Profiting from biodiversity

SIR—Many people see biodiversity as an expensive luxury which we either have a duty to pay for or simply cannot afford. A glance at almost any first-world dinner table, covered with food species from around the world, shows this is nonsense — biodiversity is an immense economic resource. Profitable and sustainable economic exploitation of biodiversity is perhaps the best way to ensure its maintenance. We recently attended a conference at Roros, central Norway in which Dan Janzen described a programme to turn Costa Rica into a country which profitably manages and exploits its biodiversity.

The first step is to take inventory. Janzen spends six months a year training 'parataxonomists' — former farmers, other rural workers and students — to provide the raw material for Costa Rica's National Biodiversity Institute. The institute itself, a private, non-profit making, public-service organization, integrates information on resources from the flora and fauna of the national parks which constitute more than a quarter of Costa Rica's land surface area with a view to exploiting it economically.

After training, the parataxonomists leave for designated patches of park to compile collections of local organisms. The collections are housed and handled at the institute by in-house curators. As the samples are identified to finer taxonomic levels, the curators call on expertise from abroad. The inventory already includes at least one potential moneyspinner. Janzen showed us a plant which, he claims, is rich in antibiotics. He refused to tell us the plant's natural location, as the intention is to develop its potential so that the economic benefits are channelled back into the preservation of Costa Rican biodiversity.

Janzen estimates that the contents of the major national parks should be catalogued within a decade: the first two classes of parataxonomists, comprising 33 students, are now working in the country's eight conservation areas. A local and regional infrastructure is now in place for the later stages of Costa Rica's biodiversity programme, which will include more fundamental research as well as commercial exploitation.

Who is paying for all this? The Costa Rican government, although an integral part of the enterprise, is not wealthy. Initially, a series of institutions, including private foundations, private donors and national governments, provided $2.53 million for the first two years of operation. In the long term, one scheme is that initial capitalization of $30 million would purchase $120 million of Costa Rican international debt. The Costa Rican government would then buy the $120 million by paying $96 million to a biodiversity institute trust fund, which would pay 3% annual interest in local currency.

The National Biodiversity Institute represents a different attitude towards third-world biodiversity than that which characterizes the developed countries, where biodiversity tends to be viewed as a common heritage of mankind. This belief must be eliminated or else biodiversity will suffer the tragedy of the commons, with everyone trying to make a quick profit but unwilling to invest in sustaining the resource. If Costa Rica benefits economically from its biodiversity, then Costa Ricans will have an economic interest in the sustainable exploitation of their own natural resources. If successful, the institute will provide a model for similar schemes elsewhere.

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Ellipses and ellipsis

SIR—Klaczko and Bitner-Mathe in their Scientific Correspondence[1] fit ellipses to wing outlines of *Drosophila*. The fit is impressive and they suggest that ellipse shape can be used to quantify wing shape.

But the same ellipse can exactly fit outlines of very different shapes (Fig. 1). Such ambiguity arises when outlines are not closed, so that only part of the ellipse need fit. Klaczko and Bitner-Mathe digitize the wing tip and front margin (from A to D in Fig. 2). Although open, this portion is evidently sufficient to fit an ellipse representative of the entire, almost closed, wing periphery. But we may be interested in the open outline AD *per se* (developmentally, this and the hind margin belong to different compartments). In that case, to avoid equating dissimilar outlines requires two further parameters specifying which arc of the ellipse is used — for instance parameters θ and $\cos^{-1}(c/d)$ in Fig. 2 (top).

Unfortunately, although in combination ellipse shape and these parameters specify fully the shape of an ellipse arc, each parameter remains difficult to interpret when the others are changing. Unless parameters can be chosen to match observed limits to variation, a change in outline recognizable as a single, simple distortion may affect all ellipse parameters in a confusing, nonlinear way. An ellipse arc geometrically intermediate between another two arcs need not have intermediate parameters; if parameters are intermediate, the arc need not appear geometrically intermediate (Fig. 3). Nor is the panacea to enter all the ellipse parameters in a multivariate analysis: we should not expect linear combinations to disentangle trigonometric inter-relationships.

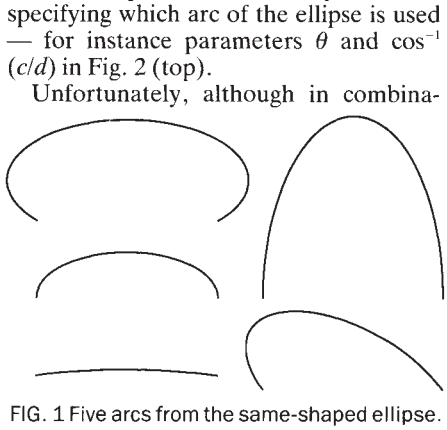

FIG. 1 Five arcs from the same-shaped ellipse.

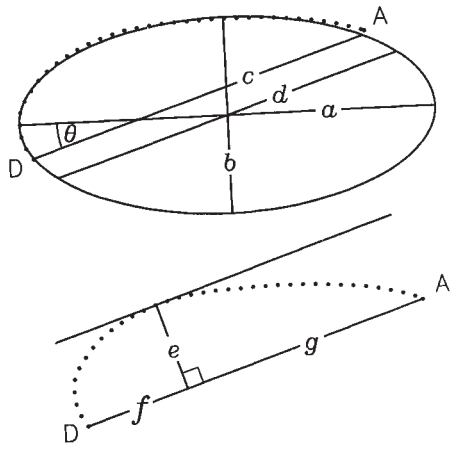

FIG. 2 Top, The dotted line AD represents the digitized wing outline, to which Klaczko and Bitner-Mathe fitted the ellipse. The line of length *c* cuts off the ellipse arc adjacent to the outline. Bottom, A simpler geometrical construction.

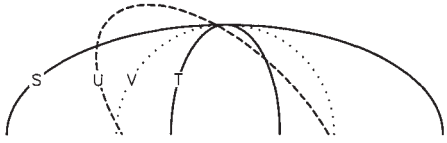

FIG. 3 Ellipses S, T and U share the same value of b/a and c/d, and U is intermediate in values of $\sqrt{ab}$ and θ. Ellipse V differs in b/a and so is not intermediate between S and T for this set of parameters.

A few-parameter summary of shape may be more comprehensible if based simply on relative distances between homologous landmarks and extremum points (see parameters $\log[e/(f+g)]$ and $\log[g/(f+g)]$ in the bottom part of Fig. 2). Alternatively, as wing venation provides a wealth of unambiguous landmarks, more advanced mathematical tools can be used to analyse shape variation[2].

Nevertheless, ellipses have been sensibly used to quantify biological shape[3]. Sampson has developed[4] a fitting algorithm unaffected by the outline's orientation which generates confidence limits summarizing variation between outlines. The technique also allows ellipses to be constrained, for instance to lie parallel to

two landmarks. Each constraint discards information, but can make the remaining ellipse parameters easier to interpret.

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Crucifixion date

SIR—We have suggested[1,2] that the most probable date for the crucifixion was on 3 April in AD 33, in part basing our claim on a lunar eclipse visible from Jerusalem on that evening. However, Clive Ruggles in News and Views[3] discussed a paper by Schaefer[4] claiming that this eclipse would not have been visible from Jerusalem. But there are several errors in Schaefer's work, so we do not think our conclusion needs to be revised.

We found that the eclipse of 3 April in AD 33 was visible from Jerusalem at moonrise: it rose with 20 per cent of its disk in the umbra and the remainder in the penumbra. The ancients, however, made no distinction between the umbral and penumbral shadows with the result that to the casual observer about 57 per cent of the Moon's disk would have been perceived as being 'in eclipse' at moonrise. Schaefer disputes this, mantaining that the rising Moon would first have become visible when only 1 per cent of its disk was still in the umbra and so the eclipse would have gone unnoticed.

The visibility of astronomical phenomena close to the horizon is determined principally by the amount of aerosol scattering in the line of sight. In estimating this, Schaefer takes the altitude of Jerusalem to be 450 m above mean sea level. But the altitude of the old city is typically 775 m. Moreover, his correction factor for the effects of relative humidity is anomalously high. These two errors alone result in the amount of aerosol extinction at the horizon being overestimated by a factor of more than 700.

We would expect the equivalent of any astronomical phenomena seen from present-day Oxford to have been easily seen from ancient, pollution-free Jerusalem: the last three lunar eclipses visible from Oxford were all observed under less than ideal conditions at times when the Moon's altitude was considerably less than the value that Schaefer maintains is required for the Moon to be seen. Moreover, Schaefer's analysis denies the possibility of the simultaneous visibilty of the Sun and eclipsed Moon as a result of atmospheric refraction — a phenomenon that has been known since the time of Hipparchus. Schaefer's analysis, based in part on a single observation of a lunar eclipse setting through the centre of the anthropogenic haze layer of Washington, DC, relies on recent measurements which are degraded by atmospheric pollution. We do not believe that the visibility conditions in ancient Jerusalem and modern-day Washington can be compared.

All calculations of ancient eclipses must take into account the cumulative effects of the inconstant rotation of the Earth due to effects such as tidal friction, for which we have adopted the results of Stephenson and Morrison, who analysed[5] ancient astronomical observations. Schaefer estimates the required eclipse parameters by averaging several disparate eclipse calculations — among which at least one is defective and another is known to be incompatible with the well known eclipses of classical antiquity. After eliminating these two calculations from the set used by Schaefer we find excellent agreement with our own work (which Schaefer has misquoted).

At last umbral contact the Moon is still visibly in eclipse to the casual observer (Schaefer's analysis takes no account of this) and, as a result, the eclipse of 3 April in AD 33 would have been perceived by the general populace as continuing until about 51 min after moonrise. We therefore reaffirm that the partial lunar eclipse on that day would have been easily visible to the casual observer in Jerusalem. We have shown[1,2] that this is the most probable date of the crucifixion and given textual evidence referring to a lunar eclipse following the crucifixion. Schaefer's paper[4] does not provide grounds for doubting this conclusion, which is based on the best available estimate of the clock error due to tidal friction[5] and realistic values for the atmospheric extinction coefficient. We will provide a more detailed response to Shaefer's paper elsewhere.

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Scientific Correspondence

SCIENTIFIC Correspondence is a relatively informal section of *Nature* in which matters of general scientific interest, not necessarily those arising from papers appearing in *Nature*, are published. Because there is space to print only a small proportion of the letters received, priority is usually given according to general interest and topicality, to contributions of fewer than 500 words and 5 citations, and to contributions using simple language.

If new results are being described, priority is generally given to communications that do not describe work in which the author is involved. Authors of contributions of this nature should explain in a covering letter why theirs has a particular claim on *Nature*'s space. Contributions may be sent to referees and, in the case of matters arising from material published in *Nature*, are sent to the author of that article for comment. A more detailed guide to authors is available from Washington or London. □

Vanishing authors

SIR—Cherry, in his News and Views article[1], gives a good account of the "case of vanishing neutrinos", but by referring only to Bahcall and Bethe for its interpretation, he creates a "case of vanishing authors". The significance of the third, or small-mass-difference, MSW solution was recognized long before the preliminary SAGE results were announced. J. M. Gelb and I were the first to describe its physical properties and to emphasize that it could yield a very small signal in gallium[2]. E. W. Kolb, M. S. Turner and T. P. Walker independently arrived at the same conclusion[3] and our numerical results were cast in analytical form by W. C. Haxton[4] and by S. J. Parke[5]. Other authors refined and extended this work.

In August 1988, the Kamiokande II team announced its first measurement of 0.46 ± 0.15 for the fraction of solar neutrinos detected versus the standard solar model prediction. Gelb and I pointed out[6] that the central value fell within the narrow range of values predicted by the third solution, but well outside the predictions of the high-mass solution. Unfortunately, the error was too large for us to draw a definite conclusion.

We did observe, however, that were the error cut in half and the central value left unchanged, then the high-mass solution could be eliminated and gallium could be used to choose between the other two. With the new results from Kamiokande II and SAGE, this is exactly what has happened.

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■ *Nature* severely restricts the number of citations in News and Views articles, which on this occasion accounts for the absence of reference to these papers in Dr Cherry's article.