Value for money

Sir — Scientists are currently judged by the number and quality of their publications[1]. Using such criteria, Robert May's "Review of world science"[2] finds that the economically developed nations fare well, whether the results are expressed in terms of citations per person or a relative citation index. In today's economic climate, he suggests that the United Kingdom obtains the "best value for money" at 168.2 citations per million pounds sterling. Such a figure implies that each publication costs £5,945 or US$9,530. May further suggests that better value for money is associated with university institutions.

Several of my South African colleagues in the life sciences at university institutions have provided me with their average costs per publication. These range from $500 to $2,400. The average is $2,000. This is not a very realistic figure because chemicals are purchased from outside South Africa and are subject to 10% import duty and 14% value-added tax. So the real cost for each publication is $1,520.

Sceptical of the quality of these publications? These colleagues are publishing in journals with impact factors from 1.7 to 5.2. This suggests that we are producing 6 papers for each produced in the United Kingdom, and 7 for each produced in the United States. If we include the cost of the manpower involved, we arrive at 24 and 28 publications respectively for the same investment — South African lecturers receive $14,000 a year (less than a quarter of salaries on offer in the United States as judged by classified advertisements). I should like to suggest that funding agencies in Europe and the United States should invest in South African science in our universities for real "value for money".

J. P. Dean Goldring
Department of Biochemistry,
University of the Witwatersrand,
1 Jan Smuts Avenue,
Johannesburg, South Africa
e-mail: 089goldr@cosmos.wits.ac.za

1. Lewison, G., Anderson, J. & Jack, J. *Nature* **377,** 671 (1995).
2. May, R. *Science* **275,** 793–796 (1997).

History of parity violation experiment

Sir — The Commentary article[1] "Parity and chivalry in nuclear physics" refers to the two first publications demonstrating the non-conservation of parity in the weak interactions[2,3]. The article is summarized by the subhead "Forty years ago, the world of physics was stunned by the discovery that nuclear beta-decay does not respect symmetry between left and right. But the credit for this conclusion has not been properly attributed." The purpose of the Commentary was "…to state for the record that the NBS [National Bureau of Standards] parity violation experiment was a collaborative team effort in which nuclear physicists and cryophysicists pooled their knowledge and expertise to carry out an experiment proposed by Lee and Yang, thus confirming their hypothesis that parity is not conserved in β-decay".

We have always regarded this epochal experiment as a team effort. When we wrote in 1957, "[w]e are also indebted to Professor C. S. Wu for reports of her preliminary results in the Co-60 experiment which played a crucial part in the Columbia discussions immediately preceding this experiment", we did not intend to apportion credit among the authors of the Letter reporting the Co-60 results. Our own experiment, "Observations of the failure of conservation of parity and charge conjugation in meson decays: the magnetic moment of the free muon", was spurred by Wu's Friday-lunch report of the status of the Co-60 experiment and was performed that Friday night, 4 January 1957, to

Tuesday morning 8 January, and our Letter was written that day. Had we heard the word from Ambler, we would have thanked "…Dr E. Ambler for reports of his preliminary results…" and we could as well have written the phrase without the "her". As for the 1978 Wolf Prize to Wu, citing the Co-60 experiment as "her most famous work", a prize for lifetime achievement awarded to Ambler could properly cite the same experiment as "his most famous work".

Wu died in New York on 16 February 1997. At a memorial service on 22 February, every speaker gave credit for the Co-60 experiment both to Wu and to the NBS team. For instance, C. N. Yang said: "In 1956 she and her collaborators at the NBS did one of the most exciting measurements …"; and T.D. Lee: "Forty years ago she and her colleagues at NBS overthrew the principle of conservation of parity."

In 1973, Wu herself published a marvellously informative and warm discussion of the Co-60 experiment[4], followed by a briefer presentation by one of us of the meson experiment[5],and also by V. L. Telegdi[6].

In that account, Wu details the origin of the Co-60 parity experiment in early spring 1956 when T. D. Lee described to her the possibility that the τ-θ particle decay anomaly could be due to the violation of parity conservation in the weak interactions. Wu suggested to Lee that the best bet for testing this hypothesis in β-decay was demagnetization-cooled Co-60, and she decided to work full-time on that. In preparing for the experiment, she and her Columbia colleagues immediately remeasured the spin of Co-60 and, on 4 June, Wu called Ambler to find out whether he would be interested in a collaboration; Ambler accepted with enthusiasm, and work began at NBS. Before going to NBS for the first time in mid-September, Wu had done experiments at 4 K on the detector designed to detect β particles from a source at milli-Kelvin temperature, had made detailed studies of magnetic field effects on the β counting and had studied back-scattering from the cerium magnesium nitrate (CMN) crystal source backing.

Wu also grew two Co-60 specimens, one with the β-emitting thin surface layer containing a few microcuries of Co-60, the other with Co-60 throughout the crystal for preliminary studies of γ anisotropy from polarized nuclei. But the surface warmed rapidly because of condensation of residual helium, and the NBS team used a CMN shell to shield the surface. By Christmas Eve, the electron asymmetry was "reproducible and huge" but rigorous checks were still to come. (R. P. Hudson says that according to the log books, this occurred on the night of 26 December, personal communication 13 March 1997.) Wu writes: "The period between January 2nd and January 8th was probably the most tense in our whole experimental venture", but these meticulous experimenters finished all the experimental checks and gathered at about 2 a.m. on 9 January to celebrate the great event.

Kurti and Sutton write: "In those days it was usual to list authors in alphabetical order, unless one was the leader of the team or the originator." It would not be amiss to regard Wu as the originator of the experiment, given the facts as related above. But the NBS team of Ambler, Hayward, Hoppes and Hudson, as well as Wu, were full collaborators and deserve full credit.

Richard L. Garwin
Columbia University,
New York, New York 10027, USA
Leon M. Lederman
Illinois Institute of Technology,
Chicago, Illinois 60616, USA

1. Kurti, N. & Sutton, C. *Nature* **385**, 575 (1997).
2. Wu, C. S., Ambler, E., Hayward, R. W., Hoppes, D. D. & Hudson, R. P. *Phys. Rev.* **105**, 1413 (1957).
3. Garwin, R. L., Lederman, L. M. & Weinrich, M. *Phys. Rev.* **105**, 1415 (1957).
4. Wu, C.S. *Adventures in Experimental Physics*, Gamma Volume 101–123 (1973), from which the information of the next two paragraphs is drawn.
5. Garwin, R. L. *Adventures in Experimental Physics*, Gamma Volume 124–130 (1973).
6. Telegdi, V. L. *Adventures in Experimental Physics*, Gamma Volume 131–136 (1973).