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Does the Gender of Offspring Affect Parental Political Orientation?
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ABSTRACT

Recently, the sex of child has been widely used as a natural experiment and shown to induce change of the allegedly stable political predisposition, however, prior results have been contradictory: in the U.K., researchers found that having daughters leads to parents favoring left-wing political parties and to holding more liberal views on family/gender roles, whereas in the U.S. scholars found that daughters were associated with more Republican (rightist) party identification and more conservative views on teen sexuality. Here, we utilize data from the General Social Survey and the European Social Survey to test the robustness of effects of offspring sex on parental political orientation while factoring out country and period differences. In analysis of 36 countries, we obtain null effects of the sex of the first child on party identification as well as on political ideology. Further, we observe no evidence of heterogeneous treatment effects. We discuss the implications of these null findings for theories of political socialization.

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Background

Familial clustering of various traits is one of the most commonly observed patterns in the social sciences; within families, husbands and wives tend to have similar educational backgrounds (Breen and Salazar 2011; Domingue et al. 2014; Mare 1991), parents and children display similar social attitudes (Glass, Bengtson, and Dunham 1986), and siblings share many healthy/deviant behaviors (Haynie and McHugh 2003). In particular, intergenerational inheritance (e.g., parent-child similarity) of social, health and economic status has garnered much attention since it forms the basis of social stratification or reproduction (Conley and Bennett 2000; Corak 2013; Hauser and Grusky 1988; Simons et al. 1991). While high parent-child correlations have been observed for a number of traits ranging from political attitudes (Jennings, Stoker, and Bowers 2009; Knoke 1972) to occupational status (Hauser and Grusky 1988) to health behaviors such as smoking (Chassin et al. 1998), social scientists have made little headway in understanding how these intergenerational associations come about. Absent natural experiments, it is difficult to ascertain the key causal factor, or sometimes even the direction of influence, while ruling out unobserved heterogeneities such as shared genetic endowments.

Political predisposition -- party identification or political ideology -- is not an exception[1]: family (e.g., parent-child relationships) has been regarded as one of the most influential aspects of socialization into political beliefs and behaviors (Alford et al. 2011; Jennings and Niemi 1968; Jennings, Stoker, and Bowers 2009; Jennings and Stoker 2001). For example, Jennings and Niemi (1968) found substantial agreement of party identification between parents and offspring in a 1965 sample. A follow-up study in 1997 by Jennings et al. (2009) confirmed the patterns of political reproduction were robust across cohorts and further discovered the stability of transmitted partisan orientation in adulthood, especially among children whose initial parent/child correspondence was higher (i.e. a non-linear effect).

[1] While party identification and political ideology might be conceptualized and measured in different ways (i.e. Liberal-Republicans and Conservative-Democrats are possible, though rare), we treat them as two of core components of political orientation in this paper as the distinction itself is not of central interest to our study.

They argue that their results provide support for an intergenerational transmission model, in which views political values are directly transmitted from parents to child via social influence and learning during early childhood. Other results regarding the intergenerational correlation of political ideology in five European countries could also be read to support this direct transmission model (Jennings 1984). Nevertheless, these studies cannot completely rule out the possibility of genetic influence or indirect influence through shared environment such as neighborhood context (Hatemi and McDermott 2012; MacDonald and Franko 2008; Settle, Dawes, and Fowler 2009; Smith et al. 2012).

Ideally, we might want to randomly make some parents become Republicans (or conservative) to see whether their children are more likely to be Republicans as compared to those of control parents who were assigned to being Democrats (or left-leaning). While this kind of experiment is impossible, the sex[2] of the (first) child provides a unique natural experiment to examine the possibility that part of the observed correlation is due to the fact that children socialize their parents (rather than the other way around). Given the absence of prenatal sex-selection, the sex of child should be random, and consequently the first order effect of the sex of child on outcomes would be unbiased by unobserved factors (such as genetics).

Namely, Warner (1991) and Warner and Steel (1999) have argued that parental active engagement to address the barriers that their daughters would face leads to changes in parents' political views and/or civic behavior favorable to their daughters. This dynamic may not lead to a shift in underlying political value distributions but rather a change in the rational interests of the parents, which in turns manifests as "novel" political attitudes and/or behaviors. It is also possible, however, that the sex of offspring does shift those underlying values through social exposure. Namely, as women have become more liberal (or Democratic) than men (Inglehart and Norris 2000; Norrander and Wilcox 2008),

[2] In this study we use gender and sex interchangeably for reasons of flow and rhythm in the prose; however, we are quite aware that these are two distinct concepts.

having a daughter instead of a son can be considered as akin to having a new liberal member randomly arriving in the family. That said, given the difficulty of estimating causal effects of network influence because of the endogeneity and reflection problems (Manski 1993), we cannot adjudicate between these two possibilities. Either way, however, effects of offspring sex on parental political attitudes and partisanship would certainly flip the usual view of social influence within the family as well as the notion that political identities are fixed early in development and robust to novel social stimuli.

We are not the first researchers to deploy this exogenous sex of offspring strategy; however, we believe that our study improves on the shortcomings of those studies that have preceded this one. For example, Washington (2008) found that a Congressperson's propensity to vote liberally – with respect to reproductive right issues in particular – was augmented by having additional daughters. Glynn and Sen (2014) found a similar effect in regards to decision making among U.S. Courts of Appeals judges: judges with daughters more often voted in gender-related cases in feminist directions, though when they extended this analysis to *all* issues they did not find that daughters significantly increased liberality within the judiciary they studied. While these studies are intriguing, the study of political elites such as judges and legislators introduces the possibility that left leaning districts or constituencies are more likely to elect (or appoint) individuals with daughters while right leaning ones prefer their elected (or appointed) officials to have sons. This would be interesting in its own right but ruins the presumptive exogeneity of the offspring sex.

That said, other recent studies using sex as a natural experiment have shown a significant effects of offspring or sibling sex mix on party identification or political ideology even among the general population. For instance, in the U.K., Oswald and Powdthavee (2010) found that having daughters led parents to favor left-wing political parties and to hold more liberal views on family gender roles.[3] Likewise, Shafer and Malhotra (2011) found that having a daughter reduced men's support for traditional gender roles (but not women's) based on analysis of the National Longitudinal Study of Youth (NLSY 97). However, these results were contradicted by other research: in the United States, Conley and Rauscher (2013) found that daughters were related to more Republican (rightist) party identification and more conservative views on teen sexuality; likewise, Healy and Malhotra (2013) found that having sisters caused men (but not women) to be more likely to identify as Republicans. Conley and Rauscher (2013) suggest this effect may be an evolutionarily rational reaction, while Healy and Malhotra (2013) emphasize the gender stereotyping of the childhood environment as a putative casual mechanism.

Even these studies that focus on non-select populations suffer from limitations. For instance, with the exception of Conley and Rauscher (2013) and Oswald and Powdthavee (2010) each of them considers the number of *all* children to be female. Thus, they introduce the possibility that liberal or conservative individuals may have different parity progression biases when it comes to the sex of offspring. In other words, it is possible that conservatives and liberals (with respect to feminist issues at least) may have different stopping rules when they have or have not achieved a certain representation of sons or daughters among their offspring. This would reverse the causal arrow. (In fact, Oswald and Powdthavee [2010] obtain insignificant results when they confine themselves to an examination of the first born child only, which provides a solution to this problem.) Likewise, except for Conley and Rauscher (2013) and Oswald and Powdthavee (2010), they do not distinguish between biological offspring and step or adopted children. Marrying into a family with pre-existing boys or girls or choosing to adopt a boy or girl oneself is certainly not an event that is plausibly exogenous to one's political ideology or partisan alignment.

Despite these limitations and sometimes contradictory valences, if any of these studies are

[3] Likewise, Urbatsch (2011) found that having older sisters led to a more liberal ideology in the U.S..

correct, such results not only challenge researchers who treat political predisposition as a right hand side (i.e. causal) variable on other political outcomes[4] but also call for revision and extension of political socialization theory wherein children are merely perceived as objects of parental socialization who develop political predispositions through a one-way learning process. Then, given that all studies can be regarded as the same type of natural experiment (though with potentially critical differences in specification), these significant-but-mixed results set up an interesting puzzle: why do they obtain opposite effects?

One plausible explanation is country or period heterogeneity (Sapiro 2004); that is, it might be the case that the differential barriers that sons and daughters face and/or the gender split in political ideology vary across countries and periods. For example, in a country without gender inequality or gender differences in political partisanship parents do not need to change their political identity when they have a son or daughter to maximize gender-specific policies to their liking nor do they become systematically socialized by their children's genders since there may not be any political differences by gender in adults or children. Alternatively, it might be simply the case that significant effects are obtained by chance (i.e. Type I error) and/or publication bias (we do not observe null effects in the literature because they do not make it to press).

To address these possibilities, we revisit the issue using independent samples to see whether effects of offspring sex on parental political orientation are robust (or versatile) across periods and countries and to test for the presence of systematically heterogeneous treatment effects. Multilevel analysis as well as period and country fixed effect models based on cross-sectional samples are

[4] The so-called Michigan school defined party identification as "an individual's affective orientation to an important group-object in his environment" (Campbell et al. 1960: 121), implying that party identification is an "unmoved mover," an affective attachment to a group, which formed young, persists throughout the course of life cycle, and is relatively invariant to the external shocks (Johnston 2006). By contrast, the so-called "revisionist perspective" considers party identification as "running tally" of citizen evaluations of others' political attitudes and events (Achen 2002; Fiorina 2002). However, Green and colleagues show that the revisionists' evidence is likely an artifact of measurement error (Donald Philip Green and Palmquist 1990; Schickler and Green 1997).

employed to verify the direction and potential significance of an effect of the sex of the first born offspring on both political ideology and partisan identification. To preview our results, we report null findings throughout all analysis, find no heterogeneous treatment effects by subgroup, and thus question the robustness of prior findings.

Data and Methods

The 1972-2012 cross-sectional samples of the General Social Survey (GSS) and the 2002-2012 repeated cross section of the European Social Survey (ESS) provide a unique opportunity to investigate the causal effects of sex of children on parent's political views across social contexts.[5] While the 1994 GSS sample used by Conley and Rauscher (2013) collects information on all of the respondents' kids, it is of limited scope both in terms of sample size (N=1,051) and period. By contrast, while the entire GSS/ESS samples lack a complete fertility history, they do, however, contain information about all children currently residing in the parental household, based on which we can infer the sex of the first child in the household by contrasting the household census against the parental report of total number of offspring. In the GSS, each household member's relationship to the household head as evinced by the household roster can be cross-referenced to the respondent's relationship to the household head to infer whether each household member is a child of the respondent by excluding respondents who were not the household head.[6] It was not necessary to apply this filter to the ESS, since the ESS assessed the relationship of each household member with respondent directly. After detecting all children in the household, we excluded respondents with no kids listed in their household roster. We additionally excluded cases with missing values for the age or gender of any children as well as the cases of which two

[5] Both datasets are publicly available and can be downloaded here: http://www.europeansocialsurvey.org and http://www3.norc.org/GSS+Website/

[6] If the survey respondent is a spouse, we excluded these cases to prevent the possibility that spouse is not the biological mother or father to the children of household head.

oldest children are the same age.[7]

The treatment of interest under this study is the first child's sex, which should be random given the absence of prenatal sex-selection and gender-specific parity progression bias.[8] However, in contrast to the 1994 GSS sample used by Conley and Rauscher (2013), in our GSS multi-year sample and the ESS sample we must address the potential concern that kids do leave their house for different reasons at different times if we consider only children who live with their parent(s), although this criticism also could apply to the previous studies using the same strategy (i.e.,Oswald and Powdthavee 2010). To minimize this problem, we restrict our sample to parents whose oldest child is younger than 17 and thus likely to be living at home and be captured in the household roster.[9] Table 1 summarizes the sample selection criteria; as a result of our analytic sample filters, in the GSS, 5,571 observations (9.8%) remain out of a total potential N of 57,061 and roughly 20 % of cases remain in the ESS. Table S1 in Appendix reports the variables' wording and coding in the GSS and ESS, respectively.

[Table 1 about here]

Dependent variables are intended to capture parents' political orientations. In the first part of our replication analysis, we adopted the same strategy as previous studies by measuring party identification. We deploy two binary indicators for right-conservative (or Republican) and left-liberal (or Democratic) party identification and one continuous scale for Republican (or right-conservative) party identification. While survey instruments for measuring party identification differ between GSS and ESS and thus might

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[7] They can be either a twin set, two consecutive births within twelve months of each other, or simply the result of measurement error.

[8] Contrary to previous studies, we do not use the number of daughters after conditioning on the total number of children as an instrument because it is only conditionally random unless there is no family-specific parity stopping rule. If there are unobserved factors (i.e. son or daughter preference) that are related to the political orientation as well as the total number of children, then the number or proportion of daughter is no longer random. We do not rely on this assumption because it is fundamentally untestable, though using the proportion of daughters gives the same results (the results are not reported here, but available upon request).

[9] In the GSS, the total number of kids is asked, and thus we additionally excluded those who report different values for the numbers of all kids and those currently cohabiting.

not be comparable, these instruments are the same ones used in previous analyses (Oswald and Powdthavee 2010)[10] except for the one continuous variable for right party index in the ESS, which is made by the combination of partisan attachment and party identification. In the second part of our analysis, political ideology is the outcome of interest. However, due to the different scales in GSS and ESS measuring the strength of political ideology, we transformed the variable into the percentage index from -100% liberal to 100% conservative by centering and dividing it by its range.

As for control variables, we do not expect the inclusion/exclusion of covariates to alter the coefficients of estimates for the offspring sex effect if the sex of the eldest offspring is random (i.e. orthogonal to other such factors). While it is reasonable to assume that children do not leave home until the age of 16 (White 1994) and thus the eldest child's sex is assumed to be random assignment within the analytic sample consisting of the cases whose eldest child is younger than 17 and living at home, we also show the estimates after accounting for several covariates to adjust remaining possible imbalances or other omitted confounders. For example, it is possible that son preference would make fathers more likely to live together with an eldest son than with a first born daughter(Dahl and Moretti 2008; Morgan, Lye, and Condran 1988) ; or it could be the case that children of young parents (especially for mothers) might leave home early (Murphy and Wang 1998). It is also possible that non-native born families follow different rules for the timing of the offspring moving out of the parental home. Thus, parent's age, sex, an indicator for native born status, and age of the oldest child are included as pre-treatment covariates, which could not be affected by our treatment variables. Additionally, we further adjust the variables such as number of children, household size, years of completed formal schooling, marital status,

[10] For the UK case, the Conservative party is classified as Right-republican party, while the Labour and Liberal Democrat parties as Left-liberal party. Due to the difficulty of classification of other parties on a unidimensional political left-right scale, individual voters for other political parties are excluded in the analysis. Among 13,403, 50.29% (6,741) respond that they do not feel closer to any particular political party and 2.13% (285) respond 'don't know/no answer.' Among party identifiers, 2,077 (32.57%) are classified as Conservative and 2,731 (42.83%) and 826 (12.95%) are respectively identified as Labour Party and Liberal Democrat, and 743 people (11.65%) who feel closer to other parties including Green Party and Scottish National Party are dropped.

labor force participation, and religion. However, these controls also could be post-treatment variables that can be affected by the first child's sex, which therefore should not be controlled for due to the possibility of un-blocking the back-door path or masking the treatment effect.[11] While it is possible that including the post-treatment variables would produce biased estimates, we will show the robustness of our findings against the inclusion of post-treatment variables, which can be a source of omitted variable biases.

Country heterogeneity

To address the potential for country-specific heterogeneity,[12] we deploy a multilevel regression model (or mixed-effects model). The following model, with country-specific intercepts and slopes can be used to measure the variance of country-specific treatment effects after accounting for pre-treatment covariates.

$$y_{ij} = (\alpha + \boldsymbol{\zeta_{1j}}) + \boldsymbol{\beta_k X_{ik}} + (\theta + \boldsymbol{\zeta_{2j}})T_{ij} + \epsilon_{ij}$$

In this model, y_{ij} is political ideology and T_{ij} is the treatment of interest for each individual *i* in country *j*. We are primary interested in estimating two parameters; θ is the fixed coefficient for the average treatment effect and $\boldsymbol{\zeta_{2j}}$ is the vector of random coefficients modeling the interactions between country level indicators and the treatment variable (i.e. $\boldsymbol{X_{ik}}$ includes pretreatment covariates and survey year dummies and $\boldsymbol{\zeta_{1j}}$ refers to the random intercept). To test whether treatment effects significantly vary across countries, we estimate $\mathrm{Var}(\boldsymbol{\zeta_{2j}})$ and test the null hypothesis of zero variance of the random slope using the likelihood-ratio test (Rabe-Hesketh and Skrondal 2012, 197).

[11] For example, since daughters might increase the risk of marital disruption more than sons (Morgan, Lye, and Condran 1988), which in turn could affect the change of parents' political views, controlling for current marital status can mask the total effect of child's gender.

[12] Because the number of available periods in the ESS is small (six at maximum), we cannot get reliable estimates for period as level-2 unit and thus we focus on country heterogeneity in this analysis.

Identification and Inference Issues

While we believe that the sex of the first child should be random given the absence of prenatal sex-selection, non-differentiation between biological and adopted/step kids might raise identification problems. To address this potential concern, Table 2 reported the results from regressing of pre/post covariates on the sex of the first child. The parent's sex, age, and the country of birth and generational age gap show no significant relationships with the sex of the first child in either the GSS or ESS samples, which supports the validity of our identification strategy. When it comes to post-treatment variables such as years of education (since child sex may affect the educational continuation decisions of parents), labor force status, and religion (to the extent that this is a fluid identification), they are statistically significantly associated with the first child's sex. Thus, we cannot know whether parental education or religion is being affected by child sex or whether they are, by contrast, affecting the sex of offspring. As the countries in ESS and the U.S. are not known to engage in antenatal sex-selection, the sex of the first child should be exogenous to any particular political tendency. Nevertheless, to account for the remaining imbalance that may be due to adopted or step children, we include post-treatment variables as well in our statistical models; results are indifferent to their inclusion or exclusion and we show both in the tables.

[Table 2 is about here]

The external validity of our estimates may be another potential concern. To increase internal validity (i.e. make sure the full census of offspring are measured), only those who have at least one (eldest) child who is younger than 17 are included in our analytic sample. We may be identifying effects that are not generalizable to all families (i.e. most notably, to those with eldest children ages 17 or over). This is an important limitation since it may be the case that party identification is only affected by offspring gender while the children are minors residing the parental home and that when they set up their own households and contact is reduced, parents revert to the "pre-treatment" political ideology.

[Figure 1 is about here]

Figure 1 illustrates the mean difference between the original sample and the analytic sample expressed in standard deviation units of the original sample (also see Table S2 in Appendix). While there are no discernible differences for our major outcome and treatment variables (less than 0.2 SD different), the analytic samples consist of parents who have more children and thus a larger size of household (which is self-evident because we exclude those who have no children) and whose eldest child is young and, by extension, who are young themselves (because we limit the sample to those whose oldest child is younger than 17), more educated, currently married and currently working. The notable difference between the ESS and GSS samples is with respect to the gender of parent; in the U.S. men are slightly more likely to be in the sample as compared to the U.K. sample or the ESS more generally. This is mainly because we only include the household head. If the sex of child is random within any particular group, then the in-sample estimates can be identified at least as local causal estimates.

Results

Party Identification

Table 3, below, reports the replication results from estimating the effects of daughters on party identification. In the U.K., daughters tend to make their parents closer to right-conservative (Conservative) party; if parents have a girl for their first child, they are more likely to lean toward right-conservative (Conservative) party. However, the size of effects of the first child's sex is quite small: a first born girl leads to a 2.5 percentage point increase in Conservative party identification, and the effects are insignificant. That said, these effects are statistically indistinguishable from zero. Though these insignificant effects may merely reflect the smaller sample size relative to Oswald and Powdthavee's (2010) sample, it is notable that they are in the opposite direction of those authors' results. This contradictory direction of effect may reflect period differences; the British Household Panel Survey (BHPS) that they used covers the period from 1991 to 2005, whereas the European Social Survey spans the years 2002 to 2012. When we restrict our analysis to the overlapping period only in order to account for potential period difference (2002-2004; see Table S3), we find that our opposite effect holds and, in fact, becomes larger. So at the very least, the bottom line is that we failed to replicate their findings in an independent sample.

[Table 3 and Figure 2 are about here]

Likewise, the effects in the U.S. are substantially smaller than the previous report by Conley and Rauscher (2013) and are also insignificant in our larger sample of pooled waves of the GSS. We examine the possibility of the period heterogeneity in Figure 2 based on our constructed GSS sample. Despite the small sample size in each period, the daughter's effect in 1994, when Conley and Rauscher (2013) found the substantial and significant effect, is statistically significant with the same direction. However, we also find significant effects of the first child's sex in 2002 and 2004; however, these effects work in the opposite direction as those in 1994. While periods when the Presidency is held by a Democrat tend to show the negative coefficients in general (and vice versa), we cannot identify a statistically discernible patterns across periods for the U.S. case. In general, we find statistically null effects of offspring sex on party identification both in the US and the UK.

To assess the possibility of heterogeneous treatment effects, we test the interaction effects with pre-treatment variables including parent's sex, age, an indicator for native-born, and the age of oldest child. For example, previous studies showed the effects of daughters were stronger or only significant to fathers (Glynn and Sen 2014; Healy and Malhotra 2013; Shafer and Malhotra 2011), which indicates the possibility that mother may not be affected by her daughter because she has already grown up with a feminine point of view. Table 4 and Figure S1 in Appendix show the general absence of significant interaction effects. This evidence evinces the possibility that previous significant findings might have been obtained by chance[13].

[Table 4 is about here]

Political Ideology

Next we turn to political ideology. The effects of the first child's sex in the 1994 GSS sample are reported in Table 5. It shows the direction of effects is with what we would expect from the previous study; daughters make their parents more conservative in the U.S. Nevertheless, the effect in the U.S. might reflect the peculiarity of 1994 sample as we demonstrated in Figure 2 or small sample sizes. These period and sample size concerns thus lead us to exploit the bigger sample size that the pooled GSS and ESS datasets provide.

[Table 5 and 6 are about here]

Table 6 reports results from analyses of these pooled data. The effects of the first sex of child are not only insignificant, but also the point estimates are themselves much closer to zero (e.g., 5.994 vs 1.047 in GSS). How small are these effects if they were significant? Having an eldest daughter instead of an eldest son would lead to a one percentage point increase in right leaning political ideology score in the U.S. and 0.3 percentage point decrease in the ESS sample. But, of course, these estimates are not statistically discernable from the null of zero effect.

[13] Due to the potential concern for multiple hypothesis testing, misspecification of functional forms and the curse of dimensionality when estimating heterogeneous treatment effects, we also employed the modified version of the Bayesian Additive Regression Trees (BART) model developed by Green and Kern (2012), which reduces researcher discretion by automating the detection of nonlinear relationships and interactions (for technical details, please see Hill (2011) and Chipman, George, and McCulloch (2010)). Figure S1 in Appendix reports the results from estimating heterogeneous treatment effects interacting with pre-treatment covariates and provides the supportive evidence for the lack of significant heterogeneous treatment effects; that is, effects across subgroups rarely change and 95% posterior intervals always include zero.

[Table 7 is about here]

As with the case of party identification, we test for heterogeneous treatment effects by interacting pre-treatment variables including parent's sex, age, an indicator for native-born, and the age of oldest child. Table 7 and Figure S2 in the Appendix report the general absence of significant interaction effects for these models. One rare exception is those who were born in the U.S are more likely to change their political ideology toward conservative by having a daughter, but the issues of multiple hypothesis testing would make the finding less convincing.

[Figure 3 is about here]

To examine the possibility of country-specific heterogeneity, we estimate the effects of the first child's sex on the political ideology in each ESS country. Figure 3 summarizes three estimates by estimating models that include no controls, pre-treatment controls, post-treatment controls. (Countries are ordered by their sample sizes.) Null effects are observed in almost all countries except for Italy, Latvia, and Croatia for which sample sizes are small and thus sampling variabilities are large. Since the effect size appears to be strangely large given the small sample size in those countries, the significant effects might simply result from the aberrations specific to those time periods in those countries. And, of course, none of these survive a Bonferroni correction for multiple hypothesis testing. (The presence of thirty-six countries in the analysis means that the proper p-value cut-off should be $p<.0014$.)

[Table 8 is about here]

Table 8 shows the results from testing the zero variance of random coefficients among 35 countries (except for the United States) using multilevel analysis. The coefficient estimates for the first child's sex do not differ from the coefficients of the country-fixed effect model and do not change irrespective of the choice between random intercept and/or random coefficient models. The log-likelihood ratio test under the null hypothesis of zero variance of random slope variances (0.32 SD) shows that we cannot reject the null hypothesis (p-value =0.58). This confirms the null patterns in Figure 3—namely, that the treatment effects across countries do not statistically vary[14].

Discussion

Despite the allegedly stable nature of party identification (especially during adulthood), two recent studies showed that the sex of child may induce a change in parental party identification. Namely, Oswald and Powdthavee (2010) found that daughters tend to make their parents alter their party identification toward left-liberal in the U.K., whereas Conley and Raushcer (2013) found the opposite in U.S. data. We re-examined these results using the GSS and ESS and found no statistical discernible effect of the first child's sex on party identification in either the U.S. or the U.K. – where significant effects on party identification were reported – nor on parental political ideology in 36 countries. Further, evidence for heterogeneous treatment effects across differences parental gender, native-born status and age, as well as the oldest child's age, turned out to be weak. As we have shown above, the prior findings were more likely discovered due to period heterogeneity than because of country heterogeneity. Eliminating the possibility of country-level heterogeneity is supported by the same null findings we obtain by adopting the same analytic strategy and using Korean General Social Survey (see Table S4 in Appendix) -- though we would not interpret models for Korean evidence as causal estimates as in the case of Europe and the U.S. due to the knowingly greater prevalence of prenatal sex-selection in Korea. Moreover, as Figure S3 in Appendix shows, the significant associations between the sex of the first child and the parent's political ideology or party identification can be obtained by chance

[14] As a fishing expedition to confirm that we were not missing any effects, we test whether the effects of daughters on political ideology (i.e. the country-specific point estimate) are significantly related to various plausible country factors. For instance, it is reasonable to suspect a link between the level of women's rights in each country and the strength of the daughter effect. To address this possibility, we relate country level coefficient estimates to 32 country level factors from the ESS MD-Country level data. Results (are not shown, but available upon request) confirm that the effect of the first child's sex at country level is not significantly related to any country level factor.

(especially in 2009), and thus researchers might mistakenly conclude that having a daughter would make Korean parents liberal if they only looked at 2009 sample.

However, we do not simply argue that previous significant effects should be ignored as wrong because it is also possible that the significant effects of daughters on parental political predisposition may be obtained again by future studies employing other independent samples using the same analytic strategy. Indeed, it remains possible that, as Warner (1991) and Washington (2008) argue, parents may actively (or subconsciously) take a daughter's perspective and become more sympathetic to women's issues or even take actions toward that end, such as voting for that child's interests. Or, as Conley and Rauscher (2013) argue, having a daughter may trigger her parent's instinct to constrain her sexuality and thus induce more conservative views of teenage sexuality. While these suggested mechanisms could explain the change of specific political views, neither mechanism can explain the change of party identification as a whole given the heterogeneity of issue-partisanship correlations and the heterogeneity of political preferences across groups of voters (Baldassarri and Gelman 2008; Baldassarri and Goldberg 2014). In other words, since party identification (or political ideology) -- either as an identity or a summary measurement of a matrix of beliefs – consists of multiple dimensions of issues and attitudes, a change of attitude in some issues may not be sufficient to trigger a change in a whole set of political attitudes measured by party identification or political ideology.

Several limitations of the present study are worth noting. One of limitation is that we could not isolate the effects of biological child versus adopted/step child with the samples we used. It is possible that there exist indeed treatment effects, but they are cancelled out by the association between political ideology and willingness to adopt or marry into a family with daughters (or sons). Another limitation is related to the timing heterogeneity. Because of the cross-sectional nature of our sample, we cannot rule out the possibility that the effect of having a daughter varies across the different moments and time in entire life-course despite the insignificant interaction effects with the age of respondent or oldest child were reported. Notably, it may also be applied to the longitudinal design because we never know the exact timing of the effect.

Another challenge is related to the challenge in estimating small effects (Gelman and Weakliem 2009). Since the size of effects in social science studies is expected to be modest in general, statistical power (or sample size) should be large enough to detect the small or modest effects. For example, Urbatsch (2011) found that having older sisters leads to a more liberal ideology based on 1994 GSS sample (N=1,783), whereas Healy and Malhotra (2013) found that having sisters causes men to be more likely to identify as Republicans using the Political Socialization Panel in 1965, 1973, 1997 (N=330) and the National Longitudinal Survey of Youth 79 in 2006, 2008 (N=1,668). Our null findings would suggest the possibility that these mixed findings in the same country using the same instruments might originate from the oddities of small samples or the period heterogeneity and/or publication bias. As a corrective to this source of bias, we here add comprehensive null findings to already polarized canon of significant results.

Our null results should have important implications for the debate on the stability of political predisposition. Prior research suggesting an effect of offspring sex on political ideology and partisanship seriously challenged the paradigm suggesting that political identity formed early in childhood (largely through parental socialization and/or genetic inheritance) and was subsequently immovable. Our challenge to those empirical claims that adult parents' political orientation were affected by the random assignment of offspring sex does not, by extension, mean necessarily that political identity in adulthood is immovable; rather, our obtaining of consistently null results should prod researchers who wish to make the fluidity argument to find additional, more robust evidence to bolster their case.

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Tables and Figures

Table1. Sample selection criteria and sample size

Country	US		UK		All countries (in ESS)	
Years of survey	1977-2012		2002-2012		2002-2012	
Number of survey waves	24		6		6	
Sample Size (total)	57,061		13,403		286,349	
Sample Selection Criteria	N	%	N	%	N	%
Exclude the following cases sequentially						
R has no children in household[1)]	45,495	79.7	9,042	67.5	176,734	61.7
Generational age gap(R's age - Oldest kid's age) < 10[2)]	113	0.2	23	0.2	781	0.3
R has two or more children who are in the same age[3)]	136	0.2	60	0.4	1525	0.5
Any of R's children has missing value in one's gender or age	275	0.5	35	0.3	978	0.3
R's oldest child is older than(>) 16	3,517	6.2	1,311	9.8	50,535	17.6
R has any child who left home[4)]	1,954	3.4	0	0.0	0	0.0
Analytic Sample Size[5)]	5,571	9.8	2,932	21.9	55,796	19.5

NOTE. 1) In GSS, sex of R's children can be ascertained only if R is the household head because GSS asked the relationship of each household member to the head of household, whereas ESS asked the relationship of each household member to R directly. 2) Parent's age must be higher at least 10 years old than oldest kid's age, and thus it must reflect the coding error. 3) This case may be twins or two consecutive births, but we exclude these cases to ascertain the sex of one eldest child. 4) In GSS, it is the case that the total number of R's children (*childs*) is not equal to the number of children in household, whereas it is unidentified in ESS. 5) In analytic sample, we additionally dropped the cases with missing covariates and dependent variables.

Table2. Linear Probability Model : the effects of pre/post-treatment variables on the sex of the first child

	US		UK		ESS	
	Model1	Model2	Model3	Model4	Model5	Model6
Sex : Female	0.015	0.043*	-0.012	0.014	-0.001	-0.001
	(0.015)	(0.020)	(0.020)	(0.022)	(0.005)	(0.005)
Age	0.001	-0.001	0.000	0.000	0.000	-0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.000)	(0.001)
Native-born	-0.014	-0.029	0.016	0.001	0.002	0.002
	(0.024)	(0.025)	(0.027)	(0.029)	(0.007)	(0.007)
Age of the oldest child	0.001	-0.000	-0.002	-0.001	-0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
N of children		0.028		-0.027		0.001
		(0.020)		(0.025)		(0.004)
Household Size		-0.006		0.015		0.000
		(0.018)		(0.021)		(0.004)
Years of Education		0.006*		-0.004		0.001
		(0.003)		(0.003)		(0.001)
Marital Status: Married		0.027		0.003		0.002
		(0.027)		(0.028)		(0.007)
Marital Status: ex-married		0.002		-0.017		0.010
		(0.024)		(0.034)		(0.010)
Labor force status : working (=ref)		.		.		.
		.		.		.
School		-0.034		-0.140*		0.003
		(0.048)		(0.069)		(0.018)
Housekeeping		-0.041+		-0.070*		-0.003
		(0.024)		(0.025)		(0.006)
Other working status		0.004		0.030		0.005
		(0.029)		(0.034)		(0.008)
Religion : Protestant (=ref)		.		.		.
		.		.		.
Catholic		-0.049*		-0.003		0.006
		(0.017)		(0.044)		(0.008)
No Religion		-0.002		-0.040		0.002
		(0.023)		(0.029)		(0.007)
Other religion		-0.019		-0.058		-0.003
		(0.029)		(0.037)		(0.008)
Constant	0.493*	0.414*	0.436*	0.552*	0.472*	0.461*
	(0.058)	(0.072)	(0.061)	(0.088)	(0.015)	(0.021)
Observations	5181	5181	2889	2889	53639	53639
Adjusted R-squared	-0.001	0.002	-0.001	0.002	-0.000	-0.000

NOTE.– Survey year dummies are controlled for in all models. In ESS sample, country fixed effects are estimated. Standard errors are in parenthesis (+ $p < 0.1$, * $p < 0.05$)

Table3. Replication Analysis : The OLS results for the effects of daughters on party identification in the UK and the US

Dependent Variables :	Republican Index (-3 to 3)			Republican (=1)	Democrat (=1)
	US				
controls included?	no	pre-cv	post-cv	No	No
Oldest = Daughter	-0.012	0.002	-0.019	-0.002	-0.003
	(0.053)	(0.052)	(0.051)	(0.012)	(0.013)
N	5164	5164	5164	5164	5164
adj. R-sq	0.010	0.045	0.081	0.011	0.004
	UK				
controls included?	no	pre-cv	post-cv	No	No
Oldest = Daughter	0.103	0.085	0.093	0.025	-0.025
	(0.105)	(0.104)	(0.103)	(0.029)	(0.029)
N	998	998	998	1003	1003
adj. R-sq	0.013	0.033	0.060	0.010	0.010

NOTE. -- pre-cv refers to the pre-treatment covariates including R's age, sex, native-born, age of oldest child and post-cv is the post-treatment covariates including the number of children(dummies), household size, R's years of education, marital status, job status, religion. All OLS regression models include survey year dummies and standard errors are in parenthesis (+ p<0.1, *p<0.05).

Table4. The interaction effects of daughter on party identification with gender, age, native-born and age of oldest child

	Republican Index (-3 to 3)							
	US				UK			
Oldest = Daughter (=OD)	0.040	-0.402	0.018	-0.085	0.182	-0.156	-0.032	0.121
	(0.071)	(0.253)	(0.106)	(0.169)	(0.161)	(0.528)	(0.207)	(0.284)
Female	-0.651*	-0.692*	-0.690*	-0.691*	-0.202	-0.284*	-0.285*	-0.284*
	(0.076)	(0.056)	(0.056)	(0.056)	(0.151)	(0.109)	(0.109)	(0.109)
Age	0.006	0.001	0.006	0.006	0.006	0.003	0.006	0.006
	(0.005)	(0.006)	(0.005)	(0.005)	(0.008)	(0.010)	(0.008)	(0.008)
Oldest Age	-0.014+	-0.013+	-0.013	-0.014+	-0.017	-0.017	-0.024	-0.017
	(0.007)	(0.007)	(0.009)	(0.007)	(0.013)	(0.013)	(0.016)	(0.013)
Native-born	0.301*	0.300*	0.299*	0.252*	0.593*	0.592*	0.595*	0.614*
	(0.090)	(0.090)	(0.090)	(0.126)	(0.153)	(0.153)	(0.153)	(0.209)
OD * Female	-0.082				-0.166			
	(0.104)				(0.211)			
OD * Age		0.012				0.006		
		(0.007)				(0.013)		
OD * Oldest Age			-0.002				0.014	
			(0.011)				(0.021)	
OD * Native-born				0.096				-0.041
				(0.178)				(0.306)
_cons	-0.935*	-0.725*	-0.923*	-0.873*	-1.271*	-1.111*	-1.168*	-1.246*
	(0.219)	(0.247)	(0.222)	(0.232)	(0.353)	(0.428)	(0.360)	(0.374)
N	5164	5164	5164	5164	998	998	998	998
adj. R-sq	0.045	0.046	0.045	0.045	0.032	0.032	0.032	0.032

NOTE.– All OLS regression model controlled for pre-treatment covariates (R's age, sex, native-born, age of the oldest child) as well as survey year dummies. Standard errors are in parenthesis (+ p<0.1, *p<0.05).

Table 5. The effects of daughters on political ideology in GSS 1994 sample

Dependent Variables:	Political ideology scale (-100:liberal to 100:conservative)		
	US		
controls included?	no	pre-cv	post-cv
Oldest = Daughter	5.994*	5.843*	5.107+
	(2.882)	(2.879)	(2.868)
N	1027	1027	1027
adj. R-sq	0.003	0.006	0.027

NOTE. -- pre-cv refers to the pre-treatment covariates including R's age, sex, native-born, age of oldest child and post-cv is the post-treatment covariates including the number of children(dummies), household size, R's years of education, marital status, job status, religion. All OLS regression models include survey year dummies and standard errors are in parenthesis (+ p<0.1, *p<0.05).

Table6. The OLS results for the effects of daughters on political ideology among entire sample

Dependent Variables:	Political ideology scale (-100:liberal to 100:conservative)		
	GSS		
controls included?	no	pre-cv	post-cv
Oldest = Daughter	1.047	1.279	0.932
	(1.299)	(1.285)	(1.262)
N	4651	4651	4651
adj. R-sq	0.002	0.023	0.065
	ESS		
controls included?	no	pre-cv	post-cv
Oldest = Daughter	0.228	0.220	0.203
	(0.385)	(0.385)	(0.382)
N	46375	46375	46375
adj. R-sq	0.046	0.049	0.063

NOTE. -- pre-cv refers to the pre-treatment covariates including R's age, sex, native-born, age of oldest child and post-cv is the post-treatment covariates including the number of children(dummies), household size, R's years of education, marital status, job status, religion. All OLS regression models include survey year dummies and standard errors are in parenthesis (+ p<0.1, *p<0.05).

Table7. The interaction effects of daughter on political ideology with gender, age, native-born and age of oldest child

	Political Ideology Scale (-100: liberal to 100:conservative)							
	GSS				ESS			
Oldest = Daughter (=OD)	-0.337	4.959	0.575	-7.869+	0.793	-0.684	-0.623	2.053+
	(1.721)	(6.290)	(2.617)	(4.348)	(0.578)	(1.987)	(0.801)	(1.194)
Female	-15.108*	-13.323*	-13.348*	-13.378*	-4.498*	-4.997*	-4.996*	-4.994*
	(1.876)	(1.396)	(1.396)	(1.395)	(0.549)	(0.400)	(0.400)	(0.400)
Age	-0.062	-0.010	-0.061	-0.061	-0.177*	-0.188*	-0.176*	-0.177*
	(0.116)	(0.144)	(0.116)	(0.116)	(0.034)	(0.042)	(0.034)	(0.034)
Oldest Age	0.270	0.272	0.234	0.271	0.072	0.072	0.025	0.072
	(0.174)	(0.174)	(0.220)	(0.174)	(0.050)	(0.050)	(0.064)	(0.050)
Native-born	5.486*	5.526*	5.535*	0.633	2.682*	2.677*	2.675*	3.657*
	(2.305)	(2.305)	(2.305)	(3.204)	(0.645)	(0.645)	(0.645)	(0.884)
OD * Female	3.655				-1.029			
	(2.588)				(0.774)			
OD * Age		-0.107				0.024		
		(0.178)				(0.052)		
OD * Oldest Age			0.085				0.096	
			(0.274)				(0.080)	
OD * Native-born				10.023*				-2.045
				(4.551)				(1.261)
_cons	-2.545	-5.108	-3.033	0.982	8.156*	8.874*	8.836*	7.554*
	(5.373)	(6.098)	(5.442)	(5.691)	(1.374)	(1.652)	(1.398)	(1.463)
N	4651	4651	4651	4651	46375	46375	46375	46375
adj. R-sq	0.023	0.023	0.023	0.024	0.049	0.049	0.049	0.049

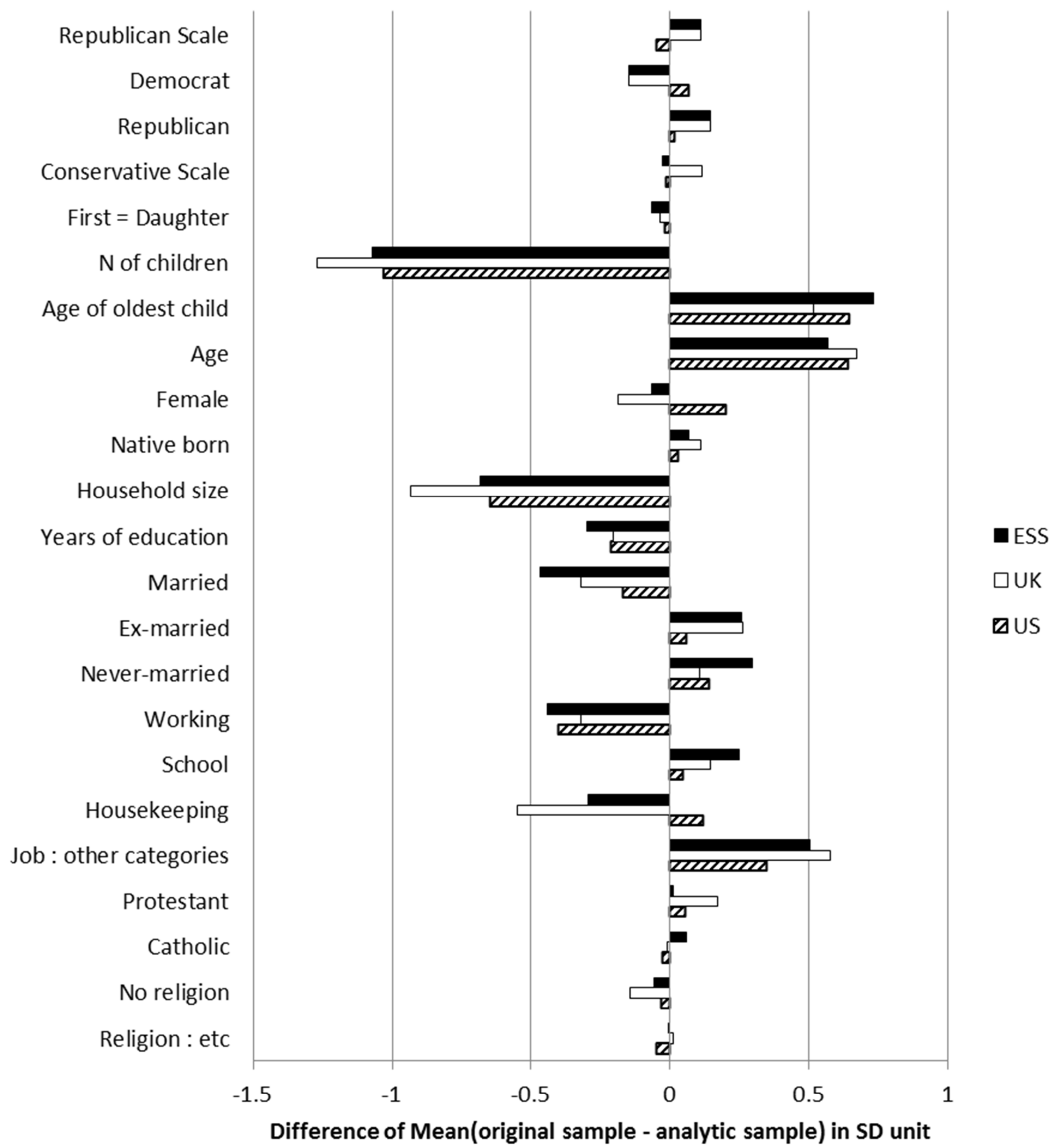
NOTE.– All OLS regression controlled for the pre-treatment covariates (R's age, sex, country of origin, age of oldest child) as well as survey year dummies. In ESS sample, country fixed effects are estimated. Standard errors are in parenthesis (+ $p<0.1$, * $p<0.05$).

Table8. Maximum likelihood estimates for the effects of first child's sex on political ideology in the ESS sample

	Random Intercept without controls		Random Intercept with controls		Random Coefficients with controls	
Parameter	Est.	SE	Est.	SE	Est.	SE
Fixed part						
Oldest=Daughter	0.23	0.39	0.22	0.38	0.28	0.39
_cons	5.85	1.66	10.28	2.06	10.26	2.04
Random part						
SD (intercept)	9.63	1.19	9.13	1.13	8.98	1.13
SD (slope)					0.32	0.41
Cor (intercept, slope)					1.00	0.00
SD (residual)	41.47	0.14	41.38	0.14	41.38	0.14
N	46375		46375		46375	
Log likelihood	-238621.79		-238511.34		-238511.03	
LR Test Results (Chi2, P-value)					0.63	0.58

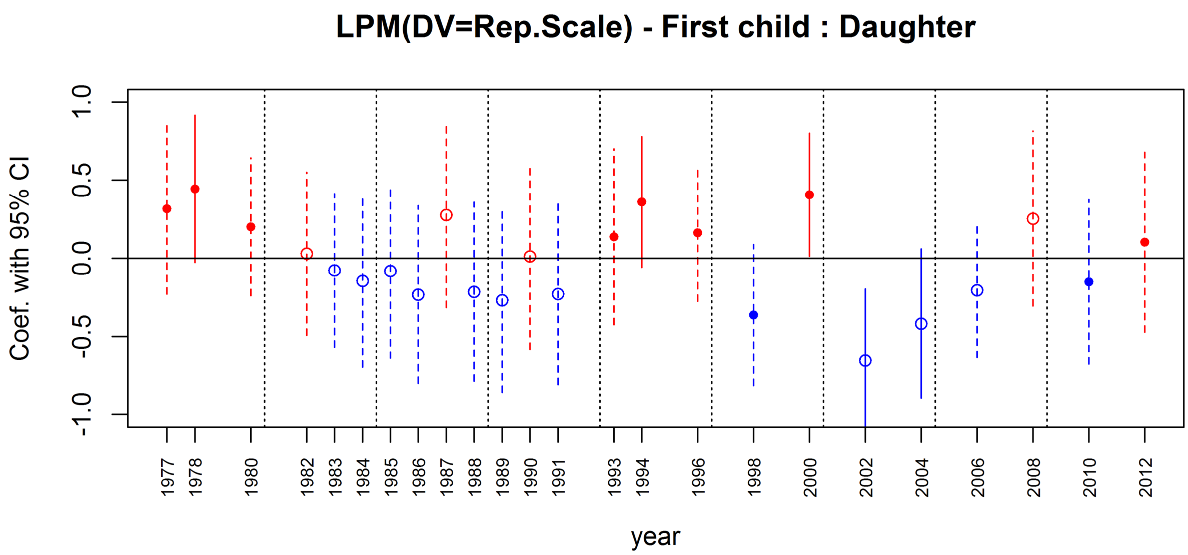
NOTE. -- A list of controls is age, sex, native-born, and age of oldest child, and survey year dummies. Random intercept and coefficients models are estimated by using *xtmixed* command in Stata and LR test is adjusted by the asymptotic null distribution for testing the zero variance of the slope coefficients using $0.5*\chi^2(q) + 0.5*\chi^2(q+1)$

Figure1. Difference in means (full sample – analytic sample) in standard deviation unit



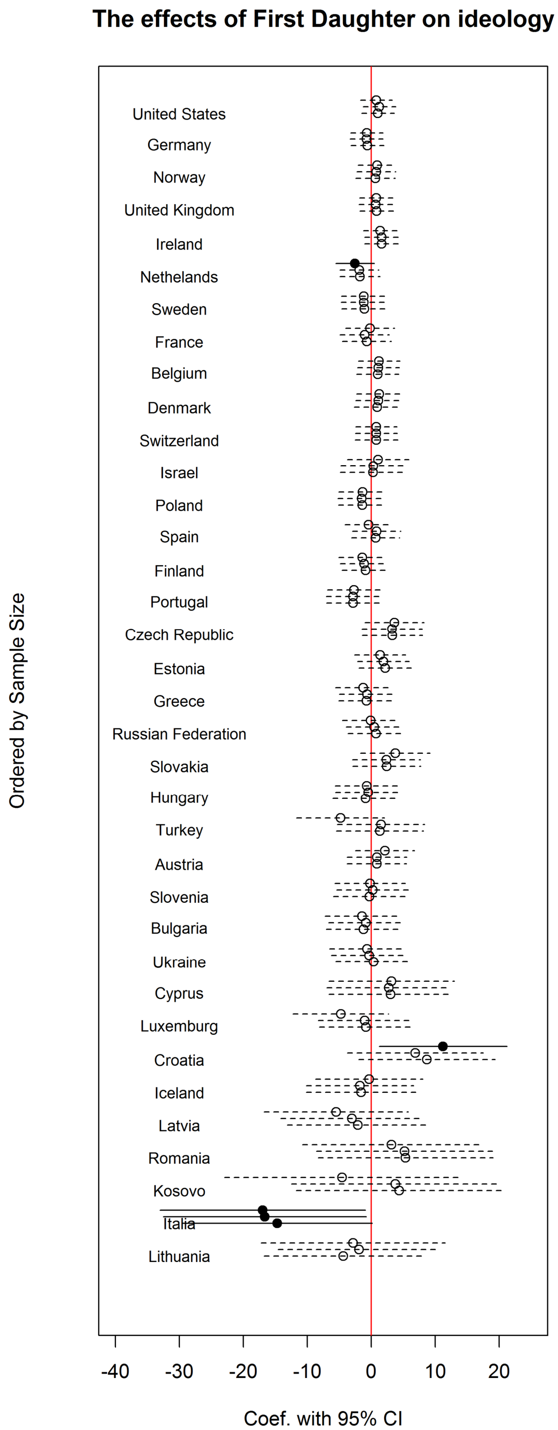
NOTE. – Means of each variable are calculated in the full sample and the reduced sample and the differences between the mean of full sample and reduced sample are later divided by its standard deviations of the full sample. Each bar represents the percentage difference in standard deviation unit.

Figure 2. Period variations : the effects of daughter on party identification in GSS sample



NOTE.—The bivariate regression estimates of effects of daughter on party identification with 95% confidence interval are plotted. The filled dot represents period when the ruling party is Democrat and the empty dot represents period when the ruling party is Republican. Statistically significant effects (at $p < 0.1$) are displayed in solid line.

Figure 3. Country level estimates in entire sample: the effects of the first child's sex on the political ideology



NOTE. – Three estimates in each country represent the effect with no control, pre-treatment controls, and post-treatment controls respectively and black dot refers to the statistically significant estimates at 10%.

Appendix

Table S1. Variable wordings and coding

	Wording and coding	
Variable Name	GSS	ESS
Dependent Variable;		
Party Identification	"Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent, or what?" - Conservative Party (=strong/weak republican) - Liberal party (=strong/weak Democrat) - Republican Scale (-3: Strong Democrat, 0 : independent, 3 : Strong Republican)	"Is there a particular political party for you feel closer to than all the other parties? Which one? How close do you feel to this party?" - Conservative Party (=Conservative) - Liberal party (=Labour or Liberal Democrat) (*:All other parties are excluded) - Republican Scale (-3 : Very close to liberal party, 0 : not at all close, 3 : Very close to conservative party)
Political ideology	"We hear a lot of talk these days about liberals and conservatives. I'm going to show you a seven-point scale on which the political views that people might hold are arranged from extremely liberal - point 1 - to extremely conservative - point 7. Where would you place yourself on this scale?" - rescaling to -100 (extremely liberal) ~ 100 (extremely conservative) by 100 * (X - 4) / 3	"In politics people sometimes talk of "left" and "right". Using this card, where would you place yourself on this scale, where 0 means the left and 10 means the right?" - rescaling to -100 (left) ~ 100 (right) by 100* (X-5) / 5
Independent Variables;		
First daughter	The sex of eldest child is female = 1	
Control Variables;		
Pre-treatment covariates		
Age	R's age	
Female	R is female (=1) [ref=male]	
Native-born	"Were you born in this country?" Yes(=1)	
Age of oldest child	Eldest child's age	
Post-treatment covariates		
Number of children	Number of R's children in total (dummies)	
Household Size	The number of persons in household	
Years of education	"What is the highest grade in elementary school or high school that you finished and got credit for?"	"How many years of full-time education have you completed?"
Marital Status;	"Are you currently -- married, widowed, divorced, separated, or have you never been married?" - Married - Divorced / Widowed / Separated - Never married [ref]	"Could I ask about your current legal marital status? Which of the descriptions on this card applies to you?" (* see note) - Married (or civil union) - Divorced / Widowed / Separated - Never married [ref]
Labor Force Status;	"Last week were you working full	"And which of these descriptions

	time, part time, going to school, keeping house or what?" - Currently Working (=working fulltime/parttime) - School - Housekeeping - Other (Temporary not working, Unemployed, Retired, Other)	best describes your situation (in the last seven days)?"- post coded - Currently Working (=paid work) - School (=education) - Housekeeping (=Housework, looking after children, others) - Other (=Unemployed, Permanently sick or disabled, Retired, Community or military service, Other)
Religion	"Which is your religious preference? Is it Protestant, Catholic, Jewish, some other religion, or no religion?" - Protestant - Catholic - None - Others (Jewish, Other, Buddhism, Hinduism, Other eastern, Moslem/Islam, Orthodox-Christian, Christian, Native American, Inter-nondenominational)	"Do you consider yourself as belonging to any particular religion or denomination?, Which one?" - Protestant - Catholic (=Roman Catholic) - None - Others (Eastern Orthodox, Other Christian denomination, Jewish, Islamic, Eastern religions, Other non-Christian religions)

NOTE. -- The different versions in category of legal marital status were asked to ESS respondents across survey waves. We treat civil union (or civil partnership) as married for the purpose of comparison.

TableS2. Mean of covariates in GSS and ESS

	US				UK				ESS			
	Total Sample		Analytic Sample		Total Sample		Analytic Sample		Total Sample		Analytic Sample	
	Mean	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean	N
Republican Scale	-0.33	56734	-0.24	5546	-0.44	5609	-0.64	1005				
Democrat	0.37	56734	0.34	5546	0.63	5634	0.70	1010				
Republican	0.26	56734	0.25	5546	0.37	5634	0.30	1010				
Conservative Scale	3.52	47876	4.10	5013	1.32	11754	-2.87	2510	3.04	243451	4.20	48113
First = Daughter	0.48	22090	0.49	5571	0.46	4361	0.47	2932	0.45	109571	0.49	55775
N of children	0.76	57061	1.96	5571	0.57	13403	1.80	2932	0.67	286349	1.77	55796
Age of oldest child	15.27	22211	8.32	5571	13.58	4327	8.35	2932	17.29	109364	8.75	15.27
Age	45.70	56859	34.43	5562	49.61	13317	36.95	2925	47.55	284914	36.99	55694
Female	0.56	57061	0.46	5571	0.55	13391	0.64	2932	0.54	286061	0.57	55777
Native born	0.91	47804	0.91	5212	0.90	13399	0.86	2932	0.91	285929	0.89	55720
Household size	2.68	57055	3.67	5571	2.38	13383	3.60	2932	2.80	286068	3.81	55793
Years of education	12.75	56897	13.42	5562	13.05	13267	13.81	2914	12.06	283128	13.29	55436
Married	0.54	57061	0.62	5571	0.48	13332	0.64	2922	0.53	282430	0.76	55203
Ex-married	0.26	57041	0.23	5570	0.25	13332	0.14	2922	0.19	282430	0.09	55203
Never-married	0.20	57061	0.15	5571	0.27	13332	0.22	2922	0.28	282430	0.14	55203
Working	0.60	57047	0.79	5571	0.50	13370	0.66	2929	0.48	284321	0.70	55524
School	0.03	57047	0.02	5571	0.05	13370	0.02	2929	0.09	284321	0.02	55524
Housekeeping	0.16	57047	0.12	5571	0.08	13370	0.23	2929	0.10	284321	0.19	55524
Job : other categories	0.21	57047	0.07	5571	0.37	13370	0.09	2929	0.33	284321	0.09	55524
Protestant	0.59	56828	0.56	5547	0.21	13377	0.15	2927	0.13	282178	0.12	55001
Catholic	0.25	56828	0.26	5547	0.06	13377	0.07	2927	0.29	282178	0.26	55001
No religion	0.11	56828	0.12	5547	0.52	13377	0.59	2927	0.37	282178	0.39	55001
Religion : etc	0.06	56828	0.07	5547	0.21	13377	0.20	2927	0.22	282178	0.22	55001

Table S3. OLS results for the effects of daughter on party identification in the UK during overlapping period (2002-2005, ESS Wave 1 and 2)

	UK (2002 - 2005)			
DV :	Conservative Party Index (-3 to 3)			Conservative Party (=1)
controls included?	no	pre-cv	post-cv	no
Oldest = Daughter	0.118	0.080	0.123	0.047
	(0.174)	(0.175)	(0.178)	(0.048)
N	321	321	321	324
adj. R-sq	-0.003	-0.001	0.009	0.002

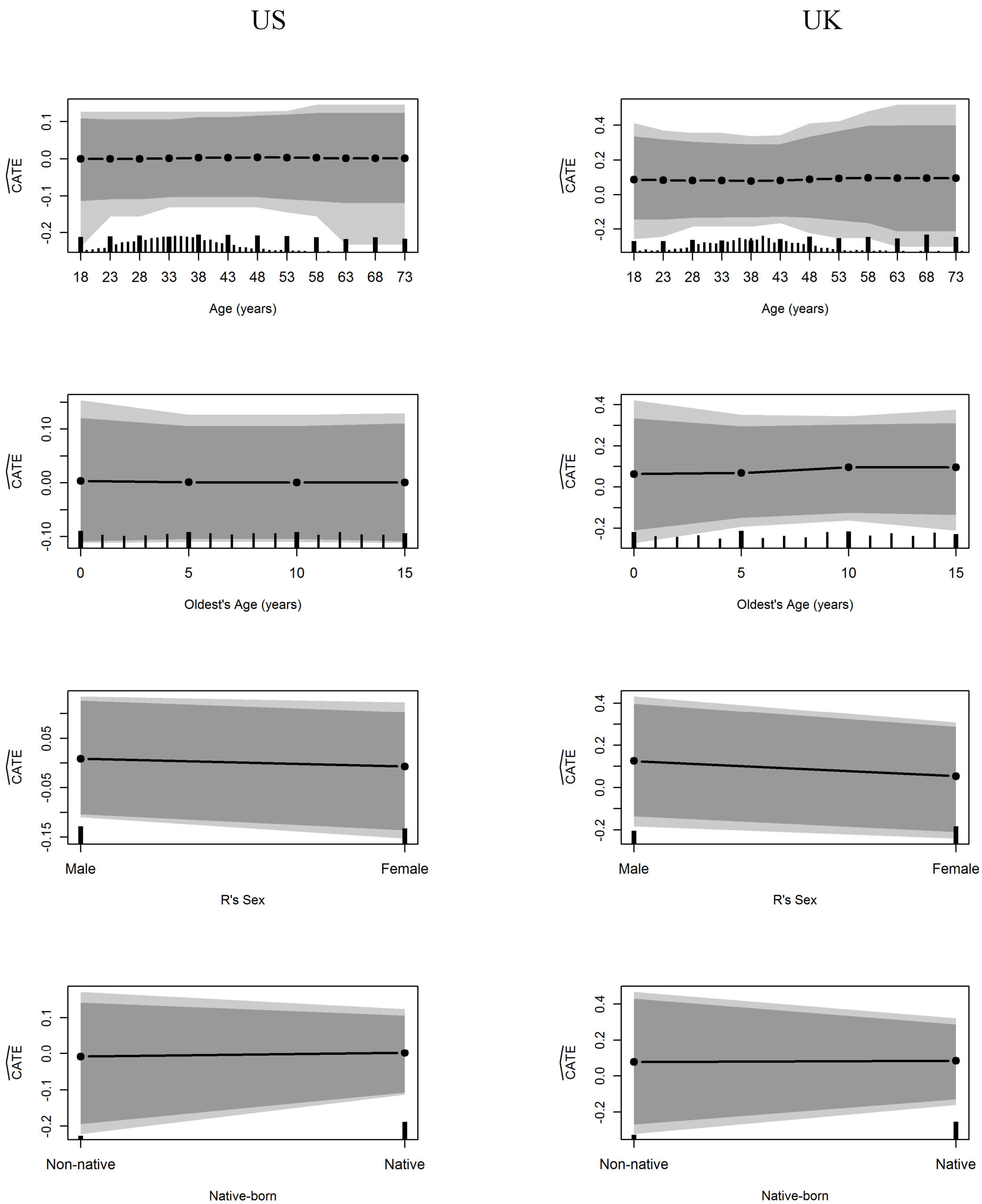
NOTE. -- pre-cv is the pre-treatment covariates including R's age, sex, native-born, age of the oldest child and post-cv is the post-treatment covariates including the number of children (dummies), household size, R's years of education, marital status, job status, religion. All OLS regression models include survey year dummies. Standard errors are in parenthesis (+ $p < 0.1$, * $p < 0.05$)

Table S4. OLS results for the effects of daughter on party identification in Korea (2003-2010, Korean General Social Survey)

	Political ideology scale (-100:liberal to 100:conservative)		Party Identification (=1 if Republican Party)	
Controls included?	No	yes	no	yes
Oldest = Daughter	-3.012	-2.026	-0.007	-0.002
	(2.099)	(2.117)	(0.019)	(0.019)
N	2283	2283	2346	2346
adj. R-sq	0.004	0.010	0.073	0.074

NOTE. – Republican party indicator represents the support for Grand National Party. The controls include R's age, sex, native-born, and age of oldest child. All OLS regression models included survey year dummies. Standard errors are in parenthesis (+ $p < 0.1$, * $p < 0.05$)

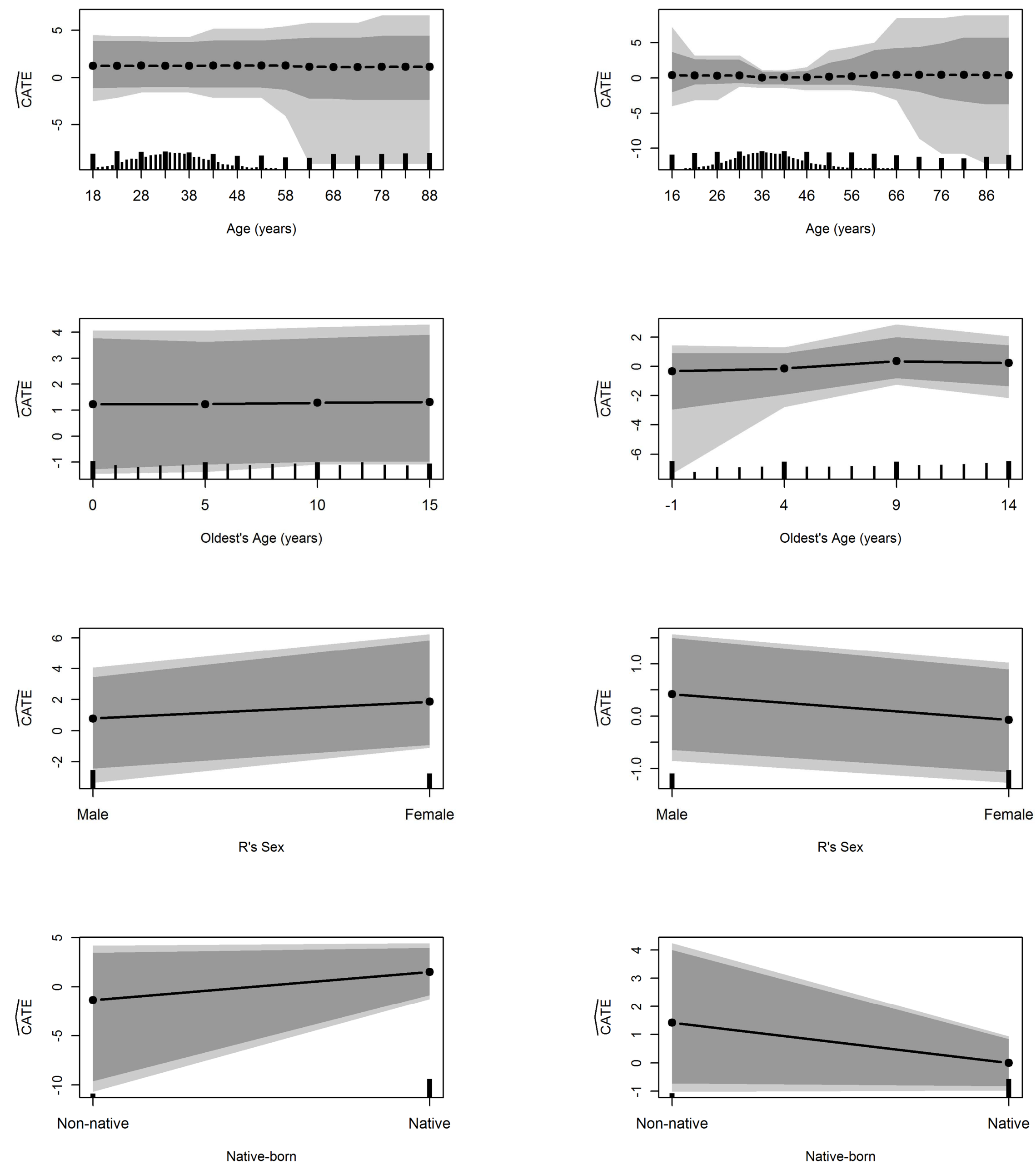
Figure S1. CATEs of the first child’s sex on party identification from BART.



NOTE. – In running BART, the default tuning parameters are used with a burn-in period of 500 draws followed by 1,000 draws from the posterior to compute CATEs based on Green and Kern (2012)’s replication codes. The dark-gray areas are point-wise 95% posterior bands and the light-gray areas are global 95% posterior bands that simultaneously account for uncertainty in all CATE estimates .

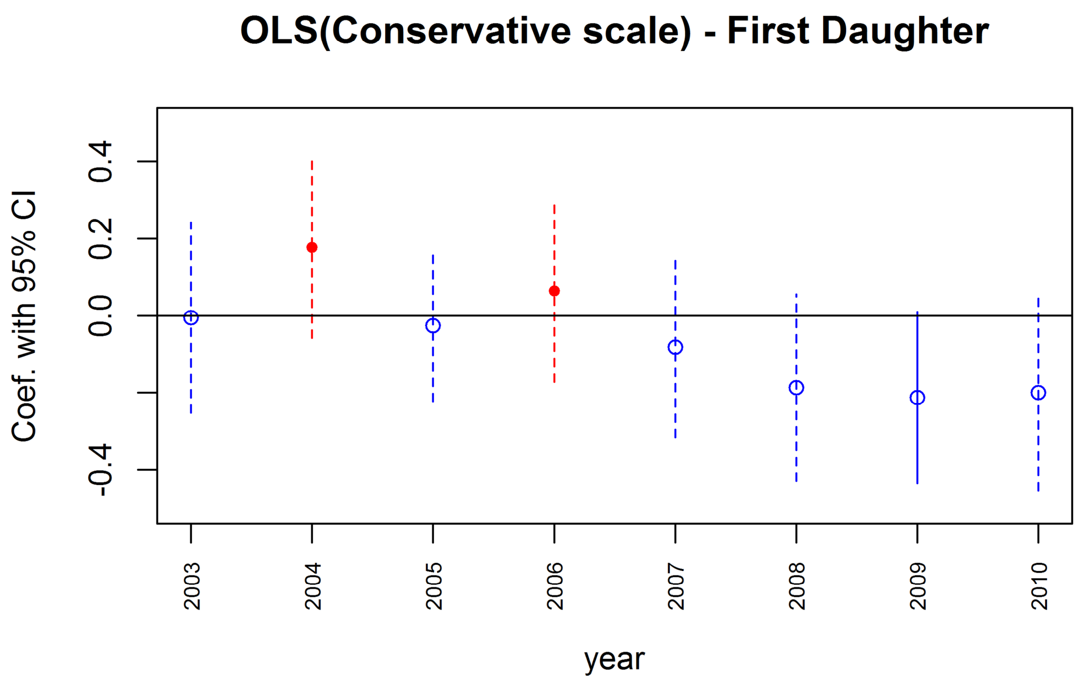
Figure S2. CATEs of the first child's sex on political ideology from BART.

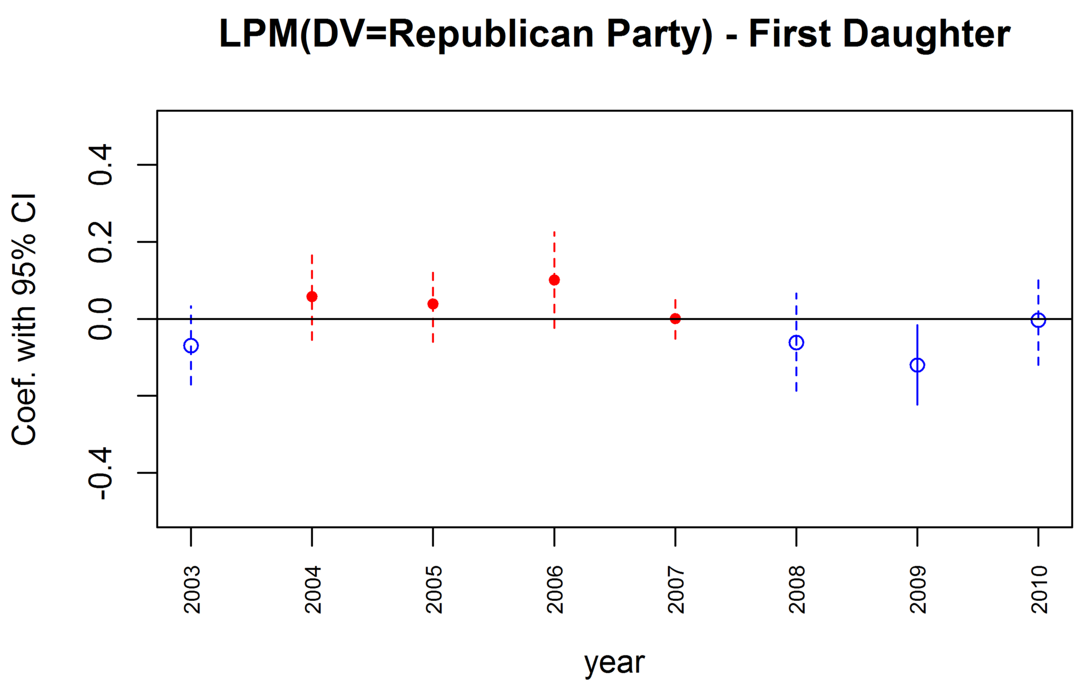
GSS ESS



NOTE. – In running BART, the default tuning parameters are used with a burn-in period of 500 draws followed by 1,000 draws from the posterior to compute CATEs based on Green and Kern (2012)'s replication codes. The dark-gray areas are point-wise 95% posterior bands and the light-gray areas are global 95% posterior bands that simultaneously account for uncertainty in all CATE estimates .

Figure S3. Period variations : The effects of daughter on party identification in KGSS sample





NOTE.—Bivariate analysis is performed and 95% confidence interval in each year is presented. Statistically significant associations are displayed in bold line (p-value < 0.1)