work," which they obviously believed in at that time, just as strongly as they now propose that all the narrow structures previously observed were statistical fluctuations. The retreat to this explanation appears to me (I have seen the EPOS II data discussed in seminars but not in publication), to have been influenced by premature publication of the APEX results. I have difficulties with this attitude because there exist identical observations in the literature for which the data were taken months apart and for which the analysis was carried out by physicists other than Tom Cowan and Greenberg. These data appear to represent a reproducible result in detail and not a statistical fluctuation. The measurements that are required now seem to be resisted, even though they are of paramount importance. Although I am a theorist, it seems clear to me that the experiments are still incomplete on both sides of the Atlantic, not having really addressed (contrary to some statements) the important thin-target excitation-function studies that may be the key to demonstrating reproducibility. Until these are done, I do not see how the issue of e^+e^- peaks can be resolved.

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Authorship: Truth in Labeling

Research in many fields has become immensely complex. It often requires a combination of knowledge, technique, skills, and inventions sufficiently diverse that only the cooperation of many scientists can result in an important new result and its publication. How then should the authorship of such a paper be described? Does it even matter how the authorship is described?

Reputation is essential to obtaining research support, employment, and promotions, and it determines career trajectories in science. There is an operational importance to authorship, for the largest single determinant of scientific reputation is the papers that bear one's name. The ability to present insightful seminars, nurture young researchers, and informally exchange useful information also affect reputation; but for most researchers, these are distant secondary contributors.

As a faculty member, I often vote for the appointment of new faculty members who have only published multi-author papers. This is perhaps becoming unavoidable in many fields, but leaves me with many questions. Can the candidate conceive a new research project, or generate an insightful idea, or solve unforeseen problems that arise during the course of research? Can the candidate write a well-structured paper? Why should the scientific literature not show the answers?

It might be argued that letters of recommendation fill the void. They do so by default, but badly. When I am reading the literature and thinking about faculty development, I would like to be able to note the originators of particularly important contributions without recourse to a letter to the head of a laboratory. And in my experience, senior scientists, aided by the privacy of a letter of recommendation or a telephone call, are not without duplicity and self-serving descriptions.

Truth in labeling of food, clothing, and drugs is effective and has resulted in better products for the consumer. The equivalent in science publication would result in fairer evaluations for young scientists, would improve their motivation, would result in a fairer funding marketplace, and thus would enhance the attractiveness of science as a career. The AAAS, in promoting science, should above all be concerned

with making a science career attractive, as the future of American science depends entirely on the talent of the individuals brought into science careers. The AAAS, in advocating accuracy in research articles, should start with the question of who was responsible for what in the papers published. I urge *Science* to try a year-long experiment to implement the suggestions made by Benjamin White and Jonathan Knight in their letters of 24 January (p. 461).

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Asbestos Removal

Readers of the Random Samples item "Deasbestosization" (13 Dec., p. 1845) about asbestos removal in Europe might be interested to know the following.

- Hundreds of buildings have already been decontaminated in England, Germany, and Switzerland, for example.
- A thorough investigation was carried out on the buildings of the Jussieu campus of Paris University by the Bureau de Recherches Géologiques et Minières (France), Fibrecount (Belgium), and Eurotec (Germany). They estimated that in most of the buildings risk exposure to asbestos was high, and they recommended a "global, rapid, and massive operation of removal of asbestos in the whole building"; they also proposed a plan of decontamination. The only major problem was the cost of the operation, and this has been settled by the French Ministry of Education and Research.
- The medical health expert quoted in the piece as opposing asbestos removal at the Jussieu campus is Étienne Fournier, a toxicologist well known in France because of his involvement with the asbestos industry.
- The National Institute for Health and Medical Research (INSERM) estimates the incidence of asbestos-related cancer to be more than 2000 workers per year in France, but less than 80 workers (or their families after their death) receive any compensation.

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Whatever the merits of the argument that "health risks from undisturbed asbestos are exaggerated," it is absurdly irrelevant to the situation at Jussieu. Asbestos dust has been filtering through the holes in ceiling panels since the building was completed in the late 1960s. A room-by-room analysis conducted last year found dangerous amounts of asbestos throughout the campus. Specialists who measured campus contamination during maintenance activities actually repeated their measurements, assuming that the high measurements were a result of instrument malfunction. My own office was one of many found to present an urgent health problem, requiring immediate attention. Fournier's report, the scientific source mentioned in your article as opposing the removal, was published well after measurements of contamination at Jussieu were made public last November.

As of this writing, 27 Jussieu employees have been diagnosed with asbestos-related illness, and four have died.

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Recombinant DNA Technique and Sickle Cell Anemia Research

The data presented in the report by Allyson Cole-Strauss *et al.* (6 Sept., p. 1386) infer a remarkable phenomenon: 50 to 80% of mutant, β^S-globin loci in a population of B cells were converted to wild-type alleles after exposure of those cells to oligonucleotides containing wild-type, β^A, sequences. This represents an absolute recombination frequency (recombinant cells/exposed cells) that is three to six orders of magnitude higher than that normally seen in cultured mammalian cells (*1*).

These data were published without the clonal isolation of a single recombinant cell line. Gene conversion was assayed on pooled-cell extracts containing a mixture of reagent oligonucleotides and chromosomal DNA in which the wild-type/mutant sequence ratio approached $10^8/1$. Under such conditions, the potential for assay artifact should be considered, yet neither a zero time point nor an end point, in the form of cloned cells, was performed.

The implications of this data should demand the utmost in experimental control.

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