The Great Pyramids of Canada

SIR — High unemployment is often conceded to be a pressing social problem, but its costs can be hard to visualize. The unemployed can become practically invisible to the wider society, and the social costs in crime rates, health expenditure and marriage breakdown are masked by the influence of other variables. But Ian Stewart's comments[1] on Stuart Wier's study[2] of the workforce needed to build the Great Pyramid of Khufu at Giza suggests a way of visualizing the magnitude of unemployment.

Suppose that, instead of sitting around feeling useless, the unemployed were *doing* something useless — such as building pyramids using only hand labour. Minimal capital equipment (for example chisels) would be required and the subsistence costs of the unemployed would be borne. Measured in 'pyramid equivalents', or 'khufus', how large is excess unemployment in the 1990s?

There is always *some* unemployment — so one should not count all of the unemployed as potentially available for pyramid construction. If we take Canada as an example, a conservative estimate of the 'natural' rate of unemployment would be 7.5% (the actual rate in 1989). Unemployment in the 1990s has been substantially higher (11.3% in 1992, 10% in October 1996), largely because monetary policy has aggressively pursued zero inflation, using high interest rates to restrain aggregate demand. The cumulative difference between actual unemployment from 1990 to 1996 and a 7.5% rate of unemployment amounts to approximately 2.7 million person-years of potential work — on average, about 450,000 person-years annually.

How many khufus is this equivalent to? Wier estimates that between 8,300 and 10,600 men working full-time for 23 years could have built the Great Pyramid. If one takes the higher estimate (243,800 person-years), and assumes a five-day working week, a month's vacation and ten holidays per year (implying 223 working days, rather than 365), a total of 379,500 person-years would be needed. Even without modern machinery, fewer people would probably be required today, as the Canadian unemployed of the 1990s are significantly better nourished, and of larger stature, than labourers of 4,600 years ago.

Furthermore, as Wier points out, "the manpower for moving stone blocks from the quarry to the point of installation is the largest single factor in pyramid construction", and presumably in the 1990s one might not choose to use the wooden sleds lubricated by clay and water that ancient Egyptians had to pull.

If, however, historical authenticity were the determining factor in choice of construction method, Canada's excess unemployed could have built about 1.2 khufus annually, or seven between 1990 and 1996. Lined up in a row, this would surely be an impressive memorial to the economic policies of the 1990s.

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Theoretical papers

SIR — I should like, from my own experience working in ecology, to draw attention to problems with the refereeing of theoretical papers. Comments by Fred Hoyle[1] in his biography suggest that similar problems exist in other areas of science.

Three of my papers were rejected by the first journal to which they were sent, but then accepted by a second journal of comparable quality. The first paper[2] was a reanalysis of previously published data using multivariate methods, and the other two were straight ideas papers[3,4]. These latter two were rejected by the first journals (in one case apparently without review) but received encouraging referees' reports after submission to the second journals, in one case with an invitation to produce a longer paper dealing with related topics.

As has been pointed out by the editors of a leading ecology journal[5], the problem with many innovative theoretical papers is to distinguish between papers that are ahead of the field (tomorrow's classic papers) and those that are simply wrong or unoriginal.

Two morals can be drawn from this story. First, theoretical papers should receive lighter refereeing than data-based papers, because it is more important that an interesting idea gets into print than it is to exclude poor ideas. Errors in a data paper may be misleading to future workers, whereas most scientists rightly treat the work of theoreticians with some scepticism and are therefore less likely to be misled by a poor theoretical paper. Theoretical papers probably receive harsher refereeing at present than do data-based papers.

Second, young theoretical scientists should not take a rejection as final. If they believe their work is good, they should resubmit it to another journal. The second set of referees' reports are often so different from the first that they might be thought to refer to a completely different paper.

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Fact and fission

SIR — C. F. v. Weizsäcker[1] quotes Lise Meitner as saying that the idea of "radium isotopes" being obtained from slow neutron bombardment of uranium was "nonsense". According to Weizsäcker, several radium isotopes were reported by Hahn and Strassmann[2] when they checked a finding by Joliot (although this appears to be a myth, as Hahn and Strassmann make no mention of the Joliot work in their report). The appearance of radium isotopes led to a paradox: it appeared to require emission of α-particles from uranium bombarded by slow neutrons with energies well exceeding those of the naturally emitted α-particles, in contradiction of earlier experiments in which no evidence for such "long-range" α-particles was found[3,4].

Scientists who disbelieve a finding and have the means at their disposal to disprove it usually try to do so. As Meitner stayed in touch with Hahn after she fled from Berlin[5], her scepticism may have spurred Hahn and Strassmann to attempt to disprove the evidence for the radium isotopes, and in this they succeeded. In a contrasting situation, when Irène Curie and Savitch[6] found an indication of an isotope of lanthanum having a half-life of 3.5 hours in the "pre-fission" days, it was Hahn who disbelieved the evidence and is reported to have teased Joliot that he should teach his wife chemistry[7]. Hahn and Strassmann then tried to disprove the existence of the lanthanum isotope, but instead confirmed it.

Important discoveries are often surrounded by myths. We now learn that Hahn telephoned Weizsäcker to tell him of the discovery of barium. This destroys at least the myth[5] that Hahn did not tell the Berlin physicists of his discovery before publication while informing Meitner, thus giving her and Otto Frisch an "unfair" advantage in proposing the fission model!

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