Author's response to reviews

Title: Trends in the prevalence and management of diagnosed type 2 diabetes 1994-2001 in England and Wales

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Author's response to reviews: see over
Dear Editorial Team

We thank the referees for their thoughtful reviews and are pleased they all recognise the relevance of this paper for primary care. We have revised the paper in the light of the comments and believe that we have answered all the points raised. We trust that the paper can now be accepted for publication.

Given the long delay in reviewing this paper we would be grateful for an early response.

Yours sincerely

DGC
Reviewer: Denise Kendrick

1. Practice was included as an indicator variable in the model (73 levels). Given the importance of practice and the size of the database we believe this was the correct approach. The loss of power will have been small given the size of the database. Year was fitted as a linear trend as an a-priori way of testing for trends. Given that there are so few years we did not think it helpful to try to assess whether the changes were non-linear. However, we have reviewed the significant trends in HbA1c targets, and accept that while the linear trends are significant, the levels in 2001 are not significantly different from those in 1997 for all cut-offs.

Change to paper
We have clarified the text in the methods to make it clear that practice was included as an indicator variable. We have modified our interpretation of trends in HbA1c levels to focus on the lack of improvement in HbA1c levels rather than claiming that levels are rising.

2. We have expanded the text to explain the basis of the algorithm – see below. A date of first diagnosis prior to registration with a practice or prior to use of the electronic records would be entered routinely and is distinct from the date on which that information is entered onto the system. It is not possible to compare against the paper record as these are not available in an anonymised data base.

Change to paper. Insert:
“The decision was largely straightforward for diabetics newly diagnosed while they were registered with practices using their electronic database. Diabetics were classified as type 1 if a Read code “C24.” was present or if insulin was given within 90 days of first diagnosis. The problems arose for those diabetics diagnosed in the past who were using insulin at the point of registration with the practice or when the practice started using the electronic database. While dates of first diagnosis were usually available, as were date of registration and first recording of events within practice, we did not have information on when insulin treatment was started prior to electronic recording. In this instance it was necessary to base the decision on the age of the patient at first diagnosis (≥35 years implies type 2) as well as the time lapse between when the patient was first seen in the practice and when an insulin prescription was issued (≥180 days implies type 2).”

3. Use of indicative codes was rare (except for HbA1c prior to 1997). Summary information is already given in the results. Providing detailed data by outcome and year would be cumbersome and of limited value, especially given that improved recording has other components beyond the use of indicative codes. It was because we were concerned about the possible influence of improved recording that we carried out analyses restricted to those practices which had a high level of recording throughout. That our findings were the same in these practices suggests that improved recording is not the explanation. We already comment on this issue in the discussion.
4. We considered providing, and indeed produced, such a figure. However, while the overall message was similar to that from figure 4, the small number of subjects in some practices with HbA1c recorded especially in earlier years meant that the percentages beyond cut-offs was highly unstable. No changes have therefore been made to the paper.

5. We thank the referee for forcing us to examine this issue. We were aware that this would involve a complicated repeated measures analysis and for this reason had held off. An examination of this issue has forced us to revise our speculation about the reason for lack of improvement in control of diabetes.

Changes to paper include: altering conclusion of abstract and discussion plus adding pieces to both the methods and the results section as follows:

“In order to assess whether changes in HbA1c over years were influenced by the changes in BMI, we regressed the recorded HbA1c levels on age, sex, practice and year before further including BMI. Various models were fitted using the SAS procedure MIXED, allowing us to take account of individuals contributing data to variable numbers of years.”

“In order to assess whether the lack of improvement in glucose control over time might be due to the steady increase in the obesity of the diabetic population, we estimated the mean HbA1c levels adjusted for age, sex and practice and then further adjusted for BMI. The mean levels unadjusted for bmi reflected the same pattern as in figure 4; that is the mean HbA1c rose from 7.73% in 1997 to 7.82% in 2000 before falling back to 7.69 in 2001. While BMI was highly significantly related to HbA1c, adjustment for it had little effect on the estimated HbA1c means for the different years.”

Minor

We have addressed all these issues except shading of figures – we prefer colour to shading. We will liase with the journal over what is best.
Reviewer: Per Wandell

Major

1. This is a good point – though we think it unlikely that it will have had much impact in practice. We now comment on changes in diagnostic criteria in the discussion when discussing possible explanations for the change in prevalence of diabetes, as follows:

“The prevalence of diabetes has risen steadily in developed and developing countries throughout the second half of the 20th century [Gadsby 2002]. Rising levels of obesity in the general population are believed to be one of the principal drivers [Gadsby 2002], and in a recent Danish study it was concluded that the rise in diabetes between 1974/75 and 1996/7 was entirely attributable to the concurrent rise in body mass index [Drivsholm et al 2001]. It seems likely that this underlies the steady increase we have observed, given that the percentage of adult males in England with a BMI over 30 rose from 13.8% in 1994 to 21.0% in 2001 while for women the trend was from 17.3% to 23.5% [Sproston 2004]. While there is a theoretical possibility that changes in the definition of diabetes introduced from 1998 on may have had some effect, it seems likely to have been limited given the steady increase in the prevalence of type 2 diabetes predated the change in diagnostic criteria; in 1998 the definition of diabetes shifted from those with a 2 hour post load plasma glucose > 11 mmol/l to a fasting plasma glucose > 7mmol/l [Alberti 1998].”

2. We had missed this study and are grateful to the reviewer for pointing it out.

Changes to paper:
We now contrast our findings with this Swedish study in the discussion.

“It is tempting to conclude that the lack of improvement in HbA1c control is attributable to the increasing obesity of the diabetic population. However, our analyses of this suggest that the changes in BMI will have only had a small effect on the observed HbA1c levels. Further investigation of this issue is certainly warranted, but the statistical analysis is complicated due to the unbalanced data whereby individuals contribute varying numbers of observations, while data recording standards are changing over time, raising the possibility that missing observations are not random. In contrast to our finding, a recent Swedish study, based on a national diabetes register, reported improvements in HbA1c, as well as in blood pressure levels and increased use of statins, between 1996 and 1999; the Swedish study also reported rising BMI levels [Gudbjornsdotir 2003]. A Dutch study [de Grauw 2002] reported similar improvements in blood pressure and cholesterol levels between 1993 and 1999. However, that study also showed improvements in HbA1c. No data on obesity were presented.”

Minor

3. We now refer to the Danish and Swedish studies (see above).
4. New references have been added (see above).
5. We have clarified the text in several places but have resisted the temptation to markedly lengthen the paper.
We now include reference to de Grauw’s paper (see above) as well as Danish and Swedish studies.

1. False positive diagnoses are not really the issue. No diagnosis of diabetes should be made without repeat testing, and for those on active treatment the diagnosis is in little doubt. Any false positives will be amongst the group we have labelled “Diet only diabetics”. We have repeated our analyses excluding this group and none of our findings are altered.

2. We accept that achieving 100% success in meeting targets is unrealistic. Some patients will prove resistant to treatment while others will opt-out. It is not possible for us to identify such patients in our database. However, our analyses look at all patients in a practice, and therefore include all treated diabetics without excluding those with a malignancy or those who are poor at attending the diabetic clinic. We have added the following sentence to the discussion:

“When assessing targets it is important to recognise that complete success is unrealistic. Some patients who comply will not respond to treatment, others will opt-out of treatment, while for others with major co-morbidities the GP may choose not to treat [de Grauw 2002]. Further work to assess the realistic target achievable would be worthwhile”

3. We fully accept that co-morbidity and in particular cardiovascular morbidity is important when considering risks amongst diabetics and we have added a sentence to emphasise this in the introduction (see below). However, this paper is primarily concerned with trends in the prevalence and management of diabetics. Co-morbidities will not have changed over the short time period of this study and will not themselves influence these trends (though changing fashions may influence treatments given such as statins in secondary prevention of CHD). Since the guidelines for treating diabetics are not dependent on these co-morbidities and because it would substantially lengthen the paper we have not included data on these issues.

“In particular, it is important to manage cardiovascular risk in diabetics because diabetes accelerates vascular occlusion and much of the excess mortality is due to cardiovascular mortality[de Grauw 1995].”

Minor points

It seems unlikely that a cohort effect exists over such a short period of time. Nor is ageing an issue since the data are age-standardized. It seems much more plausible that this is a period effect related either to changing environment (e.g. obesity) or to changing care.

We believe our study does demonstrate a steady improvement in what GPs are doing (See 2nd and third sentences of abstract conclusions). We clearly state...
that it would be unreasonable to blame GPs for the increasing obesity of their practice populations.