of two subgroups within the group of consistent responders: hyperresponders and minimal responders. Again, a significant difference in total cholesterol between the subgroups (table III in their paper) only confirms the definition.

Secondly, a significant difference in baseline characteristics such as cholesterol ester transfer activity and total cholesterol (table IV in the paper) could be due entirely to regression to the mean.^{2,3} Here, a hyperresponse or a minimal response (whether consistent or not) would be determined by the initial cholesterol values.

Thirdly, the same baseline characteristics show a significant correlation with the mean change in total cholesterol for consistent responders (table V in the paper). This can be explained by the difference between hyperresponders and minimal responders. These variables, therefore, are not predictors of response but determinants of the subgroups. With this step the argument comes full circle. It remains unclear whether any biological evidence can be derived from this circulation.

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- 1 Cox C, Mann J, Sutherland W, Ball M. Individual variation in plasma cholesterol response to dietary saturated fat. *BMJ* 1995;311:1260-4. (11 November.)
- 2 Bland JM, Altman DG. Regression towards the mean. *BMJ* 1994;308:1499.
- 3 Bland JM, Altman DG. Some examples of regression towards the mean. *BMJ* 1994;309:780.

Authors' reply

EDITOR,—Susanne Doepfner and Irene Guggenmoos-Holzmann criticise our approach, which included reporting correlations between changes in cholesterol concentrations during the two diets in the subjects defined as consistent responders. They do not mention, however, that we also found a significant correlation between changes in cholesterol when data for the whole population were considered. Given that the study was conducted among free living subjects, we consider that the correlations provide reasonable evidence for an overall consistency in response.

Doepfner and Guggenmoos-Holzmann further believe that regression to the mean is likely to explain our observations that certain variables measured at baseline differ in hyperresponders and minimal responders and that these variables may partly predict response. They seem to have overlooked our finding that variable responders showed smaller changes in plasma cholesterol during both diets than did hyperresponders (table III in our paper) while mean cholesterol concentrations were similar in the two groups, suggesting a similar degree of regression to the mean. Furthermore, regression to the mean is likely to have been reduced by the fact that one measurement of cholesterol was made at the time of screening to select subjects for the study and a second measurement served as the baseline value for the study.¹ In addition, we measured plasma cholesterol at four and six weeks during each diet. Values were similar and were therefore averaged. Thus we believe that there is little to suggest that some of our main findings "could be due entirely to regression to the mean."

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- 1 Ederer F. Serum cholesterol: effects of diet and regression toward the mean. *J Chronic Dis* 1972;25:277-89.

Effect of detection of depression in general practice

Authors do not give enough information

EDITOR,—Christopher Dowrick and Iain Buchan¹ have shown what Hoepfer *et al* showed some years ago²: that telling a general practitioner that a patient has a high score on a screening questionnaire does the patient no good. It is difficult to see why it should. I find it strange that the authors do not think it necessary to show what the doctors did for the patients in the three groups (those with conspicuous depression, detected depression, and undetected depression), although in any other branch of medicine the course of a patient's illness might be expected to bear some relation to interventions. It is hardly surprising that patients whom the doctors intended to treat did less well than those whom the doctors did not intend to treat, since they might be expected to have more severe depressive illnesses.

We are not told how good these particular doctors were at detecting depressive illnesses: possibly most of the severe depressions were being treated. We are told (and it is not that reassuring) that 24% of the subjects did not respond to the initial questionnaire and that a further 32% did not respond to follow up, but we are not told whether the default rates were equal between the groups. Most worrying of all, the depressions included in the study were prevalent rather than incident depressions, yet a treatment effect would be expected to be maximal in the latter.

The issue that Dowrick and Buchan deal with is important, and it is therefore important that experiments are carefully designed and that careful note is made of how patients in whom a depressive illness was detected were treated. A study needs to contain enough patients receiving treatment for depression to show a treatment effect.

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- 1 Dowrick C, Buchan I. Twelve month outcome of depression in general practice: does detection or disclosure make a difference? *BMJ* 1995;311:1274-6. (11 November.)
- 2 Hoepfer E, Nye G, Kessler L, Burke J, Pierce W. The usefulness of screening for mental illness. *Lancet* 1984;i:33-5.

Patients may be receiving inadequate treatment

EDITOR,—Christopher Dowrick and Iain Buchan suggest that neither diagnosis nor disclosure of depression in general practice has an appreciable impact on outcome and that detection should be seen simply as a marker of severity.¹ It is well recognised that there is appreciable unidentified psychiatric morbidity in primary care,² and Dowrick and Buchan's paper reinforces this point. Two other findings that might have had an impact on outcome, however, are not highlighted. These are, firstly, adequacy of treatment and, secondly, non-attendance at follow up appointments.

It is a matter of some concern that the authors' data suggest that even when morbidity is conspicuous and an intention to treat exists, inadequate treatment is offered. This is particularly important if conspicuous morbidity has a worse prognosis in the long term.¹

Unsurprisingly, a proportion of the subjects in the study subsequently failed to attend. It is interesting that this proportion varied across the three groups studied: the proportion who completed all three questionnaires was 71% in those whose depression was undiagnosed and undisclosed, 63% in those with undiagnosed depression that was subsequently disclosed, and 56% in those with diagnosed depression. Thus the group who do worst are the group least likely to attend subsequently. Possibly this is another

reflection of severity of illness. A second possibility is that these drop out rates are themselves a function of inadequate treatment. It has been suggested that low attrition rates may be associated with effective care.³ Might the higher attrition rates and poorer outcome in this group be associated with ineffective care?

This valuable survey has potentially important implications for the treatment of depression in primary care, especially if inadequate and ineffective treatment is being offered to those who subsequently fail to attend. The paper offers useful information to those campaigning to defeat depression.

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- 2 Goldberg D, Huxley P. *Mental illness in the community. The pathway to psychiatric care*. London: Tavistock, 1980.
- 3 Hunt C, Andrews G. Drop-out rate as a performance indicator in psychotherapy. *Acta Psychiatr Scand* 1992;85:275-8.

Study had methodological limitations

EDITOR,—Christopher Dowrick and Iain Buchan report a trial to assess whether the outcome of depression is influenced by diagnosis by the general practitioner or by disclosure of the results of screening in cases of undetected depression.¹ They conclude that disclosure did not improve prognosis at six and 12 months and that treatment of depression in primary care has little influence on the "course of a condition whose major determinants may lie outside the reach of the medical profession." We believe that this is an unnecessarily gloomy conclusion and reinforces a nihilistic attitude towards the treatment of depression and other psychiatric disorders that is commonly seen in primary care.

The authors did not consider important methodological limitations. The sample was small, so the possibility of a type II error was high. The analysis was not based on intention to treat, whereby all randomised subjects are included in the analysis and if some are lost to follow up then missing values are replaced by the last available figure. The worse outcome among those patients for whom general practitioners declared an intention to treat was confounded by the severity of depressive symptoms found in the non-randomised part of the study. Follow up assessments took place at only six and 12 months; disclosure may have a maximal effect at six weeks.² The authors do not distinguish what they call intention to treat between the experimental and control groups. Such a comparison might provide information on whether general practitioners were more inclined to do something for those patients whose unrecognised condition was disclosed. Simple disclosure of a depression score may be unhelpful for many general practitioners. More detailed feedback, with a profile of symptoms and diagnostic information, seems to affect the outcome of depression at six weeks in primary care.²

The authors note that even when general practitioners prescribed antidepressants the treatment did not always accord with the guidelines in the *British National Formulary* even though the general practitioners professed knowledge about these. The provision of guidelines for treatment together with the disclosure of a clinical diagnosis might be a more effective way to influence the outcome of unrecognised depression. When compared with patients with depression identified by general practitioners, patients with unrecognised depression have less severe symptoms and impairments in social functioning and are less likely to have

been treated for depression in the past.³ Which treatment, if any, would be the most effective in such cases has not been established as most trials have been conducted in more severely depressed patients. Randomised trials are needed to establish which is the best treatment for mild depression in primary care. Maybe when general practitioners have clearer evidence of the benefits of treating mild depression, disclosure of the diagnosis will make a difference to outcome.

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- 1 Dowrick C, Buchan I. Twelve month outcome of depression in general practice: does detection or disclosure make a difference? *BMJ* 1995;311:1274-6. (11 November.)
- 2 Lewis G, Sharp D, Bartholomew J, Pelosi A. Computerised assessment of common mental disorders in primary care: effect on clinical outcome. *Fam Pract* (in press).
- 3 Coyne JC, Schwenk TL, Fechner Bates S. Nondetection of depression by primary care physicians reconsidered. *Gen Hosp Psychiatry* 1995;17:3-12.

Authors' reply

EDITOR,—The assertion that depression has a better prognosis if detected by general practitioners¹ is refuted in our sample. Our study was not designed to test a treatment effect, and it is therefore not relevant to criticise it, as David Goldberg does, for failing to show one. Goldberg iterates our conclusion that a prevalence sample may show apparently worse outcomes than an incidence sample, though why he is worried about this is unclear. There are methodological hazards in defining incidence samples in primary care settings, particularly if incidence is confused with first recognition by a general practitioner. We are surprised that Goldberg suggests that our study duplicates Hooper *et al*'s,² since their study did not assess outcomes. In fact, our initial methodological model was his own study, which, like ours, reported data on treatment patterns as outcome rather than process measures.³

We are interested in Alan Currie and Ann Ryman's speculations on the different response rates in the study groups (data that Goldberg seems not to have noticed). We would also value comments about the difference in response rate between the sexes.

We estimated that a minimum sample size of 31 per group was necessary to detect a difference of 7 in the median score on the Beck depression inventory with 85% power and a 5% probability of detecting a false effect on a background standard deviation of 9. We chose 7 as the minimum change in the score that could be clinically important; this was influenced by previous studies.³ We therefore do not accept Paulo Menezes and colleagues' criticism of our sample size. We accept that the decision to include only full responders could introduce selection bias. Inclusion of partial responders, however, introduces another source of variation that could blur the main comparison of the study. In practice it does not: reanalysis of our data to include partial responders, as Menezes and colleagues suggest, shows a minimal effect on the statistical results, with no changes in the inferences originally drawn. Menezes and colleagues' preference for follow up at six weeks does not take account of the delay between disclosure to and potential action by a general practitioner. In this study the effect of disclosure on diagnosis was greatest after six months.⁴

We agree that comprehensive interventions

may be more effective than simple disclosure in undetected cases and welcome the proposal for further research into the best treatment of mild depression in primary care. However, we consider ourselves to be realists (not nihilists) and would not wish to encourage undue expectations of general practitioners' abilities to influence the outcome of depression. The frequency and duration of untreated depressive episodes were similar to those of treated episodes in a recent study in the United States,⁵ whose authors argue that the differing characteristics of people who seek help and those who do not make it difficult to measure the value of treatment in mitigating the impact of depressive disorders.

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- 1 Lloyd K, Jenkins R. The economics of depression in primary care. *Br J Psychiatry* 1995;166(suppl 27):60-2.
- 2 Hooper EW, Nycz GR, Kessler LG, Burke JD, Pierce W. The usefulness of screening for mental illness. *Lancet* 1984;i:33-5.
- 3 Johnstone A, Goldberg DP. Psychiatric screening in general practice. *Lancet* 1976;ii:605-8.
- 4 Dowrick C. Does testing for depression influence diagnosis or management by general practitioners? *Fam Pract* 1995;12:461-5.
- 5 Coryell W, Endicott J, Winokur G, Akiskal H, Solomon D, Leon A, *et al*. Characteristics and significance of untreated major depressive disorder. *Am J Psychiatry* 1995;152:1124-9.

Increasing the shelf life of review papers

EDITOR,—Review articles are very important for summarising medical literature. By the time they are published, however, they are two years out of date because of publication delays, and they rapidly become irrelevant because very recent articles are not included. We proposed a cure to this in our paper titled "The death of biomedical journals," suggesting that a global information server should replace the traditional paper journal system.¹ With the global information server, research findings can be presented and updated in days rather than years, and this could revolutionise information exchange not only among scientists but also between scientists and the lay public. We recently presented an example of this in a review paper in the *American Journal of Epidemiology*.^{2,3} The review concerned the applications of capture-recapture methods in human diseases. Unlike traditional reviews, we presented a short summary in the article and posted the bibliography and descriptions of the reviewed literature on the Global Health Network home page (<http://www.pitt.edu/HOME/GHNet/GHNet.html>). These review tables are easy to access: a click on the home page will bring up information concerning the applications of capture-recapture methods in a specific area, and another click will pull up the reference for a certain study. As these tables will be continuously updated, the shelf life of the review can be extended almost indefinitely.

The posting and continued updating of the review literature on the Internet is a first step to transform the traditional journal systems into a dynamic research communication and update system. The combined use of paper journals and the world wide web can considerably decrease the amount of time needed to update reviews. It also challenges the philosophy that research communication through traditional paper journal systems is not outdated as soon as it is published but can remain current as needed.

It is time that scientists begin to take control of their research communication. Dynamic review is

a start. Journals accepting review articles should require authors to establish a place on the journal's home page, or their own home page, where the literature could be updated. The *BMJ* should take the lead by including updates of review articles on its home page. These sites could also provide feedback to the authors. With the worldwide web we can now dramatically extend the expiry dates of review articles. This approach will prevent the rapid spoilage of our biomedical literature.

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- 1 LaPorte RE, Marler E, Akazawa S, Sauer F, Gamboa C, Shenton C, *et al*. The death of biomedical journals. *BMJ* 1995;310:1387-90.
- 2 International Working Group for Disease Monitoring and Forecasting. Capture-recapture and multiple-record systems estimation. I. History and theoretical development. *Am J Epidemiol* 1995;142:1047-58.
- 3 International Working Group for Disease Monitoring and Forecasting. Capture-recapture and multiple-record systems estimation. II. Applications in human diseases. *Am J Epidemiol* 1995;142:1059-68.

Possible missed diagnosis of syllogomania

EDITOR,—Timothy M Williams and colleagues report a possible missed diagnosis of Tourette's syndrome in subject SN.¹ We wish to advance the possibility of a similar misdiagnosis in another of Beatrix Potter's subjects.

In an episode recounted in *The Tale of Two Bad Mice* the mouse Hunca Munca (HM), with her spouse, Tom Thumb, removed a number of items from a dolls' house in the absence of its owner.² HM is described as having "a frugal mind," though the objects she removed included not only useful items (a bolster and a cradle) but also unnamed "small odds and ends" and "several other things." Clark *et al* describe the condition of syllogomania, in which hoarding of rubbish is seen,³ though this usually occurs in elderly people (HM is of unknown age; though she seems to share her home with young mice, we do not know if these are her offspring). Syllogomania is sometimes part of Diogenes' syndrome (one of neglect of self and home, again usually occurring in elderly people), but HM's daily devotion to cleaning the dolls' house after their visit seems to rule this out.

Clark *et al* suggest that syllogomania may be a distortion of an instinctive desire to collect things and may provide a feeling of security. HM's vulnerability to insecurity cannot be in doubt, given the climate of fear and repression in which she lives: after their first visit to the dolls' house is discovered the owner's nurse vows to "set a mouse trap." Might HM not be a bad mouse but have syllogomania?

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- 1 Williams TX, Kim, Williams G. Excessive impertinence or a missed diagnosis? *BMJ* 1995;311:1700-1. (23-30 December.)
- 2 Potter B. *The tale of two bad mice*. London: Frederick Warne, 1904.
- 3 Clark ANG, Mankikar GD, Gray I. Diogenes' syndrome: a clinical study of gross neglect in old age. *Lancet* 1975;i:366-8.