BOOK REVIEW

The art of innuendo

Of Moths and Men: An Evolutionary Tale

J Hooper

W.W. Norton & Company, New York. 2002; 377 pp.

\$26.95, hardback. ISBN 0-393-05121-8

Reviewed by Bryan Clarke

Heredity (2003) 90, 279–280. doi:10.1038/sj.hdy.6800229

It is something of a shock to discover that the events of one's youth have become material for the history of science. It is more of a shock when the characters in that history bear only a superficial resemblance to the real ones.

In the 1950s, the Department of Zoology at Oxford was an exciting place to be. Niko Tinbergen was carrying out the behavioural work that led to his Nobel Prize. Michael Fischberg and John Gurdon were, for the first time, successfully cloning a vertebrate from its somatic cells. Arthur Cain and Philip Sheppard were involved in their classic studies of natural selection in *Cepaea*, and Bernard Kettlewell was organising his big experiments on peppered moths. There was an atmosphere of ferment and sharp questioning that kept everyone on their toes. Judith Hooper has written a book about it, but the department she describes is not the one I remember, and the people in it seem to be caricatures.

She writes about the origins of ecological genetics, and particularly about Kettlewell's experiments on the evolution of industrial melanism in peppered moths. She writes well, but the tone of her book suggests that she has purposefully set out to cast doubt on the evidence for natural selection. In doing so, she forsakes the prime responsibility of historians and biographers, which is to be fair to their subjects. Her talents as a writer make this failure the more regrettable. She repeatedly implies, but never quite states outright, that Bernard Kettlewell and his colleagues fabricated their data, and argues that they were, at the least, fatally careless.

Hooper has talked to most of the surviving people who worked at Oxford during the heyday of ecological genetics. They must have told her about the virtues and the vices of the *dramatis personae*, but somehow the vices are emphasised and the virtues are neglected. The cumulative effect is powerfully slanted. Hooper deals in the same way with the scientific evidence. Experiments and observations casting doubt on natural selection are highlighted, and supportive evidence is either ignored or disparaged.

She starts off as she means to go on. In the introduction, she describes EB Ford as a 'megalomaniac' who 'headed a scientific coterie'. The experiments on peppered moths were 'establishing the Oxford biologists as masters of their world', but at their core lay 'flawed science, dubious methodology and wishful thinking', round which clustered 'a swarm of human ambitions, and self-delusions'. When she gets into her stride, people

working in ecological genetics are described as Ford's 'disciples', 'acolytes', 'underlings' or 'protégés', and those reporting evidence of natural selection were 'exulting', 'declaiming', 'raving', or 'arrogant'. Fellows of the Royal Society from the Oxford department were 'strutting'. In truth, of the four Fellows concerned, Alistair Hardy, Niko Tinbergen and Philip Sheppard were notably free of pomposity. EB Ford was eccentric and often unpleasant, but he never strutted. His mode of progress more closely resembled an insinuation. There was not a strut in the place.

Hooper imagines that it was difficult for people in the Oxford department to question authority, yet the whole system was devoted to such questioning. The greatest kudos could be gained by successfully challenging a widely held hypothesis. She regards the Oxford tendency to disputation as a taste for 'verbal jousts...as archaic as they were pointless', not realising their serious academic purpose, which was to give exercise in the use of logic. Protagonists were forcefully obliged to be clear and self-consistent. It is a practice that could benefit other institutions and people.

To make her case, Hooper exaggerates the importance to evolutionary theory of Kettlewell's experiments, and then exaggerates their deficiencies. She does it with dexterity. Noting that the experiments have been described in many textbooks, she quotes a couple of very uncritical examples. She does not distinguish clearly between a good illustration, which the experiments provide, and the foundation upon which a whole theory depends. She ignores the vast literature that establishes the role of selection by predators, summarised in turn by Poulton (*The Colours of Animals*, 1890), Cott (*Adaptive Coloration in Animals*, 1940) and Edmunds (*Defence in Animals*, 1974). She argues that peppered moths 'were meant to be the moths that proved Darwin right', as if Darwin needed help at the time. She dismisses other experiments, such as those by Cain and Sheppard on *Cepaea nemoralis*, that show the power of visual selection.

Her treatment of *Cepaea* is a good example. She does not discuss the relevant observations or experiments, but merely repeats some dismissive remarks made by Will Provine, the historian of science. I hope for his sake that she has misquoted him. Provine is reported to have said, 'When Cain and Sheppard did their work on *Cepaea*, it was considered to be one of the best examples of natural selection. Then Camille Lamotte did the same stuff in France and didn't get their results at all. Cain and all these people came up with what they called 'area effects', which took away from what they did earlier. It wasn't clear at all'.

These four sentences contain four errors. First, Lamotte's name is Maxime, not Camille. Second, he published on *Cepaea* before Cain and Sheppard, and their experiments countered his. Third, Lamotte himself found evidence for selection by predators, reported in 1951 and 1959. Fourth, the phenomenon of 'area effects' has been shown by Robert Cameron to be caused by historical changes in the environment, and does not invalidate the evidence for visual selection.

In dealing with Kettlewell's experiments, Hooper sets out to discredit them and him, making much of his assumed desire to please his 'boss' (EB Ford). She notes that peppered moths habitually rest at the bases of high branches, rather than on low vertical trunks, where Kettlewell carried out his experiments. She argues as if crypsis at one position would not be correlated with crypsis at the other. When she is faced with the difficulty that selective predation was observed, and filmed, by Niko Tinbergen, she creates a diversionary mystery about how long Tinbergen was in the field with Kettlewell, and then gratuitously mentions Tinbergen's late, and incorrect, belief in a treatment for autism. Are we to doubt Linus Pauling's work in molecular biology because he had a late, and incorrect, belief about the medical uses of vitamin C?

Referring to the set of experiments in which Kettlewell offered moths to captive birds in cages, Hooper takes him to task for changing the experimental conditions when the initial results did not show selective differences between the melanics and the non-melanics. In his later experiments, when the birds were offered other prey as well as peppered moths, selection occurred. Hooper interprets this as Kettlewell manipulating the experiments until he got the result he wanted. Apparently, she does not recognise that it is reasonable to discover the conditions that favour the phenomenon you want to study. Later work has shown that Kettlewell was quite right to change the conditions. Birds, if they are offered just one species of prey, will quickly learn to recognise its different varieties. In more natural circumstances, when they encounter several different species of prey, the predators are more easily fooled, and so exert selection.

Bernard Kettlewell was a highly gifted amateur lepidopterist. He was not a trained scientist. He never got to understand the refinements of theory, as can be

seen in his book, which is embarrassingly bad. Nonetheless, he had an extraordinary capacity for organising and executing studies in the field. More or less single-handedly, he accomplished what was then the largest and most demanding set of experiments ever carried out under natural conditions. He did it on a financial shoestring. With the benefit of hindsight it is easy to criticise Kettlewell's experiments, and with the benefit of a large research grant it would be easy to improve on them. There is no good reason, however, to doubt that his general conclusions were correct. Later work has supported them.

By emphasising Kettlewell's insecurity as an amateur among the academics, Hooper insinuates further motives for slackness or fraud. Indeed, her whole book is a treasury of insinuations worthy of an unscrupulous newspaper. It is all a great pity, because a genuine talent for writing occasionally shines through the fog. There is a good tale to be told about the origin of ecological genetics, and about the extraordinary people involved in it. Kettlewell's life was a chequerboard of triumph and tragedy, and a more sympathetic writer could have made the story into an epic. As it is, Hooper's need to favour a particular viewpoint has got in the way, and history has been rewritten to accord with the prejudices of the author.

If you want to know about industrial melanism, you should read the book by Michael Majerus. If you want to know about the people concerned, you should wait, and hope, for an account that is balanced.

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