

## Correspondence

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### Letter to the Editor

We would like to draw your readers' attention to three errors in a review article by Coid published in *Psychological Medicine* in 1983. Although some may regard this study as of historic interest only, its conclusions have not been disputed in the scientific literature.

Following a suggestion made by Schipkowensky (1973), Coid examined 15 studies of homicide by people with mental illness including studies of people found by the courts to have committed a homicide offence in an abnormal state of mind. He proposed that rates of mentally abnormal homicide 'remain the same in different countries, despite considerable differences in the overall rates of homicide' and suggested epidemiological laws relating to these conclusions.

The first error in the review was that the total and abnormal homicide rates were displayed using bar graphs for each study such that while differences in total homicide rates between the studies were clear, differences in the lower rate of abnormal homicide could not be ascertained due to scale minimization. Hence a possible relationship between abnormal and total homicides that is evident in a scatter plot could not be seen.

The second error was that the paper did not contain a statistical analysis. An analysis of the studies using linear regression does reveals a trend towards a significant association between normal and abnormal homicide ( $r=0.38$ ,  $r^2=0.147$ ,  $t=1.5$ ,  $df=13$ ,  $p=0.16$ ) using homicide rates after  $\log_{10}$  transformation to take into account skewed values.

The final error is that not all the available studies were included in the review. The inclusion of the large and frequently cited study by Boudouris (1974) produces a significant relationship between normal and abnormal homicide ( $r=0.564$ ,  $r^2=0.319$ ,  $t=2.56$ ,  $df=14$ ,  $p=0.02$ ,  $\log_{10}$  transformed rates). Hence, although the study lacked statistical power, if more complete and modern methods of data synthesis had been used, it is unlikely that the paper would have reached the same conclusions.

More recent studies of abnormal homicide from regions with a high total homicide rates have found higher rates of homicides by the mentally ill than predicted by Coid. The widespread belief in a universally

low rate of homicide by the mentally ill may have contributed to the neglect of homicide by the mentally ill as an area of study in regions with a high homicide rate and a failure to consider the common sociological factors in homicide by the mentally ill.

### Declaration of Interest

None.

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### The Author replies

The paper by Boudouris (1974) was considered but rejected for inclusion in the 1983 paper. Perhaps Large and colleagues should read it more carefully. It combined sexually motivated homicide together with homicides committed by the mentally disordered, presumably on the mistaken assumption that the two are the same (Boudouris was a sociologist). This is why Large and colleagues were able to find a relatively higher rate of abnormal homicide in one of the few North American papers available, and also why their test was able to squeak mouse-like over the line of statistical significance. But what does this really mean?

There is a dearth of information on mentally disordered offenders from countries with high homicide rates for a good reason. Mental illness exerts an almost negligible effect as a risk factor at the population level. Factors such as weapon availability, gang violence, drugs, ethnic tensions, etc. are considerably more important than mental disorder.

Large and colleagues should eventually be able to find enough studies to demonstrate the statistical correlation which I could not find back in 1983. It makes sense that if a risk factor, such as weapon availability, results in a high overall homicide rate, then persons with mental disorder who also have access to weapons should also demonstrate higher rates in that country. However, it is weapon availability that is the risk factor and not mental disorder. What it does not mean is that mental health services have an important public health role in countries with high homicide rates. But Large and colleagues will have to find studies with better information than that of Boudouris (1974).

One last issue is the use of bar charts. I would recommend retaining these in future studies because they make it entirely clear that once the base rate of homicide is taken into account, the risks from mentally disordered persons can be seen to be very small. Statistical correlations, however complex the statistical test, do not mean a causal relationship.

#### Declaration of Interest

None.

#### References

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#### A rejoinder from Large and colleagues

We acknowledge that Boudouris (1974) did count sexually motivated homicides in the category of murder due to psychiatric illness, although this can also be found in studies performed by psychiatrists (Coid, 2008). However, a statistical analysis of the samples in Coid's (1983) review plus the Boudouris data without the reported proportion of sexually motivated homicides still found an association between rates of homicide by the mentally ill and other homicides ( $p = 0.05$ ).

More puzzling than the exclusion of Boudouris (1974) was the exclusion of the landmark study by Hafner & Boker (1973) and the inclusion of a study from Bermuda with the lowest rate of homicides by the mentally ill, calculated using two events in 60

years. The 1983 review did not have a detailed description of the inclusion and exclusion criteria to explain the exclusion of 'over 95%' of the studies.

We agree that the mentally ill are responsible for a small proportion of all homicides. However, our meta-analysis of homicide in psychosis estimated that one in 630 patients with never-treated schizophrenia present after committing a homicide (Nielssen & Large, 2008).

Coid's doubts about the value of statistics, and his views of the effect of mental health services, gun usage, substance use and ethnic tensions may be correct but are not relevant to this argument. Furthermore, we did not suggest a causal association between homicides by the mentally ill and other homicides. The importance lies in whether the rates of homicide by the mentally ill and other homicides are associated. If there is no association, and the rates of homicide by the mentally ill do not vary over time or between regions, then it might be assumed the homicides are principally due to manifestations of mental illness. However, the finding of an association between rates of homicide by the mentally ill and other homicide suggests other interpretations, for example, that mental disorder increases the vulnerability of the mentally ill to the factors that contribute to total homicide rates.

In conclusion, we were pleased to read that Coid acknowledges that the samples he examined were too few in number to have the statistical power to demonstrate the presence or absence of an association between homicides by the mentally ill and other homicides. Can we now conclude that Coid's 'epidemiological laws' have been repealed?

#### References

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