Reviewer's report

Title: Low birthweight and preterm birth in young people with special educational needs: a magnetic resonance imaging analysis

Version: 1 Date: 24 October 2007

Reviewer: david gadian

Reviewer's report:

General

This paper reports on the prevalence of a history of low birthweight or preterm birth, and the neuroanatomical correlates of such a history, within the special educational needs population. This is timely and potentially important research, given the increasing awareness of the cognitive sequelae of prematurity and the increasing ability of modern neuroimaging techniques to identify the structural basis of functional impairments. One of the novel features of this paper is that it addresses the problem by selecting the special educational needs population, rather than preterm children per se, as the group of interest. In broad terms, I think this is an interesting study that is worthy of publication, but I think that a number of issues should first be addressed.

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

1. It would be useful to know the prevalence of the special needs population within the educational system. I think it would also be useful to know whether there are further categories of educational needs within the UK, for example for children who are more severely impaired, so we can better understand if the group of children who are investigated here are intermediate between those who don’t require any additional support and those who may require more extensive support.

2. On page 7, it is mentioned that IQ of all participants was assessed. However, there is no mention of IQ data in the Results section. I think it would be useful to present these data. I think it might also be helpful to provide some indication of the medical histories of the full group whose data are presented here.

3. My main question relates to analysis of the data and in particular to the (somewhat arbitrary) classification of zero that is given to all birthweights or gestational ages above a selected threshold. I can understand why this might be done when carrying out the analyses in SPM, but it does mean that a very large proportion of the data points are lumped together, as is apparent for the further analyses presented in Figures 3 and 4. It seems to me that a better way of looking at the data would be to carry out the initial SPM analyses as the authors...
have done, and then in a second-stage analysis of the regions identified on SPM they could simply plot grey matter density as a function of gestational age or birthweight. They could then carry out rather more sophisticated fits to the data which overcome the problems of having so many ‘zero’ points and of arbitrarily selecting thresholds of normality; indeed these fits could help to identify where, if at all, ‘normality’ of birthweight and gestational age kicks in, at least in terms of grey matter density within this population of children. I think that such an analysis would greatly strengthen this paper and add substantially to its novelty.

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. On page 10, it is stated that a correction for multiple comparisons was used. Was this a familywise error correction? This should be stated.

Discretionary Revisions (which the author can choose to ignore)

Which journal?: Appropriate or potentially appropriate for BMC Medicine: an article of importance in its field

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

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I have no competing interests