

Offspring Sex and Partisanship

Does the Gender of Offspring Affect Parental Political Orientation?

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Recently, offspring sex has been widely used as a natural experiment and argued to induce changes in political orientation among parents. However, prior results have been contradictory: in the UK, researchers found that having daughters led to parents favoring left-wing political parties and to holding more liberal views on family/gender roles, whereas in the United States scholars found that daughters were associated with more Republican (rightist) party identification and more conservative views on teen sexuality. We propose and examine three plausible explanations to account for these puzzling results using data from the General Social Survey and the European Social Survey; contextual (period/country) differences, heterogeneous treatment effects, and publication bias. In an analysis of thirty-six countries, we obtain null effects of the sex of the first child on party identification as well as on political ideology while ruling out country heterogeneity. Further, we observe no evidence of other heterogeneous treatment effects based on the analysis of Bayesian Additive Regression Tree models. As a corrective to the source of publication bias, we here add comprehensive null findings to the polarized canon of significant results.

Background

Similarity obtains in families; women tend to be married to men with similar educational background (Mare 1991; Domingue et al. 2014), and children often mimic the way their parents think and behave (Jennings, Stoker, and Bowers 2009; Glass, Bengtson, and Dunham 1986). Parent-child similarity (i.e., intergenerational inheritance) of social and economic status has garnered much attention since it forms a basis of social stratification and reproduction (Conley and Bennett 2000; Hauser and Grusky 1988). However, 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 factors, or sometimes even the direction of influence, while ruling out unobserved heterogeneities such as shared genetic endowments.

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Political predisposition—party identification or political ideology—is not an exception:¹ family (e.g., parent-child relationships) has played a key role in socialization of 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, Stoker, and Bowers (2009) confirmed that 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. They argue that their results provide support for an intergenerational transmission model, which views political values as directly transmitted from parents to children 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 induce some parents to 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 become Democrats (or left leaning). While this kind of experiment is impossible, the sex² of the (first) child provides a unique natural experiment to assess the possibility that part of the observed correlation is due to the fact that children socialize their parents (rather than the other way around). In the absence of prenatal sex selection, the sex of children should be random, and consequently the “first stage” effect of the sex of child on outcomes would be unbiased by unobserved factors (such as genetics).³

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 turn 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), having a daughter instead of a son can be considered 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, the 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.

Recent studies using the sex of children as a natural experiment have shown significant but contradictory results for the effects of offspring sex on party identification or political ideology.⁴ For instance, in the UK, [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. 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. 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 to resolve.

One plausible explanation of the differences obtained 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 gender, since there may not be any political differences by gender in adults or children. Another possibility comes from so-called heterogeneous treatment effects; the effect of having a daughter might be conditional on certain parental characteristics. As a woman, a mother may not change her political viewpoint just due to having a daughter, since she has already experienced the same gender situations as her daughter will face, whereas a father may switch his political position if his daughter would provide him novel opportunities to think from a woman's perspective. 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 the effects of offspring sex on parental political orientation are robust (or versatile) across periods and countries and to test systematically for the presence of heterogeneous treatment effects. To verify the direction and potential significance of effects of the sex of the firstborn offspring on both political ideology and partisan identification, we employed Bayesian Additive Regression Tree (BART) models as well as multilevel analysis based on cross-sectional samples. To preview our results, we report null findings across all analyses, finding no heterogeneous treatment effects by subgroup, and thus we question the robustness of prior findings.

Methodological Complications

Absent sex-selective abortion, it seems like a natural law that the sex of a given child is randomly determined. With this in mind, prior studies of Western societies have tended to consider having a daughter (or a son) an exogenous gender shock to the family unit. For example, [Washington \(2008\)](#) found that a congressman's propensity to vote liberally—with respect to reproductive rights and

women's issues in particular—was augmented by having additional daughters. Glynn and Sen (2014) found a similar effect with regard to decision-making among US Courts of Appeals judges: judges with daughters more often voted in gender-related cases in feminist directions, though they did not find that daughters significantly increased liberality within the judiciary they studied when they extended this analysis to *all* issues.

Nevertheless, a famous refutation to the seeming truism about the randomness of offspring sex comes from evolutionary theory—specifically, the Trivers-Willard hypothesis (Trivers and Willard 1973), which predicts that high-status individuals will have more sons because the reproductive payoff is potentially greater while lower-status individuals will skew toward female offspring since the reproductive risk is lower. Evidence in humans has been thin, however. For example, Hopcroft (2005) found that high-status men are more likely to have boys than other men (but that this effect did not hold for women) using the 1994 GSS sample, though as we will show later this is most likely due to the peculiarity of the 1994 GSS sample. Meanwhile, Freese and Powell (1999) found little evidence to the hypothesis using two other nationally representative data sets, the National Educational Longitudinal Study of 1988 (NELS) and the 1980 High School and Beyond (HSB) Study.

Evidence aside, if any version of the Trivers-Willard theory holds true, the study of political elites such as judges and legislators would suffer from potential biases especially when the seeming exogeneity of the offspring's sex of legislators hinges on the election outcomes. The status of the elected officials—if they had their children after achieving it—may skew their offspring sex ratios. More likely, however, is the potential bias that would arise from the selection into these elevated positions. Liberal constituencies may prefer to elect (or appoint) candidates with daughters, since this may make the relevant politician seem more liberal (and vice versa for conservative districts). To our knowledge, there are no extant studies to directly confirm or disconfirm the Trivers-Willard hypothesis or the effect of offspring sex on political success among elected (or appointed) officials. Thus, the study of high-status elite population is interesting in its own right but might suffer from a lack of exogeneity in offspring sex.

Even studies that focus on non-select, non-elite populations suffer from limitations. For instance, with the exception of Conley and Rauscher (2013) and Oswald and Powdthavee (2010), the sex of *all* children is considered. Thus, they introduce the possibility 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 firstborn 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 preexisting 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.

The strongest assertion against the analytic approach treating the sex of a child as a natural experiment comes from the arguably non-randomness of sex-specific prenatal survival rates. For example, [Hamoudi and Nobles \(2014\)](#) found that relationship conflict between husband and wife predicted both the sex of subsequent children and the likelihood of subsequent divorce in the National Longitudinal Survey of Youth (NLSY 79). They interpret this as an evidence to show the non-randomness of the sex of offspring because the higher level of stress, which increases the divorce risk, might be associated with having a daughter given the presence of a prenatal female survival advantage. That said, their sample is very small ($N = 587$) and thus the confidence interval for their estimate almost skirts zero. More critically, they include higher-order births (which is problematic for the reasons mentioned earlier), they cannot address sex-selective sample attrition, and they limit their sample to those who are in intact marriages in 1992, thus inducing left censoring as well. For all these reasons, their study, while intriguing, is far from the final word on the potential endogeneity of offspring sex.

Despite these limitations and sometimes contradictory valences, if any of the studies that use the sex of offspring as a predictor of parental political orientation is correct, such results not only challenge researchers who treat political predisposition as a right-hand side (i.e., causal) variable on other political outcomes,⁵ but also those who call for revision and extension of political socialization theory wherein children are merely perceived as passive objects who develop political predispositions through a one-way learning process from their parents.

Theoretical Complications

Prior literature on the effects of offspring sex on parental political views relies mainly on the mechanism of parental adoption of or reaction to the child's perspective. In this paradigm, parents react to the gender of their child in certain ways that render them consciously or unconsciously in sympathy with their child, and thus they think through their child's perspective, thereby adjusting their political identity to be aligned with that of their child ([Warner and Steel 1999](#); [Washington 2008](#); [Oswald and Powdthavee 2010](#); [Conley and Rauscher 2013](#)). The parental adoption of the child's perspective can be attributed to either a social learning process in which parents update their knowledge through interaction with their child and learn about their son's or daughter's worldviews that they have rarely thought about ([Glynn and Sen 2014](#)), or to a social contact hypothesis in which social exposure to different groups (i.e., boys or girls) makes parents more sympathetic and favorable to the groups with which they closely interacted ([Conley and Rauscher 2013](#); [Bolzendahl and Myers 2004](#)). However, theoretical complications to these models arise from several sources.

The first complication comes from the heterogeneity of parental sex preferences and parental gender experience. Socialization theory assumes that parents devote considerable time exposed to their children in order to be socialized by them. However, it is hard to ignore the possibility that parents prefer one sex of child to the other so that they selectively socialize themselves with their preferred sex of child. Prior evidence supports the idea that fathers exhibit a son preference

(Lundberg, McLanahan, and Rose 2007; Dahl and Moretti 2008) and, if this is correct, then the exogenous event of having a daughter (or additional daughters) might not trigger a change in fathers' political orientation unless they spend enough time with them. By contrast, the newness of experiencing a daughter's perspective might not be new to mothers, who also had grown up as a daughter, and thus mothers may maintain their political predispositions. Shortly, a duality exists: it is fathers who will be most influenced by having a daughter in terms of the novelty of experiencing a new perspective, though it is also fathers who may be least influenced in terms of the likelihood of having a thorough socialization experience due to prior gender biases (and the same logic is applied inversely to mothers).

The second complication comes from the difficulty of predicting the direction of effects. Since the theory cannot tell which political issues parents will focus on when they think through their child's perspective, the direction of effects on parental political orientation is likely to be unclear. For example, in the United States, having a daughter may lead parents to reduce their support for traditional gender roles (Shafer and Malhotra 2011) but simultaneously increase their conservative view of teen sex (Conley and Rauscher 2013). The policy of the Republican party in the United States is not only known to favor more traditional gender roles but also to be favorable to the stricter regulation of teen sexuality; thus, the event of having a daughter does not provide sufficient information to predict the direction of the change of parental political identity given the prior attitudinal effects that have been documented.

The presence of heterogeneous treatment effects is another possibility. First, the effect of having a daughter or son may differ across the gender of parents for the reasons mentioned above. Previous studies showed that the effects of offspring sex were stronger or significant only to fathers (Glynn and Sen 2014; Healy and Malhotra 2013; Shafer and Malhotra 2011). Second, the heterogeneity of parental issue focus may evince heterogeneous treatment effects across periods (e.g., presidential administrations) and across the different moments in life trajectories of parents and their children. For example, older parents of older children might give more attention to the issues of teen sexuality and pregnancy or younger parents of younger children might focus more on the issue of traditional gender roles and opportunities. Third, if we limit our attention to the issues of gender roles and women's rights, we might expect to observe the presence of heterogeneous treatment effects across different countries or the nativity statuses or even social classes due to the difference of the ideal gender roles in different cultures. The daughter's primary role can be perceived by her parents either as being a wife and a mother or as a professional. If a certain group of parents (e.g., ill educated, foreign born, or of certain nationalities) treat the ideal roles of daughter as pertaining to more traditional family-oriented ones such as wife and mother, then having a daughter may not necessarily lead to changes in parental political ideology.⁶

Instead of making ad-hoc predictions, we first attempt to ascertain the *presence* of heterogeneous treatment effects, which can later be used as a guide to investigate the potential competing theories. We would not specify the *direction*

of heterogeneous treatment effects because of the aforementioned theoretical complications.

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 the sex of children on parents' political views across social contexts.⁷ Although the 1994 GSS sample used by [Conley and Rauscher \(2013\)](#) collects information on all of the respondents' children, it is of limited scope in terms of both 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 is recorded in the household roster, based on which we can infer whether each household member is a child of the respondent when respondents are the household head.⁸ It was not necessary to apply this filter to the ESS, since the ESS assessed the relationship of each household member with survey respondents directly. That said, we admit the possibility that the sex of the first child in the household is not necessarily the same with the sex of the first child in one's fertility history. To account for this possibility, we excluded respondents who report any deviance between the number of children residing in the household and total number of born children in the GSS (this strategy is not possible in the ESS). After detecting all children currently residing in the household, we excluded respondents with no child 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 oldest children are the same age.⁹ Of course, it could still be the case that the total number of children in the household matches the total number in the fertility history, yet the children are different. However, we think this would be a trivial number of cases and should not bias our results.

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.¹⁰ However, in contrast to the 1994 GSS sample used by [Conley and Rauscher \(2013\)](#), in our GSS multiyear sample and the ESS sample we must address the potential concern that children 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 (e.g., [Oswald and Powdthavee 2010](#)). To minimize this problem, we restrict our sample to parents whose oldest child is younger than seventeen and thus likely to be living at home and be captured in the household roster. Table S1 summarizes the sample selection criteria; as a result of our analytic sample filters, in the GSS, 5,571 observations (9.8 percent) remain out of a total potential N of 57,061 and roughly 20 percent of cases remain in the ESS. Table S2 in the

online appendix reports the variables' wording and coding in the GSS and ESS, respectively.

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 in the United States and the UK. Due to the nature of multiparty systems in most European countries, it is hard to measure party identification as cleanly as in the cases of the United States and the UK, and therefore we decided to focus on two countries for the replication exercise. We deploy a binary indicator for right-conservative (or Republican) and a continuous scale for Republican (or right-conservative) party identification.¹¹ While survey instruments for measuring party identification differ between the GSS and the ESS and thus might not be comparable, these instruments are the same ones used in previous analyses (Conley and Rauscher 2013; Oswald and Powdthavee 2010)¹² 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 extended analysis, political ideology is the outcome of interest in the entire range of countries in the ESS as well as for the GSS sample. However, due to the different scales in the GSS and the ESS measuring the strength of political ideology, we transformed the variable into the percentage index from -100 percent liberal to 100 percent 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). Although it is reasonable to assume that children do not leave home until the age of sixteen (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 seventeen 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 firstborn 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; or perhaps families with low socio-economic status need to cohabit for longer relative to families with high socio-economic status. Thus, parent's age, sex, years of education,¹³ an indicator for native-born status, and age of the oldest child are included as pretreatment covariates, which could not be affected by our treatment variables.

Country Heterogeneity

To address the potential for country-specific heterogeneity,¹⁴ we deploy a multi-level 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:

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

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 primarily interested in estimating two parameters; θ is the fixed coefficient for the average treatment effect, and ζ_{2j} is the vector of random coefficients modeling the interactions between country-level indicators and the treatment variable (X_{ik} includes pretreatment covariates and survey year dummies and ζ_{1j} refers to the random intercept). To test whether treatment effects significantly vary across countries, we estimate $\text{Var}(\zeta_{2j})$ and test the null hypothesis of zero variance of the random slope using the likelihood-ratio test (Rabe-Hesketh and Skrondal 2012, 197).

Heterogeneous Treatment Effects

Heterogeneous treatment effects are usually obtained by estimating the average treatment effects conditioning on certain covariates or estimating the average treatment effects within subgroup. For example, to assess the treatment heterogeneity by the parental socio-economic status, we can split our respondents into high-, middle-, and low-status subsamples and then estimate the average treatment effects of the first child's sex separately for each group. However, this approach introduces several concerns, such as multiple hypothesis testing, misspecification of functional forms, and the curse of dimensionality. Specifically, if the number of covariates is large, then concerns for multiple hypothesis testing or “ p -hacking” arise (Gelman and Loken 2014). If the covariates are continuous, the problem becomes worse, since the results from estimating the heterogeneous treatment effects by including the interaction terms or by performing separate subgroup analyses might depend on how researchers choose to divide the sample into subgroups or how to specify the functional forms of interactions (e.g., linear versus nonlinear interactions). To reduce the potential for researchers' discretion when estimating heterogeneous treatment effects, Green and Kern (2012) propose the Bayesian Additive Regression Trees (BART) model as a nonparametric modeling strategy to automatically detect the treatment effect heterogeneity, which was first developed by Chipman, George, and McCulloch (2007, 2010). We use the modified version of the `BayesTree` package for R by Green and Kern (2012), which was initially developed by Chipman and McCulloch.¹⁵

Here, we briefly explain the intuition behind how BART works.¹⁶ BART is a sum-of-trees model, in which a single tree model repeatedly splits observations into homogeneous subgroups and calculates fitted values (i.e., mean) in each group. Since the decision rule to split observations and the variables that are used in each single tree model vary across different trees, the ensemble of those trees with varying size can capture both the main effect (i.e., a tree depending on only one variable) and the interaction effect (i.e., a tree depending on more than one variable) as well as the nonlinearity of variables (i.e., each tree has a different rule as to whether to split). Formally, the observed outcome vector \mathbf{Y} is modeled as a combination of sum of single trees, $g(\mathbf{X}, \mathbf{z}; \mathbf{T}, \mathbf{M})$ (i.e., a set of step functions), and a normal error:

$$\mathbf{Y} = f(\mathbf{X}, \mathbf{z}) + \boldsymbol{\varepsilon} = \sum_{j=1}^m g(\mathbf{X}, \mathbf{z}; T_j, M_j) + \boldsymbol{\varepsilon}, \boldsymbol{\varepsilon} \sim N(\mathbf{0}, \boldsymbol{\sigma}^2),$$

where M_j is a set of fitted values for each subgroup in a tree T_j , m is the number of trees, \mathbf{z} is the treatment vector, and \mathbf{X} is a matrix of pretreatment covariates. BART treats (T_j, M_j) and σ as parameters in a formal statistical model in that a prior is put on the parameters and the posterior is computed using a Markov Chain Monte Carlo (MCMC) algorithm, at each of which (T_j, M_j) and σ are redrawn.¹⁷ After fitting the response curve for the outcome \mathbf{Y} across a set of ranges of our covariates \mathbf{X} including a treatment indicator, we ask BART to generate 1,000 posterior draws from the joint posterior distribution over some subset of the sample (i.e., $C\{(f(x_1, 1) - f(x_1, 0)), \dots, (f(x_k, 1) - f(x_k, 0))\}$). In this way, we can obtain the conditional average treatment effect (CATE) conditioning on \mathbf{X} values for each individual (1) as well as for the sample (2):

$$E(Y_i(z = 1)|X_i) - E(Y_i(z = 0)|X_i) = \frac{1}{1000} \sum_{j=1}^{1000} f(1, X_j) - f(0, X_j) \quad (1)$$

$$\frac{1}{n} \sum_{i=1}^n E(Y_i(z = 1)|X_i) - E(Y_i(z = 0)|X_i) = \frac{1}{n} \sum_{i=1}^n \left[\frac{1}{1000} \sum_{j=1}^{1000} f(1, X_j) - f(0, X_j) \right]. \quad (2)$$

Based on CATEs estimated by BART, we will first show how strongly heterogeneous treatment effects are present in the data by comparing the observed distribution of heterogeneous treatment effects against ten simulated null distributions of heterogeneous treatment effects. In these null distributions, pretreatment covariates are randomly permuted, thereby breaking the association between the covariates and the outcome while the relationship between the outcome and the treatment remains intact. In other words, we can see whether the observed heterogeneous treatment effects might also have been obtained by chance among random subgroups within a random set of covariates. Next, we turn to statistically examine the strength of the heterogeneous treatment effects by conducting Wald tests.

Identification and Inference Issues

One potential threat to our identification strategy is offered by the aforementioned Trivers-Willard hypothesis, which would cause non-randomness to the sex of offspring. In this vein, evidence has been mixed; for instance, [Hopcroft \(2005\)](#) found a significant interaction effect between parental sex and parental occupational status based on the 1994 GSS sample: high-status men were more likely than low-status men to have boys, but high-status women were more likely than low-status women to have girls. We successfully replicate this result using the 1994 GSS sample (and other samples), but also show that it is an artifact of the GSS 1994 sample. Table S3 in the online appendix shows that if we predict the sex of the first child instead of the proportion of daughters, the interaction effect becomes weaker and the main effect of socio-economic status loses its significance in 1994. When we apply the same analysis to our analytic sample (i.e., the entire GSS and ESS), none of the effects are significant and the coefficient of the interaction term on the first child's sex is marginally significant

but with the opposite sign of the previous findings (while none of the main effects are significant). Further, figure S1 in the online appendix shows that the previous finding was likely to have been obtained by chance in 1994, since year-by-year analysis shows that the signs of coefficients of the interaction term as well as main effects for each parental gender are largely insignificant and vary across periods.

Although 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/stepchildren might raise identification problems. To address this potential concern, we conducted balancing tests using *t*-tests on the mean difference of each pretreatment covariate. The parent's sex, age, country of birth, years of education, and the age of oldest child 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, as shown in table 1.¹⁸ As the countries in ESS and the United States 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 stepchildren, we include pretreatment variables in our statistical models; results are indifferent to their inclusion or exclusion, and we will show both in the tables.

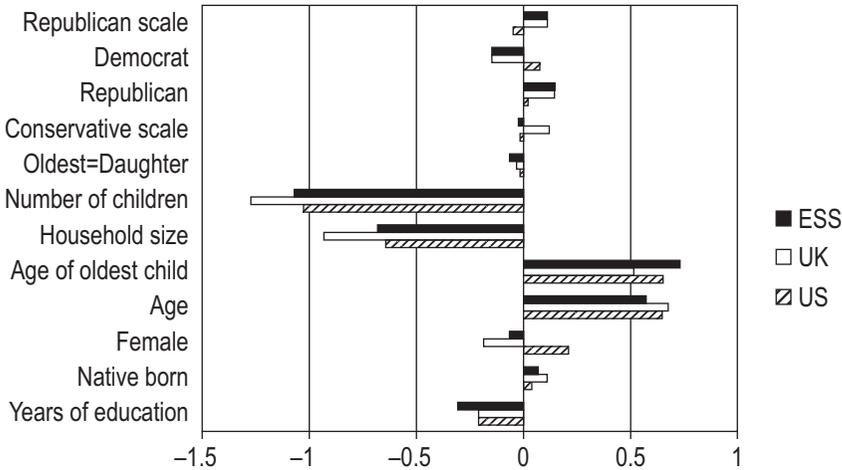
The external validity of our estimates may be another potential concern. To increase internal validity (i.e., make sure the full census of offspring are measured), only those who have at least one (eldest) child who is younger than seventeen are included in our analytic sample. We may be identifying effects that are not generalizable to all families (i.e., most notably, to those with eldest children age seventeen or over). This is an important limitation, since it may be the case that party identification is affected by offspring gender only while the children

Table 1. Identification Issue: Balancing Tests on Treatment Assignment—Sex of Oldest Child

	US			UK			ESS		
	Difference in means	T-stat	P-value	Difference in means	T-stat	P-value	Difference in means	T-stat	P-value
Female	0.02	1.21	0.23	-0.02	-0.88	0.38	0.00	-0.24	0.81
Age	0.01	0.06	0.95	0.39	1.31	0.19	0.06	0.92	0.36
Native born	0.00	-0.50	0.62	0.01	0.50	0.62	0.00	-0.23	0.82
Age of oldest child	0.12	0.90	0.37	0.01	0.07	0.94	0.00	0.09	0.93
Years of education	0.17	2.24	0.03	-0.12	-0.98	0.33	0.04	1.44	0.15

Note: Differences in means (mean among parents of firstborn daughter – mean among parents of firstborn son) are calculated by using regression models after controlling for survey year dummies. In the ESS sample, country fixed effects are estimated. The only significant difference observed is for years of education in the United States at $p < 0.05$.

Figure 1. Inference issue: Mean differences of covariates (full sample – analytic sample) in standard deviation units



Note: The differences of the means between the full sample and the analytic sample are divided by the standard deviations of the full sample. Each bar represents the percentage difference in standard-deviation units. This figure is created based on table S4 in the online appendix.

are minors residing in the parental home, and that when they set up their own households and contact is reduced, parents revert to the “pretreatment” political ideology.

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 S4 in the online appendix). While there are no discernible differences for our major outcome and treatment variables (less than a 0.2 SD difference), 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 child) 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 seventeen), and more educated. The notable difference between the ESS and GSS samples is with respect to the gender of parent; in the United States, men are slightly more likely to be in the sample, as compared to the UK sample or the ESS more generally. This is mainly because we included the household head only in the GSS. Nevertheless, if the sex of the 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 2 below reports the replication results from estimating the effects of daughters on party identification. In the UK, daughters tend to make their parents

Table 2. Replication Analysis: OLS Results for the Effects of the First Child's Sex on Party Identification in the UK and the United States

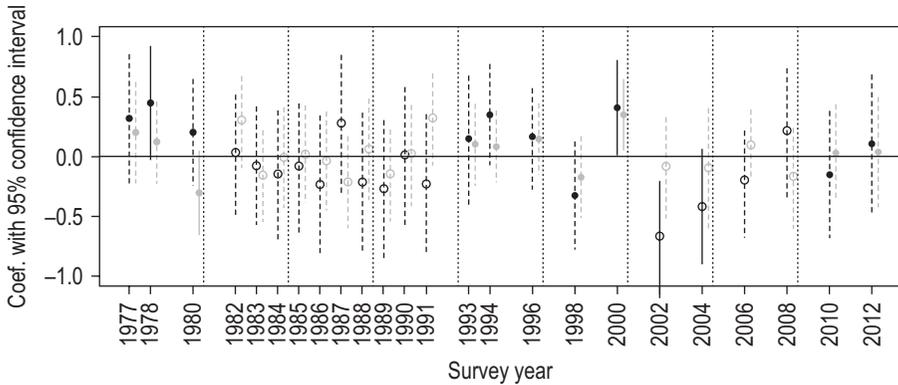
Controls included?	US				UK				UK during overlapping period (2002–2005, ESS waves 1 and 2)			
	Republican scale		Republican indicator		Republican scale		Republican indicator		Republican scale		Republican indicator	
	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Oldest = Daughter	-0.012 (0.053)	-0.002 (0.052)	-0.002 (0.012)	-0.002 (0.012)	0.10 (0.10)	0.09 (0.10)	0.03 (0.03)	0.02 (0.03)	0.12 (0.17)	0.13 (0.17)	0.05 (0.05)	0.05 (0.05)
N	5179	5179	5179	5179	1000	1000	1005	1005	321	321	324	324
Adj./pseudo R ²	0.010	0.046	0.011	0.051	0.013	0.032	0.010	0.033	-0.003	0.017	0.002	0.018

Note: A list of controls includes parent's age, sex, nativity status, years of education, and age of oldest child. All regression models include survey year dummies. Standard errors are in parentheses. None of effects are statistically significant at $p = 0.1$ or 0.05 .

closer to the right-conservative (Conservative) party; if parents have a girl for their first child, they are more likely to lean toward the right-conservative (Conservative) party. The effects of the first child's sex do not support the previous study: a firstborn girl leads to a two-percentage-point *increase* in Conservative party identification, though the effects are insignificant.¹⁹ We admit that these insignificant effects may merely reflect the smaller sample size relative to Oswald and Powdthavee's (2010) sample, but it is notable that they are in the opposite direction of the prior 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), 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.

Likewise, the effects in the United States 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. We also find significant effects of the first child's sex in 2002

Figure 2. Period variation: The effects of the first child's sex on parental party identification and political ideology in the GSS by survey year



Note: The bivariate regression estimates of effects of having an oldest daughter on party identification (black) and political ideology (gray) with 95 percent confidence intervals are plotted. The filled dots represent periods when the ruling (presidential) party is Democratic, and the empty dots represent periods when the presidency is held by a Republican. Statistically significant effects (at $p < 0.1$) are displayed by solid lines.

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 negative coefficients in general, we cannot identify a statistically discernible pattern across periods for the US case. In general, we fail to reproduce the reported effects of offspring sex on party identification in both the United States and the UK.

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 3. It shows that the direction of effects is what we would expect from the previous study; daughters make their parents more conservative in the United States. Nevertheless, the effect in the United States might reflect the peculiarity of the 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 data sets provide.

Table 3 reports results from analyses of these pooled data. The effects of the sex of the first child are not only insignificant, but also the point estimates are themselves much closer to zero (e.g., 5.99 versus 1.18 in the GSS). How small would these effects be 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 United States and a 0.3-percentage-point decrease in the ESS sample. But of course, these estimates are not statistically discernible from the null of zero effect.

To examine the possibility of country-specific heterogeneity, we estimate the effects of the first child's sex on the political ideology in each of all the 36 countries represented in the GSS and the ESS together. Figure 3 summarizes three estimates

Table 3. Extended Analysis: OLS Results for the Effects of the Sex of the First Child on Political Ideology in the GSS and ESS Samples

Controls included?	GSS 1994				GSS				ESS			
	Conservative scale		Conservative indicator		Conservative scale		Conservative indicator		Conservative scale		Conservative indicator	
	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Oldest = Daughter	5.99*	5.88*	0.03	0.02	1.18	1.49	0.01	0.01	0.264	0.260	-0.001	-0.001
	(2.88)	(2.88)	(0.03)	(0.03)	(1.30)	(1.29)	(0.01)	(0.01)	(0.380)	(0.379)	(0.004)	(0.004)
N	1027	1027	1027	1027	4664	4664	4664	4664	47705	47705	47705	47705
Adj. R-sq	0.003	0.006	-0.000	0.007	0.002	0.024	-0.001	0.031	0.047	0.050	0.041	0.049

Note: A list of controls includes parent's age, sex, nativity status, years of education, and age of oldest child. All regression models include survey year dummies. Standard errors are in parentheses. * $p < 0.05$. None of effects are statistically significant at $p = 0.1$.

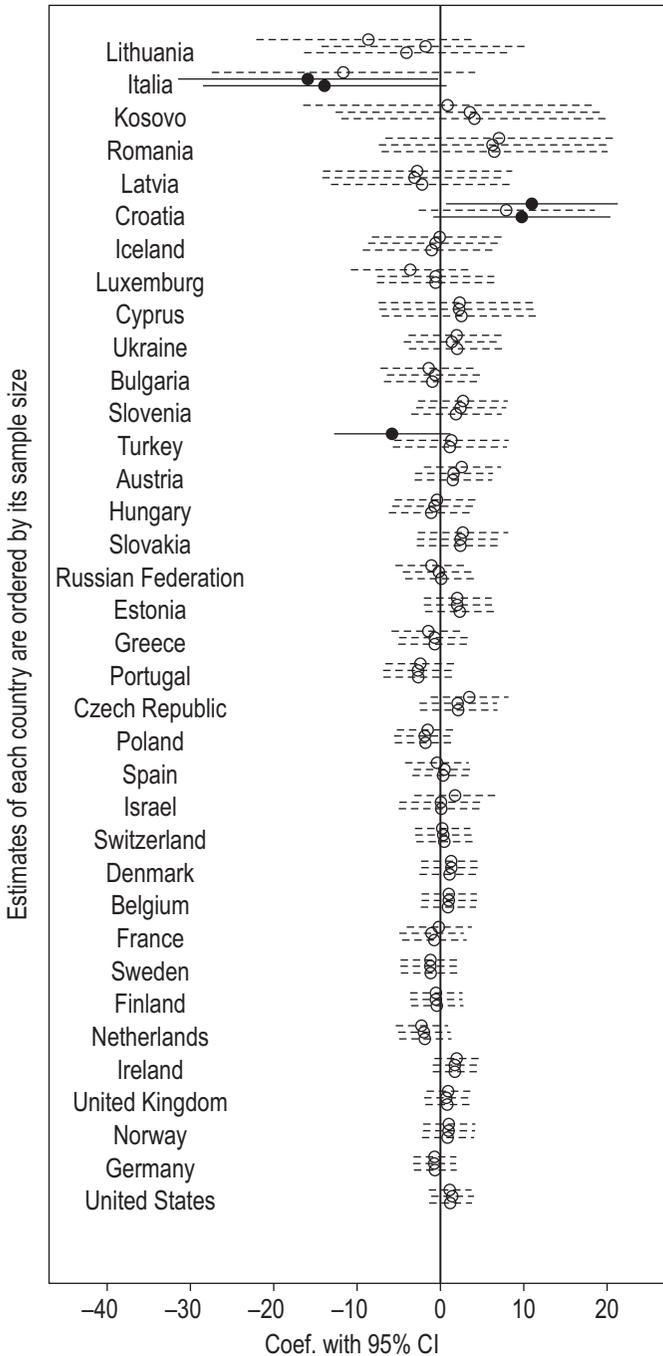
by estimating models before and after including pretreatment controls, and with adjustments by sampling weights. (Countries are ordered by their sample sizes.) Null effects are observed in almost all countries except for Italy, Latvia, and Croatia, of which sample sizes are small and thus sampling variability is large. Since these effect sizes appear to be strangely large given the small sample sizes in those countries, the significant effects might simply result from sampling aberrations or political peculiarities 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 cutoff should be $p < .0014$.)

Table S7 in the online appendix shows the results from testing for zero variance of random coefficients among thirty-five 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 effects 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 for the reported random slope variances (0.27 SD) shows that we cannot reject the null hypothesis (p -value = 0.65). This confirms the null patterns in figure 3—namely, that the treatment effects across countries do not statistically significantly vary.²⁰

Heterogeneous Treatment Effects

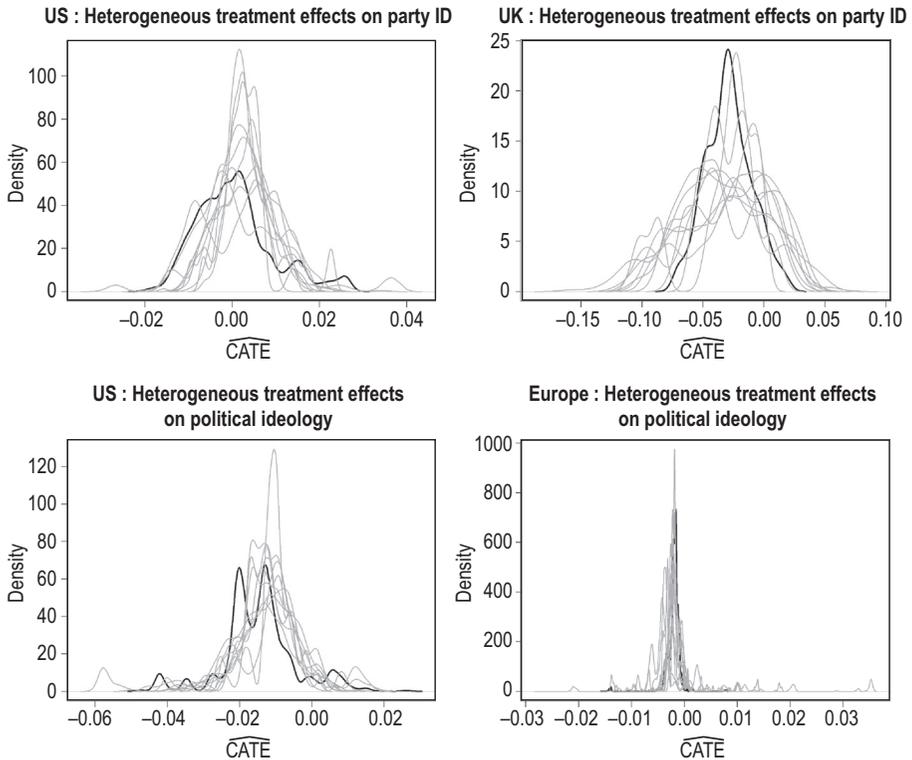
To assess the possibility of heterogeneous treatment effects, we first conduct the exercise that compares the distribution of observed CATE estimates based on the

Figure 3. Country variation: The effects of the first child's sex on parental political ideology in 36 countries



Note: The three estimates in each country represent the effect without controls, with controls, and with sampling weights adjustments, respectively; solid lines refer to statistically significant estimates at $p < 0.1$.

Figure 4. Assessing the degree of heterogeneous treatment effects: Kernel density plots of the observed CATE estimates (black) and ten simulated hypothetical CATE estimates (gray)



Note: In each graph, a black curve shows the observed distribution of conditional average treatment effects (CATE) in the analysis, and ten gray lines show ten kernel density plots of CATE estimates when individuals' pretreatment covariates are randomly permuted. The significant overlap between the observed and simulated CATEs indicates that the observed distribution of CATEs can be possibly obtained by chance.

set of pretreatment covariates against the simulated CATEs by randomly permuting the covariate matrix. Figure 4 clearly shows the general absence of heterogeneous treatment effects across all samples in the analysis inasmuch as the observed distribution and the ten simulated distributions are indistinguishable. In other words, our observed distribution of CATEs can be obtained by chance in the same way that a randomly permuted set of covariates could generate a similar distribution of CATEs. Especially when it comes to the range of observed CATEs, we can confirm that the effects are very small and their variabilities are also small (less than a 0.1-percentage-point change in political ideology score at most). In this analysis, we find that our pretreatment covariates as a whole provide no evidence for heterogeneous treatment effects, and we next turn to an examination of each covariate in each country.

Figure S2 in the online appendix shows the results in US and UK samples from a BART analysis of estimating the interaction effects of five pretreatment covariates with having an oldest daughter on party identification. It clearly shows that

the treatment effects across subgroups rarely change for all five covariates in both countries and that 95 percent posterior intervals always include zero. Table S8 in the online appendix reports the results from two-sided Wald tests on the presence of heterogeneous treatment effects on political ideology in all thirty-six countries. Notably, no single interaction effect is significant, which formally supports the results of figure 4.²¹

Discussion

Despite the allegedly stable nature of party identification (especially during adulthood), two recent studies purported to show that the sex of a 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 parties in the UK, whereas [Conley and Rauscher \(2013\)](#) found the opposite in the United States. We raised three plausible explanations to account for these contradictory results: country or period heterogeneity, heterogeneous treatment effects, and publication bias with Type I error. We then evaluated these possibilities using independent samples, including those from the GSS and ESS.

First, we found no statistical discernible effect of the first child's sex on party identification in both the United States and the UK, nor on parental political ideology in thirty-six countries. 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 applying the same analytic strategy to the Korean General Social Survey (see table S11 in the online appendix), though we would not interpret results for Korean evidence as causal estimates as in the case of Europe and the United States. due to the knowingly greater prevalence of prenatal sex selection in Korea. Moreover, as figure S3 in the online 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, and thus researchers might mistakenly conclude that having a daughter would make Korean parents liberal if they only looked at, for example, the 2007 sample.

In addition, we failed to find evidence for heterogeneous treatment effects across differences in parental gender, native-born status, age, years of education, and oldest child's age when we employed BART analysis. This complete absence of heterogeneous treatment effects not only bolsters our claims regarding null findings but also contributes to the recent discussion on the conditions that researchers must satisfy in order to claim null findings ([Rainey 2014](#); [Gross 2014](#)). Recently, for example, [Rainey \(2014\)](#) suggested a simple but powerful approach (TOST; two one-sided tests) that researchers can rely on to argue for a null finding: if the 90 percent confidence interval for the quantity of interest does not contain any meaningful effect, then reject the null hypothesis of a meaningful effect. That said, it is often possible to find heterogeneous treatment effects conditional on a set of covariates, multiple hypothesis testing concerns aside; as

a result, we encourage researchers to present *all* theoretically plausible conditional average treatment effects without discretion using BART models.

However, bearing this in mind, we are not simply arguing 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 a 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)—as either 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 orientations 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/stepchild with the samples we used. It is possible that there indeed exist treatment effects, but they are canceled 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 the parent or the oldest child that were reported. Notably, it may also be applied to the longitudinal design because we never know the exact timing of the effect.

The remaining plausible explanation to the puzzle is provided by the possibility of Type I error, which 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 small or modest effects. For example, Urbatsch (2011) found that having older sisters leads to a more liberal ideology based on the 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, and 1997 ($N = 330$), and the National Longitudinal Survey of Youth 79 in 2006 and 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 possibly period heterogeneity²²) and/or publication bias. As a corrective to this

source of bias, we here add comprehensive null findings to the already polarized canon of significant results.

Notes

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 the core components of political orientation in this paper, as the distinction itself is not of central interest to our study.
2. In this paper, 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.
3. The sex of the child has also been used as an instrumental variable despite potential concerns about violation of the exclusion restriction assumption (e.g., [Conley and McCabe 2012](#); [Angrist and Evans 1998](#)). However, in the present study, we do not use the sex of the child as an instrumental variable, and focus instead on the first-stage effect only (i.e., reduced form analysis), thereby increasing its internal validity as a natural experiment.
4. Prior studies of the effects of sibling sex mix on party identification or political ideology have also reported mixed results; in the same country (the United States), [Urbatsch \(2011\)](#) found that having older sisters led to a more liberal ideology using General Social Survey 1994 data, whereas [Healy and Malhotra \(2013\)](#) found that having sisters caused men (but not women) to be more likely to identify as Republicans using the Political Socialization Panel (in 1965, 1973, 1997) and the National Longitudinal Survey of Youth 79 (in 2006, 2008) data.
5. The so-called Michigan school defines 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”; that is, an affective attachment to a group that formed young, persists throughout the life course, and is relatively invariant to the external shocks ([Johnston 2006](#)). By contrast, the so-called “revisionist perspective” considers party identification as a “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 ([Green and Palmquist 1990](#); [Schickler and Green 1997](#)).
6. Given that traditional gender role attitudes are positively related to conservative political ideology and Republican party identification ([Brooks 2000](#); [Cotter, Hermesen, and Vanneman 2011](#)), having a daughter may not make parents more inclined to liberal parties and positions if daughters are simply perceived as future mothers or wives; that is, having a daughter may simply reinforce traditional gender role ideology, thereby leading to no change.
7. Both data sets are publicly available and can be downloaded here: <http://www.europeansocialsurvey.org> and <http://www3.norc.org/GSS+Website/>.
8. If the survey respondent is a spouse, we excluded these cases to prevent the possibility that the spouse is not the biological mother or father to the children of the household head.
9. They can be either a twin set, two consecutive births within twelve months of each other, or simply the result of measurement error.
10. 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 conditionally random only if there is no family-specific, gender-based parity stopping rule. If there are unobserved factors (e.g., 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 daughters 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 are available upon request).

11. As reviewers suggested, to account for the nonlinearity of our outcome measures, we also employed logistic regression for the binary indicators and ordered logistic regression for the scale variables; results are the same. (Please see tables S5 and S6 in the online appendix.) In the main text, we report the results from linear probability models for ease of interpretation.
12. For the UK case, the Conservative party is classified as the Right-Republican party, and the Labour and Liberal Democrat parties as the Left-Liberal party. Due to the difficulty of classification of other parties on a unidimensional left-right partisan scale, individual voters for other political parties are excluded in the analysis. Among 13,403 sampled, 50.29 percent (6,741) respond that they do not feel closer to any particular political party and 2.13 percent (285) respond “don’t know/no answer.” Among party identifiers, 2,077 (32.57 percent) are classified as Conservative and 2,731 (42.83 percent) and 826 (12.95 percent) are respectively identified as Labour party and Liberal Democrat. Meanwhile, 743 people (11.65 percent) feel closer to other parties including the Green party and Scottish National party, and were dropped from the analysis.
13. Instead of a socio-economic index, we use years of education as a proxy to measure parental socio-economic status for two reasons. First, because of the large number of missing cases in the socio-economic indices in the GSS and ESS data (45 and 12 percent are missing, respectively) additional loss of cases would be unavoidable if we were to use SEI in lieu of education. Second, recent literature found that having a daughter increased the probability of her mother’s employment (Weitzman 2013), which implies the possibility of occupation-based socio-economic index partially being driven by the sex of offspring.
14. Because the number of available periods in the ESS is limited (six at maximum), we cannot get reliable estimates for period as a level-2 unit, and thus we focus on country heterogeneity in this analysis.
15. <http://artsandsciences.sc.edu/people/kernh/publications/bart.html>.
16. We do not attempt to introduce all the technical details and their implications for BART here. For further information on BART models, please refer to Chipman, George, and McCulloch (2010); for technical details and for the application to non-parametric causal inference in observational studies, see Hill (2011) and Hill and Su (2013); finally, see Green and Kern (2012) for modeling heterogeneous treatment effects in experimental studies.
17. Chipman, George, and McCulloch (2010) provide default settings for these priors. It is shown that the default tuning parameters work quite well and BART’s MCMC algorithm yields stable results (Hill and Su 2013; Hill 2011; Green and Kern 2012), and thus we run BART models using the default tuning parameters with a burn-in period of 100 draws followed by 1,000 draws from the posterior distribution and 200 trees.
18. One rare exception is the difference in years of education between parents with a first-born daughter and a firstborn son in the United States. By nature of the hypothesis testing, it is simply plausible to observe one significant test result out of fifteen tests ($1/15 = 0.07$) at p -value = 0.05. Furthermore, it is also conceivable that parental education might be affected by the sex of the child, since child sex may affect the educational continuation decisions of parents for certain populations (e.g., young single mothers).
19. As a reviewer noted, a two–percentage-point increase can be said to achieve practical significance if the direction of the change is consistent with prior reports and robust

across periods or ages. However, we found the opposite of the relevant prior study. In addition, as shown in the US cases (in figure 2), the direction of effects of the sex of the first child seesawed across time and estimates are quite noisy. Thus, at the very least, our statistically null results also mitigate against the direction of findings in the prior literature.

20. To confirm that we were not missing any effects, we tested whether the effects of daughters on political ideology (i.e., the country-specific point estimates) are systematically 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 thirty-two country-level factors from the ESS MD-country-level data. Results (not shown, but available upon request) confirm that the effect of the first child's sex at the country level is not significantly related to any country-level factor.
21. We also tried to adopt the standard approach to assess heterogeneous treatment effects by including the interaction terms linearly in our regression models, which are reported in tables S9 and S10 of the online appendix.
22. As evinced, the previous findings (i.e., Conley and Rauscher 2013; Hopcroft 2005) that used the 1994 GSS sample, which contains the information about all biological children, are hard to be generalized to other periods. This may be due to the peculiarity of the 1994 GSS sample. Or, alternatively, as we have shown in figures 2 and S3, it could be the case that parents react to their children by focusing on particularly salient issues at the moment in time, thereby adapting their political attitudes corresponding to the time-varying political context in combination with the sex of their offspring (Highton and Kam 2011).

Supplementary Material

Supplementary material is available at *Social Forces* online, <http://sf.oxfordjournals.org/>.

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