

# Surgical Research: Master or Servant

H. A. F. Dudley, ChM, FRCS(E), FRACS, FRCS(Eng), London, England

*.... the deeper meaning of learning stays the same: it is the ability to convey to others by speech and writing a great variety of facts, thoughts and ideas with clarity and precision. It is of no avail to make the most decisive observations, to perform the most elegant experiments, or to make measurements of great accuracy if one is unable to convey the information precisely and concisely to other folk of like interest.*

G. W. Pickering [1]

Most of us will associate Bert Dunphy with wound healing, the bon mot, Anglo-American friendship, and a host of other warm attributes that have endeared him to the surgical community in all parts of the world. Yet beneath the bonhomous exterior has always been a laser-sharp critical mind that, while courteously avoiding direct criticism of surgical peers, has touched on major issues of the day with precision and pertinence. Primarily a clinician and endowed with more than the average share of clinical wisdom, he still has had an insight into clinical science that has exposed irrelevance and espoused discrimination. Therefore, it is not inappropriate in this Festschrift to address a problem related to the validity of our work as it is seen by both us and others.

I am concerned that the need to appear at Surgical Research Societies and to publish in the surgical literature may drive our research apparatus more than does an underlying desire or conviction of the value of research per se. As might be expected, the evidence I have accumulated does not permit a clear-cut answer to my worries; such an answer may not exist or my worries may come only from my own mind. However, what I have uncovered does support the idea of a cultural difference between the United States and Great Britain and also possibly that a good deal of work that authors considered worthy of

presenting to a surgical public was ultimately either stillborn or frustrated in its growth or eventually failed to engage its originators' interest. It would be folly to believe that these results should be used to generate formal policies for surgical research, but they could stimulate us to study our motivation and the hidden structure of our professional biases. I would like to think that when he reads this, Bert Dunphy would approve of such objectives.

Science, as Cooper [2] has said, "begins only when the worker has recorded his results and conclusions in terms intelligible to at least one person qualified to dispute them," and if, as I believe, we should accept this definition, then communication is an essential part of science and, within reason, is worthy of study in its right [1].

Extensive anecdotal literature on publication in science might well be summed up by two oft-quoted yet still quotable phrases: the first, "publish or be damned" (a plagiarisation of the first Duke of Wellington's "publish and be damned," which of itself was a splendid double entendre rebuke to one who was threatening him with libel); the second, "publish or perish" (the origin of which I have been unable to trace). Indeed, fury of publication is often a disease that grips us so that the publication urge far outruns the purpose of, and quality in, the message to be communicated. Like it or not, the convention that personal bibliography enshrines personal achievement drives many on to frenetic activity so as to accumulate an array of published work to be proudly displayed on bookshelves, added as appendices to curriculum vitae, or flaunted to unwilling recipients by the dispatch of unsolicited reprints which, in my case at least, flop around (usually unread) on the desk before being slipped quietly, if somewhat shamefacedly, into the trash can.

We can sense, therefore, that there is much unnecessary communication despite the filtering effects of selection committees, editors, and their advisors.

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From the Academic Surgical Unit, St. Mary's Hospital, London, England.

However, what of the reverse which is at least as important as a determinant of the impact of science through the medium of written communication? One question which I have set out to try and answer is what proportion of apparently "good" research in surgical biology fails to see the light of day and if in any way this might be related to the overall level of "productivity" (the will to survive through publication) of those who do it. I do not claim that this or any other experiment which endeavors to unravel the mysteries of communication in the sciences can yield conclusive results, but it is of some interest to make a preliminary analysis in the field. The current study was prompted by a previously unpublished investigation of Denis Halmagyi into the records of the Surgical Research Society of Australasia, which had shown that only 42 per cent of papers delivered to that organization ultimately achieved publication. Furthermore, many of these were in minor journals.

### Hypothesis

The Surgical Research Society of Great Britain (SRS) has in the past been the major forum for the early presentation of British work in surgical biology. Papers are accepted for meetings twice a year after perusal by a peer scrutiny committee and should thus in theory represent the better work being pursued in the field. Consequently, preliminary results in abstract form should reach definitive publication frequently and quickly. Abstracts and authors from a meeting of the SRS in 1972 were collated and a forward search over the next three years of subjects and authors was made in the *Quarterly Cumulative Index Medicus* to ascertain (1) if the abstract had led to a definitive publication and (2) the productivity as defined by the number of other papers shown by the authors. When joint authorship of two SRS contributors existed, a score was credited to each individual. Clearly, this leads to an overestimation of the number of papers.

It is of some passing interest that an attempt to conduct the search through MEDLARS was unavailing. Eventually, MEDLINE was used to retrieve the items identified by hand.

### Results

Fifty-one papers were delivered. Twenty-nine were published in full; four (all published) originated in the United States and were analyzed separately. Scrutiny of the unpublished papers failed to reveal that they more resembled "work in progress" than those published. In three years 107 non-American

authors had produced 896 papers, but when overlap was eliminated this figure fell to 743. Subsequent publication in the United States was 186, a nearly threefold difference. In 1970 the modal number of authors was three; scrutiny of a randomly selected group of SRS abstracts in 1975 showed that although there had been a small increase in papers with more than five authors, this was not significant (Kolmogorov-Smirnoff test,  $p > 0.2$ ). Identifiable professors were nominally associated with ten of forty-seven papers in 1970 and twenty-two of fifty-nine in 1975, a suggestive but also statistically not significant difference ( $\chi^2 = 2.47$ ;  $0.25 > p > 0.1$ ).

### Comments

It is hard to know how to interpret these results. Perhaps the best thing is to say: (1) that, as in the SRS of Australasia, a considerable number of papers (greater than 40 per cent) remain unpublished and thus must be regarded, at least in part, as stillborn work; (2) that there is a greater drive in the United States overall to publish and that the survival productivity of professors in Great Britain was possibly influencing their behavior over the half decade studied. As to the fact that a proportion of papers remained unpublished, the interpretive field remains wide open: either the work reported at an early stage was stillborn (as I have already suggested and as much science must be), or the authors were unable to summon the energy to complete it, or possibly that peer review or journal committees condemned it. The last is a vexing problem which has been extensively debated. My belief is that there should be a two tier system of evaluation—the first tier to assess the validity of the approach in terms of experimental design, presentation, and analysis, and the second tier to assess relevance and/or quality in terms of new knowledge. Perhaps the two should be reversed so that relevance comes before the rather rigid criteria of scientific probity, which our training drives us to apply but which also may eliminate the exciting and unorthodox. As Jones [3] has said, there may be an inverse law (perhaps with a power function) to relate novelty and pertinence to the probability of publication.

Another matter which is opened up by this study is that of authorship. I have discussed this in detail elsewhere [4] but it can still be regarded as open to debate. In North America it is customary for departmental heads to associate themselves with publications from their department, whatever their personal involvement. In the United Kingdom this practice is perhaps a little less common. The ad-

vantage is that recognition of the author and therefore (perhaps) the probity of the work may be easier. The disadvantage is that it may diminish the apparent involvement of those others who have really done the work. There is a delicate balance to be struck here which few of us properly appreciate, and all I am attempting to do is focus on the problem.

Finally in a more general sense, the small example I have quoted here of investigation into the matter of publication emphasizes our ignorance of the network of work-delivery-publication in surgical biology. Perhaps, in that attempts to structure behavior can be constricting, we should not study ourselves too much, but perhaps also more precise understanding of motives and objectives could eliminate much of transient worth (or no worth at all), could instruct future generations in better communication, and in the economic jargon of the day, give us a more cost-effective structure.

Nothing that I have written here is meant to de-

stroy the personally held view of many of us that research and writing are both fun. I know that Doctor Dunphy would hold to that idea. Indeed, they are complementary enjoyments because one must lead to the other. Both require effort—the need to scorn delights and live laborious days; both may create high optimism and deep pessimism during their generative phase, and both are among our potentially most important achievements and, as such, if I may return to Milton, are “the spur the clear mind doth raise.”

#### References

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4. Dudley H: The Presentation of Original Work in Biology and Medicine. Edinburgh, Churchill Livingstone, 1977.