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Emilia Brito Rebolledo
Jesse Bruhn
Thea How Choon
E. Anna Weber

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ABSTRACT

How does gender composition influence individual and group behavior? To study this question empirically, we assembled a new, national sample of United States city council elections and digitized information from the minutes of over 40,000 city-council meetings. We find that replacing a male councilor with a female councilor results in a 25p.p. increase in the share of motions proposed by women. This is despite causing only a 20p.p. increase in the council female share. The discrepancy is driven, in part, by behavioral changes similar to those documented in laboratory-based studies of gender composition. When a lone woman is joined by a female colleague, she participates more actively by proposing more motions. The apparent changes in behavior do not translate into clear differences in spending. The null finding on spending is not driven by strategic voting; however, preference alignment on local policy issues between men and women appears to play an important role. Taken together, our results both highlight the importance of nominal representation for cultivating substantive participation by women in high-stakes decision making bodies; and also provide evidence in support of the external validity of a large body of laboratory-based work on the consequences of group gender composition.

Emilia Brito Rebolledo
Department of Economics
Brown University
Providence, RI 02912
ebritore@brown.edu

Jesse Bruhn
Department of Economics
Brown University
Providence, RI 02912
and NBER
jesse_bruhn@brown.edu

Thea How Choon
St. Lawrence University
Hepburn 217
23 Romoda Drive
Canton, NY 13617
thowchoon@stlawu.edu

E. Anna Weber
U.S. Military Academy at West Point
Department of Social Sciences
West Point, NY 10996
anna.weber@westpoint.edu

Whether it is in politics, the labor force, or the boardroom, increasing the representation of women is a global concern (Chattopadhyay and Duflo, 2004; Ahern and Dittmar, 2012). This push for nominal representation is usually intended to ensure that the preferences and expertise of women are given a "seat at the table," so that they can wield substantive influence over the outcomes of high-stakes decisions. However, classic theories of "critical mass" suggest that small changes from a low baseline may not be enough to affect group decisions, especially in political bodies (Kanter, 1977; Dahlerup, 1988). Nominal representation will not necessarily translate into substantive participation if complicated group behavior inhibits female action when they are in the minority. This hypothesis also has some empirical support: experimental evidence finds that when women are in the minority of a group, and especially when they are a lone woman among a group of men, they participate less frequently (Coffman et al., 2021; Karpowitz et al., 2024).

Yet the finding that group gender composition causes consequential changes in substantive participation has never been replicated outside of a laboratory or "lab-in-the-field" type setting. This raises the possibility that this existing body of work, where the subjects – mostly students – perform tasks in laboratories or classrooms, may not translate to the types of high-stakes decision making processes in politics and the workforce that motivates this line of inquiry. Existing causal evidence from outside the lab typically concentrates on the final outcome of a decision making process (e.g. government spending), rather than on substantive participation *within* the decision making process itself.[1] This is because it is exceedingly rare to find data "in the wild" that contains the ingredients necessary for causal identification: random or quasi-random variation in the gender composition of a group paired with rich outcome data that captures the internal workings of a high-stakes decision making process.

We study how gender composition influences individual and group behavior using novel data on the internal workings of US city councils. We assembled the data by hand collecting and coding over 40,000 PDFs from hundreds of municipal government websites containing the minutes of specific city council meetings. We pair this with information on electoral outcomes assembled from both publicly available files in California and newly collected national data we built from over 2,500 FOIA requests. The resulting data contains over 500 distinct councils spanning nearly 200 cities across 34 states over the course of eight years. Our data include numerous measures of the behavior of indi-

[1] Early examples of studies that focus on outcomes of the decision making process include Gagliarducci and Paserman (2012) and Ahern and Dittmar (2012). For a more in-depth discussion of the relevant literature, see section 1.

vidual city-councilors and the outcomes of city council decision making. By leveraging close elections between a man and a woman, we isolate exogenous variation in the gender composition of the councils and estimate causal effects on group behavior, individual behavior, and the workings of local government.

We find that the nominal representation of women on city councils does translate into their substantive participation. On average, replacing a male city councilor with a female city councilor causes the share of motions offered by women to increase by 25 percentage points. These effects are largest at low levels of baseline female representation, particularly for councils that would otherwise contain a single, "token" woman. Yet, replacing a man with a woman only causes the female share of the council to increase by 20p.p. The overall effects on substantive participation are therefore larger than what would be expected if the additional women simply made their "fair share" of motions. What drives this discrepancy?

At the individual level, we find that adding an additional woman to the council changes the behavior of other female councilors. When a second woman is added to a council that would otherwise contain a single token woman, the share of motions offered by the latter increases by 14.8p.p. This increase in the share of motions comes from an additional 0.865 motions offered by the existing female councilor per meeting. This is consistent with laboratory evidence that women's behavior, and specifically the behavior of isolated women, responds to the gender composition of a group (Coffman, 2014; Bordalo et al., 2019). The impact of an additional woman on the behavior of lone women is also a particularly relevant margin in our setting, since the modal council in our sample has exactly one female councilor. Among male councilors, we find a comparably sized *drop* in participation (-0.717 motions per meeting); however, it is imprecisely estimated. Consistent with critical mass theory, we do not find evidence that adding an additional woman to a council that already has multiple women engenders further changes in their behavior.

Despite these changes in substantive participation, we find little evidence that the addition of a female councilor translates into consequential changes in spending. For example, we find null effects for the impact that an additional female councilor has on the level or composition of public expenditure. These patterns hold for both the average council *and* the subset of councils that would otherwise contain a lone, token female councilor.

Why does the apparent increase in women's substantive participation fail to translate into changes in spending? We find little evidence that dynamics related to political parties, strategic voting or other forms of coalitional behavior are responsible. For example, in a smaller sub-sample of councils where we are able to link councilors to their political affiliations, we do not find statistically precise evidence that the election of a female councilor causes large changes to the ideological composition of the council. However, due to the size of this sub-sample, the confidence interval is large. Despite this limitation, the point estimates suggest that the impact of a female win on the composition of political parties on the council is less than half the size of the corresponding impact on gender composition.

To further probe the potential for political parties to generate these patterns, we also explore the impact of a female victory on voting patterns. If the null result on spending were driven by a gender-based backlash against women's preferred policies, or an increase in party-line voting brought about by a corresponding change in political composition, we would expect to see consequential changes in voting patterns as the motions brought by women fail to pass. We find little evidence that this story is true. We find precise null effects on the share of motions passed unanimously, the share of motions rejected, and the average vote margin. These patterns are consistent with a limited role for political parties, ideological coalitions, or other forms of strategic voting behavior as the primary mechanism behind our results.

We do find suggestive evidence that close alignment of policy preferences between men and women plays an important role. To probe the role of preferences, we conduct two supplementary analyses that rely on the topical content of motions offered as a proxy for male/female tastes. First, we show descriptively that there is little difference in the topical content of the motions offered by the average male and the average female councilor in our sample. Second, we explore the causal effect of an additional female councilor on the types of motions offered. We find no evidence that the gender composition of the group changes the topics of discussion.

We argue that these empirical facts can be rationalized by a stylized model of critical mass. The model features two key parameters: psychic costs to participation that depend on group composition, which parsimoniously formalizes the insights from Kanter (1977), and a previously overlooked dimension – preference heterogeneity between men and women. The model predicts that substantive female participation can vary discontinuously with group gender composition in response to gains in nominal representation, especially at low levels of baseline representation. However, gains in substantive participation only translate into changes in voting patterns and policy outcomes when the average preferences of men and women are sufficiently different. This is because, when men and women's policy preferences agree, changes in gender composition have no effect on the topics of motions or the frequency of non-unanimous votes. To our knowledge, this is the first formal mathematical model of critical mass, and it suggests that the preferences of male and female policy makers is likely a key source of heterogeneity that could explain the diverse, and sometimes contradictory, findings in the literature regarding the policy impact of increases in women's nominal representation.[2]

1 Related Literature

Our paper relates to a rich literature in behavioral and experimental economics by being the first to estimate the causal effect of gender composition on women's substantive participation outside of a laboratory or lab-in-the-field type setting. For example, Chen and Houser (2019) and Born et al. (2022) find that women are less willing to lead mixed-sexed groups than all-female groups in a pure laboratory setting. Clayton et al. (2024) conduct a related field experiment in Malawi and find that each woman wields more influence over climate policy when women make up a larger share of group members. Karpowitz et al. (2024) conduct two, multi-year field experiments among randomly-assigned groups of college students. The authors find that when a group has only one woman, she is less likely to be chosen as spokesperson or rated as influential by her peers, as compared to women in groups with multiple women. In related work, Coffman (2014) shows that in mixed-sex groups, women are less likely than men to contribute to a group when the topic of discussion is traditionally male-associated; this harms overall group performance. Bordalo et al. (2019) find that when working in pairs to answer trivia questions, women have less confidence in their relative ability in male-stereotyped domains when their partner is revealed to be male, again leading to worse group performance.[3]

Our ability to study the effect of an additional woman on the behavior of other women on the council speaks to a broad interdisciplinary literature proposing the need for a critical mass of women (usually around 15-30%) to be represented politically in order to effect a change in policy outcomes (Kanter, 1977; Dahlerup, 1988).[4] This has sparked numerous

[2]For examples, see our discussion of Ferreira and Gyourko (2014), Chattopadhyay and Duflo (2004), Gagliarducci and Paserman (2012), and Baskaran and Hessami (2023) in the related literature section.

[3]In related work, Coffman et al. (2021) also finds that within mixed-sexed groups, women rank themselves less favorably when they are in the minority.

[4]There is also some empirical support for the notion of a critical mass of women from the study of groups outside of politics. For example, among publicly traded companies in Germany, Joecks et al. (2013) find a U-shaped relationship between firm performance and women's representation on the board. When a token

descriptive empirical studies, with sometimes mixed results pointing to complex dynamics (e.g. Childs and Krook, 2006). For example, within a sample of school board meeting minutes, Karpowitz and Mendelberg (2014) find descriptive evidence of lower participation rates for lone women compared to lone men.

There is also a causal literature on the impact of women's representation in politics; however, it is focused primarily on outcomes rather than group dynamics within the decision making body.[5] For example, Ferreira and Gyourko (2014) use a similar regression discontinuity design as we implement here and find little impact of the gender of U.S. mayors on the size of the municipal government or spending composition. In contrast, Baskaran and Hessami (2023) find that, in Bavaria, increased representation of women leads to increased spending on childcare, likely as a result of changing the topic of discussion, with important non-linearities according to the level of baseline representation. Gagliarducci and Paserman (2012) find that Italian municipalities headed by female mayors are more likely to be dissolved early and that this problem is more severe when the entire rest of the council is male.[6] Increased female representation has also been shown to affect the persistence of female political candidates, the selection of male candidates, and the provision of women's healthcare (Wasserman, 2023; Besley et al., 2017; Anukriti et al., 2022). To date, the existing empirical literature providing causal estimates on women's representation in politics has focused on the outcome of the decision making process rather intra-council participation dynamics. Our paper is the first to explore the behavioral changes that could underpin these findings, contributing to a more nuanced understanding of critical mass and women's substantive participation.[7]

We contribute to the theoretical literature on group behavior by being the first to formalize the classic model of critical mass (Kanter, 1977). Our theoretical model incorporates participation costs that depend on group composition. It predicts that, below a certain threshold of nominal representation, minorities may withhold substantive partic-

woman is present, performance is lower, but performance returns to the level of all-male boards when the share of women rises above 30%. In related work, Truffa and Wong (2022) find that the transition from all male universities to a coeducation model increased the prevalence of gender related research topics, in part by shifting the agenda of male incumbent researchers.

[5]There is also a related literature that estimates the causal effect of group gender composition outside of politics (e.g. Bagues et al., 2017; Ahern and Dittmar, 2012); however, as with the literature on political representation, this body of work has also largely focused on the outcomes of the decision making processes, rather than behavior/participation within the decision making body itself.

[6]In related work, Chattopadhyay and Duflo (2004) find that in India, the gender of local leaders does matter, with villages led by women spending more on issues which tend to be important for women.

[7]There is also a related strand of literature that looks at the impact of political representation among minorities. See Beach and Jones (2017) and Beach and Walsh (2023).

ipation in the policy-making process, in contrast to previous work that emphasizes strategic abstention by uninformed (Feddersen and Pesendorfer, 1996) or moderate (Osborne et al., 2000) agents. Furthermore, our theoretical framework conceptually separates the effects of gender through identity from effects arising from policy preference. In particular, we allow participation costs based on group identity to operate irrespective of the degree of preference alignment across genders, consistent with the experimental evidence cited above. A growing theoretical literature on committee composition has focused on the optimal selection of committee members when it affects informational incentives (e.g. Bardhi and Bobkova, 2023). In a related working paper, Hughes et al. (2024) develop a model of committee decision making among groups with diverse signals and preferences; however, their model focuses on truth-telling rather than participation. They predict that, when groups differ in their informational structures but not in their preferences, welfare is increasing in diversity. Our model adds to this literature by studying the link between nominal representation and substantive participation by minorities, as well as the effects on motions made and votes cast. There is also an important, related theoretical literature that examines the origins of women's under-representation in politics (e.g. Ashworth et al., 2024). Our model takes women's under-representation as given, and explores its impact on individual and group decision making.

2 Background and Data

2.1 City Councils in the United States

Collectively, local government in the US is responsible for managing over $1.9 trillion in revenues (Urban Institute, Accessed 2023). City councils are not only responsible for determining how the majority of these funds are spent; they are also responsible for other important regulatory functions related to zoning, local ordinances, and the administration of local programs (National League of Cities, Accessed 2023). Thus, studying how city councils make decisions is, itself, an important question.

However, the political context for city council members is frequently quite different than it is for other elected officials in the United States. For example, the typical city council is small, with an average council containing only 6 councilors (National League of Cities, Accessed 2023). Most city council elections are also explicitly non-partisan (National League of Cities, Accessed 2023). While individual councilors may run under the banner of a political party, the specific affiliation will not typically be listed on the ballot. The actual process of policy making on councils usually involves close collaboration in committee settings with other members in order to develop the details of projects and proposals.[8] While national, hot-button issues do sometimes emerge in this process, a large portion of the consequential tasks therefore involve the straightforward administrative decisions necessary for city government to function.

2.2 Electoral and Spending Data

Our data on the outcomes of municipal elections come from two sources: the California Elections Data Archive (CEDA), and a series of FOIA requests sent to cities across the United States. The CEDA data contains vote totals and election results since 1995 for nearly every city in California. For this sample of elections, we are also able to obtain the party affiliation of council members by linking councilors to voter registration records from the California voter file. We supplement the California election data with new data we obtained via a series of FOIA requests sent to over 2,500 additional US cities. The cities where we sent FOIA requests were chosen from the set of cities represented in the Annual Survey of State and Local Government.[9] These FOIA requests produced nearly 200 additional cities with usable election data spanning 38 states. Our outcomes on local government spending come from the Annual Survey of State and Local Government (for cities outside of California) and municipal spending information that is publicly available from the California State Controller's office (for cities inside of California).[10] See Appendix A.1 for more details on the election data collection and cleaning process.

The unit of observation in our analysis is a "council," which is defined at the city-by-interelection-period level.[11] This unit of observation captures the notion of a consistent set of individuals who make decisions pertaining to city government in-between elections. This is the natural unit of observation in our setting, since it is the economic unit which

[8]According to a survey conducted by the National League of Cities, 81% of city councils rely on committees in order to "Provide groups of council members the opportunity to thoroughly consider particular items of business then recommend action on those items to the full council," National League of Cities (Accessed 2023).

[9]For this data product, the Census surveys every city in the United States over five years, with a rotating sample of cities surveyed every year for a period of 5 years. We sent FOIA requests to the cities represented in the rotating sample.

[10]Most of the national data covered the period from 2007-2015; thus, we restrict our attention to 2007-2015 in both data sets to ensure a common overlap.

[11]In most cities, councilors are elected in overlapping terms. Thus, our unit of observation generally corresponds to the first half of the term of the councilor elected in the close race.

is "treated" as a result of the close gendered election (see section 3 for more detail on the research design). For public spending outcomes, we include data from all fiscal years where the majority of the year overlaps with the relevant council and keep observations at the fiscal year level. Therefore, for spending outcomes, there are usually two observations in the analysis data per council, which we account for during inference by clustering our standard errors at the city level (see section 3 for more detail).

As a result of the FOIA requests, our data has more representative geographic coverage than prior work.[12] Figure 1 plots the national geographic distribution of the municipalities in our data. Panel (a) plots the "candidate set" of cities for FOIA requests that are contained in the Annual Survey of City Government. Panel (b) shows the set of cities where we were able to obtain data on electoral outcomes either via FOIA request or via the publicly available data in California. Within the set of cities where we have electoral data, panel (b) also further distinguishes between cities where we were (and were not) able to collect data on city council meeting minutes (see section 2.3 for more discussion of the minutes data).

[Figure 1 about here.]

Table 1 provides summary statistics for both the cities that enter our analysis sample and also for the population of cities from which they are drawn. The "All" column corresponds to every city represented in the Annual Survey of City Government. The "Election" column corresponds to the sub-sample of cities where we were able to collect election data. The "Gendered Election" column corresponds to the sub-sample of cities that have at least one opposite gender election necessary to work with our regression discontinuity research design. The "Minutes Sample" column corresponds to the subset of cities with a gendered election where we collected data on the internal workings of city council meetings via their minutes (see section 2.3 for more detail on the minutes data). Panel A gives averages of census characteristics drawn from the ACS as measured in 2012. Panel B gives averages of municipal spending as measured in 2012. Panel C gives averages of council characteristics compiled from the election data.[13] Relative to the average city in the United States, the gendered election sample has a larger population

[12] Prior work looking at city council elections in the US has focused exclusively on California (e.g. Beach and Jones, 2017).

[13] Since there is substantial variation both in the time periods covered by our election data and in the years different municipalities actually hold elections, it is impossible to fix a common reference year for panel C. For that reason, we give the averages in panel C over all available time periods.

and higher annual expenditures. This is primarily due to the fact that most of the cities where we could obtain election data were large relative to the average city, as evident in the election column of Table 1.

[Table 1 about here.]

2.3 City Council Meeting Minutes

Our meeting minutes data was hand-collected by the research team over the course of 2 years by visiting individual municipal web-pages and manually downloading them.[14] The research team prioritized cities that had close gendered elections within the relevant time period, since these are the cities that contribute the most identifying power using the close gendered election research design. In total, we collected 42,610 PDFs. Figure 2 provides an example of the minutes from a city council meeting.

[Figure 2 about here.]

We then sent these PDFs to a data entry firm and had them extract basic outcomes such as meeting dates, start times, stop times, and councilors present/absent. For a sub-sample of these PDFs, we asked the firm to collect more detailed data on each motion offered at the particular city council meeting.[15] These motion-level variables included the names of the councilors who made and seconded the motion, as well as which councilors voted for or against the motion, and a categorization of the motion topics. We create outcomes of interest from the minutes data by creating "per-meeting" averages at the council level. For additional details, see Appendix A.2.

We will sometimes wish to distinguish "behavioral" effects of gender composition on individual councilors from the direct contribution of the newly elected council member who was involved in the close election. We will refer to councilors elected in close elections as a "focal" councilor, and we will refer to members of the council who were not part of the close election as "non-focal" councilors. In general, non-focal councilors may be those who were elected in prior elections, those who were elected in the same election

[14] There was enough heterogeneity across municipal government websites such that scraping them proved impossible.

[15] More precisely, we asked them to extract detailed data from 3 randomly chosen meetings per council-year.

but in separate races, or non-marginally elected candidates within the same race.[16] Using first and last names, we are able to link councilors from the minutes data directly to their corresponding entry in the municipal elections data. This allows us to create separate outcomes which capture the behavior of only non-focal councilors by gender.

Table 2 provides summary statistics for the meeting minutes data. The "All" column refers to all councils where we collected minutes data. The "1 Non-Focal" column refers to the subset of councils that, were it not for the focal woman involved in the close election, would otherwise have contained a single, token woman. The ">1 Non-Focal" column contains the subset of councils that had multiple non-focal women. Panels A and B contain variables related to the meeting process and motions offered. Panel C contains the 4 most common motion topics. Note that motion topics are not mutually exclusive and hence shares do not need to add up to one. Numerical values in this table represent averages across councils. See Appendix A.2 for more detail regarding the minutes data collection and cleaning process.

[Table 2 about here.]

The typical council in our data met 45 times at an average length of 146.7 minutes over the course of a term. 93% of motions offered during a typical term are passed, and 90% are passed unanimously. While this may seem high, nearly 45% of all motions have a purely administrative function,[17] such as calling the meeting to order or putting the minutes into record. In supplementary analysis, we specifically explore the impact of a close female win on administrative motions.[18] Other common topics of motions include those related to finance (19%), regulation (10%), and public utility management (13%). The motions themselves are are also highly predictive of real world outcomes. In appendix tables D.4 and D.5, we show that the share of motions falling into various expenditure categories are highly correlated with actual expenditures (and changes in expenditures) within those same categories.

[16]Some councils in our data elect representatives by holding open elections for multiple city council seats, in which case the "top-N" vote-getters are elected to the council. In these cases, we define a gendered election as one where the "worst winner" and the "best loser" are of different genders, which implies that the "better" or "non-marginal" winners are exogenous from the perspective of the close gendered election.

[17]While 72% of all motions have *some* administrative function, as indicated in table 2, only 44% of motions have a purely administrative function that does not include some other, substantive point of business.

[18]We find little evidence that a female election win causes an increase in the share of administrative motions made by women, an outcome which could arguable fall into the category of "non-promotable" work as in Babcock et al. (2017) (see appendix table D.1).

3 Research Design

Our goal is to estimate the causal effect of replacing a male city councilor with a female city councilor on group behavior, individual behavior, and the outcomes of city council decision making. For identification, we rely on a close election regression discontinuity design similar to prior work that has explored the impact of gender representation (Ferreira and Gyourko, 2014; Gagliarducci and Paserman, 2012) or ethnic diversity (Beach and Jones, 2017) on public policy. This strategy relies on an assumption that the conditional expectation function mapping the running variable into outcomes would, in the absence of treatment, be smooth through the cutoff (Lee and Lemieux, 2010). We discuss the testable implications of this assumption further when we develop our formal econometric model in section 3.1.

One unique feature of the city council setting is that there are many electoral races in which multiple seats are at stake in a single contest. For example, a city may have two "at large" seats for which a large pool of candidates are eligible. Voters may vote for up to two candidates, and the two highest vote-getters win seats. Therefore, to determine a close election within each race, we focus attention on the vote differential between the winner with the lowest vote total ("worst winner") and the loser with the highest vote total ("best loser"). For races where the worst winner and the best loser are different genders, we construct the female vote differential by taking the number of votes received by the female best loser/worst winner, minus the number of votes received by the male best loser/worst winner. Consistent with prior work, we normalize the vote differential by the total number of votes cast in the race and hence our running variable is the vote-share (as in Lee, 2008, for example). In practice, other sensible methods of coding the running variable lead to identical results.[19] Our treatment variable is an indicator for whether the female candidate was the winner between this pair of candidates.

A note on the interpretation of the close-election estimand. The goal of our research design is to recover the real-world causal effect of replacing a typical male councilor with a typical female councilor. As noted by Marshall (2022) and others, councilor characteristics (party affiliation, for example) other than gender might also vary discontinuously across the threshold because women and men may differ in terms of other observables in the broad population (or specifically among those involved in close elections). Thus it

[19]For example, normalizing the vote differential by the total sum of votes received by the best loser and worst winner yields a running variable that has a correlation of 0.961 with our preferred, baseline normalization.

is possible that these other characteristics are part of the "mechanism" driving the corresponding changes in the outcome, rather than gender alone.

Our view is that the combined influence of all of these female characteristics on group behavior is precisely the treatment effect of interest in our setting. Put simply, if the causal channel that changes group behavior runs through population differences in ideology or political experience across genders, then that still implies a real impact on the lived experience of women. We believe that the causal effect of group composition on the lived experience of women is a worthy estimand to study irrespective of the underlying mechanism. Thus, we will not attempt to control for or hold councilor characteristics besides gender constant across the threshold, since this would mechanically generate a form of selection on the dependent variable and result in treatment effect estimates that do not correspond to the population level ATE of replacing a man with a woman. That said, in the interest of fully exploring mechanisms, we do examine political party affiliation as an outcome in section 5.1. We do not find strong evidence that party affiliation is the primary mechanism behind our main findings. In addition, we explore how the level of political experience represented on the council varies with our treatment. We do not find evidence that a female close election win changes the average tenure of the council members (see Appendix Table D.2 for more detail).

3.1 Econometric Model

Our estimating equation takes the form:

$$Y_c = \beta D_c + F\left(W_c\right) + \Gamma X_c + \varepsilon_c \tag{1}$$

where Y_c is the outcome for council c.[20] D_c is an indicator that takes a value of 1 if the female candidate involved in the close election won. W_c is a running variable that captures the margin of victory between the winner and the loser of the close election.[21] Our baseline model specifies F as piece-wise linear within a bandwidth around the threshold. In order to ensure that our point estimates are generated using a consistent sample and hence mutually comparable within a sample across outcomes, our baseline model uses a common bandwidth for all outcomes within each sample. We choose the common

[20]As discuss in section 2, we define a "council" at the level of a city-by-electoral term

[21]Given the multi-candidate nature of some of our elections, this is more precisely described as the margin of victory between the "worst winner" and the "best loser." See section 2 for see for the exact definition of these terms.

bandwidth within each sample as the MSE-optimal bandwidth (i.e. using the RDrobust package) for a specification where Y_c is the share of motions moved by non-focal women[22] (Calonico et al., 2014). This implies that the estimates from our preferred model for the share of motions by non-focal women are identical to those found using the RDrobust package implementation of Calonico et al. (2014). However, the results are qualitatively unchanged when we select the optimal bandwidth outcome-by-outcome (see appendix tables C.1, C.2, and C.3). We also find similar results when we manually vary the bandwidth, when we fix the bandwidth across samples, and when we vary the functional form of F by changing the order of the polynomial (see appendix tables C.4 and C.5). The vector X_c contains council level control variables. For precision, our baseline model includes controls for council size and term length, and we additionally control for baseline expenditures when analyzing effects on municipal spending; however, point estimates are similar without these controls (see Tables C.6, C.7, and C.8). The parameter of interest in this model is β, which measures the expected difference in the outcome variable at the RD threshold when a woman wins an election against a man.

The key assumption we rely on for causal identification is that the conditional expectation function mapping the running variable into outcomes is continuous across the threshold in the absence of treatment (Lee and Lemieux, 2010; de la Cuesta and Imai, 2016). Under this assumption, we can interpret β as the causal effect of a female win when vote shares between the winner and loser are equally split. We provide evidence in support of this assumption in section 3.2. For inference, we account for within-city serial correlation in the outcome over time by clustering our standard errors at the city level. However, our approach to inference is also robust to additionally correcting for the sampling variability that is introduced when selecting the optimal bandwidth (see appendix tables C.9, C.10, and C.11 for tables with RDrobust bias corrected p-values).

In addition to estimating the average effect of an additional woman, we will also present estimates for two important sub-samples. The first sub-sample, which we call the "1 Non-Focal Woman" sample, corresponds to councils where the gendered election has the potential to add a second female to a council that would otherwise have only one female member. This cut of the data is motivated both by classic work on critical mass theory (e.g. Kanter, 1977) and by existing experimental work which finds that isolated, "token" women behave differently in group settings (e.g. Karpowitz et al., 2024). This is

[22] We choose this outcome to use when fixing the bandwidth because it is a key result of the paper, since it demonstrates the existence of behavioral effects stemming from group gender composition outside of a laboratory or lab-in-the-field type setting.

also a relevant margin for policy in the city council setting, since the modal council in our data has exactly one female councilor.

The second sub-sample, which we call the ">1 Non-Focal Women" sample, corresponds to councils where the gendered election has the potential to add a woman to an environment where there are already at least two non-focal women. Our formal model in section 6, which operationalizes critical mass theory for this setting, predicts that once the threshold has been reached, there should be no behavioral responses among the female non-focal councilors. Therefore, this sub-sample allows us to use the tools of causal inference to explore the thresholding effect from critical mass theory and that has been the subject of much prior, descriptive literature (Thomas, 1994; Studlar and McAllister, 2002; Bratton, 2005).

3.2 Validity of the Regression Discontinuity Design

Our key identifying assumption, which is that the conditional expectation function mapping the running variable into outcomes would be continuous across the threshold in the absence of treatment, has testable implications (Lee and Lemieux, 2010). First, it implies that the sample should be balanced on baseline and otherwise exogenous characteristics at the threshold. Table 3 provides evidence of balance for the full sample, and the two sub-samples of interest, by putting baseline and otherwise exogenous council characteristics on the left-hand side of our preferred model (equation 1). We do not find evidence of sample imbalance, individually or jointly, for the full sample or the 1 Non-Focal sample. For the >1 Non-Focal sample, we find that two characteristics are discontinuous across the threshold which may raise some concern for this sub-group; however, the joint test cannot reject that all coefficients are equal to zero which is consistent with the two significant coefficients representing false positives in light of the many coefficients checked for balance in table 3.

[Table 3 about here.]

Figure 3 plots a histogram of the running variable for our full sample. As noted by McCrary (2008), the RD identifying assumption implies that councils should be unable to manipulate the running variable to determine treatment status. This implies that the density of the running variable should be continuous around the RD threshold. Note that as described in Appendix A.2, in order to increase power, we prioritized digitizing meeting minutes for cities which had a close gendered election. This should result in a sample with increased, but still continuous, mass near the threshold, as is suggested by a visual inspection of Figure 3. More formally, we test for violations of continuity using the method described in Calonico et al. (2017) and, consistent with our identifying assumptions, we find no evidence of manipulation ($P = 0.255$). Appendix Figure B.1 displays histograms for the relevant sub-samples of interest, and Appendix Table B.1 provides the formal density test P-values for all samples. In all cases, we find evidence consistent with no manipulation.

[Figure 3 about here.]

3.3 "First Stage" Effects on Nominal Representation

Figure 4 provides evidence that winning a close election does, in fact, generate consequential changes in the gender composition of the council. Panel (a) plots the probability that the woman involved in the close election is seated on the council as a function of the running variable and hence documents that our design has the necessary first-stage effect on treatment.[23] This suggests that the RD design is "nearly" sharp, and for that reason, we will interchangeably refer to the treatment effects estimated by our preferred model as the impact of "a female winning a close election" and "replacing a man with a woman in a close election." Panel (b) plots the overall share of councilors that are female as a function of the vote-differential running variable. The point estimates suggest that a female victory causes a 20p.p. increase in the overall female share (see table 4).

In theory, the addition of a woman in a close election could cause other women (or men) to leave the council or retire before the start of the next term, which would complicate our interpretation of the treatment effects identified by model 1. We confirm that this is not the case. Panel (b) of Figure 4 provides suggestive evidence. Given an average council size of ≈ 5.5 members (see table 1), the 20p.p. jump in the overall share of the council that is female is therefore consistent with a change in the gender of one member from male to female. Panel (c) of Figure 4 provides direct evidence. It shows that a female winning a close election has no impact on the gender composition of the other members of the council. Panel (c) therefore serves as a natural "placebo" test for our preferred interpretation of the estimates as representing the impact of adding an additional woman

[23] We define a woman to be seated on the council if she appears in more than 10% of the relevant meeting minutes. This is because we do not directly observe which councilors are sworn-in after winning an election. According to this definition, we find that winning the close election causes the woman to be 82.4% more likely to be seated on the council.

to the council, since it shows that there are no *additional* downstream changes in council gender composition that are caused by a female win.

[Figure 4 about here.]

4 Findings

4.1 Overall Effects on Substantive Participation

We find evidence that nominal representation does translate into substantive participation. Figure 5 shows visual RD evidence of effects on our key outcomes related to women's substantive participation in decision making. Table 4 provides corresponding point estimate from model 1 for these key outcomes and others across three samples: the full sample of councils where we observe meeting data, the sub-sample of these councils with 1 non-focal woman, and the sub-sample of councils with multiple non-focal women.[24]

[Figure 5 about here.]

Panel (a) of Figure 5 plots the share of motions made by women in the council against the female vote margin running variable. We see a large jump in the number of motions made by women precisely at the female victory threshold. Table 4 reveals that this jump corresponds to an increase of 25p.p., which is large relative to the average share of motions made by women in this sample (22.6%), and hence the treatment effect represents a 109% increase relative to the mean. Perhaps more surprisingly, this increase in participation is also 25% larger than the overall impact that a female victory has on average female representation on the council. This suggests that the change in the number of motions made is not simply the product of replacing the motions of a man with the same number of motions now offered by the closely elected woman, a point we return to in section 4.2.

These findings are robust to other choices of outcomes and samples. For example, we find similar patterns when examining the number of motions that are made or seconded by women (see table 4). As with the average council, we also find that for both the 1

[24] Our key behavioral outcomes (participation of non-focal women) are not well defined for councils with zero non-focal women. For completeness, we include results for outcomes that are well-defined for this sample in Table D.3.

non-focal woman sample and the > 1 non-focal women sample, nominal representation does indeed translate into substantive participation (see panel B of table 4).

However, Panel B also reveals some intriguing heterogeneity by baseline representation. For the sub-sample with an otherwise isolated woman, the impact of an additional woman on the council is nearly twice as large as it is in councils where women are better represented (30.1 p.p. relative to 15.6 p.p.). This suggests that there may be important behavioral effects to these isolated women similar to those documented in the laboratory literature, a possibility we turn to directly in the next section.

[Table 4 about here.]

4.2 Behavioral Effects on Non-Focal Councilors

Why is the impact of an additional woman on substantive participation larger than the corresponding impact on nominal representation? Panel (b) of Figure 5 plots the share of motions made by non-focal women for councils where they would otherwise be isolated. Consistent with a large laboratory literature documenting the behavioral impacts of increased gender representation on lone, "token" women, we find visual evidence that adding an additional female councilor causes the non-focal woman to offer more motions. While less visually stark than the full council figure, there is a clear jump in the female motion share upon crossing the female victory threshold.[25]

Table 5 presents estimates for the impact of a narrow female victory on the behavior of non-focal council members. Importantly, all outcomes in this table are calculated *excluding* the man or woman involved in the close election and therefore identify a pure "behavioral" effect that is not directly connected to the change in nominal representation. The evidence in Table 5 confirms that the apparent visual jump in panel (b) of Figure 5 is, indeed, statistically significant. We note here that, because we use this specific outcome (share moved by women among non-focal councilors) to determine the optimal bandwidth, this implies that the point estimate and inference for this outcome in Table 5 is

[25]We acknowledge an apparent slope of the outcome through the running variable in panel (b), which is driven by the fact that female candidates tend to have larger margins of victory in councils where their substantive participation is low. However, it is important to note that, as discussed by de la Cuesta and Imai (2016), the preferred identifying assumption in close election regression discontinuity designs relies on continuity of the counterfactual conditional expectation function through the cutoff. Hence, there is no contradiction between the assumptions we rely on for causal identification and the apparent slope in panel (b) of Figure 5.

identical to that obtained from the RDRobust package[26] and hence optimally trades off bias and variance, which should address concerns about the noise apparent in panel (b) of Figure 5 (Calonico et al., 2014, 2017).[27] However, to further address the noise apparent in panel (b) of Figure 5, we also conduct an alternative form of statistical inference for this outcome based on randomly permuting the vote margin across councils. We find a permutation p-value of 0.018 (see Appendix Figure C.1). Since permutation tests of this sort do not rely on asymptotic approximations and are exact in finite sample for a sharp null (Young, 2019), this permutation p-value provides strong evidence that the apparent discontinuity in panel (b) is a real jump and not simply the result of sampling variation.

[Table 5 about here.]

We find that the addition of a second woman to a council with a lone token woman causes the share of motions made by the otherwise isolated female councilor to increase by 14.8p.p. This is large, constituting a 68% increase relative to the dependent variable mean in this sample (21.9%). It is also large relative to the overall council level effect of 30p.p. for this sub-sample, suggesting that roughly half of the change in substantive participation that accrues from a female victory in this sample comes from the behavioral changes of isolated women. This change in the share comes about via an increase in the number of motions offered by the non-focal woman of 0.86 per meeting. Point estimates for men are similar in magnitude, but negative and imprecisely estimated.

Consistent with critical mass theory (e.g. Kanter, 1977), we do not find statistically precise evidence that female victories cause behavioral changes for non-focal woman when they have more representation at baseline. In fact, the point estimates suggest the overall share of motions made by females among non-focal members actually *declines* by 4.5p.p. This suggests some possible substitution between focal and non-focal councilors when baseline representation is large. However, we note that the standard error for this finding is less precise, and hence it is difficult to make strong statistical claims about this outcome in this sample.

[26] See appendix tables C.1, C.2, and C.3 for estimates using data driven bandwidths, selected outcome-by-outcome, for all of our key outcomes of interest. The results are robust.

[27] Appendix table C.10 also provides corresponding bias corrected p-values that account for the additional sampling variability produced by the bandwidth selection process. The statistical inference is unchanged.

4.3 Municipal Spending Outcomes

We do not find evidence that the large changes in nominal representation and substantive participation translate into consequential changes in spending. Table 6 shows point estimates for spending per-capita overall and within specific sub-categories.[28]

[Table 6 about here.]

Table 6 reveals a pattern of consistent and, in many cases precise, null results. For example, the point estimates and standard errors for the total per-capita spending suggest that replacing a man with a woman causes reductions of *at most* $280 per person. Across all of the outcomes considered, only spending on Airports and, for the 1 non-focal woman sample, Libraries approach conventional levels of statistical significance. However, even these findings may be a statistical artifact of the many hypotheses explored in table 6: the P-value from a joint-F testing the hypothesis that all coefficients are zero in our full sample is 0.374.

Thus, the balance of the evidence suggests that the previously documented increases in nominal representation and substantive female participation do not appear to translate into meaningful changes in the level or composition of municipal expenditures. This is despite the fact that the motions themselves[29] are highly predictive of actual expenditures and changes in expenditures within these various categories (see appendix tables D.4 and D.5 for more detail). This suggests that a more complex model than the traditional, "classic" story of critical mass (Kanter, 1977) may be necessary to rationalize these patterns. We turn to this puzzle in the next section.

5 Mechanisms

Why does nominal representation lead to large changes in substantive participation that do not translate into shifts in the composition of public expenditure? In this section, we consider two candidate explanations: party affiliation / coalitional behavior and preferences. We find little evidence that party affiliation or coalitional behavior plays an impor-

[28] In this case, we replace baseline spending with a control for lagged spending, which improves our precision considerably. However, we obtain similar conclusions (albeit with larger standard errors) if we omit this control entirely. See appendix table C.8 for more detail.

[29] Which, as documented in sections 4.1 and 4.2, are a measure of women's substantive participation that are indeed affected by the female victory.

tant role. However, we do find suggestive evidence that close alignment of preferences between men and women could rationalize these patterns.

5.1 Party Affiliation and Strategic Voting

There is a theoretical literature in political science and sociology that predicts "backlash" effects (e.g. Yoder, 1991; Faludi, 2009) from increases in out-group representation (Blalock, 1967; Blumer, 1958). For example, if men and women have divergent political ideologies, the election of an additional woman to the council could cause the dominant group to form a voting block or otherwise vote more cohesively in response.[30] Thus the apparent incongruity between the null result on spending and the notable increase in women's substantive participation could, in part, reflect this strategic or coalitional behavior undermining gains that would have otherwise accrued to women on the council. To explore this hypothesis, we bring in two additional sources of data: the political parties of the elected officials, and the voting patterns of individual councilors.

Political parties. First, we explore the role of political parties. As discussed briefly in section 2, we are able to use data from the California voter file to learn the party registration of councilors in the sub-sample of elections that take place in California.[31] Because this data is only available for California, we will have a substantially smaller sample to use when exploring party affiliation relative to our main analysis, and this will limit statistical power when we explore these outcomes.

In order for an explanation based on divergent political ideologies between men and women to be reasonable, it must be the case that a female victory meaningfully shifts the party composition of the council.[32] Thus, we use the party affiliation data to ask whether a close female victory causes large changes in political composition.

Table 7 contains the results. We do not find statistically precise evidence of an effect on any of our party affiliation variables: share democrat, republican, other party, or unaffiliated. However, we acknowledge that the point estimates for share democrat and

[30] In related work, Bagues et al. (2017) find that male evaluators in scientific committees become less favorable towards female candidates when there is a female evaluator in the committee.

[31] We are able to match 34% of councilors to their party affiliation. However, in Appendix Table C.12, we show that the share of councilors we are able to match to their party affiliation is unaffected by a close female victory. Thus, for the analysis in this section, we construct our party affiliation outcome variables as the share of matched councilors registered in the indicated party.

[32] In appendix table D.2, we also show that the close election win does not cause statistically detectable changes in the average tenure of council members, which suggests that changes in political experience are not a relevant mechanism.

republican are moderately sized (0.090 and -0.104, respectively) and estimated with sufficient imprecision such that we cannot rule out large effects.

[Table 7 about here.]

However, if we take the point estimates seriously, there is suggestive evidence that party affiliation is not the primary mechanism. First, we note that the changes in party affiliation are smaller in magnitude, roughly half the size, of the impact on gender composition in this sample (0.236). At the same time, in this sub-sample we continue to see similarly sized effects for the impact of the additional female councilor on the share of motions presented by women (0.279 in Table 7, versus 0.247 for the main result in Table 4) with similar patterns holding in the sub-samples that contain 1 non-focal woman. Thus the magnitude of the point estimates, along with empirical findings from the large literature documenting similar behavioral effects in laboratory settings where political party is not salient (e.g. Karpowitz et al., 2024; Bordalo et al., 2019; Coffman, 2014; Coffman et al., 2021), would be consistent with the idea that party affiliation is not the primary mechanism here.

That said, due to the imprecision, these results are suggestive at best and hence we cannot be definitive on this point using the party affiliation data alone. It is also possible that, in this setting, small changes in party affiliation lead to large changes in the behavior of councilors *and* also large backlash effects that prevent the motions offered by women from passing. For that reason, in the next section, we directly examine changes in voting patterns.

Voting behavior. Next we examine the voting patterns of individual councilors. If strategic or coalitional behavior driven by political parties or backlash effects were responsible for the lack of effect on spending, we would expect to see increased levels of disagreement as men increasingly vote along gender or party lines to prevent the motions made by women from passing and thereby maintain the policy status quo. Unlike our data on political affiliation, we are able to construct these outcomes for the full sample of councils, which affords us more precision.

Table 8 contains the results. We find little evidence that a female victory leads to meaningful changes in voting patterns. For example, Table 8 reveals little change in the share of motions that pass unanimously, the share of motions rejected, the average vote margin, or the average number of votes cast for/against a typical motion. This is true both for the average council, and in councils with an otherwise isolated female councilor, where we find the largest effects on substantive participation.

[Table 8 about here.]

The null results on voting behavior in Table 8 are also quite precise. For example, the 95% confidence interval for the impact of an additional female councilor on the share of unanimous motions is small enough to rule out changes larger than 0.5p.p. (approximately 5.6% of the dependent variable mean). We find broadly similar levels of precision when we explore other outcomes, like the share of motions rejected, and when we explore alternative sub-samples.

Taken together, the evidence in this and the preceding section suggest that neither changes in party affiliation nor backlash effects are important explanations for the null effect on the level and composition of spending. In the next section, we use our minutes data to offer an alternative explanation based on the overall similarity of policy preferences between men and women on the issues decided by city councils.

5.2 Preference Alignment

Policy preferences provide another explanation that could reconcile the large changes in substantive participation with the lack of change in the composition of public expenditure. Intuitively, if men and women have similar preferences about the outcome of a policy process, but women incur additional costs to participating in the policy discussion when they are in the minority, then adding a female councilor could generate behavioral effects on participation without changing the ultimate outcome of the decision. In section 6, we will formalize this intuition mathematically. However, before we formalize this intuition, we ask whether there is empirical support for this hypothesis in the data.

To explore the role of preferences, we conduct two supplementary analyses that leverage our motion data to explore the topical content of the proposals under discussion. If the topics of motions are coarse proxies for the underlying preferences and tastes of the individuals who put them forward for a vote, then we can use them to explore this hypothesis. In appendix tables D.4 and D.5, we show that motion shares within topical categories are highly correlated with actual expenditures (and changes in expenditure) in those same categories.

First, in Table 9, we present summary statistics on the share of motions offered by men and women in our sample related to specific topics. We find little difference in the topical content of the motions offered. For example, we find that the share of finance motions (the most common category in our data after administrative motions) offered by women is only 1.4p.p. higher than the share offered by men. None of the differences in Table 9 are statistically significant at conventional levels. This evidence is consistent with the hypothesis that the preferences of men and women in our sample are similar. However, we acknowledge that this is a descriptive fact – it does not rule out the possibility that the addition of a female councilor, and the behavioral changes it engenders among the other councilors, could lead to a substantive shift in the topics of discussion. To address this, we must leverage the exogenous variation from the regression discontinuity.

[Table 9 about here.]

Next, we ask whether the election of a female councilor causes consequential changes in the topics of discussion within the council. Table 10 presents estimates from our preferred RD model, but using the share of motions related to particular topics as the outcome. We do not find statistically precise evidence that the election of an additional woman changes the types of motions being offered. For the full sample, there are two individually significant results; however, we fail to reject a joint test of the null that all coefficients are identically zero (P = 0.08). We find similar patterns in the sub-sample with 1 non-focal woman. This is consistent with the idea that electing a new female councilor has no effect on the topics of discussion.

Alternatively, even as women increase their participation, we may expect no effect on motion topics if the male majority retains agenda control (McKelvey, 1976; Ali et al., 2023). Thus, one might speculate that there may exist different thresholds of nominal representation, analogous to a progression along Kanter (1977)'s scale from "skewed" to "tilted" to "balanced" groups, where progressive levels of substantive representation are achieved. In that case, however, we would expect a larger effect on motion topics at higher baseline levels of female representation. This is not borne out by the data. In fact, Panel C in Table 10 shows insignificant and even smaller effects on motion topics across the board for the "$>$1 non-focal women" sub-sample.[33]

[Table 10 about here.]

Finally, we explore the possibility that the overall increase in motions offered by women is due to the female councilors being given an excessive administrative burden. This

[33]We are unable to further narrow down our estimate to the specific effect of reaching female majority, as those councils represent only 16% of our sample.

would be consistent with the idea that group gender dynamics often lead to women being forced to perform work that is ”non-promotable” (Babcock et al., 2017). To explore this hypothesis, we explore the impact of a female close election win on the number of female motions made per woman that serve an administrative purpose. We do not find statistically precise evidence that the administrative burden shouldered by female councilors is increasing in response to the close election win (see appendix table D.1).

Thus, taken together, the evidence in this section suggests that the close alignment between the preferences of men and women in our sample could rationalize the null results on spending despite large changes in nominal and substantive representation. This, in turn, suggests that preferences might be a more general source of heterogeneity that governs whether the representation of women will lead to consequential changes in the outcomes of policy. Motivated by this possibility, in the next section we develop a theoretical model based on the logic of critical mass that establishes this hypothesis more formally.

6 A Stylized Model of Critical Mass

In this section, we present a stylized, theoretical model of critical mass. Our goal is to provide a theoretical framework that will help interpret our results and serve as a guide for future work on this topic. The model is motivated both by the institutional details discussed in section 2.1 and the substantive empirical findings from the regression discontinuity analysis.

The structure of council decision making. Consider a city council engaged in a collaborative decision that determines public policy. The council is denoted by I and comprises N councilors. The process begins with a potential project arriving before the council. Concretely, this project could emerge via a mandate from a higher level of government, from a petition filed by a constituent, or from the routine business regularly performed within the council.

When a project arrives before the council, individual councilors sequentially choose whether to participate in a committee that will determine the project's overall quality. Denote each councilor's idiosyncratic valuation of the project by u_i. If no one volunteers to join the committee, the project fails and councilors receive the status quo utility u_0. If a project is taken up by the committee, then each council member on the committee contributes an additional value of δ_i to the project, at personal cost c_i.

Once a project is finished in the committee process, a committee member, chosen at random, proposes a motion before the council to approve the project. If the motion is approved by a majority, each councilor receives the following utility

$$U_i = u_i + \sum_{j \in I} D_j \delta_j$$

where D_j is an indicator for councilor j's participation. If the motion to approve the project fails, council members receive u_0.

We consider the unique subgame-perfect equilibrium of the game, assuming members vote truthfully and that whenever a member is indifferent between participating or not, they break the tie in favor of participating.

Preferences and Costs for Men and Women. Suppose that the council can be partitioned into two identity groups: a majority and a minority group. For simplicity and to match our setting and data, let the majority group be men ($M \subseteq I$), and we shall refer to the minority group as women ($W \subset I$),[34] although any underrepresented identity may fit the model. For simplicity, assume that for each project, all men have identical policy preferences given by $u_i = u_m$ and that all women have identical policy preferences given by $u_i = u_w$.

Motivated by the large experimental literature which documents that women are less likely to contribute to group decision making processes when they are in the minority (Karpowitz et al., 2024; Coffman, 2014; Coffman et al., 2021), we assume that councilor i faces a cost of participating $c_i = C(N_i)$, which is strictly decreasing in N_i, the number of councilors that share i's gender. Also assume that all councilors contribute equal values to the committees they join so that $\delta_i = \delta$ for all i. We show that women's participation disproportionately increases once the number of women reaches a critical mass N^*, given by $C(N^*) \leq \delta < C(N^* - 1)$. For this result to be interesting, we assume $1 < N^* < \frac{N+1}{2}$; our empirical results and the tokenism literature (e.g. Karpowitz et al., 2024) are consistent with $N^* = 2$.

Finally, assume that women choose first.[35] We now present simple comparative statics that align with the findings in our data, focusing on the on-the-equilibrium-path out-

[34] In our data, only 16% of city councils are majority female, which limits us from investigating female majority effects.

[35] This does not affect our qualitative predictions, but rules out some borderline cases that depend on the sequential order.

comes.[36]

Comparative statics. This model yields testable predictions as the number of women crosses the critical mass threshold. Compared to the council with a token woman, a more gender-balanced council sees an increase in participation (and therefore the number of motions) by *all* women. Thus, the effect on women's total motions is larger than the straightforward effect expected when replacing a man with a woman.

However, the model's predictions regarding the types of motions that are brought to a vote and the outcomes of these votes depends critically on the alignment of male / female preferences with respect to the projects brought before the council. Our results show a differential effect of achieving critical mass on two types of projects: gender-neutral, defined as $|u_m - u_w| \leq \varepsilon$, and gendered, where $|u_m - u_w| > \varepsilon$, for some $0 < \varepsilon < \delta$. Gendered projects can be female-preferred ($u_w > u_m + \varepsilon$) or male-preferred ($u_m > u_w + \varepsilon$).

PROPOSITION 1. Participation. *There exists a critical mass N^* such that, when women's nominal representation N_w increases from $(N^* - 1)$ to N^*, female participation increases disproportionately in both gender-neutral and gendered projects.*

If the number of women is below N^*, the woman councilor participates only in some female-preferred projects where her participation is pivotal, withholding participation in all male-preferred projects. If $N_w \geq N^*$, then women participate at the same rate as men in all projects. This is consistent with the behavioral effect on non-focal councilors documented empirically in section 4.2

PROPOSITION 2. Non-unanimous votes. *Whether or not the critical mass N^* is achieved, gender-neutral and female-preferred motions are passed unanimously conditional on being proposed. When critical mass is achieved, there are fewer non-unanimous male-preferred motions but more dissenting votes per non-unanimous motion.*

This proposition shows that if the addition of a female councilor causes an increase in dissent among the council members, it will only be on a specific subset of gendered topics. Thus, if preferences are broadly aligned between men and women, as they are for the average council in our data (see section 5.2), the model predicts no change in voting

[36] A complete description of councilors' equilibrium strategies is provided in Appendix E.

patterns despite the increase in substantive participation. This is consistent with the null effect we find on voting in section 5.1.

[Figure 6 about here.]

Our empirical results are consistent with a critical mass threshold at $N^* = 2$. Although we observe significant behavioral effects of women's representation on women's participation, it does not seem to result in differences in the substance of policy or disagreement within the council. This is consistent with the model predictions in the case where men and women do not have divergent preferences, a hypothesis that is supported by the data on motion topics explored in section 5.2. Interestingly, these predictions and empirical patterns are also consistent with Ferreira and Gyourko (2014), who find a similar set of null effects on spending for close female victories by US mayors.

7 Conclusion

In this paper, we examine the causal effect of gender composition on nominal representation, substantive participation, decision-making and municipal expenditures. The key to our contribution is an ambitious data set that we assembled by sending over 2,500 FOIA requests and by hand collecting and coding over 40,000 PDFs of city council meeting minutes. Equipped with this data, we use a close election regression discontinuity to explore the impact of an additional women on council behavior, both on average and as it varies with different levels of baseline representation.

Relative to existing work on the gender composition of groups, our paper is the first to directly document causal impacts on women's substantive participation outside of a laboratory setting. At low baseline levels of female representation, we find that the share of motions offered by women not involved in the close election increases by 14.8 p.p. This is similar to behavioral effects documented in the laboratory and lab-in-the-field literature (Coffman et al., 2021; Karpowitz et al., 2024). Thus, our results suggest that this rich body of work has a broad claim to external validity in high stakes, non-experimental settings.

Relative to existing work on women's representation in politics, our unique data allow us to explore the implications of critical mass theory using the tools of modern causal inference (Kanter, 1977). On the one hand, we find that the theory holds up remarkably well: the causal effect of nominal representation on substantive participation does vary non-linearly with baseline representation. On the other hand, we do not find statistically precise evidence that these increases in nominal and substantive participation shift the topics of discussion, result in meaningful changes in voting behavior, nor that they affect the composition of public spending.

We show how these patterns can be rationalized by a stylized model of critical mass that features preferences as an important source of heterogeneity. Put simply, our model predicts that women's nominal representation matters for the outcomes of public policy only when her preferences differ substantially from her male counterparts. While simple on its surface, we argue that this hypothesis could potentially explain the diverse, and sometimes contradictory, findings of the causal impact of women's representation more broadly (e.g. Ferreira and Gyourko, 2014; Chattopadhyay and Duflo, 2004; Gagliarducci and Paserman, 2012; Baskaran and Hessami, 2023). In so doing, we hope that this opens up rich areas for future work.

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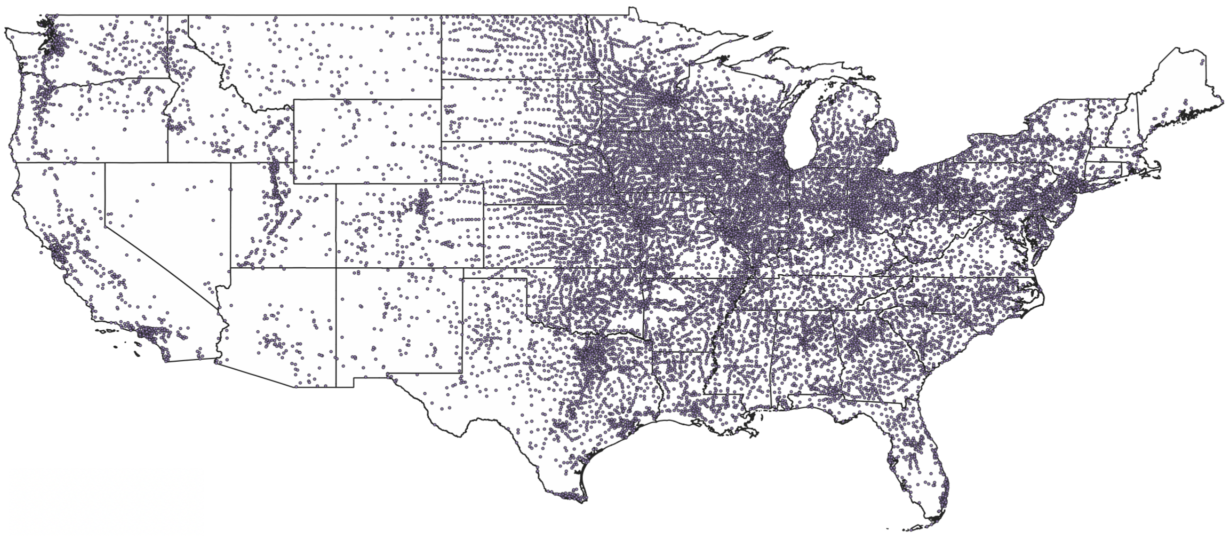
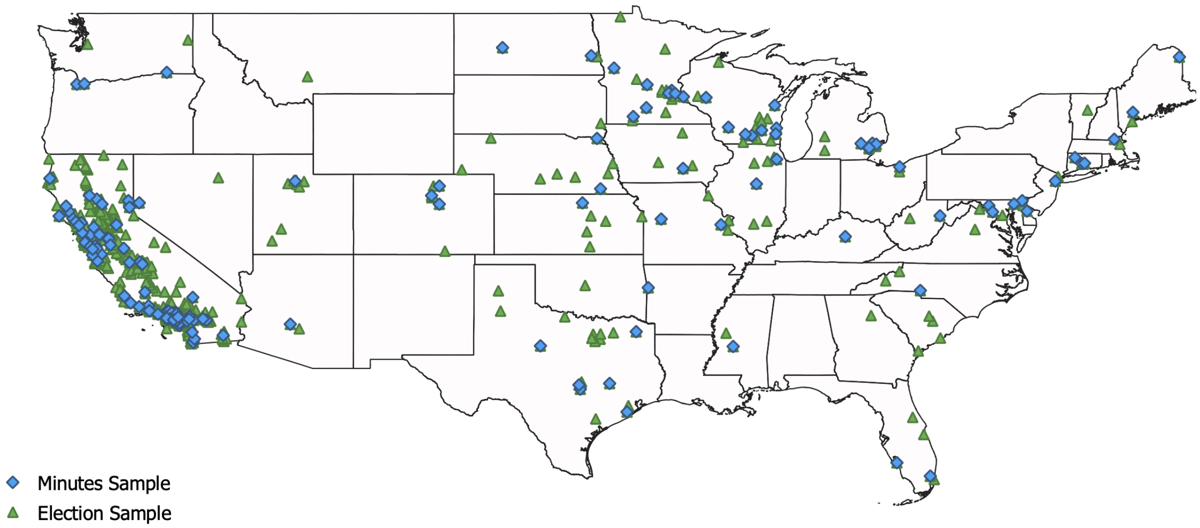
Figure 1: Geographic Distribution of City Councils

(a) Councils Represented in Annual Survey of City Government



(b) Councils Contained in Election and Minutes Data



Note: Panel (a) shows the geographic distribution of the initial set of cities and towns we used to build our analysis data. Panel (b) shows the distribution of cities and towns where we were able to collect data on electoral outcomes and the internal workings of city council meetings via their minutes. Green triangles denote cities where we were only able to obtain electoral data. Blue diamonds denote cities where we were able to obtain both electoral data and data from meeting minutes.

Figure 2: Example of Meeting Minutes

Minutes
City Council/Redevelopment Agency/Public Financing Authority
City of Huntington Beach

Monday, November 15, 2010
4:00 PM - Room B-8
6:00 PM - Council Chambers
Civic Center, 2000 Main Street
Huntington Beach, California 92648

An audio recording of the 4:00 PM portion of this meeting and a video recording of the 6:00 PM portion of this meeting are on file in the Office of the City Clerk and are archived at www.surfcity-hb.org/government/agendas/

4:00 PM - ROOM B-8

The City Clerk recessed until 4:30 PM due to a lack of quorum.

CALL TO ORDER - 4:35 PM

ROLL CALL

Present: Carchio, Coerper (arrived at 5:15 PM for Closed Session), Hardy, Green, Bohr, Dwyer, and Hansen

ANNOUNCEMENT OF COMMUNICATIONS RECEIVED AFTER AGENDA DISTRIBUTION

Pursuant to the Brown "Open Meetings" Act, City Clerk Joan Flynn announced that the following communication was received by her office after distribution of the City Council agenda packet:

Communication received from Shari L. Freidenrich, CPA, City Treasurer, dated November 15, 2010 entitled City of Huntington Beach Investment Advisory Board (IAB) Annual Report to the City Council for the Period October 1, 2009 to September 30, 2010.

28. Approved for introduction Ordinance No. 3905 amending Chapter 14.12 of the Huntington Beach Municipal Code (HBMC) relating to fees, rates and deposits for water billing.

A motion was made by Coerper, second Hardy to after the City Clerk reads by title, approve for introduction Ordinance No. 3905, "An Ordinance of the City of Huntington Beach Amending Chapter 14.12 of the Huntington Beach Municipal Code Relating to Water Billing." The motion carried by the following roll call vote:

AYES: Carchio, Coerper, Hardy, Green, Bohr, Dwyer, and Hansen
NOES: None

ORDINANCES FOR ADOPTION

29. Adopted Ordinance No. 3902 amending Huntington Beach Municipal Code (HBMC) section 8.43 relating to emergency response costs Approved for introduction November 1, 2010.

A motion was made by Hansen, second Carchio to after the City Clerk reads by title, approve for introduction Ordinance No. 3905, "An Ordinance of the City of Huntington Beach Amending Chapter 14.12 of the Huntington Beach Municipal Code Relating to Water Billing." The motion carried by the following roll call vote:

AYES: Carchio, Coerper, Green, Dwyer, and Hansen
NOES: Hardy, and Bohr

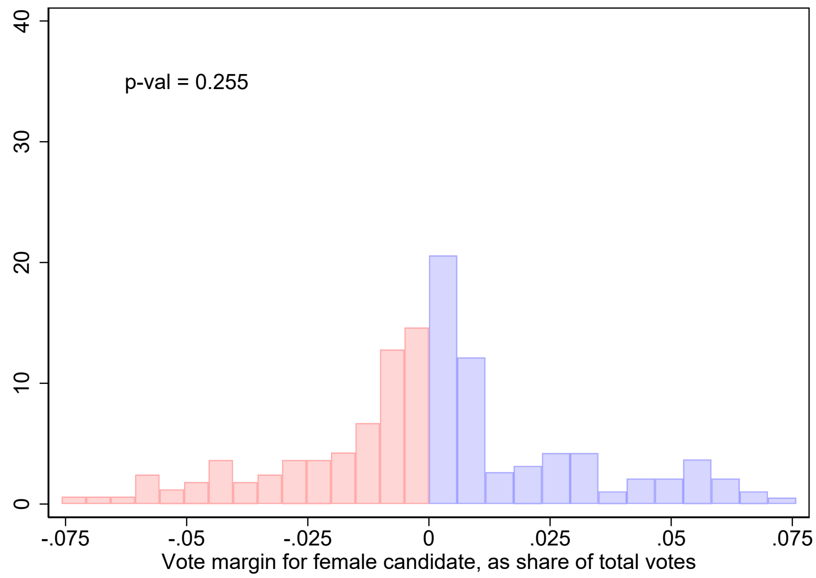
30. Adopted Ordinance No. 3903 accepting modifications to Local Coastal Program Amendment (LCPA) No. 2-10 approved by the California Coastal Commission and amend the Local Coastal Program (LCP) accordingly Approved for introduction November 1, 2010.

A motion was made by Hansen, second Coerper to after the City Clerk reads by title, adopt Ordinance No. 3903, "An Ordinance of the City Council of the City of Huntington Beach amending sections 203.06, 216.04, 216.18, 221.10 and 230.82 of the Huntington Beach Zoning and Subdivision Ordinance thereof to conform LCP Amendment No. 2-10 made by the California Coastal Commission." The motion carried by the following roll call vote:

AYES: Carchio, Coerper, Hardy, Green, Bohr, Dwyer, and Hansen
NOES: None

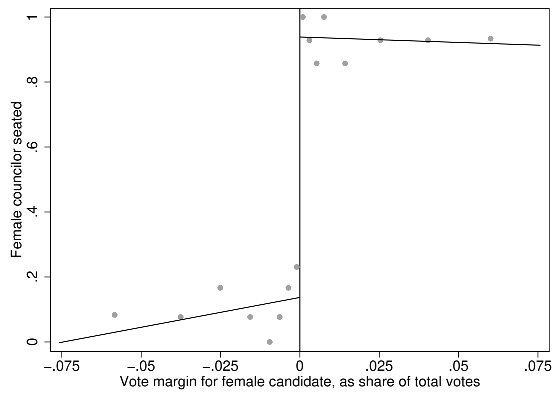
Note: This figure displays two pages taken from the November 15, 2010, meeting of the Huntington Beach city council. The left panel provides information on attendance and the meeting date / time. The right panel shows examples of motions which were offered for a vote and the outcomes of votes on those motions.

Figure 3: Density of Councils around RD Threshold, Full Sample

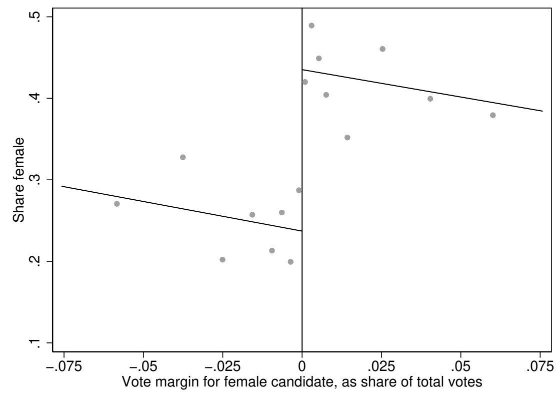


Note: This figure plots the density of cities represented in our full election sample by the vote margin of the female candidate in the cross-gender election. Note that as described in Appendix A.2, in order to increase power, we prioritized digitizing meeting minutes for cities which had a close gendered election. This should result in a sample with increased, but still continuous, mass near the threshold, as is suggested by a visual inspection of this figure. More formally, the p-value (0.255) represents the result of a statistical test for continuity of density across the threshold using the RDRobust package as described in Calonico et al. (2017). We do not find evidence of manipulation. Appendix Figure B.1 displays similar histograms for other relevant sub-samples of interest, and Appendix Table B.1 provides the associated p-values.

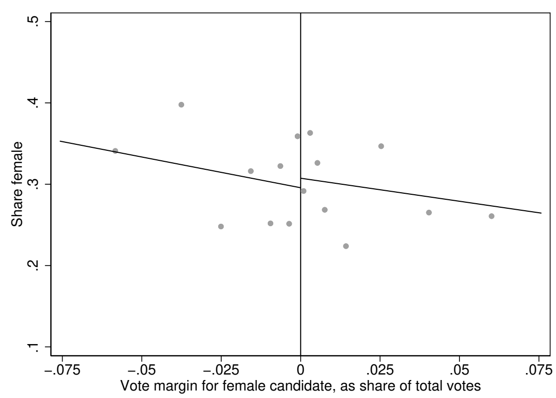
Figure 4: Discontinuities in Nominal Representation



(a) Female Councilor Seated



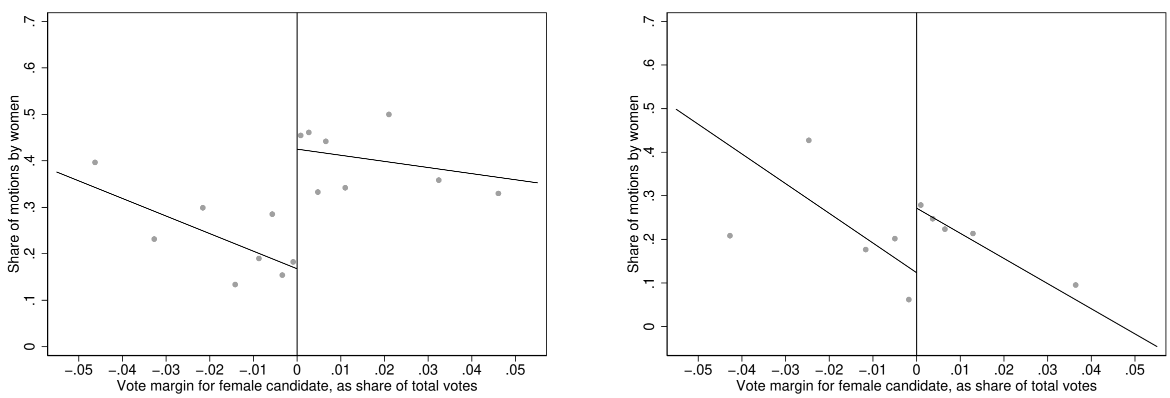
(b) Female Share of All Councilors



(c) Placebo Test: Female Share of Non-focal Councilors

Note: This figure plots the running variable (vote-share normalized to be zero at the cutoff) against measures of council gender composition within the RD bandwidth. Panel (a) plots the probability the focal female councilor is "seated" (i.e. appears in at least 10% of council meetings) during the subsequent term. Panel (b) plots the overall share of the council that is female. Panel (c) is identical to panel (b) except that it excludes the councilor involved in the close election when calculating the female share. It therefore serves as a natural "placebo test" that any effects we see must operate through the addition of a female councilor as opposed to other downstream changes in the composition of the other councilors.

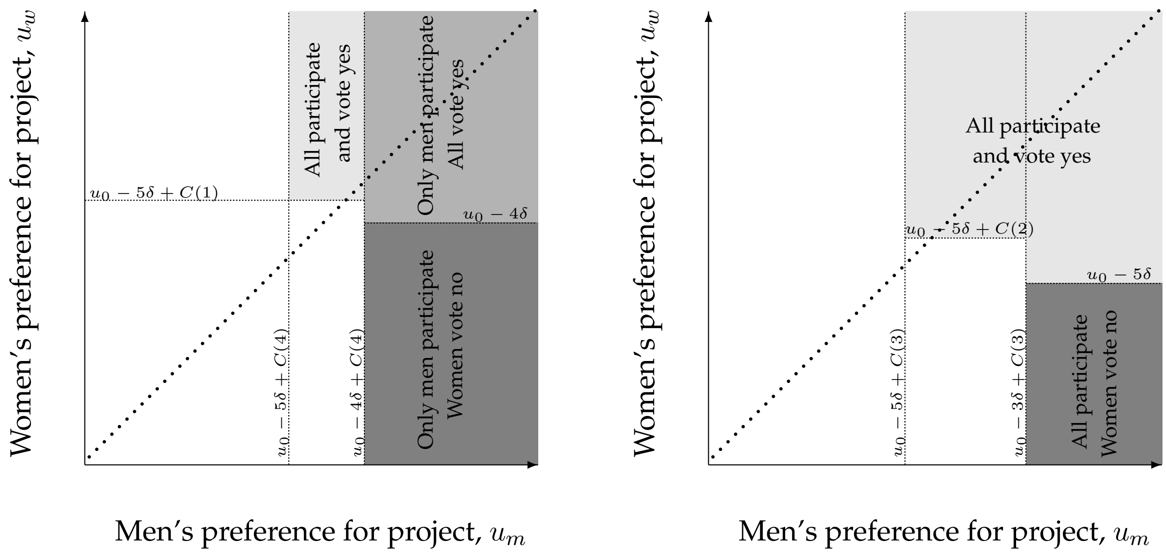
Figure 5: Discontinuities in Substantive Participation



(a) Share of Motions made by Women **(b)** Share by Non-focal Women ($N_w = 1$)

Note: This figure plots the running variable (vote-share normalized to be zero at the cutoff) against key outcomes from our meeting-minutes data within the RD bandwidth. Panel (a) displays results for the "Share of motions made by women" and is calculated using all council members. Panel (b) displays results for the "Share of motions made by Non-focal women" for the sample of councils where there is one female councilor not involved in the close election. In panel (b), shares are calculated using only non-focal (male and female) council members and hence identify a "pure" behavioral effect. Table 4 contains corresponding point estimates and standard errors. To address noise apparent in panel (b), in addition to the conventional methods of asymptotic inference presented in Table 4 (and Appendix Table C.9, which provides p-values that account for sampling variability from the bandwidth selection step), we also conduct a form of placebo inference for this outcome based on randomly permuting the vote margin across councils. We find a permutation p-value of 0.018 (see Appendix Figure C.1). Since permutation tests of this sort are exact in finite sample for a sharp null hypothesis that there is no treatment effect for *any* councils in our sample (Young, 2019), this permutation p-value provides strong evidence that the apparent discontinuity in panel (b) is not simply a result of sampling variation.

Figure 6: Preferences, Participation and Votes in a 5-Member Council



(a) $N_w = 1$: Before Critical Mass **(b)** $N_w = 2$: Critical Mass

Note: This figure illustrates comparative statics from the stylized theoretical model outlined in section 6. Panel (a) describes how female and male behavior varies with preferences for the underlying project in a council that is below the critical mass threshold. Panel (b) describes how female and male behavior varies with preferences for the underlying project in a council that has reached the critical mass threshold. The key comparative static illustrated by moving from panel (a) to panel (b) is that women's participation changes, but voting patterns do not, for projects that land in regions of the parameter space where the preferences of men and women are broadly aligned.

Table 1: Summary Statistics: Municipalities

	All	Election	Gendered Election	Minutes
Panel A: Demographics				
Total Population	9,949	63,601	71,455	68,317
Black (share)	7.8%	5%	5.3%	5.5%
Hispanic (share)	7.9%	29.5%	30.1%	24.2%
Income (per-capita)	24,321	31,224	30,937	30,702
Panel B: Spending (Millions)				
Total	26	122.7	140.4	126.5
Police and Fire	4.1	25.9	29.5	26.8
Utilities	4.1	25.6	29.3	27.1
Waste and Sewer	2.3	10.7	12.1	13.2
Panel C: Councils				
Council size		5.5	5.5	6.1
Races		1.7	2.1	2.3
Candidates		5.8	6.1	7
Female candidates		1.7	2.1	2.3
Observations (cities)	19,276	630	523	153

Note: Table 1 provides descriptive statistics for four samples of US cities. "All" corresponds to every city represented in the Annual Survey of City Government. "Election" corresponds to the sub-sample of cities where we were able to collect election data. "Gendered Election" corresponds to the sub-sample of cities where we were able to identify at least one election where the marginal winner and the marginal loser in the election were of different genders. "Minutes Sample" corresponds to the subset of cities where we were able to collect data on the internal workings of city council meetings. Panel A gives averages of census characteristics drawn from the ACS as measured 2012. Panel B gives averages of municipal spending from the Annual Survey of City Government as measured in 2012. Panel C gives averages of council characteristics compiled from the election data. Since there is substantial variation both in the time periods covered by our election data and in the years different municipalities actually hold elections, it is impossible to fix a common reference year for panel C. For that reason, we give the averages in panel C over all available time periods.

Table 2: Summary Statistics: Meeting Minutes

	All	1 Non-Focal	> 1 Non-Focal
Panel A: Process			
Attendance (share)	0.81	0.82	0.79
Number of meetings	45.3	45.2	43.4
Meeting length	146.7	138	160.7
Panel B: Motions			
Total	10.5	10.3	11.5
Moved by women (share)	0.3	0.26	0.43
Seconded by women (share)	0.35	0.3	0.5
Passed (share)	0.93	0.92	0.93
Passed Unanimously (share)	0.9	0.9	0.9
Failed (share)	0.07	0.08	0.07
Panel C: Topics			
Admin (share)	0.72	0.72	0.72
Finance (share)	0.19	0.19	0.2
Regulation (share)	0.10	0.10	0.09
Public Utility (share)	0.13	0.14	0.13
Observations (councils)	325	136	124

Note: This table presents summary statistics that describe our data on the internal workings of the city council for the sample of cities where we were able to collect it. "All" refers to all councils in this sample. "1 Non-Focal" refers to the subset of councils that, were it not for the focal woman involved in the close election, would otherwise have contained a single, token woman. "> 1 Non-Focal" contains the subset of councils that had multiple non-focal women. Numerical values in the table represent council averages. The variables in panel A are observable for every meeting of the council in the relevant term. So "Number of meetings" equal to 45.3 in the "All" column indicates that the average council met 45 times during its term (typically 2 years). "Attendance" is the average share of meetings attended by an individual councilor during a term. Panels B and C contain variables that were more costly to extract, and hence are only observable for 3 (randomly chosen) meetings per year. So for example, the variable "Total" indicates the average (across councils) number of motions made per meeting is equal to 10.5, and "Moved by women (share)" indicates that for an average meeting, 30% of these motions were made by women.

Table 3: Balance

	All	1 Non-Focal	> 1 Non-Focal
Council size	0.136	-0.329	0.512
	(0.470)	(0.373)	(0.997)
Term duration	0.093	0.084	0.209
	(0.106)	(0.182)	(0.181)
Turnover	-0.128*	-0.127	-0.272**
	(0.065)	(0.082)	(0.137)
Candidates	0.215	-1.047	2.317
	(0.895)	(1.277)	(1.557)
Female candidates	0.145	-0.765*	1.240**
	(0.316)	(0.435)	(0.543)
Races	0.002	-0.083	0.377
	(0.252)	(0.280)	(0.496)
Total population	8.734	-16.809	40.595
	(26.410)	(20.257)	(61.470)
Black (share)	0.021	-0.002	0.041
	(0.019)	(0.014)	(0.043)
Hispanic (share)	-0.027	0.005	-0.088
	(0.050)	(0.097)	(0.078)
Per capita income	-3.027	-3.401	4.114
	(4.887)	(9.546)	(5.491)
Joint P-value	0.209	0.332	0.179
Observations	325	136	124

Note: This table presents evidence in support of our identifying assumption for the entire sample (All), for the sub-sample of councils with only one non-focal member (1 Non-Focal), and for the sub-sample of councils with more than one non-focal member (>1 Non-Focal). Each number in this table is estimated using a separate regression and our preferred model (equation 1). Rows denote baseline and other exogenous council characteristics which were placed on the left hand side of model 1. Standard errors, clustered at the city level, are reported in parentheses. P-values from a joint test across outcomes are reported in the second to last row. "Observations" provides the number of councils contained in each column. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Impact on Nominal and Substantive Representation

	Full sample (1)	$\bar{Y}$ (2)	1 Non-Focal Woman (3)	$\bar{Y}$ (4)	> 1 Non-Focal Women (5)	$\bar{Y}$ (6)
Panel A: Nominal Representation						
Council female share	0.198***	0.235	0.199***	0.184	0.167***	0.400
	(0.037)		(0.010)		(0.042)	
Panel B: Substantive Representation						
Motions moved by women	1.700***	1.737	2.087***	1.469	1.674*	2.826
	(0.469)		(0.588)		(0.899)	
Share moved by women	0.247***	0.226	0.301***	0.182	0.156*	0.379
	(0.048)		(0.054)		(0.080)	
Share moved or seconded by women	0.335***	0.371	0.394***	0.338	0.217***	0.574
	(0.060)		(0.072)		(0.083)	
Bandwidth	0.076		0.055		0.070	
Observations	325		136		124	

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1). Rows denote different outcome variables, calculated using all council members, including those involved in the close election. Thus, results in this table incorporate both behavioral effects on the other councilors as well as the impact of replacing a man with a woman. Odd columns denote different sub-samples of interest. Even columns report dependent variable means for the sub-sample used in the preceding column. Standard errors, clustered by city, are reported in parentheses. Appendix tables C.1,C.6, and C.9 show that these results are robust to (1) selecting the data driven bandwidth outcome-by-outcome; (2) dropping controls from the model which are included only for precision (and are therefore not "design based"); and (3) adjusting the inferential procedure to account for additional sampling variability introduced in the bandwidth selection process. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Behavioral Effect: Impact on Non-Focal Councilors

	1 Non-Focal Woman				> 1 Non-Focal Women			
	Men	$\bar{Y}$	Women	$\bar{Y}$	Men	$\bar{Y}$	Women	$\bar{Y}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No. of Motions	-0.717	5.560	0.856**	1.469	0.687	4.806	0.467	2.826
	(0.906)		(0.362)		(0.881)		(0.765)	
Share moved by women			0.148**	0.219			-0.045	0.452
			(0.058)				(0.095)	
Share moved or seconded by women			0.219***	0.270			0.142	0.481
			(0.064)				(0.087)	
Bandwidth	0.055		0.055		0.070		0.070	
Observations	136		136		124		124	

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1). Rows denote different outcome variables, calculated using *only* those council members who were not involved in the close election (i.e. non-focal councilors). Thus, results in this table represent pure behavioral effects. Odd columns denote different sub-samples of interest. Even columns report dependent variable means for the sub-sample used in the preceding column. Standard errors, clustered by city, are reported in parentheses. Appendix tables C.2,C.7, and C.10 show that these results are robust to (1) selecting the data driven bandwidth outcome-by-outcome; (2) dropping controls from the model which are included only for precision (and are therefore not "design based"); and (3) adjusting the inferential procedure to account for additional sampling variability introduced in the bandwidth selection process. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Effects on Per Capita Municipal Spending

	Total	Public Utility	Health and Hospital	Parks and Recreation	Library	Housing and Com. Dev.	Airports and Water Ports	Police and Fire	Sewerage and Waste	Roads and Parking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: All councils										
RD estimate	-38.452	-2.012	6.023	-14.833	-5.115	19.289	16.717**	11.885	-41.357	-44.890
	(123.392)	(44.313)	(9.443)	(13.942)	(5.177)	(40.067)	(8.363)	(20.837)	(55.610)	(38.663)
$\bar{Y}$ =	1943.420	346.339	43.541	96.842	30.700	82.654	16.707	412.077	236.724	231.033
Bandwidth	0.075	0.075	0.075	0.075	0.075	0.075	0.075	0.075	0.075	0.075
Observations	361	361	319	359	336	361	317	348	319	322
Panel B: 1 non-focal women										
RD estimate	101.065	-19.421	-5.829	-21.668	-12.070**	103.363	17.964*	40.074	-0.296	-4.017
	(115.167)	(54.343)	(15.453)	(23.146)	(4.690)	(92.616)	(9.697)	(41.725)	(32.759)	(28.196)
$\bar{Y}$ =	2015.504	320.570	78.754	105.677	33.643	94.113	32.201	402.336	268.547	245.980
Bandwidth	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055
Observations	148	148	134	146	138	148	133	143	134	134
Panel C: >1 non-focal woman										
RD estimate	-147.084	-82.800	1.860	8.677	-8.021	-16.694	2.894	-2.103	-4.974	-16.891
	(128.751)	(66.792)	(1.807)	(13.891)	(10.932)	(19.533)	(2.997)	(14.655)	(38.811)	(25.149)
$\bar{Y}$ =	1944.416	347.455	17.268	88.327	33.561	70.150	2.730	431.536	196.802	206.931
Bandwidth	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070
Observations	137	137	123	137	129	137	123	131	123	124

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1). Columns denote different outcome variables, specifically per-capita municipal expenditure levels within the indicated category. Note that "missingness" in the expenditure data can vary by category within a city-year and hence there are slight differences in the sample (and corresponding sample size) used for estimation across columns. Panels denote different sub-samples of interest. Standard errors, clustered by city, are reported in parentheses. Appendix tables C.3,C.8, and C.11 show that these results are robust to (1) selecting the data driven bandwidth outcome-by-outcome; (2) dropping controls from the model which are included only for precision (and are therefore not "design based"); and (3) adjusting the inferential procedure to account for additional sampling variability introduced in the bandwidth selection process. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Effects on Political Composition for California Sub-sample

	Party Affiliation Outcomes				*Representation and Participation*	
	Democrat	Republican	Other	Unaffiliated	Council Female	Motions
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All councils						
RD estimate	0.090	-0.104	-0.042	0.056	0.236***	0.279***
	(0.107)	(0.121)	(0.030)	(0.055)	(0.055)	(0.070)
$\bar{Y}$ =	0.422	0.474	0.019	0.085	0.256	0.241
Bandwidth	0.076	0.076	0.076	0.076	0.076	0.076
Observations	116	116	116	116	116	116
Panel B: 1 non-focal women						
RD estimate	-0.078	0.018		0.061	0.202***	0.319***
	(0.229)	(0.204)		(0.076)	(0.023)	(0.093)
$\bar{Y}$ =	0.412	0.490	0.000	0.098	0.217	0.194
Bandwidth	0.055	0.055	0.055	0.055	0.055	0.055
Observations	49	49	49	49	49	49
Panel C: >1 non-focal woman						
RD estimate	0.200	-0.137	-0.094	0.032	0.168***	0.155
	(0.163)	(0.179)	(0.063)	(0.119)	(0.053)	(0.123)
$\bar{Y}$ =	0.500	0.333	0.046	0.120	0.448	0.434
Bandwidth	0.070	0.070	0.070	0.070	0.070	0.070
Observations	42	42	42	42	42	42

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1) and the California sub-sample, where we observe candidates' party affiliation (see Appendix A.1 for further detail on this data). Columns show results for share democrat, republican, other, and unaffiliated. Results for female representation and participation are included for comparison. Panels denote different sub-samples of interest by baseline female representation. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Effects on Voting Patterns

	Full sample	$\bar{Y}$	1 Non-Focal Woman	$\bar{Y}$	$>$ 1 Non-Focal Women	$\bar{Y}$
	(1)	(2)	(3)	(4)	(5)	(6)
Share unanimous	0.005	0.899	-0.015	0.894	0.011	0.905
	(0.026)		(0.052)		(0.033)	
Share rejected	0.005	0.023	0.013	0.019	0.005	0.028
	(0.008)		(0.011)		(0.017)	
Vote margin	-0.001	0.937	-0.008	0.937	-0.004	0.933
	(0.019)		(0.034)		(0.030)	
Votes in favor	0.007	5.974	-0.062	5.633	0.036	6.551
	(0.072)		(0.140)		(0.111)	
Votes against	0.003	0.160	0.010	0.154	0.027	0.173
	(0.044)		(0.068)		(0.076)	
Bandwidth	0.076		0.055		0.070	
Observations	325		136		124	

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1). Rows denote different outcome variables, calculated using all council members, including those involved in the close election. Thus, results in this table incorporate both behavioral effects on the other councilors as well as the impact of replacing a man with a woman. Odd columns denote different sub-samples of interest. Even columns report dependent variable means for the sub-sample used in the preceding column. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Gender Differences in Motion Topics

	Moved by men	Moved by women	Difference
Panel A: Topics Related to Spending Categories			
Public Utilities	0.140	0.128	0.012
			(0.014)
Health and Hospital	0.008	0.012	-0.004
			(0.003)
Parks and Recreation	0.037	0.027	0.009
			(0.007)
Library	0.007	0.005	0.002
			(0.002)
Housing and Community Development	0.019	0.021	-0.002
			(0.005)
Airports and Water Ports	0.002	0.002	0.000
			(0.001)
Police and Fire	0.028	0.028	0.000
			(0.006)
Sewerage and Waste	0.023	0.025	-0.002
			(0.005)
Road and Parking	0.058	0.064	-0.006
			(0.009)
Panel B: Other Common Topics			
Administration	0.710	0.714	-0.004
			(0.019)
Finance	0.183	0.197	-0.014
			(0.015)
Property	0.164	0.177	-0.014
			(0.015)
Regulation	0.096	0.106	-0.010
			(0.013)

Note: This table shows the distribution of motions on a given topic as the share of total motions presented separately for motions moved by male and female councilors. Topics are not mutually exclusive. Topics in panel A correspond to the categories analyzed for spending outcomes in Table 6. Panel B contains other frequent topics. Difference is calculated as share moved by men − share moved by women. Heteroskedacticity robust standard errors are contained in parentheses.

Table 10: Effects on Motion Topics

	Public Utility (1)	Health and Hospital (2)	Parks and Recreation (3)	Library (4)	Housing and Com Dev (5)	Airports and Water Ports (6)	Police and Fire (7)	Sewerage and Waste (8)	Roads and Parking (9)
Panel A: All councils									
RD estimate	-0.014	0.009	0.006	0.002	-0.013	-0.000	0.021**	0.008	-0.030**
	(0.022)	(0.008)	(0.009)	(0.003)	(0.010)	(0.001)	(0.009)	(0.012)	(0.014)
$\bar{Y} =$	0.130	0.009	0.033	0.004	0.026	0.002	0.022	0.024	0.062
Bandwidth	0.076	0.076	0.076	0.076	0.076	0.076	0.076	0.076	0.076
Observations	325	325	325	325	325	325	325	325	325
Panel B: 1 non-focal women									
RD estimate	-0.052	0.025**	0.010	0.001	-0.014	0.002	0.019	0.000	-0.042*
	(0.034)	(0.012)	(0.013)	(0.005)	(0.018)	(0.002)	(0.014)	(0.014)	(0.022)
$\bar{Y} =$	0.140	0.007	0.043	0.004	0.024	0.002	0.024	0.029	0.054
Bandwidth	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055
Observations	136	136	136	136	136	136	136	136	136
Panel C: >1 non-focal woman									
RD estimate	0.001	0.006	0.008	0.006	-0.004	-0.001	0.016	0.017	-0.033
	(0.034)	(0.013)	(0.014)	(0.005)	(0.017)	(0.001)	(0.014)	(0.025)	(0.021)
$\bar{Y} =$	0.120	0.009	0.022	0.006	0.030	0.001	0.021	0.022	0.063
Bandwidth	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070
Observations	124	124	124	124	124	124	124	124	124

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1). Columns denote different outcome variables, specifically the share of motions corresponding to the indicated category. These shares are calculated using motions moved by all council members, including those involved in the close election. Thus, results in this table incorporate both behavioral effects on the other councilors as well as the impact of replacing a man with a woman. Panels denote different sub-samples of interest. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix to "Gender Composition and Group Behavior: Evidence from US City Councils"

by Emilia Brito, Jesse Bruhn, Thea How Choon, and E. Anna Weber

A Data Appendix

A.1 Additional details of election data

This section describes the creation and cleaning of the FOIA election data sample for cities not in California used in the main analysis of this paper.

We began by identifying a nationally-representative list of municipalities beginning from the U.S. Census Bureau's "Annual Survey of State and Local Government Finances." We included all local governments with a type listed as "city" which were part of the 2008-2011 sub-sample to ensure at least six consecutive years of government spending data would be available.[37] This resulted in a list of 3,512 municipalities, 3,369 of which were not in California.

Our next step was to identify contact information for local election officials for each municipality. We obtained the name and email address of election officials as listed on public city websites. To assess the feasibility of obtaining this information, for a small number of states, the authors and research assistants searched online for this information themselves. We then hired workers on Amazon mTurk to obtain the rest of the contact information in a similar way.[38] We obtained potential contact information for 2,790, or 82.8% of the potential municipality sample among states other than California.

To all identified potential election officials, research assistants sent FOIA requests in 2017 requesting all council election returns since 2008. The exact text of the request was:

> My name is [researcher]. I am a graduate student at Boston University working on gender issues in city government. Our project aims to measure the impact of female representation on public policy.

[37] According to the census "In years ending in '2' and '7' the entire universe is canvassed. In intervening years, a sample of the population of interest is surveyed."

[38] mTurk workers were provided with the instructions "I provide you with a city and state. You will use google to find the e-mail address and name of the city clerk." Workers were paid per city. We made three requests for each city, and if a city returned different responses, we emailed all of the identified potential contacts.

This is a Freedom of Information Act request for the following information about municipal elections in [city]:

- The date of each city council, city manager, or mayoral election that has occurred in the city since 2008
- The name and gender of each individual who ran for office in each election and the vote totals they received.
- The name of each individual elected.
- In the case of city councils, the number of male and female councilors before and after the election.

Thank you for your help! Please email me if you have any questions.

504 or 18.1% of cities to whom we sent a request responded in some form. 82 or 16.2% of responses did not contain valid election data because either the city did not consider the request to be a valid open records request and did not provide any data, vote totals were not included, winners were not clearly identified in the documents, or full councilor names were not provided. This results in a set of 422 cities which responded with usable election data.

The authors and research assistants manually entered data from provided documents in an ad-hoc fashion. It was not possible to systematize this process as data was provided in a wide variety of formats including Excel spreadsheets, photocopies of election returns, and hand-written notes on vote counts. Ultimately, we were able to digitize data from approximately 223 non-California cities which provided data. For each municipal council race in these cities, we recorded the names of all candidates, vote counts for each candidate if the race was contested, and the winner(s) of each race. Our election data sample covers 6.9% of the originally identified set of 3,369 non-California municipalities.

For cities in California, we rely on public data from the California Election Data Archive "CEDA" which covers all municipalities in the state. We combine data from these two sources to form our analysis sample.

Our research design relies on comparing close elections between men and women. Since the raw election data does not contain gender, we infer candidates' genders using first names. Using information from the Social Security Administration, we calculate for each first name the share of babies born between 1950 and 2000 given that name which are male. If more than 99% are male, we assign any candidates with that name as male.

If less than 1% are male, we assign any candidates as female. For candidates in contested elections whose names fell within an intermediate range, we conducted internet searches to establish candidate gender from news articles or photographs. For a handful of cities, the gender of councilors was provided in the response to the FOIA request, in which case we rely on this information.

A.2 Additional details of minutes data

This section describes the process for collecting and digitizing data on city council meetings.

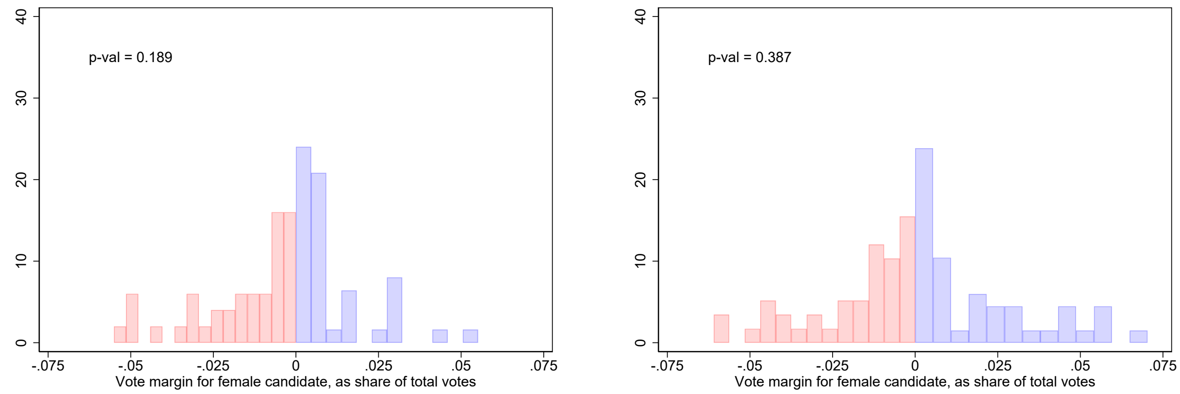
The authors and research assistants manually downloaded meeting minutes files as available online from city council websites for the period overlapping with the study period. We collected meeting minutes for a set of 274 cities. We downloaded documents for all years overlapping with the study period for which documents were available. To maximize estimation power within time and budget constraints, we prioritized collecting meeting minutes for cities with a particularly close cross-gender election. Across all cities in our election data sample, the average vote differential in the closest cross-gender election was 4.46%; in the sub-sample for which we collected minutes, the average differential was 2.85%.

To digitize these collected meeting minutes, we engaged Hi-Tech iSolutions to manually enter data from the files.[39] We ultimately digitized the meeting minutes of 218 cities, and the average vote differential among digitized cities was 3.2%. For each of these cities, for every meeting for which we had collected minutes, the data entry firm recorded the meeting date, start time, end time, the councillors present at the meeting, and those recorded as absent if available. Then, for three randomly selected meetings per city-year, we requested information on every motion made during the meeting. For each motion, the data entry firm recorded the date of the meeting where the motion occurred, a brief description of the topic of the motion, the councillor who made the motion, the councillor who seconded the motion, and the names and count of councillors who voted for, voted against, or abstained from the vote on the motion.

[39] It was not feasible to automate the digitization of the meeting minutes because the wide variety of file naming conventions and document structure would make automation difficult.

B Density Tests

Figure B.1: Density of Councils around RD Threshold by Baseline Female Representation



(a) 1 Non-Focal Woman **(b)** > 1 Non-Focal Women

Note: This figure plots the density of cities by the vote margin of the female candidate in the cross-gender election for two sub-samples: councils with only one non-focal woman in panel (a), and councils with more than one non-focal women in panel (b). The p-values represent the result of a formal test for continuity of density across the threshold using the RDRobust package as described in Calonico et al. (2017). We do not find evidence of manipulation in either sample.

Table B.1: Density Tests

	Full Sample	1 Non-Focal Woman	>1 Non-Focal Women
	(1)	(2)	(3)
t-stat	1.138	1.313	0.864
p-val	0.255	0.189	0.387
Bandwidth	0.076	0.055	0.070
Observations	325	136	124

Note: This table shows the result of a formal test for continuity of density across the threshold using the RDRobust package as described in Calonico et al. (2017) for the full sample and two sub-samples: councils with only one non-focal woman, and councils with more than one non-focal women. We do not find evidence of manipulation.

C Robustness Checks

Data driven bandwidth selection, outcome-by-outcome

Table C.1: Impact on Nominal and Substantive Representation – Data driven bandwidth outcome-by-outcome

	Full sample (1)	$\bar{Y}$ (2)	1 Non-Focal Woman (3)	$\bar{Y}$ (4)	>1 Non-Focal Women (5)	$\bar{Y}$ (6)
Panel A: Nominal Representation						
Council female share	0.198*** (0.036)	0.235	0.197*** (0.010)	0.184	0.167*** (0.042)	0.400
Panel B: Substantive Representation						
Motions moved by women	1.734*** (0.471)	1.737	1.541*** (0.570)	1.469	1.672* (0.897)	2.826
Share moved by women	0.250*** (0.049)	0.226	0.276*** (0.051)	0.182	0.155* (0.080)	0.379
Share moved or seconded by women	0.333*** (0.059)	0.371	0.341*** (0.066)	0.338	0.218*** (0.082)	0.574

Note: This table is identical to table 4 in the main text, except that we use the methods described in Calonico et al. (2017) to select a data-driven bandwidth outcome-by-outcome, rather than fixing it to be common across outcomes within the same column. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table C.2: Behavioral Effect: Impact on Non-Focal Councilors – Data driven bandwidth outcome-by-outcome

	1 Non-Focal Woman				>1 Non-Focal Women			
	Men	$\bar{Y}$	Women	$\bar{Y}$	Men	$\bar{Y}$	Women	$\bar{Y}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No. of Motions	-0.604	5.560	0.934**	1.469	0.850	4.806	0.373	2.826
	(0.850)		(0.366)		(0.898)		(0.777)	
Share moved by women			0.148**	0.219			-0.045	0.452
			(0.058)				(0.094)	
Share moved or seconded by women			0.180***	0.270			0.134	0.481
			(0.058)				(0.089)	

Note: This table is identical to table 5 in the main text, except that we use the methods described in Calonico et al. (2017) to select a data-driven bandwidth outcome-by-outcome, rather than fixing it to be common across outcomes within the same column. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table C.3: Effects on Per Capita Municipal Spending – Data driven bandwidth outcome-by-outcome

	Total	Public Utility	Health and Hospital	Parks and Recreation	Library	Housing and Com. Dev.	Airports and Water Ports	Police and Fire	Sewerage and Waste	Roads and Parking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: All councils										
RD estimate	-49.540	-2.336	3.676	-15.478	-7.736**	18.489	15.610**	12.903	-31.964	-42.428
	(108.304)	(44.028)	(8.530)	(14.180)	(3.693)	(37.898)	(7.905)	(21.777)	(41.813)	(37.308)
$\bar{Y}$ =	1943.420	346.339	43.541	96.842	30.700	82.654	16.707	412.077	236.724	231.033
Observations	361	361	319	359	336	361	317	348	319	322
Panel B: 1 non-focal women										
RD estimate	114.272	-10.417	-9.173	-20.054	-10.654**	107.051	19.237*	64.723	-4.815	-16.177
	(110.508)	(50.743)	(14.631)	(21.653)	(4.302)	(93.687)	(10.169)	(46.138)	(32.578)	(28.127)
$\bar{Y}$ =	2015.504	320.570	78.754	105.677	33.643	94.113	32.201	402.336	268.547	245.980
Observations	148	148	134	146	138	148	133	143	134	134
Panel C: >1 non-focal woman										
RD estimate	-153.590	-82.377	0.714	2.357	-8.773	-16.776	-0.623	-1.604	-4.933	-15.722
	(128.684)	(66.467)	(1.772)	(15.560)	(8.833)	(19.560)	(2.077)	(13.769)	(37.679)	(25.263)
$\bar{Y}$ =	1944.416	347.455	17.268	88.327	33.561	70.150	2.730	431.536	196.802	206.931
Observations	137	137	123	137	129	137	123	131	123	124

Note: This table is identical to table 6 in the main text, except that we use the methods described in Calonico et al. (2017) to select a data-driven bandwidth outcome-by-outcome, rather than fixing it to be common across outcomes within the same column. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Specifications with alternative bandwidths, alternative functional forms, alternative approaches to inference, and that exclude control variables

Table C.4: Impact on Nominal and Substantive Representation – Alternative Specifications

	Full Sample			1 Non-Focal Woman			> 1 Non-Focal Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Bandwidth = 0.05									
Council female share	0.197***	0.189***	0.204***	0.200***	0.207***	0.214***	0.159***	0.120**	0.075
	(0.039)	(0.053)	(0.064)	(0.011)	(0.015)	(0.018)	(0.043)	(0.054)	(0.058)
No. of Motions Moved by Women	1.851***	1.795**	2.325**	2.226***	2.964***	2.885**	1.360	0.299	0.835
	(0.514)	(0.777)	(0.953)	(0.607)	(0.907)	(1.123)	(0.983)	(1.320)	(1.612)
Share moved by women	0.255***	0.258***	0.335***	0.315***	0.390***	0.394***	0.117	0.031	0.096
	(0.053)	(0.077)	(0.095)	(0.056)	(0.084)	(0.109)	(0.085)	(0.113)	(0.132)
Share moved or seconded by women	0.343***	0.321***	0.377***	0.409***	0.463***	0.505***	0.186**	0.056	0.043
	(0.066)	(0.093)	(0.118)	(0.076)	(0.105)	(0.134)	(0.088)	(0.114)	(0.134)
Panel B: Bandwidth = 0.075									
Council female share	0.198***	0.195***	0.192***	0.199***	0.202***	0.223***	0.167***	0.145***	0.106*
	(0.037)	(0.045)	(0.058)	(0.010)	(0.013)	(0.017)	(0.041)	(0.049)	(0.057)
Motions moved by women	1.703***	1.932***	1.814**	1.512***	2.821***	2.809***	1.701*	0.767	0.169
	(0.469)	(0.631)	(0.858)	(0.574)	(0.742)	(1.056)	(0.888)	(1.179)	(1.466)
Share moved by women	0.248***	0.267***	0.263***	0.249***	0.369***	0.412***	0.157**	0.083	0.011
	(0.048)	(0.063)	(0.085)	(0.050)	(0.065)	(0.096)	(0.079)	(0.099)	(0.123)
Share moved or seconded by women	0.336***	0.349***	0.323***	0.348***	0.461***	0.486***	0.218***	0.136	0.019
	(0.060)	(0.078)	(0.103)	(0.066)	(0.087)	(0.117)	(0.082)	(0.101)	(0.125)
Panel C: Bandwidth = 0.1									
Council female share	0.197***	0.199***	0.188***	0.198***	0.203***	0.207***	0.171***	0.158***	0.127**
	(0.036)	(0.043)	(0.052)	(0.009)	(0.011)	(0.015)	(0.040)	(0.046)	(0.054)
Motions moved by women	1.649***	1.805***	1.905**	1.373**	2.146***	3.245***	1.736**	1.315	0.201
	(0.457)	(0.560)	(0.752)	(0.573)	(0.674)	(0.897)	(0.840)	(1.083)	(1.351)
Share moved by women	0.235***	0.270***	0.263***	0.224***	0.331***	0.406***	0.153**	0.134	0.039
	(0.047)	(0.057)	(0.075)	(0.050)	(0.057)	(0.077)	(0.076)	(0.091)	(0.112)
Share moved or seconded by women	0.328***	0.350***	0.342***	0.328***	0.422***	0.498***	0.219***	0.182*	0.076
	(0.057)	(0.071)	(0.091)	(0.065)	(0.078)	(0.099)	(0.078)	(0.094)	(0.115)
Polynomial	1	2	3	1	2	3	1	2	3
Observations	325	325	325	136	136	136	124	124	124

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1), except that we manually vary the bandwidth and/or the order of the polynomial. Rows denote different outcome variables, calculated using all council members, including those involved in the close election. Thus, results in this table incorporate both behavioral effects on the other councilors as well as the impact of replacing a man with a woman. Panels denote different bandwidths around the threshold, and numbered columns denote polynomials of different order for F. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table C.5: Behavioral Effect: Impact on Non-Focal Female Councilors – Alternative Specifications

	1 Non-Focal Woman			> 1 Non-Focal Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Bandwidth = 0.05						
No. of Motions Moved by Women	0.979***	1.303**	0.736	0.157	-1.052	-1.263
	(0.376)	(0.516)	(0.516)	(0.821)	(1.112)	(1.317)
Share moved by women	0.165***	0.231**	0.218*	-0.101	-0.224	-0.205
	(0.061)	(0.092)	(0.118)	(0.104)	(0.145)	(0.173)
Share moved or seconded by women	0.228***	0.209**	0.212*	0.098	-0.050	-0.084
	(0.067)	(0.096)	(0.120)	(0.091)	(0.113)	(0.133)
Panel B: Bandwidth = 0.075						
No. of Motions moved by women	0.415	1.411***	1.129**	0.493	-0.508	-1.376
	(0.381)	(0.456)	(0.568)	(0.756)	(0.979)	(1.235)
Share moved by women	0.086	0.226***	0.246**	-0.042	-0.164	-0.256
	(0.053)	(0.072)	(0.104)	(0.093)	(0.126)	(0.158)
Share moved or seconded by women	0.173***	0.264***	0.210*	0.145*	0.034	-0.095
	(0.058)	(0.079)	(0.108)	(0.086)	(0.102)	(0.123)
Panel C: Bandwidth = 0.1						
No. of Motions Moved by Women	0.279	0.960**	1.677***	0.565	0.036	-1.141
	(0.400)	(0.411)	(0.549)	(0.721)	(0.920)	(1.124)
Share moved by women	0.059	0.175***	0.268***	-0.037	-0.092	-0.225
	(0.053)	(0.063)	(0.085)	(0.087)	(0.114)	(0.145)
Share moved or seconded by women	0.152***	0.235***	0.282***	0.164**	0.087	-0.027
	(0.057)	(0.069)	(0.091)	(0.082)	(0.097)	(0.113)
Polynomial	1	2	3	1	2	3
Observations	136	136	136	124	124	124

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1), except that we manually vary the bandwidth and/or the order of the polynomial. Rows denote different outcome variables, calculated using *only* those council members who were not involved in the close election (i.e. non-focal councilors). Thus, results in this table represent pure behavioral effects. Panels denote different bandwidths around the threshold, and numbered columns denote polynomials of different order for F. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table C.6: Impact on Nominal and Substantive Representation – No controls

	Full sample	$\bar{Y}$	1 Non-Focal Woman	$\bar{Y}$	$>$ 1 Non-Focal Women	$\bar{Y}$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Nominal Representation						
Council female share	0.194***	0.235	0.216***	0.184	0.138**	0.400
	(0.038)		(0.022)		(0.059)	
Panel B: Substantive Representation						
Motions moved by women	1.704***	1.737	2.225***	1.469	1.607*	2.826
	(0.469)		(0.586)		(0.888)	
Share moved by women	0.243***	0.226	0.319***	0.182	0.132	0.379
	(0.049)		(0.056)		(0.086)	
Share moved or seconded by women	0.334***	0.371	0.418***	0.338	0.194**	0.574
	(0.060)		(0.072)		(0.095)	
Bandwidth	0.076		0.055		0.070	
Observations	325		136		124	

Note: This table is identical to table 4 in the main text, except that we omit those controls which are included for precision in our baseline specification. Thus, the results in this table are estimated using only controls which are "design-based" i.e. by controlling for the running variable (linear and piecewise around the cutoff) and nothing else. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table C.7: Behavioral Effect: Impact on Non-Focal Councilors – No controls

	1 Non-Focal Woman				> 1 Non-Focal Women			
	Men	$\bar{Y}$	Women	$\bar{Y}$	Men	$\bar{Y}$	Women	$\bar{Y}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No. of Motions	-0.943	5.560	0.916***	1.469	1.096	4.806	0.407	2.826
	(0.998)		(0.349)		(1.386)		(0.749)	
Share moved by women			0.163***	0.219			-0.071	0.452
			(0.059)				(0.100)	
Share moved or seconded by women			0.233***	0.270			0.141	0.481
			(0.064)				(0.089)	
Bandwidth	0.055		0.055		0.070		0.070	
Observations	136		136		124		124	

Note: This table is identical to table 5 in the main text, except that we omit those controls which are included for precision in our baseline specification. Thus, the results in this table are estimated using only controls which are "design-based" i.e. by controlling for the running variable (linear and piecewise around the cutoff) and nothing else. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table C.8: Effects on Per Capita Municipal Spending – No Controls

	Total	Public Utility	Health and Hospital	Parks and Recreation	Library	Housing and Com. Dev.	Airports and Water Ports	Police and Fire	Sewerage and Waste	Roads and Parking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: All councils										
RD estimate	-196.340	-78.440	-137.983	-18.110	-6.798	25.472	-7.709	22.705	-95.895	-109.516*
	(497.427)	(132.417)	(141.888)	(37.602)	(20.827)	(61.446)	(28.952)	(119.164)	(93.986)	(65.476)
$\bar{Y}$ =	1943.420	346.339	43.541	96.842	30.700	82.654	16.707	412.077	236.724	231.033
Bandwidth	0.075	0.075	0.075	0.075	0.075	0.075	0.075	0.075	0.075	0.075
Observations	361	361	319	359	336	361	317	348	319	322
Panel B: 1 non-focal women										
RD estimate	-531.216	-59.016	-543.224	-18.112	-6.521	85.314	-72.692	85.539	-104.107	-124.673
	(953.387)	(129.974)	(509.010)	(75.948)	(41.650)	(119.385)	(81.066)	(251.081)	(133.304)	(110.131)
$\bar{Y}$ =	2015.504	320.570	78.754	105.677	33.643	94.113	32.201	402.336	268.547	245.980
Bandwidth	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055
Observations	148	148	134	146	138	148	133	143	134	134
Panel C: >1 non-focal woman										
RD estimate	-162.699	-194.717	12.670	-30.494	-18.882	-47.571	16.720	-42.774	-45.496	-127.508**
	(703.459)	(244.355)	(14.962)	(34.288)	(23.998)	(36.288)	(14.892)	(87.849)	(124.275)	(58.127)
$\bar{Y}$ =	1944.416	347.455	17.268	88.327	33.561	70.150	2.730	431.536	196.802	206.931
Bandwidth	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070
Observations	137	137	123	137	129	137	123	131	123	124

Note: This table is identical to table 6 in the main text, except that we omit those controls which are included for precision in our baseline specification. Thus, the results in this table are estimated using only controls which are "design-based" i.e. by controlling for the running variable (linear and piecewise around the cutoff) and nothing else. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table C.9: Impact on Nominal and Substantive Representation – Bias Corrected P-values

	Full sample (1)	$\bar{Y}$ (2)	1 Non-Focal Woman (3)	$\bar{Y}$ (4)	> 1 Non-Focal Women (5)	$\bar{Y}$ (6)
Panel A: Nominal Representation						
Council female share	0.198***	0.235	0.199***	0.184	0.167***	0.400
	(0.037)		(0.010)		(0.042)	
Bias corrected p-value =	[0.000]		[0.000]		[0.004]	
Panel B: Substantive Representation						
Motions moved by women	1.700***	1.737	2.087***	1.469	1.674	2.826
	(0.469)		(0.588)		(0.899)	
Bias corrected p-value =	[0.002]		[0.001]		[0.545]	
Share moved by women	0.247***	0.226	0.301***	0.182	0.156	0.379
	(0.048)		(0.054)		(0.080)	
Bias corrected p-value =	[0.000]		[0.000]		[0.464]	
Share moved or seconded by women	0.335***	0.371	0.394***	0.338	0.217***	0.574
	(0.060)		(0.072)		(0.083)	
Bias corrected p-value =	[0.000]		[0.000]		[0.202]	
Bandwidth	0.076		0.055		0.070	
Observations	325		136		124	

Note: This table is identical to table 4 in the main text, except that we additionally report p-values that correct for asymptotic bias introduced when selecting the optimal bandwidth in square brackets. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table C.10: Behavioral Effect: Impact on Non-Focal Councilors – Bias Corrected P-values

	1 Non-Focal Woman				> 1 Non-Focal Women			
	Men	$\bar{Y}$	Women	$\bar{Y}$	Men	$\bar{Y}$	Women	$\bar{Y}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No. of Motions	-0.717	5.560	0.856**	1.469	0.687	4.806	0.467	2.826
	(0.906)		(0.362)		(0.881)		(0.765)	
Bias corrected p-value =	[0.404]		[0.006]		[0.121]		[0.569]	
Share moved by women			0.148**	0.219			-0.045	0.452
			(0.058)				(0.095)	
Bias corrected p-value =			[0.010]				[0.181]	
Share moved or seconded by women			0.219***	0.270			0.142	0.481
			(0.064)				(0.087)	
Bias corrected p-value =			[0.013]				[0.799]	
Bandwidth	0.055		0.055		0.070		0.070	
Observations	136		136		124		124	

Note: This table is identical to table 5 in the main text, except that we additionally report p-values that correct for asymptotic bias introduced when selecting the optimal bandwidth in square brackets. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

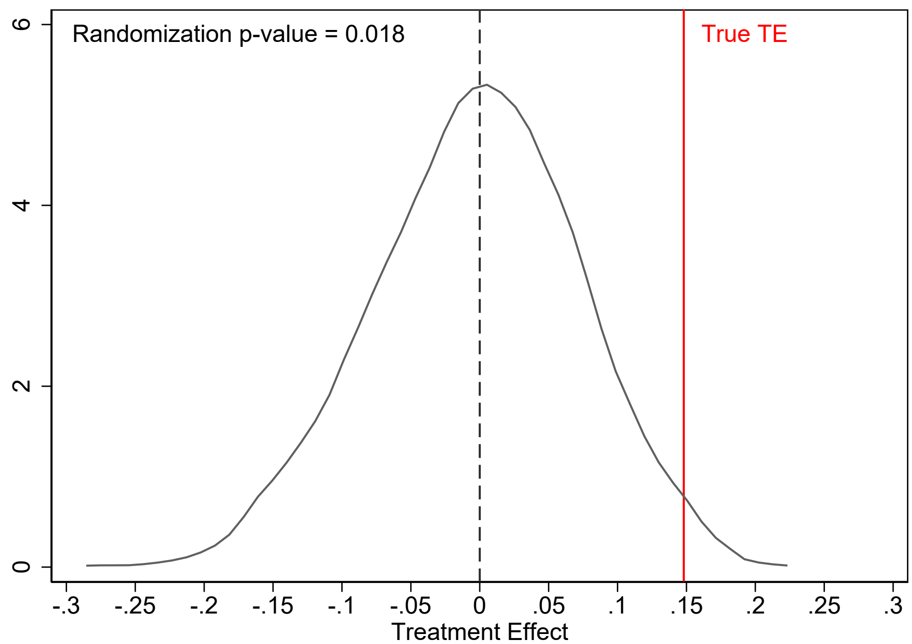
Table C.11: Effects on Per Capita Municipal Spending – Bias Corrected P-values

	Total	Public Utility	Health and Hospital	Parks and Recreation	Library	Housing and Com. Dev.	Airports and Water Ports	Police and Fire	Sewerage and Waste	Roads and Parking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: All councils										
RD estimate	-38.452	-2.012	6.023	-14.833	-5.115	19.289	16.717**	11.885	-41.357	-44.890
	(123.392)	(44.313)	(9.443)	(13.942)	(5.177)	(40.067)	(8.363)	(20.837)	(55.610)	(38.663)
Bias corrected p-value =	[0.692]	[0.992]	[0.456]	[0.167]	[0.122]	[0.626]	[0.114]	[0.498]	[0.386]	[0.194]
$\bar{Y} =$	1943.420	346.339	43.541	96.842	30.700	82.654	16.707	412.077	236.724	231.033
Bandwidth	0.075	0.075	0.075	0.075	0.075	0.075	0.075	0.075	0.075	0.075
Observations	361	361	319	359	336	361	317	348	319	322
Panel B: 1 non-focal women										
RD estimate	101.065	-19.421	-5.829	-21.668	-12.070**	103.363	17.964*	40.074	-0.296	-4.017
	(115.167)	(54.343)	(15.453)	(23.146)	(4.690)	(92.616)	(9.697)	(41.725)	(32.759)	(28.196)
Bias corrected p-value =	[0.528]	[0.715]	[0.246]	[0.375]	[0.013]	[0.176]	[0.054]	[0.146]	[0.661]	[0.245]
$\bar{Y} =$	2015.504	320.570	78.754	105.677	33.643	94.113	32.201	402.336	268.547	245.980
Bandwidth	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055
Observations	148	148	134	146	138	148	133	143	134	134
Panel C: >1 *non-focal woman*										
RD estimate	-147.084	-82.800	1.860	8.677	-8.021	-16.694	2.894	-2.103	-4.974	-16.891
	(128.751)	(66.792)	(1.807)	(13.891)	(10.932)	(19.533)	(2.997)	(14.655)	(38.811)	(25.149)
Bias corrected p-value =	[0.373]	[0.465]	[0.773]	[0.860]	[0.167]	[0.346]	[0.922]	[0.736]	[0.995]	[0.794]
$\bar{Y} =$	1944.416	347.455	17.268	88.327	33.561	70.150	2.730	431.536	196.802	206.931
Bandwidth	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070	0.070
Observations	137	137	123	137	129	137	123	131	123	124

Note: This table is identical to table 6 in the main text, except that we additionally report p-values that correct for asymptotic bias introduced when selecting the optimal bandwidth in square brackets. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

C.1 Additional validity checks

Figure C.1: Permutation Test



Note: This figure shows the distribution of "placebo" treatment effects obtained from 1,000 random permutations of the running variable for the sample of councils with 1 non-focal women. The red vertical line shows the true treatment effect (0.148). "Placebo" treatment effects are centered around 0 and only 1.8% of them fall to the right of the true treatment effect. This number is included in the figure as the randomization p-value.

Table C.12: Balance on Availability of Party Affiliation Information

	Full sample	1 Non-Focal Woman	>1 Non-Focal Women
	(1)	(2)	(3)
RD estimate	0.046	0.144	0.052
	(0.059)	(0.123)	(0.080)
$\bar{Y}=$	0.304	0.303	0.316
Bandwidth	0.076	0.055	0.070
Observations	154	66	54

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1) for the California sub-sample, where we observe party affiliation for a subset of candidates (see Appendix A.1 for further detail on this data). Observing a candidate's party affiliation is the outcome variable. Columns show results for different sub-samples of interest by baseline female representation. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

D Additional results

Table D.1: RD Effects on Administrative Motions

	All	Women	Non-focal Women
	(1)	(2)	(3)
Panel A: All councils			
RD estimate	-0.008	-0.074	-0.018
	(0.030)	(0.058)	(0.059)
$\bar{Y}$ =	0.717	0.700	0.700
Bandwidth	0.076	0.076	0.076
Observations	325	266	217
Panel B: 1 non-focal women			
RD estimate	-0.040	-0.088	-0.037
	(0.048)	(0.119)	(0.124)
$\bar{Y}$ =	0.724	0.684	0.684
Bandwidth	0.055	0.055	0.055
Observations	136	114	102
Panel C: >1 non-focal woman			
RD estimate	0.100**	0.049	0.026
	(0.048)	(0.052)	(0.055)
$\bar{Y}$ =	0.699	0.717	0.717
Bandwidth	0.070	0.070	0.070
Observations	124	118	115

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1). Columns denote different outcome variables related to the frequency with which different groups make administrative motions. The column labeled "All" denotes the share of all motions offered which have an administrative function. The column labeled "Women" denotes the share of all motions offered by female councilors which have an administrative function. The column labeled "Non-focal women" denotes the share of all motions offered by female councilors who were not involved in a close election which have an administrative function. Panels denote different sub-samples of interest. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table D.2: Impact on Council Tenure

	Full sample	$\bar{Y}$	1 Non-Focal Woman	$\bar{Y}$	$>$ 1 Non-Focal Women	$\bar{Y}$
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure	-0.032	1.279	0.176	1.182	-0.363	1.463
	(0.157)		(0.251)		(0.246)	
Bandwidth	0.076		0.055		0.070	
Observations	325		136		124	

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1) with average tenure (i.e. terms served on council) as the outcome variable. Odd columns denote different sub-samples of interest. Even columns report dependent variable means for the sub-sample used in the preceding column. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Results for councils with zero non-focal women

Table D.3: Impact on Nominal and Substantive Representation: Councils with 0 Non-Focal Women

	0 Non-Focal Women (1)	$\bar{Y}$ (2)
Panel A: Nominal Representation		
Council female share	0.202***	0.000
	(0.011)	
Panel B: Substantive Representation		
Motions moved by women	1.718***	0.000
	(0.351)	
Share moved by women	0.433***	0.000
	(0.128)	
Share moved or seconded by women	0.504***	0.000
	(0.089)	
Bandwidth	0.066	
Observations	65	

Note: Each number in this table is a treatment effect estimate generated from a separate regression using our preferred model (equation 1). Rows denote different outcome variables, calculated using all council members, including those involved in the close election. Standard errors, clustered by city, are reported in parentheses. Column 2 reports dependent variable means for the 0 Non-Focal Women sub-sample. Means are all 0 as control councils have 0 female councilors. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Motions and Spending

Table D.4: Motion Topics and Total Expenditure per capita by Category (Topic-by-Topic)

	Public Utility (1)	Health and Hospital (2)	Parks and Recreation (3)	Library (4)	Housing and Com. Dev. (5)	Airports and Water Ports (6)	Police and Fire (7)	Sewerage and Waste (8)	Roads and Parking (9)
Share of motions related to topic	205.452	-748.998**	73.847	497.882**	417.533*	584.884**	-113.304	434.135**	278.365*
	(208.180)	(330.612)	(88.531)	(229.478)	(248.101)	(274.511)	(165.214)	(177.094)	(162.814)
Observations	858	794	856	817	858	791	840	794	800

Note: This table displays coefficients (β) from separate univariate regressions of the form $Y_c = \alpha + \beta X_c + \epsilon_c$ where Y_c is expenditures per capita in a given category for council c, and X_c is the share of motions proposed in council c that are related to that same category. Columns denote different categories of expenditure. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

Table D.5: Motion Topics and Total Expenditure per capita by Category (Averages)

	(1)	(2)	(3)	(4)
Share of Motions	763.274***	792.829***		
	(152.344)	(135.362)		
Share of Motions by Women			638.317***	767.752***
			(171.743)	(150.998)
Observations	3,330	3,330	3,330	3,330
City FE		Y		Y

Note: This table displays coefficients (β) from a regression of the form $Y_{ck} = \alpha_{j(c)} + \beta X_{ck} + \epsilon_{ck}$ where Y_{ck} is expenditures per capita in category k for council c, $\alpha_{j(c)}$ is a fixed effect for city $j = j(c)$, and X_{ck} is the share of motions proposed in council c that are related to category k. Odd columns do not include the city fixed effect and hence describe the "average" relationship between motion shares and expenditures across all categories. Even columns do include the city fixed effect and hence highlight how *within city* changes in the share of motions related to a given category are related to *within city* changes in expenditures per capita in that same category. Rows distinguish between all motions and results for motions made by women. Standard errors, clustered by city, are reported in parentheses. Stars denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1.

E Detailed Results From Stylized Model

E.1 Backward Induction

In this section, we describe the process of backward induction which leads us to the equilibrium of the game: beginning with the final voting stage, then proceeding backwards to men's, then women's, decisions on whether to participate. In equilibrium, each councilor's participation decision depends on the number of participants thus far, the number of councilors who have yet to decide, and the minimum number of participants needed to lead a project to a successful outcome.

Final Voting Stage

In the final voting stage, the motion passes iff men vote yes, i.e.

$$u_m + \sum_j D_j \delta_j \geq u_0$$

Never-Participating Conditions

Proceeding backwards to the last man's participation decisions, let us consider the following parameter conditions:

Case 1: $u_m + N\delta < u_0$

The motion will fail no matter what, so everyone never participates.

Case 2: $u_m + N\delta - C(N_m) < u_0 \leq u_m + N\delta$

If everyone else has participated, the last man is pivotal[40] but decides not to participate. If not everyone else has participated, the motion will fail no matter what, so the last man does not participate. Since last man will never participate, the motion will fail no matter what, so everyone else never participates, regardless of history.

Men's Participation Decisions

Case 3: $u_m + N\delta - C(N_m) \geq u_0$

[40] We refer to councillor i's participation as being pivotal after a given history if, according to the equilibrium strategies, the project will pass if and only if councillor i participates.

Then there exists $x \in \{0, 1, ..., N\}$ which denotes the minimum number of participants that satisfies $u_m + x\delta - C(N_m) \geq u_0$. The last man participates iff at least $x - 1$ have participated so far.

For any man i, let n_i be the number of men who choose after i. Then i will participate iff at least $x - 1 - n_i$ have participated so far, since we assume $\delta > C(N_m)$ for a male-majority council. (Proof by induction.)

Women's Participation Decisions

Having pinned down the men's strategies, it only remains to work out the women's participation decisions under Case 3 above. Let n_i be the number of councilors who choose after councilor i, and define N^* by $C(N^*) \leq \delta < C(N^* - 1)$.

For a woman i,

(a) If $N_w < N^*$, participate iff exactly $x - 1 - n_i$ participated so far (she is pivotal) and $u_w + x\delta - C(N_w) \geq u_0$.

(b) If $N_w \geq N^*$, participate iff either:

  (i) at least $x - N_m$ participated (the project will pass no matter what), or

  (ii) y councilors participated, where $x - 1 - n_i \leq y \leq x - 1 - N_m$, i.e. women are pivotal, and $u_w + (y + 1 + n_i)\,\delta - C(N_w) \geq u_0$.

(Proof by induction).

To understand a woman's participation decision, we must consider whether her participation is pivotal to passing the project. If the project is expected to be implemented regardless of her participation, she will participate as long as the marginal benefit to the project, given by δ, is greater than the personal cost $C(N_w)$ of participating. In other words, she participates if and only if critical mass of women N^* has been reached. On the other hand, if her participation is pivotal, the determination is between passing and failure. In this context, she will participate as long as her overall expected benefit from the project is greater than the status quo u_0.

E.2 On-the-Equilibrium-Path Outcomes

Since our predictions depend only on on-path outcomes, it is helpful to describe them here. It is apparent from Section D.1 that different types of projects are affected differently when the number of women reaches critical mass.

Here, we wish to distinguish between gender-neutral projects, defined as $|u_m - u_w| \leq \varepsilon$, and gendered projects, where $|u_m - u_w| > \varepsilon$, for some $\varepsilon \geq 0$ yet to be determined. Starting with $\varepsilon = 0$, we derive our main results summarized in Propositions 1-3. Where our predictions differentiate gender-neutral and gendered projects, we then find the largest value ε and the criteria under which our data can conclusively reject the hypothesis $|u_m - u_w| \leq \varepsilon$. This allows us to test whether our empirical findings on women's participation are driven by purely "behavioral" costs, similar to those found in the tokenism literature, or whether they are partly driven by differences in policy preferences across gender, implicit in most interpretations of critical mass theory. It will also allow us to make sense of our empirical findings regarding motion topics and spending.

Below, we sketch out proofs for the propositions from Section 5.1.

PROPOSITION 1. Participation. *There exists a critical mass N^* such that, when women's nominal representation N_w increases from $(N^* - 1)$ to N^*, female participation increases disproportionately in both gender-neutral and gendered projects.*

The proof of Proposition 1 follows directly from Section D.1. We have pinned down N^* as the smallest group size for which the participation cost $C(N_w)$ dips below δ, altering the conditions for participation. In equilibrium, when $N_w < N^*$, a woman will participate only if she is exactly pivotal, with the end result that exactly x councilors participate on-path. In some of these instances, $x < N$ so that not all women participate. However, when $N_w \geq N^*$, the conditions for participation are a strict superset of the same under $N_w < N^*$, as women also participate on projects that would have passed regardless. This brings us to the observable outcome that, once critical mass is reached, women participate fully whenever men do.

PROPOSITION 2. Non-unanimous votes. *Whether or not the critical mass N^* is achieved, gender-neutral and female-preferred motions are passed unanimously conditional on being proposed. When critical mass is achieved, there are fewer non-unanimous male-preferred motions but more dissenting votes per non-unanimous motion.*

In equilibrium, all projects brought to a vote must pass, otherwise some councilor must not be playing best response. Therefore, on-path, our model predicts that the male majority vote yes on all motions within our observation. In the case of gender-neutral and female-preferred projects, women trivially also vote yes, leading to unanimous motions on-path.

Therefore, non-unanimous votes occur with the female minority dissenting on male-preferred projects, which can only happen on-path when $x \leq N_m$. On reaching critical mass, there are two effects going in the same direction. First, there is a mechanical effect when N_m decreases, narrowing the set of projects that satisfy this condition. Second, within this narrower set, there is a behavioral effect boosting women's participation, which increases the subsequent quality of the project. In some cases, it is enough to switch the women's votes to a unanimous "Yes". The two effects can be seen in the narrowing and shortening (respectively) of the non-unanimous region at the bottom right of Figure 6.

Finally, since men and women vote en bloc, when N_w increases, there is a purely mechanical effect increasing the number of dissenting votes for each non-unanimous motion.

Finding ε As a purely theoretical exercise, we wish to derive the largest value ε for which our predictions in Proposition 2 enable us to differentiate gender-neutral and gendered projects. Essentially, when u_w and u_m are "close enough", it is "as if" the project is truly gender-neutral. Below, we use ε to define how close is "close enough".

To satisfy Proposition 2, we need only determine which male-preferred projects are unanimously or non-unanimously passed on the equilibrium path. A project is non-unanimously passed iff

$$\text{Men participate and vote yes:} \quad u_m + \delta \sum_j D_j - C(N_m) \geq u_0$$

$$\text{Women vote no:} \quad u_w + \delta \sum_j D_j < u_0$$

Combining these two conditions gives $u_m - u_w > C(N_m)$. This gives us a maximum value for ε that works for all male-majority, female-minority councils, which is $C(N-1)$.