

REVIEWS

Apologia for Paul Kammerer

THE MIDWIFE TOAD. Arthur Koestler. Hutchinson, 1971. pp. 187. £2.

We are presented here with an account of what is called "the greatest scientific scandal of the first half of our century", whose "hero and victim was one of the most brilliant and unorthodox biologists of his time. He was 45 years old when the joint pressures of an inhuman Establishment and his own all-too-human temperament drove him to suicide. He had been accused of the worst crime a scientist can commit: of faking his results. Yet an obituary article in *Nature*, which is probably the world's most respected scientific journal,* called his last book 'one of the finest contributions to the theory of evolution which has appeared since Darwin'."

The book ends with a plea that Kammerer's experiments be repeated with an open mind for "Nothing that transpired in the controversy justifies the abandonment of a line of research with far-reaching theoretical implications".

Such is the framework of this book by Arthur Koestler, which is a historical account of the once celebrated controversy centred around the experiments of Paul Kammerer.

It is difficult for a practising geneticist to read such a book objectively, especially one written in the persuasively emotive style of so skilful a writer as Koestler. We have been here before so many times and all evidence has always receded, so that it is difficult to read without irritation. Yet, since the urge to believe in the inheritance of acquired adaptations seems so deep seated in so many people, and has in recent times had such disastrous consequences in Russia, it behoves us to take this book very seriously, and to ask what sort of case is made. Especially is this so because the author's name will inevitably draw a large admiring readership.

After an introductory chapter the book begins with a brief "account" of the difference between Lamarckian and neo-Darwinian evolution, and goes on to an account of Kammerer's experiments and the resulting controversy leading to the denouement of Kammerer's suicide after the discovery that the only remaining specimen (preserved) of the modified midwife toad had been injected with indian ink.

As the book is written, Kammerer is the hero and Bateson the villainous representative of the "inhuman Establishment". Trying to discount the effect of this, I come to the conclusion that there may be a case that Kammerer might have been treated better at the time and referred to less harshly since. It does not seem by any means certain that *he* doctored the preserved specimen of the midwife toad. Neither does it seem certain that a specimen that had earlier been demonstrated in Cambridge and London was the same one or that it itself had been doctored, though according to Cannon's account of the occasion in London, Bateson whispered to him "it looks to me like a spot of black ink", and Koestler does not mention this. Nor does it seem that Kammerer's experiments with salamanders and *Ciona*, which Kammerer regarded as more cogent, were criticised in sufficient detail. The evidence that Kammerer was fraudulent rather than naïve has probably been accepted too readily by many. Likewise I think a case could be made

* Note the scholastic appeal to authority.

that Lamarckian or Buffonian postulates have been treated with ridicule too often—Koestler quotes in particular Darlington's "a disreputable and ancient superstition". In fact these views were quite reasonable as hypotheses, and our view of their truth should depend on fact not their respectability, the fact being, of course, that cases claiming to show inheritance of adaptive acquired characteristics that have been properly looked into have always proved will-o'-the-wisps.

This said, however, we are bound to turn from the case for Kammerer and consider how Koestler himself handles it. Here we find weaknesses that cannot be ignored. First and most important, it is assumed throughout the book that, if it could be shown that Kammerer got the results he described, it would follow that the conditions of his experiments caused the hereditary modifications he claimed to observe. Nowhere does the author cogently and critically discuss the pitfalls of such interpretation, or the difficulty of designing experiments that could only be thus interpreted and could not be explained by inbreeding, segregation, and/or unconscious or natural selection. And this despite the fact that Koestler quotes other parts of relevant literature such as Bateson's *Problems of Genetics* in which the cardinal point was made quite clearly as follows: "Until the normal and undisturbed course of descent under uniform conditions is ascertained with exactness, it is useless to survey the consequences of external interference."

I will not speculate here whether this omission is a consequence of naïvety on Koestler's part or whether it was intentional. It would certainly have greatly weakened his "argument" especially as he quotes, seemingly viewing it as in Kammerer's favour, evidence that variants that Kammerer claimed to have produced also occur naturally, even the nuptial pads of the midwife toad, which Bateson, perhaps wrongly, regarded as the most critical because he did not know of the similar natural variants later reported by Kandler (p. 103). In addition, if the account of the *Ciona* experiments is accurate, it would seem that the offspring of the modified *Ciona* grew in the same medium as their parents so that induction is a conceivable alternative explanation of the results.

This sin of omitting such critical discussion is compounded with sins of commission, involving imputations of motivation to, and interpretations of the written words of, the anti-hero Bateson that add up to what can only be described as "smear". Consider (p. 68): "Bateson . . . dismissed Kammerer's fifteen years of work with *Salamandra* with the single remark that 'salamanders corresponding with Dr Kammerer's several patterns can be had from the dealers'. . . the implication of Bateson's remark was that Kammerer had simply bought his specimens 'from the dealers'." Koestler does not hesitate to impute this meaning to Bateson, though it is equally plausible that Bateson was referring to the possibility that the results were explicable through heterozygosity of Kammerer's initial material. And just before this imputation we have Koestler's view of Bateson's motives (p. 63): "Bateson set out for Lake Balkash to prove the inheritance of 'acquired adaptations' and was unable to do so. Kammerer came 'uncommonly near to proving it' without spending eighteen months with the savages in the steppes and feeling like a fool afterwards. Was he to succeed where Bateson had failed?" Such writing is I think disreputable.

The imputation of undesirable motives in what should be scientific argument is fashionable in certain quarters nowadays as a means of avoiding

critical thought. This does not make Bateson's motives for attacking Kammerer relevant to assessment of Kammerer's work.

However, Koestler treats them as relevant, so that we are forced to ask what Koestler's motives themselves are, to ask why he wrote this book and wrote it as he did. The answer seems to be that Koestler dislikes what he understands of contemporary evolution theory, and what he thinks are its consequences. "In the Lamarckian view, evolution is *cumulative*; in the Darwinian, *repetitive*; it could go on for millions of generations without any evolutionary progress." He seems unable to stand the possibility that progress may not be inevitable, unable to understand that evolution through natural selection can be a creative process, and, perhaps more important, unable to accept that evolution is a stochastic process. At the same time there is sympathy for the heretic (though Bateson also was a heretic): "The secret of his (Kammerer's) attraction (to Koestler) lies partly in his complex character and tragic fate, but above all in his heretical ideas, . . . Kammerer refused to accept the Darwinian theory of evolution based on random variations—haphazard variation produced by *blind* chance. . . ." (Reviewer's italics.) Hence Koestler hankers after Bergson, and quotes with evident approval Kammerer's wish that acquired characters be inherited, so that the results of education may "enter into the life sap of generations" and that thus the hereditary material may be transformed into "a new and better one for the future" (Kammerer's heart was in the right place). So also he takes every opportunity to denigrate "orthodox" individuals, quoting a description of Noble who discovered the indian ink as "a ruffian", and stressing that amateurs (Kammerer started as a musician) are not regarded as respectable by established scientists. Likewise he makes capital out of the irrelevant fact that Romanes lifted someone else's bibliography for an article on hybridisation in the *Encyclopaedia Britannica*, and accepts as outright proof Fisher's evidence that Mendel's data was "faked—or to use a more polite term, doctored" (so why all the fuss about the indian ink?) though several geneticists have pointed to other explanations of the too close fit of Mendel's data to expectation.

The sad thing is the result: a book which, though it may be intended to invoke dispassionate and critical reconsideration of Kammerer, is in fact emotive polemic, in which the orthodox is anti-hero and in which the heretic is hero, whether or no he is right, suffering from the "inhuman Establishment" (note the capital letter) with its "*doctrine* of genetics" (p. 37, reviewer's italics).

The author of this book is the same man who wrote that notable essay in "The God that Failed". Is he still unable to recognise that scientific truth is not to be judged by the motivations of scientists, nor by whether one likes what one thinks are its consequences, nor by its consonance with any faith, but is only to be approached by painstaking attention to the factual evidence, to the logical justification of interpretations of that evidence, and, most essentially, to the design and execution of experiments that test these interpretations?

It is certainly a good thing that old work that has been dismissed should be reviewed from time to time, but this is not the way to do it. It is to be hoped that Koestler will not now see fit to write an apologia for Lysenko.

J. M. THODAY

Department of Genetics, University of Cambridge