ATTEMPTED SUICIDE AS LANGUAGE

DEAR SIR,

Dr. Thompson is quite right to take us to task for using 'language' rather loosely in our paper on so-called attempted suicide (better termed 'parasuicides') as a form of communication (*Journal*, July 1970, pp. 121–2). Unfortunately we had no space in our paper in which to discuss distinctions between different kinds of communication, and a little inaccuracy often makes for both brevity and clarity.

Though we can scarcely develop the theme in a letter, we would suggest that it is useful to distinguish three main levels of communication:

1. The direct, uncoded expression of an affective (or similar) state, such as an angry blow or a cry of pain.

2. Non-verbal but culturally determined communication, such as a ceremonial bow or the raising of an eyebrow.

3. Verbal communication.

In our view parasuicide usually belongs to the second category. It is more than a simple expression of affect precisely because it is culturally defined or *coded*. It may thus be used by an individual to convey something other than itself, that is to say, it has one of the properties of symbolic communication. This means, of course, that the link between the act itself and that which it signifies is established by the subculture and is not an idiosyncratic association of the patient, in which case it would be meaningless. In this respect only does parasuicide have any of the characteristics of communication at the level of language. Dr. Thomson's own position on this point is not quite clear.

There is, incidentally, a wider issue here. It is often held by psychiatrists that much of the behaviour of most patients can be viewed as some kind of communication. Yet, in this country at least, very little research seems to have been carried out on how to define communication operationally and to study it objectively, let alone to distinguish its varieties. Unless this is rectified, the very concept of communication will go the way of 'dynamic', and for all practical purposes will cease to mean anything.

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GILLES DE LA TOURETTE'S SYNDROME

DEAR SIR,

Dr. N. Prabhakaran (*Journal*, May 1970, pp. 539–41), presents what he describes as the first case of Gilles de la Tourette's syndrome to be reported from India.

He further states that this syndrome has not been reported outside Europe and America. Dr. S. J. M. Fernando (*Journal*, June 1967, p. 614), too made a similar statement, in response to which Dr. A. Chakraborty (*Journal*, January 1968, p. 125) pointed out that two cases had already been reported in the *Indian Journal of Psychiatry*, one in 1962 and the other in 1966.

Within the last two and a half years, I have seen seven cases of this syndrome. I hope to report these in detail in the near future.

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DEAR SIR,

Dr. N. Prabhakaran's paper on the syndrome of Gilles de la Tourette states that this syndrome has not been reported outside Europe and America. Recently I have been preparing a report on a seventeen year old New Zealand boy with this syndrome, and while reviewing the literature have located two cases reported from India (Chakraborty, 1962, and Saroja Bai, 1966), and two cases recorded in Australia (Ellison 1964, and McKinnon, 1967).

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DEAR SIR,

I refer to Professor N. Prabhakaran's interesting report (3) of a case of Gilles de la Tourette's Disease in an Indian, which he states was described 'for the

first time outside Europe and America'. I wish to point out that the first such case, which was also the first case outside the white and negro races was, so far as I know, described by me in 1963 (4). This was in a Chinese boy in Hong Kong. I have subsequently seen two more in the Chinese here (5). All had multiple tics, copralalia and various echo-phenomena.

Concerning the occurrence of the condition in Asians, isolated unreported cases are said to have been seen in the native population in Taiwan (Hsu, written communication, 1969), Korea (Lee, written communication, 1970), Thailand (Suwana, written communication, 1970) and Malaysia (Simons, written communication, 1970). As regards Indians, antedating Professor Prabhakaran's report are the reports of two cases. The earlier report (1) which was in 1962 was on a 16-year old boy who, I am quite satisfied after going through the report, was not suffering from Gilles de la Tourette's disease. The main features were shouting and talking to himself, withdrawal, odd behaviour and posturing. But he had none of the typical features of the syndrome—tics, copralalia and echo-phenomena—and was more likely to be suffering from childhood schizophrenia or something else. Unsettled though the criteria for diagnosis of the syndrome may be, it is doubtful if the diagnosis should be made without multiple tics, which appear central to the condition. The other report was on an 8-year old boy (2). Here, although the resemblance to the syndrome is closer, there is nevertheless room for doubt. The main features given were 'fits . . . every few minutes . . . each attack involves all the four limbs and head and neck with the vocal utterances of barking and flickering of the eyelids—all for a fraction of a second'. It is most unusual even for severe tics to take the form described. The clinical picture is reminiscent of myoclonus.

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BASIC PSYCHOANALYTIC CONCEPTS

DEAR SIR,

I have been practising psychiatry and psychoanalysis for more than 45 years. After hospital appointments in Germany I was consultant at the Tavistock Clinic for 21 years. I am at present Director of Group Psychotherapy at the Columbus (Ohio) State Hospital. I am a Fellow of the American Academy of Psychoanalysis and of the Royal Society of Medicine.

For many years I have been very interested in the question of transference and non-transference relationships in any therapeutic setting, including psychoanalysis. To give an axample: in 1965 I said at a Congress in Madrid, 'The transference is not the only operative relationship within the analytic setting. It is always combined with the realistic human interaction, which is very different from an irrational projection-relationship.'

This fact of a dual relationship should be realized very clearly. Ultimate help can only be expected—after an often necessary revision-therapy—from a personal interaction relationship without which no form of therapy can be effective. This relationship is as old as Methuselah and need not be newly named 'working or therapeutic alliance'. This seems unnecessary and only blurs the issue.

Long before 1965 I mentioned these facts in various publications. To me it seems irrelevant whether this form of relationship is implicitly built into the fabric of analytical theory. It was everywhere long before, and the innovation is the forging of transference into a powerful therapeutic tool, while the other part of the interactions is not new at all. Freud gradually modified his ideas, as I think, and tried to put his new concept into existing conditions with increasing insight and experience. To regard the human interaction as a 'by-product' of analytic thinking seems to be unwarranted. Why should everything be put to the account of psychoanalysis? Transference in its special use is a new idea, general human interaction has always been there.

There are still very few remarks about the question of transference in the American literature. I myself have used various opportunities of speaking at international meetings and have included the problem in my writings, lately in my just published book 'Contemporary Dynamic Psychotherapy'.

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