A request for clarifications and additional data

While I congratulate Eady and colleagues on their attempt to explore the important issue of treatment outcomes for individuals with Down syndrome and dementia and the considerable effort that has gone into collating this data, I am concerned about the way some of the data are presented and are used to support the conclusions drawn in this article. I would like to request some clarifications and additional data.

Three of these relate to the increased survival for those on drug treatment. First, the abstract states a difference in mean survival of 5.59 versus 3.45 years for treated versus untreated groups but as far as I can see these figures are not adjusted for the fact that the ‘no treatment’ group are older at the time of diagnosis (means 56.66 versus 53.81 years, similar standard deviations) and have significantly higher Dementia Questionnaire for People with Learning Disabilities (DLD) scores indicating that this group had more severe symptoms of dementia at diagnosis (p. 156). It would be informative to know the means and standard deviations for actual age at death of both groups. Second, the Kaplan–Meier survival curves (Fig. 1, p. 157) do not seem to take into account the age differences between the groups at diagnosis and in my view are therefore misleading. Third, the Cox regression calculations of hazard ratios reported, suggesting that treatment extends survival, do not include any control for the individual variations in the extent of the progression of the disease in the analyses. The paper states that the authors have data on DLD scores and clinician’s stage assessments (early, middle, late, p. 156) at diagnosis and these differ between the drug treatment/no treatment groups. While these measures are estimates of disease progression at best, why was one of them not used as well as age at diagnosis as a covariate? Without any control for differences in disease progression I do not think the strong claim of a survival benefit for treatment can be substantiated.

Regarding the short-term benefits of treatment, there are no benefits evident on DLD social scores and the benefits (slowing of decline) on DLD cognitive scores at 6 months are lost at 12 months. In my view, this should have been made explicit in the abstract and discussed more fully in the paper. In addition, I am aware that this pattern of ‘benefit’ is similar in patients with Alzheimer’s disease in the general population but for individuals with an intellectual disability a slowing of cognitive decline followed by a more rapid decline as indicated by these data may be more difficult for them to cope with. It would be informative to see the actual means and standard deviations for the DLD measures at the 6-month and 12-month time points. I also understand a more rapid decline is experienced when these drugs are stopped.

Finally, authors, reviewers and publishers need to recognise that many people searching for information will not read beyond the abstract and take care to ensure it is a fully accurate summary when publishing findings and their implications.