

1923 Nobel Prize in Physics with Fletcher.

References

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■ **Philip Wyatt's commentary** on authorship was quite interesting. I help to maintain the authorship lists for two large collaborations in particle physics. I have three comments that may be of interest:

► High-energy physics collaborations have authorship rules that may conflict with the guidelines of the journals, and most authors may not be able to defend the conclusions in the paper. *Physical Review Letters* states "Authorship should be limited to those who have made a significant contribution to the concept, design, execution or interpretation of the research study. . . . Other individuals who have contributed to the study should be acknowledged, but not identified as authors" (http://www.aps.org/policy/statements/02_2.cfm).

► Remarkably, I am in a field where people have not, on average, read a majority of their own papers.

► I have no realistic solution.

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■ **The commentary by Philip Wyatt** presented intriguing data confirming the evolution of increasingly multiple authorship. It prompts speculations on the social psychology of science. During five decades as a university professor, I experienced that evolution, both in the authorship of astrophysics and planetary-science papers and in research modes used by our graduate students.

Throughout my career I strove to create new physics ideas, in contrast to new data or calculations. Those single-author publications primarily involved seeking new interpretations of phenomena, and by virtue of being original they did not lend themselves to multiple authorship. My large number of single-author papers seems out of step today.

I observe that my single-author papers are not as heavily cited as my multiple-author works, and that my single-author papers are often uncited even by researchers that use the ideas from them. I suggest that the evolution to multiple authorship occurred in part



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Journal of American Chemical Society, March 2011

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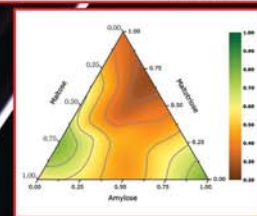
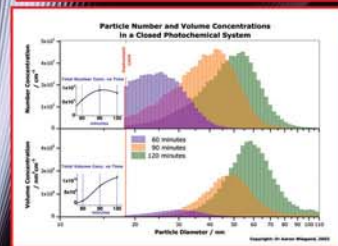
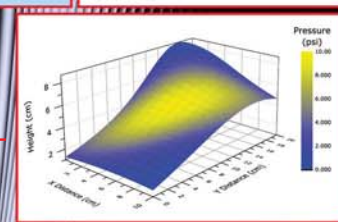
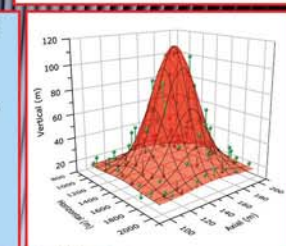
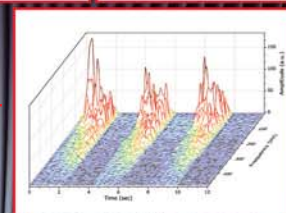
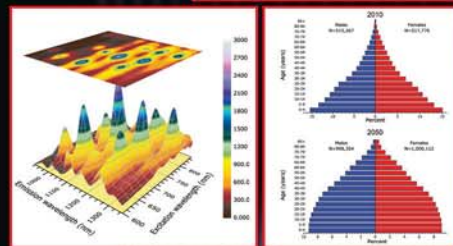
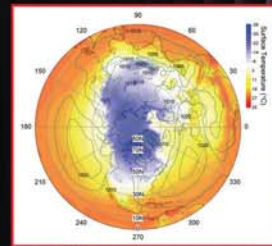
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because one's work is more readily recalled when multiple coauthors also cite its significance. Multiple-author publications present not only the new idea but beefier sections on its consequences. Citing new single-author concepts is harder than citing 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.

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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."