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 He_2 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 He_2.

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

papers are becoming the most important metric by which scientists are judged. Unfortunately, once one makes sense of the h-index, for example, it collapses under the burden of irrelevant authors. It doesn't take a rocket scientist to figure out that multiauthor articles eventually result in higher h-indices.

I regret having carelessly stated the helium question I often ask newly minted PhDs. Both R. Bruce Doak and Willem Wieme pointed out my error. The actual question I ask is about the molar mass at standard temperature and pressure. For my commentary, I should have selected one of my simpler questions: Why do stars have different colors? As with the helium question, I get a 75% failure rate on that one. That new PhD physicists can't answer those and even more basic questions tells me that they, like other graduate students in the sciences and engineering, spend too much of their time generating papers at the expense of learning the foundations of their subject.

My statement that "I have found the presence of the basic building blocks of the science decreasing with each passing year" appears very well corroborated by even a casual reading of our international ranking in scientific competitions—for example, in the 42nd International Physics Olympiad.

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Can a scientist knock on heaven's door?

The book *Knocking on Heaven's Door: How Physics and Scientific Thinking Illuminate the Universe and the Modern World*, by Lisa Randall, was reviewed by Alain Blondel on page 54 of the March 2012 issue of PHYSICS TODAY. The book title is intriguing but certainly not appropriate, coming as it does from a theoretical physicist. Particularly unsettling is that, in Blondel's words, Randall "argues that scientists have not found any problem that demonstrably requires the intervention of a supernatural being animated with a purpose."

I am disturbed at how we scientists can so readily extrapolate our meager knowledge, acquired over a mere few hundred years, into such a broad claim. For example, we have no theory, and most probably never will, on the biggest question: how life originated. To imagine that DNA and the genetic code came about by chance takes a lot of faith. Mathematicians tend to run out of zeroes when they calculate the probabilities. Moreover, other questions such as the chicken-and-egg problem remain extremely intriguing.

Life remains a mystery, as does this universe, but to think that science is getting any closer to resolving the biggest question of all is arrogance.

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Putting the Savannah River Site where it belongs

The authors of "The many uses of electron antineutrinos" (PHYSICS TODAY, March 2012, page 46) have managed to move the Savannah River Site. The site, where Frederick Reines and Clyde Cowan first sighted neutrinos (actually electron antineutrinos) at reactor P, is in Aiken and Barnwell Counties on the South Carolina side of the Savannah River, not in Georgia as stated in the article. In recognition of that important first sighting, the city of Aiken hosted a neutrino celebration in the summer of 2010, complete with the unveiling of a roadside historical marker about the event.

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Corrections

April 2012, page 52—The last sentence of the caption for figure 3 should read, "In particular, the Sun is about 8 kpc from the galactic center."

May 2012, page 42—The last two sentences in box 1 should read, "Atoms became the subjects of chemistry and the basis for statistical models of gases. In the early 20th century, with Jean Perrin's observation of Brownian motion in a colloidal suspension, the concept of the atom returned at long last to the realm of physics."

June 2012, page 14—Although Grigori Volovik and others realized the important connection between zero-energy modes and *p*-wave vortices early on, the first to relate self-adjoint (and thus explicitly Majorana) zero-energy modes to the vortices and to novel (non-abelian) statistics were Nicholas Read and Dmitry Green, in work published in 2000. ■