There is thus an apparent contradiction: for each of the age groups separately the proportion of males has *increased* from 1970 to 1975, but for the combined age groups the proportion has *decreased* over this time period. Surely both cannot be correct?

Another example of this phenomenon can be found in Table I of Glover and McCue (*Journal*, March 1977, **130**, 282) where the authors are investigating the effect of electrical aversion therapy on alcoholics.

For neither of these examples are the differences significant, but this need not always be the case. If, for example, the numbers referred to thousands of patients, rather than to single patients, then the comparisons would be highly significant.

The apparent paradox (Simpson, 1951) arises because we intuitively expect the probabilities over all ages to be the average of the probabilities in the under 65 and 65 and over age groups. In fact, this intuitive idea is only half the truth. The probabilities in the total *are* averages of the sub-table probabilities but they are *weighted* averages and the weights are not the same. To make this clear let x = male, y = under 65, and z = 1970, with x', y', z' being the complementary categories. Then we have, from elementary probability theory:

and
$$P(x \mid z) = P(y \mid z) P(x \mid y, z) + P(y' \mid z) P(x \mid y', z)$$

with
$$P(x \mid z') = P(y \mid z') + P(x \mid y, z') + P(y' \mid z') P(x \mid y' z')$$

and
$$P(y \mid z) = 429/739 \neq 258/515 = P(y \mid z')$$
$$P(y' \mid z) = 310/739 \neq 257/515 = P(y' \mid z').$$

So, although in the example $P(x \mid y, z) < P(x \mid y, z')$ and $P(x \mid y', z) < P(x \mid y', z')$, the different sets of weights mean that the weighted average of $P(x \mid y, z)$ and $P(x \mid y', z)$ is greater than the weighted average of $P(x \mid y, z')$ and $P(x \mid y' z')$.

If y and z were independent $P(y \mid z)$ would be equal to $P(y \mid z')$ and similarly $P(y' \mid z)$ would be equal to $P(y' \mid z')$ so in this case the paradox would not occur.

Simpson's paradox is one of the simplest illustrations of probabilistic phenomena which at first appear to contradict intuition. It demonstrates that intuition can be unreliable as a tool for comprehending statistical theory. If problems of interpretation can arise with something as simple as a $2 \times 2 \times 2$ cross-tabulation, then clearly very great care must be taken with the more complex analyses typical of psychiatric research.

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OUTCOME OF SCHIZOPHRENIC ILLNESSES

DEAR SIR,

Johnstone *et al* (*Journal*, January 1979, **134**, 28–33) draw what may be unreasonable conclusions from their data concerning the prediction of outcome in schizophrenic illnesses. They state that there "was no significant difference in outcome between Feighner-positive and Feighner-negative cases . . .", but their Table III indicates that 15 of 20 Feighner-positive cases had a poor outcome (75 per cent) compared to 6 of 16 Feighner-negative cases (37.5 per cent). In other words, the positive cases were twice as likely as the negative ones to do poorly. The small sample size apparently prevented this difference from achieving statistical significance. In their discussion, however, the authors ignore the high risk of incorrectly accepting the null hypothesis, a not uncommon problem (*New Eng. J. Med.*, **299**, 690–4, 1978).

In addition, the authors do not provide information about the correlations between social isolation, the variable they identify as important in prediction, and the other predictors, including the Feighner criteria. One might at least suspect a good correlation between social isolation and being Feighner-positive.

Finally, it is a bit puzzling that the authors decided to exclude two patients "who scored only 4 on the social functioning assessment", indicating a "good outcome in social terms", because they "had been readmitted to in-patient care with a recurrence of psychosis . . . In this respect these patients differed from the rest of the good outcome group, and it did not seem appropriate to include them". These cases raise questions about the validity of the outcome

measures, and one would like to know how the results might have been affected by including them.

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CHEAPER CARE OF PARASUICIDES

DEAR SIR,

The paper by Newson-Smith and Hirsch (*Journal*, April 1979, **134,** 335–42) appears to show that social workers can be about as effective as psychiatrists in screening parasuicides, and closely follows reports by Gardner and others (1977, 1978) that briefly trained house physicians can also match psychiatrists in this endeavour. Does this mean that special psychiatric services of the kind originally recommended in the Hill Report (Central Health Services Council, 1962) and developed in Regional Poisoning Treatment Centres are expensive luxuries?

We wish to draw attention to the fact that psychiatrists in Charing Cross Hospital and in Cambridge, who were emulated so well by social workers or house physicians, were very generous in their offers of further psychiatric treatment and did not leave much margin for error.

Psychiatric (and social work) after-care recommended for parasuicides by psychiatrists

	% In-patient	% Out-patient (social worker only)	% None
Cambridge (Gardner et al, 1978)	20	49 (8)	31
Charing Cross Hospital (Newson-Smith and Hirsch, 1979)	27	65 (13)	8
Edinburgh R.P.T.C. (unpublished report, 1977)	10	32 (14)	58

Compared with psychiatrists at the Edinburgh Regional Poisoning Treatment Centre (R.P.T.C.) they recommended twice as many patients for in-patient psychiatric treatment and twice as many for out-patient follow-up. It is very unlikely that these large differences in the utilization of expensive psychiatric resources can be explained by differences in the patient populations. Where is the saving?

It seems that the psychiatric services for para-suicides in these other centres largely depend on junior psychiatrists, who share out the work and have other commitments, and therefore have diluted experience and supervision. In Edinburgh most parasuicides are also assessed by a junior psychiatrist, but he has a major commitment to the R.P.T.C. for six months, where he is trained and supervised by two consultant psychiatrists who have a special interest in this area. They have learnt over the years to be much more selective and sparing in the use of psychiatric after-care for their annual 2,000 patients, most of whom are not mentally ill. Meanwhile, the repetition rate has not increased.

If less experienced colleagues refer on 60–80 per cent of parasuicides, the psychiatrist might as well see them all in the first place. The Edinburgh model may be cheaper.

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A CONTAGION HYPOTHESIS

DEAR SIR,

This brief report of a small explorative study may be of interest. A simple socio-metric method was used to determine whether the high prevalence of reported emotional disturbance in an all-female student hall of residence was to some extent the result of case-to-case spread.

Nearly 90 per cent of the 155 residents returned a questionnaire and 23 per cent of respondents answered in the affirmative to the question 'Have you been emotionally disturbed or nervously unwell since the beginning of term?' (Two large psychiatric mor-bidity surveys in the same university had shown that a positive answer to this question was highly correlated with medically detected psychiatric morbidity and poor academic performance). The students were