SOME PSYCHIATRIC NON-SEQUELAE OF CHILDHOOD BEREAVEMENT

DEAR SIR,

I have been reluctant to respond to the paper by Munro and Griffiths (1969), as much of what I would write would be a recapitulation of material that you have already published (Hill and Price, 1967; Hill, 1969); indeed, the latter paper is adjacent to the article of Munro and Griffiths. But since the controversy has continued, and Prof. Munro continues to be less than fair to workers who have studied the relationship of childhood bereavement to later depression (Munro, 1970), I feel that I should contribute my comments.

There are certainly many discrepancies in reported studies, but in many of these, especially the earlier ones, the methodology employed has often suffered from gross faults (see Gregory, 1958; and Hill and Price, 1967). Difficulties in comparability also arise because of differing diagnostic fashions and variation of severity in the illness of the sample studied. It may also be that strictly local factors may affect the findings; for example, the drift of orphans into towns or to quote Munro and Griffiths, 'It would seem that the depressives of this part of London (Hampstead) are a singularly unfortunate group!' The authors are far more correct than they appear to appreciate, for Hampstead has the highest suicidal rate of all London boroughs (Sainsbury, 1955)!

Given this variability, what then is the validity and value in such studies? If the methodology is adequate and the population well defined, then we have said something valid about that population which may be more generally applicable. Unfortunately, there are certain defects of method in the Munro and Griffiths study which diminish the value of their contribution. A similar distribution for age in the experimental and control groups is vital, and it is not possible to assess this point on the data provided; in-patients cannot strictly be controlled with out-patients, as we know that social factors play some part in determining whether or not a patient is admitted to hospital (Lawson, 1966), and in addition careful investigation of general medical out-patients has revealed a high incidence of psychiatric disturbance, largely depressive illness (Maclay, 1965). It seems likely that there was a greater proportion of city dwellers (*vide supra*) in the Leeds Infirmary control group than in the psychiatric population seen in mental hospitals within 25 miles of Leeds: the misuse of the 1921 census as a control population in this sort of work has already been gone into at length (Hill and Price, 1967). Perhaps the most serious criticism of Munro and Griffiths' study arises from the severe limitation imposed by the smallness of the population studied, especially as they conclude that there is no significant difference between the groups. Their largest minimally homogeneous group is of 103 patients (male *plus* female, which in itself confers a major lack of homogeneity in this type of work). Certainly, if the size of the population studied by Hill and Price were scaled down proportionally to 103 all the significant findings would disappear.

In his letter Prof. Munro brings up some other specific points that are worthy of discussion.

Death is a preferred item for investigation, *not* because it necessarily has a more overwhelming effect upon the bereaved individual than other forms of absence, but because it is a well-defined event which is usually remembered and can be precisely dated. Its meaning and effect certainly vary for different individuals, but it is apparent that the class 'death' will be more homogeneous in effect than the class 'death plus all other causes of absence'.

The examples that Prof. Munro gives in his letter (Tables I and II) to illustrate the variability of parental loss in different subgroupings of depressive illness suffer from the absence of any statistically significant differences in the rates of loss between any of these subgroups.

He implies that the bereavement rate of other series is inflated by the inclusion of personality disorders, the omission of which is largely responsible for his own negative findings. The diagnosis of personality disorder is probably the least precise of all psychiatric categories. The Brown and Epps study which he quotes avoids this difficulty by using the objective criterion of admission to prison, but this group is likely to be very different from the general run of 'personality disorder' seen in the clinic. Positive associations of psychopathic disorders with parental loss have usually been found in the earliest years of childhood rather than in the 10–19 groups found by Hill and Price and by Birtchnell.

What then is the value of defining a factor which is of such small effect that it requires a large population for its adequate demonstration?

It is of some practical value to be able to predict from sociological data that the risk of attempted suicide in one defined group of depressed patients is substantially higher ($\times 2\frac{1}{2}$) than in depressed patients generally. A closer examination of the associations and consequences of the loss of a father in girls aged 10–14 (the most specific association with attempted suicide and depression) might lead us to an understanding of other factors of even greater significance to the outcome than the bereavement itself. Conversely, the examination of the consequences of other types of similar loss, for example, a father's rejection of his daughter at this time, might be rewarding. Overenthusiastic advocates of the importance of the sequelae of bereavement are in error if they do not accept this factor as being only one among a number, but equally it is naïve to dismiss this factor on the grounds that many individuals who are bereaved in childhood do not become depressed or attempt suicide in adult life.

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SHORTCOMINGS OF SCIENTIFIC PSYCHIATRY

DEAR SIR,

The paper *A Controlled Study of LSD Treatment in Alcoholism and Neurosis* by R. Denson and D. Sydiaha (*Journal*, 1970, **116**, 443–35) illustrates the shortcomings of the scientific approach in psychiatry. The controlled study is objective, the patients are classified according to psychiatric diagnoses, and the result of the treatment is evaluated with the aid of objective rating scales.

Patients were 'allocated at random to Treatment and Control groups', they had no choice of a therapist, and as they had been referred for this treatment from other centres they met a set of people with whom they had had no previous relationships.

After the treatment the patients returned to their referring psychiatrists 'who were expected to continue to provide standard treatment to the members of both groups' without having been present during the vital LSD experiences. One gains the impression from the paper that nobody was present during the LSD session, which was carried out 'in single rooms'.

LSD produces a variety of experiences; hallucinations, re-living of early childhood and insight into life as a whole with all its challenges. The authors of the paper admit that these experiences are intense and are liable to produce anxiety. They reduced this anxiety by giving the patients dextroamphetamine.

What people under LSD need is not a tablet but the relationship with a therapist who knows their problems and whom they trust (he may use drugs to modify the experience if he considers it necessary).

The patients who formed the scientific material for Denson's and Sydiaha's study were deprived of essential help. The scientific, objective approach might easily have driven some of them to a psychotic state or to suicide. It is not surprising that 'the supposed therapeutic benefits of LSD treatment in alcoholism and neurosis were not demonstrated by this experiment'.

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THE PHYSIOLOGY OF FAITH

DEAR SIR,

Dr. Allen's psychopathological approach to faith taken as a basis for the total field of human experience (*Journal*, March 1970, p. 352) limits rather than enlarges the understanding.

Medical training very rightly begins with a long period of study of the normal, and so should the study of faith.

The extremes of neurosis or psychosis, simply because they may have some religious content, are very poor material indeed with which to begin such a study. Such bizarre experiences may be included at a later stage, and with a due sense of proportion to the whole, but it is only after having taken full account of the phenomenon of faith throughout the world, in persons and communities of many kinds, many races and many creeds, that anything like an adequate picture can be formed, and this would require a highly trained team of experts in a variety of specialist roles to produce anything worth while.

To use the abnormal to interpret the normal is putting the cart before the horse, and so to use psychopathology in an attempt to gauge a person's faith in God is more likely to distort reality than to clarify it.

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