

Gender Quotas and Female Entrepreneurship: Evidence from Italian Municipalities

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Abstract

This paper estimates the effect of gender quotas in local politics on female entrepreneurship. We exploit the introduction of a 2012 Italian reform mandating female candidate representation in municipal elections for municipalities above 5,000 inhabitants, and implement a difference-in-differences event-study design comparing municipalities first exposed to the reform with never-treated municipalities, restricting attention to a narrow population window around the binding threshold. Using a balanced panel of 607 municipalities observed from 2007 to 2022, we find that quota exposure increases female entrepreneurship by approximately 2–3%. The effect is concentrated in regions with lower baseline gender equality, consistent with a catch-up mechanism whereby female political visibility matters most where it is most novel. We find no evidence that the effect operates through increased municipal spending on childcare or social-care services, ruling out the most natural policy channels. The evidence is instead broadly consistent with a role-model interpretation: the increased visibility of women in local political leadership appears to shift the perceived feasibility of entrepreneurial entry among women in gender-unequal environments.

Keywords: Female entrepreneurship, gender quotas, gender inequality, local politics, gender role models.

JEL: D72; L26; J16.

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1. Introduction

Women remain significantly underrepresented in entrepreneurship. Across all OECD countries, women are on average 1.5 times less likely than men to be working on a start-up (OECD, 2021), and despite a positive trend in recent decades, female entrepreneurs continue to represent a minority of business owners globally (GEM, 2021). Understanding what drives women into entrepreneurship is not only a matter of gender equity: new firms contribute to innovation, job creation, and economic growth (Guiso et al., 2020; van Praag and Versloot, 2007), and closing the gender gap in entrepreneurship would unlock a substantial share of unrealized economic potential (OECD and EC, 2017).

Several factors have been identified as barriers to female entrepreneurship, including differential attitudes and risk preferences (Eckel and Grossman, 2008; Gneezy et al., 2009, 2003), unequal access to credit (Alesina et al., 2013; Muravyev et al., 2009), the scarcity of female role models (Bosma et al., 2012), and the broader institutional framework — including the availability of childcare services, family policies, and prevailing gender norms — that shapes the constraints women face in combining work and care responsibilities (Del Boca et al., 2020; Grinza et al., 2022). This paper investigates whether increasing the visibility of women in positions of local political leadership — through a gender quota reform — can stimulate female entrepreneurship, and through which channels this effect operates. Female politicians are particularly well-suited to serve as role models: they are embedded in local communities, highly visible, and occupy prominent positions that signal to other women what is achievable. At the same time, female politicians may directly affect women’s economic outcomes by steering municipal spending toward areas that reduce barriers to female labor market participation, such as childcare and social-care services. Disentangling these two channels — role model versus policy — is a central objective of this paper.

Italy offers an ideal laboratory for this investigation. The country combines traditionally high gender gaps in the labor market and conservative gender role attitudes (Del Boca et al., 2020; Alesina et al., 2013) with a sharp, well-defined policy intervention: Law No. 215 of 2012, which mandated female candidate representation in municipal council elections for municipalities with populations above 5,000 inhabitants. The law introduced two key provisions — a list quota requiring neither sex to exceed two-thirds of candidates, and a double-preference voting rule encouraging cross-gender preference expression — and has been shown to significantly increase female representation in local councils (Andreoli et al., 2022). Despite this conservative backdrop, female entrepreneurship in Italy has been on a steady upward trend: the share of female entrepreneurs rose from around 25% in the early 2000s to nearly 29% by 2013 (Colombelli et al., 2021). At the same time, female self-employment in Italy retains a significant necessity-driven component, as women are more likely than men

to enter entrepreneurship in response to limited employment opportunities (OECD and EC, 2017), making it a particularly policy-relevant context in which to study the effects of female political representation on women’s economic choices.

We implement a difference-in-differences event-study design that compares municipalities first exposed to the reform in 2014 with never-treated municipalities that also held elections in the same year, restricting attention to a narrow population window around the 5,000-inhabitant threshold at which the quota became binding. This approach maximizes the comparability of treated and control municipalities in terms of both size and electoral timing, and yields a balanced panel of 607 municipalities — 155 treated and 452 controls — observed from 2007 to 2022. The entrepreneurs we study are individual self-employed entrepreneurs (*imprese individuali*), as recorded in the ASIA archive provided by ISTAT, which covers the universe of self-entrepreneurs in Italy and includes gender information derived from fiscal codes. This population is predominantly composed of small and micro-enterprises operating across a wide range of sectors,¹ and is therefore highly relevant to the study of female self-employment at the local level.

The results tell a coherent and interpretable story. Quota exposure increases female entrepreneurship by approximately 2–3%, but the response is not immediate: event-study coefficients are close to zero around the baseline year and turn distinctly positive only several years after treatment, consistent with the gradual nature of the adjustment process. The effect is strongly heterogeneous: it is concentrated in municipalities located in regions with lower baseline gender equality, as measured by the Regional Gender Equality Indexes (Di Bella et al., 2021), and the pattern extends robustly across the main sub-dimensions of the index — work, money, knowledge, time, health, and power. This catch-up pattern — whereby the quota has the strongest entrepreneurial consequences precisely where gender equality is weakest — is one of the central findings of the paper, and suggests that female political visibility acts as a catalyst for change most powerfully where it is most needed. On the mechanism side, we find no evidence that the quota triggered an increase in municipal childcare spending or in spending on social-care services for the elderly and disabled, even in the subgroups where the entrepreneurship effect is strongest, ruling out the most natural spending-based policy channels. While we cannot exclude the existence of other policy mechanisms that we do not directly observe — such as targeted training programs or administrative support for female entrepreneurs — the absence of any detectable movement in care spending makes it difficult to attribute the effect primarily to a policy channel. The

¹According to ISTAT, 86.4% of active enterprises in Italy belong to the micro-enterprise segment, with fewer than 10 employees. See <https://www.istat.it/en/archive/177113>.

evidence is instead broadly consistent with a role-model interpretation: female political representation appears to matter most where it is most novel, shifting the perceived feasibility of entrepreneurial entry in environments where gender norms are most constraining.

This paper contributes to several strands of literature. First, it adds to the growing body of work on the effects of female political representation on economic outcomes (Andreoli et al., 2022; Baskaran and Hessami, 2018; Beaman et al., 2012), extending this literature to entrepreneurship — a domain that has received comparatively little attention despite its central importance for local economic dynamism and job creation. Second, it contributes to the literature on the determinants of female entrepreneurship (Oggero et al., 2020; Colombelli et al., 2021), highlighting the role of local political role models as a driver of women’s occupational choices and showing that the supply of visible female leaders can matter as much as material constraints in shaping entrepreneurial decisions. Third, by explicitly and systematically testing the policy channel alongside the role model channel using direct spending data, it speaks to the broader debate on how female leadership translates into economic outcomes (Matsa and Miller, 2013; Maida and Weber, 2020), providing novel evidence on which mechanisms are and are not at work. Finally, the heterogeneity findings speak to the literature on gender equality and female leadership (Lucifora and Vigani, 2022; Flabbi et al., 2019), qualifying it by showing that the entrepreneurial impact of female political representation is largest precisely where gender equality is most lacking — consistent with a catch-up mechanism rather than a critical mass effect.

The rest of the paper is organized as follows. Section 2 reviews the relevant literature and develops the conceptual framework, focusing on the role model and policy mechanisms. Section 3 presents the institutional background and the empirical strategy. Section 4 describes the data and provides descriptive statistics. Section 5 presents and discusses the results. Section 6 concludes with policy implications and directions for future research.

2. Literature and conceptual framework

Despite a positive trend in recent decades, women remain significantly underrepresented among entrepreneurs. Several factors contribute to this gap: differential attitudes and risk preferences (Eckel and Grossman, 2008; Eagly and Crowley, 1986; Ones and Viswesvaran, 1998; Gneezy et al., 2009, 2003), unequal access to credit (Coleman, 2000; Ongena and Popov, 2016; Basiglio et al., 2019; Muravyev et al., 2009; Alesina et al., 2013), a disproportionate burden of unpaid care work and the broader institutional framework — including childcare availability, family policies, and prevailing gender norms — that shapes the constraints women face in combining work and care responsibilities (Del Boca et al., 2020; Grinza et al., 2022), and the scarcity of female role models (Bosma et al., 2012; Oggero et al., 2023).

This paper focuses on two channels through which increased female representation in local politics could help close this gap: a policy channel, whereby female politicians steer municipal resources toward areas that reduce women’s care burden and facilitate their entry into the labor market and entrepreneurship; and a role model channel, whereby the increased visibility of women in positions of power shifts other women’s beliefs, aspirations, and perceived feasibility of entrepreneurial entry. We discuss each channel in turn.

2.1. *The policy mechanism*

Female politicians may exhibit distinct policy preferences and steer municipal resources toward areas that reduce barriers to women’s economic participation. Studies document that women in politics are more likely to support redistributive policies, child-related expenditures, and more liberal policy agendas (Edlund et al., 2005; Alesina and La Ferrara, 2005; Miller, 2008; Clots-Figueras, 2011), and tend to prioritize areas such as childcare and social services (Funk and Gathmann, 2015). If female-led councils allocate more resources to childcare or elderly care, this could directly reduce the care burden on women and lower the barriers to entrepreneurship — a mechanism broadly consistent with the *women helping women* hypothesis documented by Matsa and Miller (2013) in the context of private firms.

However, the empirical evidence on the policy impact of female representation remains mixed. Some studies find positive effects: Svaleryd (2009) shows that female representation in Swedish municipalities increases childcare spending relative to elderly care, while Chattopadhyay and Duflo (2004) find that female village leaders in India allocate more resources to public goods relevant to women. Others find no significant effects, including studies on U.S. cities (Ferreira and Gyourko, 2014) and Spanish municipalities (Bagues and Campa, 2021; Carozzi and Gago, 2023).

Evidence from Italian municipal gender quotas — the specific institutional setting of this paper — is equally mixed. Andreoli et al. (2022), the study most directly related to ours as it exploits the same 2012 reform, documents higher spending on local security and lower administrative costs following the introduction of gender quotas, but finds limited evidence for changes in other expenditure categories, including those most relevant to women’s care burden. Dominici et al. (2024) find that increased female representation shifts resources away from culture and toward education, suggesting that female politicians do influence spending priorities but not necessarily in the directions most conducive to reducing care constraints. Rigon and Tanzi (2012) find no significant effect of politicians’ gender on the allocation of public resources more broadly, concluding that gender-based differences in policy preferences do not systematically translate into different spending outcomes, consistent with the median voter theorem. Taken together, this Italian evidence suggests that even where gender quotas

have demonstrably increased female political representation, their translation into spending changes that would directly benefit women’s labor market participation remains far from guaranteed.

Overall, the direction and magnitude of policy effects remain context-dependent and empirically uncertain. We therefore directly test whether the introduction of gender quotas led to an increase in municipal spending on childcare and social-care services for the elderly and disabled — the expenditure categories most directly linked to the care burden constraining women’s labor market participation.

2.2. *The role model mechanism*

A second channel through which female political representation could influence female entrepreneurship is a role model effect. Role identification theories posit that individuals are attracted to others with similar traits and aspirations and are motivated to reinforce this similarity through observation and emulation. Complementarily, social learning theories emphasize that role models facilitate the acquisition of new skills, behaviors, and norms (Gibson, 2004). Because this mechanism relies on perceived similarity, individuals are more likely to identify with role models who share key characteristics, including gender.

It is well established that role models influence career decisions and entrepreneurial intentions (Van Auken et al., 2006a,b; Krueger JR et al., 2000). In entrepreneurial families, parental role models increase the likelihood of self-employment (Chlosta et al., 2012), and role modeling is broadly recognized as the primary mechanism underlying the intergenerational transmission of entrepreneurship (Fairlie and Robb, 2007; Hoffmann et al., 2015; Lindquist et al., 2015; Sørensen, 2007). Beyond the family context, regions with high entrepreneurship levels may inspire new entrants through the increased availability of role models (Fornahl, 2003; Guiso et al., 2020; Lafuente et al., 2007; Minniti, 2005; Sternberg, 2009). Evidence from corporate boards suggests that greater gender diversity is positively associated with firm performance and that women play an increasingly important role in fostering entrepreneurial activity (Avolio Alecchi and Radović-Marković, 2013), though comparatively little is known about the determinants of women’s participation as entrepreneurs in small and micro-enterprises.

Empirical evidence across contexts supports the relevance of gender-congruent role models. Female role models improve academic performance in Uganda (Riley, 2022), increase women’s enrollment in male-dominated fields in the United States (Porter and Serra, 2020), and are associated with broader social changes in Brazil (La Ferrara et al., 2012; Chong and La Ferrara, 2009) and India (Jensen and Oster, 2009). Exposure to counter-stereotypical female role models also increases women’s labor market participation (Olivetti et al., 2020;

(Bell et al., 2018; Jannati, 2024), raises their likelihood of employment in STEM occupations (Chhaochharia et al., 2022), and enhances self-confidence and willingness to compete (Schier, 2020). This evidence suggests that individuals are more responsive to role models who share their gender, a pattern likely to extend to entrepreneurial choices (Andersson and Hammarstedt, 2011; Hernandez, 1995; Bosma et al., 2012).

Can female politicians serve as role models? Beaman et al. (2012) show that increased female representation in Indian local councils raises young women’s aspirations and reduces gender gaps in educational attainment. Baskaran and Hessami (2018) find that the election of a female mayor in Germany increases female representation in local councils by approximately four percentage points, suggesting role model spillovers within the political sphere. Greater female presence at the top may expand the set of observable role models available to women (Booth et al., 2000), and the scarcity of such models has been identified as a key barrier to female advancement.

The role model effect is likely stronger in contexts characterized by greater gender inequality, where female political leadership is more novel and therefore more salient as a signal of what is achievable. Oggero et al. (2023) find that role modeling through mothers matters most in countries with high gender gaps, suggesting that role models have the largest impact precisely where they are least common. In highly unequal environments, the arrival of a female leader may act as a catalyst that challenges prevailing norms and expands women’s perceived opportunity set.

While the role model mechanism is theoretically well-grounded and consistent with the heterogeneity patterns we document, it cannot be tested directly. It emerges instead by exclusion: if the spending-based policy channels receive no support in the data, the role model interpretation becomes the most plausible remaining explanation. We acknowledge that other unobserved policy mechanisms cannot be fully ruled out, but the absence of movement in the most natural and relevant spending categories would make it difficult to attribute the observed effect primarily to a policy channel.

3. Empirical strategy

3.1. The gender quota reform

Our empirical strategy is based on the introduction of gender quotas in local politics in Italy at the end of 2012. The reform was enacted through Law No. 215 on November 23 of 2012, with the aim of enhancing female representation in Italian municipal councils. The law applies to municipalities with a population of at least 5,000 residents and targets council elections occurring after 2012, incorporating two key measures. First, it introduces a “list quota”, whereby neither sex may represent more than two-thirds of the candidates on the

electoral list. Failure to comply results in the removal of candidates of the over-represented sex exceeding the two-thirds threshold. Second, it introduces a “double gender preference”, whereby voters may express two preferences instead of one, on the condition that the two preferences are addressed to candidates of different sexes.² Importantly, the reform did not interfere with the pre-established electoral calendar, which had been determined prior to 2012.

The reform has had a significant impact on female representation within municipal bodies. Andreoli et al. (2022) document an approximately 13.9 percentage point increase in the share of female councilors following the introduction of the quota. Beyond this direct effect, the law has also led to an increase in the appointment of women to executive positions in local government (Donà, 2018),³ playing a pivotal role in elevating female representation within the internal dynamics of local politics.⁴

3.2. The difference-in-differences event-study model

The empirical strategy compares municipalities first exposed to the quota in 2014 with never-treated municipalities that also held a local election in 2014, restricting the sample to municipalities with a population between 4,000 and 6,000.⁵ This design maximizes comparability between treated and control municipalities in terms of both population size and electoral timing, and limits concerns about cohort heterogeneity, anticipatory responses, and spillover effects from earlier-treated municipalities.

We focus on the 2014 treatment cohort for two main reasons. First, within the 4,000–6,000 bandwidth, it is the dominant post-reform cohort: as shown in Table A.1 in the appendix, 155 municipalities — corresponding to 52.4% of all post-reform treated municipalities in this window — were first exposed to the quota in 2014, making it the most empirically relevant cohort within the chosen bandwidth. Second, and more importantly, the 2014 cohort is the first cohort to be exposed to the reform, which makes it uniquely well-suited for clean identification. Later-treated municipalities are more likely to have already adjusted their behavior before formal exposure, as information about the reform — its practical implica-

²The law also stipulates that mayors must appoint executives in alignment with the principle of equal opportunities, and that municipal statutes must explicitly “guarantee” the presence of both sexes in executive and non-elected collegial bodies, as well as in affiliated agencies and institutions.

³In municipalities with fewer than 15,000 inhabitants, the mayor can only select executive members (the “assessori”) from individuals elected to the municipal council.

⁴A similar conclusion is reached by analyzing our data, which document a positive, large in magnitude, and strongly significant impact of gender quotas on an overall index of female representation in local politics, constructed based on the gender of the mayor and vice-mayor and the proportion of female councilors (results available upon request).

⁵Results are robust to alternative bandwidth restrictions of 4,250–5,750 and 3,500–6,500, as shown in Figure A.1 in the appendix.

tions, political consequences, and electoral dynamics — may have diffused through party networks, media coverage, and administrative learning. This type of anticipatory adjustment is difficult to detect and cannot be easily accounted for in the analysis. In addition, later cohorts offer a shorter post-treatment horizon, which is particularly problematic given that the outcomes we study may adjust only gradually after the reform. By focusing on the 2014 cohort, we obtain the cleanest possible comparison within our preferred population window and the longest post-treatment horizon for tracing dynamic effects. Never-treated municipalities that held elections in the same year serve as the comparison group, yielding a balanced panel of 607 municipalities — 155 treated and 452 controls — observed from 2007 to 2022.

A natural concern with any DiD design is whether the identifying assumption of parallel pre-treatment trends is plausible. In our setting, this assumption is supported by several features of the design. First, we restrict attention to a narrow population window of 4,000–6,000 inhabitants around the 5,000-inhabitant threshold, ensuring that treated and control municipalities are highly comparable in size and in the structural characteristics correlated with population. Second, the population threshold that governs treatment assignment was established prior to the reform and is unrelated to local entrepreneurship outcomes. Third, the pre-determined nature of the reform’s electoral timing — driven by pre-established electoral calendars rather than endogenous choices — further limits the scope for selection into treatment. We provide direct empirical support for the parallel trends assumption through the pre-treatment coefficients of the event-study specifications, which should be close to zero and display no systematic pattern if the assumption holds.

The exclusion restriction requires that crossing the 5,000-inhabitant threshold affects female entrepreneurship only through its effect on female political representation, and not through other channels. A potential concern is that municipalities just above the threshold may systematically differ from those just below in terms of female labor market participation and entrepreneurship. This concern is addressed by three features of our design. First, the narrow 4,000–6,000 bandwidth ensures that treated and control municipalities are close to each other in size, minimizing structural differences. Second, municipality fixed effects absorb any time-invariant differences across municipalities. Third, we estimate controlled specifications that interact year indicators with pre-determined baseline municipality characteristics, allowing for flexible differential time trends across municipalities — we describe these specifications in detail below.

Formally, let y_{it} denote the natural logarithm of the total number of female entrepreneurs in municipality i and year t . Let D_i be an indicator equal to one for municipalities first exposed to the gender quota reform in 2014, and zero for the never-treated control munic-

palities. Let α_i denote municipality fixed effects, λ_t denote year fixed effects, and let $Post_t$ be an indicator equal to one from 2015 onward.

Our main specification is a difference-in-differences event-study model:

$$y_{it} = \alpha_i + \lambda_t + \sum_{\tau \neq 2014} \beta_\tau (D_i \times \mathbf{1}\{t = \tau\}) + \varepsilon_{it}. \quad (1)$$

The omitted year is 2014, so each coefficient β_τ measures the difference between treated and control municipalities in year τ , relative to the treated-control gap in 2014.⁶ This specification serves two purposes: it provides a direct assessment of pre-treatment trends, and it traces the dynamic response after treatment, showing when the effect emerges, whether it grows over time, and whether it remains persistent. To summarize the post-treatment effect in a single coefficient, we complement the event-study with a static difference-in-differences specification:

$$y_{it} = \alpha_i + \lambda_t + \theta (D_i \times Post_t) + \varepsilon_{it}. \quad (2)$$

The coefficient θ captures the average post-treatment difference between treated and never-treated municipalities relative to the 2014 baseline. Since this specification imposes a constant post-treatment effect and masks dynamic adjustment, the event-study in Equation (1) remains our preferred specification.

We also estimate controlled versions of both specifications, which allow municipalities with different baseline characteristics to follow different aggregate time paths. This is implemented by interacting year indicators with a vector of pre-treatment municipality characteristics X_i^0 , defined as averages before 2014 and including GDP per capita, immigrant rate, employment rate, female population share, birth rate, and childcare spending per capita. Since these controls are measured before treatment, they are not affected by the reform. The controlled specifications absorb differential trends correlated with initial local conditions, reducing the risk that the estimated treatment effects capture pre-existing differences across municipalities rather than the causal effect of quota exposure. The controlled event-study and static specifications are:

$$y_{it} = \alpha_i + \lambda_t + \sum_{\tau \neq 2014} \beta_\tau (D_i \times \mathbf{1}\{t = \tau\}) + \sum_s (X_i^{0'} \delta_s) \mathbf{1}\{t = s\} + \varepsilon_{it}, \quad (3)$$

⁶We use 2014 as the baseline year to align the analysis with the institutional timing of the reform: 2014 is the election year in which the quota became widespread within our bandwidth, and 2015 is the first full year in which the resulting change in political representation could affect local outcomes. Using an earlier year as baseline would conflate the pre-treatment period with years in which the reform was already enacted but not yet applied in our sample. Results are robust to using 2013 as the baseline year instead (Figure A.2 in the appendix).

$$y_{it} = \alpha_i + \lambda_t + \theta (D_i \times Post_t) + \sum_s (X_i^{0'} \rho_s) \mathbf{1}\{t = s\} + \varepsilon_{it}, \quad (4)$$

where δ_s and ρ_s are vectors of year-specific coefficients that allow the effects of baseline characteristics to vary flexibly over time, without imposing municipality-specific linear trends that would be poorly suited to the non-linear dynamic patterns visible in the data.

Standard errors are clustered at the municipality level throughout, allowing for arbitrary serial correlation in the error term within municipalities over time — the appropriate level of clustering given that treatment varies at the municipality level and outcomes are observed repeatedly within the same units.

4. Data, variables, and descriptive statistics

The analysis draws on three main data sources, merged at the municipality-year level. The first is the ASIA archive, provided yearly by ISTAT, from which we extract data on individual self-employed entrepreneurs (*imprese individuali*) — the legal form in which a single person owns and operates a business as sole proprietor.⁷ The gender of each entrepreneur can be unambiguously derived from the fiscal code (*codice fiscale*) — an alphanumeric identifier that encodes personal information including gender and date of birth — and we also observe the municipality of activity, which allows us to collapse the individual-level data to the municipality-year level and construct our key dependent variable: the log number of female entrepreneurs in each municipality and year. This population — predominantly composed of small and micro-enterprises operating across trade, services, and professional activities — is directly relevant to our research question, as it captures the type of locally embedded entrepreneurial activity most likely to be influenced by local role models and municipal policy choices (OECD and EC, 2017).

The second data source is the “Anagrafe degli Amministratori Locali e Regionali” (AALR), provided by the Italian Ministry of the Interior, which records the electoral history of each Italian municipality, including the year in which elections were held. The third source, also from ISTAT, provides municipality-level information on public spending on childcare services and on care services for the elderly and the disabled — used to test the policy channel — as well as a set of economic and socio-demographic variables used as controls in our regressions. We further complement the dataset with the Regional Gender Equality Indexes (R-GEIs) provided by Di Bella et al. (2021), which offer comprehensive indicators of gender equality for each of the 20 Italian administrative regions, discussed in detail in Section 5.

⁷ASIA (Archivio Statistico delle Imprese Attive) is the Italian Statistical Register of Active Enterprises, updated annually by ISTAT through the integration of administrative and statistical sources. It covers all active enterprises in industrial, commercial, and service activities.

The merged dataset covers the period 2007–2022, excluding 2018, for which the ASIA data are unavailable.⁸ The full dataset covers 8,438 Italian municipalities. Restricting to municipalities with populations between 4,000 and 6,000 yields 842 municipalities. Our estimation sample further restricts attention to municipalities within this range that held a local election in 2014, yielding a balanced panel of 607 municipalities — 155 treated and 452 controls — observed over 15 years, for a total of 9,105 municipality-year observations.

Table 1 reports before-and-after comparisons in female entrepreneurship for the estimation sample. Before the reform, treated municipalities already exhibited higher levels of female entrepreneurship than controls — 69.0 versus 54.4 in levels, a gap of 14.6, and 4.217 versus 3.972 in logs, a pre-reform gap of 0.245 log points. After the reform, both groups experience growth, but the increase is larger among treated municipalities: 0.097 log points in treated versus 0.065 in controls, yielding a raw difference-in-differences of 0.032 log points. These descriptive patterns provide suggestive evidence of a positive treatment effect, while the pre-reform level differences between groups underscore the importance of the regression framework developed below.

Table 1: Before-and-after comparison in female entrepreneurship

Variable	Group	Before	After	Change / DiD
Female entrepreneurs	Treated	69.002	76.520	7.517***
	Control	54.364	58.181	3.817***
	Difference (T-C)	14.638***	18.339***	3.701***
Log female entrepreneurs	Treated	4.217	4.314	0.097***
	Control	3.972	4.037	0.065***
	Difference (T-C)	0.245***	0.277***	0.032***

Notes. Sample: municipalities with population 4,000–6,000 that held a local election in 2014. Before = 2007–2014; After = 2015, 2016, 2017, 2019–2022. Number of municipalities = 607 (155 treated, 452 controls); total municipality-year observations = 9,105. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports pre-treatment averages of the control variables separately for treated and control municipalities. The two groups are broadly similar, but some statistically significant differences emerge. Treated municipalities are located in somewhat more economically advantaged provinces: GDP per capita is 27,677 euros in the treated group versus 26,409 in controls, and immigrant and employment rates are also slightly higher among treated municipalities. At the municipality level, the female population share is virtually identical across groups, while treated municipalities display a slightly higher birth rate and substantially higher baseline childcare spending per capita — 16.7 euros versus 10.6 euros, a statistically

⁸The 2018 wave of the ASIA dataset was lost due to a file corruption issue that occurred during the transfer of data from ISTAT to the research institution through which the data were accessed.

significant difference of 6.1 euros. These pre-treatment differences, while not dramatic, motivate the use of controlled specifications that allow municipalities with different baseline characteristics to follow different aggregate time trends, thereby reducing the risk that the estimated treatment effects capture pre-existing differences rather than the causal impact of quota exposure.⁹

Table 2: Baseline balance in covariates

Variable	Treated	Control	Difference (T-C)
GDP per capita (province-level, euros)	27,676.7	26,408.7	1,268.0*
Log GDP per capita (province-level)	10.193	10.140	0.053**
Immigrant rate (province-level)	0.0849	0.0750	0.0099***
Employment rate (province-level)	0.4252	0.4130	0.0122**
Female population share	0.5081	0.5080	0.0001
Birth rate	0.0092	0.0088	0.0004***
Baseline childcare spending per capita	16.693	10.624	6.069***

Notes. Sample: municipalities with population 4,000–6,000 that held a local election in 2014. Entries are pre-treatment averages (before 2014). The sample size is slightly smaller than the main estimation sample due to missing values in some control variables for 8 municipalities: the controlled specifications are based on 599 municipalities (153 treated, 446 controls) and 8,985 municipality-year observations.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5. Results

5.1. Main results

We estimate the average effect of quota exposure on female entrepreneurship using Equations (2) and (4). Table 3 reports the results. The estimated post-treatment effect is positive and statistically significant in both specifications: quota exposure increases female entrepreneurship by approximately 3.2% in the specification without controls and by approximately 2% in the specification with controls. The consistency of the result across specifications reinforces confidence in the identification strategy.

The event-study estimates in Figure 1 provide a more detailed picture. Panel (a) reports the uncontrolled event-study and panel (b) the controlled version. Both panels show that the pre-treatment coefficients are close to zero and display no systematic trend, providing direct support for the parallel trends assumption. After treatment, the pattern is consistent

⁹The female population share is the ratio of females to total population. The birth rate is the number of births divided by total population. The employment rate is the number of employed individuals over total population, measured at the provincial level as municipality-level data are not available. GDP per capita is expressed in euros at nominal prices, also measured at the provincial level, and enters the regressions in logarithms.

Table 3: Static difference-in-differences estimates for female entrepreneurship

Outcome	No controls		With controls	
	Estimate	N	Estimate	N
Log female entrepreneurship	0.032*** (0.011)	9,105	0.020* (0.010)	8,985

Notes. Post-2014 DiD estimates reported. Sample: municipalities with population 4,000–6,000 that held a local election in 2014. Number of municipalities = 607 (155 treated, 452 controls) for the specification without controls, and 599 (153 treated, 446 controls) for the specification with controls. Standard errors clustered at the municipality level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

across both specifications: coefficients remain close to zero in the first post-treatment years and turn distinctly and persistently positive only five years after the treatment, persisting through the final post-treatment years.¹⁰

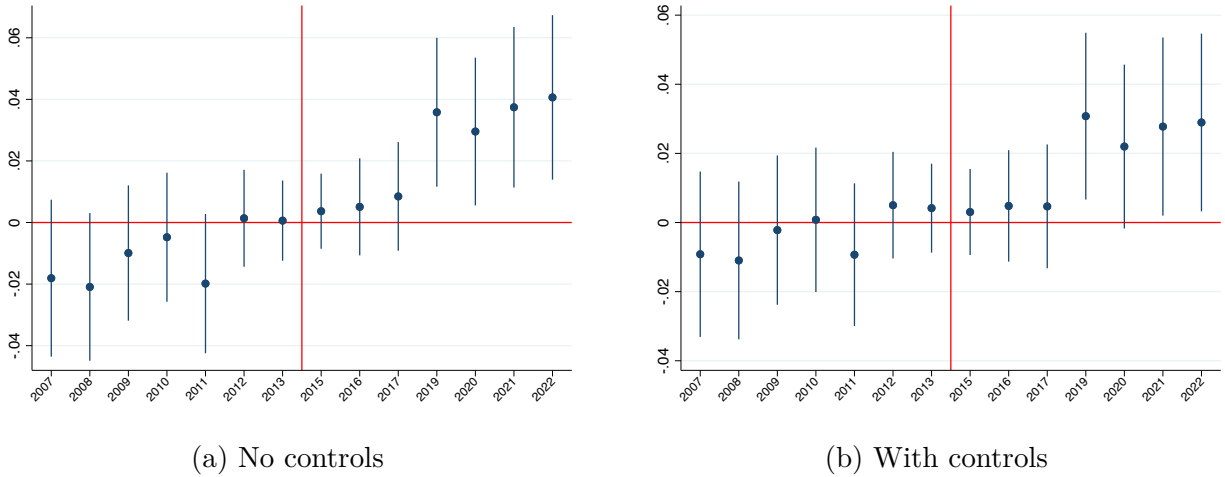


Figure 1: Quota effects on log female entrepreneurship

This delayed and gradually building response is an important feature of the results. Both the policy and the role model channel are consistent with such a gradual buildup: female politicians take time to develop and implement new policies, and the translation of those policies into behavioral change requires additional time; similarly, the psychological processes underlying role model effects — observational learning, updating of beliefs and aspirations, and the decision to act — unfold gradually rather than overnight. In either case, starting a business is itself a time-consuming process, involving ideation, planning, and bureaucratic

¹⁰Results are stable to narrower (4,250–5,750) and wider (3,500–6,500) bandwidths, as shown in Figure A.1 in the appendix, and are robust to using 2013 as the baseline year instead of 2014 (Figure A.2 in the appendix).

procedures, which further contributes to the observed lag. The evidence on which of these channels is most consistent with the data is examined in Sections 5.3 and 5.4, after first documenting a pattern of heterogeneity in the entrepreneurial response across different local gender equality environments.

5.2. Gender equality and the catch-up pattern

The aggregate effect documented in the previous subsection masks a striking degree of heterogeneity. To explore it, we split the sample according to the Regional Gender Equality Indexes (R-GEIs) provided by Di Bella et al. (2021), which are synthetic indicators of the overall degree of gender equality at the NUTS-2 level for each of the 20 Italian administrative regions. The R-GEIs regionalize the Gender Equality Index (GEI) of the European Institute on Gender Equality (EIGE), aggregating indicators across six domains — work, money, knowledge, time, power, and health — into a single composite score via a weighted geometric mean, capturing dimensions such as female labor market participation, income, educational attainment, unpaid care work, and health outcomes.¹¹ We classify municipalities as low-R-GEI if they belong to regions below the median, and as high-R-GEI if they belong to regions above the median, and run the event-study estimation separately on each subsample.

The results reveal a clear and robust catch-up pattern, which is one of the central findings of this paper. As shown in Figure 2, the post-treatment increase in female entrepreneurship is entirely concentrated in low-R-GEI municipalities: event-study coefficients turn positive after treatment and remain significantly positive in the later post-treatment years, with both the uncontrolled panel (a) and the controlled panel (b) telling the same story. In high-R-GEI municipalities, by contrast, the profile is flatter and remains not statistically different from zero throughout the post-treatment period. The quota has the strongest entrepreneurial consequences precisely where gender equality is weakest — a pattern consistent

¹¹R-GEIs refer to 2013–2015 and are provided as a geometric mean across the three years (Di Bella et al., 2021). Since these years overlap with the early post-reform period, endogeneity concerns may arise. However, several considerations mitigate this concern. First, gender equality is a slow-moving phenomenon capturing deep-rooted regional characteristics unlikely to change significantly over such a short horizon. Second, and more importantly, we use the R-GEIs exclusively to classify regions as above or below the median — a binary classification that is highly robust to small changes in the underlying index values. For the reform to invalidate this classification, it would need to shift entire regions across the median threshold, which is implausible given that treated municipalities are spread relatively uniformly across the Italian territory. Third, our main results show that the entrepreneurial response to the reform emerges only with a delay of several years — the effect is close to zero in the first post-treatment years and becomes positive only from around 2019 onward. This gradual buildup implies that the quota could not have meaningfully affected female entrepreneurship levels in the 2013–2015 window used to construct the R-GEIs, further alleviating endogeneity concerns. Fourth, as a further robustness check, we exclude the Power domain — the dimension most likely to be directly affected by the reform — and results are qualitatively unchanged.

with a mechanism whereby female political participation matters most where it is most novel.

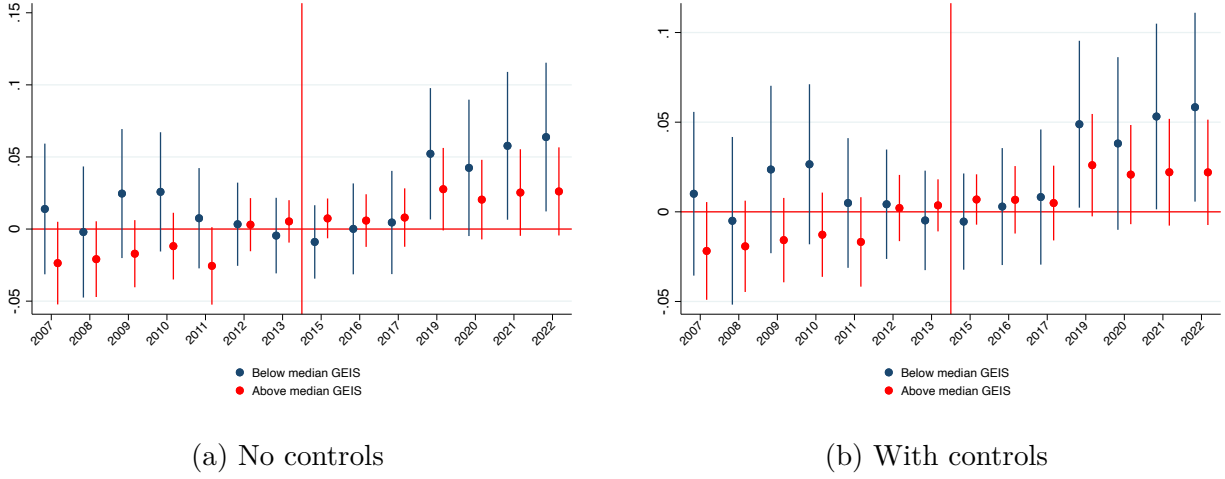


Figure 2: Female entrepreneurship by baseline R-GEI median split

This catch-up pattern is further reinforced when the overall R-GEI score is replaced by its component domains. Figure 3 reports the domain-specific splits, all estimated using the controlled specification. The same qualitative dynamic appears across all six dimensions: low-score regions consistently display an upward post-treatment trajectory in female entrepreneurship, while high-score regions remain substantially flatter. The pattern is clearest for work, money, and knowledge, while time, health, and power are somewhat noisier, though they point in the same direction. The robustness of the catch-up result across such diverse dimensions of gender equality — labor market participation, income, education, unpaid care, health outcomes, and political representation — suggests that it reflects a broad and deep feature of the local gender environment rather than any single specific dimension.

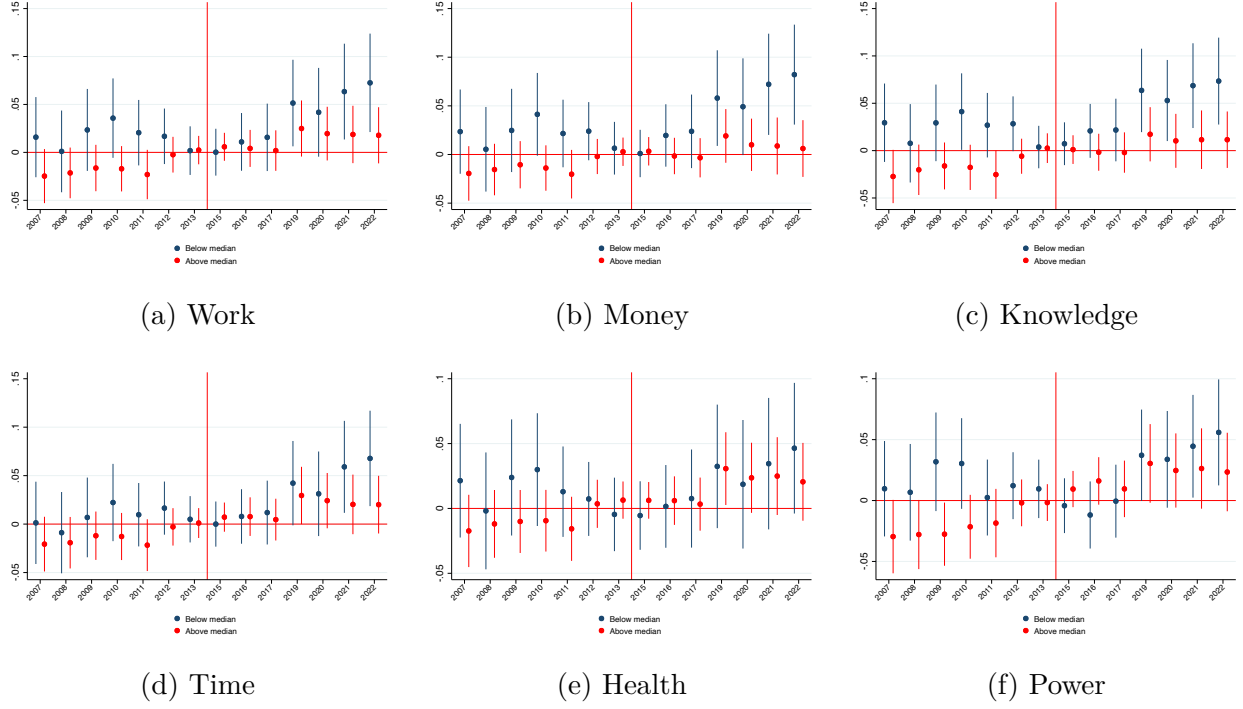


Figure 3: Female entrepreneurship by baseline R-GEI domain, controlled specifications

What explains this catch-up pattern? Two channels are potentially consistent with the evidence. Under a policy channel, female politicians in low-equality contexts may face stronger incentives to promote gender-friendly policies, or the marginal impact of such policies may be larger where baseline constraints are more binding. Under a role model channel, the visibility of a female political leader is likely to carry a stronger signal precisely where female leadership is rare — the arrival of a woman in a position of power is more novel, more salient, and therefore more likely to shift other women’s beliefs and aspirations about what is achievable. Distinguishing between these two interpretations requires direct evidence on the channels through which the quota operates, which we turn to in the next two subsections.

5.3. *Childcare spending*

Having documented that the entrepreneurial response to the quota is concentrated in municipalities located in low-gender-equality environments, we now turn to the question of which channel drives this effect. The two subsections that follow test the policy channel directly, by examining whether quota exposure triggered an increase in the municipal spending categories most directly linked to the care burden constraining women’s labor market participation. If the policy channel is operative, we would expect to observe not only an increase in female entrepreneurship in low-equality contexts, but also a corresponding increase in care-related spending in those same contexts. We begin with childcare spending, the most natural and

direct policy lever available to municipal governments.

The childcare outcome is defined as municipal childcare spending per capita, constructed by summing two ISTAT sources — directly managed municipal nursery and spring-section services, and spending for the same services when provision is entrusted to third parties — and dividing the total by the municipality’s 2013 population. Because this spending panel is available for a shorter set of years than the entrepreneurship panel (starting from 2011), the childcare event-study is estimated on the years for which the outcome is observed, with the same 2014 normalization and the same post-2015 treatment contrast in the static specification. In the full 607-municipality sample, all municipalities have observed childcare records; however, only 218 ever show positive direct-management spending, while 365 show positive spending once third-party provision is included. The childcare outcome — as well as the elderly and disabled home-care spending outcomes discussed in the following subsection — is expressed as $\log(\text{spending per capita} + 1)$.¹²

The analysis proceeds in two steps. We first verify that the municipalities driving the entrepreneurship effect are indeed those with lower baseline childcare availability — a prerequisite for the policy channel to be plausible. We then test whether childcare spending itself increased in those municipalities following quota exposure.

The first step confirms the expected pattern. As shown in Figure 4, when municipalities are split by baseline childcare spending per capita, the post-treatment increase in female entrepreneurship is concentrated in municipalities with lower baseline childcare spending, with panel (a) and panel (b) telling the same story without and with controls. This is coherent with the R-GEIs result: the entrepreneurial response is largest in places that appear most constrained at baseline, whether those constraints are measured by regional gender equality — a broad composite index encompassing labor market, income, education, care, health, and power dimensions — or by the more specific dimension of municipal childcare spending availability.

¹²The $\log(x + 1)$ transformation is motivated by the prevalence of zero-spending observations. As a robustness check, we re-run all specifications in levels — for childcare, elderly home-care, and disabled home-care spending alike — and results are unchanged; all results are available in the appendix (Figures from A.3 to A.6). As concerns childcare, the results should be interpreted as evidence on municipal spending in these administrative categories, not as a complete measure of childcare access, quality, or privately provided services.

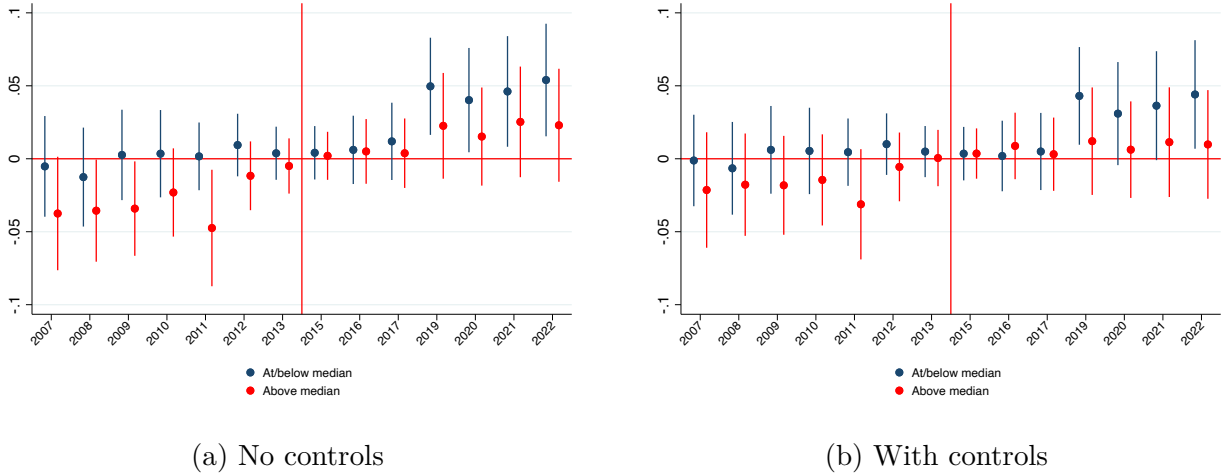


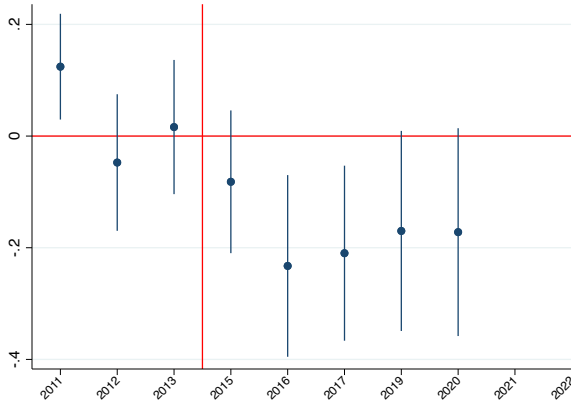
Figure 4: Female entrepreneurship by baseline childcare spending

The second step, however, points in a different direction. When childcare spending per capita — rather than female entrepreneurship — is used as the dependent variable in Equations (1) to (4), the estimated post-treatment effect provides no evidence of an expansion in childcare spending. The static difference-in-differences estimates are non-significant, with point estimates of -0.190 without controls and -0.121 with controls. The event-study results in Figure 5, panels (a) and (b), are consistent with this: treated municipalities do not experience a sustained post-treatment rise in childcare spending per capita relative to controls. Moreover, including childcare spending as a control in the female entrepreneurship regression does not attenuate the treatment coefficient, providing further evidence against a childcare-mediated mechanism. The subgroup analyses sharpen this conclusion. Among municipalities with low baseline childcare spending — precisely those where the entrepreneurship effect is strongest — the estimated effect of the quota on childcare spending per capita is close to zero and statistically insignificant in both specifications, as shown in panels (c) and (d) of Figure 5.

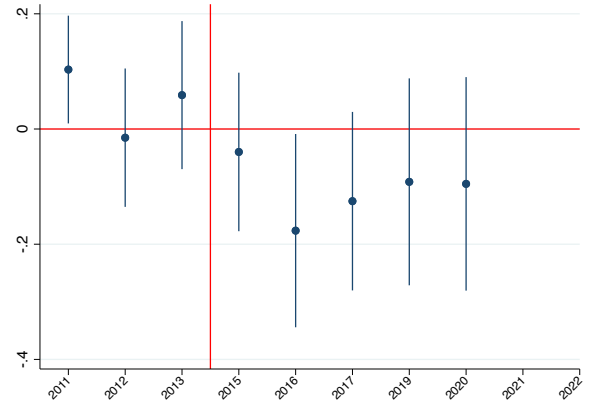
Since the R-GEI captures a broader notion of gender inequality of which childcare availability is only one component, we further verify that the null result on childcare spending holds when splitting by regional gender equality. In low-R-GEI municipalities, the point estimates are not statistically different from zero as well, as shown in panels (a) and (b) of Figure 6. The data therefore do not support the hypothesis that the quota raised female entrepreneurship by increasing municipal childcare spending in the places where one would most expect that channel to operate.

This conclusion is further reinforced by the age-composition of the entrepreneurial response. If the quota had operated through an expansion of childcare availability — thereby relaxing the care constraints on mothers — we would expect the entrepreneurship effect to

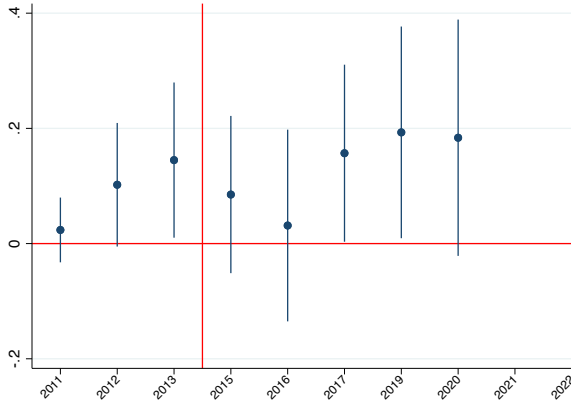
be concentrated among women aged 30–40, the group most likely to have young children and most directly affected by improved childcare access. Instead, the post-treatment increase in female entrepreneurship is broad-based: both the 30–40 age group and the remaining female entrepreneurs move in the same positive direction after treatment, with the effect outside the 30–40 band being if anything more stable and precisely estimated. This pattern is difficult to reconcile with a childcare-driven explanation and is shown in Figure [A.7](#) in the appendix.



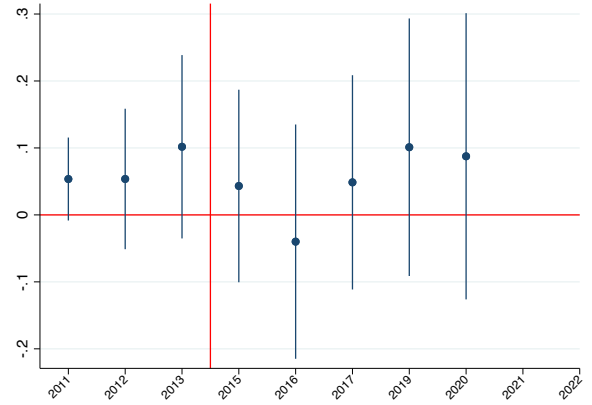
(a) Full sample, no controls



(b) Full sample, with controls

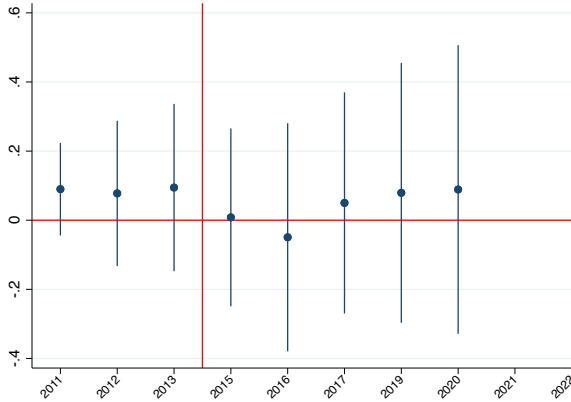


(c) Low baseline-childcare municipalities, no controls

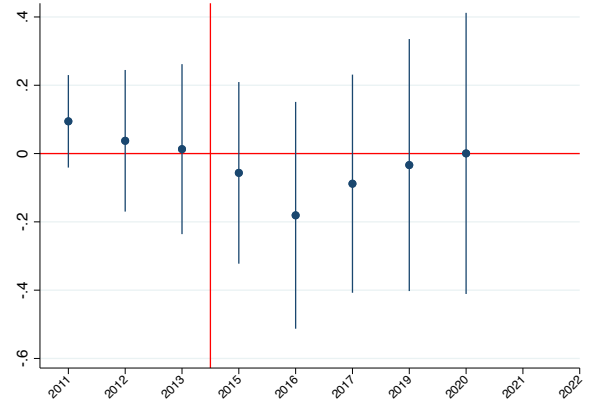


(d) Low baseline-childcare municipalities, with controls

Figure 5: Quota effects on log childcare spending per capita



(a) Low baseline-R-GEI municipalities, no controls



(b) Low baseline-R-GEI municipalities, with controls

Figure 6: Quota effects on log childcare spending in low-R-GEI municipalities

5.4. Other care-related spending

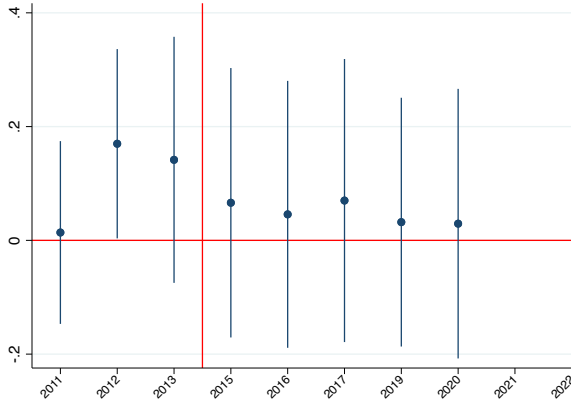
The childcare results provide no support for the policy channel. To assess whether this conclusion extends to other care-related spending categories that are theoretically relevant to women’s labor market participation, we expand the mechanism analysis to two additional outcomes available in ISTAT administrative data: home-care spending for the elderly and home-care spending for people with disabilities. These outcomes are converted into per-capita terms using the 2013 population anchor. In the full 607-municipality sample, 606 municipalities have an observed series for each measure; 526 ever record positive disability home-care spending, and 582 ever record positive elderly home-care spending. Among treated municipalities, the corresponding counts are 129 and 146 out of 155.

The motivation for examining these additional spending categories is straightforward. Women are not only the primary providers of childcare — they also disproportionately bear the burden of care for elderly or disabled family members (Del Boca et al., 2022). If the quota operated through a general expansion of municipal care provision, we would expect this to be reflected not only in childcare spending but also in spending on home-care services for the elderly and disabled, particularly in the municipalities and regions where the entrepreneurship effect is strongest.

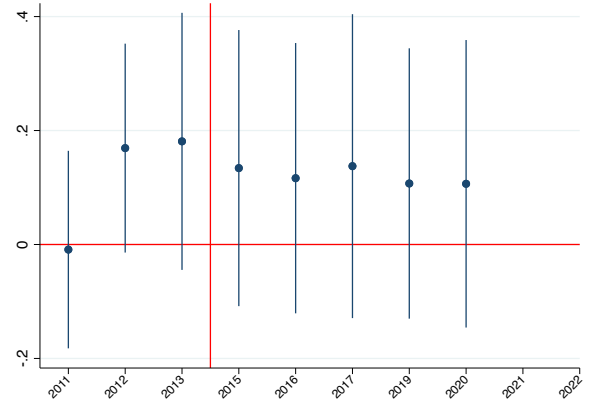
The evidence does not support this expectation. Using disability home-care spending per capita and elderly home-care spending per capita as the dependent variables in Equations (1) to (4), we find that in municipalities located in low-R-GEI regions, both outcomes yield imprecise estimates with no statistically significant post-treatment effects, as shown in Figure 7. The series are noisy and display no clear post-treatment break in either specification. The same pattern emerges when attention is restricted to municipalities with low

baseline childcare spending: the estimated post-treatment effect on disability home-care spending is essentially zero, while elderly home-care spending is somewhat more positive at around 0.1, but remains statistically indistinguishable from zero in both specifications. Figure 8 confirms this in dynamic form: there is no sustained post-treatment increase in either spending category in the municipalities that drive the female entrepreneurship result.

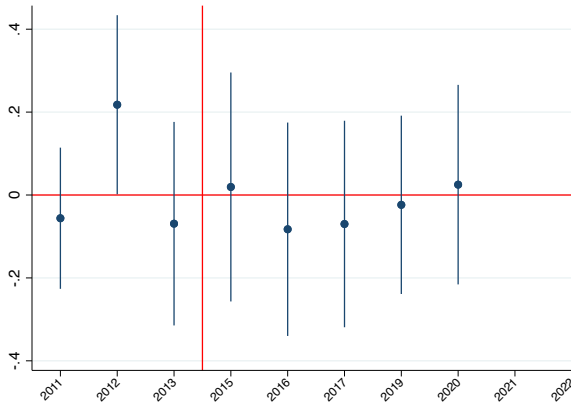
Taken together, the childcare and home-care results point to the same conclusion: the quota did not operate through an expansion of municipal care spending. The consistency of this null result across three distinct spending categories — childcare, disability home-care, and elderly home-care — each motivated by an independent theoretical rationale linking care provision to women’s labor market participation, strengthens the case against a policy channel interpretation.



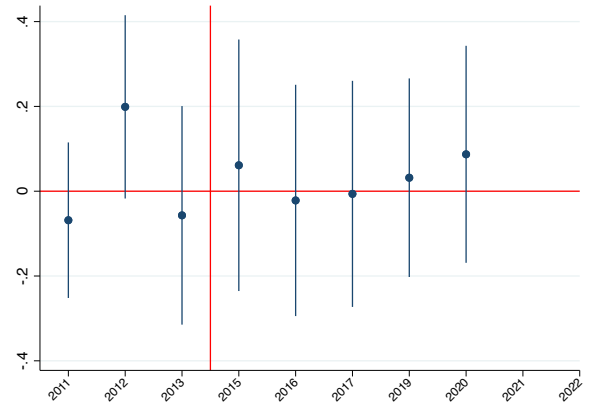
(a) Disability home care, no controls



(b) Disability home care, with controls

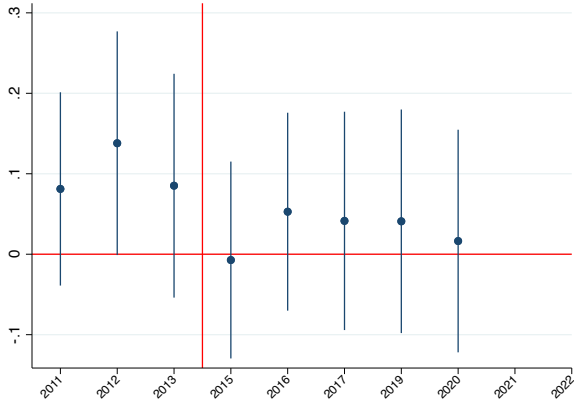


(c) Elderly home care, no controls

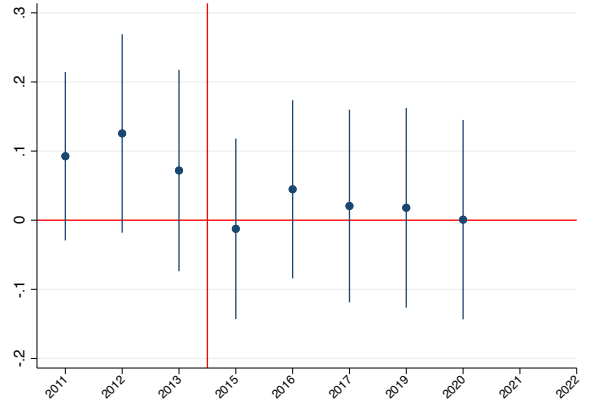


(d) Elderly home care, with controls

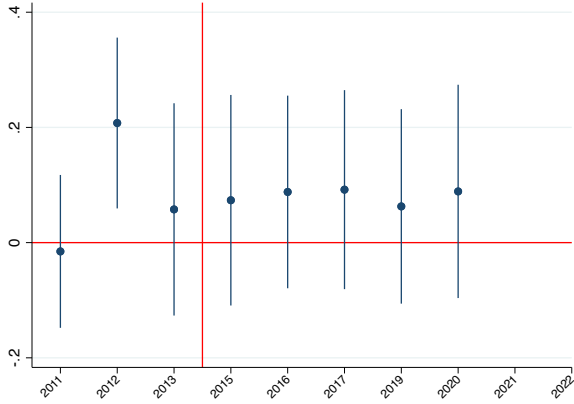
Figure 7: Other municipal care spending (log) in low-R-GEI municipalities



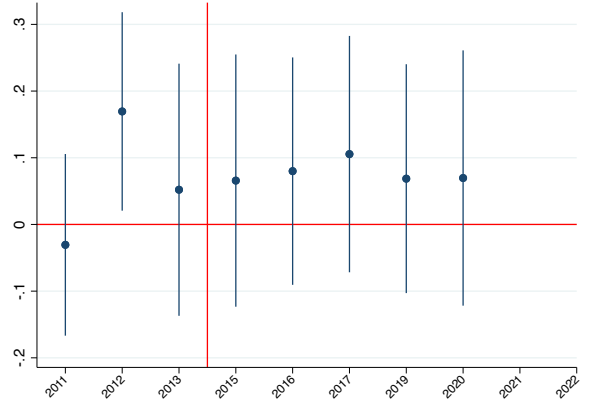
(a) Disability home care, no controls



(b) Disability home care, with controls



(c) Elderly home care, no controls



(d) Elderly home care, with controls

Figure 8: Other municipal care spending (log) in low baseline childcare spending municipalities

5.5. Interpreting the mechanism: evidence consistent with a role-model channel

The direct evidence on municipal spending rules out the care-spending policy channel: none of the care-related spending categories we examine — childcare, disability home-care, or elderly home-care — shows any evidence of a post-treatment increase, even in the subgroups where the entrepreneurship effect is strongest. The consistency of this null result across three distinct spending categories, each independently motivated by the disproportionate care burden that women bear, makes it difficult to attribute the observed entrepreneurial response to a policy channel operating through care provision.

The remaining evidence is broadly consistent with a role-model interpretation. The delayed nature of the entrepreneurial response — emerging only five years after the election — aligns with the time required for role model effects to unfold: women must first observe the change in political representation, then gradually update their beliefs and aspirations about

what is achievable, and finally act on those updated beliefs. This process of internalization takes time, and the gradual buildup visible in the event-study coefficients is precisely what one would expect from such a mechanism. The concentration of the effect in low-R-GEI municipalities is also consistent with a role model interpretation: where female political leadership is rare, its arrival carries a stronger signal — a catalyst that challenges prevailing norms and expands women’s perceived opportunity set most powerfully precisely where it is most novel. Both features are in line with prior research showing that role model effects are stronger in contexts characterized by greater gender inequality (Oggero et al., 2023).

This interpretation should be stated carefully. The data do not allow us to prove a role-model mechanism directly, and we are not claiming to have isolated it in the same way one would isolate a mediator in an experimental design. Moreover, the delayed effect and the concentration in low-equality environments are consistent with both channels — as discussed earlier, policy effects too take time to materialize and may be larger where baseline constraints are more binding. What distinguishes the role model interpretation is not these features alone, but their combination with the absence of any detectable movement in care spending. One might further conjecture that female-friendly councils stimulate female entrepreneurship through targeted policy initiatives — such as training courses or legal and bureaucratic assistance for aspiring female entrepreneurs — channels we cannot directly observe or rule out. However, such mechanisms are difficult to reconcile with the absence of any movement in care spending: even well-designed support programs are of limited use if women still face binding care constraints, and the data provide no evidence that those constraints were relaxed. Taken together, the evidence points away from specific expenditure-based mechanisms and is broadly consistent with a broader shift in the local gender environment brought about by the increased visibility of women in positions of power, though we acknowledge that other non-spending policy channels cannot be fully ruled out.

6. Conclusions

This paper estimates the effect of gender quotas in local politics on female entrepreneurship, and investigates the channels through which this effect operates. We exploit the introduction of the 2012 Italian gender quota reform in municipal elections and implement a difference-in-differences event-study design comparing municipalities first exposed to the reform in 2014 with never-treated municipalities that also held elections in the same year, restricting attention to a narrow population window around the 5,000-inhabitant threshold. Using a balanced panel of 607 municipalities observed from 2007 to 2022, and drawing on individual-level data from the ASIA archive provided by ISTAT, we estimate the effect of quota exposure on the stock of female individual entrepreneurs.

Three main findings emerge. First, quota exposure increases female entrepreneurship by approximately 2–3%, a result that is robust across specifications and bandwidth choices. Second, the effect is not immediate: event-study coefficients remain close to zero in the first post-treatment years and become positive only several years after the reform, pointing to a gradual adjustment process rather than an instantaneous response. Third, the effect is highly heterogeneous. It is concentrated in municipalities located in regions with lower baseline gender equality, as measured by the Regional Gender Equality Indexes (Di Bella et al., 2021), and this catch-up pattern extends across all six sub-dimensions of the index — work, money, knowledge, time, health, and power — with the clearest patterns for work, money, and knowledge. Female political representation thus has its strongest entrepreneurial consequences precisely where gender equality is initially weakest.

We then investigate the mechanisms behind this result. The evidence provides no support for the most natural spending-based policy channels. We find no detectable post-treatment increase in municipal childcare spending, nor in home-care spending for the elderly or for people with disabilities, even in the subgroups where the entrepreneurship effect is strongest. The entrepreneurial response is also not narrowly concentrated in the age range most directly linked to childcare constraints. The absence of any detectable movement across three distinct spending categories — each independently motivated by the disproportionate care burden that women bear — makes it difficult to attribute the observed increase in female entrepreneurship to an expansion of municipal care provision.

The remaining evidence is broadly consistent with a role-model interpretation. The delayed response and its concentration in low-equality environments fit a mechanism in which increased exposure to women in visible local leadership positions gradually shifts beliefs, aspirations, and the perceived feasibility of entrepreneurial entry, especially where female leadership is rarer and therefore more salient. At the same time, this interpretation should be stated with caution. Our data do not allow us to identify the role-model mechanism directly, and we cannot fully rule out other channels operating outside the expenditure categories we observe. What the evidence does show clearly is that the effect does not appear to work through the main care-spending channels most commonly invoked in this context.

These findings contribute to the literature on female political representation and economic outcomes by extending it to entrepreneurship — a domain that has received comparatively little attention despite its central importance for local economic dynamism — and to the literature on female entrepreneurship by highlighting the role of local political visibility as a driver of women’s occupational choices. The heterogeneity results are consistent with a catch-up mechanism rather than a critical-mass view in which female leadership matters only where equality is already relatively advanced.

From a policy perspective, the results suggest that increasing women’s visibility in positions of power may generate economic spillovers beyond the political sphere, and that these spillovers are likely to be largest precisely where gender inequality is most entrenched — pointing to a self-reinforcing logic of gender quota policies in more unequal contexts. At the same time, the absence of measurable changes in care-related spending indicates that representation alone is unlikely to remove the structural constraints women face in combining work and care responsibilities. Closing the gender gap in entrepreneurship may therefore require complementary female-friendly policies — including investments in care infrastructure, targeted support for female entrepreneurs, and broader institutional reforms aimed at reducing gender gaps in the labor market — alongside efforts to increase female political visibility. Future research should investigate other channels through which female political leadership may affect women’s economic behavior, and whether the effects documented here generalize to other institutional contexts and persist over longer time horizons.

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Appendices

A. Additional tables and figures

Table A.1: First post-reform treatment year in the 4,000–6,000 bandwidth

First treatment year	Count	Share among post-reform treated (%)
2014	155	52.4
2015	33	11.1
2016	50	16.9
2017	56	18.9
2018–2019	2	0.7
Total post-reform treated	296	100.0
Never treated (that held an election in 2014)	452	–

Notes. 4,000–6,000 population bandwidth. Counts report the first post-reform treatment year. Municipalities first treated in 2013 are excluded. The table describes treatment timing in the bandwidth, not the final estimation sample.

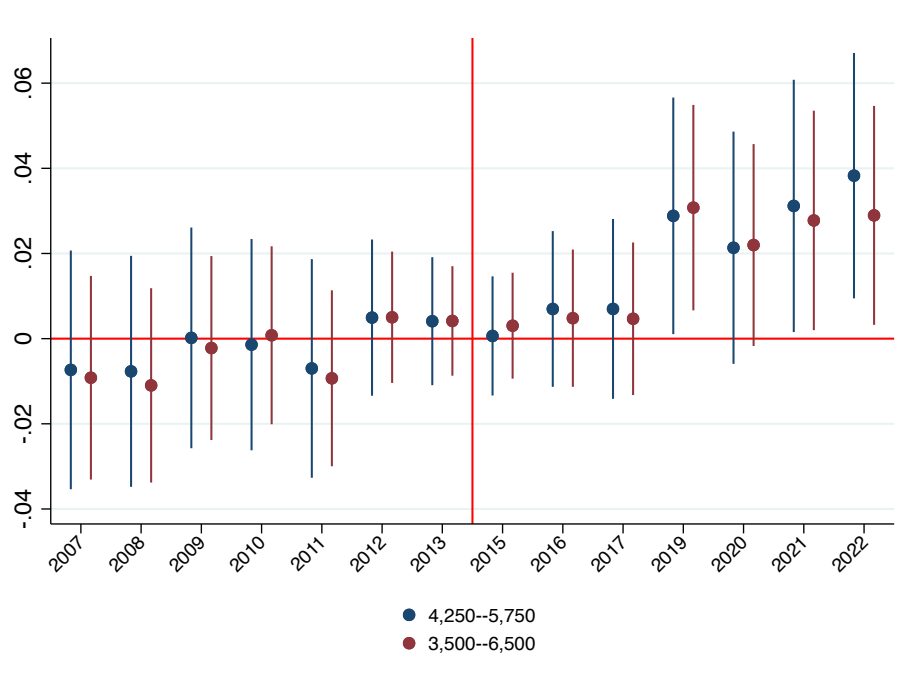


Figure A.1: Bandwidth robustness check, controlled specifications

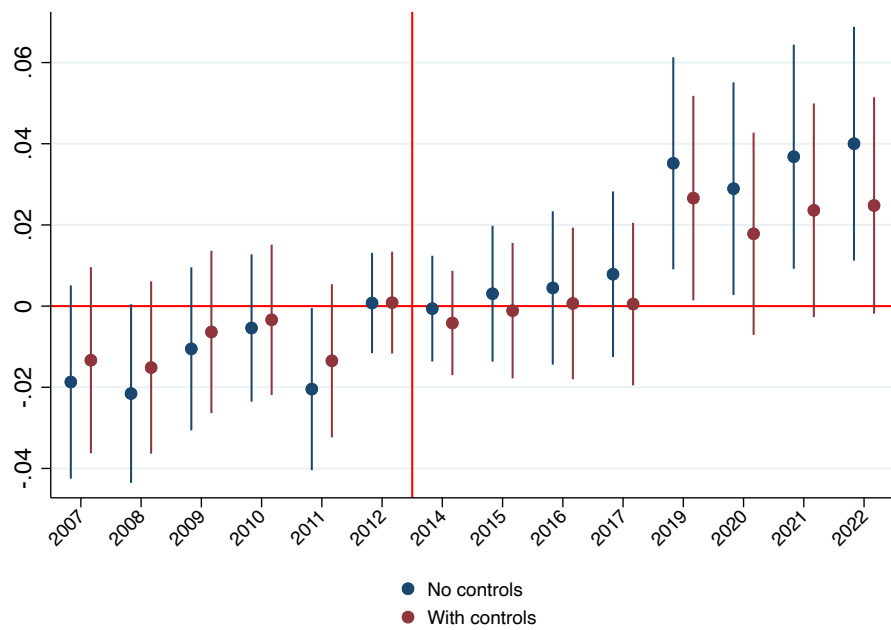
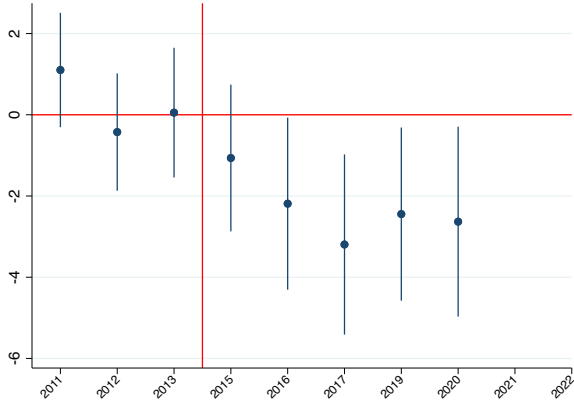
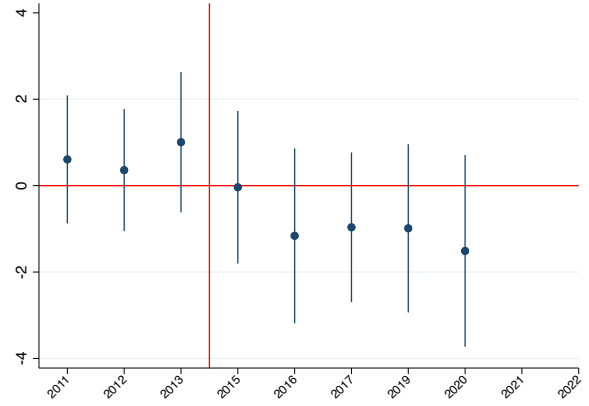


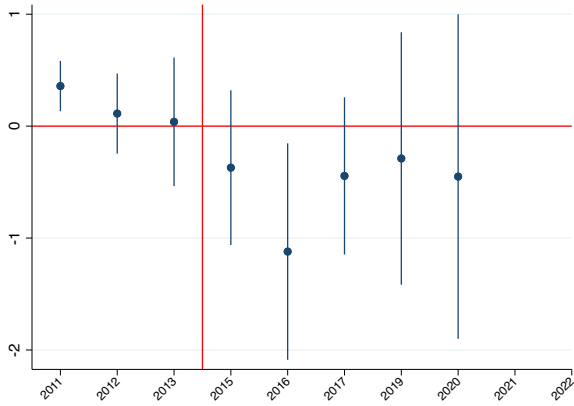
Figure A.2: Controlled event-study estimates for female entrepreneurship using 2013 as the omitted year



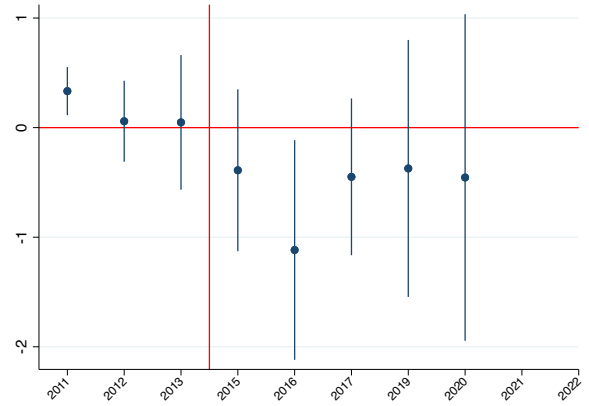
(a) Full sample, no controls



(b) Full sample, with controls

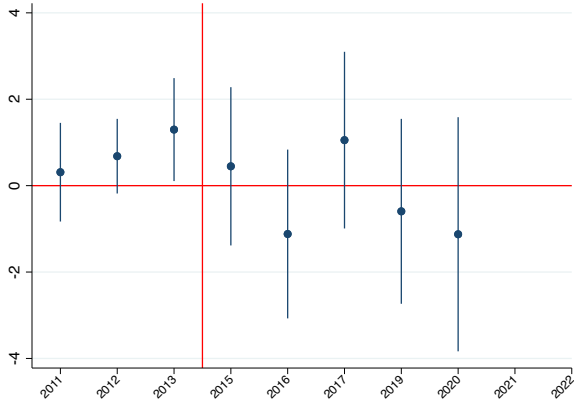


(c) Low baseline-childcare municipalities, no controls

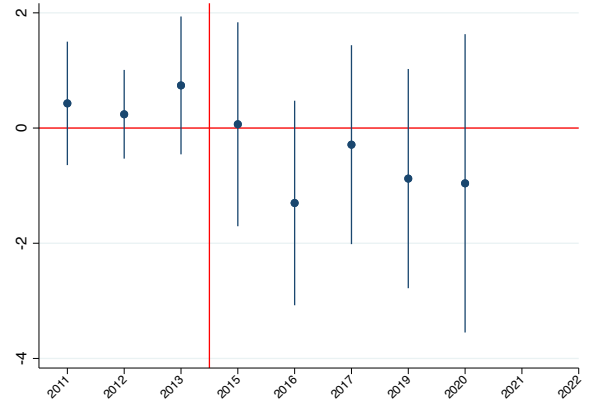


(d) Low baseline-childcare municipalities, with controls

Figure A.3: Quota effects on childcare spending per capita (levels)

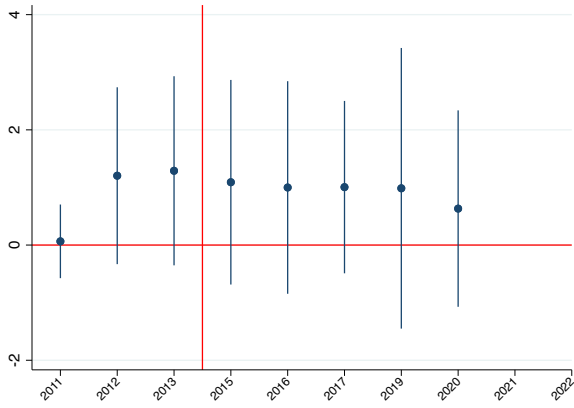


(a) Low baseline-R-GEI municipalities, no controls

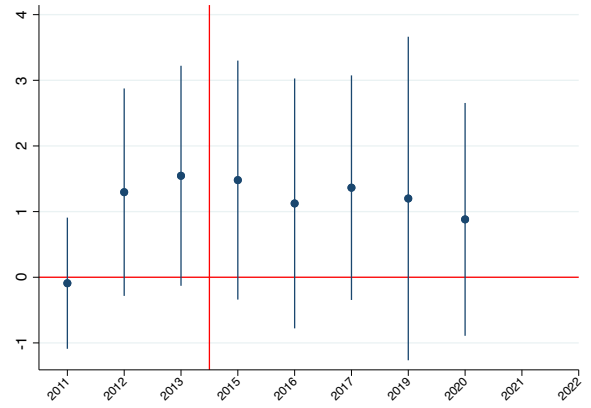


(b) Low baseline-R-GEI municipalities, with controls

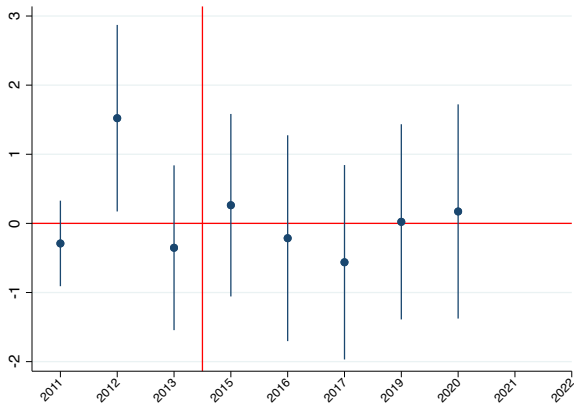
Figure A.4: Quota effects on childcare spending in low-R-GEI municipalities (levels)



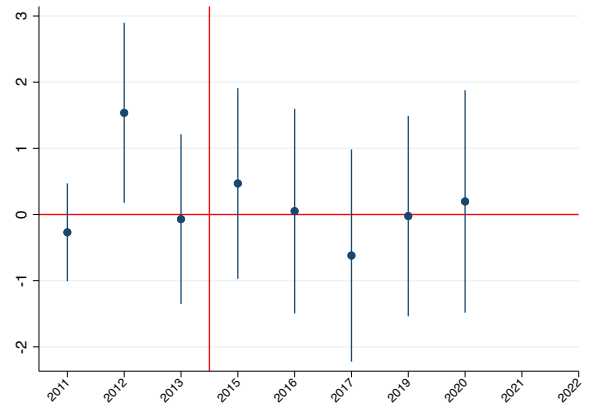
(a) Disability home care, no controls



(b) Disability home care, with controls

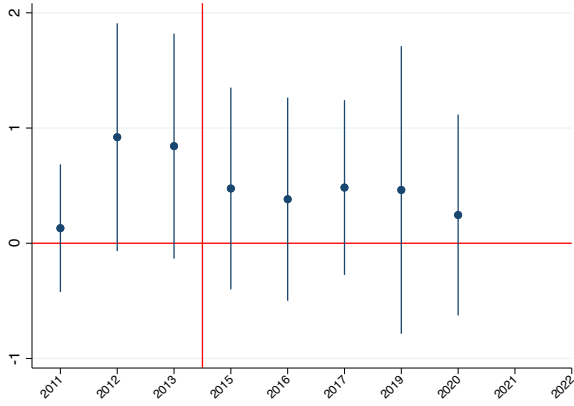


(c) Elderly home care, no controls

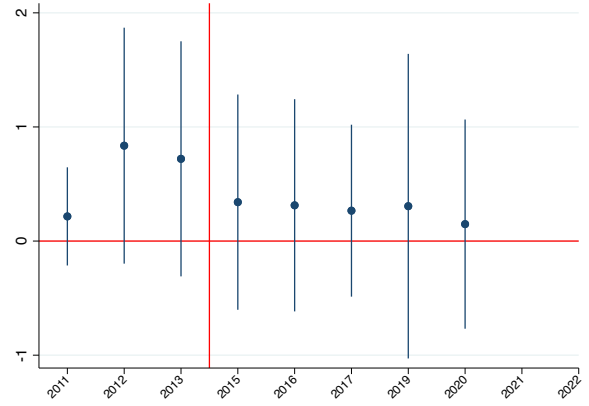


(d) Elderly home care, with controls

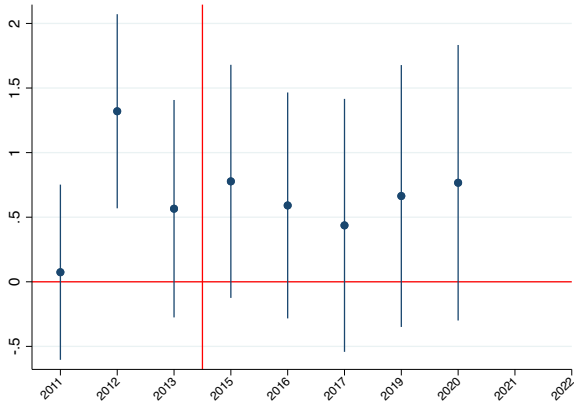
Figure A.5: Other municipal care spending in low-R-GEI municipalities (levels)



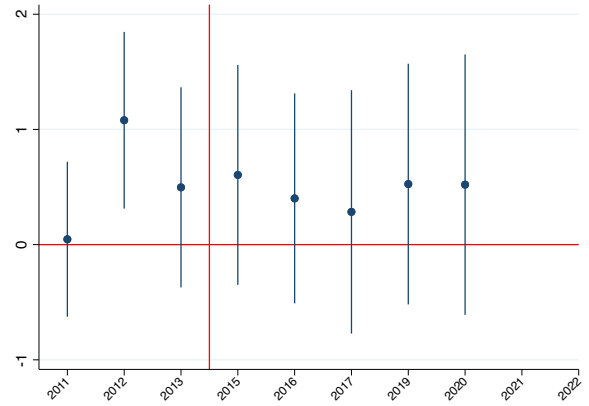
(a) Disability home care, no controls



(b) Disability home care, with controls

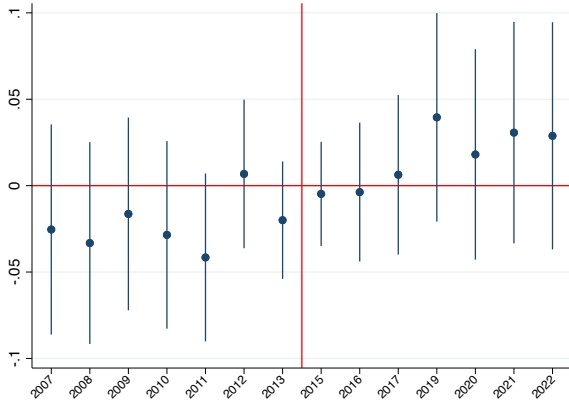


(c) Elderly home care, no controls

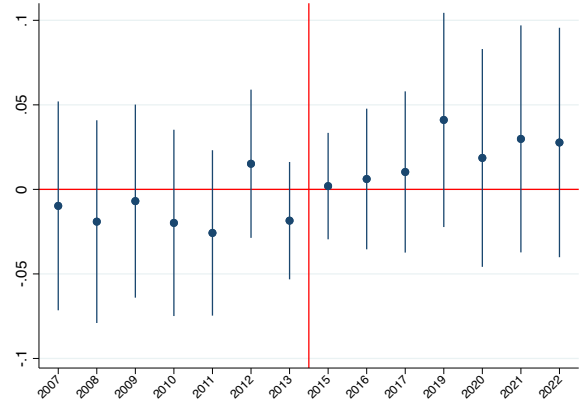


(d) Elderly home care, with controls

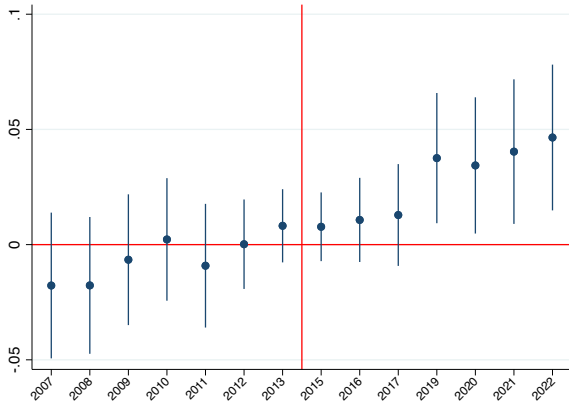
Figure A.6: Other municipal care spending (levels) in low baseline childcare spending municipalities



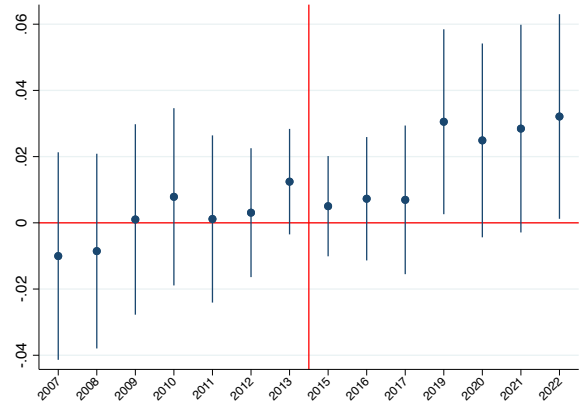
(a) Women 30–40, no controls



(b) Women 30–40, with controls



(c) Rest of women, no controls



(d) Rest of women, with controls

Figure A.7: Female entrepreneurship by age composition