

because one's work is more readily recalled when multiple coauthors also cite its significance. Multiple-author publications present not only the new idea but beefier sections on its consequences. Citing new single-author concepts is harder than citing new data or computations because we are often unsure later where new ideas came from.

A paper with coauthors is probably more likely to be recognized as seminal than one with a single author, and I expect that a citation index will show more citations to multiple-author papers. As an example, one of my publications, with two well-known coauthors, was chosen by the editors of the *Astrophysical Journal* as one of the 50 most influential papers of the 20th century.¹ I have long puzzled that my best-known and most-cited papers are those having multiple coauthors. Perhaps having multiple coauthors increases one's scientific visibility because coauthors citing its significance become vectors for that work. Mathematics of social networks (see the article by Adilson Motter and Réka Albert, *PHYSICS TODAY*, April 2012, page 43) probably addresses that idea.

Reference

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■ **Implied** in Philip Wyatt's commentary is the idea that as measured by the number of authors on a paper, creativity and scientific knowledge have decreased over time. Not only does that simple treatment miss underlying issues related to government funding and the job market, but it also bypasses the key question of how to measure success.

Companies, funding agencies, and universities all need some method of characterizing and judging the scientific vision of researchers to determine those most likely to succeed at a given task. The initial filtering is typically done without ever meeting the scientist face to face; mustering committees to interview every candidate or grant applicant is not worth the headaches and expenses. With increased competition and a fast-paced environment, people responsible for hiring and funding must therefore find other indicators of

the quality of a candidate's work. Unlike prospective undergraduate and graduate students, scientists cannot take a standardized test on their specialized knowledge. Thus the number of papers has emerged as a chosen indicator; who, then, can blame researchers for pushing for recognition on even small parts of a published result?

The connection between an increase in the number of authors per paper and any supposed decrease in the creativity or knowledge of researchers seems tenuous at best. There might be more noise, but the signal is probably the same; the problem boils down to one of measuring originality.

Reversing the trend that Wyatt observes is equivalent to finding ways of judging scientists on the merits of their scientific acumen. For example, there have been recent attempts at improving the use of an impact factor¹ through the g-index,² the h-index,³ and others that aim to derive some sense of the importance of a particular researcher. Being more nuanced, those types of methods probably have a better chance of correlating with a researcher's ability than a straight article count. All scientists should participate in this discussion in as many ways as possible, such as through these pages (see letters in *PHYSICS TODAY*, November 2010, page 12, and March 2011, page 9).

If we as scientists can change the rubric used to judge success by rewarding outstanding papers and articles rather than their numbers, the pressure to publish would decrease, and perhaps in a few decades we would see a reversal in the number of authors per paper.

References

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2. L. Egghe, *Scientometrics* **69**, 131 (2006).
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■ **Praise to** Philip Wyatt for having the courage to say what needed to be said, and praise to *PHYSICS TODAY* for publishing it. Wyatt has hit the nail squarely on the head.

The emphasis on multiple authors is ultimately driven, as Wyatt implies, by the ongoing obsession with citation count as *the* marker of achievement. Never mind that prominent and less-prominent journals have openly and repeatedly recognized that citation count has little correlation to quality or relevance of work. High citation counts have even gone to papers later identi-

fied as falsified. But I think Wyatt's ultimate point is that with so many authors, it is nearly impossible to identify who was truly the brains behind a paper. In hiring and promotion, for example, that identification is paramount. But how, given present circumstances, can such decisions be made?

Single-author papers would seem to be the sure indicator of creativity, but given today's pressure to include other authors, the lone author becomes a pariah. Other factors have contributed to the relative rarity of single author papers. One is the increasing percentage of female scientists and engineers. In my view, women are cooperators; those willing to write as sole author are very few and far between. Another reason is the amount of time involved in writing a good single-author paper. The majority of them require a perspective acquired from deep, longer-term immersion in a subject.

Profound experimental results often emanate from multiauthor groups, but profound theory rarely does.

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■ **Unfortunately** for Philip Wyatt, sarcastic irony only works when the proffered irony is valid. I refer to his harangue that "I have found the presence of the basic building blocks of the science decreasing with each passing year. When a recent PhD in a physical science said that helium formed diatomic molecules, I knew we were in trouble!"

Wyatt might wish to consult the 1996 paper by Wieland Schöllkopf and Peter Toennies.¹ That would be, let's see, 16 years ago now. Schöllkopf and Toennies diffracted helium atoms and dimers from a manmade transmission diffraction grating to show, beyond the shadow of a doubt, that the neutral helium dimer exists as a stable diatomic species (albeit extremely weakly bound). Given the highly quantum mechanical nature of this extraordinary dimer and the fact that it has perhaps the most weakly bound ground state of any dimer, it is of considerable fundamental interest.

In fact, the interaction between two neutral helium atoms has been a test bed for quantum mechanical calculations dating back to John Slater's pioneering work² in the 1920s, and the helium dimer itself has been the subject of numerous experiments since the beautiful Göttingen measurements of Schöllkopf and Toennies. Clearly, the younger generation has no monopoly on the lack of "basic building blocks."

Contrary to the trend toward many authors on a paper, as also bemoaned by Wyatt, I point out that there were only two authors on that groundbreaking 1996 Göttingen paper.

References

1. W. Schöllkopf, P. Toennies, *J. Chem. Phys.* **104**, 1155 (1996).
2. J. Slater, *Phys. Rev.* **32**, 349 (1928).

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■ **Philip Wyatt's** commentary was an enjoyable read. The vista he describes is, unfortunately, exactly where the publish-or-perish culture has brought the physics community.

I wholeheartedly agree with Wyatt's opinion on the authoring of papers, but I must object to one specific statement in his commentary: "When a recent PhD in a physical science said that helium formed diatomic molecules, I knew we were in trouble!"

Helium and all other rare gases indeed form diatomic molecules, albeit in excited states commonly referred to as excimers. Radiation (around 60 nm) resulting from decay of He₂ to the dissociative ground state was discovered in 1930 and is referred to as the Hopfield continuum.¹ Then again, maybe the student was indeed referring to ground-state He₂.

Reference

1. J. J. Hopfield, *Astrophys. J.* **72**, 133 (1930).

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■ **Wyatt replies:** Hassel Ledbetter's suggestions and criteria for authorship are excellent and should be read carefully by all pending authors of a scientific paper, especially the principal contributor to the work. Purportedly, Ledbetter is writing a book that addresses those important matters in greater detail.

Mark Brandon's praise of the journals *Nature* and *Science* for attempts to handle scientific coauthorship is noble and reasonable, but it seems to miss the main problem with the current plethora of authors. The basic objective for many papers nowadays is to generate citations—which are important for getting funding and even finding a job. It would be quite surprising to see each article followed by a statement specifying what each listed author actually did. It will never happen. Nevertheless, I have never heard of either of those

journals confirming or rejecting the presence of any listed author.

The belief that scut work—making and confirming measurements; collecting, processing, and reviewing data; or reviewing and correcting the manuscript—represents a "significant intellectual contribution" worthy of coauthorship has never struck me as doing anything to encourage creativity. Apparently, *Science* has now partially resolved the matter for some multiauthor articles. For several such works published each week, *Science* adds an asterisk, usually to two author names, with the statement, "These authors contributed equally to this work." But what about the remaining gaggle of authors? Since their contributions are not even weighted by *Science*, perhaps the "equally contributing" authors should relegate their names to an acknowledgment list at the end of a two-author article!

Brandon also raises a different type of authorship issue—based perhaps on greed, envy, or ego—that might occur with single-author papers such as Robert Millikan's. There can be little doubt that Harvey Fletcher's suggestion of using oil, and later his innovative electrode design, made the measurements much easier or even possible. Yet hadn't Millikan come up with the single-particle idea that started with water and ended up with oil? Nevertheless, I agree with Brandon. An even more egregious example was the case of Selman Waksman and his purported discovery of streptomycin. His graduate student Albert Schatz most certainly should have shared the Nobel Prize. As it was, he had to pursue litigation just to be recognized as a co-discoverer and to receive a share of patent royalties. In Millikan's defense, though, he did get Fletcher a fine position at Bell Labs, where he contributed great physics.

Tomek Kott's assertion that my commentary implies that "as measured by the number of authors on a paper, creativity and scientific knowledge have decreased over time" is not correct. The number of authors per se has nothing to do with the decrease of creativity; rather, among the plethora of authors, probably only a few are the creative entities. Perhaps a better metric would be the creativity per author listed. I don't recall focusing on "success," either. An important observation was that so much effort seems focused on writing papers just to secure funding or a job that many fundamental building blocks needed as tools to spur creativity in our young scientists are lost in the process.

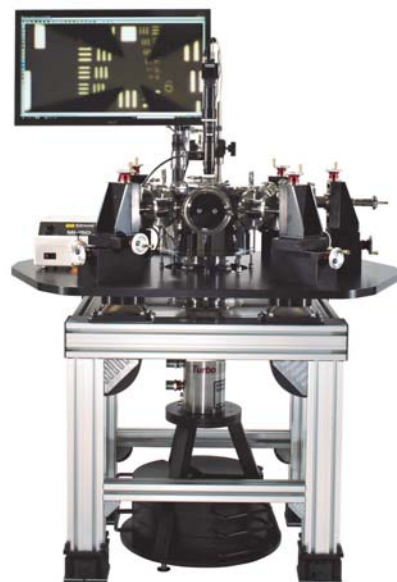
I agree with Kott that multiauthor

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