Reviewer’s report

Title: Quality of life and metabolic status in mildly depressed patients with type 2 diabetes treated with paroxetine: A double-blind randomised placebo controlled 6-month trial

Version: 3 Date: 18 October 2006

Reviewer: Miranda van Tilburg

Reviewer’s report:

General

This generally well designed study aims to test the effects of paroxetine on quality of life and metabolic control in a primary care sample of non depressed type 2 diabetic patients. Previous studies have shown that SSRI’s can improve glycemic control in depressed tertiary care patients. This study extends the literature by focusing on a primary care sample that is non-depressed.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

The relationship between metabolic control and depression in diabetic patients has been well established. Treating the depression (either pharmacologically or psychotherapeutically) improves glycemic control. These observations emphasize the importance of screening and treating depression in diabetes. Even though there is data to suggest that non clinical depressive symptoms may influence glycemic control, the results are mixed and not as strong as for clinical depression. In addition, no studies have shown a benefit of SSRI’s for treating subclinical depression in diabetes. The current study treated mainly non depressed patients (patients with meeting 1-4 criteria for major depression in the DSM IV are considered not clinically depressed; it seems from the text that anyone with 1-5 symptoms were included; and only 10 were actually diagnosed as being depressed). It is not clear why non suicidal moderately to severely depressed patients were excluded. If ethical issues about non treatment were leading this decision, clearly reducing the trial time would have been a better solution. Six months is a long time; clinical benefits are expected sooner. In addition, authors would have to weigh the ethical issues of treating non-depressed patients with and SSRI. Since most of the sample was non-depressed this creates a bottom effect where paroxetine cannot improve depressive symptomatology (as was proven in figure 2); therefore the risks (side effects, stigma) clearly outweigh the expected benefits.

Treatment effects were assessed with Mann Whitney U-test. This test is helpful but doesn’t compare the placebo and paroxetine groups directly. I would suggest adding ANCOVA analyses to compare the two groups at follow-up while controlling for baseline HADS, QoL and Ghb levels. In addition, it would be helpful if responder/non-responder analysis and Intention to Treat analysis are added.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

(1) Who screened patients for depression and was the clinical interview guided at all (e.g., SCID)?
(2) The number of patients in the text and in the figure does not seem to be equal (e.g., according to text 19 patients started on paroxetine and in figure these are 21). It would also be helpful to indicate the number of patients in both groups that were left in the final analysis of the data.
(3) The subanalysis which included only the depressed patients should be in the result section rather than the discussion so more details can be given. Also, it should be noted that N’s were too small (only 5 patients in the paroxetine and 5 in the placebo group) for any test of significance to be meaningful and valid. A simple graph showing trends would be more insightful.
(4) In Table 2&3: Please explain what the abbreviation LOCF stands for?
(5) Data was collected at 1-3-6 month of treatment. It is not clear which data is used for analyses. Please...
indicate in text and in tables clearly which time period is used.

Discretionary Revisions (which the author can choose to ignore)

It may be helpful to examine how Ghb and QoL change over the three time periods. Can we expect benefits of paroxetine at 1 month? Does the benefit increase or decrease over time?

In the introduction authors are not clear on how SSRIs are influencing metabolic control. They seem to both suggest a direct effect of SSRI on glycemic control as well as an indirect effect through depression.

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

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:
I declare I have no competing interests