

ERL and the impact of small groups of authors

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EDITORIAL

ERL and the impact of small groups of authors

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Abstract

Evidence from the 100-most cited papers ever published in ERL indicates the disproportionately large scientific impact of small groups of authors. The median number of authors on these 100 most-cited papers was 3.5, and 72 out of the 100 most cited papers had 5 or fewer authors. This indicates that small groups of authors often produce the work with the greatest impact, even in an inter-disciplinary setting. This suggests that it may be wise to institute policy changes that discourage inflation of author lists and that encourage the funding of research conducted by single investigators and small groups of researchers.

As *Environmental Research Letters* (ERL) reaches its 10th anniversary, it is worthwhile to take a look back at characteristics of the papers that have had the most impact.

Specifically, I take this opportunity to examine the number of authors of the most impactful papers in this leading multi-disciplinary journal. We all know that citation counts are a poor substitute for a qualitative evaluation of impact, but citation counts are nonetheless the readily available measure of impact that I rely on here.

Author lists have been growing over time. Christopher King, writing on a blog site 'Science Watch' in 2013 (<http://sciencewatch.com/articles/single-author-papers-waning-share-output-still-providing-tools-progress>), documented an approximate doubling in the average number of authors per paper as listed in the Thomson Reuters Web of Science, from less than 2.5 authors per paper in 1981 to greater than 5 authors per paper in 2012. Over these same three decades, single author papers declined from approximately 1 out of 3 papers published to 1 out of 9 papers published. This certainly accords with my own experience. People who would have been satisfied with a mention in the acknowledgments section some decades ago, now expect to be included on the author list, even if their contributions were extremely minor. Since there is little downside in adding co-authors to papers, lead authors avoid the risk of offending minor contributors.

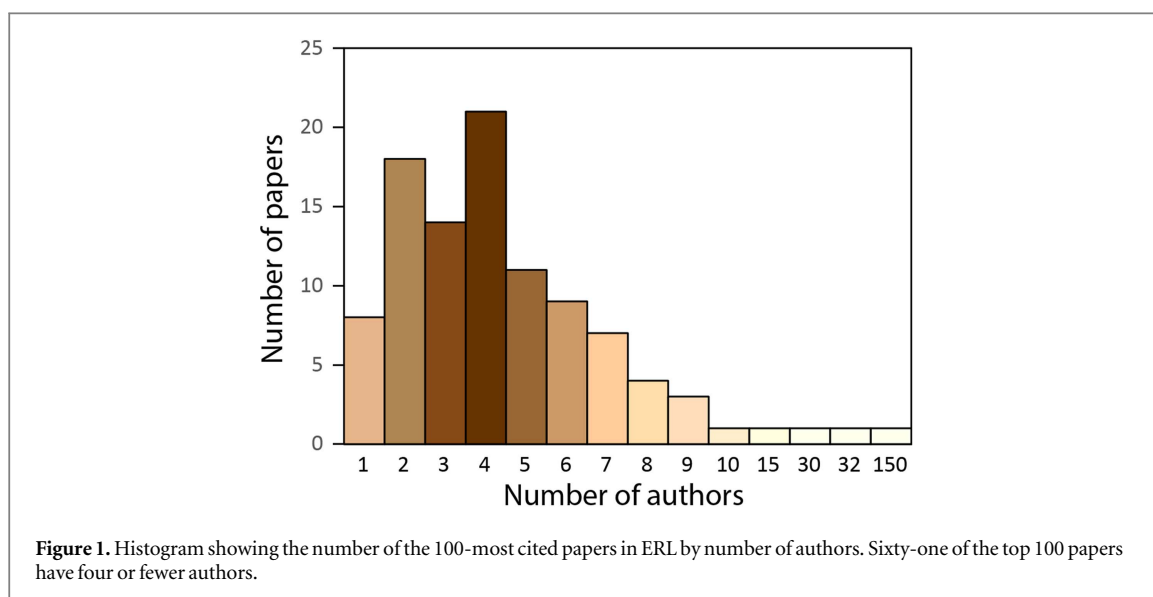
Funding agencies, such as the US National Science Foundation, have been encouraging relatively large multi-institutional inter-disciplinary grant proposals. Increasingly, successful proposals have been involving

several institutions (<https://nsf.gov/statistics/infbrief/nsf12325/>). Published articles increasingly involve authors from multiple institutions (<https://nsf.gov/statistics/seind14/index.cfm/chapter-5/c5s4.htm>).

Given both of these trends, it is instructive to look at the distribution of number of authors on the 100 most-cited papers published in the history of ERL (figure 1). The median number of authors on the 100 most-cited papers is 3.5, and 72 out of the 100 papers had 5 or fewer authors. I take this as evidence that small author teams can be particularly effective at producing high impact papers.

Why might large author lists be a concern?

- (1) Typically, with large author lists, some authors do an order-of-magnitude more work than other authors, but the co-authors share approximately equally in credit.
- (2) Sometimes, authors are added for purely political purposes, where someone feels that adding 'big names' to the author list would increase the likelihood of passing through the review process, or would in some way increase the amount of attention paid to a paper.
- (3) If a paper with 10 authors were to be cited 1000 times, it would increase the H-index of 10 people; where as if that same work were written by one author, it would only increase the H-index of 1 person. Thus, the 10-author paper affects H-indices 10 times as much as a single author paper, even if they were to have the same number of citations.



- (4) If a paper has 100 authors, each of these 100 authors are motivated to cite the work, and this large number of self-citations increases the likelihood that others will cite the work. Thus, for two papers with the same number of citations, it is likely that papers with fewer co-authors is of greater quality than the paper with more authors.
- (5) Being a co-author on a paper is supposed to mean that you are able to defend every statement made in a work, but especially in papers with large multi-disciplinary teams, authors simply accept the judgement of co-authors and cannot independently attest to the veracity of every statement in the published work. Thus, such works undermine the traditional values associated with co-authorship of a scientific paper.

I speak from experience. I have been on papers where I, as co-author, did 100 times more work than other co-authors. I have seen cases where people demanded co-authorship simply for providing already published data. I have been approached to be co-author on papers for purely political purposes without being asked to make any real contributions, and have been disturbed to see that some of my colleagues have accepted such ‘no-work’ co-authorships. Some of my most highly cited papers that contribute to my H-index are large multi-author works involving just a few days of my work. I have been on papers where I was asked to contribute some modeling work, but was not in a position to evaluate claims made in other parts of the paper, yet I accepted co-authorship as a form of non-monetary payment for the modeling work I contributed.

Further, it seems that increasingly grant money is going to large multi-institutional multi-disciplinary consortia, at the expense of single-investigator and other small-group investigations. Such funding

models favor scientists who spend a lot of time networking and engaged in institutional political games, and disfavor the scientist who spends a lot of time in the office, lab, or field site engaged in primary research, largely oblivious to current scientific fashion among the funding agencies. Sometimes, in the funding game, the politically savvy triumphs over the highest quality science.

So, what are possible solutions? Regarding publications, I see two reforms that would be useful.

One reform would involve given the option to author teams of attributing a percentage contribution to all co-authors of a paper, where these contributions sum to 100%. There could be a default attribution available to author groups who find it too socially-charged to partition the credit sensibly. For example, in large multi-author works, by default, the first author could get one-third of the credit, with the remaining credit distributed equally across co-authors. A variation on the H-index could add 0.1 instead of 1 to your H-index if you were attributed only 10% of the credit for producing the paper. Instituting a policy like this would introduce a cost to lead authors of adding co-authors who contributed little substantial to a work, and would give a fair way of giving some small amount of credit to co-authors who made a real but minor contribution. H-indices could also be improved by removing self-citations by co-authors from the citation count.

Another possible reform would build on the example of the Intergovernmental Panel on Climate Change, which distinguishes between Coordinating Lead Authors and Lead Authors. In my experience, it is rare that more than three authors do approximately equal amounts of work on a single study. This reform would limit Coordinating Authors to some small number (say, three or five). If the number of authors extended beyond this, they would need to be listed as co-authors who are not in a coordinating role.

Coordinating Authors, as now, would need to be able to attest to the veracity every statement in a published work, but co-authors would need attest to the veracity of only the part of the full work to which they directly contributed.

Regarding funding, I feel the pendulum has swung too far towards large multi-institution multi-disciplinary teams. Often, such teams involve substantial bureaucratic overhead that detract from the process of doing science. There needs to be a recognition that single authors can produce multi-disciplinary studies, and that there is much of value in disciplinary work. Large coordinated science projects have proven extremely valuable, but there needs to be greater appreciation of the value of single-PI and small group research.

I am co-author on two of the top-100 most-cited papers in the history of ERL. One of these was a large-group paper with 15 authors (Jackson *et al* 2008) and the other has two only authors (Myhrvold and Caldeira 2012). So, I clearly see the value of both large-group and small-group science.

I have now co-authored 19 papers published in *Environmental Research Letters* (not including this!).

I have published more papers in ERL than in any other journal. I like to publish in *Environmental Research Letters* for several reasons: (1) I find that articles published in *Environmental Research Letters* reach a broad audience, and that *Environmental Research Letters* will often help publicize papers, either by coordinating release with our institutional press release or through environmentalresearchweb.org. (2) I believe that open-access is the future of scientific publishing and that cost of disseminating results must be considered part of the cost of doing research. (3) *Environmental Research Letters*, through IOP Publishing, is a project of the Institute of Physics, a non-profit society, so any revenue surplus will go to support science, and not a set of profit-maximizing investors. (4) I find the review process to be highly professional, fair and efficient.

Reference

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