Conference Reports

MODELS OF INTERDISCIPLINARY RESEARCH AND SUBDISCIPLINE FORMATION

Notes on the Conference "Between Physics and Biology: Chemical Sciences in the Twentieth Century", 29-30 May 1999, München, Germany

Under the patronage of the UNESCO, the international science community has been shaping itself during the past decades, apparently inspired by a mixture of Prussian style of social organization, Aristotelian-Porphyrian principles of conceptual division, and peculiarities of historical traditions. The INTERNATIONAL COUNCIL OF SCIENTIFIC UNIONS (ICSU) is now the widely acknowledged umbrella organization, under which, amongst a wealth of other *natural* science unions, the INTERNATIONAL UNION OF THE HISTORY AND PHILOSOPHY OF SCIENCE (IUHPS) is the umbrella organization designed for societies and associations related to the history and philosophy of science. Next in the hierarchy comes a DIVISION OF HISTORY OF SCIENCE (DHS) and a DIVISION OF LOGIC, METHODOLOGY AND PHILOSOPHY OF SCIENCE (DLMPS). If you suspect that the latter might belong to the FÉDÉRATION INTERNATIONALE DES SOCIÉTÉS DE PHILOSOPHIE (FISP), you should note that the ICSU generally covers only natural sciences.

There are two different, but indirectly related, reasons why I am telling this story. First, the conference, which I am going to comment on, was the first major conference of the recently (1998) founded COMMISSION ON THE HISTORY OF MODERN CHEMISTRY (CHMC) that is next in the organizational hierarchy: ICSU/IUHPS/DHS/CHMC. (I should add that the sister COMMISION ON THE HISTORY OF MODERN PHYSICS [CHMP] was also involved in the conference organization.) Second, a running topic of most of the talks of the conference was, more or less explicitly, subdiscipline formation – of course not in the line ICSU/IUHPS/DHS/CHMC, but somewhere in-between chemistry, physics, mathematics, biology, geology, cosmology, and technology. While the guiding principle of the formation of CHMC is a clear-cut object-based conceptual division, *i.e.* the historical focus on 20th century chemistry, all case studies presented in the conference prove that subdiscipline formation in the natural sciences is sometimes extremely intricate, and it even goes quite different ways. Of course, there are plenty of simply treatable cases in 20th century chemistry, comparable to the CHMC formation, as the ongoing fragmentation into 'element-x-chemistries' or 'substance-class-y-chemistries'; but no participant mentioned these less spectacular developments of mainstream chemistry. Instead, the focus was on interdisciplinary fields, mainly on previously hot topics that were quite often appreciated by one of the three relevant Nobel Committees; *e.g.* on quantum chemistry, computational chemistry, nuclear science, radiochemistry, surface and solid state science, geo- and cosmochemistry, biochemical genetics, molecular biology, biotechnology, polymer chemistry, materials science.

To say the least, the two-day conference was a unique opportunity to learn a great many details about the historical background of so many spectacular developments of 20th century science from distinguished experts of each of the fields. Thanks to the extraordinary organization by CHMC President CHRIS-

TOPH MEINEL assisted by CARSTEN REINHARDT, 79 participants from 15 different countries could enjoy four well-prepared half-day sections, each with three (precirculated!) high-quality papers, one commentator, and a lively discussion, all embedded in a very friendly atmosphere. Since many of the topics are too recent to be yet included in history of chemistry textbooks, the conference as well as the Commission – in a certain way encouraged by the *fin de siècle* – actually break new and important ground in the history of science.

I am too less a historian to be able to comment on the many historical details presented in each of the extremely interesting talks that were a great pleasure to listen to. However, looking from the (disciplinary) outside upon the conference as a whole, it was apparent to me that *subdiscipline formation* is one of the hot topics of historians of modern chemistry, despite the fact that 'subdiscipline formation' was not included in the conference title. Of course, the topic is not that new, since we have some excellent historical studies on the formation of physical chemistry, biochemistry, *etc.*, as well as a lot of categories to grasp the object of formation, such as 'discipline', 'research field', 'research program', 'paradigm', 'style of thinking', 'research school', *etc.*, depending on the weight given to object-based, problem-based, cognitive, methodological, sociological, psychological *etc.* aspects to define the identity of scientific activities. While the participants of the conference obviously relied on an object-based definition concerning their own scientific identity (CHMC), it was not always clear what category their prefix- of suffix-chemistry study cases were supposed to belong to. It is perhaps due to this lack of conceptual clarity or rigidity that the papers as a whole brought about an extraordinary rich pattern of both successful and unsuccessful dynamics of xy formation. By analyzing the papers from my selective and oversimplifying point of view, I extract the following remarkable diversity of models to describe interdisciplinary activities.

HELGE KRAGH (Denmark) describes the successful formation of "*geo-*, *astro-and cosmochemistry*" as the application of concepts, methods, and instruments of physical chemistry to problems of geo- and cosmo-sciences. The '*application model*' combines pre-existent tools of the field x with pre-existent problems of field y (at least at the beginning). In general, there is no strict necessity for a collaboration of x-scientists and y-scientists. The new science may also be performed either by x-scientists acquainted with y-problems or by y-scientists acquainted with x-techniques. The application model is also prevailing in MARIKA BLONDEL-MÉGRELIS' (France) specific Nancy case of 'theoretical chemistry', i.e. applying physical methods to chemistry.

ANDREAS KARACHALIOS (Germany) describes the development of pre-war physical organic chemistry in Italy as a "boundary discipline between physical chemistry, organic chemistry and quantum physics" that required an "amalgamation of different scientific concepts and languages". Unlike the 'application model', the 'amalgamation model', or to point out more clearly the chemical metaphor, the '*synthesis model*', has no preformatted functionality for each of the disciplines involved and allows for more than two parties to participate.

An even more open approach is presented by BERNADETTE BENSAUDE-VINCENT'S (France) description of the development of *materials sciences* as a coalition of experts from many different traditional disciplines, united by a problem based approach as well as by an anti-disciplinary attitude. The '*coalition model*' stresses that problems come from outside of traditional disciplines and that new techniques may be developed in the course of fruitful collaboration. Far from becoming a new discipline, the coalition is dynamic, perhaps only temporary, with varying partners and an open future.

In a more specific case study of the Cambridge school of *molecular biology*, SORAYA DE CHADAREVIAN (UK) emphasizes the coincidence of specific local research traditions and "institutional ecologies", among which are social and political institutions including research funding. The '*ecological model*' describes the formation of a discipline (surprisingly, her term is 'science' instead of 'discipline') in socio-biological terms; researchers coming from different traditional disciplines try to find and stabilize a new common identity of research practice within a niche of the socio-scientific environment.

YASU FURUKAWA's (Japan) describes the development of *polymer science* as a stepwise growth with changing teaching disciplines: After physical chemists had paid attention to colloids, organic chemists' began to cultivate colloids/polymers as their very own objects; later polymer science became "physicalized", before it has finally been expanded to biology. The '*growth model*', probably less of biological than of pedagogical origin, gives an account of the interdisciplinary evolution of a new field that *successively* benefits from various disciplines without being involved in disciplinary claims and struggles.

HANS JÖRG RHEINBERGER's (Germany) approach of *biochemical genetics*, his case study is on Alfred Kühns investigation of pigment formation in butterflies, stresses the inherent dialectics of what he calls 'experimental systems'. Once an experimental system is designed for a certain research program, researchers may be confronted with unexpected difficulties forcing them either to modify the experimental setting or to redefine the program, such that for instance other disciplines are getting involved. The '*dialectical model*' stresses the inherent dynamic of experimental systems that store, among other things, sort of calls for new forms of transdisciplinary research.

Another case of inherent driving forces is presented as part of the paper of ANA SIMÕES (Portugal) & KOSTAS GAVROGLU (Greece) on the history of *quantum chemistry*, the early phase of which is similar to the '*application model*'. However, with the application of computer calculation methods, the newly-fledged 'interdisciplinary discipline' tended to split again into a physics orientated and a chemistry orientated part, depending on whether classical chemical concepts should be retained and explicated in new terms, or erased in favor of pure calculations (calculational chemistry). The '*re-splitting model*' accounts for subdiscipline formation driven by inherent tensions that are inherited from traditional bias, disciplinary rivalry or imperialism.

Other papers reveal models of interdisciplinarity or putative interdisciplinarity that were less successful or turned out to be not at all interdisciplinary.

RUTH SIME (USA) in her study on the "*The Search for Artificial Elements*" points out how a certain functional division of labor and competence between physicists (Meitner) and chemists (Hahn, Straßmann) – the former directing the experiments through theory, the latter carrying them out including chemical analysis – led them astray for some years. Both parties started their common search for transuranic elements with wrong premises, and they blindly "trusted each other's expertise without always understanding each other's limitation". Despite the fact that they were finally lucky to discover nuclear fission instead of transuranic elements, the case shows that the '*functional division model*' does not guarantee interdisciplinary success.

While at first glance the *hunting for new elements* in the 1920s and 1930s seems to have been a transdisciplinary research program of chemists and physicists, BRIGITTE VAN TIGGELEN (Belgium) shows that both parties were following completely different approaches, incl. different notions of element, research methods and existence proofs. The '*parallelism model*' makes clear that putatively interdisciplinary research may substantially suffer from a lack of mutual understanding, and occasionally converts into counter-disciplinarity.

Now that we have already addressed the pseudo-interdisciplinarity, pseudo-novelty of interdisciplinary research is another interesting case. NICOLAS RASMUSSEN (Australia) in his study on "Biotechnology before the 'Biotech Revolution'" recalls that pre-war biotechnology in the US before the rise of genetic engineering showed already the same kind of setting of industrial-academic research, the same coalition of life sciences and technology, with the same promises and aims to manipulate life, though on a different level than DNA. By emphasizing continuity instead of rupture, the '*novelty myth model*' draws attention to the question who does actually benefit from the talk of 'novelty'.

The case that is probably most intricate to be pressed into my typology of models of interdisciplinary activities, is MARY JO NYE's (USA) biographical sketch of Michael Polanyi' scientific career. In essence, it is at first a story of his interdisciplinary research in solid state and surface science, performed at the wrong time and with wrong approaches, at least in the view of the contemporary academic credit awarding institutions. The second part of the story is Polanyi's disciplinary shift towards sociology and philosophy of science reflecting on the norms of scientific practice. While the first part reminds us that interdisciplinary research still needs to fit the present academic environment (*ecological model*), the second part raises the question what kind of model would be suitable for, say, philosophy of chemistry. Since I am personally too much involved in that field, a historian will certainly do better to answer that question one day.

In conclusion, the impressive diversity of models of interdisciplinary activity in the 'chemical sciences', implicitly presented in the conference, provoke two further questions. First, is each of the analyzed models peculiar to the corresponding interdisciplinary research field, perhaps even dependent on some contingent historical circumstances; or could the models be transferred to the understanding of other interdisciplinary developments? Moreover, do we get some general hints from the case studies about how a *successful* interdisciplinary research program should be designed? And secondly, to what extend are the analyzed models the outcome of the favorite analytical perspective of each of the corresponding authors and/or the results of my simplifying imagination?

The answers will certainly not be given at the next CHMC conference, because that is on *instrumental techniques of modern chemistry*, and it will take place in London, 5-6 August 2000. Inquiries concerning the CHMC as well as the next conference may be addressed to the Executive Secretary: *Dr. Peter Morris, The Science Museum, London SW7 2DD, UK.*

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THIRD ISPC SUMMER SYMPOSIUM Columbia, South Carolina, USA, July 28 - August 1, 1999

"Columbia can be very hot in summer", said the organizer to potential participants during the last conference (Cambridge 1998, see HYLE 4, p. 169-170), and so it was. DAVIS BAIRD – he and his team of the Department of Philosophy of the University of South Carolina did an excellent and brilliant job – had indeed not underestimated the situation, because this summer was one of the hottest ever since in the United States. Nevertheless, all approximately 45 participants from six countries (approx. 80% from the US) of the Third Summer Symposium on the Philosophy of Chemistry by the International Society for the Philosophy of Chemistry evidently enjoyed the conference. 33 oral papers were given, 12 on epistemology and general topics, 6 on historical issues, 5 about explanation in chemistry, 4 on theoretical concepts, two on ethics in chemistry, and one about didactics. Only few examples will be referred to very briefly here. It goes without saying that I do not intend to undervalue the contributions not mentioned.

TONY EDMONDS, a professional analytical chemist from Loughborough University, UK, spoke about "A Philosophical Approach to Analytical Chemistry". Edmonds approaches philosophy – in a very serious meaning – by refreshingly looking at new things rather than reasoning within stiff traditional frames. Thus, one of his results is that analytical chemists are performing a triad: purification, synthesis, and comparison. To a reasonable extend this is as striking as unusual, since at least part of the literature on general issues of analytical chemistry does not even discuss or mention one of these concepts at all. The participants will certainly remember for a long time Edmonds' demonstration "Counting Oranges" that referred to correct/false comparison. JOHANNES HUNGER, who received his Ph.D. in chemistry in Heidelberg and is now at the Centre for Philosophy of the Natural and Social Sciences of the LSE, London, UK, called his talk "Explaining Molecular Structures". He criticized traditional models of explanation in the natural sciences by using three examples: Neural Network simulations, Molecular Mechanics, and *ab initio* calculation methods. According to Hunger, van Fraassens theory is more promising in application to chemical explanation. Hunger claimed an autonomous character of chemical explanation and the need to adapt modern philosophical approaches. In his "Models and Material Theories" the organizer himself, philosopher Davis Baird made clear that there are good reasons to take instruments and material models into account whenever scientific knowledge is discussed. As one example, he took the DNA ball-and-stick model as applied by Watson and Crick and the likes. Such material models do have an impact on the making of scientific views. A highlight of his standard-setting talk was a video tape clip that shows the actor Goldblum in "Double Helix", puzzling around with pieces of a DNA model.

As a result of his valuable efforts during the last years, Davis Baird was elected as a member of the Scientific Committee of the ISPC (which now consists of AKEROYD, BAIRD, ROTHBART, RUTHENBERG, and SCERRI) during the business meeting. The next ISPC conference will take place in Poznan, Poland, August 7-10, 2000. (For information contact EWA ZIELONACKA-LIS, zielo@main.amu.edu.pl).

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