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Does Electing Women Reduce Corruption? A Regression Discontinuity Approach

Previous studies uncovered a negative relationship between the proportion of women in public office and corruption. These findings have inspired anti-corruption programs around the world. It remains unclear, however, whether there is a causal link between the share of women in office and malfeasance. For instance, gender differences in political experience or access to corruption networks might explain this relationship. We leverage the gradual implementation of gender quotas in Spain to isolate the effects of female descriptive representation on public misconduct and adjudicate between alternative explanations. The analyses suggest a causal link between gender and malfeasance in office: the reform generated an exogenous increase in the share of women elected, which led to a decrease in corruption that was sustained over time. This finding enhances our understanding of the effect of public officials' characteristics on policy outcomes, and of the role of parity laws in promoting political change.

In 2013, the governor of the most populous state in Mexico (Estado de México) announced a new anti-corruption strategy. The state traffic police would be composed entirely of women, and only women officers would be allowed to issue traffic violations. According to Carlos Ortega Carpinteyro, a local police chief, the policy was developed based on a common understanding that "women are more trustworthy and take their oath of office more seriously."[1]

Similar programs were implemented in Mexico City (Moore 1999) and in Lima, Peru (Karim 2011). These initiatives were backed by two prominent studies that established a link between gender and corruption: in multiple world regions, women were found to be less likely to condone corruption (Swamy

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et al. 2001), and countries with a higher percentage of women in public office had lower levels of corruption (Dollar, Fisman, and Gatti 2001).[2]

Three main arguments have been proposed to explain the empirical association between gender and corruption. The first explanation is that socially induced gender roles make women officials more law abiding than men, and less tolerant of corruption (e.g., Dollar, Fisman, and Gatti 2001). According to this argument, gender differences in internalized behavior are a product of distinct socialization processes and therefore should remain stable over time. The second explanation challenges this view by suggesting that there are no systematic differences between men and women public officials. Instead, the relationship between gender and corruption should be driven by differences in opportunities to participate in public wrongdoing (Goetz 2007). Since women political leaders all over the world tend to be relative newcomers to the policymaking process, their unfamiliarity with existing corruption networks can drive the cross-national patterns observed. Indeed, studies of political corruption often assume that the number of years of experience in office is associated with more opportunities to engage in malfeasance (Klašnja 2015). Finally, other authors suggest that the gender composition of political institutions has no bearing on corruption. According to this explanation, either a third process drives both women representation and public accountability (e.g., Sung 2003), or the causal arrow flows in the opposite direction: corruption obstructs women's descriptive representation (Bjarnegård, 2013).

Given the substantive relevance of this question and the multiple explanations that have been put forward, our article seeks to (1) estimate the causal impact of an exogenous increase in women representation and (2) help adjudicate between different families of explanations. To do so, we leverage the gradual implementation of gender quotas in Spanish local elections. The analyses are based on a new dataset of local corruption cases, and an unobtrusive measure of public wrongdoing in urban planning.

Spain offers an interesting setting to test the gender–corruption link for several reasons. First, since legislated gender quotas were introduced gradually over two electoral cycles (2007 and 2011) and based on population thresholds, a regression discontinuity design (RDD) allows us to isolate the causal effect of a shift in the gender composition of local government. Second, as an established liberal democracy, Spain is a context where we should expect quotas to provide a clean slate of candidates, recruited from new networks and given autonomy to perform their mandate (Bjarnegård, Yoon, and Zetterberg 2018). Third, and most important, the sequential introduction of the quota makes it possible to adjudicate between the three families of arguments described above.

The regression discontinuity estimates provide mixed evidence that gender quotas led to an immediate decline in corruption, and this effect did not seem to disappear two terms after the introduction of the quota. This pattern does not support the notion that the decline in corruption following the introduction of quotas is simply due to lack of experience. If this mechanism were at play, the impact of the quota should disappear over time as women newcomers acquire more political experience. Our findings, although tentative, are more consistent with the argument that the gender composition of Spanish local governments has a sustained causal effect on corruption levels.

Our empirical strategy overcomes some of the limitations of previous work in this area. First, most studies that rely on observational data cannot entirely disentangle the causal effect of an increase in women representation from pre-existing characteristics of the polity (Sung 2003). This problem is compounded by the fact that gender stereotypes influence perceptions of women politicians. Using observational data, O'Brien (2019) shows that female-led parties are perceived as more moderate than male-led ones. Several survey experiments, in turn, suggest that female politicians are perceived to be more ethical and trustworthy (e.g., McDermott 1998; Barnes and Beaulieu 2014; Wiesehomeier and Verge 2020). Since women officials are assumed to engage in more principled behavior (Barnes and Beaulieu 2014), perceptions-based measures of corruption like Transparency International's Corruption Perceptions Index may be affected by these stereotypes and thus lead to biased inferences. We reduce the risk of reverse causation due to gender stereotypes since neither measure of the outcome variables is based on public perceptions. Second, by adjudicating between different mechanisms, the analysis advances over prior work that has relied on causal inference methods to estimate the effects of women representation on corruption (Brollo and Troiano 2016; Esarey and Schwindt-Bayer 2019). Finally, our work complements recent research relying on RDDs. Bauhr and Charron (2021) used the discontinuities produced by close local elections in France to isolate the effect of electing a woman as mayor. In turn, we leverage exogenous variation in the gender composition of municipal councils as a whole. This strategy allows us to provide a broader picture of the behavior of local political elites, accounting for the role of social networks, and to directly test the relationship originally proposed by Dollar, Fisman, and Gatti (2001).

The study must be interpreted in light of recent research on the context-dependent nature of the gender–corruption link. Some studies have argued that the relationship between gender and corruption is specific to advanced democracies with strong electoral accountability mechanisms (Esarey and Chirillo 2013; Esarey and Schwindt-Bayer 2018). Spanish municipalities fit this description. They therefore represent a relatively "easy" case in which to uncover a relationship. We are cautious about generalizing our findings: gender quotas may not have an equally positive and sustained effect in polities with weaker accountability systems.

Women and Corruption

In the early 2000s, two prominent studies uncovered a relationship between women representation in public office and corruption. In a cross-national analysis of over 100 countries, Dollar, Fisman, and Gatti (2001) noticed that the proportion of women in national parliament was associated with lower perceptions of corruption. The result held after controlling for social and economic development, education levels, civic freedom, and ethnic fractionalization. Swamy et al. (2001) corroborated these results with individual-level data. Together, these studies launched an ongoing discussion about the nature of the relationship between gender and corruption as well as the mechanisms that may underlie it. This section describes three different arguments identified in the literature.

First Argument: The Gender–Corruption Correlation is Spurious

The first family of arguments rejects the existence of a causal link between office holders' gender and corruption. Accordingly, adherents of this view maintain that the empirical association found in cross-national studies between more women in office and lower levels of corruption is spurious. They advance two (complementary) explanations to account for this correlation. The first is that both gender equality and government accountability are the result of developments of modern liberal democracy (Sung 2003, 718). For instance, constitutional liberalism's focus on protecting minority rights has generated incentives to increase women representation in government. Simultaneously, liberal democracies have promoted a set of tools to enhance public accountability, which includes competitive elections with motivated oppositions, independent judiciaries, and a free press. All of these dynamics help reduce corruption. Hence, good institutions would account for *both* better gender balance and a cleaner government.

The second explanation for the existence of a spurious correlation is based on reverse causality: corruption might serve as an obstacle to women representation. Bjarnegård (2013) argues that some aspects of clientelism in male-dominated polities create barriers for women representation. Along the same lines, Stockemer (2011) suggests that political recruitment in highly corrupt contexts tends to exclude women. These arguments have led multiple authors to think about women descriptive representation as a response to existing levels of corruption (e.g., Sundström and Wängnerud 2016; Watson and Moreland 2014). This line of research is representative of the lack of consensus about the nature of the relationship.

Both explanations maintain that increasing the proportion of women representatives does *not* have a causal effect on corruption levels. Hence, this argument predicts that introducing gender quotas will not reduce corruption in either the short or longer term.

Second Argument: Lack of Experience and Access Explains Why Women Officials are Less Corrupt

This argument suggests that the empirical association between gender and corruption results from differences in men's and women's access to corruption networks, and to opportunities to exploit them (Alhassan-Alolo 2007; Bjarnegård 2013; Goetz 2007). Accordingly, the vast majority of women political leaders around the world are relative newcomers who are less familiar with male-dominated corruption networks and the rules of illicit exchange. As Goetz explains, if "women do exhibit preferences for less corrupt behavior, that may simply be because they have been excluded from opportunities for such behavior, and that effect is bound to change over time as greater numbers of women enter public office" (2007, 102). Indeed, several survey experiments show that the stereotype that female officials are more honest can be explained in part by perceptions that women are outsiders to corruption networks (Barnes, Beaulieu, and Saxton 2018; Barnes and Beaulieu 2019; Wiesehomeier and Verge 2020; but see Brierley and Pereira 2022). This argument is consistent with theories of differential association and opportunity in criminology (Sutherland, Cressey, and Luckenbill 1992), according to which people commit crimes when they have frequent interactions with those who condone such behavior *and* have the opportunity to engage in criminal behavior.[3]

Women politicians, disproportionately less experienced in public office, may lack the connections and know-how to engage in malfeasance. As Klašnja (2015) suggests, it often takes time for an office holder to learn to be corrupt, i.e., to develop the network of accomplices and cronies that will enable malfeasance. According to this logic, an exogenous increase in the share of women politicians implies a sharp rise in the proportion of newcomers in government. It is harder for less experienced officials to form the collusion networks needed to engage in corrupt exchanges. Hence, a sudden jump in the number of women in public office may reduce corruption, but this effect is likely to be short-lived. This argument implies that, as women politicians acquire experience and develop connections in office, they will be more likely to engage in illicit deals.[4]

Importantly, this argument does not imply that experience in office fully accounts for gender differences in access to corruption networks. More experienced women legislators may still be marginalized by their peers in different ways (Barnes and Beaulieu 2019; Franceschet and Piscopo 2008; Schwindt-Bayer 2010). The expectation is simply that the likelihood of marginalization weakens over time, as well as the opportunities to develop new corruption networks. Bauhr and Charron (2021) and Kerevel and Atkeson (2013) find evidence consistent with this argument in France and Mexico, respectively.

Third Argument: The Share of Women Politicians has a Causal Effect on Corruption

A third family of arguments establishes that there is a causal link between the gender composition of elected officials and the level of political corruption. According to this perspective, an increase in the proportion of women politicians will curb malfeasance. Four distinct mechanisms have been advanced to explain the causal link between gender and corruption. We discuss each of them in turn.

One of the most prevalent explanations for the gender–corruption link points to differences in socialization (e.g., Dollar, Fisman, and Gatti 2001; Torgler and Valev 2006; Wängnerud and Grimes 2012). According to this argument, socially constructed gender roles make women more law-abiding than men, and less tolerant of corruption. Women, the argument goes, have stronger anti-corruption norms and tend to have more self-control than men (Torgler and Valev 2006). Such gender differences vis-à-vis corruption are due to internalized rather than conscious behavior, and therefore should remain largely stable over time.[5]

A second mechanism posits that women are, on average, more risk averse than men (Croson and Gneezy 2009; Frank, Lambsdorff, and Boehm 2011; Rivas 2013). To the extent that engaging in misconduct is risky—because the perpetrator can be caught and punished—risk-averse officials will be more reluctant to participate in this kind of behavior. Consistent with this argument, researchers have shown that women are less likely than men to accept bribes in situations involving a risk of detection (Schulze and Frank 2003). However, other authors have noted that these patterns are context specific (Alatas et al. 2009; Armantier and Boly 2011; Magalhães and Pereira 2022). Consistent with these arguments, Esarey and Schwindt-Bayer (2018) have shown that the relationship between gender and corruption is stronger in democracies with greater electoral accountability. Arguments based on this mechanism posit that gender has a conditional causal effect on corruption: to the extent that engaging in malfeasance carries no risk of punishment, there will be no gender-based differences in malfeasance.

A third type of argument highlights how, even if women have the same risk attitudes as men, they may still be more reluctant to engage in corrupt activities. The reason, the argument goes, is that women politicians are judged more severely by voters than male officials. When faced with a corruption scandal, women politicians sometimes receive more severe punishments than male office holders. The existence of higher standards for women has been tested both with observational data (O'Brien 2015; Esarey and Schwindt-Bayer 2018) and with survey experiments (Eggers, Vivyan, and Wagner 2018a; Batista Pereira 2020, among others). This argument thus leads to the expectation that, even with similar levels of risk aversion than men, women will be less willing to participate in malfeasance because they face higher penalties than men.

A fourth explanation attributes the beneficial effect of women representation on corruption to an “interest mechanism” (Alexander and Ravlik 2015; Bauhr, Charron, and Wängnerud 2019). This argument posits that women in office focus on different policy areas than men. In particular, they tend to concentrate their action on social welfare and public service provision. Such differential focus, the argument goes, curbs corruption in two ways. First, it reduces “grand” corruption (fraud, embezzlement) by turning away from urban development in favor of other policy areas where such corruption is less likely to occur. Second, women politicians invest in uprooting “petty” corruption in public services as bribes can significantly reduce access and quality in welfare provision.

These four mechanisms—differences in socialization, risk aversion, risk of punishment, or policy priorities—share a common empirical prediction: an exogenous increase in the proportion of women politicians will decrease corruption, and this effect should be sustained over time. Note that, while the mechanisms refer to individual-level traits, the gender quota produces a change in the composition of a collective body. The logic is that a change in the share of women in local councils might disrupt pre-existing corruption networks. If incoming politicians are more averse to participating in corruption, they will be less likely to collaborate in malfeasant endeavors or turn a blind eye if irregularities occur. As a result, the remaining shady officials will be more reluctant to engage in dishonest dealing and the overall level of corruption will decrease. In the empirical section we test whether this is the case.

Methodological Issues

Despite the substantive importance of understanding the relationship between gender and corruption—as its propensity to shape public policy suggests—few studies have been able to identify the causal effect of women descriptive representation on corruption for two reasons (Bauhr and Charron 2021; Brollo and Troiano 2016; Esarey and Schwindt-Bayer 2019). First, the difficulty associated with measuring corruption has generated data limitations that have restricted the scope of prior research. The most common approach used in the literature is to rely on perceptions of public wrongdoing, such as Transparency International’s Corruption Perceptions Index. However, using perception-based measures of corruption to test the effects of women representation can be problematic. Recent studies have shown that the presence of women politicians systematically reduces concerns of corruption due to stereotypes associating women with higher moral behavior (Barnes and Beaulieu 2014). This means that perceptions of corruption might improve as more women are elected to office, not necessarily because malfeasance actually drops, but due to the stereotypes that associate women officials with law-abiding behavior. Hence, if we are interested in understanding whether a change in the gender composition of government influences *actual* levels of corruption, relying on perceptions may lead to biased inferences. Moreover, most previous studies have conducted experiments on student populations to test various theoretical arguments, but these are not always comparable to political elites (Butler and Kousser 2015).

A second challenge faced by existing scholarship relates to the construction of a counterfactual. In an ideal world, to test the independent effect of electing more women on a given outcome, we should either randomly assign women to office or identify an exogenous shock in women descriptive representation. Otherwise, it is hard to rule out the myriad of confounders that may lead to both the election of women and existing levels of corruption.

RDDs leveraging close races have recently been adopted to overcome this challenge, with promising results (e.g., Bauhr and Charron 2021; Brollo and Troiano 2016).[6] Bauhr and Charron (2021), in particular, persuasively report a negative causal relationship between electing a woman mayor in competitive elections and corruption in French municipalities. However, this empirical solution can only test the effect of electing one individual candidate (a state legislator or mayor, for instance), yet most scholarship in this field maintain that the gender composition of the legislature as a whole is the main force behind changes in public misconduct (e.g., Esarey and Schwindt-Bayer 2018). In the next section we describe how we overcome these challenges using the case of Spain's gender quotas in local elections.

Testing the Impact of Gender Quotas on Corruption

The implementation of gender quotas in Spanish local elections offers a rare opportunity to exploit an exogenous shock in the gender composition of local governments. The policy was introduced gradually over two electoral cycles, based on population thresholds.[7] Thus we can use an RDD approach to isolate the causal effect of women descriptive representation on corruption.

Spanish Gender Quotas and Women's Representation

In 2007, Spain introduced legislated gender quotas for national, European, regional, and (partially) local elections.[8] All of these electoral contests employ a closed-list proportional representation system (Colomer 2004).[9] The quota required all party lists to include at least 40% of candidates of either sex. Hence, each list must contain at least 40% women. To prevent parties from strategically placing candidates (e.g., relegating all women to the bottom of the list), the quota includes a zip clause: for every five ordered candidates, at least two (but no more than three) must be women.

Crucially for this study, the new policy was implemented gradually in local elections. For the 2007 municipal election, the new law applied only to municipalities with more than 5000 inhabitants (15.7% of the 8122 local jurisdictions). Four years later, in 2011, the quota was extended to all municipalities with more than 3000 inhabitants. The quota does not apply to those with fewer than 3000 residents—representing roughly 12.6% of the national population.[10] Hence, if the adoption of quotas led to a shift in the gender composition of local governments, using an RDD we can treat this change as exogenous for municipalities close to the cutoffs.

Before testing the impact of the Spanish gender quota on levels of municipal corruption, we examine the quota's effect on the actual level of women representation. This can be understood as a manipulation check (Mutz and Pemantle 2015). First, we evaluate whether the introduction of quotas generated a discontinuity in the proportion of women councilors. To do so, we estimated locally weighted polynomial regressions of the share of women elected to the municipal council below and above the population thresholds.[11]

Figure 1 displays the proportion of women councilors below and above the quota thresholds. Both plots show a clear discontinuity around the population cutoffs. The share of elected women increased by about 5 percentage points around the thresholds in both elections. These jumps in the proportion of women councilors are statistically significant in both cases (see Table A1 and Table A2 in the Supplementary Appendix). The quota thus produced a relevant increase in the proportion of women in municipal councils. In a second manipulation check we compare changes in women representation around the population thresholds, relative to the previous period, to rule out the possibility that the discontinuity is due to factors other than the quota (see Figure A1).

The evidence suggests that the introduction of gender quotas increased women representation in Spanish municipal councils. Hence, if the gender of office holders indeed influences the likelihood of corruption, we should observe that this reform affected levels of malfeasance in public office.

Adjudicating between Theoretical Arguments

The gradual implementation of gender quotas in Spanish municipalities allows us to estimate two types of causal effects: immediate effects and experience effects. *Immediate effects* refer to the quota's causal impact in the first term (four years) after its implementation. *Experience effects* constitute the difference between the quota's impact in the *second* term relative to the *first*.[12] The experience effect thus allows us to determine whether the causal effect of an increase in women representation is sustained over time.

FIGURE 1
Share of Women Elected to Municipal Councils, by Population.
Note: The panels present scatterplots of the share of women elected by population size in the 2007 and 2011 elections, respectively. Local polynomial regressions were estimated on each side of the quota thresholds adopted in each year (identified by the vertical bars). The threshold of implementation for the quota was 5,000 inhabitants for the 2007 election and 3,000 in 2011.

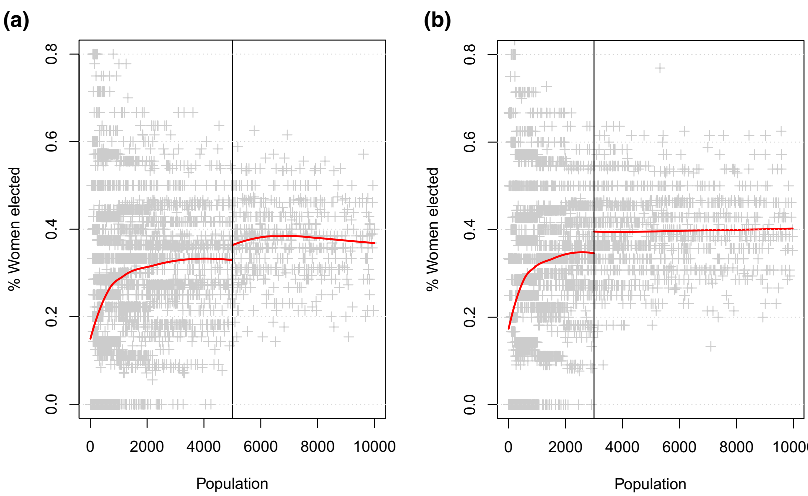


Figure 2 summarizes the empirical strategy to estimate *immediate* and *experience* effects.

We can estimate the immediate effect of introducing the gender quota around two population cutoffs. For municipalities with more than 5000 inhabitants, the first period with quotas is the 2007–11 term (*Immediate Cutoff* $_{07}$ in the figure). For those with a population of 3000–5000, the first term with enforced quotas is 2011–15 (*Immediate Cutoff* $_{11}$). To estimate the experience effect, we exploit the fact that, in the 2011–15 period, municipalities with a population over 5000 experienced their second term with quotas while those with a population just below 5000 people—but above 3000—were in their first term. Thus, comparing corruption levels for 2011–15 around the 5000-inhabitant threshold (*Experience Cutoff* in Figure 2) we can measure the effects of experience with the quotas.[13]

Estimating both the *immediate effect* and the *experience effect* of quota adoption allows us to empirically adjudicate between the three families of arguments articulated above. Each of them generates a distinct set of empirical predictions (see Table 1).

The *Spurious Correlation* argument posits that the correlation observed between gender parity in government and corruption in previous studies is due to either omitted variable bias or inverse causation. In this context, gender has no independent effects on corruption. According to this argument, the introduction of a gender quota will not impact corruption, either in the short or long term (first row of Table 1). The *Lack of Experience* argument states

FIGURE 2

Identification Strategy: Immediate and Experience Cutoffs.

Note: Depiction of population cutoffs produced by the sequential roll-out of gender quotas in Spain.

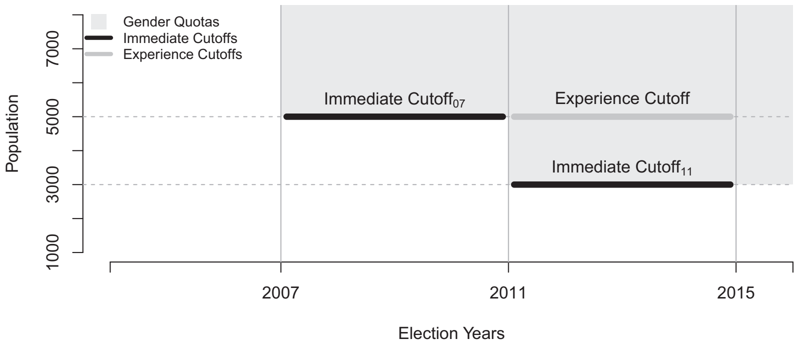


TABLE 1
Summary of Predictions for the Effects of Women Descriptive Representation on Corruption

	Corruption	
	Immediate Effect	Experience Effect
Spurious Correlation	0	0
Lack of Experience	−	+
Gender-Corruption Causal link	−	0

Note: Entries are predictions for RD estimates of an increase in women representation in public office on levels corruption according to each argument; “+” indicates a positive coefficient, “−” corresponds to a negative coefficient, while “0" defines a null coefficient.

that women politicians tend to be less corrupt because they have fewer opportunities than their male colleagues to engage in corrupt activities. According to this line of thought, this difference is driven by the fact that women in office, on average, have less political experience than men. This argument predicts that newly elected women would have a harder time gaining access to existing corruption networks. However, this difference should fade out over time, as newcomers become ingrained in the political process (Goetz 2007). Consequently, this argument suggests that introducing gender quotas will lower corruption in the short term, but that over time malfeasance will return to its previous level of equilibrium (second row of Table 1). Finally, the third family of arguments states that there is indeed a *Gender–Corruption Causal Link*. As noted above, several mechanisms have been put forward to explain this link: differences in socialization, or in risk attitudes, higher standards applied to women, or gendered policy priorities. All of these mechanisms posit a gender–corruption link that should endure over time (third row in Table 1): a larger presence of women in public office should be associated with a reduction in corruption in both the short and long run. In sum, both the *lack of experience* and the *gender-corruption causal link* imply that the gender quota will increase the proportion of politicians who are reluctant to engage in malfeasance. Empirically, the key difference is that the second argument posits that this effect will fade out over time.

Having defined the empirical predictions that arise from each family of arguments, the next section describes our empirical strategy and discusses which of these three arguments is most consistent with the evidence.

Data and Method

To adjudicate between the alternative predictions, we compiled an original dataset with information on the gender composition of Spanish local governments and two complementary measures of corruption.

Municipal Council Data

To identify the gender composition of municipal councils, we requested data from the Spanish Ministry of Finance (Ministerio de Hacienda y Administraciones Públicas).[14] From these data, we gathered information on the number of seats in each municipal council, the share of women elected, the gender of the mayor, and the majority party. This dataset was then merged with annual population data gathered by the national statistics office (Instituto Nacional de Estadística).

Outcome Variables: Measures of Corruption

It is difficult to measure corruption due to its clandestine nature. Assessing subnational-level corruption is particularly challenging because the most commonly adopted indices are only available at higher levels of aggregation. However, given the salience of local corruption in Spain, researchers have developed valuable complementary measures.[15] The analyses rely on extended versions of two measures previously adopted in the literature: revealed corruption and urban development. A full description of how the variables were constructed can be found in Appendix D.

Revealed Corruption captures cases of corruption involving criminal charges against local elected officials. The main source of this measure is the Fundación Alternativas: a Spanish think tank that commissioned a study to record all local corruption scandals as published by national, regional, and local newspapers since 2000. The organization hired journalists from left- and right-leaning newspapers in each Spanish province to collect the data locally. This dataset has been triangulated and extended in subsequent studies (Costas-Pérez, Solé-Ollé, and Sorribas-Navarro 2012; Fernandez-Vazquez, Barberá, and Rivero 2016).[16] The combined dataset identifies cases of exposed corruption in the electoral cycle prior to the introduction of the quota (2003–07) and the first term since (2007–11). For the 2011–15 term we have updated the dataset following the search criteria adopted by both sets of authors. For a corruption case to be included in the dataset it must fulfill three requirements: (1) judicial charges are brought for corruption-related offenses or abuse of public office; (2) the set of actors being accused includes at least one local elected official; and (3) the events associated with the case took place in the period under analysis.[17] The final dataset includes 293 cases: 129 from 2007–11 and 165 from 2011–15. A dichotomous variable was created for each municipality term, which takes a value of 1 if a corruption case took place in that four-year period, and 0 otherwise.

By definition, revealed corruption cases are only a subset of *actual* corruption. This would be problematic in the context of this study if the likelihood of a corruption case being judicially investigated varied discontinuously around the quota thresholds. To our knowledge, there are no reasons to believe this is taking place in the Spanish context.[18] Still, in order to mitigate this concern and develop a broader sense of the phenomenon, we complement the analyses with a second measure of corruption.

Urban Development captures amendments to existing land-use plans over time.[19] The measure was originally developed by Solé-Ollé and Viladecans-Marsal (2012) to quantify "influence [over city officials] wielded by land developer lobbies" (10). Bribes in exchange for adjustments to land regulations are the most common type of local corruption in Spain (e.g., Fundación Alternativas 2007; Darias, Martín, and González 2012; Sánchez 2007). This variable unobtrusively captures the phenomenon by measuring the change in land supply for urban development between two elections. The idea is that, after accounting for prior levels of demand, an abnormally large increase in the amount of land allocated for construction signals potential wrongdoing in public office.[20] Large increases in urban land do not necessarily imply malfeasance. Hence, this indicator has intrinsic measurement error, making estimation less precise. Still, we believe it is an important complement to the measure of revealed corruption and a way to assess the robustness of the patterns uncovered.

Specifying the RDD

The Spanish case offers the opportunity to adopt an RDD around two population thresholds used to gradually implement gender quotas: 5000 in 2007 and 3000 in 2011. It is possible to employ an RD design because observations are assigned to treatment—the gender quota—based on the values of a given covariate (population size), and the probability of receiving the treatment conditional on that covariate shifts abruptly at the threshold. Under appropriate assumptions, by looking at municipalities around these cutoffs it is possible to treat the increase in women representation produced by the adoption of quotas as exogenous. Hence, our empirical strategy is to examine whether levels of corruption change significantly around the thresholds introduced by the quota.

Our approach addresses two potential issues that population-based RD designs face: sorting and compound treatments (Eggers et al. 2018b). The first issue refers to the possibility that municipalities could manipulate their own population data and effectively self-select into treatment or control. This would violate the RDD assumption that the impact of the running variable (population) on the outcome variable (corruption) is continuous around the threshold (Butler and Butler 2006). To test for this possibility, we conducted McCrary sorting tests for both quota thresholds. The results of these tests (reported in Figure E1) provide no evidence of sorting.[21]

The second concern associated with population-based RDDs is compound treatments—the possibility that a population cutoff might affect assignment to more than one treatment. (Eggers et al, 2018b). This issue applies to our case study. Municipalities with more than 5000 people are given more powers than those below 5000. Mayors' salaries also increase at the 5000 threshold. To address this issue, we leverage the fact that in our study period the rules that define population-based jumps in salary and responsibilities are *time invariant*. We thus follow Bagués and Campa (2021) by adopting a *discontinuity-in-differences* approach. The key feature of this approach is that the outcome of interest (the indicator of corruption) is measured as the change between two time points—specifically, how corruption changes once the quota is implemented, relative to its pre-quota level. Assessing changes over time allows us to rule out the effect of time-invariant institutional differences (such as mayors' salaries or municipal powers) and isolate the effect of the introduction of gender quotas. To provide a consistent baseline throughout the analyses, we use the 2003–07 term as the reference period, before the first quota threshold was introduced.[22]

We follow the current standard procedure of using a non-parametric estimator within a narrow window around the population thresholds. This method involves estimating two separate functions—above and below the cutoff—with each observation being down-weighted as the distance to the population cutoff increases. Defining the size of the window within which these functions are estimated—the bandwidth—is a central aspect of the procedure. We use the method proposed by Calonico, Cattaneo, and Titiunik (2014), which entails selecting the bandwidth that minimizes the mean-squared error of the regression. All the models were estimated using the rdrobust package in R.

Results

The empirical analysis is presented in two steps. First, we discuss the immediate causal effects of introducing quotas: in 2007 around the 5000 inhabitants threshold and then in 2011 around the 3000 threshold. Second, we explore the experience effects of the quota by looking at municipalities around the 5000 inhabitants threshold in the 2011–15 period, when municipalities above the cutoff experienced the second term of quota implementation while those below were still in the first term of application (see Figure 2). Together, we find evidence that corruption decreased immediately after the introduction of the quota, and this effect is sustained over the medium term.

Immediate Effects

Figure 3 illustrates the immediate effect of the gender quota for municipalities around the 5000 inhabitants threshold established for the 2007 election.[23] Panel (a) displays revealed corruption and panel (b) urban development. Both outcomes are measured as the difference between the first period with the quota (2007–11) and the baseline period (2003–07). The dots represent average values of each outcome variable within small population bins, and the lines are fourth-order local polynomials estimated separately on each side of the threshold. The population cutoff is identified by the vertical bar. Both panels suggest that the exogenous increase in the share of women elected, generated by the adoption of quotas in 2007, led to a decrease in corruption over the next four years.

Table 2 presents a formal test of these immediate effects. We compare the change in corruption levels between municipalities

FIGURE 3
Immediate Effects of the Gender Quota around the 5000 Inhabitants Threshold, 2007–2011.
Note: The dots show average values of the outcomes measured as the change between the 2003– 2007 and 2007–2011 terms, within small population bins. The lines are fourth-order polynomials fit on each side of the threshold. The plots are based on the procedure developed by Calonico, Cattaneo and Titiunik (2015).

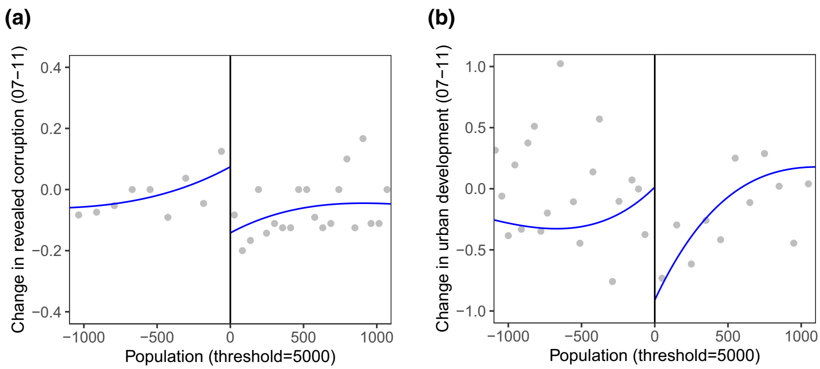


TABLE 2
Immediate Effects of Gender Quotas on Corruption around the 5000 Population Threshold Applied in the 2007 Election

	Comparison: Below/Above Gender Quota	
	Δ Revealed Corruption	Δ Urban Development
	(1)	(2)
Estimate	−0.232	−1.041
Robust standard error	0.097	1.216
p-value	0.017	0.392
Bandwidth	1042	1094
N below/above	206/189	196/165

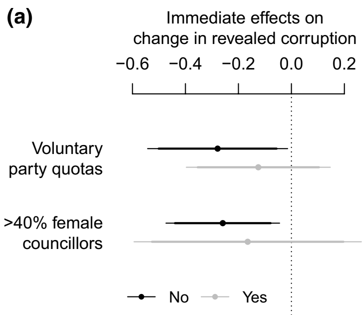
Note: Entries are estimates of the intent-to-treat effects of adopting gender quotas around the 5,000 threshold in 2007 (robust SEs from rdrobust). The outcome variable is indicated in the column header. Revealed corruption is the probability of a corruption case emerging. Urban development is the change in the amount of urban land weighted by existing built-up land. Both outcomes are measured as *change* relative to the 2003–07 term.

just above the 5000 cutoff with those just below. The estimated effect of quota adoption on revealed corruption (column 1) is negative and statistically significant (−0.205; p-value = 0.016). The magnitude of the estimate is equivalent to 61.0% of the standard deviation of revealed corruption, suggesting a sizeable effect.[24] The outcome variable in column 2 is the change in urban development. Large positive values signal that a local government is lenient vis-à-vis the pressures of urban developers. The RD estimate suggests that the introduction of the quota *reduced* urban expansion in the following term. This estimated drop in urban development, however, is not statistically significant (p-value = 0.406).

The exogenous shock imposed by the gender quota was not homogeneous across municipalities. As Figure 1 shows, some local governments already elected over 40% women councilors prior to the introduction of the quotas. If the impact on corruption is due to a shift in the gender composition of municipal councils, we should see larger effects in municipalities where the quotas induced a larger increase in women representation. We capture treatment intensity in two ways: (1) municipalities where the incumbent party *did not* have voluntary quotas when the new quota was adopted and (2) municipalities where the share of women councilors in the 2003–07 term was below the quota imposed in the following election.[25] Figure 4 presents the RD estimates for each subgroup of municipalities around the 5000 threshold in 2007. The results are consistent with our prediction. The effects of gender quotas tend to be larger (more negative) in local governments if the incumbent party did not have voluntary party quotas (upper estimates) and where the share of women councilors was below 40% before the quota was introduced. Yet since treatment heterogeneity is not causally identified, these results should be interpreted as suggestive.

The 2011 extension of gender quotas to municipalities with 3000–5000 residents facilitates an additional test of the immediate effects of the quota. Figure 5 presents evidence of the change in the corruption indicators for municipalities right below and right above the new population threshold (3000 inhabitants). As before, dots reflect averages within population bins and the lines are fourth-order polynomials. Table 3 presents the formal RD tests. The findings are mixed. The estimate for the quota's immediate effect on urban development is negative and statistically distinguishable from zero (estimate −1.136, p-value = 0.017). The coefficient suggests that municipalities that implemented the quota in 2011

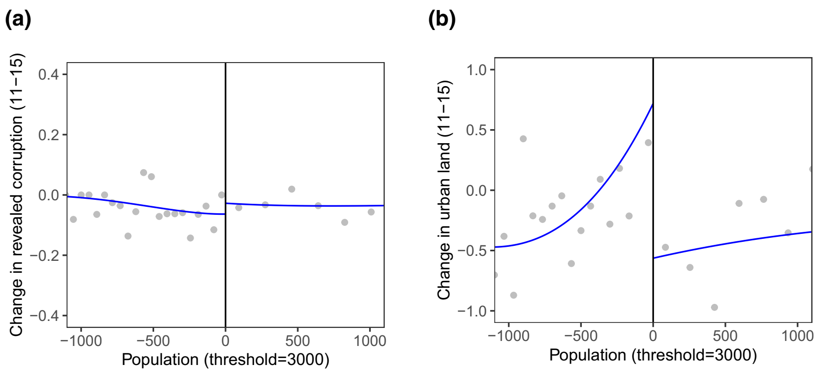
FIGURE 4
Immediate Effects of the Gender Quota in 2007 by Treatment Dosage.
Note: Entries are RD estimates of the effects of adopting gender quotas in 2007 by “treatment dosage” (lines are 95% confidence intervals). High-dose units are defined either as municipalities where (1) the incumbent party did not have voluntary gender quotas in 2007 (upper estimates) or (2) the share of women councilors in 2003 was below the 2007 quota level (lower estimates).



were less inclined to foster urban development than those that did not. In turn, the estimate of revealed corruption is fairly close to zero and unreliable (estimate = 0.044, std error = 0.041). This result may be due to the fact that the source of information used to produce this measure—news reporting about corruption cases—is less accurate for smaller municipalities.[26] Taken together, the RD analyses of the short-term effects of the quota offer partial support for the claim that adopting gender quotas produces an immediate drop in corruption.

The validity and robustness of the findings are discussed in detail in Appendix E. Falsification tests (Table E1 and Table E2) confirm the lack of discontinuities on relevant variables realized prior to the adoption of the policy, including lagged outcome variables (e.g., Caughey and Sekhon 2011; Klašnja and Titiunik 2017). The same substantive results are obtained when the analyses are replicated without the discontinuity-in-differences approach (Table E4). Finally, a sensitivity analysis shows that the effects are stable across bandwidths of different sizes (Figure E2).

FIGURE 5
Immediate Effects of the Gender Quota around the 3000 Inhabitants Threshold, 2011–2015.
Note: The dots show average values of the outcomes measured as change between the 2011–15 and 2007–11 terms, within small population bins. The lines are fourth-order polynomials fit on each side of the threshold.



The Effects of Experience in Office

In this section, we assess the experience effects in municipalities that adopted gender quotas in 2007. The response variables are now measured in the 2011–15 term, four to eight years after the exogenous shift in the gender composition of local governments. This analysis compares municipalities above the 5000 inhabitants threshold, which are in their second term with the gender quota, to those immediately below the cutoff, which are in their first. If the *Lack of Experience* argument is correct, we should observe that corruption indicators progressively bounce back up in the second term once the newly elected women acquire experience and access.[27] In other words, this argument predicts positive RD estimates. If, however, the *Gender–Corruption Link* argument is correct, corruption levels should stay more or less the same in the second term, i.e., the RD estimates should not be statistically distinguishable from zero.[28]

Table 4 presents the RD estimates for revealed corruption and change in urban development. Regardless of the outcome, the point estimates are negative but fairly small in absolute magnitude

TABLE 3
Immediate Effects of Gender Quotas on Corruption around the 3000 Inhabitants Threshold Applied in the 2011 Election

	Comparison: Below/Above Gender Quota	
	Δ Revealed Corruption	Δ Urban Development
	(1)	(2)
Estimate	0.046	−1.227
Standard error	0.048	0.578
p-value	0.340	0.034
Bandwidth	1158	1046
N below/above	606/417	475/294

Note: Entries are estimates of the intent-to-treat effects of adopting gender quotas around the 3,000 threshold in 2011 (robust SEs from rdrobust). The outcome variable is indicated in the column header. Revealed corruption is the probability of a corruption case emerging. Urban development is the change in the amount of urban land weighted by existing built-up land. Both outcomes are measured as change relative to the 2003–07 term.

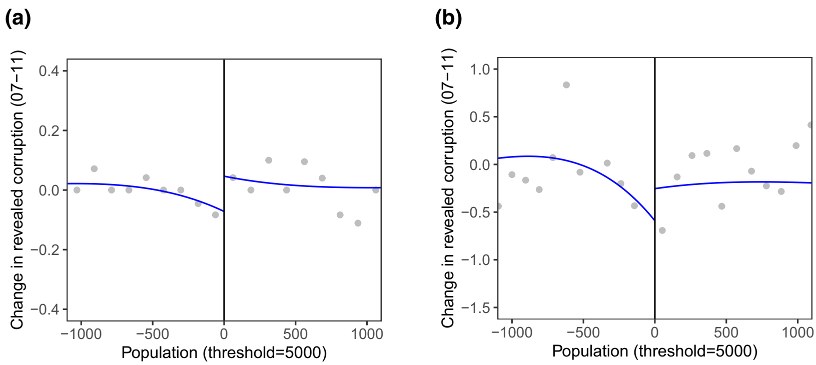
TABLE 4
Experience Effects of Gender Quotas on Corruption around the 5000 Inhabitants Threshold

	Comparison: Below/Above Gender Quota	
	Δ Revealed Corruption	Δ Urban Development
	(1)	(2)
Estimate	0.127	0.599
Standard error	0.087	0.940
p-value	0.144	0.524
Bandwidth	1240	1078
N below/above	254/220	192/161

Note: Entries are estimates of the intent-to-treat effects of the second application of the gender quotas, relative to the first application (robust SEs from rdrobust). The outcome variable is indicated in the column header. Revealed corruption is the probability of a corruption case emerging. Urban development is the change in the amount of urban land weighted by existing built-up land. Both outcomes are measured as *change* relative to the 2007–11 term.

and indistinguishable from zero. Figure 6 illustrates these findings. The two panels reveal no discernible differences between municipalities in their first and second terms after the quota adoption. These results do not support the *Lack of Experience* hypothesis.

FIGURE 6
Experience Effects of the Gender Quota around the 5000 Inhabitants Threshold, 2011–2015.
Note: The dots show average values of the outcomes measured as change between the 2011–15 and 2007–11 terms, within small population bins. The lines are fourth-order polynomials fit on each side of the threshold. Plots are based on the procedure developed by Calonico, Cattaneo, and Titiunik (2015).



According to this line of argument, we should observe an increase in corruption four to eight years after the quota's adoption, as the effects of electing inexperienced public officials would fade out. However, both coefficients are small and unreliable. The null results are more consistent with the *Gender–Corruption Causal Link* argument, according to which quotas should have a negative effect on corruption in the short term and this effect should be sustained over time.[29].

Conclusion

According to a report by the World Economic Forum, corruption around the world soaks over 5% of global GDP (WEF 2012). Hence, understanding whether women public officials are less likely to engage in corruption is of both theoretical and practical importance. The empirical correlation between a higher share of women in office and lower levels of malfeasance has been well established (Dollar, Fisman, and Gatti 2001), but it is not clear whether there is a causal link between the gender of elected officials and their propensity to engage in corruption.In this article we exploited the gradual implementation of gender quotas in Spain to provide a causal estimate of the effect of women representation on corruption. Given that the implementation was based on population thresholds, the quota introduced an *exogenous* increase in women descriptive representation around these cutoffs. We used an RDD approach to gauge the effect of this exogenous increase in female political power. Additionally, we test three families of arguments related to the impact of an increase in women representation: the *Spurious Correlation* thesis, the *Lack of Experience* account, and the *Gender–Corruption Link* argument.

The results offer some support for the argument that increases in women representation cause a drop in levels of malfeasance. Both in 2007 and 2011 we uncover (partial) evidence that introducing gender quotas immediately decreased levels of incumbent misconduct in municipalities close to the relevant population cutoffs. Moreover, this short-term effect does not go away as the newly elected representatives acquire more experience. Leveraging the sequential implementation of quotas, we show that there is no significant difference in the risk of malfeasance between municipalities where quotas have been in place for eight versus four years. This pattern is not consistent with the argument that introducing gender quotas reduces corruption only because it increases the number of unseasoned politicians in office. According to this perspective, new public officials lacking political experience would not have access to corruption networks and therefore would not have the opportunity to engage in malfeasance. If this were the case, we should observe that, as women politicians acquire more experience, they tend to replicate the corrupt patterns of their senior colleagues. That is not what we observe, at least in the eight-year period analyzed: the reduction in corruption survives into the second term after the quota's implementation. The evidence thus suggests that the correlation observed in multiple studies since the original contribution by Dollar and co-authors (2001) *does* translate into a causal relationship.

Our article complements previous work that has sought to estimate the causal impact of women representation on corruption. First, our empirical strategy based on population thresholds suggests that the causal effects uncovered in RDDs that leverage close elections extrapolate to less competitive contexts (de la Cuesta and Imai 2016). Second, while Bauhr and Charron (2021) and Brollo and Troiano (2016) find that electing a woman mayor reduces the number of irregularities in local administration, our study shows that changing the gender of the mayor is not a necessary condition for this to occur; increasing the share of women councilors could have a similar effect, and may be more durable. The effects uncovered here persist for at least two terms. Bauhr and Charron (2021), on the other hand, find effects only in the first term after a woman mayor is elected. A possible explanation for this discrepancy is that mayors alone have a harder time changing the culture and routines of a local government. However, differences in the context or in the measures of corruption may also explain these results. Only more scholarship will allow us to provide a more definitive answer to this question.

In closing, it is important to note that, while our evidence is consistent with a causal link between gender and corruption, the data do not allow us to adjudicate between the many causal mechanisms that underlie this relationship. Given the relatively low levels of personalization in Spanish local politics, double standards are a less plausible mechanism. Gendered socialization or risk aversion, on the other hand, may be stronger candidates in this context since most local representatives occupy part-time positions and have limited political ambition beyond local government (Gómez and Cazorla 2018), making them more similar to the general public from which evidence on gendered norms often comes. Still, we must remain agnostic as to which mechanisms ultimately sustain the gender–corruption causal link. Future individual-level research with elected officials should address this open question.

Another note of caution regards the generalizability of our findings. First, prior work argues that the gender–corruption link should be strong in Spain (Esarey and Chirillo 2013; Esarey and Schwindt-Bayer 2018). Accordingly, politicians' gender has a greater effect on corruption in parliamentary systems with strong electoral accountability, where corruption is not the norm and press freedom is respected. Spanish local governments fit nearly all of these criteria.[30] In contexts with weaker accountability mechanisms the introduction of quotas may produce smaller effects.

At the same time, the Spanish gender quotas generated only moderate changes in the share of women local councilors. This fact can constrain the magnitude of the effect of such a quota on political corruption. Since the gender–corruption causal link arguments posit a monotone relationship between gender composition and malfeasance, we believe that a larger change in the share of women local representatives could have produced a more substantial change in corruption.

A third relevant consideration is that our findings speak to a shift in the gender composition of local councils resulting from the introduction of quotas. Whether the same patterns occur when women representation increases gradually without a quota remains an open question. Legislated quotas may have larger effects on corruption if they force parties to recruit candidates from non-traditional candidate pools (Bjarnegård, Yoon, and Zetterberg 2018; Barnes and Holman 2020). On the other hand, politicians elected through quotas may have less autonomy in office, reducing the chances of changing the status quo (Chauchard, Heinze, and Brulé 2022). We leave it for future work to provide a more complete answer to this question.

Data Availability Statement. The data that support the findings of this study are openly available in Harvard Dataverse at https://dataverse.harvard.edu/.

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NOTES

1. NPR, September 28, 2013. Available from http://www.npr.org/2013/09/28/226903227/mexican-states-anti-corruption-plan-hire-women-traffic-cops.

2. We define corruption here as the abuse of public office for personal gain (OECD 2014).

3. Alhassan-Alolo (2007) finds support for this argument based on a small survey experiment with public officials in Ghana. When exposed to an opportunity to take part in public wrongdoing, or to networks of corruption, men and women officials respond in the same way.

4. The *lack of experience* argument predicts that the Spanish quotas will have a causal impact but one that will be driven not by gender but by an increase in the proportion of representatives with little experience and access to corruption networks. In other words, gender is not the causal factor. Experience (and lack thereof) is.

5. An observationally equivalent argument suggests that women are less corrupt due to innate gender differences: women's higher moral nature carries over to public life. This argument was articulated by several classical philosophers up to Freud (Goetz 2007). However, current research has not relied directly on this argument.

6. Another approach to estimating the causal impact of the share of women in elected office is to employ an instrumental variable (Esarey and Schwindt-Bayer 2019).

7. Two studies have previously examined the consequences of implementing the gender quota in Spain. Casas-Arce and Saiz (2015) found that parties were not maximizing their electoral results prior to the introduction of quotas, which suggests pre-existing discrimination in the political recruitment process. Bagués and Campa (2021), in turn, concluded that introducing quotas was unrelated to different measures of policy output.

8. Ley Orgánica 3/2007, March 22, came into effect two months before the local elections, and was part of a broader reform to promote the effective equality of men and women. The law included a temporary exemption for municipalities between 3000 and 5000 inhabitants where the quota would only come into effect in 2011 (Verge and Lombardo 2018). We find no evidence of anticipatory behavior for this subset of municipalities (Table A5).

9. Local mayors are not popularly elected in Spain. The election generates a set of council members who later appoint the mayor—usually the leader of the majority party/coalition. Municipalities with fewer than 250 inhabitants use an open-list majoritarian system and are thus excluded from the main analyses.

10. Data from 2011 Spanish National Statistics Bureau, Padrón Municipal).

11. The analysis considers municipalities with fewer than 10,000 inhabitants, since larger municipalities have very different dynamics with regard to both women representation and corruption, and would not be comparable to cases around the thresholds. This decision is standard in studies of Spanish municipalities (e.g., Bagués and Campa 2021; Casas-Arce and Saiz 2015) and excludes 9.4% of the cases in any given year.

12. This operationalization is inevitably arbitrary. Looking at a longer time frame is currently not possible, since the second electoral cycle ended in 2015; the relevant outcome data for the subsequent elections are not yet available.

13. 71% of Spanish local councilors stayed in office two terms or less (see Appendix C for more details). Hence, we believe it is appropriate to treat the second term—four to eight years after first joining office—as a period when representatives can be considered experienced legislators. To the extent that experience matters in this context, this eight-year period should capture this effect.

14. The data were made available by the Dirección General de Coordinación de Competencias con las Comunidades Autónomas y Entidades Locales.

15. According to public opinion data, the share of Spaniards worried about corruption and fraud increased from less than 2% in the first decade of the twenty-first century to over 12% in 2012 (Fernandez-Vazquez, Barberá, and Rivero 2016).

16. Costas-Pérez, Solé-Ollé, and Sorribas-Navarro (2012), for instance, compared the original database with another list of cases compiled by the right-wing newspaper *El Mundo* and found no meaningful differences nor evidence of partisan bias.

17. Links to each individual corruption case are available in the online appendix.

18. The validity of this measure also rests on the assumption that, given the same evidence, men and women legislators have the same probability of being judicially charged in corruption cases.

19. Data on the local distribution of land comes from the Spanish general directorate of land registry (Dirección General del Catastro).

20. An interesting feature of this measure is that it captures a collective decision-making process. Although the mayor and council member responsible for city planning have a central role in these matters, no major alterations of land use regulations are possible without the support of a majority of the council.

21. We also conduct balance tests of several covariates across both population thresholds. These covariates are measured before the introduction of the quota. Hence, if the RDD assumption holds, there should be no discontinuity around either threshold for these predetermined covariates. This assumption is borne out by the data. We only find a significant discontinuity for one of the 23 outcomes tested (see Appendix Tables Table E1 and Table E2.

22. The results are substantively the same when the baseline in the discontinuity-in-differences framework is the previous term (Table E3), or when the outcomes are measured in levels rather than change (Table E4).

23. Figures 3, 5, and 6 provide a non-parametric approach to assess the validity of the design. See Tables 2–4 for the corresponding RD estimates and standard errors.

24. To provide a more intuitive estimate, consider the RD estimate using the level of revealed corruption in the 2007–11 period, without taking the difference relative to the pre-treatment period (2003–07). The results suggest that the probability of experiencing a corruption case in the following term decreased by 15.9 percentage points among local governments that adopted gender quotas compared to municipalities just below the cutoff. See Table E4 for the full results.

25. Parties with voluntary quotas prior to the 2007 election were the Galician Nationalist Bloc, Canarian Coalition, Catalan Republican Left, Initiative per Catalonia—Greens, United Left, the Party of Catalan Socialists, and the Spanish Socialist Party. Importantly, the fact that the share of women councilors was >40% before the introduction of the quota does not mean the quota had no impact. Without the introduction of gender quotas, it would be possible for a municipality to have an all-female local council in 2003 (before the first quota) and no women councilors in 2007. This test simply distinguishes between municipalities that were more or less likely to be impacted by the introduction of gender quotas.

26. Figure B1 describes the immediate effects of the quota around the 2011 threshold by treatment intensity. As in 2007, the results tend to be more negative

in high-dosage local councils, but the estimates are imprecise and therefore the results are not conclusive.

27. The underlying assumption in this test is that women elected in 2007 stay in office during the 2011–15 period, once they have acquired political experience. This assumption would be problematic if women elected in quota municipalities were systematically less likely to stay in office than their male colleagues or women in non-quota municipalities. Appendix C shows that the turnover rate of women councilors in quota municipalities is *not* systematically different than that of these other groups.

28. We do not discuss the Spurious Correlation argument here because the immediate effects described above demonstrate that there is no empirical support for this hypothesis.

29. A possible explanation for the lack of discernible experience effects is that reelection rates are relatively low in Spain (see Appendix Section C).

30. The exception is the electoral system, which is not personalized in Spanish local elections.

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Supporting Information

Additional supporting information may be found in the online version of this article at the publisher's web site:

Appendix S1 Supporting Information