

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Investigating service delivery and perinatal outcomes during the low prevalence first year of COVID-19 in a multiethnic Australian population: a cohort study
AUTHORS	Melov, Sarah; Elhindi, James; McGee, Therese; Lee, Vincent; Cheung, N Wah; Chua, Seng Chai; McNab, Justin; Alahakoon, Thushari I; Pasupathy, Dharmindra

VERSION 1 – REVIEW

REVIEWER	Delius, Maria Ludwig-Maximilians-University , Obstetrics and Gynecology
REVIEW RETURNED	28-Mar-2022

GENERAL COMMENTS	Thank you very much for this interesting and perfectly elaborated article!
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REVIEWER	Al-Obaidly, Sawsan Hamad Medical Corporation, Obstetrics and Gynecology Department, Women's Wellness and Research Center
REVIEW RETURNED	04-Apr-2022

GENERAL COMMENTS	Thank you for your efforts in conducting this multiethnic study of perinatal outcome during the first year of Covid-19 pandemic. The study results included reduction in vaginal births, spontaneous preterm birth, SGA infants with marginal increase in the adverse neonatal outcome, however, it was not clear why? And the explanations given were not convincing enough! There were too many uncertainties in the discussion section.
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REVIEWER	Cavallaro, Francesca The Health Foundation
REVIEW RETURNED	14-Apr-2022

GENERAL COMMENTS	<p>Review</p> <p>Thank you for the opportunity to review this paper on the impact of COVID-19 changes in maternity care delivery and community practices on birth outcomes in Australia. The paper is original and its strength lies in the presentation of multiple interrelated birth outcomes, which is helpful to understanding how COVID-19 restrictions changed the dynamics of obstetrics. However, several additions may help strengthen the manuscript, I have included suggestions below.</p> <p>Abstract</p> <p>- The main outcome measures lists "preterm birth" but the results refer to "spontaneous preterm birth"; I suggest specifying both</p>
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	<p>iatrogenic and spontaneous in main outcome measures, and additionally reporting results for iatrogenic in the abstract</p> <ul style="list-style-type: none"> - I suggest referring to the increase in caesarean births rather than the decrease in vaginal births, since caesareans are the only category with an increase and it may be unclear to readers whether vaginal includes instrumental deliveries or not (this comment applies throughout the paper). <p>Strengths and limitations</p> <ul style="list-style-type: none"> - Would it be possible for authors to include a number/percentage to quantify the “low prevalence of COVID-19 in the community”? (this comment also applies to Introduction) <p>Introduction</p> <ul style="list-style-type: none"> - “Maternity care in Australia ... experienced a disparate range of changes” – do the authors mean “experience of maternity care” (based on the quoted study)? - “population who initially experienced minimal COVID-19 community transmission, a short lockdown period but experience significant obstetric service and societal changes...” and “yet having considerable preventative measures implemented” – I appreciate the authors have included more details on this in the Methods, however I think it would be useful to add a number/percentage for the burden of COVID-19 in this population, and a few examples of obstetric service and societal changes most relevant to maternity outcomes, with references where possible. - “We aim to identify pandemic-related morbidity” – I suggest “identify the burden of adverse birth outcomes associated with the COVID-19 pandemic (direct and indirect)” or similar. The current wording, to me, is unclear on whether the paper focuses more on direct or indirect morbidity. <p>Methods</p> <ul style="list-style-type: none"> - I suggest restricting the pre-COVID phase to 31st January 2018 onwards, to avoid any bias related to seasonality of births - P5, lines 22-31: I suggest splitting and re-grouping these sentences with the information related to pregnant women in one sentence, and the general population in another sentence, for clarity. - It would be useful to relate the small number of pregnant women with a recorded COVID infection to the total number of women giving birth in the COVID period, and to state explicitly (in the Methods and early on in the Discussion) that due to this small rate, most of the observed differences in outcomes will be attributed to indirect COVID effects related to changes in maternity service provision and maternal exercise/social support, rather than direct effects of COVID infection - Changes to maternity service delivery are helpfully explained, it might be useful to add to one of the paragraphs on social restrictions what effects these might have had on maternal risk factors for adverse birth outcomes (e.g. stress, social support, exercise), with references where available - Data analysis and statistical methods section: it would be useful for the authors to state why they report findings from three different models (rather than just reporting the fully adjusted model for each outcome). Given these results are not very different, and that it is unclear in the Results section which model results are being reported, the authors may wish to move models 1/2 to supplementary materials, if they were to comment on the difference in estimates in the Results (I do not think this is the case currently).
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	<p>Results</p> <ul style="list-style-type: none"> - “There were 34 103 singleton births for the three district hospitals “ – this information is redundant with the final sentence in the paragraph. I would suggest starting the paragraph with the breakdown of births by study period, then reporting the place of birth across both periods. - Table 2: I suggest adding a line with the composite neonatal adverse birth outcome (any of the four outcomes) - Table 3: It may be more intuitive for readers to refer to caesarean sections as the delivery mode outcome, rather than vaginal births (see earlier comment) <p>Discussion</p> <ul style="list-style-type: none"> - Impact on exclusive breastfeeding: do authors have any information on length of stay? If women stayed less time after birth, they might have had less time to establish breastfeeding. I agree with the authors’ assessment that there may have been reduced opportunities for staff to provide breastfeeding support (as a side note, this seems like such an unnecessary adverse outcome given the low prevalence of COVID 19 in Sydney, contrary to the Italian study cited at the start of the paragraph) - “The multiethnic population with an even distribution between SES quintiles strengthens the generalisability of our findings to other populations” – do the authors mean to other Australian populations? The main impact of COVID-19 on obstetric outcomes is likely to have been through service delivery and community behaviour changes, which may have been very different e.g. in Italy, or Kenya. It would be helpful if the authors could help draw out which populations their results might also apply to. - I felt the Discussion was a bit of a missed opportunity to bring all the findings together and draw out the explanation and interrelationships between them for the benefit of readers. As the authors note, the strength of the study is reporting on several birth outcomes which help show how the ecology of obstetric services changed over this time period. In my mind, the following points would have been worth commenting on: <ul style="list-style-type: none"> o There was no change in iatrogenic preterm rates, suggesting that providers did not change their management of preterm complications – which overall could be seen as good news o How do the authors interpret the change in SGA? Could this be due to reduced physical activity as with preterm birth? Does it suggest a reduction in stress among pregnant women (rather than an increase which might have been expected from the anxiety-inducing pandemic atmosphere)? o Caesarean section rates increased by almost 20%, suggesting that mothers and/or providers had lower risk thresholds either during labour or for antepartum complications for immediate delivery. It would be useful to comment on possible reasons for this increase. Is it possible that there was poorer surveillance of some conditions e.g. hypertension due to remote antenatal care, and less successful preventive management led to more caesareans? o The evidence of increased adverse neonatal outcomes is very important to comment on – and slightly counterintuitive in the context of reduced preterm births. Could the increase in caesareans have contributed to this? (it seems unlikely to me, but I am not a clinician) Did the reduction in preterm births and in SGA lead to bigger babies, and therefore more prolonged labour/emergency caesareans? When picking apart the composite indicator, it seems there was no change/minor reduction in stillbirth/low apgar/intubation, so all this increase is driven by
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	<p>admission to NICU. Were there any changes to NICU admission protocols in COVID, or to provider decisions in practice e.g. in light of infection risks?</p> <ul style="list-style-type: none"> o I suggest not overinterpreting the adverse neonatal outcome finding, but if there is a chance that this reflects the danger of “underintervening” in late pregnancy for some women with e.g. hypertension, then this would be an important finding. Overall the authors are uniquely placed as researchers and clinicians to help explain the interrelated web of these outcomes, and it would strengthen the paper to incorporate these points in the discussion o Great to see a comprehensive discussion of exclusive breastfeeding and highlight of the longer-term health effects. It would also be worth commenting on the longer-term effects of increased caesareans for both mothers and babies <ul style="list-style-type: none"> - One limitation not mentioned in the Discussion is how likely it is that the observed changes were due to changes in the population of women giving birth between the two years. Due to lockdown, women may have been more likely to give birth at a hospital nearest to their residence (rather than work) and/or to move neighbourhoods – this may mean that the population of women giving birth in the three study hospitals had a different obstetric risk makeup before than during COVID. It would be useful if authors could address this limitation. <p>Conclusion</p> <ul style="list-style-type: none"> - I suggest starting the conclusion with the main finding of the study (reduction in spontaneous preterm birth, no change in induction or iatrogenic preterm birth, but increase in caesareans) - The argument made in the first two sentences is a bit difficult to follow, I would suggest rewording <p>Minor points of feedback</p> <ul style="list-style-type: none"> - Some commas seem to be unnecessary or missing a previous comma, making some sentences a little confusing (e.g. “maternity care in Australia during the first year of the pandemic in 2020, experienced ...” or “Western Sydney with its multicultural population, is an ideal ...” in the Introduction, among others). - P. 6, line 51 – suggest specifying “area-level socioeconomic status” here and throughout - Table 1: one p-value is listed as “0.00” - Results, “Interestingly” – this word choice feels colloquial here, I suggest “Notably” or “in contrast” instead (although the authors are right, it is very interesting) - Discussion “may be correct dietary” – there may be a missing word here - Discussion “even distribution between SES quintiles” – I suggest specifying “between national SES quintiles”, otherwise it may be confusing (since, by definition, SES quintiles within the study sample would be evenly distributed)
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Dr. Maria Delius, Ludwig-Maximilians-University

Comments to the Author:

Thank you very much for this interesting and perfectly elaborated article!

Response: We thank the reviewer for their time and positive review.

Reviewer: 2

Dr. Sawsan Al-Obaidly, Hamad Medical Corporation

Comments to the Author:

Thank you for your efforts in conducting this multiethnic study of perinatal outcome during the first year of Covid-19 pandemic.

The study results included reduction in vaginal births, spontaneous preterm birth, SGA infants with marginal increase in the adverse neonatal outcome, however, it was not clear why? And the explanations given were not convincing enough! There were too many uncertainties in the discussion section.

Response: We thank the reviewer for their time and have made changes to the discussion to provide more coherence. Response and more details of changes are described below.

Reviewer: 3

Response: We thank the reviewer for a thorough and detailed review. We have addressed all comments, suggestions, and edits below. We feel the constructive review has improved our manuscript and hope our responses address the reviewers concern with our manuscript.

Dr. Francesca Cavallaro, The Health Foundation

Comments to the Author:

Review

Thank you for the opportunity to review this paper on the impact of COVID-19 changes in maternity care delivery and community practices on birth outcomes in Australia. The paper is original and its strength lies in the presentation of multiple interrelated birth outcomes, which is helpful to understanding how COVID-19 restrictions changed the dynamics of obstetrics. However, several additions may help strengthen the manuscript, I have included suggestions below.

Abstract

- The main outcome measures lists "preterm birth" but the results refer to "spontaneous preterm birth"; I suggest specifying both iatrogenic and spontaneous in main outcome measures, and additionally reporting results for iatrogenic in the abstract

• Response: added to the abstract-

'Main outcome measures Induction of labour, caesarean section delivery, iatrogenic and spontaneous preterm birth'

and in results

'...no change in iatrogenic preterm births (aOR 0.94; 95% CI 0.80-1.09)'

- I suggest referring to the increase in caesarean births rather than the decrease in vaginal births, since caesareans are the only category with an increase and it may be unclear to readers whether vaginal includes instrumental deliveries or not (this comment applies throughout the paper).

• Response: Increase in caesarean births has been added to abstract, Table 3 and appropriate edits in the manuscript.

Strengths and limitations

- Would it be possible for authors to include a number/percentage to quantify the "low prevalence of COVID-19 in the community"? (this comment also applies to Introduction)

• Response: Edits to manuscript with added text in strengths and limitations- "...indirect effects of COVID-19 against a background of low COVID-19 prevalence in the local health district with a total of six women with COVID-19 during pregnancy for the study period."

Introduction

- “Maternity care in Australia ... experienced a disparate range of changes” – do the authors mean “experience of maternity care” (based on the quoted study)?

• Response: The quoted study provides evidence for the range of experiences across Australia both for maternity care service delivery and the experience of receiving maternity care. We have added another reference that improves clarity of the variety of service delivery changes as well as experience to ensure the general term ‘Maternity care’ is appropriate in this sentence.

- “population who initially experienced minimal COVID-19 community transmission, a short lockdown period but experience significant obstetric service and societal changes...” and “yet having considerable preventative measures implemented” – I appreciate the authors have included more details on this in the Methods, however I think it would be useful to add a number/percentage for the burden of COVID-19 in this population, and a few examples of obstetric service and societal changes most relevant to maternity outcomes, with references where possible.’

• Response: Increased detail has been added to both introduction and methods for low prevalence and service changes:

Additional text to last paragraph of the introduction:

‘There was a total of 632 cases recorded of COVID-19 in the local health district for the study period. However, considerable preventative measures were implemented in the region including access to telehealth, ability to work from home, restrictions in healthcare settings such as mask wearing and health-screening questions on entry to all hospitals.

Response: Additional text to methods: ‘For the study period a total of 39 pregnant women had a confirmed COVID-19 diagnosis in NSW, six of these were in the study health district and none of these were admitted to hospital for COVID-19 complications’

- “We aim to identify pandemic-related morbidity” – I suggest “identify the burden of adverse birth outcomes associated with the COVID-19 pandemic (direct and indirect)” or similar. The current wording, to me, is unclear on whether the paper focuses more on direct or indirect morbidity.

• Response: Text edited to include in the last paragraph in introduction; ‘We aim to identify indirect and pandemic-related morbidity...’

Methods

- I suggest restricting the pre-COVID phase to 31st January 2018 onwards, to avoid any bias related to seasonality of births

• Response: We thank the reviewer for this suggestion. After careful consideration, we feel the impact of birth seasonality would be fewer or greater births in the month of January. The only time dependent variable in our study is the COVID period. So, removing January would only increase or decrease the number of births in the pre-covid period. After discussion with the statistician, the inclusion of the month of January we believe will not result in significant bias but it will have the negative impact of reducing our sample size. Respectfully we feel the time period for analysis should remain. We are also mindful that at the current stage of result reporting the post hoc modification of design may be considered inappropriate.

- P5, lines 22-31: I suggest splitting and re-grouping these sentences with the information related to pregnant women in one sentence, and the general population in another sentence, for clarity.

- It would be useful to relate the small number of pregnant women with a recorded COVID infection to the total number of women giving birth in the COVID period, and to state explicitly (in the Methods and early on in the Discussion) that due to this small rate, most of the observed differences in outcomes will be attributed to indirect COVID effects related to changes in maternity service provision and maternal exercise/social support, rather than direct effects of COVID infection

• Response: The second paragraph in methods has been edited as suggested and further detail on prevalence in the local health district with COVID-19 pregnancy details added.

In discussion, text has been added to the first paragraph: 'In the study population only six women were recorded to have experienced COVID-19 infection during pregnancy therefore the outcome changes identified in this study are likely related to the indirect effects of COVID-19.'

Changes to maternity service delivery are helpfully explained, it might be useful to add to one of the paragraphs on social restrictions what effects these might have had on maternal risk factors for adverse birth outcomes (e.g. stress, social support, exercise), with references where available

- Response: added to paragraph 4 of methods evidence of potential changes in background stress as a factor for changes in birth outcomes: 'The potential changes in background stress for women during the peripartum due to these restrictions and service delivery changes may potentially impact perinatal outcomes.'

Data analysis and statistical methods section:

-It would be useful for the authors to state why they report findings from three different models (rather than just reporting the fully adjusted model for each outcome). Given these results are not very different, and that it is unclear in the Results section which model results are being reported, the authors may wish to move models 1/2 to supplementary materials, if they were to comment on the difference in estimates in the Results (I do not think this is the case currently).

- Response: Presenting the data in some granularity with the different models and variables we have adjusted for, we feel will provide more information for readers and the opportunity to understand the effect and contribution of maternal characteristics, models of care and additional relevant factors to the overall adjusted estimates. The points raised by the reviewer is valued and we will accept editorial decision.

Also, we now have referred to the difference in estimates in both the results and discussion.

Results: 'There was no difference in effect size between model 1 and the fully adjusted model that included birthweight, mode of delivery, length of stay <24 hours and gestational age/preterm variable.'

Discussion: 'Intuitively the reduction in breastfeeding should be linked with the increase in early discharge however the 25% reduction in full breastfeeding was present for all models. Other factors are therefore more likely influencing this outcome.'

Results

- "There were 34 103 singleton births for the three district hospitals " – this information is redundant with the final sentence in the paragraph. I would suggest starting the paragraph with the breakdown of births by study period, then reporting the place of birth across both periods.

- Response: The first sentence has been removed from text and the paragraph re-ordered as the reviewer suggests.

- Table 2: I suggest adding a line with the composite neonatal adverse birth outcome (any of the four outcomes)

- Response: Composite neonatal adverse outcome data had now been included in Table 2.

- Table 3: It may be more intuitive for readers to refer to caesarean sections as the delivery mode outcome, rather than vaginal births (see earlier comment)

- Response: Table 3 had been amended to now refer to increase in caesarean section births rather than a reduction in vaginal births.

Discussion

- Impact on exclusive breastfeeding: do authors have any information on length of stay? If women stayed less time after birth, they might have had less time to establish breastfeeding. I agree with the authors' assessment that there may have been reduced opportunities for staff to provide breastfeeding support (as a side note, this seems like such an unnecessary adverse outcome given

the low prevalence of COVID 19 in Sydney, contrary to the Italian study cited at the start of the paragraph)

- Response: The length of stay an important consideration and one we considered may impact on feeding at discharge. This is why it has been included as a variable to be adjusted for in the analysis. On a side note, ongoing research during the high COVID-19 prevalent period has found unfortunately breastfeeding at discharge rates have continued to decline, again adjusting for the reduced length of stay.

- “The multiethnic population with an even distribution between SES quintiles strengthens the generalisability of our findings to other populations” – do the authors mean to other Australian populations? The main impact of COVID-19 on obstetric outcomes is likely to have been through service delivery and community behaviour changes, which may have been very different e.g. in Italy, or Kenya. It would be helpful if the authors could help draw out which populations their results might also apply to.

- Response: Clarification has been added to text in discussion of relevant similar populations and strength of study population:

‘strengthens the generalisability of our findings to other high-income populations with universal health coverage such as the United Kingdom. A more homogenous population may provide a possible explanation of changes to be specific cultural drivers however the diversity of the study population supports the explanation to likely be societal and service delivery related.’

- I felt the Discussion was a bit of a missed opportunity to bring all the findings together and draw out the explanation and interrelationships between them for the benefit of readers. As the authors note, the strength of the study is reporting on several birth outcomes which help show how the ecology of obstetric services changed over this time period. In my mind, the following points would have been worth commenting on:

- o There was no change in iatrogenic preterm rates, suggesting that providers did not change their management of preterm complications – which overall could be seen as good news

- Response: Added to text potential explanation of lack of change for iatrogenic preterm in the context of local campaigns:

‘The stable iatrogenic preterm birth rate is a positive finding and reflects no change in clinical management for this important obstetric outcome. This may partly be associated with the ongoing understanding of the adverse outcomes associated with late preterm births and recent national initiatives such as ‘every week counts’ that has occurred over the study period.’

- o How do the authors interpret the change in SGA? Could this be due to reduced physical activity as with preterm birth? Does it suggest a reduction in stress among pregnant women (rather than an increase which might have been expected from the anxiety-inducing pandemic atmosphere)?

- Response: Added to discussion text the potential link in drivers between spontaneous preterm birth and reduction in SGA:

‘There may potentially be a uniquely cumulative improved immune environment for pregnant women during the COVID-19 period. Underlying factors such as consistent diet stabilising the microbiome and less maternal inflammatory triggers or burden from exposure to environmental and infectious factors may be the reason for improved spontaneous preterm birth rate and SGA outcomes.’

‘It is possible that the drivers for the reduction of spontaneous preterm birth and SGA are similar and multifactorial. They may include the opportunity for partners and pregnant women to work from home with the associated reduction in stress, early correct dietary advice and care from their primary care provider.’

- o Caesarean section rates increased by almost 20%, suggesting that mothers and/or providers had

lower risk thresholds either during labour or for antepartum complications for immediate delivery. It would be useful to comment on possible reasons for this increase. Is it possible that there was poorer surveillance of some conditions e.g. hypertension due to remote antenatal care, and less successful preventive management led to more caesareans?

• Response: New text added for possible explanation and link with other findings.

‘The increase in caesarean section births in this study is a concerning finding that may indicate changes in clinical decision making during the COVID-19 period of a lower threshold trigger for immediate delivery. However other factors may also be involved such as less surveillance during pregnancy with maternal reluctance to present or be in hospitals as demonstrated by the increase in early discharge . Another human factor that may be involved in the rise in caesarean section birth is the difficulty of midwives in birth unit to develop a rapport with the women in their care to adequately assess their non-verbal cues, recent studies have identified midwives report a loss of ‘women-centred care’ during the COVID-19 pandemic.^{20 21} Clinician may rely more on electronic ‘socially distant’ continuous cardiotocography (CTG) monitoring for fetal assessment. Evidence suggests increased CTG monitoring leads to higher caesarean section rates.’

o The evidence of increased adverse neonatal outcomes is very important to comment on – and slightly counterintuitive in the context of reduced preterm births. Could the increase in caesareans have contributed to this? (it seems unlikely to me, but I am not a clinician) Did the reduction in preterm births and in SGA lead to bigger babies, and therefore more prolonged labour/emergency caesareans? When picking apart the composite indicator, it seems there was no change/minor reduction in stillbirth/low apgar/intubation, so all this increase is driven by admission to NICU. Were there any changes to NICU admission protocols in COVID, or to provider decisions in practice e.g. in light of infection risks? I suggest not overinterpreting the adverse neonatal outcome finding, but if there is a chance that this reflects the danger of “underintervening” in late pregnancy for some women with e.g. hypertension, then this would be an important finding. Overall the authors are uniquely placed as researchers and clinicians to help explain the interrelated web of these outcomes, and it would strengthen the paper to incorporate these points in the discussion

• Response: Interpretation of increased adverse neonatal outcomes results added to text in discussion:

‘There was a marginal increase in the composite adverse neonatal outcome largely driven by the increase in neonatal admissions. Although it is beyond the scope of this study to determine the causes for the increased admission, there was no alteration in admission criteria for the neonatal or special care nursery during the study period. However, the increase in caesarean birth may have contributed through the associated known increased risk of NICU admission with a caesarean section birth.’

o Great to see a comprehensive discussion of exclusive breastfeeding and highlight of the longer-term health effects. It would also be worth commenting on the longer-term effects of increased caesareans for both mothers and babies

• Response: Added to text:

‘The increase in caesarean section births also have known immediate and long-term associated morbidity for women and their infants, therefore measures to counter the rise in caesarean births are recommended.’

- One limitation not mentioned in the Discussion is how likely it is that the observed changes were due to changes in the population of women giving birth between the two years.

• Response: Add to text in discussion paragraph three, is the limitation that may exist regarding population changes between periods however adjustment for some demographic changes have been made in the analysis.

‘A more homogenous population may provide a possible explanation of changes to be specific cultural drivers however the diversity of the study population supports the explanation to likely be societal and

service delivery related. A limitation is the difficulty of identifying all changing population drivers however adjustment was made for known factors.'

Due to lockdown, women may have been more likely to give birth at a hospital nearest to their residence (rather than work) and/or to move neighbourhoods – this may mean that the population of women giving birth in the three study hospitals had a different obstetric risk makeup before than during COVID. It would be useful if authors could address this limitation.

- Response: In our public system women are restricted to provision of service to their home address. Therefore, there would be limited change due to closer to work preferences. This factor has been added to methods to improve clarity and interpretation.

'Women in the public health sector in NSW are triaged to their nearest public hospital for pregnancy care according to their home address and pregnancy complications. Therefore, during the pandemic period there would be limited changes in referrals pathways for the district obstetric population.'

Conclusion

- I suggest starting the conclusion with the main finding of the study (reduction in spontaneous preterm birth, no change in induction or iatrogenic preterm birth, but increase in caesareans)

- Response: Added to text at start of the conclusion: 'In a low COVID-19 prevalent population this study found no change in inductions of labour or iatrogenic preterm births. However, an increase in caesarean births and a reduction in SGA and spontaneous preterm births was identified.'

- The argument made in the first two sentences is a bit difficult to follow, I would suggest rewording

- Response: These sentences have been rewording and clarification of improved health added with an additional supporting reference.

Minor points of feedback

- Some commas seem to be unnecessary or missing a previous comma, making some sentences a little confusing (e.g. "maternity care in Australia during the first year of the pandemic in 2020, experienced ..." or "Western Sydney with its multicultural population, is an ideal ..." in the Introduction, among others).

- Response: The manuscript has been reviewed and the above comma's have been amended.

- P. 6, line 51 – suggest specifying "area-level socioeconomic status" here and throughout

- Response: 'area-level' has been added to text and tables.

- Table 1: one p-value is listed as "0.00"

- Response: Error corrected now <0.01

- Results, "Interestingly" – this word choice feels colloquial here, I suggest "Notably" or "in contrast" instead (although the authors are right, it is very interesting)

- Response: We agree that 'notably' is a better choice and the text has been amended.

- Discussion "may be correct dietary" – there may be a missing word here

- Response: We thank the reviewer there is a word missing and has been added to text 'dietary advice.'

- Discussion "even distribution between SES quintiles" – I suggest specifying "between national SES quintiles", otherwise it may be confusing (since, by definition, SES quintiles within the study sample would be evenly distributed)

- Response: The text has been amended to include 'national'.

VERSION 2 – REVIEW

REVIEWER	Cavallaro, Francesca The Health Foundation
REVIEW RETURNED	23-Jun-2022
GENERAL COMMENTS	Many thanks for the opportunity to review the revised version of this manuscript. The authors have addressed all of my comments, and I have no further suggestions for improving the manuscript. Congratulations to the authors on a very interesting paper.