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

presumably because the EEG changes were not lateralized, which would seem to bear out one's suspicion that once the current has overcome the resistance of extra-cerebral tissues it will pass into a highly conductile medium in which any kind of localization is impossible.

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DEAR SIR,

We are pleased that Dr. Levy shows such interest in our paper. May we comment on his points?

(1) From our study, we are impressed that bilateral ECT is no more effective than unilateral ECT in relieving depression. As is clear in our paper, our method of measurement was to allow psychiatrists in the hospital to recommend the number of ECT treatments in each case on their own clinical judgement of relief from depression. They were throughout quite unaware which sort of ECT was being given. Table I shows that the average number of treatments given in each group was not significantly different, thus indicating equal clinical effectiveness of each type of treatment.

(2) The double blind measurements of confusion after ECT were easily done. We had no need to employ an 'Olympic sprinter', but we did employ professionals who could read. If Dr. Levy would look at Table V again he will note that the '1·45 seconds' he alludes to in his letter is in fact 1 minute 45 seconds.

(3) It is not true to say categorically that the EEG assessor 'was quite unable to guess correctly which treatment had been given'. It is apparent from our paper that although she was unable to guess correctly in 100 per cent of cases, she did make 35 correct forecasts from 59 patients. This is significantly better than by random assignment. In 25 cases EEG changes were lateralized (see Table III). We would not like to draw conclusions from this, except to say that in some of our cases 'there are persistent detectable changes' in EEG's four days after completion of the course of ECT.

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UNWANTED PREGNANCY

DEAR SIR,

Dr. Heller, in his review of Prof. Schulte's book on Unwanted Pregnancy (*Journal*, August, 1969), seems to assume that because serious minded psychiatrists in Switzerland should interpret the Abortion Act there illiberally, only light-hearted psychiatrists whose balances need adjustment ought to be liberal in England.

I am sure that, while extrapolation from Switzerland to England may be justified, Dr. Heller himself would not wish his comments to be regarded as being applicable in every society. But it might be worth while to emphasize this point.

In Zambia, as in Switzerland, attempted suicide following refusal of termination is extremely rare among both the African and the European populations. However, septic abortion is extremely common, particularly among the African population, and deaths from this cause are also frequent. One of the gynaecologists working here has found that all of the patients whose requests for termination on psychiatric or social grounds were refused recently later returned to him as emergencies with septic abortion.

Alternative solutions to termination rarely work here, as the patient usually rejects one's proffered advice as soon as she gathers that one is refusing the termination that she wants; indeed, it would be rather surprising if the support that Prof. Schulte offers in Switzerland were always accepted.

In this country, therefore, the most important 'psychiatric' indication for termination is a determination on the part of the mother to go to an abortionist if she cannot have her pregnancy ended legally. Any psychiatrist working here who was burdened with a 'modicum of conscience' in Dr. Heller's sense would find himself contributing a good deal to the miseries of an already overburdened people.

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COMMUNITY PSYCHIATRY

DEAR SIR,

In reply to Dr. Levine's letter (*Journal*, October, 1969, p. 1227), I am not aware that I have any 'powerful drive to reduce the number of beds' under my care. The reduction in bed occupancy has simply taken place in response to the needs of the situation.

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