Birth Spacing in the Presence of Son Preference and Sex-Selective Abortions: India's Experience over Four Decades

Response to Editor and Reviewer Comments

Please find attached the revised version of my paper, "Birth Spacing in the Presence of Son Preference and Sex-Selective Abortions: India's Experience over Four Decades." I want to thank the three reviewers and the editor for the constructive comments and suggestions, which I believe substantially improved the paper.

I have substantially rewritten the paper, so before responding to the individual comments, I provide an overview of the revised paper and the most significant changes.

To clarify the contribution of the paper, I now focus on three questions. First, how has birth spacing changed over time with the introduction of sex selection and the other significant changes in India? Second, with substantial increases in spacing, how biased is our standard fertility estimates? Finally, how has infant mortality changed, and is there an effect of sex selection on infant mortality risk?

I have added four new sections: two analytical sections, one on fertility and one on infant mortality, and two background sections, one on female education and labor force participation and one on the conceptual framework. I have removed the section on regional differences.

Section-by-section overview of significant changes:

1. I have completely rewritten the "Introduction" section to provide background on India, discuss why examining birth spacing is relevant, and highlight main results.

2. The first new section covers how female education and labor force participation changed over time in India. Both female education and labor force participation are factors in spacing decisions. That labor force participation has declined even with new groups received more schooling helps address the question of whether the expanded access to education might change behavior.

Because labor force participation differs by education level, I now divide the original eight or more years of education group into two groups: eight to eleven and twelve or more years of education. The eight to eleven group has the lowest labor force participation of any group, and twelve or more has one of the highest.

3. The second new section provides a conceptual framework. I first work through the likely general changes based on the increasing female education and the declining labor force participation and second through the potential effects of access to sex selection. I end with a summary of the predictions.

4. I have further shortened the "Estimation Strategy" section and edited it to focus on the motivation for the choice of model.

5. The Data section is also shorter, and the discussion of potential effects of variables is now in the conceptual framework section.

6. With the new education grouping, I have completely rewritten the section on how birth spacing has changed (the original Results section). Instead of going through each education level in detail, I now focus more broadly on how average birth spacing and parity progression have changed over time and then on the effects of sex selection, drawing on examples to illustrate the changes. The graphs are now easier to read.

7. The original subsection "Distribution of Birth Spacing Within Spells" is now a new separate section "Distribution of Birth Spacing" and has been completely rewritten. Instead of showing the conditional survival graphs, the section now includes figures with the 25th and 75th percentiles for birth intervals. These figures allow me to focus on the changes in both short and long intervals. Both to tie in with the subsequent mortality discussion.

8. The new section, "What has Happened to Fertility?" looks at the degree of tempo

effect that arises from the longer spacing by comparing a TFR-like measure with the predicted cohort fertility based on the hazard model results.

9. The new section "Mortality and the Changing Birth Spacing" examines how the association between infant mortality and birth spacing has changed over time by the education group and whether the increased use of sex selection increases mortality risk.

10. Finally, the conclusion has been completely rewritten.

The revised paper is just over 8,000 words per the author guidelines.

Editor

1. First, greater explanation of the unique contributions of this study is needed. As the reviewers note, the literatures in the areas of son preference and birth spacing, for example, are extensive, and clearer and more compelling motivations for the work should be provided.

   [Response:]

   To better address why and how the changes in birth spacing are relevant, I have added two new sections. One discusses the bias that arises in fertility estimates from the significant increases in birth spacing, both from sex selection and from secular changes, and show that we likely have substantially overestimated how early and fast fertility fell in India. The other shows that mortality has fallen for all groups no matter how long birth intervals are and that there is no apparent adverse effect of high use of sex selection on infant mortality.

2. Second, the paper lacks a conceptual framework to organize (and motivate) the analysis. Following on the comments of one reviewer, one suggestion would be to focus on the role of women's education, with a more thorough description of the context

of changing education in India over the last half century and clearly lay out a framework for the mechanisms through which birth spacing is affected by educational expansion.

[Response:]

I address this in two parts.

First, I included a new descriptive section on how female education and labor force participation has changed over time in India. I include the discussion of labor force participation because theories and prior results reveal it to be a crucial component in the link between female education and birth spacing. I argue that "Sanskritization" is behind the continued low labor force participation despite the expanded schooling and the associated changes in the composition of the education groups. The implication is that "Sanskritization" likely carries over to other decisions, such as sex selection use.

Second, I added a conceptual framework that focuses on how birth spacing is likely to respond in a situation where labor force participation is low and declining, and son preference is strong. A more general theory overview would be interesting, but that is not feasible given the word constraint. The conceptual framework section also discusses predictions for how access to sex selection is likely to impact birth spacing and summarizes the expected effect of the changes in India on birth spacing.

Writing these sections also showed that my original classification of education was too broad because labor force participation differs markedly within the original eight-plus education group. I have, therefore, redone all analyses in the paper with four education groups.

3. This relates to a third issue, which is space limitations and decisions regarding what to include in the manuscript. In the last decision letter you were asked to "reorganized to focus on the most important issues." Although the author has done a good

job of focusing the paper in the revision, there are still questions about lack of detail in some places and perhaps too much in others. One suggestion is to drop the analysis on regional differences and focus the paper more centrally on the topic of maternal education, for example (as noted above).

[Response:]

I agree that the regional analysis added little to the paper, and have deleted this section. What I consider the two most important issues related to birth spacing are fertility and mortality. I, therefore, now focus on how both relate to spacing, in addition to examining how birth spacing changed.

There are other relevant questions, such as the effect on the mother that I have ignored because of space considerations.

As part of the rewrite, I have removed any remaining discussion that might make the paper seem more like a methodological paper. For example, I have shortened the "Data" and "Estimation Strategy" sections and rewritten the description of the method in the "Introduction" section.

Reviewer 1

This paper studies birth variation in birth spacing in relation to son preference and (access to) sex selective abortion. The data cover four decades of birth histories in India, but have no direct information on access to or use of sex selective abortion. Using discrete time competing risk models, where the "risks" are the birth of a boy and the birth of a girl, patterns of birth spacing are produced that are plausibly in line with increasing access and use of sex selective abortion over time and a strong positive relation between mother's education level and the use of sex selective abortion. While many studies have discussed causes and consequences of the missing girls problem due to sex selective abortion, the author(s) claim that this is the first paper that systematically analyzes the link with birth spacing. If this is indeed the case (I am not following this literature close enough to be completely sure) then this is an innovative and important study that will be of interest to readers of Demography. I have some suggestions for changing the exposition.

1. The model is well explained, but the paper could be more explicit on why a model is needed. Some of the conclusions drawn from average probabilities, sex ratios, etc. (particularly those not conditioning on specific values of the covariates) might also be directly visible in the raw data. If so, the data description part could perhaps be extended.

   [Response:]

   Space limitations mean that I cannot add a more detailed examination of the raw data to the data description. I have instead tried to address this in three ways. First, I have added a discussion in the "Estimation Strategy" section of why hazard models are the preferred way of dealing with censoring. Second, I highlight that censoring of birth spells increases with parity and time in the "Data" section. Third, I added a comparison to the birth spacing numbers from the NFHS reports, which do not attempt to deal with censoring and, therefore, have shown virtually no change over

time.

2. Details on the estimation results are not presented. I assume there are too many of them to be included in the paper. Still, the paper extensively discusses model specification issues such as the proportionality assumption and the way that is handled by splitting the sample. I would like to see some discussion of model specification tests informing the reader whether this is necessary and whether the resulting flexible specifications indeed provide a good fit to the data.

   [Response:]

   I have tried to move away from pitching the paper as a methodological contribution. As part of this, I shortened the empirical model discussion and removed any mention in the "Conclusion" section. I agree that a more in-depth examination of how a proportional model stack up against the non-proportional model would be fascinating. However, I believe this is best done separately, given the already considerable length of the paper and the Appendix.

   Furthermore, the non-proportionality assumption makes the model more flexible than it would have been without, but cannot introduce any bias compared to the proportional model. If the non-proportionality assumption is not needed, the only thing affected is the efficiency of the estimates. However, the reverse is not the case, with bias possible if proportionality does not hold.

3. I found the analysis of geographical differences not very exciting. It leads to several pages with not so interesting figures of regional survival curves. I would suggest dropping the details (perhaps make them available in an online appendix) and briefly summarize the main findings.

   [Response:]

   I agree and have removed the section, as discussed above.

4. The concluding section raises some interesting points but these are only indirectly related to the content of the paper. The development of family planning possibilities with increasing availability and use over time seems to be important for understanding the general decline in fertility and lengthening of birth intervals. It is mentioned in the introduction but not in the conclusions. The relation with labor market participation may be of interest, but what is said about it was not clear to me. Why would longer birth intervals disrupt labor force participation (p. 32)

   [Response:]

   Unfortunately, the questions on labor force participation are not detailed enough to allow a direct analysis using this data. I have instead included a discussion of how female labor force participation has changed over time in the two new background/conceptual framework sections. Furthermore, I now directly discuss the prior literature and theory on labor force participation and birth spacing in the "Birth Spacing: Mechanisms and Prior Findings" section.

   While one might expect that increasing income and female education would also lengthen average birth spacing through better access and knowledge of contraception, the empirical relationship between contraception use and birth spacing is ambiguous (Tulasidhar, 1993; Whitworth and Stephenson, 2002; Bhalotra and van Soest, 2008; Yeakey, Muntifering, Ramachandran, Myint, Creanga, and Tsui, 2009; Kim, 2010; van Soest and Saha, 2018). Furthermore, the ability to successfully use "low efficacy" contraceptive methods is increasing in education, while there is no difference by schooling when it comes to modern contraceptives (Rosenzweig and Schultz, 1989). Because there is not sufficient historical information on contraception use in the data and that the prior literature does not provide a clear relationship, I have not included this discussion in the paper.

Specific comments:

1. p.7: eq. 2: I would call this a piecewise constant baseline hazard rather than a piecewise linear one.

   [Response:]

   I have changed this throughout the paper.

2. p.7 after eq. 3: Sex composition of previous children is in principle endogenous if there is sex selective abortion (it is essentially a lagged dependent variable). This is not taken into account. This is, at least in theory, a limitation of the current model that should be acknowledged. (I do not think it is easy to remedy without explicitly incorporating unobserved heterogeneity.)

   [Response:]

   That is correct and something that would be interesting to address in a future paper. For now, I have included a comment on this in the Estimation Strategy.

3. p.7: Why use intervals of three months? Did the author(s) perform any sensitivity checks?

   [Response:]

   I tried shorter than three months, but because the method requires events to happen in each period, the estimations would often fail to converge. For the same reason, I combine some three months intervals to ensure enough information, especially when intervals are long and the likelihood of a next birth low.

4. p.9: It might be useful to already mention that Hindus only are analyzed in the introduction, since religion might play an important role when choosing sex selective abortion.

   [Response:]

   I have included this in the rewritten "Introduction" section and the abstract.

5. p.10: Sterilization also seems an endogenous decision in this context, this should be acknowledged as an additional limitation. How often does this occur? (It might even be possible to treat this as an additional competing risk.)

   [Response:]

   Across the entire sample, 36 percent of women report sterilization for either themselves or their husbands. The decision does, however, not lend itself easily to incorporation into the model as an additional competing risk. First, almost nobody is sterilized after their first birth (only around 0.5 percent), although this goes up to 19 percent after the second birth, and 27 percent after the third. Second, a majority—often up to 75 percent—of sterilizations take place in the same month as the last birth or within eight months of it. Most of the sterilizations are, therefore, never included in the model samples because those begin at nine months after the prior birth. Hence, the absolute numbers are too small for the competing risk model to work correctly, primarily because the likelihood of sterilization depends strongly on the sex composition of prior children. For a given parity, the fewer boys, the lower the probability.

   The results presented here are conditional on a prior birth and no sterilization within the nine months after the preceding birth. The main concern is that the parity progression probabilities are too high because they do not account for sterilization. There are two impacts of this.

   First, it biases downward the differences in the parity progression probabilities across sex compositions. I discuss this in the "Data" section.

   Second, the predicted cohort fertility would be too high. I, therefore, estimate the likelihood of sterilization within the first nine months using a Logit model (results not shown in the paper). I then predict the probability of not getting sterilized and use that to scale down the parity progression probability when predicting cohort

fertility. In practice, the effect of the scaling is relatively small, 0.05 to 0.25, which is consistent with the lowest use among women who have the highest estimated parity progression probabilities based on the hazard model.

6. p.9: why put the marriage duration threshold at 22 years? Sensitivity check?

   [Response:]

   I discuss this in the Appendix section "Recall Error and the Sex Ratio". Since most of the surveys start showing significantly biased sex ratio from around 22 years of marriage on, I drop all observations where the wedding took place 22 years or more. Changes of a couple of years more or less do not substantially change the results. However, a cut significantly higher than 22 does eventually make the sex ratio very uneven in the first period, even for less-educated women. There are no effects on the more recent periods.

7. Results: The many small figures are hard to read and I did not find them an attractive way to present the results. Perhaps the author(s) can come up with an alternative?

   [Response:]

   I have tried to address this by increasing the font size for the legends and turning the graphs sideways. Originally the results were presented in table format (now available in the Appendix). Prior referees found it hard to get an overview of the results that way, and, on balance, I agree with that. Hence, I have kept the graphs for the spacing discussions. For the fertility section, I present the results as a table.

Reviewer 2

This study uses four waves of the National Family and Health Surveys to examine the interrelationship between son preference, sex ratios at birth, and birth spacing in India, how this varies between different regions, by parity and the sex composition of the children in the household, by maternal educational level, and over time.

1. On the one hand this feels like a clear and polished piece of research. On the other hand, I wonder how much we can learn from this study given that there is already an extensive body of research that examines son preference and SRBs in India, including how this varies by parity, sex composition of the children already in the household, maternal educational level, and over time. It seems to me that the question is, what is the net contribution of adding birth spacing as an additional dimension to this whole discussion.

   The authors attempt to address this point by offering four motivations for why we should care about spacing as an additional dimension. First, they say that "researchers have made extensive use of birth spacing as a measure of son preference, and it is critical to understand to what extent spacing is still a useful measure of son preference" (page 2). To my mind, the authors do not offer a particularly compelling explanation anywhere in the paper for why it is 'critical to understand' whether spacing remains a useful measure of son preference. Understanding son preference and how that is linked to sex-selective abortions and differential stopping behaviour (and gender dynamics more generally), is indeed critical – but I don't really see that the critical importance of understanding spacing in relation to this.

   [Response:]

   I have reframed the paper to be about birth spacing more generically (see also my response below) and tied it to two issues where changes in spacing have direct relevance: fertility estimates and infant mortality. I have removed most of the discus-

sion of spacing as a measure of son preference, except to note that we now have cases where son preference results in longer spacing without boys than with because of sex selection.

2. The second and third motivations offered by the authors are the potential consequences of (short) birth spacing for maternal health, child health, and long-term child development. For example, the authors cite literature showing that short spacing can have severe consequences for child outcomes, particularly in low-income settings. However, understanding the potential consequences of birth interval length is much more confusing when long birth intervals are a consequence of (sex-selective) abortions. One reason why short birth intervals have a negative effect on child outcomes is because of maternal nutrient depletion and insufficient time to recover from the previous pregnancy. Children born after long birth intervals where those intervals have been punctuated by multiple abortions will not see the same benefits as a child born after a long interval that is not punctuated by abortions since the mother would not be fully recovering in that intervening period. Likewise the costs and benefits for mothers themselves of short/long spacing may be overwhelmed by the effects of the abortions and interrupted pregnancies.

   [Response:]

   Thank you for this fascinating point on the potential adverse effects of the increased use of abortions. I have included this as one of the two main reasons why the changes in spacing are worth examining and have introduced a new section on infant mortality and how it varies by spacing and sex composition over time. There is, however, no apparent adverse effect of being a son born after girls and with longer spacing in situations with high use of sex selection. Hence, if there is a negative effect of multiple abortions, it is more than countered by the protective effect of higher maternal education. It is still possible that we would see more of an impact among

less-educated women, but this group is, currently, not heavy users of sex selection.

To focus the paper, I no longer discuss the potential effects on mothers, although I still find that an exciting topic for future analysis. The same goes for the long-term development of children.

3. The fourth and final motivation offered is that we don't know what determines spacing behaviour in low- and middle-income countries. However, the arguments that the authors develop here are more about contraceptive use, declining fertility, and women entering the labor market. Those factors don't seem to be addressed elsewhere in the paper at all.

   [Response:]

   As mentioned above, I now include a discussion of the changing spacing's effect on fertility estimates. Similarly, although I cannot directly examine the relationship between labor force participation and spacing because of data limitations, I do discuss the low and falling labor force participation in India and the effects this may have on spacing. I have removed the discussion on contraceptive use (see also my response above).

4. In the end, this study offers a careful descriptive account of how birth spacing has changed alongside sex ratios at birth, but little more. We should indeed care about sex ratios at birth – and people do – but there are many studies published on that topic, and it is not clear that this one adds something important to that particular debate. If it does, the authors have not articulated that clearly. I think that understanding the consequences of birth spacing for maternal and infant health is also extremely important. But the authors do not study that link empirically. For these reasons, I find the contribution of the study rather marginal and confusing.

   [Response:]

As described above, I have substantially rewritten the paper and introduced new analyses to illustrate better how and why examining birth spacing is necessary, including linking birth spacing and infant mortality directly. I am, however, open for suggestions for alternative ways to focus the paper and the motivations.

Reviewer 3

The author(s) have considered and addressed reviewers' comments and have revised the paper thoughtfully. The changes to the presentation of the results and added explanations are especially well done. There is much to be liked in the revised version. While the revised paper is stronger than the earlier version, there are still some issues that require further attention.

1. The paper still lacks a stronger narrative on the motivation and framework for the empirical work. Four reasons are mentioned as motivations; but, with the exception of the first, all other reasons, such as the impact of spacing on child and mother's health, are not really motivating the empirical work of this paper (they are rather evidence for importance of spacing).

   [Response:]

   I have entirely rewritten the paper to focus on how and why examining birth spacing is relevant, as described above. As part of that rewrite, I now estimate the relationship between spacing and infant mortality, mainly to understand whether the increased use of sex selection may increase mortality risk. Although I still find the effects on the mothers' health a fascinating question, I have decided not to discuss this effect because of lack of space.

2. The conceptual framework is not fully developed. The paper suggests possible explanations at various places but they don't add up to a coherent narrative. For instance, it is not clear whether maternal education acts through access for sex selective technology or preference for sons. The core idea of the paper that sex selection changes the relationship between spacing and son preference needs to be developed further. There is significant amount of good empirical work, but without proper development of framework it is difficult to evaluate the contribution and understand the findings, and also to see its contribution beyond the Indian case.

As an illustration, the paper looks at changes over time during which the access the sex selection has become increasing difficult AND also almost all groups (by education, religion…) have seen an decline in fertility. How do we consider the impact of these on spacing, sex selection and sex ratios? How do we put the findings in the context of changing educational attainments of women? The paper doesn't provide us a framework to think about the broader changes during the time period under consideration.

[Response:]

The second of the two new background sections draw together the different theories on the relationship between maternal education, labor force participation, birth spacing, and use of sex selection, and use those to guide the hypothesized changes.

To address the broader changes in India, the first of the two new background sections show how female education has increased, while female labor force participation has decreased. I argue that the low female labor force participation is an indication of "Sanskritization". The pool of educated women has expanded, but the behavior of the group does not change much.

I also show that although fertility fell, it is higher than generally acknowledged, precisely because longer birth intervals biases downward our standard fertility measures.

3. I'm not fully convinced of the main conclusions of a "substantial increase in the use of sex selection" or of an "an almost complete reversal of the traditional spacing patterns" for urban educated women. They don't seem to be supported by the findings. For urban women this seems to apply only for third spell. The substantial increase in sex selection is also not evident from the findings for all groups or for all spells. The paper does acknowledge the complexity, but at places (including in the abstract) seems to stretch the interpretation beyond what is shown in the findings.

The conclusions are presented in way that implies that they apply to different spells or groups. But the findings are more limited and differ clearly by spells. This should be presented accurately in the text.

[Response:]

That was, indeed, an overstatement of the results. I now discuss the three possible outcomes—no sex selection, sex selection with no change in relative order by sex composition, and sex selection with a change in the order—and provide examples of each.

4. While I really liked the methodology and visual presentation of the findings, in my reading, I don't see the paper's contribution to methodology. Though the paper makes a claim for methodological novely, I see it more as an adoption of a widely used technique to spacing rather than a proposing a new technique. Despite the changes, the paper still reads as a piece demonstrating a methodology rather than signficant contribution to the demogrpahic literature.

   [Response:]

   I have tried to move away from pitching the paper as a methodological contribution. As part of this, I shortened the empirical model discussion and removed any mention in the "Conclusion" section. I hope that the two new analytical sections and the two background sections make the paper more of a definite contribution to the literature.

5. Minor: It might be a disciplinary style, I would recommend cutting down on the footnotes.

   [Response:]

   I have removed or incorporated into the text a large number of footnotes.

6. I would also not go into the effect of child spacing in countries that are very dissimilar to the context of India, unless there is a wider point being made.

[Response:]

I have removed this discussion.

7. Some points are not clear: for instance, "On the other hand, increased reliability of access and effectiveness of contraceptives can lead to shorter intervals between births if women used to have longer spacing to avoid having too many children by accident (Keyfitz, 1971; Heckman and Willis, 1976)."

   [Response:]

   This discussion is no longer in the paper because of the new conceptual framework. If other parts are unclear, please let me know.

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