

Public support for medical research

Sir — A majority of the American public — not just a select few in the Congress (*Nature* 385, 375; 1997) — support doubling funding for medical research in the United States. Public opinion polls, commissioned by Research!America and conducted by the Charlton Research Company between 24 January and 6 February found that 60 per cent or more of the respondents in Alaska, Louisiana, Ohio and Wisconsin favour the idea of doubling total US national spending on medical research by the year 2002.

Not by coincidence, similar polls conducted in the home states of Senator Connie Mack (Republican, Florida) and Senator Phil Gramm (Republican, Texas) showed the majority of their constituents stating that medical research should be made a higher national priority.

The suggestion by Senator Arlen Specter (Republican, Pennsylvania) that his Senate colleagues should “look toward alternative methods of financing” as well as giving the National Institutes of Health a yearly boost is also supported in previous Research!America polls (both national and statewide). More than half of the public agreed to pay \$1 more in taxes per week, \$1 more for each prescription drug, and/or \$1 more per week for health-care insurance if they could be assured the funds would be used to support additional medical research.

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Passing the buck

Sir — We read with concern of yet another scandal in science — the case of fraudulent data in *five* papers on which Francis Collins, head of the Human Genome Project, is the senior author (*Nature* 384, 6; 1996). Although we sympathize with Collins, we find it strange that a scientist initially willing to assume the senior authorship of a paper is later able to lay all the blame on his coauthor when things go awry. Almost inevitably, it is senior authors who get most of the credit for important papers (and sometimes the accompanying Nobel prizes), so why not the blame also? Even more mysterious is Kenneth Ryan's assertion that “it could happen to anybody”. Surely a scientist closely involved in the work published under his or her name should be able to prevent serious deception — after all, in the Collins case, the fraud was uncovered by an anonymous referee

without intimate knowledge of the work.

Reflecting on his own case, David Baltimore has suggested that scientists are “much too quick” to support allegations of scientific misconduct. Maybe the reason is that many scientists know all too well how large laboratory research occasionally operates — with senior faculty members involved only peripherally in continuing work — and extrapolate this general knowledge to controversial cases.

Standard criteria of authorship do not always translate into practice. How else can one explain faculty members who produce numerous papers each year while simultaneously travelling to meetings, delivering lectures and perhaps also consulting, running companies or managing large research organizations? How can one possibly stay abreast of research in the laboratory in such circumstances? It is often difficult for graduate students or postdocs to remind the faculty involved that their grant money does not automatically entitle them to coauthorship; and the name of a senior scientist on the title page may in any case assist a hard-working young researcher in getting a manuscript published.

We believe that authorship must imply a substantial intellectual contribution to a paper or significant participation in the actual research. Indeed, we would like to suggest the convention that a faculty member in charge of a laboratory be acknowledged separately in papers (for example as “Authors X and Y, under the guidance of Z”) unless he or she has made direct and clear contributions to the research being reported.

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Novel paper titles

Sir — Many years ago, my colleague Dr Stuart Warren expressed the opinion that the word ‘novel’ in the title of a paper could usually be replaced by ‘yet another’.

Apply the Warren criterion to the data published in *Nature* by Friedman and Karlsson (385, 480; 1997) and all becomes clear: as the rate of publication increases, there are bound to be many more papers of the ‘yet another’ kind. True novelty remains as elusive as ever.

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The earliest Americans

Sir — Several of the assertions made by Paul Bahn in his review of *American Beginnings* (*Nature* 385, 128–129; 1997) are debatable, and as editor of that book I should like to comment on them.

I do not know whether there is a consensus about when the first people entered America. There are assuredly those who see it as much predating the earliest certain evidence for the Clovis people, the first generally accepted American population. Here, as with other such problems, one simply follows the evidence and the thought that seem most persuasive. My characterization of Clovis as the “founding population” can hardly be seen as “astounding”. It is perhaps the most widely held view.

The “numerous” earlier radiocarbon dates from South America are simply not “widely accepted”, but then neither are they so numerous. The three sites discussed were chosen as examples because, in recent years, they have been held by their advocates to be among the most convincing cases of pre-Clovis occupations. They have been the subjects of much discussion. But to term my “dismissal” of these sites “ill-informed” in the case of Pedra Furada, “unworthy” in that of the Meadowcroft dates and presumably at best inattentive in the instance of Monte Verde — rejecting its “widely accepted date of 13,000 BP” and ignoring “the indisputable human artefacts and features associated with its possible date of 33,000 years BP” — would seem to call into question the credibility of the reviewer rather than that of the reviewee.

That dismissive attitude is said to “permeate even [my] treatment of the Beringian material”. The single cited example is “...the probably very early Russian site of Diring, [which the editor set aside,] claiming it does not ‘have any bearing on the subject of this volume’”. Let it be noted that the age of Diring is variously estimated at 2 million to 500,000 years or less. The propriety of excluding it from a discussion of American origins seems obvious.

(The Guthrie article was included because it presents a unique and invaluable depiction of late Pleistocene large mammal assemblages in interior Alaska. The article is old; the data remain current.)

Finally, I submit that the “balanced and informed view” is in fact here. That Paul Bahn thinks otherwise may reflect a rather superficial familiarity with the problem and, perhaps, accords with his finding the array of archaeological evidence presented, “a dull read”.

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