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Essay review: Cold case reopened

Der Fall Paul Kammerer, Klaus Taschwer.
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Like a mystery novel, Klaus Taschwer's account of Paul Kammerer's life and work begins at the end. The controversial zoologist sets out alone on a warm September morning in 1926, for a hike on a mountain trail atop the Schneeberg, southwest of Vienna, and never comes back. He is found with a revolver in his hand, a bullet in his head, and a suicide note in his pocket. He was forty-six years old.

What drove him to it? Was it perhaps that his evidence for the inheritance of acquired characteristics—the infamous midwife toad—had been exposed as forgery? For many years, the answer was widely presumed to be yes.

But there were lingering doubts about Kammerer's guilt, and Taschwer is not the first to make a mystery of it. The novelist Arthur Koestler did the same in his *Case of the Midwife Toad* (Koestler, 1971). He aimed to rehabilitate Kammerer and revive Kammerer's ideas about heredity and evolution, and to that end, he called into question everything that had once looked like an open-and-shut case. Koestler not only searched for personal motives for the suicide, unrelated to any embarrassment Kammerer might have felt about the forgery, but he also cast about for a different forger, as well as a plausible motive. But who else had access to the specimens, and what could anyone else have gained by tampering with them? Koestler's finger pointed in the general direction of neo-Darwinians who, he argued, must have felt the need to discredit Kammerer's findings on heredity, which were incompatible with their doctrine. But he had nothing to incriminate anyone in particular, and left us a case without a solution.

Taschwer, science editor for the Viennese daily *Der Standard* and an accomplished historian as well, can now do better than that. The author of works on Konrad Lorenz, anti-Semitism at the University of Vienna, and other topics in modern Austrian science and medicine (Taschwer, 2015, 2016; Taschwer & Föger, 2009), he knows Kammerer's Vienna like no one else, and is in a position to name suspects and reconstruct motives that extend beyond the scientific realm.

After telling the tale of the suicide, Taschwer reviews the speculation about Kammerer's motives, as it appeared in the Austrian, German, and American press in the weeks after the incident. Initially there was little awareness of the forgery charges, and attention focused on the state of his career. On the one hand, there were disappointments such as his rejection for a professorship in Vienna or rising skepticism about the inheritance of acquired characteristics, but on the other, he had just spent months packing and preparing for a move to Soviet Russia. He was supposed to start up a laboratory in Moscow, so his scientific future was not so bleak. Maybe, as some writers suggested, he just could not bear to go through with the move and to leave the Viennese social and cultural scene behind. Or maybe it was something else entirely, like one of his love affairs—some of which were known to have been

stormy—gone bad.

Then, about three weeks after his death, *Pravda* published the suicide note Kammerer had sent to the Moscow Academy of Science, and the speculations took a new turn. The letter called attention to the recent revelation about the midwife toad. In one set of experiments, Kammerer had modified the mating behaviors of midwife toads, which are technically not toads at all, but frogs that mate on land instead of in the water. Kammerer had coaxed them to mate in the water instead, and the habit appeared to be heritable. Later generations even started to display dark, rough, nuptial pads (“heat stripes” in the translation, below) on their front legs, as seen on male frogs in the mating season (Kammerer, 1909). Only one pad, on one specimen from that line of water-breeding midwife toads was still intact in 1926, and the herpetologist G. Kingsley Noble, of the American Museum of Natural History in New York, found that it had been doctored with India ink. As Kammerer put it in his letter:

Presumably you all know about the attack upon me made by Dr. Noble in *Nature*, of August 7, 1926. The attack is based upon an investigation of the exhibits of alytes (toads), with heat stripes, proving my theory. . . . The principal matter of importance in this is an artificial coloring, probably with India ink, through which the black coloring of the skin in the region carrying the stripes is said to have been faked. Therefore it would be a matter of deception that presumably will be laid to me only.

Who besides myself had any interest in perpetrating such falsifications can only be very dimly suspected. But it is certain that practically my whole life's work is placed in doubt by it. (Kammerer, 1926, 494)

Who indeed had any interest in falsifying the specimens? Many possibilities have been explored before, in the press and in letters to *Nature* in 1926, in Koestler's book, and elsewhere, but none entirely satisfactory. The simplest solution would be to think the worst of Kammerer, that he inked the toad himself and fabricated the rest of his results as well (Alphen & Arntzen, 2016; Weissmann, 2010). But that idea generates some complications. He worked in a busy lab, the *Biologische Versuchsanstalt* (Institute for Biological Experimentation)—nicknamed the Vivarium—and too many people saw the experiments in progress and the modified animals. The director of the laboratory, Hans Przibram, who always vouched for Kammerer, had no vested interest in promoting Lamarckian heredity, though he considered it a possibility. By all accounts Kammerer was very idealistic about science and enthusiastic about his research, but also hasty and impatient. It is much easier to suspect him of sloppiness and error than of fraud.

It is hardly necessary to invoke fraud in order to explain Kammerer's results. The claimed morphological and behavioral modifications were not at all implausible, given the variability and plasticity of amphibians, and there are many alternative explanations and possible sources of honest error (Gliboff, 2006; Gould, 1972; Waddington, 1971). He changed their coloration or reproductive patterns, delayed or accelerated metamorphosis, or got them to express what were likely ancestral conditions or characteristics known from related species (For an

overview of the experiments in English, see Kammerer, 1912, pp. 421–441). Vestigial nuptial pads have been observed on midwife toads in the field, for example. I see no reason to doubt that at the end of the experiments, the animals looked and behaved as reported in Kammerer's papers and attested to by Przibram and other Vivarium scientists. That does not mean we have to accept Kammerer's explanations of *how* they became modified, merely that wrong results are not by themselves evidence of fraud. The intent to deceive must also be shown.

How, then, to explain the ink? Maybe it was intended only to enhance the appearance of a pad, not create it. Koestler considered the possibility that a naïve, but well meaning laboratory assistant touched up the deteriorating specimen (Koestler, 1971). Elsewhere, I have argued that Kammerer did it to reduce the glare when he tried to have photographs made of it (Gliboff, 2006). In support of the enhancement hypothesis, I cited Kammerer's earlier dispute with German geneticist and journal editor Erwin Baur over his retouching of salamander photographs submitted to Baur's journal (Baur, 1914; Kammerer, 1911a). Kammerer wrote to Baur, explaining that he only did it to restore dark spots that had been washed out by glare from the wet skin, and that it was justifiable because it made the photos look more like the actual specimens, not less. This was at a time when publishing norms for photographic evidence were not yet well established, and hand drawings still commonly used. If an author could be trusted to draw an entire picture, why not to touch up a photo? Kammerer could easily have applied similar reasoning to a little darkening of the nuptial pads for the camera.

The problem with my solution to the case lies in the timing of the events. The photos were made years before Noble's discovery. Would Kammerer really have displayed an inked toad all that time, even to a skeptical audience at Cambridge in 1923? Kammerer's friend, the botanist and Mendel biographer Hugo Iltis, recalls him saying,

Do you think I'm a Dummkopf or an idiot? Because that's what I would have to be if I left a forgery with ink standing around openly in the laboratory, where so many of my enemies have entry, or if I even sent it to a scientific congress. (Iltis, 1951)

Iltis found this defense convincing, but I am not so sure. Kammerer did some foolhardy things in his life. Why not this one as well? But there are additional problems with an early date for the inking. Koestler, for instance, raised the question of how long the spot would remain distinct after the ink was injected. At his request, the biologists Holger Hydén and Vivarium alumnus Paul Weiss tried to replicate the tampering, and found that the ink dissolved or thinned out in a matter of weeks. Koestler inferred from this that the toad must have been injected in December 1925 or January 1926, since Noble made his discovery in late January or early February. But the inference is less than conclusive, because, as they told Koestler themselves, Hydén and Weiss did not know what ink or what preservative was in the original midwife toad. All they showed was that some ink dissolved in some preservatives (Koestler, 1971, pp. 109–113).

Probably the greatest weakness of the enhancement hypothesis is that it requires that no one have noticed the ink for a long time, not even on the trip to Cambridge, where several biologists looked at the specimen. But I wonder how hard anyone really looked before Noble, or whether anyone who looked was enough of a herpetologist to pass judgment on the authenticity of the pads. Against the idea that the tampering could have been missed, Taschwer points out that Noble described the injecting job as crudely done, and the needle-hole easy to spot.

One more main possibility is that the specimen was injected maliciously, and that Kammerer was framed. That version was dramatized by Anatoly Vasilyevich Lunacharsky, the Soviet People's Commissar for Education, in a play and a movie made soon after the scandal. Lunacharsky depicted a conspiracy of Catholic, capitalist, and aristocratic forces against Kammerer and his science. That science was a threat to them because it supported dialectical materialist assumptions

about human nature and improvability. But who in particular could be suspected, and what could the motive have been just then in 1926?

Koestler, too, supported a frame-up thesis, but he sought his villains not among the political and religious ideologues, but among scientific ones. The neo-Darwinians, he insinuated, had no honest way to respond to Kammerer's evidence for Lamarckian heredity, or for that matter, to his arguments about the nonrandomness of variation and of coincidences generally. Indeed Koestler's original interest in Kammerer might have been in connection to his own theory of coincidences. He elaborated on it, and defended Kammerer further, in his next book (Koestler, 1972a). But the idea that the neo-Darwinians could brook no opposition or deal with challenging cases is a gross distortion of early twentieth-century biology. No one suppressed Kammerer's results. He published prodigious numbers of articles in leading academic journals, textbooks, and the popular press, and went on lecturing tours with his modified animals. His main results were prominently discussed in articles and textbooks by mainstream geneticists and Darwinians of all stripes, who did not resort to accusations of fraud in order to reject Kammerer's conclusions.

Still it commonly asserted that the biologists of the day rejected Kammerer's results out of hand because of their narrowminded expectations and inability to deal with challenges (Begley, 2009; Dunbar, 1983). If only they had knowledge of modern epigenetics, so the latest version goes, they could have understood and accepted some of the midwife toad results. In particular, the water-breeding trait behaved, according to Kammerer, like a dominant Mendelian allele, but only when inherited from the father (Kammerer, 1911b), a result that supposedly baffled classical geneticists, but which epigeneticists now call parent-of-origin effects (Vargas, 2009; Vargas, Krabichler, and Guerrero-Bosagna, 2016; Wagner, 2009). But, aside from the fact that the particular epigenetic model that was proposed makes unsafe assumptions about the genetic basis of the mating behavior, and did not fit the experiment anyway (Gliboff, 2010), scientists do not usually deal with exceptional cases in that way. On the contrary, early Mendelians, including Koestler's arch-villain, the British geneticist William Bateson, made special studies of what was then still “non-Mendelian” heredity, including parental effects, epistasis, latency, and gene-gene- and gene-environment interactions (Bateson & Saunders, 1902, pp. 119–123, 132). Such phenomena presented opportunities for new research, new discoveries, and expansion of the Mendelian program. No one was interested in defending some doctrinaire interpretation of Mendel's original paper (Gliboff, 2015). Bateson's suspicions of Kammerer were raised not by the unexpectedness or incomprehensibility of the result, but by what he perceived as evasive responses to his requests for more information about the nuptial pads. Indeed, he took an interest in them because he initially thought they might illustrate his own concept of a mutation, not because they were a threat.

Finally, even if we were to concede that Kammerer had scientific enemies with motives for discrediting him, who in the field was close enough to the Vivarium to get in there and do the deed? And, again, why bother in 1926? Kammerer's animal stocks had died out by then, he had no new experiments in the pipeline, he had no academic job, and the inheritance of acquired characteristics had been losing support for years. It is on the issue of motives that Taschwer makes his most important contribution.

Where Lunacharsky and Koestler set their scientific crime stories on a world-stage, and pitted Kammerer against ideologies, Taschwer's story plays out locally, in Vienna. He favors a frame-up hypothesis of his own, but with identifiable people and personal motives. He has done yeoman's work in reconstructing infighting and treachery at the University, identifying Kammerer's enemies and friends, and in revealing the wider social and cultural circles to which Kammerer belonged and the audiences that he tried to reach with his science.

Once his main character is dead and buried, Taschwer devotes a chapter to the subsequent politicization of Kammerer's work (pp. 22–38). Lunacharsky's association of Kammerer with communism had a

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