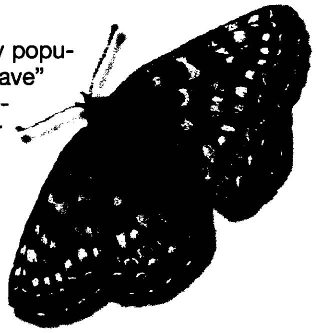
LETTERS

Observations

NELSON/JASPER RIDGE BIOLOGICAL PRESERVE

A reader asks why studying "the demise" of a butterfly population was more important than intervening to "try to save" it (right, *Euphydryas editha bayensis*). Physicists present their case that a collision event warrants "further investigation." "Fairer evaluations for young scientists" might result, it is said, if scientific papers described "who was responsible for what." And asbestos removal is advocated for the Jussieu campus of the Université de Paris.



Butterfly Watching

With respect to the Research News article "Much-studied butterfly winks out on Stanford preserve" by Ellen McGarrahan (24 Jan., p. 479) about the loss of the Jasper Ridge checkerspot butterfly, it is not clear why watching the demise of this population (about a dozen individuals in a preserve in California) was an important research opportunity that precluded intervening to try to save the butterflies. Nor is it clear what data were garnered, what specific hypotheses were under examination, and what possibilities were ruled out by this "enlightening" study of extinction.

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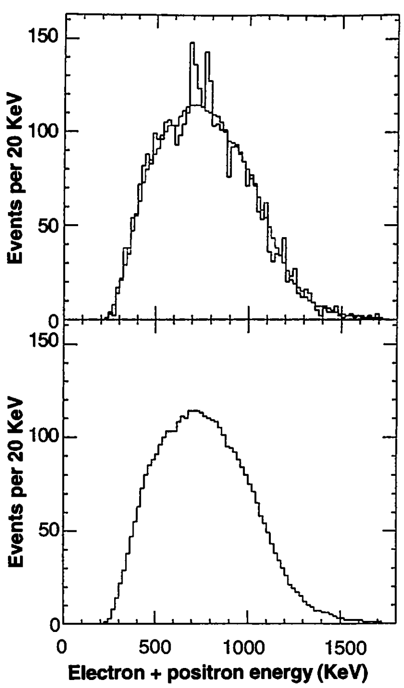
APEX: An Unexplained Event

In Gary Taubes' article "The one that got away?" (News & Comment, 10 Jan., p. 148), we are portrayed as being at odds with "virtually every nuclear physicist who has worked on the [APEX] experiments." While that may be true, the article could be interpreted as casting doubt on our scientific judgment and implying that somehow our support was "enlisted" by Jack Greenberg for other-than-scientific reasons. Thus, we outline here the evidence for structure in the electron positron sum-energy spectrum from the APEX experiment that Greenberg has shown us which makes us believe further investigation is warranted.

The top drawing in the accompanying figure shows the sum-energy spectrum as analyzed by Greenberg and his colleague Guangsheng Xu, overlayed on a background spectrum. The bottom spectrum is the background alone. The peaks between 680 and 800 thousand electron volts (keV) are in the same sum-energy region as seen in the EPOS I experiment (see the EPOS, 1990 spectrum from Taubes' article, p. 149).

We ask three questions about this structure and these peaks. First, is the deviation from the background statistically significant? By standard statistical analysis, the probability that a statistical fluctuation from the background of this magnitude would occur in the sum-energy region previously seen in the EPOS I experiment is 3.5×10^{-5}. The probability that such a deviation would occur by chance at any value of sum-energy in the spectrum is 3×10^{-3}. Thus, we consider



the structure to be statistically significant.

Second, is the structure somehow manufactured by the cuts? The background spectrum shown is made with the use of the same analysis, but electrons and positrons from different events are chosen; that is, they are randomly associated. Such a background can also be generated by electrons and positrons in the same event, but out of time coincidence, and no structure is seen. We conclude from this that whatever the cause, the structure only occurs with correlated electrons and positrons in the same event and in time.

Third, is there adequate motivation for the cuts? Greenberg's analysis imposed only two additional cuts on the data in the spectrum shown in the article from APEX, 1995: (i) the opening angle between the electron and positron was to be greater than 135°, and (ii) there were to be only one electron and one positron in the event. The primary reason for the first cut was that the EPOS I data suggested that the electrons and positrons observed in the peaks were emitted back to back. Thus, a cut was made that would have high acceptance for such events. The second cut was intended to yield a cleaner sample. Both these cuts appear to have been well motivated.

The spectrum shown can be divided into three statistically independent samples: (i) events where both heavy ions were observed and electrons went to the right and positrons to the left of the apparatus, (ii) events where both ions were observed and the electrons went to the left and positrons to the right, and (iii) events where only one ion was observed. All three spectra show the same structure, albeit with the expected reduction in statistical significance.

With regard to Taubes' description of the generation of such structure by randomly generating events, the data shown are the total sample and have not been selected beyond the cuts shown. Thus if the peaks were a statistical fluctuation such as was generated randomly in a computer, they would have occurred with at most a 3 in 1000 probability.

The structures have been reproduced by a member of the APEX collaboration and have been seen by many in the collaboration. To our knowledge, they have not been explained in a scientifically consistent way. Nothing in Taubes' article refutes this result.

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Because of my involvement in the development of the theory of strong fields referred to in Taubes' article, I followed the details of the electron + positron (e^+e^-) experiments closely from their inception. There are clearly anomalous aspects of the observation of so-called e^+e^--resonances that puzzled me right after the apparent observation of correlated e^+e^- emission. In particular, a particle interpretation seemed unlikely because it was in sharp contradiction with well-established physics (for example, the Lamb shift, g-2, Delbrück scattering). However, this does not detract from the possibility that positron lines and later the e^+e^- sharp sum-energy lines have been observed for which an explanation might be found in the context of known physics, such as quasi-atomic phenomena correlated with nuclear effects. This is a separate consideration.

Greenberg was extremely cautious when making public statements and consistently insisted on further clarification of the positive findings in detail, as he now does for the negative results from APEX that are based on incomplete measurements and analysis.

When I doubted the existence of a new particle in an opening address at the German Physical Society meeting in Berlin in 1988, I was assailed by many experimentalists because of "my discouragement of their 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