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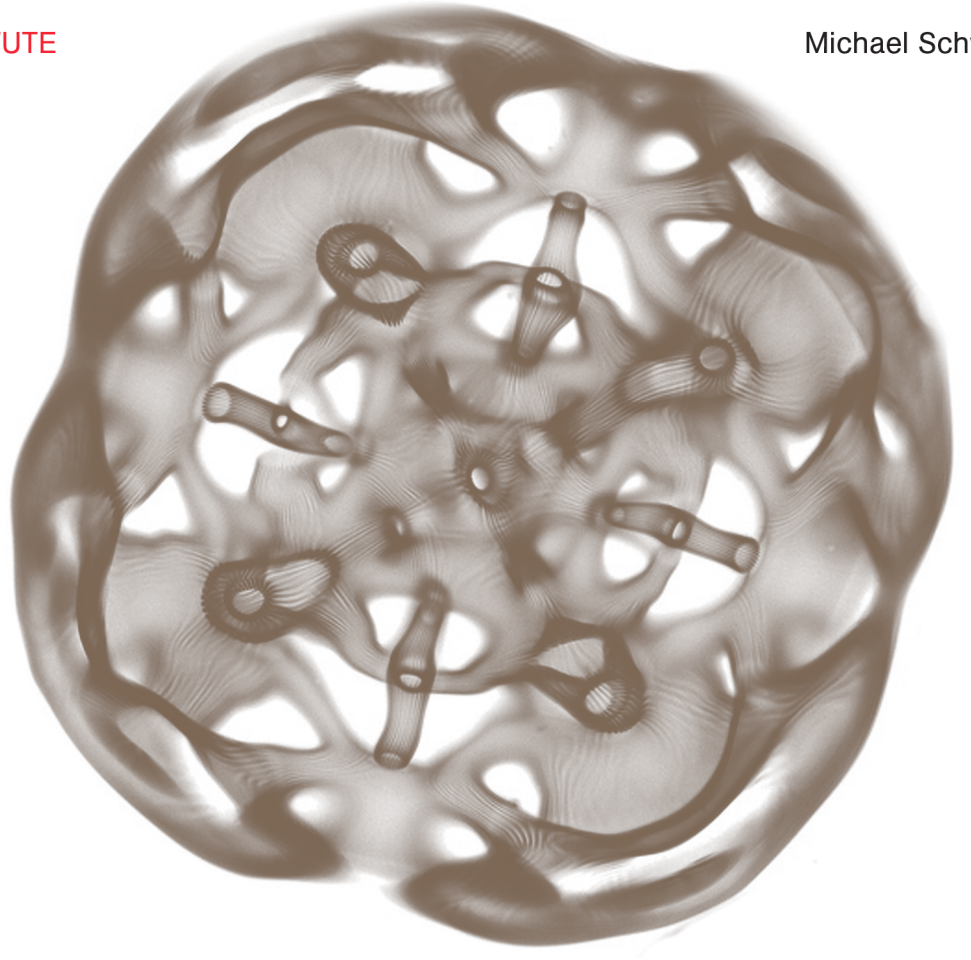
Experimental Systems Future Knowledge in Artistic Research



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SERIES

Michael Schwab (ed.)

Experimental Systems

Future Knowledge in Artistic Research

Edited by Michael Schwab

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Forming and Being Informed

Hans-Jörg Rheinberger
in conversation with Michael Schwab

Hans-Jörg Rheinberger spoke with Michael Schwab on 15 January 2013 in his office at the Max Planck Institute for the History of Science in Berlin.

EXPERIMENTAL SPIRIT

MICHAEL SCHWAB: *In a 2012 paper titled "Experiment, Forschung, Kunst" (Experiment, Research, Art), you talk about "experimental spirit" as a complement to experimental structure (Rheinberger 2012b, 13). Can you elaborate on what you mean by "experimental spirit"?*

HANS-JÖRG REINBERGER: There are two aspects that appear to me to be important with respect to what I call "experimental spirit." This is the first, and it begins with a caveat. One usually associates "spirit" with spirituality, a purely mental activity. However, in my understanding of "experimental spirit," the interaction of the experimenter with his or her *material* lies at the centre. If one is not immersed in, even overwhelmed by, the material, there is no creative experimentation. In the course of the interaction with the material with which one works in an experiment, the material itself somehow comes alive. It develops an agency that turns the interaction into a veritable two-way exchange. It's both a forming process and a process of being informed. The experimental spirit has a haptic quality. "Haptic" here points beyond mere sensory impression; it carries an epistemic connotation.

What is the second aspect that you associate with "experimental spirit"?

The second aspect is related, and it has to do with the focus on science as *practice*, as compared with the focus on science as a theoretical system. Experimental spirit means, to state it in traditional language, a plea for an inductive rather than a deductive attitude —although this is not my vocabulary.

So, do you reject Popper?

There are a lot of very interesting things in Karl Popper's major early book, *Logik der Forschung*, published in 1935, which was only translated a quarter of a

century later, when it appeared as *The Logic of Scientific Discovery* in 1959. Popper does actually, in this book, speak about what Hans Reichenbach (1938) called the "context of justification," and not the "context of discovery." To be sure, Popper conceives of science as a dynamic process, not as a system of propositions. However, despite this research-friendly, forward-looking attitude, he shares a backward-looking attitude with the brands of philosophy of science that were characteristic of the first half of the twentieth century. In short: theory first. With Popper, this attitude took the form of "hypothesis first." Laboratory work comes second. The experimenter has to try hard to achieve what the hypothesiser would like to see. While I don't want to get rid of theory in empirical science, I nevertheless propose a reversal of poles: science is first and foremost a practical activity, although a theoretically laden one. This activity comes in a huge variety of guises. What unites them is that they are, on the whole, particular kinds of epistemic engagements with the world. This, of course, means that science has to be seen as a process deeply inserted in the materiality of our world, a collective engagement that cannot be reduced to the ingenious activity of an individual spirit who has the last word. This also means getting rid of the age-old thinking about *Erkenntnistheorie* as being about an I, an ego, a subject that tries to cast a theoretical net over an object. Instead, let us be a little bit more humble and see the experimenting subject as engaged in an activity that has, to put it in Ian Hacking's (1983, 150) words, "a life of its own," and one that is in need of many good eyes to see and many good ears to hear. Let us get rid of what could be called the tyranny of the subject.

What is the role of the subject as you describe it in the generation of knowledge?

Every experiment is about future. And the hand is the carrier of that future. This is the reason why I reproach the epistemological tradition for having narrowed down the notion of experiment to a matter of mere testing. If the future is dealt with in the classical epistemological tradition, it's always in the theory, for example, as prediction. My counter-position is that the future is in the experiment, and experimenting is about handling and engaging.

At the same time, experimentation displays a very special kind of engagement. On the one hand, an experiment is designed to exclude the experimenter as a subject from what is going on. On the other hand, paradoxically, to be able to do that you need closeness in order to arrive at the point where you can efface yourself in the experimental process and delegate the interaction to the bits and pieces of matter you are working with.

So you'd say that when somebody learns the experimental spirit, he or she also has to learn a type of handling?

Yes. Laboratory education does not happen from one day to the next. It is a protracted process. It usually takes years of engagement with a particular material in order to arrive at the kind of "extimacy," to use Jacques Lacan's (1986) wonderfully appropriate expression, that makes you a good experimenter.

Do you see a problem with Polanyi's notion of "tacit knowledge," which you refer to in Toward a History of Epistemic Things *(1997, 77–78), in particular in respect to a future that seems to exceed what we may tacitly know?*

Michael Polanyi makes a very interesting and good point with his idea about the uncircumventability of tacit knowledge. He is completely right to point out that you can't make everything explicit. There always remains something that you cannot logically resolve when you practise your trade as a scientist. His argument allows one to detach oneself from the logical positivist tradition and, with it, from the belief that science takes place at the level of language and more narrowly in the realm of logic. Polanyi made a very good point in his time, but I think we now have to go beyond that. He basically treats tacit knowledge as a residual category; but we should also acknowledge its prospective potential.

In the history of art, Marcel Duchamp, at the beginning of the twentieth century, could claim to have given up what he termed "retinal" painting. This signalled to some that the practice of making and the dirty handling of stuff had lost importance while conceptual practices moved into the foreground. Might this run in parallel with developments in the history of science?

With this, Duchamp is very much in harmony with the philosophy of science of his time. Historically, it *is* strange that, on the one hand, the nineteenth century was a century in which there was an explosion of experimental and empirical science, spreading out in a plethora of different disciplines, while on the other hand, the accompanying theoretical reflection shied completely away from the practical aspects of science and established itself in the sublime realm of theory. This counter-movement in epistemology to the actual development of the sciences appears strange at first sight; it may well hang together with the age-old, continuing struggle between science and religion over the authority to tell the truth. But this would be another discussion.

In a different text, you speak about the importance of "a sharp sense for secondary sounds" (Rheinberger 2010, 5, my translation). Does this imply that the experimental spirit enters the experimental situation from its margins?

If you want to be a productive researcher, you have to conduct your experiments in such a way that you can be surprised by the outcome, so that unexpected things can occur. This only happens if, on the one hand, experiments are precisely set up but, on the other hand, are complex enough to leave the door open for surprise. The magnitude of such surprises is itself constituted in a recursive or iterative loop. It doesn't expose itself in a flash of enlightenment at one particular point in time. That is how people who have effected major breakthroughs in science usually depict their own achievements in hindsight, which I think is due to a self-stylisation that can only come after the fact. The surprises, when they show up for the first time, are of a minor magnitude, and

may even make their appearance as contaminations, which is why they often tend to be overlooked. The experimental spirit lies precisely in not overlooking these small effects.

EXPERIMENTAL SPACE

In what kind of space does experimentation take place?

Today, science is predominantly carried out in all kinds of laboratories. Even field-science has become laboratory-shaped. Laboratories are semi-closed spaces—"esoteric" spaces, to put it in the words of Ludwik Fleck (1979)—full of jargon and opaque to everyday experience. An outsider no longer understands what goes on there. If you really want to understand what drives the sciences from within, you have to open these research boxes, these islands of "access to an emergence," as Gaston Bachelard (1949) put it. It is not enough to look at the sciences from the point of view of the dissemination of their results, for instance, or of the impact of economic interests, and so on. Research is a highly complex thing in itself. In art, we are, I think, confronted with a similar situation and thus must not fall prey to the idea that we can understand either the sciences or the arts from an altogether exoteric perspective, although the exoteric belongs to them as well.

How heterogeneous are those semi-closed spaces?

As far as the notion of space is concerned, one obviously has to narrow it down and also historicise it. All the categories I use are historically infused. Without considering this aspect, one misses the core of the attempt. If we talk about spaces of knowledge-acquisition from a historical perspective, we realise that the laboratory as a space of experimentation is a relatively recent development. For seventeenth- and eighteenth-century natural history, one of the predominant spaces of knowledge-acquisition was, for instance, the botanical garden, while one of the main instances of knowledge acquisition in the medical realm, at least from the end of the eighteenth century right up to our days, has been the clinic. Thus it has to be said that the experimental laboratory is one epistemic space among others, which means that epistemic spaces themselves come in a historical and contemporary multiplicity. That multiplicity, or heterogeneity, repeats itself fractally, if one considers the microstructure of an epistemic space such as the laboratory. A counter-example would be a Taylorist industrial production process, where you have a very clear division of labour and where every part of the process fits neatly with the rest. This is not the way laboratories are constructed. Laboratories are much closer to what Claude Lévi-Strauss (1962) characterised as "bricolage." If you look at any particular piece of laboratory equipment, you will see that it is constructed from a lot of ad-hoc arrangements that make it work in a local setting. You couldn't even export it to the next building—it might no longer work there. This idiosyncrasy of the laboratory is a very central aspect of experimental work, of creative experimental work.

What are the particular spatial conditions that allow for epistemic phenomena to occur? Would a word such as "density" or "saturation" be appropriate to convey how those minute moments and events seem to pull the experiment together?

I think the notion of density is an appropriate description, or perhaps "thickness," which reminds one, of course, of Clifford Geertz (1973). "Thick description"—a notion Geertz applies to anthropological narratives—tries to keep present all the different aspects that go, for instance, into the everyday life of a population in a village in the north of Mauritania. One could describe the scientific work carried out in a laboratory as an enactment of epistemic thickness. The experimental situation in the empirical sciences is usually characterised by theoretical under-determination and by material over-determination. That is the situation in which the scientific spirit has to engage itself. I think notions such as densification, oversaturation, or condensation might express this. Alternatively, one could say that laboratories are spaces of heightened awareness.

Are some technical objects just in the background, while the closer one gets to an epistemic situation, the more attention needs to be paid to the technical objects that are implied?

In *Toward a History of Epistemic Things* I wanted to convey the idea that the experimental process plays out a dialectic between epistemic things and technical objects, and that there exists a functional relationship between them rather than a substantial one. Epistemic things that have reached a certain point of clarification can be transformed into technical objects—and vice versa: technical objects can become epistemically problematic again. The technologies with which one works are normally used as black boxes; they can, however, be reopened and become things of epistemic interest. It was this dialectic between the epistemic and the technical that appeared to me—and still appears to me—to be at the core of the scientific process of experimentation. The technical object and epistemic thing respectively are the material correlates to the interplay between stability and change, which keeps the experimental process intrinsically open to the future, although, or even because, full use is made of earlier acquisitions. In an experimental system each sort of thing is articulated with the other. If one now tries to characterise what such a laboratory space is made up of, one can certainly introduce a good number of further specifications related to its technical setup. The electron microscope is a good example. The magnification power of the instrument might be at the centre of one's experimental work; but in order to be able to use it, one needs an infrastructure that goes way beyond the instrument and the experimental probe to be inserted into it. There has to be a continuous high-voltage power supply, and the instrument needs a special, solid foundation without which one can't get good pictures, and so on. So there is, from the instrument, a continuous expansion right into the architecture of the laboratory space.

In the history of art, technical objects have come to prominence in debates around the specificity of media. In contrast to this, it seems to me that in the scientific context, technical objects may simply be looked at as productive instruments that in comparison leave much less of a mark on the object being produced, making it less necessary to conceptualise outcomes around and dependent on these instruments. Is this a fair point to make?

There is a widespread attitude among scientists—in particular when they attempt to convey what they do to a larger audience—that makes these instruments tend to disappear from sight. They appear to be there just in order to look through; they are not thick. Such assumed transparency contributes to the neglect of the material and practical side of the process of scientific knowledge acquisition. We could speculate about why the "spontaneous philosophy"—to use a term of Louis Althusser's (1974)—scientists apply when they reflect upon their own work almost always points in this direction. However, in science studies over the past three or four decades, with their focus on the practical aspects of doing science, of science in the making, these media have become "untransparent." They have acquired a presence of their own and are being thought of as not just enabling scientific knowledge acquisition but also determining what can be known and what not. Seen from a media perspective, one could even go so far as to claim that all these instruments used by the sciences are the media without which they would never even be able to get at their bits and pieces of knowledge. A whole world would be foreclosed to them—and to us—had we not this *Zwischenreich der Medien* (in-between kingdom of media) that has grown overwhelmingly massive in the course of the last 150 years. Today, scientific instrument development and construction has even become a significant part of advanced industrial production on a global scale, so it has achieved an equally massive economic presence.

It is important to develop an awareness of the thickness or untransparency that comes with the usage of heavy instrumentation. Unconsciously living in such a media landscape has potentially disruptive effects on the production of science. I think there is an ad hoc awareness of the mediatedness on the part of the scientists in the laboratories, but I find it interesting—and intriguing—that as soon as scientists go public, they have a strong tendency to leave all that behind and to convey a picture of what they are doing as if the instruments were absent—or transparent, for that matter.

In order to speak to the public, scientists may need to sacrifice thickness, or différance*, as Derrida ([1976] 1997, 60; 1982) termed it in relation to language.*

Language is a medium as well, and so is written language—even more so. It comes with its own thickness, and it comes in grades. Writing up, tracing, sketching, is part and parcel of the experimental process. The protocol, in a way, belongs right inside the experimental process—it is an integral part of it; it participates in the thickness of the experiment. The research article, printed in a journal, has gone through a fairly regulated process of purification, but still reflects the experimental goings-on. The textbook in turn abstracts from

the experiment; it is completely fixated on results. The public speech of the scientist, finally, we could say, is as far away from the laboratory as you can get.

Is the "patchwork," as you call it (Rheinberger 2012a), that makes up empirical science actually also an experimental system where the "patches" on a higher level function like technical objects?

Experimental systems don't come in isolation. As a rule, they are part of broader landscapes, or cultures of experimentation. They form ensembles with a patchwork structure. The *in vitro* protein-synthesis system described in *Toward a History of Epistemic Things*, for instance, was part of a broader culture of biological *in vitro* experimentation that was already taking shape at the beginning of the twentieth century. Patchworks of experimental systems have a peculiar, semi-permeable structure. On the one hand, they are characterised by a certain circulation of materials, research technologies, and researchers among the patches. On the other hand, the patches retain a certain identity; they don't fuse with each other, they remain idiosyncratic generators of novelty. But I would be cautious about seeing experimental systems as technical objects that themselves constitute a higher level of creating novelty. I prefer to characterise this higher level as an experimental culture. Its structure feeds back into its elements, but there is no mimicry between the levels.

Has experimentation the way you describe it affected our culture at large? If yes, has this become problematic, in particular, if one looks at how corporations and governments "experiment" with economic realities?

There is a long and on-going sociological discussion about our modern "risk society" (Beck 1992; Krohn and Krücken 1993). And there is a more recent discussion about societal experimentation on a "real-time" scale (Groß, Hoffmann-Riem, and Krohn 2005). In our societies, we are constantly confronted with economic, social, political, cultural, and technical decisions that come with unintended, or unthematised, consequences. They equally ask for permanent reorientations. As far as new technologies are concerned, their development is usually connected to scientific experimentation. But society, for that matter, is not to be compared with a *scientific* laboratory. That would lead us into a technocracy, if not scientocracy. However, democracy as such is a permanent *political* experiment in which many different forces interact in agonistic and antagonistic ways. Fleck (1983) has even argued that our modern sciences with their openness and at least potential accessibility for everybody are a role model for a democratic process. Be that as it may, the modern sciences and democracy actually are historical co-products and ideally should be resources for each other.

TECHNOLOGY

Can any type of activity and any technology that helps to stabilise epistemic phenomena become part of an experimental system? What, then, about disciplines and disciplinary boundaries?

Experimenters are usually opportunistic in their use of research technologies. As far as disciplines are concerned, they are strongly connected to an institutional perspective. For a long time, the history of disciplines was a main focus of the history of science as a whole. While there continues work to be done in this direction, my approach was a different one. I wanted to do a kind of bottom-up history. Therefore, my starting point was experimental systems with their immediate surroundings. As I have said, experimental systems have a life of their own, and this life must be characterised in all its facets. Going one step further and conceiving of something like ensembles of experimental systems was the next obvious step upwards to understand fields or areas of scientific activity as structures of their own, without necessarily implicating the institutional aspects that disciplines carry with them. These ensembles or patchworks of experimental systems—experimental cultures—can become historically prominent or fade into the background and become marginal again without necessarily coinciding with disciplinary boundaries.

What may be the current role and value of disciplines?

Disciplinarity comes in different degrees. One would have to work much more historically on this topic, but it appears to me that there was a time in the development of our Western sciences—particularly in the nineteenth and early twentieth centuries—when we had processes of differentiation in the sciences that resulted in a host of different disciplinary ramifications and reifications. All these specialties tried to demarcate themselves from one another by more or less clear-cut boundaries. Much of the development of the natural sciences in the twentieth century has tended to undo these boundaries again, first in the form of hybrid disciplines such as biochemistry, biophysics, or even biophysical chemistry. When it comes to characterising what happens at the research fronts today, even these disciplinary boundaries no longer appear to be so important; sometimes they even act as impediments. Usually, if you have a research problem in these areas of inquiry, disciplines function as resources, but they no longer define the boundaries of the research problems themselves.

What is the role of institutions in enabling or disabling the formation of certain phenomena—that is, who is driving the development of experimental cultures?

I believe that in the long run the sciences are best served if one lets them be driven by themselves from below. Institutions are at their best if they don't prevent this drive. The power of institutions to shape experimental systems and experimental cultures top down is limited, as historical experience shows.

What can and must be done socially and politically is to create a frame, an academic environment, in which the self-correcting power of the sciences can unfold within the social and ethical limits that societies consider to be their standards. These standards themselves are under constant negotiation, to which of course the development of the sciences contributes its share.

You wrote (Rheinberger 2012a, 38) that the nineteenth century displayed an eigenideologischer Überschuss *(self-ideological excess) that the twentieth century replaced with the pragmatics of technology. In the context of such an ideologically determined nineteenth century, doesn't a notion such as "technical object" limit the analysis to an aspect that only became important in the twentieth century?*

The very term "technical" is in need of critical scrutiny and differentiation. We could here return to Bachelard (1949), who claims that what he calls "application" belongs to the very core of modern science. This means that a particular relation between epistemicity and technicality would have operated from the beginning of what we consider to be modern science. What became, in addition, more and more important in the course of the nineteenth century and even more so in the twentieth century is that technicality acquires much bigger contours; big technical systems have come to shape and reshape our everyday reality (Mayntz and Hughes 1988).

How can one approach a notion such as "technical object" from an arts perspective given that not all art engages with technology?

When one talks about "objects," one is always in danger of falling prey to reifications, in particular if one talks about technical objects. What I mean, basically, when using this pair of concepts—epistemic things, technical objects—is that there is an irreducible interplay between identity (the technical) and difference (the epistemic) in our processes of knowledge-acquisition. This also means that there is, and remains, an intimate relation between epistemicity and technicity to science as a whole, at least as it has been operating over a period of some four hundred years in our Western countries.

While the term "object" carries some definiteness with it, there is something indefinite about "thing." For me, the choice of the notion of epistemic *thing* is tightly bound to this constitutive vagueness, while the choice of the notion of technical *object* is bound to its being more or less clearly delineated.

Might a focus on technicity be problematic as art moves into the epistemic realm?

I am not a friend of tight homologies. It is very clear that there is no one-to-one homology between scientific and artistic activity—otherwise these two realms would collapse into each other anyway. We also need to be aware of perhaps irreducible differences while nevertheless working on a conceptual framework in which to talk about these differences and bring them into the realm of comparability.

Let us consider the art market for a moment. On the one hand, it constantly re-evaluates works of art of the past. On the other hand, as far as art production is concerned, it has a drive into the future. You're bound to do something new with respect to what has been there already. There clearly is this aspect of reaching out into the future and an exploratory element in artistic activity that is valued by the market. Whether the dialectics between epistemic things and technical things so characteristic of experimental science can be used as a point-by-point description of how art reaches out into the future, I don't know. Probably one will encounter limits that require other, or additional, conceptual frameworks.

While the art market is important to many artists, much artistic research seems also to critically distance itself from that market. Are there similar tendencies to be observed, for example, in nineteenth-century science?

We tend to use historical generalisations and talk about "the science" of "the nineteenth," "the eighteenth," or "the twentieth century" in our conversation, but we should be careful not to overstate it. The life sciences, for instance, as compared to the physical sciences of the nineteenth century, may well have operated according to a different stage of development. We should be careful about using the term "science" in the singular and, instead, look at the whole epistemic enterprise as an intrinsically pluralistic one.

There's another generalisation that should be treated with caution. When we talk about the kind of knowledge claims that scientists make, we should explicitly talk about "scientific knowledge," because it is clear that there are also knowledge claims associated with artworks, for example. Knowledge is being produced in music and literature and in other areas of culture, but the way it articulates itself is qualitatively different from the way knowledge claims are articulated in the sciences. Even within the sciences you have quite a number of different ways of making knowledge claims—think of the practices of mathematics versus those of the experimental sciences. There are lots of forms of knowledge around us, in everyday life as well as within the horizon of artistic production. These knowledges in one way or the other hang together, but they do not coincide. In recent history of science there is an increasing tendency to envisage a history of knowledge (*Wissensgeschichte*) and no longer keep the history of science (*Wissenschaftsgeschichte*) apart from the rest of knowledge. Knowledge effects have a much broader distribution in our intellectual life, and that should be taken seriously.

And, after all, why should "research" be restricted to scientific knowledge? This is a limitation that I don't think is justified. Nevertheless, we have to take note of the fact that within the last two hundred years, "research" has been connected, and more and more restricted, to scientific knowledge-production. I think we should arrive at a wider notion of what it means to do research—in terms of searching processes that can of course be different in different areas.

Would you say that the intensified focus on technology that you described as characteristic of the twentieth century sciences has conditioned the types of exploration that can be chosen?

I am not sure that this is really the case. Of course you can say that the environment of these exploratory spaces has become highly populated with all sorts of bits and pieces of technology that, for instance, a chemist at the beginning of the nineteenth century couldn't even have imagined—an electron microscope, an ultracentrifuge, for example. In that sense, the arsenal on which you can draw in a particular research process is incomparably more technically sophisticated than, let's say, 150 years ago. They form a technically more sophisticated and therefore also more constrained environment. On the other hand, these technologies don't act only as constraints. Through their very multiplicity, they create options and possibilities for interstices and things to eventually show up that, without them, never would have shown up and wouldn't even have been imaginable. There is thus a proliferation of technical boundaries, to be sure, but I don't see an "over-technologisation" of the research process as a whole that would ultimately lead to the disappearance of the epistemic dimension altogether.

Similarly, in the context of science, may research that depends on non-propositional modes of communication be disadvantaged?

Historically it is correct that in certain areas of science texts have been—and continue to be—the dominant form of communication, but there is a development within the different sciences over time. Sometimes the textual and the formulaic becomes less prominent, sometimes it becomes more prominent. Sometimes the visual becomes less prominent, sometimes it becomes more prominent. In the life sciences, even in the molecular ones, the visual has plainly gained in prominence in the past half century. When I studied biochemistry in the 1960s the textbooks abounded in text and formulae. If you look at a molecular genetics textbook of today, text is reduced to a minimum, and formulae, if at all, are mostly used in connection with overwhelming sequences of cartoon-like drawings and computer images.

GRAPHEMATICITY

You distinguish between a graphemetic space of inscriptions and traces and a space of representation in science. The graphematic space seems to include, for example, drawings or graphs but not words.

Indeed, words don't play a big role in the space of graphemes, of inscriptions. It's the traces that count here.

You argue that traces in the graphematic space are differentially reproduced—and not represented—in the discursive space of representation. I am not sure, however, if you conceive of those spaces as co-original, since it seems to me that you prefer the materiality of the former. If I compare this to the arts, it may be claimed that the space of material encounter—the studio—where an artist presumably engages his or her subjectivity is actually a reconstruction and a simplification that historically was used to support the idea of the artist as genius. Could it be that an emphasis on materiality is, perhaps for other reasons, also problematic in the history and theory of science?

I wouldn't talk here about subjectivity. If anything, I would talk about uniqueness. For instance, you wouldn't organise a publication according to the principles that you use in your notebook, because the latter is meant to be a trace collector that helps you organise your bench work. A publication is thus secondary to the laboratory activity, without which of course it wouldn't exist, but it also has to be organised in such a way that it conveys knowledge to a potentially global community. If you want to continue your work as a scientist, the optimal thing that can happen to you is that somebody else picks up what you have been doing and integrates it into his or her own work. Your reputation as a scientist depends on these acts in the space of representation without which your graphematic activity would also come to a halt. In that sense, the representational space is as necessary as the graphematic and, in a way, probably even co-originary—you can't separate them.

My emphasis on the graphematic space—you may look at it as a bias—is due to the necessity of getting away from looking at science only from the historically privileged perspective of the space of representation that has dominated the history of science so far. What have historians of science largely relied on when doing their work? Published papers.

One lesson I take from the history of art is the shift from the processes of making as the primary site of art, to criticism, discourse, and ultimately, the market. As a consequence, what is made and how it is made may now be looked at as secondary.

We might have to do with two historically counter-running correctives. I think it is misleading to shape the whole question into one of primacy. Even if I sometimes use the notion of "originarity" in connection with traces, for instance, one has to be very cautious about it. It is helpful here again to have recourse to Derrida ([1976] 1997), who suggests that we free the question of origin from a teleological framework. Everything is always already in the midst of things, where the question of what is primary and what is secondary loses its sense and where things interact with one another forward and backward.

It's always good to think about the dichotomies we have on the table. One such dichotomy that has come up repeatedly in our conversation is that between the epistemic and the technical as a way to make sense of the experimental process. There is, as I said, a dialectic between epistemicity and technicity and a constant oscillation between looking at something as being technically defined and looking at something as being epistemically open. One and

the same entity can appear, or be handled, in a certain context as a technical or as an epistemic entity. As I said, it is not the materiality of the entity that defines whether it's a technical object or an epistemic thing. Another dichotomy is that between materiality and—not quite ideality, but other, less heavily material forms of being-there: for instance, graphematicity. Obviously there are different regimes of materiality. In the process of an experiment, you have the material level of the arrangement of the experiment, but then you have a layer of graphematicity. Basically, what you produce in the experiment is traces, very often indexical ones that are somehow connected to the process under investigation. Usually those traces are of a volatile character. If you want to preserve them for further work, you have to find ways of stabilising these traces. In this process of transformation, which we can address as a transition from traces to data, you gain durability and lose materiality, as so lucidly described by Bruno Latour (1988, 1993). You come to a level that in one way or another ends in paperwork, inscriptional items still very near to the experiment and pretty far from a scientific article. The arrangements, rearrangements, interconnections, and transformations of traces that derive from the experiment are part and parcel of the knowledge-production process. Knowledge effects don't automatically spring out of the experiment. There is a level of creativity involved in the production of this second-order reality that goes along with experimentation.

What does matter or material mean to you, in particular when it is contrasted with form? Why do you refer to George Kubler's The Shape of Time *([1962] 2008), an art historian who focuses on formal sequences, while at the same time emphasising material aspects?*

Don't forget that Kubler calls his endeavour a history of *things*. You can certainly claim that there is an element of formalism in Kubler. But tellingly, he opens his book by taking his distance from Ernst Cassirer, whom he—problematically—sees as locating the achievements of the sciences and the arts completely in the realm of the symbolic.

My bias toward materiality has to do with my own formation and with my own background in the empirical sciences. There, even abstractions come in materialised form. Even a model is only a model if it is, in one way or the other, embodied, be it only with pencil on paper, which also has its very concrete materiality. But this also means that I have a wider conception of materiality.

What about imagination?

I describe experimental systems as exteriorised spaces of imagination.

Somehow I wouldn't want to exclude from what may simply be called "thinking" the types of surprises that you describe as resulting from experimental systems.

If something remains in the realm of dreams, it will never come to have any impact on a historical process such as the sciences or the arts. Exteriorisation is a precondition for something to become workable and interactive. Edmund

Husserl (1978) is absolutely right when he states that even the formal sciences—which to him meant mathematics—would be completely unthinkable and historically not understandable without the exteriorisation that writing has brought to this form of engaging with the world.

Which, of course, led Derrida to focus on the exteriority of writing.

The early Derrida took this point up from Husserl and put it at the centre of his thoughts about science and objectivity.

Given the exteriority of writing, in my understanding, within the graphematic space, both material and sign are co-created. Rather than—from a representational perspective—conflating the graphematic with the material, should one not better—from a graphematic perspective—focus on the complex relationship between material and sign, which representation disavows?

Of course, you can make the point that in the space of representation—let us stick with the notion of representation here for the moment—it is precisely its potential of becoming disconnected from the graphematic space that makes it fruitful.

How is ontic complexity outside the lab related to the epistemic complexity within the experimental landscape?

I think here we have to consider two different orders. Without epistemic complexity and without the establishment of an epistemic space—graphematic and representational—you wouldn't be able to say anything about ontic complexity. What we have here is retro-action. What we call ontic complexity is the product of epistemic complexity in exactly the sense that Hacking (1983, 130–46) uses the term "reality" when he says that this is a second-order concept. Only when alternative ways of representation—or, as I would say, spaces of experimentation—come into being, does reality, as something beyond, become a problem to talk about.

Could research be associated with the graphematic and science with the representational space, while maintaining that they both operate in tandem?

Why not? The sciences, as we know them today, are unthinkable without research. Nevertheless, research is not the whole science. Science, in its stabilised technical form, is embodied in many products we use in our everyday life, from cars to electronic gadgets. The education system also belongs to our scientific reality: more scientists are needed in order to go on with research. You need a transmission system where the state of the art can be given over to the next generation as it is, fixed in relatively uncontroversial form in textbooks. There is more to the epistemic universe of our societies than just research. In terms of percentage, research amounts to probably no more than some-

thing between ten and twenty percent. Certainly less than one third of the money allotted to science and education goes into research activities, properly speaking.

Are you suggesting that research takes place in a marginal space?

I would certainly not say that it is marginal; it is an integral part of the entire epistemic space.

In a Derridean sense: the margins of philosophy?

Very necessary margins.

EXPERIMENTAL HISTORY

Is the notion of "experimental system" part of your *experimental system?*

Yes, if I can take your somewhat tautological question to mean: "Does the work of the historian of science also take place in a sort of experimental system?" Eduard Dijksterhuis ([1959] 1969) argued in this direction, and his words were taken up by Georges Canguilhem (1975), who also stressed that the history of science is to be seen as the laboratory of epistemology. Working with the notion of "experimental system" in order to make sense of certain aspects of history of science has in itself an experimental character. You try out how far it takes you, what kind of phenomena you are able to cover with it and where it has—first, historically and second, narratively—its boundaries. Historically: I myself never went further back than the late eighteenth century in my historical case studies, but if it comes to early modern science—let's say sixteenth- or seventeenth-century science—it is by no means evident that the notion of "experimental system" would help. The historical range of the notion is open for debate. Narratologically: We already talked about experimental systems as being embedded in cultures of experimentation. Here we encounter the problem that time spans matter—"time" comes in many registers for someone who studies the history of science. By using "experimental systems" as your historical unit of analysis, you operate mainly in a short-term range. A case study, like the one I did on the history of protein biosynthesis research, occurs within the lifetime of one particular scientist or a group of scientists and their particular experimental system, whereas if you want to understand what characterises a century of scientific activity, you will in all probability have to choose another unit, say "experimental cultures." When it comes to covering several centuries, it may even become problematic to take "experimentation" as the centre of your focus. In *A Cultural History of Heredity* (2012), a book I recently wrote together with Staffan Müller-Wille, we took the notion/phenomenon of heredity to cover a period of about four centuries. This notion/phenomenon, of course, then has to be set and seen in its various historically changing practical contexts. So it is clear that in order to write such a long-term narrative, the

object of one's focus needs to change. Doing history of science is, in itself, a very multifaceted activity imbued with epistemic problems. It is an experimental space in itself.

It is thus important to be clear about the level at which one talks, since a notion such as "experimental system" operates at a meta-level. As a historian of science, I try to make sense of the process of scientific research in a particular time frame and to find conceptual tools in order to characterise this process. However, this does not imply that a scientist, when working, operates and thinks in the same categories and framework, even if, in this case, the notion of "experimental system" is an actor's category.

Did such meta-level reflections help you during the time you worked as a scientist in the laboratory?

I would be lying if I claimed that my philosophical reflections helped me to do good experiments. I would even be inclined to claim the contrary and suggest that one should forget about this meta-level of reflection while actively engaged in research work. In the act of knowledge-production, the connection between the base-level and the meta-level is probably less tight. This doesn't mean that there is no relevant relation between these two levels. At times, scientists also need to switch into a reflexive mode—if only when they have to write a grant proposal. Looking at how these different contexts relate to each other, we probably arrive at a complicated structure, but one that comes nearer to the actual situation.

Has the analytical unit "experimental system" run its course?

Our negotiation with the world under a knowledge perspective leads to different "ways of knowing"—to use the words of John Pickstone (2000). Once in place, they don't just disappear again. They tend to stay, but they change their relative importance over time. Experimental systems played a very minor role before the eighteenth century, if they played a role at all. They came to acquire a predominant role in the later nineteenth, which they kept throughout the whole twentieth century. Their future fate is not predetermined by this role. Indeed, what we observe today as "big science," including global consortia that involve not only hundreds but sometimes even thousands of people all over the globe, is in need of a characterisation for which the notion of experimental system is probably not enough.

If you take seriously the material with which you work, there can always come a point where you can no longer get along with the concepts you use. Then you will need to find other conceptual tools to get out of the impasse. Let us not ontologise these categories, be they "epistemic things" or "technical objects" or "experimental systems." These notions themselves are historically and narratologically situated. We should not reify them. It is important to be attentive to the resistance with which the material presents you when you approach it through these categories.

SCIENCE AND ART

Looking at science-art collaborations it seems that by and large artistic practice isn't granted access to the scientific spaces of experimentation and that it simply functions to communicate science to the public.

Yes, of course, there is this function. For example, if you look around in the Max Planck Institute for the History of Science or go to the Max Planck Institute for Molecular Genetics across the street here in Dahlem, you will see the work of artists on display that was inspired by what the researchers are doing there. An inspiration that goes the other way around is harder to imagine in such a context. Usually, these are one-way enterprises where the science involved in the cooperation remains untouched by these artistic activities.

Within a theory of experimental systems, shouldn't one expect more interaction between artistic and scientific practices on the level of research? Is there perhaps a principal problem with science-art collaborations?

No, I don't think there is a principal obstacle. Over the past several years, I have experienced a particular kind of collaboration between an artist, Hannes Rickli, and a number of biological laboratories in Switzerland, Germany, and the United States. Rickli participates in the data-stream production of these labs without following the direction that the scientists take when they process their data. He manipulates and somehow reconfigures virtually the same graphematic material that scientists use in the creation of their models. In the regular meetings between the artist, the scientists of these laboratories, and a small group of art historians and historians of science, one could see that the scientists were really affected by the artist's work. They came to learn to see what they did with their data in a new light—the traces with which they worked along trodden paths became thick again for them. However, for this to happen, one needs scientists who are receptive and don't say, "Why should I lose a day in playing around with this?" It is maybe a special situation, but what happened there is indeed something of a two-way communication between artists and scientists.

As the disciplinary boundaries lose importance, has a shared aesthetic space become possible where, beyond the processing of sense data, artistic *concerns also matter?*

We have been living with the divergence of aesthetics and epistemics for probably two hundred or three hundred years. For quite some time, the epistemic sided with truth and the aesthetic with beauty; the two realms appeared to be more and more separated from each other. However, there have been developments in the arts, at least from the late nineteenth century and over the twentieth century that no longer define themselves in terms of the beautiful. On the other hand, the relation between science and truth has also been problematised along different axes. Moreover, there is a growing awareness of the fact that aesthetic processes also carry a knowledge element along with them and

that epistemic processes have aesthetic connotations. Doing science is, after all, a way of seeing. However, I am not saying that we should try to do away with these boundaries altogether. After all, we can't jump over history. I think Jacques Derrida's attitude concerning the dichotomies of occidental philosophy in his *Grammatology* ([1976] 1997) is still valid and valuable. We can't just get rid of occidental metaphysics with its millennial tradition. Whether we like it or not, we are in it. All we can do is shift boundaries from within. We should not have the illusion that we can start with a clean slate, but we can sharpen our awareness about these boundaries and then try to rework them from within.

How does this apply if one moves from art's productive role regarding perceptions to, for example, post-conceptual art as a type of systems art that seeks to produce events outside systematic or institutional definitions?

The main thrust of any experimental system is that it is able to point beyond itself. It would be boring if it did not work according to such a tendency of immanent transcendence, as it is so nicely described by philosopher and art historian Edgar Wind in *Experiment and Metaphysics* (2001).

Returning to the issue of practice with which we started, the notion of experimental approaches to art-making seems to imply that one's hands could be put to work differently. Experimentation can be seen as a way to cleanse late-Romantic expectations of authorship from what are otherwise very similar processes of moving materials around. While the practice of handling paint, for example, may be identical, the spirit of its handling has completely changed, so that a different artistic experience may be had. Is that something one can also trace in the sciences?

It is all about the epistemic effects of these acts and activities. They don't just occur out of the blue. Just to give an example: I have the impression that an artist like Cézanne, who painted hundreds of apples in his countless later still lifes, must have been caught in a kind of experimental system. It was all about tiny changes and iterations—doing it again and again and always with a small differential gesture. I am interested in the creation of differences through such processes of iteration, be it in the sciences or in the arts. Holding these small differences against each other produces knowledge effects. The very process of iteration brings these slightly different variants of an experimental process into contact with each other. It is not the relation between a thinking mind and object on the table in front of it, the classical relation between a knowing subject and an object posed before it; the basic idea is to introduce multiplicity at the object-level itself and thus to get rid of the classical dualistic structure of epistemology. Musical variations are a wonderful example of processes of iteration. In this sense, I think, scientific and artistic activities share something in common, although their respective knowledge effects may be of a different order or a different kind. Certainly the product is of a different kind. In the case of Cézanne, these still lifes can be seen in museums, whereas in science, comparable things are usually buried in protocol books. If they happen to

mature into a real product, then it's a publication, but trying to understand the whole process from the perspective of the publication or the finished painting is probably not enough. Looking at the way these things come into being, we may see similarities between the two creative activities—although, by the way, I don't like the notion of creativity. It tends to obscure the materiality of the process, and to locate itself on the spiritual side. An artist's studio is not only an aesthetic space, it is usually also an epistemic work-space with a lot of intellectual as well as material investment, an investment that tends to disappear in the product. But for the artist, it's an integral aspect of his or her work, without which she or he probably would not be motivated to carry out that work.

Given that at various points in your scientific discourse you make reference to art, it seems as if the closer one gets to moments of epistemic emergence, the more metaphors are required that implicate the arts.

At a very general level, we can identify points of comparison within what we call—for lack of a better notion— "creative activities." In all these areas of cultural activity, people are working—let us put it very generally—at the boundaries of the unexplored, of the unknown, to narrow it down for the sciences. They have to develop strategies that allow them to reach out into an uncharted space, while lacking the means to characterise that space from the point at which they stand. Reaching out into the unexplored is something that appears to me to be a common characteristic of all these activities, although, when it comes to the description of the details, the way this happens might turn out to be very different in a scientific, as opposed to, for instance, a literary exploration. It's all about activities that are situated at the boundary between the explored and the unexplored, where the explored usually takes the form of an arsenal from which you arm yourself in your work. We are confronted with a movement that is reaching out into a space that has a horizon that we cannot see—or, as Thomas Kuhn (1992) once very aptly put it, we are being driven into it from behind. We are not being driven into this open horizon by something identifiable out there that would tell us where we would have to arrive at; rather, we are being driven by the current state of the art—as the saying goes—but we know that we don't want to be captured in and bound to the current state of the art. A similar metaphor can also be found in Kubler ([1962] 2008). As an artist, he says, you stand in the dark at the end of a mineshaft that the generation before you has driven into the ground. Your exploratory activity is based on the fact that the shaft's end doesn't tell you which direction you should take. You know the direction that has been taken before you, and now you are in a situation where you are informed by what happened so far, but unable to act according to a far-reaching anticipation. I think this is a situation that has epistemic aspects to it, questions of orientation, be it in literature, music, the visual arts, the sciences in all their variety. These activities use widely different means and operate according to widely different gestures, behaviours, and forms of realisation, and the products that come out of these activities form universes that in no way coincide with each other. We are surrounded by a multiplicity of

cultural achievements irreducible to one another; but in spite of this multiplicity, there is something that these activities have in common.

What I just called "exploratory activity" is something that situates itself in the space of *bricolage*, and this is a dangerous space. It is an unsecured space. To return to an earlier moment in our conversation, it is not something that one would wish to impose on everyday life, or on society as a whole. These spaces are thus bounded and contained as exploratory spaces. And yet, our everyday life and our societies depend on them.

Can there be rules that one should follow if one is engaging in a research process? What structure does this activity have, given that one is constantly occupied with undoing structure? Structures can become obstacles that need to be overcome, as Bachelard (2002) suggests in his reflections on what he calls the "epistemological obstacle." He says that to establish something as a scientific fact creates at the same time a feat and an obstacle that henceforth has to be overcome again. Here we have once more the dialectic between the technical and the epistemic, under yet another perspective.

Why do you think that people with an arts background have such an interest in your work?

The question is very hard to answer because I am surprised myself about the resonance beyond the bounds in which my work was conceived. I can envisage two aspects that people with a background in the arts might find attractive. One is my focus on the materiality of the research process, and the other is that the kind of historical epistemology that I favour makes scientific activity appear less hermetic than it is usually seen to be. However, I like the interest, because it drives and challenges me to try to answer questions that I would not even have asked myself. When I am sitting with somebody like you, who is asking me all these crazy and sometimes hard-to-understand questions, it forces me, first, to reflect in novel ways and from novel perspectives about what I have been doing. Second, it brings the relations between the arts and the sciences into focus, and I think that this relation is in need of much more serious attention and much more historical as well as epistemological investigation.

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