

Comments on: “Gendered Economic Policy Making: The Case of Public Expenditures on Family Allowances”

This paper explores whether greater female representation, or a critical mass of women, in parliament results in higher expenditure on family allowance across OECD countries. The author finds that lower chambers with female representation at or above 30% result in a significantly greater fraction of public spending on family allowance. The author suggests these results are consistent with the Citizen-Candidate Model (as opposed to the Median Voter Theorem), citing that women prefer family oriented policies and that electoral winners implement their preferred policy under the Citizen-Candidate Model.

This paper is interesting and it has some key elements which can contribute to the literature. In particular, an exploration of the critical mass hypothesis is useful. I also liked the exploration of alternative samples and covariates. Indeed, I believe the paper could be improved with a deeper focus on these issues. Specifically, the author could further investigate the level of critical mass, consider interacting this key variable with fractionalization, and provide more discussion/exploration of the dependent variable (public spending on family allowance).

Primary Comments:

1. Because the author aims to investigate the effect of a critical mass of female parliamentarians on public spending for family allowance, it might be useful to spend some time motivating either why this policy is important to look at in and of itself, or (more importantly) why this policy might be particularly well supported by women -- over and above other policies that women favor. Consider: if the dependent variable is public spending on family allowance (as a fraction of either GDP or of total government expenditure), this variable may actually decrease if funds are spent on other policy initiatives that are strongly supported by women (social welfare, health, maternal/paternal benefits, education).

2. If the author's goal is to provide evidence for one theory over another (Citizen-Candidate versus Median Voter) I would have liked to see a clearer treatment of the model comparison. Specifically, are there any cases (demographic structures) under which both models would generate similar predictions? If this comparison is not a primary concern of the paper, the discussion could be effectively cut back.

3. The author might consider a one year lag in the measure of female parliamentary representation, since policy changes can take some time to implement. That said, an increase in spending on a pre-existing policy might be quicker than the introduction of a new family allowance.

Did any (many) of the OECD countries ever have zero spending on family allowance? If so, it might be interesting to explore whether a critical mass of female parliamentarians was associated with the introduction of such a policy and/or with the type of allowance provided (i.e. do some countries have means tested cash benefits while others have tax rebates?) It would also be

helpful to have a more detailed description of the family allowance variable (what policies it encompasses) and how it may vary across country.

4. The paper would benefit from a greater exploration of the key explanatory variable (female parliamentary representation). I was curious as to why the author stopped at the 30% threshold? Does the theory suggest that women have no effect prior to the threshold and then a constant effect thereafter? Are results similar if the threshold is 40%? What proportion of the sample is represented in each threshold? (This would be helpful to see in table 1).

It might also be interesting to test an interaction of the fractionalization variable with your threshold dummy (or with the fraction of female parliamentarians). Your results suggest that greater fractionalization, while it may theoretically lead to higher public spending, does not increase spending on family allowance. However, one might expect that the impact of fractionalization on women favored spending would be seen only in fractionalization parliaments with a critical female mass.

Another consideration is that if strategic alliances are more important in a fractionalized parliament, such parliaments may have a lower critical level at which minority groups can influence policy. It would be interesting to explore this hypothesis.

Minor Comments:

1. The description of the empirical model and techniques is a bit long, and occasionally contradicts itself. For example, on page 9, equation (1) suggests that the author will estimate a model with country and year fixed effects (consistent with the notes below Table 2). However, the author then states that country-specific time trends are used. Perhaps these were included in an alternative specification as a robustness check which the author then omitted?

2. The author might consider reducing other parts of the estimation methods discussion, some of which are straight forward and don't require elaboration/presentation (inconsistency of pooled OLS p.13), and some of which are never presented in the paper anyway (fixed effects with lagged dependent variable p.10).

3. Finally, though it reads well in general, the paper could be improved with the correction of grammatical errors, and with a bit more care over word choice, for example the cases of "prove" (p.2) or "unique" (p.9).