The endeavour to become international

We read with interest the inaugural editorial by Peter Tyrer (2003). We especially welcome his hope to continue the quest of his predecessor to make the Journal ‘the leading international journal of general psychiatry’. Responding to his invitation for feedback, we offer the following comments and suggestions.

As suggested by Patel & Sumathipala (2001), evidence to influence mental health policies and practices at the international level will often have to come from research done both within and outside the cultural and health systems of Western Europe and America. In 1996 to 1998, of the articles published in the Journal, only 6.5% were from the ‘rest of the world’ (Patel & Sumathipala, 2001). Between 1991–1992 and 2001–2002, the regional distribution of contributions has remained largely the same (65–69% from the UK, 3–4% from Asia, Africa and South America) (Catapano et al, 2003). Obviously, the Journal has a long way to go in obtaining contributions from and with relevance to countries across the world. The negligible representation of members based in low- and middle-income countries on the Editorial Board (one among 69 members) (Saxena et al, 2003) is also incongruent, perhaps even incompatible with being truly international.

We suggest a few steps that might be taken by the Journal under the new Editor. First, more Editorial Board members should be recruited from low- and middle-income countries. It is likely that at least some suitable candidates from psychiatrists and researchers working in Asia, Africa and Latin America can be found if a serious search is made. Second, the Journal should use international relevance as a criterion in selection of articles for publication, in addition to the criterion of scientific excellence, which should remain uncompromised. Third, the Journal should be proactive in attracting and supporting submissions from low- and middle-income countries. This could include, for example, appointing regional Deputy Editors, launching special sections and themes (e.g. ‘Psychiatry around the world’, referred to by Wilkinson, 2003) and assistance with editing for authors whose first language is not English.

We believe that concrete steps like these will make the Journal’s aim of becoming truly international more easily achievable.


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Editor’s response: The points made by Drs Saxena and Sharan are well taken and, on behalf of the Journal, I have to plead mea culpa to the charge of Western parochialism. The Journal will take these criticisms on board and hope that readers will note a move in the direction suggested by Drs Saxena & Sharan shortly. As they say, the criterion of scientific excellence should remain uncompromised and this should remain the clearest of guides.

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Suicide and self-harm

What conclusions should we draw from the article by Gairin et al (2003) on attendance at the accident and emergency department in the year before suicide? That if you do not do your homework, you will make mistakes. Although they criticise the National Confidential Inquiry and make 18 references to it, they do not seem to know what it does.

The National Confidential Inquiry into Suicide and Homicide by People with Mental Illness has been based in Manchester since 1996, covering only one of the years studied by Gairin et al. It was set up to identify all deaths by suicide of people who had been under the care of specialist mental health services in the previous 12 months (Appley et al, 1997). Our remit (not to mention our funding) does not extend to emergency departments. Our method of case ascertainment (Appley et al, 2001) is to obtain lists of suicides and undetermined deaths from the Office for National Statistics and to check these against records held by local mental health services. We then collect further information from each patient’s consultant psychiatrist. Gairin et al seem to think that we rely on voluntary reporting by health districts.

The Inquiry has been notified of 35 000 suicides since 1996 and has collected detailed information on over 9000 people in contact with mental health services. Gairin et al’s assertion that we ‘must record the occurrence of hospital attendances for self-harm’ for all patients is a bold one, especially when it is based on five misclassified cases in one region. The issue is not whether self-harm is important, but the best way of collecting information about it in a national study. As a first step we are now carrying out a psychological autopsy study of 300 suicides by mental health patients, obtaining details of attendances in emergency departments and general practice, and interviewing the families of those who have died.

Gairin et al are also critical of policy makers for not recognising that self-harm is a key indicator of suicide risk. They must have missed the fact that the National Suicide Prevention Strategy for England includes a section on preventing suicide following self-harm (Department of Health, 2002).

Declaration of interest

The authors all work on the National Confidential Inquiry into Suicide and Homicide by People with Mental Illness.
We disagree with the National Director for Mental Health that the evidence is not strong enough to support such a policy; it is at least as good as the evidence for the wholesale introduction of standardised risk assessment in mental health services. If further evidence is needed, then we are not sure that a study restricted to ‘mental health patients’ (and therefore presumably excluding the very people we are discussing) is the answer. It would, however, be a relatively simple matter to attempt to replicate our findings in a multi-centre prospective monitoring study at those other centres that run accurate accident-and-emergency-based clinical databases.

What is early intervention?
Drs Pelosi and Birchwood (2003) have provided some stimulating thoughts about the implementation of early intervention for psychosis. Perhaps one of the underlying difficulties that may lead to the dichotomy of views expressed by the two authors is a confusion about what constitutes ‘early intervention’. Pelosi rightly identified both the lack of evidence and theoretical restriction in clinical usefulness based on the epidemiology of schizophrenia and the sensitivity and specificity of screening for the disease. It seems reasonable to question the widespread and costly implementation of a service based on such shaky evidence.

However, there is a sharp contrast between the concept of early intervention as a service aimed at secondary prevention, with treatment in prodromal phases of schizophrenia, and the way in which it is defined in the UK Government’s Mental Health Policy Implementation Guide (Department of Health, 2001). Here, it is clear that the service should primarily be focused on interventions in people who have already developed psychotic symptoms, with various broad-ranging strategies to ensure early identification and referral and good links with employment and education institutions ensuring a high-quality and holistic service.

None of this is rocket science and the argument that it could be provided by existing community mental health teams might seem attractive were it not for the failure over many years of existing teams to truly address these issues. Experience from other areas of health care, such as cancer services, suggests that specialisation often leads to improvements in quality of services and the same might be expected within the context of early intervention for psychosis.

Early intervention provides an opportunity for significant improvements in the way in which young people with devastating illnesses are managed, and it is essential that psychiatrists lend the full weight of their experience and expertise to ensuring the success of these teams.

Psychiatric services for ethnic minority groups: a third way?
The publication of the debate on separate psychiatric services for ethnic minorities (Bhui/Sashidharan, 2003) highlights the unmet needs of some of these people. Their progress on the pathway to mental health care has suffered through poor recognition of mental illness because of issues related to language, idioms of distress and other cultural factors. Bhui rightly points out that the majority of ethnic minority services are run by the voluntary sector and are outside the National Health Service (NHS). Their limitations include: limited involvement of NHS psychiatrists; targeting of only certain ethnic groups; restriction to small geographical areas; and short-term funding. The statutory sector has mainly catered only for those groups with severe mental disorders, sometimes involving law and order issues but not addressing the needs of the majority who have less severe mental disorders. This may mean that depressive illness, which goes undetected and untreated, leads to considerable suffering.

In planning culturally competent services, the notion of a specific service for each cultural group is unrealistic. In areas where 25% of the population are ethnic minority groups speaking up to a hundred languages, creating services for individual ethnic groups seems unattainable. There is another problem in that specific services for ethnic minority groups raise fears of
'ghettoisation' and further marginalisation of those already marginalised.

With Professor Sashidharan’s dislike for words such as ‘separate’, ‘different’ and ‘them’, one gets the impression that he wants a ‘melting pot’ approach to address inequalities in service provision. Whatever perspective we may have, ethnic groups have their own identity and specific needs; thus, a ‘mosaic-like’ approach, with better awareness of individual needs in a broader perspective is required.

Caution is needed regarding reference to cultural matters. Sometimes, everything is attributed to ethnicity or culture, while at other times the existence of cultural impact is completely denied. Concentrating on cultural differences may lead to important diagnostic signs being missed. Cultural sensitivity is not a fixation on culture and it should not be a synonym for unexplained variance.

On the basis of our own experiences in Manchester and Toronto, we propose a third approach – founded on Professor Kirmayer’s ‘cultural consultation model’ (Kirmayer et al, 2003) – as an interim option. This in some respects lies midway between the opposite poles of the debate. This model proposes the operation of a specialised multi-disciplinary team that brings together clinical experience with cultural knowledge and linguistic skills essential to working with patients from diverse cultural backgrounds. A team built on the cultural consultation model aims to give advice to other clinicians rather than take on patients for continuing care. The latter will be reserved for cases where there are difficulties in understanding, diagnosing and treating patients where cultural factors may be important. The assessment will usually involve two or three interviews with the patient and his or her family, which should result in a clear cultural formulation, diagnosis and treatment plan. The members of this team will be a resource for clinicians in primary care, social services, mental health and other related disciplines. They will also be involved in the training of interpreters, culture link workers and members of the mainstream and existing community services.

Until ‘they’ become ‘us’ we have to find a way forward that is both financially and logistically viable and that allows mainstream services to provide a culturally sensitive approach to all groups rather than a service to a minority of those in need.


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Author’s reply: Waheed and colleagues raise some important dilemmas in the debate on specialist services for ethnic minorities. We already have specialist services for many cultural groups in the voluntary sector and statutory sector. I agree that within the statutory sector, there would be insufficient funds to equip a large number of new specialist services in all parts of the country for all subcultural groups. Yet, we currently rely on just such an underfunded solution within the voluntary sector to plug gaps in psychiatric service provision. Specialist services may continue to exist in response to unmet need rather than by design.

There are some problems with the cultural consultation model. First, this solution is not novel, and was established in Bradford some two decades ago, only to be brought to an end due, I believe, to lack of funds for such a specialist service! The approach can be successful, but not because of the structure it imposes. Improvements in the quality of care will not be achieved by simply restructuring the services, as entrenched attitudes and skills deficits will simply be transferred into new services. All practitioners should have the necessary skills, knowledge and attitudes for a modern multiculturally capable service. Who will be qualified to lead such a service, and what are the capabilities necessary for workers in such a service? Moodley (2002) addressed these issues for psychiatrists following development work by the Transcultural Special Interest Group within the Royal College of Psychiatrists.

Irrespective of the service model, any service can respond to the needs of Black and minority groups only if the workforce is skilled and continues to acquire new knowledge and skills to work with new migrants. Motivating the workforce to acquire skills is essential, but current workloads, rapid changes in services and waves of new policy deter the acquisition of new skills and the development of innovative paradigms for service delivery. Until these issues are addressed, we rely heavily on specialist services that have managed to attract and motivate staff to be creative and tailor packages of care. A specific problem of the consultation model is that specialists are expected to be the fount of all wisdom on cultural issues, absolving the rest of the workforce from these responsibilities (Bhui et al, 2001). Furthermore, no single consultant can ever claim to be an expert on all cultures of the world. However, a consultant can reasonably be expected to communicate general principles, aptitude and methods in order to discover more about mental distress in the context of unfamiliar cultures using, for example, ethnographic approaches. Yet, those seeking advice from such a service must be able to change their practice. Business efficiency can work against improving the cultural capability of services and warrants more attention by purchasers and providers (see Bhui, 2002). Irrespective of the service model, organisational cultural capability, a motivated workforce and optimal learning conditions will diminish the need for specialist services, but not in the foreseeable future. In the meantime we can learn from these specialist services, but their existence is inevitable and necessary if the cultural capability of the NHS workforce does not improve.

Declaration of interest
K.B. is Secretary to the Transcultural Special Interest Group of the Royal College of Psychiatrists, and Director of the MSc programme in transcultural mental health-care at Queen Mary, University of London.


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Neuroimaging psychopathy: lessons from Lombruso
Blair (2003) outlined a neurobiological basis for psychopathy. The orbitofrontal cortex has also been implicated in psychopathy by other authors (Dolan, 1999). A
strength of Blair’s article was its proposal of an integrated model of psychopathy in which the process of socialisation is impeded at a neural level. Such a ‘biosocial’ theory seems to make intuitive sense. However, concerns arise based on the drawing of parallels with research done over a century ago by Cesare Lombroso, an Italian psychiatrist and criminologist. Modern researchers share with Lombroso (and some of his predecessors, such as Pella and Gall; see Walsh, 2003) a desire to explain criminality in terms of innate biology. But as Gould states (Gould, 1980), ‘Major ideas have subtle and far reaching extensions’ and a brief glance at Lombroso’s theory and its ‘social extension’ can flag up the dangers associated with modern neuroimaging in this area.

Lombroso believed that 40% of criminals were ‘born criminals’ who could be distinguished by physical features including relatively long arms, prehensile feet with mobile big toes, low and narrow forehead, large ears, thick skull, large jaw, etc. (Gould, 1980). A particularly unnerving aspect of Lombroso’s work is that he campaigned on the basis of his theory for a preventive criminology: ‘society need not wait for the act itself, for physical and social stigmata define the potential criminal. He can be identified, watched and whisked away at the first manifestation of his irrevocable nature’ (Gould, 1980). Lombroso also ‘recommended irrevocable detention for life for any recidivist with the telltale stigmata’ (Gould, 1980).

This should serve as a warning in the modern era, where the spirit of Lombroso lives on. One fears a scenario in which a brain scan diagnosis of psychopathy legitimises the preventive incarceration of a ‘high-risk’ individual, and in which a static neurostructural deficit may lead to a therapeutically nihilistic approach to such an individual on the grounds that he is ‘beyond rehabilitation’. Combining the above two positions, the perception of an individual as both dangerous and unchanging may lead to a ‘lock them up for good’ ethos.

Lastly, there are dangers in assuming a causal link between psychopathy and structural brain change. One consequence of this, in terms of individual responsibility, would be the inappropriate invocation of a deterministic argument by a defendant seeking exculpation for an offence.


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Scientific psychiatry?

We write in response to the editorial by Dr Turner (2003), who wishes to revitalise Jaspers’ view that psychiatry cannot extricate itself from the humanities. With the ascendency of biological psychiatry this idea is important to remember. However, Dr Turner’s article does little to advance this idea and contains some possible misconceptions. Turner’s interpretation of Donald Davidson’s work does not clear things up. Academic philosophers are still actively debating what Davidson’s philosophy amounts to. In this situation, an appeal simply to his authority is misdirected.

On specific points, Turner needs to be challenged. First, he seems to interpret Davidson as denying the possibility of a scientific psychopathology. Biological psychiatrists are not trying to solve the mind–body problem or trying to discover the strict psychophysical laws that Davidson claims do not exist; rather, they are trying to find correlations between mental phenomena and physical processes. That such correlations exist seems obvious, as anyone who has taken a mind-altering substance can confirm or as Penfield’s neurosurgical experiments vividly showed. Davidson’s attack on the idea of strict causation between physical events and mental events serves not so much to prohibit the possibility of a science of psychology, but rather to deny such a science predictive powers equal to those of physics. This is a consequence of Davidson’s philosophy of mind, whereby despite being ontologically an unabashed materialist he claims that the use of mental predicates is dependent upon normative and holistic concerns of society and language, and that these are not properties of the physical order. Davidson has indeed accepted the points made by some of his critics (Davidson, 1987), that empirically discovered helpful generalisations, so-called ceteris paribus laws, may be formalised and be of great utility. This surely is a worthy enough goal for psychology and psychiatry.

Second, Turner also suggests that there is no possibility of improvement in descriptive psychopathology (Turner, 2003). This is simply assertion and suggests that the author believes that phenomenology as a discipline ended with Jaspers in 1913, and further that Jaspers provided an adequate account of the subjective experience of mental disorder. Current opinion seems to regard Jaspers’ ideas as either obstructive to progress in psychopathology with his notion of the ‘un-understandability’ of some psychotic symptoms (e.g. work on cognitive models of psychosis; see Frith, 1992; Garety & Hemsley, 1994) or an obscure first start which petered out because he overcomplicated things (Cutting, 1997). Work on phenomenology continues to inform scientific research and clinical practice (Kapur, 2003).

Our view is that psychiatry’s potential adversely to drift from the humanities can be rectified by close attention to the phenomenology that forms the point of entry to the subject. Turner has given up on this project whereas to us it seems barely to have begun!


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Dr Turner is quite wrong to argue that Donald Davidson has shown there ‘cannot, in any useful sense, be a science of the mental because of the impossibility of either strict psychological or strict psychophysical laws’ (Turner, 2003: p. 472). It is true that Davidson (1970) argued that there could not be strict laws relating mental events either to physical events or to each other, but its lack of strict laws does not endanger
the scientific status of psychiatry, since strict laws are rare in science.

Davidson argues that the relationship between a cause and an effect is strictly lawful if and only if the cause is always followed by the effect irrespective of what else is going on; a sentence stating that the cause occurred must logically entail a sentence asserting the existence of the effect. Davidson (1993: pp. 8–9) concedes that this very demanding conception of a law is 'something that one could at best hope to find in a developed physics' and that 'there are not, and perhaps could not be expected to be, laws of this sort in the special sciences. Most, if not all, of the practical knowledge that we (or engineers, chemists, geneticists and geologists) have that allows us to predict and explain ordinary happenings does not involve strict laws'.

In 'the special sciences' (by which philosophers mean 'all the sciences except physics') laws hold only under normal circumstances; unlike strict laws, they may fail to hold if circumstances are sufficiently abnormal. Davidson's view is quite consistent with the existence of laws in psychiatry that are not strict but are as robust and useful as laws in genetics, chemistry or geology. Unless one thinks that chemistry, genetics and geology are useless, this means that there could be laws robust enough to make psychiatry a useful science of the mind. If psychiatry counts as a science in the same sense as genetics counts as a science, even the most fervent proponents of scientific psychiatry should be satisfied.

Author's reply: Concerning the substantive philosophical issues, while Drs Murphy and Owen and colleagues are correct that Davidson himself embraces non-strict laws, the important question has always been whether or not his anomalous monism, like any form of non-reductive materialism, is entitled to them. Essentially, as many of Davidson's commentators have pointed out, non-strict laws lead to intractable difficulties with mental causation (Kim, 1993). The upshot is that non-reductive materialism faces the horns of an interpretationist–reductionist dilemma. My editorial makes it plain which horn I prefer to be impaled on and my discussion of 'Philosophical Anthropology' (Turner, 2003) was an attempt to explain why interpretationism is not compatible with laws of any kind. The reason, which is worth reiterating, is that mental states qua interpretations are not, as Murphy and Owen et al assume, brute data. Understanding their meanings is a presupposition of formulating the very laws on which non-reductive materialism is allegedly based (Von Wright, 1971).

This brings me to Owen et al’s puzzling claim that biological psychiatrists are not trying to solve the mind–body problem. One reason the claim is puzzling is that Owen et al’s ‘correlations’ are the very non-strict laws that, by their own admission, have played a crucial role in recent attempts to solve the mind–body problem. In any case, I think we can safely say that the mind–body problem, like Owen et al’s argument, would be helped considerably by the discovery of non-strict laws. The authors, of course, realise this and proceed to inform us that their existence is ‘obvious’. I must say that if their existence were as obvious as Owen et al make out, then it is unlikely that they would have had to rely on Penfield to justify their claims. Indeed, it is interesting that while Owen et al are keen to remind us that Jaspers is not the last word in psychopathology, they are oblivious to the implications of allowing that Penfield is the last word on psychophysical correlations.

From the hermeneutical perspective what makes mental states mental states is that they are rationally and holistically related to one another. Once these relations are removed, as they are, for example, in hallucinations, autochthonous delusions and ‘Penfieldesque’ states, then it becomes difficult to justify the claim that the phenomena in question are mental states. This is where Jaspers’ notion of ‘un-understandability’ comes in. Un-understandability is introduced by Jaspers precisely to signal that in some circumstances the search for understanding must be replaced by the search for psychophysical correlations.

Criticisms of criticisms aside, what does seem obvious is that the dividing line between psychopathology and normality can only be arbitrarily drawn. This suggests that Owen et al are really advocating, not extricating psychiatry from the humanities, but extricating humanity from the humanities. Ridiculous as this may seem, it should come as no surprise since it is what most biological psychiatrists secretly think is possible anyway.


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