

Correspondence

PSYCHIATRIC DISORDER IN STUDENTS OF PSYCHOLOGY

17 April, 1964

DEAR SIR,

The recent article by Dann (*British Journal of Psychiatry*, March, 1964, pp. 186-190), was an intriguing one. His impression concurs with our own (as psychologists ourselves) so that we were already sympathetic to the author's hypothesis. Whilst, in general, he makes his point concerning the overall incidence of disorder, on closer reading the paper has several major shortcomings, and a sprinkling of minor errors. The chief defects are as follows:

1. The author makes the point that the ratio of men to women reading psychology is higher than the same ratio amongst the non-psychologists. This is quite untrue, and had the author cast his figures in percentage form, it would have been apparent, as in Table I here.

TABLE I
Percentage of Men and Women Reading Psychology and Other Subjects

	Psycho- logists	Non- Psycho- logists	Total	N
Men ..	4.2	95.8	100	937
Women	7.1	92.9	100	339

Within each academic group, 62 per cent. of the psychologists are men, compared with the 74 per cent. of the non-psychologists who are men. This considerably diminishes the author's conclusion that "males who are liable to psychiatric disorder tend to read psychology" (p. 190—penultimate paragraph).

2. Tables II and III present slightly different sets of figures. In all, there are five places, excluding sub-totals, in which the figures from one table do not tally with those in the other, and no explanation is given for the discrepancies.

3. On p. 189, the author cites Table IV (the source of which is not mentioned as evidence) that (a) non-residence does not increase disorder, (b) the figures support his criteria for diagnosing psychiatric disorder.

Since the proportion of non-resident students is not given for any university other than Swansea, and since the incidence rates vary considerably, both conclusions are unwarranted. Further, the author does not cite the total university population on which the Swansea incidence was calculated.

4. On p. 187, the author states "... the data in this paper have been subjected to a careful statistical analysis; the exact methods used are too complex for discussion here". This is inadmissible practice in a scientific paper, and further, in several places in the text, he states that differences are "significant" or "not significant" without giving either the statistical technique used or the calculated figure or the probability level.

5. In discussing whether the possible disposition to disorder differs between psychologists and non-psychologists (p. 189), he dismisses any attempt to do this for three reasons, none of which is convincing and for none of which the author cites any evidence.

6. Finally, the whole method of comparing the very small group of psychologists with the very large group of non-psychologists seems unsound. It could well be that the figures mask differences between other schools of study which are equally as great as those the author cites and which would lead to different overall results.

In conclusion, perhaps the most important case the author has made is that a replication of this study should be carried out, using sounder methods of design and assessment.

D. F. HOOPER, B.A., Ph.D.

Research Associate in Mental Health

G. S. TUNE, B.A., Ph.D.

Research Fellow

Harvard School of Public Health.