Mechanisms Underlying Familial Influence on Elite Political Behavior: Evidence from the U.S. Circuit Courts of Appeals*

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Abstract

This article contributes to the literature addressing family influences on elite political behavior. By empirically assessing the influence of sibling gender on judicial decision-making, we are able to present evidence on the mechanism by which *child, sibling and other relatives'* gender may influence elite political behavior. We build on a dataset in Glynn and Sen (2015, AJPS) by mining various archival sources to compile data on the gender of circuit judges' siblings. We find no evidence that male judges' votes on so-called "women's issues" (employment discrimination based on gender or pregnancy, reproductive rights/abortion, and Title IX) are affected by whether they have a sister, and we are able to rule out large effects of a sibling's gender on male and female judges' votes. Our results imply that the relationship between family member gender and elite political behavior is driven by the desire to avoid costs of discrimination, rather than learning from family members.

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This article contributes to the literature addressing family influences on elite political behavior. Specifically, we examine the influence of sibling gender on judicial decision-making. Significantly, our approach allows us to present evidence on the mechanism by which *child, sibling and other relatives'* gender gender may influence elite political behavior. Though there is a large body of work in sociology assessing various effects and correlates of family background, very little of that research is concerned with *political* attitudes—let alone political behavior. The smaller literature in political science addressing familial influence is typically based on surveys of non-elite samples (for one review, see Urbatsch 2014, Ch. 1); much of the focus here is on parental socialization of children (e.g., Jennings, Stoker and Bowers 2009).

A number of early studies in judicial politics investigated how the *social backgrounds* of judges shaped their behavior (for reviews, see George 2008, 1349–1355 and Tate and Handberg 1991). However, these studies emphasized professional, partisan, and basic demographic factors; they did not consider family environment per se (on this point, see Glynn and Sen 2015, 39). The exception is the line of studies reviewed and culminating in McGuire (2015), on the effect of a judge's birth order on his subsequent judicial behavior.

Our immediate point of departure is Glynn and Sen (2015), which demonstrates that U.S. Courts of Appeals judges are more likely to vote in a liberal (i.e., pro female plaintiff) direction on so-called *women's issues* (i.e., employment discrimination based on gender or pregnancy, reproductive rights/abortion, and Title IX) if they have daughters, as opposed to sons. Following Washington (2008), which showed that U.S. Members of Congress are more likely to vote liberally if they have daughters, Glynn and Sen (2015) observes that, *conditional on the number of children one has*, the number of daughters one has can be treated as-if randomly assigned.[1] This allows for a causal interpretation of the correlation the authors find between having daughters and voting liberally on women's issues. Still, there remain questions about the mechanism that underlies the relationship. Glynn and Sen (2015, 40–42) considers several possibilities (see also Washington 2008, 313).

Protectionism. One, it could be that judges become *protectionist* of their daughters: on

[1] A potential complication, fertility stopping rules (Washington 2008, 317–318), is discussed below.

this theory, a desire to protect daughters from harm would make judges more likely to vote liberally on employment and education discrimination cases, but more conservatively (i.e., pro-government) on criminal cases. However, the authors ultimately dismiss this mechanism, since they find no relationship between daughters and ideological direction of the vote in *criminal* cases (Glynn and Sen 2015, 47).

Lobbying. A second possibility is that daughters affect judges by *lobbying* their parents: on this account, a daughter places social pressure on her judge parent to vote liberally. (Under the version proposed in Glynn and Sen (2015), daughters are assumed to be relatively liberal on all issues, and each daughter's lobbying has an independent impact on their parents.) But the authors also reject this mechanism, since (i) daughters appear to affect voting only on women's issues, and (ii) even on women's issues, having *more than one* daughter has little or no additional effect on voting (i.e., beyond the effect of the first daughter) (Glynn and Sen 2015, 51).

Learning. It also could be that judges' preferences change when they learn from a daughter about her experiences with educational and employment discrimination. The idea is that increased knowledge of difficulties facing women leads to greater empathy and thus a leftward shift in voting on women's issues. This leads to the expectation that daughters would liberalize voting only on women's issues, and that this effect would be greater for male judges, as they have "more to learn" (Glynn and Sen 2015, 40). The authors find substantial evidence for the first proposition, and more marginal support for the second.[2]

Costs. A final possibility is that judge preferences realign because of financial costs associated with having daughters who are discriminated against on the basis of gender. For example, judges with daughters "might have a pecuniary interest in seeing employment discrimination against their daughters be outlawed (Glynn and Sen 2015, 42)" or hope to avoid "the monetary hardship of an unwanted grandchild (Washington 2008, 313)." This mechanism implies that daughters would affect voting behavior specifically on women's issues, for

[2] The effect for male judges is statistically significant, the effect for female judges is not, but the difference between two effects is not statistically significant.

both male and female judges. Because of the "suggestive evidence" that the effect is greater for male than female judges, Glynn and Sen (2015, 51) favors learning, rather than costs, as the mechanism at play, but notes that "*definitively adjudicating between the two theories is difficult (emphasis added)*."

We hasten to emphasize that the learning and costs mechanisms are conceptually distinct. It is true that for a judge to respond to costs of discrimination, she has to learn about the existence of discrimination from *somewhere*. But there are any number of information sources besides one's female family members that could give the requisite information—for example, news reports, discussions with friends and colleagues, not to mention legal cases read or decided. Conversely, simply because a judge is aware that costs to discrimination exist, that does not necessarily mean that he expects those costs to accrue *to him*.

Turning now to the sparse (Urbatsch 2014, 12) literature on the political influence of siblings, we note two studies. Urbatsch (2011) also takes advantage of the (quasi-)random nature of child gender assignment to examine how one's political attitudes are shaped by a sibling's gender. The study, using data from the 1994 General Social Survey, demonstrates that respondents whose next-oldest sibling is female are more likely to identify with the Democratic Party and hold liberal positions on a range of issues. The theoretical explanation is that the next-oldest sibling is typically the most influential on one's socialization, and the widely-recognized ideological gender gap means sisters are typically more liberal than brothers (for details, see Urbatsch 2011, 694-701). (We note that Urbatsch's (2011, 700) theoretical explanation, though more focused on childhood socialization, is at least arguably consistent with the learning mechanism discussed above: "Women's s[k]epticism about markets may result from observing sex-based pay discrimination [...] such reluctance could spill over to younger siblings whom women help socialize. An individual whose next-older sibling is female, then, has a close, lifelong association with someone relatively likely to be acceptant of interventionist state activity.") Healy and Malhotra (2013) uses two panel surveys of non-elite samples to show that (conditional on the number of siblings) men who have at least one sister express more conservative views on gender roles and are more likely to be Republican; however, women's views and party identification are not affected by whether they have at least one brother.[3] The authors also suggest a mechanism underlying this result: survey evidence indicates that, growing up, boys who had no sisters were more likely to perform the "female-stereotyped household task" of washing dishes than boys with sisters (Healy and Malhotra 2013, 1024, 1032–1034), plausibly leading to a less-traditional view of gender roles.

Theory and Hypotheses

The studies reviewed above suggest several different ways in which judge voting could be influenced by their siblings' gender. Since the protectionism and lobbying mechanism from Glynn and Sen (2015) found little support, we give further consideration only to the learning and costs theories therein, along with Healy and Malhotra's (2013) explanation based on gender roles.

The learning mechanism implies a sister will have a liberalizing influence, particularly for male judges on women's issues. Given that most siblings stay in contact "throughout the life course," and that, indeed, "sibling relationships are the longest-lasting relationship that most individuals share" (Whiteman, McHale and Soli 2011), it is straightforward that the potential for siblings to learn from each other about discrimination is there, just as it is for parents to learn from (adult) children. Thus, we have $\mathbf{H_L}$: *Conditional on the number of siblings, having at least one sister causes male judges to vote more liberally on women's issues.*[4]

We argue that unlike the learning mechanism, the costs mechanism is expected to work differently for sibling influence than for parent-child relationship. Surely, the type of monetary costs that threaten parents if their daughters are subject to gender discrimination in

[3] Healy and Malhotra (2013) take an instrumental variables approach, using next-younger sibling gender as an instrument for sibling gender makeup; this approach is not typical in the literature (e.g., Washington 2008; Glynn and Sen 2015).

[4] We test the effect of having at least one sister, since Glynn and Sen (2015, Table 5, Column 1) find that the probability of voting liberally on women's issues is not a linear function of the number of daughters; rather the presence of at least one daughter is what drives the results.

employment, education, or access to reproductive health care, are inapplicable to sibling-sibling relationships; at a minimum, the potential costs to parents dwarf those to siblings. Indeed, the sociological literature is clear that obligations, including financial obligations, to adult siblings are of a different class than the obligations of a parent to an adult child. (E.g., "obligation [toward siblings] is much lower than that reported for parents and children and approximately on a par with that felt toward grandparents..." (White 2001, 556).) In light of such considerations, we propose $\mathbf{H_C}$: *The gender of a given sibling affects a judge's probability of voting liberally on women's issues, at most, minimally.*[5]

Finally, we can straightforwardly derive an expectation from the gender-role theory in Healy and Malhotra (2013). Since growing the presence of at least one sister is thought to lead men to support traditional gender roles, we have $\mathbf{H_{GR}}$: *Conditional on the number of siblings, having at least one sister causes male judges to vote more conservatively on women's issues.*

Data, Methods, and Analyses

Data. Our dataset is based on that compiled by Glynn and Sen (2015, 42–44), available at: `dvn.iq.harvard.edu/dvn/dv/ajps`. As relevant here, that dataset compiles information about circuit court judge voting, 1996–2002, in 990 gender-related cases (i.e., women's issues: Title IX, employment discrimination, pregnancy discrimination, abortion, and other reproductive rights suits brought by female plantiffs), and includes information about the number and gender of a judge's children, along with other standard control variables (circuit, race, gender, political party, age). We build on this dataset by searching census records, obituaries, oral history interviews, biographical profiles, and other archival sources to locate information on the number and gender of judges' siblings. (The sources of our data—i.e., electronic copies of documents and files—will be made available on request. Data and code will be posted upon

[5]In other words, a sharp null effect of sibling gender, in combination with the significant effect for child gender in Glynn and Sen (2015) would be evidence that costs drive both the relationships.

publication.) We were able to locate sibling gender information for 214 circuit judges who voted on cases in our dataset. We were unable to find information about sibling gender for 29 judges. After we correct missing data from the Glynn and Sen (2015) dataset on various controls, the sample sizes of the models we present are somewhat larger than the analogous ones in Glynn and Sen (2015)—compare $N = 2113$ in our Table A3 to $N = 1507$ in Columns 4 and 5 of Table 5 in Glynn and Sen (2015).[6]

Methods. The key insight in Washington (2008) and Glynn and Sen (2015) is that conditional on the decision to have a child, the gender of a child can be treated as-if randomly assigned; in other words, the number of daughters one has, conditional on the number of children is plausibly random (of course, it is crucial to control for the number of children, since the number of daughters one has is a function of the number of children one has).

A potential threat to this line of reasoning are "fertility stopping rules," under which parents decide to have another child based on the gender composition of their existing set of children. For example, imagine a hypothetical society where there is a norm of having no more than n children. Assume that there are two types of parents: one type that abides by the norm unconditionally, and another type—which is "biased in favor of sons"—that abides by the norm unless they have no sons among the first n children, in which case they will have another child in hopes of having at least one son. In this society, the gender of a given child is not random conditional on the number of children in a family; specifically, conditional on having $n+1$ children, a child is more likely to be a girl than a boy. Note that the deviation from randomness caused by such potential fertility stopping rules is not problematic for inference in and of itself; it is problematic only if the attitudes that give rise to the fertility stopping rule (a) affect the behavior (outcome) of interest and (b) are not controlled for in the analysis.

As studies of this kind acknowledge (e.g., Washington 2008, 317), it is impossible to absolutely rule out bias from fertility stopping rules. There are several reasons to believe, however, that fertility stopping rules are less of a threat to inference here than in Glynn and Sen (2015) and Washington (2008). First, fertility stopping rules are not easy to implement. Even today,

[6] We employ listwise deletion during estimation, as do Glynn and Sen (2015).

up to one-third of births in the developed world are unplanned (Urbatsch 2014, 123); this proportion was surely greater in the first half of the 1900s, when the judges in our study and their siblings were born. Second, in Glynn and Sen (2015) and Washington (2008), which focus on effects of having daughters, the concern is that judges' and legislators' attitudes about gender, as reflected through their (potential) use of fertility stopping rules, are shaping their *own* voting behavior. In our analysis, that threat is one level removed: bias could occur if a judge's *parents'* attitudes reflected in the use of fertility stopping rules shaped their child's (i.e., the judge's) subsequent voting behavior. This is surely less of a threat, even if it cannot be entirely dismissed. Finally, we can bring some empirical evidence to bear on the prevalence of the fertility stopping rule perhaps most likely to cause bias—whereby conservative parents are biased in favor of sons and so stop having children only once they have at least one son. As suggested in the illustration above above, this could (given certain ancillary assumptions) lead conservative parents to be more likely to have daughters than sons, even conditional on the number of children parented. A rough empirical test can be conducted as follows. Select the gender of a random child in a family, and examine whether conservative parents are more likely to have more children if that child is female (for a conceptually very similar test, see Washington 2008, 317). For simplicity, we take the judge as the random child, and, of necessity, assume that judges appointed by Republican presidents have parents who are relatively conservative. A positive association between a Republican judge being a woman and the number of siblings would indicate that the fertility stopping rule hypothesized may be in effect. But in fact, the correlation is tiny and negative ($-.03$). Thus, it seems safe to proceed on the assumption that fertility stopping rules will not bias our results.[7]

[7]Readers may question whether sample selection bias is a possible threat to this research design. The hypothetical concern is that some unmeasured attitude u associated with siblings' gender affects whether someone becomes becomes a lawyer or a judge and thus eligible to enter the sample. If u is itself associated with judge votes on women's issues (the argument goes), failure to adjust for u may bias the effect of siblings' gender on women's issues estimated in the sample. There are several reasons that such concerns are unwarranted. The theoretical reason is that u (supposing it exists) is post-treatment; that is, sibling gender is logically and causally *prior to* u (given that sibling gender can be treated as-if randomly assigned, as we have argued above). Thus, even if u were observed, it would be inappropriate to adjust for it, if one is interested in the effect of sibling gender (*cf.*, for example, the canonical example of sample selection bias (Heckman 1974; Sartori 2003, 114–115), wherein the estimated effect of education on wages is biased by the ommission of intelligence, which is causally prior to educational attainment). For readers not convinced by the theoretical

Analyses. We test H_L and H_{GR} simultaneously, since each hypothesis centers on the effect for male judges of having at least one sister, in votes on women's issues; the difference is that H_L (Learning) predicts a sister will cause more liberal voting, whereas H_{GR} (Gender Roles) predicts a sister will cause more conservative voting. Following Glynn and Sen (2015), our sample is based on the population of cases involving women's issues where the plaintiff is female; in testing H_L and H_{GR}, we consider only male judges' votes. In Column 1 of Table 1, we present a (judge-vote level) logit regression model predicting the binary outcome *Liberal Vote*, as a function of whether the judge *Has A Sister* (=1) or not (=0). Indicator variables for whether a judge is *Catholic*, *African-American*, *Latino*, and whether a judge was appointed by a *Republican*[8] are as defined in Glynn and Sen (2015). Unlike Glynn and Sen (2015), we enter *Age* at the time of the vote into the regression, not age at time that the judge was commissioned. We also control for whether a judge *Has A Daughter* (=1) or not (=0), and enter fixed effects for the number of siblings and children, circuit, and year. Column 2 adds fixed effects for issue area. Thus, these specifications are analogous to those in Table 4, Columns 3 and 4, respectively, from Glynn and Sen (2015, 48). (A host of alternative specifications, including various judge-level weighted least squares regressions, do not reveal any differences that are relevant to our hypotheses. Our inferences are robust to twoway clustering by judge and case, bootstrap clustered standard errors by case, and random effects (intercepts) by judge. These specifications are included with replication code.)

We find no support for either H_L or H_{GR}; there is no evidence that having a sister, for male judges, has an effect on voting in gender-related cases. Though the coefficient on Has a Sister is negative—as H_{GR} predicts, and contrary to H_L—the standard error is greater than

argument, we note that empirically, there is no evidence that a u as posited above exists. A recent, thorough analysis of sibling gender on labor market outcomes concludes that there is no evidence that sibling gender is associated with occupational choice (Rao and Chatterjee 2018, 1735). Moreover, our analyses of the 1979 National Longitudinal Survey of Youth dataset (available on request) produced no evidence that sibling gender is associated with whether an individual works in the legal field, as opposed to some other occupation. Finally, we note that both Glynn and Sen (2015, 52) and Washington (2008, 317–318) present arguments ruling out sample selection bias in their applications.

[8]There is a reasonable argument that whether an appointee was appointed by a Republican, and perhaps whether he is Catholic, is post-treatment and could induce bias. We include those variables here to maintain comparability with earlier work (Glynn and Sen 2015; Washington 2008), but we also estimate models where Republican and Catholic are omitted, which do not reveal relevant differences (see also footnote 12).

Covariate	Coefficient (1)	Coefficient (2)
Has a Sister	−0.090	−0.081
	(0.16)	(0.17)
Republican	−0.493	−0.492
	(0.13)	(0.13)
Age	0.006	0.003
	(0.01)	(0.01)
Catholic	0.024	−0.006
	(0.14)	(0.15)
African American	0.171	0.171
	(0.28)	(0.28)
Hispanic	0.125	0.082
	(0.33)	(0.33)
Has a Daughter	0.244	0.279
	(0.15)	(0.15)
FEs: Issue Area		X
FEs: Child, Sibling, Circuit, Year	X	X
Constant	−0.002	1.580
	(0.53)	(0.75)
N	1756	1754

Table 1. Dependent variable: Did male judge vote in favor of female plaintiff (i.e., liberally) in case involving women's issue? (1 = yes). Logit coefficients; standard errors in parentheses. Two observations in Model 2 dropped because of perfect prediction.

the magnitude of the coefficient. Thus, we consider next $\mathrm{H_C}$ (Costs). Since, under the costs mechanism, we should see a negligible effect of sibling gender for both male and female judges in gender-related cases, we also include female judge votes in the regression testing $\mathrm{H_C}$ (Table A3, Appendix). Otherwise, the sample is defined the same as for the regression testing $\mathrm{H_L}$ and $\mathrm{H_{GR}}$.

The means by which one should test a hypothesis that an effect will be negligible deserves some discussion. As an initial matter, it is important to point out that "statistical insignificance" is neither a necessary nor a sufficient condition for an effect to be substantively negligible.[9] Rather, evidence for a negligible effect entails an effect estimate that is near zero, and is precisely estimated (i.e., has a small standard error). More formally, we can follow

[9]It is not sufficient because an imprecisely estimated effect can be large and statistically insignificant, and it is not necessary because a very precisely estimated effect can be significantly different from zero, yet substantively negligible.

Rainey (2014), which points out that testing the hypothesis that some effect Δ is smaller in magnitude than m entails rejecting the *null* hypothesis that Δ is in $(-\infty, -m] \cup [m, \infty)$. Moreover, rejecting the null at the .05 level, in practice, entails ascertaining that the 90% confidence interval for Δ includes only values smaller in magnitude than m—that is, the 90% confidence interval for Δ must be a subset of $[-m, m]$).

It remains to set $|m|$, the threshold for a negligible effect. Defining m is unavoidably subjective, and—as Rainey (2014, 1085) notes—"the definition must be debated by substantive scholars for any given context because the appropriate m varies widely across applications." Here, we propose that, if the average effect of having one sibling be a sister, as opposed to a brother, changes the probability of a liberal vote in a given case by less than 0.1, that entails evidence in favor of H_C. Even as we acknowledge that this threshold is open to dispute, we emphasize that—regardless of the choice of m—the 90% confidence interval for the effect size contains useful information about the range of effect sizes that are plausible.

Specification	Effect Estimate	90% CI
Baseline (Table A3, Col 1)	0.0006	[-.028, .029]
Add Area FEs (Table A3, Col 2)	0.0028	[-.026, .031]
N	2113	2113

Table 2. Effect estimate for change in probability of voting liberally on women's issue when a judge's sibling is a sister, rather than a brother. See text for details.

Our estimates of effect size to test H_C are based on two judge-vote level logit regressions identical to those presented in Table 1, except we include a linear term for the number of *Sisters* a judge has instead of the binary indicator Has a Sister, and (as mentioned) our sample includes both male and female judges, so we control for whether a judge is a *Woman* (see Table A3, Appendix). From these regressions, we calculate our quantity of interest: the change in probability that a judge votes liberally on a case involving a women's issue when she has a sister, rather than a brother.[10] Other variables are set at their in-sample values.

[10]Technically, we calculate the change from zero sisters to one sister, but the effect sizes associated with

These effect estimates, and their associated 90% confidence intervals are presented in Table 2. For each specification, the estimated change in probability of a liberal vote caused by having a sister, rather than a brother is less than .01. The 90% confidence intervals indicate that effects greater than .04 are unlikely, and allow us to reject our null hypothesis that $|\Delta| > 0.1$.[11] We also estimate a number of alternate specifications (included in the replication code); across 36 specifications, the mean effect estimate is .014.[12]

Discussion and Conclusion

To summarize our empirical results: contrary to H_L, we find no evidence that male judges vote more liberally on women's issues if they have a sister; contrary to H_{GR}, we find no evidence that male judges vote more conservatively on women's issues if they have a sister; in support of H_C, we find that judges' votes on women's issues are influenced, at most, minimally by whether a given sibling is a sister, rather than a brother. We turn now to theoretical implications.

Most straightforwardly, these results suggest that any conservative attitudes about gender roles that result from having a sister do not affect behavior in the context we study here. This is true even though the attitudes measured in Healy and Malhotra (2013) are specifically about the respondent's approval of women working outside the home, and our sample is in fact composed overwhelmingly of employment discrimination cases.

Second, our results are consistent with the proposition that male judges are not learning from their sisters about difficulties facing women, or at least that their behavior is not affected by any learning that takes place. We argue that our findings imply that costs, rather than learning, are driving the results in Glynn and Sen (2015) and Washington (2008). (Recall that

going from one sister to two, or two to three, etc., differ only infinitesimally (and only due to logit's functional form).

[11] A post hoc power sensitivity analysis can be thought of as complementary. Our sample size, given $\alpha = .05$ and $\beta = .2$ (i.e., power = .8), allows us to detect effect sizes as small as .04, according to the method in Faul, Erdfelder, Buchner and Lang (2009). We thank the Editor for suggesting this approach.

[12] These specifications include alternative operationalizations of the key covariate (as a binary indicator and as fixed effects), weighted least squares models, models where Republican is omitted as potentially inducing post-treatment bias, and models where both Republican and Catholic are omitted as potentially post-treatment.

Glynn and Sen (2015, 51) states that empirically adjudicating between the two explanations is difficult; Washington (2008, 313) does not attempt to address the mechanism question.) We have proposed that tangible costs are much more likely to accrue to a parent whose daughter faces discrimination in education, work, and health care, than they are to a sibling whose sister is similarly maltreated. If we are right about this, then it straightforward that an explanation based on costs accounts for both our results and those in in Glynn and Sen (2015). Specifically, we conclude that a family member's gender will only affect judge behavior (and perhaps other elite political behavior) insofar as the actor in question stands to suffer tangible losses if a family member is discriminated against on the basis of gender.

There are two other ways to interpret the implications our results have for the mechanism at play in Glynn and Sen (2015). First, it may be that learning *is* what drives the impact of having daughters, but learning from sisters does not occur (or does not affect behavior if it does occur). This cannot be ruled out, but even this possibility has implications for the learning mechanism, in that it narrows its scope considerably: specifically, it suggests that the mechanism only operates on the very closest personal relationships (i.e., daughter-father, but not sister-brother). Granted, it is reasonable to suppose that male judges learn less from their sisters than from their daughters about gender discrimination.[13] But if male judges are learning from their sisters—even if to a lesser degree than from their daughters—we would expect to see at least a weak positive relationship between having a sister and voting liberally. Instead, the relationship we find for male judges is in fact negative (though statistically nonsignificant). Thus, there is no evidence for even a weaker version of the learning mechanism operating through sisters.

A second, more pessimistic, interpretation calls each proposed mechanism into doubt. Under this interpretation, the differences among our results, those in Healy and Malhotra (2013), and those in Glynn and Sen (2015) are attributable to the so-called file drawer problem, whereby statistically nonsignificant results do not enter the scientific record—either because

[13]But note that the working careers of judges are bound to overlap more with the careers of their siblings than with the careers of their children; this perhaps implies greater *opportunity* for judges to learn about gender discrimination from their sisters as compared to from their daughters.

editors and reviewers prefer to publish statistically significant results or because researchers prefer to submit significant results for publication—resulting in a set of published results that are not representative of true effect sizes and potentially contradictory (Franco, Malhotra and Simonovits 2014). Indeed, Lee and Conley (2016) presents evidence that the impact of child gender on political orientation among the mass public is close to 0, and that certain published results to the contrary cannot be accounted for by heterogeneous treatment effects (whether as a function of country, time period, or respondent characteristics).[14]

Such questions of interpretation aside, we have clearly demonstrated that the effect of sibling gender on judge voting on women's issues is, at most, negligible. We have argued that this implies that the liberalizing effect of having a daughter on male judge voting is in fact caused by a mechanism other than learning—namely, the costs mechanism. We acknowledge that two alternative interpretations of our results cannot be ruled out; still, we emphasize that all three interpretations imply a reduced role for learning from women as a mechanism for causing political elites to support feminist policies. And indeed, the narrowed scope of the learning mechanism has clear implications for how we understand the effect of family and personal relationships on elite behavior in general. Of course, additional empirical assessment of our results' implications is warranted (across different relationships and contexts)—we leave this for future work.

[14]Still, it should be noted that the Glynn and Sen (2015) and Healy and Malhotra (2013) analyses appear to be reasonably powered, which reduces the concern that the effects presented are exaggerated or have the wrong sign (Gelman and Carlin 2014).

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Appendix

Covariate	Coefficient (1)	Coefficient (2)
Sisters	0.003	0.013
	(0.08)	(0.08)
Republican	-0.457^{*}	-0.456^{*}
	(0.12)	(0.12)
Age	0.007	0.005
	(0.01)	(0.01)
Catholic	-0.003	-0.026
	(0.12)	(0.12)
Woman	0.105	0.079
	(0.15)	(0.15)
African American	0.154	0.159
	(0.26)	(0.27)
Hispanic	0.257	0.236
	(0.29)	(0.29)
Has a Daughter	$0.276^{\dagger}$	0.304^{*}
	(0.15)	(0.15)
FEs: Issue Area		X
FEs: Child, Sibling, Circuit, Year	X	X
Constant	0.097	1.426
	(0.50)	(0.67)
N	2113	2113

Table A3. Dependent variable: Did judge vote in favor of female plaintiff (i.e., liberally) in case involving women's issue? (1 = yes). Logit coefficients; standard errors in parentheses.
† $p < 0.1$, * $p < 0.05$