at the same altitude and not too distant. According to Fischbach *et al.* we would expect to find at this second site a difference in apparent weight ΔW:

$$\Delta W = a\,Mg\left(\frac{B_1}{\mu_1} - \frac{B_2}{\mu_2}\right) \qquad (1)$$

Taking a value for a derived from the Eötvös data as given in ref. 1 and masses of one kilogram, the largest value of ΔW that could reasonably be obtained would be for masses of an appropriate stainless steel and lithium hydride (^{6}LiH) respectively, for which $\Delta W \approx 2.5 \times 10^{-7}$ N, equivalent to a 25 μg difference in apparent mass. Replacing the ^{6}LiH by beryllium would reduce the apparent mass difference to 14 μg but would avoid possible dangers involved in the handling of kilogram quantities of ^{6}LiH. If, instead, we take the value of a deduced from the geophysical measurements of G, we find a value of ΔW equivalent to 1.5 μg and 0.9 μg for ^{6}LiH and Be respectively.

The practicality of carrying out such an experiment with a worthwhile accuracy requires first a transportable balance having a reproducibility better than 1 part in 10^9 (equivalent to 1 μg for a 1kg mass). Traditional knife-edge balances, although there are at least two which achieve such reproducibility[4,5], are certainly not transportable so would not be suitable. Such balances are much too delicate and require extreme care in the mounting, handling, and adjustment of the beam and knife edges. However, a flexure-strip balance has recently been described[6] that is much less subject to these constraints. The beam and flexure elements are designed to be assembled without any adjustment and to be relatively robust. Preliminary results indicate that a reproducibility of 5 parts in 10^{10} can be achieved in the comparison of 1 kg masses. A similar performance may eventually also be possible using a hydrostatic balance[7]. Second, we need to be able to construct pairs of appropriate test masses that are not subject to significant differences in air buoyancy or sorption effects. Adequate test masses could be made by enclosing up to about 1.5 kg of the test material (^{6}LiH, Be and stainless steel) in stainless-steel vessels made to have nearly identical external volumes and surface finishes. The stainless steel should be chosen to be one of those already used for stainless-steel mass standards, such as Immaculate V, Nicral D or an equivalent AISI 310 steel, whose magnetic and other properties are suitable. Provided that the external volumes of the test masses differ by only a few parts in 10^4, the differences in their respective air-buoyancy corrections are sufficiently small to be measurable without significant error. Vacuum weighing would of course remove this problem altogether but would, perhaps, introduce further problems due to desorption. It is not difficult to ensure that the values of the masses themselves and the heights of their centres of mass are sufficiently close for the demands placed on the knowledge of g and its vertical gradient to be modest.

Although the quickest tests of the ideas of Fischbach *et al.* could probably be made by repeating the Eötvös experiment using already existing equipment[3], we think it worth while pointing out that an experiment made using a beam balance could give results of ample precision. Indeed, if the force predicted is shown to exist its presence would, among many other things, lead to modifications in high-precision weighing procedures, to say nothing of its implications for the present definition of the kilogram.

C.C. SPEAKE
T.J. QUINN

Bureau International des Poids et Mesures, Pavillon de Breteuil, F-92312 Sevres, Cedex, France

1. Fischback, E., Sydarsky, D., Szafer, A., Talmadge, C. & Aronson, S.H. *Phys. Rev. Lett.* **56**, 1, 3 – 6 (1986).
2. Maddox, J. *Nature* **319**, 173 (1986).
3. Keyser, P.T., Niebauer, T., Faller, J.E. *Phys. Rev. Lett.* (in the press).
4. Almer, H.E. *J. Res. NBS* **76**C, 1 – 10 (1972).
5. *BIPM Com. Cons. Masse:* Document 85/4; *Bull. NRLM* **33**(2), 7 – 18 (1984).
6. Quinn, T.J., Speake, C.C. & Davis, R. *Metrologia* (in the press).
7. Probst, R. & Kochsiek, M. *Metrologia* **19**, 137-146 (1984).

Blym clone could be contaminant

SIR—In their reply to my recent paper[1] Cooper and Diamond[2] raise two points — first, that some of the clones I describe are contaminated by the original *HuBlym*-1 clone and, second, that the results of a single transfection assay, not described in my paper, conflict with other assays reported in the paper.

The original clone described in the paper is one of several isolated and designated as non-transforming (using the transfection assay) and reported by Diamond *et al.*[3]. There has never been any suggestion that this clone, which has the same sequence as *HuBlym*-1, is a contaminant.

It is not possible to exclude the possibility raised by Cooper and Diamond that the other clones described in this paper are indeed contaminants. Unfortunately, the clones, which I left in the laboratory, were disposed after my departure and before the issue of contamination was raised with me. These clones had, however, been designated as transforming or non-transforming in transfection assays read by Cooper and it was precisely because of this classification that they were subsequently selected for sequencing.

Cooper and Diamond cite a transfection assay performed by me in May 1984, when the sequencing of the Burkitt's clones was 90 per cent complete and the cloning and selection of the gene from normal human embryo fibroblast DNA had just started. This particular transfection was carried out at a time when the assay was not working reproducibly and was an exercise in trying to find out what the problem might be. Therefore this result, which conflicted with several other assays carried out previously and read by Cooper, cannot be considered in any way accurate or meaningful.

It is gratifying that Cooper and Diamond confirm my conclusion regarding the nucleotide sequences of *HuBlym*-1.

JOAN DEVINE

Celltech Ltd, 244-250 Bath Road Slough, Berkshire SL1 4DY, UK

1. Devine, J.M. *Nature* **321**, 437-438 (1986).
2. Cooper, G.M. & Diamond, A. *Nature* **321**, 438 – 439 (1986).
3. Diamond, A., Cooper, G.M., Ritz, J. & Lane, M.A. *Nature* **305**, 112–116 (1983).

Writing enzyme units in the correct way

SIR—There seems to be a great deal of ambiguity and a frequent scientific error in the literature when the specific activity of enzymes is reported. Very often, it is expressed as "nmol/min/mg", as "nmol/min per mg" or as "nmol per min per mg". (See, as examples, refs 1–7, but many others could be cited.) Phonetically it seems correct but I believe it to be mathematically incorrect. It should be expressed as "nmol × min^{-1} × mg^{-1}".

The expression "$a/b/c$" is indeed ambiguous, because

$$(a/b)\,/c = \frac{a/b}{c} = \frac{a}{b\times c}$$

and

$$a/(b/c) = \frac{a}{b/c} = \frac{a\times c}{b}$$

therefore, in the linear form "$a/b/c$", the use of parentheses is obligatory.

To express the term

$$\frac{a}{b\times c}$$

it is necessary to write $a\times b^{-1}\times c^{-1}$ or $a/(b\times c)$.

And similarly, to express the term

$$\frac{a\times c}{b}$$

we must write $a\times c\times b^{-1}$ or $a/(b/c)$.

The formula that is derived for enzyme activity is defined correctly as "nmol × min^{-1} × mg^{-1}" or as "nmol/(min × mg)". The common expression "nmol/min/mg" is therefore ambiguous.

The same holds true for other units expressed in a similar formal way, such as mutations/cell/hour and disintegrations/min/tube. I urge the adoption of the correct designation for specific activity of enzymes, and similar units.

MORENO PAOLINI

Istituto di Farmacologia, Università degli, Studi, Via Irnerio, 48–40126, Bologna, Italy

1. Rifkind, A.B. & Muschick, H. *Nature* **303**, 524–526 (1983).
2. West, D.C., Hampson, I.N., Arnold, F. & Kumar, S. *Science* **228**, 1324–1326 (1985).