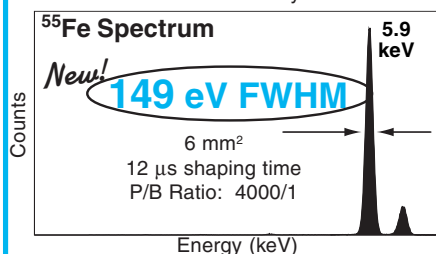


X-Ray and Gamma Ray Detectors

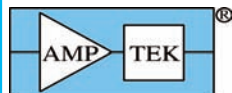
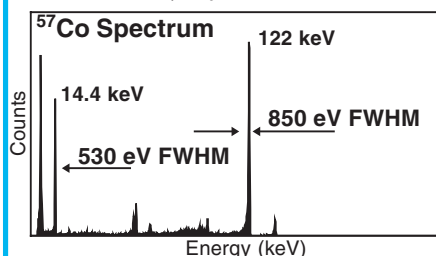
XR-100CR

With Si-PIN for X-Ray Detection



XR-100T-CdTe

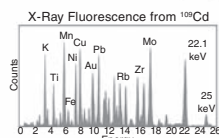
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- X-Ray Tube Beam Monitor
- Heavy Metals in Plastic
- Vacuum Applications
- Paper Analysis
- Portable Instruments
- Semiconductor Processing
- Toxic Dump Site Monitoring
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should be elevated to a status approaching that of paper authorship; the contribution to science of a good referee's report identifying some fundamental error and barring publication is greater, perhaps, than the contribution of several mediocre published papers. To implement such elevated status, departments could simply agree to count the refereeing beans along with those for authorship. And journals could publish referees' names along with an accepted paper—does anonymity really exist in the present system? Referees rejecting papers should also be acknowledged somehow.

Michael Ibison

(ibison@earthtech.org)

*Institute for Advanced Studies at
Austin
Austin, Texas*

Gad-el-Hak replies: From these and many other letters I received, it appears that there is a consensus on the crux of my Opinion piece. We merely need to mull over some of the details.

Lance Nizami's points regarding youth, citation cartels, and personal acrimony are well taken. Nevertheless, I still think that counting a normalized number of citations com-

bined with the opinions of several experts is the least subjective way of judging the worthiness of one's publications. I have no simple answer for another good point Nizami tackles: "Exactly whose work is to be evaluated?"

Notwithstanding Vladimir Krasnopol'sky's opening sentence, I do not take credit for defining the established impact factor, but I agree that two years is insufficient time to accumulate a representative number of citations, considering that one of those two years does not exist in practice. The important thing is to use a standard definition that applies equally to everyone, whether the writer's field has many journals or only a few. How I would like to be in the field of planetary science, in which 80% of the publications appear in two journals, in contrast to the 250 journals in fluid mechanics! Still, we ought not to change the definition or the rules to fit the discipline.

I agree with Michael Ibison's suggestion that quality may improve if we all give sufficient credit to the task of refereeing. However, the reviewers' anonymity, though difficult to uphold, does have considerable value, and I do not have a simple

solution to the obvious contradiction. Although far from being infallible, the entire enterprise of citation index and impact factor is better than the alternative, straightforward bean counting.

Although generally favorable, none of the letters I received directly addresses the crux of the problem: How do we put the brakes on the growth rate of journal and book pages? Yes, we should strive for quality, but in order to drive down the demand and hence the supply for journal pages, we must tame our unrealistic expectations of anyone who is up for tenure or promotion. My own minuscule, nonscientific survey revealed two points. First, scientists in the trenches support limiting the number of publications in any résumé submitted to a hiring, tenure, or promotion committee or to a funding agency. Second, university administrators—the ones who can make and enforce such decisions—unfortunately have shown no interest in the discussion.

Mohamed Gad-el-Hak

(gadelhak@vcu.edu)

*Virginia Commonwealth University
Richmond*