

# Shaping biomedical objects across history and philosophy: A conversation with Hans-Jörg Rheinberger

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**ABSTRACT:** Historical epistemology, according to the historian of science Hans-Jörg Rheinberger, is a space through which «to take experimental laboratory work into the realm of philosophy». This key concept, together with the crucial events and challenges of his career, were discussed in a public conversation which took place on the occasion of Rheinberger's retirement. By making sense of natural phenomena in the laboratory, the act of experimenting shapes the object; it is this shaping which became the core of Rheinberger's own research across biology and philosophy into history. For his intellectual agenda, a history of the life sciences so constructed became «epistemologically demanding».

**KEY WORDS:** Hans-Jörg Rheinberger, historiography of science, cultural history.

**PALABRAS CLAVE:** Hans-Jörg Rheinberger, historiografía de la ciencia, historia cultural.

A group of colleagues and friends met with Hans-Jörg Rheinberger on the evening of February 24th, 2011, at the *Residencia de Estudiantes* in Madrid. This discussion about his career was part of a workshop convened to pay tribute to Rheinberger, whose contributions had been influential in the career trajectories of all the attendees (\*). The event was conceived as

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a *Fest* on the occasion of his retirement, following more than 20 years of study in the history and philosophy of the life sciences. On that evening, an intense discussion with Rheinberger took place in the form of a round table, a choral interview in which the organizers and audience asked Rheinberger questions about his academic and intellectual background. The Residencia de Estudiantes kindly recorded the conversation, which we have transcribed, working hand with hand with Rheinberger. It is a dialogue in which the origins of Rheinberger's intellectual and scientific interests are recounted and commented on. To provide context, some early information about his youth and family life has been added to the original conversation and interview which, overall, provides a general insight into a career that brought Rheinberger to the history and epistemology of biology.

Hans-Jörg Rheinberger was born in Grabs (Switzerland), grew up in Vaduz (Liechtenstein), and studied biology, chemistry and philosophy at the universities of Tübingen and Berlin. He was awarded a masters degree in philosophy, a PhD in Biology, was researcher at the Max Planck Institute of Molecular Genetics in Berlin and Professor of History of Science at the universities of Lübeck and Göttingen, prior to being appointed Director of the Max Planck Institute for the History of Science in Berlin. In 1997, from this position, he developed a networking project entitled *The Cultural History of Heredity*, which involved scholars from Europe and the Americas. Rheinberger has carried out and promoted research into the history and epistemology of experimentation, and has contributed extensively to the development of studies and discussions on the history and philosophy of the life sciences.

Among his many significant publications, *Toward a history of epistemic things*, in which Rheinberger develops his biological epistemology, has been particularly influential<sup>1</sup>. This epistemology had many influences, including a combination of the German philosophical tradition with a post-Foucauldian approach, and the use of historical materialism to study the history of scientific practice, embedded in the scientific certainties of twentieth-century biology. *An epistemology of the concrete* and, together with Staffan Müller-Wille, *A cultural history of heredity*, are Rheinberger's most recent texts<sup>2</sup>.

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1. Rheinberger, Hans-Jörg. *Toward a history of epistemic things: Synthesizing proteins in the test tube*. Stanford: Stanford University Press; 1997.
  2. Rheinberger, Hans-Jörg. *An epistemology of the concrete: Twentieth-century histories of life*. Durham: Duke University Press; 2010; Müller-Wille, Staffan; Rheinberger, Hans-Jörg. *A cultural history of heredity*. Chicago: University of Chicago Press; 2012.

As is the case with a number of contemporary historians of science and medicine, Rheinberger's work as a historian and as a scientist have been mutually reinforcing, illustrating one of the many ways experimental practice can engage in a dialogue with the humanities. As a historian of the life sciences and as a biologist, he linked the spaces of history and biology, proposing a philosophical relationship between them. Rheinberger's contributions to the epistemology of the life sciences through his conceptualization of the practices of contemporary biological research and his circulation of such terms as «experimental system» and «epistemic thing» were discussed during this public conversation.

**María Jesús Santesmases:** The first question I would like to ask is about your childhood and youth, to what extent did your education —your *Bildung*— contain the origins of your interests and ambitions.

**Hans-Jörg Rheinberger:** I grew up in a micro-country, Liechtenstein, in an even tinier village, Vaduz, between the Rhine valley and the Alps. My first language was an Alemannic dialect. I only began to learn «High German» in my third year of primary school. Then I attended secondary school, the only «Gymnasium» in the country, led by the congregation of the Marist Brothers, who had fled from Nazi Germany to Liechtenstein in 1936. But politics was not an issue in my secondary school education. What left a deep imprint, however, was a particularly agile and multi-talented teacher. He gave lessons in Latin, German literature, philosophy, drawing, painting, typewriting, stenography, photography and biology. He sparked my interest in all of that. He even gathered a few pupils around him to learn Russian. He used to be one or two lessons ahead of us and transmit what he himself had learned the day before. He was of the opinion that Russian was one of the world languages that an educated citizen of our time should be able to understand. No political connotations. Just cultural curiosity, although one must not forget it was at the time of the «thaw». I think I learned a lot from Frater Ganss, as he was called. I heard of DNA for the first time in one of his biology classes in the early 1960s. I think it was him who convinced me that studying biochemistry would be a future-oriented decision. He himself was a doctor in philosophy whose dissertation had been on Seneca. And indeed, I started out my university education in biochemistry at the small German university town of Tübingen. Not without continuing to take lessons in Russian and proudly sitting in a café trying to read a Russian

newspaper available at the only kiosk with international press products in the center of the city.

**Santesmases:** And your family?

**Rheinberger:** A stimulating atmosphere reigned at home as well. My parents were both practicing medical doctors; had the economic situation of the family been different, my father probably would have chosen to become a historian. When my father toured the countryside in the afternoons to visit patients, he took us children with him. Quite early on, however, I decided that I would never become a doctor. My father knew everything about the history of the family and the country in which I grew up, together with my younger brother and sister. My father's archaeological interest had come down to him from his own father who had been the self-taught founder of local archaeology. He, my grandfather, was an artist and architect who had been educated at the Academy of Arts in Munich. We had a nineteenth-century composer in our family —Josef Rheinberger, who taught and lived in Munich as well— who was held in high esteem by my father who loved music and had played the violin as a young boy. I had piano lessons early on, from age nine if I remember correctly. My mother was more inclined to literature and had a substantial library at home —literary Catholicism I would say. A patient of my father's, a well-known German editor, was publishing a library of Russian world literature at the time— and we received all the volumes in the order they were published. We learned to love nature by spending the summer months in the mountains and touring all the peaks in the surrounding Alps. Science proper, however, was not an issue at home. And when I grew older I became quite annoyed by the weight of the traditional values that reigned supreme in the family.

**Matiana González-Silva:** Can you explain your personal career, and even more than explaining your trajectory, explain how these different areas of expertise —biology, history and philosophy— have mutually influenced each other in your intellectual trajectory?

**Rheinberger:** It might not be easy to explain this briefly, but I will try to be as brief as I can. I started out my academic life after secondary school at the University of Tübingen, in Germany, which at that time, in the middle of the 1960s, was still not part of the big world, but a small provincial place

to be: a very traditional German university town in which, interestingly enough, there had existed a number of Max Planck Institutes since the end of the Second World War. It was also the first university in Germany where one could study biochemistry with a diploma, a hybrid science between biology and chemistry that in the rest of Germany did not yet exist as an academically qualified discipline in its own right during the 1960s. I was fascinated by the perspective of being a student of a specialty that was really new and could only be studied here. That is how I started out.

After a year or so, however, I realized that this would not be a job for the rest of my life. I was disappointed by all the preliminary courses and exams one had to do there: taking a course in botany, taking a course in zoology, in anatomy, in mathematics, doing everything else but biochemistry. There was simply no biochemistry in this first year. And nobody explained why one had to do all this. I got the impression that probably I had chosen the wrong thing, and that I should try something else.

I abruptly shifted gears after a year and switched to philosophy, from a small scientific specialty to the queen discipline of the humanities. Tübingen was actually a good place to study philosophy at that time as a number of both nationally and internationally very well-known figures were there, among them Ernst Bloch and Walter Schulz. That switch found its continuation when I went from Tübingen to Berlin after another year in the province. In those times, if you wanted to graduate in a discipline like philosophy you would have to choose one or two additional minor subjects as your second and third specialties. It so happened that at that time in Berlin there was again something very new to be studied that did not exist in the rest of Germany: the Technical University had a chair for general linguistics in its Humanities Department. That meant one could study aspects of languages without connection to a particular language such as German or French. I was fascinated with this option, and it became my first minor. I should probably add at this point that language, writing poems included, had fascinated me since my adolescence. After having completed my master's thesis in philosophy in 1972, my aborted attempts to study biochemistry made themselves felt again. In addition, I had moved, with my thesis, to philosophy of science. So I thought it might be a good thing to somehow finish studying the science I had started out with. These were among the reasons why I turned back to the sciences. And since one could not study biochemistry proper in West Berlin at the Free University, I took biology and chemistry in parallel. It is a somewhat convoluted background,

but that is how I came to have one foot in the humanities and the other in the sciences.

**González-Silva:** Science as the subject of reflection in your philosophical and historical work is clearly informed by your activity as a scientist. But what about the other way round? How did your epistemological and historical works and thoughts affect your practice in the laboratory? How do you feel they might have had an influence?

**Rheinberger:** Actually, when I returned to biology and chemistry and started all over again to basically undergo a second academic education, I was very much permeated by my socialization in philosophy, and my idea was to go through that process as soon as possible and then get back to philosophy of science. But then things started to develop in a different way. When I had to do my diploma thesis in biology, I was lucky enough to get a place as a diploma student at one of the Max Planck Institutes in Berlin, the Max Planck Institute for Molecular Genetics, a job I almost failed to get because when I came to the interview I was naïve enough to tell the supervisor that I would like to do my diploma thesis and then quickly go back to philosophy. He was not very delighted by the prospect of having a diploma student who was only interested in working for a year and then, when he had learned the techniques in order to really tackle a serious problem, would disappear again. Nevertheless, he found it interesting enough that such a strange guy came and said «Actually I want to do philosophy but I need to finish my biology», that he told me to come back in a couple of months if I was still interested. I was finally accepted in the laboratory and after a year the problem was solved for me anyway: I had become so fascinated with laboratory work that I had forgotten my philosophical ambitions for the time being. So I simply went on with my experimental work, ending up with a PhD, and as was still the habit for academics in those days in Germany, my habilitation.

But coming back to your question: it was not easy for me to learn the lesson that all the beautiful philosophy of science I had read in books did not help me to do good experiments. I was rather confronted with having to forget this kind of training in thinking about generalities and musing about concepts. I was becoming immersed in a work that was much more bound to technical handling, to feeling one's way through in experimentation, and getting the questions back from the material one was working with, rather

than coming with a very important problem and then trying to solve it in the laboratory. My experience was that it worked the other way around: the questions arise from the laboratory bench, and they have to be solved in one way or the other by the means at hand. So if you are working in a laboratory that has no electron microscope, for example, you cannot use electron microscopy to solve your problem. You have to work with the technologies that are around, unless you are lucky to have the opportunity of becoming acquainted with another technology by getting a month or two off, and going into another laboratory in order to learn a technique that helps you solve your problem. But it is a very different way of going about questions if one is working as an experimental scientist. So I probably should briefly answer the question as follows: what I had to do at first was to forget about philosophy in order to become a working scientist.

**Miguel García-Sancho:** A specific space in which you sought to integrate the practice of biology with the epistemological study of this discipline as a historian of biology was the laboratory of Paul Zamecnik at the Massachusetts General Hospital in Boston. This was the laboratory in which transfer RNA was discovered and where an influential test tube system for synthesizing proteins was designed. This laboratory has been the main object of your book *Toward a history of epistemic things*, a book which has inspired a generation of historians and philosophers of biology. What led you to address this laboratory historically and to study the history of the events that took place in this particular setting?

**Rheinberger:** I must quickly come back to the former question and complete my reply, because otherwise I cannot answer this one. I did not forget about philosophy during the time I was in the laboratory. What I meant was that what I had learned in philosophy did not help me very much to do good experiments. But thinking in terms of philosophy had been a kind of experience that on the other hand I did not want to quit altogether. So in parallel and in my free time over the weekends, I continued to cultivate my interest in questions of philosophy. I even taught a little philosophy of science to science students, and gradually, over time, more history of science. I had come to the conclusion that if there was a lesson to be learned from doing experimental laboratory work and taking that over into the realm of philosophy, making it work in the humanities, what we needed to have was subject matter, things to work on. And it appeared

to me that a good way to work this through for a philosopher of science would be to turn oneself to historical questions. And so I started to see whether in one way or the other, I could make sense of the theoretical ideas of what science was by looking into the history of the sciences. This became my secondary occupation during all the time I was working in the laboratory: my spare time was devoted to what I today would call historical epistemology. At a certain point I had to make up my mind whether the second half of my academic career should be in molecular biology or in this historically-modified field of my early philosophical interest. After a long hesitation I came to the conclusion that I wanted to do the second, leaving the laboratory and really becoming a full-time, epistemologically-motivated historian of science.

Now to your question. The reason why I chose Paul Zamecnik's Laboratory is, speaking frankly, this: I went back in time about 10 years in the field in which I myself had been working as a molecular biologist and started to study the «prehistory» of the experiments I had carried out when working in the laboratory. For me that proved to be a very fruitful and rewarding exercise because in that way I could carry over a little bit of the experiences I had had when working as a scientist into my activity as a historian of science. I could read all these papers starting from the early 1940s into the 1960s with the eyes of my own experience, and this turned out to be very productive. And it was a productive period of the laboratory of Paul Zamecnik at the Massachusetts General Hospital in Boston. What I saw there was rather familiar to me and I realized that, as opposed to somebody whose training is in history of science alone, I could also read between the lines and practice another reading of a scientific text, differently from somebody who did not have that experimental experience. So working with that historical material turned out to be very rewarding. Here I came to see that the experimental work I had done in the laboratory had been fruitful for my work in the history of science. Alas, not the other way around, as I have told you. And strangely enough, doing history helped me to understand better my own former laboratory practice, and it was also here that my former exercises in philosophy, above all French philosophy, suddenly acquired a new dimension and a new meaning.

**García-Sancho:** The biological laboratory and the epistemological study of biology are, to some extent, spaces of negotiation between the practicing biologist and the historian and philosopher of biology. What were the

main challenges you faced when studying this concrete space of biological practice at Paul Zamecnik's Laboratory?

**Rheinberger:** There were quite a number of challenges that came up. Maybe the major challenge for me was very personal. I had been working for roughly 15 years in the laboratory as a molecular biologist, as a bench worker doing my experiments every day. After having decided to go into the history of science, I also had to switch communities. I had learned to negotiate with scientists in their conferences, smaller conferences or larger congresses. I had learned to talk about what I was doing and I had become familiar with the community. Now, I had to leave it and become acquainted with another, quite different community, that of historians of science and philosophers of science. I chose Paul Zamecnik's laboratory as my object of historical work for reasons explained above. It was an enquiry into the pre-history of my own laboratory work. At the same time, I was trying to catch up with my own philosophical past and see whether I could find alternative categories that would help me to make sense of laboratory work more generally, but in a rather idiosyncratic way.

The main problem at the beginning was that when I was starting to talk to the history and philosophy of science community, I had to make myself understandable. My new colleagues were not used to the strange mix of a very narrow focus on laboratory work on the one hand, and on the other hand categories and concepts from French philosophy that were more or less foreign to historians and philosophers of science — dominated as the field was by the Anglo-Saxon tradition. It was an interesting experience; I would not have wanted to miss it. The questions I received were always introduced by caveats such as «If I understand you correctly» or «I am not sure if I understand you correctly». [laughs]

**González-Silva:** *If I understand you correctly*, I would say that historical epistemology and the philosophy of experimental practices became one of the *leitmotifs* in the work that followed your experimental life and the questions that you posed — from a philosophical point of view— about what it means to do experiments. So for people who are not familiar with this very influential term, I wonder whether you can explain your vision of historicizing epistemology.

**Rheinberger:** It is difficult. But probably one can. To explain how I came to my position necessarily means to get personal. I experienced laboratory work as a big challenge. It is hard work. Ninety per cent of what one is trying does not lead to anything. In order to survive as a respected scientist, you have to devote all your time to the endeavour. It is a challenge, and it is something that can go wrong. In a way, I wanted to take this kind of challenge over to my new field, and work on something that would take all my time there as well.

When I was joining the history of science community, a movement had just started that turned out to be very consequential for the history of science. History of science as a history of ideas was starting to be replaced by a focus on the social, and in particular also the material context in which science is practiced. You can do that of course without much sophistication, but you can also try to do it with theoretical sophistication, and that is what I wanted to do: to bring a little epistemic challenge to this new practice. One of the key concepts that became rather important for me was the concept of «epistemic object» or «epistemic thing». It is part of that effort because it postulates that in order to understand how the sciences, at least the experimental sciences, develop over the course of time (I would not say in every instance, but in the long run) one must understand how natural phenomena are being shaped in the laboratory in a way so that one can, with the means at hand, make sense of them. That means that shaping scientific objects is a necessary premise for their conceptualization. So concepts do not come alone in the natural sciences: they compact and correlate with this continuous effort of shaping phenomena that we do not encounter in our world in this pure form. Doing experimental science is thus a shaping process, and not just a passive observational business. And I would say that is even true for astronomy: we cannot touch the moon or the sun, but we are nevertheless materially interacting with their emanations when we are doing astronomical work.

**González-Silva:** Alongside your theoretically demanding work as a philosopher and historian, and your experimental work, you also translated, among others, Jacques Derrida into German. In fact, you were working in an intellectual ambiance strongly influenced by historical materialism, which highlights the importance of historical change, historicizing received categories and focusing on the material aspects of reality. My question is what role you think this theoretical framework had for your scholarly efforts.



Figure 1.—Hans-Jörg Rheinberger between Miguel García-Sancho and Matiana González-Silva at the *Residencia de Estudiantes*, Madrid, on February 24, 2011. © Residencia de Estudiantes, Madrid

**Rheinberger:** I started to study in 1966. These were rather turbulent times for students. It was also the high point of a rediscovery of Marxist literature —above all the writings of Marx himself— that had been buried under world wars and cold wars; it was an important ingredient of my early student days. The general gist of that literature was that if you want to understand historical processes, you have to look not so much at what people are saying, but what people are doing, how they have been and are acting and living.

The discussion at that time in Germany, in the middle and toward the end of the 1960s, was relatively underdeveloped in comparison to the debates that were going on for instance in France, but also in Italy, where taking up Marxist themes could be much more freely practiced as there were strong communist parties in these countries and there were precedents for such discussions. That tradition was practically non-existent in post-war Germany and we had the feeling at that time as students that something important was going on in Europe —and the world at large— that we should not miss. Particularly important to me were the French philosophical discussions of

the 1960s. French had been my first foreign language at school and during my early years as a student I frequently spent summers in Paris.

Out of these interests and contacts grew my decision to translate one of the books that I read at that time, which happened to be *De la grammatologie* by Jacques Derrida<sup>3</sup>. Derrida usually is not narrowly associated with leftist positions, but you only have to look at the interviews that Derrida gave later in the 1980s and early 1990s to see how he himself was shaped by that discussion. His decision to start out with a very thorough analysis of the writings of the late Edmund Husserl are part of this whole context, as an attempt at an alternative *materialist* phenomenology that could lay claims to be based on the writings of Husserl himself, the *father* of phenomenology. There was also a more trivial aspect to that work. Translating at that time was a way for us students to make our living, so we could get a little money from it and survive economically on our own feet, and the work one was doing was nevertheless related to the studies one was pursuing. It was bringing things together at the theoretical level but also at the material and economic level. I do not know how that is in Spain today, but the opportunities to make one's living as a student, it appears to me, have considerably changed over the forty or fifty years since.

**García-Sancho:** The approach of theorizing from the historical transformation of very basic material practices and categories of science has been a main avenue of your research program at the Max Planck Institute for the History of Science in Berlin. Could the scientific program of your department at the Max Planck Institute be considered as an institutional embodiment of this attempt to integrate the theory and the practice of biology in a historical and philosophical agenda?

**Rheinberger:** That is complicated to answer. It is still the case in the Max Planck Society that if a director of a Department in one of its Institutes is appointed, then he or she is expected to decide about the program of the Department according to his or her interests, and nobody else will decide on what is being done there. The director however has the responsibility to explain these interests to an international committee every two years

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3. Derrida, Jacques. *De la grammatologie*. Paris: Editions de Minuit; 1967. German translation by Hans-Jörg Rheinberger and Hans Zischler. Frankfurt: Suhrkamp, 1992. See also Hans-Jörg Rheinberger. *Translating Derride*. *Dalhousie French Studies*. 2008; 82:85-91.

and demonstrate that what the group is doing is meeting the standards of what is internationally considered to be of excellent quality, but that is the only requirement. What I tried to do during my time at the Max Planck Institute was to remain faithful to the program I had shaped for myself in the years before. But this is only one aspect. The other is that I also wanted to provide a space where people who came with different ideas could also develop their own programs and participate in this basic freedom. If there was one thing that remained constant over time, it was the constant challenge of writing a history of science that is epistemically demanding; not teaching how to do it, but keeping it as a constant challenge. This means that if you want to become a historian of science, you should not just go to the archives and dig out documents. That is also important, and it is part of the job of the historian of science to recruit new resources that so far have not been looked at, but not for their own sake. Take the good old notion of case study seriously, which means that a case study has to tell you something that points to and goes beyond the case you are working on.

That is exactly what I was trying to do with the book you have been mentioning, the case study on the laboratory of Paul Zamecnik. I did that study not because it was Zamecnik, and not because I was particularly interested in that group. It was because I had the impression that I could make an argument on the basis of their work, on the example of this very concrete material, that points beyond itself and that gives incentives to engage in a discussion —hopefully an ongoing discussion. As far as I can see it, exactly this is happening at our workshop.

**Santesmases:** Another question remains to be posed, about the origins of your research program at the Max Planck Institute on the cultural history of heredity. Was it, at least in part, a consequence of contemporary culture's intense focus on genetics as well as the intellectual challenge of bringing the term «heredity» back into the historical and philosophical realm?

**Rheinberger:** The project on the *Cultural history of heredity* has several roots. My own interest in the history of heredity goes back to common work with my colleague Peter McLaughlin in the late 1970s and the 1980s. The phenomenon of Gregor Mendel had occupied us, as it probably has every historian of biology once in his or her life, and at the same time it became clear to us that on the basis of our limited investigation, we were far from being able to solve the puzzle. The chance for a more encompassing

investigation was only opened when I joined the Max Planck Institute. There I participated, in the middle of the 1990s, in a collective effort to write a critical history of the gene concept organized by Raphael Falk and Peter Beurton<sup>4</sup>. I tried to deal with the gene as an embodied concept whose changes are best understood as a result of the change of the experimental practices in which it became embedded in the course of the twentieth century. I was deeply unsatisfied with many philosophers' attempts to try to define exactly what a gene is. I came to the conclusion that a certain imprecision in the definition of scientific concepts, rather than being deleterious, can be a driving force in the research process. At the same time, I was intrigued by the discrepancy between the reification of what genes are in public discourse and their fluidity at the research front.

Later, at the beginning of the 2000s, Staffan Müller-Wille and I took up the challenge to extend this endeavour in the direction of a cultural history of heredity. An additional incentive came from a historiographical debate rising as a consequence of the «practical turn» in the history of science with its concomitant preference for short-range case studies. We know these kinds of titles (freely invented): «A history of paper chromatography in Finland 1943-1952», «Solving the puzzle of peptide bond formation at the Massachusetts General Hospital, Boston, 1951 to 1956», and the like. But what about a *longue-durée* history of heredity —if not of the life sciences as a whole— from the early modern period to the present? One knew these kinds of histories, based on an overarching history of ideas approach. However, could there be such a long-term history and could one remain faithful at the same time to some kind of practice perspective? Undertaking these challenges was a historical and a historiographical adventure.

It was, above all, a great experience in community building that will hopefully last for some time to come. The project rested in its essence on the cooperation of an international and an interdisciplinary group of scholars, and it took the form of a series of workshops extending over a period of about a decade. These workshops served as an opportunity to meet regularly for all those interested in the history of heredity around the world. Over the course of the years, the workshops moved more or less chronologically from the early modern period to the present. A number of

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4. Beurton, Peter; Falk, Raphael; Rheinberger, Hans-Jörg, eds. The concept of the gene in development and evolution: Historical and epistemological perspectives. Cambridge: Cambridge University Press; 2000.

colleagues accompanied us all along the way; others shared their specific expertise with us more sporadically. Without this background, Staffan and I would never have been able to write our *A Cultural History of Heredity*<sup>5</sup>. And respectively, I hope that everyone participating in this collective endeavor also profited from it in one way or the other.

**From the audience, Emilio Muñoz:** You have seen that scientists and philosophers, or historians of science are trying to break boundaries between them to some extent, but at the same time you have been working just at these boundaries. In connection with the two scientific communities, the philosophers/historians and the molecular biologists, how do you judge your relationship with them and how do you think these communities have maintained their relationship with you?

**Rheinberger:** This is a very interesting and also a very important question. But I would like to make it a little bit more complicated, and not make it only a question about the community of the scientists on one hand, and on the other the communities of the human scientists, or humanists. My experience has been, and still is, that the boundaries between historians of science and philosophers of science erected over a century are as difficult to cross as the boundaries between working scientists and historians of science or philosophers of science. If you look into the physical sciences, I guess that crossing the boundaries between theoretical physicists and philosophers of science, for instance, is much easier. If it comes to a field like biology, crossing boundaries between biologists and historians of biology is easier to manage than with philosophers of science. In any case it is fruitful to try it. I would even claim that today it is very important that our academic structures encourage such interactions, not in the spirit of a shallow idea of interdisciplinarity, but to create and keep open spaces for people who have been socialized into different specialties within biology (the boundaries of which can be as difficult to cross as, for instance, the boundaries between biology and physics).

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5. Müller-Wille; Rheinberger, n. 2. See also Müller-Wille, Staffan; Hans-Jörg Rheinberger, eds. *Heredity produced: At the crossroads of biology, politics, and culture, 1500-1870*. Cambridge: MIT Press; 2007. Project's website with links to pre-print publications: [http://www.mpiwg-berlin.mpg.de/en/research/projects/DeptIII\\_Cultural\\_History\\_Heredity](http://www.mpiwg-berlin.mpg.de/en/research/projects/DeptIII_Cultural_History_Heredity) (last accessed April 2013).

It is important not only to have the opportunity to talk to each other, but to be forced to talk to each other. Even if it is difficult to understand each other, something will remain. The important thing is the impulse one gets for reflection beyond what one is actually doing, and what usually absorbs oneself. I think this is a constant challenge, and even for fields like the history of science and philosophy of science, if I look back over 20 years, there have been disappointments in this respect.

When I joined the field towards the end of the 1980s and the beginning of the 1990s, there were many places around the globe where historians and philosophers of science were trying to join forces. Many of these places fell apart again towards the end of the millennium, and the interactions no longer exist the way they did before. It is not something that is given once forever; it is a constant challenge, a permanent boundary work, and I think we are obliged to do that as good citizens of our smaller or larger academic villages.

**From the audience, Richard Burian:** It seems to me that the opportunities for confrontation have changed rather dramatically among the life scientists, that the current situation with interdisciplinary teams is that they rather frequently find themselves calling for outside help with talking to each other, and that is a more important role because they are demanding help from philosophers and historians to enter into their discussions. Do you see this as something that has changed since the time you were in the laboratory yourself, and if so, what are the opportunities, or do you see it rather differently than I do?

**Rheinberger:** This is a question that is not easy to answer, and probably the situation within the sciences, in particular the life sciences, has been changing dramatically. I still remember that when I came into the laboratory, at the end of the 1970s, it was very difficult to survive as somebody who had the ambition to be a theoretical biologist; that sort of thing was not held in very high esteem. There was one journal, *Theoretical Biology*—still in its early years, I think—but if you were trained as a biologist, this was not a popular field. You had to work at the bench and do your empirical work before you could possibly move to theory. Now, within the life sciences themselves, things have been changing. There is talk about systems biology and similar things, and the need for synthetic views has become much stronger because the data that are being created on a grand scale are

becoming so overwhelming that new skills are needed. On one hand, they come from the development of areas like bioinformatics, but also from areas that in earlier times were connected to philosophy. I know, for instance, of a very renowned analytical philosopher who for quite a number of years was hired by people working in the life sciences who were feeling that in order to deal with the massive data coming from genome sequencing projects, they needed basic categorical and conceptual structures in place in order to relate to and deal with them in a fruitful fashion. People call it the creation of ontologies, and philosophers come into this business and they can have an important say in developing such new conceptual structures.

So there are many interactions between different scientific fields. The problematic thing is that we are habitually caught by these kinds of disciplinary structures that to a certain extent are still shaping our universities and make the impression that they were and would remain there forever. But the sciences are actually very much like living bodies that constantly change, and with that change the options and the possibilities of interaction. It is first and foremost the movement, the dynamics of the sciences themselves that create new possibilities for interaction. One has to seize upon them in one way or another. And I think there are also today other and different options for creating new interactions between the life and the social sciences, more than forty or fifty years ago.

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