($r = -0{\cdot}95$) and Koczapski *et al* (1985) ($r = -0.89$). Also, their observation that weight gain was greatest during the fourth quartile of the day (6 p.m. to 12 midnight) is consistent with our finding that fluid consumption is greatest during that quartile among chronically psychotic polydipsic patients. (Vieweg *et al*, 1986).

We offer another explanation as to why their patient receiving lithium had the poorest correlation between plasma sodium and body weight, since our work (Vieweg, 1987*b*) and that of Goldman *et al* (1988) provide compelling evidence that levels of antidiuretic hormone are inappropriately present among hyponatraemic chronically psychotic patients subject to water intoxication. Also, we have used lithium to promote diuresis in such patients (Vieweg *et al*, 1988*a*).

Sleeper (1935) and we, more recently (Vieweg *et al*, 1988*b*), have emphasised the importance of an empty bladder in assessing water dysregulation among chronically psychotic patients. Such patients, particularly those likely to experience water intoxication, may have residual volumes of up to 3 l of urine. We suspect that Delva & Crammer's patient receiving lithium had varying degrees of urinary retention at the time of weighing which confounded their findings. The data on their other seven patients offer compelling evidence that the haemodilution secondary to antidiuresis explains the relationship between plasma sodium and body weight. Thus, unless they suggest that lithium promotes brisk natriuresis, methodological error must be operative.

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SIR: We agree that the lower correlation between plasma sodium and body weight ($r = -0.65$) in the lithium-treated patient may have been due, at least in part, to variably incomplete bladder emptying. This is one possibility among several; another is that variable amounts of unabsorbed water may have been present in the gut at the time of weighing.

Although the cause of the lower correlation in this patient remains unclear, the problem of incomplete bladder emptying is one that should be taken into account in the design of studies for this group of patients. Incomplete emptying can be prevented by bladder catheterisation, and though this method is used routinely in animal experiments, complications, especially in males, make its use problematic in human subjects. In addition, many chronic psychiatric patients with polydipsia would find this procedure unacceptable. At present, therefore, the simplest way to correct for incomplete urine samples is to measure urinary creatinine excretion and we have used this method in all our recent work.

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The Mind-Body Problem: Another Defect in Training

SIR: Yorke (*Journal*, January 1988, **152**, 159–163) has recently discussed the lack of confidence that assails many psychiatrists as a consequence of their failure to acquire a systematic grasp of psychological theory. Training in this subject is clinical, empirical, and piecemeal, and the result is optimism in the presence of patients we can treat, and helpless amateurishness with the rest, whom we are at a loss to

understand (Glover, 1958). His remarks are entirely convincing, as is his description of "the dialogue of the deaf" between 'organicists' and 'psychologicists', if I may coin an ugly term.

But I do not think that the remedy he proposes is sufficient. Teaching more psychology, even systematically, will never succeed in bridging the gap between mind and body until we begin to confront the nature of the gap itself. Campbell (1984) has presented the problem succinctly and graphically in the form of four incompatible propositions: (a) the human body is a material thing; (b) the human mind is a spiritual thing; (c) mind and body interact; and (d) spirit and matter do not interact.

Examination of the tetrad shows that any three of these propositions are compatible, but they always contradict the fourth. To reject any one of them is to 'have a philosophy' about the mind-body problem, but unfortunately *each* of the four seems *prima facie* to be true. In particular, we find it hard to reject (d). The mysteriousness of the idea of spirit-mind influencing material body is the fuel that drives the 'organicist' to reject psychologising as on a par with fortune-telling and superstition, and it is this difficulty of his that makes it so hard to teach psychology in medical school and afterwards. 'Spirit' simply does not fit comfortably into the medical curriculum.

It is my contention that "the dialogue of the deaf" would assume a very different character if we could only prevail on the protagonists to spell out plainly their positions in regard to Campbell's dilemma. The hard-line behaviourist or materialist, for example, would be forced to say unambiguously that 'the mind' is *not* a spiritual thing (i.e. reject (b)). He would then be required to characterise every *prima facie* psychological phenomenon that presents in the clinic (reports of misery, obvious denial, delusions with a readily understandable content given the patient's situation) as a bit of behaviour or a neural event. This would have two welcome results: (a) he could be comfortable with the consistency of his approach, and with the theoretical nature of those clinical findings for which there is room in his conceptual scheme; and (b) he might be chastened into a new modesty as he encounters a series of difficulties with much of his material. For he would soon discover that there is a great deal which he must either ignore or violently corrupt.

The 'dynamic' psychiatrist, on the other hand, would be forced to reject (d) (surely not (c)?), and say that in human beings, at least, matter and spirit *do* interact. But Pond (1986) and Kendell (1983) are almost certainly correct in saying that Freud's naive concept of "psychic energy" *fails* as an answer to the mystery of mind-body interaction. Hume (1975) once marvelled: "Were we empowered by a secret wish to remove mountains or control the planets in their orbit, this extensive authority [to initiate voluntary movement] would not be more extraordinary, nor more beyond our comprehension". Yet the mystery arises every time we lift a cup from its saucer (Eccles *et al*, 1985). Since no one has come up with a convincing answer to this problem, the 'analytical' clinician would have to admit the difficulty each time he introduced a psychodynamic explanation into a case discussion, and not pretend, as he so often does, that his body of knowledge (let us grant it is knowledge) is part of the same scientific discourse that embraces physics, chemistry and organic medicine).

As for the easy-going eclectic, self-examination will show him either that he has no position on the question at all, or that he rejects (b) (preferring to see 'the mind' as an extended metaphor, like some analysts), (c) (which makes him a parallelist), or (d) (like the analyst in the example above). The first alternative leaves him in the irrational muddle from which most of us suffer (Benjamin, 1986), but with the loss of his erstwhile complacency. The other three alternatives expose him to a series of standard (and difficult) objections which professional philosophers know by heart, but which I will not review here. In any event, he might develop a new respect for his extremist opponents in the behaviourist and analytical camps.

The upshot of my agument is that the training of psychiatrists, and perhaps of every medical student, must include a thorough study and frank discussion of that part of philosophy called the mind-body problem. Our ignorance of this subject, not of psychology, is at the root of our discomfort, and of the acrimonious debate that results. Let the amateur dualist learn that his position apparently entails scepticism about other minds. Let the behaviourist struggle to explain sensations and decisions. Let the would-be parallelist hear what philosophers in history have thought of his position. We would all be the humbler for the experience.

There is nothing anomalous about arguing for a place for philosophy (certainly alongside psychology) in the medical curriculum. Philosophy, after all, is where medicine and psychiatry began. The time has come to acknowledge how great is its influence on everyday psychiatric theory and practice.

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Failure to Convulse with Electroconvulsive Therapy

SIR: Replying belatedly to this report by Sharpe and Andrew (*Journal*, January 1988, **152**, 134–136), I would like first to ask why their electrodes are soaked for 30 seconds? I suggest that they would have fewer failures if they ensured soaking of at least 30 minutes.

But to return to the basic problem of the ECT-resistant patient: the simple answer for 25 years has been inhalent convulsant therapy with flurothyl (Indoklon). I still have a small supply, but this preparation is no longer manufactured. I have for some little time been trying to get production re-started, although the Ohio Chemical Company no longer exists. The main reason for this therapy falling into desuetude was the unreliability (less so with great care) of the face-mask contact, which I hope to resolve – perhaps using a more concentrated mixture.

A case in point: a few months ago I was called in by a colleague to consider planning a course of ECT for one of his patients. The patient failed to respond on seven consecutive occasions with three different machines, but, switching to flurothyl, we achieved a fully successful programme with satisfactory outcome.

If anyone is interested I have a tape made here last year; the original study was reported by Rose & Watson (1967).

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Depression in Adolescents and Young Adults

SIR: Klerman (*Journal*, January 1988, **152**, 4–14) proposes an increase in depressive illness in adolescents and young adults to explain recent studies. He also mentions an increase of depression in children. What he is describing is the unexplained phenomena of the symptomatology of minimal brain dysfunction or attention deficit disorder (ADD), which always contained signs of depressed mood being ascribed to depressive illness. The overlaps of depressive illness and attention deficit disorder according to DSM–III criteria has been repeatedly documented.

A child with attention deficit disorder, experiencing continual social and academic failure, has good reason to feel depressed. This does not warrant a diagnosis of depressive illness.

All children or adolescents with behavioural difficulties have depressive symptoms. Their parents frequently suffer from the same genetically transmitted instability, and for the same reason present intermittent symptoms of depression. The majority of suicide attempters are overreactive, labile, and impulsive, the traits associated with ADD. Unfortunately, they are treated with antidepressants at full dosage, experience unpleasant side-effects, and do not return for further treatment. Most of these individuals achieve greater stability with small doses of antidepressants (Huessy & Wright, 1970). But this positive response to tricyclics, in small doses with immediate improvement, cannot be used as an argument for the diagnosis of depression. The immediate response to small doses represents a biochemically different response from the one seen to bigger doses over three weeks in major depressive illness.

Since 13% of all children suffer from identifiable problems of overreaction, lability, and impulsivity, and since these childhood problems turn out to be lifetime problems, there are many more young adults with depressive symptoms secondary to their lifelong problems of instability than there are individuals with major depressive illness. We have compared adults with depressive illness with patients with the adult form of childhood behavioural disorders, and documented how they differed in drug response and family history (Huessy *et al*, 1979). Both types met DSM–III criteria for major depression.

It is this diagnostic confusion which lies at the basis of Dr Klerman's discussion and which is leading to unfortunate contamination of recent epidemiological studies.

Patients with major depressive illness have 'clean' childhood histories. Adults with behaviour disorders have problem childhood histories and family histories. Since, numerically, there are many more of the