

Comments on Hans-Jörg Rheinberger's "From Experimental Systems to Cultures of Experimentation"

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It is a great pleasure, but also a difficult task, to comment on Hans-Jörg Rheinberger's rich and important paper on experimental systems and the culture of experimentation. There are three principal difficulties in this task. First, Professor Rheinberger's paper is based in part on a series of extremely careful and persuasive case studies,¹ which, however are not directly employed in his paper. I will try to attach a tiny bit of biological flesh to the rather abstract bones of the resulting exposition. Second, it is important to bring out the great value of the *categories of discourse* (foreign to Anglo-American philosophy of science) in terms of which he has developed his analysis. They form the basis for a novel analysis of experimental biology and experimental sciences quite generally, with important and radical philosophical consequences. Finally, it is important to articulate some deep, but subtle, issues about the proper location and strength of the morals that he draws from his work. I shall offer some criticisms that I believe he *must* address in order to support in full the radical position I understand him to propound. This trilogy of tasks is a tall order for a brief commentary.

Before beginning the real work, I must emphasize that Rheinberger's extraordinarily valuable case studies demonstrate a deep understanding of the experimental systems that he analyzes. I am persuaded of the soundness of his account of the character of experimental work in the biological sciences — an account that provides him with a very robust basis for the serious challenges that he raises for conventional positions in Anglo-American philosophy of science. My disagreements with him do not rest on a criticism of the particulars of his account of the creation of what he calls *epistemic things* in the process of developing, and of working with, experimental systems. Rather, they rest on worries about the *status* of those things and their relation to issues about the reference of theoretical terms in science. In spite of a broad area of agreement, I shall try to show that he draws mistaken — or at least insufficiently argued — philosophical conclusions from his analysis of experimental systems and his implicit account of the sociology of experimental work. It is on this set of disagreements that I shall concentrate.

¹I am familiar only with parts of his work on twentieth century topics. This includes: *Experiment, Differenz, Schrift. Anmerkungen zur Geschichte epistemischer Dinge* (Marburg, Basilikenpresse, 1992); "Experiment, Difference, and Writing. I. Tracing Protein Synthesis," *Stud. Hist. Phil. Sci.* **23** (1992): 305-331'; "Experiment, Difference, and Writing. II. The Laboratory Production of Transfer RNA," *Stud. Hist. Phil. Sci.* **23** (1992): 389-422; "From Rous' Chicken Tumor I Agent to Microsomes: Research Trajectories and Strategies of Representation" [preliminary version of an article forthcoming in *Science in Context*]; "Experiment and Orientation: Early systems of in vitro protein synthesis," *J. Hist. Biol.* **26** (1993): 443-471.

The central issue about which we disagree concerns the accessibility of “the same” scientific objects from different experimental systems. This issue ramifies widely: it is connected to perennial issues about reference, truth, and progress in science and it leads into familiar issues about the nature of long-term progress in science generally, and in biology in particular. Thus, I shall touch on old and familiar quarrels about so-called scientific realism, issues that are of focal interest to many of the participants in this colloquium.

I

First let me explore certain accomplishments of Rheinberger’s analysis — accomplishments that should transform a lot of what philosophers of biology do when they focus closely on experiments. Rheinberger introduces a number of interrelated categories for describing experiments. Central among these are *epistemic things*, *experimental systems*, *ensembles of experimental systems*, *epistemic practices*, *bifurcations*, *conjunctures*, and *experimental cultures*. Perhaps the most important of these is the *experimental system*. Experimental systems require and establish a context of stabilized practice. They include, for example, properly managed stocks of *Drosophila melanogaster* or *Chlamydomonas reinhardtii*, subjected to certain classes of interrelated and intermeshed experimental protocols. They also include the *in vitro* systems, studied intensively by Rheinberger, that were used in attempts to simulate and dissect the mechanisms of protein synthesis in living cells. The protocols include a great deal of technique and technology, routinized in various ways. Note that in cases involving specific organisms, there is an immense amount of labor required to domesticate and control the properties of the experimental organism,² and that in both the *in vivo* and *in vitro* cases it is immensely difficult to locate and control the seemingly irrelevant details that make all the difference in the regularity of the behavior of the system and in its ability to produce or reproduce certain phenomena of interest.³

As Rheinberger stresses, citing Fleck, in experimental systems of both sorts, no single run by itself counts as producing a result or finding — typically not even the culminating run of

²See the special section of the *J. Hist. Biol.* **26** (2) (1993) on “The Right Organism for the Job,” including R. Burian, “How the Choice of Experimental Organism Matters: Reflections on an Epistemological Aspect of Biological Practice,” pp. 351-367.

³One of the many nice examples Rheinberger has produced in his case studies concerns the analysis of cellular components by means of differential centrifugation. The seemingly-simple substitution of a sucrose buffer for a phosphate buffer in which to resuspend centrifuged materials at intermediate stages of this process, a step taken by Hogeboom, Schneider and Palade around 1947, allowed the clean separation of mitochondria from secretory vesicles for the first time and opened the way to definitive establishment that mitochondria contain the respiratory enzymes crucial to the production of ATP. Vastly oversimplifying Rheinberger’s subtle analysis of the point, this was a crucial step in constituting mitochondria as scientific objects of a certain kind, namely, objects that supplied chemical energy to the cell. The most important oversimplification in thus isolating a single element of the complex and intertwined account in which Rheinberger embeds this episode is that the sequence of technical refinements yields a never-ending series of surprises, creating and removing epistemic objects in unpredictable ways so that a particle identified as cancer-producing, for example, becomes a normal constituent of a normal cell, and, with further technical refinements, switches identity three or four times. The object is reconstituted — reproducibly reconstituted — in new guises, which Rheinberger interprets in terms of features of the system of signification produced by the (changing) epistemic practices attached to the use of the (changing) experimental system.

a long series in which various artefacts and sources of error have been carefully eliminated, calibrations verified, and so on.⁴ This aspect of experiment, i.e., the extended character of attempts to create a finding (or an epistemic object) by hunting down and removing artefacts in order to get a clean result, is commonplace in the folklore of different sciences. The details differ according to the typical artefacts and systematic difficulties of different disciplines and systems, but all good experimentalists know that some systems never come clean in spite of Herculean efforts by first-class experimental groups — and that one cannot know in advance which ones will prove recalcitrant. In Rheinberger's terms, one needs to stabilize the experimental system if it is to produce clean findings. Without this, it cannot produce novel scientific or epistemic objects (though stabilization of the system is no guarantee of significant findings). Again, *when a new system does produce clean findings, they are often, perhaps typically, novel*, for otherwise they count only as a reinforcement of the already accepted background — and if nothing more is forthcoming, unless there is an immense gain in practical convenience, the system is soon abandoned. This aspect of experimental work has not entered seriously into the philosophical literature. One of the great virtues of the present paper and of Rheinberger's case studies in biochemistry is to place such features of experimental work into the foreground of our thinking. They should have been there long ago, and we must thank him for emphasizing them.

Note that it is not just regularity that is at stake in experimental systems. It is also the “dialectics of fact and artefact” (*MS p. 15*). Fact and artefact mark a distinction crucial to what Rheinberger calls the constitution or creation of an ‘epistemic thing’ (*MS p. 7*) or a ‘scientific object’ (*MS pp. 7-8*). Experimental systems must be reproducible, but their purpose, the purpose of the ‘epistemic practices’ of which they constitute the essential material part, is “to produce knowledge that we do not yet have.”⁵ This means that the objects they produce⁶ cannot be fixed in advance, cannot be accurately anticipated. “In the last resort, it is the future technical transformation which grants the scientific object a ‘legitimate position’ within the history of knowledge” (*MS p. 9*). Thus, when experimentalists produce epistemic objects, they maintain an experimental system or experimental arrangement “near the borderline of its breakdown” (*MS p. 10*), they operate at what Stuart Kauffman would call “the edge of

⁴Such classic experiments as the Meselson-Stahl and the Pajamo [Pardee, Jacob, and Monod] illustrate this point nicely. The latter, in particular, although presented as a neat single experiment in many publications represents a series of experiments spread over a year, with a follow-up year of debate among its authors about interpretation of the work and revision of the interpretation, guided by occasional experimental cross-checks. On both experiments, the original sources are widely cited. Appropriate secondary sources, providing references include H. F. Judson, *The Eighth Day of Creation: The Makers of the Revolution in Biology* (New York: Simon and Schuster, 1979) and G. Stent, *Molecular Genetics: An Introductory Narrative* (San Francisco: Freeman, 1971). F. L. Holmes, work in progress, will cover the Meselson-Stahl experiment in beautiful detail. For PaJaMo see also K. F. Schaffner, “Logic of Discovery and Justification in Regulatory Genetics,” *Stud. Hist. Phil. Sci.* **4** (1974): 349-385.

⁵This formulation is drawn from p. 5 of Rheinberger's original lecture manuscript. In the publication version, he now phrases the point thus: “Experimental systems ... are systems of manipulation designed to give unknown answers to questions which themselves we are not yet able clearly to ask” (*MS p. 6*).

⁶Recall that scientific objects are redefined by altering the constitutive conditions that produce them in the laboratory (*MS p. 7*) and that “the experimental conditions contain the scientific objects in the double sense of this expression: They embed, and they restrict them” (*MS p. 8*).

chaos.” This is how experimental systems can produce surprises and novelties, rather than serving as mere vehicles for tests. But it also means that only the future can determine what is fact, what artefact. For reasons such as these, Rheinberger, like the late Paul Feyerabend, considers the use of experimental systems *simply* to test theories in single runs a degenerate special case (*MS p. 6*).

The development of experimental systems requires an incredible amount of exacting routine — protecting experimental organisms against infection, ensuring their proper and consistent nutrition, matching them with experimental protocols, finding the proper way to prepare reagents for use in *in vitro* systems, adjusting the sequence of steps and the components employed in a protocol to achieve separation of unknown mixtures, developing elaborate technologies, etc. I would also stress something Rheinberger may not emphasize sufficiently — the importance of the questions under investigation. Very often, the experimental routine (and routines) in question are built up over scientific generations in the explicit attempt to answer open-ended, partially defined, partially redefinable, *questions*.⁷ These questions, even though ill-formed (for often they cannot become well formed until the experimental system produces a novel finding or object — see n. 6), are the common property of a laboratory group or a group of competing laboratories and, importantly (though Rheinberger may disagree), of a disciplinary or interdisciplinary community. The focus on questions, I think, is one key to the ability of experimental systems to yield Rheinberger’s ‘epistemic things’.⁸ With the focus on questions, one of the points that he wants to make becomes much clearer: when the use of an experimental system creates a new epistemic object, new questions are necessarily opened up and (at least some) old ones transformed or discarded on ground that they incorporated false assumptions.

Although there are some minor issues about experimental systems and epistemic practices about which Professor Rheinberger and I may disagree, he has put a great many important plain truths about experimental systems before us. It will take us a while to absorb his account of such systems and the practices with which they are associated, but doing so will force us, I believe, to rethink in deep and consequential ways our views about the creative roles of experiment in science and about the nature and importance of experimental verification and falsification. These topics cry out for extended comment, but I shall wash my hands of them in order to tackle other business.

II

⁷One way in which a scientist’s career can turn tragic is when s/he establishes clear questions — questions that clearly should be answered and ought to be answerable, at least in the long run — but cannot develop or find an experimental system adequate to the task. On one reading of the career of William Bateson, this was exactly the situation he landed in; he formulated seemingly clear questions about the connections of genes with development, insisted that one standard that any adequate theory of genes must meet is to provide a serious account of the role they play in controlling development, but was unable to find or produce adequate tools, or a useful experimental system, with which to attack this problem.

⁸He is at least sometimes inclined to agree. The manuscript for Rheinberger’s oral presentation at the colloquium included this passage at p. 25: “The model object...embodies what within a research program are interesting questions.... Whether a bacterium, for instance, is considered as a model of genetic replication, of the production or function of antibiotics, or of causing certain diseases will materially change its structure in terms of its life as an epistemic thing.”

What is missing in Rheinberger's account? Let me set up my concerns carefully, for it might easily seem that I am only complaining that he did not write a different paper than the one he did. That is *not* the form of the criticisms that will follow. Rather, I wish to open up what should turn into a long-term debate about matters on which I believe Rheinberger has reached premature conclusions — conclusions that he has staked out in a preliminary way in his paper, but which I do not believe he has adequately supported.

The chief issue concerns Rheinberger's implicit attack on, or dismissal of, long-term scientific realism. I shall first try to show that this is a fair characterization of his position. Then I shall connect this dismissal to three features of his paper: (1) his treatment of epistemology as *descriptive* rather than *normative*,⁹ (2) his relative inattention to the interrelations among scientific communities (which are, in part, connected by means of *questions* and *epistemological norms* in ways that transcend experimental systems) and (3) his failure to probe deeply enough the ways in which work with different experimental systems can be used to triangulate on the same processes or objects. All three of these issues shift the focus of attention from experimental systems toward experimental cultures.

Am I being fair to Rheinberger? I think so. It is important to note that his own laboratory work on ribosomes is much too much sophisticated and he is much too strongly committed to the experimental production of epistemic objects to be accused of any sort of naive anti-realism. He is serious when he treats scientific objects or epistemic things as realities created by an experimental system. Such objects and such systems, as *he* stresses, have a life of their own. Nonetheless, he is also utterly serious in his later arguments that such epistemic things are signifiers that cannot refer in a traditional sense to stable objects independent of the experimental systems in which they are — should one say 'created' or 'found'? Bachelard's concept of *phénomènotechnique*, to which Rheinberger alludes, is a useful tool for analyzing the sorts of issues raised here.¹⁰ Scientific objects are created by the use of techniques that create a space of representations, and we shuttle back and forth between different spaces of representation. In doing so, we abandon nature and deal with created objects, objects that are themselves *signifiers*, objects to which nothing corresponds 'out there', objects which could not be created if we let untamed nature into our test tubes. Thus, upon closer inspection, "representation ... may be taken to be equivalent to bringing scientific objects into existence" (*MS p. 14*). In this regard, Rheinberger uses the term 'representation' "sous rature", i.e., erased. It is defanged philosophically — but (as I shall insist) this erasure must come at a price. We always face the risk of a rupture in which one epistemic thing disappears and another takes its place. And such ruptures mean that scientific objects cannot be correctly interpreted in terms of "adequation or approximation [to] something 'out there', neither conceptually nor materially" (MS p. 13). It is Rheinberger's defense of this position, which it seems fair to characterize as strongly anti-realist, that I shall challenge.

What must be explored is how one might justify the claim that scientists working with different experimental systems, and thus different *epistemic* things, might nonetheless have good grounds to hold that they had gotten hold of different parts or aspects of the same elephant. Consider, for example, the sorts of studies that we say, loosely and partly retrospectively, bear

⁹See section III of this comment for a clarification of this characterization.

¹⁰On this, see the dissertation by Teresa Castelão, "Scientific Phenomenology and Science Studies: Gaston Bachelard and the Concept of Phénomènotechnique," Science and Technology Studies, Virginia Polytechnic Institute and State University, Blacksburg, VA, 1993

on the DNA molecule. (There is nothing special about this choice. One could easily make precisely parallel points about experimental studies and theories of, say, the electron, or of Rheinberger's favorite object of study, ribosomes. I choose DNA only because it allows me to use some otherwise inaccessible shorthand.) Consider, for example, how we should evaluate the relations among such diverse and diversely interrelated work as Avery's with *Pneumococcus*, Hershey and Chase's with T phages, Franklin's and Wilkins's with x-ray crystallography, Crick and Watson's with molecular models, Jacob and Monod's with phage λ and *E. coli*, Meselson and Stahl's with density gradient centrifugation, recent biochemical studies revealing the existence of enzymes that cut DNA at specific sequences of nucleotides, follow-up work using those enzymes in new experimental systems to yield so-called restriction fragment length polymorphisms and the sequencing gels to which Rheinberger alludes in passing. (Obviously the full list about which I should ask would be at least a few hundred items long.) The interesting thing is that we have reached a position from which we can understand all of this work in terms of a series of approaches to a common object or class of objects, the DNA molecule or various DNA molecules.

What needs to be highlighted here is that we develop norms and standards according to which some such cross-experimental system identifications are correct, and others incorrect. What Rheinberger identifies as bifurcations sometimes really are so in the sense that the original object, recovered and processed by a new technique, really turns out not to be at all what it was originally thought to be.¹¹ The epistemic object, the thing about which we sought to produce knowledge that we did not yet have, not only surprised us, but it proved to be something different than what we thought it was, what we constituted it to be. But note: one part of the stabilization of experimental systems requires us to fix them to reproducible reference points — typically by forcing them to re-produce findings made with other systems. To produce a lasting scientific object, one must not only re-produce it regularly and consistently within *an* experimental system or even a series of such systems; one must also mesh the findings gotten by genuinely different techniques and systems, one must triangulate on it and continue to be able to do so as new techniques and systems are developed. Thus, one must show by some technique or other that the daughter strands of DNA after replication fit with the structure of the model proposed by Watson and Crick. In this respect, the elegant findings of Meselson and Stahl, the product of an experimental system that they devised, one that matches Rheinberger's account very nicely, answers a pre-existing question not tied to that experimental system and satisfies a trans-community, trans-experimental system norm that must be met in order for a scientific object — in this case DNA molecules — to be constituted as a permanent finding, rather than as a transient epistemic thing tied to a particular experimental system.

III

On p. 3 of the text for his oral presentation at the colloquium, a text generally very close to the published paper, Rheinberger described his “historically informed epistemology of experimentation” as providing “descriptive coordinates for an understanding of ... [the] ‘discursive practices’ [of the sciences].” This account of Rheinberger's enterprise was

¹¹In Rheinberger's case study “Rous' Chicken Tumour I Agent,” (*loc. cit.*, n.1), the Rous tumor agent, or what was thought to be such, captured by differential centrifugation, proved to be a crucial constituent of normal cells; tumor agents thus came to be re-constituted as microsomes.

transformed between the oral presentation and the published paper. Now, the “epistemology of experimentation ... [provides] conceptual coordinates for an assessment of what, within the framework of a sedimentation analysis, or archaeology of knowledge, could be called the practical ‘dispositions’ and ‘depositions’ of the sciences” (*MS. pp. 2-3*). But the change from description to assessment seems to do no real work. His primary (and valuable!) concern is to describe the workings of experimental systems and of ensembles of such systems. *Conjunctures* and *bifurcations* provide a descriptive apparatus in terms of which he presents a phenomenology of the encounter of multiple experimental systems. He uses this apparatus to describe how scientists handle what, in other terminologies, have been called anomalies. But where are the assessments, and of what? So far as I can tell, if there are assessments concerning co-reference, they concern the ‘depositions’, not the procedures of the scientists. When we reach the ensembles of experimental systems within which bifurcations and conjunctures occur, there is not a word about the norms and standards relevant to determining, in a particular case, *which* of these outcomes, conjuncture or bifurcation, (if either) is appropriate or justified and what constraints are appropriate to the process of striving for resolution of the anomaly or anomalies at stake. In this respect, Rheinberger’s account remains as descriptive as his original text suggested it would be.

In order to address problems about the relations between the ‘epistemic objects’ created in experimental systems to objects ‘out there’, it is not sufficient to describe ensembles of experimental systems and their fusions and divergences. Rheinberger *has* taken a stand on our inability to break the circle of representation; he holds that we cannot reach objects ‘out there’. We should not begrudge him that stand, but we should insist that in this paper he has not adequately motivated his erasure of reference, let alone provided a strong argument in its favor. He cannot do the latter by means of his elegant descriptions of experimental systems and of the spontaneous creation of epistemic objects by use of those systems — or by his description of the bifurcations and conjunctures that arise in the interactions of various experimental systems. To address those problems, we must return to classic concerns about the *norms* pertinent to the evaluation of co-reference. Those norms are not immutable; for our purposes, many of the relevant ones are learned, even constructed, in the course of doing science. But when we deal with theoretical science, there is a strong sense in which those norms are trans-theoretical, for they concern how to evaluate claimed co-reference (or failure of co-reference) of particular terms, either in a particular context of application¹² or as generally intended within particular theoretical contexts.¹³ More generally, issues about the co-reference, or failure thereof, of terms referring to the particular epistemic objects created in particular experimental systems go beyond considerations pertinent to just those experimental systems. We may not be able to resolve such questions without decades of work, but that work is guided by (evolving) norms regarding what it takes to establish co-reference — norms that simply have not been recognized or addressed in Rheinberger’s paper. For the differences between Rheinberger’s position and mine to be properly adjudicated, these norms — and with them what Rheinberger calls *experimental cultures* and the communities that create those cultures — must return to center stage.

¹²Example: “Priestley’s use of ‘dephlogistication’ *on this occasion* picked out the same process as Lavoisier’s use of ‘oxygenation’.”

¹³Example: “In spite of the differences of meaning Bateson and Morgan attached to the term ‘gene’, when they used such terms as ‘the *short* allelomorph of *Pisum*’, they typically referred to the same entity — an entity different than either of them conceived, namely a section of a DNA molecule.”

The central concern here is how best to understand the enchaining of, and interactions among, ensembles of experimental systems. Following my Pittsburgh mentor, Wilfrid Sellars, I argue that we constitute our communities (including scientific communities) by including one another within the group to whom we apply norms (including, especially for scientific communities, epistemic norms). For this reason, I do not think we can achieve an epistemology of experimentation if we are satisfied with establishing descriptive coordinates, or “conceptual coordinates for an assessment of what, within the framework of a sedimentation analysis, or archeology of knowledge, could be called the practical ‘dispositions’ and ‘depositions’ of the sciences.” If we stop there, we will not understand the norms *employed prospectively*(!) for ensuring membership in a common community and for connecting the findings of one system to those of another. These are the norms that govern the process that, following Bill Wimsatt, I have called “triangulation.” And I do not think that we can understand the community (perhaps I should say the meta-community) formed among scientists who work on, say, DNA unless we can comprehend, or fix, the norms that bind their work to “the same” object. This is the ground on which proponents of Rheinberger’s position and proponents of a realist counter must meet in the coming years.

There is much about which Rheinberger is right, of course. An object such as the DNA molecule is in no sense directly available to us. We employ experimental systems in the ways he describes to create the epistemic objects that become the foils for trying to work out what it is our experiments have revealed. And experimental systems “inextricably co-generate the phenomena, or epistemic entities, and the concepts which these entities come to embody” (*MS p. 6*). But he is not right, I believe, in drawing the consequence that we have only relations and correlations among signifiers to work with. In ordinary life, we employ epistemic norms to enable us to work our way from *indefinite* descriptions to reach a common object — to secure reference to *the same thing, event, or process*. In experimental science, these rather vague norms taken from ordinary life are amplified, altered, and transformed as we learn by means of *phénoménotechnique* how better to capture the objects that we once could identify only vaguely. We forge our communities in part by forging the means to achieve co-reference and to ensure that the norms for evaluating co-reference work adequately for the purposes in hand. It is these norms that must be taken into account in our debates over the status of the scientific objects, found and created, that we encounter in our experimental systems, for it is these norms, if anything, that let us secure reference to objects even when they are not available as *givens* to copy.

To resolve the issues between Rheinberger and me, we must investigate, perhaps even improve, these norms. They are what bind experimental systems of the sorts he has so brilliantly analyzed to scientific communities and they are what allow those systems to be applied to questions that transcend particular experimental systems. It is these norms, which themselves evolve as we learn more about the world, that we must articulate and evaluate in our attempt to fathom whether we can secure reference to scientific objects in some way that transcends Rheinberger’s erasable reference to *epistemic* objects. It is these norms in terms of which we must evaluate our successes and failures to triangulate on the same objects by use of multiple experimental systems. With this, I hope to have said enough to initiate a debate about the status of the objects produced in, or found by use of, experimental systems — and to move that debate to deal with the larger communities and experimental cultures within which the development of particular experimental systems is embedded. I expect this debate to continue for some years as

we find our way about in this difficult topic, aided immensely by Rheinberger's case studies and his philosophical elaboration of the results of those studies.