Women and Power: Unpopular, Unwilling, or Held Back?

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Pablo Casas-Arce

Arizona State University

Albert Saiz

Massachusetts Institute of Technology

We use Spain's Equality Law to test for the existence of agency problems between party leaders and their constituents. The law mandates a 40 percent female quota on electoral lists in towns with populations above 5,000. Using pre- and postquota data by party and municipality, we implement a triple-difference design. We find that female quotas resulted in slightly better electoral results for the parties that were most affected by the quota. Our evidence shows that party leaders were not maximizing electoral results prior to the quota, suggesting the existence of agency problems that hinder female representation in political institutions.

I. Introduction

In most of the world's democracies, men are largely overrepresented in powerful positions in the public arena. Several theories may explain why

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women do not reach influential positions more often. Women could generally be “unwilling” to run for office. They may prefer alternative occupations, perhaps because they offer a better balance between professional and personal life. This could potentially leave a less competitive pool of available female candidates. Voters may have preferences for male representatives, rendering female candidates relatively “unpopular.” Parties would therefore rationally respond to this bias by fielding fewer women candidates. Alternatively, party leaders, who historically have been men, may decide to field fewer women at the expense of lowering votes. This practice may persist because of the inability of voters to control the actions of their leaders. This paper shows that the existence of such agency problems between political leaders and voters leads to female underrepresentation, harming the political aspirations of women, and likely lowering the welfare of voters.

When party leaders act in the best interest of their constituents, they select candidates to maximize electoral results. Hence, any constraint imposed on the discretion of party elites should result in worse electoral outcomes for those parties. We focus on the constraints imposed by female quotas on the selection of candidates. Exploiting an exogenous change in electoral rules, we test whether the introduction of female quotas effectively changed the electoral results of affected parties.

Spain’s government passed the Equality Law in 2007 to promote gender parity. The passage of the law—an indirect effect of the Madrid terrorist bombings—had been completely unanticipated by local political parties, candidates, and voters. It required parties to field lists for local elections with a minimum of 40 percent female candidates. However, the quotas applied only to municipalities with more than 5,000 inhabitants. The law effectively increased the presence of women on the affected lists by around 8 percentage points. This represented an increase of 27 percent in the number of female candidates. Moreover, the law forced parties to maintain the same minimum percentage of women in every five-position bracket of the list. As a result, the number of women in the top five positions also increased by a similar amount.

Using nonquota municipalities as controls and first-differencing vote shares by party and town between 2003 and 2007, we can factor out local and party-specific confounders as well as general changes in voters’ attitudes toward female politicians. We find that parties affected by the quota increased their vote share by more than their counterparts in the control group. Furthermore, parties that were forced to make larger relative increases in the number of female candidates slightly improved their electoral performance relative to other parties within the same municipality.

Voter turnout in the municipalities affected by the quota was not reduced as a result of the larger number of additional women candidates.

Thus the evidence shows that, at the margin, voters seemed to be happier with more balanced lists. These results are not consistent with the existence of substantial voter aversion to female candidates.

We also show that parties did not experience issues finding suitable female candidates to comply with the quotas. The quota was not associated with increased list attrition or difficulties forming new lists. Parties did not need to retain past female candidates more often or to promote existing female candidates to top positions either. Together with the results on electoral outcomes, this evidence is not consistent with the existence of major supply constraints for high-quality female candidates.

There is a large literature on the agency conflicts between politicians and voters (Barro 1973; Ferejohn 1986; Banks and Sundaram 1993; Persson and Tabellini 2000; Maskin and Tirole 2004; Myerson 2008). Although there is evidence that voters hold politicians accountable (Besley and Case 1995, 2003), democratic institutions are unlikely to eliminate agency costs entirely (Shleifer and Vishny 2002). Our paper shows that such agency problems lead to female underrepresentation, harming the political aspirations of women and likely lowering the welfare of voters.

There has also been considerable research trying to understand the effect of female candidates on electoral outcomes, with mixed results (Dolan 2004; Lawless and Fox 2010). Surveys consistently show the importance of gender stereotypes (McDermott 1997, 1998; Koch 2000; Lawless 2004). Women candidates are seen as more liberal, having an advantage in issues related to education, health, or poverty; however, men are seen as more competent managers when the issues relate to economics, crime, or the military (Dolan 2005a, 2005b). Polls also show that voters tend to vote for candidates of their own gender (Plutzer and Zipp 1996; Smith and Fox 2001).

However, the net effect of these findings on actual electoral results is unclear. Most of the literature in political science finds that female candidates who run for office tend to win at rates similar to those of males (Darcy and Schramm 1977; Welch et al. 1985; Burrell 1992; Gaddie and Bullock 1997; Fox and Oxley 2003). Some papers claim that women candidates obtain fewer votes (Frechette, Maniquet, and Morelli 2008), while others find a net positive effect (Hogan 2010). The caveat is that these studies are based on correlational evidence and suffer from (i) endogeneity problems (parties strategically choose candidate gender contingent on expected results)[1] and (ii) omitted ability biases (they could reflect voter discriminatory preferences that prevent all but the most extraordinary women from entering competitive elections). To the best of our knowledge, ours is the first attempt to measure the effect of female can-

[1] For instance, using data from the introduction of gender quotas in France at the national legislative level, Murray (2008, 551) finds evidence that "women are indeed placed in the most difficult seats" as candidates.

didates on votes using a quasi-experimental design that exogenously increased their number in a treatment group of municipalities and parties and left a control group untreated. Thus, we can eliminate the effects of potentially confounding factors on electoral outcomes.

Our paper complements previous evidence about the importance of internal political party dynamics in accounting for female underrepresentation (Sanbonmatsu 2002, 2006; Murray 2008; Bagues and Esteve-Volart 2012). It also relates to recent research on the policy effects of women in power (e.g., Chattopadhyay and Duflo 2004; Beaman et al. 2009).[2] This literature has focused mainly on the policy outcomes of female elected leaders, the effects of quotas on the number of elected women, or the change in attitudes toward women once they are elected, and not on the intrinsic theories that could account for women's underrepresentation.

Our research follows an extensive literature on discrimination in labor markets. There is evidence of gender discrimination in hiring (Goldin and Rouse 2000) and in product markets (Ayres and Siegelman 1995). A related literature on ethnic discrimination has studied market-driven preferences for residential segregation (e.g., Saiz and Wachter 2011) and in other less conventional environments (Kahn 2000; Szymanski 2000; Price and Wolfers 2010). Municipal elections allow us to study a relevant setting, yet one in which a very good performance measure is available: electoral results.

Finally, the paper is distantly related to an emerging literature that tries to explain gender inequality in outcomes using differences in tastes and attitudes (Crosson and Gneezy 2008). Women tend to display negative attitudes toward competition (Gneezy, Niederle, and Rustichini 2003; Gneezy and Rustichini 2004; Niederle and Vesterlun 2007). They also tend to see themselves as less qualified to run for office (Fox and Lawless 2004; Lawless and Fox 2005). These theories could explain why the average woman may be less likely to seek power. However, they cannot by themselves account for the lack of women at the top. In competitive environments, there could be enough women at the right tails of the ambition and ability distributions to satisfy a demand for more balanced gender allo-

[2] The literature on the impact of women in power on parliamentary votes, budget levels, budget composition, government stability, and government efficiency is now quite large. Note that we do not have much to contribute to this specific literature since we are not examining the policy impact of the additional elected women due to the quota. Instead, we use the natural experiment to learn about the causes of female underrepresentation on the candidate lists. For the reader interested in women politicians and their effect on policy, other examples of this burgeoning literature include Welch (1985), Swers (1998), Rehavi (2007), Clots-Figueras (2009), De Paola, Scoppa, and Lombardo (2010), Ferreira and Gyourko (2010), Funk and Gathman (2010), Cavalcanti and Tavares (2011), and Gagliarducci and Paserman (2012). Campa (2011) is the most relevant reference to our work: it studies the impact of quotas in Spain on the provision of public goods.

cations. We argue that one cannot fully understand female underrepresentation in positions of power without considering the interaction between the demand side of political markets (voter preferences), the marginal supply of qualified female candidates, and the industrial organization of the market: the role of parties, political leaders, and the degree of competition (Becker 1957).

The paper proceeds as follows. In Section II, we develop a simple model of electoral competition. The model shows that introducing a female quota cannot possibly lead to better electoral results for the affected party under vote-maximizing behavior. Section III describes the experimental design and data, and Section IV presents the results. We offer concluding remarks in Section V.

II. Female Representation and Quotas: Framework

Consider two political parties competing in an electoral contest. Each party selects the candidates to place on the ballot. We further assume that they choose ballots to maximize their expected vote shares. The selection of candidates is constrained by the distributions of characteristics among the pools of potential candidates available to their parties.

The expected vote share for party 1 is $\int g(x_1, x_2, \nu)\,dF(\nu)$, where $g(x_1, x_2, \nu)$ is the vote share of party 1, x_i is a K-dimensional vector that describes the characteristics of the candidates that party $i \in \{1, 2\}$ places on the ballot, and ν is a random shock that captures the influence of factors that voters observe during the campaign but that party officials cannot observe when choosing candidates. We assume that ν is orthogonal to both x_1 and x_2. Because there are only two parties, this is a zero-sum game.

We denote the pool of candidates available to party i by the set $\Omega_i \subset R^K$, so that $x_i \in \Omega_i$, and we assume that Ω_i is bounded. These sets capture the idea that parties face trade-offs because they have finite resources for recruiting candidates and a finite pool of potential candidates.

Assume that there exists a Nash equilibrium to this game, (x_1^*, x_2^*). Now suppose that party 1 has to choose candidates from the set $\hat{\Omega}_1$ such that $\hat{\Omega}_1 \subset \Omega_1$ and $x_1^* \notin \hat{\Omega}_1$. Party 1 is therefore constrained to select a ballot from a subset of the original set of candidates that does not contain the party's equilibrium strategy. On the other hand, the strategy set for party 2 remains unchanged. Further, let $(\hat{x}_1^*, \hat{x}_2^*)$ be some Nash equilibrium of the new game.

Proposition 1. The expected vote share for party 1 given any Nash equilibrium of the new game is weakly less than the expected vote share for party 1 in the original game, that is,

$$\int g(\hat{x}_1^*, \hat{x}_2^*, \nu)\,dF(\nu) \leq \int g(x_1^*, x_2^*, \nu)\,dF(\nu).$$

Proof. Because this is a two-player, zero-sum game, party 1 seeks to maximize its expected vote share while party 2 seeks to minimize party 1's expected vote share. Since Nash equilibria involve both players choosing best responses, we have

$$\int g(\hat{x}_1^*,\ x_2^*,\ \nu)\, dF(\nu) \leq \int g(x_1^*,\ x_2^*,\ \nu)\, dF(\nu)$$

because $\hat{x}_1^*$ is not a best response to x_2^*. Further, we also have that

$$\int g(\hat{x}_1^*,\ \hat{x}_2^*,\ \nu)\, dF(\nu) \leq \int g(\hat{x}_1^*,\ x_2^*,\ \nu)\, dF(\nu)$$

because x_2^* is not a best response to $\hat{x}_1^*$. Combining both inequalities yields the result. Q.E.D.

Party 1 chose its original equilibrium strategy in a game in which both parties were trying to maximize their own expected vote shares. As a result, party 1 cannot expect a higher vote share following the introduction of a policy that eliminates its ability to choose a strategy that it played in the equilibrium of the original game. Thus, if party 1 gets more votes, on average, following the introduction of such a policy, we must conclude that in the previous regime the two parties were not playing a vote share–maximizing strategy. The result suggests a simple explanation for our finding below that female quotas improve the electoral results of affected parties: quotas force some local party officials to choose female candidates that were "better"—in the sense of maximizing vote share—but party insiders preferred not to field them in the first place.

While this result does not generalize to N party settings without additional assumptions, we have established conditions such that the result does hold when party 1 faces more than one opponent.[3] Also, in our empirical work below, we present analyses in which we restrict the sample to municipalities that are dominated by two parties.

III. Experimental Environment

Town councilors in Spain are elected using closed lists (i.e., people vote for a list rather than for an individual person). Most lists concur under the umbrella of national or regional parties. Each list must present a number of candidates equal to the number of council seats at stake.[4] The

[3] The results are available from the authors on request.

[4] For example, the number of seats is 11 for municipalities between 2,000 and 5,000 inhabitants and 13 for municipalities between 5,000 and 10,000. Note that, in 2003, 94 percent of the lists in these population ranges obtained fewer than seven seats, and only one list in about 30,000 reached 11 seats. In practice, therefore, all candidates appearing after numbers 7–8 on a list can generally be understood as "filler" candidates.

seats are apportioned proportionally to vote shares using the d'Hont method. The law, however, establishes a minimum vote threshold of 5 percent in order for a list to qualify for the apportionment of seats.

Council members are drawn from each list using the exact order in which the candidates are listed. Upon convening for the first time, the council elects a mayor, typically the first person on the most-voted list.[5] The council also acts as a representative legislative body, passing and enacting all local budgets, laws, regulations, zoning, and tax codes for a period of 4 years.

On March 22, 2007, the Law for the Equality of Women and Men (the Equality Law henceforth) was passed by the Spanish Parliament. It required candidate lists in all elections to contain at least 40 percent of candidates from each gender. Moreover, in order to prevent parties from placing all women at the bottom of the list, the law required this proportion to be maintained for every bracket of five positions. Importantly, the law declared municipalities with fewer than 5,000 inhabitants exempt from the quota.

The law applied for the first time to the municipal elections held on May 27, 2007. In the previous election of May 25, 2003, no such legal change had been contemplated. Indeed, the passage of the law was made possible by the 2004 general election results, which were largely unanticipated (Montalvo 2011). Only days before the Madrid train bombings of March 11, 2004, the Christian-Democratic Party (PP) was widely expected to win the elections. The bombings and postattack management from the incumbent party changed the sentiment of many voters toward the Social-Democratic Party (PSOE), which won the elections 4 days after the terrorist strike.[6] It is therefore quite unlikely that the share of female candidates in the municipal elections of 2003 reflected an anticipation of the female quotas that were imposed in 2007.

The Spanish State Department (Ministerio del Interior) collects information related to the electoral process. On request, we obtained a nonconfidential subset of its data. We were provided with the names of all candidates by list in all municipal ballots in the 2007 and 2003 elections, their gender in 2007 (a disclosure required by the Equality Law), their list's affiliation with major parties, information about each individual's position on the lists, and the outcome of their candidacy.[7] We imputed gender in 2003 by using the first name of the candidate. Names in Spain have a very strong gender orientation, and only a very small portion of candidates in 2003 had gender-ambiguous names. We also have infor-

[5] In fact, only the first person on each list can be considered in the initial mayoral vote.

[6] Gender parity in lists had been an important point in PSOE's electoral platform (Verge 2006).

[7] Other characteristics, such as birth date, were suppressed from the data for confidentiality reasons.

mation about the number of votes for all lists presented, the fraction of null or blank votes, estimated populations in the municipal census, and the number of registered voters in each town.

Since the law applied only to municipalities with more than 5,000 inhabitants, we can obtain meaningful results only around this threshold. In order to ensure both comparability and large enough sample sizes, we restrict our study to municipalities with populations below 10,000 inhabitants. However, the results are not in the least sensitive to variations in this threshold. We also utilize data on unemployment rates (a very good local socioeconomic status indicator in Spain) and other economic characteristics of the towns. Results do not change with the inclusion of these controls and are omitted inasmuch as most specifications are in first differences. Furthermore, observables are invariant around the population level at which the quota binds. In contrast, there is a very strong effect of the quota on the number of women across lists on both sides of this discontinuity: a differential increase of 8 percentage points in towns with populations above 5,000 (see table 6 in Sec. IV.D). This figure amounts to a differential 27 percent increase in the number of women on lists in the towns affected by the quota.[8]

IV. Do Voters Dislike Female Politicians? Evidence

A. *Women and Electoral Results: Descriptive Evidence*

While the political slogan "when women run, women win" has been used to promote female political candidacy, the issue of the impact of women on electoral results is ultimately an empirical one. An extensive literature has studied the impact of parties fielding female candidates. Previous approaches were based on either studying self-reported voter preferences as captured in surveys or comparing outcomes by gender in single-candidate elections. The former approach may not faithfully capture actual behavior or provide reliable field predictions. Results from the latter approach need to be interpreted with caution: because the share of women running for office is low, those who actually run constitute a very selected group (Anzia and Berry 2011). Finding no gender differences in vote shares between marginal candidates is completely consistent with the existence of general voter biases against women; these biases could explain why only a relatively small percentage of extremely capable female candidates can afford to compete.

[8] Women amounted to 30 percent of the total number of candidates in the 2003 municipal elections in towns with populations below 10,000—the ones we study here. Results about the causal impact of the quota on female representation are robust to considering alternative control-treatment samples and to the inclusion of polynomials in population, municipal fixed effects, and other controls. See Campa (2011) and Casas-Arce and Saiz (2011) for further analyses.

In order to illustrate the pitfalls of correlational approaches and to better understand the patterns of women's participation before the quotas, we begin by showing the associations between the share of women on lists and electoral outcomes in 2003. We focus on small towns with populations below 10,000—our sample of interest. The dependent variable in table 1 is the voting share, and the unit of observation is each of the 14,333 party lists competing in the relevant 4,582 municipalities. The main dependent variable is the share of women in the top five positions of each list as, presumably, women vying for top positions were more visible to voters. We will focus on the overall female share in Section IV.C.[9]

The descriptive evidence in table 1, column 1, shows that lists with more women at the top fared significantly worse in the elections of 2003. The results are robust to including municipal (col. 2) and party (col. 3) fixed effects. In column 4, we focus on the two major national parties, which amounted to 60 percent of lists and 67 percent of votes in our sample. We find that whenever one of the major national parties fielded a relatively large number of women, it tended to fare worse. These negative results are quantitatively large, suggesting that increasing the female share of candidates at the top by 20 percent (one woman) was associated with a loss in vote share of 3.7 percentage points (about 10 percent of the average vote share).

As in previous correlational studies, this evidence cannot be interpreted as capturing voters' gender preferences or candidate ability. In fact, reverse causality is likely a major issue. In panel B of table 1, we run the inverse regression with the female candidate share on the left-hand side, focusing on the two main national parties. Were the main parties systematically fielding fewer women in municipalities where they were likely to win more seats? To test this hypothesis, we instrument each party's vote share in the 2003 municipal elections using the vote share they obtain, at the municipal level, in the 1999 European Parliament elections. The candidates and substantive issues in such elections are common to the whole country and are completely unrelated to local elections. While European elections are widely perceived as irrelevant by voters, they elicit general political sentiment for or against the major national parties (Binzer Hobolt and Wittrock 2011). This measure of general political orientation in the town is very strongly associated with the local vote for one or the other party in municipal elections: the R^2 in the first stage of two-stage least squares (2SLS) is .49.[10] The results of this instrumental

[9] Results are very similar if we focus on the total share of women on the lists. In these local elections, many voters can be swayed to vote for a list because of personal or family relationships with the candidate(s), regardless of their position on the lists.

[10] Solé-Ollé and Viladecans-Marsal (2012) show that local ideological preferences for each party in Spain are very persistent over time (since the first democratic elections in 1978) and do affect the outcomes of municipal elections.

TABLE 1
OBSERVATIONAL ASSOCIATION BETWEEN FEMALE PARTICIPATION AND ELECTORAL RESULTS

	A. Lists with More Women at the Top Tend to Obtain Fewer Votes: Share of Total Votes for List in 2003 Elections			
	(1)	(2)	(3)	(4)
Share of women in list (top five positions)	−.112***	−.129***	−.136***	−.186***
	(.009)	(.012)	(.011)	(.019)
Municipality fixed effects	No	Yes	Yes	Yes
Party fixed effects	No	No	Yes	Yes
Limit to only two main national parties	No	No	No	Yes
N (Lists)	14,333	14,333	14,333	8,503
Municipalities	4,582	4,582	4,582	4,441

	B. Mostly Due to Reverse Causation: Share of Women on List (Top Five Positions)		
		IV: Vote Shares in 1999 European Elections	
	OLS (1)	(2)	(3)
Share of total votes for list in 2003 elections	−.129***	−.108***	−.100***
	(.013)	(.017)	(.036)
List wins 2003 elections			−.005
			(.010)
Municipality fixed effects	Yes	Yes	Yes
Party fixed effects	Yes	Yes	Yes
Only two main parties	Yes	Yes	Yes
N[a]	8,114	8,114	8,114
Municipalities	4,057	4,057	4,057
F-test of excluded instruments	NA	2,567	1,059
p-value of Sargan test	NA	.0025	.002

NOTE.—H_0: Parameter = 0. Numbers in parentheses are standard errors. The results in panel A display estimates of the OLS coefficients of the share of women in the top five positions of a party's list on the total vote share won by that list in the 2003 municipal elections. Column 1 displays uncontrolled regressions; col. 2 introduces municipality fixed effects (all the variation is in the female share within a town); col. 3 introduces major political party fixed effects; and col. 4 is limited to the lists fielded by the two larger national parties, PP and PSOE. Panel B focuses on the two major national parties (PP and PSOE). It shows the reverse regression in which the share of women in the top five positions in the party list in 2003 is the dependent variable and the share of votes in 2003 is the main explanatory variable. With unbiased expectations, the average share of votes should proxy for ex ante expectations about electoral outcomes. Column 1 limits the estimation to municipalities where both PP and PSOE presented a list (89 percent of all relevant municipalities). In col. 2 we use a 2SLS specification and instrument for the share of votes won by the two major parties by municipality in 2003 with the parties' vote share in that locality in the 1999 European elections. We restrict all estimates in the table to municipalities with populations below 10,000 inhabitants in order to make the results comparable with those in other tables.

[a] The number of lists from PSOE and PP with European 1999 data.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

variables (IV) strategy (table 1, panel B, col. 2) are statistically indistinguishable from the ordinary least squares (OLS) reverse causation equation (table 1, panel B, col. 1). They suggest that the correlation between the share of female candidates and electoral results is driven by the actions of parties, not by the preferences of voters.

Of course, it is possible that the local parties that were likely to win in 2003 had already been in power previously. Historically, executive teams have tended to be male. Furthermore, it seems likely that parties could try to minimize turnover in governing teams. Nevertheless, controlling for the lists' winning the 2003 elections (i.e., obtaining the highest vote share) does not change the results (table 1, panel B, col. 3, including a dummy for the most-voted lists). Nonwinner lists that expected to increase their number of seats in the local legislative body tended to decrease their share of women at the top. Rather than winning elections, it seemed to be winning seats that changed the internal party dynamics of female promotion. Note that "winner status" is certainly an endogenous regressor that should be negatively associated with the female candidate share if voters really disliked female-laden governing teams (as opposed to women on nongoverning lists). However, both variables happen to be uncorrelated after controlling for vote shares.

It is clear that in towns where a party was generally strong and expected to gain many seats in the municipal elections, fewer women placed on its list. Conversely, more women ran for office in towns where the party was in a more competitive situation. These results are consistent with agency problems between political leaders and voters pushing women aside from positions of power. They also illustrate the difficulties associated with studying female electoral success in a context of substantial endogeneity.

B. A Natural Experiment Induced by the Quota: "Bipartisan Male Holdout" Lists

The previous results do not conclusively prove the existence of agency problems that hinder female advancement. Fortunately, the introduction of the Equality Law provides us with an experimental design that is akin to the random forceful introduction of more women onto the electoral lists where they used to be underrepresented. If the vote-maximizing best response of a party was to include fewer women, the new constraint on behavior should reduce its vote share.

We focus on a subset of lists in which the competitive environment was well defined and the quota was uniquely binding. Specifically, we begin by focusing on municipalities where the two most-voted lists obtained a joint share of more than 80 percent of the council seats in the 2003 local elections and also fielded lists in 2007: we call these bipartisan towns. We further limit our attention to municipalities where one of the main parties was initially fielding less than 40 percent of females (we call these lists "male holdouts") while their main competitor was already above that threshold in the 2003 elections. Towns with a male holdout list are interesting to study because only one of the major parties was affected by the quota, and we can directly apply the conceptual test in proposition 1. If parties were maximizing vote shares in 2003, we expect the party affected by the quota to fare worse in 2007.[11]

We implement a difference-in-differences matching design (Smith and Todd 2005) by comparing changes in vote shares before and after the quota, across male holdout lists in quota and nonquota bipartisan municipalities. Of all bipartisan towns with a single male holdout list and a population under 10,000, we have only 144 in which the quota became binding by law in 2007. There were 1,345 bipartisan towns with one male holdout list that remained unaffected by the quota in 2007 (populations below 5,000). Thus, we have many more observations in the nonquota group. Furthermore, the observable characteristics of male holdout lists in the quota and nonquota groups are somewhat different (table 2).

Treatment and control groups that differ on observables should not be directly compared (LaLonde 1986; Heckman et al. 1998); therefore, we estimate a propensity score equation to produce a closely comparable control group. Specifically, we first estimate the equation on a quota dummy using observable characteristics and match each of the 144 observations in the "quota male holdout" group with the 10 closest "nonquota male holdout" observations by propensity score (with repetition).[12] Importantly, results are not sensitive to changing the matching technique or the number of matched control observations. Table 2 shows that matching on the propensity score does very well in selecting nonquota lists that were ex ante similar to those in the quota sample. Note that by taking first differences on the outcome of interest, we mitigate other potential omitted variables problems (Smith and Todd 2005).

In table 3 we compare the evolution of male holdout parties across quota (treatment) and nonquota (control) bipartisan municipalities. While the major list competing with the male holdout party in each of these towns was not forced to increase its female share, it could have done so as a best reaction to the changes by the male holdouts. Therefore, we cross-tabulate the main outcome of interest—the 2003–7 change

[11] In an earlier version, we used an alternative sample including all municipalities (not necessarily bipartisan) with a single male holdout list in 2003. The results, available from the authors on request, are unchanged.

[12] Each thusly paired "control" observation is given a weight of 1/10. Since many of the control observations are matched to several treatment observations, we end up with 591 comparable male holdout lists in municipalities where the quota did not apply. We use the routine "psmatch2" by Leuven and Sianesi (2012).

TABLE 2

MALE HOLDOUT LISTS IN BIPARTISAN ELECTIONS: OBSERVABLE CHARACTERISTICS

		Nonquota		Differences	
	Quota: All (1)	All (2)	Weighted Matched Sample (3)	Raw (1) − (2) (4)	Matched (1) − (3) (5)
Initial vote share	.45	.49	.45	−.04***	.00
	(.01)	(.00)	(.01)	(.01)	(.01)
Initial share of women on list	.17	.14	.17	.03***	.00
	(.01)	(.00)	(.00)	(.01)	(.01)
Initial electoral participation	.73	.78	.73	−.06***	.00
	(.01)	(.00)	(.00)	(.01)	(.01)
Number of parties in election	3.94	2.96	3.90	.99***	.05
	(.09)	(.03)	(.05)	(.09)	(.09)
Initial Herfindahl index (vote shares)	.42	.49	.42	−.07***	.00
	(.01)	(.00)	(.00)	(.01)	(.01)
Socialist Party (PSOE)	.24	.32	.23	−.08**	.01
	(.04)	(.01)	(.02)	(.04)	(.03)
Conservative Party (PP)	.55	.49	.56	.05	−.01
	(.04)	(.01)	(.02)	(.04)	(.04)
Izquierda Unida (IU)	.05	.03	.04	.02	.01
	(.02)	(.00)	(.01)	(.02)	(.02)
Esquerra Republicana de Catalunya (ERC)	.03	.02	.03	.00	.00
	(.01)	(.00)	(.01)	(.01)	(.01)
Convergencia I Unio (CIU)	.06	.05	.06	.01	.00
	(.02)	(.01)	(.01)	(.02)	(.02)
Partido Andalucista (PA)	.01	.01	.01	.00	.00
	(.01)	(.00)	(.00)	(.01)	(.01)
Bloque Nationalista Galego (BNG)	.00	.00	.00	.00	.00
	(.00)	(.00)	(.00)	(.00)	(.00)
Number of lists/municipalities	144	1,354	591	1,498	735

NOTE.—H_0: |difference in means| > 0. Entries are the means and standard deviations (in parentheses) of the observable variables that apply to male holdout lists in bipartisan elections. Some of the characteristics pertain to the lists and some to the municipalities in which the lists competed. Bipartisan male holdout lists are defined by the following characteristics: (i) they were one of the two most-voted parties in their municipality in 2003 and participated also in the 2007 elections; (ii) they are in municipalities where the two most-voted parties obtained 80 percent or more of the council seats in 2003; (iii) these lists were fielding less than 40 percent of female candidates in 2003, while their main competitors were already above that level in the 2003 elections. In col. 1, we show descriptives for two-party male holdout lists in municipalities where the quota became binding in 2007 (populations between 5,000 and 10,000). In col. 2, we show data for municipalities where the quota did not apply in 2007 (populations between 0 and 5,000). In col. 3, we present descriptive statistics for those male holdout municipalities that we matched to the quota ones using propensity scores. In order to calculate propensity scores, we used all the variables in this table to fit a quota-dummy logit model. We then matched each observation in the quota group to the 10 closest observations in the nonquota group based on the estimated propensity score, allowing for repetition across matches. Columns 4 and 5 present tests of differences in means between the quota and the broader and matched nonquota samples, respectively. While quota and nonquota observations are very different, we cannot reject equality in averages across the quota and matched nonquota groups.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

TABLE 3
EVOLUTION OF VOTE SHARES OF MALE HOLDOUT LISTS IN BIPARTISAN ELECTIONS

	Average Change in Vote Share of Male Holdout Lists between 2003 and 2007 by Number of Women in Competitor Party			
	Unchanged (1)	Increasing (2)	Decreasing (3)	All (4)
	Matched Municipalities in Which the Quota Did Not Become Binding			
1. List had less than 40% women in 2003 and 2007	−.061	−.070	−.065	−.063
	(.009)	(.017)	(.013)	(.007)
N complier control	153	49	132	334
2. List had less than 40% women in 2003 but 40% or more in 2007	−.021	−.088	−.030	−.033
	(.010)	(.015)	(.014)	(.007)
N noncomplier control	134	38	85	257
3. Overall in control lists	−.042	−.079	−.052	−.050
	(.007)	(.011)	(.010)	(.005)
N in control	287	87	217	591
	Quota Binding			
4. List was bound by the quota in 2007	−.020	−.001	.060	−.009
	(.008)	(.016)	(.032)	(.007)
N in treatment quota	100	31	13	144
	Treatment Effects			
5. Average effect of a binding quota	.022	.078	.113	.042
	(.010)	(.018)	(.022)	(.008)
6. Wald estimate of effect of gender parity	.042	.147	.176	.074
	(.019)	(.039)	(.038)	(.014)

NOTE.—All coefficients in rows 5 and 6 are statistically different from zero with p-test values such that $p < .01$. Each cell in this table displays average changes in vote shares between 2003 and 2007 among male holdout lists in bipartisan towns in 2003 that also competed in 2007. See table 2 for the definition of bipartisan male holdout lists. Rows 1–4 cross-tabulate data by quota compliance in 2007 and by the average reaction of lists competing in the same municipalities as male holdouts (cols. 1–4). Rows 1–4 show average changes in voting shares, displayed together with estimated standard errors of their means (in parentheses) and the number of observations in each cell. Nonquota observations correspond to the propensity score–matched observations in table 2, col. 3. Row 1 focuses on lists in towns where the quota did not apply and that fielded less than 40 percent of female candidates in the top five positions (compliers with their control [nonquota] assignment). Row 2 focuses on data for lists that were in nonquota towns but decided to apply the 40 percent quota (noncompliers with their control [nonquota] assignment). Row 3 shows average vote share changes for all nonquota observations. Row 4 shows average vote share changes for male holdout lists that were affected by the quota in 2007. Row 5 presents average differences in outcomes between quota and nonquota observations and corresponding standard errors in parentheses. Row 6 presents Wald estimates (and standard errors in parentheses) that adjust for the actual probability of receiving the 40 percent female treatment between quota and nonquota groups. Column 1 focuses on male holdout lists in towns where the competing parties had changed their female share between 2003 and 2007 by less than 10 percentage points on average (vote-weighted). Column 2 displays results for male holdout lists in municipalities where all other lists increased their female share in 2007 by more than 10 percentage points. Column 3 shows estimates for male holdout lists in municipalities where all other lists decreased their female share in 2007 by more than 10 percentage points. Column 4 shows aggregate results for all male holdout lists in the quota and matched control samples.

in vote share of the male holdout list—by the reaction of its competitor (columns), based on whether its female share did not change (col. 1), increased (col. 2), or decreased (col. 3) in 2007. Our focus is on column 1, where only male holdout lists substantially changed female representation as a result of the quota (representing 70 percent of the treatment group).

In the first row we examine male holdout lists in which the quota was not enforced and that kept fewer than two women in the top five positions in 2007 (compliers). These lists suffered a substantial drop in vote share between elections. Noncomplier lists in the control group (lists that increased their female share to at least 40 percent, despite not being required to do so) experienced a lower reduction in vote shares (row 2), and so did the lists that were actually subject to the quota (row 3).

There was a substantial number of noncomplying lists that were not bound by the quota but behaved as if they were. This could be due to general social change favoring more women at the top but also may have been related to an "encouragement effect" of the law. In this environment, Wald IV estimates are useful in comparing relative changes in outcomes to relative probabilities of receiving the actual treatment of interest. We therefore present both the binding-quota effect proper (the overall difference between quota and nonquota groups)—in row 5—and a Wald IV estimate of the local treatment effect of moving toward gender parity in the top positions—in row 6.[13] We obtain the latter by dividing the former effect by the difference in the probability of receiving a gender-parity treatment (fielding at least 40 percent women candidates) across municipalities above and below the 5,000 population threshold. In column 4, we present these results aggregating across reaction functions of competing parties. Contrary to the previous correlations, lists with growing numbers of women at the top (voluntarily or forced) tended to see slightly better electoral outcomes.

The results in table 3 are clearly inconsistent with a negative impact of women candidates on electoral outcomes. However, as demonstrated in LaLonde (1986), nonparametric treatment effect estimates could be sensitive to specification and the composition of the treatment and control samples. Taking first differences in the outcome of interest and making the samples comparable by matching may attenuate—but not totally eliminate—these problems (Heckman et al. 1998; Smith and Todd 2005). Thus, in table 4 we extend the analysis to the whole (unmatched) sample of male holdout lists in bipartisan towns. We start by repeating the previous difference-in-differences (col. 1) and Wald IV (col. 2) specifications, using changes in voting share between 2003 and 2007 as the main

[13] Because the law mandates a gender quota in the top five positions, the closest that it can come to parity is a 40 percent minimum.

TABLE 4
PARAMETRIC EFFECT OF QUOTA ON MALE HOLDOUT LISTS IN BIPARTISAN ELECTIONS

	ΔSHARE OF VOTE 2003–7			SHARE OF VOTE IN 2007	
	OLS	IV = Quota Dummy		OLS	IV = Quota Dummy
	(1)	(2)	(3)	(4)	(5)
Quota was enforced	.026**			.022***	
	(.011)			(.010)	
Gender parity in top five positions (Wald IV)		.046**	.071***		.054**
		(.020)	(.026)		(.025)
List's 2003 share of vote				.145	.140
				(.108)	(.118)
List's 2003 share of vote squared				.678***	.692***
				(.123)	(.133)
List's initial female share in the top five				.036	−.018
				(.024)	(.037)
Initial vote share Herfindahl index in town				−.382***	−.387***
				(.027)	(.027)
Number of parties in election in town				−.052***	−.052***
				(.006)	(.006)
Initial voter turnout in town				.016	.002
				(.062)	(.053)
Main party fixed effects	No	No	No	Yes	Yes
Quadratic in population levels	No	No	No	Yes	Yes
Limit to population between 4,000 and 6,000	No	No	Yes	No	No
R^2	.004			.624	
Observations	1,498	1,498	123	1,498	1,498
F-test of excluded instruments	. . .	994.74	40.119	. . .	62.985

NOTE.—Standard errors, clustered at the region level, are in parentheses. Column 1 presents OLS regressions with the change in vote share by electoral list between 2003 and 2007 as the dependent variable. All male holdout electoral lists in bipartisan municipalities with populations below 10,000 are included. See table 2 for the definition of bipartisan male holdout lists. Column 2 shows the counterpart IV regression in which we instrument for changes to gender parity in the list using a "quota applies" dummy. Column 3 restricts the IV regressions to municipalities with populations between 4,000 and 6,000. Column 4 presents results from an OLS specification in which the vote share by list in 2007 is the main dependent variable. We then control for a quadratic in vote shares in 2003 and other controls, including political party fixed effects and a quadratic in population levels. The main independent variables are a dummy for the Equality Law (the quota is binding in municipalities with populations above 5,000). Column 5 shows the equivalent IV specification.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

dependent variable.[14] Column 3 repeats the IV estimates for male holdout lists in towns with populations between 4,000 and 6,000, close to the threshold around which the quota was imposed. While the sample is much reduced and the instrument much weaker, the results are similar.

In column 4, we focus on the vote share level in 2007 for each male holdout list as the main dependent variable. We then control parametrically for lagged vote share in 2003 and its square, party fixed effects, and characteristics of the municipalities: a polynomial in the town's population, the number of lists in the election, the 2003 Herfindahl index, and a quadratic in lagged voter turnout.[15] Column 5 presents the Wald IV estimates, where we instrument for the list moving to gender parity with a dummy for the quota being enforced in its town. Using the broadest control group and parametric methods, we find that forcing male holdout lists to gender parity increased their vote share by about 5.4 percentage points, representing an exchange of about 118 votes in the local elections under consideration.

Again, we can reject the hypothesis that male holdout lists that were forced to enact gender parity fared worse. In fact, the evidence is more consistent with the view that they tended to do slightly better, suggesting that political leaders were not maximizing electoral results prior to the introduction of the quota.

C. *Relative Growth of Female Candidates and Vote Share*

We now generalize our results to all candidates and electoral lists affected by the quota. It is well known that "friends and neighbors" of candidates have a strong impact on the outcomes of local elections (Key and Heard 1949; Smith 2010). New female candidates may have been effective in canvasing voters from their social networks in the relatively small towns that we study, irrespective of whether they were likely to be elected themselves. Furthermore, in many towns, several or even all parties were affected by the quota, but to different degrees: those with fewer women were forced to field more. We can therefore use the larger variation in the effects of the quota on the total share of women on the lists to estimate the broader impact of female candidates on relative electoral outcomes.

In order to motivate this broader empirical approach, we start by showing—in table 5, column 1—that the further a list was forced by the quota to increase its number of female candidates, the better it fared

[14] Standard errors in all regressions henceforth are clustered at the regional level, but they are not sensitive to the level of geographical clustering (Barrios et al. 2012).

[15] The coefficients on a quadratic in population are highly insignificant: there is no evidence of additional trends in changes in vote shares for male holdout lists across towns of different population levels.

TABLE 5

POSITIVE IMPACT OF WOMEN ON ELECTORAL RESULTS: ALL LISTS AND CANDIDATES

	Change in Share of the Vote for List 2003–7					
	All Towns (1)	All Towns (2)	Excludes Two-Party Male Holdout Towns (3)	Population 4,000–6,000 (4)	Noncompetitive Elections (5)	Competitive Elections (6)
Change in female share on the list		.424***	.474***	.303	.518***	.272*
		(.116)	(.183)	(.257)	(.200)	(.155)
(.4 − female share in 2003) × quota binding	.166***					
	(.056)					
Observations (lists by municipality in 2007)	11,556		7,793	1,195	2,141	9,415
Number of municipalities	4,370		2,876	376	737	3,627
2SLS: instrument	None (OLS)	(.4 − female share in 2003) × quota binding				
Controls for quadratic in female share in 2003	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Party fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Party vote share in 2003					
F-test of excluded instrument	NA	135.52	70.06	17.12	60.22	90.977

NOTE.—Standard errors, clustered at the region level, are in parentheses. The main dependent variable in all specifications is the change in vote shares between 2003 and 2007 by list in all repeat lists in municipalities with populations below 10,000. All specifications include town and national/regional party fixed effects and use 2003 vote shares as weights. Column 1 presents OLS regressions in which the main independent variable is the interaction between a dummy taking value one if the quota was binding for the list (the quota applied in its municipality, and the list was initially fielding less than 40 percent women candidates) and the distance between 40 percent and the share of female candidates on the list in 2003. As in the other specifications in this table, the regression controls for a quadratic in the lists' 2003 female share: the rationale here is that an additional arbitrary piecewise function of the distance from the initial female share to 40 percent—applying only to an arbitrary set of towns—should not matter unless the quota did have an effect on electoral results. In cols. 2–6, we use the preexisting distance from a 40 percent female share interacted with a dummy for the quota being binding in 2007 to instrument for the change in the female share in the list between 2003 and 2007. The instrument is typically very strong, as captured in the first-stage F-statistics of the last row: parties that were further away from the quota in towns where it was applicable were forced to introduce more women on their lists. Column 3 excludes municipalities with a single male holdout list as defined in table 2: here, there is no overlap with the samples in tables 2, 3, and 4. Column 4 focuses on municipalities with populations between 4,000 and 6,000. Columns 5 and 6 present results for the alternative subsamples of noncompetitive and competitive elections. We define noncompetitive elections as those in which one party enjoyed an absolute majority in the municipal council in 2003.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

in the 2003 elections. The unit of observation in the OLS regression is each of the municipal lists concurring in the 2003 and 2007 elections. The dependent variable—on the left-hand side—is the change in the vote share of each list. The main independent variable is $D_{ki} \times (0.4 - \mathrm{FemSh}_{ki,03})$, where D_{ki} is a dummy that takes value one if municipality i's population in 2007 was above 5,000 and party k's female share in 2003 (denoted by $\mathrm{FemSh}_{ki,03}$) was below 40 percent. The further away a list was from the quota in the towns where it was binding, the more women it was forced to incorporate. Of course, voter preferences could be changing over time, affecting parties with different initial numbers of women in different ways. Therefore, we also control for a quadratic in the initial list's share of women in 2003 on the right-hand side in all specifications in table 5.[16] Municipal fixed effects are included in all specifications as well. If all lists in a town were similarly affected by the Equality Law, we would not expect the quota to have much effect on vote shares. By controlling for town fixed effects, we are effectively looking at deviations of the change in the number of women and vote shares relative to their town's averages. Finally, we include main party fixed effects (e.g., PSOE, PP) to account for broader changes in political sentiment and weight all observations—party lists—by their vote share in 2003 to obtain voter representative results.

The regression considers 11,556 repeat lists in 4,364 municipalities. The results (table 5, col. 1) indicate that lists in quota towns that were forced to increase their female representation more dramatically fared relatively better, gaining voting share. Note that we are first-differencing the dependent variable, controlling for initial female shares (including a quadratic term), and saturating the model with town and party fixed effects. It is thus extremely difficult to account for the additional importance of a particular function of the initial relative distance from a 40 percent female share in lists and towns where the quota was binding using alternative explanations.

In column 2 we use $D_{ki} \times (0.4 - \mathrm{FemSh}_{ki,03})$ as an instrument for the endogenous regressor of interest: $\Delta\mathrm{FemSh}_{ki}$, the change in the share of women in party k of municipality i between 2003 and 2007. The inclusion of municipal fixed effects signifies that we are looking at relative changes in vote and female shares within municipalities. Overall, the identification strategy is conceptually based on triple-differencing the data and considering simultaneously differences in vote shares by party between 2003 and 2007,[17] differences in propensities to introducing

[16] Note, therefore, that the empirical strategy is not based on comparing lists with many initial female candidates to lists with few. Rather, we are conditioning on the initial number of women and comparing parties affected by the quota to nonaffected parties.

[17] Note that some lists dropped from contention in 2007, and some new lists appeared; however, the probability of attrition or new list formation was orthogonal to the application of the quota (Casas-Arce and Saiz 2011).

more females on the lists between quota and nonquota towns in 2007, and relative differences in how far from the quota each party was in 2003 within its municipality. The results suggest that a party that was forced to increase its female share by 10 percentage points more than its opponents experienced a 4.2 percentage point gain, which would, on average, imply 53 votes shifting party allegiance out of about 2,500 cast. While the effect is relatively modest, we can reject the hypothesis that parties that were fielding fewer women did so to maximize their electoral results.

In column 3 we exclude towns with bipartisan elections and male holdout lists in order to make sure that we are not solely exploiting the same source of variance as in tables 3 and 4. The results are unchanged.

Note that our research design is not based on a regression discontinuity because the outcome (vote shares) cannot possibly change in the aggregate across both sides of the boundary that defines the quota (population 5,000) and because of the heterogeneity and interdependence of the "treatment dosage" across observations. Nevertheless, it is still interesting to analyze whether or not the results are sensitive to focusing on municipalities that are very close in terms of population. Concretely, in column 4 of table 5, we limit our attention to municipalities with populations between 4,000 and 6,000. The 2SLS results are broadly similar to previous ones, but standard errors are about 2.5 times as large as the ones in the baseline estimation (col. 2) and the instruments are consequently weaker. These features are solely driven by the smaller sample size (fewer than one in 10 of the original municipalities): parameter estimates do not change much, but standard errors steadily decrease as we go on increasing sample sizes on both sides of the population distribution.

Finally, we separate municipal elections on the basis of their degree of competitiveness (cols. 5 and 6). We operationalize a definition of noncompetitive municipalities as those where the largest party obtained seven or more seats (out of a maximum total of 13) in 2003, an absolute majority. Parties with severe agency conflicts would be less likely to survive in very competitive electoral environments. Hence, we would expect parties to represent the preferences of voters more closely in competitive elections (Besley and Case 2003). Consistent with this, parties seem to have deviated more from the optimal candidate mix in noncompetitive environments (col. 5) and gained larger voter shares when forced to field more women.[18]

[18] The one-sided test with the hypothesis that the coefficient on competitive municipalities is larger yields a *t*-statistic of 1.511 and—given the large sample—a *p*-statistic of .065. Alternative definitions of noncompetitive environments—such as a high Herfindahl index in initial vote shares or the difference between the party with the most votes and its closest competitor—always yield the same picture: parties with "exogenously forced" women improved their electoral performance more strongly in less competitive towns.

D. *Availability of Willing Women, Voter Turnout, and Other Margins of Adjustment*

Our results so far point to the presence of agency problems with party leaders that limit women's political careers. We now conduct further tests for other explanations.

One concern with our earlier results is that the quota forced some parties (especially those with few women) to drop out of the 2007 elections because of a lack of female candidates. In our earlier working paper (Casas-Arce and Saiz 2011), we show that there was no effect on list attrition or increased difficulty in forming new lists: even lists in quota municipalities with no female candidates at the start were no more likely to drop out of the elections after the quota.

If qualified women had been scarce, another margin of adjustment for parties could have been to increase the retention rate of incumbent female candidates. Since we have the full names of candidates, we can trace their participation across years and lists. Interestingly, in nonquota municipalities, the probability that a woman had already been on a party's list in the previous election—32 percent—was much lower than the equivalent percentage for men—43.55 percent. Women candidates experienced more turnover between elections than men, a fact that is not explained by their position on the list (similar results are obtained conditioning on rank in the list). Therefore, if qualified women candidates were truly scarce, one would expect more efforts geared toward increasing their retention rate. In panel A of table 6, we estimate linear probabilistic models in which the observations are all candidates in the 2007 election (postquota). The left-hand-side (dependent) variable is a dummy or indicator variable describing an attribute of each candidate within a party. The main independent variable is a dummy for municipalities where the quota was binding. We control for population polynomials (cubic) that differ across both sides of the level that enforces the quota (population at 5,000) and party and regional fixed effects. In column 1 the dummy variable takes value one if the candidate is a woman. In column 2 the dependent variable is a dummy that takes value one if the candidate is a woman appearing on the list for the first time in 2007. The coefficient estimates in the latter column are similar to those in the former (if anything, higher), suggesting that parties did not fill the new female positions required by the quota by increasing retention but rather by bringing in new women candidates.[19] In columns 3 and 4 we repeat these regressions, focusing on the top five positions. While the quota required more female candidates overall, parties could have easily shifted incumbent lower-ranked candidates to the

[19] Notice that all women in newly formed parties are new candidates. For this reason, and although party attrition and formation are unrelated to the quota, the regressions concentrate on those lists that were present in both the 2003 and 2007 elections.

TABLE 6
Effects of Quotas on Female Candidate Promotion and Voter Turnout

	A. New Women on Lists Where the Quota Was Binding (2007 Election; OLS)			
	All Candidates		Top Five Positions	
	Female Candidate (1)	Female Candidate Appearing Only on the 2007 List (2)	Female Candidate (3)	Female Candidate Appearing Only on the 2007 List (4)
Quota applies	.079***	.086***	.058***	.057***
	(.010)	(.012)	(.013)	(.017)
National party and *comunidad autonoma* fixed effects	Yes		Yes	
Observations: all 2007 candidates on all repeat lists	110,229		59,128	
Observations: municipalities	4,662		4,662	
R^2	.02	.02	.02	.01

B. Voter Turnout in Towns Where the Quota Was Binding

	Town's Average Voter Participation Rate: 2003 (OLS) (1)	Town's Average Change in Voter Participation Rate: 2003–7 (IV: Quota Applies × Distance from Quota) (2)
Town's average share of female candidates: 2003	.073*	
	(.04)	
Town's average change in share of female candidates: 2003–7		.020
		(.034)
Observations: municipalities	4,718	4,718
Other town variables in table 2	Yes	Yes
R^2	.30	.21

Note.—Standard errors are in parentheses. Panel A offers the results of OLS regressions in which the dependent variable is a dummy taking value one if the characteristics in the top row apply (e.g., female = 1, male = 0). Each individual observation corresponds to a candidate in the 2007 elections. The sample contains all electoral lists that were present in both the 2003 and 2007 elections, in towns with populations below 10,000. All regressions include national party and *comunidad autonoma* (region) fixed effects. The coefficient presented corresponds to the quota dummy (towns above 5,000) and therefore captures the estimate of the discontinuity caused by the quota on the share of candidates for which the dependent variable takes value one (e.g., change in female share). Regressions include cubics in population on both sides of the discontinuity. Panel B presents OLS regressions in which the unit of observation is each municipality. In col. 1, we focus on the voter participation—or turnout—rate (valid votes/registered voters) in 2003 (prequota): the main explanatory variable is the total share of female candidates in the elections, and we also control for all town observable variables contained in table 2. In col. 2, the main dependent variable is the change in the voter participation rate by town between the 2003 and 2007 elections. The main explanatory variable is the change in the share of female candidates, which we instrument in a 2SLS specification using the interaction between a quota dummy and the initial distance of the average town's female share from 40 percent (variables in table 2 that pertain to the municipality are also included).

* $p < .1$.
** $p < .05$.
*** $p < .01$.

top positions. What we find, however, is that most of these top five positions were actually filled with new women candidates. When parties were forced to place women in top positions, they did not experience difficulties filling their lists, and new women stepped in to take on the challenge.[20] This evidence from Spain is highly consistent with the evidence from French municipalities after the introduction of a parity law that established a 50 percent female quota. Bird (2003) finds that according to a survey of 600 mayoral candidates in France, "78 per cent considered that it was 'easy' to apply the parity law in selecting candidates for their lists" (15).

In panel B of table 6, we consider voter turnout by municipality. If voters disliked female candidates, general animus against lists with more women could be expressed via lower voter turnout. In fact, turnout seemed to be higher in municipalities with more women on the lists before the quota (col. 1) after controlling for observables (number of parties, Herfindahl index of vote shares, and a polynomial in population). Social capital—as measured by voter turnout—and female representation were positively associated, a finding that we flag for future research. Moreover, when looking at the change in voter participation vis-à-vis the change in the aggregate share of female candidates (instrumented by the interaction between a quota dummy and 40 percent minus the town's share of female candidates in 2003), we find a positive—but statistically insignificant—effect of female candidates on turnout (col. 2). We conclude that the presence of female candidates on the lists did not reduce voter turnout.

The bulk of the evidence is therefore not consistent with either the existence of major supply constraints on qualified female candidates prior to the quota or the existence of voter preferences for male candidates.[21] It suggests that for most parties the strategy of increasing the number of female candidates was feasible and profitable. Nonetheless, party leaders failed to implement it before the quota was imposed. Although the evidence does not directly speak to the mechanism through which this occurs, it suggests the existence of agency problems within some parties that harm female participation.

[20] Casas-Arce and Saiz (2011) also show that the new women candidates on the lists were not more likely to have surnames or names associated with a higher socioeconomic status, suggesting that the quality of female candidates did not increase.

[21] Of course, the latter could be a reflection of male voters becoming more disengaged together with a compensatory increase in female voter participation. Note that the origin of the voters should not matter to conclude that parties were not following vote-maximizing strategies. Furthermore, we did not find prima facie empirical support for a hypothesis based on differences in postquota gender turnout. While the vote is secret and we will never be able to know the identity of voters, we tried to find significant differences in outcomes across towns with relatively larger shares of female registered voters in Casas-Arce and Saiz (2011); we did not find any.

V. Conclusion

Using a natural experiment provided by the introduction of female quotas in Spain, we show that parties modestly gained vote share when more women were mandated onto their lists. The results demonstrate that forcing parties to accept more women through quotas can increase female participation without necessarily decreasing their electability. They also suggest that agency problems between party leaders and voters are important in accounting for female underrepresentation.

We also find that women were much less likely to run at the top of lists that were highly likely to win seats in the local councils. Therefore—as in Sanbonmatsu (2002), Murray (2008), Bagues and Esteve-Volart (2010), and Verge and de la Fuente (2014)—the evidence suggests that internal party dynamics seem to be especially at play against women when power is at stake. The results are consistent with local party leaders implicitly discriminating against women by not engaging them often enough (Bird 2003; Lawless and Fox 2005). They could also be explained by the endogenous emergence of rent-seeking male coalitions within the parties (Reuben et al. 2010). Alternatively, the results could indicate that women are effective at generating political ideas and enticing voters but are less adept at elbowing out internal party competitors.

We also find that the agency problems between party leaders and voters are more pronounced in noncompetitive elections. Hence, the results suggest that an alternative to the use of quotas to increase female representation might be to increase competition in the electoral process. In fact, women's representation is much larger in proportional electoral systems (Norris 2006; Norris and Krook 2011), where each vote counts, as opposed to majoritarian systems, where many seats are de facto owned and noncompetitively allocated by the leadership of the locally dominant party, especially after redistricting. Affecting the behavior of political machines vis-à-vis gender issues and understanding how competition changes internal party dynamics could thus be key to improving women's chances at equal participation. We leave the exploration of these hypotheses for future research.

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