The Impact of the 2017 Women's March on Female Political Representation*

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Abstract

Can female-led protests improve women's participation in the political sphere? The 2017 Women's March is the largest single-day protest to ever happen in the US and a showcase of female leadership. I exploit the geographic variation in congressional districts' exposure to protests to investigate the impact of the March on the supply of female politicians in partisan primaries. I use difference-in-differences designs and event-study analyses that leverage distance from the nearest protest. I find that the March affected the supply of female Republican politicians: doubling the distance leads to a 29% drop of probability of having at least one female candidate and to a 60% drop of the share of female candidates. The March has a contemporaneous effect on the demand for females in Republican open primaries: doubling the distance causes a 58% drop in the share of votes for females, but there is no evidence of an effect on the probability that a woman wins her primary. I find no evidence of any effect in the Democratic primaries. Moreover, I investigate the consequences for women's representation in federal politics, finding that doubling the distance decreased the probability of having a female US House Representative by 33%, regardless of party affiliation.

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1 Introduction

Women make up one fourth of parliamentarians worldwide, a number that is far below equal political representation (UN, 2022). As of 2022, women accounted for 28% of seats in the US House of Representatives, a figure that has steadily increased from 5% in 1990 (CAWP 2022). Despite the progress made during the last 30 years, the persistence of gender disparity may suggest that women face barriers in accessing public offices at the federal level. Top-down policies such as the introduction of gender quotas have are effective in reducing the gender gap in political representation (Beaman et al., 2009; Pande and Ford, 2012; Casas-Arce and Saiz, 2015), but little is known about the impact of bottom-up feminist activism on women's choice to run for office and on the consequences of such a choice for the political gender gap.

Barriers to equal political representation could either result from a bias in favor of male politicians or from a differential willingness of women to run for political offices. In the US there is contrasting evidence about voters' discrimination towards women (Anastasopoulos, 2016; Ono and Burden, 2019), while parties' bias against female politicians conflicts with their desire to maximise electoral outcomes (Casas-Arce and Saiz, 2015). Hence, discrimination seems not to be the main obstacle for women: gender gaps in political representation are more likely to originate from gender gaps in candidatures in the US context (Lawless and Fox, 2008). Unequal female political representation therefore stems from women's differential willingness to run for office: if females do not want to act as political leaders, then we can only observe male dominated parliaments.

There are competing explanations to why women do not want to act as political leaders. First, there is a differential perceived self efficacy of potential male versus female political candidates (Fox and Lawless, 2014), which relates to the gender gap in leadership willingness (Alan et al., 2020). Second, the psychological burden of political failure differs between males and females, leading to the emergence of a gender gap in women's propensity to run for local offices after an electoral loss (Wasserman, 2018). Third, negative attitudes towards female leaders could explain women's differential willingness to run for office: if the society does not expect women to behave as leaders, breaking the norm and running for office entails bearing a cost (Akerlof and Kranton, 2000).

Can feminist mobilization affect women's choice to run for office? And what are the consequences for female political representation? I study the 2017 Women's March: the non violent protest that inaugurated Donald Trump's presidential mandate. This case is a well-suited testing ground for at least three reasons: first, the 2017 Women's March is the largest single-day protest with both a political and a feminist root that ever happened in the US, with a turnout of more than 3 million people; second, the March was an unprecedented showcase of female leadership (see Figure 3); third, the Women's March on the US Capitol was accompanied by 613 Sister Marches, providing enough geographic variation to allow for econometric identification.[1]

I find that the March had a positive effect on the supply of female candidates in the Republican Primaries. The event also caused a contemporaneous increase of the demand for female politicians in Republican primaries open to unaffiliated voters. Nonetheless, the gender composition of the candidates moving forward to the general election is not affected by the uprisings. As regards female representation in the US House, I find that the March caused an increase in the probability of electing a woman in the 2018 midterm elections and that results are driven by Democratic women.

The main empirical challenge in estimating the impact of the March is that historical ties with feminist movements and local dissent for the Trump administration are determinants of protests that also correlate with women's willingness to run for office and with their probability of being elected in the US House. Another challenge is reverse causality: it is ex-ante unclear whether the March has an impact on the behavior of female politicians or the other way around. To address these problems, I exploit the geographic partition of the US electoral process into congressional districts and I assign each district to a population weighted average of nearest protests. In particular, for each district I create the population weighted distance to the nearest protest, meaning a distance which accounts for the geographic distribution of the constituency within each congressional district.

The distance to the nearest protest is a continuous treatment variable that provides enough exogenous variation to both assess the impact of the March in difference in differences designs and to provide extensive evidence in support of parallel trends through event study parameterizations. Treatment is defined over proximity to protests because I start from the assumption that a Sister March occurs if and

[1] In this paper the term Women's March refers to the full set of Marches: both to the one on the Capitol and to all the Sister Marches that happened on the same date. Moreover, sometimes I refer to the 2017 Women's March simply as the March.

only if there is a cluster of *resisters* (Fisher, 2019) who organized it. I disregard protest size as it might be misleading in this context: most protesters travelled to take to the streets in symbolic places (see Map 4). Distance instead captures exposure to the increased women's local political activism that was triggered by the March (Fisher, 2019). The research design entails that the greater the distance to the nearest protest, the lower the exposure to the 2017 Women's March. Hence, I interpret a negative effect of distance as a positive effect of exposure to protests.

Fisher (2019) surveys protesters of the 2017 Women's March and follows them over time. She finds that most of them were Democratic women who subsequently became civically engaged. I use survey data to establish that the peer effects of these women increased the political participation of women in the general population. In particular, I show that feminist uprisings had a differential "bite" on female versus male participation in local politics: the March increased female civic engagement only.

I next rely on historical records of US House partisan primaries for both Republican and Democratic races to analyse the responsiveness of the extensive and intensive margin of the supply of female politicians to the March. Partisan primaries make an ideal setting for testing whether the March affected women's choice to run for office because they are the ground that parties use to select who shall be listed on the general election ticket. I use the presence of at least one female candidate in the partisan primary to measure the extensive margin of the supply and the share of female candidates to measure the intensive margin. Moreover, I use the share of votes for female candidates in partisan primaries to assess the impact of the March on the demand for female politicians and evaluate whether the event impacts the probability that a woman wins her primary.

I find that the March had an effect on the supply of female of Republican politicians in the primaries: doubling the distance to the nearest protest decreased the probability of having at least one female candidate by 29% and the share of female candidates by 60%. Moreover, I find a simultaneous increase in the demand for female politicians in the Republican primaries. Nonetheless, the increase of the share of votes for females is diluted by the increase of the share of female candidates: I find no evidence of an effect of the March on the probability that a woman wins her Republican primary. I perform an heterogeneity analysis of the votes for women across primary election systems, finding that the increased demand for female politicians is confined to open Republican primaries. In open primaries voters don't need to be registered with the party to be entitled to cast a ballot. As regards Democratic primary races, the sign of the estimated parameters is in line with theoretical expectations but not statistically significant at conventional levels.

To asses the implication of the change in women's willingness to run for office on female political representation, I use general election returns. More specifically, I focus on the probability to elect a female US House representative and I find that doubling the distance causes a 33% drop with respect to the sample pre-treatment probability. Results seem to be driven by the election of Democratic women, which is caused by the increase in the share of votes for them in the general election. Moreover, I assess the effect of the March on turnout in general elections: exposure to protests has a positive effect on turnout. This result in line with the actions and the goals of the *resisters* (Fisher, 2019).

I show that strategic placement of female candidatures in congressional districts exposed to the March is not driving the results: district shopping is virtually absent for women. I perform two sets of placebo in treatment tests, assessing whether distance from the nearest protest matters beyond news coverage and beyond proximity to urban clusters. I still find that the March had a positive effect on the supply of female Republican politicians, suggesting that the behavior of female candidates is affected by exposure to the feminist uprisings more than by news coverage of the event or proximity to cities.

Findings are robust to relaxing the strong parallel trend assumption underpinning difference in differences with continuous treatment (Callaway, Goodman-Bacon and Sant'Anna, 2021), meaning to applying binary difference in differences estimation strategies. In particular, results are qualitatively and quantitatively stable across six different binary treatment definitions (i.e. discretizing distance according to six different thresholds), suggesting that the March affected women's choice to run for office and female representation at the federal level.

There could be competing explanations to why the effects are stronger among Republicans than among Democrats. On one hand, it seems reasonable to think that the constraints imposed by negative attitudes towards female political leaders are more binding among conservatives. This implies that the supply of female Republican politicians is more responsive to reductions in the social cost of acting as leaders that was triggered by a showcase of female leadership such as the Women's March. On the other hand, the complementarity between the March and the Democratic Party electoral campaigning strategy may prevent identification within Democratic races (Fisher, 2019).

A competing mechanism behind this findings is that the Republican Party could have favored females' candidatures in congressional districts more exposed to the March because it wanted a woman to list on the general election ballot, so as to maximise electoral gains by means of *strategic descriptive representation* (Weeks et al., 2022). Nonetheless, the absence of any evidence of an effect of the protests on the probability that a woman wins her Republican primary plays against this latter hypothesis: Republican women did not make it through the primaries and were not listed on the general election ticket.

To the best of my knowledge, this is the first paper studying the interplay between protests and the supply of politicians. Moreover, it is the first study with a focus on the behavioral effects of feminist protests.

The remainder of the paper is organized as follows: section 2 reports the related literature; section 3 contextualizes the 2017 Women's March and details the institutional context of federal elections; section 4 describes the data used throughout the analysis; section 5 explains the empirical strategy; section 7 discusses the results; section 8 assesses behavioral and alternative channels; section 9 explains the robustness checks and section 10 concludes.

2 Related literature

This paper relates to several strands of the literature, encompassing the fields of economics, sociology and political science. The theoretical roots of the empirical analysis lie in identity economics, the theory that posits how social movements transform social categories and norms (Akerlof and Kranton, 2000; Kranton, 2016). The empirical study that most closely relates to this paper is Levy and Mattsson (2021), who find that the #MeToo movement influenced attitudes towards gender violence up to the point of influencing women's choice to report sexual harassment crimes. In a contemporaneous paper Larreboure and González (2021) analyse the impact of the 2017 Women's March on the votes obtained by women and ethnic minorities in the 2018 US House general election using a weather shock as an instrument for county-level protest size. My analysis contributes to the debate on the impact of the 2017 Women's March in at least three ways: first, my unit of analysis are congressional districts rather than counties;

second, I apply a difference in differences identification strategy rather than an instrumental variables approach; third, I focus on analysing the supply of female politicians in partisan primaries rather than the demand for female politicians in general elections.

This study also contributes to the empirical works analysing the impact of protests on attitudes, beliefs, policy preferences and elections in the United States, establishing that female-led protests affect the behavior of female politicians. Madestam et al. (2013) study the 2009 Tea Party rallies and show that the event shifted public opinion towards the views expressed by the movements' leaders and paved the way of the Tea Party to the Congress. A large number of studies analyze the impact of the Black Lives Matter (BLM) movement: Mazumder (2019) studies how the 2014 wave of BLM protests had an impact on racial attitudes, weakening racial prejudice among the youngest cohorts but strengthening it among the oldest; Reny and Newman (2021) use the random timing of George Floyd's assassination to show how the 2020 wave of BLM affected public opinion, fuelling negative attitudes towards the police. Similarly, Ebbinghaus, Bailey and Rubel (2021) shows how BLM led to the adoption of state police reforms, while Klein Teeselink and Melios (2021) use a rainfall shock to assess the impact of BLM on the 2020 presidential elections. Wasow (2020) analyzes how the 1960s black led protests affected public opinion, showing the effectiveness of non violent protests in shifting individuals' beliefs. Adopting an historical perspective Mazumder (2018) describes how non violent civil rights movements had the power to prime the American identity, ultimately changing individuals' beliefs and shaping the political landscape even years after their occurrence.

Last, my findings also relate to the literature on the impact of the Arab uprisings (a female-led wave of protests) on women's empowerment (Bargain, Boutin and Champeaux, 2019; El-Mallakh, Maurel and Speciale, 2018), and support the studies claiming that exposure to strong female role models has an impact on women's behavior (see, among others, Matsa and Miller, 2011; Jensen and Oster, 2009; La Ferrara, Chong and Duryea, 2012).

3 Background

3.1 The 2017 Women's March

The day of Donald Trump's presidential victory, a woman from Hawaii posted on a Facebook page a simple statement: *"I think we should march."* (Kearney, 2016). Her post went viral overnight and spurred the largest single-day protest in the US history to-date: 1% of US residents took the streets a few weeks later, on January 21^{st}, 2017 (Broomflied, 2017; Easley, 2017).

American citizens who marched were outraged by Trump's misogynist declarations (Lusher, 2016) and concerned that his presidency would threaten civil rights, the environment, reproductive rights and, most importantly, the right to equality. The success of the movement is related to the two principles of *intersectionality* and *inclusion*: diverse interests groups, motivated by concerns about gender, race and class, gathered behind the women's flag, advocating change (Fisher, Dow and Ray, 2017). The Women's March movement is an intersectional coalition promoting equity, a structural difference with respect to previous feminist movements.[2] Despite the diverse interest groups that supported the March, the event was a manifestation of female leadership: millions of people wearing *pussyhats* (i.e. pink hats knitted by volunteers) took to the streets and showed the world what women were capable of organizing (Pussyhat Project n.d., see also Figure 3)

The March had a twofold legacy: it inaugurated a series of protests against the Trump administration and it favored the emergence of local *Resistance* groups.[3] Fisher (2019) administered surveys to protest participants and follows them over time. Thanks to her work, we know that most of protesters that turned out on January 21^{st}, 2017, were college educated women that identified themselves as Democrats. Her book describes how the same *resisters* that marched on January 21^{st} kept on marching on DC up

[2] *Intersectionality* is a characteristic of the fourth wave of American feminism (Fisher, Dow and Ray, 2017). Among the organizers of the 2017 Women's March we find: *Black Lives Matters, Occupy Wall Street, OkayAfrica, Gatherings for Justice* and the *American Civil Liberties Union*. Pro-life movements were excluded from the protests (Fisher, 2019). Most importantly, American feminism has been historically criticized for having a mostly white liberal identity (Davis, 1981).

[3] Fisher (2019), p.5, defines the *American Resistance* as: "People working individually and through organizations to challenge the Trump Administration and its policies. The Resistance includes people working as individual citizens (and non-citizens), through their profession as lawyers, scientists, artists, or professional athletes. It also includes organizations that run the gamut in terms of their levels of professionalization. The violent fringe that stirred in response to White supremacist activities around the US - the Antifa - is also part of the Resistance to the degree that it is focusing specifically on targeting the Trump agenda. In contrast to other claims, anonymous people in the Trump Administration who have challenged President Trump as a person but support his broader policy agenda are not part of the Resistance"

to the 2018 midterm elections, while at the same time they actively engaged in local political activities within their congressional districts.[4] One of the aims of the *resisters* was to increase turnout in the 2018 midterm elections, with the explicit intention of "Flipping seats from red to blue and winning democratic majorities." (Swing Left, n.d.). Hence, there was a high degree of complementarity between the Democratic Party electoral campaigning strategy and the Women's March movement.[5]

Since the March was a mobilization against the newly elected president, 500,000 people marched on the US Capitol in Washington DC (Jamieson, 2016), making it the largest gathering of the day in absolute terms. Nonetheless, the largest March in relative terms took place in the town of Seneca Falls, where on January 21^{st}, 2017, there were more protesters than residents (see map 4). The first American feminist wave was inaugurated by the Seneca Falls convention in 1848: the meeting anticipated the debate on women's right to vote in elections by more than two decades. This paper uses distance as a continuous treatment variable precisely because mobility of citizens to gather in symbolic places translates to classical measurement error in protest size (see Appendix A for a discussion).

3.2 Institutional context

The House of Representatives is the lower chamber of the bicameral legislature of the US federal government and it is composed of 435 voting members. Each representative is elected to represent one congressional district for a two-year mandate through single member plurality voting (US Gov, 2020).[6] Congressional districts (CD or districts from now on) are apportioned to states on a population basis and must be all equal in terms of population. Every 10 years (i.e. 5 elections) the Federal Government re-apportions CD to states based on the decennial census (US Census, 2020). Most state parliaments have the authority to re-draw CD boundaries before elections (Ballotpedia, 2020*b*).[7] As a result of this institutional framework, districts' boundaries may change from one election to the next. Across the 2012-

[4] Among the protests where the 2017 Women's Marchers turned out we can name the following: March for Science, People Climate March, March for Racial Justice, March for our Lives and the Families Belong Together (Fisher, 2019).

[5] As an additional anecdote, Amanda Arias Lewis (the executive assistant of the Women March organization), was the office manager of senator Sander's private suite for the 2020 presidential elections (Fisher, 2019).

[6] In SMP electoral systems voters cast a ballot directly for candidates rather than for parties. SMP voting system is also known with the name of *first past the post* SMP voting systems favour the party that can influence the distribution of its voters across electoral districts. Indeed, winning by small margins in most of districts and losing by large margins in all the others can still pay with a representation premium.

[7] 33/50 states parliaments can rule over redistricting by means of a state law. The law is introduced by the party who rules the state legislature (state's upper and lower chamber). The remainder states rely on different redistricting procedures.

2018 elections, four states were affected by redistricting: Florida, North Carolina and Virginia in 2016; Pennsylvania in 2018. The Pennsylvanian 2018 congressional redistricting was mandated by the state's Supreme Court with the aim of remedying to an illegal partisan gerrymander. [8]

To run for office as US House representative, an individual must satisfy three federal requirements: having attained the age of twenty-five years, being a citizen of the US since at least seven years and being an inhabitant of the state where s/he is elected (US Constitution, 2020).[9] On top of federal requirements, candidates must satisfy a variety of state-level provisions. Indeed, the US is one of the few countries that does not have federal ballot access laws (Ballotpedia, 2020*a*).

Ballot access laws usually impose two requirements to congressional candidates: to file a petition with a minimum number of signatures from citizens endorsing the candidature and to pay a registration fee.[10] The minimum required number of signatures, the registration fee and the deadlines for complying with the filing requirements may vary from one state to another and from one election to the next, but they do not vary along a gendered dimension (Ballotpedia, 2020*a*). Within this regulatory framework, a US citizen has three different ways to ensure that s/he is entitled to a federal office: s/he can seek the nomination of a state recognized political party, s/he can run as an independent candidate and s/he can run as a write-in candidate.

Primary elections constitute the ideal setting to test whether the March affected women's choice to run for office because they are the ground used by political parties to nominate the candidates they want to list on the general election ballot. Any citizen who meets both federal and state level ballot access requirements has the right to compete in the primary. In the US, primary elections are disciplined by state laws which are often nuanced: their legal framework can vary from one election to the next. The National Conference of the State Legislatures classifies primary elections in different categories based on their openness: closed, partially closed, partially open, open to unaffiliated voters, top two and top four (NCSL, 2020). This categorization emphasizes that primary election systems usually differ in the pool

[8]Pennsylvanian Republicans −the faction who was violating redistricting laws− petitioned the Supreme Court to oppose the redistricting plan, but failed.

[9]The only federal requirement that is disciplined outside the US Constitution concerns the financing of electoral campaigns. Candidates must file a petition to the Federal Election Commission (FEC) within 15 days from making campaign expenditures exceeding $5,000. The FEC cannot prevent any individual who correctly files the petition from running for office and has to authorize the candidates within 10 days from receiving the relevant documentation (FEC, 2020).

[10]In some states both the petition and the fee are normative requirements, in others it is sufficient to meet one of them. In most of the states the petition is an essential requirement.

of voters entitled to cast a ballot.

Most notably, while general elections regularly take place every two years, the regulatory fragmentation that characterizes primary elections implies that we do not observe Republican nor Democratic primary races for each House seat that is up for election.[11] Nonetheless, both the Republican Party (also known as Great Old Party, i.e. GOP) and the Democratic Party (DP) usually call the primaries before general elections. In the US 90% of Congress members are re-elected (Fowler and Hall, 2015), which translates in most primary races being de facto uncontested.

4 Data

To empirically assess the impact of the March on women's choice to run for office and its consequences for female political representation, I build a novel CD level dataset using several data sources. The main challenge in the construction of a CD level panel is redistricting. To address the changes in electoral geography that may plague my estimates, I follow Autor et al. (2020); Calderon, Fouka and Tabellini (2021); Ferrara et al. (2021) and build population crosswalk files to bridge political geographies. As a result, I obtain a balanced panel of congressional districts for the years 2012-2018.[12] To build the crosswalks I leverage US congressional districts TIGER/Line Shapefiles (US Census Bureau, 2020*b*) and 2010 population raster data with a 1x1km resolution (Fang and Jawitz, 2018).[13] The exact procedure followed to bridge political geographies is described in Appendix B.[14]

I begin by collecting historical records of US House Democratic and Republican primaries (Miller and Camberg, 2020).[15] I focus on two main variables: a dummy for having at least one female candidate

[11]General elections take place after primaries, and they coincide with midterm and presidential elections. General elections usually determine who will seat in the House for the following two years. There are rare cases when a candidate running in a top-two or top-four primary system gets the absolute majority of votes. When this happens, there is no need to call the general elections and the primary winner is directly entitled to seat in the House. Top-two and top-four partisan primary races are excluded from my analysis.

[12]Such a time span is chosen for two reasons: first, the analysis is truncated in 2018 to avoid picking up the effect of the 2018 Women's March. Second, crosswalking CD to add more pre-treatment years would entail ending up with heavily processed data for the years prior to 2012. Results on the panel of CD that were not affected by the 2010 federal redistricting are available upon request (i.e. results with a smaller sample size, but with more pre-treatment years).

[13]The 2012-2020 congressional districts were apportioned to states based on 2010 Census.

[14]All the results are robust to addressing redistricting following Fowler and Hall (2015): if a state passes a redistricting bill, then all the CD within that state are coded with a new identifier. Results are also robust to using a balanced panel of administrative units and to dropping all the states affected by redistricting.

[15]Primary election returns are available up to 2018 only. Primary election returns for the 2020 election cycle are available in the America Votes 34 (2021). The data collection to expand the time period of the analysis is in progress. The Miller and

in the primary race and the share of women running in each party's primary. These measures allow to analyse the impact of the March on the extensive and intensive margin of the supply of female politicians. Moreover, I analyse the share of votes for females and the dummy for female primary winner.

To analyse the impact of the March on the probability that a woman is elected in general elections, I augment the data with records from the Center for American Women and Politics (CAWP, 2022) and code a dummy for female US House representative. The share of votes for females in general elections comes from the MIT (2020). Since the MIT (2020) data does not contain candidate's gender, I follow Wasserman (2018) and use Social Security Name Files to infer gender. Turnout in general elections and the share of votes obtained by the Democratic Party in the previous US House election come from the Dave Leip's Election Atlas (2020). Population density is calculated using US Census Bureau (2020*a*).

I also use protest data from the Crowd Counting Consortium (CCC; Chenoweth and Pressman, 2017; Fisher et al., 2019). The CCC is an academic project whose mission is to collect information on marches, protests, strikes and riots in the US. Data are crowd sourced and verified by affiliated scholars through fact-checking. Sobolev et al. (2020) show the reliability of the CCC data as compared to protest estimates from cell phones and Twitter data. The raw crowd estimates contain information on any protest event, including minor marches. According to the CCC, there were 614 Women's Marches throughout the US. To infer the exact geographical coordinates of each March, I follow Wallace, Zepeda-Millán and Jones-Correa (2014) and rely on the GIS geocoding service.[16] Hence, for each March I have latitude and longitude (i.e. protest geographical points).

Last, I build the district level population weighted distance to the nearest protest. I do so leveraging population raster data with a 1x1km resolution (Fang and Jawitz, 2018), a 5x5km grid of the US (Talbert and Reichert, 2018) and the CD TIGER/Line Shapefile of the 116^{th} Congress (i.e. the Congress elected in 2018 – the first election year after the March). The definition of the population weighted distance to the nearest protest is discussed in section 5.1. The first order effect of the 2017 Women's March is shown through a comparative analysis between the Fisher (2019) qualitative results and a quantitative assessment of the impact of uprisings on female civic engagement measured with survey data (CCES,

Camberg (2020) data do not contain primary election returns for the states that adopt a top-two or top-four primary system (i.e. California and Washington) and for Louisiana, where partisan primaries do not take place.

[16] Before resorting to the GIS geocoding service, I manually aggregate protest locations using the US cities comprehensive database (Simplemaps, 2020) to make sure that all the protest locations correspond to a populated place listed in the US Census Bureau. This procedure leaves me with 604 protest locations.

2020). Table 1 reports summary statistics of all the variables.

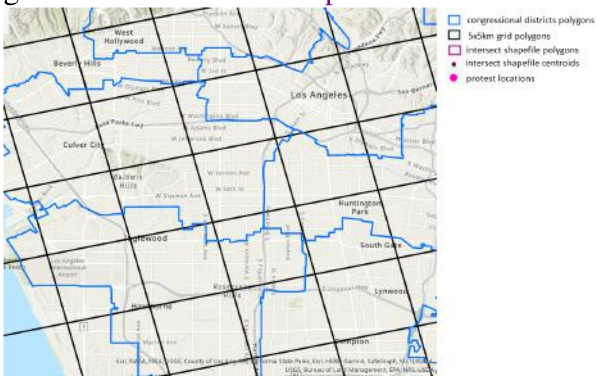
5 Empirical strategy

This paper exploits geographical variation in exposure to the nearest protest as a continuous treatment in difference in differences research design. As discussed in section 3.1, protest size is unlikely to capture the strength of local *Resistance* groups because of measurement error (Fisher, 2019) (see also Appendix A). Nonetheless, under the assumption that a Sister March occurs if and only if there is a cluster of *resisters* who organize it, the distance to the nearest protest is a valid proxy for the constituency's exposure to local *Resistance* activity. Subsection 5.1 defines the population weighted distance to the nearest protest, subsection 5.2 discusses the econometric specifications and subsection 5.3 focus on statistical inference.

5.1 Definition of the population weighted distance to the nearest protest

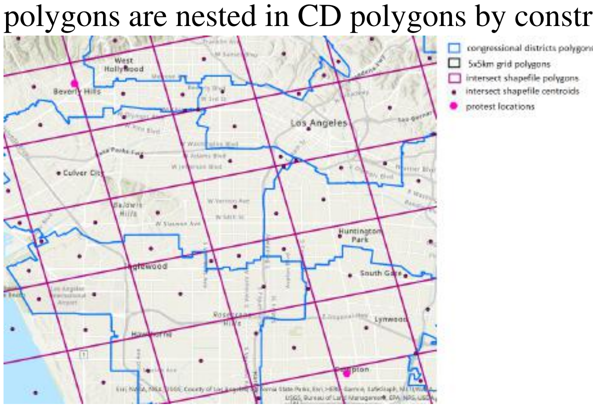
Congressional districts have to be all equal in terms of population (US Constitution, 2020), but they are highly heterogeneous in terms of area. Moreover, the geographic distribution of districts' population may be highly diversified from one CD to the other. The procedure used to define the population weighted distance to the nearest protest accounts for geographically heterogeneous population distributions, which is crucial to assess constituencies' exposure to protests. I proceed in the following steps:

1. Intersect the 5x5km grid of the US with CD 116 Tiger/Line shapefiles $\rightarrow$ generate the intersect shapefile

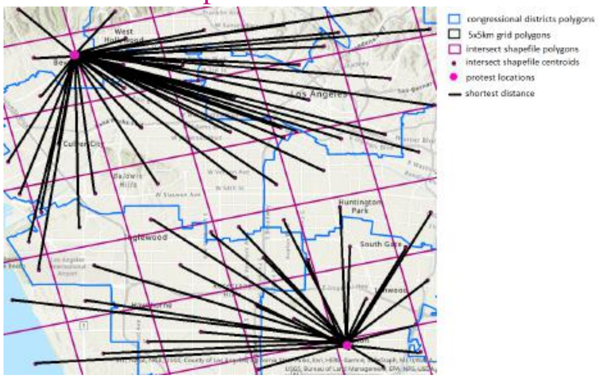


2. For each polygon in the intersect shapefile, generate the geographical centroid and compute the population using the 2010 raster file

3. Overlay the protest locations and the intersect shapefile (recall that intersect polygons are nested in CD polygons by construction)



4. Compute the shortest distance between the centroid of each intersect polygon and the nearest protest location



5. Generate the population weighted distance applying the following formula of a simple population weighted average. Table D1 in Appendix D contains an example of the group structure of the data that allows to compute the population-weighted distance.

$$pop\text{-}weighted\, distance_{CD} = \sum_{intersect\ cells\ \in CD} \frac{population_{intersect}}{population_{CD}} * distance_{intersect}$$

Figure A1 shows the distribution of the population weighted distance to the nearest protest (Panel a) and of its logarithmic transformation (Panel b). The preferred exposure measure that will be used in the econometric analysis is the natural log of the population weighted distance (Panel b). There are at least two reasons for preferring this transformation: first, the log reduces concerns about treatment effect heterogeneity; second, the normality of the treatment variable eases the interpretation of the continuous DiD parameter (Callaway, Goodman-Bacon and Sant'Anna, 2021).

5.2 Difference in differences with continuous treatment

The aim of this analysis is to assess the impact of being exposed to the March in a continuous difference in differences research design. Moreover, I test the plausibility of the strong parallel trends using an event study parametrization. I test the hypotheses that being exposed to the March led women to enter into federal politics, ultimately changing the results of general elections.

I estimate the following equation, separately for Democratic and Republican primary races:

$$y_{pdt} = \theta_d + \Gamma(d)_{st} + \delta POST_t \cdot log\ distance_d + X'_{dt}\mu + \epsilon_{pdt} \tag{1}$$

Where y_{pdt} is a dummy for having at least one female candidate, the share of female candidates, the share of votes for females and the dummy for female elected in the partisan primary p of district d in election year t. θ_d are CD fixed effects and $\Gamma(d)_{st}$ are state-election fixed effects that control for changes in ballot access and primary laws that might have a differential impact on women's versus men's incentives to run for office.[17] I also assess the stability of δ when allowing changes in laws to be captured by state specific linear time trends rather than by state-election fixed effects.[18]

$POST$ is a dummy for the election year after the 2017 Women's March (i.e. 2018) and $log\ distance_d$ is the natural logarithm of the population weighted distance as defined in section 5.1. The logarithm is

[17]Note that CD are geographically nested in states, while election years and primary type fixed effects are nested in state-election fixed effects.

[18]More specifically, I estimate the following equation:

$$y_{pdt} = \theta_d + \Phi_s \cdot trend_t + \delta POST_t \cdot log\ distance_d + X'_{dt}\mu + \epsilon_{pdt} \tag{2}$$

.

I also plan to assess the stability of the coefficients using primary type fixed effects and controlling for changes in ballot access laws. The data collection for these steps is in progress.

used to smooth out possible non linearities in the distance effect and to ease the interpretation of the δ. X'_{dt} are time varying controls: population density and the share of votes obtained by the Democratic Party in the previous US House elections.

As regards the probability that a woman is elected to represent the district in the US House, I estimate the following equation:

$$y_{dt} = \theta_d + \Gamma(d)_{st} + \alpha POST_t \cdot log\ distance_d + X'_{dt}\mu + \epsilon_{dt} \tag{3}$$

Where y_{dt} is a dummy for female US House representative, the share of votes for females in general elections and the turnout in general elections. The remaining components are defined as in equation (1). The only difference between equation (1) and (3) is that general election returns do not have a partisan dimension (p).

The main challenge to the identification of the causal effect of the March is that protests are more likely to occur (and to be larger in magnitude) in places with greater underlying political grievance and historical ties with feminist movements (see map 4). While I can directly control for population density and for the share of votes obtained by the Democratic Party in the previous US House elections, local dissent for the Trump administration and historical ties with feminist movements are unobservable determinants of protests that are likely to correlate also with the supply of female politicians and with the probability that a woman is elected to represent the district. It is therefore crucial to establish the plausibility of the identifying assumption in equations (1) to (3) to give a causal interpretation to the DiD coefficient (i.e. it is crucial to assess the plausibility of the strong parallel trends).

When the treatment is normally distributed and under strong parallel trends, the DiD parameter can be interpreted as a weighted average of Average Causal Responses at different log distances weighted by the distribution of the log distance variable. Strong parallel trends is a stricter assumption than standard parallel trends: it requires to impose assumptions on potential outcomes under different log distances rather than on untreated potential outcomes only. More specifically, assuming strong parallel trends is close to assuming that all the districts would have experienced the same path of outcomes had they been assigned the same log distance (Callaway, Goodman-Bacon and Sant'Anna, 2021). Equations 4 and 5 flexibly parameterize the DiD specifications so as to assess the absence of pre-treatment differential time trends. Moreover, section 9 analyses the impact of the March using binary difference in differences, which require weaker identifying assumptions.

A concern to identification would arise if districts assigned to different log distances were characterized by different post treatment growth rates in the potential outcomes. For instance, if we think that districts more exposed to the protests are at the same time affected by changes in ballot access or primary laws that have a differential impact of females' versus male's incentives to run for office, it might be hard to believe that the strong parallel trends would continue to hold post treatment. Four things should be reassuring in this regard: first, I control for changes in ballot access provisions and primary laws in two different ways and the estimated parameters are highly comparable across model specifications; second, the distance variable arises from a geostatistical analysis of the administrative partition of the electoral process and is likely to be relatively exogenous to district specific differential time trends; third, I formally test the lack of pre-treatment differential time trends through an event-study parametrization; fourth, I perform several robustness checks relaxing the strong parallel trends assumption (i.e. discretizing distance, see section 9).

I assess the plausibility of the strong parallel trends assumption for the supply of female politicians by estimating equation (4) separately for Democratic and Republican primaries:

$$y_{pdt} = \theta_d + \Gamma(d)_{st} + \sum_{\substack{\tau=2012 \\ with\ \tau\neq 2016}}^{\tau=2018} \delta_\tau \cdot log\ distance_{d\tau} + X'_{dt}\mu + \epsilon_{pdt} \tag{4}$$

Equation (4) has the same structure as equation (1), with the only difference being that $log\ distance_{d\tau}$ is the interaction between a time-varying battery of dummies for each election year (τ) and the district specific log distance. Hence, δ_τ are the estimated parameters for the lagged variables and for the lead variable. The absence of lead effects in estimates of equation (4) would be suggestive of the absence of violation of the strong parallel trends assumption.

Last, I estimate equation (5) to test the plausibility of the strong parallel trends assumption for the probability that a woman is elected to represent the district in general elections:

$$y_{dt} = \theta_d + \Gamma(d)_{st} + \sum_{\substack{\tau=2012 \\ with\ \tau\neq 2016}}^{\tau=2020} \alpha_\tau \cdot log\ distance_{d\tau} + X'_{dt}\mu + \epsilon_{dt} \tag{5}$$

Here, again, the only difference between equations (4) and (5) is that general elections do not have a partisan dimension (p).

5.3 Inference

A potential threat to the validity of inference in this analysis is the existence of spatial correlation in both the treatment and the dependent variables. Indeed, the supply of female politicians is greater in more progressive areas of the country (e.g. on the West coast rather then in the South of the country). The same holds true for the probability that a woman is elected in general elections. Moreover, neighboring districts are characterized by similar treatment values. These spatial patterns increase the probability of Type I errors and may induce to over-reject the null hypothesis of no effect of the March, leading to incorrect conclusions (Conley, 1999; Colella et al., 2019).

I allow the error term in equations 1 to 5 to be both auto-correlated over time and spatially correlated across districts that are located below an optimal distance threshold. Spatial correlation across districts is introduced using a uniform spatial pattern matrix. Moreover, in absence of previous knowledge about the optimal distance threshold for spatial correlation in this setting, I follow Colella et al. (2019) and: (i) estimate standard errors for a large set of potential optimal distance thresholds; (ii) check for the presence of a non-linear patterns; and (iii) retain the distance threshold that yields to the most conservative standard errors. Appendix C presents the figures used to choose the optimal distance thresholds: 255km for primary election outcomes, 135km for general election outcomes. I also report standard errors: (i) relaxing this threshold; and (ii) clustering at the district level. (i.e. allowing the residual to be correlated over time only).

6 The first order effect of the March

The 2017 Women's March was a turning point for the US democracy, as many local movements emerged after the feminist uprisings. The mission of these movements was to swing left the following US House elections, as the *resisters* felt threatened by the election of Donald Trump as US president. Fisher (2019) describes how the people that turned out to protest in January 21^{st}, 2017, were mostly college educated

Democratic women who became increasingly engaged in local politics. This paper is based on the idea that exposure to protests is a valid measure of exposure to *Resistance* activity. In particular, I start from the hypothesis that a Sister March occurs if and only if there is a cluster of *resisters* who organize it. It follows that distance to the nearest protest -regardless of its size- captures exposure to the increased civic engagement of protesters. Based on this rationale, the first order effect of the feminist uprisings would be to trigger an increase of female civic engagement in the general population.

Fisher (2019) administered surveys to protesters over time and finds that 58% contacted and elected official, 42% reported having attended a town hall meeting, 31% worn a safety pin for social justice, 23% participated in a direct action and 21% contacted the media to express a view during the past year (p.55). The author interprets these responses as a signal that the *resisters* were doing more than marching: they were increasingly participating in institutional politics within their congressional districts.

The Cooperative Congressional Election Survey (CCES, 2020) is the largest political survey administered in the US.[19] It contains a survey item measuring if the respondent attended a local political meeting during the past year[20], making possible to quantitatively assess whether the feminist uprisings had a differential "bite" on female versus male civic engagement in the general population. Moreover, the direct correspondence between the Fisher (2019) and the CCES (2020) survey items allows to assess whether the channel that conveys the causal effect of the March is exposure to the increased local political participation of highly educated Democratic women. [21]

I estimate the following equation:

$$y_{idt} = \theta_d + \Phi_s \cdot trend_t + \gamma POST_t \cdot log\ distance_d + X'_{idt}\mu + \epsilon_{idt} \tag{6}$$

Where y_{idt} is a dummy equal to 1 if the responded attended a political meeting during the past year. Controls are birth year, education, race dummies, employment dummies and marital status dummies. Table 6 reports the results of the causal effect of the March: doubling the distance leads to a 9% drop in female civic engagement. I find no evidence of an effect of the feminist uprisings on male civic engagement, suggesting that the feminist uprisings have a "bite" on females only. This result suggests that

[19] The large sample size of the survey allows to limit concerns about the representativeness of the survey at the district level (the level of treatment variation). See Warshaw and Rodden (2012) for a discussion.

[20] School board and/or city council

[21] There is no direct correspondence between the other survey items used by Fisher (2019) and the CCES survey items.

female protesters' increased political participation has peer effects on women in the general population. The following sections assess whether increased civic engagement triggers shifts in the supply of female politicians in the primaries.

7 Results

7.1 Primary elections

Table 2 presents the estimates of the causal effect of being exposed to the March on congressional partisan primaries (equation (1)). Columns 1-4 report the estimates on Republican races, columns 5-8 on Democratic races. Panel A shows the baseline DiD specification, Panel B adds the controls and Panel C substitutes state-election fixed effects with state-specific linear time trends. Throughout the analysis, I report standard errors corrected for spatial correlation using the optimal distance threshold, relaxing the threshold and clustering at the district level (see section 5.3).

Table 2 shows that the March had a positive effect on both the extensive and the intensive margin of the supply of female Republican politicians: the greater the distance from the nearest protest, the lower the supply. In particular, doubling the distance to the nearest protest decreases the probability of having at least one female candidate in the Republican party's primary by 7 percentage points, which corresponds to a 29% drop of the sample pre-treatment probability.[22] Moreover, doubling the distance leads to a 7 percentage points drop in the share of female Republican candidates, which corresponds to 60% drop with respect to the pre-treatment share.

The uprisings also caused a simultaneous increase in the demand for female Republican politicians, as highlighted by the behavior of the share of votes for females in the primary: doubling the distance leads to a 7 percentage points drop in the share of votes for females. Nonetheless, the probability that a woman wins her Republican primary is not affected by the protests: the increase of the demand for female Republican politicians is diluted by the increase of the intensive margin of the supply (i.e. the share of votes for females is diluted by the share of female candidates).

[22]
$$\Delta P(woman\ candidate) = \frac{\delta}{Dep\ var.\ mean} \tag{7}$$

As regards Democratic primary races, the sign of the point estimate of the DiD coefficient (δ in equation (1)) is line with theoretical expectations, but not statistically significant at conventional levels. Figure 5 provides some evidence in support of strong parallel trends.

7.1.1 Votes for females and primary openness

Primary election systems vary in their openness to unaffiliated voters. I assess whether the demand for female politicians varies heterogeneously across primaries open to unaffiliated voters and primaries that impose some restrictions on the pool of voters entitled to cast a ballot: Table 3 shows the heterogeneous effect of the March on the share of votes for females in primaries. Columns 1-3 report the estimates on open races, columns 4-6 on non-open races. Columns 1 and 4 show the baseline difference in difference specification, columns 2 and 5 add time-varying controls (population density and support for the DP in the previous US House election) and columns 3 and 6 substitute state-election fixed effects with state-specific linear time trends. Panel A shows the results on Republican Primaries, Panel B on Democratic races.

The positive effect of the protests on the share of votes for females in primaries is confined to open Republican primary races only: doubling the distance to the nearest protest decreases the share of votes for females by 10 percentage points. Hence, the demand for female politicians is stronger in primary elections open to unaffiliated voters, where voters may not be registered with the Republican Party and may decide not to vote Republican in general elections.[23]

7.2 General elections

As regards the effect of the March on general election results, Table 4 presents estimates of equation (3). Columns 1-3 report the estimates on the share of votes for females, columns 4-6 in the dummy for female elected and column 7 on the turnout. Columns 1 and 4 pool together Republican and Democratic female representatives, columns 2 and 5 refer to female GOP candidates and columns 3 and 6 focus on female Democratic candidates. Similarly to Table 2, Panel A shows the baseline DiD specification, Panel B adds the controls and Panel C substitutes state-election fixed effects with state-specific linear time trends. The

[23] I also assess the impact of the March on the turnout in primary elections, finding no evidence of an effect of the protests on turnout in primaries. Results are available upon request.

March had a positive effect on the probability of electing female US House representatives: doubling the distance to the nearest protest decreases the probability of electing a female US House representative by 6 percentage points, which corresponds to a 33% drop of the sample pre-treatment probability, regardless of party affiliation. Results seem to be driven by Democratic women, for which I find a decrease of 35% of the sample pre-treatment probability. I find no evidence of an effect for Republican women. Moreover, this result is due to the increase in the share of votes for Democratic female candidates in general elections and is coherent with the increased demand for female Republican politicians being concentrated in primaries open to unaffiliated voters.

Last, the March seems to have a positive effect on turnout in general elections: doubling the distance causes a 2% drop in the turnout. This result suggests that the March was effective in achieving one its main goal: convince US citizens to turn out to vote. Moreover, (Fisher, 2019) describes how the *resisters* were locally active in actions to increase turnout, suggesting that the local political activism ignited by the March is channeling the causal effect identified by these regressions (Section 6 details this aspect). Figure 6 provides evidence in support of the strong parallel trends assumption. Parallel trends seem not to hold perfectly for general election results, suggesting that the causal impact of the March on General elections must be interpreted with a grain of salt.

8 Mechanisms

8.1 District shopping

Any US citizen that satisfies federal and state ballot access requirements is entitled to run for elective office (US Constitution, 2020). Federal requirements impose that the candidate must be resident of the *state* where s/he is elected, not of the congressional district. Hence, female Republican candidates could strategically switch district within the same state so as to run in districts more exposed to the March, with the aim of capitalizing on the feminist uprisings.

Table 5 shows descriptive statistics on the number of candidates switching congressional district in subsequent elections, with breakdown by treatment status. Districts located below the median population weighted distance to the nearest protest are classified as *close*, meaning treated by the 2017 Women's

March. Panel A shows the results on females, Panel B on males. The table shows that district shopping is virtually absent for females: only three women switched district between the 2016 and 2018 elections. Moreover, Figure 7 shows the number of candidates in partisan primaries, with breakdown by gender and first timers status. Almost all the women running in the 2018 Republican primaries where seeking a federal office for the first time in their political career. These evidence should reassure the reader in that district shopping is not driving the results.[24]

8.2 News coverage

On January 21^{st}, 2017, more than four million people took to the streets in around 600 different protest locations. The event received wide media coverage in national and local news and the topic was salient on social media platforms such as Facebook and Twitter (the movement was born on Facebook).

To understand whether media coverage of the event triggers a behavioral response, one should start acknowledging the complexity entailed by assigning congressional districts to one media outlet or to a combination of media outlets: TV signal, radio waves and social media networks encompass congressional district's boundaries. Moreover, citizens self select into where to source news. For instance, as regards TV broadcasts, Republicans are more likely to self select into watching Fox News, while Democrats are more likely to watch CNN. Even if one would choose to assign each congressional district to a weighted average of media outlets, it would be impossible to establish which branch of the constituency is exposed to which media. This is a fundamental challenge, because different media may speak about the event with different intensities and sentiments, triggering different behavioral responses.

The same applies to social media: the relevance and the sentiment of the topic depends on each individuals' personal network. Related to social media, an additional layer of complexity is added by foreign influence operations aiming at sabotaging the feminist wave. Bradshaw and Henle (2021) perform a qualitative analysis of twitter accounts then linked to the Russian GRU and IRA, focusing on the communication of feminism and women's rights.[25] The study finds that countermovent narratives about feminism demobilizes civil society and generates virality around divisive topics. Moreover, the accounts

[24]The whole analysis presented in this paper can be repeated on first timer candidates. Results are virtually identical and available upon request.

[25]The Russian GRU is the foreign military intelligence agency of Russia, while the IRA is a Russian company engaged in online propaganda and influence operations which pursues Russian business and political interests.

linked to the Russian federation were indirectly targeting high profile feminist politicians, activists and public figures.[26]

While understanding whether different narratives of the event on the media trigger different behavioral responses is beyond the scope of this project, it is important to understand whether distance to the nearest protest matters beyond news coverage. Indeed, this paper builds on the hypothesis that distance captures the salience of the uprisings for the districts' constituencies. However, a potential threat to identification arises if the salience of the event for the electorate depends on news coverage.

I start from the hypothesis that if all of the constituencies' behavior depends on news coverage and not on what happens in proximity to where they live, then the effect of distance should fade away once we control for news coverage.

I test this hypothesis by relying on Nexis Uni, a Lexis Nexis resource which contains US news. I perform a search of the count of newspaper articles mentioning together the Women's March *and* the Sister March location. I repeat the same search for a placebo protest: I retrieve the count of articles mentioning together Black Lives Matter *and* the Sister March location.[27] This procedure allows each protest location to be assigned to a 2017 Women's March news count and to a placebo news count. The placebo is important because the salience of the March in the news needs to be cleaned of the general effect of media coverage of protest locations. I then assign each district to a population weighted average of news coverage of the nearest protest following the steps described in Section 5.1.

Last, I create the following time invariant measure of the 2017 Women's March news salience:

$$log\ news\ salience_d = log(\frac{pop - weighted\ news\ count\ WM}{pop - weighted\ news\ count\ BLM}) \tag{8}$$

Figure D3 shows the distribution of the log news salience, while Table 7 shows that all the results

[26] An example is Linda Sarsour, who was one of the leaders of the 2017 Women's March and a potential Arab American candidate for elected office. Her candidature was backed by Bernie Sanders, one of the most progressive liberals of the country. The Russian twitter accounts described her as radical jihadi who enfiltered American feminism. Linda Sarsour had to step away from the Women's March movement and from seeking elected office (Fremson, 2022).

[27] I apply the filters to restrict the search to news in English published in newspapers located in the United States. I restrict the dates of publication from January 21^{st}, 2017 to 1^{st}, 2018. I elicit such a time period so as to avoid picking news about the 2018 Women's March. Once I apply the filters, the total number of articles mentioning the Women's March is 5,912, while the number of articles mentioning Black Lives Matter is 5.865. I exclude Sister March locations with ambiguous names (e.g. Accident, Reading, Sharon. Around 4% of protest locations). BLM was one of the movements supporting the 2017 Women's March, hence there are news mentioning together the two movements and the Sister March location. Table E2 shows the cross tabulation of the news coverage dummy and of the placebo dummy.

discussed in section 7.1 hold when augmenting equation (1) with the interaction between the dummy for after the March and the log news salience. This result confirms that distance matters beyond news coverage and constitutes the first placebo in treatment test provided in this paper.

8.3 Distance to the nearest urban cluster

Protests are usually organized in cities, and proximity to urban clusters may itself trigger an effect on the supply of female politicians in times of critical junctures.

I test whether the behavioral effect of the March flows through proximity to highly populated urban clusters by augmenting equation (1) with the interaction between POST and the time-invariant log population-weighted distance to the nearest urban cluster with at least X inhabitants, with $X \in \{50k, 100k, 150k, 200k, 250k, 300k, 350k, 400k\}$. Figure D4 shows the distributions of the distance to the nearest protests and to the nearest urban cluster: collinearity between distance to the nearest protest and distance to the nearest urban cluster is a challenge in this setting.

Table 8 shows the results on the share of female Republican candidates, while Table 9 shows the results on the dummy for at least one female candidate. The point estimate of the DiD coefficient is stable to the introduction of the additional variable in equation (1) and it never flips sign, even though the variable introduced is collinear with the treatment. These result constitutes additional placebo in treatment tests.

9 Robustness checks

Strong parallel trends may be an unbelievably stringent assumption. Moreover, DiD parameters may be hard to interpret when the treatment is continuous (Callaway, Goodman-Bacon and Sant'Anna, 2021).[28] To overcome these potential caveats, I perform several robustness checks discretizing the population weighted distance to the nearest March. More specifically, I classify congressional districts as *close* (i.e. treated) and *not close* (i.e. untreated) so as to assess the stability of the estimated parameters when relaxing the identifying assumption. Section 9.1 discusses the baseline difference in differences specification, while section 9.2 presents a distribution-based approach to treatment assignment. The goal

[28]See section 5.2 for a discussion of the interpretation of DiD parameters in this paper.

of this latter robustness check is to show that the binary DiD results hold across multiple discretization of the continuous distance variable.

9.1 Binary DiD: baseline

The identifying assumption in binary difference in differences requires that the counterfactual trend behavior of potential outcomes would have been the same in *close* and *not close* districts in absence of treatment. Indeed, the classical parallel trend assumption imposes restrictions on untreated potential outcomes only. So as to assess the robustness of the findings when relaxing strong parallel trends, I begin by classifying as *close* all the districts below the median of the population weighted distance variable: all the districts that had a March within 24.41km (see table E1). Next, I estimate equation (9) to assess the impact of the March and equation (10) to check the plausibility of the parallel trend assumption.

$$y_{pdt} = \theta_d + \Gamma(d)_{st} + \gamma POST_t \cdot close_d + X'_{dt}\mu + \epsilon_{pdt} \tag{9}$$

$$y_{pdt} = \theta_d + \Gamma(d)_{st} + \sum_{\substack{\tau=2012 \\ with\ \tau\neq 2016}}^{\tau=2018} \gamma_\tau \cdot close_{d\tau} + X'_{dt}\mu + \epsilon_{pdt} \tag{10}$$

All the variables in equation (9) are defined as in equation (1), with the only difference being that $close_d$ is a dummy for districts below the median population weighted distance. Similarly, all the components of equation (10) are defined as in equation (2), with the only difference being that $close_{d\tau}$ is a time-varying battery of dummies for *close* districts in each election year. Last, note that equation (9) and (10) refer to primary election results (i.e. the dependent variable and the error term have a partisan dimension p). The same equations can be used to analyse general election results by dropping the partisan dimension p .

Tables 10 and 11 show the results of being *close* to the March on primaries and general election results. Being *close* to the March increases the probability of having at least one female candidate in the GOP primary by 8 percentage points, a magnitude similar to the effect of doubling the distance to the nearest March in the continuous DiD case, even though barely significant at conventional levels. As regards the share of female candidates in Republican primaries, being *close* to the March causes a surge of 49% with respect to the sample pre-treatment share. Similarly to the continuous case, being close to the March also increases the share of votes for females. The point estimate in Democratic primaries is in line with theoretical expectations, but not precisely estimated. Moreover, table 11 shows that being close to the March increases the probability of electing a female US House Representative by 8 percentage points, regardless of party affiliation. Again, results seem to be driven by the election of Democratic women. Figures 8 and 9 report the event study plots that provide evidence in support of the parallel trends assumption. Note that these figures present estimates of γ_τ when not including controls, to provide evidence that parallel trends hold unconditional on covariates. To sum up, all the findings of the continuous DiD continue to hold in the binary specification. In particular, the magnitude of the estimated parameters is highly comparable across model specifications.

9.2 Binary DiD: a distribution based approach

A valid concern related to the distance based binary DiD (equation (9)) and event study (equation (10)) estimates is whether the estimated parameters are identifiable only when discretizing distance arbitrarily. To address this potential caveat, in this section I check the robustness of the results across multiple discretization of the population weighted distance variable.

Table E1 shows the distribution of the population weighted distance, while Figure A1 panel (a) shows its probability density function. Based on the distribution of this variable, I begin by classifying as *close* all CD located within the 50^{th} centile of the population-weighted distance and then I progressively classify as *close* all CD located within the 55^{th}, 60^{th}, 65^{th}, 70^{th}, 75^{th}, 80^{th}, 90^{th}, 95^{th} and 100^{th} centiles. I estimate a battery of difference in differences specifications (i.e. equation (9)), changing each time the definition of *close*. Figures E1, E2 and E3 plot the estimated δ and α for each centile of the population-weighted distance variable. All the core results discussed in section 7 and 9.1 remain qualitatively and quantitatively stable from the 50^{th} through the 75^{th} centile of the population-weighted distance. Most notably, the γ parameters decrease and become indistinguishable from zero beyond the 80^{th} centile of distance: when classifying as *close* districts that were actually *far* from the March, it is impossible to identify any effect.[29]

[29] The results of this robustness check for non-core results are available upon request.

Next, I check the lack of pre-trends across the binary treatments that convey promising DiD estimates, progressively classifying as *close* CD located within the 50^{th} through the 75^{th} centile of the population weighted distance (i.e. 6 different binary treatments). Figures E4, E5 and E6 show the plot of the γ_τ (equation (10)) separately for each of the 6 binary treatments. Results remain comparable to the estimates discussed in section 7. All in all, the robustness checks suggest that results are not driven by the arbitrary choice of the binary treatment assignment and should reassure the reader about the plausibility of the parallel trends across different treatment definitions.

10 Conclusion

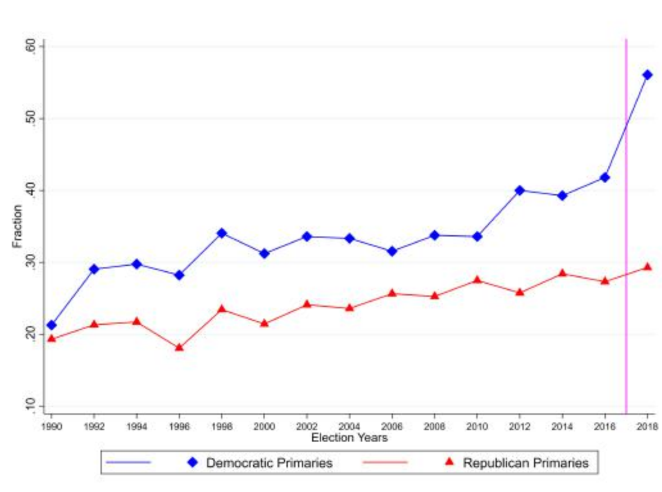
I show how the 2017 Women's March had a positive effect on the propensity of women to run for political offices at the federal level, ultimately reducing the gender gap in the US House. These results are consistent across flexible difference in differences parameterizations (i.e. DiD and event study analyses) and treatment definitions (i.e. using continuous and binary treatments). Furthermore, the effects are heterogeneous along party lines, greater for Republicans and virtually absent for Democrats.

The 2017 Women's March was a showcase of female leadership and favored the emergence of local *Resistance* groups: Fisher (2019) documents how the same *resisters* that marched on January 21^{st}, 2017, then engaged in local political activities within their congressional districts. The *resisters* were mainly college educated white women, identified themselves as Democrats and had a median age of 43 years. The peer effects of these female *resisters* cause an increase in the political participation of women in the population, which channels the effects of the uprisings. Moreover, the peer effects of these women are identifiable on marginal conservative women, who were pushed to place candidatures in the GOP primaries more exposed to the March.

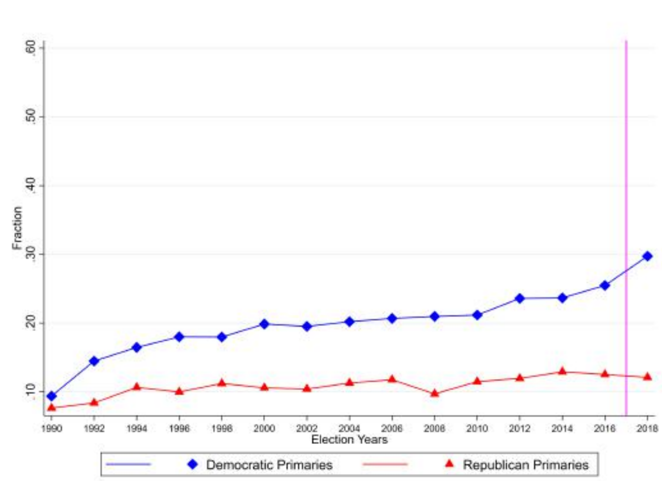
One limitation of this study is that it is difficult to disentangle women's choice to run for office from parties' choice to strategically sponsor female candidates: the informal process that leads a woman to place her candidature in the party's primary of a specific congressional district is not reflected in election records. To overcome this limit and disentangle the two components, Moresi (2023) replicates this analysis on data on female candidatures for local political offices. Indeed, the electoral process that leads a woman to enter into local politics is relatively detached from Party politics, allowing to further assess the importance of women's versus party's choice (Wasserman, 2018).

My findings provide evidence that grass-root feminist activism can shape the supply of female politicians in congressional partisan primaries and have consequences on female political representation at the federal level. Moreover, this paper establishes that social movements can have behavioral effects on the out-group of the organizing members.

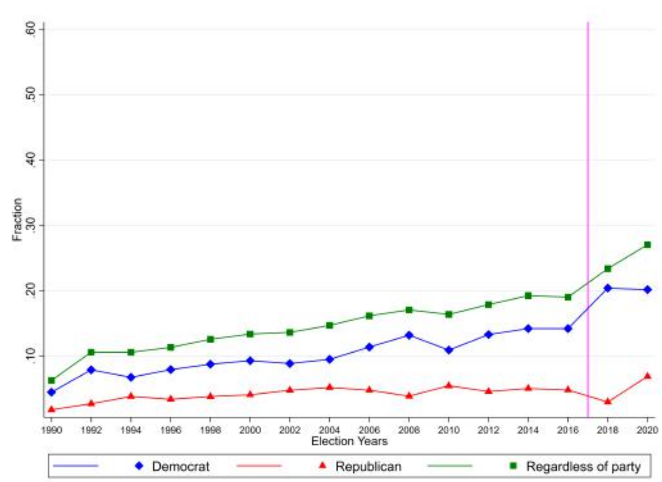
Figure 1: Supply of female politicians and female representation over time



(a) Fraction of districts with at least one female candidate



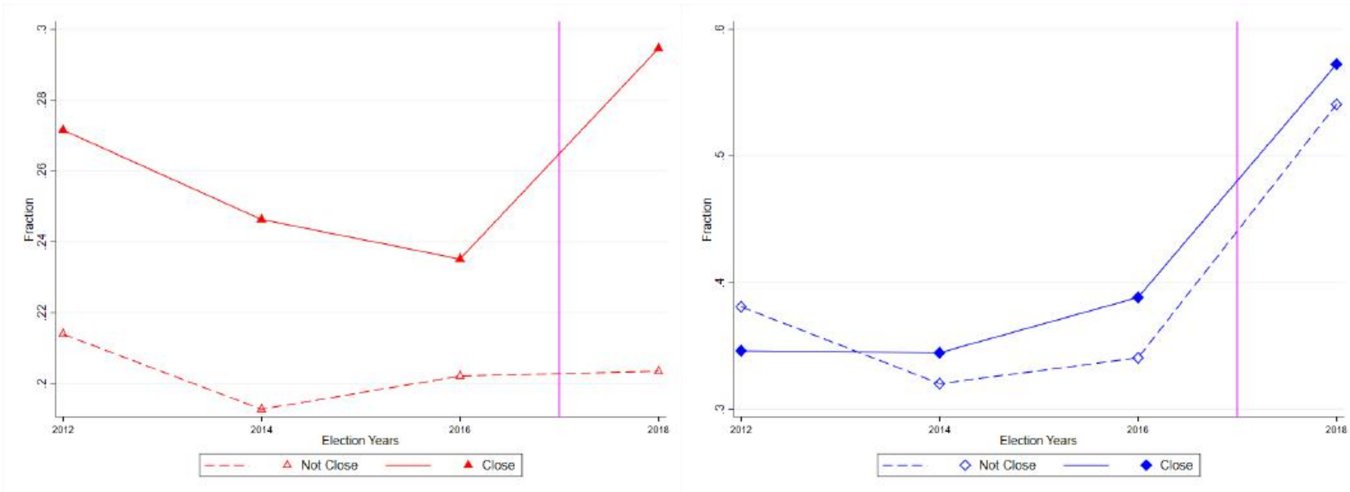
(b) Fraction of female candidates



(c) Fraction of districts with a female US House representative

Notes: The magenta line represents the 2017 Women's March.

Figure 2: The paper in a graph: districts with at least one female candidate by treatment status



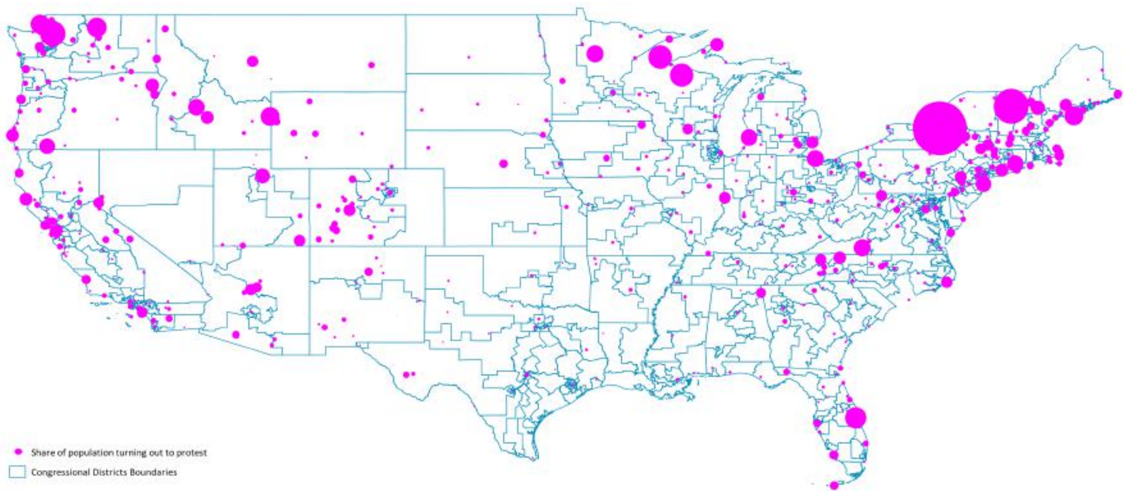
(a) Republican Primaries (b) Democratic Primaries

Notes: Close districts are below the median distance from the nearest 2017 Women's March (i.e. are the districts more exposed to the protests). *Not Close* districts are above the median distance. We can observe that the supply of female Republican politicians increases in treated districts only, while the supply of female Democratic politicians increases everywhere. The magenta line represents the 2017 Women's March.

Figure 3: The 2017 Women's March, a showcase of female leadership

Source: Google Images, query *"2017 Women March"*.

Figure 4: The feminist roots of the 2017 Women's March



Notes: share of population turning out to protest in populated places. The biggest dot is Seneca Falls, the location that hosted the first American women's rights convention in 1848. The map highlights that protesters gathered in symbolic places so as to take to streets: in the town of Seneca Falls the share of population turning out to protest exceeded one. Since protesters moved so as to take to streets and then went back home to engage in local political activities (Fisher, 2019), protest size is affected by measurement error (see Appendix A).

Table 1: Descriptive statistics

Primary Elections	Republican Primaries			Democratic Primaries		
	N	mean	sd	N	mean	sd
Dummy for at least one female candidate	1,350	0.228	0.413	1,362	0.406	0.483
Share of female candidates	1,350	0.123	0.266	1,362	0.254	0.358
Share of votes for females	1,350	0.124	0.277	1,362	0.279	0.384
Dummy for female elected	1,350	0.120	0.319	1,362	0.299	0.450

General Elections	Share of votes for females			Dummy for female elected		
	N	mean	sd	N	mean	sd
Regardless of Party	1,722	0.249	0.295	1,728	0.197	0.392
Republican	1,611	0.048	0.148	1,728	0.044	0.203
Democratic	1,603	0.157	0.256	1,728	0.153	0.354

	Turnout		
	N	mean	sd
	1,728	253,816	74,930.490

Control Variables	Share of votes for the DP in previous US House elections			Population density (residents per km^2)		
	N	mean	sd	N	mean	sd
	1,709	0.496	0.226	1,728	939.32	2,716.409

Channels	Civic engagement from survey data (dummy)			Log salience of the March in the news		
	N	mean	sd	N	mean	sd
	195,891	0.131	0.34	1,728	-0.116	0.878

Log distance to nearest urban cluster Above X inhabitants, with X ∈			
	N	mean	sd
50k	1,728	2.690	0.825
100k	1,728	3.058	0.849
150k	1,728	3.273	0.876
200k	1,728	3.391	0.889
250k	1,728	3.481	0.919
300k	1,728	3.583	0.962
350k	1,728	3.657	0.987
400k	1,728	3.703	0.993

Treatment	Distance to nearest protest					
	N	mean	sd	min	p50	max
Levels, kilometers	1,728	32.143	24.046	4.081	24.665	156.435
Logs	1,728	3.237	0.683	1.406	3.205	5.053

Notes: The table reports summary statistics for 432 congressional districts observed from 2012 to 2018 (Alaska and Hawaii are excluded from the analysis because of lack of population raster data). The number of obs with primary election records is smaller than the number of obs with general election records because the Miller and Camberg (2020) data do not contain returns for the states that have a top-two or top-four primary system. See section 3 for an explanation of the difference between the number of Republican and Democratic primary races. The number of obs for the votes obtained by women in general elections has some missing because I have been able to assign gender to 92% of candidates contained in the MIT (2020) using Social Security Name Files.

Table 2: Primary Elections

	Republican Primaries				Democratic Primaries			
	(1) Dummy for at least one female candidate	(2) Share of females	(3) Share of votes for females	(4) Dummy for female elected	(5) Dummy for at least one female candidate	(6) Share of females	(7) Share of votes for females	(8) Dummy for female elected
PANEL A:								
POST · log distance	-0.085**	-0.074**	-0.077**	-0.052	-0.038	-0.040	-0.044	-0.016
	(0.043)	(0.029)	(0.031)	(0.037)	(0.055)	(0.035)	(0.035)	(0.045)
	[0.041]	[0.028]	[0.030]	[0.035]	[0.051]	[0.034]	[0.034]	[0.045]
	{0.046}	{0.030}	{0.031}	{0.038}	{0.052}	{0.033}	{0.036}	{0.046}
Observations	1,325	1,325	1,325	1,325	1,341	1,341	1,341	1,341
R-squared	0.510	0.546	0.591	0.613	0.551	0.572	0.606	0.591
Adj R2	0.209	0.267	0.339	0.376	0.274	0.309	0.363	0.339
PANEL B:								
POST · log distance	-0.077*	-0.072**	-0.077**	-0.050	-0.053	-0.053	-0.055	-0.025
	(0.044)	(0.031)	(0.032)	(0.037)	(0.052)	(0.035)	(0.035)	(0.047)
	[0.042]	[0.030]	[0.031]	[0.036]	[0.048]	[0.034]	[0.034]	[0.046]
	{0.047}	{0.031}	{0.031}	{0.038}	{0.051}	{0.033}	{0.036}	{0.046}
Observations	1,310	1,310	1,310	1,310	1,324	1,324	1,324	1,324
R-squared	0.515	0.544	0.588	0.611	0.556	0.578	0.609	0.592
Adj R2	0.209	0.256	0.329	0.366	0.277	0.312	0.362	0.336
PANEL C:								
POST · log distance	-0.065*	-0.062**	-0.068***	-0.045	-0.016	-0.034	-0.027	0.013
	(0.038)	(0.026)	(0.026)	(0.031)	(0.046)	(0.033)	(0.034)	(0.042)
	[0.036]	[0.025]	[0.025]	[0.031]	[0.045]	[0.033]	[0.034]	[0.043]
	{0.041}	{0.027}	{0.027}	{0.033}	{0.044}	{0.032}	{0.033}	{0.040}
Observations	1,328	1,328	1,328	1,328	1,343	1,343	1,343	1,343
R-squared	0.486	0.522	0.567	0.585	0.523	0.542	0.580	0.562
Adj R2	0.262	0.312	0.378	0.403	0.314	0.341	0.395	0.370
Dep. var. mean	0.221	0.119	0.120	0.116	0.406	0.256	0.281	0.299

Notes: Panel A reports the results of the baseline specification, which includes district and state-election fixed effects only. Panel B reports the results using the same specification as Panel A, but adds controls (i.e. the share of votes obtained by the Democratic Party in the previous US House elections and population density). Panel C includes district and year fixed effects, but substitutes state-election fixed effects with state-specific linear time trends and includes the controls. Standard errors corrected for spatial correlation using a 255km threshold in (), using a 300km threshold in [], clustered at the district level in {}. Stars refer to standard errors corrected for spatial correlation using the optimal threshold (255km). *** p<0.01, ** p<0.05, * p<0.1

Table 3: Share of votes for females and primary openness

	Open Primaries			Non-Open Primaries		
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: Republican Primaries						
POST · log distance	-0.107**	-0.109**	-0.100**	-0.045	-0.043	-0.048
	(0.047)	(0.048)	(0.044)	(0.039)	(0.040)	(0.034)
	[0.047]	[0.047]	[0.044]	[0.038]	[0.039]	[0.034]
	{0.045}	{0.046}	{0.043}	{0.041}	{0.042}	{0.038}
CD fixed effects	Y	Y	Y	Y	Y	Y
YEAR fixed effects	Y	Y	Y	Y	Y	Y
State-election fixed effects	Y	Y		Y	Y	
State specific linear time trends			Y			Y
Controls		Y	Y		Y	Y
Observations	650	643	652	675	667	676
R-squared	0.596	0.600	0.587	0.587	0.580	0.552
Adj R2	0.373	0.372	0.407	0.306	0.284	0.347
Dep. var. mean	0.113	0.113	0.113	0.129	0.129	0.129
PANEL B: Democratic Primaries						
POST · log distance	-0.034	-0.038	-0.020	-0.053	-0.070*	-0.056
	(0.052)	(0.052)	(0.052)	(0.043)	(0.042)	(0.039)
	[0.050]	[0.051]	[0.052]	[0.043]	[0.039]	[0.040]
	{0.051}	{0.052}	{0.050}	{0.049}	{0.049}	{0.044}
CD fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
State-elections fixed effects	Y	Y		Y	Y	
State specific linear time trends			Y			Y
Controls		Y	Y		Y	Y
Observations	633	625	635	708	699	708
R-squared	0.603	0.604	0.576	0.605	0.611	0.587
Adj R2	0.375	0.371	0.386	0.346	0.347	0.402
Dep. var. mean	0.253	0.253	0.253	0.302	0.302	0.302

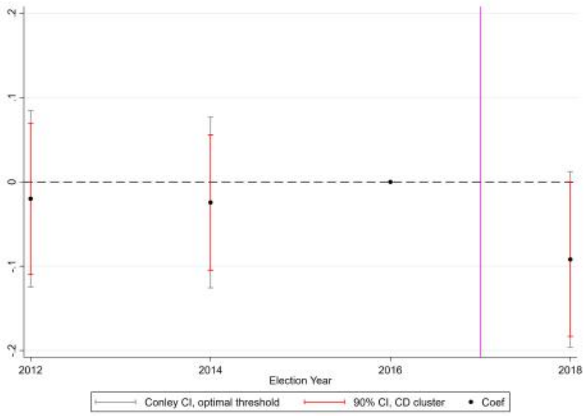
Notes: Panel A reports the results on Republican Primaries, Panel B on Democratic Primaries. Controls are the share of votes obtained by the Democratic Party in the previous US House elections and population density. Standard errors corrected for spatial correlation using a 255km threshold in (), using a 300km threshold in [], clustered at the district level in {}. Stars refer to standard errors corrected for spatial correlation using the optimal threshold (255km). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: General Elections

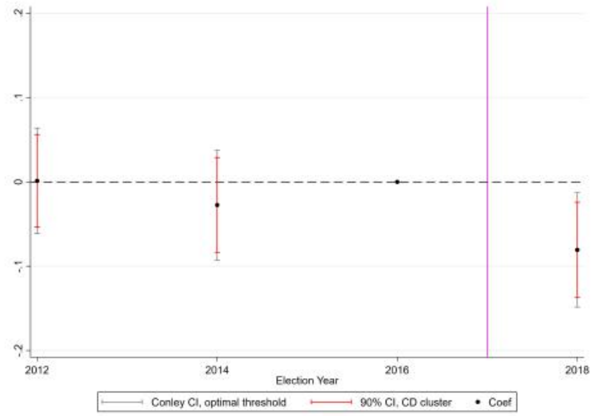
	Share of votes for females			Dummy for female elected			Turnout
	(1) Regardless of party	(2) Republican	(3) Democratic	(4) Regardless of party	(5) Republican	(6) Democratic	(7)
PANEL A:							
POST · log distance	-0.044	-0.006	-0.036*	-0.063**	-0.008	-0.055**	-5,359.770**
	(0.027)	(0.012)	(0.020)	(0.029)	(0.013)	(0.026)	(2,158.881)
	[0.027]	[0.012]	[0.021]	[0.028]	[0.013]	[0.025]	[2,119.016]
	{(0.026}	{0.012}	{0.022}	{0.026}	{0.012}	{0.025}	{2,309.032}
Observations	1,698	1,586	1,580	1,704	1,704	1,704	1,704
R-squared	0.709	0.702	0.763	0.835	0.807	0.846	0.938
Adj R2	0.552	0.524	0.621	0.747	0.703	0.763	0.904
PANEL B:							
POST · log distance	-0.049*	-0.008	-0.039*	-0.070**	-0.012	-0.058**	-5,656.675**
	(0.027)	(0.012)	(0.021)	(0.030)	(0.012)	(0.026)	(2,400.839)
	[0.028]	[0.012]	[0.021]	[0.029]	[0.013]	[0.025]	[2,399.971]
	{0.027}	{0.012}	{0.022}	{0.027}	{0.012}	{0.025}	{2,438.081}
Observations	1,679	1,568	1,564	1,685	1,685	1,685	1,685
R-squared	0.710	0.700	0.764	0.836	0.811	0.846	0.944
Adj R2	0.550	0.517	0.619	0.746	0.707	0.762	0.913
PANEL C:							
POST · log distance	-0.022	-0.006	-0.022	-0.050**	-0.008	-0.042**	-2,982.032
	(0.022)	(0.011)	(0.016)	(0.023)	(0.012)	(0.020)	(2,026.982)
	[0.022]	[0.011]	[0.017]	[0.023]	[0.012]	[0.019]	[2,139.546]
	{0.021}	{0.011}	{0.017}	{0.022}	{0.011}	{0.019}	{1,908.275}
Observations	1,703	1,593	1,587	1,709	1,709	1,709	1,709
R-squared	0.688	0.695	0.748	0.827	0.811	0.838	0.924
Adj R2	0.563	0.561	0.637	0.759	0.736	0.774	0.894
Dep. var. mean	0.247	0.046	0.157	0.195	0.041	0.154	252,853

Notes: Panel A reports the results of the baseline specification, which includes district and state-election fixed effects only. Panel B reports the results using the same specification as Panel A, but adds controls (i.e. the share of votes obtained by the Democratic Party in the previous US House elections and population density). Panel C includes district and year fixed effects, but substitutes state-election fixed effects with state-specific linear time trends and includes the controls. Standard errors corrected for spatial correlation using a 135km threshold in (), using a 200km threshold in [], clustered at the district level in {}. Stars refer to standard errors corrected for spatial correlation using the optimal threshold (135km). *** p<0.01, ** p<0.05, * p<0.1

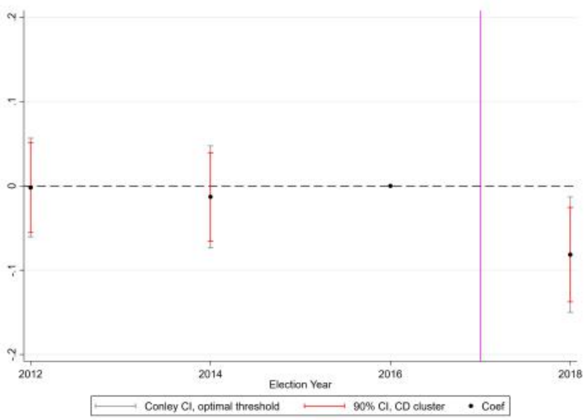
Figure 5: Strong parallel trends: Republican Primaries



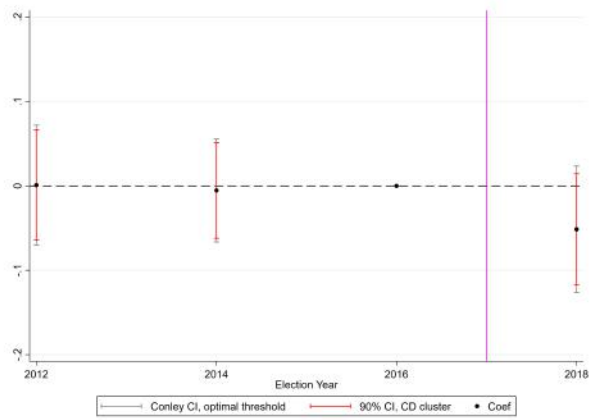
(a) Dummy for at least one female candidate



(b) Share of female candidates



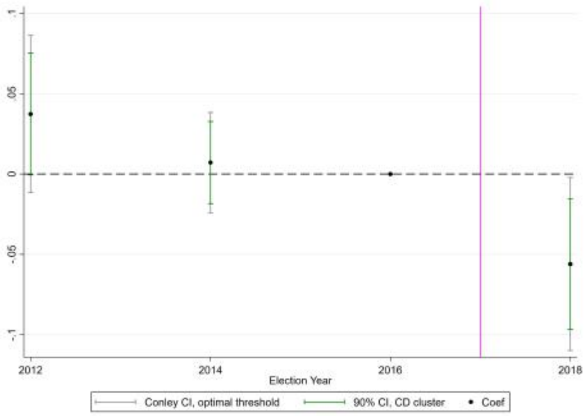
(c) Share of votes for females



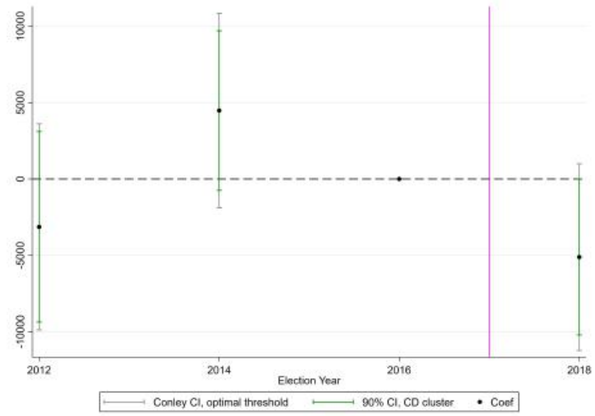
(d) Share of votes for females

Notes: plot of δ_τ, equation 4. The parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the Democratic Party in the previous House elections). Results without including controls are qualitatively and quantitatively similar and are available upon request. Strong parallel trends for Democratic Primaries are in Appendix D), Figure D1.

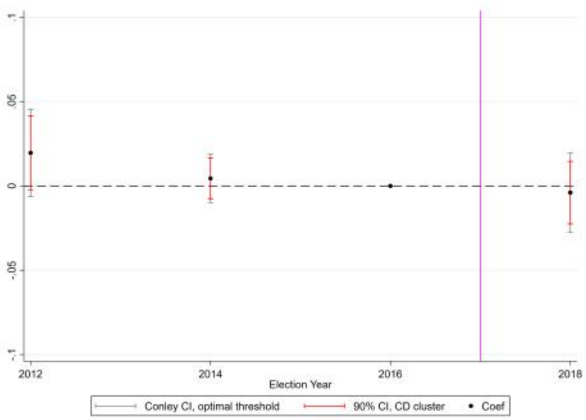
Figure 6: Strong parallel trends: General Elections



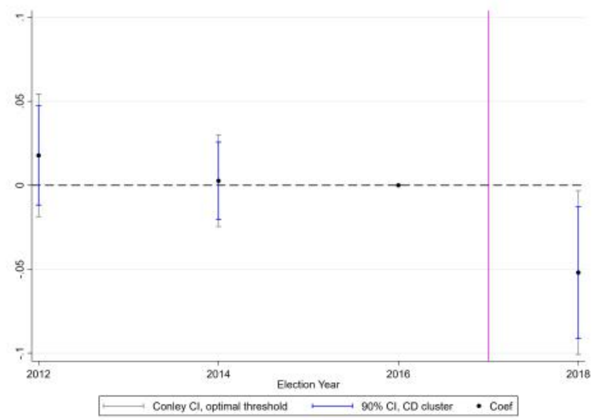
(a) Dummy for female elected, regardless of party



(b) Turnout, total ballots casted



(c) Dummy for Republican female elected



(d) Dummy for Democratic female elected

Notes: plot of δ_τ, equation 4. The parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the DP in the previous House elections). Results without including controls are qualitatively and quantitatively similar and are available upon request. Strong parallel trends for the share of votes for females in general elections are in Appendix D), Figure D2.

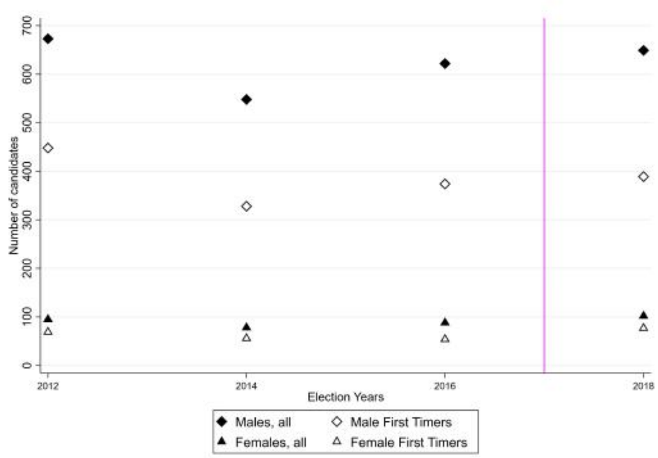
Table 5: District Shopping

PANEL A: Females	Between 2012 and 2014		Between 2014 and 2016		Between 2016 and 2018	
	To Not Close	To Close	To Not Close	To Close	To Not Close	To Close
Democratic Primaries	0	0	1	2	1	1
Republican Primaries	0	1	4	1	1	2

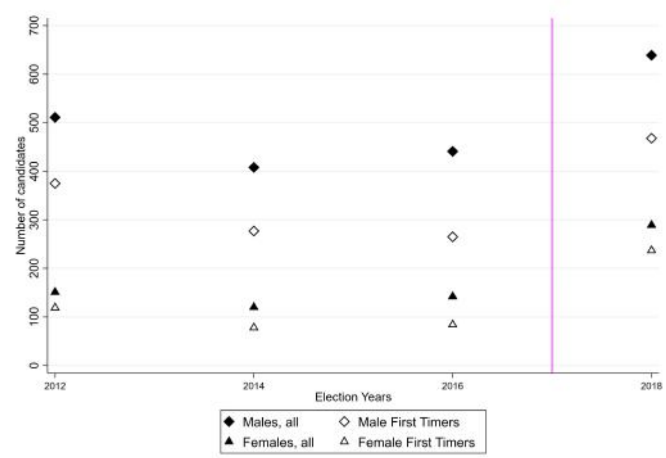
PANEL B: Males	Between 2012 and 2014		Between 2014 and 2016		Between 2016 and 2018	
	To Not Close	To Close	To Not Close	To Close	To Not Close	To Close
Democratic Primaries	4	2	3	4	2	8
Republican Primaries	4	1	4	4	7	8

Notes: Candidates switching congressional district in subsequent elections, broken down by destination districts (i.e. *Close* and *Not Close* to the 2017 Women's March) The table shows that district shopping is virtually absent for female candidates.

Figure 7: Number of candidates in partisan primaries



(a) Republican Primaries



(b) Democratic Primaries

Notes: The figures plot the number of candidates running in partisan primaries, with breakdown by gender and first timer status. The figures suggest that after the March there has been an increase in first-timer congressional candidates and provides additional evidence that district shopping is not driving the results.

Table 6: Civic engagement from survey data

	Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)
POST · log distance	-0.005	-0.004	-0.003	-0.009**	-0.008**	-0.010***
	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.004)
CD fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Controls		Y	Y		Y	Y
State-specific linear time trends			Y			Y
Observations	88,048	87,983	87,983	107,843	107,724	107,724
R-squared	0.013	0.046	0.047	0.010	0.044	0.044
Adj R2	0.008	0.041	0.041	0.006	0.040	0.040
Dep. var mean	0.162	0.162	0.162	0.105	0.105	0.105

Notes: The dependent variable is a dummy for whether the respondent attended any local political meeting during the past year. Controls are birth year, education, race dummies, employment dummies and marital status dummies. Results are qualitatively and quantitatively stable to substituting state-specific linear time trends with state-election fixed effects and are available upon request. The table reports suggestive evidence that the feminist uprisings had a "bite" on female civic engagement only. Stars refer to standard errors clustered at the district level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7: The role of news

Primary Elections	Republican Primaries				Democratic Primaries			
	(1) Dummy for at least one female running	(2) Share of females	(3) Share of votes for females	(4) Dummy for female elected	(5) Dummy for at least one female running	(6) Share of females	(7) Share of votes for females	(8) Dummy for female elected
POST · log distance	-0.0837**	-0.0717**	-0.0746**	-0.0489	-0.0398	-0.0443	-0.0483	-0.0196
	(0.0422)	(0.0290)	(0.0308)	(0.0363)	(0.0560)	(0.0360)	(0.0367)	(0.0474)
	[0.0408]	[0.0279]	[0.0295]	[0.0344]	[0.0521]	[0.0345]	[0.0350]	[0.0464]
	{0.0466}	{0.0304}	{0.0310}	{0.0379}	{0.0524}	{0.0338}	{0.0361}	{0.0463}
POST · log news salience	-0.00946	-0.0230	-0.0244	-0.0229	0.0138	0.0318	0.0361	0.0280
	(0.0361)	(0.0238)	(0.0248)	(0.0302)	(0.0411)	(0.0319)	(0.0325)	(0.0395)
	[0.0370]	[0.0242]	[0.0257]	[0.0312]	[0.0372]	[0.0278]	[0.0273]	[0.0355]
	{0.0379}	{0.0260}	{0.0275}	{0.0319}	{0.0412}	{0.0287}	{0.0306}	{0.0389}
CD fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
YEAR fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
State-election fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,325	1,325	1,325	1,325	1,341	1,341	1,341	1,341
R-squared	0.510	0.546	0.591	0.614	0.551	0.572	0.606	0.591
Adj R2	0.208	0.267	0.339	0.376	0.274	0.309	0.364	0.339
Dep. var. mean	0.224	0.120	0.121	0.117	0.406	0.254	0.279	0.298

Notes: Standard errors corrected for spatial correlation using a 255km threshold in (), using a 300km threshold in [], clustered at the district level in {}.

Table 8: Distance from urban clusters: intensive margin of the supply

	Share of female candidates in the Republican Primaries							
Distance to the nearest urban cluster with at least X inhabitants, with X ∈	(1) 50k	(2) 100k	(3) 150k	(4) 200k	(5) 250k	(6) 300k	(7) 350k	(8) 400k
POST · log distance	-0.0723**	-0.0613*	-0.0807**	-0.0884**	-0.0718**	-0.0535	-0.0534	-0.0479
	(0.0364)	(0.0356)	(0.0390)	(0.0386)	(0.0363)	(0.0362)	(0.0369)	(0.0368)
	[0.0326]	[0.0326]	[0.0386]	[0.0375]	[0.0361]	[0.0350]	[0.0356]	[0.0352]
	{0.0363}	{0.0368}	{0.0375}	{0.0373}	{0.0362}	{0.0359}	{0.0358}	{0.0359}
POST · log placebo distance	-0.00248	-0.0147	0.00622	0.0131	-0.00239	-0.0185	-0.0182	-0.0227
	(0.0294)	(0.0263)	(0.0240)	(0.0226)	(0.0228)	(0.0233)	(0.0227)	(0.0231)
	[0.0273]	[0.0248]	[0.0234]	[0.0211]	[0.0216]	[0.0219]	[0.0210]	[0.0210]
	{0.0316}	{0.0309}	{0.0268}	{0.0264}	{0.0260}	(0.0249}	{0.0243}	{0.0247}
CD fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
YEAR fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
State-election fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,325	1,325	1,325	1,325	1,325	1,325	1,325	1,325
R-squared	0.546	0.546	0.546	0.546	0.546	0.546	0.546	0.546
Adj R2	0.266	0.266	0.266	0.266	0.266	0.266	0.266	0.267
Dep. var. mean	0.120	0.120	0.120	0.120	0.120	0.120	0.120	0.120

Notes: Standard errors corrected for spatial correlation using a 255km threshold in (), using a 300km threshold in [], clustered at the district level in {}.

Table 9: Distance from urban clusters: extensive margin of the supply

	Dummy for at least one female running in the Republican Primaries							
Distance to the nearest urban	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
cluster with at least X inhabitants, with X ∈	50k	100k	150k	200k	250k	300k	350k	400k
POST · log distance	-0.0715	-0.0786	-0.0734	-0.0791	-0.0563	-0.0291	-0.0187	-0.0168
	(0.0592)	(0.0625)	(0.0613)	(0.0611)	(0.0567)	(0.0600)	(0.0571)	(0.0571)
	[0.0566]	[0.0598]	[0.0591]	[0.0588]	[0.0544]	[0.0577]	[0.0549]	[0.0547]
	{0.0607)	{0.0634)	{0.0621)	{0.0632)	{0.0614}	{0.0606}	{0.0589}	{0.0589}
POST · log placebo distance	-0.0154	-0.00692	-0.0113	-0.00535	-0.0261	-0.0492	-0.0571	-0.0581
	(0.0485)	(0.0506)	(0.0415)	(0.0398)	(0.0381)	(0.0390)	(0.0356)	(0.0358)
	[0.0487]	[0.0515]	[0.0424]	[0.0400]	[0.0382]	[0.0394]	[0.0362]	[0.0364]
	{0.0489}	{0.0515}	{0.0439}	{0.0432}	{0.0423}	{0.0403}	{0.0385}	{0.0380}
CD fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
YEAR fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
State-election fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,325	1,325	1,325	1,325	1,325	1,325	1,325	1,325
R-squared	0.510	0.510	0.510	0.510	0.510	0.511	0.511	0.511
Adj R2	0.208	0.208	0.208	0.208	0.208	0.209	0.210	0.210
Dep. var. mean	0.224	0.224	0.224	0.224	0.224	0.224	0.224	0.224

Notes: Standard errors corrected for spatial correlation using a 255km threshold in (), using a 300km threshold in [], clustered at the district level in {}.

Table 10: Binary DiD, Primary Elections

	Republican Primaries				Democratic Primaries			
	(1) Dummy for at least one female candidate	(2) Share of females	(3) Share of votes for females	(4) Dummy for female elected	(5) Dummy for at least one female candidate	(6) Share of females	(7) Share of votes for females	(8) Dummy for female elected
PANEL A:								
POST · close	0.087	0.066**	0.072**	0.050	0.042	0.044	0.042	-0.009
	(0.053)	(0.031)	(0.032)	(0.035)	(0.059)	(0.047)	(0.045)	(0.059)
	[0.048]	[0.029]	[0.029]	[0.032]	[0.056]	[0.047]	[0.046]	[0.060]
	{0.057}	{0.034}	{0.035}	{0.040}	{0.065}	{0.042}	{0.046}	{0.060}
Observations	1,325	1,325	1,325	1,325	1,341	1,341	1,341	1,341
R-squared	0.510	0.544	0.589	0.613	0.550	0.572	0.605	0.591
Adj R2	0.208	0.264	0.337	0.375	0.274	0.308	0.363	0.339
PANEL B:								
POST · close	0.087*	0.064**	0.072**	0.049	0.053	0.052	0.049	-0.000
	(0.053)	(0.032)	(0.033)	(0.036)	(0.059)	(0.048)	(0.047)	(0.061)
	[0.048]	[0.030]	[0.030]	[0.033]	[0.056]	[0.048]	[0.047]	[0.062]
	{0.056}	{0.035}	{0.036}	{0.041}	{0.065}	{0.043}	{0.046}	{0.060}
Observations	1,310	1,310	1,310	1,310	1,324	1,324	1,324	1,324
R-squared	0.515	0.542	0.587	0.610	0.556	0.577	0.608	0.592
Adj R2	0.209	0.254	0.327	0.365	0.277	0.311	0.361	0.335
PANEL C:								
POST · close	0.078	0.057*	0.067**	0.044	0.007	0.033	0.021	-0.037
	(0.048)	(0.029)	(0.029)	(0.031)	(0.054)	(0.045)	(0.043)	(0.057)
	[0.042]	[0.028]	[0.027]	[0.030]	[0.053]	[0.044]	[0.044]	[0.058]
	{0.051}	{0.032}	{0.033}	{0.037}	{0.057}	{0.040}	{0.042}	{0.054}
Observations	1,328	1,328	1,328	1,328	1,343	1,343	1,343	1,343
R-squared	0.486	0.520	0.566	0.585	0.523	0.542	0.580	0.563
Adj R2	0.262	0.310	0.376	0.403	0.313	0.341	0.395	0.370
Dep. var. mean	0.221	0.119	0.120	0.116	0.406	0.256	0.281	0.299

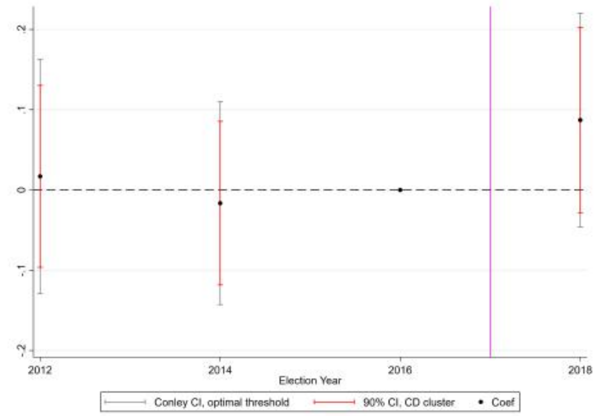
Notes: Districts are classified as *close* if they are below the media distance from the nearest protest. Panel A reports the results of the baseline specification, which includes district and state-election fixed effects only. Panel B reports the results using the same specification as Panel A, but adds controls (i.e. the share of votes obtained by the Democratic Party in the previous US House elections and population density). Panel C includes district and year fixed effects, but substitutes state-election fixed effects with state-specific linear time trends and includes the controls. Standard errors corrected for spatial correlation using a 255km threshold in (), using a 300km threshold in [], clustered at the district level in {}. Stars refer to standard errors corrected for spatial correlation using the optimal threshold (255km). *** p<0.01, ** p<0.05, * p<0.1

Table 11: Binary DiD: General Elections

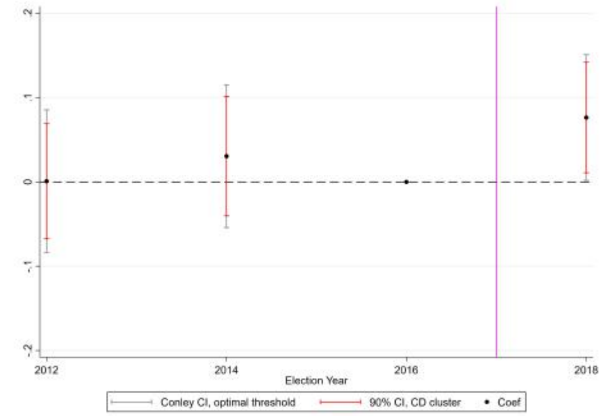
	Share of votes for females			Dummy for female elected			Turnout
	(1) Regardless of party	(2) Republican	(3) Democratic	(4) Regardless of party	(5) Republican	(6) Democratic	(7)
PANEL A:							
POST · close	0.037	0.003	0.042	0.085**	0.019	0.066*	3,022.515
	(0.031)	(0.014)	(0.027)	(0.038)	(0.016)	(0.036)	(2,704.230)
	[0.032]	[0.015]	[0.027]	[0.038]	[0.016]	[0.036]	[2,811.235]
	{0.032}	{0.015}	{0.027}	{0.034}	{0.016}	{0.034}	{3,203.090}
Observations	1,698	1,586	1,580	1,704	1,704	1,704	1,704
R-squared	0.708	0.701	0.763	0.835	0.807	0.846	0.937
Adj R2	0.551	0.524	0.621	0.747	0.703	0.763	0.904
PANEL B:							
POST · close	0.043	0.003	0.045*	0.094**	0.024	0.071*	3,522.141
	(0.031)	(0.014)	(0.027)	(0.039)	(0.015)	(0.036)	(3,060.607)
	[0.032]	[0.015]	[0.028]	[0.038]	[0.015]	[0.036]	[3,182.171]
	{0.033}	{0.015}	{0.027}	{0.034}	{0.015}	{0.034}	{3,304.081}
Observations	1,679	1,568	1,564	1,685	1,685	1,685	1,685
R-squared	0.709	0.700	0.763	0.836	0.811	0.846	0.944
Adj R2	0.548	0.517	0.619	0.747	0.707	0.762	0.913
PANEL C:							
POST · close	0.020	0.003	0.031	0.077**	0.018	0.059*	2,394.862
	(0.027)	(0.012)	(0.023)	(0.033)	(0.015)	(0.031)	(2,842.665)
	[0.028]	[0.013]	[0.024]	[0.032]	[0.014]	[0.030]	[2,998.991]
	{0.028}	{0.000}	{0.000}	{0.030}	{0.000}	{0.028}	{2,842.508}
Observations	1,703	1,593	1,587	1,709	1,709	1,709	1,709
R-squared	0.688	0.695	0.748	0.828	0.811	0.838	0.924
Adj R2	0.563	0.561	0.638	0.759	0.736	0.774	0.894
Dep. var. mean	0.247	0.046	0.157	0.195	0.041	0.154	252853

Notes: Districts are classified as *close* if they are below the median distance from the nearest protest. Panel A reports the results of the baseline specification, which includes district and state-election fixed effects only. Panel B reports the results using the same specification as Panel A, but adds controls (i.e. the share of votes obtained by the Democratic Party in the previous US House elections and population density). Panel C includes district and year fixed effects, but substitutes state-election fixed effects with state-specific linear time trends and includes the controls. Standard errors corrected for spatial correlation using a 135km threshold in (), using a 200km threshold in [], clustered at the district level in {}. Stars refer to standard errors corrected for spatial correlation using the optimal threshold (135km). *** p<0.01, ** p<0.05, * p<0.1

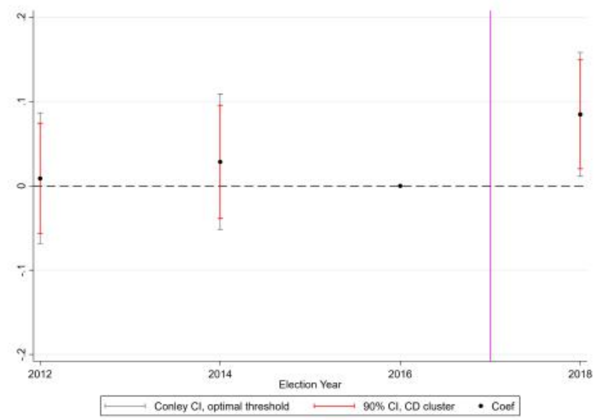
Figure 8: Unconditional parallel trends: Republican Primaries



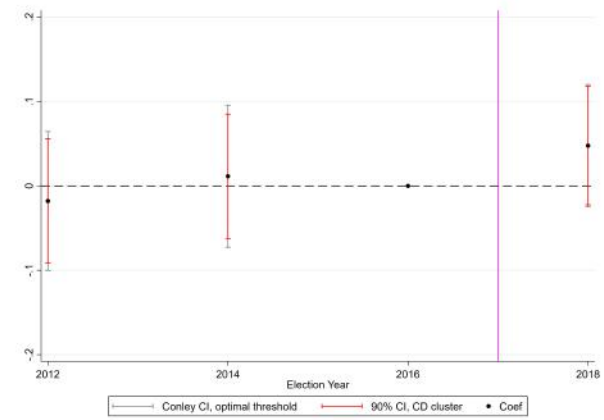
(a) Dummy for at least one female candidate



(b) Share of female candidates



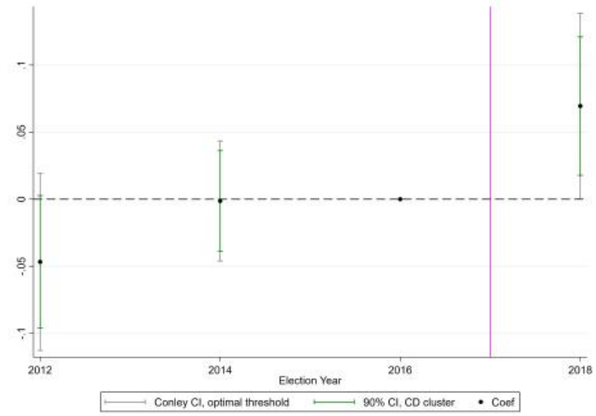
(c) Share of votes for females



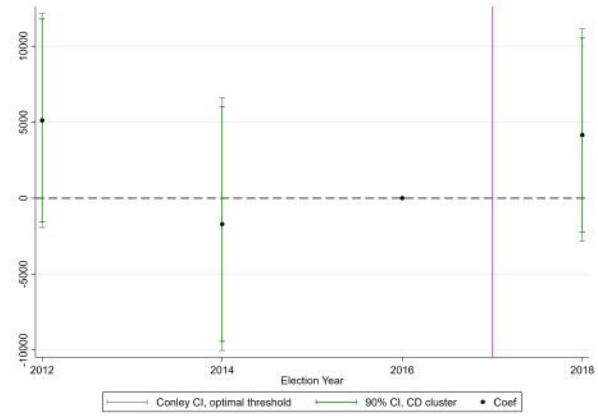
(d) Share of votes for females

Notes: plot of γ_{τ}, equation 10 (binary DiD). The parameters are estimated on the baseline binary specification to show that parallel trends hold unconditionally.

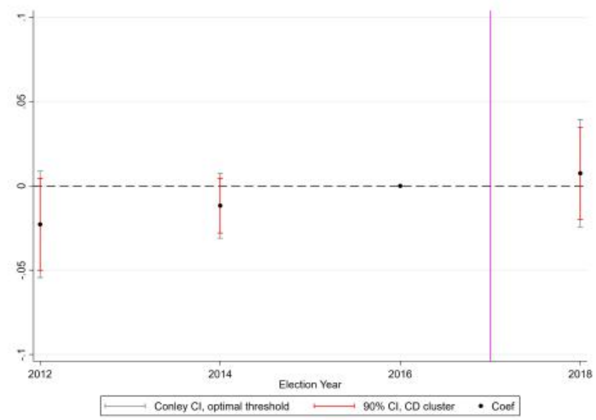
Figure 9: Unconditional parallel trends: General Elections



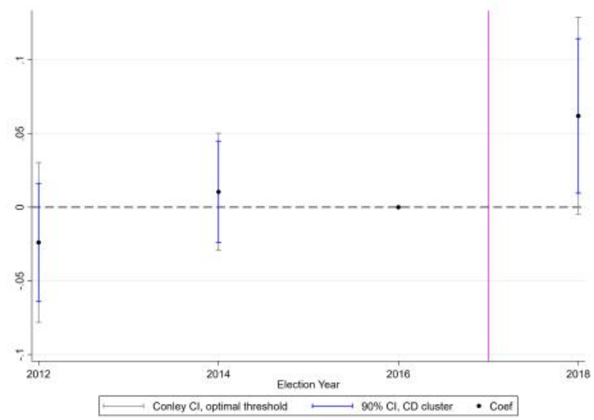
(a) Dummy for female elected, regardless of party



(b) Turnout, total ballots casted



(c) Dummy for Republican female elected



(d) Dummy for Democratic female elected

Notes: plot of γ_τ, equation 10 (binary DiD). The parameters are estimated on the baseline binary specification to show that parallel trends hold unconditionally.

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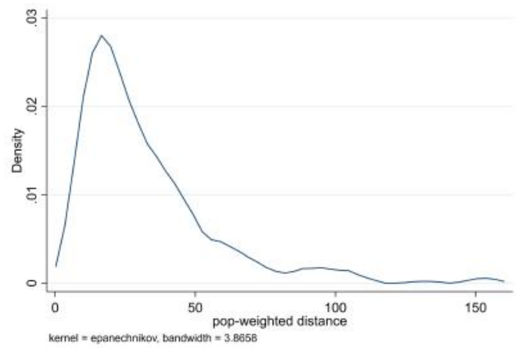
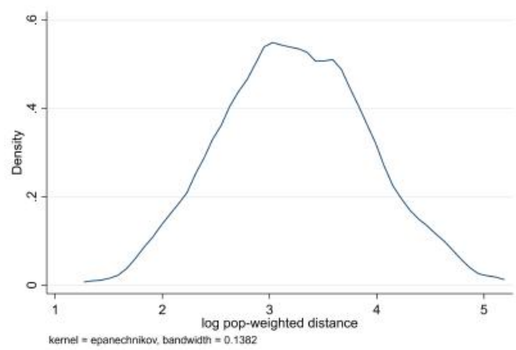
A Appendix I - Measurement error in protest size

A.1 Distribution of distance and size of the nearest protest

Figure A1: Distribution of the population weighted distance to the nearest protest



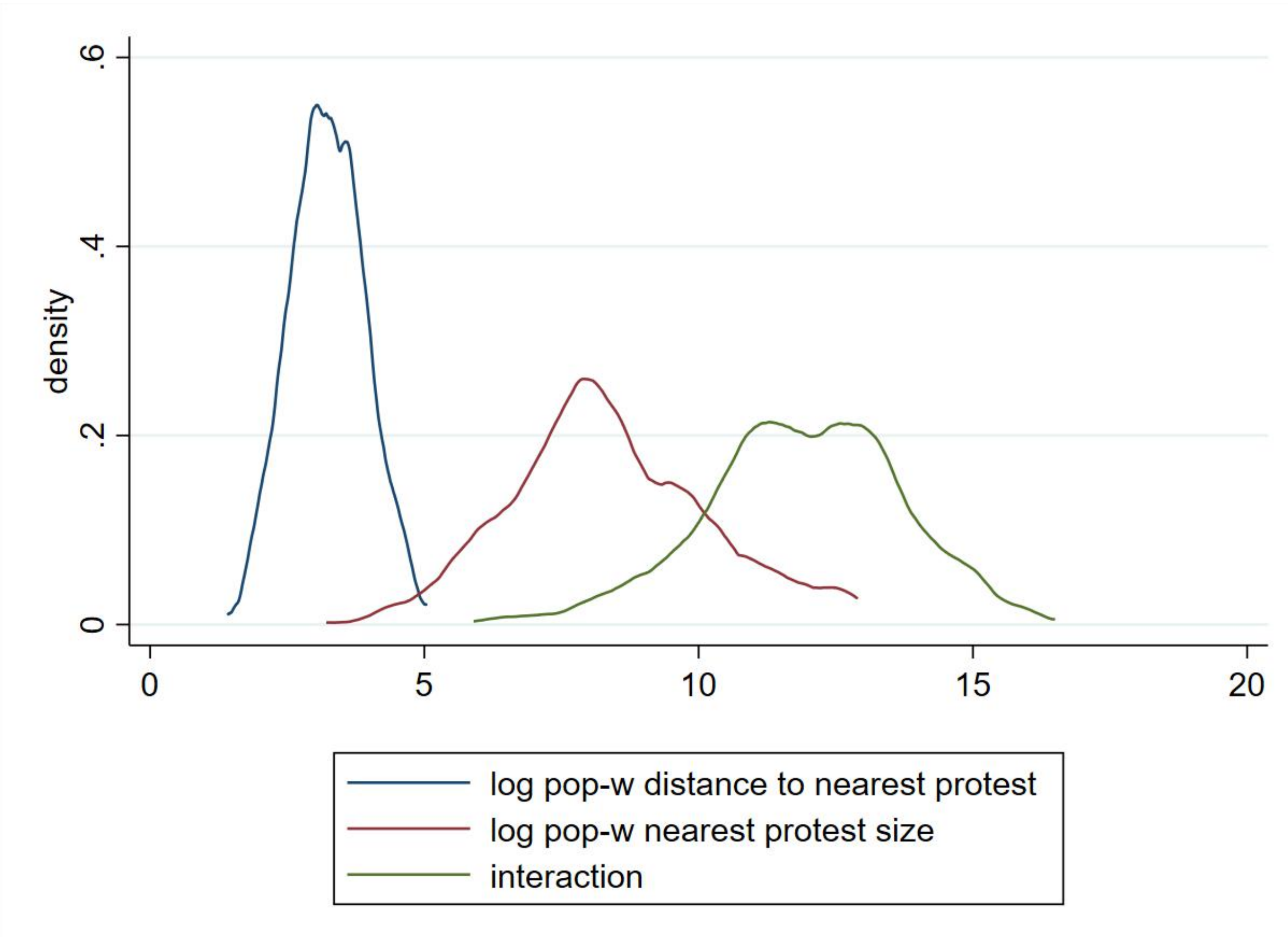
(a)



(b)

Panel (a) shows the pdf of the population weighted distance in level, panel (b) shows the pdf of its logarithmic transformation.

Figure A2: Distribution of treatment variables



Notes: Distribution of treatment variables: population weighted distance and population weighted protest size. The picture shows the collinearity between the log protest size and its interaction with the log distance.

A.2 Measurement error in protest size biases estimates towards zero

Table A1: Continuous Did
Dependent variable: dummy for at least one female candidate

	Republican primaries			Democratic primaries		
	(1)	(2)	(3)	(4)	(5)	(6)
POST· $size$	-0.0155	-0.0170	-0.0280**	-0.00443	-0.00342	-0.00682
	(0.0135)	(0.0133)	(0.0131)	(0.0182)	(0.0178)	(0.0163)
	[0.0133]	[0.0131]	[0.0137]	[0.0186]	[0.0182]	[0.0157]
	{0.0156}	{0.0156}	{0.0149}	{0.0187}	{0.0188}	{0.0168}
CD fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
State-election fixed effects	Y	Y		Y	Y	
State-specific linear time trends			Y			Y
Controls		Y	Y		Y	Y
Observations	1,325	1,310	1,328	1,341	1,324	1,343
R-squared	0.509	0.514	0.487	0.550	0.556	0.523
Adj R2	0.207	0.208	0.263	0.274	0.276	0.314
Dep. var. mean	0.222	0.222	0.222	0.352	0.352	0.352

Notes: size is log(pop-weighted size of the nearest protest). Standard errors corrected for spatial correlation using a 255km threshold in (), using a 300km threshold in [], clustered at the district level in {}. Control variables are population density and the share of votes obtained by the DP in the previous US House elections. Stars refer to standard errors corrected for spatial correlation using the optimal threshold (255km). *** p<0.01, ** p<0.05, * p<0.1

Table A2: Continuous Did
Dependent variable: share of female candidates

	Republican primaries			Democratic primaries		
	(1)	(2)	(3)	(4)	(5)	(6)
POST$\cdot$ $size$	-0.00970	-0.0113	-0.0152*	0.00340	0.00458	0.00264
	(0.00854)	(0.00829)	(0.00872)	(0.00936)	(0.00929)	(0.00846)
	[0.00776]	[0.00732]	[0.00864]	[0.00916]	[0.00899]	[0.00818]
	{0.0104}	{0.0104}	{0.0101}	{0.0110}	{0.0110}	{0.0100}
CD fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
State-election fixed effects	Y	Y		Y	Y	
State-specific linear time trends			Y			Y
Controls		Y	Y		Y	Y
Observations	1,325	1,310	1,328	1,341	1,324	1,343
R-squared	0.543	0.541	0.520	0.571	0.577	0.542
Adj R2	0.262	0.252	0.310	0.308	0.310	0.340
Dep. var. mean	0.122	0.122	0.122	0.239	0.239	0.239

Notes: size is log(pop-weighted size of the nearest protest). Standard errors corrected for spatial correlation using a 255km threshold in (), using a 300km threshold in [], clustered at the district level in {}. Control variables are population density and the share of votes obtained by the DP in the previous US House elections. Stars refer to standard errors corrected for spatial correlation using the optimal threshold (255km). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

B Appendix II - Bridging political geographies

To obtain a balanced panel of congressional districts for the years 2012-2018, I build on the work of Ferrara et al. (2021). Crosswalking political geographies is crucial to have a geographically stable unit of analysis. In particular, I follow the steps described hereafter.

1. Use the 113^{th} CD shapefile as a reference map (i.e. Congress elected in 2012)

2. Intersect the maps of all of the other CD years with the reference year. This will generate 3 intersect files ($2012-14$, $2012-16$ and $2012-18$).

3. Overlay the population distribution raster for 2010 on each intersect file to generate a population count for each polygon

4. Export the 3 intersect files

   For each intersect file:

5. Generate the total population of each CD, by summing the populations of all of the intersect polygons that are nested into each CD polygon.

6. Divide each intersect files polygon population by its CD population to generate weights for harmonizing the origin CD level data to the reference (i.e. 2012) CDs.

7. Multiply the relevant data values in each of the origin Congress years by the weights for each polygon.

8. Finally, collapse (sum) these within the 2012 Congress year's CDs (i.e. the 113^{th} Congress).

C Appendix III - Optimal correction radius for standard errors

In absence of previous knowledge about the optimal distance threshold that should be used to correct for spatial correlation, I follow Colella et al. (2019):

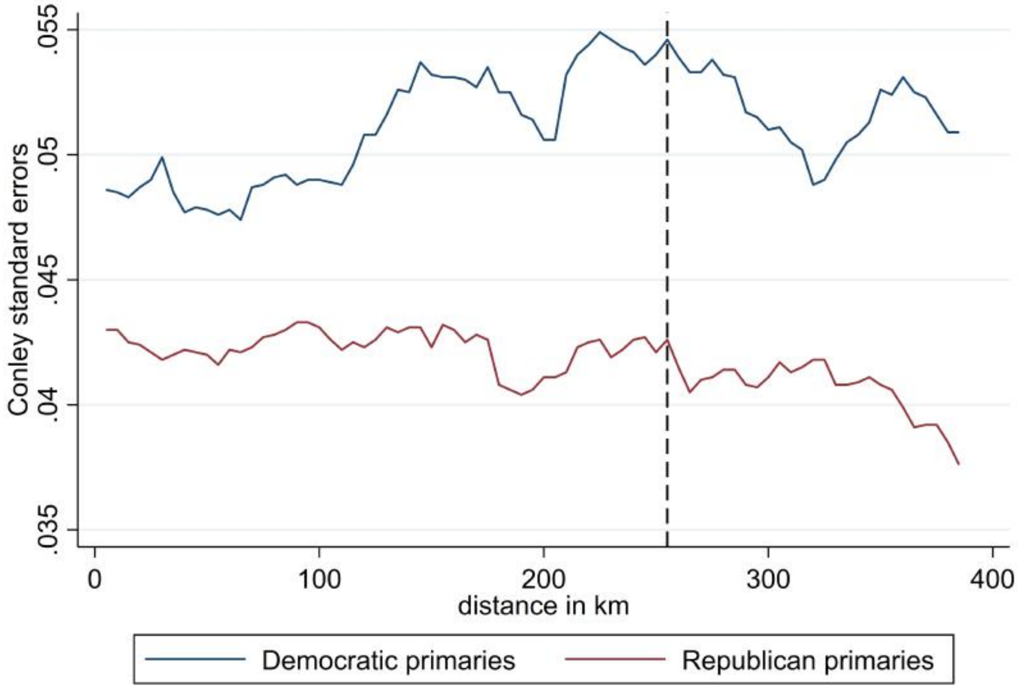
1. Estimate standard errors for a large set of potential optimal distance thresholds

2. Check for the presence of a non-linear patterns

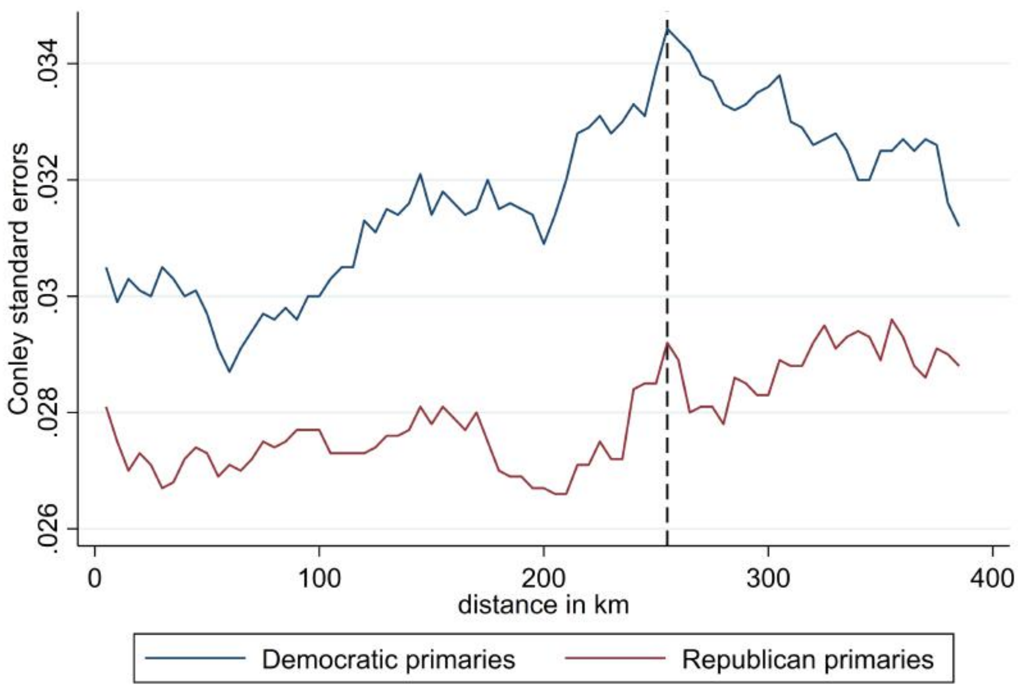
3. Retain the distance threshold that yields to the most conservative standard errors

The following Figures report standard errors as a function of distance. Moreover, each Figure highlights the distance which corresponds to the largest standard errors in each election cycle, meaning the distance which corresponds to the internal maximum of the function.

Figure C1: Optimal distance threshold for primary elections



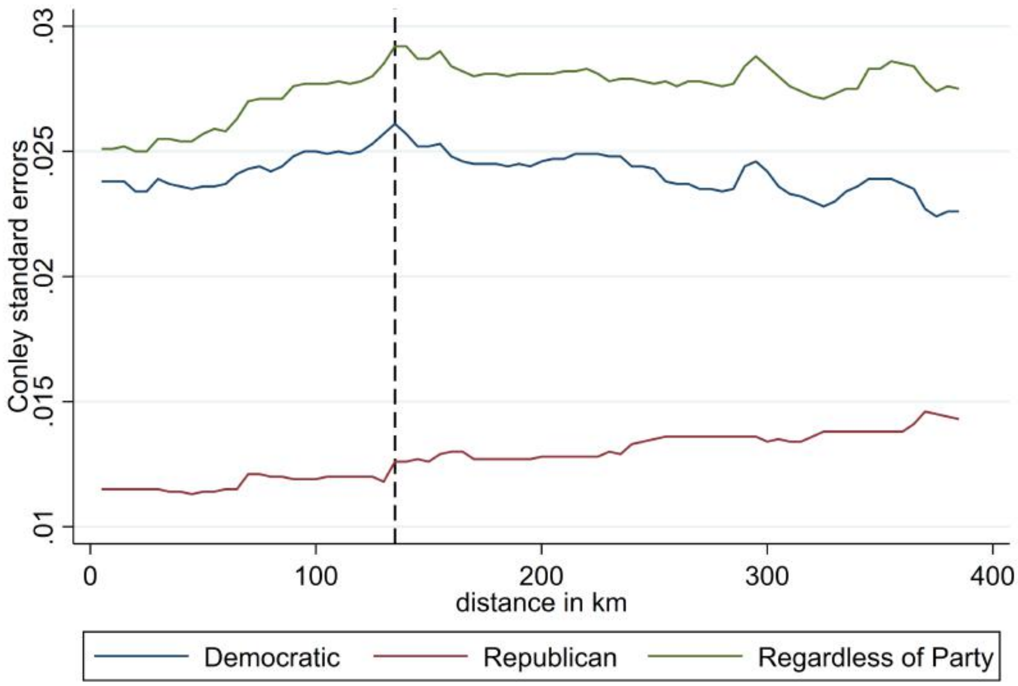
(a) Dummy for at least one female candidate



(b) Share of female candidates

Notes: Optimal correction radius for standard errors in primary elections. The results discussed in the paper are evaluated according to the most conservative standard errors: correcting for spatial correlation up to 255km.

Figure C2: Optimal distance threshold for general elections



(a) Dummy for female US House Representative

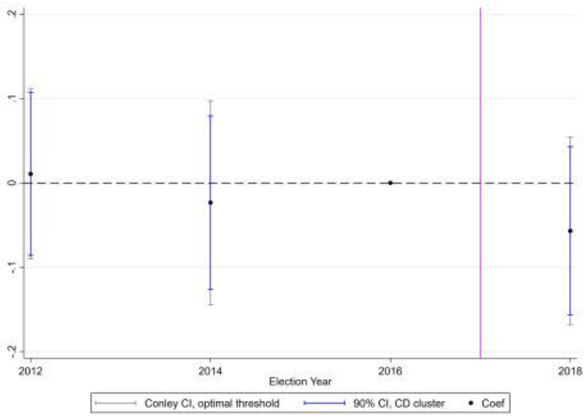
Notes: Optimal correction radius for standard errors in general elections. The results discussed in the paper are evaluated according to the most conservative standard errors: correcting for spatial correlation up to 135km.

D Appendix IV - Other results

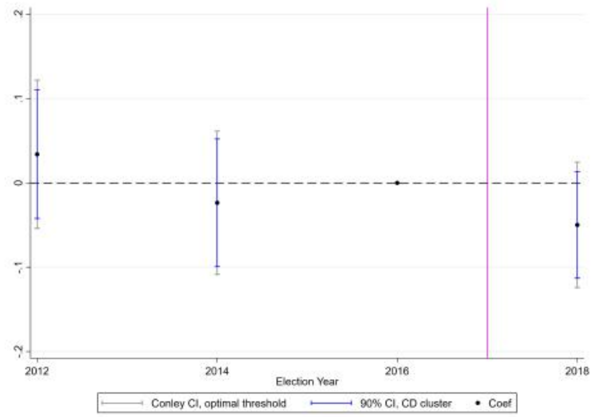
Table D1: Intersect file, an example:

CD fips	intersect id	pop	CD pop	distance	pop-weight	pop-weigh dist	CD pop-weigh dist
601	1	110k	711k	5	0.15	0.75	7.63
601	2	114k	711k	6	0.16	0.96	7.63
601	3	160k	711k	7	0.24	1.68	7.63
601	4	142k	711k	10	0.19	1.9	7.63
601	5	185k	711k	9	0.26	2.34	7.63
602	...						

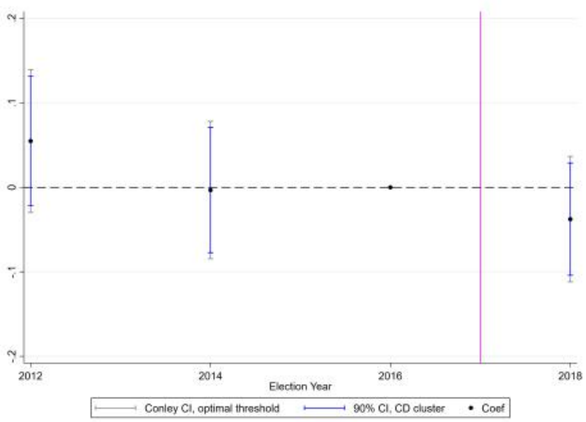
Figure D1: Strong parallel trends: Democratic Primaries



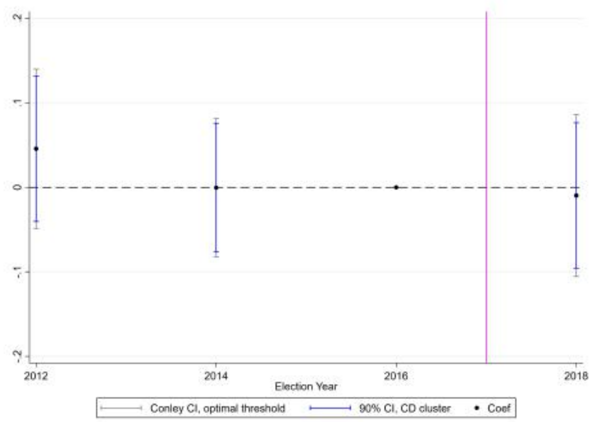
(a) Dummy for at least one female candidate



(b) Share of female candidates



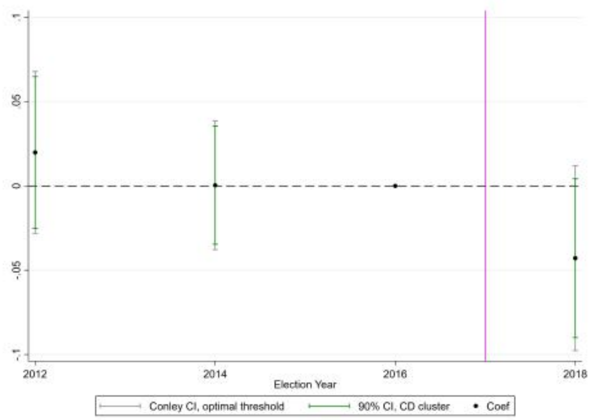
(c) Share of votes for females



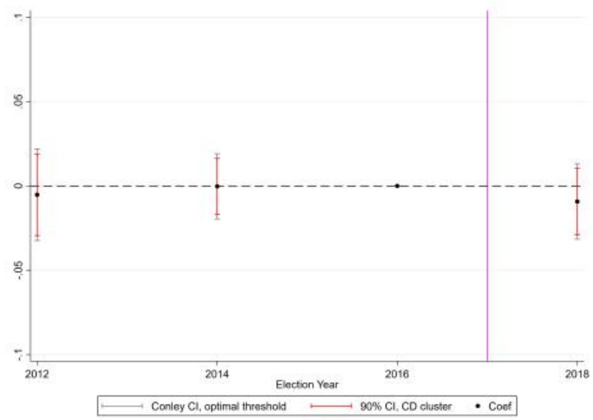
(d) Share of votes for females

Notes: lot of δ_τ, equation 4. The reported parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the DP in the previous House elections). Results without including controls are qualitatively and quantitatively similar and are available upon request.

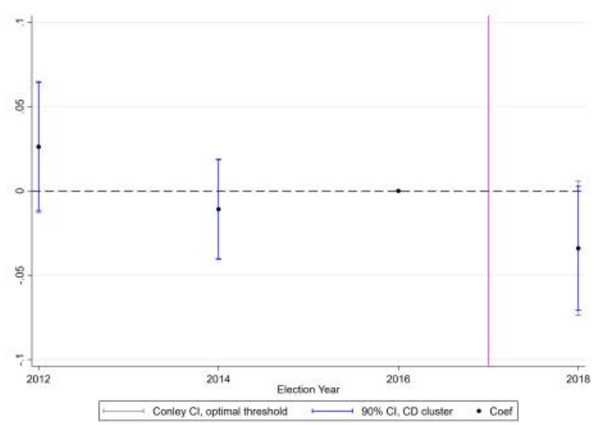
Figure D2: Strong parallel trends: General Elections



(a) Share of votes for females, regardless of party



(b) Share of votes for Republican females



(c) Dummy for Republican female elected

Notes: plot of δ_τ, equation 4. The reported parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the DP in the previous House elections). Results without including controls are qualitatively and quantitatively similar and are available upon request.

Figure D3: Distribution of the 2017 Women's March news salience

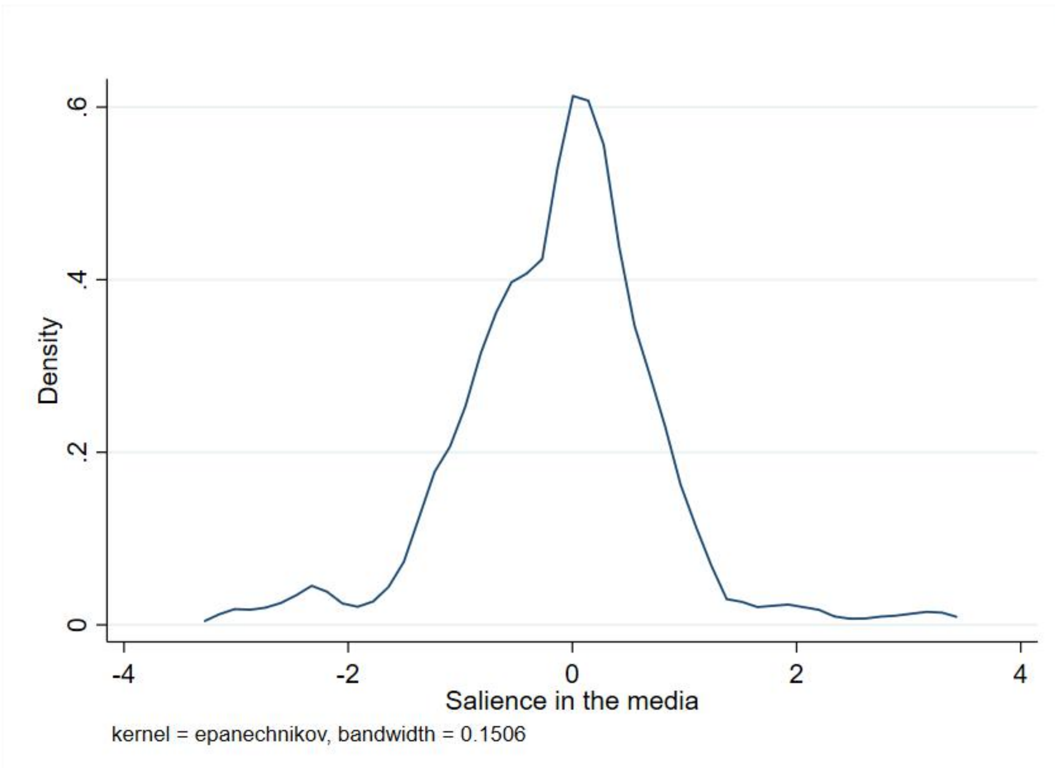
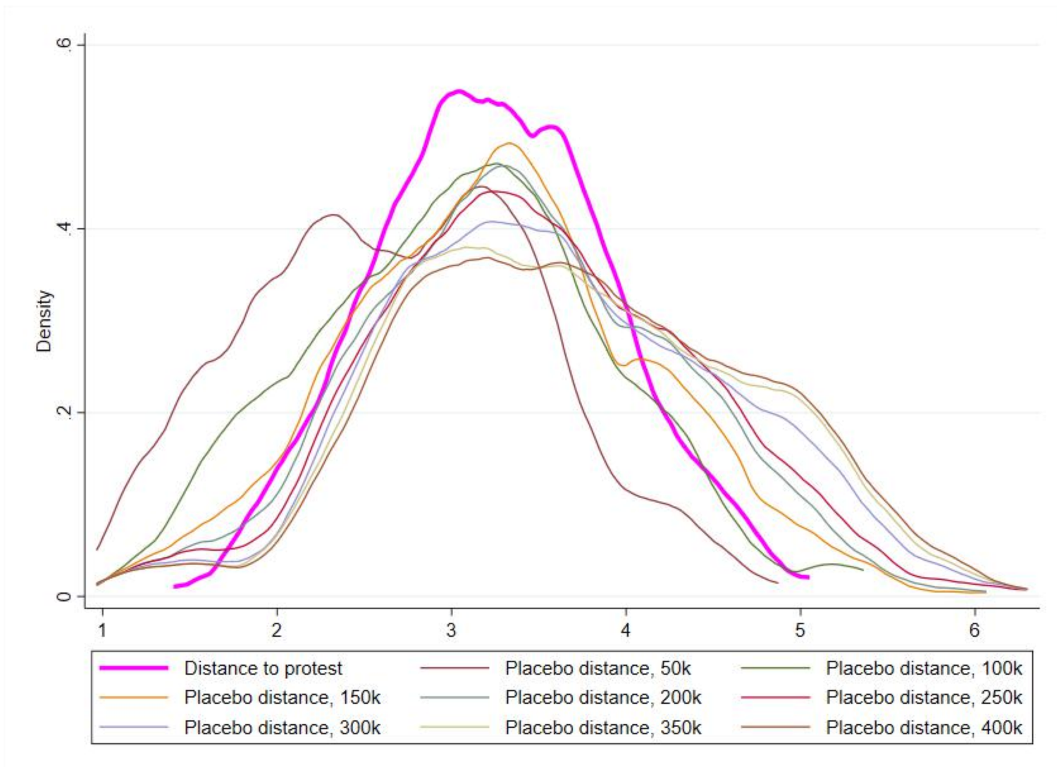


Figure D4: Distribution of distances from urban clusters



Notes: The figure shows the distribution of the population weighted distance between the district and the nearest urban cluster with at least X inhabitants, with X $\in$ {50k, 100k, 150k, 200k, 250k, 300k, 350k, 400k} (i.e. placebo distances). It allows to have an intuition about the extent to which collinearity between these distances and the distance to the nearest protest may be an issue when assessing the role of distance from urban clusters.

E Appendix V - Additional Robustness Checks

E.1 Binary difference in differences: a distribution-based approach

Table E1: Distribution of the population weighted distance above the median.

Centiles of distance	Kilometers
p50	24.41
p55	27.52
p60	30.48
p65	32.95
p70	36.21
p75	40.94
p80	44.44
p85	51.02
p90	61.37
p95	81.04
p100	147.74

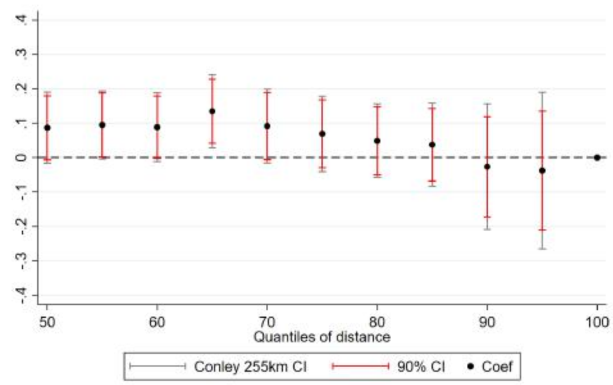
Notes: The table shows the distribution of the population weighted distance to the nearest protest as defined in section 5.1

Table E2: Cross tabulation of news count and of placebo news count

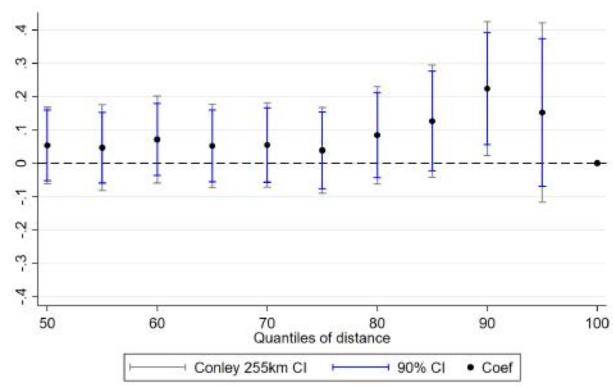
News dummy WM	News dummy BLM		Total
	0	1	
0	159	47	206
1	87	249	336
Total	246	296	542

Notes: This table contains the cross tabulation of the news count and of the placebo news count. Such a tabulation is useful to assess the number of articles mentioning together the WM, BLM and the Sister March location.

Figure E1: Robustness of γ (equation 9) across the distribution of the population weighted distance
Dependent variable: dummy for at least one female candidate



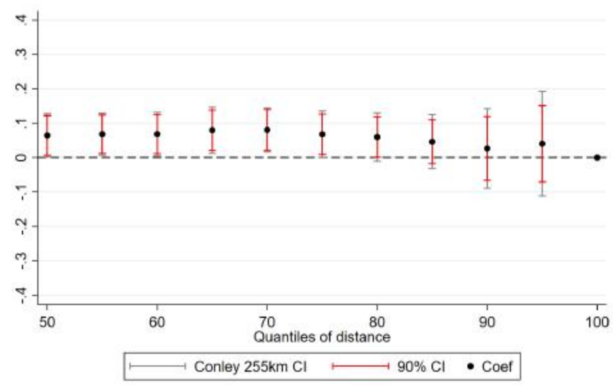
(a) Republican primaries



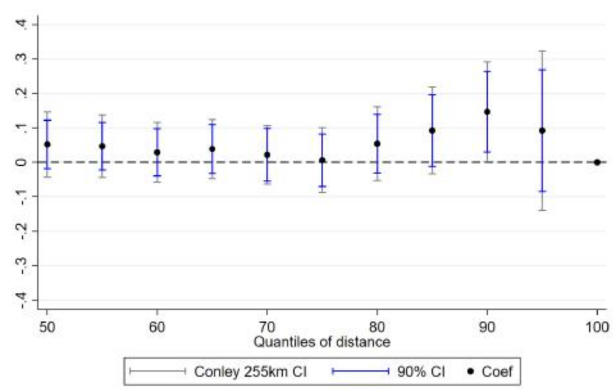
(b) Democratic primaries

Notes: districts are classified as *close* if they are associated to a distance below the corresponding distance quantile (reported on the x axis). The reported parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the DP in the previous House elections). The 90% confidence interval refers to standard errors clustered at the CD level.

Figure E2: Robustness of γ (equation 9) across the distribution of the population weighted distance
Dependent variable: share of female candidates



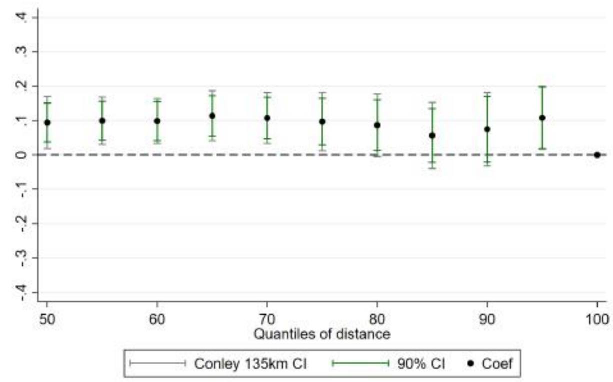
(a) Republican primaries



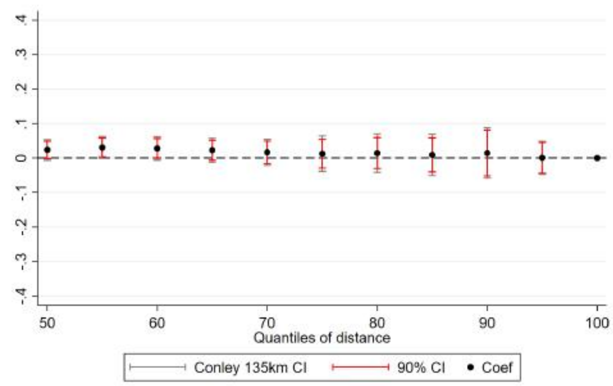
(b) Democratic primaries

Notes: districts are classified as *close* if they are associated to a distance below the corresponding distance quantile (reported on the x axis). The reported parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the DP in the previous House elections). The 90% confidence interval refers to standard errors clustered at the CD level.

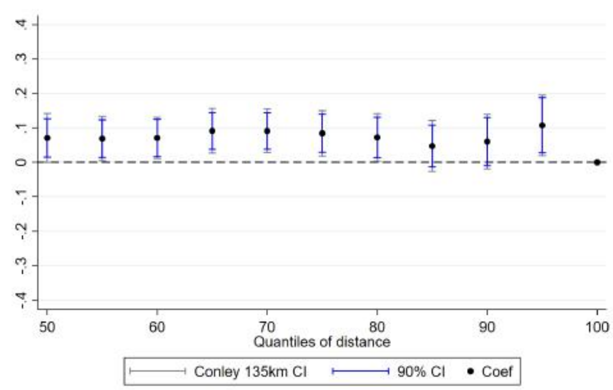
Figure E3: Robustness of γ (equation 9) across the distribution of the population weighted distance
Dependent variable: dummy for female US House representative



(a) Regardless of party affiliation



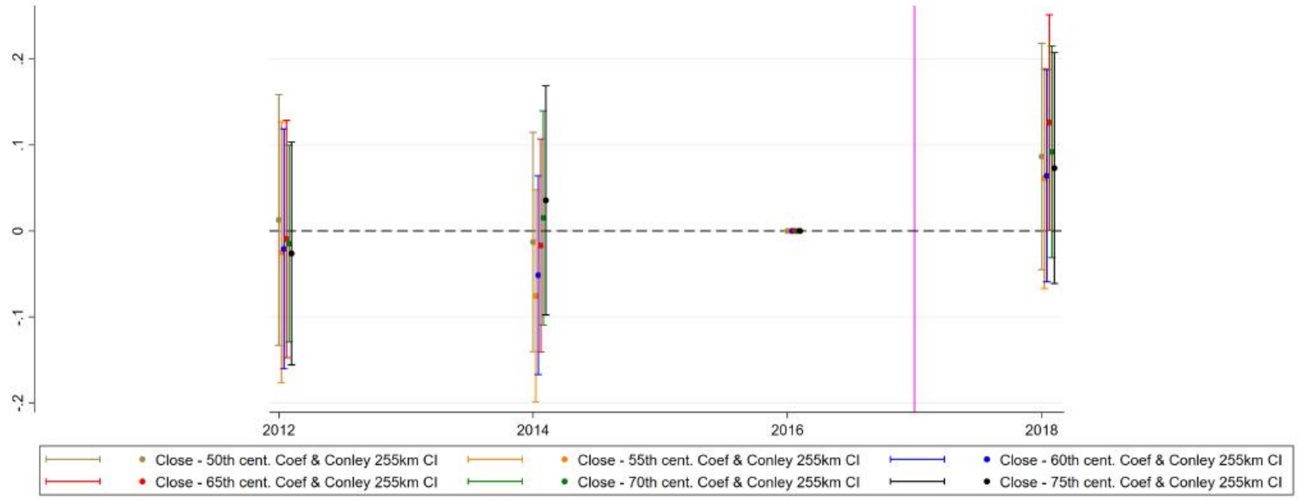
(b) Republican



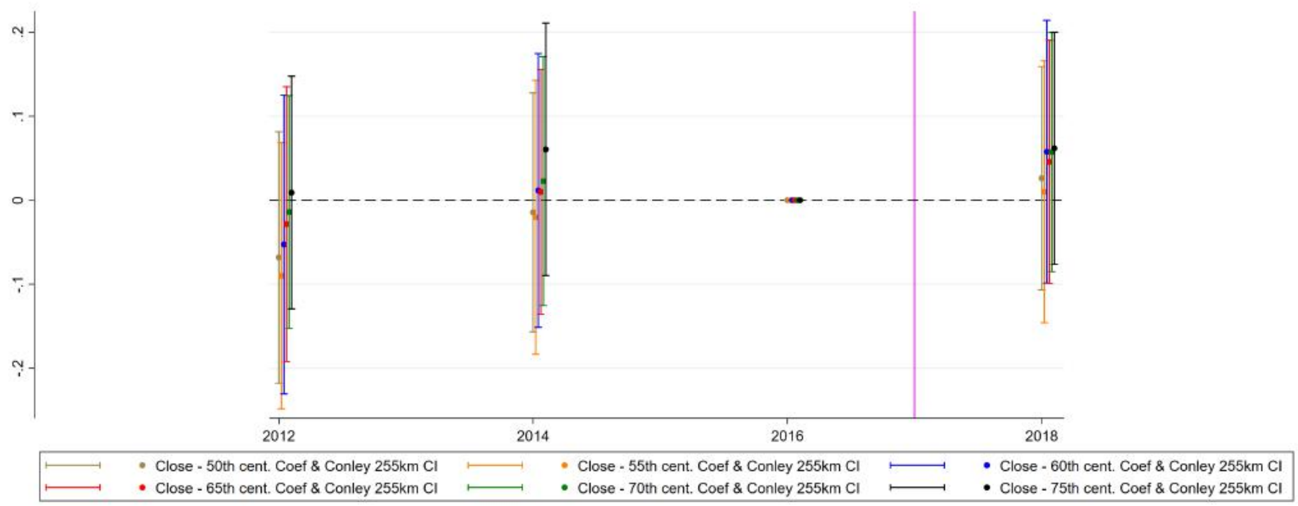
(c) Democratic

Notes: districts are classified as *close* if they are associated to a distance below the corresponding distance quantile (reported on the x axis). The reported parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the DP in the previous House elections). The 90% confidence interval refers to standard errors clustered at the CD level.

Figure E4: Robustness of γ_τ (equation 10) across the distribution of the population weighted distance
Dependent variable: dummy for at least one female candidate



(a) Republican primaries

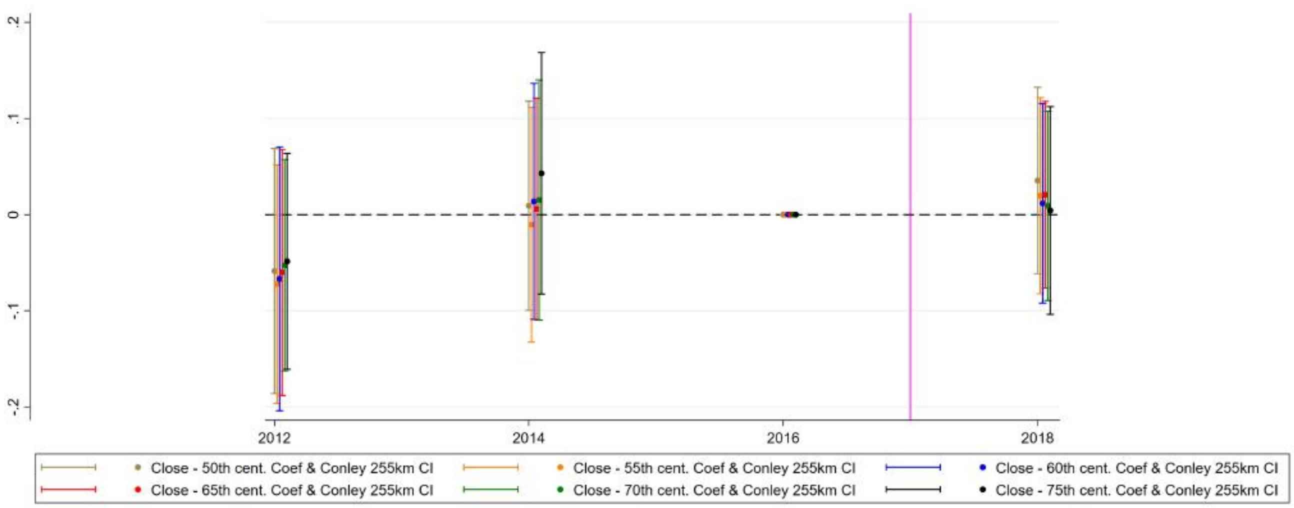


(b) Democratic primaries

Notes: districts are classified as *close* if they are associated to a distance below the corresponding distance quantile. The reported parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the DP in the previous House elections). Standard errors are corrected for spatial correlation using the optimal distance threshold. The figure provides evidence supporting parallel trends for six different binary treatment definitions.

Figure E5: Robustness of γ_τ (equation 10) across the distribution of the population weighted distance
Dependent variable: share of female candidates

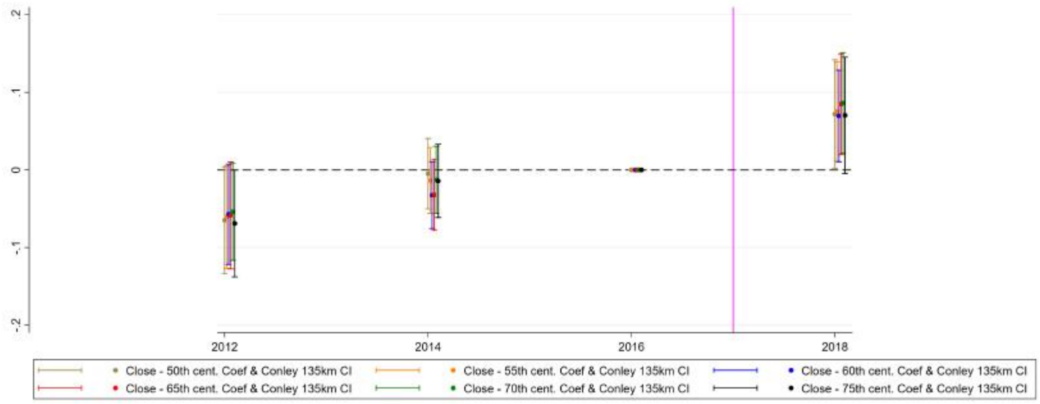
(a) Republican primaries



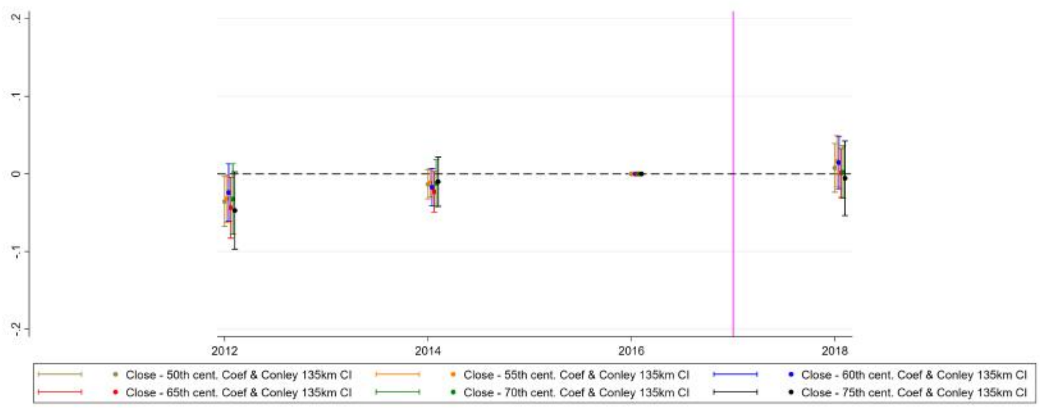
(b) Democratic primaries

Notes: districts are classified as *close* if they are associated to a distance below the corresponding distance quantile. The reported parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the DP in the previous House elections). Standard errors are corrected for spatial correlation using the optimal distance threshold. The figure provides evidence supporting parallel trends for six different binary treatment definitions.

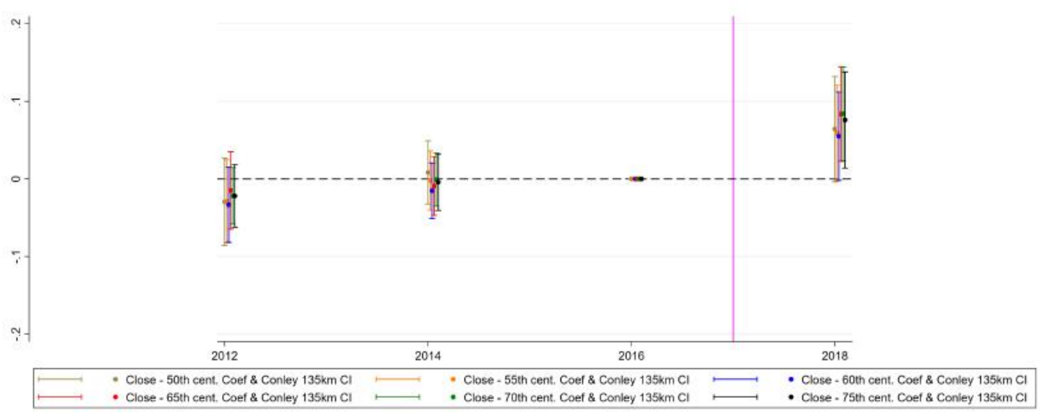
Figure E6: Robustness of γ_τ (equation 10) across the distribution of the population weighted distance
Dependent variable: dummy for female US House representative



(a) Regardless of party affiliation



(b) Republican



(c) Democrat

Notes: districts are classified as *close* if they are associated to a distance below the corresponding distance quantile. Standard errros are corrected for spatial correlation using the optimal threshold of 135km. The reported parameters are estimated on the full specification, which includes state-election fixed effects and controls (i.e. population density and share of votes for the DP in the previous House elections). Standard errors are corrected for spatial correlation using the optimal distance threshold. The figure provides evidence supporting parallel trends for six different binary treatment definitions.