Preventive psychiatry within public health

Thank you, BJPsych, for your timely editorial on population mental health. It is a pity that the authors did not mention the strategy for England entitled No Health without Mental Health, launched in February (before their final submission) as this strategy did involve precisely the teamwork of psychiatrists, public health specialists and economists that they see as ‘the challenge’. Their ‘must list’ for psychiatry begins with help ‘to remedy the consequences of adversity and vulnerabilities’.

A key weakness of UK attempts to address health inequalities has been a failure of leadership – and the common mental disorders show a steeper social gradient than common physical illnesses such as heart disease. Can the Royal College of Psychiatrists take a lead in addressing the antecedents of adversity and vulnerability, not just the consequences? Desolate, impoverished neighbourhoods spawn childhood mental illness and rising unemployment breeds desperate drinking and suicidal despair.

In the original National Health Service Act 1946, maternity services were the exemplar of planning equitable care on the basis of population health needs . . . but in England today many maternity services are at breaking point, with antenatal care services widely sacrificed to maintain staffing for deliveries. The College could speak with unique authority on the need for better antenatal care, to prevent a generation blighted by neurodevelopmental problems.

I suspect that consultant psychiatrists are, on average, better educated, more articulate and able to reflect than, say, Members of Parliament. Urban degeneration, unemployment and the breakdown of comprehensive health services need to be linked explicitly to escalating economic and social costs of mental illness. Only the College could ‘join up the dots’ convincingly for MPs to respond to urgent population mental health needs.

There is a timely opportunity to test such specialist influence on national policy. Thanks to heroic lobbying by thousands of women before the last election, the training and deployment of 4200 extra health visitors became one of the government’s top 10 priorities. The editorial on preventive psychiatry describes ‘opportunities to break the intergenerational transmission of risk’. Can psychiatric expertise now permeate into the skill set and effective practice of these 4200 public health practitioners?

Declaration of interest

W.C. is Editor of the Journal of Public Mental Health and currently involved with the national demonstration site for a Victims and Vulnerable Persons Index in North Lincolnshire.

Authors’ reply: We are indebted to Caan for an opportunity to further debate the potential of preventive psychiatry within a public health context. A failure to address inequalities reflects not only a failure of leadership but also lack of commitment by all sectors to recognise potential benefits in human capital and economic savings over the next decades. The Royal College of Psychiatrists’ position statement, which informed the Department of Health strategy No Health without Mental Health, sets out the evidence base and the need for further research. Recognising the role of psychiatrists and specialists in primary, secondary and tertiary prevention as well as the need for further development to include a role for specialists with appropriate training and accreditation processes is vital.

Preventive psychiatry is not new and remedying the consequences of adversity and vulnerabilities are but one of a number of preventive activities that already take place within existing psychiatric practice. The editorial sets this out alongside the new challenges facing specialists but also the wider public health community. The prevention of violence and hostility between adults and young people has been long recognised as a core task of preventive psychiatry. As set out in the College’s position statement, protecting and promoting health and optimal maturation of young people while taking account of complex interactions between biology and the environment are key objectives and are also at the heart of more complex approaches to medicine in general; preventing gender violence, sexual exploitation and abuse, promoting best parenting, nutrition, exercise, and education, protecting mental capital and physical health, and delivering interventions that develop mature adults who enjoy the responsibilities of adulthood while still enjoying the pleasures of life over the life-course are clearly important objectives. These policy priorities, although challenged by the need for more evidence and related research questions, are as important in low- and middle-income countries as in their higher-income neighbours.

These ambitious frameworks require local adaptations and actions, which incorporate an understanding of people’s lifestyle, attitudes, beliefs, cultures and status reflected in the delivery of interventions. Existing universal and global policies are being challenged by socially excluded groups and by people with multiple health problems, as well as those presenting with novel phenotypes. There is a role for specialists to be central to both policy and delivery, and to inform other stakeholders of the many varieties of personal distress and illness that are often lumped together under the title of mental health; an approach that would not be acceptable, say, for infectious diseases (see Lemkau). Inclusive and progressive policies and practices must protect the health and well-being of the population as a whole but also of the most vulnerable, including those victim to inequalities and social exclusion or those with complex needs that do not conform to unitary concepts of what constitutes mental health, illness and mental disorder; these opportunities must be seized while also


dealing with economic and financial crises that have an adverse impact on population mental health.


Homicide rates and income inequality

There is evidence that psychosocial factors other than those discussed by Swinson et al affect homicide rates and it is important to know whether these disproportionately affect individuals diagnosed as mentally ill. Specifically, there is evidence that income inequality strongly influences rates of violent crime, including homicide. Wilkinson & Pickett have claimed that income inequality strongly influences rates of violent crime, individuals diagnosed as mentally ill. Specifically, there is evidence important to know whether these disproportionately affect illness. There has been a marked increase in income inequality in recent years but, from the data which we have available to us, we are unable to comment as to whether this is also the case among those with mental illness, and whether there is any causal association with homicide rates. In future research we hope to explore the data using deprivation indices which might provide further information on any association between income inequality, mental illness and homicide.

1 Wilkinson R. Why is violence more common where inequality is greater? Ann NY Acad Sci 2006; 1036: 1–12.


Authors’ reply: We were looking for factors which corresponded to the overall rise in homicides in people with psychosis; factors which showed increases of a similar magnitude, over a similar timescale. This was the case for drug misuse, allowing us to infer an association. Evidence has been found linking income inequality to both violent crime and rates of substance misuse, although this has been disputed and there is controversy over the validity of the association found between income inequality and mental illness. There has been a marked increase in income inequality in recent years but, from the data which we have available to us, we are unable to comment as to whether this is also the case among those with mental illness, and whether there is any causal association with homicide rates. In future research we hope to explore the data using deprivation indices which might provide further information on any association between income inequality, mental illness and homicide.


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Observational BALANCE

We read with interest Kessing et al’s timely and welcome paper supporting, by way of observational cohort study, the findings of BALANCE. Lithium again is shown to be superior to valproate for the management of bipolar disorder. The strength in this case comes from bridging the gap between the relatively brief follow-up in randomised control trials (RCTs) and the real-life situation faced by clinicians managing a lifelong illness of unpredictable course. Although the enriched study design in BALANCE aimed to maximise the generalisability of the findings to a clinical population, limitations inevitably remained in terms of including patients who had shown a differential previous response to either lithium or valproate, diagnostic heterogeneity within the sample population, and frequency of comorbidity compared with the general population. The limitations of observational cohort studies are multiple and well documented. One key concern is confounding by indication, but more general problems exist with group biases and masking of cause and effect relationships.

Kessing et al used ‘switch to’ and ‘add on’ as proxy outcomes for the efficacy of mood stabilisers. It would have been interesting, if possible, to separate the ‘switch to’ group from the ‘add on’ groups. The ‘add on’ outcome probably represents a treatment failure; however ‘switch to’ is likely to be a combination of lack of efficacy and poor tolerability. Indeed, their findings suggest that the initial, very rapid increase in incidence of switch/add on is related to tolerability rather than efficacy, whereas in BALANCE this finding would have been lost by drop-out during the run-in period. This is unlikely, however, to explain the superiority of lithium that is clearly present in both outcome measures. It was previously argued that observational studies would overestimate treatment effects and that they hold little value in assessing therapies; however, comparative studies with RCTs, across various branches of medicine have now dismissed this. This sort of complementary approach, reconfirming findings from RCTs over long follow-up periods, is an important addition to the evidence base for treatment. This is especially true in areas where the disorder under investigation is chronic relapsing–remitting, and when the exclusion criteria of RCTs can often mean that external validity is low. If, as has been suggested, bipolar disorder is a heterogeneous condition with subtypes associated with preferential response to specific mood stabilisers (which can be identified by symptoms, clinical course and family history), then the observational study carries even more weight when compared with the RCT as it ‘allocates’ patients to treatments on the basis of...
predicted response, rather than randomisation. Bias can then be minimised by propensity score matching (controlling for unmeasured biases). This method was not employed by Kessing et al.,


Authors’ reply: We certainly agree on the mentioned advantages and disadvantages of observational studies and on the strengths of combining findings from randomised trials with those of observational studies.

Further, we agree on the possibility of the suggested analyses with ‘switch to’ and ‘add on’ as two separate outcomes. We chose the combined outcome measure as using two separate outcome measures (in addition to hospitalisation as an outcome measure) would decrease the statistical power to a low level in some of the analyses. In addition, one of the advantages of using the combined outcome measure is that the results may turn out to be more clear to guide clinical decisions on whether to use lithium or valproate in long-term treatment of bipolar disorder following a number of clinical situations (depression, mania, mixed episode or remission).

Propensity score matching (or other ways of introducing propensity score in the analysis) is a viable alternative to the approach based on multiple Cox regression models used in our paper. However, much experience (e.g. Sturmer et al2) suggests that the results thus obtained would not tend to be substantially different. The limiting factor seems to be the available amount of covariate information.


Role of postcards in reducing suicidal behaviour

The article by Hassanian-Moghaddam et al1 provides useful insights into the potential utility of postcard intervention in reducing suicidal behaviour. The authors by virtue of this study have found that among participants who had self-poisoned, nine postcards sent sequentially over a period of 12 months produced reduction in suicidal ideation and suicide attempts. The study deserves accolades for various reasons, including a large sample from a non-Western population and a randomised control design, ensuring an over 90% retention rate and nearly equal rates of loss to follow-up in both groups. The results of the study are illuminating but their generalisability and applicability in day-to-day clinical practice needs to be analysed against the backdrop of following limitations.

(a) The study provided for assessment of outcomes only at 12 months. It would have been better if the assessments were performed more frequently such as once in 2 or 3 months.

(b) The study at no point assessed suicidal intent among participants.

(c) Instead of employing any standard sampling technique, the participants of the study included consecutive individuals with poisoning, admitted from March to June 2006 in the Loghman-Hakim Poison Hospital.

(d) Baseline assessment did not include a comprehensive psychiatric evaluation that could have ascertained the specific psychiatric diagnosis of the participants and permitted subgrouping of the participants based on psychiatric diagnosis, thereby providing a valuable opportunity to study the differential impact of postcard intervention in reducing suicidal ideation and suicidal attempt among the participants with different psychiatric disorders.

(e) There is no mention in the article of whether the delivery of the postcards was confirmed by the recipients.

(f) The participants were masked to study outcomes but the research psychologist was not masked to allocation, and this could have inadvertently influenced responses at follow-up.

(g) Individuals may have got some clue about the study outcomes from the questions asked of them and this could have influenced the final results of the study.

(h) A small minority of participants withdrew from the postcard intervention but the specific reasons for the same were not assessed.

To make the postcard intervention more acceptable and effective, one needs to ascertain the specific reasons which made the participants withdraw from this intervention.


Authors’ reply: Drs Jhangee & Bhatia have mentioned a number of strengths and limitations, which were specifically addressed in the paper. The other issues that were raised are addressed below.

(a) Postcards are a minimal intervention sustained over 12 months. Optimal assessment is end of treatment and at follow-up, which allows comparison with similar studies.1,2

Repeated contact and assessment might ‘wash out’ the effect...
of intervention and telephone contacts might specifically influence suicide attempts.\(^3\) The costs for three assessments for over 2000 participants would have been considerable and the additional benefits of end-points measured before treatment completion are unlikely to offset the additional costs.

(b) Instruments assessing suicidal intention (rather than ideation) are contextualised to an episode of self-harm, suicide attempt or ideation. These were relatively uncommon and so intention would only have been measurable in a minority, if there was an instrument for the relevant languages and shown to be valid in the study population. Had there been such an instrument it might have been considered for baseline assessment.

(c) Using consecutive admissions is superior to any alternate sampling strategy. We acknowledged the limitations of restriction to a 4-month period.

(d) Psychiatric diagnostic assessments were done for all in-patients. We were mindful of the dangers of subgroup analyses in general. Initially we analysed for gender based on benefit only for women\(^1,2\) and a differential gender repetition rate of self-harm or poisoning in Western populations. We accepted the editorial suggestion of a second analysis based on previous suicide attempt at baseline, since this might be the highest risk factor for subsequent suicidal behaviour. Postcards in Persia and Postcards from the EDge intended to develop interventions available to almost all emergency departments with patients who had self-harmed, even emergency departments without psychiatric services required for diagnosis; so analysis based on psychiatric diagnosis was of low importance. We have tested alternate approaches to psychiatric diagnosis, which had low agreement with clinical diagnosis.\(^4\)

(e) There were several post hoc analyses based on recall of the number of postcards received. Since this was an efficacy trial, we conducted the main analyses based on randomisation, not exposure or dosage of the intervention.

(f) The research psychologist was not masked to allocation and study.

(g) There were two points in the paper that suggested that a substantial response bias was unlikely. The report of hospital treated episodes was accurate. Although ideation and attempt were significantly different, self-cutting was not, which would require a differential response bias in favour of two outcomes but against another.

(h) It would be useful to know the reasons for withdrawal. However, less than 2.3% of the treatment group withdrew, suggesting acceptability was rather good and improved retention in treatment would be small. The most innovative analysis addressed the issue of the possible impact caused by individuals withdrawn or lost to follow-up. We expect that sensitivity analyses’ that account for all possible outcomes might become a future standard for reporting randomised controlled trials that cannot guarantee an intention-to-treat analysis based on all participants or which rely on imputed values for non-ignorable missing binary end points.

Valproate v. Lithium in the treatment of bipolar disorder in clinical practice: observational nationwide register-based cohort study. BIP, 199, 57–63. Table 1 (p.59), final column, row 7: the hazard ratio (95% CI) for Index episode: mixed, with mania/mixed episode as the outcome is 1.59 (1.16–2.18). This typographical error does not affect the findings of the paper.

Psychiatric history and subthreshold symptoms as predictors of the occurrence of depressive or anxiety disorder within 2 years. BIP, 194, 206–212. Table 3, p. 209: The values for Social phobia, \(n\) (%) should read: No subthreshold anxiety disorder at baseline 31 (3.3), History of social phobia 14 (15.4), History of panic disorder 2 (3.1), History of agoraphobia 5 (9.3), History of GAD 8 (9.2), No history of anxiety 12 (1.7), Subthreshold anxiety at baseline 25 (11.0), History of social phobia 6 (18.2), History of panic disorder 4 (12.1), History of agoraphobia 6 (20.0), History of GAD 4 (9.3), No history of anxiety 12 (9.4), Total 56 (4.8). The values for Generalized anxiety disorder, \(n\) (%) should read: No subthreshold anxiety disorder at baseline 22 (2.3), History of social phobia 5 (5.5), History of panic disorder 1 (1.5), History of agoraphobia 2 (3.7), History of GAD 6 (6.9), No history of anxiety 11 (1.5), Subthreshold anxiety at baseline 16 (7.0), History of social phobia 1 (3.0), History of panic disorder 4 (12.1), History of agoraphobia 3 (10.0), History of GAD 2 (4.7), No history of anxiety 8 (6.3), Total 38 (3.3). The erroneous values in the table do not affect other values, including the ones listed in the column Any disorder, \(n\) (%), or any of the statistical analyses or conclusions presented in the paper.

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Role of postcards in reducing suicidal behaviour
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