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AUTHORS' REPLY,—Mr Owen and colleagues are correct that we did not directly refer to the shortened lifespan in gross obesity because no operation has yet been shown to reverse this trend successfully. We did not discuss the Scopinaro biliopancreatic bypass because only its originator has substantial experience of it.

We were perhaps a little uncritical in quoting reports of dysplasia and gastritis in some of the gastric procedures. But no other reports on the Alden procedure have been published so far that refute the assertions of McCarthy *et al.* We remain unconvinced, however, that gastric operations for gross obesity are a satisfactory solution; the small proximal pouch (50 ml) impairs normal eating for the rest of the patient's life. In a masterly review Näslund compared gastric bypass surgery and gastroplasty and pointed out that little is known about the physiology of these operations.[1] The several published modifications to these surgical techniques point to the difficulty of obtaining consistently good results.

We take issue with Mr Owen and colleagues over the ease of reversing gastric bypass. In our experience great difficulty has occurred, with not insignificant morbidity.[2] Unfortunately, five years' follow up is not enough to draw firm conclusions. Jejunoileal bypass remains a very successful operation even after 10 years, and only after that time was it abandoned. So once again we advise patience before drawing conclusions.

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Type A behaviour and coronary heart disease

SIR,—Dr Tom Sensky's cautious optimism about type A behaviour (11 July, p 69) has been confirmed by a recent meta-analysis that examined all studies relating type A behaviour to coronary heart disease published between 1950 and 1985.[1]

The authors showed that the effect size (Pearson's r) for all measures of type A behaviour and all coronary heart disease groups was 0·136 ($p \ll 0{\cdot}0001$). This means that type A behaviour accounts for about 2% of the variance in coronary heart disease. Although this is a small effect size, the reliability of the finding and the number of people affected make it non-trivial. The more detailed results of the study were instructive and included highly significant effect sizes on coronary heart disease for the following personality indices: Jenkins activity survey, 0·067; type A structured interview, 0·221; extraversion, 0·071; anxiety, 0·124; and depression, 0·205.

Other results from the same study were relevant to Dr Sensky's concerns about the relation between the structured interview and self report measures: the "hard driving" component of the Jenkins activity survey had an effect size of 0·153, larger than either the type A (all measures) or the total Jenkins activity survey score effect sizes. Further, "job involvement" on the Jenkins activity survey was not related to coronary heart disease while "speed and impatience" were weakly related, leading the authors to conclude that this finding seemed to contradict the popular notion of the workaholic who develops coronary heart disease.

Both Dr Sensky's and Booth-Kewley's and Friedman's studies fail to emphasise that those who have developed the type A concept have been reinventing the wheel. As Eysenck said, "proponents of the concept often argue and write as if well established models of personality did not exist and fail to relate their concepts to these dimensions."[2] Fortunately, this is beginning to change and there are now reports that type A behaviour is related to neuroticism, trait tension, anxiety, depression, and extraversion.[2,3] If these obvious empirical investigations had been done earlier we might have discovered before now that a single component of the Jenkins activity survey predicts coronary heart disease better than the whole scale.

Sensky comments that the type A concept is not a unitary one, and, indeed, this conclusion has occurred to psychologists concerned with a scale which, superficially, appears to describe a "type."[2,4] Eysenck, for example, concluded, "Clearly type A behaviour is not unitary; certain aspects of it are closely related to neuroticism, others to extraversion and the two groups of type A behaviour traits are themselves almost unrelated. . . . In this general description, it will be noted, there is no trace remaining of the concept of type A behaviour as such; the concept has been shown to be a chimera, stemming from perfectly correct observations of the originators of the concept, followed by psychometrically inappropriate analysis, and disregard of much better established personality dimensions."[2]

In a discipline that now accepts psychology as one of its basic sciences these are embarrassingly elementary lessons. The finding that personality predispositions relate to coronary heart disease is important, and its investigation requires an awareness of the appropriate methods and skills.

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3 Langeluddecke PM, Tennant CC. Psychological correlates of the type A behaviour pattern in coronary angiography patients. *Br J Med Psychol* 1986;59:141-8.
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Managing psychopathic offenders

SIR,—Dr Derek Chiswick does not appear to have addressed the fundamental question of whether psychopaths are suffering from some form of mental abnormality that actually requires treatment—however ineffective current treatment may be (18 July, p 159). He is not correct when he states that psychopaths are not treated with electroconvulsive therapy, as a preliminary analysis of data on 131 psychopaths (men and women) in Broadmoor and Rampton Hospitals found that 29% had received electroconvulsive therapy at some time in their lives, with 55% previously admitted to ordinary psychiatric hospitals, not infrequently on a hospital order, but invariably under the legal category of mental illness (J W Coid, Royal College of Psychiatrist's conference, 1987). One in four women and one in five men had suffered a schizophrenic breakdown (usually short lasting) and 61% had had a depressive illness during their lifetime. These patients tended to receive diagnoses of personality disorder (DSM-III multiple axis 2). Their histories also showed a surprisingly high incidence of early cerebral trauma, emotional and physical deprivation, and depression and personality disorder in their first degree relatives.

Some of the issues seem confused, with one argument made against another problem. For example, Dr Chiswick is right to point out the unsatisfactory situation of mental health review tribunals discharging dangerous psychopaths back into the community, but this is not directly relevant to their need for treatment in a therapeutic setting. Risk to the public from future offending may well be high when psychopaths were compared with mentally ill subjects in the studies quoted, but none compared a similar group of personality disordered subjects released from prison without treatment. It may be premature to conclude from the statistical associations of the study by Dell *et al*[1] that the discharge of psychopaths relates merely to the seriousness of the offence while that of schizophrenics relates to the chronicity of illness. It is also unfair to describe the endeavours of the small group of special hospital staff who are actively concerned with psychopaths as mere "time fillers."

Certainly many legal aspects of the detention of psychopaths outlined by Dr Chiswick are not entirely satisfactory, and treatment programmes for these patients are sometimes patchy and poorly organised. However, careful unbiased research into their treatment and its effectiveness is badly needed. Perhaps more importantly, research is needed into what is actually being treated. Reducing the numbers of psychopaths detained in psychiatric hospitals does not relate to their need for treatment but to how staff feel about attempting treatment. In the case of the state hospital at Carstairs it most probably related to the terrible events of the past.[2] Dr Chiswick is right to describe the management of psychopathic disorders as a problem that will not go away. Excluding them from hospital merely dumps highly disturbed individuals into an already overcrowded prison system with no hope of treatment before their return to the community.

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Prediction of resources needed for treatment of renal failure

SIR,—We are aware of the project of Ms Ruth Davies and Mr Huw Davies (25 July, p 271) and hope that their model will produce precise predictions of value. Unfortunately, no such predictions are available and no comparisons may be drawn. The superiority of the model they propose awaits corroboration other than that provided by references to their own work. Furthermore, there is debate about the relative merits of mathematical models. We chose to use a steady state approach to avoid the difficulties that arise if too detailed an attempt is made to give year by year numbers and costs for a renal replacement programme. We are satisfied that our approach provides a minimal target of need as we have used the most favourable implications for outcome in every case—for example, with regard to transplant availability, the proportion of patients suitable for independent dialysis, and the costs for different methods of treatment including continuous ambulatory peritoneal dialysis.