

Darwin and Mendel versus Watson and Crick

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IN THE JANUARY 1998 ISSUE of *The FASEB Journal*, Professor Biagioli argues that authorship of a scientific paper is an arbitrary category, one determined by cultural events outside the practice and purview of scientists themselves. This is a new idea to me, one that offers scientists remarkable freedom if they would grasp it. If, as Professor Biagioli argues, authorship—ownership of the discovery of an aspect of natural mechanism, or law—is not an aspect of property or of perks, but of responsibility for the lasting veracity of that discovery, then the discoverers themselves seem perfectly placed to reconfigure the business end of science so that authorship takes on a clearer meaning, one more commensurate with the actual practices of scientists than the current, property-driven situation. All the more ironic that Professor Biagioli himself—not a lab scientist, but a scholar of the history of science—seems too modest to make his own concrete suggestions for changes that might make authorship scientifically authentic. Perhaps I can help.

I accept entirely his argument that the perk of courtesy authorship is an embarrassment to the business of scientific discovery; worse, it can all too easily shade from an expectation that one will show courtesy to one's mentor into a genteel sort of blackmail, especially when the mentor's grants are not being renewed so easily as in the past. On the other hand, the argument that discovery is purely quantal, with only one discoverer per facet of natural mechanism, seems to me as artificial as any of today's systems for allocating credit. Surely Mendel is a discoverer in his sense and so is Darwin, and their single-author publications neatly fit his model. But what of Watson and Crick?

Their two notes to *Nature*—"A Structure for Deoxyribose Nucleic Acid," *Nature* April 25, 1953, pp. 737–738, and "Genetical Implications of the Structure of Deoxyribonucleic Acid," *Nature* May 30, 1953, pp. 964–967—must be on everyone's list of major revelations of natural mechanism by the analysis of data. In these two papers, they collaboratively described not just DNA's structure as an anti-parallel double helix held together by hydrogen-bonded base

pairs, but also the genetic implications of the structure—its capacity to both store and replicate the information held by its sequence of base pairs. Which of the treasures in these two notes is Watson's atom of discovery and which is Crick's? A silly question: the discovery has linked them for eternity, conferring on them a sort of joint immortality that neither would have had alone and thereby offering prima facie disproof of Professor Biagioli's theory of atomistic discovery.

Can we generalize from this disproof? Ought we not to recognize the natural utility of very small collaborations and reward joint work more clearly than we now do, rather than lumping it in with the pathological proliferation of authorship into the dozens? I had one data set at hand to test this notion after reading Professor Biagioli's article: my own curriculum vitae. I found a smooth distribution of multiple authorship, centering on two or three authors, with almost two-thirds of my papers having three authors or fewer (**Fig. 1**). As for the papers with more authors, Professor Biagioli is correct. In my own case, four or five was the maximum number of authors for any paper with really new stuff in it. The larger authorships—mine go up to

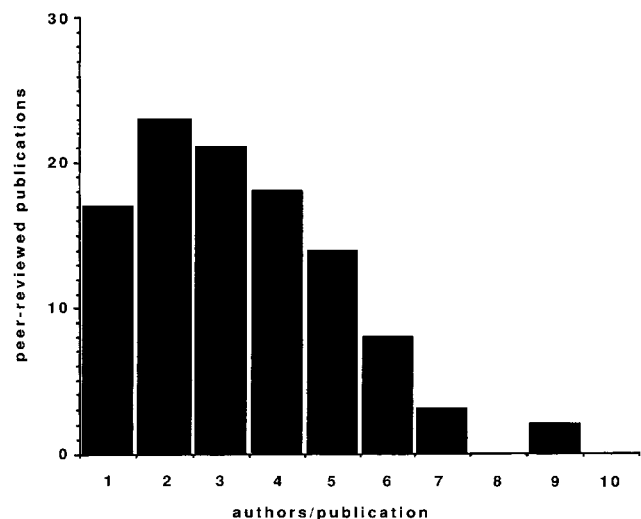


Figure 1. Number of authors, including me, on each of my peer-reviewed publications, 1966–1996.

¹ Dr. Pollack is a new member of the Journal's Editorial Board.

nine 'authors'—were on 'filler' papers: reviews and proceedings, and the like.

I then asked myself: If I had to give up all papers except those with a particular number of authors, which number would I pick? The answer was immediate. Seen in retrospect, my best papers were the ones with one or two other authors; of those, the most interesting (to me) happen to have three authors. Contra Professor Biagioli, geography and social class seem to have made no difference: from the distance of a decade or two, the papers I wrote alone are simply not as important as the ones I wrote with one or two others, whether my coauthors were col-

leagues at other institutions and peers in terms of age and accomplishments or my own students or post-docs at the time.

I would like to know from our readers whether my judgment of my own work extends to their judgment of their work as well. Normalized for your paper of peak importance, how does retrospective significance correlate with the variable of multiple authorship in your case? If we find a general self-awareness among productive scientists that their best work is collaborative, would that not be a good place to start when reconfiguring the reward system for grants and academic appointments?