Brit. J. Psychiat. (1972), **120**, 471–6

Correspondence

Letters for publication in the Correspondence columns should be addressed to:
The Editor-in-Chief, British Journal of Psychiatry, Chandos House, 2 Queen Anne Street, London, W1M 9LE.

PSYCHIATRIC SEQUELAE OF THE BELFAST RIOTS

DEAR SIR,

H. A. Lyons in 'Psychiatric Sequelae of the Belfast Riots' (*Journal*, March 1971) presents conclusions that are not supported by his evidence. His investigation was of a *self-selected* sample of the Belfast population, and it is not possible to make any generalization about the population on the basis of such a study. The atypicality of Lyons' sample is shown most strikingly by the fact that 63 per cent of the Part I sample and no less than 90 per cent of the Part II sample had previously received psychiatric treatment.

Without a study of a random sample of the Belfast population at large, or at least a control group for his present study, Lyons' *Conclusions* (p. 272) are plainly absurd.

ROBERT MOORE.

University Department of Sociology,
King's College,
Old Aberdeen, AB9 2UB.

DEAR SIR,

The purpose of my paper was to study the psychiatric consequences following the severe Belfast riots of August 1969. Patients attending their general practitioners with psychiatric symptoms, and admission rates to mental hospitals, are well recognized and accepted methods of studying psychiatric morbidity in communities which have well developed psychiatric services (Hewetson, J. C. *et al.*, 1963; Kessel, W. I. N. and Shepherd, M., 1962; Norris, V., 1959; Shepherd, M., Brown, A. C. and Kalton, G. 1966; Taylor, S. J. L. and Chave, S., 1964).

The patients studied were a self-selected sample in that they were those who developed symptoms severe enough to consult their family doctor or be referred to a psychiatrist. There obviously were many others who developed symptoms of anxiety but accepted these as a normal reaction to the stress situation; but those who went to their doctor would represent the more severely disturbed, thus reflecting the psychiatric morbidity.

As regards patients referred to psychiatrists: all those admitted to day hospitals or the Belfast area mental hospital during the six week period following the riots were screened by the independent psychiatrist involved, and those whose illness was in any way related to the riots were selected for further study.

As the references quoted in my paper indicated, hospital admission rates have frequently been used during times of war, both international and civil, to study psychiatric morbidity. These studies have shown that it is the vulnerable who break down at these times, and the Belfast finding of a high incidence of previous psychiatric illness is in keeping with this, and could not be regarded as atypical.

The suggestion by Moore that a random sample should be studied is rather naive, indicating some lack of appreciation of the conditions existing in a riot situation, where high levels of suspicion and hostility would render co-operation in sampling unlikely. Furthermore it is important to assess patients clinically in the acute situation, as retrospective studies are fraught with inaccuracy. If one were to attempt a random sample in these circumstances one might well encounter a random bullet!

H. A. LYONS.

Purdysburn Hospital,
Belfast, 8.

REFERENCES

HEWETSON, J. C., *et al.* (1963). 'Incidence of psychiatric disorders in general practice.' *Practitioner*, **190**, 127.

KESSEL, W. I. N., and SHEPHERD, M. (1962). 'Neurosis in hospital and general practice.' *Journal of Mental Science*, **108**, 159–542.

NORRIS, V. (1959). *Mental Illness in London.* Oxford University Press.

SHEPHERD, M., BROWN, A. C., and KALTON, G. (1966). *Psychiatric Illness in General Practice.* London: Oxford University Press).

TAYLOR, S. J. L., and CHAVE, S. (1964). *Mental Health and Environment.* London: Longmans.

THE LANGUAGE OF SCHIZOPHRENIA

DEAR SIR,

I should like to offer a couple of critical comments on the very welcome paper by Maher on the 'Language of Schizophrenia' (*Journal*, January 1972).

Firstly, concerning the table of type-token ratios from schizophrenic speech and written material: I feel it should have been made clear that the T.T.R.

is only comparable if it is based on samples of constant size in number of words, i.e. where the number of tokens, that is the divisor in the ratio, is constant. This has been most often chosen as 100. As the divisor increases so the value of T.T.R. inevitably declines, the number of types being approximately related in a *logarithmic* fashion to the number of tokens in any sample (Herdan, 1960). The figures quoted for Critchley's subjects were 0·65 and 0·26. Consulting the original paper (Critchley, 1964), the actual ratios can be seen to be 54/79 and 331/1,241 respectively. I have recomputed these as log type/log token. The values become 0·90 and 0·94 respectively, which can be seen to be not very dissimilar. The other values quoted by Maher in his table are based on studies where 100 has been chosen as the sample size. Other studies have chosen different sizes of tokens, e.g. 900 (Salzinger, Portnoy and Feldman, 1964); 25 (Feldstein and Jaffe, 1962); 200 (Silverman, *in preparation*). Sample size is no mere arbitrary consideration, as Salzinger *et al.* found rank orders in matched pairs for T.T.R's considerably different between 100 and 900 word sample sizes (Salzinger, Portnoy and Feldman, 1964).

The second point concerns evidence for the 'immediacy hypothesis'. In point of fact it can be argued that Salzinger's results (Salzinger, Portnoy, Pisoni and Feldman, 1970) show, at least for 'low guessability' words, that 'distant' context *is* of greater benefit, *proportionately*, in the prediction of words from schizophrenic utterances over normals. Salzinger ignores the baseline predictabilities on going from contexts of 4 to 8 words, but when this is regarded from the viewpoint of *proportionality* the results become consistent with my own observations comparing 4th and 5th word deletion patterns with Cloze procedure (Silverman, *in press*). This supports the view that *inappropriate repetition* is of considerable significance as the encoding difficulty in schizophrenic subjects, as is also suggested in Maher's publication.

G. SILVERMAN.

University Department of Psychiatry,
Whiteley Wood Clinic,
Woofindin Road,
Sheffield S10 3TL.

REFERENCES

CRITCHLEY, M. (1964). 'Neurology of psychotic speech.' *Brit. J. Psychiat.*, **110**, 353–64.

FELDSTEIN, S., and JAFFE, J. (1962). 'Vocabulary diversity of schizophrenics and normals.' *J. Speech and Hearing Research*, **5**, 76–8.

HERDAN, G. (1960). *Type-Token Mathematics*. The Hague: Mouton.

MAHER, B. (1972). 'The language of schizophrenia: a review and interpretation.' *Brit. J. Psychiat.*, **120**, 3–17.

SALZINGER, K., PORTNOY, S., and FELDMAN, R. S. (1964). 'Verbal behaviour of normal and schizophrenic subjects.' *Ann. N.Y. Acad. Sci.*, **105**, 845–60.

—— —— PISONI, D. B., and FELDMAN, R. S. (1970). 'The immediacy hypothesis and response-produced stimuli in schizophrenic speech.' *J. abnorm. Psychol.*, **76**, 258–64.

SILVERMAN, G. (1972). 'Psycholinguistics of schizophrenic language.' *Psychological Medicine*. In press.

DEAR SIR,

I should like to call attention to what seems to me to be a basic weakness in Dr. Brendan Maher's erudite and intriguing paper on the 'Language of Schizophrenia'. His summary and analysis of research on differences in speech patterns between schizophrenic patients and normal controls is useful and interesting, though the findings are hardly sensational, i.e. that schizophrenic speech is less predictable than normal speech and far more likely to include tangential (my word, not his) associations. He points out, correctly I am sure, that normal speech (except when barriers are deliberately let down, as in psychoanalysis or word association tests) is one in which there is continuous inhibition of distracting associations, and that schizophrenic speech shows far less inhibition of such associational intrusions.

It is with his hypothesis as to the reasons for the difference that he seems to have become so obsessed with attending to the mechanism that he quite forgets the individual who is speaking. His hypothesis is that the 'inability' to inhibit 'irrelevant' associations is due to deficiency of attention, which he believes, for reasons that are not made clear, to be biologically mediated. The examples he gives of schizophrenic speech are then interpreted as if the patient wanted to say what he, the researcher, would think reasonable. One of his examples starts as follows:

'See the Committee about me coming home for Easter my twenty fourth birthday. I hope all is well at home, how is Father getting on. Never mind, there is hope, heaven will come, time heals all wounds . . .'.

He then goes on to speculate that the writer wanted only to express his wish to go home for his birthday and that the rest were irrelevant intrusions that he did not know how to inhibit. It does not occur to him that the writer might not have wished to say what is expected and conventional. He is apparently unaware that the ambivalence which the normal person generally represses is near the surface in the schizophrenic; and that his kind of communication, with associational patterns characteristic of dreams or of waking fantasy, is admirably designed to express such ambivalence. His speech is sometimes hard to understand because he speaks in a kind of shorthand,