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AUTHORS' REPLY: Some interesting points are raised by Driscoll & Thompson. Firstly, Levitt & Joffe (1988) actually suggested that the small numbers in their study groups might have led to a type I error (not type II error), a point which is difficult to grasp. However, our finding is similar to theirs in that we reported lower mean testosterone levels in the depressed group of men with melancholia ($n=11$) compared with the non-depressed control group ($n=10$). In the light of the thorough study by Rubin *et al* (1989), where blood sampling occurred at 30 minute intervals for 26 hours in 16 endogenously depressed men (according to Research Diagnostic Criteria) and 16 individually matched controls, and no significant differences in total testosterone were found, we hesitate to say that our results may reflect a type II error; more so since (as we stated) Rubin *et al* found testosterone levels to correlate positively with melancholia in the subgroup of six men with melancholia according to DSM–III criteria, and all of *our* patients were melancholic. Because of our small sample, we were particularly stringent in statistical analyses, and a test of significance showed $P=0.025$ for levels of salivary testosterone in the depressed men compared with non-depressed controls.

Secondly, in the depressed group itself we found significantly lower salivary testosterone levels to be associated with more severe depression. One possible cause (of several) suggested was hypercortisolaemia. Having examined the correlation between cortisol and testosterone, there is no support for this. The correlation between cortisol and testosterone was $r=0.042$ ($P=0.9$; $n=11$) before dexamethasone and $r=-0.11$ ($P=0.7$; $n=10$) after dexamethasone. This finding therefore concurs with that of Yesavage *et al* (1985).

Thirdly, is there an age factor? We found no association of testosterone levels with age ($r=-0.26$, $P=0.4$; $n=11$), neither was there an association of depression, as measured by the Hamilton Rating Scale for Depression and the Montgomery and Åsberg Depression Rating Scale, with age ($r=0.08$, NS and $r=0.22$, NS).

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Attitudes to mental illness

SIR: I read with interest the two articles (Brockington *et al*, *Journal*, January 1993, **162**, 93–99; Hall *et al*, *Journal*, January 1993, **162**, 99–108) describing the survey of attitudes to mental illness in Malvern and Bromsgrove. Factor analysis indicated fear of the mentally ill as a main component, but in this case in fact almost 85% of the scores were positive "showing the absence of fear of the mentally ill among most people in the general community" (p. 95). This is a little surprising bearing in mind that a substantial proportion of the literature suggests that members of the 'general public', at least on occasion, view patients and former patients as unpredictable and dangerous. One might have therefore expected at least some ambivalence from subjects in this respect. In spite of the study's sophistication, and its use of vignettes, it may be that an additional design could usefully have complemented their study; for example, the discourse analytic approach advocated by Potter & Wetherell (1987), Gilbert & Mulkay (1984), and others, in which subjects are freely encouraged to express their opinions in dialogue and

in which the subtle, thoughtful, and often equivocal nature of people's views might be allowed to emerge. Rather than seeing attitudes as enduring, homogenous, and as hypostatised internal mental states, it may be more productive and realistic to acknowledge that people construct and express 'attitudes' for particular purposes, in different argumentative contexts, and with differing effects. In the terms of Billig's (1987, p. 225) rhetorical approach, attitudes represent "unfinished business in the continual controversies of social life". The analysis of the structure of argumentation may shed light upon what are often the competing and contradictory results of survey research.

In the second of the two articles, Hall *et al* misrepresent the study by Segal *et al* (1980) whom they suggest "found that the mentally ill may seem most socially integrated in neighbourhoods with little social cohesion, and that there was more resistance to their placement in residential as opposed to commercial areas" (Hall *et al*, p. 106). In fact, Segal *et al* (1980) do not find this at all. Hall and colleagues appear to be quoting that paper's review of previous research, thus; "Trute and Segal (1976) . . . found the highest levels of social integration among residents of sheltered-care facilities that were located in neighbourhoods with little social cohesion" and ". . . Piasecki (1975) found that more resistance was encountered in residentially, as opposed to commercially, zoned areas" (Segal *et al*, 1980, p. 346). But Segal and colleagues go on immediately to say of such studies that "These are simple, unidimensional descriptions with limited practical utility . . . " (ibid).

In fact, Segal *et al* (1980) suggest that what they called the "liberal, non-traditional neighbourhood" may be the "accepting community that researchers have sought" (pp. 352–353). This kind of community is characterised by its liberality, departure from convention, high rate of criminal activity, and other factors, but this should not necessarily be taken to indicate a lack of 'social cohesion'.

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An Eysenck for the 1990s?

SIR: Dr Andrews (*Journal*, April 1993, 447–451) states that "polemics distract from the essential task of psychiatry" and in this we can only agree. It is sad that Dr Andrews fails to follow his own advice in his recent review. He has taken on the self-appointed role of '*Malleus maleficarum*', the protector of a naïve public from the evils of dynamic psychotherapy. As a result it would be quite possible for an observer to think that cognitive–behavioural therapists are immune from therapeutic error and that transference and countertransference can be totally expunged by keeping the therapy brief and focused.

It is subtly suggested that good clinical care is really an extension of good cognitive therapy. The work of Meares & Hobson (1977) suggests, however, that the therapist is *always* at risk of becoming a persecutory figure to vulnerable patients. Their contribution, based firmly in the psychodynamic tradition, is now an accepted part of teaching doctors to be aware of interpersonal issues and the risks in *any* therapeutic approach. That such training is necessary for all psychiatrists, whether or not they eventually adopt a psychodynamic approach, is explicit in the guidelines for training of the Royal College of Psychiatrists.

Dr Andrews is right, however, to suggest that forming an alliance, understanding the 'undercurrents', being professional and enthusiastic in addressing treatable symptoms, and persisting in providing care and support are all essential to good practice. Indeed, the thrust of much of the recent literature has favoured such an integrationist approach.

Eysenck (1952), in his classic analysis of psychotherapy versus natural remission, did a great service to psychotherapy researchers by prompting a thorough review of outcome assessment methodology, although it is now known that there were serious flaws in his own study. We hope that Dr Andrews' review could have as beneficial an effect in the 1990s as Eysenck had in the 1950s. We are concerned, however, that Dr Andrews is generating a myth rather than a piece of reasoned appraisal.

His most extraordinary claim is that dynamic therapy is dangerous but cognitive–behavioural therapy is safe because "there are, as yet, no substantiated reports of harm" and because the treatment is short and so does not lead to transference and countertransference complications. We are surprised at such a disingenuous statement, and would point out that there is little to support his view that cognitive–behavioural therapy is intrinsically safer than other modes of therapy. All therapists need to be helped to identify areas of their practice which