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Link between outcome and service quality is not clear

In their study of rehabilitation services in England, Killaspy and co-authors conclude that ‘Quality of care is linked to better clinical outcomes in people with complex and longer-term mental health problems’.

We are not persuaded that this conclusion is justified by the results reported.

The study investigated the relationship between measures of quality and clinical outcomes across a number of services at different locations in England. The scales used to measure outcomes consisted of the Resident Choice Scale (RCS), the Manchester Short Assessment of Quality of Life (MANS), the Your Treatment and Care (YTC), the General Milieu Index (GMI) and the Global Assessment of Functioning (GAF).

Our main argument is that of these the RCS, YTC and GMI are actually measures of the service user’s perception of the quality of care rather than measuring what most people would understand by the term ‘clinical outcome’. Although these measures are widely referred to as outcome measures, we argue that the term ‘clinical outcome’ should refer to factors such as symptom level, relapse rates, functioning, carer burden and quality of life. The RCS, YTC and GMI scales were reported to be associated with the objective measures of service quality. However, we would argue that this result simply demonstrates that patients are able to make a valid judgement about the quality of the services they receive.

On the other hand, we agree that the MANS and GAF are indeed measures of clinical outcome. However, the MANS, which does measure quality of life, did not show any association with measures of service quality and the authors did not report any results for the GAF.

Based on these findings we are unable to agree with the authors’ conclusion that they have demonstrated an association between service quality and clinical outcomes. This negative finding is aligned with that of a study that they cite and which failed to demonstrate an improvement in outcomes following the implementation of the quality and outcomes framework for diabetes.

It seems that it is difficult to convincingly demonstrate a real relationship between care quality and clinical outcomes. However, we would argue that attempts to do this should use outcome measures of real relevance, such as the Health of the Nation Outcome Scales (HoNOS), GAF and MANS.

Authors’ reply: On the basis of their definition of outcome, Vincent & Curtis challenge the conclusions of our national study of mental health in-patient rehabilitation services where we found that the quality of services was positively associated with better clinical outcomes. In doing so, they seem to adopt a medical model and ignore important insights arising from patient-centred concepts of recovery. They regard ‘symptoms, functioning and relapse’ as key, whereas a recovery-oriented approach would also value the quality of therapeutic relationships, the promotion of autonomy and better quality of life.

Our aim was to try to bridge this ideological divide by placing patient-reported outcomes on an equal footing with so-called clinical measures. At the risk of excessive pedantry, it would be fair to say that measures of patient experiences of care, such as those we used in our study, could be considered assessments of process rather than outcome, although they are nevertheless an important aspect in the measurement of service quality. In any case, such measures are usually referred to as patient-reported outcome measures. We found a positive association between our service managers’ assessments of the quality of their own services (using our standardised measure, the Quality Indicator for Rehabilitative Care, QuIRC) and patients’ experiences of care. This corroborates the service managers’ ratings and strongly suggests that improving service quality will result in a better service user experience – surely an ‘outcome’ everyone can relate to as worthwhile.

We also found that greater quality of mental health rehabilitation services was associated with greater service user autonomy. We gave a number of possible reasons why we did not find a positive association between service quality and service user quality of life, the main one being that the measure we used focuses on experiences outside of in-patient setting (relationships with family/partner, work, income, etc.). Our findings reflect the reality for people in in-patient mental health rehabilitation units who tend to have lengthy admissions (in our study, their current admission was 18 months on average, with 8 of these in the rehabilitation unit) due to the severity and complexity of their symptoms and severe impairment of social functioning, all of which have a negative impact on their social inclusion and quality of life. Nevertheless, it is absolutely correct that rehabilitation services should (and do) aim to help service users achieve a successful community life which, ultimately, should be reflected in their quality of life.

Our findings reflect the focus of our study – in-patient mental health rehabilitation services deal with individuals at the beginning of their rehabilitation, when they are most severely unwell and least able to engage in the community. Later phases of our research will provide further data on the longitudinal outcomes, including social functioning and successful community living.

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Missing ethnic density data

To investigate the concept of ethnic density one would have to draw a sample representative of the rich diversity of ethnicities in England. Modood & Berthoud define ethnic group as:

‘a community whose heritage offers important characteristics in common between its members and which makes them distinct from other communities. There is a boundary, which separates “us” from “them”, and the distinction would probably be recognised on both sides of that boundary. Ethnicity is a multi-faceted phenomenon based on physical appearance, subjective identification, cultural and religious affiliation, stereotyping, and social exclusion.’

The study by Das-Munshi et al included White British, Irish, Black Caribbean, Bangladeshi, Indian and Pakistani respondents. In the EMPIRIC study, ethnicity was defined by self-assessment using the same categories as the 1991 Census. The 1991 Census collected data on nine ethnicity groups: White, Black Caribbean, Black African, Black other, Indian, Pakistani, Bangladeshi, Chinese and any other ethnic group. Das-Munshi et al’s study has not looked at four of the nine ethnic subgroups listed and is therefore not representative of the ethnicities in England.

Das-Munshi et al have not explained the significant non-participation of candidates (37.8%) in the study, making it difficult to draw conclusions from the data. Owing to various factors – including that of participants having moved out of the survey area or to an unknown new address, or the participant having died or reached the age of 75 after the sample was drawn – 11% of the sample was no longer eligible for interview.

Of eligible individuals, refusals were received in person directly from the selected participant in 18% of cases, a further 2% refusing by post and 2% being proxy refusals. Most non-contacts resulted from the interviewer being unable to make contact with the participant, although there were also cases where no contact was made with anyone at the household after four or more telephone call attempts. The most common reason for any unproductive outcome was that the participant was away on holiday or in hospital throughout the survey period.

The issue of non-response has been discussed in the Method under ‘Survey design.’ Weights to account for non-response bias were derived using stepwise logistic regression techniques using data which were available from previous Health Surveys for England. A number of important demographic predictors of non-response were included in the models (including individual and household predictors as well as primary sampling units). Interactions by ethnicity were also included in the models, in order to account for the differing probabilities of response by ethnic group. As mentioned in our paper, wherever possible we used these survey weights to correct for non-response in our analyses.

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Authors’ reply: Dr Topiwala raises two additional points which we have discussed in our paper. The original EMPIRIC survey omitted a number of ethnic minority groups. We were therefore unable to analyse associations for these groups as we did not have the data. As we mention in the ‘Limitations’ section of our paper, since this survey (2002) there have been a number of other new migrant groups to Britain, for example from Eastern Europe. Therefore we caution that the findings should not be generalised outside of the ethnic minority groups surveyed in our study.

Cardiovascular fitness and serious depression in adulthood

The report by Åberg et al of an association between lower cardiovascular fitness at age 18 and serious depression in adulthood is interesting but their conclusion that the results ‘strengthen the theory of a cardiovascular contribution to the aetiology of depression’ is questionable. The authors recognise that although they controlled for psychiatric symptoms and disorders prior to and at the time of baseline assessment of cardiovascular fitness, they did not obtain information ‘which could help in defining subsyndromal affective problems’, nor details of ‘other possible confounders such as personality, smoking and low self-esteem’ that may increase risk for both poor fitness and depression. Seemingly lower cardiovascular fitness is not a risk factor for bipolar depression (or mania) and may not be a risk factor for females. It is not stated whether consideration was given to psychiatric disorders apart from depression (e.g. anxiety disorders) and this leaves uncertainty as to whether lower cardiovascular fitness at age 18 is only a risk factor for non-bipolar depression in males. Considering that all study participants were sufficiently fit for recruitment into national service, it would be difficult to explain how a mere difference in cardiovascular fitness could (as suggested by the authors) contribute to cause ‘severe’ depression even before the age of 30. Given a recognised relationship between adverse developmental experiences, personality traits and depression, it would be important to obtain information about such potentially confounding variables as well as ‘subsyndromal problems’ at the time of assessing cardiovascular fitness. It is suggested that in light of such missing information, the reported finding does not act as yet ‘strengthen the theory of a cardiovascular contribution to the aetiology of depression’. It is more an interesting association that warrants further investigation.
Authors’ reply: We would like to clarify a few points regarding our conclusion that our results ‘strengthen the theory of a cardiovascular contribution to the aetiology of depression’. First, we talk about a ‘contribution’ which does not necessarily imply a direct causal pathway, stating that ‘our findings are not explanatory with respect to causal chains leading to the onset of depression’. In line with this, we did include a careful discussion about other possible confounding mechanisms, i.e. factors that may increase the risk for both poor fitness and depression – for example, childhood factors, personality, self-esteem and subsyndromal affective problems. By including parental educational level as a confounder and by performing subanalyses within full brother pairs, many of the early childhood risk factors could be accounted for.

Second, as the conscription routines included extensive questions regarding every possible previous and present mental health problem in combination with separate examinations by professional psychologists and physicians, we believe that subsyndromal affective problems were not often overlooked. Also, to further reduce baseline misclassification, we did perform separate analyses excluding incident cases in the first year.

Third, we would like to stress that not all study participants were fit for recruitment into national service, but that the conscription test was used to select suitable recruits. Participation in the conscription tests was compulsory according to Swedish law and exemptions were granted only for incarcerated males and severe chronic medical disabilities (approximately 2–3% of the yearly male population). We can therefore consider our study a population study. After conscription, about 40 000 individuals were considered ‘unfit’ due to a cardiovascular fitness stanine score 1–3. All these ‘unfit’ young men were included in our study.

Fourth, the question of whether cardiovascular fitness may be related to increased risk for other types of psychiatric disorders in adulthood is one that we will continue to pursue in future analyses of the national conscription data.

Taken together, we still argue that the data ‘strengthen the theory of a cardiovascular contribution to the aetiology of depression’, which in our paper stands in direct connection with the sentence: ‘although the results in the present population-based prospective study are compelling, a number of confounders could not be measured and intervention studies are needed to determine whether physical exercise in young adulthood can prevent future onset of depression.’ We fully agree with de Jonge & Roest in their editorial that a greater understanding of the mechanisms underlying these associations, including complex bidirectional models, may provide opportunities and strategies for prevention.


New paradigm: developmental psychopathology

Strong on diagnosis, but weak on prescription, Bracken et al.’s critique of contemporary psychiatry suffers from the very difficulty which they decry. They rightly complain that current paradigms ignore the psychosocial, fail to combat stigma, and that academic psychiatry has little impact on clinical practice. They cogently argue that the relational aspects of treatment, whether avowedly psychotherapeutic or pharmacological, outweigh any supposed specificity in their effectiveness.

Sadly, their remedies are vague and anodyne: encouraging service user involvement, acknowledgement of complexity, taking account of ‘systems of meaning’. Motherhood and apple pie anyone? This anti-psychiatry rehash sounds the retreat rather well, but as a call to arms is feeble; it knows what it is ‘anti’, but lacks a convincing ‘pro’.

Yet there is in fact an exciting way forward, one where academic psychiatry and psychology convincingly combine to enhance work in the clinic. Developmental psychopathology is the current cutting edge, drawing on attachment theory, neuro-imagining and epigenetics. We are beginning to see how developmental experience inscribes itself on the brain, and sometimes on the genome; how the interaction of adverse developmental processes within the social milieu sows the seeds for psychiatric disorder. This provides the intellectual and evidential underpinning for effective psychotherapeutic treatments, which enhance resilience through fostering mentalising and mindfulness skills, promoting a sense of agency, and validating appropriate help-seeking. Psychiatrists-of-the-future’s enthusiasm needs to be fanned by this flowering of environmental neuroscience, rather than doused with thin foam of post-modern angst.


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Territorial disputes are a zero sum game: if one side gains ground, it can only be at the expense of the other. As clinical psychologists, it was therefore with a wry smile that we read the recent paper by Bracken and colleagues, which calls for psychiatry ‘to move beyond the dominance of the current, technological paradigm’ and towards an understanding of mental health problems not as...
diseases of the brain, but as involving ‘social, cultural and psychological dimensions’.

We agreed with much of the paper’s substance, yet found ourselves concerned by the implied route to implementation. Given their audience, Bracken et al. can be forgiven for failing to acknowledge the existence of clinical psychology; yet their arguments owe a great deal to advances, both theoretical and empirical, in this field. By calling for psychiatry to shift its epistemology and praxis, it might seem not only that they want to adopt an alternative philosophy, but quietly to move their tanks onto the lawns of fellow professionals.

One could follow their argument to a different conclusion. If the goal is a mental healthcare system in which problems are seen principally as ‘social, cultural, and psychological’ in origin rather than biomedical, then the case for having medically trained professionals in positions of seniority is substantially weakened. Rather, clinical leadership would need to be provided by people who have received a comparatively extensive training in psychological, social and cultural causes of distress.

Reforming the whole of psychiatry from the inside out can hardly be the most practical means of realising this vision. Instead, consider that there are some 10 000 clinical psychologists in the UK, the majority of whom work in the National Health Service (NHS). A substantial number of psychiatric posts go unfilled, while clinical psychologist posts are being cut and downgraded across the country despite training places being vastly over-subscribed. We could begin by imposing a moratorium on filling psychiatric posts and use the money saved (about £100 million, at a conservative estimate) to reverse the process of downgrading, increase the number clinical psychologists at higher leadership grades and expand the number of training places. That – at zero net cost to the NHS – could help move us towards Bracken and colleagues’ vision.

To be clear, this is not an ‘anti-psychiatry’ argument. We do not dispute psychiatric expertise in several technical areas, principally psychopharmacology. Although the benefits of antipsychotic medication have often been greatly overstated and the utility of diagnostic categories is a source of constant dispute, we would not be among those who deny that psychiatric posts are being cut and downgraded across the country despite training places being vastly over-subscribed. We could begin by imposing a moratorium on filling psychiatric posts and use the money saved (about £100 million, at a conservative estimate) to reverse the process of downgrading, increase the number clinical psychologists at higher leadership grades and expand the number of training places. That – at zero net cost to the NHS – could help move us towards Bracken and colleagues’ vision.

We do not claim to have all the answers and value the work of Professor Holmes, for example in relation to the role of narrative in mental health practice. However, we would caution against any attempts to explain the insights of psychodynamics through the discourse of neuroscience. We fear that this is another example of what the physician and philosopher Raymond Tallis calls ‘neuromania’, a contemporary intellectual fashion which seeks to explain every aspect of the human condition through the terms of neuroscience. One of Freud’s greatest insights was the realisation that relationships are at the heart of mental health work, both in terms of explaining how problems emerge as well as offering solutions. Although neuroscience can offer some speculative ideas, it cannot be used to ground a science of interpersonal dynamics. In reality, human relationships, meanings and values are given their coordinates by the social context in which they exist. This context is deeply textured with cultural, linguistic, political and economic dimensions. It is the product of centuries of human history and simply cannot be grasped with the reductionist logic of biomedicine.

We are not too sure what to make of Professor Holmes’s tone in referring to our ‘encouraging service user involvement’. We would like to reiterate that we do indeed see this as a vital ingredient in any progressive debate about the future of psychiatry.

Kinderman & Thompson support our analysis but seem afraid that we are attempting to create a psychiatry that will seek to colonise the territory of other disciplines such as their own (psychology). This is a misreading of our project and our intentions and we can reassure them that we have no tanks to move onto anyone’s lawn! If human suffering fell neatly into specific domains there would probably be no need for psychiatry at all. Neurologists would deal with the brain and its disorders, endocrinologists would grapple with our hormones and psychologists could work with thoughts and feelings. However, human reality is not neat, and human suffering is often multidimensional. There aren’t discrete domains. At its best, psychiatry involves an attempt to bring medical insights and practices to bear on the complex nature of mental problems. Such problems can emerge through purely psychological pathways but, most often, they involve social, economic, political and biological
We do not accept the accusation that we failed to acknowledge ‘the existence of clinical psychology’, given the number of direct references to psychological research in our paper. Most of our discussion of the literature on counselling and psychotherapy is based on research by psychologists and our discussion of the ‘recovery approach’ points directly to the work of Professor Mike Slade (a psychologist).

We seek a different, not an expanded, psychiatry. We are not colonisers but neither do we believe that the answer is simply to replace psychiatrists with psychologists. Indeed, much of contemporary academic and clinical psychology is also guided by a technological paradigm.

The change we seek is not a replacement of one group of professionals with another. It is about a different ‘way of seeing’ what mental health work is about. Moving beyond the technological paradigm does not involve a rejection of everything we do now. It offers a different way of understanding why some of the things that we do work well, while at the same time appreciating the fact that some people are damaged by the way in which psychiatry frames their problems and intervenes in their lives. Crucially, it involves a rethinking of the nature of mental health expertise and, with this, a commitment to rethinking the power structures of our field.


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