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Editorial

EXIT LINES

I am handing over my position as editor of *Genetical Research* to Professor W. G. (Bill) Hill from the beginning of 1996, and have been asked to write something about the journal's history from 1960 to 1995. Geneticists tend to be unimpressed by studies of the history of science unless Darwin is involved; but Robert Kohler's book 'Lords of the Fly' (University of Chicago Press, 1994), on the intimate history of the famous Columbia Fly Room, shows what can be done with the help of private correspondence, though the book would have been better without the sociological jargon. Reading that fascinating study may make one think of changing the refrain. 'Lock up your daughters' to 'Lock up your Lab notes and letters'. Nevertheless I have kept all the *Genetical Research* records, apart from a few lost when the office moved; so perhaps some Post-modern Sociological Historian will get busy on those 36 years of correspondence (Robert Kohler refers in his Preface to 'the volatile world of post-modern academic disciplines, especially in the social sciences').

Meanwhile, I offer a few memories without guaranteeing their absolute accuracy. A journal editor can influence events, intentionally or not. R. A. Fisher's important paper 'The correlation between relatives on the supposition of Mendelian inheritance' was published in the *Transactions of the Royal Society of Edinburgh* (1918, vol. 52: 339–433), I believe because it was rejected by Karl Pearson when submitted to the Royal Society of London. This not only chilled relations between Fisher and Pearson but also between Sewall Wright and Fisher because Wright apparently ignored Fisher's paper (actually he never saw it, as he told me). Fisher once said to me that Karl Pearson used mathematics like a sledgehammer, and I wish I could quote his view of Sewall Wright's Path Coefficients – I never dared to ask him.

Returning to my main theme, a large group of geneticists and animal breeding students gathered in Edinburgh in the late 1940s and started writing papers. The *Journal of Genetics* was the world's first genetical journal, founded by William Bateson and R. C. Punnett in 1910, owned by Bateson then passed on (sold?) to Punnett in 1926, who edited it until he retired in 1946. He then offered it for sale at a Genetical Society meeting, where Fisher failed to buy it (because he had been cool to Punnett?), and J. B. S. Haldane bought it as a wedding present for his new wife, Helen Spurway. so Fisher and Darlington started *Heredity* in 1947, possibly with the aim of sinking the *Journal of Genetics*. Thus the Edinburgh geneticists had two British genetical journals to publish in. We had problems, however, in that (1) *Heredity* began rejecting the papers we sent to it and (2) we sent our papers to Haldane, but a main topic of conversation at annual dinners of the Genetical Society became who had had his manuscript waiting for publication with Haldane the longest. Two years was often quoted. In this position like many others, I was told how to solve the problem: call on Haldane and enquire politely about your MS. Sitting at his desk, he will work his way down though a desk-high pile of manuscripts beside him, find yours near the bottom, look quickly through it, say Harrumph and accept it. This method worked perfectly for me (although my paper contained an elaborate path coefficient diagram); but later Haldane took on John Maynard Smith as sub-editor, which led to rapid processing of manuscripts.

This happy system collapsed in 1957 when the Haldanes took the *Journal of Genetics* with them permanently to India. So, after sending one or two papers to *Zeitschrift für Inductive Abstammungs- und Vererbungslehre* (*ZIAV* in short), we persuaded C.U.P. to start a new journal for us, which became *Genetical Research* {GR}. Forbes Robertson and I were sorting *Drosophila* in our shared room when several colleagues burst in to say joyfully that we now had a new journal – but we hadn't got an editor and 'whatever shall we do'. This lament went on until our etherized flies were waking up, so I said 'For Heaven's sake let us sort in peace. If you go away, I'll edit the journal if you can't find anyone better'. They went out saying 'Hooray', and I now think it was a clever plot to entrap me, but that's how I became editor of GR in 1960.

Genetical Research is owned by the Cambridge University Press but was at first printed by Spottiswoode, Ballantyne & Co in Colchester, because the Cambridge presses were too busy printing Bibles. Editing turned out to be quite easy, since the geneticist's technical tricks were fewer, simpler and easier to grasp than they are now. Gene and symbol naming ranged from the logical to the impossible, of course. The best system was that of *E. coli* and *Salmonella typhimurium*, but I was caught out when I insisted that George Dawson and his group at Dublin should stick to the classical symbol *try*, shortly after Yanofsky had officially renamed it *trp*. Two students of *Physarum polycephalum* had moved from Jennifer Dee's group to other locations, and both sent me papers at the same time on this species, using different symbols for the same gene mutations, but we managed to get them to agree. As gene symbols multiply, short of having a full-time symbol expert in the team, we rely on our referees to point out symbol misnaming.

Split infinitives, finally accepted in spite of Bernard Shaw, and American spelling, which we and the Press came to accept, meant discarding my prejudices. We undoubtedly helped many authors to convert their Ph.D. theses into proper papers by drastic pruning, and persuaded others that the reader was not sitting in their head as they wrote but had to be given all the relevant information. A useful editorial function was to spot the occasional manuscript which had been published elsewhere under a different title, and manuscripts which had been split up into several little bits offered to different journals, a trick we are strongly prejudiced against. And one author sent me a 'fully revised' manuscript identical with the original, on which we had asked for a number of changes. Mathematically minded authors occasionally send us equations with subscripts attached to subscripts, needing a microscope to read, or novel symbols not in the Printer's vocabulary; and they sometimes supply proof corrections which neither I nor even the printer can interpret. I also learnt that a few presumptive referees never reply to letters, and others advise rejection of every manuscript you send them, while two referees may disagree entirely in their assessment of a paper.

From the start, GR attracted plenty of good papers on a variety of organisms, including the mouse (18 papers), *Drosophila* (8) and fungal genetics (5) of the 39 published in the first year. Three numbers per volume have appeared consistently, so the 66 volumes printed to date must include nearly 2000 papers. Alan Robertson and Mary Lyon, as both contributors and long running members of our Editorial Board, had a major influence in maintaining the high quality of many of the papers we published. Alan Robertson was an excellent referee of mathematical papers, including those of Motoo Kimura, which brought in many papers from authors who appreciated his criticisms, and in one case robbed the journal of some kudos it should have had. On 28th July 1967, Kimura sent us a paper entitled 'Genetic variability maintained in a finite population due to mutational production of neutral and nearly neutral isoalleles', which Alan Robertson reviewed. This led to some revision and it was not published until June 1968 (GR Vol. 11 No. 3). It is a substantial and important paper of 23 pages. Five months later (18th December 1967), Kimura sent a very short paper to *Nature* (about 2 pages) entitled 'Evolutionary Rate at the Molecular Level', which was published in the February 1968 Number. This latter paper is routinely quoted as Kimura's first publication on his neutral theory of molecular evolution while the GR paper surely deserves that name; and of course, had we fully appreciated its significance we would have put it into our December 1967 number! Alan Robertson's correspondence with Kimura was unfortunately lost after Alan's death. Arguments about priority usually arise over papers from different authors, not over papers from a single author in two different journals.

Mary Lyon kept us well supplied with mouse papers, including a number on the 'little *t*' mystery, and mouse genetics has continued to have an important place in the journal, including a Festshrift double number for Mary Lyon's official but not unofficial retirement in 1990. I was delighted that we managed to capture this for GR. The most famous paper we have published, as judged by the fact that it has been frequently quoted and argued about ever since, is 'A mechanism for gene conversion in fungi' by Robin Holliday, GR Vol. 5, 282–304, 1964. Very few papers have had such a long productive life.

Further influences on GR were that ZIAV was converted into *Molecular and General Genetics* in 1967 (a title I considered ridiculous at the time but now wish I had thought of first). This journal, in spite of its very high price and small sales, became the fashionable journal for molecular genetics, so we lost some papers in this up and coming area. In 1969 C. D. Darlington, then owner of *Heredity*, offered it to the Genetical Society, who naturally set about building it up as the main British genetical journal, expecting that it would take all the best papers from GR. However, GR survived the new competition in good health, and it is interesting that now at least three genetical journals are edited in Scotland – GR at ICAPB and *Genetics and Development* at the MRC Unit of Human Genetics, both in Edinburgh, and *Animal Genetics* at the Roslin Institute near Edinburgh. *Molecular Microbiology* made a fourth when it was started, being first edited in Dundee. In the last 25 years new genetically oriented journals have been multiplying, stimulated I suspect by publication of the annual accounts of *Heredity* in the Genetical Society Newsletter, which showed publishers that journals were a good business proposition.

There are other experiences I could discuss, such as I as editor of GR being referred to the British Press Council, a body of Newspaper Magnates set up to chide news photographers who exceed all bounds; or our starting a book review section in 1982 which has brought in 700 books of which we have published reviews on about 350, my contribution being 70. It is noteworthy that over the years, the number of excellent books published has steadily increased, a major source of these being the Cold Spring Harbor Laboratory press.

While I managed to edit GR without help for some years, and continue my research, expert assistance

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first by Neil Willetts and then by David Finnegan and Trudy Mackay as executive editors, have been of enormous benefit to the journal more recently.

Genetical knowledge is now expanding particularly into databases, with help from the so-called Superhighway and it won't surprise me if we soon see not only new journals but also rejected manuscripts appear on World Wide Web. An electronic journal for rejected papers would be an interesting novelty, especially if labelled 'The Alternative Genetical Research Journal'.

Looking back over 36 years, I can only remember having very good relations with Cambridge University Press, most of our authors, and the referees who replied to our requests for an opinion. I don't propose to retire gracefully, as I find myself editing an Encyclopedia of Genetics – a more difficult task than editing a journal. Meanwhile, I am very pleased to be able to hand over to Bill Hill a journal of high quality which is in good health and should outlast the century.

Eric Reeve