Our research design, randomising after admission and after some early in-patient default, does not allow for a meaningful overall comparison of default rates between the in-patient and day patient groups.

Less than 50% of all referrals to our day hospital end up attending—confirming the basic point made by Drs Davidson and Connolly. We try, however, to arrange a preliminary contact and explain fully the implications of day attendance prior to admission so that our early default rate for patients actually admitted is close to the lower reported figure of 16%.

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Seasonal Affective Disorder

DEAR SIR,

The paper by James *et al* (*Journal*, October 1985, **147**, 424–428), on winter depressions provided a welcome bit of light relief. Patients were recruited through a newspaper advertisement and reported that they felt less depressed after exposure to bright light than dim light, during a cross-over trial. The authors report that they did not inform the patients of their prediction that bright light would be superior, yet that must surely have been obvious (unless, of course, the patients themselves were very dim).

At one point in the discussion the authors, commenting on the fact that even their dim lights had some antidepressant effect, acknowledge that "we would have predicted that the publicity about the previous studies would have biased patients against responding to the dim lights". Could they throw some light on why they do not mention this as a possible explanation of their findings?

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Drs Rosenthal and James Reply

DEAR SIR,

Dr King raises an important point, which was not addressed in our article, though we have discussed it in detail elsewhere (Rosenthal *et al*, 1985).

It is conceivable that different antidepressant effects in response to bright and dim light treatments might have occurred as a result of different expectations. Such patient expectations might have been generated by the appearance of the light and might have been amplified by comparing the two light conditions and speculating about our underlying hypothesis. While patients were able to make comparisons during the second phase of the cross-over, they had no basis for comparison during the first phase as they were not shown both types of light before the first treatment. If expectations based on a comparison of the two conditions were responsible for the superior effect of bright light, one would have expected that those who received dim light second, after they had already been exposed to the bright light condition, would have responded more poorly than those who received it first. Conversely, one would have expected that those who received bright light second would have responded better than those who received it first. However, analysis of Hamilton Depression Ratings showed no ordering effect, which would suggest that differential expectations resulting from comparison of the conditions did not play a significant role in the different antidepressant effects.

That still leaves open the possibility that, regardless of the order of presentation, the bright light might have generated more powerful expectations of improvement than the dim light. Such a possibility can never be ruled out with certainty in any study where it is impossible to administer the intervention on a double-blind basis. However, the following observations, based on all our light treatment studies, suggest that a placebo mechanism alone is not a plausible explanation for all the resulting antidepressant effects. 1) There is generally a lag time of approximately two to four days both for response after initiating light treatment and for relapse following withdrawal of light. One would expect a placebo to show a more rapid and variable time course for response and relapse. 2) Light treatment has been effective repeatedly and for prolonged periods of time within individuals, whereas placebos generally decrease in efficacy over time and with repeated use. In some cases light treatment initiated in the fall has prevented the development of winter depression. 3) As stated in the paper which Dr King cites, we have found the pattern of response to both bright and dim light to be consistent over the years. In addition, the difference between the efficacy of bright and dim light treatments has not grown progressively larger from year to year, as one might have predicted had the response of patients been due to expectations generated by widespread publicity.

In order to address the question of patients' expectations more directly, future studies of light therapy should incorporate some means of evaluating expectations systematically before each treatment condition.

Although Dr King's comments were provided in a light-hearted vein, we found them illuminating and

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trust that he will be similarly enlightened by our response.

NORMAN E. ROSENTHAL
STEVEN P. JAMES

Reference

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Depression: Distress or Disease?

DEAR SIR,

Professor Brown and his colleagues (*Journal*, December 1985, **147**, 612–622) are right to suggest that many of the affective disorders seen by psychiatrists are clearly related to psychosocial adversity. The other part of their argument, that 'cases' in the community do not differ greatly in severity from those seen by psychiatrists, is questionable. Symptoms are widely distributed in members of the general population; many people have a few symptoms but few have many. This distribution is curvilinear, without points of discontinuity. Wherever we decide to place the threshold for defining cases, provided an appreciable proportion of the population is so recognised, the severity distribution of cases is likely to reflect the underlying distribution of symptoms. In other words, most cases will be mild, a few severe. This has been noted in every community survey using the PSE-ID-CATEGO system that has provided such details, and is independent of the actual prevalence quoted.

Brown and his colleagues argue for similarity in severity between cases in the community and among patients. They do so on the basis of their findings in the Islington community and those reported by Sashidharan (1985) from his series of patients in Edinburgh. This would be justifiable only if the thresholds used in the two studies were the same. Both report results using the PSE-ID-CATEGO system, so the symptom ratings are combined in the same way to construct thresholds for case recognition. However, the criteria for recognising symptoms might well have been stricter in the Edinburgh study than in the Islington survey. Certainly the reported population prevalences in the Edinburgh survey are far lower (3.9% using the Bedford College case definition; Dean *et al*, 1983) than those reported in Islington (17%) or by the same team in Camberwell (15%—Brown & Harris, 1978). Moreover, the criteria for symptom recognition among the patients who form the basis of Sashidharan's paper (1985) could well be even stricter, as these were based on interviews by psychiatrists, whereas the Edinburgh community survey was carried out by lay interviewers. It is known that psychiatrists tend to have stricter criteria (Wing *et al*, 1977; Sturt *et al*, 1981). An appropriate test of the hypothesis can be made if data are collected by the same interviewing team from a sample of the general population and from a comparison group of local psychiatric patients. There is such a study, although it is not mentioned by Brown and his colleagues (Wing *et al*, 1981). Unlike that of Sashidharan (1985) this comparison was based on the full 140-item PSE, and demonstrated marked differences in severity between patients and cases in the Camberwell community, defined as ID5 and above (Wing *et al*, 1978).

It is possible that we were rating symptoms more strictly than the Bedford College group, as our case prevalence based on Bedford College criteria was around 8.0%. Lowering our threshold for identifying symptoms would have increased the prevalence, but it would also have made the disorders in patients seem more severe.

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The Use of SRQ-20 in Primary Care Situations

DEAR SIR,

I was interested to read the article on the use of this instrument in Sao Paulo (*Journal*, January 1986, **148**, 23–26) as we have used it in an attempt to

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