Communications for this section will be published as space and priorities permit. The comments should not exceed 350 words in length, with a maximum of five references; one figure or table can be printed. Exceptions may occur under particular circumstances. Contributions may include comments on articles published in this periodical, or they may be reports of unique educational character. Specific permission to publish should be cited in a covering letter or appended as a postscript.

To Publish or Not To Publish?

To the Editor:

Two points raised in the editorial, “What Constitutes Prior Publication?” (Chest 1987; 91:2-3), particularly concern me.

Investigators are frequently asked to describe results of their research at satellite symposia organized with the official sanction of major scientific meetings. The researcher is then asked to submit a manuscript of 1,200 words or so summarizing the material presented, which is published with other papers from the symposium in a supplemental issue of a major journal. A 1,200 word manuscript is simply too short to report the complete results of most studies in adequate detail. In addition, such papers are not regarded by one’s colleagues (and promotion committees) as having the same importance as full papers published in regular (ie, fully peer-reviewed) issues of the same journals. Therefore, one wishes to publish a complete manuscript in a peer-reviewed journal.

The strictures which editors are promulgating in regard to prior publication place investigators in an untenable position. One can play the hermit and refuse to participate in satellite symposia which one’s colleagues, the organizers of the main scientific meeting, and the journal editor publishing the supplement all appear to encourage as a legitimate professional activity, or one can participate and risk having one’s significant papers refused on grounds of “prior publication.” It seems to me there must either be a moratorium on publishing supplements, or a recognition by editors that there is place and need for both brief symposium papers and complete (albeit partially duplicative) peer-reviewed publications.

There is also a place for papers reporting larger numbers of cases (the practice termed “self-plagiarism” in the editorial) or longer duration of therapy following up on earlier, more preliminary results. When the initial effort produces results of interest to the scientific community, it is in the interest of science (and often of patient care) that they be communicated as quickly as possible rather than hoarded until all the results are in. There is also the real-world consideration that establishing priority in discovery can make or break investigators’ careers—grants, promotions, and even Nobel prizes can hinge on it.

A later report of additional cases enhances the statistical power of the study and assures us of the correctness of the initial results. A report of longer duration of therapy extends the initial results in another way that is scientifically and clinically important.

Obviously all of these types of publication can be abused, and certainly repetitive publication should be minimized. Restrictions that are too severe, however, may harm scientific communication.

James F. Burris, M.D., F.C.C.P.,
Georgetown University School of Medicine,
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Erratum

To the Editor:

In our article, “Alterations in immunoregulatory T-cell subsets in cigarette smokers” (Chest 1986; 90:39-44), figures 2 and 3 were erroneously transposed. The illustration displayed as figure 3 with the title, “Helper cells (% of lymphocytes)” is actually figure 2 and should be placed with the legend for figure 2. The illustration displayed as figure 2 with the title, “Suppressor cells (% of lymphocytes)” is actually figure 3 and should be placed with the legend for figure 3.

U. Costabel, M.D., F.C.C.P.
Abt. Pneumologie,
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Peer Review in Medical Journals

A Bad Example

To the Editor:

Dr. Robin (Chest 1987; 91:252-55) has maintained his stimulating and provocative tradition by challenging the conventional peer review system used by many distinguished medical journals for evaluating manuscripts for publication. One of his major concerns, and rightly so, is that manuscripts containing errors in research methodology and/or interpretations of findings, if published, may result in harm to patients. In support of his position, he cites two specific examples, one relating to the treatment of breast cancer, about which I am not qualified to comment, and one relating to the possible protective effect of barbiturates in the treatment of global cerebral ischemia, about which I am qualified to comment since I was Editor-in-Chief of the journal at the time the article was published.

Dr. Robin is critical of the latter publication because it suggested that barbiturates may protect the brain from global ischemia; as a consequence, innumerable patients were treated with a potentially harmful therapy until the authors did another study published eight years later, which contravened their earlier work. In attempting to prove his point, he has chosen the wrong example. What he failed to recognize and acknowledge is that the article was accompanied by an editorial which was written by two noted authorities in the field of barbiturates and brain protection. In their editorial, Drs. Rockoff and Shapiro articulated in a clear and comprehensive way the methodologic flaws of the study, and concluded by stating that additional laboratory studies and controlled human studies must be done before high-dose barbiturate therapy has clinical application in the treatment of cerebral ischemia.

In contrast to Dr. Robin’s conclusion, this represents peer review at its best. Both prospectively and retrospectively, the peer review process worked to the benefit of patients. First, the topic was and is important because global ischemia is a devastating event for which there is no specific, assured, ameliorating treatment. Second, the Bleyaert study was flawed, a criticism incidentally that is applicable to every study, but the flaws were well defined for the reader in the article and accompanying editorial. Third, the editorial provided appropriate, conservative guidelines for both the investigators and readers as to how they might use this information. And finally,

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neither Dr. Robin nor I have any data proving that any patient was harmed by publication of this article. Progress in research requires publication of controversial, inconclusive studies, which the Bleyaert study was. When done in a responsible manner, it ultimately leads to improvement in patient care—which it did.

C. Philip Larson Jr., M.D.,
Professor of Anesthesia and Surgery (Neurosurgery),
Stanford University School of Medicine,
Stanford

REFERENCES
1 Robin ED, Burke CM. Peer review in medical journals. Chest 1987; 91:252-55
3 Rockoff MA, Shapiro HM. Barbiturates following cardiac arrest: possible benefit or Pandora's box? Anesthesiology 1978; 49:385-87

Reprint requests: Dr. Larson, Department of Anesthesiology, Stanford University School of Medicine, Stanford 94305

To the Editor:

It is an honor to be addressed by Dr. Larson, who is not only a distinguished physician but one who played a pivotal and direct role in the mass use of barbiturates for cerebral ischemia.

Mother History is a fickle mistress. The editorial of Rockoff and Shapiro must have been anticipated by several favorable peer reviews as well as a favorable editorial board review to explain the final publication of the Bleyaert paper. While the editorial does point out a few of the shortcomings of the paper, it is by no means thorough in this respect. And one cannot conclude from the editorial that 1) the paper was fatally flawed, and 2) to base clinical application on such a flawed paper was to invite disaster.

For example, the editorial by Rockoff and Shapiro concludes:

"We clearly need additional studies to support this fascinating initial report. Confirmation of an effective postarrest resuscitative action will then necessitate controlled human studies to establish guidelines for dose, timing and duration available, clinical application of high-dose barbiturate therapy for ischemia of the brain should be tempered with caution (emphasis mine)."

This is hardly a clarion call to desist from using the therapy because the basic study was seriously flawed.

One would have predicted, both from the published peer review article and from the editorial, that barbiturate therapy would be used extensively in patients; of course, that is exactly what happened. The addition of the editorial to the peer review process is not a glowing affirmation of the value of the peer review process.

The editorial did not prove to be of benefit to patients. There are many devastating clinical events for which the treatment is worse than the disease—I do not agree with Buckhardt (the father of psychosurgery) that "a dangerous treatment is better than no treatment at all." Every study is flawed, so the flaws of the Bleyaert study is no different than those found in every study—that is to say that a horse is an animal, Aristotle is an animal, therefore, Aristotle is a horse. I do not know what conservative guidelines are applicable to an erroneous piece of work except, Don't use it in patient care!

The most useful part of the editorial was an accurate description of the risks of barbiturates:

"It is also important to note that large-dose barbiturate therapy is not without risk. In addition to prolongation of the need for mechanical ventilation (with its attendant hazards), treated animals needed more cardiovascular support than controls. Laboratory evidence does not yet exist to determine the effects of this type of therapy on the function of other organs, especially the heart with coronary insufficiency. Hoff found frequent cardiopulmonary prob-

lems in primates given large doses of pentobarbital. In addition, patients surviving cardiac arrest will often have myocardial dysfunction and infarction. Anyone who has treated patients with massive barbiturate overdose does not need to have the dangers emphasized further."

I must disagree with Dr. Larson that neither he nor I have any data that any patient was harmed by the paper.

As he was extensively involved in the clinical application of massive barbiturates and, leaving aside isolated case reports of harm, has he reviewed the records at his institution to enumerate the evidence of prolonged ventilatory depression, the incidence of post-barbiturate hypotension, the incidence of massive fluid administration given to correct the hypotension, the number of patients developing fluid overloading after fluid administration, and the number of patients having pulmonary fluid catheters placed to guide "safe" fluid administration? We both know that many patients were harmed; neither of us has counted the numbers.

Incidently, does your institution continue to use high-dose barbiturates for brain ischemia? And if its use has been abandoned, was this change related to the recognition of an unfavorable risk-benefit balance?

This, then, raises the final issue. It would be an excellent contribution to future patient welfare if Dr. Larson and his colleagues would review this intraepidemic from its inception to its culmination, using either data from his own institution or from the general experience. I am delighted to offer a title for his review, "The Rise and Fall of Massive Barbiturate Therapy for Cerebral Ischemia," and to volunteer any help that I could provide in assembling such a useful contribution. To paraphrase Santayana, the patients of those who do not learn from history are bound to suffer.

Eugene D. Robin, M.D., F.C.C.P.,
Stanford University School of Medicine,
Stanford

The Need to Test Home Monitoring for SIDS

To the Editor:

Dr. Robin's article, "SIDS in Home Monitoring" (Chest 1987; 91:765-68), makes a very correct point about the use of apnea monitoring in the home. It seems incomprehensive that a prospective study has not been carried out to this date.

It is clear that Dr. Robin has a built-in bias against the efficacy of the method. I am somewhat surprised that a scientist would express his opinion without the benefit of data. He appears to play especially fast and loose with statements concerning the "disastrous psychosocial consequences" to the families of these children. The cited reference is hardly in the public germain. Unfortunately he is only able to reference one article in that regard. His prejudicial statements are far in excess of any data to support these facts. I would hope that he would show the same constraint in his statements that he asks of those who are proponents of the method.

T. James Gallagher, M.D., F.C.C.P.
University of Florida College of Medicine,
Gainesville

To the Editor:

Thank you for caring enough to write. If home monitors are being used at Gainesville, is it unscientific of your school to use them in the absence of data establishing efficacy? And if so, how can you rationalize that grossly unscientific conduct?

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