Dear Sergii,

I read your thesis with great interest, thank you for writing it. It is a well-researched monography with many interesting findings, and you should be proud of your accomplishment! I will focus my comments mainly on the third chapter, although it is worth noting that I was happy to see that the Scandinavian results presented in the second chapter align with our own analyses in Kabatek & Ribar (2020). There, we show that the fertility choices of Dutch couples are also indicative of a modest daughter preference. You may want to add this reference to the text to strengthen the external validity of your findings.

The third chapter is a competently-performed extension of the analyses conducted by Kabatek & Ribar (2020). I wholly agree with your statement that it is important to examine the same research question in different cultural contexts. The Dutch and the U.S. findings should not be taken as universally applicable, and further analyses are necessary to assess whether the family dynamics uncovered thus far are specific to the various cultural and institutional features of the studied societies. In fact, my own work on China (still in progress) documents an existence of a strong divorce mechanism that is indicative of a time-invariant son preference in Chinese society. This is presumably not too surprising, but it confirms that different societies may well be subject to different influences and attitudes, and that son preference should not be dismissed as a potential mechanism that drives marital stability of parents around the world.

I appreciate that the third chapter studies not only the divorce outcomes but also the marital formation, which—in your context—evidently exhibits idiosyncrasies that are contingent on children's sexes. This is a valuable analytical extension of the event history models, and I believe it can be refined further (I will elaborate on this below). The reassessment of Max van Lent's personality study is interesting as well, providing further context to the baseline findings.

There are several things that I would like you to address and/or reflect upon. My first point concerns the discussion of Kabatek & Ribar (2017), where you state that the authors do not assess whether parents with daughters are less likely to marry. While the 2017 DP version of our manuscript does not explicitly comment on this type of behavior, the relevant empirical tests can actually be found among the summary statistics. The assessment is then made explicit in the published version (pg. 8), where we state that "An auxiliary test reveals that there were no gender differences in legitimisation rates among firstborn children who were born out of the wedlock". Thus, we did not need to model marriage formation, because Dutch mothers of firstborn girls are just as likely to marry as Dutch mothers of firstborn boys, and they take about the same amount of time to do so. I would recommend adjusting the discussion of our work accordingly and dropping the references to the 2017 DP.

Your study is distinct in that the gender of the firstborn does appear to influence marital formation / legitimisation rates. This is interesting, but it also complicates the analysis and interpretation of the divorce findings. Consider a standard model of marriage entry, in which couples decide to enter marriage based on the quality of their match. One of the likely consequences of lower marriage rates among families with firstborn daughters is that the quality of marginal marriages (in terms of spousal match) will be higher among the families with firstborn daughters. Accordingly, these marginal marriages may well prove more stable than the marginal marriages among families with firstborn sons, because the least-stable couples with firstborn daughters did not even enter into a marriage (and hence also into the risk set of your divorce model). You should acknowledge this as one of the possible mechanisms underlying the negative divorce-risk estimate for daughters aged 0-5 and clarify that the estimate could very well arise from endogenous sorting of couples into marriage. The good news is that this sorting is unlikely to invalidate the positive effect for older daughters (if anything, it would imply that the latter effect is underestimated).

When reading the data description, I was confused by the terminology. You are splitting the sample into ‘married’ and ‘single’ subsamples, which raises the question about partnered mothers who are living in de-facto relationships. I understand that these fall into the ‘single’ category, however this is a contradiction in terms. It would be better to call the ‘single’ category ‘unmarried’, and to show how many mothers in this category are actually single, how many are cohabiting with a partner, and how many are engaged in other types of de-facto relationships. This would also be an interesting statistic from the perspective of bringing additional context and insight into the marriage formation decisions.

In Table 3.1, it is not clear whether the time-variant characteristics (employment, urbanisation, family size, satisfaction) are measured pre-birth or post-birth. This is important, because post-birth characteristics can be influenced by child’s gender, and as such they do not constitute valid outcomes for balancing tests. I will also note that the fact that ‘single’ mothers with sons are on avg. 1 year younger than those with daughters (Table 3.2.) should not be linked to your finding that mothers with firstborn sons marry faster. This would require much more complex correlation structure. For example, younger mothers would have to be less likely to marry quickly than older mothers (meaning that older mothers with sons disappear from the single sample, thus pulling the average age downwards). Single mothers with daughters dropping out of the survey seems to be a much more likely explanation, and I would encourage you to put more emphasis on this mechanism throughout the discussion of your marriage formation results. This is because selective sex-specific attrition detracts from the causal interpretation of the presented findings, and the readers should be explicitly pointed to this limitation. It should be also mentioned as a caveat of the marriage formation analysis in the concluding remarks.

Returning to the time-variant characteristics, you may have noticed that the principal model of Kabatek & Ribar (2020) does not include any time-variant socio-demographic controls. This is to ensure that the estimates are not distorted by endogenous employment/migration/fertility decisions post-birth. I would encourage you to do the same. I understand that the size of your sample may warrant some time-varying controls (to account for observed differences between the two groups), but I will note that the results would be stronger without these controls. A reasonable compromise might be to only include pre-birth values of the time-varying characteristics that you deem important to control for.

In addition, there are several minor editorial points that deserve mentioning. First, eliminate inconsistencies in the placement of footnotes (at the end of the sentence, footnotes should be placed after the fullstop). Second, some of the DP studies that you cite are already published (e.g., Cools & Patacchini’s 2019 *Labour Economics* article). You should scrap the references to the DP versions (unless the DP version contains additional material that was not eventually published). Third, the writing needs further copyediting focused on missing words and incorrect spellings. I would be happy to provide you with an annotated version of the thesis (in which I highlight some of the issues).

Altogether, I view the thesis positively, and I believe that it satisfies the formal and content requirements for a PhD thesis in economics. Certain adjustments are still needed, however I am confident that the thesis can be recommended for a defense. The comments raised in this letter should be fairly straightforward to address, and they should not require major revisions of the data or the modelling framework. The comments pertain mainly to writing, asking for expanded discussions, corrections of the cited material, clarifications of the terminology, and further attention to the limitations of your findings. Personally, I put particular emphasis on the last item. We are all aware that no analysis is perfect, and I appreciate authors who are forthright about the limitations of their studies (both when discussing the results and also in the conclusions). This is particularly pertinent to studies of highly-sensitive topics (such as marriage, divorce, and gender), where the risks of misinterpreting the results could have substantive real-life consequences. From the methodological perspective, using time-invariant / pre-birth controls is likely to strengthen the study's design further, and it is unlikely to bring about drastic changes of the presented results. I would certainly recommend switching to this model specification instead of the one that is currently favoured. However, if you find yourself strapped for time, feel free to focus on the writing instead. The model adjustment can be deferred to the point when you're preparing the study for a journal submission.

Once again, thank you for writing your thesis, and congratulations on your achievement!

I am looking forward to the defense.

Dr. Jan Kabatek