

EDITORIAL COMMENT

Innovation, Journal Reviewers, and Journal Editors

The Game Is Worth the Candle*

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New ideas are the basis for practical therapies that ultimately benefit patients. At the time of their initial enunciation, really creative new ideas regarding biology, medical procedures, or medical devices often seem strange, or even may be considered “crazy” and unlikely to stand the test of time. So how are we exposed to these innovative ideas? The vast majority of academic investigators would like to publish their attempts to test such new approaches in the “best” journals that reach the appropriately interested audience.

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There are many anecdotes about the difficulty encountered by innovators who tried to publish early work on truly revolutionary methods such as echocardiography and angioplasty. Investigators recognize that this *Journal* has the largest readership in, and influence on, the field of cardiovascular disease, and they would like to have their work published here. However, the number of articles submitted for review has skyrocketed in the past few years, and now <20% of the manuscripts submitted to this *Journal* will be accepted for publication. It is the editors' job to decide which manuscripts to publish, realizing that they will disappoint the majority of their colleagues. The rejected ideas and findings may or may not be published elsewhere. How should a set of editors position their policies to benefit their readers and their field by sifting out the “wheat from the chaff” while trying to spot the truly seminal discoveries and innovative methods? This is a difficult and serious game.

A manuscript submitted for review is the result of a long sequence of events. When a significant scientific problem or medical need is recognized, it fosters thinking by inventive physician-scientists and others to find a solution to that need. Innovations, as contrasted with evolutionary advances, often rely on a person or team assimilating isolated or

apparently unrelated past observations for application to a new problem. This is an arcane process, and these days the team often consists of physician-scientists, engineers, physicists, chemists, and technicians. They work to design experimental models of the problem for investigation. They expend significant resources to test their hypothesis. They labor to write a clear and objective manuscript and they are forced, by convention, to acknowledge the multiple limitations of their approach and conclusions. This enormous effort is respected by the journal editors and reviewers through their rigorous process for handling and judging manuscripts. Both the creation of the manuscript and the review of the work are difficult and careful processes.

The process of manuscript review for medical journals is critically dependent on the background knowledge of the people involved with the assessment of the manuscript. Reviewers have a frame of reference from their training, education, and experience within which they interpret all new data. The experts who provide peer review and assist editors often know much regarding the background information on which a manuscript is based, but they now are presented with data in a new context. Really new concepts are rare and often are said to be “out of the box.” This phrase recognizes that we usually think inside a box that is delineated by our experience. In this issue of the *Journal*, as is customary, there are articles describing very early work. I assume the editors had to think long and hard to establish priority for these articles compared with others received.

One of these articles in this issue of the *Journal* uses a carotid intra-arterial electrode catheter for sympathetic nerve stimulation (SNS) to the heart with the goal of studying the effects of this intervention on cardiac hemodynamics and electrophysiology (1). This appears to follow on prior work from this group exploring the effects of transvenous catheter stimulation of cardiac parasympathetic nerves in humans (2). The results of the current work in swine are of such magnitude that one may find them surprising. The systolic arterial pressure increased acutely in a graded fashion up to 100% from baseline during SNS while peripheral vascular resistance was unchanged. However, SNS provoked atrial fibrillation in many animals, and SNS in the left carotid caused accelerated idioventricular rhythms as well. Could this eventually be a therapy for acute and severe left ventricular failure and cardiogenic shock? I assume that the reviewers of this article could properly judge the construction of the experiment and the analysis of the results, but they cannot know all one would wish to know to fully evaluate and understand the basis for, or the clinical implications of, these findings. I imagine the editors had differences of opinion about the importance, and thus the priority, that they should give such an article. I further imagine that some around the table thought this was an impractical idea that might never be useful clinically whereas others thought this development was so important that it must have higher priority. We know how the

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discussion came out in this case because the article is here for all to see.

Exposure of really new approaches to clinically recognized problems is one of the important functions of journals such as this one. Editors have no crystal ball to allow them to predict which among the manuscripts with “crazy” ideas will turn out to be the basis for a great advance in patient care. Authors naturally gripe about this somewhat imperfect process when their own work is not accepted. However, I think we need to step back occasionally and commend the editors and reviewers for sorting through these difficult issues, making the positive decision to take chances on far-out ideas and bringing these challenging manuscripts to publication. I accept that those involved in the process of judgment do not have easy, reproducible answers to the issues of scientific understanding of “out of the box” ideas testing that they can reproducibly apply in the present and future. Nevertheless, many would agree with me that the huge effort expended in evaluation of such innovative work is justified. The cardiovascular community, including the commercial partners of that community, can be proud of the

improvements in the quality and duration of life for our patients that have come from their innovations over the 40 years of my medical career. “The game is worth the candle.” We should continue to read this *Journal* with the expectation of finding the newest ideas and the most innovative methods that point us to future advances in cardiovascular patient care.

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