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DEAR SIR,

Positive, as well as negative, findings with respect to birth order and schizophrenia have been in the literature for some time. The positive findings run this way and that, early and late birth positions having been inculpated about equally often for different samples of patients. Support of the postulate that birth order, for whatever reason, bears a significant relation to the probability of developing schizophrenia demands, not merely positive, but consistent findings. Barry and Barry (1967) and others have pointed out, however, that birth order might relate to vulnerability to schizophrenia in different ways in different cultures and, within cultures, in different ways for the two sexes or for members of small versus large sibships. That is, a sample, partitioned into cells by sex and sibship size, would show a particular *pattern* of birth order across these cells.

The point made in the paper by myself and assistants (Van den Bosch and Denham) was that, when partitioned by such variables, samples from similar cultures would then be expected to follow similar *patterns* across the cells. Our paper was concerned with a search for pattern consistencies in birth order among samples of schizophrenic patients. Detailed data were available for five Western samples: Granville-Grossman's (1966) British sample and four U.S. samples—two from our own studies, one reported by Barry and Barry (1967) and Solomon and Nuttall's (1966) sample consisting of males only. Comparability of these samples was discussed briefly in the paper. In analyses by two methods we partitioned the data by sex and sibship sizes 2–4 and 5+. Intersample inconsistencies, therefore, cannot be attributed to sex, family size or major cultural differences as Drs. Barry and Barry seem to believe. Analyses of a sixth sample, the Indian patients studied by Sundararaj and Rao (1966), were included for interest, but, as we emphasized, not for comparison with the Western material; and pattern consistencies were sought only among the U.S. and British samples. Evaluation of the stability of the Indian data would, of course, have to be based on other Indian studies.

The above letter introduces no quarrel with our contention that the data failed to evidence significant birth order effects associated with schizophrenia in the females. No birth order effects emerged in the analyses of small sibships in the four (*N.B.* 4, not 3) Western samples, and a positive finding (preponderance of late birth positions) appeared for females of large sibships only in the Barrys' 1967 sample.

According to our analyses, males, the other half (approximately) of most schizophrenic patient populations, also demonstrated no consistent birth order pattern across the samples. It is chiefly this conclusion to which Drs. Barry and Barry object. They have previously proposed (Barry and Barry, 1967) a specific pattern of birth order effects for male schizophrenics of Western cultures: over-representation of early birth orders in small sibships and over-representation of late birth orders in large sibships. The Barrys believe that the hypothesized relation is upheld by intersample consistencies in the data for at least the small sibships. Of the five male samples examined by us, three (our two and Solomon and Nuttall's) exhibited a significant excess of early birth positions. The remaining U.S. sample (Barry and Barry) and the British one (Granville-Grossman) technically showed no birth order effect. If one prefers a more liberal interpretation of data, however, one may say that a statistically non-significant trend is present toward early birth orders in the Barry and Barry sample and, equally, toward late birth orders in the Granville-Grossman sample. The Drs. Barry apparently prefer the latter interpretation for their own work, but they lose sight entirely of the Granville-Grossman sample with its opposing trend. They are impressed instead by the 'remarkably uniform' findings for the four U.S. samples, and in particular the substantial outnumbering of last-born patients by the first-born. Careful inspection of the data structures for these samples suggests somewhat less overall uniformity than the Barrys suppose. For example, their own sample gave exactly the reverse picture for sibship size 4, with an excess of late-born rather than early-born individuals (see Fig. 1 of our paper, *Journal*, June 1969, p. 665); in sibship size 3, our 1954–56 sample did not deviate at all from random expectations, our 1934–36 and the Barrys' samples displayed no difference between intermediate and first-born ranks, and the Solomon and Nuttall cases had an excess of intermediate but not of first-born positions.

Turning to the data on large sibships (sizes 5+), Drs. Barry and Barry note that two samples contained an excess of later-born males. They are referring to their own sample and to Granville-Grossman's British study, which they suddenly resurrect, having bypassed it when considering the small sibships. The Solomon and Nuttall sample—which the Barrys included in the discussion of the small sibships and which appeared at that point to fit their hypothesis—somehow vanishes when the large sibships come under scrutiny; *early* birth positions were over-represented in this sample for the large, as well as the small, sibships. Drs. Barry and Barry assert that we dismissed the highly significant findings of a late birth effect in their and Granville-Grossman's samples because these did not coincide with the negative results obtained in our own two samples. We did not put aside the positive findings for quite such egocentric reasons. The five Western samples available for examination in our paper yielded three different types of results for males from large sibships (two late, one early, two negative). This random distribution clearly does not indicate cross-sample consistency. More important, the two samples that agreed on a positive result for large sibships did not do so for small sibships, and vice versa. Piecemeal selection of data from sample cells that fit a particular hypothesis and rejection of discordant cells is not an appropriate way to test for pattern consistency. As the data stand, no consistent pattern can be claimed across the cells of male sibships for all five studies, or indeed for any two but our own, whose positive findings we also 'dismissed' after deliberation. The data as a whole negated any significant relationship between birth order and schizophrenia.

Other data considered in our paper were in accord with this conclusion. One observation that did seem to merit further exploration was the tendency for small sibships to display *relatively* earlier birth orders than large sibships within the same sample. Going from small to large sibships, birth order means for the given sample went from early to less early, from early to late, or from late to later. But analyses of nine samples of non-schizophrenic subjects, revealed a similar trend in a variety of other subject groups. Drs. Barry and Barry state that each of the findings can be explained. This is probably true. One of the weaknesses of the birth order hypotheses is the ease with which attractive *a posteriori* explanations suggest themselves for almost any type of observed effect. Thus, both eminence and psychopathology are suggested as outcomes of psychosocial pressures associated with early birth positions. (This explanation cannot be applied, of course, to Slater's (1962) sample of homosexuals, which contains a preponderance of *late*-born cases from small sibships and an even greater preponderance of such cases from large sibships.) It is not clear how Drs. Barry and Barry explain the pattern of findings for the large sample of males from the 1947 survey of 11-year-old Scottish schoolchildren, a group which was undistinguished as to psychological characteristics (see pp. 670–1 of our paper). We were unable to suggest why this general population sample should deviate from the usually expected random distribution of birth ranks. The solution seems to be provided, however, in the analyses of the same material presented in Price and Hare's (1969; see also Hare and Price, 1969) excellent dissection of the sources of bias that may distort birth order distributions in specific ways in general population samples. Both the Scottish schoolchildren and a large sample which Hare and Price consider to be representative of the British general population follow the birth order pattern which Drs. Barry and Barry believe to be associated with schizophrenia. Biases due to marital and reproductive changes in the population largely account for the pattern in the general population samples. Most of the positive, inconsistent findings for schizophrenic samples probably have their origins in similar sources of bias.

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UNILATERAL AND BILATERAL ECT

DEAR SIR,

In their article entitled 'EEG, memory and confusion in dominant, non-dominant and bi-temporal ECT' (*Journal*, September 1969, pp. 1059–64), Drs. Sutherland *et al.*, criticize my suggestion that bilateral ECT may produce greater relief from depression than unilateral. I do no see how their study allows them to make any comment on this point, as no assessment of depression appears to have been attempted.

I should also be interested to know how they were able to carry out double blind measurements of such things as 'time taken to breathe spontaneously', when this was sometimes as short as 1·45 seconds. Did they employ an Olympic sprinter?

Finally, why did they not comment on the fact that the EEG assessor was quite unable to guess correctly which treatment had been given? This is