

Daughters Do Not Affect Political Beliefs in a New Democracy*

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Abstract

Having a daughter shapes parents' attitudes and behaviors in gender-egalitarian ways, a finding documented in multiple industrialized democracies. We test whether this travels to a young middle-income democracy where women's rights are tenuous: South Africa. Contrary to prior work we find no discernible effect on attitudes about women's rights or partisan identification. Using a unique dataset of over 7,500 respondents and an equivalence testing approach, we reject the null hypothesis of any effects of 5 percentage points or greater at conventional levels of statistical significance. We speculate that our null findings relate to opportunity: daughter effects are more likely when parents perceive economic, social, and political opportunities for women. When women's customary status and *de facto* opportunities are low, as in South Africa, having a daughter may have no effect on parents' political behavior. Our results demonstrate the virtues of diversifying case selection in political behavior beyond economically wealthy democracies.

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1. INTRODUCTION

Families shape political beliefs (Glass et al., 1986; Elder and Greene, 2012; Healy and Malhotra, 2013; Greenlee, 2014). The effect of one family feature, the sex of children, has been documented among elites including U.S. representatives (Washington, 2008),¹ U.S. judges (Glynn and Sen, 2015), and corporate executives (Dahl et al., 2012; Cronqvist and Yu, 2017), and in the general public in Australia, Canada, the U.K., the U.S., and China (Warner, 1991; Warner and Steel, 1999; Oswald and Powdthavee, 2010; Prokos et al., 2010; Shafer and Malhotra, 2011; Sun and Lai, 2017; Greenlee et al., 2018; Perales et al., 2018; Sharrow et al., 2018). Table 1 summarizes prior work on political “daughter effects,” showing the empirical consensus that daughters move parents’ political attitudes and behaviors in a more feminist direction, generally toward the political left.

We ask whether daughters change parents’ beliefs in a young middle-income democracy where women’s rights remain tenuous: South Africa. On issues of gender rights, South Africa is *de jure* progressive, but *de facto* not. This provides an opportunity to study the micro-foundations of public opinions about gender, diagnosing how context may (fail to) influence attitudes. Our unique data, which includes 7,500 respondents and, crucially, data on family structure for each respondent, allows us to test for daughter effects on both attitudes and partisan ideological identification. Contrary to prior research, we find null effects. Using the two-one sided test (TOST) approach (Robinson and Froese, 2004; Lakens, 2017; Hartman and Hidalgo, 2018) for analyzing null effects, we reject the null hypothesis of even very small effects at conventional levels of statistical significance.

These null effects are empirically important and theoretically informative. Contextual differences between South Africa and more established democracies provide an opportunity to learn about when familial effects may, or may not, apply. We speculate that one important feature is opportunity: Having a daughter may be more likely to affect parents when they perceive that economic, social, and political opportunities exist for women. Although women’s legal, economic, and family rights are codified in South African law, women’s *de facto* status largely still comports with traditional gender roles and social expectations around work and family. When women’s customary

¹Though see Costa et al. (2018) for evidence that this dynamic may no longer occur among U.S. representatives.

Table 1: Prior Empirical Work on Political and Social ‘Daughter Effects’

Note: + indicates a positive statistically significant effect. – a negative statistically significant effect. \emptyset a “null effect” as interpreted by the original study’s authors. Effect heterogeneity is indicated by subgroup in parentheses, W for women, M for men.

Study	Population	Dependent Variable	General Effect?
<u>Attitudes:</u> Perales et al (2018)	Australian Parents	Traditional gender roles support	+
Prokos et al (2010)	U.S. Parents	Affirmative action policies	+ (W), –(M)
Shafer & Malhotra (2011)	U.S. Parents	Non-traditional gender roles support	\emptyset (W), + (M)
Sharrow et al (2018)	U.S. Parents	Gender equity policies support	+
Sun & Lai (2017)	Chinese Parents	Non-traditional gender roles support	\emptyset
Warner (1991)	Canadian Parents	Non-traditional gender roles support	+
Warner & Steel (1999)	U.S. Parents	Gender equity policies support	+
<u>Partisan ideology:</u> Conley & Rauscher (2013)	U.S. Citizens	Left-wing ideology/partisanship	–
Lee & Conley (2016)	U.S. and U.K. Parents	Left-wing ideology/partisanship	\emptyset
<u>General Public Behaviors:</u> Greenlee et al. (2018)	U.S. Fathers	Voting for Hillary Clinton	+
Oswald & Powdthavee (2010)	U.K. Parents	Left-wing voting (electoral)	+
<u>Elite Behaviors:</u> Costa et al (2019)	U.S. Representatives	Pro-women’s issues voting (roll-call)	\emptyset
Glynn & Sen (2015)	U.S. Court of Appeals Judges	Feminist voting (judicial decisions)	+
Washington (2008)	U.S. Representatives	Pro-women’s issues voting (roll-call)	+

status and *de facto* opportunities are low, having a daughter may not cause parents to reevaluate their views. While we resist drawing definitive conclusions about cross-national differences, this demonstrates the virtues of diversifying case selection in the study of political behavior beyond economically wealthy and well-established democracies.

2. *De Jure* AND *De Facto* WOMEN'S RIGHTS IN SOUTH AFRICA

Like many emerging democracies, women's rights in South Africa are well protected in law, but less so in practice. Following the 1994 election, the African National Congress (ANC)-led government made constitutional and voluntary governance changes to advance women's equality. The post-*apartheid* constitution included gender equality as a core principle. The first democratic parliament passed several pieces of legislation related to women's rights, including expanded access to abortion and increased protection against domestic violence (Hassim, 2003, 101). By one measure, Htun and Weldon's (2012) index of legal protections to combat violence against women, South Africa moved from a score of one (out of ten) in 1995 to eight in 2005. Across fourteen legal provisions on women's *de jure* economic rights measured by the World Bank, South Africa claimed none before 1993, but had achieved thirteen by the year 2000 (World Bank, 2017). The ANC has voluntarily adopted a "zebra-list" quota in which men and women candidates are alternated on party lists, making the country a world leader in women's parliamentary representation.

Yet despite these gains many South African women experience low *de facto* status. Women are much more likely to be employed in the informal sector and contribute more than twice the amount of unpaid household work (World Economic Forum, 2016). These dynamics are borne out in social expectations about gender roles. In the 2010 - 2014 World Values Survey, more respondents agreed than disagreed that a woman earning more money than her husband "is almost certain to cause problems in the home" and over half of respondents agreed that "when a mother works for pay, the children suffer." Finally, violence against women remains acute (Goldblatt and Meintjes, 1998). An estimated 50.3 percent of female homicides in South Africa are due to intimate partner violence (IPV) (Abrahams et al., 2009). In the most recent World Values Survey, 60 percent of respondents reported that it is at least sometimes justifiable for a man to beat his wife. In short, South African

women’s *de facto* status falls far short of their *de jure* rights in the post-*apartheid* era.

3. DAUGHTER EFFECTS IN A CHANGING SOCIETY

This discrepancy between *de jure* and *de facto* rights makes South Africa an important case to test for the existence of daughter effects. If women’s *de jure* rights are important in shaping parents’ beliefs, daughter effects should appear in South Africa. Research suggests parents change their assessment of daughters based on their future potential. In India, exposure to female village leaders changes parents’ career and educational aspirations for their daughters and lowers the rate of sex-selective abortion, neglect, and infanticide (Beaman et al., 2012; Kalsi, 2017). Similarly, Qian (2008) finds that higher tea prices, a crop traditionally picked by women, lowers the rate of missing women in China due to parents’ changing assessment of women’s economic value. When women are granted legal rights, parents may view their daughters differently or more equally due to greater perceived opportunities, thus leading to the existence of daughter effects.

However, changes to women’s *de jure* rights may not affect how parents experience having a daughter if these rights are slow to translate to *de facto* opportunities. Parents may not see daughters as having opportunities in practice, or they may see these new opportunities as dangerous if women working outside the home are subject to harassment or violence. If this view dominates, we would not expect to observe daughter effects in the South African case: Parents may not change their political beliefs when having a daughter if they view women’s opportunities as more related to their *de facto* status than *de jure* rights.

4. DATA AND RESEARCH DESIGN

To test whether daughter effects are present in South Africa, we use unique data from the South African Social Attitudes Survey (SASAS), a large repeated cross-section collected by the Human Sciences Research Council (HSRC), South Africa’s statutory research agency for the social sciences and humanities (HSRC, 2011). The data are yearly cross-sections of a nationally representative sample of South Africans, surveying views on a range of political, social, and economic issues. We use data collected from 2004 through 2012, for a total of 43,948 respondents. SASAS collects a

small amount of data on the entire household and their relationship to the respondent, from which we can establish the household-dwelling family structure of each respondent.² This feature sets SASAS apart among political attitude surveys in sub-Saharan Africa; for example Afrobarometer, the most widely used such survey collects no systematic information on family structure.

We leverage the natural experiment that occurs when a sperm initially fertilizes an egg, and in the overwhelming proportion of cases, the resulting fetus is assigned either two X chromosomes or an X and a Y. As our main design, we focus on the effect of the sex of the first child which we see as the strongest possible design in this setting (see Lee and Conley, 2015; Oswald and Powdthavee, 2010). This avoids possible bias introduced by families who employ “differential stopping rules,” where decisions about whether to have another child are affected by the sex of the first child. Because families that continue to have children until they have a child of the desired sex are likely different from families that do not employ differential stopping rules, we focus only on the sex of the first child. Due to randomness in sex assignment, those whose first child is male should be good counterfactuals for those whose first child is female.

Although the sex of the first child is as-if random at the point of conception and sex-selective abortion in South Africa is rare (Garenne, 2002), concerns remain. First, the data collection procedure means that we do not know if there are children who have already left the home. To address this, we subset our sample to respondents aged 45 and younger, thus focusing on those who are less likely to have adult children.³

Second, it is possible that children may be placed into a different household post-birth, or that the children living in the household were adopted, and not biologically conceived by the parents who raise them. If this occurred in such a way that boys and girls were not adopted by comparable families, it would introduce the possibility of systematic error where certain types of parents are more likely to adopt children of a particular sex. To evaluate the risk of this potential confounding in the SI we compare “treated” and “untreated” respondents across pre-treatment covariates –

²Household is defined in the survey as “all persons who eat from the same cooking pot and who were resident 15 out of the past 30 days.”

³Results presented in the SI show that, aside from improving precision due to increased power, including all ages in the sample does not materially change the results. The same applies, though precision decreases as power decreases, when sub-setting to those under the age of 35.

race, sex, and age of the respondent, all of which are typically important covariates in South Africa (Ferree, 2011). Imbalance is relatively small in magnitude, but to be conservative we estimate all effects both with and without controlling for these same covariates.

5. RESULTS

We examine the effect of sex of first child on five outcomes: attitudes towards preferential hiring of women; views on abortion in two cases (a birth defect in the fetus and a low-income mother who can't afford to raise a child); a gender equality battery (only included in the 2008 survey),⁴ and partisan support of the dominant political party, the center-left ANC. Figure 1 visualizes these five outcomes as proportions supporting a particular policy, split by the sex of the first child. None of the differences are large, and for most policies, respondents whose first child is a son are slightly more progressive. We statistically test for treatment effects by assuming a null hypothesis of “no effect” and an alternative hypothesis of a “non-zero effect,” using ordinary least squares (OLS), with and without controlling for covariates:

$$Y_i = \alpha_t + D_i\beta + X_i^T\gamma + \epsilon_i$$

Where α_t represents a survey fixed-effect for a survey in time t , D_i is a dummy variable indicating that the first child was a girl ($D_i = 1$) or a boy ($D_i = 0$), and β is the estimate of the causal effect of first-child gender on Y_i , a variety of outcomes. $X_i^T\gamma$ represents a set of optional linear controls for a variety of pre-treatment covariates, and ϵ_j is an individual-level error.

Table 2 confirms what visual inspection of Figure 1 suggests: the effect of having a daughter is largely indistinguishable from zero, aside from a negative and statistically significant result on abortion rights (about three percentage points *less* supportive, and thus counter to expectations).

⁴This battery asked respondents to report the degree to which they agreed with the following statements: “A working mother can establish just as warm and secure a relationship with her children as a mother who does not work,” “A child younger than 5 years is likely to suffer if his or her mother works,” “All in all, family life suffers when the woman has a full-time job,” “A job is alright, but what most women really want is a home and children,” “Being a housewife is just as fulfilling as working for pay,” and “A man’s job is to earn money, a woman’s job is to look after the home and family.”

Figure 1: Gender Attitudes by Sex of First Child.

Note: This figure shows the means responses for the “treated” (gray, daughters) and the “control” (black, sons) across our five key dependent variables. The figure suggests that there is little-to-no difference between the two groups on any of our outcome variables.

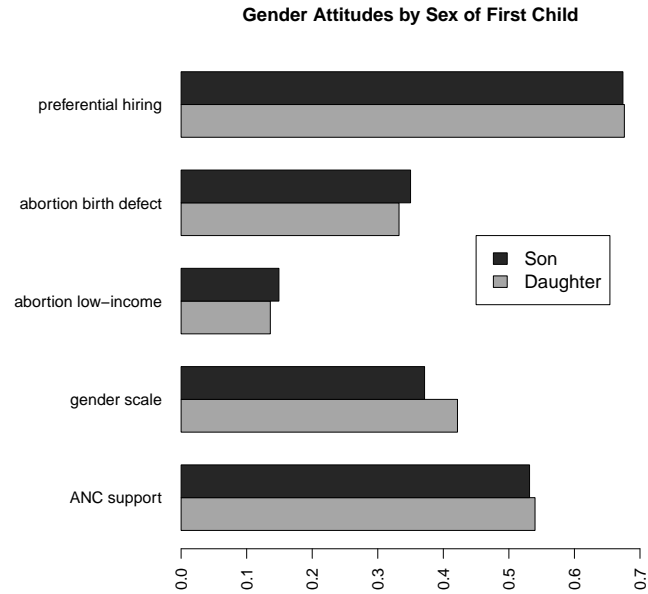


Table 2: The Effect of Having a Daughter on Attitudes and Partisanship.
Note: OLS linear probability models. Covariates used: race dummies, linear age, sex dummy, urban dummy. Robust standard errors in parentheses. See SI for replication using appropriate generalized linear models (GLM).

	<i>Dependent variable:</i>									
	preferential hiring	abortion	(defect)	abortion (low-income)	gender scale	ANC support				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
first_daughter	0.002 (0.011)	-0.005 (0.010)	-0.016 (0.011)	-0.013 (0.011)	-0.012 (0.008)	-0.013 (0.008)	0.049 (0.063)	0.046 (0.063)	0.009 (0.011)	-0.008 (0.010)
Covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	7,558	7,558	7,558	7,558	7,558	7,558	1,008	1,005	7,518	7,518
R ²	0.00003	0.076	0.013	0.024	0.006	0.008	0.001	0.019	0.003	0.288
Adjusted R ²	-0.0002	0.075	0.013	0.023	0.005	0.007	-0.0004	0.013	0.003	0.287

*p<0.1; **p<0.05; ***p<0.01

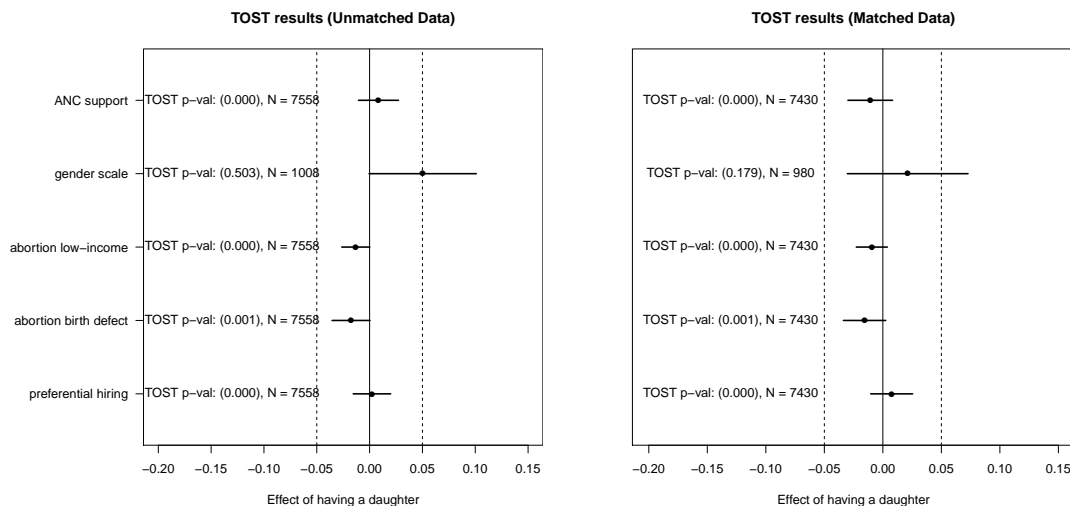
While such results are often interpreted as a null effect, a well established literature in statistics notes that to properly draw conclusions about a null effect, one should invert the hypotheses being tested. Conventional statistical testing allows us only to reject, or fail to reject, the null hypothesis of a zero effect. To test *for* a zero effect, we should instead test the null hypothesis of a “non-zero effect” against the alternative hypothesis of “no effect,” referred to as equivalence testing. We employ a two-one sided test (TOST) approach (Robinson and Froese, 2004; Lakens, 2017; Hartman and Hidalgo, 2018), and calculate a formal p-value by specifying a theoretical effect size λ , any smaller than which we would consider a small or approximately zero treatment effect. We then test the null hypothesis that the effect is greater than or equal to λ , or less than or equal to $-\lambda$.

For each outcome, we estimate the effect of having a daughter within the TOST framework in two ways: first, by simply comparing the proportion of respondents supporting the outcome whose first child is a daughter, and those whose first child is a son, and second by matching respondents on age, sex, race, and survey year, and repeat the process. The results are presented in Figure 2, with $\lambda = \pm 5$ percentage point treatment effect for ease of interpretation. This means that an effect greater than 5 percentage points is considered a non-zero effect, which is roughly the lower bound of statistically significant results reported in prior studies. This choice of λ is somewhat arbitrary, so for completeness in the SI we show how the p-values for each test vary as a function of λ . Generally, it appears that our conclusions hold down to $\lambda \approx 2.5$ percentage points.

Figure 2 is interpreted by assessing the (lack of) overlap between the 95% confidence intervals and the vertical dotted lines, and the corresponding TOST p-values. Out of the 10 tests we conduct, in only two – scoring above the median on a gender equality scale – were we unable to reject the null of a statistically significant non-zero effect for our definition of a small effect. Importantly, in both of these cases we have much less data because the question was only asked in one year, and the TOST confidence interval itself also includes 0, suggesting that very little can be concluded either way.

In the SI we estimate the results separately by sex of respondent. For the unmatched data, we find no nonzero effects for any outcome, for either sex. Among women in the matched dataset, we likewise find no nonzero effects. For men in the matched dataset, for three of the outcomes,

Figure 2: Two-One Sided Test of Daughter Effects for Matched and Unmatched Data.
Note: For all outcome variables except the gender scale, we can reject the null that the effect of having a daughter is greater than five percentage points, indicated by the vertical dotted line.



we find no meaningful effect, but having a daughter is associated with an increase in support for preferential hiring, and a decrease in support for abortion in the case of a birth defect. While these latter two estimates are distinguishable from 0, they are not distinguishable from an effect of 5 percentage points. We find it most plausible that these effects are the result of statistical noise given the number of tests being conducted, particularly since they are not in a consistent direction. Thus, in total, we interpret the body of evidence to suggest that the sex of the first child has either no discernible effects or very marginal and theoretically inconsistent effects on the beliefs and preferences of South African parents. The SI also contains several additional specifications, including subsetting the sample to parents with only one child. The results we present below are not substantively changed across specifications.

6. DISCUSSION AND CONCLUSION

It seems clear that personal experiences and circumstances shape political socialization, attitudes, and behavior (Huckfeldt et al., 1993; Kasara, 2013; Sands, 2017). Evidence from industrialized democracies suggests that one such experience is having a daughter, which substantively shifts

parents' attitudes and behaviors. Yet this finding does not travel to South Africa, a young middle-income democracy where women's rights are far more tenuous. Having a daughter as opposed to a son does not appear to change parents' attitudes towards gender equality or partisanship. We find no consistent effects for either mothers or fathers across a range of specifications and outcomes. Our evidence further suggests these results are not simply due to noise or low statistical power, but are "true nulls."

Other authors have argued that parents may see the formal codification of women's rights as a signal that their daughter will have more opportunities than in the past, providing incentives to treat their sons and daughters equally. We speculate that in the South African case this process may not unfold because women's *de jure* status remains stubbornly low. Across a number of measures, including access to education, the formal labor force, and freedom from violence, South African women still fare significantly worse than men. Parents, then, may not see the increase in women's *de facto* legal rights and protections as meaning much in terms of opportunities for their daughters, and thus have little reason to update their behaviors or attitudes. We speculate that a potential pre-condition for daughter effects exists: men and women may need to have relative *de jure* equality before having a daughter changes parents' attitudes. Notably, the only other study that reports similar nulls studies China, where women's status is also low (Sun and Lai, 2017).

Research examining how gender attitudes change over time suggests that citizens must be exposed to women in non-traditional roles for many years or even decades before they begin to update their beliefs about gender equality (Beaman et al., 2012; Clayton, 2018). Changes in women's legal status appears to not cause parents of daughters to change their beliefs in the years immediately following reforms. It may take some time before advancements in women's legal rights are reflected in public opinion toward gender equality, and thus before family experiences will act as a conduit for these new beliefs.

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