practice during the course of or under a government contract"—even if the invention was conceived and patented years before the contract was executed. Large corporations rarely make this mistake. They either refuse proffered government contracts entirely or they reduce their previously conceived inventions to practice before entering such contracts. While my own sense of justice is outraged at the treatment accorded Offner by the government, it seems that Senator Gaylord Nelson is no less outraged in the opposite direction. To his way of thinking the government has played the dual role of Santa Claus and the Tooth Fairy by allowing Offner and other small inventors to collect royalties from parties other than the US Government.

If Offner were to deal with the Department of Energy today, he would find his position vastly more difficult. Instead of simply taking a royalty-free license, the government would have the right to demand the entire right, title and interest to his invention. Thanks to Senator Nelson, this identical result would obtain today if Offner's invention were first actually reduced to practice during a government-guaranteed loan, despite the fact that the loan was fully repaid on time and with interest. With friends like Senator Nelson, the small inventor needs no enemies.

BETSY ANCKER-JOHNSON
Argonne National Laboratory
Argonne, Illinois
5/30/78

Demand for good physicists

G. Adomian in his letter on tenure (June, page 48) seems to imply that for the physicists who are not "on the frontier" a "junior college would be in order or industry."

In the very same issue (page 54), in a book review, it is reported that Bell Telephone scientists Arno Penzias and Robert Wilson confirmed the existence of a cosmic microwave background, predicted by the big-bang theory, while engaged in a satellite communication experiment.

Is cosmology far enough "on the frontier"?

The same youthful naivety is displayed by Adomian when he proposes that only "superbright" and "200 IQ" new graduates should be considered for tenure.

As a good physicist, Adomian should know that "superbright" has no operational meaning and that IQ tests are not as reliable as Newton's equations.

M. MARTINI
Oak Ridge, Tenn.
6/20/78

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In his letter (June, page 48) G. Adomian makes the incredible statement, "Good people are still being hired." This letter clearly implies that if a physics PhD cannot get a job, he is no good. The author would have us believe that all is right with our American system, and the problems lie with certain PhD's who have a "lot to learn."

Strong exception to Adomian's letter must be made, and a challenge is in order. For any criteria he produces to define a *good physicst*, a large number of physicists meeting these criteria and no longer employable as physicists can be demonstrated. Unless, of course, Adomian insists on defining *good* physicists as *employed* physicists. A few case histories will provide anecdotal evidence that good physicists—even excellent physicists—cannot find work.

Sharing an office for a year with physicist A, a theorist who worked intently and alone, was unnerving. Every two months or so he completed a paper and mailed it to *Physical Review*, and a few weeks later a postcard arrived informing him that his paper had been accepted for publication. Over the course of a year he submitted six original manuscripts to the *Physical Review*, and not a single referee voiced a single criticism of his work. This journal has very high standards, and certainly physicist A was a good physicist. Unfortunately for physics, he was forced to take his powerful intellect elsewhere.

Physicist B was good enough to go to one of the leading graduate schools, and there he excelled and was able to write his PhD thesis under the direction of a highly respected Nobel laureate who developed a special affection for physicist B and his work. Unable to find employment as a physicist, Dr. B programmed computers for several years before returning to school in another field.

These examples are too often the rule rather than the exception, and the list could go on and on, but only one more tragedy will be related here. Graduating number one in his college class, physicist C was offered seven fellowships to do graduate work, which he successfully completed. Though he completed his PhD in a hard year for finding jobs, five research groups offered him post-doctoral positions. For several years he held temporary positions, publishing some twenty papers in leading journals. Then, like physicists A and B before him, he was unable to continue and left physics.

The past decade has been a difficult one for physics. Many research fields have had their funding slashed severely. The number of students entering physics had decreased, and science no longer gets the cream of the crop. Many excellent physicists, both young and old, have had their careers smashed by the decreased number of jobs. To dismiss them as being "not good" is an injustice to them and their abilities.

Perhaps one day physicists will learn to

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work together for the common good of their science and its practitioners, rather than slandering those who succumb to our fickle government's policies.

WILLIAM J. MEGGS
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Miami, Florida
6/28/78

THE AUTHOR REPLIES: M. Martini seems to think I'm downgrading industry. That's nonsense. Most of my experience is in industry. I have found it challenging and rewarding and may return to its despite having the sacred tenure, which my critics value so highly. My original letter (June, page 48) simply referred to the tenture question and Yaes's statement (February, page 83) that *older* and, by implication, *duller* professors should be forcibly retired to make room for *younger* and, by assumption, *brighter* researchers. (Try less politics and harder work and you may be an "older duller" professor someday.) Instead of thinking a PhD entitles one to a tenured job (no further work of course) at a university, explore industry and government. Explore other fields. Start your own field. Universities cannot absorb the PhD's being turned out. Industry *does need people who are good* (I have just returned from a consulting trip and every division head I spoke to mentioned their urgent personnel needs). No one is guaranteed a particular kind of employment. If all you know is the area of your thesis and there are no jobs there, adapt or be unemployed. Of course psychologists, social reconstructionists, and egalitarianists may quarrel about the significance of IQ tests as Martini does. It is nevertheless true that some people are much brighter and more capable than others and demonstrably so. However, I did not advocate hiring only 200-IQ graduates. (Neither would I go as low as 120 or 130). Brightness is not correlated with chronological age and cannot even be eliminated by the public school system. My sentence was conditional and pointing out the irrational connection of "bright" with "young" and a tongue-in-cheek corollary that if, indeed, everyone degenerates at 40 or 50, then we should start high. If that appears illogical, why are you in physics?

Where cosmology is on the frontier is irrelevant to my statement. (I find neither the big-bang theory nor a simple satellite measurement impressive. So NASA allowed someone to put a voltmeter on one of their vehicles. Big deal.)

William Meggs's letter charges me with the "incredible statement" that good people are still being hired. *We* are hiring good people! The corporations I consult for are hiring good people! Meggs says I would have everyone believe that "all is right with our American system." Ah hah! Now maybe the problem is clearing up. Meggs or his friends could go to Sweden; they could vote for me in 1980 instead of Carter, or take one of a dozen other alternatives, but I can't reply more specifically to irrationalities and irrelevancies. Apparently he believes in guaranteed employment. They have that in China.

Fewer PhD's should be turned out. A PhD does not mean automatic tenured employment. Notice how physicians hold their number down. Many persons who would like to cannot become physicians. Allen Bakke, for one, was prevented from realizing his aspirations by the social reconstructionists despite ability. How far would a young intern get who demanded that the senior surgeons be retired so he could move up? I have slandered no one and do not believe in the unions that Meggs apparently wants. If we get them, seniority rather than merit will be the criterion. I believe in only merit as a criterion. Meggs and Martini and Yaes apparently believe in inverse seniority.

It is illogical to demand that the young graduates get jobs at the expense of present job holders (who earned it without the help that the former demand) simply because they are younger. Meggs's anecdotes are of no significance. Did no one want his friend anywhere? If so, Q.E.D.

Read my letter again in the context of Yaes's letter. Try to find a field in which your abilities are needed rather than expecting security. Security is an illusion. It is not found in our system, and its price would be too high. Unless you would seriously propose firing Richard Feynman or C. N. Yang to make room for a young PhD, then you agree on the basic premise. If you *would* propose that, I'm not interested in replying to idiots. I'm tired of those who would destroy our universities or those who think the goal of the profession is full employment.

If you can solve problems, you can get a job. If you can't, you will understand what you can use your PhD diploma for.

G. ADOMIAN
University of Georgia
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7/25/78

Sins of authors and referees

The "ongoing and pervasive situations" which, in two separate instances came face-to-face to David Eimerl ("Referee Standards", February, page 15) are indeed rather common, as probably many a colleague knows who is often asked to review papers for journals or proposals for grants. The dangers could be even greater than Eimerl suggests, and I should like to add a third and a fourth "situation" of similar type.

▸ The classical concept that the author of a scientific paper has fulfilled his obligations towards other scientists who have published related material earlier, simply by quoting a number of references, has lost an important precondition on which it was founded. For quite some time now, we have not been justified in assuming (or pretending to assume) that the reader of a scientific paper will automatically recognize the quoted papers of other scientists. There are too many papers, and the scientists during their studies and later work have too little time to know them all. In many cases, this disappearance of the truly well-read colleague is of no consequence for our referencing habits: If we directly quote a scientific fact and then add the name of the scientist who found it or described it, we have done our duty. There are, however, other cases in which, most often near the beginning of a paper, there is a presented a list of references that relate to former work on the same or a neighboring topic, without any specific reference to the one or the other item to be discussed in the paper. This habit than allows the author to represent his material as if he were the one who has found the essential facts. Instead of going on by saying that *A* described a hypothetical effect in this or that way and arrived at this or that conclusion, and then confronting the other scientist's findings with his own, the author just presents his own findings and does not disclose how much he really owes to the other scientist or scientists. This practice is widespread, and one is sometimes in doubt, whether or not this "trick" is applied innocently. Not only are older sources frequently suppressed; even recent discoveries by younger colleagues are denied the acknowledgment they deserve and often urgently need for their own deserved promotion. I certainly hope that by bringing this to the attention of future authors and future reviewers I can contribute to reducing this seemingly honorable dishonesty.

▸ The damage done by bad scientific work is sometimes much larger than even Eimerl states. Reviewers often let bad papers pass, either because the reviewer is too permissive and does not realize that he is in fact doing damage not only to science but also the reviewed author in the long run, or because a reviewer is too permissive towards himself and does not admit the fact that the content of a paper or proposal is indeed outside of his own expertise. In both cases there is the danger that such permissiveness may contribute to the creation of a wide pseudo-scientific practice, even a pseudo-scientific community. One example may be found today in the domain of the problem of atmospheric electricity's influences on biological systems including humans. Such a situation can grow to become either very grotesque or frightening. The authors of such pseudo-scientific papers quote each other, have their