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his comments deserve further clarification. First of all, at the time of publication of the October issue at least three other groups, in addition to the Ford group, had completed measurements of atmospheric OH below 30 km. The groups involved include: a German group under the direction of D. Ehhalt and D. Perner; a Florida-Atlantic University group under the direction of C. R. Burnett (in cooperation with J. Noxon of the NBS Boulder Labs) and the Maryland group under my direction. Both the Maryland and the German groups have since given detailed reports on their measurements at the "International Free Radical Conference" at Laguna Beach, California on 3 January 1976. All three measurements have now been submitted for publication in major journals.

Secondly, the use of the phase "ambient OH measurement" by Wang might have led to some confusion on the part of the reader. Any measurement in local air does indeed involve the sampling of ambient air, but this may or may not be representative of natural tropospheric air depending on the location of the local air relative to anthropogenic pollution sources. Only measurements of natural tropospheric or stratospheric OH levels are of major concern with regard to the fluorochlorocarbon–ozone question.

The Ford measurements made at ground level in Dearborn, Michigan during summer months certainly may have been a measure of ambient OH, but they were not a measure of natural tropospheric OH levels. Thus far, only the Maryland group has successfully interfaced a tunable dye-laser system with an aircraft platform and carried out measurements of natural OH levels above the atmospheric boundary layer. Burnett has been looking at column densities of OH in the atmosphere from a mountain observatory (using high-resolution interferometry) and he too has been looking at natural levels of OH. In these experiments, however, the principal region where reliable data can be collected is in the stratosphere, where OH levels are much higher. The German group has been looking at OH (via laser-absorption spectroscopy) near ground level and their measurements would also properly be called ambient OH measurements.

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Since versus because

With reference to the edited version of my recent review of Child's *Molecular Collision Theory* (March, page 59) I call your attention to an amusing lapse in the concluding paragraph, where a "since" has become "because." The two words are not the same. Thus one says fondly to one's wife, on the occasion of an anniversary, "I have aged ten years since I married you," *not* "I have aged ten years because I married you."

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Prejudice in physics?

In the interesting article by Samuel Goudsmit on the discovery of electron spin, in the June issue, there appears on page 42 the following sentence:

> "When my former student Robert F. Bacher was considered for a position at Cornell University in 1934, R. C. Gibbs asked me in confidence, on behalf of F. K. Richtmyer, whether Bacher was Jewish—if so, he would not have got the job."

I can perhaps believe that Gibbs made the inquiry referred to, but the implication that it was made on behalf of F. K. Richtmyer is clearly incorrect. The facts are these:

- R. C. Gibbs and F. K. Richtmyer were not on speaking terms and had not been for many years. (The animosity between them was always a cause of embarrassment and regret to me, but that is another story.) The idea that F. K. Richtmyer would have asked Gibbs to make an inquiry of that nature is just not credible.
- Gibbs was chairman of the physics department, and Richtmyer never was. Quite apart from the animosity, there is no reason why Gibbs should have consulted Richtmyer rather than other members of the department about Bacher's appointment.
- Richtmyer was deeply involved in the graduate school, of which he was dean; consequently he was inactive in the affairs of the physics department at that time.

Although I disapprove generally of speculations on things of this kind, I feel compelled to make the following conjecture on the origin of Goudsmit's remark: It seems to me likely that Gibbs somehow misled Goudsmit (possibly unintentionally) into believing that his inquiry was being made on behalf of F. K. Richtmyer. The idea naturally occurs to me that calumny could have been Gibbs's motive, but I tend to think it was not; there are many ways in which such a misrepresentation could have arisen, and I don't believe that Gibbs was a vicious man. In any case I think it is highly unfortunate that Goudsmit was so easily deceived.

I am aware that there was considerable antisemitism at Cornell University at that time—that was Goudsmit's point of course. Antisemitism was evil then at Cornell, and it is evil now in the Soviet Union, in the United Nations, and elsewhere in the world, but I think one has to be careful about making specific accusations. I never heard my father make an antisemitic remark, and I know of no basis for assuming that he had antisemitic attitudes. He was known at Cornell for his opposition to discriminatory practices of any kind, especially in regard to foreign and minority students. The suggestion that he would or could have prevented Bacher's appointment if the answer to Gibbs's question had been "yes" is in my opinion unwarranted and regrettable.

R. D. RICHTMYER
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THE AUTHOR RESPONDS: I am happy that Robert Richtmyer throws some new light on the incident surrounding Robert Bacher's appointment at Cornell University. It is not impossible that Gibbs misled me. It was the very first and the only time that I encountered open antisemitism among physicists, outside Germany. I was upset about it and mentioned it to Bob Bacher at once; the letter is in the archives of the American Institute of Physics. Gibbs approached me on the subject at the 1935 Washington meeting, definitely mentioning Richtmyer. In those days physicists discussed only physics. It was impossible for me to know whether Gibbs, whom I had met a few times, or Richtmyer, whom I had never met, had any antisemitic tendencies. My colleagues at Michigan inferred that such prejudices originated with academic administrators and regents and that deans were forced to act accordingly.

S. A. GOUDSMIT
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Need for good references

An economic procedure to determine whether or not time should be spent in reading a scientific paper is to study the abstract carefully (if it well written) and to study the list of references carefully (if it is complete enough). In this list, the experienced scientist will find other papers he already knows and thus get a feeling on which foundation the new paper stands. He will also be able to assess the quality of referencing, which tells him something of the quality of the paper. While writing a good abstract is an art some of us must still learn, and adequate referencing is a science necessary for a good scientist, the completeness of information in the reference list is usually beyond the influence of the author and depends on the policy of the journal.

I mention that the title of a paper and both the beginning page and end page are essential contents of a reference, and thus of a scientific publication. Saving printing space (and thus money) by omitting either the title or the end page or both degrades the quality of the publication.

The usefulness of a reference list—especially in a book—can be further enhanced by making it into an author index. This requires some work, but very little additional printing costs.

HANS DOLEZALEK
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AIP COMMENTS: To include the titles of articles in bibliographic citations could indeed be very helpful to some readers. Some of the journals published by AIP permit or even encourage this practice. Most discourage it to save space. However, since many of our journals now have a format that often results in some empty space at the end of an article, we should re-evaluate current practice. Dolezalek's comment comes at an opportune time, since AIP's Publication Board is in the process of revising the *AIP Style Manual.*

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Physics problems in fusion

We should like to correct some statements concerning controlled fusion contained in the lead letter on page 9 of the April issue. ["The energy crisis: what physicists can contribute" by Tau Yong Chiang.] The letter states, "The problems in plasma and laser fusion [meaning, presumably, the magnetic and inertial confinement approaches] are largely engineering in nature but there are some problems [meaning physics problems?] as well." It then cites the need for increasing $\beta = 8\pi nKT/B^2$ (which is principally a problem for tokamaks) and several technological issues associated with the (still quite speculative) electron-beam pellet approach to fusion.

There is no question that the engineering and technological problems that must be solved before we achieve usable energy from controlled fusion range from formidable to staggering, dwarfing even those of the Apollo program. However, there are also a wide range of extremely subtle and challenging physics questions whose understanding will make a vital contribution to the extrapolation from present experiments to the first working fusion reactor and to the development of more advanced systems involving "clean fusion" (having only, or largely, charged reaction products); direct conversion of particle kinetic energy to electricity (avoiding or minimizing thermal cycles); and so on. For the most part, these problems are essentially classical in nature, involving electrodynamics and the non-equilibrium statistical mechanics of nonlinear, cooperative, collective, many-body phenomena, and hence are sometimes characterized as "applied," in contrast to the "basic" question of elementary-particle physics. Nonetheless, these problems are difficult as well as interesting, and they are squarely centered in physics, rather than engineering or any other discipline.

Theoretical problems include anomalous transport of particles and energy across magnetic fields; large-amplitude wave interactions with particles and with other waves; nonlinear beam–plasma interactions, and stability of exotic magnetic-field plasma configurations. Expertise in mathematical and computational physics is also required in the development of good, physically accurate numerical models for simulating the multitude of complex interrelated physical phenomenon occurring in hot plasmas. On the experimental side, one of the major problems that has inhibited progress towards fusion is our inability to make precise and detailed measurements of what is actually happening in a plasma. In almost all cases, advances in complex areas depend on our ability to measure what is going on. The high temperature and low density for magnetic fusion and the extreme density and short times for pellet fusion make plasma diagnostics an extremely challenging area for experimental physicists.

The compelling advantages of controlled fusion as one of the most satisfactory long-term solutions to the energy problem are too well known to warrant repetition here. Its principal disadvantage is just the multitude of challenging technical problems, both physics and engineering, that must be overcome. In wartime, our most talented scientists have concerned themselves with pressing problems of national urgency, such as the Manhattan project and the development of radar. The problems of energy are just as real, although less dramatic, and the fusion program could benefit greatly from increased involvement by our best physicist, both new PhD's and nature scientists.

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BURTON D. FRIED
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Support for rebuttals

We would like to support the suggestion made by C. LePair[1] recently and by Robert L. Chaplin[2] earlier for improving the peer-review system in NSF and perhaps other government funding agencies. The modification proposed is that, after the reviews are received by the agency and *before* they are acted on by the program director, they are sent to the principal investigators to defend themselves and/or to clear up any misunderstandings. Such a rebuttal statement becomes then a part of the data the program director has to work with. The time honored peer-review process in journals obviously incorporates this feature. Indeed, it is hard to see how peer review can be used meaningfully without the opportunity to rebut what may be a simple misunderstanding or error on the part of the reviewer.

References

1. C. LePair, PHYSICS TODAY, May 1976, page 13.
2. R. L. Chaplin, PHYSICS TODAY, January 1974, page 121.

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New journals not needed

N. P. Mermin and K. G. Wilson (March, page 11) raise a valid point in questioning whether the physics community needs yet another journal such as *Communications on Physics*. This first issue of this journal completely falsifies the arguments presented by David Caplin *et al.* This issue contains four papers in solid-state physics—a field amply covered by existing journals. Two papers are from British authors who have access to excellent journals in the UK without page charges. One is from Japan that could have gone to a well established Japanese journal avoiding any need for long-distance phone calls; and the final paper is from the US and supported by an NSF grant. None of the papers (average length approximately seven pages) could really be said to have warranted rapid publishing. We need fewer journals, not more.

The solution to this problem is in the hands of the physics community. We should refuse invitations to act as referees or editors for these journals and refuse to submit papers to them. Finally, let us not forget those working in less privileged countries. Every new journal decreases their chance of catching up with the scientific community or maintaining their position in it.

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THE EDITORS COMMENT: Brian Wybourne's attempt to condemn *Communications on Physics* after looking at only the first issue seems to us rather hasty. Our arguments for starting the journal were presented fully in the March issue and cover most of the points that Wybourne raises. We need add only that since then we must have had more evidence of discontent with previous letter