PSYCHOLOGICAL CHANGES IN CHRONIC SCHIZOPHRENICS FOLLOWING DIFFERENTIAL ACTIVITY PROGRAMMES

DEAR SIR,

The qualifying remarks of Phillips (p. 574) raise points which had previously prevented him from accepting co-authorship for the study under discussion (Hamilton, *Brit. J. Psychiat.*, March, 1964, p. 283). Editorial requirements of brevity had prevented me from dealing with some of these adequately in the article.

1. With regard to the alleged shortcomings of the tests employed, it is true to say that the results obtained from the Activity-Withdrawal and Clinical Rating Scales in the repeat study are probably less valid and reliable than the measures obtained by these devices in the first study (Hamilton and Salmon, *J. Ment. Sci.*, 1962, p. 505). This would reflect, however, on the obtained improvement trends as well as on the absence of hypothesized significant improvements.

2. If the measures of motor speed and skill (reaction times and track tracer score and goal discrepancy index) contained, against usual practice, unreliable responses or runs, these should, of course, have been excluded by the experimenter. To what extent unreliable test scores or procedures for individual subjects can be said to vitiate the results from a whole test is a moot point, since each subject was his own control.

3. The fact that Mr. Phillips found some difficulty in applying a modified version of the Vineland Social Maturity Scale and tended to be more rigorous in his scoring cannot really be said to affect the results from the 1962 study which showed significant improvements for the Workshop group; only comparisons between the original and the repeat study would be affected. In any case, these methodological difficulties cannot be said to be relevant to one experimental group (Workshop) only and its intra-group follow-up comparisons; they would apply equally to all experimental groups, and thus would not affect the second aim of this series of studies, namely the comparative rehabilitative efficacy of different activity programmes, unless there were objective evidence that one experimental group produced significantly more unreliable scores than another.

4. In assessing the validity of my conclusion that the weight of the objective evidence, despite its limitations, favours Workshop Therapy (a conclusion drawn with appropriate caution, viz. Hamilton, 1964, p. 286) the concurrent study on changes in I.Q.s should, I believe, be taken into consideration (Hamilton, *Brit. J. Psychiat.*, 1963, p. 642). This follow-up study permitted the conclusion that patients who had undergone a quasi as well as a realistic Workshop regimen made superior all-round intellectual gains compared with other subgroups. This held whether comparing samples of different N.s or equal N.s, so that a hypothesis to account for statistical differences between samples in terms of the differential power of tests of significance and stability of the median for different sized samples is disconfirmed. A separate study may well support Phillips' hypothesis, but at present, in the absence of empirical evidence, we remain in the field of conjecture and the only available evidence—in relation to intelligence changes—does not support the conjecture.

5. My general feeling on the problem of hypothesizing a relationship between degree of improvement of a population sample on the one hand and original basal level on the other, or on the problem of whether two samples with somewhat different basal levels come from one or two populations in relation to these studies, is that they are problems best tackled by further empirical work rather than by polemics. Such issues can rarely be settled by inference, even if inference were tempered by plausibility. For the work undertaken at Springfield Hospital it was simply impossible to produce experimental groups fully matched on 15 parameters for several studies. The best that could be achieved was to minimize the number of significant differences between groups receiving, say, Workshop Therapy and Occupational Therapy. This, I feel, was achieved with some success in both the major studies, so that there is insufficient evidence to postulate that basal levels and thus populations within each study were so markedly different that on *a priori* grounds they should be expected to mark out different incremental gradients. Nor can I accept Phillips' contention that the two studies (Hamilton and Salmon, 1962; Hamilton, 1964) were carried out on populations which must be considered different populations on clinical, statistical or methodological grounds. While the patients taking part in the second study were undoubtedly older and more impaired, the overlap was considerable and the groups were significantly different on initial testing on only 4 out of 13 experimental measures, apart from age and chronicity.

6. Phillips' methodological criticisms, as distinct from his criticisms of the measures used in the study with which I believe I have dealt, seem to say something like this: if a larger group (Workshop) has a greater chance of producing significant results, if it starts from a lower basal level and if the learning curves of subjects in these studies have some of the

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characteristics of all other learning curves, then the conclusions of the rehabilitation studies are unsupported. This, I submit, is poor logic. Even if the limiting contentions were true—and Phillips has produced no good evidence that they *are* in relation to all the studies—this could only mean that alternative explanations *might* account for the direction of the findings if and when appropriately tested, because Phillips has not drawn on internally conflicting or contradictory figures or test results. It would be poor logic on my part now to suggest that the introduction of Workshop Therapy to all mental hospitals at an even more realistic level should be halted pending definitive answers to some remaining theoretical issues.

Yours faithfully,

VERNON HAMILTON.

Postscript. Miss Phillida Salmon, co-author of the original paper (Hamilton and Salmon, *J. Ment. Sci.*, 1962, p. 505) wishes to associate herself with these comments.

DEAR SIR,

May I comment briefly on three of the points raised by Dr. Hamilton?

1. He now cites another study (Hamilton, *Brit. J. Psychiat.*, 1963) in support of the superiority of workshop therapy. Unfortunately there are seventeen inconsistencies, implying at least twenty-one errors, in its Tables (details available on request), and without reliable data it is not possible to know what to make of it. In any case, I argued not against the *proposition* that workshop therapy is superior (which may very well be true), but against the *inference* from the results of Hamilton and Salmon (1962) and Hamilton (1964) to the proposition, i.e. that it was unsupported by the evidence.

2. He argues that the initial between-group differences in each study are insufficiently marked to be responsible for the different trends shown by the groups. But these trends are themselves of doubtful significance. I think it would be better science not to interpret them at all, but if they are to be considered, as he has done, then largely insignificant initial between-group differences should, in all consistency, be considered as well.

3. In his final paragraph he reformulates my arguments, and submits that his reformulation is poor logic. I agree: but it does not accurately represent my position, which I had better clarify. There were, in the above pair of studies, two separate sources of possible bias in favour of the workshop group: its larger N and its lower initial position on a negatively accelerated improvement curve. It is not possible to say whether the (doubtfully significant) greater improvement trends in the workshop group resulted from one, or the other, or both, or from a real superiority of workshop therapy, and the inference that the latter is the case is unjustified.

Yours faithfully,

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PHENOTHIAZINE EFFECT ON HUMAN ANTIBODY SYNTHESIS

DEAR SIR,

Drs. J. C. Saunders and E. Muchmore have done good service to your readers by bringing to their attention an important but still little recognized potential hazard of phenothiazine medication. Their paper (12) unfortunately omits to quote experimental work which has already been carried out in this area. The significant contribution which they themselves have made cannot therefore be viewed in proper perspective. Moreover, they may give the impression that their findings lack support from other quarters.

Eight years ago Goldman (3) reported that infections are numerically the most frequently encountered complications in the institutional use of chlorpromazine and reserpine, while one year before that Rosenow (11) reported that he found the titre of antibodies to a haemolytic streptococcus to be significantly lower in patients on chlorpromazine medication than in a comparable non-tranquillized group of patients.

Some of the early experimental work with various species of animals and various pathogenic organisms (1, 2, 6, 9, 10) led to inconsistent conclusions.

More recently, this writer (4) has shown in a carefully controlled experiment that in mice to which were administered various dosages of chlorpromazine and *S. enteritidis* inocula increased daily dosage of chlorpromazine shortened the average length of survival for each level of *S. enteritidis* inoculation. The relationship was highly significant ($p < \cdot 005$). Blood cultures taken from various groups of mice during the experiments demonstrated an earlier onset as well as a more prolonged *S. enteridis* bacteraemia in those infected mice which were on chlorpromazine medication. A similar finding was previously reported by Maral and Cosar (7) in their experiments on tranquillized rabbits inoculated with pneumoccoci. These observations have subsequently been confirmed by other workers.

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