Of Flies and Fly Culture

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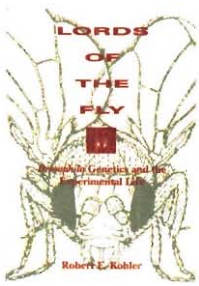

Lords of the Fly
by Robert Kohler
University of Chicago Press,
Chicago & London
1994, pp.321

I first heard about the book 'Lords of the Fly' a few years ago, when a colleague and fellow drosophilist recommended it to me. In particular, I remember him recounting a hilarious incident, described in the book, of how Calvin Bridges, duped by a female confidence trickster into believing that she was an Indian maharaja's daughter, almost left T H Morgan's fly group believing that his friend's father, the maharajah, would set up a fly lab for him in India. It was, thus, with some interest that I started on the book, expecting some kind of 'inside' history of Morgan's fly lab, the scene of so many path breaking discoveries in genetics during the first few decades of the twentieth century. Being both a student of genetics, and a practising drosophilist, the prospect of learning more about the fly people who largely shaped genetics during its youthful years after the rediscovery of Mendel's laws was an exciting one, and I started reading the book practically as soon as I got my hands on it. After about an hour of reading, I realized that I was getting more than I had expected from the book. It was not just about the fly people – Morgan, Muller, Sturtevant, Bridges, Schultz, Dobzhansky, and others – but about the nature of experimental science, and the sociology of a *Drosophila* lab and, more broadly, the fly community. In addition to bringing alive the personalities who worked in Morgan's fly lab, the book also tried to trace the reasons for the development of what one might call fly culture; not just in bottles and vials, but as a set of principles and attitudes towards doing science that came to characterize the community of *Drosophila* researchers.

Kohler's book is, in fact, one that can be read at many levels, and by many people. Students for whom genetic mapping and sex determining mechanisms have perhaps been relegated to a couple of introductory background chapters in current genetics textbooks devoted largely to DNA, may find it interesting to read the details of how some of these major early advances and discoveries in genetics came about. The book provides insights into the early history of genetics, as many of the important advances in our understanding of heredity and variation came directly or indirectly from the fly group, or one of its offshoots. For example, we typically learn about inversions in genetics courses through cytogenetics, whereas inversions were first inferred from odd patterns of data obtained from crosses done for mapping purposes, and cytologically verified much later when the technique of examining salivary

gland polytene chromosomes was developed. For drosophilists, of course, the book is also a treasure trove of information of how their craft was refined, as it was in Morgan's fly lab at Columbia that the first *Drosophila* culture medium was developed, as were the first incubators and the first equipment for anesthetizing flies. It was also in the fly room that half-pint milk bottles and eight dram vials became the standard culture vessels for *Drosophila* rearing. For historians and sociologists interested in science as a sub-culture, too, the book should prove interesting. Indeed, in a strict sense, it is from this point of view of science as a sub-culture that Kohler has written the book.

The major thesis that Kohler tries to advance in this book is that when a community is formed around the production of a particular product – be it art, grain, machine or biological model system like *Drosophila* – then the development and evolution of the rules, norms and conventions governing that community are greatly affected by the nature of the product itself. This notion of the 'moral economy' has been applied to studies of societies, and Kohler argues that it applies to specific scientific endeavours as well. In fact, all the delightful historical details about life, friendship and rivalries in the fly lab are essentially being used by Kohler to make the case that the moral economy of the fly group (the unique fly culture), as well as the choice of research problems tackled by the group, were strongly affected by the choice of *Drosophila melanogaster* as a model organism. It is an interesting proposition. However, although I too believe that choice of organism does have an effect on the choice of research problems, as well as on the working atmosphere, I felt that at times Kohler seemed to be overstating his case.

Kohler starts off by considering the questions of why *Drosophila*, and why *Drosophila melanogaster*? After all, Morgan was a student of the embryology of marine invertebrates. However, he was also interested in the role that heredity played in evolution, and was skeptical about both Mendelism and Darwinism, being somewhat more inclined towards the mutational theory of evolution propagated by Hugo de Vries. Moreover, Morgan was a staunch believer in experimental biology and, consequently, wanted to do large scale multigenerational evolutionary experiments in an attempt to empirically evaluate the role of mutation in evolutionary change. It was for this work that he started using *Drosophila melanogaster*, a widespread human commensal, that was perhaps the easiest fruit fly species to capture and breed in large numbers. In the course of these large scale experiments, due to the large numbers of flies being used, many mutations began to crop up. Crosses between mutant and non-mutant flies revealed some unexpected patterns of phenotypes in the offspring from reciprocal crosses and, thus, sex-linked inheritance was discovered. The realization that a gene could be localized to a specific chromosome – in this case the X chromosome – gave some support to the hypothesis that

genes were located on chromosomes. Studies on crosses of individuals carrying more than one mutation showed that the law of independent assortment did not always work, leading to the realization that genetic recombination between different genes occurred to different degrees, and sometimes not at all. From this observation, as Sturtevant recalled in an interview with G E Allen, came the realization that "*the variations in the strength of linkage already attributed by Morgan to difference in the spatial separation of the gene offered the possibility of determining sequence in the linear dimension of a chromosome.*" Sturtevant further recalled that he "*went home and spent most of the night (to the neglect of my undergraduate homework) in producing the first chromosome map.*"

Once the mapping of genes on chromosomes began in earnest, genetics was forever altered and so was the *Drosophila* system. Sturtevant and Bridges, both precocious youngsters, changed the direction of research in the fly lab to a wholesale mapping project. To this end, they essentially 'constructed' the standard fly: a creature homogenized by careful breeding into an optimal tool for mapping. Needless to say, this optimal standardized fly bore little resemblance to its wild counterparts in terms of its genetic architecture and, thus, was soon no longer useful for evolutionary experimentation but only for mapping. The fact that two youngsters – Sturtevant was nineteen when he made the first genetic map – had the freedom to alter the direction of a research programme, and the contours of the model system used, brings us to the other issue, that of fly culture. Morgan's lab, unusually for that time even in the United States, was based on principles of merit, not seniority. Every one worked together in a rather cramped space, and proprietary rights over ideas were not recognized. All ideas were discussed by the whole group, and subjected to a kind of natural selection through critical scrutiny. Whoever ended up doing the experiments would generally get the privilege of authorship. This atmosphere did lead to some problems because a few people felt they were not getting due credit for their ideas but, on the whole, it worked well. As fly group members set off to establish their own laboratories elsewhere, a network of fly workers came into being, and the free and open exchange of materials, ideas and techniques continued among fly groups.

This cooperative spirit of open exchange was characteristic of the fly culture, and to a great extent still is. No other group of biologists has had so much free flow of material and ideas among its members as the fly workers. This atmosphere – which is very different from that prevailing in many fields of biology – is similar to the spirit in which Ghalib wrote

Surma-e-muft-e-nazar hoon meri qeemat yeh hai
Ki rahey chashm-e-khareedar pey ehsaan mera

(I am kaajal given free, the only recompense I claim:
the favour done be kept in mind by every eye that I adorn)

Kohler's argument is that the very nature of the mapping project was such that the traditional approach of dividing different lines of work among different students in a lab would just not work. No one person could do the mapping project alone, and every addition of a gene to the growing maps had repercussions for everybody's ongoing work. Similarly, the maintenance of the vast numbers of distinct populations of flies itself was a task that required the efforts of many people and, conversely, without those populations, nobody would be able to work on mapping.

Kohler also deals with two major extensions to this early work on genetics which was focussed mainly on mapping and chromosomal mechanics. One line of work, begun by Beadle and Sturtevant in the fly lab, and later brought to fruition by Beadle and others using a different system – the fungus *Neurospora* – was to integrate genes with functional biology. This line of work led to the understanding that genes gave rise to enzymes which then catalyzed specific steps in metabolic pathways, and led to the development of experimental biochemical genetics. The other major line of work was to integrate genetics with evolution. This was begun by Dobzhansky and Sturtevant, and eventually its focus shifted from the standard *Drosophila melanogaster* to wild populations of *Drosophila pseudoobscura*, as the homogenized lab fly was no good for understanding the role of heredity and variation in evolutionary change.

Overall, this is a book I would strongly recommend to both students of biology and to those interested in the history of science and ideas in general. For practising scientists, too, there is much to be learnt from the story of the fly lab and Morgan's boys. Very few people would give undergraduate students the kind of academic freedom that Morgan gave to Sturtevant and Bridges, yet the returns would seem to vindicate his approach. Also today, in an era in which the goal of much research seems to be obtaining a patent rather than knowledge, it is heartening to read of a time and group in which cooperation not competition, and sharing not secrecy, were the order of the day. Genetics benefited greatly from the fly culture and today, perhaps, we need to rethink our views about the fundamental nature of the scientific endeavour. Is it a cooperative and cumulative acquiring of knowledge to be made freely available to all, or is it a competitive and secretive search for information that could yield great monetary returns to a few?

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