

Authors' reply: We are delighted that these responses to our editorial expand on issues that we could not explore more fully. Tulloch gives a cogent account of the typical methodology – not now being followed in England – for deriving case-mix groupings and finds the present plan wanting. He suggests a slow, careful change to commissioning based on activity and case-mix. Kingdon *et al* make the case for a system in which both diagnosis and care pathways are central in costing and thus purchasing, only *en passant* asking the crucial question of how (not whether) cost can be firmly linked to the quality of services delivered.

Both letters focus on which type of data should be chosen. The intended benefit of case-mix systems is to improve the direction of resources towards the greatest local need. If that were the only eventual use of the data items under discussion then mental health units should collect whichever (activity counts, clusters, diagnoses, pathways, etc.) best satisfy criteria such as Fetter's, as Tulloch implies. But data, once collected, have many other uses and misuses.

Kingdon *et al* argue on theoretical grounds that diagnostic categories should be better indicators than clusters of the type and quantity of care that is required by patients. Yet as Tulloch points out, findings from international analysis of variance studies of actual resource consumption within diagnostic groupings have tended to lead to their abandonment. We can add that similar methodology was used in mental health services in England from the early 1990s by the National Health Service Information Authority, testing both diagnostic and multidomain descriptors of patients' problems, in national and multi-site trial data-sets.¹ Diagnostically defined healthcare resource groups were abandoned by the Department of Health, not only because of the modest reduction in variance achieved, but also because of resistance by non-psychiatrists to the collection of diagnostic data. There was also resistance to informatics in general by a substantial proportion of clinicians, including senior Royal College of Psychiatrists' leaders at the time, although that is no longer the case.² Clusters were seen by policy makers as more likely to be acceptable. The fact that they become mandatory on 31 December 2011 with only this discussion in the *Journal* suggests that this approach is working.

As Kingdon *et al* point out, diagnostic categories enable us to use therapeutic research findings to decide which type of drug or psychosocial approach is chosen, but that does not much affect overall costs, and people often retain the same diagnosis throughout many life changes. By contrast, multidomain scores include more factors that indicate whether someone currently needs admission or frequent contact with paid professionals, which are the main financial determinants. And since the mental health clustering tool (MHCT) includes symptomatology ratings, and separates clusters into broad diagnostic groups anyway, the statistical benefits of diagnosis have not been entirely lost. Until there is more empirical evidence from costing studies, the relative merits of diagnostic versus multidomain data will remain debatable. Of course their value in outcomes and other quality monitoring, and predicting prognosis, must also be considered in developing mental health informatics generally. Prognosis is important because there is more 'value' in resolving a situation that would otherwise become chronic.

We do not support the automatic assignment of patients to any form of treatment, pathway or package of care on the basis of MHCT scores alone. The data may raise retrospective questions about clinical judgements, but should not replace them.

So what should we be doing about commissioning? Tulloch suggests in effect returning to the 1993 position and starting again. We do not think this is possible; while we looked away, boats were burnt. Kingdon *et al* propose the combination of diagnoses with

pathway data for costing purposes, but do not say quite how. The strong argument against using intervention counts, pathway data or other activity measures on their own for remuneration is that there is no safeguard against unnecessary, ineffective or inefficient interventions or pathways. Tariff 'matrices' in which prices are applied to cells containing both broad diagnoses and clinical management data have been proposed in the past,¹ but as we said above, they were abandoned. The large number of resulting categories should theoretically reduce costing variance, but it may be that commissioners would not in practice be able to use them effectively.

Yeomans concentrates on Routine Clinical Outcomes Measurement (RCOM), arguing strongly for its development and enhancement, while wisely refraining from almost suggesting 'Payment by Outcomes', which would violate Goodhart's law, succinctly put by Strathern: 'When a measure becomes a target, it ceases to be a good measure.'³ We agree with nearly all his points, especially on the importance of feedback, which are, notwithstanding the dated survey he quotes, already coming to pass in some parts of England, as are developments in Patient-Reported Outcomes Measures (PROMs). Efforts to usefully involve HoNOS in clinical work itself are being reported, although from the other end of the earth.⁴ As he says, HoNOS are a start but not the last word in outcomes measures, and we would caution against using them for thresholds for referral or discharge. Validity in groups is no guarantee of validity in individual cases.

With exceptions, we have been slow to grasp the twin nettles of outcomes and costing of services, and if we are to regain the initiative, we have to think widely and deeply about what systems we think will work best for service users, even while change in these very systems is accelerating. A start would be made when trusts have clinical, outcomes, intervention, costing, human resource and finance data on the same spreadsheets for themselves.

Declaration of interest

A.M. receives payment for training in HoNOS65+.

- 1 Carthew R, Elphick M, Page A. *Report on the Development of Mental Health Groupings*. NHS Information Authority, 2003.
- 2 Royal College of Psychiatrists. *Information-based Funding, Quality and Outcomes for Mental Health: Statement of Principles*. Royal College of Psychiatrists, 2009.
- 3 Strathern M. 'Improving ratings': audit in the British University system. *Eur Rev* 1997; 5: 305–21.
- 4 Stewart M. Making the HoNOS(CA) clinically useful: a strategy for making HoNOS, HoNOSCA, and HoNOS65+ useful to the clinical team. *Aust NZ J Psychiatry* 2008; 42: A5.

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Dissociation: a valid concept?

I was saddened by Harold Merskey's review of the second edition of *Attachment, Trauma and Multiplicity: Working with Dissociative Identity Disorder* (edited by Valerie Sinason).¹ My sadness was not primarily caused by his critical assessment of some of the material presented, but by his inference that dissociative identity disorder and dissociative disorders in general do not exist. Anyone unfamiliar with dissociative disorders reading his comments would be forgiven for being persuaded of this.

Dissociative disorders have been recognised in both DSM-IV and ICD-10 for some 25 years now. Yet among psychiatrists in particular, they continue to be denied or misdiagnosed, causing serious re-traumatisation for a significant number of patients.

Merskey writes of the absence of 'critical statement[s] by a professional society', but fails to cite the acknowledged leaders in the field, the International Society for the Study of Trauma and Dissociation (ISSTD; www.isst-d.org) and the European Society for Trauma and Dissociation (ESTD; www.estd.org). The ISSTD includes among its members a number of eminent psychiatrists and psychologists and it has produced extensive online guidelines on treatment. The charity First Person Plural, in association with the ESTD and Cheshire & Wirral Partnership NHS Foundation Trust, has produced a training and information DVD.²

Furthermore, the National Institute for Health and Clinical Excellence's guidelines accept the existence of dissociative disorders. It has not yet produced a treatment protocol for this condition and recommends that clinicians follow the guidelines of the best informed organisation (www.isst-d.org/education/treatmentguidelines-index.htm).

It should be noted that many psychiatric services and community mental health teams across the country are now implementing treatment protocols for dissociative identity disorder and dissociative disorders that are not only effecting significant changes for patients but are also bringing about cost savings for services.³

Declaration of interest

R.A. is President of the European Society for Trauma and Dissociation.

- 1 Merskey H. Book review: *Attachment, Trauma and Multiplicity: Working with Dissociative Identity Disorder*. *Br J Psychiatry* 2011; **199**: 347–8.
- 2 First Person Plural. *A Logical Way of Being: The Reality of Dissociative Identity Disorder and Other Complex Dissociative Conditions* (DVD). FPP (<http://www.firstpersonplural.org.uk>).
- 3 Lloyd M. How investing in therapeutic services provides a clinical cost saving in the long term. *Health Serv J* 2011; 1 Sept.

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Author's reply: Dissociation begins with hypnotists, was developed by Janet, promoted by Freud and ruined by the absurdities of multiple personality disorder.¹ Consider Janet² hypnotising 'Lucie', an alternative personality of this patient producing automatic writing:

- Q. 'How are you?'
A. 'I don't know.'
Q. 'There must be someone there who hears me.'
A. 'Yes.'
Q. 'Who is it?'
A. 'Someone other than Lucie.'
Q. 'Ah, Indeed!'
A. 'Another person.'
Q. 'Would you like us to give her a name?'
A. 'No.'
Q. 'Yes it will be more convenient.'
A. 'Alright, Adrienne.'
Q. 'Very well Adrienne. Do you hear me?'
A. 'Yes.'

In 1889 Binet observed that Janet '... himself created her by suggestion'.³

Hacking⁴ showed that the first 19th-century fugue states in young men were in French military conscripts exploiting the novel

long-distance continental railways. In older persons fugues are only found with dementia. Experimental attempts by excellent social psychologists over 60 years have completely failed to replicate repression⁵ and dissociation. Freud's own accounts of his cases with alleged repression/dissociation were completely unreliable,⁶ particularly as shown in the Freud–Fliess correspondence.⁷ Further, Pope *et al*⁸ have shown that a phenomenon like dissociation (i.e. losing complete trace of some important event and then recovering it through memory) has not been found so far in world literature preceding 1786, and by then Mesmer was actively using hypnotic procedures. If dissociation is a genuine human experience, it is remarkable that it was not known before that time.

There is no case of proven 'dissociation' fulfilling Pope's criteria without organic disorder, although many cases of alleged dissociative memory loss exist, not to mention the generally rejected syndrome of dissociative identity disorder, of which dissociation is the artefactual foundation no matter how much the name or term may be changed.

- 1 Merskey H. The manufacture of personalities: the production of multiple personality disorder. *Br J Psychiatry* 1992; **160**: 327–40.
- 2 Janet P. *L'Automatisme psychologique*. Felix Alcan, 1889.
- 3 Binet A. *Les altérations de la personnalité*. Felix Alcan, 1892. Reprinted as *Alterations of Personality* (ed DN Robinson). Georgetown University Publications of America, 1977: 146.
- 4 Hacking I. *Mad Travelers. Reflections on the Reality of Transient Mental Illnesses*. University of Virginia, 1998.
- 5 Holmes DS. The evidence for repression: an examination of 60 years of research. In *Repression and Dissociation. Implications for Personality Theory, Psychopathology, and Health* (ed JL Singer). University of Chicago Press, 1990: 85–102.
- 6 Grunbaum A. Made-to-order evidence. *Unauthorized Freud: Doubters Confront a Legend* (ed FC Crews). Viking, 1998: 76–84.
- 7 Masson JM (trans & ed). *The Complete Letters of Sigmund Freud to Wilhelm Fliess, 1887–1904*. Belknap Press, 1986.
- 8 Pope Jr HG, Poliakoff MB, Parker MP, Boynes M, Hudson JI. Is dissociative amnesia a culture bound syndrome? Findings from a survey of historical literature. *Psychol Med* 2007; **37**: 225–33.

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Childhood sexual abuse and chronic fatigue syndrome

We have read the important article on the premorbid risk markers for chronic fatigue syndrome in the 1958 British birth cohort¹ with a lot of interest. The authors reported that parental physical abuse, childhood gastrointestinal symptoms and parental reports of many colds were independently associated with self-reported chronic fatigue syndrome (CFS), after adjusting for psychopathology.

Notably, the authors did not comment on the fact that parental physical abuse, but not sexual abuse, was predictive of CFS, even though childhood sexual abuse is a well-documented risk factor for CFS. More precisely, chronic fatigue was significantly predicted by childhood sexual abuse in a population-based study by Taylor & Jason.² Also, childhood sexual abuse and emotional abuse were most effective in discriminating CFS cases from control individuals in two population-based studies by Heim *et al* (as well as emotional neglect in one of these studies).^{3,4} A possible reason for this inconsistency is the relatively low frequency of sexual abuse in the study by Clark *et al*¹ (6.3%), compared with its frequency in the others studies (>26%).^{2–4}