Reviewer’s report

Title: Ethnic disparities in the control of type 2 diabetes risk factors in southwestern American veterans: the Diabetes Outcomes in Veterans Study

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Reviewer: Kevin Fiscella

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GENERAL

Previous studies have shown that control of CVD risk factors among type 2 diabetics differs by race and ethnicity. This study aims to replicate these findings among a sample of patients at VA medical centers in the SW who are on “stable” diabetic medication regimens that include long-acting insulin, in the process, the study determines whether observed differences are related to differences in patient factors. The study findings show no differences in either lipid control or blood pressure, but significant differences in A1C levels and doses of insulin by race. Neither of these differences is explained by patient level factors and readers are left to wonder what is different about diabetic control compared to blood pressure and lipid control.

The strengths of the study include detailed patient variables including patient socioeconomic barriers, transportation, knowledge, attitude, cognitive dysfunction, depression and social support. Also use of the VA minimizes access barriers and SES differences between patients. The primary limitations include failure to control for visit frequency, possible provider effects, small sample size, lack of data on non-participants and unknown generalizability.

Compulsory Revisions

1) Page, 4, paragraph 1. It is not clear that supposed physiological differences by race and ethnicity among diabetics are particularly relevant to this paper. Regardless of rates of complications by race or ethnicity, disparities in diabetic control are important. Many readers may incorrectly infer that such purported differences represent genetic differences although this has yet to be definitively shown. Furthermore, a recent meta-analysis by Lanting et al (Diabetes Care, Sept 2005) concluded that most racial and ethnic differences are explained by confounders. In the only two instances where racial and ethnic differences were noted, ESKD and retinopathy, the associations with race and ethnicity differed between the UK and US. Rather than take on this controversy, I would recommend focusing more clearly on what is and what is not known regarding racial and ethnic disparities in risk factor control among diabetics and specifically how this study addresses that gap in knowledge.

2) Page 5, paragraph 2. Although the data are derived from the prospective DOVES study, the analysis is cross-sectional thus limiting ability to reliably assess changes in regimens over time. The paper attempts to address this limitation by restricting enrollment to patients on stable regimens for 2 months and controlling for duration of insulin treatment. However, many diabetics, even those without suboptimal control of risk factors, are seen less frequently than every two months. This creates potential bias between groups in terms of relative stability. In other words, it is possible that minority patients were in the process of having their regimen changed more often than majority patients, but these differences were missed by misclassification of subjects as truly on stable regimens. The finding of no difference in duration of medical or insulin treatment by race or ethnicity tends to mitigate against this possibility, but does not exclude it. More importantly, recent data (see December 2005 of the American Journal of Public Health) show that African American and Hispanic
VA patients are seen significantly less often than non-Hispanic white patients. This creates a potential bias in misclassification for study eligibility as suggested above, but also represents a plausible explanation for the findings. Since most dosage changes occur at visits, fewer visits over 8 years could result in differences in insulin dosage of the order of magnitude that were observed. This major limitation should be acknowledged in the discussion.

3) Page 5, paragraph 2. Why was a 2-month period used to define “stable”? Was this definition empirically based? A rationale should be provided.

4) Page 5, paragraph 2. If the authors have access to number of visits either from subject self-report or electronic health records, they should assess whether differences in number of visits for diabetes explains their findings.

5) Page 6, paragraph 3. The study fails to adequately control for provider effects. Failure to increase insulin in response to suboptimal glucose control represents what is termed “clinical inertia” (see papers by Ziemer DC et al Diabetes Educator, 2005 and Shah BR et al Diabetes Care, 2005). It is possible that a substantial number of minority patients were seen by a provider with greater clinical inertia. Ideally, this would be assessed using random effects models that include facility and physician level variables, but the sample is probably too small. This should be acknowledged as a limitation.

6) Page 7, paragraph 2. “Baseline A1C” needs to be clearly defined? Does it refer to the most recent A1C level available at the time of study enrollment regardless of how long ago it was done or did all subjects undergo A1C levels at enrollment for the purposes of the study? It sounds like the later but this needs to be clearer.

7) Page 11, paragraph 3. As noted study limitations need to be more clearly acknowledged. This also applies to the absence of findings of differences in BP control and lipids.

8) Page 13, paragraph 2. The African American subsample size is small and findings are of questionable generalizability. Specifically, the authors should be more cautious in drawing firm conclusions based on a selected sample of 35 African Americans from SW VAs. Without knowledge of the characteristics of the larger population of diabetics within these medical centers or participation bias by race or ethnicity, it is difficult to generalize these findings to all diabetics within these medical centers much less to diabetics within the VA nationally. More caution is needed in the interpretation of their findings in both the abstract and discussion sections.

Minor Compulsory

1) When during the prospective DOVES was this cross-sectional analysis done? In other words, were data all data derived from baseline assessments?

2) How were alcoholism and substance abuse assessed? What morbidities that might affect glucose homeostasis were considered?

Discretionary

1) Abstract-background. Why not mention previous work documenting disparities in diabetic control and associated CVD risk factors?

2) Abstract-methods. It is helpful to distinguish primary from secondary outcomes variables rather than lumping them all together.
3) Page 4, paragraph 2. It is not clear why disparities in SES make the SW an appropriate region. Ideally, variation by SES would be modest (as it probably is in the VA) in order to more clearly isolate race and ethnicity effects.

4) Page 7, paragraph 2. Use of a subject flow diagram might help readers to better understand how the sample was constructed.

5) Was any consideration given to over-sampling minorities to increase their numbers in the total sample? Although race and ethnicity are not available through pharmacy records, minorities could be oversampled through a combination of use of Hispanic surnames and sampling from neighborhoods (based on geocoded addresses) that include large proportions of African Americans and Hispanics. Use of this approach would also provide some indication of differential study enrollment by race and ethnicity.

6) Do the authors have any data from other sources, such as VA EHR data, on all medical center patients with a diagnosis of type II? It would be helpful to compare characteristics of the entire diabetic population in these medical centers with the study sample as a means of assessing generalizability.

7) Why was metformin assessed separately from other OHAs?

8) Page 11, paragraph 2. Some of the barriers proposed are not well supported by the data. There were no differences in number of injections per day by race or ethnicity. There is not evidence (to my knowledge) that glucose monitoring is associated with improved diabetic control. It is not clear how cultural barriers related to images of wellness would affect insulin dose.

9) Page 12, paragraph 1. The authors’ cite 14 year old data suggesting that African American diabetics report less frequent glucose monitoring, but there is no evidence (to my knowledge) that glucose monitoring is associated with improved glucose control. While patient-provider interaction is clearly a possibility, it is notable that none of the patient level variables that might be mitigated by patient-provider race or ethnicity concordance such as language preference or attitudes were predictive of insulin dose in the multivariate model. This might be acknowledged. I agree that competing demands can affect management of diabetes, but it is not clear from these data why minorities might have greater competing demands.

10) It also might be worthwhile to note in the paper the need for future research to examine physician stereotypes. For example, it possible that many physicians stereotype minority patients as at greater risk for hypoglycemia (in the absence of any evidence). This area warranting study based on other work on physician stereotyping.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No
Declaration of competing interests:

I declare that I have no competing interests.