The editorial consists of a series of sweeping statements, as if of fact, of a generally psychodynamic nature. Psychodynamic formulations are, at best, hypotheses, and are intrinsically tautologous in nature; thus, for example, the same mental process can result in two quite different patterns of thought, depending how far along the chain one stops, as with Freud's formulation of paranoia as a function of latent homosexuality. Such explanatory systems are, therefore, from a scientific standpoint, extremely weak, since they are intrinsically unfalsifiable. Dynamic psychology is no basis on which to propound a thesis with such potentially profound implications.

Central to the whole argument seems to be the proposition that patients do not behave badly; instead, the most outrageous behaviour is a maladaptive expression of inner distress which calls for care and sympathy rather than censure. This is a question of moral belief. While in some instances people behave badly as a result of distress or pathology, most bad behaviour that is encountered in psychiatric practice is the result of conscious, wilful decisions on the part of patients; this is at one with mainstream Western moral philosophy and the principles of English law.

I do not consider that Watts & Morgan do mental health professionals justice in their formulation. In my experience, psychiatric staff are remarkably tolerant of extremes of offensive and violent behaviour, particularly when these occur in patients with well defined mental illness, and to accuse them of acting on the basis of unresolved countertransference hate when they are abused or assaulted by patients who are in full control of their faculties is unwarranted, and to propose that in so doing they directly place the patient at high risk of suicide is improper.

Watts & Morgan's thesis would appear to absolve patients of all responsibility for their actions, which is as absurd as the Szaszian rejection of the concept of diminished responsibility – the truth lies somewhere between these two poles. If, in the absence of clear-cut pathology causally related to untoward behaviour (and the link must be established by more substantial evidence than psychodynamic speculation), we deny patients recognition of their responsibility for their acts, then we also deny them recognition of their essential human dignity, while at the same time creating an intolerable burden for us as mental health professionals. The concept of 'omnipotence' referred to so frequently by Watts & Morgan has much more in common with the paternalism of their approach than with what actually goes on psychiatric wards and in community mental health centres.

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AUTHORS' REPLY: Having already made our case, we are content in the main to leave others to assess for themselves Dr Davis' response to it. It does, however, seem important to respond in more detail to Dr Davis' anxieties concerning what he regards as potential adverse medicolegal aspects of the concept which we propose.

Setting limits for difficult behaviour, thereby deciding on the degree of personal responsibility appropriate to each individual, is practically a day-to-day task which any psychiatrist has to face. It also happens to be one of the most difficult. Suicide can occur after limits have been set with scrupulous care, and such a situation should not reflect adversely upon the health care professionals concerned. At no point does our editorial imply that patients should be absolved indiscriminately from personal responsibility for what they do. We merely propose that the many complex factors which beset us as we manage suicide risk, and these concern not only those relevant to the patient but our own reactions as well, should be reviewed systematically and objectively. We believe that such an approach should help to reduce the risk of adverse medicolegal repercussions, rather than increase it as Dr Davis fears. Finally, may we say that we object to his implication that the concept of malignant alienation reflects badly on the dedication and tolerance of mental health professionals, whom it is intended to enable rather than denigrate.

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Managing the manipulative therapist

SIR: Malignant alienation refers to the process by which carers develop negative feelings for their difficult patients which are inadequately dealt with, so that they start rejecting their patients under the cover of rationalisation. Patients are thereby exposed to progressively greater risks of suicide. I fully concur with all their points, but was surprised that Watts & Morgan did not discuss the concept of manipulation.

The term 'manipulation' may refer to a therapeutic manoeuvre "used to designate instances of crude efforts to run the patient's life" (Stewart, 1985). This practice has only qualified support. Aside from describing what therapists do, the term is more often used to describe coercive behaviour by patients. Frequently descriptions of 'manipulative patients' are overtly slanderous. 'Manipulation', when loosely used, tends to imply: (a) the patient has engaged in a negative behaviour ('manipulative' implying he/she does so habitually) that angers the therapist; (b) he/she has done so purposefully (as opposed to responsively); (c) the behaviour should be resisted or countered by staff – supposedly for the benefit of the patient (this is often done by staff attempting to out-manoeuvre or out-manipulate the patient).

This process goes against the very essence of suicide prevention as it implies therapeutic defiance rather than a therapeutic alliance. This will be fuelled by poorly contained countertransference hate. The jargon of the concept fuels the rationalisation – giving a misguided sense of understanding.

I propose that the term 'manipulation' be dropped, as all too often it is used in a way that is not only unhelpful, but hazardous. The degree of understanding of patient behaviour fostered by this term is that which might be expected of lay people. Professionals would do better recognising that acting out by distressed patients is usually largely responsive rather than solely purposeful. Behavioural concepts of reinforcement and dynamic concepts of defence mechanisms provide much deeper levels of understanding. They also permit the fostering of therapeutic alliances rather than confrontations when staff endeavour to help difficult, distressed patients.

STEWART, R. (1985) Psychoanalysis and psychoanalytic psychotherapy. In *Comprehensive Textbook of Psychiatry* (eds H.I. Kaplan & B.J. Sadock). Baltimore: Williams and Wilkins.

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Treating negative symptoms

SIR: The treatment of negative symptoms in schizophrenic patients is a difficult clinical problem; thus, positive clinical trials in this area, such as that of Duinkerke *et al* (*BJP*, October 1993, **163**, 451–455), are always of interest. However, some methodological questions arise from this study.

The use of type II schizophrenia as an inclusion criterion is unclear. Crow (1981) has pointed out that he regards type I and type II as symptom clusters rather than patient types. Thus it is uncertain whether the authors operationalised the concept of type II schizophrenia and also uncertain whether the presence of type I (positive) symptoms constituted an exclusion criterion. In this respect, and also to estimate the generalisability of the study, it would be interesting to know what proportion of the patients screened met the study entry criteria. The use of a Wilcoxon signed rank test to examine the difference between a 'positive' and 'negative' BPRS subscore is problematic unless clinical significance has somehow been assigned *a priori* to such a difference.

Further questions arise concerning the statistical procedures. The number of patients studied was small (33) and no confidence intervals or power calculations are provided. There are also no data on the inter-rater reliability of the SANS in this study, which apparently took place in six centres. No correction has been made for multiple significance testing of the individual SANS items; however, since these items are not statistically independent, a simple Bonferroni correction would be too conservative. Resolution of this problem depends in part on which variables were defined as primary before the study was performed (Oakes, 1993).

Accepting nonetheless that a true effect of ritanserin has been shown, this could be due to an effect on at least two other factors different from but often confused with negative symptoms: extrapyramidal symptoms (EPS) and depression. The authors themselves quote evidence that ritanserin acts on both. In this study, a depression rating scale was not used, but there was a significant effect on the BPRS item 'depressed mood'. As for EPS, low scores on the Simpson–Angus scale do not exclude neuroleptic-induced phenomena such as poverty of gesture or mask-like facial expression, since these do not appear in the scale. None of the patients in the study was taking anti-Parkinsonian medication, so the presence of EPS must be seriously considered. In this respect it would be interesting to know the average daily neuroleptic dose in chlorpromazine equivalents.

Finally, although such an 'add-on' study neatly avoids the ethical difficulties of placebo treatment, it raises the problem of pharmacokinetic interactions. Serum neuroleptic levels were not measured and therefore a pharmacokinetic effect cannot be ruled out. The use of ten different neuroleptics (of five different chemical classes) in 33 patients is a further complication.