', almost 150 pages long, thesis. In Uppsala his thesis fell outside existing research traditions both in chemistry and in physics. When the young student Arrhenius came up to the Professors in Chemistry and Physics in Uppsala and explained that sodium ions existed free in water, their reaction is more than understandable.

But less than six months after the public defence of the thesis, Arrhenius was appointed *Docent* in Physical Chemistry in Uppsala. Even if *Docent* was just an honorary title, one reason for this quick recognition was that Arrhenius had found support from an international research field. Especially from Wilhelm Ostwald, chemist, physicist, Nobel laureate, monist, philosopher etc, at this time Professor in Chemistry in Riga, and later from Jacobus van't Hoff, Professor of Chemistry, Mineralogy and Geology, and later Honorary Professor and Member of the Royal Prussian Academy. They were all working in the same research tradition, but as seen, under a host of different institutional names. Under all circumstances it is considered that with the initiated cooperation between these three men the study of the electrical properties of electrolytes in the middle of the 1880s had reached the stage of *inner institutionalisation*, but that it happened within many different institutional contexts.

So let us turn to *outer institutionalisation*. The proverbial ribbon was cut when Ostwald's Institute was opened in 1887, and the name used for the new research area was also used as a name for a new institution. However, *outer institutionalisation* is a long process and not by necessity connected to a specific disciplinary name. In Stockholm the research field became institutionalised at Stockholm University when Arrhenius was appointed Professor of Physics in 1895. From this position Arrhenius could propagate his new science and continue to expand the new research area, electrical properties in electrolyte solutions, into new fields like immunochemistry and cosmic physics. The name above the door could thus differ when it came to outer institutionalisation. It was not until 1908 that the name Physical Chemistry was used for an institute headed by Arrhenius, namely, the Nobel Institute for Physical Chemistry, Stockholm.

The establishment of the first Chair formally designated as Physical Chemistry in Sweden was in Uppsala in 1912. The Chair was especially created for The Svedberg. Arrhenius supported the idea to designate a Chair for Svedberg, but there were no collaborations at all between the two scientists. One reason was that Arrhenius did not do much innovative science during this epoch. His institute was declining, and can be called one the greatest scientific failures in the history of science in Sweden. This statement deserves a more thorough study, not in order to dethrone Arrhenius, but since failures are often more interesting and tell more about how science functions in society than do the successful cases. How come that the successful "wild horde" failed here? This is a serious question however relative is the concept of failure.

Perhaps a better explanation for the non-existence of collaborations between Svedberg and Arrhenius is that their research areas differed. When matriculating at Uppsala University Svedberg immediately took up the study of colloid chemistry, a field with no research tradition in Sweden, but one that was internationally strong and growing. Svedberg chose colloid chemistry because he wanted to prove the existence of the atoms. Therefore he constructed an ultra-microscope by which he thought it would be possible to see colloid particles the size of an atom by direct observation. By this time Svedberg considered himself a colloid chemist, and as such he became an important member of an international colloid network.

Svedberg and Arrhenius thus worked in two different research traditions, both of which received their outer institutionalisation under the same name: physical chemistry, in Uppsala, as a Department of the University, and in Stockholm, as the Nobel Institute of Physical Chemistry.

After having become Chair-holder in physical chemistry in 1912, and thus a "physical chemist", Svedberg concentrated on the study of variation in particle size in colloids. For this reason he turned to proteins, each protein by then was considered to consist of very small particles (molecules) with varying size. Svedberg constructed the ultra-centrifuge, in order to measure sedimentation speeds, and thus molecular weights. With the decline of colloid chemistry in the 1920's the research field changed into the field, "study of the physical properties of large chemical molecules", but all the time Svedberg retained his position as Professor of Physical Chemistry. It was in this research tradition that Svedberg's most well-known student, Arne Tiselius, worked. In line with the object of the research area, to determine physical properties of large chemical molecules, Svedberg suggested Tiselius to try measuring their electric characteristics. This was the beginning of the development of electrophoresis.

## The Institutionalisation of Biochemistry in Sweden

By the middle of the 1930s Tiselius seemed to be without any possibilities to continue in science, due to the curious Swedish University system. Lobbying started and after a private donation, Tiselius in 1939 was appointed Professor in

Biochemistry at Uppsala University. Under this name he continued to work, more or less in the same way, as he had been doing as an assistant to Svedberg in the Department of Physical Chemistry.

Hans von Euler had been Professor of Organic Chemistry at Stockholm University since 1905. He worked in the new research field of enzyme chemistry, where he belonged to an international network with close contacts especially with Germany. From his position as Professor in Organic Chemistry, he created the Biochemical Institute at the University around 1930, placing himself as its Head. The Institute was established to support his studies in the research field of enzyme chemistry. Enzyme chemistry thus received its outer institutionalisation in the form of a Biochemical Institute, headed by a Professor of Organic Chemistry.

Within the research tradition at Euler's institute, fermentation chemistry soon became a research area in itself, and Euler's pupil Karl Myrbäck was in 1932 given a Personal Chair in Fermentation Chemistry, donated by the brewer's association in Stockholm. In 1947 the chair was turned into a tenured position and renamed Organic Chemistry and Biochemistry. Finally, in 1963, Myrbäck became Professor of Biochemistry but all the time he continued to work on fermentation.

In the 1930s another Chair in Biochemistry was established at the Karolinian Institute, where the physician Hugo Theorell had created a Department of Biochemistry. This department's background was medical, and it had few, if any, similarities with the different research traditions in which Tiselius and Euler had been trained.

From these two cases, physical chemistry and biochemistry, it is obvious that the sign above the entrance to a department does not automatically tell what kind of research is going on behind that door. The difference in types of work behind the same name can be immense, and behind different names we can also find similar research. The scientists themselves did not seem to bother what to call their research areas. Arrhenius was satisfied by being Professor in Physics, and Euler did not mind keeping his position as Professor of Organic Chemistry at the same time that he was Head of a Biochemical Institute. In the published correspondence between Emil Fischer and Arrhenius the editors (Horst Remane and Levi Tansjö) point out that to both scientists, there was practically no difference between "allgemeine Chemie" and "physikalische Chemie". If someone would have called Arrhenius immunochemist or cosmic physicist, he would not have argued. Tiselius, as a pupil of Svedberg, all his life considered himself more of a physical chemist than a biochemist, but accepted without problems to be called Professor of Biochemistry.

## Some Uses of Disciplinary Names

The advantages with an *outer institutionalisation* of a research area have been mentioned: the scientists reach a secure position from which to carry out the kind of scientific work they want to do. The positions give the possibilities to encourage their own scientific specialities. That was enough, even if Euler certainly must have been pleased to hear Frederick Hopkins call him, “a biochemist in all but name”. If a scientist’s research area can profit by a formal change of disciplinary identity then every scientist will gladly do so. This is more important than to keep, or to continue to use, a certain disciplinary name. From his secure position as Professor of Organic chemistry, Euler supported the study of enzymes under the name of biochemistry; and Tiselius supported the study of large biochemically important substances under the name of physical chemistry from his position as Professor in Biochemistry.

For tactical reasons disciplinary names were often loaded with non-scientific meaning. When approaching donors, governments and other possible economical benefactors, a new disciplinary name is an argument that the donors will be supporting something new and something modern. This is also an argument by which the presumptive donor can be honoured as a modern progressive member of society, if he donates the money.

In the case of Tiselius, biochemistry was explicitly associated to the question of the origin of life, which made it even more tempting for donors-to-be. Donors were also certainly impressed by Tiselius’ and Svedberg’s intense cooperation with the Rockefeller Foundation in its programme for studying life with the help of chemistry and physics. The name biochemistry was easily associated with this research programme, but the choice of name was still fundamentally tactical, in this case it was a successful tactic.

Another use of disciplinary names should be mentioned. They can be used against competitors or to stop the career progression of other scientists. A referee or a committee member can state that this is not “physical chemistry”, thereby preventing another applicant from a competing research area from getting a position. Statements like that often tell what a referee or member means by a discipline, but many times they are used to stop a competitor one cannot stop by referring to competence in science. In this sense, once *outer institution-*

*alisation* has been achieved, disciplinary names may function conservatively since they can be used to prevent the emergence of new ideas. This happened for example with Arrhenius in Uppsala, when he tried to break into an old research tradition with new concepts and a new outlook on the properties of chemical solutions.

**Scientist's Identity**

Finally we come to the question of a scientist's identity. From what has been said it is clear that the identity as expressed in disciplinary names does not say much. So let us look at what some scientists themselves saw as their identity.

As mentioned, Tiselius had no tenured position by the middle of the thirties. The only Chair open for competition at the Universities was a Chair in Inorganic Chemistry. He decided to apply and in order to stand a better chance in the competition he decided to do some work in the field. He chose zeolites, something he never had worked with before. The choice of zeolites is understandable, since zeolites are inorganic substances with comparatively complex structure, and therefore in a sense closer to the research field of large molecules and complicated structures that Tiselius had been working with in biochemistry/physical chemistry.

By applying for the position, he wanted to promote himself and his own research, not any particular discipline. Contra factual questions are of dubitable interest in history, but the thought arises, that if Tiselius had been appointed Professor in Inorganic Chemistry, the way he would have developed the discipline would be in line with the traditions from Svedberg, as did his work in biochemistry, and that Tiselius would still consider himself basically belonging to the research tradition of Svedberg, a tradition called Physical Chemistry. The identity he wanted to keep was the identity of being a "scientist".

If a scientist wants to promote himself, to build an institution, raise money for a certain research area, etc, it is better to stress an identity as a "famous scientist", as a "Professor", or much better as a "Nobel laureate", than any disciplinary identity. Everyone knows that a Nobel laureate is a genius, whereas not everyone during the 1930's knew that the property of large biologically active chemical substances was an interesting research area. But for donors and benefactors to learn, or to be given an impression, which sciences are interesting they have to trust someone and such trust was created by fame, by the title of Professor, and particularly by the Nobel Prize. Nobel laureates in chemistry often consider themselves physicists; Arrhenius, and not least Ernest Rutherford are not the only cases, but they gladly accepted the prize. The idea that someone would say no to the prize just because it was the wrong discipline is absurd. The possibilities that the prestige of the prize gave were enormous, and it is certainly not by chance that the most important institution builders in Sweden during this epoch, Manne Siegbahn in physics, Hugo Theorell in medicine and Svedberg, Euler and Tiselius, in chemistry, were all Nobel laureates.

When in the middle of their careers, both Arrhenius and Svedberg received, during their scientific heydays, calls from abroad. This caused scientists in Sweden to act in order to "save" for Sweden, not a "physical chemist" or a "colloid chemist", but above all a "famous scientist". With that identity it is easier to raise money, and therefore also to succeed in *outer institutionalisation* in order to guarantee the continuity of a research field that already has passed the stage of *inner institutionalisation*.

But if money could be raised and status received by the scientist being a "famous scientist", instead of being "physical chemist" or "biochemist", and if disciplinary identity is of minor interest when it comes to *inner institutionalisation*, one key question to put is how really interesting is the question of disciplinary identity? Is belonging to a discipline or being famous the most important, when it comes to the institutionalisation of scientific work? Should we not, when studying institutionalisation look at what scientists are doing, rather than take for granted that what they say they are doing is what they are doing? I think we all agree on this, but the author also believes that our habit to think of disciplines as cognitive categories still can prevent us from asking some relevant questions. To think of disciplines, institutionalisation and identity in terms of *inner*- and *outer institutionalisation* might help to understand the complicated processes by which scientists create an identity, new institutions come into existence, and new disciplines emerge.

## Notes

1. This paper has not been discussing the use of disciplinary names as analytical tools. The interest has been in the actual use of such names. To use them as analytical tools in order to organise and to explain historical material is of course a possibility. However, in that case the use the historian makes of such a name automatically differs from how the actors use it; this makes them less suitable for this purpose.
2. The empirical material in this essay is mainly based on my article "Naturvetenskaplig institutionalisering: The Svedberg, Arne Tiselius och biokemin"[Institutionalisation of science: The

Svedberg, Arne Tiselius and biochemistry], in Sven Widmalm (red.), *Vetenskapsbärarna. Naturvetenskapen i det svenska samhället, 1880-1950* (Stockholm, 1999), 117-143, where empirical details and sources can be found.

## *Bibliographical postscript*

The classical work on the development of physical chemistry is Johns Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton, 1990). For Arrhenius see Elisabeth Crawford, *Arrhenius: From the Ionic Theory to the Greenhouse Effect* (Canton: Mass., 1996). There are no good full length biographies of Svedberg, Tiselius and Euler. Valuable information on the Swedish scientific scene around 1900 can be found in Sven Widmaln, *Det öppna laboratoriet: Uppsalafysiken och dess nätverk 1853-1910* [The open laboratory: Physics in Uppsala and its network 1853-1910] (Stockholm, 2001), and in Svante Lindqvist (ed), *Center on the Periphery: Historical Aspects of 20$^{th}$ Century Swedish Physics* (Canton: Mass., 1993).