clinical judgment.⁴ Following head injury the mechanism of injury, including estimated forces, should play a part in selecting patients for radiography. In addition, while it is desirable to minimise waiting times for children in accident and emergency departments, too rapid a passage through the department may prevent repeated clinical examination and can thus lead to premature discharge of patients with potential intracranial complications.

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Personality and fatal diseases

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In our review of Eysenck and Grossarth-Maticek's research on personality and disease we suggested that they may not understand just what they are claiming.¹ Eysenck's recent reply in this journal seems to confirm this.²

In responding to our criticisms Eysenck outlined their hypotheses on the causes and prevention of cancer and ischaemic heart disease. But it is their claims and not their hypotheses that are under consideration. Eysenck and Grossarth-Maticek maintain they have identified personality types that increase the risk of cancer by about 120 times and the risk of ischaemic heart disease by about 25 times. They have invented a method of psychological treatment, creative novation therapy, which brings about massive reductions in death rates over the next decade. Also, a leaflet on this treatment (entitled *How to Achieve Emotional Independence and a Healthy Personality*) plus an explanatory introduction claims a reduction in deaths over the next 13 years in those with disease prone personalities from around 80% to 32%.^{3,4}

We are accused of being vague in mentioning many errors, inappropriate analyses, and missing details in the publications on this research programme. We value this opportunity to be more specific, to clarify just a few of the questions raised by ourselves and others,⁵ which Eysenck has failed to answer,^{2,6} and to outline additional findings from these authors' investigations.⁷⁻⁹

The criticisms

Firstly, Eysenck has made it clear that the trial of individual creative novation therapy (which he now calls the Maudsley intervention project⁸) involved only 192 subjects throughout, rather than 192 pairs, half of whom were lost to follow up. We note his assertion that "none of the referees considering the paper, nor the editor of the journal which published it . . . had the slightest doubt about the design of the study."² If he considers the following paragraph from his account of the trial, however, he may understand why we were misled: "Originally 134 pairs of type I probands and 138 pairs of type II probands had been approached with an offer of psychotherapy: of the former, one or both members leaving in 34 cases, of the latter, in 46 cases, thus leaving 100 pairs of type I probands and 92 pairs of type II probands."⁴ The referees and editor of *Behaviour Research and Therapy* did well to understand the design so clearly.

Secondly, Eysenck and Grossarth-Maticek seem to have difficulty with the concept of matching. They pair matched subjects on sex and five other variables (age, smoking, cholesterol concentration, blood pressure, and personality type) and "randomly assigned one of each pair to therapy and one to the control group."² Why, then, do the sex distributions in the two groups differ?^{2,8,9}

Thirdly, it is useful to know that there were two surveys in Heidelberg in 1972 and only one other in 1973 "of several samples selected by different methods, the results from only some of which have been analysed."² Will Eysenck now confirm that this last survey investigated 29 800 subjects?

Fourthly, more details are required on the training of the "100 or more workers who collected the original and follow up data"² and on the reliability of the research interviews in their hands. The remarkable accuracy of prediction of cause of death years later would be impossible if there were the slightest misclassification of personality types.^{10,11}

Fifthly, Eysenck and Grossarth-Maticek have been criticised for not using survival analysis to present the results of their randomised controlled trials.¹² They found massive differences in death rates in their two groups (treatment and control) over the ensuing decade, and these techniques would give useful information on when the divergence in mortality began.

Sixthly, the use of survival analysis techniques is not an academic nicety. It could cast some light on the relevance of a fundamental omission from the main description of their randomised controlled trial of individual creative novation therapy.^{3,4} In later publications it emerges that there was a subgroup of 41 people in their study who had type III hypertensive changes on funduscopy (including haemorrhages around the optic disc and retinal oedema) and a mean systolic blood pressure of 207.4 mm Hg.⁸ About two years later there were 51 subjects with type III retinal changes; this subgroup's mean systolic blood pressure after the creative novation therapy trial is said to be 211.7 mm Hg. We can only hope that these data are incorrect. An alternative conclusion is that Eysenck and Grossarth-Maticek have identified people with dangerously high blood pressures and entered them into a psychotherapy trial without ensuring that they received effective antihypertensive treatment. Further questions arise therefore. What methods were used to measure blood pressure? Were subjects' family doctors

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informed about these alarming clinical findings? Which research ethics committee granted permission for their experiment?

Seventhly, Eysenck maintains in his response to our article that there have been previous similar results and published replications of his findings on the cause and prevention of fatal diseases. Examination of the papers cited simply does not support this.¹³⁻¹⁶ For example, Kissen and Eysenck's early comparison of patients with bronchial carcinoma and other lung diseases showed that the patients with cancer were "somewhat extraverted" (mean extraversion score of 7.58 v 7.08) and "markedly lower in neuroticism" (mean neuroticism score 3.36 v 5.05).¹³ Quander-Blaznik has also examined the responses to a personality questionnaire of 31 people with bronchial carcinoma and 35 with other lung diseases.¹⁴ Once again there were modest differences between the groups. These results are not surprising in view of the numerous, inconsistent findings on psychological profiles of patients with malignancies,¹⁷ and they bear no comparison to the astounding claims of Eysenck and Grossarth-Maticek. Although Schmitz claims to have replicated Eysenck and Grossarth-Maticek's findings, the design of his study is completely different and his vague description of methods and results makes it impossible to assess its importance.¹⁵ Schmitz investigated 100 people with "psychosomatic complaints and behaviour disorders" referred for training in relaxation techniques. These patients were categorised by using the Eysenck and Grossarth-Maticek personality typology and asked about their medical diagnoses during the past five years. The major finding was that "cancer-prone personality" (type I) was associated mainly with complaints of allergies and "6 out of 7 persons (all of them females) who suffered or were still suffering from cancer or from pre-cancerous [sic] abnormalities could be assigned to type I." The cancers and pre-cancerous conditions are not specified.

One study, however, can be seen as an attempted replication. Eysenck and Grossarth-Maticek themselves have examined subgroups of 715 subjects prone to cancer (type I) and 469 prone to ischaemic heart disease (type II) from their large 1973 study in Heidelberg to examine hypothesised dangers of psychoanalysis.⁴ Nine years later 3.4% of type I and 2.1% of type II subjects had died of cancer, and 2.6% of type I and 3.4% of type II participants had died of ischaemic heart disease. This is a convincing non-replication of their previous investigations which showed after about 10 years' follow up a 39% mortality from cancer in type I and a 25% mortality from ischaemic heart disease in type II subjects. Eysenck and Grossarth-Maticek seem once again not to have realised what their own work indicates.

Our final specific criticism relates to the appearance, disappearance, and reappearance of the personality type characterised by "rationality-antiemotionality." As Temoshok has pointed out, rationality-antiemotionality was highly correlated with the incidence of cancer in early papers but it was not included in the four categories of personality described in the main papers in *Behaviour Research and Therapy*.¹⁸ Although Eysenck states that the relation of rationality-antiemotionality to their main typology is "clearly stated" in an earlier article (reference 7 in Eysenck's recent *BMJ* paper), the only information provided is the mean score for rationality-antiemotionality in each personality type plus a correlation coefficient.¹⁹ Rationality-antiemotionality has now reappeared. In another substudy from the large 1973 survey 36 people were identified for each of six personality types. After 13 years no fewer than 16 of the 36 with a personality

characterised by rationality-antiemotionality had developed rheumatoid arthritis.⁷ Eysenck and Grossarth-Maticek have not attempted to explain this latest remarkable finding. As rationality-antiemotionality is now claimed to be the principal risk factor for two major idiopathic diseases Eysenck's reasons for reverting to use of this concept need to be stated more clearly.⁶

Conclusions

Our concern has been to clarify the methods and analyses of a body of research which, if accurate, would profoundly influence public health policies on cancer and heart disease. Other critics have been more challenging in what they have alleged,^{5 10 11 20} and in our opinion the controversy which now surrounds one of academic psychology's most influential figures constitutes a crisis for the subject itself.²¹ The seriousness of the detailed allegations by van der Ploeg,²⁰ although refuted by Eysenck and Grossarth-Maticek, should in themselves prompt these authors to re-examine their own findings after appropriate further training in the methodology of medical research.

Perhaps the most skilfully worded criticism on this subject was made not about Eysenck but by him in a debate on the relation between smoking and cancer.²² In disputing the findings of Doll and Hill's epidemiological studies on this association he comments: "What we have found are serious methodological weaknesses in the design of the studies quoted in favour of these theories, statistical errors, and unsubstantiated extrapolations from dubious data to unconfirmed conclusions." Eysenck owes it to himself and to his discipline to reconsider critically his own work on this subject.

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