Conflicts of interest
There are no conflicts of interest.

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REFERENCES
1. Vidyadharan V, Harish MT. Comments on “specific learning disabilities: Issues that remain unanswered”. Indian J Psychol Med 2018;40:590-91.
2. Ministry of Social Justice and Empowerment. Notification, 2018, Gazette of India (Extra-Ordinary); 2018 Jan 4. Department of Empowerment of Persons with Disabilities (Divyangjan).

How to cite this article: Kohli A, Sharma S, Padhy SK. Authors’ response to the comments on “Specific learning disabilities: Issues that remain unanswered”. Indian J Psychol Med 2018;40:597-9.

Comments on “Prenatal Depression and Infant Health: The Importance of Inadequately Measured, Unmeasured and Unknown Confounds”

Sir,
Andrade[1] commented on a recent article by Coburn et al.[2] where he mentioned that the authors[2] have not accounted for confounding variables in their research and have considered only the role of socio-economic status as a significant factor during pregnancy that leads to infant health outcomes. He has suggested some inadequately measured, unmeasured and unknown confounds that could be playing a possible role in relationships between antenatal depressive symptoms in mothers and child health outcomes. He also stated that the authors[2] have implicated cause-effect relationships when discussing possible interventions that may be beneficial for women during pregnancy to reduce the risks of poor infant health outcomes. However, authors[2] have clearly addressed most of the concerns raised by Andrade.[1] I am critically examining the comments made by Andrade[1] against the referred study.[2]

1. The referred study[2] was undertaken in low economic status population among American-Mexican women. The emphasis of the authors is to highlight the importance of providing care to such women who may be underprivileged and may not have access to the identification of depressive symptoms. Authors have cited the literature[3,4] indicating the importance of studying depressive symptoms among pregnant women in poor income families. Therefore, the choice of this sample characteristic is justified, hence raising doubts about the sample’s inadequacy can be avoided in light of the aims and scope of the original study.[2] On the other hand, the commentator has not provided any references in support of his suggestions that poor living conditions, poor access to nutritious food and poor access to quality medical care can be responsible for poor maternal and infant health. What are the chances that these significant factors would overcome the inadequacy of the Coburn et al.’s study? Moreover, authors have taken other demographic variables too like mother’s age and her educational level, presence or absence of a romantic partner, other children at home and being born in Mexico. Aren’t these variables justified in light of the present study? The authors have conceptualised their study by taking account of a few factors which they may have considered appropriate. Nevertheless, many other important...
variables could be considered which adds to the limitations of this study.

2. Maternal physical illness; maternal use of medications (including antidepressants); maternal use of alcohol, cigarettes and illicit drugs; and other variables have been suggested by the author as unmeasured confounds. The original article does mention the need for including such variables in their 'Limitations and Future Directions' section. Moreover, the participant mothers were not clinically diagnosed depressed patients (or the relevant information was not provided). The depressive symptoms were measured irrespective of their clinical status. Therefore, the inclusion of antidepressant use factor may not be considered necessary. However, looking at the aims of the Coburn et al.'s study, it may be too limiting to judge the validity of this research based on the exclusion of such variables. Any research cannot account for several variables at once. Therefore, these limitations can be overcome in later research. The importance of this research cannot be undermined by the exclusion of such factors. The commentator has again not provided any references to support for the importance of suggested variables. How would the reader judge the relevance of these suggested factors?

3. The commentator's discussion on unknown confounds, mainly stressing the role of genetic factors, seems beyond the scope of the original paper – a longitudinal study of this nature. First of all, the author has again not provided any reference to this suggestion. Second, how many unknown confounds can be held responsible? There is no fixed or approximate number for the existence of such unknown confounds. Discussing the role of such confounds is theoretically appropriate, but attributions made for them by Andrade[1] in the context of present research seem unimportant and hence, can be taken for discussion in the aims of some other research study.

4. The objectives of the study do not point to any cause–effect relationship as mentioned by the commentator. However, the authors gave possible suggestions that can be implemented to improve maternal mental health. They emphasised the need to incorporate the screening and prevention of depression among pregnant women, as is commonly practised with postpartum depression. This recommendation lines with the need and importance of the study, rather than being an overly judged causal association. Any clinical research may have its implications to modify the current practices of intervention on the target population. This does not indicate that the causal relationships are being inferred from the observational design of the study. This study is longitudinal in nature and has suggested potential factors that need to be taken care of while providing health care to pregnant women.

FINAL COMMENTS

The author has explicitly criticised the study by Coburn et al. due to its lack of adequacy in accounting for various possible confounding variables. The original study has taken several confounds as mediators. Therefore, the criticism seems inappropriate. The limitations of the study may be suggestive of some more common and relevant confounders, but owing to the aims of the study, the limited number of confounding factors do not seem to pose a major weakness.

Andrade[1] has made comments on theoretical grounds and has not provided relevant literature to support the critical comments. It would have been a more informative article if the claims could be supported in the context of some relevant studies. The only references provided were those of some statistical studies about confounding variables.

The reader gets only partially helpful impression due to the criticism because (a) the role of confounders considered in research is ignored, which pertains to the problems of the study, (b) the criticisms were made on the basis of the limitations of the study, not on the basis of its merits and conceptual issues and (c) the discussion about previous research could provide better insights into what the author purported to convey.

Financial support and sponsorship
Nil.

Conflicts of interest
There are no conflicts of interest.

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Author’s Response to Comments on “Prenatal Depression and Infant Health: The Importance of Inadequately Measured, Unmeasured and Unknown Confounds”

Sir,

As far as I could make out, in response to the Learning Curve article[1] on the importance of inadequately measured, unmeasured, and unknown confounds, Verma[2] makes four broad points: (1) that the study of Coburn et al. [3] was methodologically adequate; (2) that my criticism of the study[3] was unjustified; (3) that the authors[3] did not intend to imply a cause–effect relationship between antenatal depression and infant health; and (4) that my article[1] did not cite references to support the arguments for the existence of potential confounds.

Learning Curve articles are intended to teach young academicians about statistics, research methods, how to read a research paper, and related matters of a scientific and academic nature. Such teaching is best understood with the help of examples. As clearly indicated in the abstract, the observational study of Coburn et al.[3] was presented as an example, and the message that the teaching conveys applies not only to this study[3] but to all studies with an observational research design.

With regard to the first point, observational studies can never ever be methodologically adequate for the very reason that my article[1] describes: it is difficult to identify and satisfactorily measure all the known confounds and impossible to measure unknown confounds. As a simple example, there is a long list of reasons why babies may experience common health disturbances during the first 12 weeks of life; just how many of these did Coburn et al.[3] record and adjust for in their analyses? If prospective studies cannot be comprehensive, then retrospective studies are even less likely to be comprehensive in their identification and measurement of confounds, and in adjustment for confounds.

This does not mean that observational studies should not be performed. Such studies are necessary for hypothesis generation. However, such studies can also be found dreadfully wrong at the time of hypothesis testing,[3] and so a prudent reader would want to know whether or not to take the findings of an observational study at face value or to read between the lines and consider alternate explanations that include confounding.[3]

With regard to the second point, I am sure that Coburn et al.[3] did their best with the available resources. However, responsible execution of a study does not

REFERENCES

1. Andrade C. Prenatal depression and infant health: The importance of inadequately measured, unmeasured, and unknown confounds. Indian J Psychol Med 2018;40:395-7.
2. Coburn SS, Luecken LJ, Rystad IA, Lin B, Crnic KA, Gonzales NA. Prenatal maternal depressive symptoms predict early infant health concerns. Matern Child Health J 2018;22:786-93.
3. Chung EK, McCollum KF, Eio IT, Lee HJ, Culhane JF. Maternal depressive symptoms and infant health practices among low-income women. Pediatrics 2004;113:e523-9.
4. Rahman A, Iqbal Z, Bunn J, Lovel H, Harrington R. Impact of maternal depression on infant nutritional status and illness: A cohort study. Arch Gen Psychiatry 2004;61:946-52.