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## Experimental Systems: Difference, Graphematicity, Conjecture<sup>1</sup>

At a symposium devoted to the structure of enzymes and proteins held in 1955, Paul C. Zamecnik talked about his laboratory's attempts to integrate amino acids into proteins in test tubes. When, in the discussion to follow, Sol Spiegelman referred to his own experiments on the induction of enzymes in yeast cultures, Zamecnik responded as follows: "We would like to study induced enzyme formation, too; but that reminds me of a story Dr. Hotchkiss told me of a man who wanted to use a new boomerang but found himself unable to throw his old one away successfully."<sup>2</sup> This remark is insightful concerning the auto-feedback character which is typical of experimentation in the research process. It expresses a fundamental experience of scientific work. The more familiar a scientist is with *his* experimental set-up, the more effectively *its* inherent possibilities open up. Formulated paradoxically, the more an experimental system is tied to the skill and experience of the researcher, the more independently it develops. The subject enters into a kind of inner connection to its object.<sup>3</sup> The boomerang is an image of this relationship, which can also be called virtuosity.

Alan Caren once asked Alfred Hershey what a scientist's dream of happiness was, and the latter replied: "To have one experiment that works, and keep doing it all the time." As Seymour Benzer reports, the first generation of molecular biologists referred to this situation as being in "Hershey heaven".<sup>4</sup>

François Jacob's autobiography, which contains a reflected record of a research process matched by only few scientists' own testimonials, contains the following sentences: "In analyzing a problem, the biologist is constrained to focus on a fragment of reality, on a piece of the universe which he arbitrarily isolates to define certain of its parameters. In biology, any study thus begins with the choice of a 'system.' On this choice

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<sup>1</sup> This text is a reworked version of the second part of *Experiment-Differenz-Schrift. Zur Geschichte epistemischer Dinge* (Experiment-Difference-Writing. On the History of Epistemic Things), Marburg an der Lahn: Basiliken-Press, 1992. For this version, the extensive scientific-historical part in the "Konjunktoren" (Conjunctures) section was shortened and the transitions smoothed out. The text has been translated into English for the first time.

<sup>2</sup> Zamecnik et al., 1956. Symposium on structure of enzymes and proteins: given at Research Conference for Biology and Medicine of the Atomic Energy Commission, sponsored by the Biology Division, Oak Ridge National Laboratory, Gatlinburg, Tennessee, April 4-6, 1955.

<sup>3</sup> What Lacan formulated for the sciences "conquered" by structuralism also applies here: "The subject is, as it were, internally excluded from its object." (Lacan 2006, p. 731.)

<sup>4</sup> Quoted in Judson 1979, p. 275.

depend the experimenter's freedom to manoeuvre, the nature of the questions he is free to ask, and even, often, the type of answer he can obtain."<sup>5</sup>

The contexts in which these three quotations stand could hardly be more different. They speak of experimentation in light of familiarity, satisfaction, and analytic constraints. But they all converge in one respect: they suppose an *experimental system* to be the smallest functional unit, to be a scientist's working unit. This has consequences for the theory and history of science. When asked what drives the research process, it is advisable to begin by characterising the experimental systems, their structure, and their dynamics, and not with an original, unavoidable primacy of theory (however it is formulated).

Should one rather speak of *experimental reasoning* then? If everything depends on the choice of a "system" – the experimenter's room for manoeuvre, the scope of the questions that he can ask, and the kind of answers he can receive – then even the expression "experimental reasoning" might still be misleading. The system's grammatical structure presupposes "reasoning" as the *genus proximum* whose specific difference consists in that it is being guided by an experiment. But what is at issue is the exact opposite: a movement oriented through instrumental peripheral conditions *in* which reasoning is torn into the game of material entities. Gaston Bachelard called the instruments of science "theories materialised."<sup>6</sup> He formulated it more sharply later: "Contemporary science thinks with/in its apparatuses."<sup>7</sup> Thus, it is the "scientific real" itself in its "noumenal contexture" which is "able to define the axes of experimentation."<sup>8</sup> At issue is the *writing game*, the tracing game of science. The expression alludes to Wittgenstein's discussion of the "language-game". "I shall," says Wittgenstein, "call also the whole, consisting of language and the actions into which it is woven, a 'language-game'".<sup>9</sup> We cannot retreat behind this *interweaving*. "Our mistake is to look for an explanation where we ought to look at what happens as a 'proto-phenomenon'. That is, where we ought to have said: *this language-game is played*."<sup>10</sup> Thus, I am not looking for a "logic" of the relationship between experiment and theory.<sup>11</sup> Nor am I speaking of theories, experiments, and instruments as relatively autonomous, intercalating layers of a scientific whole that manifests itself in postponements, side-shifts, extensions and

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<sup>5</sup> Jacob 1988, p. 234.

<sup>6</sup> Bachelard 1984, p. 13. As a mirror image of this, one could refer to theories as 'machines idealised.'

<sup>7</sup> Bachelard 1951, p. 84.

<sup>8</sup> Bachelard 1984, p. 13, translation altered.

<sup>9</sup> Wittgenstein 1953, paragraph 7.

<sup>10</sup> Wittgenstein 1953, paragraph 654.

<sup>11</sup> In the classic formulation of Karl Popper: "The theoretician puts certain definite questions to the experimenter, and the latter, by his experiments, tries to elicit a decisive answer to these questions, and to no others. All other questions he tries hard to exclude." (Popper 2002, p. 89.)

abortions.<sup>12</sup> My remarks concern, rather, that which can be irreducibly regarded as the *experimental situation*: a situation that offers technological conditions for the existence of scientific objects, differential reproduction of experimental systems, conjunctures of such systems and graphematic representation as the decisive aspects of practical production of what one might call “epistemic things.” What is meant by this?

To illustrate these considerations, I will cite an episode from the experimental history of protein biosynthesis: laboratory production of transfer ribonucleic acid in the 1950s. This molecule would go on to occupy a central place in the scaffolding of molecular biology. It formed the bridge between DNA, which stores genetic information, and proteins, which realise the biological function of the DNA. It was among the levers with which the genetic code could be deciphered. The history of the early test-tube production of these nucleic acids as soluble RNA has to be viewed, however, against a biomedical, bioenergetic and biochemical background, which initially had nothing to do with these later events. It is not a matter of the usual history of a “discovery,” but of what Foucault would have called the “archaeology” of what today is called transfer RNA.<sup>13</sup>

### **Experimental systems**

First, though, I would like to provide a few methodological and conceptual clarifications. How should an *experimental system* be understood? In traditional philosophy of science<sup>14</sup>, experiments are normally viewed as singular instances, as staged tribunals that are organised and conducted in order to corroborate or refute theories. Some time ago, though largely without success,<sup>15</sup> Ludwik Fleck argued convincingly, based on a historical analysis of biomedical research practice, that experimenters, contrary to the conventional views of philosophers of science, deal with everything except individual experiments. “Every

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<sup>12</sup> Cf. e.g. Galison 1988.

<sup>13</sup> “...what I am attempting to bring to light is the epistemological field, the episteme in which knowledge, envisaged apart from all criteria having reference to its rational value or to its objective forms, grounds its positivity and thereby manifests a history which is not that of its growing perfection, but rather that of its conditions of possibility; in this account, what should appear are those configurations within the space of knowledge which have given rise to the diverse forms of empirical science. Such an enterprise is not so much a history, in the traditional meaning of that word, as an ‘archaeology’.” (Foucault 1994, pp. xxi) The concept of the “archaeology” of knowledge is discussed in detail in Foucault 1969.

<sup>14</sup> Cf. Popper 1984, footnote 10. The idea of the experiment as the test of an hypothesis is, however, still virulent in more recent social constructivist approaches which explicitly deny the naked experiment the ability to decide on controversies in science (see e.g. Collins 1985).

experimental scientist knows just how little a single experiment can prove or convince. To establish proof, an entire *system of experiments* and controls is needed, set up according to an assumption [...] and performed by an expert.”<sup>16</sup> According to Fleck, in research we do not deal with individual experiments, nor, as a rule, with a clear-cut relationship between theory and experiment, but rather with a complex experimental arrangement set up in such a way that it produces knowledge that we *do not yet have*. And even more important: we deal with experimental systems which normally *do not provide clear answers*. “If a research experiment were well defined, it would be altogether unnecessary to perform it. For the experimental arrangements to be well defined, the outcome must be known in advance; otherwise the procedure cannot be limited and purposeful.”<sup>17</sup>

Thus, experimental systems can be regarded as the smallest functional units of research; they are set up in order to give answers to questions that we are not yet able to formulate clearly. In a typical case, an experimental system is, in Jacobs’ words, a “machine for making the future.”<sup>18</sup> It enables one in the first place to formulate questions that can be answered. It is a vehicle which serves to materialise questions. It cogenerates, if you will, the phenomena or material delineations and the concepts they embody. An individual experiment, on the other hand, as a sharp procedure for testing a sharp idea, is in no way the simple, elementary unit of experimental science, but rather the degeneration of an elementarily complex situation.<sup>19</sup>

## **Reproduction and difference**

So asking questions and giving answers within such an experimental system is an inherently open and inconcludable game. It is quite possible and common that a scientific object – and

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<sup>15</sup> Discussion in theory and history of science about the experiment and the experimental practice of sciences has actually only been taken up intensively in the last ten years, although less so here in Germany than in the Anglo-Saxon countries and in France.

<sup>16</sup> Fleck 1979, p. 96, emphasis added.

<sup>17</sup> Fleck 1979, p. 86. The chapter “Observation, Experiment, Experience,” from which these quotations were taken, is today still an unsurpassed masterpiece of describing a research activity in the biosciences.

<sup>18</sup> Jacob 1987, p. 13.

<sup>19</sup> Here I take up an idea that Gaston Bachelard formulated for the objects of contemporary physics as follows: “In very general terms simple always means simplified; we cannot use simple concepts correctly until we understand the process of simplification from which they are derived.” Bachelard 1984, p. 138, translation slightly altered.

this applies even more to its ultimate transformation into a technological one<sup>20</sup> – is not even conceivable at the time an experimental setup is established. But once a surprising outcome has emerged and is sufficiently stabilised, it is difficult to avoid the illusion of a logical path of thinking, let alone a teleology of the experimental process. I would like to quote François Jacob again: “How does one trace a path chosen for research work? [...] How does one re-create a thought centred on a tiny fragment of the universe, on a “system” one turns over and over to view from every angle? How, above all, does one recapture the sense of a maze with no way out, the incessant quest for a solution, without referring to what later proved to be *the* solution in all its dazzling obviousness”?<sup>21</sup> The construction principle of a labyrinth consists precisely in the fact that the already existing walls determine the space and the direction in which new walls can be added. A labyrinth cannot be planned. It builds itself. It forces one to proceed by “groping” and “grasping.”<sup>22</sup>

An experimental system owes its temporal *coherence* to its *reproduction*, and its development depends on whether one manages to *produce differences* without destroying its reproductive coherence. Together, these two factors make up its *differential reproduction*.<sup>23</sup> The construction process is dominated by a kind of probing movement which with regard to the scientific object can be described as a “*jeu des possibles*”<sup>24</sup> or a “‘game’ of difference.”<sup>25</sup> I would like to suggest that it is precisely the way in which it is “falling prey to its own work” that makes the scientific enterprise similar in a certain sense to what Derrida called “the enterprise of deconstruction.”<sup>26</sup> To play this game productively requires “*Erfahrenheit*”<sup>27</sup> on

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<sup>20</sup> On the relationship of scientific objects or epistemic things and technological objects or technical conditions cf. Part IV “‘Das ‘Epistemische Ding’ und seine technischen Bedingungen’ (The ‘Epistemic Thing’ and its Technical Conditions) in Rheinberger 1992.

<sup>21</sup> Jacob 1988: 274.

<sup>22</sup> Jacob 1988: 255, among other places.

<sup>23</sup> The concept of reproduction is complex. Here it does not mean so much the restoration of something used, nor the repeatability of a process as often as one likes (i.e. the ‘reproducibility’ of results), but rather keeping a movement going that enables an experimental system to be ‘productive’.

<sup>24</sup> Jacob 1982. The title of this ‘Essai sur la diversité du vivant’, ‘Le jeu des possibles’, refers to the “bricolage” of the evolutionary process as well as to the process of the sciences. Insofar as the scientific dynamic is at issue, the “possible” has to be taken in both senses: on the one hand it is that which never will have existed because things always turn out differently than expected; on the other hand, “one always has to have decided what is possible” (p. 22, translation altered).

<sup>25</sup> Derrida 1976, pp. 23-24 and elsewhere.

<sup>26</sup> Derrida 1976, p. 24.

<sup>27</sup> Fleck 1979, p. 96. “*Erfahrenheit*” is not simply “experience.” Experience connotes an ability to judge. *Erfahrenheit* - experiencedness - is a form of practice in which this ability is shown.

the part of the experimenter, something that can perhaps best be paraphrased using the paradoxical expression ‘acquired intuition’.<sup>28</sup>

We can conclude from what has just been said that one never knows exactly where an experimental system will lead. As soon as one knows exactly what it produces it is no longer a *research* system. An experimental system in which a scientific object gradually takes on contours in the sense that certain signals can be handled in a reproducible way, has to simultaneously open windows in which new signals are visible. Once it is stabilised in one respect, it can and *must* be destabilised in another in order to arrive at new ‘results’.<sup>29</sup> Stabilisation and destabilisation are interdependent. In order to remain productive, an experimental set-up has to be sufficiently open to produce unforeseeable signals and to let new technologies, instruments, and model substances seep in. If, on the other hand, it becomes too rigid, it stops being a “machine for making the future”; it degenerates into a testing facility geared to production of standards or replicas. Thus, it loses its immediate function as a research tool. As a stable sub-system it can, however, be integrated into other, still productive experimental systems and thus contribute towards producing new results in an indirect way. The transformation of former research systems into stable, technical sub-systems of other research arrangements lends the experimental process its specific material kind of growth and information storage. On account of the same mechanism, however, it also produces a historical burden. Usually, due to this mechanism, ‘new objects’ have to be brought to light by ‘old tools’. In the long term, however, a degenerated research system is completely replaced by technological systems which embody the current, stabilised knowledge in a more efficient form. Thus, the historian of science normally deals with a ‘museum of abandoned systems’ and must in the very first place reconstruct the context in which they made sense at all. So an experimental system is initially a research activity, is subsequently transformed into a technological system, and finally is replaced.

To remain a research system, such an arrangement must therefore be set up ‘differentially’. If it is organised such that the production of differences itself becomes the organising principle of its reproduction, then one can say that it obeys the kind of subversive,

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<sup>28</sup> It numbers among the attempts to do justice to the ‘surplus’ of scientific action, to what lies beyond all methodological axiomatisation. Cf. e.g. Michael Polanyi’s “tacit knowledge” (Polanyi 1967) or San MacColl’s “intimate observation” (MacColl 1989).

<sup>29</sup> In the everyday language of the laboratory scientist the ‘result’ is the unit with which the productivity of an experimental system is expressed. The ‘result’ is usually not the scientific statement concluding a programme, but rather a stone whose place in the mosaic still has to be found.

displacing movement Jacques Derrida referred to as “différance”.<sup>30</sup> In this sense, differential reproduction lends science, or better, individual research systems, their own internal time and turns the process into a “historial” one.<sup>31</sup>

## Representation

How does that which I provisionally termed the “writing game” take place within a research system, with its formal dynamic as a machine for producing the future? This leads us to the problem of *representation*. But what does representation mean?

Here ‘representation’ is generally to be understood in the sense in which German technical chemistry language uses the term in conjunction with the process of production, characterisation, isolation and purification of a substance. We will see that such a usage of the term leads to a fundamental undermining of its classical connotation, namely being something that stands for something else.<sup>32</sup> Within the framework of a given experimental system, a scientific object is unfolded within a space of material representation and brought to articulation. One certainly misses the specific nature of the procedure if one considers it simply as the ‘theoretical’ representation of a ‘reality’ of any kind.<sup>33</sup> What *practically* occurs in a research process is the realisation, i.e. production of scientific objects with the help of things that can already be viewed and handled as sufficiently stable material forms of knowledge. In turn, a realised scientific object itself becomes a tool, a technical construct, which makes it possible to new research arrangements. It is incorporated in the process of the realisation of that which one does not yet know. The only proof of its scientific form, or character, is the fact that, and only the fact that, it promises to change an already modelled piece of nature which the technological arrangement embodies at the present

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<sup>30</sup> Derrida 1976, p. 23. In ‘The différance’ the term is paraphrased as follows: “We will designate as *différance* the movement according to which language, or any code, any system of referral in general, is constituted ‘historically’ as a weave of differences.” (Derrida 1982, p. 12).

<sup>31</sup> Derrida 1976, p. 23; cf. the detailed Part III “Historialität, Spur, Dekonstruktion” (Historiality, Trace, Deconstruction) in Rheinberger 1992.

<sup>32</sup> The problem of ‘representational’ thinking has a long history in that which today is called Post-structuralism. For orientation cf. Bennington and Derrida 1991, among others the chapters entitled ‘Le signe’ and ‘L’écriture’, pp. 26-63.

<sup>33</sup> An experimental system embodies a knowledge horizon which can only be handled by manipulating the system itself. Subsequently the arrangement of its significant units can be transformed to an arrangement of *a second order* – to a graphic, algebraic, or other arrangement on paper (or in a calculator). And this is not simply an abstractive doubling process. It is itself again a representation in the sense of a purification, a selection, which can retroactively have an effect on the arrangement of the *first order*. Since moreover the arrangement of the first order has more signification possibilities than the arrangement of the second order expresses, other arrangements of the second order also have the possibility in principle to have an effect on a given experimental system.

moment. This process is in no way target-oriented from the very outset. It has to be “felt out” by those processes that Jacob described as “the abortive trials, the failed experiments, the false starts, the misguided attempts”.<sup>34</sup> Ultimately, the only guide through this landscape, as Goethe would have said, is “the kind of procedure itself”.<sup>35</sup> The latter alone produces the reference to which direction one can turn and where one has to change direction.

Representation: so what happens when the experimenter produces a chromatogram using a DNA sequence gel and a series of test tubes to which round filters are assigned, with which, in turn, its measuring units of radioactive decay can be correlated? All of these ‘epistemic things’<sup>36</sup> are objects of experimental interpretation. They embody certain aspects of the scientific object in a palpable form that can be handled in the laboratory. It is the arrangement of these graphematic traces or graphemes and the possibility of pushing them around in the space of representation that comprise the experimental writing game. It is out of these units that the experimenter composes his ‘model’.<sup>37</sup> How are the graphemes constituted? A polyacrylamide gel in a biochemical laboratory, for example, is at the same time an analytical tool for dividing macromolecules and a graphematic pattern of components that are made visible as coloured, fluorescent, UV-absorbing or radioactive spots. It *is* the scientific object, the realised model, which in turn is compared with other such models, to find out whether it ‘fits’. So the comparison here is not between ‘nature’ and its ‘model’, but rather between different graphematic traces that can be produced. That which conveys to us a sense of certainty of *one* ‘reality’, which we *ascribe* to the scientific object studied, is nothing other than this kind of fit. The scientific real is a world of traces.

The production of “inscriptions”<sup>38</sup> is neither a purely arbitrary process nor completely dictated by the material, technical conditions and instruments of the respective

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<sup>34</sup> Jacob 1988, p. 281.

<sup>35</sup> Goethe 1962, p. 315.

<sup>36</sup> Cf. the more detailed Part IV “Das ‘Epistemische Ding’ und seine technischen Bedingungen” (The Epistemic Thing and Its Technical Conditions) in Rheinberger 1992.

<sup>37</sup> Astonishingly, to date there has been little acknowledgement in scientific theory of the role played by dealings with model components, say, in the development of the alpha helix structure of proteins by Linus Pauling and the DNA double helix by Watson and Crick. Such models occupy the position of experiments where the comparison between different experimental representations is not or not yet possible. An experimental system always implies a model which in turn has to be made explicit by a further experimental system.

<sup>38</sup> Bruno Latour and Steve Woolgar distinguish between machines that “transform matter between one state and another” and apparatus or “inscription devices” that “transform pieces of matter into written documents” (Latour and Woolgar 1986, p. 51; cf. also Latour 1987, p. 64f.). It is often not possible to clearly distinguish between the two. What is a polyacrylamide gel? It transforms matter – it divides molecules – and it produces an inscription – for example a series of blue spots. Perhaps one has to go a step further and view the totality of the experimental arrangement – including both kinds of machines – as a graphematic activity. A written table or a printed curve is



system. In the process of production and differential reproduction in experimental systems, there is a constant interplay between presentation and absention. And this is the case because every grapheme is the suppression of another. In any attempt to show or strengthen ‘something’, at the same time an effort is inevitably made to make ‘something else’ disappear. It is like playing with pegs. When you drive in one, you drive out another. In an ongoing research process, one normally does not know which of the possible signals should be suppressed and which strengthened. This means that for a shorter or longer period the presentation/absention game has to be kept reversible. One must ensure that the research object can oscillate between different interpretations, i.e. realisations.

English translation: Burke Barrett

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