Brownlie and Saren (this issue) observe that few innovative papers find their way into leading marketing journals. They place much of the blame for this state of affairs on the reward structure found in academe. I agree with them. Marketing academics are governed by the "publish or perish" motto, a short-run orientation (decried in the classroom, ironically) that is inimical to knowledge development.

Ideally, as Tuckman and Leahey (1975) indicate, publications allow faculty to demonstrate innovative scholarship, disseminate ideas, solve problems, and so on. Publications also garner perquisites of a more pragmatic nature, such as salary increases, promotion, enhanced job mobility, tenure and the like. With regard to the economics profession, Tuckman and Leahey calculate that the lifetime returns to publication of the first article, assuming a 5% discount rate, range from $12,340 for an assistant professor to $10,256 for an associate professor, to $6,958 for a full professor. There is every incentive to be published.

Nevertheless, it is hard to refute the AMA Task Force's (1988, p.6) assessment that the publish or perish mentality "...has led to conditions under which a significant contribution to knowledge may not be at the forefront of most participants' thoughts as they engage in the research and publication process". The overwhelming need to publish can be expected to divert attention from the study of important and innovative marketing topics to those that are more likely to end up in print (Hubbard and Armstrong 1994). Churchill (1988), for example, notes that academicians seem to be more concerned with applying increasingly sophisticated methodologies to increasingly less important issues. Indeed, the researcher desirous of publication would be well advised to heed Armstrong's (1982) author's formula and: (1) not pick an important problem; (2) not challenge existing beliefs; (3) not obtain surprising results; (4) not use simple methods; (5) not provide full disclosure; and (6) not write clearly.

What can be done to increase the publication incidence of important, innovative works in marketing? Three suggestions, from a far lengthier list of possibilities, are offered below.

First, it is necessary to develop alternative evaluation procedures for the tenure and promotion of faculty. These changes should be designed to provide sufficient time for faculty to work on important, as opposed to publishable, topics. The goal is to get away from counting the number of publications, and to focus instead on determining their quality and impact on the field. For example, W.A. Phillips (1958) produced few papers in his career at the London School of Economics, but one of

1Correspondence to be addressed to: College of Business Administration, Drake University, Des Moines, IA 50311, USA.
them gave rise to the “Phillips Curve”. Similarly, as Broad (1981) informs us, when J. Watson was promoted to the rank of Associate Professor at Harvard in 1958, he listed 18 papers on his c.v. One of these, published in 1953, described the structure of DNA. Today, Broad contends, an individual seeking promotion in this field may list 100 papers. Admittedly, these examples are extreme but they are used to illustrate that publication counts, so in vogue in academe, are by themselves an inadequate basis for measuring the importance of someone’s work. If any counting is to be done, it should be of citations of an individual’s work. While certainly imperfect, citation counts offer a better measure of the researcher’s impact on the discipline than publication counts (Hubbard 1992).

Second, more recognition should be afforded to the value of scholarly monographs. The monograph offers the potentially ideal vehicle for conveying innovative, important, and controversial ideas. Unfortunately, the pressure to publish lots of refereed journal articles frequently means that scholars do not have the time to write such monographs. If they do choose to do so early in their careers, they are unlikely to be tenured and/or promoted. Later in their careers they may be too burnt out. Under the present academic research-publication system, it is all too easy to imagine Keynes being admonished at Cambridge for “working on that damned book”, when his journal output was lackluster. Too bad.

I confess that the above recommendations would be difficult (impossible?) to implement. On the other hand, for hundreds of years universities have made prodigious contributions to knowledge without the whip of publication counts.

Third, and more readily achievable in principle at least, is the need to improve the quality of manuscript-reviewing. It is a notoriously unreliable procedure (Cicchetti 1991). Some reviewers take their job seriously and it shows in the quality of their work. It is just as obvious, however, that many others do not. Furthermore, as Brownlie and Saren note, the reviewing process tends to be adversarial in nature. The AMA Task Force (1988, p.14) goes so far as to call it “masochistic”.

Incentives could be offered to improve the quality and civility of the reviewing process. Since monetary incentives are unlikely to be lucrative enough to accomplish this, why not consider offering members of journal editorial boards a substantial reduction in their teaching loads. Moreover, the number of reviewers assigned to a paper should be reduced from the three, and occasionally four, now commonly used. As Churchill (1988) remarks, editors can usually make decisions with two reviews.

Finally, determining what constitutes innovative scholarship is not necessarily a simple matter. But for goodness sake don’t let the US National Academy of Sciences make that decision. In a Newsweek obituary for Dr. Jonas Salk, creator of the first polio vaccine, it was observed: “...[T]he scientific establishment never embraced Salk. He was never elected to the National Academy of Sciences; academicians sniffed that his work was not ‘original’” (Begley 1995, p.63). Only in academe!

References
