Correspondence

Edited by Kiriakos Xenitidis and Colin Campbell

Contents

- Abortion and mental health
- Longitudinal course of cognition in schizophrenia
- Neural correlates of formal thought disorder
- Public attitudes towards mental illness
- Is there core diffusion tensor imaging pathology in schizophrenia?

Abortion and mental health

The December 2008 issue, with its original papers by Fergusson et al., Dingle et al. and its commentaries, was of great interest to us. Fergusson et al have overcome some of the methodological problems of previous studies. Nevertheless, their latest study has weaknesses: the women’s abortion status is not verified objectively, only by self-report. There were 133 abortions in 117 women but insufficient data to distinguish the effects of differing numbers of abortions; it is known that women having more than one abortion may differ in many respects from those having a single abortion. Also, because of the relatively restrictive law in New Zealand – ‘continuance of the pregnancy would result in serious danger . . . to the . . . mental health of the woman’ – some selection bias may have been in operation, allowing only women with more traumatic histories to access abortion. We will not discuss the Dingle et al. paper, as its failure to account for pregnancy intention (wantedness and timing) in those giving birth means that the comparator is inappropriate.

The Royal College of Psychiatrists’ Position Statement of 14 March 2008 mentions that a full systematic review is needed. This has now been done. Only four studies fell into the authors’ ‘good evidence and low risk of bias’ category. All four studies showed a neutral effect of abortion on mental health, indicating no significant differences between the study comparison groups. So Fergusson et al.’s study can be regarded as the first good-quality study to show a possible negative effect when attempting to answer the question: what is the relative risk of mental health problems for women who chose abortion compared with those who chose to have a live birth and who reported that the pregnancy was unwanted/initially distressing?

As clinicians working in the field of sexual and reproductive health, we favour the approach of Oates et al. We are supportive of their idea that abortion is not a psychiatric issue and that the Royal College of Psychiatrists should not develop a guideline on abortion. We would never want to go back to the psychiatric referral hurdle-jumping situation before and immediately after the Abortion Act came into force. The adverse effects of denied referral hurdle-jumping situation before and immediately after abortion. We would never want to go back to the psychiatric

Authors’ reply: We would like to thank Rowlands & Guthrie for their positive comments about our paper. We do not agree that the Dingle et al. paper should be dismissed on the grounds that it uses an inappropriate comparison. Although this dismissal is consistent with the opinions stated in the review your correspondents cite, it reflects a common misunderstanding. There are, in fact, two closely related causal questions that one can ask about abortion and mental health. The first concerns whether or not abortion is an adverse life event that increases risks for mental health problems of women who chose abortion compared with those who chose to have a live birth and who reported that the pregnancy was unwanted/initially distressing?

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is important to take pre-abortion mental health into account (which we did).

Rowlands & Guthrie also suggest that an adequate review of this issue has been conducted by Charles et al. We do not agree. This review has been criticised on the grounds of investigator bias and these concerns are supported by the somewhat anomalous conclusions the authors draw. For example, the study by Schmierge & Russo using the National Longitudinal Study of Youth (NLSY) data is ranked as one of the four ‘good’ studies despite the facts that only 40% of women having an abortion were included, and mental health was measured using a single scale of depression, with this outcome being assessed up to 13 years after the abortion. All of these features will bias results towards the null hypothesis. In addition, Charles et al. failed to distinguish between the different causal questions that may be asked about abortion, and misleadingly dismissed a number of studies showing links between abortion and mental health on the grounds that abortion, and treatment-resistant. Significant associations between cognitive change and change in negative symptoms are more likely to occur in these studies than in naturalistic follow-up studies. Third, we feel the authors missed an opportunity to include the distinction between patients with first-episode or chronic schizophrenia and geriatric patients as a potential moderator variable. This could have contributed to a better understanding of the probably complex cognitive pathways during the lifespan.

Despite the number of longitudinal neurocognitive reports, less is known than was originally supposed about the course of cognition in schizophrenia. Only a small subset (n=4) of longitudinal reports have compared these neurocognitive pathways with those of healthy controls over at least 1 year. When reviews are not focused on the neurocognitive effects of antipsychotics, stable long-term performances and, in some cases, cognitive gains could be expected, thus offering a rather pessimistic picture for cognitive enhancement. This approach seems more useful for understanding the long-term natural history of cognition in schizophrenia. Conversely, this meta-analysis relies on short-term data, mostly from clinical trials, and therefore more likely overestimates the potential for cognitive improvement.

In summary, it would be misleading if the conclusions were regarded as a major leap forward instead of as tentative hypotheses awaiting further investigation. Since the possibility remains that the current findings are more valuable to researchers, a new meta-analysis that takes into consideration these and other limitations might be more helpful for clinicians, patients and caregivers.
Authors’ reply: Balanzá-Martinez et al made a detailed review of our article1 and also provided several suggestions for future research. Although we agree with the authors that the conclusions of our article are by no means definitive, we disagree with them on the following important issues.

First, Balanzá-Martinez et al suggest that our meta-analysis ‘more likely overestimates the potential for cognitive improvement’ as compared with other reviews (the authors cite as an example the review by Rund2). On the contrary, we think that the comparison of results in individuals with schizophrenia with controls, which is an original feature of our review, has a sobering effect as it points out that ‘practice [is] more likely than cognitive remediation to account for most of the improvements observed’. On the other hand, reviews (such as the one cited earlier) in which performances in people with schizophrenia are not compared with those of controls could mistake improvement in results for improvement in cognitive abilities.

Second, commenting on the methods we used in our meta-analysis, Balanzá-Martinez et al criticise three of our options: (a) the inclusion of studies with a test–retest duration under 1 year; (b) the inclusion of open trials (especially clozapine trials); and (c) the fact that we did not differentiate between ‘patients with first-episode or chronic schizophrenia and geriatric patients’. They suggest therefore that it would be better to limit the analyses to a subset of the available data assuming, without formally testing, that some of the studies’ characteristics significantly influence results.

At the time we made our analysis, there were only 11 studies with more than a year test–retest interval and only 4 reported data for a control group. It was not possible to limit our analysis only to this subset of studies, let alone further exclude studies or separately analyse subgroups of studies.

Instead, we chose to include all methodologically sound studies and test the role of potentially confounding variables (including test–retest interval). By using this method, we limited the loss of important available information. It is our belief that we could not gain more insight from fewer data.

As we report in our article, only 2 out of the 17 variables tested showed a larger improvement in studies with shorter test–retest intervals. Thus, for the vast majority of variables, excluding studies with shorter test–retest intervals would lead to an unjustified loss of information.

Third, Balanzá-Martinez et al consider that their suggestions may lead to results ‘more helpful for clinicians, patients and caregivers’. We think that limiting the analysis to studies with large test–retest intervals (which usually have high attrition rates) or excluding the more naturalistic, open trials would achieve just the contrary.

In conclusion, although we agree with Balanzá-Martinez et al that the conclusions of our meta-analysis are not definitive, we also consider that to improve our knowledge on the subject we need new data, not new analyses, in subsets of (the same) data.

Neural correlates of formal thought disorder

Horn et al3 investigate a very important, somewhat underexplored area of neural correlates of schizophrenic speech disturbance. Given the probability of underlying deficits in contextual integration and theory of mind, formal thought disorder yields a fertile ground for structural and functional connectivity analysis in schizophrenia. Although the use of hitherto unused techniques such as resting perfusion scan to study formal thought disorder must be lauded, the results of this preliminary study must be treated with caution for various reasons.

The composite score of the Scale for the Assessment of Thought, Language and Communication (TLC) has been used as a measure of severity of formal thought disorder. The authors have administered the scale 45 min before the scanning procedure for each participant. It is widely perceived that uncontrolled generation of thought is required to reliably measure formal thought disorder in schizophrenia. The TLC itself lacks a standardised practical method of eliciting such thought flow in contrast to some recently developed instruments.2 The cross-sectional use of the TLC to measure formal thought disorder severity must be treated with prudence.

The authors extract components from the Positive and Negative Syndrome Scale (PANSS) using factor analysis and demonstrate that none of these components correlate with formal thought disorder severity as measured by the TLC total score. The validity of factor analysis in such a small sample is questionable and not in synchrony with available factorial structures of PANSS.7 As a result, all principal components extracted were from negative symptoms in PANSS (except the conceptual disorganisation item, which was rightly excluded from further analysis). Consequently, the results only show a lack of correlation between severity of formal thought disorder and negative symptoms as measured by PANSS. Findings from the magnetic resonance imaging may still be explained by positive symptoms alone and not by formal thought disorder. Lastly, the pervasive issue of sample size in neuroimaging studies becomes more prominent when correlation analyses are attempted in whole brain analyses.

Authors’ reply: Palaniyappan suggests using the Thought and Language Index (TLI) instead of the TLC in order to quantify formal thought disorder. The TLI, he argues, would have the advantage over the TLC of a standardised method of eliciting thought flow. Unfortunately, the TLI was established after the start of our study,1 whereas the TLC was an established instrument that has been successfully used in numerous studies as a reliable instrument to quantify formal thought disorder.2 We agree that future studies might benefit from the application of the more standardised TLI. However, the distribution of the severity of formal thought disorder in the patient group should not change.


Public attitudes towards mental illness

It is tempting to accept the conclusion from the paper by Mehta et al\(^1\) that the Scottish anti-stigma campaign ‘see me’ has successfully influenced public attitudes north of the border. However, it is not clear that this is an appropriate conclusion from the data they present.

They describe random sampling techniques whereby 2000 adults representative of the UK population were surveyed. One presumes that this would give rise to cohorts in England which were roughly ten times larger than those in Scotland. Comparing the year 2000 against 2003, they observed a deterioration for 17/25 stigma questionnaire items in England against only 4/25 in Scotland, and concluded that Scotland’s dubious distinction of having done less badly suggested that ‘see me’ had been effective. Can they assure us that this difference did not arise simply because the much larger samples in England would be more likely to show a statistically significant difference than smaller Scottish samples?

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the lesser goal of showing core pathology (in other words, the Venn diagram intersection). Coreness of pathology allows for diversity, without having to reconcile everything.

Second, we note that the secondary analysis was performed to disentangle the effects of the disorder from that of illness duration and treatment. The authors achieved this by using fractional anisotropy scores extracted from the principal analysis, which were then used to compare chronically with briefly medicated patients. Perhaps newly diagnosed, antipsychotic-naïve patients help most to partition out these effects\(^3\) but they are not essential. An alternative approach is voxel-based ANOVA covarying for illness duration and atypical antipsychotic duration/dosage since this can help maximise anatomical coverage (particularly in the striatum where antipsychotic effects are detectable at even 2–3 weeks of treatment\(^4\)).


Authors’ reply: We agree that the process of establishing a definitive extent of white matter disruption in schizophrenia, and its relationship with illness duration and antipsychotic medication, is likely to be a lengthy one – larger studies such as ours notwithstanding. But we would like to clarify our reasoning with respect to Chua & McAlonan’s comments and the methodological alternatives they suggest.

First, we suspect that a ‘core pathology’ for white matter abnormalities in schizophrenia may be rather more elusive than the reconciliation we attempted. Although the recent meta-analysis by Ellison-Wright & Bullmore\(^2\) has found areas of most common difference in the 15 studies they examined, it should be noted that only a fraction of the studies they looked at shared these differences – and the history of diffusion tensor imaging in schizophrenia is full of such conflicts.\(^2\) Although there may indeed be areas of greater difference, the evidence is against any difference that is common to all.

Second, with regard to distinguishing the effects of duration of illness and antipsychotic medication, drug-naïve cohorts clearly offer enormous potential as the authors acknowledge. Such cohorts are difficult to obtain in high-income countries however, and the alternative approach they suggest – of ANOVA with covariation – has similar difficulties, since duration of treatment and illness will be so strongly correlated in most samples. We also note that the studies they cite as demonstrating the effectiveness of this approach either did not covary for medication exposure\(^3\) or did not use diffusion tensor imaging.\(^4\)

Longitudinal course of cognition in schizophrenia
Vicent Balanzá-Martínez, Manuel J. Cuesta, Celso Arango, Benedicto Crespo-Facorro and Rafael Tabarés-Seisdedos
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