Reviewer's report

Title: Differential glatiramer acetate treatment persistence in treatment-naive patients compared to switchers from interferon.

Version: 2
Date: 22 February 2015
Reviewer: Tomas Kalincik

Reviewer's report:

The authors have addressed the majority of my concerns. I appreciate the clarification of several points and the additional important information provided. I have only a few outstanding comments:

Minor essential revisions:

1) l.97-99: I have some reservations as to whether any exposure to interferons prior to glatiramer acetate, irrespective of its recency, can be termed “treatment switch”. There are multiple possible scenarios (e.g. recommencing therapy after pregnancy, a “treatment holiday” of several years in those with inactive disease or lack of compliance etc.) which would fulfill the relatively broad definition of treatment switch used here. The authors should either restrict the analysis to the true switchers (defined by a maximum “intertreatment gap”) or adjust the terminology to reflect the relatively broad rules for previous exposure to interferons.

2) As part of the limitations subsection of the Discussion (p 12), the authors should state that the analysis was not adjusted for disability, which is known to be associated with the probability of treatment discontinuation, and therefore the results could be biased if the levels of disability differed between the cohorts (which is not possible to assess objectively, as this information was only available for 55% of the studied cohort).

3) l.163: The break-down of the post-switch therapy per group should be given either in absolute numbers or as a proportion of the patients who stopped GA within each subgroup. In fact, it seems that while 100% of the “switchers” who stopped GA escalated therapy, the treatment-naïve patients stopping therapy continued following a variety of pathways. As it stands, this information is somewhat hidden.

4) In the Methods, the authors state that they used both logrank test as well as the proportional hazards model to compare discontinuation rates between the cohorts. While this may seem as a duplicity (where the adjusted Cox model is the superior model), in fact, the authors (correctly) only report the results of the Cox model to compare the two groups.

5) l.121: I assume that the “increased T2 lesion load” refers to both new and enlarging T2 lesions.

6) The manuscript still contains errors and would benefit from a language
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I have received conference travel support and consultancy/speaker honoraria from Novartis, Biogen Idec, Sanofi Aventis, Genzyme, Teva, BioCSL and Merck Serono.