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.

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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 antedated 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 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 flow catheters placed to guide "safe" fluid administration? We both know that many patients were harmed; neither of us has counted the numbers.

Incidentally 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 iatroepidemic 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.

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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 incomprehensible 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 german. 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.
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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?
Without broadening the issues, how many of the current modality measurements used in ORs and ICUs have been subjected to vigorous clinical trials which establish efficacy and safety? Scientific validation is not synonymous with widespread noncritical acceptance of various forms of management.

My "bias" is not directed at home apneic monitoring. It is directed at the mass and noncritical use of a host of management modalities. Home apneic monitoring is by no means the worst offender.

The data base that is quoted was the one that is available. If you have a better data base, it should be published by all means. I offer my column as a vehicle.

The summary of Harsell's studies is in the public domain. It can be obtained from the NICHD. What you may mean is that the study has not been peer-reviewed and published in a journal. This raises an interesting, if tangential, issue. Several years ago, the University of Utah completed a small but important cost/benefit analysis of home apneic monitoring. The study concluded that the cost/benefit and risk/benefit ratios were unfavorable. It was universally rejected for publication. My impression is that the major reason for rejection was that the experts in the field could not accept the idea that their endeavors were not patient-effective.

In addition to Harsell's study there are two well-documented cases of infanticide or attempted infanticide as a result of home apneic monitoring. In addition, I have been contacted by a number of mothers, each with her story of severe psychosocial harm from monitoring, anecdotal to be sure, but nevertheless persuasive (to me.)

The only other report I could find in the literature which looks at psychosocial risk was from a group who found only "appropriate" emotional responses by mothers whose infants were being monitored. The study was conducted using a ghetto population in a large Eastern city. I contacted one of the authors, who informed me that ghetto mothers did not seem to be as concerned with the welfare of their children as more affluent mothers. I found this conclusion to be unacceptable as a scientific evaluation and to display a degree of racism which raised important issues about the objectivity of the study, and I have consequently decided the data are not acceptable.

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A Case History

To the Editor:

It is with great interest that I read the article, "Pulmonary Eosinophilia and Coccidioidal Infections" (Chest 1987; 91:734). Patient 2 was originally admitted to our hospital and we transferred her to Stanford. The course of her hospitalization was as follows.

The patient was in excellent health until the onset of pleuritic chest pain on September 16, 1985, while on vacation. The next day, pulmonary rumbles were noted, but not pain or fever. On September 20, she presented with fever and dry cough. Four days later, she was admitted to the hospital with a white blood cell count of 19,000, including 73 segs, 10 percent eosinophils and diffuse bilateral interstitial pneumonia on chest x-ray film. Coccidiomycosis topped the differential diagnosis, and skin tests and serologies were performed. The cold agglutinins were positive at 1:32. Two days later, skin test results came back negative, and an open lung biopsy frozen section showed no inflammation. The Health Department reports were negative for fungus on direct stains of the lung biopsy specimen. The next day, permanent sections revealed eosinophilic pneumonia, with coccidiomycosis of primary concern. IgE level was drawn. Special stains of lung biopsy specimens were negative for organisms. Therapy with SoluMedrol was started, and lung slides were sent to Dr. Luise Katzenstein for pulmonary pathology consultation. On September 30, the IgE level was 12 X normal. Reexamination of all sputum samples for coccidiomycosis spores was negative. Therapy was begun for oral Candidiasis. On October 4, the patient was still intermittently febrile, but the lungs and sputum had cleared. Herpes sore appeared on the lip. Another hypotensive flushing reaction with IV nafillin, 19 percent eosinophilia and PCN allergy. Dr. Pappagianis reported that the coccidiomycosis serologies were negative by the immunodiffusion and comp fix methods. Dr. Katzenstein's consultation was received on October 7, advising that it was not typical of eosinophilic pneumonia, and she suspected a hypersensitivity reaction to some inhaled substance, with a diagnosis of eosinophilic infiltrate with necrosis. The next day, serologies were sent to Dr. Fink, Milwaukee, for inhaled allergic precipitants. The eosinophilia had cleared, but respiratory symptoms still required therapy with prednisone, 60 mg daily, to control. The patient was discharged on October 11 on therapy with ketoconazole and prednisone. A week later, fungal culture of the open lung biopsy specimen reported Aspergillus fumigatus. The patient became symptomatic again by November 5, with severe reticulonodular infiltrates bilaterally, anemia with 6.8 WBC count, toxic granulations, a left shift and no eosinophils in peripheral blood. Repeat fungal cultures were obtained, and serologies sent to Dr. Pappagianis. Therapy with prednisone, 3 mg tid, and Imuran was instituted. Two days later, sputum was negative for eosinophils and the patient's condition had subjectively improved. On November 11, we consulted with Dr. Raffin of Stanford and Dr. Fauci of the NIH. They suggested an immunologic phenomenon, similar to Wegener's, and recommended switching from Imuran to Cytoxan. The patient deteriorated and was transferred to Stanford on November 14. Three days later, the Public Health Service reported Candida albicans from the November specimen. Dr. Pappagianis reported, on November 22, that serologies from samples we submitted as well as those submitted from Stanford were positive for Coccidioides. On December 16, the Public Health Service further reported that the final fungal culture we had submitted was Candida albicans, and mold was also present although they were unable to isolate it from the yeast.

In the article, the authors state that "serologic studies and skin tests play an important role in diagnosis of Coccidiomycosis as well as other infections. In the second case appropriate serologic studies and skin testing could have led to the diagnosis of a coccidioidal infection." I submit that appropriate skin and serologic tests were performed expeditiously. Although there is a three to four week delay in receiving final fungal culture results, our initial open lung biopsy and sputum cultures were negative, so hastening the results would not have helped.

Since IgE is not the first antibody to be formed in response to an antigen challenge (IgM is first), why was the IgE level elevated but IgM and IgG Coccidiomycosis serologies negative at initial admission? Perhaps this patient was first exposed to Coccidiomycosis in the four weeks between discharge and readmission, while on prednisone therapy. It is equally likely that Aspergillus precipitated the initial eosinophilic microabscessing condition, as is much more commonly reported. Faced with all the negative study results and worsening status of the patient, plus her response to steroid therapy, that treatment was logical and, in fact, necessary in this patient.

Is it acceptable editorial practice to allow publication of an article in which the authors did not consult the primary care physicians before publishing to obtain all facts?

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