# Does the Election of Women Cause Other Women to Vote or Run For Office in the US? A Regression Discontinuity Approach* 

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#### Abstract

Does the election of women in the US cause other women to vote or run for office themselves? The election of women to local legislatures reduces bias against female officeholding and increases women's political participation in India, though it remains unclear whether the same effects exist in polities like the US where a substantial number of women already hold office. I employ a regression discontinuity approach and data on 3,813 state legislative elections where a woman opposed a man and find that the election of additional women to office does not cause more women to run for office, win office, or to turn out vote in the US. The precision of these null estimates rules out even substantively small effects. The results shed light on the remaining barriers to women's participation in American politics; specifically, these challenges appear to go beyond what further increases in female officeholding can itself erode.


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## 1. Introduction

Less than one hundred years ago, women in the United States could not vote in federal elections, nor did any women hold federal office. Today, it is commonplace for more women than men to cast ballots in US Presidential elections, while thousands of women hold significant political offices. Nevertheless, despite the remarkable progress in women's political inclusion that has been made over the last century, there remains much to be done to close gender gaps in political participation in the United States and around the world.

A number of activists, policymakers, and scholars have argued that the election of more individual women to office might itself further increase women's political participation: victorious female candidates are expected to increase political empowerment among women in the mass public leading more women to vote and to "break glass ceilings" for other women who hope to hold office by showing political elites, voters, and women themselves what women can accomplish in office. Thus Burns et al. (2001, p. 9) write that "when women are in an environment where women seek and hold visible public offices, [women] are more politically interested and informed, and [gender] disparities in psychological orientations to politics shrink."

In this paper I present evidence that the election of additional female officeholders does not significantly increase other women's participation in US politics at the mass or elite levels. Though pathbreaking findings from India's unique policy experiments show that the election of the first women to office there produce dramatic increases in other women's participation in the mass public and presence in government by changing voters', elites', and women's own attitudes towards women's participation and officeholding (Beaman et al. 2009; Bhavnani 2009; Deininger et al. 2012) - and broader cultural shifts in women's role (Beaman et al. 2012) - I exploit a natural experiment and find no evidence that the same causal linkages exist in the US.

I do so with a regression discontinuity approach and data describing 3,813 state legislative elections in the United States where a woman opposed a man. This dataset and
research design have several novel features. First, though correlative studies have struggled with challenging endogeneity problems, the approach uses a quasi-experimental design to identify the causal effects of electing women. In addition, focusing on state legislative elections squarely focuses the analysis on the most crucial training ground for future statewide and federal officeholders, ${ }^{1}$ and thus where much systematic bias against women holding office first begins, as well as the level of government where substantial effects have been found in India. The US state legislative level also features uniquely plentiful data, allowing for more statistically precise tests than would be possible with other data.

The results show that the election of a woman in one state legislative district in the US has no significant causal effect on the political behavior of other women: women who live in districts nearby districts where women have won the previous election are not more likely to run for or win office as a result; and women who are represented by a woman instead of a man are similarly no more likely to vote as a consequence. The precision of these null estimates is substantial enough to rule out even substantively small effects: the mobilizing effect of being represented by a woman is statistically zero and, at most, smaller than that of receiving a GOTV postcard in the mail; and the effect of a woman's victory on the probability that other women run for office is an order of magnitude smaller than the effect of a woman being asked to run by a party leader. Sorting around the discontinuity cannot account for these patterns, which are also robust across a number of specifications.

The results shed new light on the remaining barriers women face to enjoying full representation in American politics, both in the electorate and in elected office. With a number of women already in office and the once-rapid growth in women's participation and officeholding slowing (CAWP 2010), the election of additional women in the United States thus appears unable to itself further break down the barriers to participation that American women face.

[^1]Though evidence from India shows that female officeholding in local legislatures can meaningfully decrease voters', elites', and women's own biased expectations about women's political capabilities, other structural barriers to women's political participation appear to endure in the US that go beyond what the election of more women alone can address.

## 2. Can The Election of Women Empower Women?

Women's political participation continues to fall behind men's in the United States in important respects. Among the mass public, women are significantly less likely to contact their elected officials, contribute to political campaigns, and affiliate with a political organization (among other political acts), patterns that have clear implications for women's political equality (Verba, Schlozman, and Brady 1995, p. 254; Burns et al. 2001). Among the ranks of elected officeholders, women's underrepresentation is even starker: as of 2012, only $17 \%$ of US Senators and $16.6 \%$ Members of the US House are women. These gaps in women's officeholding likewise have clear consequences for women's substantive representation: female legislators are more likely to sponsor and vote for women's interest legislation (Gertzog 1995; Niven 1998; Chattopadhyay and Duflo 2004), and women represented by women also have more positive evaluations of their representatives (Lawless 2004). ${ }^{2}$

Seeking to ameliorate these persistent inequalities, scholars have devoted a great deal of attention to understanding the roots of women's underrepresentation in the mass public and among elected officeholders (see Burrell 2004 and Dolan 2006 for broad reviews). One novel hypothesis about the origins of women's participation has gained particular prominence: that the election of female officeholders itself causes more women to politically participate, both in routine acts in the mass public and by actually running for elected office themselves.

First, scholars have long argued that historically underrepresented groups like women

[^2]may feel more empowered by or have greater trust in a representative who is a member of their group (e.g., Banducci, Donovan, and Karp 2004; Beaman et al. 2012; Bobo and Gilliam 1990; Gay 2002; Mansbridge 1999; Pantoja and Segura 2003; Williams 1998) and thus participate in politics to a greater extent when they have descriptive representatives. In addition, women running for office might be expected to target more women for mobilization with their campaigns (following the logic of Fowler and McClure 1989 and Rosenstone and Hansen 1993).

Supporting these hypotheses, a wealth of research uncovers strong relationships between the presence of many historically underrepresented groups in government and many modes of participation (e.g., Burns et al. 2001; Gay 2002; Harris, Sinclair-Chapman, and McKenzie 2005, 2006; Banducci, Donovan, and Karp 2004; Washington 2006; Wolbrecht and Campbell 2007; Atkeson and Carrillo 2007; Broockman 2012; Deininger et al. 2012; Beaman et al. 2012).

Existing scholarship also provides a number of compelling reasons to expect that the election of one female politician might "break a glass ceiling" and lead to the election of others. First, though many voters appear to apply sexist stereotypes (e.g. Koch 2000), voters also appear to diminish their biases against female candidates as a result of witnessing a woman hold office (e.g. MacManus 1981; Beaman et al. 2009; Bhavnani 2009; Healy 2012). Party elites who witness women serving in office might be expected to similarly decrease their biases against female candidates as well (e.g., Beaman et al. 2009; Bhavnani 2009; see also Niven 1998; Sanbonmatsu 2006). ${ }^{3}$ Finally, if women themselves are more likely to "underestimate" their own "qualifications to seek and win elective office" or have absorbed sexist ideas about the role of women in the political sphere (Lawless and Fox 2005, 2011), other women in politics might serve as 'role models' and decrease these biases (see also Schramm 1981; Beaman et al. 2012).

Pioneering findings from India's unique policy experiments grant strong support these

[^3]expectations: Beaman et al. (2009) and Bhavnani (2009) find that districts randomly reserved for female officeholders feature voters, elites, and women with significantly less biased views of women's political potential, which led to significantly more women running for and winning office in these districts as a result; Deininger et al. (2012) similarly finds that women in these reserved districts are significantly more likely to participate in politics in a variety of ways; and Beaman et al. (2012) find that introducing a female leaders produces significant shifts in adolescent girls' educational attainment, career aspirations, and household roles.

However, despite this strong evidence that the election of women to local political bodies can shift expectations and stereotypes about women in a polity like India where few women have held office before, there are good reasons to doubt that the election of additional women could do so in a country like the United States where a number of women already hold office. Instead, deeper barriers to women's participation in politics may persist in the United States that go beyond what the presence of individual female officeholders can further erode, such as deeply ingrained sexist ideas about leadership, incorrigible outgroup bias among party leaders, and gender gaps in access to other resources: in the context of the male-dominated US Senate, for example, voters, elites, and women might change their gender views very little in reaction to the election of hypothetical new $18^{\text {th }}$ female Senator (there are currently 17), even if the election of the first female US Senator (Hattie Caraway, in 1931) did have large effects in mobilizing women to vote or in showcasing women's true potential to voters and party leaders. To question whether the election of female officeholders can lead other women to run for and win office in the US is thus not to reject that women still face barriers to political participation in the US; on the contrary, it is to investigate whether the barriers women do face are beyond what we do know that individual female officeholders can erode alone.

In summary, despite compelling reasons to expect that findings from India might replicate in the American context, there also remains uncertainty about their generalizability.

However, despite this uncertainty, understanding to whether the election of one woman can increase other women's participation and officeholding in a place like the United States is a substantively important question with significant theoretical and practical consequences.

For practitioners and policymakers, understanding whether the election of women causes other women to vote or to run for office can shed light on the best strategies for increasing women's representation - if electing one female politician can have downstream consequences for other women's participation, for example, approaches taken by groups like EMILY's List may yield even greater gains for women (in the public and among elites) than is currently appreciated. On the other hand, the lack of such an effect would underscore the need for a renewed attention to underlying biases to women's participation that would not disappear on their own. With the once steady growth in women's underrepresentation slowing (CAWP 2010), the remaining barriers to women's equal representation in the United States may go beyond what the elections of individual women can erode (see Duflo 2011 for related discussion).

For scholars, these hypotheses also have broader theoretical significance for understanding the barriers to participation that women face in the US and similar countries. As evidence from India shows that the election of women changes voter and elite attitudes towards female officeholding and can increase individual women's participation in politics, discovering similar effects in analogous elections in the United States would suggest that voter and elite bias are still at significant play in the US in ways that exposure to additional female politicians can erode. On the other hand, if similarly situated successful female candidates have no effect on other women's candidacies or turnout in the US as they do in India, this would imply that more attention is due to addressing the other underlying, engrained, and structural biases in US politics - such as sexist animus on the part of party elites and sexist socialization of women - that go beyond what the elections of women are expected to be able to address.

In these ways, evaluating these hypotheses in the context of the US, especially with the
availability of similar results from India, can provide significant insight into the barriers that remain for women in US politics and guidance for the direction of future research on this issue.

Still, empirically addressing these questions about the effect of women's officeholding has proven challenging, and empirical evidence remains mixed, for two main reasons. First, existing studies (e.g., Ferreira and Gyourko 2012) have contended with limited data: the relatively small number of countries that have female representatives has limited cross-country comparisons, and the continuing scarcity of female representatives has limited cross-district approaches as well. Data on women's voter turnout is also difficult to gather in many areas, further limiting research.

A second persistent challenge is selection bias. We do not yet fully understand the reasons why women are elected in the places that they are, meaning that associations between the election of a woman in one district and the outcomes of interest (e.g, Healy 2012) are difficult to convincingly tease apart because we cannot be sure that we have controlled for all potentially confounding factors. Though scholars have found experimental evidence that female politicians have changed perceptions about women's candidacies in the context of particular case studies and in the lab (MacManus 1981; Gitelson and Gitelson 1981), the difficulty in identifying causal relationships between these entwined correlations has limited the development of large-N field research outside of India. ${ }^{4}$

In the next section I describe how the natural experimental design sought to overcome these issues and identify the effect of additional female officeholding in the US on other women's participation at the mass and elite levels.

## 3. Data and Methodology

## A Regression Discontinuity Approach

[^4]Though the United States does not feature a randomized reservation system like India's that would allow for straightforward identification of the causal effect of electing women, a natural experimental approach is available: a regression discontinuity design. Regression discontinuity designs have grown increasingly common in political science and economics and have been applied to identify causal effects in a wide variety of contexts where traditional approaches have difficulty (e.g. Lee 2008). RDDs can estimate the causal effect of a variable of interest when it is determined at a sharp discontinuity: For example, Lee (2008) finds that Democrats who "just win" Congressional elections receive a substantial increase in their vote shares in subsequent elections over those who have "just lost" the previous election (in other words, the incumbency effect). Regression discontinuity designs can thus use endogenous data to estimate the causal effect of a variable of interest if it is determined discontinuously (such as an election is). (For more on RDDs and for a formal proof of their properties, see Lee (2008).)

In this context, I use the discontinuity in who wins general elections at the $50 \%$ point in the female candidate's two-candidate vote share to estimate whether women are more likely (1) to vote in subsequent elections when a woman has just won the previous election or (2) to run for or win office in other districts nearby districts where a woman has just won the previous election. I use with data on thousands of state legislative elections where a woman opposed a man, yielding a large dataset with the possibility of exploiting a unique opportunity for drawing causal inferences. Analyzing state legislative elections also squarely focuses the analysis on the most crucial training ground for future statewide and federal officeholders and on elections most similar to those that have been examined in India.

To illustrate how the RD works, Figure 1 summarizes how an RDD would uncover a "breaking of the glass ceiling" phenomenon whereby one woman's victory caused other women to be more likely to run for office nearby. The X -axis in Figure 1 refers to the vote share a female candidate received in an election against a man in District A at Time 1. The Y-axis refers
to the number of female candidates who run for or win office in other districts near District A at Time 2. As depicted in Figure 1, a man won all elections at Time 1 in Figure 1 to the left of the dotted line in each panel, corresponding to areas where a woman's two-candidate vote share was below $50 \%$; above $50 \%$ in two-candidate vote share, and to the right of the vertical dotted line, women won all races against men at Time 1 . Though the sorts of districts that tend to occur at each point are endogenous, elections can be considered quasi-randomly determined just at the discontinuity, and thus differences in subsequent patterns across the discontinuity can be causally attributed to the election of a woman in District A. That is, any 'jump' in the dependent variable at the discontinuity can be attributed to the effect of crossing the discontinuity (i.e., of a women winning in District A) because there should be no systematic differences between the sorts of districts where women happen to have "just won" or "just lost" contests against men. ${ }^{5}$

## [FIGURE 1 HERE]

## Data

Data to explore these questions came from several sources. First, the Center on American Women and Politics (CAWP) at Rutgers University collects and graciously makes available data on all female candidates for state legislature since 1999. In order to conduct the regression discontinuity on observations where either a woman or a man might have won office, I dropped all cases where a woman ran against another woman or a woman ran unopposed. I also restricted the data to races that occur in even-numbered years, since data for odd-numbered election years was not readily available. ${ }^{6}$ I finally dropped all legislative chambers where there are multimember districts since specifying a regression discontinuity is not straightforward in a multicandidate race. This yields a dataset of 3,813 state legislative elections where a woman opposed a man in 2002, 2004, 2006, and 2008. (The years 2000 and 2010 are excluded because

[^5]redistricting of between 2000/2010 and 2002/2012.)
Second, I matched these data to data on state legislative election returns gathered from state legislative websites. The resulting dataset describes the vote share garnered by the female candidate and the male candidate in each of the 3,813 races.

The dependent variables came from two sources. First, in order to assess conditions for female candidates in nearby districts in subsequent elections, I matched the election return and CAWP data to data from the US Census that describe the geographic location and boundaries of state legislative districts. This identifies other districts were 'nearby' each of the 3,813 main districts and thus where elites and potential female candidates might learn of women's electoral victories (or defeats) in these districts and ultimately see women serve.

To measure which districts were 'nearby' each other district, I first created a dataset describing the distance between each of the 6,652 state legislative districts in the US and every other district (describing about $6652^{2} / 2 \approx 22$ million unique district pairings). There are multiple ways to express the distance between two two-dimensional objects: I chose to do so with the common and simple method of comparing the distances between the centroids (that is, the geographic 'average location' or 'center of gravity') of each of the districts. Figure 2 shows an example of how this procedure, executed using a Geographic Information System (GIS), determined which state legislative districts were within 50 miles of the state house district for San Francisco, California. In the Figure, the centroid of the San Francisco district is denoted by the dark black dot, with the large gray circle corresponding to the area within 50 miles of this centroid. Ten other state house district centroids fall within this area, corresponding to the ten shaded districts that are therefore considered to be 'within 50 miles' of the San Francisco district. Other districts with centroids beyond the shaded area are thus not considered to be 'within 50 miles' of the San Francisco district.
[FIGURE 2 HERE]

To construct the final dependent variable for this analysis, I matched this linked distance data with the datasets describing every state legislative election since 2002 and the data on female candidates from CAWP, yielding an indicator variable for whether a woman was a candidate in or won every state legislative election since 2002. I then computed the total number of contests and the total number of female candidates and victors in those contests in other districts within an arbitrary distance of each district to calculate the final dependent variables.

For women's voter turnout behavior, I purchased data from Catalist, a well-known voter data clearinghouse. Catalist provided me with data on the total number of votes cast in the general election and the number of votes cast by women specifically in every even-numbered election year between 2000 and 2010 from their comprehensive US voter files. This dataset yielded two variables of interest: dividing the number of votes women cast by the total number of votes yields the share of the electorate which women comprised in each election, and dividing the number of votes cast by women by the female population yields women's voter turnout in each election.

## 4. Results: Does Electing Women Empower Women To Vote?

## Specifying The RD

To specify the regression discontinuity, I employed the standard approach of estimating a function on each side of the discontinuity and then estimating the difference between these functions at their limits. General practice for doing so is employing a high-order polynomial on each side of the discontinuity or using local linear regression; I present the results using both approaches. The distance between the values of the two regressions on each side of the discontinuity when they reach the threshold captures the causal effect of the discontinuity on the dependent variable while naturally holding all else constant. The polynomial model for the hypothesis that the election of a woman in one district causes other women in that district to vote therefore takes the following form:
where the left hand side of the equation describes women's voter turnout in district $i$ at Time 2 (with Time 2 defined as whenever the next election takes place in district $i$ ), $\alpha$ is a constant, $V_{i}$ is the Woman's Two-Candidate Vote Share within district $i$ at Time $1, F_{i}$ is a dummy variable set to 1 if $V_{i}>1 / 2$ (that is, if the woman wins in $i$ at Time 1 ) and 0 otherwise, and $\varepsilon_{i}$ is the error term. $\gamma$ is the coefficient of interest that captures the causal effect of a women's prior victory on the dependent variable. ${ }^{7}$

The choice of 'bandwidth', or how far from the discontinuity to use data for estimating the functions' values at the limit, presents a tradeoff. Narrower bandwidths use fewer observations, limiting the estimation procedure to information on observations most similar to the exogenous elections right at the limit. Conversely, wider bandwidths estimate the model using a greater number of observations, including those that are further from the discontinuity. In the text I present estimates for a $15 \%$ bandwidth, though Table A1 in the Appendix shows that the results for a variety of other bandwidths are essentially identical. This $15 \%$ figure means that only elections where a female candidate garnered between $35 \%$ and $65 \%$ of the final twocandidate vote are included in the estimation procedure.

## Results: Do Female Politicians Mobilize Women To Vote?

Does the election of a woman in the US cause other women to vote? Table 1 presents the results. The first two columns present the estimates for the effect of electing a woman in a district on women's voter turnout in that district in the next election (that is, $P($ Woman $\mid$ Voted $)$ ) and the second two columns estimate the effect of a woman's victory on the proportion of voters who are women in that district in the next election ( $P($ Voted $\mid$ Woman $)$ ). In each case, the line in bold represents the estimate of $\gamma$ in equation (1), the causal effect of the election of a woman on

[^6]the dependent variable. The coefficients on the polynomials (the $\beta$ s in equation (1)) are not presented to conserve space, though in all cases I used a fourth-order polynomial. The constant term describes the average value for the dependent variable at the left-hand limit. Recall that the sample sizes are smaller than the full dataset because the procedure is limited to observations within 15 percentage points of the discontinuity; similar results for smaller and larger bandwidths are shown in Table A1 in the Appendix.
[TABLE 1 HERE]
The first two columns show that the effect of a woman's victory on women's voter turnout is statistically zero. The second column includes state-year fixed effects to provide a more precise upper bound on the estimated effect of a woman's victory on women's voter turnout.

With fixed effects, the model implies with $95 \%$ confidence that the effect of a woman's election on women's voter turnout in subsequent elections is most likely zero and, at most, not more than 2.8 percentage points. The Imbens and Lemieux (2008) estimator (which is more efficient than polynomials and thus has smaller standard errors; see Table A1) similarly estimates with $95 \%$ confidence that women's voter turnout does not rise by more than 0.9 percentage points because of a woman's election. To put this latter estimate in substantive context, the mobilizing effect of being represented by a woman on women's turnout thus appears to be, at the most, even smaller than the mobilizing effect of receiving a simple 'civic duty' GOTV postcard (Gerber, Green and Larimer 2008)). This upper bound is also nearly an order of magnitude smaller than the estimated effect of being asked to vote face-to-face or being sent a 'social pressure' mailer (Gerber and Green 2000; Gerber, Green, and Larimer 2008). In summary, the results show that the effect of being represented by a woman on women's voter turnout in the US is statistically and substantively zero.
[FIGURE 3 HERE]

Panel (a) of Figure 3 displays these results graphically, with the data binned in 0.5-percentage-point wide bins and the model from column 1 superimposed. The X axis refers to women's share of the two-candidate vote in election 1, and the Y axis to women's voter turnout in election 2. The dark line shows the estimate for the true underlying pattern on each side of the discontinuity, with the bright red line dividing cases where women just won elections at Time 1 from those where women had just lost. As is clear from the raw data, there is no significant increase in women's voter turnout due to a woman's victory in the previous election.

## Women's Share of the Electorate

Voter turnout may not be the best proxy for women's empowerment, however: raw turnout statistics do not take into account effects that women's victories may have on men's turnout, and it is important to understand whether women represent a greater share of the electorate relative to men, not only in isolation.

The third and fourth columns thus estimate the effect of a woman's victory on women's share of the electorate - that is, the proportion of all votes cast that were cast by women. The results again imply that the effect of electing a woman is statistically zero: furthermore, the estimates imply with $95 \%$ confidence that women make up no more than 0.9 percentage points more of the electorate due to a woman's victory in the previous election. The raw data and fitted model shown in Panel (b) of Figure 3 makes this null result similarly clear. The election of women does not appear to be able to meaningfully increase women's presence in the electorate.

## Women Appear on the Ballot More Frequently

One reason these null results are theoretically surprising is that they imply that female incumbents' re-election campaigns do not mobilize a significant number of women to vote. The fifth and sixth columns of Table 1, and Panel (c) of Figure 3, show that when a woman just wins an election, a woman is about twice as likely to be a candidate in a subsequent election. One way to interpret the null results for women's voter turnout and presence in the electorate is thus by
considering the discontinuity as an instrument of sorts for women's presence on the ballot in the next election (e.g. Broockman 2009); in other words, even though a woman is (exogenously) much more likely to be running for election at Time 2, women appear no more likely to vote as a result. Even though female candidates might be expected to target female voters for mobilization in their re-election campaigns (e.g. Fowler and McClure 1989; Rosenstone and Hansen 1993), these findings imply that women running for office do not succeed in turning out more women to vote to a meaningful extent. Table A1 in the Appendix shows that these results are robust to a number of alternative bandwidths and consistent when using the bandwidth and estimation procedure recommended by Imbens and Lemieux (2008). Even when considering data only very near the discontinuity or including much more data, the estimates remain essentially identical and never approach statistical significance.

In summary, the findings imply that there are at best minimal causal effects of women's seeking and holding office on women's political participation in the electorate. Though by their nature these statistical procedures are unable to establish that the effects are truly zero, the null estimates are precise enough to rule out all but substantively minimal effects. Although there is an extremely strong association between women's voter turnout and the performance of female candidates (indeed, in this data, the association between women's voter turnout and women's performance at the ballot box has a stratospheric $t$-value of over 18), the regression discontinuity approach reveals that the election of women to local legislatures does not play a causal role in this association in the US as it does in India (Deininger et al. 2012). Underlying barriers to women's participation that effect both women's officeholding and women's voter turnout appear more responsible for this pattern in the US.

## 5. Do Female Politicians Break Glass Ceilings?

District Distance Criteria: Where Can One Woman's Victory Be Expected To Cause Other Women To Run?

Though the election of women does not appear to cause more women to vote, can the election of one woman cause other women to run for (and win) office in other areas nearby? If party leaders or individual women learn from or are inspired by women's victories or service in office, women might be more likely to run for and win offices in nearby districts as a result.

To test such hypotheses, I specify the following equation:

$$
\begin{equation*}
\frac{\sum \text { FemaleCandidatesNearby }_{i}}{\sum \text { ElectionsNearby }}=\alpha+\beta_{1} V_{i}+\beta_{2} V_{i}^{2}+\beta_{3} V_{i}^{3}+\beta_{4} V_{i}^{4}+\gamma F_{i}+\beta_{5} V_{i} F_{i}+\beta_{6} V_{i}^{2} F_{i}+\beta_{7} V_{i}^{3} F_{i}+\beta_{8} V_{i}^{4} F_{i}+\varepsilon_{i} \tag{2}
\end{equation*}
$$

where the left hand side describes the proportion of elections with female candidates in other districts near district $i$ at Time 2 (with Time 2 again referring to whenever the next election is held in district $i$ ), the other parameters refer to the election results in district $i$ at Time 1 and are the same as in equation (1), and where $\gamma$ now captures the causal effect of electing a woman in district $i$ at Time 1 on whether other women run for office in districts nearby district $i$ at Time 2 .

I employ two different criteria to define 'nearby' districts - first, by the centroid distance measurement procedure described in the previous section and shown in Figure 2, I consider whether a woman was a candidate in or won all elections in other districts within 75 miles of each district, and, second, estimate the same quantities for elections in the 10 other districts closest to each district. The latter approach may help balance out spatial irregularities across areas: some states have larger districts than others, some districts are larger than others, and some districts are so rural (such as that for northern Alaska) that there are no others within even 100 miles of its center. In all cases I only consider effects on elections for seats in the same chamber and the same state, though the results are the same when considering the effects on all districts.

## Results: Successful Female Candidacies Do Not Generate More Female Candidacies

The first two columns of Table 2 and panels (a) and (c) of Figure 4 show the estimates for the effect of a woman's victory on the likelihood that a woman appears on the general election
ballot in nearby districts. Panel (a) and the first column of Table 2 show these estimates within the context of all districts within 75 miles, and Panel (c) and the second column of Table 2 show them for the 10 closest district to each districts. In each case the results are statistically zero; there is at most a negligible effect of electing one woman on the likelihood that other women run for office. As with the panels in Figure 3, Figure 4 shows the raw data binned in 0.5-percentage point wide bins with the fitted model superimposed. It is especially clear from the Figure that the effects of electing a women on other women's candidacies are essentially zero.

## [TABLE 2 HERE]

[FIGURE 4 HERE]
Substantively, this null result is especially stark in comparison to the effect of other factors on women's candidacies. While the $95 \%$ confidence interval from the Imbens and Lemieux (2008) (in Table A1) procedure estimates that women in the 10 closest districts are at most 2 percentage points more likely to run for office as a result of a woman's victory, Fox and Lawless (2010) find that women are about 37 percentage points more likely to seriously consider running for office when asked to do so by a political gatekeeper, an estimate more than an order of magnitude greater. Likewise, Fox and Lawless (2005) estimate that women are up to 55 percentage points more likely to seriously consider running for office if they view themselves as more qualified, an estimate that again far outstrips even the most optimistic estimate for the impact of witnessing other women's victories.

## Female Victories

Unsurprisingly given the prior null results, the estimates for the number of women who win nearby contests are likewise statistically zero. This shows that the previous null result does not belie a quality effect whereby more talented women might run, increasing women's victories even though candidacy rates remained the same. Rather, in other districts nearby, women are at most a few percentage points more likely to win office as a result of a woman's victory in the
previous election nearby; panels (b) and (d) in Figure 4 display this null result graphically.
The results are identical when considering upper chamber elections only, the effect of upper chamber elections on lower chamber elections, and open seat elections only. Table A1 in the Appendix shows that the results continue to hold with a number of different distance criteria, including in a smaller and larger geographic radius and a greater and fewer number of nearby districts included. Table A1 additionally demonstrates that these estimates remain robust to a number of different bandwidth choices and are identical when using the Imbens and Lemieux (2008) estimation procedure.

In summary, the results show that the election of a woman in one district is expected to increase the likelihood that a woman runs for office in a nearby district by at most a few percentage points. Women face numerous barriers to office in the United States; however, given the number of women who already hold office here, electing more women is itself unlikely to erode these structural barriers.

## 6. Internal and External Validity

## Can Sorting Explain The Null Results?

The estimates presented in the previous sections turn on the assumption that women actually win and lose elections "near-randomly" at the discontinuity. Though the assumptions of the design do not require that there are no overall differences between districts where women lose and win tend to win, they do require that there are no differences at the limit, that is, that parties and candidates are not so adept at manipulating elections that near-winners tend to have systematic advantages over near-losers. If parties and groups can forecast campaigns' expected vote shares with a high enough degree of precision, more powerful actors may be able to focus their resources intensely on races where they might otherwise just lose (or reverse an election result in legal wrangling afterwards), leading to a systematic bias where more powerful parties and groups tend to be overrepresented right on the winning side of the discontinuity.

Caughey and Sekhon (2011) and Grimmer et al. (2012) provide evidence that such a bias exists in US House elections, with parties that just win these contests much more likely to have had access to significant financial and other resources than near-losers (however, see Snyder et al. 2012). In this paper's case, were such a bias to exist it would likely tilt the results toward uncovering a false effect and thus is unlikely to be of concern. However, as these authors note, a strength of the RD design is that the identifying assumption can be tested, and I do so to further ensure the robustness of the results. To do so, I conduct a placebo test estimating the 'effect' of a woman winning one contest on women's voter turnout and the likelihood that women had been nominated for or won other contests nearby at the same time. We know that there can be no causal effect of a woman's victory on other women's nominations and victories at the same time; therefore, if this procedure were to find significant effects in either direction, it would be of concern that the design might be invalid.

## [TABLE 3 HERE]

## [FIGURE 5]

Table 3 and Figure 5 show the results of placebo tests, using the same specifications as in Tables 1 and 2 yet replacing these dependent variables with the same measures calculated during the same year. Reassuringly, the tests correctly estimate that there is no causal effect of female candidates' victories on other events that have occurred simultaneously: the estimates never approach statistical significance and are essentially identical in size to the coefficients estimated for differences in these variables Time 2. This underscores the null results - the point estimates for pre-existing differences between these districts in Tables 1 and 2 almost exactly match the estimates in Table 3. In summary, the designs assumptions hold well, granting further confidence in the null results. ${ }^{8}$

[^7]
## External Validity

Though the regression discontinuity approach outlined here has substantial advantages over other potential approaches, the wider generalizability of the result must be interpreted with care. RDDs estimate local average treatment effects in two senses - the estimates are specific to the kind of races that occur near the discontinuity, and, in this case, to the kind of areas where women tend to oppose men in general elections in the first place. Both of these points deserve careful consideration.

First, the RDD estimates the causal effect specific to the kinds of races that occur near the threshold - in this case, very competitive elections. This is likely a strength of the design in this setting, however, because it is after these very close elections that one would expect the hypothesized effects to be greatest were they to exist. First, it is to these elections that voters and elite political observers alike generally pay the closest attention. Elites and women considering running for office might be expected to glean the most about women's abilities to win such elections as well, and voters can be expected to be most acquainted with who is running for office in competitive elections (e.g., Kam and Utych 2011). ${ }^{9}$ In addition, female candidates might be expected to mobilize female voters as much as they possibly could while running for reelection after a narrow victory. It is thus particularly surprising that women's victories in these very high salience contests had no detectable effects.

In addition, the necessity of restricting the analysis to elections where the candidates were of the opposite gender somewhat limits the generalizability of the results to the sorts of areas
candidates to sort across the discontinuity in state legislative elections. Second, because political organizations do not tend to exist along gender lines in the US (and gender is not a mere proxy for party, as there are actually a substantial number of female state legislators in both parties), it seems additionally implausible that sorting could occur along gender lines.
${ }^{9}$ From a strictly rational point of view, rational elites and women would not learn anything from a woman's bare loss or victory, but in this respect the natural experiment is identical to the case in India - neither would a rational actor there learn anything about women's electoral abilities from a woman's victory in a contest where only women are eligible to run anyway. However, as in India, the theoretical expectation is that a rational actor would learn from observing women actually holding office, interacting with constituents, preparing to run for re-election, etc., which they could not have were a man to have won.
where women already tend to be nominated for offices that they are likely to win. On the one hand, focusing on these races underscores the theoretical point that, in polities (like the US) where many women already hold office there are limited participatory externalities to the election of additional female officeholders - the existing biases against women's participation in these areas go beyond what their elections can erode alone. Still, one way to address this concern is by estimating the discontinuity only in the areas where few other women hold office nearby, and doing so reassuringly yields the same results: the findings are not an artifact of the slight overrepresentation in the dataset of areas where women tend to run most already.

In summary, though the results' uniquely strong internal validity do come with limits to their wider generalizability, the findings do not appear sensitive to these limitations, and there is good to think that some of these limitations focus the design on the cases where these effects might be most likely to occur and that are most similar to the Indian comparisons.

## 7. Conclusion

Reflecting hopes about what the growing presence of women in government might herald for women's place in politics, Burns et al. (2001, p. 9) write that "when women are in an environment where women seek and hold visible public offices, they are more politically interested and informed, and [gender] disparities in psychological orientations to politics shrink." Numerous scholars, policymakers, and activists have mounted arguments like these about the causal effect of women's descriptive representation, recently buoyed by pioneering findings from India's policy experiments that the election of women to local legislatures can have a number of empowering effects for other women's participation in politics at the mass and elite levels.

In this paper I tested whether female officeholding can do so in similar elections in the United States with a regression discontinuity analysis of thousands of state legislative elections where a man opposed a woman.

First, the results show that there is no significant causal effect of electing additional
women in the US on other women's voter turnout. The estimates are statistically zero and precise enough to rule out effects even smaller than those yielded by the receipt of a simple GOTV postcard (and those less than a third of the size of that of being asked to vote face-to-face).

In addition, the results showed that a woman's victory in one district has no meaningful effect on the likelihood that other women run for or win office in other nearby districts in subsequent elections. These null estimates were also statistically precise, and implied that the effect of electing women on other women's candidacies is, at most, more than an order of magnitude smaller than other scholars' estimates for the effect of a woman being asked by a party leader to run for office (Fox and Lawless 2010). Despite the existence of strong associations between these outcomes and women's victories that many scholars have uncovered, a regression discontinuity approach reveals that the election of women does not appear to play a meaningful causal role in them.

Substantively, these results have several novel implications. First, though scholars and practitioners alike have expressed optimism that the election of more women might increase women's participation in politics at the mass level, these results imply that these gains appear to have been fully reaped in the United States and that other means will thus be necessary to further promote women's participation. ${ }^{10}$

The results also shed new light on the context-dependent nature of when female officeholders can "break glass ceilings" and pave the way for other women to run for and win office. Though new results from India show that the election of female politicians significantly decreases voters', elites', and women's own biases against female candidates and leads to more female officeholding and an expanded cultural role for women there (Beaman et al. 2009;

[^8]Bhavnani 2009; Beaman et al. 2012), the same does not appear to be true in the United States. Rather, these findings suggest that, with it now relatively commonplace for women to hold office in the United States, the challenges that remain to women's officeholding in the US cannot be addressed through the corrective measures that come with electing women for the first time: though changing elite's expectations about women's ability to win office, decreasing voter bias against women, and educating women about women's potential in office appear to reap significant rewards when few women hold office, other factors appear to be more at play in limiting women's participation once female officeholding is more commonplace. Consequently, these results suggest that the biases that do continue to hold women from office in the US are disappointingly durable and insensitive to counterexample, thus highlighting the role played by longer-term biases such as sexist socialization of women and the enduring prejudices of party leaders (e.g., Lawless and Fox 2005; Niven 1998).

In sum, the results thus underscore that attention to the enduring barriers to women's underrepresentation in politics is essential in the United States (and will likely grow more essential in places like India once the gains from female officeholding are fully reaped there). Groups like EMILY's list have undoubtedly done a great deal to improve women's representation in politics - and the first female officeholders in the US almost surely challenged many's ideas about women's role in public life - but these results show that there are limits to what the election of individual women can do to change the political climate for women: as the election of more women appears unlikely to itself be able to remove these remaining barriers to officeholding and participation that women face, efforts to elect individual women must also be complemented by a continuing attention to the underlying biases women face. Only a careful attention to these issues, their roots, and what can and cannot address them can the gap be bridged between women's presence in government today and what equality demands. With the once steady growth in women's participation and representation slowing (CAWP 2010), this
focus is more important now than ever.

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Table 1. RD Estimate of Electing a Woman on Women's Voter Turnout

| Time | Time 2 |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| Dependent Variable | Women's Voter Turnout |  | Female Share of the Electorate |  | Woman on Ballot |  |
| Woman Won At Time 1 | $\begin{aligned} & \hline-0.008 \\ & (0.029) \end{aligned}$ | $\begin{aligned} & \hline-\mathbf{0 . 0 0 7} \\ & (\mathbf{0 . 0 1 8}) \end{aligned}$ | $\begin{gathered} 0.004 \\ (0.003) \end{gathered}$ | $\begin{gathered} 0.004 \\ (\mathbf{0 . 0 0 3 )} \end{gathered}$ | $\begin{gathered} \mathbf{0 . 4 3 1 * *} \\ (0.073) \end{gathered}$ | $\begin{gathered} \mathbf{0 . 4 3 2} * * \\ (0.077) \end{gathered}$ |
| Degree of Polynomials | 4 | 4 | 4 | 4 | 4 | 4 |
| Year / State FEs? | No | Yes | No | Yes | No | Yes |
| Constant | $\begin{gathered} 0.535^{* *} \\ (0.002) \\ \hline \end{gathered}$ | n/a | $\begin{gathered} 0.530^{* *} \\ (0.002) \\ \hline \end{gathered}$ | n/a | $\begin{gathered} 0.451^{* *} \\ (0.062) \\ \hline \end{gathered}$ | n/a |
| $\mathrm{R}^{2}$ | . 007 | . 005 | . 009 | . 014 | . 248 | . 250 |
| N | 2588 | 2588 | 2588 | 2588 | 2588 | 2588 |

Notes: ${ }^{* *}=p<.001, *=p<.10$, all tests two-tailed. Bandwidth is 0.15 in all cases. The Table shows that women are not significantly more likely to vote or to comprise a greater share of the electorate due to being represented by a woman. This is additionally surprising given that women are twice as likely to appear on the ballot during the next election when they win.

Table 2. Effect of Women's Victories on Women's Candidacies Nearby

| Time |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: |
| Dependent Variable | Female Candidates <br> per Contest |  |  |  |
|  | Female Victors per |  |  |  |
|  | Contests | 10 | Contests | 10 |
| Distance Criteria | Within | Closest | Within | Closest |
|  | 75 Miles | Contests | 75 Miles | Contests |
| Woman Won At Time 1 | $\mathbf{0 . 0 2 8}$ | $\mathbf{0 . 0 0 7}$ | $\mathbf{0 . 0 2 1}$ | $\mathbf{0 . 0 0 4}$ |
|  | $\mathbf{( 0 . 0 4 0 )}$ | $\mathbf{( 0 . 0 4 5 )}$ | $\mathbf{( 0 . 0 2 9 )}$ | $\mathbf{( 0 . 0 3 2 )}$ |
| Degree of Polynomials | 4 | 4 | 4 | 4 |
| Constant | $0.453^{*}$ | $0.468^{*}$ | $0.267^{*}$ | $0.274^{*}$ |
|  | $(0.028)$ | $(0.030)$ | $(0.021)$ | $(0.022)$ |
| $\mathrm{R}^{2}$ | .003 | .001 | .005 | .002 |
| N | 2404 | 2433 | 2404 | 2433 |

Notes: $*=p<.10$, all tests two-tailed. Sample sizes differ slightly between the columns because 29 state legislative districts in the US are so remotely located that no other districts exist within 75 miles of their centers. The Table shows that women's victories do not cause other women to run for office in other districts nearby.

Table 3. Placebo Tests for Sorting Around the Discontinuity

| Time |  |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| Dependent <br> Variable | Women's <br> Voter <br> Turnout | Female <br> Share of the <br> Electorate | Female Candidates per <br> Contest |  | Female Victors per <br> Contest |  |
| Distance <br> Criteria | - | - | Contests | 10 | Contests |  |
|  |  |  | Within 75 | Closest <br> Contests | Within 75 <br> Miles | Closest <br> Contests |
| Woman Won | $\mathbf{0 . 0 1 8}$ | $\mathbf{0 . 0 0 3}$ | $\mathbf{0 . 0 3 5}$ | $\mathbf{- 0 . 0 0 2}$ | $\mathbf{0 . 0 2 6}$ | $\mathbf{- 0 . 0 1 0}$ |
| at Time 1 | $\mathbf{( 0 . 0 3 2 )}$ | $\mathbf{( 0 . 0 0 3 )}$ | $\mathbf{( 0 . 0 3 7 )}$ | $\mathbf{( 0 . 0 4 0 )}$ | $\mathbf{( 0 . 0 2 6 )}$ | $\mathbf{( 0 . 0 2 9 )}$ |
| Degree of | 4 | 4 | 4 | 4 | 4 | 4 |
| Polynomials |  |  |  |  |  |  |
| Constant | $0.454^{*}$ | $0.530^{*}$ | $0.417^{*}$ | $0.439^{*}$ | $0.248^{*}$ | $0.262^{*}$ |
|  | $(0.021)$ | $(0.002)$ | $(0.027)$ | $(0.029)$ | $(0.019)$ | $(0.021)$ |
| $\mathrm{R}^{2}$ | .003 | .011 | .002 | .002 | .003 | .001 |
| N | 2364 | 2364 | 2403 | 2433 | 2403 | 2433 |

Notes: $*=p<.10$, two-tailed test. The Table tests for pre-existing differences between districts on either side of the discontinuity in the dependent variables of interest. Reassuringly, none exist. Sample sizes differ slightly between the specifications because voter turnout data was unavailable for a very small number of districts.

Table A1. Results for Alternate Bandwidths and Geographic Areas

| Specification | 0.05 | 0.10 | 0.15 | 0.20 | 0.25 | Imbens/ Lemieux |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| Dependent Variable <br> Women's Voter <br> Turnout | $\begin{gathered} 0.030 \\ (0.052) \end{gathered}$ | $\begin{gathered} 0.015 \\ (0.036) \end{gathered}$ | $\begin{gathered} 0.008 \\ (0.029) \end{gathered}$ | $\begin{gathered} 0.004 \\ (0.025) \end{gathered}$ | $\begin{gathered} 0.008 \\ (0.022) \end{gathered}$ | $\begin{aligned} & -0.011 \\ & (0.010) \end{aligned}$ |
| Women's Share of the Electorate | $\begin{gathered} 0.004 \\ (0.005) \end{gathered}$ | $\begin{gathered} 0.004 \\ (0.003) \end{gathered}$ | $\begin{gathered} 0.004 \\ (0.003) \end{gathered}$ | $\begin{gathered} 0.003 \\ (0.003) \end{gathered}$ | $\begin{gathered} 0.002 \\ (0.002) \end{gathered}$ | $\begin{gathered} 0.001 \\ (0.001) \end{gathered}$ |
| Woman on Ballot Next Year | $\begin{gathered} 0.435 * * * \\ (0.121) \end{gathered}$ | $\begin{gathered} 0.390^{* * *} \\ (0.089) \end{gathered}$ | $\begin{gathered} 0.431 * * * \\ (0.073) \end{gathered}$ | $\begin{gathered} 0.411 * * * \\ (0.049) \end{gathered}$ | $\begin{gathered} 0.414 * * * \\ (0.059) \end{gathered}$ | $\begin{gathered} 0.408 * * * \\ (0.032) \end{gathered}$ |
| Candidates/Contests, 5 Closest Contests | $\begin{aligned} & -0.061 \\ & (0.096) \end{aligned}$ | $\begin{gathered} 0.059 \\ (0.072) \end{gathered}$ | $\begin{gathered} 0.030 \\ (0.059) \end{gathered}$ | $\begin{gathered} 0.031 \\ (0.051) \end{gathered}$ | $\begin{gathered} 0.040 \\ (0.046) \end{gathered}$ | $\begin{gathered} 0.012 \\ (0.022) \end{gathered}$ |
| Victories/Contests, 5 Closest Contests | $\begin{aligned} & -0.061 \\ & (0.072) \end{aligned}$ | $\begin{gathered} 0.010 \\ (0.053) \end{gathered}$ | $\begin{aligned} & -0.010 \\ & (0.043) \end{aligned}$ | $\begin{gathered} 0.005 \\ (0.038) \end{gathered}$ | $\begin{gathered} 0.017 \\ (0.034) \end{gathered}$ | $\begin{aligned} & -0.000 \\ & (0.015) \end{aligned}$ |
| Candidates/Contests, 10 Closest Contests | $\begin{gathered} 0.030 \\ (0.078) \end{gathered}$ | $\begin{gathered} 0.004 \\ (0.055) \end{gathered}$ | $\begin{gathered} 0.007 \\ (0.045) \end{gathered}$ | $\begin{aligned} & -0.007 \\ & (0.039) \end{aligned}$ | $\begin{gathered} 0.002 \\ (0.035) \end{gathered}$ | $\begin{aligned} & -0.007 \\ & (0.018) \end{aligned}$ |
| Victories/Contests, 10 Closest Contests | $\begin{gathered} 0.005 \\ (0.054) \end{gathered}$ | $\begin{gathered} 0.009 \\ (0.039) \end{gathered}$ | $\begin{gathered} 0.004 \\ (0.032) \end{gathered}$ | $\begin{gathered} -0.001 \\ (0.028) \end{gathered}$ | $\begin{gathered} 0.002 \\ (0.025) \end{gathered}$ | $\begin{aligned} & -0.003 \\ & (0.012) \end{aligned}$ |
| Candidates/Contests, 15 Closest Contests | $\begin{gathered} 0.027 \\ (0.063) \end{gathered}$ | $\begin{gathered} 0.018 \\ (0.046) \end{gathered}$ | $\begin{gathered} 0.029 \\ (0.038) \end{gathered}$ | $\begin{gathered} 0.019 \\ (0.033) \end{gathered}$ | $\begin{gathered} 0.015 \\ (0.030) \end{gathered}$ | $\begin{aligned} & -0.010 \\ & (0.016) \end{aligned}$ |
| Victories/Contests, 15 Closest Contests | $\begin{gathered} 0.016 \\ (0.068) \end{gathered}$ | $\begin{gathered} 0.014 \\ (0.032) \end{gathered}$ | $\begin{gathered} 0.015 \\ (0.026) \end{gathered}$ | $\begin{gathered} 0.007 \\ (0.022) \end{gathered}$ | $\begin{gathered} 0.001 \\ (0.021) \end{gathered}$ | $\begin{aligned} & -0.009 \\ & (0.010) \end{aligned}$ |
| Candidates/Contests, within 50 Miles | $\begin{gathered} 0.019 \\ (0.068) \end{gathered}$ | $\begin{gathered} 0.002 \\ (0.054) \end{gathered}$ | $\begin{gathered} 0.018 \\ (0.045) \end{gathered}$ | $\begin{gathered} 0.026 \\ (0.040) \end{gathered}$ | $\begin{gathered} 0.028 \\ (0.036) \end{gathered}$ | $\begin{aligned} & -0.005 \\ & (0.019) \end{aligned}$ |
| Victories/Contests, within 50 Miles | $\begin{gathered} 0.036 \\ (0.045) \end{gathered}$ | $\begin{gathered} 0.002 \\ (0.036) \end{gathered}$ | $\begin{aligned} & -0.014 \\ & (0.032) \end{aligned}$ | $\begin{aligned} & -0.005 \\ & (0.027) \end{aligned}$ | $\begin{aligned} & -0.002 \\ & (0.025) \end{aligned}$ | $\begin{aligned} & -0.017 \\ & (0.012) \end{aligned}$ |
| Candidates/Contests, within 75 Miles | $\begin{gathered} 0.036 \\ (0.061) \end{gathered}$ | $\begin{gathered} 0.010 \\ (0.047) \end{gathered}$ | $\begin{gathered} 0.028 \\ (0.040) \end{gathered}$ | $\begin{gathered} 0.023 \\ (0.036) \end{gathered}$ | $\begin{gathered} 0.012 \\ (0.033) \end{gathered}$ | $\begin{aligned} & -0.002 \\ & (0.014) \end{aligned}$ |
| Victories/Contests, within 75 Miles | $\begin{gathered} 0.013 \\ (0.040) \end{gathered}$ | $\begin{gathered} 0.004 \\ (0.032) \end{gathered}$ | $\begin{gathered} 0.021 \\ (0.029) \end{gathered}$ | $\begin{gathered} 0.021 \\ (0.025) \end{gathered}$ | $\begin{gathered} 0.013 \\ (0.023) \end{gathered}$ | $\begin{aligned} & -0.001 \\ & (0.012) \end{aligned}$ |
| Candidates/Contests, within 100 Miles | $\begin{gathered} 0.035 \\ (0.060) \end{gathered}$ | $\begin{gathered} 0.008 \\ (0.043) \end{gathered}$ | $\begin{gathered} 0.026 \\ (0.036) \end{gathered}$ | $\begin{gathered} 0.027 \\ (0.032) \end{gathered}$ | $\begin{gathered} 0.020 \\ (0.029) \end{gathered}$ | $\begin{gathered} 0.003 \\ (0.013) \end{gathered}$ |
| Victories/Contests, within 100 Miles | $\begin{gathered} 0.028 \\ (0.038) \\ \hline \end{gathered}$ | $\begin{gathered} 0.008 \\ (0.029) \\ \hline \end{gathered}$ | $\begin{gathered} 0.024 \\ (0.025) \\ \hline \end{gathered}$ | $\begin{gathered} 0.028 \\ (0.022) \\ \hline \end{gathered}$ | $\begin{gathered} 0.018 \\ (0.020) \\ \hline \end{gathered}$ | $\begin{gathered} 0.003 \\ (0.010) \\ \hline \end{gathered}$ |

Notes: ${ }^{* * *}=p<.01 . *=p<.10$. The Table shows that the null estimates hold using a variety of other geographic distance criteria and bandwidth criteria. The bandwidth refers to the window of observations used for estimating the underlying model; for example, a bandwidth of 0.15 means that only observations where a female candidate captured between $35 \%$ and $65 \%$ of the two-candidate vote are included.

Figure 1. Hypothesized "Breaking of the Glass Ceiling" Pattern


Female Candidate's Vote Share, in District A, in Year 1
Notes: The Figure visualizes how a regression discontinuity design would discover the effect of a woman's victory on other women's candidacies in subsequent elections. The $X$-axis describes a woman's share of the two party vote in a district at Time 1; women won races to the right of the discontinuity. Even though the overall relationship is endogenous, the difference between the estimates of the data's true underlying form at the limit captures the causal effect of electing a woman in other districts near district $A$ at Time 2.

Figure 2. District Distance Measurement Procedure Example


Notes: The Figure illustrates the procedure used to measure which other districts are nearby each state legislative district. The centroid of each district is calculated, then other centroids within a certain distance of each district's centroid are identified.

Figure 3. Effect of Women's Victories on Women's Voter Turnout


Notes: The circles show the data's average values in 0.5-percentage point wide bins, with the size of the circles corresponding to the number of observations in each bin. The dark lines show the model estimated on each side of the discontinuity, which is shown in red. The distance between these lines at the discontinuity captures the causal effect of women's elections. Panels (a) and (b) show that the election of a woman does not cause more women to vote in subsequent elections. These null results hold despite the increased likelihood that women will appear on the ballot in subsequent elections. Female officeholding and campaigning thus do not appear to meaningfully increase women's voter turnout.

Figure 4. Effect of Women's Victories on Other Women's Candidacies Nearby


Notes: The circles show the data's average values in 0.5-percentage point wide bins, with the size of the circles corresponding to the number of observations in each bin. The dark lines show the model estimated on each side of the discontinuity, which is shown in red. The distance between these lines at the discontinuity captures the causal effect of women's elections. The Panels show that women's victories in one district do not cause more women to run for or win office in other districts nearby in subsequent elections.

Figure 5. Placebo Tests for Sorting


Notes: The circles show the data's average values in 0.5-percentage point wide bins, with the size of the circles corresponding to the number of observations in each bin. The dark lines show the model estimated on each side of the discontinuity, which is shown in red. The distance between these lines at the discontinuity captures the pre-existing differences between districts on each side of the discontinuity. As expected, there are no significant pre-existing differences between districts on each side of the discontinuity.


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[^1]:    ${ }^{1}$ Over half of Members of Congress began their political careers in state legislatures (Maestas et al. 2006).

[^2]:    ${ }^{2}$ Women's growing representation has more general consequences, as well: because barriers to entry for political office are higher for women than for men, the women who do enter politics are generally more talented than are men (Anzia and Berry 2011).

[^3]:    ${ }^{3} 64 \%$ of female local officeholders reported to Niven (1998) that "party leaders discouraged" them from running for office "because of their gender" and a substantial share of party leaders report believing that women are less electorally viable and less qualified to hold office than are men (Niven 1998; Sanbonmatsu 2006; see also Lawless and Fox 2005, 2010).

[^4]:    ${ }^{4}$ Thus though Niven (1998) finds in his survey of party chairs that chairs "who have experienced more diversity" that is, serve in areas with more female politicians - are "less likely to rely on perceptual bias" in their evaluation of female candidates, and Palmer and Simon (2008, Ch. 5) note that successful women tend to "cluster" in certain areas, both works do not pin down a causal role for the election of women.

[^5]:    ${ }^{5}$ Note that the data is specified at the level of randomization to accurately estimate the statistical uncertainty associated with the estimates (e.g., Green et al. 2001).
    ${ }^{6}$ This excludes Louisiana, Mississippi, New Jersey, and Virginia.

[^6]:    ${ }^{7}$ See Imbens and Liemieux (2008) for technical details on the local linear regression estimator.

[^7]:    ${ }^{8}$ It is not surprising that the biases Caughey and Sekhon (2011) and Grimmer et al. (2012) document in postwar US House elections would not materialize here. First, state legislative candidates have far fewer resources than Congressional candidates, meaning that it should be much more difficult for financially or otherwise advantaged

[^8]:    ${ }^{10}$ These null results are also particularly surprising given than women are expected to mobilize female voters to a greater extent; however, the results show that women who live in districts where women were (exogenously) much more likely to be a candidate for office were no more likely to vote as a result. Given that campaigns have powerful tools to mobilize the citizens they wish (Rosenstone and Hansen 1993), this suggests that female politicians probably do not attempt to mobilize women to a meaningfully greater degree either.

