

## Letters to the Editor

### Re: The Prevalence of Psychological Morbidity in West Bank Palestinian Children

*Dear Editor:* We are writing to bring to your attention our concerns about the above-referenced article (1). We believe that it is an example of the misuse of science to further a political agenda. Below we list a few of the significant scientific flaws we found in our review of the article. Together, they raise questions about the integrity and credibility of the research.

The authors hypothesize that “settlement encroachment” caused what they characterized as psychological morbidity, but they did not define that term, measure it validly, or empirically test their hypothesis. Along with unsubstantiated “anecdotal evidence” and “personal observation,” the authors cite only a political source (Noam Chomsky), not a scientific reference, to support their hypothesis. Although they assert that Palestinian children feared the Jewish settlement communities near the Bethlehem-area villages studied, they fail to cite a single documented case wherein Jewish residents were involved in violence against children, and they ignore the documented evidence that children from a Palestinian school stoned hundreds of Israelis on a frequently travelled road in the same area. The authors also fail to note the well-documented campaign of incitement to violence to which Palestinian children were exposed. That exposure bears directly on interpretation of the results and is a credible alternative explanation for the possible morbidity found.

Although the authors report using the “standardized” Arabic Rutter A2 scale for assessment of children’s behaviour, the reference cited did not indicate that this instrument (2) had been standardized on a Palestinian Arab population. The scale is no longer used by Palestinian researchers (P Vostanis, personal communication, 2004 October 4). Other references cited did not address or support contentions in the study.

The authors cite a reference to support the claim that “displacement and military occupation” since 1948 have caused “violent and psychological pressures.” A check of the reference (3) finds that it did not study or

support those contentions. The authors also cite a study (4) to support the contention that “in the absence of direct traumatic events, poor psychological status may arise from anticipating such events” (1, p 62), although the subjects of their study were clearly not, as our authors imply, living in a “period of relative calm.”

To summarize, the article contains numerous assertions not supported by the authors’ own references, ignores available data that can explain the results, fails to define terms properly, and presents as conclusions unsubstantiated conjecture. None of the authors has a mental health background, and their use of references that do not support their statements calls their credibility into question.

We are surprised that your editorial peer review process failed to find these errors. Mixing politics with science is a risky proposition, especially without solid evidence to support the conclusions reached.

The complete analysis of the article is available at [www.spme.net/Mansdorf\\_10\\_20\\_04.pdf](http://www.spme.net/Mansdorf_10_20_04.pdf)

We urge all to read it.

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Scholars for Peace in the Middle East (partial list of endorsers)

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### Reply: The Prevalence of Psychological Morbidity in West Bank Palestinian Children

*Dear Editor:* We respond here to the letter from Scholars for Peace in the Middle East concerning our paper revealing that, in July 2000, more than 42% of Palestinian children in the West Bank exhibited psychological morbidity (1).

Several criticisms were raised concerning the Discussion section of our descriptive study. Principal among these was the assertion that we should have tested the hypotheses generated in our study. According to basic statistics, a descriptive study attempts to reveal patterns associated with a specific disease or phenomenon without relying on a prespecified hypothesis. Thus ours is not a hypothesis-testing or proving but, rather, a hypothesis-generating investigation. To

suggest that we test hypotheses generated from our Discussion reveals a serious lack of understanding of research methods.

Exception was taken to our use of “settlement encroachment” as a possible cause of psychological morbidity and to our use of a reference by Noam Chomsky describing this phenomenon. According to first-hand observation and serious historical scholarship, our statement is a valid, supported hypothesis properly placed in the Discussion section of the paper. Further, we stand by Prof Chomsky’s thorough, well-researched work and recommend further recent works by Israeli scholars describing the same phenomenon (2).

The correspondents question our anecdotal evidence concerning the fear that Palestinian children have of settlements, contending that acts of violence were not verified. We did not state that acts of violence occurred, nor was our descriptive study designed to verify interviewee statements. We point out that violence against Palestinian children has been well described in the literature and by regional and international human rights organizations. We agree with the correspondents that Palestinian children may be subjected to a campaign of incitement to violence. This incitement (as mentioned in our paper) may include the significant presence of the Israeli Defence Forces (IDF) in the occupied territories; illegal settlement encroachment; and ongoing, violent displacement of the Palestinian population.

Objection was raised concerning our statement that Palestinian children under investigation were living in a “period of relative calm.” This objection is at odds with the extremely violent conditions imposed by the IDF directly following the period of our investigations: Over 600 Palestinian children have been killed (see [www.btselem.org/english/Statistics/Casualties.as](http://www.btselem.org/english/Statistics/Casualties.as)) and more than 7000 wounded (3) since September 2000. During the previous period (1987–1992), 1533 Palestinians were killed during the first Intifada ([www.btselem.org/English/Statistics/First\\_Intifada\\_Tables.asp](http://www.btselem.org/English/Statistics/First_Intifada_Tables.asp)).

The correspondents mention no criticism of our methodology or statistical analysis. The Rutter Scales (A2 and B2) were used in the Gaza Strip in 2000 (after validation) (4), but we agree that they are employed less often in recent studies. Researchers no longer attempt to flag Palestinian children with evidence of psychological morbidity, since it is well established that many Palestinian children

require formal psychiatric assessment on a large scale. Research now largely focuses on screening for DSM-IV criteria for depression or posttraumatic stress disorder, common among populations living with war and highly prevalent in the West Bank and Gaza (5–7).

Thus our brief communication remains one of countless studies revealing that Palestinian children are not thriving under occupation (8,9). Finally, we conclude that the criticisms directed toward the *Canadian Journal of Psychiatry* editorial process and authors are vacuous and unfounded.

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## Forensic Risk Assessment and Dangerous Driving

*Dear Editor:* In the January 2005 issue of the *Canadian Journal of Psychiatry*, risk assessment in psychiatric practice is reviewed. Four helpful articles provide general psychiatrists with an up-to-date, current overview of forensic practice and risk assessment.

In particular, “The Clinical Use of Risk Assessment,” by Graham Glancy and Gary Chaimowitz, carefully reviews the current practice of acting on a risk assessment and the duty to protect a specific population at potential risk of physical threat or injury (1).

But what about the risk to the general population from forensic patients as a result of their dangerous driving behaviours? Under the 2000 Canadian Medical Association guidelines (2), general physicians as well as general psychiatrists are mandated to report drivers with psychiatric illness to their local ministry of transportation when the illness is thought to interfere with their ability to drive a motor vehicle safely. Unfortunately, no clinically useful instruments are readily available to guide clinicians in this important risk assessment.

Current findings from a metaanalysis of the world literature on driving risk and psychiatric illness reveals significant findings related to substance use but a deficit in other diagnostic categories that are relevant to forensic psychiatry (3). In particular, there are no significant data available to guide clinicians regarding the degree of risk associated with a diagnosis of antisocial personality disorder and driving. This lack of available evidence-based data relating to psychopathy is compounded by inherent difficulties faced by forensic psychiatrists in clinical practice. There appears to be an inherent conflict in asking leading questions regarding potentially dangerous driving styles that may inhibit open disclosure about other more immediately relevant clinical forensic issues.

McGill University is currently conducting a Canadian-based survey on psychiatrists’ knowledge of and practice in psychiatric illness driving and reporting styles (4). It would be interesting to compare a representative group of forensic psychiatrists’ current practice of reporting high-risk drivers, during the course of their clinical practice, with a sample of more general psychiatrists not engaged in forensic work.

The development of a simple screening device for problem driving would be useful to all physicians in everyday practice. Such an instrument might have a place in the overall forensic risk assessment. As Glancy and Chaimowitz note in their article, “Further instruments should be established and well validated” (1, p 13). We are currently developing a clinical screening instrument, the

Jerome Driving Questionnaire, that we hope will be useful in general clinical practice, as well as in more specialized areas such as forensic work. Preliminary data indicate that this instrument shows clinically useful correlations with on-the-road driving assessments, made by experienced driving instructors, of driving risk in nonclinical populations of novice drivers (5).

### References

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## Re: Forensic Risk Assessment and Dangerous Driving

*Dear Editor:* Once again, the ever vigilant Dr Jerome raises an important issue omitted from our article "The Clinical Use of Risk Assessment" (1). The issue of risk assessment and dangerous driving was thought to be beyond the scope of our contemporaneous mandate; however, it is an issue that bears deliberation.

It would be interesting to consider how frequently driving risk is considered in the general practice of psychiatry. This issue may be addressed in the study currently being conducted at McGill. For instance, in the emergency room, should this be a part of any assessment? It could be argued that patients who are suicidal, homicidal, or suffering from a psychiatric disorder and who take psychiatric medication may have a condition that could interfere with their ability to drive a motor vehicle safely. Bearing in mind that the courts have stated that we have no discretion (1), are these conditions reportable?

There are also ethical issues to be considered. If a mildly suicidal patient comes into the emergency room for help, he or she may not be appreciative when he or she has to leave without a driver's licence. This then raises the issue of informing patients of the limits of confidentiality before they seek advice (2).

We have argued previously that, as the limits to psychiatric confidentiality are eroded, our therapeutic efficacy may also be eroded (3).

Thank you again, Dr Jerome, for increasing our awareness and anxiety in the practice of our profession in these increasingly difficult times.

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