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Editor: Greg Wilkinson

Consensus Statement: Panic Disorder

SIR: We welcome the consensus statement (*Journal*, April 1987, **150**, 557–558) from a group of British psychiatrists that the status of panic disorder as a separate entity is not strongly supported by available clinical and scientific evidence.

Their statement, arising from a meeting arranged by a pharmaceutical company, devotes a fair amount of space to drug therapy for panics, but very little to non-drug methods of treatment. It omits to mention that systematic exposure lastingly relieves the most common type of panic – phobic panic. Evidence for the durable value of systematic exposure rests on numerous controlled studies and follow-ups four to seven years later: far longer than after any drug study. In chronic panic, enduring improvement after exposure was achieved without the side-effects and with less of the relapse on ceasing medication that can be troublesome in drug treatment.

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Temperament, Personality and Personality Disorder

SIR: Rutter (*Journal*, April 1987, **150**, 443–458) discusses a paper we have written on the subject of childhood temperament (Graham & Stevenson, in press). We have suggested that temperamental characteristics can be helpfully viewed as minor variations of behaviour which, in extreme form, can be regarded as psychiatric disorders, and have put forward evidence to support this view. We have not, as Rutter suggests, conceptualised temperamental characteristics as "mini-disorders" (his term, not ours) and indeed, in minor form, we see no good reason at all to think of such characteristics in pathological terms. Rutter rejects our view on five grounds, none of which, for reasons we state here, seems to us to constitute a valid objection.

Extremes of temperamental traits do not in themselves constitute disorder: This categorical statement by Rutter carries with it little meaning without definition of the terms concerned. We would argue that temperamental traits, such as activity, emotionality and 'socialisability', when shown in extreme form, do indeed represent handicapping forms of behaviour, and have cited evidence in the paper to which he refers.

Most of the symptoms of child psychiatric disorder are not part of temperamental concepts: We have not argued that *all* psychiatric disorder can be viewed as extremes of temperamental characteristics. Such a view would be absurd, and we have made this clear in our paper in which we cited anorexia nervosa and autism as two disorders that could not be so conceptualised. We have argued that extremes of the common varieties of temperament do constitute common disorders, and that much childhood depression, anxiety, hyperactivity, and antisocial behaviour can usefully be conceptualised in this way.

Patterns of correlation with change in behaviour rather than current disorder do not fit easily with our hypothesis: We are unaware of any significant body of work examining the relationship between background factors such as family disharmony or school failure with *change* of behaviour, although some such work certainly exists. Indeed, our own longitudinal study (Richman *et al*, 1982) represents one of the most substantial in this area. Most examination of background factors in relation to childhood disorder has concerned disorders of varying duration. There is no reason, however, why, given our hypothesis, change in background factors should not relate to a change in behaviour along a continuum from everyday behaviour to trivial, mild, moderate, or severe disorder, nor why temperament construed in the way we suggest should not be related to later behavioural change.

Our suggestion bypasses evidence on the indirect path by which temperament may lead to disorder via influences on other peoples' reactions: In fact, our hypothesis would have no difficulty in accommodating such evidence. If, for example, everyday but subclinical behaviour of an irritating type leads to parental rejection of child A more than everyday behaviour of a less irritating type in child B, it would not be surprising if child A's reaction to rejection involved a more extreme and disordered form of the same behaviour.

Our hypothesis fails to take account of the repeated finding that the effects of temperament may vary by sex: We are not suggesting that our hypothesis

can be used to explain all phenomena that relate to temperament – it would be highly suspect if it did. On the other hand, the hypothesis is certainly not disconfirmed by differential responses to temperamental characteristics in boys and girls.

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RICHMAN, N., STEVENSON, J. & GRAHAM, P. (1982) *Pre-school to School: A Behavioural Study*. London: Academic Press.

SIR: I am sorry that Graham & Stevenson feel that my use of the term 'mini-disorder' does not represent their concept of temperament. Nevertheless, it does seem in keeping with their argument in the earlier paper I cited (Stevenson & Graham, 1982). With respect to the observed association between temperament and the later development of behaviour disorder, they stated: "in fact we are merely observing a mild (or not-so-mild) problem turning into a larger one".

Their letter is helpful in clarifying their theoretical position, but let me comment briefly on the implications of the view that "much" child psychiatric disorder constitutes extremes of temperament. Firstly, the notion that psychiatric disorder implies more than just the extreme of a single behavioural trait is not just mine. The criteria for most psychiatric disorders in both ICD–9 and DSM–III require patterns of multiple symptomatology and social impairment. Moreover, as noted in my paper (Rutter, 1987), many *common* disorders are defined in terms of types of behaviour that are not part of any concept of temperament. That is obviously the case with problems such as enuresis or encopresis, but it is also so with many conditions, such as phobic states, that have a closer connection with temperamental features. The development of, say, school phobia or agoraphobia involves something different from a general escalation in anxiety; that is why they are differentiated from anxiety states.

Secondly, with acute disorders there is the need to account for onset and remission. It does not seem likely that this is accountable for in terms of accentuations and reductions in temperamental features. Nevertheless, I agree that evidence both for and against is lacking. Also, it is common for psychiatric disorders in childhood to exhibit considerable situation-specificity. This phenomenon requires a form of explanation that does not fit readily into a concept of extremes of temperament.

Thirdly, with pervasive chronic disorders there is the assumption of continuity between normal variations in the temperamental feature and its apparent equivalent in the pathological condition. Again, the empirical evidence is sparse, but it seems likely that often this is not the case. For example, Graham's demonstration that some cases of the hyperkinetic syndrome respond to an oligoantigenic diet (Egger *et al*, 1985) suggests that the disorder is not just the manifestation of a high level of temperamental activity.

Fourthly, I appreciate that the Graham & Stevenson view by no means excludes circular processes involving interactions with environmental influences. However, what it surely finds more difficult to encompass are processes that have results that involve the emergence of types of behaviour that are different from accentuations or diminutions of temperamental features.

Finally, there is the most basic point of all: the assumption that, if psychiatric disorder is just an extreme of temperament, both should share the same set of correlates. But is that so? Certainly there is a lack of supporting evidence. The implication, for example, is that the genetic basis of the two is the same and that the environmental features associated with psychiatric disorder should apply similarly to temperament.

I chose to discuss Graham & Stevenson's concept because of my high respect for their research and I would like to take the opportunity of publicly thanking them for their courtesy and helpfulness in letting me see and comment on their paper in advance of its publication. Our views on temperament differ in crucial respects but we are entirely at one in the view that empirical evidence will decide between our concepts. Such evidence is being obtained by them (as well as by other investigators), and I await their findings with great interest.

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