

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Fetal Alcohol Spectrum Disorders from Childhood to Adulthood: A Swedish Population Based Naturalistic Cohort Study of Adoptees from Eastern Europe
AUTHORS	Landgren, Valdemar; Svensson, Leif; Gyllencreutz, Emelie; Aring, Eva; Grönlund, Marita; Landgren, Magnus

VERSION 1 – REVIEW

REVIEWER	Philip A. May University of North Carolina at Chapel Hill, North Carolina, USA
REVIEW RETURNED	08-Jul-2019

GENERAL COMMENTS	<p>This is an important paper that accomplishes the objectives described by the authors: a follow up study of adoptees from Eastern Block countries in Sweden. The descriptive data are detailed and also the vignettes are well done. Both will be of use to clinicians and researchers alike.</p> <p>From my point of view, the only things that need to be addressed are some minor typos (typographical errors or grammar) on the following pages: abstract line 13 (outcomes), p.6 lines 43-45, (reword: it has since been)p.9 - line 21 (15 females), p.11, line 10 (not symptoms, but traits), and p. 16 - line 25 (i.e.).</p> <p>I also think that the lack of OFC growth or stunting that persists into adulthood is a point that may warrant more emphasis in the paper, and I would encourage the authors to add more on this if they would like to do so.</p>
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REVIEWER	Deepa Singal University of British Columbia Canada
REVIEW RETURNED	31-Jul-2019

GENERAL COMMENTS	<p>Reviewer comments</p> <p>This is a well-done study that provides important clinical information on a complex and under studied disorder. The small sample size limits generalizability, however this manuscript lays the foundation for important future work and the importance of appropriate diagnostic, treatment and support resources for a vulnerable population throughout the lifespan. This information is critical for decision makers and health, social and educational providers who are charged with allocating resources for this</p>
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Comments on writing style:

This manuscript should be carefully edited to ensure ideas and pertinent information are concisely presented. For example, in the abstract when describing participants, the statement can be shortened to “A population based cohort consisting of thirty seven adopted individuals diagnosed with FASD in childhood”. Follow up time/years should be stated clearly in either the outcome measures or results section (which it is, therefore duplicate information can be removed). Unnecessary descriptive words can be removed for example “carefully examined adoptees” – “carefully examined” may be removed.

Major comments:

There is a knowledge gap in the peer reviewed literature regarding the adult trajectory of FASD, and clinical presentation of symptoms in adulthood, this study begins to fill an important gap. However, the framing of this manuscript may be problematic and create stigma without intention.

First, there are potential problems with the language the authors use to describe their objective. The objective is stated in the abstract (lines 12-14) as “To characterize the adult outcome of FASD in children adopted to a socially favorable and stable rearing environment” – These individuals have a diagnosis of FASD as children, and FASD is a lifelong disorder. The concept of FASD as an “outcome” in adulthood is not typically how this disorder is described - children diagnosed with FASD are adults with FASD – in keeping with the language commonly used to describe FASD and other lifelong diagnoses, the authors may choose to re-phrase their objective as follows “ To describe clinical characteristics of young adults with FASD...” The background sentence in the abstract (line 8-9) can also be changed to “ To further understand FASD in adulthood is a major public health interest....”

Furthermore, in the objective and throughout the paper, authors make the claim that an adoptive environment is a more “optimal environment” versus remaining with the birth mother and/or being placed in foster care or out of home placements. While there is evidence demonstrating that finding a stable, caring and nurturing adoptive family is better than children being placed in temporary foster homes and other type of placements, recent research is demonstrating that adoptive children still have lower academic performance and higher diagnosed disabilities. Recent reports

have also raised questions about coercive and dishonest treatments of birth families in international adoptions, and disruptions in the adoption processes. The authors do not present evidence in their study that the adoptive families the children in this study are indeed more “optimal”, as there is little information presented about either the birth parents or the adoptive families. The claim that the adoptive family environment is a more optimal environment than the birth families without presenting evidence to support this statement may negate and undermine the global work being done to de-stigmatize mothers with alcohol and substance use. Various efforts are being undertaken in North America, the UK and in Europe to de-stigmatize women with alcohol and substance use issues to facilitate treatment seeking, the reporting of alcohol use, and ensuring these women and their children receive much needed support services. The claim by authors that these children are in a more optimal environment because their birth mothers were using alcohol is an extrapolation and may cause further stigmatization to women who have substance use issues.

Moreover, as child welfare systems are being overhauled around the world to better support both mothers and children, emphasis is being placed on a “family welfare” approach versus a “child safety” approach. This means that if welfare agencies identify children at risk, instead of being removed from the home (child-safety approach), the whole family, and the child, is given intensive home supports to try to remove the risks and treat and support the mother and parents, while the child stays with the family. Australia, Sweden and several European countries have taken this approach and have made progress in reducing child poverty and violence, which are major risk factors of taking children out of their birth families. In this paper, the claim that adoptive families are more optimal without the data to back up this claim may be more harmful towards efforts being made to one, de-stigmatise birth mothers and women with alcohol use issues, and two, efforts to support the whole family, and mother-child dyad in staying together. As such, the authors may consider re-phrasing, or removing of statements referring to an adoptive home as more optimal.

Furthermore, there is no comparison group of children included in this study from a “sub-optimal environment” to draw comparisons or conclusions. Therefore, the necessity of framing the paper using this lens does not add much to the interpretation of results. The authors may choose to simply acknowledge that this study is a descriptive study of clinical characteristics and presentations of individuals with FASD in young adulthood who were adopted as children”.

The authors may choose to discuss or unpack the influence of the adoptive environment in the discussion section and suggest future research using a comparison group or children who grew up in different environments to further explore this topic.

Specific comments:

Abstract:

1. Line 8-9 change to “To further understand adults with FASD is a major public health interest”
2. Objective – authors may change to “To describe clinical characteristics of adopted children with FADS in the adulthood”
3. The type of assessments (screen, survey, self-report etc.) and how these assessments were conducted, by whom should be provided to the reader – how was the data obtained and who collected it.

Strengths and limitations:

1. Authors should provide a concise, once sentence description of how the strength or limitation should affect interpretation of study results, for example, “Small sample size, limiting the generalizability of results to a XXX segment of the population”.

Introduction:

1. Language stating the adoptive environment is “optimal” should be revised – refer to comments above.

Methods

Design, Setting and Participants:

1. Line 34 – 2,8? Should it read 2.8?
2. Data regarding the clinical diagnosis and who made the diagnosis of FASD in childhood in these children are unclear, further information about the clinicians/process of diagnosis should be added.
3. The authors say that there was insufficient data about many parents, exposure and birth status, and it is unclear if all children diagnosed with FASD in this cohort indeed had confirmation of maternal alcohol consumption during their prenatal period – this information should be added. If all children do not have a confirmation of prenatal alcohol exposure – this should be discussed in the limitation section, as these children could be mis-diagnosed with other neurodevelopmental disorders such as Autism, ADHD etc. and could affect the generalizability of results.

Assessment at adult follow-up

1. Mean follow up age of participants should be provided.
2. Line 25 – readers should not have to look to previously published study to obtain critical information – more details about the procedure used should be described in this manuscript.
3. A brief statement about the use, validity and reliability of scales used could be added to enhance the quality of the study.

Discussion:

1. References to an “optimal environment” should be removed, as authors cannot prove that being adopted is a more “optimal environment” – see comments above.
2. FASD is a lifelong disorder – the way that the authors have written their summary statement implies that children with FASD should “outgrow” their disorder – “the proportion *still meeting* diagnostic criteria” – it is well documented in the literature that the primary characteristics of FASD do remain with individuals throughout their lifespan. Authors may choose to remove the word “still” and provide more context about FASD being a life long disorder, where most symptoms and clinical presentations persist.
3. The discussion/and or conclusion section would benefit from discussion and acknowledgement about the lack of supports and resources available for adults with FASD, as well as the challenges faced by individuals with FASD as they transition to adulthood. In most countries that provide supports to children with FASD in the social, education, justice and health sector, these supports become more scarce and decrease in availability after the age of 18. Results of this study point to the challenges faced by these individuals in adulthood and the need for support throughout the life span. While authors touch on this briefly, the message of this important paper would be

	<p>strengthened with a more nuanced discussion about the need for support for individuals with FASD in adulthood.</p> <p>4. The discussion section could benefit from a more fulsome discussion about the limitations and the limitations effect on the interpretation of study results, such as small sample size, generalizability, lack of information about the birth mothers alcohol consumption (if this is the case), lack of information if the adoptive environment is “more optimal” versus staying with the birth mother – i.e. no comparison group.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1 comments: *I also think that the lack of OFC growth or stunting that persists into adulthood is a point that may warrant more emphasis in the paper, and I would encourage the authors to add more on this if they would like to do so.*

A: We have emphasized this in the discussion, now reading:

“However, the patterns of stunted growth figures of -2 SD for height and head circumference returned in adulthood, and implies an altered trajectory of growth in FAS. Stunted growth patterns in children when encountered clinically should not be evaluated only medically in a strict sense, but also using thorough multidisciplinary neurodevelopmental examinations longitudinally.[41] A fragmented health care where so termed psychiatric, social or medical signs are assessed as isolated phenomena, otherwise struggles to detect intricate biopsychosocial patterns as exemplified by FASD”

Reviewer 2 comments (R2) comments on writing style, Author (A):

A: We are thankful for the many editing suggestions from Reviewer 2, and have kept most of them. We here discuss the major comments.

1. R2: ...describing participants... can be shortened...follow up time/years stated clearly...Unnecessary descriptive words ...removed for example carefully examined...

A: We have shortened according to suggestion but maintained that it concerns “adopted” individuals which is important for the further discussion concerning an etiological diagnosis such as FAS and symptom-based diagnoses more or less related in these cases to suboptimal pregnancy; for instance autism, attention deficit, epilepsy, DCD and Intellectual Disability.

“A population-based cohort of thirty-seven adoptees diagnosed with FASD in childhood.”

2. R2...Follow up time/years should be stated clearly in either the outcome measures or result section.

A: It is stated in the result section but we removed the information that each individual was seen up to three times “...up to three follow up...” we also added information about the involved professionals since the question is raised by R2 under the heading Specific comments, Design, Setting and Participants ...”who made the diagnosis...” and question about the “...process of diagnosis” that it is a clinical work-up on both occasions, i.e. child- and adulthood.

We removed the words “carefully examined adoptees” according to R2’s suggestion as an “unnecessary descriptive word” and replaced with a more detailed explanation of what was meant

Outcome measures now reads:

“Assessment consisted of clinical evaluations of social, medical, psychiatric, neuropsychological, adaptive, and ophthalmological status by a physician, ophthalmologist, orthoptist, and psychologist on up to three occasions.”

3. *R2 Major comments: ... “the framing of this manuscript may be problematic and create stigma...the authors may choose to rephrase their objective as follows...”*

A: We have followed the suggestion in order to avoid stigmatization of birth mothers and adoptive parents as well and emphasized that these children were adopted from orphanages not directly from birth mothers and as fairly old children. They were regarded as developmentally impaired by adoption authorities in the countries of origin and were in custody because of maternal alcoholism.

“To describe the clinical characteristics of young adults with FASD adopted from orphanages to a socially more favorable and stable rearing environment as children.”

4. *R2: The background sentence in the abstract line 8-9 can also be changed.*

A: We changed that and removed “the adult outcome of” and replaced it with “in adulthood”. “Fetal Alcohol Spectrum Disorders (FASD) are a global health concern. To further understand FASD in adulthood is a major public health interest.”

5. *R2: ...throughout the paper, authors make the claim that an adoptive environment is a more “optimal environment” versus remaining with the birth mother...*

A: We think that the stigmatization issue is less present if we clearly point out that the adoption is not directly from birth mothers but from orphanages. And we can also change words from “optimal” to “more favorable” although from a general point of view, at least in Sweden at the time when these adoptions were carried through, adoptive parents were closely “investigated” by social workers before considered qualified to adopt a child, which obviously is not the case for parents in general. Using the word “optimal” here was more to elucidate the effort to separate prenatal and constitutional etiological factors from postnatal environmental factors. Adoption studies in this sense are historically an established method in research but of course not as powerful as homozygotic twin studies.

6. *“Recent reports have also raised questions about coercive and dishonest treatment of birth families in international adoptions...”*

A: We have encountered in media these adoption cases and they are simply put horrific. But to our knowledge, these are rare public cases in Sweden and has been about adoptees from south America and Asia. In clinical practice we have met several children with FASD from these continents, as well as South Africa, but all we met from former eastern European countries in the 90’s with FAS were *from orphanages and had almost all an explicit prenatal alcohol exposure*. Another kind of “stigmatization” at work within the adoption community in Sweden has rather been that the adoption parents feel shame and guilt because the adopted children are very often difficult to raise, which has been implicitly attributed to insufficient parental skills. The adoptive parents have been very much relieved by understanding their child to have prenatally determined functional impairments causing their upbringing to be difficult, not the other way around. That insight makes life and support to the child/adult much easier in many ways. The adoption agencies on the other hand, often these days feel accused and sad because of the few (?) very unfortunate cases of “coercive and dishonest” adoptions presented in the media, which questions the agencies legitimacy. This is of course not an apology for “coercive and dishonest” adoption procedures.

VERSION 2 – REVIEW

REVIEWER	Deepa Singal University of British Columbia, Canada
REVIEW RETURNED	08-Oct-2019
GENERAL COMMENTS	Thank you to the authors for addressing reviewer concerns.