External Review for CERGE-EI PhD Dissertation by Geghetsik Afunts

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Summary and Comments

Thank you for inviting me to read this fascinating dissertation written for the degree of PhD at CERGE-EI.

The dissertation is broadly in the theme of applied economics: two chapters cover questions within family economics on divorce and fertility, while the third chapter studies a question on inflation expectations. These are exciting questions to be working on.

The first chapter explores how the introduction of Unilateral Divorce Laws in the US impacted the education structure of new marriages and divorces. In this context, education structure means the relative education of the husband vs wife: whether the match is hypergamous (H>W), hypogamous (W>H), or homogamous (H=W). The chapter builds on an existing literature looking at the overall impact of UDL by considering whether these impacts might have differed for these various marriage types. The findings show that while UDL did not change the structure of marriage inflows, it did reduce the relative likelihood of divorce of H=W and W>H couples. These two groups were previously most likely to divorce, so it improved their stability to the extent that H=W couples became the *most* stable type after the law changes. Indicative evidence suggests that the age gap decreased among these two groups, suggesting that they perhaps were better matched.

I do not find any substantive changes that need to be made in order for this chapter to be presented for defence. However, for future work on this chapter, I have a few suggestions:

> *While no evidence is found of a change in inflows by education group, I wonder whether the evidence is consistent with a change in selection among these various groups. Over time, and independent of UDL, there has been a rise in the educational attainment of women relative to men, and this has driven a trend of rising W>H and W=H couples (seen in the non-interacted coefficients in Table 1.2). This tells me that these two groups are becoming less select over time, while H>W couples are becoming more select. If women are now more educated, it's relatively easier to form a W>H match, and relatively harder to form a W<H match. This type of selection could explain the UDL effect on divorces of these types: the ones becoming more select (H>W) are now less stable and more likely to take advantage of the easier divorce law when it takes effect. Is there a way to explore (perhaps descriptively) the trend over time in the characteristics of the three groups, to shed some light on increasing or decreasing selection within groups? Perhaps some survey data could help here.*
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> *The structure of the chapter could be improved ahead of attempting publication in a journal. Specifically, the data and empirical approach go back-and-forth; for example, p. 12 discusses a result (3.3 log odds) using a method that is discussed in the next section. It shouldn't be necessary to jump forward to a method, then jump back to the result. I think the whole*

section could be presented in a much more linear way: data (description of the data structure); descriptive statistics (presenting basic summary stats of the dataset used – e.g. I was missing a number telling me the number of marriage/divorce certificates by education group, which tells me what sample size the estimates in Figure 1.1 are based on); methodological approach showing the log odds approach, as well as estimating equation for inflows and outflows presented in later sections; basic results on homogamy as in Fig 1.1, log odds etc; main results on inflows and outflows. Basically, once you get to the results, all of the method should have been explained already.

I was missing an explanation for Figure 1.2 i.e. what regression equation it is based on, what we learn – only one sentence at bottom of p.15. If it's important it needs more text space, if not then should be removed.

p.24 the last sentence "this is responsible for much of the reduction in stability gaps" feels somewhat too strong, given the results. We can see simultaneously that the age gap reduced, and that stability increased – but not that one caused the other, or indeed that one explains a large part of the variation in the other. Another challenging aspect of this part of the analysis is that the age gap between W>H and W=H is already smaller at baseline than for H>W (seen from the uninteracted coefficients in Table 1.5). If age difference is important for stability, then these two groups should have been more stable at baseline than H>W, except they were not.

The second chapter addresses the following question: did the introduction of Joint Custody Laws in the US affect the educational homogamy of parents? Specifically, a literature has shown that educationally homogamous parents are more likely to have children. Joint Custody Laws increased the likelihood of co-parenting, and so may have increased the fertility differential of homogamous and non-homogamous parents (the latter may have found it more costly to make childcare arrangements with their partner should a divorce occur). The empirical results suggest this is the case: using the staggered introduction of the JCLs, births were more likely in states with high homogamy and a JCL law in place.

I do not find any substantive changes that need to be made in order for this chapter to be presented for defence. My main concern with this chapter is that JCL laws may have not been rolled out randomly. For future work on this chapter, I have a few comments:

The fact that existing work (Halla 2013) shows that JCLs increased fertility suggests that these laws may have actually reduced fertility costs, at least for some sections of the population. I wonder how this can be rationalised with the results on homogamy? Specifically, for those couples who have a (hypothetical) divorce probability of zero, the JCL laws should have no effect. The argument is that homogamous couples are more stable, so why did the JCL laws affect their fertility? On the other hand, JCL laws most affected unstable couples as the probability of reaching the divorce state was higher. These couples should have then reduced their fertility. So my question is, how can we rationalise the overall positive fertility impact in Halla (2013) with the idea that JCLs increased negotiation costs for non-homogamous couples, which would imply no change in fertility for homogamous couples and a decline in fertility for non-homogamous couples?

I would also like to see some discussion of the potential endogeneity of the JCL laws. Were they more likely to be implemented in more homogamous states? If yes, does this bias your

estimates? A first step to exploring this would be to run a descriptive state-year level regression, regressing the time varying implementation of JCLs on a variety of time-varying state characteristics such as FLFP, fertility, etc, and also homogamy over time.

It was not entirely clear to me which regression equation is estimated in Tables 2.1 and 2.2, and what the dependent variable is. If the birth coverage is not 100% in all states, are you using weights?

Chapter 3 tackles a question in a somewhat different field: the role of the war in Ukraine on inflation expectations. Using the fact that the start of the war was unanticipated, and survey data from Germany, the paper shows that individuals' inflation expectations rose immediately with the start of the war. This may be because individuals expected higher energy prices, or because poor economic outcomes tend to be associated (by individuals) with high unemployment and inflation.

I do not find any substantive changes that need to be made in order for this chapter to be presented for defence. However, for future work on this chapter, I have a few suggestions:

Is there anything in particular that happened on 1st March? In Figure 3.3, this seems to be the first day when inflation expectations are significantly higher than they were previously.

It would be interesting to explore whether individuals' expectations over unemployment, growth or fuel prices tend to be quantitatively correct, for example by using previous waves of the survey and actual data on these outcomes. Is the % increase in these expectations after the start of the war quantitively accurate, given the actual increase in fuel prices etc. that followed? Are the expectations "correct"?

Recommendation

Overall, I find this dissertation covers fascinating questions. The empirical methods are up-to-date and well-executed. The dissertation is well written. Congratulations to the student on these three chapters. I recommend that this dissertation is presented for defence, and I have no substantive comments that need to be incorporated ahead of the defence.