Racism and health. Challenge to racism must continue

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Racism and health

Challenge to racism must continue

Editor—As McKenzie highlights in his editorial, the emphasis to date has been on the role of racism in recruitment and career development. This must continue despite initiatives and legislation such as the recent Race Relations Amendment Act, as racism still exists in the NHS.1 We need to continue to challenge racism not only from our colleagues but also from our patients—zero tolerance is needed.

The importance of racism on health and health care will not diminish owing to increasing migration to the United Kingdom particularly from east European countries. The often hostile reception of the public, media, and some politicians reinscribes the negative attitudes that prevail, and these may manifest through acute and chronic stress to the detriment of the individual. Research on evaluating the mechanism for racism and health outcomes is in early infancy; most studies are being conducted in the United States. We agree with McKenzie that further funding is needed in this area.

The biological models alluded to seem plausible, but before investigating these further substantial research needs to be done first to define, measure, and validate “racism” as an epidemiological variable. Then we need studies to disentangle the effect of racism on health. Urgency and opportunity exist to initiate a national ethnic cohort study within the planned UK Biobank study (www.ukbiobank.ac.uk) to include examination of the effect of racism on health outcomes.

Paramjit S Gill clinical senior lecturer
Department of Primary Care and General Practice,
University of Birmingham Medical School,
Birmingham B15 2TT
p.s.gill@bham.ac.uk

Raj S Bhopal Bruce and John Usher professor of public health
Department of Community Health Sciences,
University of Edinburgh Medical School,
Edinburgh EH8 9AG

Competing interests: None declared.

Author’s reply

Editor—I agree with Gill and Bhopal that the challenge to racism must continue, and there should be zero tolerance. Racism is complex, and so the response will need to be wide based.

Research has been conducted in the United States and United Kingdom on the nature of racism. There will always be a need to refine measures, but survey tools and instruments are available, supported by a robust literature, that are being used in the United Kingdom.

I support the need for a national ethnic minority cohort study to try to disentangle the effects of racism on health. This should be separate from Biobank. Biobank samples people aged over 45. It will not be able to address the effect of racism on younger people and pregnant women. It will not address ecological effects. It will not address the changing demographics of areas or populations that may influence the impact of racism on health.

Some may be uncomfortable with the scientific paradigm of Biobank and may call for consultation with black and ethnic minority groups. Oversampling of people of black and ethnic minority groups within Biobank would be useful. If the samples simply reflected the percentage of the ethnic minority populations over 45 in the United Kingdom the study may not have sufficient power for meaningful subgroup analysis. It could be argued that the research would not have delivered equity because it is not as useful to minority groups. It could be argued that it is therefore discriminatory. I am unsure whether this could lead to a challenge under the Race Relations Amendment Act.

However, oversampling in Biobank should not be confused with a proper research effort that addresses the issue of racism and health. Biobank may be useful, but it is too limited to offer the answers required.

Kwame McKenzie senior lecturer in transcultural psychiatry
Department of Psychiatry and Behavioural Sciences, Royal Free and University College Medical School, London NW3 2PF
k.mckenzie@rf.ac.uk

Competing interests: None declared.

Australia’s Aboriginals suffer disproportionate burden of ill health

Editor—McKenzie has highlighted the association between racism, morbidity and mortality.1 I respond as an epidemiologist for Aboriginal health in central Australia.

About 20 000 Aboriginal people live in central Australia, most of them in small communities scattered across the vast desert area. Aboriginal Australians have a disproportionate burden of ill health compared with their non-Aboriginal counterparts. Their life expectancy is about 20 years lower, and 58% of Aboriginals die before the age of 55.2 Their morbidity load is far greater than that of non-Aboriginals, and the bulk of morbidity is due to chronic diseases such as diabetes, heart disease, and chronic obstructive airways disease. From birth weight to life expectancy, disparities in health indicators between the two populations are glaring.

Much of the research work in the past has been directed at socioeconomic status, cultural factors, and position in the social hierarchy. Not much attention has been given to racism and its effects on health status. McKenzie’s editorial implies that racism may be aetologically important in the development of illness.3 Overt or implicit racial discrimination is recognised to be the underlying cause for poor health status.4 Although research based evidence is scant to show that racism being the underlying cause for health inequalities in central Australia, there are some setbacks in the system that may be aggravating the disparity:

- Inappropriate responses from health services
- Lack of coordination among various categories of healthcare staff
- Absence of mechanisms for evaluating the effectiveness of services
- Lack of a public health and epidemiological approach
- High turnover of healthcare staff.

These factors may well be the consequences of intrinsic racism in the system. The disturbing health inequalities between Aboriginal and non-Aboriginal population are acknowledged time and again, but no appropriate action seems to be in place to address these problems and reduce the inequality gap. Requisite skills and knowledge to rectify these problems may be lacking among key managerial staff. I hope the above factors are given serious consideration while planning services that would in turn generate effective solutions.

Ruby Kaul epidemiologist
Department of Health and Community Services,
P.O. Box 721, Alice Springs, Northern Territory
0871, Australia

Competing interests: None declared.

(11 January)

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Letters

(11 January)
Itual truth—“It doesn’t matter what you believe as long as it makes you happy.” Of far greater importance is “Is it true or not?”

Consider Christianity as an example. The experience of many is that faith brings a deep joy that surpasses day to day happiness and setp. However, Christianity does not always bring happiness. Christ claimed he was God’s son and was crucified for it. Many who followed him since have been martyred for their beliefs, and countless more suffer daily persecution for being called “Christian.” Living out the Christian faith in itself hard work. Therefore it matters whether this faith is based on truths.

The spiritual side of clinical care is important. We should be careful, however, to avoid misleading spiritual platitudes that bring happiness at the expense of truth.

Matt J Hawker senior house officer, ophthalmology Worcsresteri Hospital Acute Hospitals NHS Trust, Worcestcr WR5 1BD mandsi@freernet.co.uk

Competing interests: MH is a Christian.

Spirituality is not everyone’s cup of tea for treating addiction

Editor—Culliford’s editorial about spirituality in medicine raises some important issues. In alcohol and drug treatment the spiritual dimension can play a part in recovery. The 12 step model developed by Alcoholics Anonymous has several important therapeutic elements, one of which is engagement with the self-help groups, were spiritual practices, along with attendance and spiritual practices at least once daily. The results showed that 90% of the participants had a belief in a higher power, and the amount of time that they devoted to any form of spiritual practice. The most serious problem is the absence of identified resources to allow improvements in diabetes care to be implemented effectively. We are informed that there will be some funding for retinal cameras, but no indication is given about how the revenue generated will be ringfenced. In many areas new monies, if any, will not be guaranteed that these will be ringfenced. In many areas new monies, if any, will not be guaranteed that these will be ringfenced. In many areas new monies, if any, will not be guaranteed that these will be ringfenced. In many areas new monies, if any, will not be guaranteed that these will be ringfenced.

National service framework for diabetes leaves questions open

Editor—The much delayed national service framework for diabetes has major implications for primary and secondary care services in England. Many of its proposals are to be welcomed, but some of them are vague, with little indication of how they can be implemented.

The most serious problem is the absence of identified resources to allow improvements in diabetes care to be implemented effectively. We are informed that there will be some funding for retinal cameras, but no indication is given about how the revenue generated will be ringfenced. In many areas new monies, if any, will not be guaranteed that these will be ringfenced. In many areas new monies, if any, will not be guaranteed that these will be ringfenced. In many areas new monies, if any, will not be guaranteed that these will be ringfenced.
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diabetes care in many districts. The resource and staffing consequences of the national service framework need to be addressed urgently, otherwise its impact will be minimal, care overall will not improve, and the morbidity and mortality in the diabetic population will not be reduced.

Richard H Greenwood chairman richard.greenwood@nmuh.nhs.uk

K M Shanks honorary secretary

P Winocour honorary secretary

Association of British Clinical Diabetologists, London W1P 1HQ

Competing interests: None declared.

Carovery risk scores and prescribing in diabetes

Using risk tables to assess cardiovascular risk in type 2 diabetes has drawbacks

Entror—Hall et al describe the use of primary prevention risk tables in type 2 diabetes.1 We agree that targeting cardiovascular risk in diabetes is a priority, but their proposals imply an overreliance on risk scores as the sole determinant of cardiovascular risk assessment in diabetes. This approach might lead to withholding treatment in some people whose risk is underestimated by risk scores.

Unfortunately, the decision at what level of cardiovascular risk to start lipid lowering treatment in diabetes is not straightforward. The authors’ oversimplified approach, although convenient, is unscientific and flies in the face of epidemiological evidence which suggests that type 2 diabetes should be regarded as a disease group for secondary rather than primary prevention.2 Using the Framingham equation to evaluate cardiovascular risk in diabetes entails caveats. These include a low baseline prevalence of diabetes in the Framingham cohort and the omission from the equation of triglyceride concentration, an important determinant of cardiovascular risk in type 2 diabetes.3 The low prevalence of diabetes in the cohort leads to wide confidence intervals in the predicted risk. Thus, in a diabetic man with average risk factors, the upper 95% confidence interval crosses the 15%, 10 year threshold from the age of 40 onwards.4 People with risk scores below a chosen cut-off point may therefore have a higher true risk. The proposals by Hall et al would lead to a rigid prescribing protocol whereby all patients with scores above a threshold (for example, 15%) would receive treatment, whereas those below would not. Furthermore, an overemphasis on the risk score might be at the expense of ignoring other key factors not represented by the Framingham risk equation, such as ethnic group, family history, microalbuminuria, and triglyceride concentration.

Jamie Smith specialist registrar jamiem.smith@virgin.net

Roger Corrall consultant physician Department of Diabetes and Endocrinology, Bristol Royal Infirmary, Bristol BS2 8HW

Competing interests: None declared.

Authors’ reply

Editor—The purpose of our paper was to identify whether there was clinical value in having cardiovascular risk scores. We wanted to identify whether having an integrated single score of macrovascular cardiovascular risk highlights to clinicians that a clinical issue needs addressing, which influences their prescribing habits. Our paper indicates that this is the case and that in the setting of a busy clinic having to assess a multitude of individual risk factors may result in cardiovascular risk being overlooked. Having established that an integrated score is useful to practising clinicians, the next challenge is to identify an appropriate risk score to use.

Smith and Corrall correctly indicate that the New Zealand risk score, and others based on Framingham, all underestimate the cardiovascular risk in diabetes, although data from Tayside indicate that this is probably not to the extent suggested by Hafler et al.5,6 The level of risk chosen to start treatment is an arbitrary cut-off point which can be adjusted—for example, to 15% from 20%—if thought desirable. The real answer is to define the epidemiology of cardiovascular risk in diabetes more accurately so that more accurate tables can be developed.

Graham P Leese consultant in diabetes graham.leese@ruh.nhs.uk

Lesley Hall medical student

Roland Jung consultant Ninewells Hospital, Dundee DD1 9SY

Competing interests: None declared.

Failing to bark and barking

Editor—Le Fanu’s Sherlock Holmes style case of the missing data and dog that failed to bark had amusing elements but also mis-understandings, errors, and accusations of concealment, implying bad faith.7

Le Fanu had set out to validate and explain such trends. Our 1999 paper on trend results was big news internationally, including the BMJ; but apparently Le Fanu missed it.

Mortality data are freely downloadable from the World Health Organization. The dearth of publications on mortality trends in scientific journals is not from conspiracy. Any novice can have a go, so editors and reviewers are overburdened with enthusiasts trying to prove things badly. Works of scholarship may have an uphill fight for publication because routine mortality statistics are considered trivial and unscientific compared with laboratory studies.

• Americans did not study immigrants just from Japan. Studies go back 75 years, classically contrasting New York’s Italians with its Jews, and Irish and Norwegians in the United States with brothers in the home country, more recently focusing on Hispanics and newer ethnic groups. Disease rates are easier to study with first generation migrants, place of birth providing a census denominator, than later on.

• Smoking shows all or none differences in one population, where diet has historically been more uniform. Diet differs greatly between populations, thereby determining a population’s susceptibility to cardiovascular disease and also to specific cancers. The cholesterol story is well established, but diet is now known to contain more varied contributors to coronary risk than dairy fats alone.

• Le Fanu’s unoriginal suggestion that coronary disease has an infective origin would not in itself explain why it took 10 years to cross the Atlantic as an epidemic, and half a century to reach eastern Europe. Lifestyle fits better. Existing explanations for disease trends must give way to better ones, but they must be more specific than that.

Le Fanu claimed in the Sunday Telegraph Magazine in 2000 that I published research that I knew to be false, that was nonsense and quackery, and that I was a danger to the public.8 Although now apparently running with the fox as well as hunting with the hounds, by characterising MONICA collaborators as not barking he claims the opposite role for himself.

Hugh Tunstall-Pedoe professor of cardiovascular epidemiology

Cardiovascular Epidemiology Unit, University of Dundee, Ninewells Hospital, Dundee DD1 9SY

ktnustin@nhs.net

Competing interests: IHP was a project author and principal investigator in the WHO MONICA project.

A longer version of this letter complete with references is available at bmj.com/cgi/eletters/325/7378/1490/#S0160

1 Le Fanu J The case of the missing data. BMJ 2002;325:1490–5 (21 December)

2 Kiejneziuc Z Heart disease mortality declining with fewer and less deadly attacks. BMJ 1997;315:1397.

3 Le Fanu J Scientists who should carry a health warning. Sunday Telegraph Magazine 2000 (July 9).

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Competing interests

Consent was not obtained

Editor—So, the editor of the BMJ is happy to coauthor research involving undeclared deception of subjects and publish it in his journal.1 Three hundred readers were unwitting dupes. They took part without being informed what the real object of the exercise was, no informed consent here, at least not by the standard of being willing to show the subject the research protocol.

This sort of research violates Kant’s categorical imperative—act as if this were a universal principle. It seeks payment in a coin that it devalues for others. BMJ readers should be warned: next time a researcher contacts you for an opinion, it is probably a hoax.

What is ironic about the paper is that the results have meaning only if the respondents were more honest than the authors. My warning to researchers is as follows: if you deceive your subjects what right have you to expect they will do the same to you?2

Stephen J Senn professor of pharmaceutical and health statistics
University College London, London WC1E 6BT
stephens@public-health.ucl.ac.uk

Competing interests: SJS is a consultant to the pharmaceutical industry and an academic. His career is therefore furthered by publication.


It’s my journal, and I’ll write if I want to

It’s my journal, and I’ll write if I want to

Editor—Richard and colleagues just walked Through the door Like a king with his court. He says he’s had a great idea And surely to publish they ought. Sorry, my muse left on holiday after that. Please feel free to add some more verses. Like other respondents, I applaud the BMJ’s crusade to enlighten readers about the issues of conflicting interests.3 The paper by Chaudhry et al should surely have been submitted to another journal, or if not then someone else should have it.4 The BMJ’s peer journals are not the BMJ itself.

Could we be informed as to how long the paper was out at the reviewers and how quickly it got accepted in its final version? It was published within six weeks of acceptance. That’s nice. I have had work published in the BMJ, on one occasion after the manuscript (and I am quoting directly from the correspondence at the time) “lay in the top drawer” of a staff statistician’s desk for six months. I doubt that would ever happen to an editor’s paper.

These are small points but if the BMJ’s crusade is to be credible and successful, inhouse guidelines about staff submissions should be torn up and replaced by a rule that BMJ related work is only submitted elsewhere.

Jonathan O B Hourihan senior lecturer; infection, inflammation, and repair
Mailpoint 218, Southampton University Hospitals NHS Trust, Southampton SO16 6YD
j.hourihan@soton.ac.uk

Competing interests: None declared.


Barriers to managing heart failure in primary care

Heart failure clinics provide crucial link between primary and secondary care

Editor—Fuat et al surveyed attitudes towards managing heart failure in general practice.1 Points of particular note included difficulties in assessing subtle early signs of heart failure, difficulties in interpreting echocardiography reports, and concerns about the number of drugs recommended for patients with heart failure.

This study further strengthens the case for specialist heart failure clinics as outlined in the national service framework for coronary heart disease.2 Such clinics have a multidisciplinary team consisting of physicians (specialist and primary care), specialist nurses, and cardiac technicians, and these teams facilitate a coordinated approach to diagnosing, assessing, and managing heart failure. Objective evidence of cardiac dysfunction may be obtained and interpreted by a cardiologist, with the subsequent formulation of a treatment strategy.

We believe that this is preferable to open access echocardiography services, with the difficulties in interpretation highlighted by Fuat et al.3 Specialist dedicated nursing provides a crucial bridge between hospital and community care, allowing continued clinical assessment and appropriate titration of drug treatment, as well as continued patient education. Such nursing has been associated with a significant reduction in hospital readmission for heart failure.4 Widespread awareness of current treatment guidelines seems to be lacking in primary care; dedicated nursing services

can be instrumental in implementing guidelines.

Heart failure is thought to invariably affect elderly people. This is simply not true. Clearly the disease becomes more prevalent with age, but diagnosis is delayed in many young patients with dilated cardiomyopathy because of this perception.

Heart failure has a high prevalence and is associated with a terrible prognosis, despite the availability of evidence based treatments. General practitioners are at the sharp end of an escalating problem, and specialist clinics provide invaluable support for the growing burden of chronic heart failure.

Russell C Davis consultant cardiologist
R.C.Davis@bham.ac.uk

Gurbir Bhatia research fellow
Michael Sozin research fellow
Jane Stubley senior heart failure specialist nurse
Sandwell and West Birmingham NHS Trust, West Bromwich B71 4HJ

Competing interests: None declared.

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**Congestion charging**

**Walking classes also need road space reallocation**

**EDITOR—We welcome Roberts’s editorial, making the health case for the London congestion charge, particularly as the revenue is required to be spent on transport. We agree that physically active transport such as walking and cycling is likely to increase. This must be monitored adequately, with attention given to changes in activity levels and broken down by sociodemographic groups, to assess the impacts on health and inequalities.**

**Other potential effects of congestion charging include improvements in access for emergency vehicles. However, not all are positive: the impact of the policy will depend on which complementary measures are introduced at the same time.**

**Firstly, we disagree that less car travel will result in fewer crashes; this impact is difficult to predict.** It depends whether journey times are shorter because of less time queuing at junctions or because of higher speeds. If traffic reduction is greater than was predicted, travel speeds may become substantially faster. While shorter journey times could reduce exposure to the risk of collisions, higher speeds could increase the risk by a greater amount.

**Secondly, congestion charging alone could adversely affect equity: road space vacated by people who are deterred by the charge could be occupied by the wealthy, who are less price sensitive.**

Both effects can be effectively combated by simultaneously introducing measures to reallocate road space and giving priority to buses, preferential access to disabled drivers, and effective protection to cyclists and pedestrians. This is largely true of the London congestion charge, but it is important to consider when other towns and cities follow suit.

Even London has been timid about pedestrianising road space—Soho and Covent Garden seem ideal candidates. Experience shows that whereas such schemes tend to be initially opposed by local businesses, once the schemes are implemented they become popular.

Stanley Feldman professor
28 Moore Street, London SW3 2QW

Competing interests: None declared.

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**Political polemics are masquerading as science**

**EDITOR—Roberts in his editorial on congestion charging is wrong in almost all of his assertions.** The number of cars on the road in central London has remained stable over the past 30 years until it fell by 18% last year (Transport for London statistics). Slow journeys and congestion are due to more people working in central London and poor road management.

Roberts acknowledges that the vulnerable groups are pedestrians (10-18 times the accident rate of car drivers per 2 km journey) and cyclists (13 times the accident rate per 2 km journey), yet he thinks that increasing the size of both of these vulnerable groups by encouraging people to walk or cycle will decrease the number of accidents. Statistical nonsense.

It is generally agreed that the only pollutant to constitute a health hazard at current levels is a small particle emission. How can moving from clean petrol driven cars to dirty diesel buses (buses do not have to conform to any emission standards) help this?

Since there are only about 16 state schools within the congestion charge zone it is absurd to suggest it is going to make mothers happy to allow their children to walk to school throughout the country.

To compare Ken Livingstone to Edwin Chadwick is particularly absurd. Chadwick’s plans for sanitation were drawn up at the request of parliament and largely implemented without opposition. He is remembered for his controversial Poor Law Act of 1834, which confined the poor to institutions where families were separated and deliberately subjected to discomfort “to punish them for their indolence.” There may be a case for congestion charges but it is not on the grounds of improving health or safety. I cannot believe that this editorial was critically reviewed before publication.

Stanley Feldman professor
28 Moore Street, London SW3 2QW

Competing interests: None declared.


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