

## Correspondence

### THE PREDICTABILITY OF SPEECH IN SCHIZOPHRENIC PATIENTS

DEAR SIR,

Manschreck *et al* (*Journal*, 1979, 134, 595–601) recently reported that the speech of thought-disordered schizophrenic patients was less predictable than the speech of non-thought-disordered schizophrenic patients. Rutter, Draffan and Davies (*Journal*, 1977, 131, 67–68) had earlier found no effect. Unfortunately, there are several flaws in the Manschreck *et al* study, and I should like to comment on each.

- (1) The study compared only five thought-disordered patients with five non-thought-disordered patients. We are not told how the ten were recruited, nor even whether they were randomly selected. Our own study examined twenty-five schizophrenic patients who were randomly selected from recent acute admissions.
- (2) Ratings of thought-disorder were based on a clinical interview, and cut-off points were selected so that only severely thought-disordered patients were included in the thought-disordered group. Our own study used the well standardized Bannister-Fransella grid test. Manschreck *et al* are sceptical about this test, and it is unfortunate that they did not include it in their own study so that it could be assessed against their preferred procedure on the same sample of patients.
- (3) It is traditional to take speech samples of around 200 words, and it is often reported that predictability changes as the passage progresses. Manschreck *et al* do not report the length of their samples nor whether length was constant across speakers.
- (4) As in our own work, predictability was assessed by Cloze procedure, under both fifth-word deletion and fourth-word deletion. Each rater clozed every passage under fifth-word deletion, and then, one week later, clozed them all again under fourth-word deletion. Of course they were likely to remember from the previous week, and any comparison between the two deletion patterns is meaningless.

- (5) The statistical analysis is poorly described, and there is no evidence that account was taken of the fact that every passage was clozed by every rater, so that the ratings were not independent.

I am afraid that these problems of methodology mean that it is simply quite impossible to draw any implications from the data.

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DEAR SIR,

We were most interested to read the comments of Salzinger, Portnoy and Feldman on our paper 'The predictability of speech in schizophrenic patients' (*Journal*, March 1978, 132, 228–32).

Salzinger *et al* first reported that schizophrenic speech was less predictable than normal speech in 1964 (Salzinger *et al*, 1964), and they went on apparently to confirm the finding in 1970 (Salzinger *et al*, 1970). The only study which has subsequently reported the same result was by Silverman (1972), but the findings were very difficult to interpret since they were based on a poorly controlled comparison between only seven 'actively schizophrenic' patients and seven 'other' patients, three of whom in any case had a history of schizophrenia. No other published study that we know of has managed to reproduce Salzinger's findings. Cheek and Amarel (1968) compared schizophrenic and alcoholic patients, Hart and Payne (1973) compared 'overinclusive' with 'non-overinclusive' patients, and we compared schizophrenic and normal patients (Rutter *et al*, 1978), but none of us could find a difference between groups. What is more, the mean Cloze score for our own 1978 schizophrenic group, 47.2 was virtually identical to the 48.9 we had found the previous year (Rutter *et al*, 1977) in a study of thought-disorder in 25 schizophrenic patients—the largest sample in any of the published studies.

Salzinger suggests that the difference between his findings and ours may be attributable to medication: his patients were not under medication; ours were. In fact, there is no published evidence to support the argument; and indeed the result of his own single-case study contradicts it, at least for small doses of chlorpromazine (Salzinger *et al*, 1961). A definitive

study remains to be done, but among the plethora of possible hypotheses to account for the differences in our results, we hold this to be an unpromising line of research.

The more likely interpretation, we believe, is to do with subject sampling. First, it is never clear how Salzinger *et al* select their subjects. In none of the papers do they state that selection was random; nor is the method of diagnosis described; nor are the diagnoses of the physically ill controls reported. In all our studies, including Rutter *et al* (1976) in which the prediction of speech by schizophrenic patients was examined, all subjects were diagnosed by doctors not connected with the project, the schizophrenic patients were randomly selected from the whole population diagnosed schizophrenic, and the controls were randomly selected from chest wards of general hospitals (1976) or from a physical rehabilitation unit (1978). Second, Salzinger *et al* acknowledge in their papers that the same data are sometimes used in more than one of their studies. Ten of the twenty-three passages included in the 1966 study had already been used in 1964; and all ten of the 1970 passages had been used in 1966. We have always used independent samples.

In the remainder of the letter, Salzinger *et al* go on to argue that there are three principal reasons for doing research on speech in schizophrenic patients. One is to relate the behaviour to other important aspects of psychopathology. This was exactly the purpose of our 1977 study, in which we tested the suggestion that the supposed unpredictability of schizophrenic speech might be related to thought-disorder. Cloze scores were correlated with scores on the Bannister-Fransella test of thought-disorder, but no relationship was found. Salzinger's second reason for doing this sort of research is to relate the findings to theory, and he contends that his own Immediacy Hypothesis fits the evidence well. This is not so for our own data. The hypothesis predicts that the second 100 words of a schizophrenic passage will be less predictable than the first 100, and also that the increase in predictability which fifth-word deletion is said to bring over fourth-word deletion will be greater for normal speech than schizophrenic speech. In our 1977 paper, we found no difference between the first and second 100 words; and, in the 1978 paper, we found that the second 100 words of the schizophrenic passages were significantly *more* predictable than the first 100, the opposite of Salzinger's prediction. There were no effects of deletion pattern in either study. Salzinger's third goal is to develop objective measures of important classes of behaviour. Indeed, but such measures, however objective, must discriminate between the relevant groups if they are

to be useful. We have never found that Cloze procedure distinguishes between schizophrenic and normal speech, and so we have recently turned to a new technique, 'reconstruction'.

The results of our first experiment with this technique are to be published shortly in the *Journal* (Rutter, 1979). Transcripts of the ten schizophrenic and ten normal passages from our 1978 paper were first given in unpunctuated form to an arts graduate who, blind to the design and purpose of the study, read them, listened to the tape-recordings, and inserted full stops where she believed they should be. Her performance was compared with that of several other judges and was found to be reliable. The first ten sentences of each passage were then shuffled into random order, except that the first was marked and always placed on top, and students were asked to reconstruct what they believed to be the correct order. While there was no difference between types of passage in the number of times students obtained correct runs of two, runs of three or more were achieved only half as frequently for the schizophrenic passages as for the normal passages. The difference was highly significant statistically, and was much larger than has ever been found with Cloze procedure. We believe that this is an important and promising finding. There is a detectable abnormality in schizophrenic speech which affects its comprehensibility—but it stems from the relationships *between* sentences rather than the content of individual sentences.

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DEAR SIR,

In 'The predictability of speech in schizophrenic patients' Dr Rutter and his colleagues conclude that the "literature [on schizophrenia] abounds with inconsistent results" (p. 231) because they failed to find a significant difference in predictability, as measured by the Cloze procedure, between schizophrenic and normal speech. They attribute their failure to find the difference Salzinger *et al* (1964, 1970) did to methodological factors, but they go on to say that "if so, the phenomenon lacks robustness and can be of little intrinsic interest" (p. 231). One wonders whether they would view blood pressure measurements in the same way. What if Investigator A failed to confirm a difference in blood pressure between hypertensive and normal blood pressure patients found by Investigator B, when A's normal subjects, but not the hypertensive ones, were exercised before the measurement?

What methodological differences might be considered here? The most interesting is that Rutter *et al*'s patients, unlike Salzinger *et al*'s, were receiving antipsychotic medication. A not too radical interpretation of the differences in results is that the medication improved the performance of the schizophrenic patients. Chapman and Chapman (1973) report that cognitive behaviour such as is involved in the Cloze procedure is improved by such drugs when given over a long enough period of time, in a large enough dose. The only study on the effect of tranquilizers on Cloze procedure was done on small acute doses by Salzinger *et al* (1961) who found reduced predictability of speech, but the Chapman review of the literature would have predicted such an effect in that case.

The research by Rutter *et al* begs for a drug study rather than a statement decrying the low state of

research in schizophrenia. There are a number of other differences between Salzinger *et al*'s and Rutter *et al*'s study: normal and schizophrenic subjects of the first study were matched, while in the second, only groups were made 'comparable', and the monologue was elicited by itself in the first study but collected as part of an interview (with no specification as to when in that interview) in the second study.

One more word about robustness of measures and whether the Cloze procedure is of 'little intrinsic interest' as the authors imply. One object of research in schizophrenia is to create objective measures of functioning of important classes of behaviour. The Cloze procedure is objectively scorable and it taps the extent to which people understand each other, a socially significant behaviour. A second object is to embed it in a theory (Salzinger, 1973) relating it to other findings; the Immediacy Hypothesis, which states that schizophrenic behaviour is primarily controlled by temporally close stimuli, fits the data particularly well as tested by a modification of the cloze procedure applied to schizophrenic speech (Salzinger *et al*, 1970; Salzinger *et al*, 1978) but also with respect to cloze performance executed by schizophrenic patients (Blaney, 1974; DeSilva and Hemsley, 1978). A third object is to validate the measure in question by relating it to significant psychopathological variables. The correlation between cloze scores on schizophrenic speech and the length of time the patients had stayed in a psychiatric hospital during six months' follow-up was  $-.47$  (Salzinger *et al*, 1966).

It seems to us that it is far better to investigate why there is a difference in results when trying to repeat an experiment than it is to glory in 'inconsistent findings'.

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