It seems clear that a small but significant number of people are simply ill-served by the existing format of mainstream mental health services. It may be (as I have heard in a European 'quality' forum) that such people are just peculiarly difficult. This seems unlikely, given a recent outcomes study we did of the most alienated and intractable of our referrals – people who live on the street and who have not been engaged by the sustained efforts of experienced street outreach teams. The intervention concerned was involuntary admission to hospital under a section of the Mental Health Act.[4] One year later, the majority were still engaged with the specialist mental health team and were still in accommodation. Here is an area ripe for research – the vital factors that enable such teams to engage effectively, and to maintain that engagement, with homeless people with psychotic disorders.

1 Craig TKJ, Timms PW. Out of the wards and onto the streets? Deinstitutionalization and homelessness in Britain. *J Ment Health* 1992; **1**: 265–75.

2 Winkler P, Barrett B, McCrone P, Csémy L, Janoušková M, Höschl C. Deinstitutionalised patients, homelessness and imprisonment: systematic review. *Br J Psychiatry* 2016; **208**: 421–8.

3 Salisbury TT, Thornicroft G. Deinstitutionalisation does not increase imprisonment or homelessness. *Br J Psychiatry* 2016; **208**: 412–3.

4 Timms P, Perry J. Sectioning on the street – futility or utility? *BJPsych Bull* 2016; doi: 10.1192/pb.bp.115.052449 [Epub ahead of print].

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Author's reply: I am grateful for the letters published by Mundt and Timms & Craig as they raise several important points. Regarding the comments by Mundt, I agree that the mental health of prison populations is of serious concern and it deserves to be urgently addressed by developing and implementing cost-effective services.

I also agree that in countries which underwent deinstitutionalisation and were included in our review,[1] excessively long-term hospital stays for psychiatric patients no longer commonly occur. After all, this was one of the main reasons that deinstitutionalisation was pursued. However, in the Czech Republic, for instance, 16% of in-patients with schizophrenia still stay in hospital for more than a year and hundreds remain in psychiatric hospitals for decades.[2] Therefore, unfortunately, our review is not just of historical value but conveys an important message for current mental health systems in the majority of Central and Eastern European countries.

I acknowledge that neither our review nor ecological studies can (dis)prove whether new cohorts of patients who became imprisoned in the era after deinstitutionalisation would have also become imprisoned if the mental care systems were still hospital based. We have also admitted that the cohort of patients followed or traced in studies included in our review are not representative of all deinstitutionalized patients.[1] However, what our study shows is that – contrary to some interpretations – there is scant evidence of adverse consequences for people who have been discharged from long-term institutional care. Our main point is that despite the importance of the data provided by ecological studies, these can be hardly helpful in showing whether there is a direct link between deinstitutionalisation and criminality. Moreover, it seems that ecological studies testing the Penrose hypothesis may have further important limitations,[3] and as such are arguably of inherently limited value. Indeed, linkage studies could be theoretically much more relevant, but, regrettably, Mundt does not cite any of them.

Our review[1] casts doubts on statements such as 'the general prison population has increased in all the countries, and this may be linked to the processes of deinstitutionalisation and reinstitutionalisation'[4] or 'changes in capacities of psychiatric hospitals and prisons appear to be linked'[5] contained in the discussions and conclusions of some of the ecological studies. Our paper shows that at the individual level these statements have negligible empirical support, and they might be detrimental to mental health care reforms in countries of Central and Eastern Europe.[1] As Salisbury & Thornicroft[6] argued, individual countries should focus on developing optimally balanced mental health care systems suitable to their setting.

There seems to be a clear consensus that substantial investment in community care is a *condicio sine qua non* of successful deinstitutionalization, which is why I suggest that cost-effective investments into mental health should replace the number of psychiatric beds as the 'hydraulic' in the updated Penrose hypothesis.

I would like to thank Timms & Craig for complementing our review with their depiction of some of the pressing issues related to the current homelessness among people with mental health problems in South London. Their insights are extremely valuable and should be considered when pursuing mental health care reforms in the countries of Central and Eastern Europe. Unfortunately, although we know that homelessness associated with mental illness is a serious problem in the Czech Republic as well, this issue is extremely under-studied and only anecdotal evidence is available. I agree with the suggestion that more research is needed to understand what enables community teams to engage effectively with 'the most alienated and intractable' patients. This might be especially important when it comes to the period immediately following a discharge from in-patient psychiatric care, which is associated with other concerning phenomena, such as re-admissions[2] and suicides.[7]

1 Winkler P, Barrett B, McCrone P, Csémy L, Janoušková M, Höschl C. Deinstitutionalised patients, homelessness and imprisonment: systematic review. *Br J Psychiatry* 2016; **208**: 421–8.

2 Winkler P, Mladá K, Krupchanka D, Agius M, Ray MK, Höschl C. Long-term hospitalizations for schizophrenia in the Czech Republic 1998–2012. *Schizophr Res* 2016; **175**: 180–5.

3 Tsai AC, Venkataramani AS. Penrose Hypothesis not supported. *JAMA Psychiatry*; 72: 735–736.

4 Priebe S, Badesconyi A, Fioritti A, Hansson L, Kilian RT, Torres-Gonzales F, et al. Reinstitutionalisation in mental-health care: comparison of data on service provision from six European countries. *Br Med J* 2005; **330**: 123–6.

5 Mundt AP, Chow WS, Arduino M, Barrionuevo H, Fritsch R, Girala N, et al. Psychiatric hospital beds and prison populations in South America since 1990: does the Penrose hypothesis apply? *JAMA Psychiatry* 2015; **72**: 112–8.

6 Salisbury TT, Thornicroft G. Deinstitutionalisation does not increase imprisonment or homelessness. *Br J Psychiatry* 2016; **208**: 412–3

7 Winkler P, Mladá K, Csémy L, Nechanská B, Höschl C. Suicides following inpatient psychiatric hospitalization: a nationwide case control study. *J Affect Disord* 2015; **184**: 164–9.

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Trial of an intervention to reduce suicidal ideation and behaviour

We take issue with the presentation of the findings of the study by Armitage *et al*,[1] on two counts. First, the title is misleading because there is no evidence that the intervention reduced suicidal behaviour, which was not measured as an outcome – not even through the obvious mechanism of recording re-attendance following non-fatal self-harm. Neither is it entirely accurate to imply, as the title does, that the intervention was designed to reduce suicidal behaviour. In the body of the paper, the authors indicate that they did not nominate a primary outcome measure because – in what they call an exploratory trial – they weren't clear what effects they expected.

Second, there is a stage at which flaws in the design or delivery of a trial mean that no safe conclusions can be drawn from it. Loss to follow-up of 50–60% at 3 months and >85% at 6 months is not compatible with a plausible intention-to-treat analysis, and using last observation carried forward cannot rescue the situation – especially when (as is the case here) the last observations in question are pre-intervention baseline measures taken at the same time that participants were given the materials for the intervention to which they had been randomised. The authors indicate that they are aware of some of the other problems with their trial – for example, outcomes not collected by researchers masked to allocation and uncorrected analysis of multiple outcomes. Under the circumstances, their conclusion that their intervention showed promise – a conclusion about which they were surely not in initial equipoise – cannot be regarded as sound.

1 Armitage CJ, Rahim WA, Rowe R, O'Connor RC. An exploratory randomised trial of a simple, brief psychological intervention to reduce subsequent suicidal ideation and behaviour in patients admitted to hospital for self-harm. *Br J Psychiatry* 2016; **208**: 470–6.

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Authors' reply: We welcome the opportunity to reply to House & Owens' two observations. We will address their concerns in the order in which they present them: the title and the limitations associated with the methodology, respectively.

First, the title does not say that suicidal behaviour was changed. The title states that the aim of the intervention was 'to reduce subsequent suicidal ideation and behaviour'. Moreover, we insisted on the term 'exploratory' remaining in the title against the suggestion of a reviewer and explicitly did not present the 6-month outcomes as main findings.

Second, House & Owens are correct that there are methodological limitations to the study, but these are recognised in the paper. Indeed, the final sentence of our conclusion chimes with the points they raise: 'Further research is required to replicate the findings with a more complete data-set and objective outcome measures'. In total, we highlighted six points of caution with respect to the interpretation of the findings. Methodological limitations are inherent in exploratory studies of this kind, but we endeavoured to address them by making weaknesses explicit and adjusting analyses to mitigate the effects of limitations as far as possible.

We believe our approach of last observation carried forward is a good example of adjustment to the limitation of missing data here. As House & Owens note, the observation carried forward method is not perfect and may introduce bias, which we assumed most readers would be well aware of and therefore did not flag in our 'limitations' section. In terms of the present study, the observation carried forward was pre-intervention, rather than post-intervention. Given that our randomisation check demonstrated no differences between conditions pre-intervention, any score carried forward would, if anything, bias results against finding an effect of the intervention. Had we chosen instead to measure outcomes immediately post-intervention and carried these observations forward, then the findings would have been biased in favour of finding an effect of the intervention. We would therefore argue that, cognisant of the limitations of last observation carried forward *per se*, we adopted the most rigorous approach possible within the given context (a busy emergency department in a low- and middle-income country).

Another example of our attempt to make the limitations of our study explicit concerns the points we make about the measurement of the main outcome variable. It was not possible to record re-attendance following non-fatal self-harm, and so we had to rely on the use of the Suicidal Behaviors Questionnaire – a self-report measure of suicidal ideation and behaviour. In the discussion, we state that 'it would be useful to have a more objective outcome measure, such as future hospital admissions, although this is not currently possible in the Malaysian context'. It behoves us to conduct research such as ours in low- and middle-income countries, even if it can only be exploratory at the present time.

In sum, we stand by our conclusion that this paper details a promising approach to intervention that is worthy of further research.

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Evolutionary theories in disordered eating psychopathology

It was disappointing to find that the editorial by Murray *et al*[1] 'Evolving eating disorder psychopathology: conceptualising muscularity-oriented disordered eating' made no mention of evolutionary formulations of eating disorders despite the somewhat suggestive title. This is a significant omission, as evolutionary theories provide a cogent explanatory framework not only for the newly described male variant of eating disorder referred to in the article but also for eating disorders as a whole. Also, the editorial's title promised to deliver a new conceptual framework for eating disorders in the light of this new variant, but all it did was produce the familiar tautology regarding cultural body ideals that has had such poor explanatory value in the case of female eating disorders. The formulation's lack of predictive power has meant that little, if any, progress has been made in the past few decades in researching the aetiology of these conditions despite the huge quantities of data collected. The weakness of the cultural norm (of desirable shape, beauty etc.) formulation is evident from the fact that it attempts to explain the phenomena of eating disorder by assigning causation to the very thing it is meant to explain (e.g. the drive for thinness in women or muscularity in men).

Hence, if these societal ideals are arbitrary and not related to any underlying biologically based motives, we should observe the reverse pattern in some societies (e.g. more men than women wishing to be thinner and more women than men wishing to be more muscular), but there is no evidence of such a scenario.

The sexual competition hypothesis (SCH) for eating disorders[2] is an evolutionary formulation that can help answer the 'why' question that has so far defeated mainstream, non-evolutionary theories. It proposes that all eating disorders stem from the phenomenon of intrasexual competition taken to pathological extremes. In the case of females, competition is