

International and regional variation in the prevalence of common mental disorders: do we need more surveys?[†]

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Vicente and his colleagues present admirably concise findings from a large epidemiological survey of non-psychotic psychiatric morbidity in four different geographical locations in Chile (Vicente *et al*, 2004, this issue). Without gainsaying the importance of psychiatric morbidity in that country, many readers, including local decision-makers, may find it difficult to assimilate these results.

METHODOLOGICAL VARIATION PERSISTS

There is now an extensive literature based on innumerable surveys of psychiatric morbidity from around the world, although it may be argued that some low-income countries and regions continue to be neglected. This literature has been summarised expertly in this journal (Cooper & Singh, 2000) and elsewhere (de Girolamo & Bassi, 2003). Indeed, even so-called ‘third generation’ studies, based on rigorous application of explicit diagnostic criteria and standardised clinical interviews, now span over a quarter of a century. Interpretation and synthesis of findings have been beset by difficulties arising from methodological variation in the choice of study measures, sampling and statistical methods. Despite substantial financial investment in these studies, it remains unclear whether the geographical differences in reported prevalence rates for specific disorders reflect genuine variations in morbidity. Most of these studies were not necessarily designed to be compared with one another, but rather to provide data for local health planners. Nevertheless, the lack of comparability remains disappointing. The fact that two large, expensive and well-designed studies in the same country reported substantially different rates of disorders undermined

confidence in both and raised questions about the validity of this type of research (Robins & Regier, 1991; Blazer *et al*, 1994). Another methodological complication concerns the extent to which study results are adjusted for associations with socio-economic status and other potential risk factors. The paper by Vicente *et al* is a good example of this problem, in that unadjusted morbidity rates and odds ratios were compared with findings from an earlier study in Chile presenting adjusted results. The extent of confounding by socio-economic variables is likely to vary across settings, making comparisons of unadjusted findings particularly difficult to interpret.

WORLD MENTAL HEALTH 2000: THE NEXT GENERATION?

An important question, therefore, is what fourth-generation epidemiological studies should look like. The answer suggested by de Girolamo & Bassi (2003) is that they should be large, cross-national and targeted at low-income countries. Although the first two of these criteria are met by the World Mental Health 2000 (WMH-2000) project, it is not clear that this actually represents a genuine paradigm shift.

Big it may be, but the usefulness of WMH-2000 is open to debate even before data collection has been completed. We should not lose sight of the opportunity costs of a study that aims to interview around a quarter of a million individuals. The methodological sophistication of WMH-2000 is not at issue. Rather, this hugely ambitious enterprise remains fundamentally limited by its cross-sectional design and perhaps more importantly by continued dependence on categorical systems of phenotypic classification whose validity becomes even more questionable in non-Western settings. For most observers, it is beyond question that vast

scientific strides were achieved through the introduction of operational diagnostic criteria and the standardised clinical instruments that were developed on the back of these. However, in many ways the past 30 years of psychiatric research may also be described as a triumph of diagnostic reliability over validity. We may now be at the limits of what this approach is capable of achieving. Shorter & Tyrer (2003) have again drawn attention to the lack of validity in existing categorical models of non-psychotic morbidity, and cite the pharmaceutical industry as the main beneficiaries of this quagmire. For precisely this reason, it is a matter of concern that WMH-2000 was partly funded by ‘a major pharmaceutical company’ (International Consortium in Psychiatric Epidemiology, 2004). Although it may be argued that this was necessary to enable some of these surveys to occur at all, making potential conflicts of interest more explicit would help to dissipate further criticism.

Proponents of WMH-2000 will point to the crucial data that this research will supply for estimating disability-adjusted life years and other World Health Organization indicators, without which the true burden of psychiatric disorders would remain unknown. It is therefore worth noting the serious criticisms of some of the methods (and practices) that have been used in the past to derive global health indicators. Musgrove (2003) illuminated the processes that led to the publication of a report comparing health care systems around the world. Shockingly, over 60% of the data on which these league tables were based were imputed. Given the political uses to which the findings are likely to be put, it is imperative that the methods and results of WMH-2000 are reported transparently. Failure to do so may carry the risk that its findings will be overinterpreted. One example of this, as demonstrated by Vicente *et al* (2004), is the tendency to infer that findings from a small number of selected towns, cities or regions are representative of national populations.

MEASURING PSYCHIATRIC MORBIDITY

There has been a great deal of debate about the measures used to assess psychiatric morbidity in population-based surveys, including arguments about whether

[†]See pp. 299–305, this issue.

standardised lay interviewer-administered schedules are inferior to those that require the judgement of an expert clinician (Brugha *et al*, 1999; Wittchen *et al*, 1999). This issue may, however, be a moot point. In truth, there can probably be no definitive diagnostic gold standard in the absence of systems of classification based on pathology and aetiology. It is questionable whether further studies comparing different instruments or efforts to refine existing diagnostic measures will make much of a difference. One can almost predict the comments that will follow publication of WMH-2000 findings, based on a modified version of the Composite International Diagnostic Interview (CIDI). Although these results will probably prove to be internally consistent (that is, reliable) in many settings, there will almost certainly be debate about their validity. It will hardly be surprising if, once again, researchers, health planners and policy makers are left wondering how to interpret the finding that (for argument's sake) the prevalence of social phobia is higher in South Africa than in Colombia. Our criticisms are not intended to diminish the Herculean efforts of the WMH-2000 investigators, but rather to question the scientific and philosophical bases on which this unparalleled epidemiological enterprise is founded.

One important but rarely aired concern is the validity of findings based on the recall of distant events (Patten *et al*, 2003). The originators of CIDI set great store by its capacity to identify episodes of psychiatric disorder that occurred many months or even years before the time of interview. Although this has been hailed as a methodological breakthrough, it is perhaps understandable that many remain sceptical about this epidemiological Holy Grail, which appears to offer the prospect of 'research-free' or 'cross-sectional longitudinal' data. The paper by Vicente and his colleagues is a prime example of research that may be of variable validity. It is not altogether clear why, for example, the authors chose to report retrospective data over 6 months rather than concentrating exclusively on the more robust current cross-sectional findings.

HYPOTHESIS-FREE CROSS-SECTIONAL RESEARCH

There has been a tendency in the UK, as well as elsewhere, to fund serial

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cross-sectional surveys rather than genuinely prospective research. We would argue that limited research resources are probably better invested in answering specific questions about the onset, outcome and treatment of psychiatric morbidity. Prospective research is expensive, and few developing countries are in a position to fund this kind of research. International funding agencies have a good opportunity to help redress this balance. It is paradoxical that funding for prospective studies is often declined on the grounds of underdeveloped hypotheses, even though these are conspicuous by their absence from most cross-sectional studies. Viewed in this context, we were disappointed by the lack of *a priori* hypotheses in the paper by Vicente and his colleagues, particularly as this was an opportunity to compare rates of psychiatric morbidity in urban and rural populations, and among those of differing ethnicity.

CONCLUSIONS

If not more cross-sectional surveys, then what? First, the time has probably come to embrace dimensional models of non-psychotic morbidity, particularly given consistent evidence that even mild and moderate levels of anxiety and depression (which are the most prevalent in all community surveys) are associated with adverse outcomes. Second, research is needed that is prospective, population-based and hypothesis-driven. Third, population-based research is needed that integrates epidemiology and neurobiology. This research is needed to elucidate the processes that link deprivation and morbidity, and to discover why the common mental disorders are characterised by recurrence and relapse. Fourth, even if countries and funding agencies are unable or unwilling to fund costly longitudinal research, the evidence accumulated so far indicates with sufficient certainty that psychiatric disorders exist all over the world. Rather than continuing to 'count the dead', it is perhaps more urgent for developing countries to try to ascertain whether these people can be helped at all

within their limited resources. We suspect that this path might prove a much more rewarding venture for local planners, politicians and the people of those countries who have to endure the double burden of being both poor and forgotten.

DECLARATION OF INTEREST

R.A. is from Chile, where he trained and worked. He has conducted research in Chile, including cross-sectional surveys and randomised controlled trials. S.W. acted as a reviewer for the paper by Vicente *et al* (2004, this issue).

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