Correspondence

EDITED BY MATTHEW HOTOPF

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Patients and clients

We write to register our disquiet at the use of the word ‘client’ instead of patient in the pages of the British Journal of Psychiatry (e.g. Marsden et al, 2000). The choice of words has far-reaching consequences, and we believe that this change in usage represents a shift in our relationship with patients, and erodes professional standards. We would like to point out that patients themselves prefer to be called patients, rather than clients, customers, consumers or service-users (Upton et al, 1994).

The word client is derived from the Latin verb to hear or obey and referred to a commoner who was under the protection of a patron – a relationship that was by no means equal. The current dictionary definition captures this relationship well and gives neither side their proper dignity. The Oxford English Reference Dictionary (Pearsall & Trumble, 1999) defines client as ‘a person using the services of a lawyer, architect, or professional person other than a doctor, or of a business; a customer’. It is a term also used by other, not always very professional, callings.

We note the word is already used widely in documents from the Department of Health, The Sainsbury Centre, and other social research and planning groups but, none the less, we think that it should be resisted, particularly in our own professional documents. We ask the Editor to address this as a matter of policy. We would also like to encourage other psychiatrists and professions related to mental health, including managers, to use the word ‘patient’ in their local documents, as we have succeeded in doing in our own Trust.

This is a change in the use of language which only serves to muddy the waters of our professional work. Not to address this issue, whether through exhaustion or a misguided attempt to please non-medical colleagues, is to collude with this process. Our relationship with our patients is the cornerstone of clinical practice, and we should be fighting to maintain it.


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Editor’s response: Although the Journal generally uses the word ‘patient’, it is not our policy to censor an author’s use of the word ‘client’ if it has been expressly and consistently used, on the assumption that the author has used that word for a good reason. There are people who, in some circumstances, accept the professional care of a psychiatrist but who do not regard themselves as patients. For example, people seeking treatment for drug dependence are unlikely to see themselves as having a ‘mental illness’ and thus may not see themselves as patients. I think we would be in danger of misrepresenting a proportion of users of mental health services (and antagonising a proportion of authors) if one word or the other were outlawed in the Journal. Instead, I believe authors are careful in their choice of language, reflecting their concern for the feelings of the people with whom they work.

Psychiatric features of vCJD similar in France and UK

We report a case of new variant Creutzfeldt–Jakob disease (vCJD) in a French 36-year-old woman. The patient was initially admitted to our psychiatric department in the suburbs of Paris. She presented with psychiatric symptoms for 6 months: major depressive disorder and personality change including apathy, emotional lability and infantile affect. There were no specific psychiatric features allowing distinction from common depressive disorders. Drug treatments, clomipramine (125 mg/day) and venlafaxine (200 mg/day), were used with no benefit. She subsequently presented with delusions and auditory hallucinations that occurred transiently over a period of some hours. The delusions coincided with the onset of cognitive impairment: disorientation and memory impairment. Electroencephalogram showed non-specific slow-wave activity.

Neurological symptoms developed 7 months after the onset of depressive symptoms and included cognitive impairment, ataxia, myoclonus, excessive daytime drowsiness and headache. The patient tested negatively for the P14.3.3 protein in cerebrospinal fluid. She had no history of potential iatrogenic exposure. She was referred to a neurology department. Imaging investigations and neuropathological confirmation by cerebral biopsy have been detailed elsewhere (Oppenheim et al, 2000). The patient died in a state of akinetic mutism.

The psychiatric features of this French case of vCJD are clinically consistent with the cases identified in the UK (Zeidler et al, 1997; Allroggen et al, 2000; Will et al, 2000). The patient fulfilled diagnostic criteria for vCJD used by the National CJD Surveillance Unit (Will et al, 2000). The relationship between vCJD and bovine spongiform encephalopathy highlights the need for clinical surveillance in France as in the UK.


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**Book reviews in the electronic age**

Doctors, perhaps more than any other professionals, need to keep abreast of their subject. They need every encouragement to do this. Crown et al (2000) gave us a valuable reminder of the importance of book reviews in stimulating interest in new publications. As a reader, I enjoy a mixture of reviews, from reviews of leading textbooks to a review of a biography of Iris Murdoch, who suffered from dementia (Garner, 2000). Book reviews are a valuable part of the Journal.

But technology has moved on and the printed word is no longer the only way in which doctors obtain information or exchange ideas. The internet is increasingly becoming the first recourse for doctors seeking information. It is also being used more and more by patients, who come to the doctor clutching triumphantly printouts of material downloaded from the web. A patient may not always fully understand the latest paper he or she has found on the website of an American university, but will soon lose confidence in the doctor if he or she is entirely unaware of it.

There are hundreds, if not thousands, of medical websites, and the busy practitioner needs guidance as to which are worthwhile and which are not. It would be an immense service to readers if the Book Review Editors could extend the valuable work they do to encompass the new medium of the internet.


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We were pleased to see that Crown et al (2000) are considering bringing us into the 21st century with their suggestion regarding the review of websites. We feel that the proliferation of unreviewed information is a potential source of patient distress and general confusion. One way forward would be for authorities (e.g. a national medical association) to review sites and give ‘seals of approval’. There are several rating scales for websites, all with varying quality criteria and little testing for reliability and validity (Jadad & Gagliardi, 1998). There are also concerns about possible litigation if a reviewing authority gives a bad review, for example, and the site loses traffic as a result (Terry, 2000). Although “Rating the quality of medical websites may be impossible” (Delamothe, 2000), we think it is useful to have some framework within which individuals can think about websites and compare their value for their own particular information needs.

We have been developing a standardised format to assess websites, looking at two main areas in a more qualitative way.

First, ease of operation and layout, scored on a visual analogue scale. Second, an assessment of content under such headings as quality issues, provenance, and frequency of updates. The general public and the media should have some guidance as to which websites should be taken seriously and which should be avoided at all costs. When our project is complete we will seek publication conventionally and on our own website (www.ask-a-psychiatrist.co.uk).

Few people have access to psychiatric textbooks and libraries, but access to the web is likely to become almost universal in the developed world over the next few years. We should take advantage of this opportunity to promote the understanding of mental health issues by encouraging people to access reliable sources of information.


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**Evolutionary theory and psychiatry**

While we welcome Abed’s (2000) editorial and endorse the arguments presented in it, we feel that it gives an inadequate account of the important British contribution to the burgeoning new discipline of evolutionary psychiatry. Instead, it leaves one with the impression that Darwinian psychiatry is largely an American achievement and that Abed and Marks are the only home-grown contributors to it. This is unjust, as the application of evolutionary theory to psychiatric disorders began in this country 40 years ago at a time when American psychiatry was still in thrall to psychoanalysis and behaviourism. Bowlby’s (1958) classic paper on the phylogeny of the child’s bond to its mother and his life’s work on the consequences of rupturing that bond for later psychopathology were seminal contributions to the development of the new evolutionary paradigm, as were Price’s papers in the 1960s on social dominance and the evolution of mental illness (e.g. Price, 1967; further references in Stevens & Price, 2000a). Since then, British psychologists and psychiatrists have contributed as much to this fascinating field of enquiry as our American colleagues, whom Abed cites. In particular, we would draw attention to the work of Crow (1995) on the Darwinian approach to the origins of psychosis, Gilbert (1992) on depression and the evolution of powerlessness, and our own work on the evolutionary basis of the major neurotic, ‘functional’ psychotic, and reproductive disorders (Stevens & Price, 2000a), and the phylogeny of schizophrenia (Stevens & Price, 2000b). In addition, significant work has been published by Archer (1992) on ethology and human development, Bailey (1987) on human paleoanthropology, and Birtchnell (1993) on how humans relate.

A major criticism advanced against ‘adaptationist’ explanations of psychiatric disorders is that they are untestable and therefore of little use; this is untrue. We have responded to this criticism by providing testable predictions based on evolutionary insights (Stevens & Price, 2000a, pp. 258–274). As Abed rightly suggests, scientific method requires “a theoretical framework that generates testable predictions”, that “demands what questions to ask” and that “suggests what avenues of research are likely to bear fruit”. Evolutionary psychiatry is now sufficiently advanced in this country for this programme to be implemented. Unfortunately, an uninformed reader would not have gathered this from Abed’s editorial.

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Near-fatal methylphenidate misuse
We report a near fatality of a 15-year-old girl who took one 10 mg tablet of methylphenidate supplied to her by a boy who was legitimately receiving it for hyperkinetic disorder. Both were residents in a local secure unit. Later investigation revealed that the boy would accept his supervised dose and then secrete it into his palm. After ingestion of the tablet the girl described seeing birds and stars. She suddenly collapsed and developed cyanosis. Her breathing stopped and artificial respiration was required for several minutes before spontaneous respiration resumed. There was difficulty finding her pulse and her blood pressure and temperature were elevated. She was hospitalised and recovered fully within 24 hours. She was amnesic of the episode but did recall receiving a tablet from the boy concerned. Blood investigations were normal. Urine toxicology, including a specialised test requested for methylphenidate, revealed no methylphenidate or other drugs including 3,4-methylenedioxyamphetamine (MDMA; ‘ecstasy’). This was consistent with a small methylphenidate dose, rather than an overdose, having been taken.

The most comfortable way of interpreting this incident was that it was a dangerous idiosyncratic reaction to methylphenidate and that these do occur. Also, the clinical signs of methylphenidate poisoning (i.e. hyperpyrexia, delirium, respiratory depression, convulsions and cardiac arrhythmias) are similar to those of ecstasy poisoning (Solowy, 1993). The latter was ruled out by appropriate tests.

This incident demonstrates that methylphenidate, contrary to some popular assertions, can be a drug of abuse. Oral misuse has rarely been reported, as this medication is not particularly effective in producing a ‘high’ (Garland, 1998). However, there have been reports of methylphenidate-induced euphoria in children misusing it (Corrigall & Ford, 1996). Increasing use of methylphenidate makes it likely that misuse of this nature will occur. Methylphenidate and ecstasy toxicity have been found to have similar manifestations. Education of clinicians and families in awareness of these risks and the resulting effects is important.


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National Service Framework and suicide
The National Service Framework (NSF) for Mental Health has identified that the reduction of suicide by one-fifth by 2010 is both a national priority and a high-level performance indicator (Department of Health, 1999). However, relying on suicide rate as a parameter, and in isolation, could be criticised for being inappropriate as a performance indicator of mental health, and for being difficult to monitor and interpret at local level because of the small number of such events.

Counting the number of occurrences of an event that happens relatively infrequently, such as suicide, given the number of times that it could happen, follows a Poisson distribution. A Poisson distribution is specified by just one parameter – the mean. So, for example, if we know that in North Cheshire (population 350 000) the suicide rate is 11 per 100 000 with an annual average of 39 reported incidents, we can calculate the probability that there will be no suicides or a certain number of suicides within a year (Ryan et al, 1985) (Table 1).

According to Table 1, the probability of there being ≤35 suicides in a year within North Cheshire is P=0.29, meaning that there is a 29% chance that the number of suicides could be reduced from 39 to 35 (10% reduction) by chance alone. On the other hand, the probability of having 45 suicides or more in North Cheshire in a given year is P=(1–0.85)=0.15, which means that there is a 15% chance that there could be an increase in the number of suicides from 39 to 45 (15%) per year by chance alone. Neither the increase nor the reduction in the probability of suicide depends on any specific changes in the locally available mental health services.

The NSF target of 20% reduction in suicide rate means that in North Cheshire the rate will come down from 11 per 100 000 (i.e. a reduction of 8 suicides per year from 39 to 31) by 2010. Again, looking at the Poisson probability function, one can expect a 15% chance (P=0.147) of 31 suicides or fewer occurring by chance alone. This means that we have a 15% chance of achieving the NSF target in reduction of suicide by good luck alone and without any effective psychosocial intervention or change in practice.

One may conclude that some change in suicide rate is expected to occur, and indeed does occur, within the same locality and independently of the mental health services, and is of no real significance. This could plausibly imply that a reduced suicide rate may be, at least partly, a statistical illusion due to reasons totally unrelated to the effectiveness or quality of psychiatric care. What is needed is a valid quality control model (similar to ‘control charts’ used so effectively in industry), and not just a single parameter, in order to reflect the complexity of suicide. Variables such as staffing

Table 1 Cumulative distribution function for suicides in North Cheshire

<table>
<thead>
<tr>
<th>Suicides in region (n)</th>
<th>P</th>
</tr>
</thead>
<tbody>
<tr>
<td>≤ 15</td>
<td>&lt;0.0001</td>
</tr>
<tr>
<td>≤ 20</td>
<td>0.0006</td>
</tr>
<tr>
<td>≤ 25</td>
<td>0.0113</td>
</tr>
<tr>
<td>≤ 30</td>
<td>0.1472</td>
</tr>
<tr>
<td>≤ 35</td>
<td>0.2939</td>
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<td>≤ 40</td>
<td>0.6446</td>
</tr>
<tr>
<td>≤ 45</td>
<td>0.8508</td>
</tr>
<tr>
<td>≤ 50</td>
<td>0.9633</td>
</tr>
</tbody>
</table>

I. Assuming Poisson distribution, mean 39, base population 350 000.
levels, consultant vacancies, resources, community psychiatric nurses, beds, unemployment, housing, education, crime, alcohol and drugs, inflation index, morbidity, levels, deprivation, etc. should be included and adjusted for within the model, so that we may interpret some genuine changes in suicide rates in a realistic and meaningful way, locally and nationally.

Unfortunately, we will never be sure of the number of suicides that we actually prevent every day, but we will always remember, or be reminded of, those that we fail to prevent.


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**What constitutes intensive treatment?**

Burns *et al* (2000) refer to 'programme fidelity' as an important issue in assertive community treatment. According to their data, the intensive case management (ICM) group received a mean of 3.35 face-to-face contacts per 30 days with a mean duration of 40.6 minutes. This works out at 136.0 minutes per 30 days or 31.7 minutes per 7 days. I do not regard seeing a patient for 30 minutes a week, or an hour a fortnight, as 'intensive'.

The original paper by Stein & Test (1980) clearly indicates that an essential part of their 'training in community living' programme was active work with community members: both informal carers and other lay people who came in contact with patients (e.g. employers). Burns *et al* give a figure of 0.13 contacts per 30 days (0.03 contacts per week, or one contact every 230.7 days). There is no mention of contacts with other lay people. Again, this must be regarded as falling well short of the Stein and Test model.

Burns *et al* clearly state that most of the activity outcomes were highly skewed and statistically not a normal distribution. This is evident from the fact that many of the standard deviations are of similar size to the mean. This inflates the mean value of the events and thus the average face-to-face contact. It would be interesting to plot out the total duration of face-to-face contact with each patient against the treatment outcome, to see whether there is any beneficial effect from higher levels of face-to-face contact, or possibly even a threshold effect.

From the practical point of view of implementing assertive outreach, I am puzzled by the activity data. For a case manager to give 31.7 minutes of face-to-face contact with a patient every 7 days multiplied by a case-load of 15, would occupy 475.5 minutes or 7.93 hours per week. We are not given an average duration for the non-face-to-face contacts, but if one assumes 30 minutes for each of the other types of contact, we get a figure of 3.8 minutes per patient every 7 days; with a case-load of 15 this comes out at 87 minutes or 1.45 hours per week per case manager. This accounts for 9.38 hours per week. Assuming a 40-hour working week, this leaves over 30 hours per week unaccounted for. Even with travel time and leave, there does seem to be rather a lot of unaccounted time. Is there an important component of assertive community treatment (ACT) we are not being informed about?


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**Authors’ reply:** Dodwell raises three important questions in his letter. How intense is intensive? Can levels of contact be related to outcome? What are the staff doing with the rest of their time?

Before addressing these, we would like to reiterate the purpose of our paper. We set out to determine whether the ICM teams really did achieve a different way of working and make more frequent patient contact than standard treatment (SCM) teams. There has been doubt expressed about this in the past and our study is able forcefully to reject these doubts. Any failure to demonstrate differences of outcome in the UK700 study (UK700 Group, 1999) cannot be attributed to a failure of the ICM staff to establish more intensive contact with their patients. Our paper also confirms that they were more persistent in their follow-up and involved carers more. The St George’s group, who were clearly influenced by Stein and Test (Stein & Test, 1980), established a mean contact frequency near to their (St George’s) target of two per week.

How intense is intensive? We do not know and, as far as we can ascertain from published scientific literature, neither does anyone else. Although there are published quality standards and targets for contact frequency (Teague *et al*, 1998), we found no publications of prospective data. Our figures appear low and this, in part, reflects the very rigorous and conservative approach we took to data collection. Data were also collected in the early stages of these teams’ functioning and would probably underestimate the contact frequency of a mature team. We know from work in other areas, however, that clinicians usually overestimate clinical activity when judged retrospectively. One of us (T.B.) has visited several demonstration ACT teams in the USA and from a clinical impression would not consider the St George’s team’s current contacts of around 25 per week per case manager to be much below that in good US teams.

Can levels of contact be related to outcome? Our means do conceal considerable variation, with some patients only being seen monthly (often during a prolonged period of engagement) and some being seen daily for long periods. Low contact can as easily represent severe problems with engagement as it can superior adjustment and fewer clinical needs. Some of the patients with the worst outcomes had the most contact because they were so ill. We have not attempted to test this correlation because of the difficulty of developing a convincing hypothesis – we would not hypothesise that there is a linear relationship between contact and outcome.

What are the staff doing with the rest of their time? This is surely a general question rather than one about ICM. The SCM staff recorded about as much time per week if their case-loads are accounted for. Many phone calls were unrecorded because they were short and there is considerable travel time involved in community mental health work. Staff also attended ward rounds, team meetings, supervision, etc. We had anticipated that mental health staff would spend about 50% of their working
time in direct clinical contact. Our study suggests that this may be something of an overestimate.

Virtually all of the major outcome papers from the UK700 study have attracted correspondence implying a partial implementation of good practice (McGovern & Owen, 1999; Gournay & Thornicroft, 2000). Whenever presented at meetings the results generate very strong feelings because they do not bear out what advocates of this approach want to hear. Our critics are confident that they know what goes on in ACT teams and other forms of assertive outreach. However, detailed exploration of the literature in this area for a PhD (M.F.) fails to find evidence for even such basic questions as ‘how intense is intense?’ Numerous policy statements about what is desirable, yes – but evidence of what happens, no. It is the purpose of research to replace conviction with knowledge. In the area of assertive outreach this is sorely needed. The UK700 study overall, and this paper in particular, helps reduce the gap between rhetoric and reality.


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**One hundred years ago**

**Asylum reports – Scottish Royal Asylums**

_Dr. Urquhart refers to the dangers that are accompanying the fashionable drugs which are now so accessible to the public. “The abuse of such substances as antipyrin, which seems to have taken its place in the domestic medicine chest, to the detriment of the race, is almost as formidable as the indiscriminate and continuous unauthorised dosing with sulphonal and cocaine. Valuable as these remedies are when appropriately prescribed, each entails its own special dangers. As soon as an anodyne or a soporific comes into general use, the results are recorded in the statistics of our medical institutions. We have lately reported a death consequent on a relatively small dose of sulphonal, and apparently due to its disorganising effect on the system. This drug was placed before the public as an absolutely safe hypnotic not many years ago, and it is now used with a freedom which is perfectly appalling; yet it has not been ascertained in what cases sulphonal is eminently dangerous, or where an idiosyncrasy exists forbidding its administration. We have also had under treatment a patient who fell a victim to that insidious drug cocaine. Consequent on the relief experienced, he was enabled for a time to carry on an extensive business; but, while thus deadening the pain of persistent neuralgia, he was only treating a prominent symptom, without combating the underlying causes of his malady.”*

**REFERENCE**

_Journal of Mental Science, January 1900, XLVI, 191–192._

Researched by Henry Rollin, Emeritus Consultant Psychiatrist, Horton Hospital, Epsom, Surrey

**Corrigendum**

Disclosing the diagnosis of dementia (letter), _BJP_, 177, 565. The authors’ names should read: A. Ahuja, D. D. R. Williams.
Patients and clients
P. Calloway, C. Denman, N. Hymas and C. Lawton

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