Table 1
Multivariate comparisons of actual costs for costed sample and predicted costs for uncosted sample

		Costs per patient week (£)	
		DLP sample	Control sample
Actual cost for costed sample	mean	173.0	253.5
	sd	191.9	259.5
	n	74	68
Predicted cost for uncosted sample	mean	174.0	322.3
	sd	115.5	196.5
	n	16	24

made, to allow predictions from the costed group onto the uncosted group. These equations are cited in the paper (*BJP*, August 1994, **165,** 195–203) and explained in more detail in Knapp (1994). The admission characteristics of every patient meeting the inclusion criteria for the study were entered into multiple regression analyses to predict costs 12–20 months after admission. Separate regression equations were estimated for the DLP and control groups. The results of the predictions are summarised in Table 1. The DLP patients for whom costs were missing in this 12–20 month period are predicted – on the best evidence available from the study – to be no more or less costly than the costed sample (P=0.952). In marked contrast, the uncosted control group patients are much *more* costly (P=0.046) than the costed sample. Had the uncosted patients been included in the final period of the evaluation, the cost-effectiveness advantage to the DLP sample would have been greater.

The generalisability of the DLP results is certainly open to discussion. In our paper we ourselves enter four caveats. The fifth caveat suggested by Drs Weich and Lewis – missing observation bias – is unfounded. They are, of course, quite right to argue for asylum (in the fullest, truest sense of the word) for people with acute mental health problems, and one spin-off from the multivariate analyses cited here is that they help to demonstrate who does and who does not benefit from community-based treatment.

KNAPP, M. R. J. (1994) Community mental health services: towards an understanding of cost-effectiveness. In *Evaluation of Community Psychiatric Services* (Eds F. Creed and P. Tyrer). Cambridge: Cambridge University Press.

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AUTHOR'S REPLY: Weich & Lewis seem to have misread our report. They write that the home-based DLP was "driven by the ideological view that patients should never be in hospital"; we wrote that "Patients were not strenuously kept out of hospital at all costs, being admitted briefly if warranted" (*BJP*, August 1994, **165,** p. 181).

Weich & Lewis identify only "two distinct interventions: 'acute' and 'post-discharge' care" – omitting the planning for discharge during in-patient phases, and the attempts to prevent relapse. They seem to dismiss ("if there were any benefits") the highly significant increase in satisfaction of both patients and relatives in the DLP compared to the control group. This was clear by month 11 in the first year and continued to the final rating 45 months after the start. Is user satisfaction a trivial benefit?

"The DLP group fared better in relation to the control group at the end of the trial, when discharge was under the control of other clinical teams": This is a further misunderstanding. At the end of Phase I, at 20 months after trial entry, most DLP patients who had been admitted were still under the care of the DLP team, not of another clinical team. The transfer of responsibility for crisis admissions to other teams took place only in the third year of patient intake late in the study, and so affected few patients in their first year after trial entry, by which time the DLP was already superior to the control group not only on satisfaction but also on length of stay and on some clinical and social variables. The transfer of responsibility was a salutary natural experiment; it was followed by a trebling of the duration of those crisis admissions.

Our Audini *et al* report (*BLP*, 1994, **165,** 204–210) indicated the problems of maintaining gains from community care beyond the first couple of years. The problem the DLP had in this regard may

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have had more to do with the fallout from the media ordeal it underwent than from any other factor.

Regarding Weich & Lewis's suggestion for an intent-to-treat analysis, we obtained bed-day data from almost every patient who entered the trial. Analysis of this variable did constitute an intent-to-treat analysis and these data accounted for the bulk of the economic benefit. Regarding other variables, substitution of the missing data with values from previous data points in the trial may have been even more misleading than omitting those patients from the analysis. Regarding our repeated tests of statistical significance, these had to be done to know what happened separately at each rating time point, as the DLP might have been more helpful at some times than at others. This was indeed what we found.

Drs Weich & Lewis write of "satisfaction ratings by assessors who were not blind to treatment status". In fact our assessors did not rate satisfaction. If what is intended is a criticism that the assessors were not blind, we pointed out (p. 182) that "For safety, raters had to ask carers about risks during the coming interview, and so could not be blind to treatment group". We would be curious to know how Drs Weich & Lewis would deal with this methodological problem which is almost inevitable in patients who are so unpredictable and not infrequently dangerous.

We are sure that the writers' colleagues and others would be interested in the evidence that "the standard of routine post-discharge care at the Maudsley (the control treatment) was notoriously bad at the time". Regarding 'realistic' approaches, the DLP's problem/goal-oriented method of working is now part of the Thorn training programme for community nurses in London and Manchester.

In what seems to be their takeaway message, the authors allude to a "powerful argument in favour of asylum for the acutely mentally ill, particularly in our inner cities". This has long been obvious to workers with the seriously mentally ill in their homes and neighbourhoods. It was part of the DLP's ideology on its first day of operation, as indicated in our top paragraph above. The need for some in-patient beds for this population is clear from the way the DLP was run. We found that such beds should be run by the same team which does the community care – i.e. community care is not enough. Continuity of care, too, is important. Ideally there would be no need for an interface if a patient's care was continuously in the hands of the same keyworker and team both in the community and in the ward.

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Investigating multiple personality disorder

SIR: I commend Piper (*BJP*, May 1994, **165**, 600–612) for acknowledging the existence of multiple personality disorder (MPD), irrespective of how narrowly he chooses to define the condition or how rare he considers it to be.

I will not respond to a number of points and criticisms of the disorder and myself raised by Piper, as many of these issues have been covered in previous exchanges of letters elsewhere. In particular, his concerns about the reliability and validity of the Dissociative Experiences Scale (DES) were answered recently in the *American Journal of Psychiatry* (Piper, 1994; Putnam & Carlson, 1994).

I concur with Piper that a major part of the problem with the acceptability of MPD lies in the ambiguity of the DSM criteria. I too have criticised the gross inadequacies of these criteria. We have the empirical data to sharply delineate MPD from other psychiatric disorders. The failure to do so in the DSM–IV reflects a political rather than a scientific problem.

I agree with Piper that the nature of the alter personalities is the critical issue. Not surprisingly, he is able to extract from several decades of clinical literature, differences of opinion as to what the alter personalities constitute and how fully developed they are. The MPD field has now reached a general consensus that the alter personality states are limited psychological constructs organised around a contextualised sense of self with a restricted range of affect, behaviour and autobiographical memory (Putnam, 1992). Although early clinical accounts tended to dramatise the individuality of these entities, more recent work emphasises their component nature within the larger psychology of the individual. More focused clinical and experimental inquiry regarding the cognitive continuities and discontinuities of the alter personalities states should inform future debate.

MDP may also be considered within the larger domain of the study of dissociation, both normal and pathological. Many different kinds of data exist demonstrating that trauma is significantly associated with increased dissociation. In more recent

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