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Editor: Ian Pullen

Conditional probability and sibling sex

SIR: The letter from Lacey *et al* (*Journal*, August 1991, **159**, 291) is a good example of the misuse of Bayesian statistics. Assuming that men and women are equally represented in the population, the probability of a particular sibling being female is $\frac{1}{2}$. This probability is unaffected by my knowledge as to whether other siblings are male or female, older or younger, bulimic or not, or whatever. Specifically, if there are two siblings and one is a female, the probability that the other is female is $\frac{1}{2}$; if I happen to know also that the elder sibling is female, this does not change the situation at all. Any other conclusion is counter-intuitive, as Goodman (*Journal*, August 1991, **159**, 290) neatly illustrates.

Referring to the original paper (*Journal*, April 1991, **158**, 491–494), it can now be seen that Table 2 is incorrect. The purpose of this table is to establish that bulimic females come from all-female sibships more often than expected by chance. The position of the index case within the sibship is clearly irrelevant to this question. Since all the index cases were female, the probability of an all-female sibship of two is simply the probability that the other sibling is female, i.e. $\frac{1}{2}$. Similarly, the probability of an all-female sibship of three is $(\frac{1}{2})^2$, i.e. 1/4; of four, $(\frac{1}{2})^3$, i.e. 1/8; of five, $(\frac{1}{2})^4$, i.e. 1/16. If these figures are used, the expected numbers of such sibships become 40.5, 15.5, 5.5 and 1.5 respectively, which are very similar to the observed numbers of 45, 20, 6 and 2. This suggests that Lacey *et al* have not proved their hypothesis that bulimic girls come from all-female sibships more often than expected by chance.

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SIR: It seems clear that Dr Goodman is right in his critique of Lacey *et al*'s data on the siblings of patients with bulimia nervosa (*Journal*, August 1991, **159**, 290). It is the authors who appear to be in error, both in the original article (*Journal*, April 1991, **158**, 491–494) and in their reply (*Journal*, August 1991, **159**, 291). They argue that by chance alone bulimic women will be less likely to come from same sex sibships than mixed ones and try to prove this using conditional probability theory. They point out that the possibilities for a sibship of two in the population are: MM, MF, FM, FF. If subjects of either sex are selected at random as cases then the probability of coming from any pair is 1/4. The authors are then correct in their statement that the conditional probability of a sibship of two girls given that one is a girl is 1/3 (i.e. 1/4 / 3/4). However, as their study involved women, prior selection has altered the probabilities of sibling pair occurrence. By including only women, the chance of them coming from an all-female sibship rises from 1/4 to 1/2 (all-female sibships contain two women and so are twice as likely to be selected as male–female ones). In a similar way, assume for simplicity that all the women in the population are taken as coming from sibships of two represented by MM, MF, FM, and FF. Fifty per cent of women would then be in an all-female sibling pair (even though only 25% of pairs are all female). In other words, women would be expected to have an equal chance of having a brother or a sister. Common sense tells us that this is so and it is not surprising that this is what Lacey *et al* found in their study. The authors should

reanalyse their data using probabilities that take into account the effect of single sex selection.

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SIR: Lacey *et al* (*Journal*, August 1991, **159**, 291) have repeated their claim that female bulimics from two-child families would be expected, by chance, to have twice as many brothers as sisters. This erroneous claim led them to interpret their own data that brothers and sisters were roughly equally common as evidence that all-female sibships represent a risk factor for bulimia. They are right, of course, that male–female sibships are roughly twice as common in the general population as female–female sibships. They have forgotten, however, that they were twice as likely to ascertain the latter since either sister could present at their clinic. Assume, for example, that bulimia led to clinic referral in 1 in 1000 females. Lacey *et al* would then have had a 1 in 1000 chance of including any sibship with just one female (such as a male–female sibship), but a 2 in 1000 chance of including any sibship with two females (assuming the risk was equal but independent for both sisters). After allowing for this unequal ascertainment, male–female and female–female sibships should have been equally common in their sample. Since their results do not differ significantly from this expectation, their findings do *not* suggest that women from all-female sibships are at greater risk of bulimia.

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Apparent decrease in schizophrenia

SIR: Eagles (*Journal*, June 1991, **158**, 834–835) comments on the findings of Der *et al* (1990) as part of a growing body of evidence that suggests the incidence of schizophrenia is decreasing (e.g. Eagles *et al*, 1988). Methodological and diagnostic complexities notwithstanding, these observations are compelling, as they have been noted in both hospital (first-admission) and community-based populations.

Eagles cites possible explanations for this phenomenon, including changing environmental risk factors such as decreased perinatal injury and decreased prevalence and/or incidence of various infectious diseases. To this list we would like to add changing patterns of exposure to illicit and recreational drugs.

Bowers (1987) studied data from Connecticut state hospitals for the years 1967–1979 and concluded that an increase in first admissions of substance-abusing patients was followed in three to five years by an increase in first admission rates for schizophrenic and paranoid disorders. The association was particularly strong for young psychotic patients. Similarly, McLellan *et al* (1979) reported that five out of 11 military veterans with stimulant and hallucinogen abuse requiring repeated hospital admission developed psychotic disorders during a six-year follow-up. Psychoses were specific to stimulant abusers when compared to patients abusing depressants and opiates.

We agree with Eagles that study of the social and demographic features of the specific population which has shown the greatest decline of schizophrenia would be helpful in explaining this apparent decrease. Among those features worth investigating would be recreational drug use and its relationship to the incidence and course of psychotic illness in that population.

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DER, G., GUPTA, S. & MURRAY, R. M. (1990) Is schizophrenia disappearing? *Lancet*, **335**, 513–516.

EAGLES, J. M., HUNTER, D. & MCCANCE, C. (1988) Decline in the diagnosis of schizophrenia among first contacts with psychiatric services in North-East Scotland, 1969–1984. *British Journal of Psychiatry*, **152**, 793–798.

MCLELLAN, A. T., WOODY, G. E. & O'BRIEN, C. P. (1979) Development of psychiatric illness in drug abusers: possible role of drug preference. *New England Journal of Medicine*, **301**, 1310–1314.

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The human brain and political behaviour

SIR: Hugh Freeman's explanation of political behaviour in largely psychological – psychopathological terms made interesting reading (*Journal*, July 1991, **159**, 19–32). However, to extend the argument, "a crooked molecule behind a crooked thought" to "a crooked molecule behind a crooked policy" appears too simplistic, hardly capable of explaining any complex sociocultural phenomenon.

The paper's major thrust is that social and political changes result from individual actions, which are in turn influenced by personality and psychopathology. Although individuals as leaders do seem to change