Report on the thesis "Parenting of Sons or Daughters, Household Decision Making and Family Characteristics" authored by Sergii Maksymovych

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Summary of the thesis: The thesis consists of three essays in family economics. The first essay examines the link between the mode of decision-making within households (shared d-m by both partners, dominant d-m by fathers, or dominant d-m by mothers) and the level of material deprivation in the households. In the second essay, the author examines parental preference for specific gender of children and asks whether such preference is due to (i) gender bias or (ii) differential costs of raising children. In the third essay, the author examines whether the gender of the firstborn child has an impact the probability of marriage formation and dissolution.

Overall evaluation. Let me state upfront that I believe that thesis **satisfies** formal and content requirements for a PhD thesis in economics. I **recommend** it for a defense, conditional on addressing the major comments. I believe the thesis addresses important questions that are of interest to the academic community as well as other relevant stakeholders. I believe the defendant is well oriented in the relevant literature. In my view, he uses appropriate modeling tools and econometric methods to answer his questions.

Below I summarize every chapter in greater detail and outline my major comments. I provide also more extensive lists of minor comments that take a form of non-binding recommendations.

Essay 1: Decision Making in the Household and Material Deprivation

In this first essay, the author examines the link between the mode of decision-making within households (shared decision-making by both partners, dominant decision-making by fathers, or dominant decision-making by mothers) and the level of material deprivation in the households. The author uses cross-sectional EU-SILC data for 18 countries. Apart from using female income as a proxy for female control, he also adds direct measures of decision-making into the empirical specification. To address potential reverse causality, he uses an instrumental variable strategy, based on regional-level variation. In the end, the author concludes that balanced decision-making by both partners leads to less material deprivation, while dominant decision-making by either mothers or fathers leads to higher material deprivation.

Major comments:

Inconsistencies between sections 1.1 (introduction), 1.3 (empirical strategy) and 1.4 (results) need to be removed. In particular,

- The introduction mentions three IVs for balanced decision-making, all measured at the regional level: (i) the share of 4-years-old in formal childcare, (ii) the gender gap in weekly work-hours, and (iii) the gender gap in unemployment. The discussion of the results (p.20) mentions only the first two instruments.

- The introduction on page 7 states that OLS and 2SLS estimates are in accord, but they differ significantly in magnitude, suggesting a bias toward zero in OLS. The implications of the direction of the bias need to be discussed.
- Section 1.3 mentions that the covariates include the number of daughters, length of cohabitation, hours of housework, but Table 1 does not include these variables.
- Column (2) in Table 1 does not include the interactions between the decision controls and household characteristics, as announced in section 1.3.
- Section 1.3 specifies that the main outcome is <u>a sum</u> of six binary variables, which is not correct. Page 11 mentions seven material deprivation conditions.
- The empirical model is estimated based on a cross-section of households, although the model 1.1 suggests a panel data structure (subscript j is redundant).

Policy implications of windfall transfers. The author argues that the prior literature might not be completely right when it attributes the estimated effects of windfall transfers to women on lower material deprivation to increased female control. The author asserts that the estimated effect might be due to more balanced control between partners (i.e. more female control and simultaneously more male control). I believe that one piece is missing in this argument, namely the <u>evidence of an increase in the male control</u> in areas over which the man would not have control without the financial transfer to women. Otherwise, the more balanced control due to the transfer truly is only an increase in female control.

Minor comments:

- The author could explore which particular unilateral decisions within household are associated with higher material deprivation.
- Selected IVs should be discussed upfront in the section of the empirical strategy.
- The author should provide evidence that the correlation between childcare accessibility and women's employment is not high (as claimed in footnote 21), which is key for identification.
- Specification 1.1 could account for marital status within households.
- The essay should clarify if it uses two data points per household in estimation, one for each partner. If not, which answers were selected, male or female? If two data points per household are used, the estimates and standard errors should account for it.
- Explain how same-sex couples are treated.
- Regarding writing, the main takeaway within each paragraph should come as the first, not the last sentence.
- The average value of the outcome variables should be reported directly in Table 1.
- Male and female earnings should be transformed into logarithms in Table 1.
- The section of results should start right before section 1.4.1. The text at the very beginning of section 1.4 belongs to the discussion of the empirical strategy.
- A better title for section 1.3 is empirical strategy rather than empirical specification.
- The author keeps wrongly referring to equation 1.1 as 2.1.
- Figure 1.A in the Appendix has an incorrect label A2

Essay 2: Parental Gender Preference in the Balkans and Scandinavia: Gender Bias or Differential Costs?

In the second essay, the author examines the link between the gender of first-born child and further fertility decisions in the households. Next, he asks whether the fertility decisions are driven by gender bias or differential costs of upbringing children of different genders. For estimation, he uses EU-SILC data for the Balkan and Scandinavian countries. Data about household consumption patterns help him argue that the preference for boys in the Balkan countries is driven by gender bias and the preference for girls in Scandinavian countries is driven by differential costs of upbringing children.

Major comments:

Disentangling the mechanisms conceptually. My main comment is related to the conceptual difference between the hypotheses of gender bias and differential costs of raising children. I do not believe the author currently makes the distinction sufficiently clear.

- Appendix 2A1 is supposed to bring more light into this issue, but I believe it does the opposite. For instance, the left and right panels in Figure A1 are conceptually identical, as they show that marginal utilities from boys or girls are different both in the case of gender bias and differential costs. But marginal utilities from girls and boys should be identical in the right panel if only differential costs are the main driver of the fertility decisions. My impression is that this appendix could be entirely omitted.
- I believe that the concluding section also mixes the two concepts when it states that "*lower cost of daughter quality incorporates gender-specific personal characteristics and their usefulness for parents*" (p. 61)
- I believe that readers would benefit from a summary of the theoretical predictions before the results appear in the introduction.

Disentangling the mechanisms empirically. It is obviously a challenging task to disentangle gender bias from differential costs empirically, especially without exogenous variation in costs. The author uses data about household consumption patterns to shed more light into this.

- I would be in favor of rewriting the interpretation of the main results, because I believe that the author finds mixed evidence regarding the differential cost hypothesis in Scandinavia. When I check his predictions from p. 39 I find the following: "*parents of a child of the more expensive gender should have fewer children thereafter (✓), spend less on themselves (both parents)* (X)*, spend less on adult public goods* (X)*, and spend more on children* (X)*. Moreover, parents of a "more expensive" child should report higher sums needed to make ends meet.* (X)'" This does not look like clear evidence in support of the differential costs hypothesis.
- I could not find the following evidence in the results: "*higher outlays on daughters*" in Scandinavia and that "*households with first-born daughters are more likely to have the entire set of ten important children consumption items*." (p. 60)
- The author occasionally interprets the results as a link between "the number of children in the household" and household consumption decisions, but the main specification

does not use the number of children as an explanatory variable, but rather the gender of the firstborn child. (p. 34)

- The difficulty to distinguish between gender bias and differential costs translates into the policy discussion. If one of the findings is that girls are more expensive in Scandinavian countries than boys, but otherwise there is no gender bias, then the policy-maker would like to subsidize the cost of human capital for girls, not for boys.
- I would abstain from causal language in the concluding section 2.6 when the author describes evidence about specific mechanisms of parental fertility decisions.

Acknowledgements. I understand that the second essay is written based on a co-authored working paper. This needs to be acknowledged, perhaps in a footnote below the essay's title.

Minor comments:

- The essay could defend the selection of countries within Scandinavia and Balkan. Why not use data for Romania or Finland or other countries?
- When the estimates are (not) statistically significant, the essay should indicate at which level or at least state "*at the conventional levels*". (e.g. regarding the evidence about replacing worn-out furniture)
- It is not correct to say that some estimates are "*much less statistically significant*" (p. 53).
- On page 51, the author claims to have estimated a Probit specification to check for consistency, but he might have meant to say to check robustness.
- Table 2.4 should describe clustering of the standard errors.
- Section 2.4 could be titled "Empirical strategy" rather than "Empirical analysis"
- Parity-three progression method in the second sentence of the introduction is too technical.

Essay 3: The Impact of the First-Born Child's Gender on Family Formation and Dissolution: Evidence from Russia

In the third essay, the author examines the link between the gender of the firstborn child and the probability of marriage formation and dissolution. He applies duration models to Russian longitudinal household data. In line with the existing literature, he finds that marriages are more likely to be dissolved when parents have a school-age daughter (aged 6-18). For preschool children (0-5), he finds that a first-born daughter reduces the likelihood of marriage, but quite unexpectedly it also reduces the likelihood of marriage dissolution.

Major comments:

Mechanisms. The essay discusses up to five mechanisms why first-born pre-school daughters reduce the likelihood of marriage dissolution. The mechanisms include, for instance, an argument of cumulative effects of existing policies that favor women in Russia that might dissuade parents from divorcing

- While I believe that such policies favoring females may exist and that they may have some effect, I would expect the effect for pre-school daughters to be roughly equal to the effect for school-age daughters. I believe that the auhtor should give more weight in the discussion to mechanisms that are age-specific, such as the likelihood of re-marrying after the divorce of young mothers with first-born sons compared to relatively older mothers with first-born sons.
- I find it interesting that first-born pre-school daughters reduce the probability of marriage dissolution, but they seem to have no statistically significant impact on co-habitation termination. This might be potentially informative about the underlying mechanism, because it seems that the formal dissolution of the marriage is what matters to the parents of first-born sons.
- Without a major reason, the essay introduces two of the mechanisms before presenting the results and the other three mechanisms after the results. I believe this discussion needs presented in one place.

Null hypotheses statements. The author states the null hypothesis twice and not coherently. On pages 91-92, he states H_0: Beta1_6_18 = 0 vs. H_a: Beta1_6_18 ≠ 0, and also H_0: Beta1_0_5 = 0 vs. H_a: Beta1_0_5 ≠ 0, and also H_0: Alpha1_0_5 = 0 vs. H_a: Alpha1_0_5 ≠ 0. On page 93, he writes "*I expect, in line with previous studies, the coefficient Beta1_6_18 to have a positive value and the coefficient Alpha1_0_5 to have a negative value. At the same time, I expect first-born daughters aged 0-5 years to have a negative impact on family dissolution, i.e. for Beta_1_0_5 to be negative.*" I believe the hypothesis statements should be unified. Alternatively, the hypotheses statements on page 93 could be dropped.

Minor comments:

- The concluding section mentions that son preference has not been established for Russian context yet, but the author has all the data to test it. (page 98)
- The introduction refers to two mechanisms behind the estimated effects of first-born gender on marriage dissolution without introducing the mechanisms upfront. (page 81)
- The discussion that the estimates might have larger magnitude in Russia than in other countries (page 80) belongs to the discussion of the external validity in the conclusions, rather than to the introduction.
- Tables 3.4, 3.5 and 3.7 should report the baseline probabilities of marriage formation, dissolution and cohabitation termination, respectively.
- The author keeps referring to Tables 3.4, 3.5 and 3.7 as Tables 4, 5 and 7.