Race: talented minorities face a 'revolving door'

SIR — Your News story 'Researcher refuses to back down over race case' (*Nature* **447**, 762–763; 2007) calls attention to the courageous stand taken by James Sherley, an assistant professor at Massachusetts Institute of Technology (MIT) who believes that he was denied tenure because of racial discrimination.

Remarkably, although there has long been a high percentage of African-American students at leading US universities — 10% of incoming undergraduates at MIT, for example — very few have so far made it through to tenure. Only about 1% of biology professors at US universities are African-Americans. Although one-third of assistant professors overall may make it to tenure at MIT, hardly any African-American assistant professors have ever done so in MIT's core disciplines of science and engineering. The same seems to be true at most leading US universities, leading to what has been termed a 'revolving door' for even very talented young African-American scientists such as Sherley, who last year won a National Institutes of Health Pioneer award for innovative work.

How can it be that white and black scientists who initially seem equally talented have such different chances of making it to tenure? I would argue that it is because present tenure policies are unintentionally designed to prevent the success of even the most talented minority scientists.

At places like MIT, only a fraction of faculty make it, even if they're white. In the face of pervasive racial barriers, how can talented minorities have a fair chance in such a steeply competitive timed-tenure system? These barriers can include lack of equal space and resources, lack of mentoring by senior faculty, lack of inclusion in faculty activities such as invitations to speak in seminar series, a general lack of recognition and support, and a hesitancy among white students to join the labs of minority faculty or to be referred to minority labs by senior faculty.

In the face of so many obstacles, how is it fair to argue that Sherley does not deserve tenure because he didn't publish quite as many papers as white assistant professors who did not face any of these barriers? Although the MIT faculty and administrators who have considered his tenure application are for the most part well-meaning, they seem to be unaware of the reality of persisting racial barriers. They unfairly prefer to attribute lack of success to inability.

In a survey of MIT students in 1985, it was found that African-Americans have surprisingly few meaningful faculty contacts, most of those being with the tiny percentage of the faculty who are ethnic minorities. If

minority students and faculty are to be successful, there is an urgent need for universities to re-evaluate and redesign their policies that control retention of ethnic minorities on their faculty.

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Race and tenure case was not handled fairly by MIT

SIR — Although tenure evaluations are not primarily accountings of publications, you reported in your News story 'Researcher refuses to back down over race case' (*Nature* **447**, 762–763; 2007) that I published six peer-reviewed research papers during the years before the decision taken by Massachusetts Institute of Technology (MIT) about my tenure.

My years as a principal investigator before MIT's decision include research at the Fox Chase Cancer Center in Philadelphia. MIT's tenure decision should have been based on my comprehensive work as a principal investigator, not limited only to time at MIT. The MIT faculty personnel record submitted for my tenure evaluation listed 41 scholarly articles published, in press, or accepted for publication, including 11 peer-reviewed primary-research articles, two peer-reviewed review articles, five peer-reviewed proceedings papers and four book chapters (two peer-reviewed). Not included in this total are four research manuscripts submitted to peer-reviewed journals and 10 published patent applications.

Your comparison of my tenure application with those of two other faculty awarded tenure at the same time is not a fair comparison, because people who arrive at an institution mid-career are not comparable to those who began their faculty careers at the institution at which they later apply for tenure. Their research programmes are at a different stage of maturity, and often the projects undertaken differ significantly in degree of challenge and impact. Even so, another mid-career faculty member received MIT tenure within the same timeframe as my application, largely on the basis of contributions that had been made before arrival there.

My main complaint against MIT is the manner in which my case was decided by the faculty chair. For example, at MIT, when a tenure-case decision is being made, review of the case is prohibited outside its department. If the case is not advanced to the next level of review, it is sealed. So why was a professor who is neither a member of my faculty nor an expert in my field — stem-cell biology —

asked by the faculty chair to review the case before the decision was announced?

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Foundation active in fight to cure Huntington's

SIR — Your News Feature on biomedical philanthropy 'Love or money' (*Nature* **447**, 252–253; 2007) states that the High Q Foundation is a successor to the Hereditary Disease Foundation (www.hdfoundation.org). You also state that CHDI is a successor to the Cure Huntington's Disease Initiative of the Hereditary Disease Foundation. The word 'successor' could give readers the false impression that the Hereditary Disease Foundation either no longer exists or is no longer active. This is incorrect. The Hereditary Disease Foundation, since its inception in 1968, has vigorously encouraged and respectfully supported researchers seeking to find treatments and cures for Huntington's disease as rapidly as possible and continues to do so.

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Friendly clarification from City of Brotherly Love

SIR — I thoroughly enjoy *Nature's* insightful columns and News and Views, and of course take a special satisfaction when researchers from my institution, the University of Pennsylvania (or U Penn) are featured. However, in a sidebar 'Body and mind' within the News Feature 'Brain craze' (*Nature* **447**, 18–20; 2007), a researcher is reported to work at Pennsylvania State University (Penn State) in Philadelphia. There have been occasional other instances in your pages of confusion over Pennsylvania and its universities.

Neither Penn State nor the University of Pittsburgh (Pitt) is in Philadelphia, the state's largest city, located in its southeastern corner. U Penn is a private university in the city, founded by its most famous citizen, Benjamin Franklin.

Penn State is the state's (and maybe the country's) largest public university, and it has many campuses, the main one located in University Park, Pennsylvania. The legend is that the location of the university was chosen

by drawing a large 'X' from the four corners of the state and placing the university at the centre of it — in order to make it equally accessible to all students.

The University of Pittsburgh (Pitt) is a public university that resides, as one might have guessed, in Pittsburgh, the second largest city in Pennsylvania and at the western edge of the state.

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Animal welfare is not just another bureaucratic hoop

SIR — I disagree with C. Jimenez's reply, in Correspondence, opposing Victoria Buck's suggestion of making animal-welfare sections in scientific papers compulsory ('Animal-welfare section in papers would be a burden' *Nature* **447**, 259; 2007). We all have a great many bureaucratic hoops to jump through these days, but we should not take a dismissive attitude to animal-welfare issues.

Animal-rights extremists have made life a misery for some scientists in the United Kingdom, despite our having one of the best-regulated licensing and ethical review processes in the world. National legislation requires scientists wishing to carry out experiments on animals to be licensed, and strict enforcement by both the legislature and by the local ethical review committees ensures that there are very few infringements.

The exchange between Buck and Jimenez did not address ethical approval statements, but for the record I do not think it an onerous task to include in scientific papers a paragraph stating the legislation(s) and local ethical review process under which the work had been approved. Many journals, including the *Nature* journals, already make compliance a condition of publication (see www.nature.com/authors/editorial_policies/experimental.html).

Although we must be robust in our defence of the need for appropriate animal experimentation, it is pointless to antagonize those individuals who will never be persuaded of its need or relevance. The 3Rs requirement goes some way to assuaging the disquiet of the more reasonable objectors, and hence should not be dismissed.

We live in a cynical world where everything is questioned, and the scientist is no longer seen as an ivory-tower figure. We are all accountable to the agencies that fund us and regulate our use of experimental animals and human tissue samples. Our ability to pursue science gives us a privilege that few others enjoy, that of unravelling the biological processes that

make us what we are. We are enabled in this occupation by the silent consensus, and hope, of people all over the world. We abuse that consensus at our peril.

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Animal welfare: reporting details is good science

SIR — C. Jimenez, in Correspondence, considers that detailed information on the way animals are handled and treated should not be placed in published papers (*Nature* **447**, 259; 2007).

I disagree, because it is a fundamental principle of the scientific process that when a paper is published, the study can be repeated from the description given in the methods, thereby allowing external validity to be assessed. To this end, variables that might affect the results need to be reported accurately.

It is well-documented that making even a slight change to a laboratory animal's environment or husbandry can have profound influences on its biological functioning. Cage size can influence metabolism, baseline rectal temperature, the fever response, feeding behaviour and behavioural responses in predator-prey interactions. The type of flooring in a cage can affect blood pressure, heart rate and body temperature. Other factors that influence physiology and behaviour include housing laboratory mice as singletons or pairs, the complexity of the cage and the extent to which animals are handled.

Variables such as these, which might be changed to improve the welfare of the animals, should be reported in published papers as an essential component of the accurate reporting of science.

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UNAIDS rejects claims of exaggeration and bias

SIR — We would like to provide our perspective on your Book Review of two books criticizing the Joint United Nations Programme on AIDS (UNAIDS), 'Time for a change?' (*Nature* **447**, 531–532; 2007), and the coverage of this issue at www.nature.com/news/2007/070528/full/070528-6.html.

In his book *The AIDS Pandemic: The Collision of Epidemiology with Political Correctness*, James Chin accuses UNAIDS of exaggerating data for the sake of advocacy, which is not true. Nor are UNAIDS data influenced by political or fundraising

agendas. The UNAIDS Secretariat and the World Health Organization work closely with other technical partner organizations to assist countries in better understanding their HIV epidemics so they can respond appropriately. Estimations are produced in close collaboration with national epidemiologists and governments, using methodologies recommended by an international team of experts chaired by a leading academic from Imperial College London.

UNAIDS is committed to providing the most accurate information available and continues to be transparent in publicizing the methods used to assess the magnitude of the past and current epidemics. UNAIDS has always stated that countries should use the most comprehensive and most recent data available. Reassessments of earlier published estimates of prevalence, incidence and mortality have been made, and we expect that there may be adjustments in the future.

Helen Epstein's *The Invisible Cure* also makes inaccurate statements about the work of UNAIDS: in particular, we have always advocated the reduction of number of sexual partners as an effective strategy for HIV prevention, as can be seen from our reports and other contributions to the published record. All UNAIDS documents on the prevention of sexual transmission of HIV advocate abstinence, reduction of sexual partners and correct use of male and/or female condoms. (See, for example, http://data.unaids.org/Global-Reports/Bangkok-2004/UNAIDS_Bangkok_press/GAR2004_pdf/GAR2004_ExecSumm_en.pdf)

UNAIDS and its partners will continue their mission to gather the best-quality data to assist in shaping an effective global response to AIDS.

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Chinese recorded classical nova two millennia ago

SIR — I agree with Michael M. Shara and colleagues (*Nature* **446**, 159–162; 2007) that the star Z Camelopardalis was a classical nova a few thousand years ago. In fact, a record of the eruption exists in Chinese documents of the time. There was, apparently, a report of a 'guest star' in October–November 77 BC (P. Y. Ho *Vistas Astron.* **5**, 127–225; 1962). The position in the sky fits Z Camelopardalis. This seems to be the oldest classical nova recorded in any surviving text.

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