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ON THE POSSIBLE TRANSFORMATION AND VANISHMENT OF EPISTEMIC OBJECTS

Abstract: *When considering the question of possible transformation and disappearance of scientific objects, it is useful to distinguish between epistemic and technical objects. This paper presents preliminary observations and offers a typology of obsolescence. It is based on several case studies drawn from the history of life sciences. The paper proceeds as follows: first, the dynamics of epistemic objects is considered through the examples of Carl Correns' study of "xenia", Alfred Kühn's work on physiological developmental genetics, and Paul Zamecnik's research on the protein biosynthesis. Second, the phasing out of technical objects is then separately discussed and illustrated by the example of radioactive tracing in biology – until recently, an established technique of visualization.*

Keywords: *epistemic objects; technical objects; obsolescence; Carl Correns; Alfred Kühn; Paul Zamecnik; radioactive tracing*

O možné transformaci a mizení epistemických objektů

Abstrakt: *Zabýváme-li se otázkou možných transformací a mizením vědeckých předmětů, je užitečné rozlišovat mezi epistemickými objekty a objekty technickými. V předkládaném textu autor shrnuje některá předběžná pozorování a podává návrh, jak chátrání vědeckých předmětů typologizovat. Opírá se přitom o několik případových studií vycházejících z oblasti dějin přírodních věd. O dynamice epistemických objektů nejprve pojednává na příkladech Corrensova studia tzv. „xenie", Kühnova výzkumu v oblasti fyziologické vývojové genetiky a Zamecnikova výzkumu biosyntézy proteinů. V druhém kroku a odděleně pak autor sleduje fenomén opouštění technických objektů, k jehož ilustraci volí a diskutuje příklad radioaktivního stopování, které v biologickém výzkumu ještě donedávna platilo za etablovanou techniku vizualizace.*

Klíčová slova: *epistemický objekt; technický objekt; zastarávání; Carl Correns; Alfred Kühn; Paul Zamecnik; radioaktivní stopování*

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Introductory Remarks

Let me start with a preliminary conceptual clarification: The title talks about the possible transformation and vanishment of *epistemic objects*. What is an epistemic object, or an epistemic thing? Epistemic objects are the targets of research, those things about which we would like to know more. That is why they are particularly elusive. In their elusiveness, they stand in contrast to the technical objects – instruments, procedures, apparatus of sorts – with which they are supposed to be brought into interaction. Epistemic objects are thus characteristically underdetermined. Technical objects, in contrast, are characteristically determined. Both kinds of entities do not only come into being in the course of research processes and are integral constituents thereof. They can also become obsolete. In addition, they can become transformed into each other, of which I will give an example later.

Actually, in the first category, the epistemic things, we can observe – roughly speaking – two major forms of obsolescence: Either they turn out to be untenable as objects of epistemic concern under continued scrutiny, or one loses interest in them in the course of the research process because they do not keep what they promise. Objects in the second category tend to get out of use because they become replaced by more efficient devices. However, if we have a closer look at particular instances, we are confronted with much more nuanced situations and, correspondingly, more fine-grained ways of fading. We need to sort out these forms – or shapes – of "obsolescence", to pick up a term that art historian George Kubler uses in his book *The Shape of Time*, where he invites us to study the "common traits of invention, change, and obsolescence that the material works of artists and scientists both share in time."[1]

What I would like to do in this paper is to give a few historical examples for both kinds of entities – epistemic things as well as technical objects – ceasing to be of particular interest and consequently disappearing from the scene. The examples I am presenting are drawn from a number of case studies in the history of the life sciences that I have been conducting over the past 25 years. I start with the first category, the epistemic objects or epistemic things.

[1] George KUBLER, *The Shape of Time. Remarks on the History of Things*. New Haven: Yale University Press 1962, p. 10.

From Xenia to Mendel's Laws: Carl Correns

Let us first have a look at the German botanist Carl Correns (1864–1933) and his re-discovery of Mendel's laws in the years before and around 1900.[2] Correns set out for an investigation of an epistemic object that had been intriguing botanists for a long time and that had roused already the particular interest of Darwin. The phenomenon had been dubbed "xenia". It related to the observation that in certain plant species, characteristics of the pollen-giving variety did not only show up in the first hybrid generation, but already on the pollinated mother plant, preferably in the fruits and seeds. According to the accumulated literature, corn and peas were among the hot candidates for the phenomenon to be observed. The underlying physiological process was not known when Correns took up the issue, and it was exactly the problem that he intended to solve in the end.

But in order to do so, he wanted, to start with, to experimentally produce a clear-cut case in point of the phenomenon that was beyond doubt. Accordingly, Correns chose to settle on corn, starting from 1894, and peas, starting from 1896, as his experimental plants. This determined his experimental strategy: to produce as many different crosses as possible with a number of carefully preselected varieties. After three generations of crossing, in the summer of 1898, Correns still had failed to identify a clear-cut case of xenia with his peas. With corn, things looked a bit better in this respect. But he made an intriguing additional observation. His second generation of hybrids tended to show the original characters of the parents in a one-to-three relation, although the scatter of the results was quite wide.

At this point, Correns decided to revise his strategy. On the one hand, he pursued the clear cases of xenia in corn in order to prepare for a physiological analysis. On the other hand, he decided to grow a large amount of second-generation hybrids from the first generation hybrid of one of his original crosses between pea varieties yielding green and yellow germs, respectively, in order to see whether the observed segregation values held.

A year later, Correns learned from the recent literature that his colleagues Sergei Nawaschin (1857–1930) in Russia and Léon Guignard (1852–1928) in France had solved the riddle of the xenia: The phenomenon rested on a double fertilization process, resulting in a triploid endosperm surrounding the embryo. To pursue his efforts in this direction thus became meaningless,

[2] For a detailed description, see Hans-Jörg RHEINBERGER, *An Epistemology of the Concrete. Twentieth-Century Histories of Life*. Durham: Duke University Press 2010, chap. 4, and references therein.

and he contented himself to prepare for a summary of his work on xenia, a preliminary account being published late in 1899 and the extended report two years later.

On the other hand, Correns collected large numbers of seeds from his pea crosses as of 1899 and established the segregation ratio of 3:1 beyond doubt. Although he had to acknowledge that after all Gregor Mendel had reported the same results already three decades earlier, this finding completely reoriented Correns's work for the decades to come. Within a couple of years, he advanced to one of the founders of classical genetics. It is interesting in this respect that the point of intersection for the rise and fall of the two epistemic objects was the biological method of hybridization. Correns had started out to use that method in order to establish a physiological phenomenon. The phenomenon he observed was also a result of hybridization, but it revealed itself to be genetic in character, and above that, a purely formal, statistical correlation.

What do we have to do with in this case? It depends from which angle we look at the story. From the perspective of Correns's own research trajectory, we have to do with a coming into being of a new and the *replacement* of an older epistemic object. But the microstructure of this replacement is less clear-cut. In a first step, we observe the coexistence of two epistemic objects, one handed down, and the other one newly emerging. In a second step, one of them loses its interest for Correns and recedes into the background, not through his own experimental moves in the present case, but through the achievements of others. In the end, it leads to a complete reorientation of Correns's research focus. He had entered the episode as a physiologist, and he emerged from it as a formal genetician.

From the perspective of the research fields in play, things look a bit different. Whereas xenia had been looked at until then as being the result of an otherwise undefinable "influence" of the pollen on the mother plant, with Nawaschin and Guignard's work they were brought under the paradigm of heredity resulting from a non-standard fertilization process, and this very paradigm of heredity was just acquiring contours through the parallel work of Correns on the character distribution in pea hybrids. Thus, what happened here with respect to the epistemic objects, we should possibly rather address as a *displacement*.

From Hormones to Enzymes: Alfred Kühn

Let us turn now to the second example. It concerns the work of German zoologist Alfred Kühn (1885–1968) on physiological developmental genetics from the middle of the 1920s to the middle of the 1940s.[3] I will concentrate on a decisive episode in that work between 1930 and 1940 that resulted in two major changes in perspective. Before arriving at this point, however, I need to sketch the steps that set the stage for that change.

Kühn, together with his assistant Karl Henke (1895–1956), had started to work on the genetics of the wing pattern of the flour moth *Ephestia kühniella* in the middle of the 1920s, at a point in time where Drosophila melanogaster had already conquered the genetics laboratories around the world. The regular screening of their mass cultures of the butterfly yielded a red-eyed spontaneous mutant in 1929 that revealed itself to be recessive in nature and on which the future work came to rest. The complex wing pattern receded into the background as an epistemic object, whereas the comparatively "simple" switch in the eye color from black to red soon moved center stage. Thanks to an ingenious set of transplantation experiments performed by Kühn's doctoral student Ernst Caspari (1909–1988) in the early 1930s, it became clear that a substance was involved in the color change that was able to freely diffuse from wild type tissue transplants to the mutant recipient thereby changing the color of the eyes from red to black again. In tune with the scientific community's interest in bio-agents at the time, Kühn and Caspari addressed the substance as a "hormone" that was missing in the recessive mutant but could diffuse from transplants to the host tissue, thereby repairing the loss.

This new epistemic object now attracted all the attention of the laboratory. Years of purification efforts followed as a prerequisite for the biochemical characterization of the substance. The result of these efforts was that the assumption of a hormone in action had to be abandoned: What diffused from the wild type organ to the mutant host was a modified amino acid, whose production was obviously inhibited in the mutant due to the defect of an enzyme that catalyzed this step. The idea of a hormone as the epistemic object of the observed transformation now became obsolete. It was replaced by an amino acid derivative. But there was more: what emerged now was the vista of what Kühn called "gene action chains" acting on "substrate chains" via the intermediate of enzymes whose production could be hampered by

[3] For details see RHEINBERGER, *An Epistemology of the Concrete*, chap. 6, and references therein.

genetic mutations. With that, he had laid the ground for biochemical genetics as it became dominant in early molecular biology.

How can we summarize this case? Obviously the multiple transformations of the scientific object are even more complex than in the previous example. On the one hand, we can see that on several occasions, an epistemic object that was pursued for a while falls out of the trajectory: first, the pattern of the wing veins of the flour moth; then a hormone as a genetically triggered actor in eye pigment formation. As the trajectory unfolds, they *disappear* as epistemic objects from the process; they become *obsolete*. But the two cases also differ in one important aspect. In the first instance, a relatively complex epistemic object becomes replaced by a relatively simple one. In the second case, a relatively simple epistemic object, a hormone, turns out to be an even more simple substance, an amino acid derivative. But at the same time, it turns out to be merely an element of a vastly more complex network of a physiological combined with a genetic action chain. The strategic place in the experiment is the same, but it has become a much more complex structure of a different kind, at whose center resides another class of bio-agents, no longer hormones, but enzymes.

We see here in all perspicuity that in productive, dynamic experimental contexts, emergence and obsolescence are coupled phenomena. As a rule, an obsolete epistemic object becomes replaced by an emergent one. More precisely, we could say that obsolescence is conditioned by emergence. Becoming obsolete has, as a rule at least, less to do with the intrinsic qualities of the epistemic object, but more with a displacement of promise from one epistemic object to another. In this sense, we can look at experimental trajectories as a succession of epistemic objects with a longer or shorter endurance. The acts of replacement that punctuate the trajectory can be addressed in a Bachelardian sense as "epistemological ruptures."[4] They constitute minor reorienting or major, path-breaking turning points in an experimental trajectory.

From Oncology to Molecular Biology: Paul Zamecnik

Let me now come to my third example, the experimental trajectory of Paul Zamecnik's (1912–2009) group at Massachusetts General Hospital in Boston between 1945 and 1960, a detailed description of which is given in my book

[4] Gaston BACHELARD, *The Formation of the Scientific Mind*. Manchester: Clinamen Press 2002 (orig. 1938).

Toward a History of Epistemic Things.[5] I will briefly retrace a few of the major turning points in the group's effort to establish an in vitro system in which to study the mechanism of protein biosynthesis. The initial goal of the group was to find a clue for the metabolic differentiation between healthy and cancer cells using rat liver as a probe. A difference in the velocity of protein synthesis showed up indeed in the first experiments on the way to an in vitro system, but this difference remained mute experimentally: It did not lend itself further specification. Instead, a control became relevant. It showed that the incorporation of amino acids into proteins was dependent on a supply of energy. In pursuing this line, Mahlohn Hoagland (1921–2009), then a coworker of Zamecnik, was indeed able to demonstrate the activation of amino acids through linkage to ATP as a prerequisite for the incorporation process. Gradually, over the coming years, the energy dependence of the biosynthetic process under analysis took lead over and replaced the original oncological question.

In pursuing this line, yet another control became crucial for the further experimental itinerary. Around the middle of the 1950s, Zamecnik decided to test, as a side activity exploring his in vitro system, the nucleic acid synthesizing capacity of his cellular extracts. To that end, he added radioactive nucleic acid building blocks to the fractions, not without checking in parallel their protein synthesis activity by adding radioactive amino acids. To his surprise, a fraction of the radioactive amino acid appeared to be bound to a nucleic acid, not to protein. In analyzing this compound he identified a small, soluble ribonucleic acid in one of the fractions. He had identified this component in that fraction already before, but deemed it to be a degradation product of the much bigger microsomal RNA that he was unable to remove from the fraction – a contaminant of the system thus.

A control experiment thus turned out to be the starting point of a completely new line of research, and a contaminating small RNA of the supernatant fraction of the system became a new epistemic object, and one of an unheard-of kind: a hybrid molecule containing building blocks of nucleic acids and amino acids, a molecule that biochemistry simply had been ignorant of up to that date. In the further process of investigation, that amino acid-laden soluble RNA turned into "transfer RNA" when it became clear that this compound was the essential mediator in the uptake of amino acids into proteins along an RNA-template, the messenger RNA. Over a period

[5] Hans-Jörg RHEINBERGER, *Toward a History of Epistemic Things. Synthesizing Proteins in the Test Tube*. Stanford: Stanford University Press 1997.

of 15 years, from 1945 to 1960, working out that system had led the MGH group from cancer research right into the core of nascent molecular biology.

We can see here another aspect of the relation between epistemic and technical objects at work. Epistemic things can be turned into technical objects. As soon as the major features of transfer RNA had been identified – its amino acid carrier function, its coding adaptation function – it was turned into an object that operated as a molecular tool. It became the "Rosetta stone", as one of the protagonists put it, to solve the epistemic riddle of the genetic code.

In summarizing this case, we can discern at least four interesting transformations that enrich our picture of the dynamics of epistemic objects: First, We start with an epistemic object that does not lend itself to further experimentation – an "idling" object, so to speak: the difference between normal and malignant cells in protein synthetic activity. Second, A "contaminant" that resists being purified away turns into an epistemic object that has unique biochemical characteristics whose exploration will become a field of activity for the next three decades. Third, we observe the transformation of an epistemic object that has become stabilized – amino acid activation – into an experimental subroutine, that is, into a technical object in the context of the given experimental setup. And fourth, the biochemical epistemic object dubbed "soluble RNA" turns into a molecular biological object endowed with the powers of genetic information transfer – transfer RNA. In this fourth case, the biochemical object in the strict sense of the word remains, but its meaning and with that, its functional stake is being transformed: it starts to act as a molecular biological entity.

What we see in addition on this example is the tight relation between epistemic object and experimental system. The transformation of the epistemic object induces a shift in the direction of experimentation, and vice versa: a shift, and be it only a control, in experimentation can give rise to a new epistemic object.

The Phasing Out of Technical Objects

Let me come, in the last part of this paper, to the obsolescence of research technologies, that is, the technical objects implied in the research process. Their ways of phasing out have to be considered separately, since they differ considerably in that respect from the performance of epistemic objects. I will have a look at two research technologies that were narrowly involved in the construction of the third research trajectory described above, to

the extent that without them the whole trajectory would not have been possible.[6]

The first of them is a research procedure. Radioactive tracing – or radioactive labeling – allows one to follow the metabolic fate or certain biological compounds both in vivo and in vitro through the incorporation of radioactive isotopes into biomolecules. It became possible when, around 1940, radioactive isotopes such as tritium, carbon-14, sulphur-35, and phosphorus-32 became available for research, first as byproducts of cyclotrons, later and in much greater amounts, of atomic reactors. This made the labeling of biomolecules feasible that mainly consist of hydrogen, carbon, nitrogen, phosphorus, and sulphur. It allowed for tracing their path – and that of the molecules to which they were attached –, and often also the molecular nature of the transformations occurring in particular metabolic cascades. The advantage over the classical methods of microchemistry were at least twofold: First, the sensitivity of measurement was raised by several orders of magnitude, and second, it made it possible, at least in principle, to follow a particular reaction in samples of a limited purity, due to the fact that the radioactive marker carried with it the potential of distinction that left everything else in the background.

But in order to exploit this potential, new measuring instruments were needed that were adapted to the particularities of these tracers. As the name betrays, they were used in only trace amounts, and moreover, with the exception of phosphorus, they were all weak beta-ray emitters that could hardly be traced by conventional Geiger-Müller counters. The liquid scintillation counter was such an instrument. It was developed in the late 1940s and early 1950s, and it greatly facilitated the counting of samples containing these weak beta-ray emitters. Together, the tracing procedure and the counting instrument provided a powerful technology that decisively helped to push biology into the realm of the molecular in the course of the 1950s and the 1960s.

Forty years later, toward the end of the millennium, the technology – both the tracing procedure and the liquid scintillation counter – was on the verge of disappearing from the general equipment of a standard molecular biological research laboratory. In the 1990, advertisements such as the following could be found in the advertisement pages of research journals: "Make the move to non-isotopic assays!" What had happened? Over the course of time, visualization procedures had been developed that surpassed

[6] For more details see RHEINBERGER, *An Epistemology of the Concrete*, chap. 9.

radioactive tracing both in sensitivity and in versatility. Not negligible was an additional advantage: The new visualization technologies – among them fluorescent labeling – came without the danger of contamination and the additional burden of waste disposal that had haunted radioactive tracing from the beginning. It was thus an accumulation of different advantages that created a situation in which, after almost half a century, radioactive tracing became superseded by more convenient and more efficient alternatives.

We can rightly talk in this context of the *supersession*, or *replacement*, of one technical object by another one. The history of research technologies in particular is a history of *abandoned* instruments and procedures. Most of them have a historically determined life cycle and then disappear from the shelves. They are shaped to help bring new epistemic objects into being, and the frontiers at which this happens change continuously. We could, in parallel to the notion of epistemological rupture, possibly talk here about technological ruptures.

These ruptures happen, however, in a different time dimension. Whereas it is characteristic for epistemic objects to become obsolete more or less in a sudden break, it appears to be characteristic for technical objects to disappear in a more gradual fashion. Another, possibly more important difference concerns the character of the obsolescence itself. Whereas technical objects usually become replaced, their position being taken by another, more convenient, more precise, or more efficient technology, the vanishment of scientific objects happens in a different register. Usually, a scientific object disappears from the scene because people lose interest in it, without another epistemic object replacing it in the strict meaning of the word. The loss of interest can – sometimes at least – be due to the fact that the object of concern reveals itself to be phantasmal. In the majority of the cases, however, it simply becomes marginalized and is being dropped because it ceases to keep the promise that it seemed to offer, without being shown either not to exist or to lead on a wrong track.

These are, however, only preliminary observations and considerations for a typology of obsolescence in the sciences that has not yet been written.