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# DO GENDER QUOTAS PRODUCE SPILLOVER EFFECTS? EMPIRICAL EVIDENCE FROM ITALIAN MUNICIPALITIES

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**Abstract**

This paper analyses the spillover effects of gender quotas on women's involvement in politics and the labour market. It uses a rich data set that provides information on all the Italian municipalities characteristics and administrators between 1981 to 2012. In 1993, Italy introduced gender quotas. The law was in force until 1995 when it was abolished because it was declared unconstitutional. Because of the short period covered by the reform, some municipalities never voted under the gender quota system. It allows identifying a treatment and a control group exogenously and estimating the results comparing the two groups and through a Difference-in-Difference approach. The results suggest inconsistent evidence of the increased female representation of women in neighbourhood units. Furthermore, there is no consistent evidence of cross-sectoral spillover effects of gender quotas on the female unemployment rate in the long run. Still, the share of housewives over the total female population aged 15 or older seems to be affected by the reform, even if the effect offset over time. The results are robust to a number of robustness checks, including controlling for northern and southern regions.

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# Contents

# Introduction

The under-representation of women is still a problem in several contexts, such as corporate governance, academia, and policy-making (Braga and Scervini, 2017). In 2020, the Global Gender Gap index showed that the most substantial gender disparity is political empowerment, where only 24.8% of the gender gaps have been closed (World Economic Forum, 2020). A deep understanding of the lack of female political representation may clarify the origin of gender disparities in other contexts and, closing the gender gap in political representation may help alleviate gender disparities in other areas of society through policy-making. Furthermore, female politicians may reduce gender prejudices, be role models for other women to enter politics, and promote policies that reduce formal and informal barriers to female engagement. Thus, policies aimed at raising female participation in politics, such as gender quotas and how they work, are a topic of particular interest (Hessami and Lopes da Fonseca, 2020).

This paper contributes to the previous literature by providing the first to my knowledge study on both the geographical and cross-sectoral spillovers of gender quotas. Indeed, the positive impact of increased female politicians can be expected within jurisdictions and sectors, and across them, i.e. the effect of gender quotas can spill over to other areas and sectors. This thesis is broken into three parts. In the first part, a preliminary analysis is conducted to understand whether gender quotas increased the efficiency of politicians. Spillover effects in other geographical areas and across sectors may be encouraged by an increase in the performance of politicians, thanks to the gender quotas. Thus, before proceeding with the analysis for the spillover effects, it could be relevant to conduct this preliminary analysis. While the previous literature has analysed the effect of gender quotas in terms of quality and performance (Baltrunaite et al., 2012; Braga and Scevini, 2017), this thesis focus on efficiency to have an objective measure of the administrative capacity of the politicians. Indeed, efficient financial management is crucial for implementing policies, and it is plausible that an increase in efficiency leads to an increase in the perceived performance of the politicians. Having understood the impact of gender quotas on efficiency, I proceed to investigate my main research questions. In the second part, I study whether gender quotas produce geographical spillovers, raising the female political representation in neighbourhood areas. In the third part, I analyse whether gender quotas have cross-sectoral spillover, affecting the labour market regarding the female unemployment rate and share of housewives over the female population aged 15 or older.

It is often difficult to analyse the effects of gender quotas empirically because it is hard to disentangle the intervention results from other confounding factors related to temporal trends. This paper overcomes this problem thanks to a natural experiment produced by the introduction in Italy of gender quotas, establishing a minimum number of candidacies by gender. In Italy, municipal gender quotas were introduced in 1993. They reserved a percentage of the places in the party lists for Municipal Council elections for female candidates. In 1995, the Constitutional Court repealed these gender quotas. Because of the short time, this law was in force and considering that the municipal elections usually took place every four years[1], not all municipalities voted under the gender quotas policy. Indeed, since gender attitudes or political systems do not influence the timing of voting, casual factors led municipalities to vote under the law. Because of this exogenous shock, it is

[1] In 1993, the duration of a legislative term was shortened from five to four years, then restored to five years in 2000.

possible to identify a treatment and a control group, composed respectively of 7.703 and 397 municipalities well distributed between the entire country. Several studies have exploited this framework for different purposes (De Paola et al., 2010; Rigon and Tanzi, 2011; Baltrunaite et al., 2012; De Paola et al., 2012; Braga and Scevini, 2017); however, it is worth mentioning the work of De Paola et al. (2010) because this thesis is strictly connected to their work. De Paola et al. (2010) show that gender quotas directly raise female representation. They also show how exposing voters to female leadership helps to break down negative stereotypes. Consequently, a higher share of women is elected, even after having abolished the reform. While De Paola et al. (2010) focus on the impact of gender quotas on female political representation in the same geographical unit, this paper focuses on neighbourhoods and other areas of society. Specifically, this thesis complements the study of De Paola et al. (2010) extending the period of the analysis and testing for other outcomes, such as performance measures and labour market outcomes. Furthermore, new models are employed to investigate the geographical spillovers.

This thesis uses a rich administrative data set, which provides information on the composition of the municipalities political bodies, the efficiency of the Municipal Council and the characteristics of the municipalities. The complete data set spans from 1981 to 2012, excluding the years when the law was in force.

Different methodologies are used to investigate the research questions. Firstly, a preliminary regression is run to understand whether there was an increase in the efficiency in the management of the municipal administration. For this analysis, two financial indicators are used as outcome variables: the speed of revenues collected and the speed of payment. The speed of revenues collected is the ratio between the collected tax and transfer revenues and the total amount of assessed revenues that a municipality should collect. The speed of payment is the ratio between the outlays paid, and the outlays committed in the municipality budget. Since the measures are available only from 1998 to 2008, it is not possible to compare before and after the reform. Considering that the treated and control group assignment is random, there should be no differences between the two groups other than the treatment effect. Thus, comparing the treatment and control group using OLS, it is possible to make causal claims about the impact of gender quotas. I found that gender quota municipalities positively impact the speed of revenues collected but not the speed of payment. The finding of this thesis on the speed of revenues collected, the evidence of an increase in the quality, the performance of the politicians (Baltrunaite et al., 2012; Braga and Scevini, 2017), and the break down of negative stereotypes (De Paola et al., 2010) make possible to test for the other research questions.

A Difference-in-Difference (DiD) approach is conducted to investigate the spillover effect across geographical units in terms of female representation. The share of treated municipalities in a Labout Market Area (LMA) measures the treatment intensity. LMAs are subregional geographical areas where the bulk of the labour force lives and works and where companies can find most of the labour force necessary to occupy jobs. These areas are particularly suited to test the effects of gender quotas in the neighbourhoods since they capture small and homogeneous areas. The approach used compares the changes in women's representation rates for municipalities in an area with a high share of treated municipalities with municipalities having a low share of treated municipalities in the neighbourhoods. This model is based on Romarri (2020) but differs from it because Romarri (2020) uses a standard Difference-in-Difference using a dummy variable equal to one whether there was a treated municipality in a LMA. On the other hand, this study uses the share of treated municipalities in a LMA, which defines the continuous treatment. An additional analysis is conducted to check whether spillover effects are heterogeneous over time. To do so, the standard variable for the post-treatment period in the DiD is replaced with the number of elections after abolishing the reform. The results show an influence of the share of treated municipalities in the neighbourhoods, particularly strong after three or four elections after the reform. However, the results were mainly driven by the high percentage of the treated municipalities in the sample. Excluding them, there were no significant results except for the Municipal Council. The geographical spillover effect for

the Council seems particularly strong after four elections after the abolishment. However, excluding the treated municipalities, the analysis suffers from the effects of a small sample size. To overcome this sample size problem, a spatial lagged X model is used to test the geographical spillover effects on female political representation. It allows for the spatial interaction to consider the direct and the indirect impact of the treatment, which correspond to the spillover effects. The results show high geographical spillover effects for the female representation in the Executive Committee. However, since the results are strongly biased by the methodology chosen, they should be interpreted with caution. Despite this, this thesis is the first study to conduct any form of analysis of the geographical spillover effects of gender quotas.

To conclude, a Difference-in-Difference approach is used to test the cross-sectoral spillover effects, i.e. whether increased political representation affects the labour market. DiD compares the changes in women's unemployment rate and share of housewives for municipalities affected by the law with those not affected. The results show no long-run effects on the female unemployment rate and housewives' share over the female population aged 15 or older. Adding multiple timing interaction effects to check for the presence of the impact that disappears over time, the findings show different effects in various directions.

To check the robustness of the results, this thesis shows that different temporal trends among Southern and Northern regions do not drive the findings. To do so, I interact the variables of interest with a dummy variable *south*, which is equal to one for municipalities located in the south of Italy and zero otherwise.

The thesis will be structured as follows; the next section presents a review of the central literature. In the third section, the theoretical framework is outlined. In the fourth section, the institutional framework will be described, followed by the data and methodology. Further, the estimate results and the robustness check will be presented and lastly, a discussion and some concluding remarks.

# Literature review

## 2.1 Quality and performance of politicians

This paper contributes to several strands of literature examining the introduction of gender quotas in politics. First, it relates to work on the impact of gender quotas on the quality and the performance of politicians. This strand is explored because of its connection with the empowerment of women. Higher quality and improved performance of politicians after introducing the gender quotas may enhance the spillover effects of the gender quotas in geographical areas and sectors not affected by the law. In contrast, a decrease in quality and performance may discourage spillover effects. Furthermore, the differences in policy outcomes among genders may produce favourable conditions for women, encouraging the spillover in other sectors, such as the labour market [1].

### 2.1.1 Quality of elected politicians under gender quotas

The existing literature uses multiple proxies to measure the quality of elected politicians. Among the most used, there is the average education of the politicians and the share of politicians with high-skilled jobs. Indeed, education is positively correlated with the quality of politicians, and because of that, it can be considered an indirect evaluation for it (Besley et al., 2011; Fortunato and Panizza, 2011). The share of politicians with high-skilled jobs is used because of the potential correlation between labour market ability and skills in the political arena (Galasso and Nannicini, 2011).

One or both measures have been used in the gender quotas literature in different countries. While the results generally show that female politicians that entered with the gender quota are at least as qualified as male politicians (O'Brien, 2012; Allen et al., 2014; Josefsson, 2014; Bagues and Campa, 2017), evidence from Sweden and Italy suggests that gender quotas increased the average quality of all elected politicians, attracting female politicians who are more qualified than the male candidates they replace (Baltruinaite et al., 2012; Belsey et al., 2017). On the other hand, in India's seat reservations, Chattopadhyay and Duflo (2004) find that gender quota candidates have less education and less political experience.

### 2.1.2 Differences in efficiency among genders

In addition to the research on quality, gender literature has also found gender differences in efficiency. According to Ferreira and Gyourko (2014), the gender of the mayor is uncorrelated with both local government size and the composition of municipal spending. In contrast, according to Gagliarducci and Paserman (2012), local government administrations led by women have a higher probability of early termination of the legislature. In the Italian framework considered by this analysis, Braga and Scervini (2017) evaluate politicians' performance and efficiency. They found that elected women are more efficient than men in the municipal administration, proxied by the size of the Executive Committee.

[1] Further details are provided in the Theoretical framework, section (3.1.1)

However, a smaller Municipal Executive may do not necessarily mean that an administration is more efficient. To offer an objective measure of the administrative capacity of politicians, this thesis uses two financial efficiency indicators for the management of the municipal government: the speed of revenues collection and the speed of payment. These two measures have been used in the Italian framework for other research questions by Gagliarducci and Nannicini (2010), Gagliarducci and Paserman (2012) and Drago et al. (2014).

### 2.1.3 Differences in observed policy outcome among genders

The literature has investigated not only the changes in efficiency but also the changes in policy outcomes. The available evidence indicates significant gender differences in preferences for policies (Carroll, 2001). Specifically, women are more likely than males to enact policies and invest in public interventions that address women's concern, such as childcare, water supply, health, and the environment (Rehavi, 2007; Funk and Gathmann, 2008; Clots-Figueras, 2011). Gender differences in legislators' behaviour emerge since women are more liberal than men and tend to support women's issues (Welch, 1985; Thomas and Welch, 1991; Swers, 1998; Washington, 2008).

Furthermore, Franceschet and Piscopo (2008) prove that gender quotas create a mandate for female lawmakers to represent the interests of women. In India, Chattopadhyay and Duflo (2004) found that women elected as leaders under the reservation policies invest more in public goods that are more closely related to the needs of women. In the Italian framework considered, despite Rigon and Tanzi (2011) do not find effects of the gender of politicians on the spending categories, Braga and Scevini (2017) found that that the gender of politicians affected the efficacy of policies targeted to women and households, proxied by the fertility rate, but not the quality of life, proxied by the internal migration rate.

## 2.2 Spillover effects

This paper also relates to work on whether gender quotas effectively work to empower women. Two dimensions should be considered in order to answer this question. The first is whether increasing female political participation acts as a path breaker to more women entering politics in geographical areas not affected by the gender quota system. The second aspect is whether gender quotas are connected to differences in observed policy outcomes and women's empowerment in other sectors not directly connected to politics, such as the labour market.

### 2.2.1 Geographical spillover effects of female representation in politics

A small number of studies investigates geographical spillover effects of female political representation (Bhalotra et al., 2013; Broockman, 2014; Baskaran and Hessami, 2018; Gilardi and Dablac, 2019). Bhalotra et al. (2013) and Broockman (2014) examined these effects at the State level, respectively, in India and the United States, finding no impact of a woman being elected on the participation of women in neighbouring districts. At the municipal level, Gilardi and Dlabac (2019) found that electing women to political offices in Swiss municipalities has positive but decreasing consequences on the share of female candidates in neighbouring units because the election of female politicians influences the motivation of women to run for office. Also, Baskaran and Hessami (2018) show that in Germany, the additional advancement of female council candidates from their initial list rank when the mayor is female spreads to neighbouring municipalities.

However, to my knowledge, no studies have investigated the geographical spillover in the context of gender quotas, and exposure to women who attained office through gender quota may give different results to exposure to women who reached office competitively.

### 2.2.2 Cross-sector spillover effects of gender quotas on female unemployment

In addition, a limited number of studies find a relationship between gender quotas in politics and the labour market. One of the first contributions is by Beaman et al. (2011), who demonstrates that exposure to female politicians elected by gender quotas reduces the gender disparity in career aspirations among adolescents and their parents. Only Bagues and Campa (2017) have investigated if the implementation of the gender quota impacted female and male unemployment rates. The authors claim that even if the more prominent presence of women in policy-making does not seem to affect the budget composition, it is still possible that there is a change in the way policies are implemented that has a positive impact on the labour market. However, they do not find any statistically significant effect, both in the short and medium run.

# Theoretical framework

## 3.1 Spillover effects of municipalities gender quotas in unaffected jurisdictions

This thesis's first main research question investigates whether there are geographical effects of gender quotas in unaffected jurisdictions. It involves two macro-themes: the first one regards the breakdown of stereotypes and the factors that stimulate women's political participation. In contrast, the other theme considers the diffusion mechanism that produces spillovers.

### 3.1.1 Breakdown of stereotypes to favour spillover

In this subsection, I argue that gender quotas can decrease gender stereotypes by impacting the speed of revenues and the speed of payment. It matters for this thesis because an increase in efficiency may break down negative stereotypes about female politicians and enhance spillover effects. Female politicians seem to be hampered by group stereotypes, i.e. culturally shared association linking most or all group members with particular characteristics. Stereotypes have an impact on self-perception and on perceivers observations of what individuals do regularly. Perceivers are more likely to believe that the skills and personality characteristics needed to carry out a task are characteristic of that group of people if they regularly encounter that group participating in that activity (Eagly and Steffen, 1984). Thus, gender stereotypes tend to associate some activities, such as leadership activities, with men and others, such as being housewives, with women. Because of that, gender stereotypes permeate many domains, such as politics, the labour market and academia. (Braga and Scevini, 2017). In politics, gender stereotypes discourage women from running for office and lead voters to favour and elect male candidates (Akerlof and Kranton, 2000). However, different psychology's studies state that mere exposure to female leaders can reduce party and voters bias and increase women's expectations of their group (Dasgupta and Asgari, 2004).

Thus, gender quotas may help increasing female participation because exposure to counter-stereotypes may break down negative views about minorities by allowing them to show their capabilities (Coate and Loury, 1993; Dasgupta and Asgari, 2004). In this way, citizens' attitudes toward appropriate roles for women may change due to exposure to female politicians. In addition, where anti-female biases are based on statistical discrimination, increased female participation in politics, keeping competence constant across genders, may help voters change their beliefs. Indeed, during an election, voters make their choice while uncertain about the relative competence of the available candidates. Considering a simple model for statistical discrimination and assuming that voters are risk-averse, it results that voters penalise politicians for whom they lack information (Aigner and Cain, 1977; Bearman et al., 2009). Since voters see very few women politicians, the ability to extract information from a signal about the female candidate is weak, and it causes statistical discrimination. Thus, if gender quotas result in a higher female representation, for a similar level of competencies across genders, gender quotas will decrease statistical discrimination as measured by the difference in the evaluation between male and female leaders (Bearman et al., 2009). In this case, political parties will also understand from experience that women are electorally competitive. As women candidates will be accepted, as usual, parties may feel compelled to conform to these expectations (Matland

and Studlar, 1996; Gilardi and Dablac, 2019). The breakdown of negative stereotypes may also lead women to decrease their doubts about their political ability (Fox and Lawless, 2004, 2011; Lawless and Fox, 2010) and their fear of not be adequately recognised in the electoral context (Kanthak and Woon, 2015). Greater availability of successful female politicians may make other women more likely to consider running by altering their perception or suitability of a political career (Lawless and Fox, 2010; Gilardi, 2015; Gilardi and Dablac, 2019). Thus, if gender quotas break down negative stereotypes, then a temporary affirmative action program could result in permanent benefits for minorities to the degree that these stereotypes underlie discrimination (Coate and Loury, 1993).

On the other hand, gender quotas could negatively affect women's representation if elected women have less experience and, as a result, their performance is not as good as that of their male counterparts. Indeed, voters may not consider differences in experience in their evaluations, and they can conclude that female politicians are not as good as male politicians. Thus, if affirmative action does not erode or indeed exacerbate negatives attitudes toward a minority group, the policy must be maintained permanently for that group's gains to be protected (Coate and Loury, 1993).

All these studies suggest that gender quotas can increase or decrease negative stereotypes toward female politicians depending on how well female politicians perform. Since negative stereotypes are an obstacle to the efficiency of gender quotas in the geographical units affected by the reform, in the neighbourhoods and other sectors, it is pivotal to check for the efficiency of politicians before proceeding. To do so, I test for the efficiency of municipalities government in the treatment and control group using the speed of revenues collection and the speed of payment. As explained before, the speed of revenues collected is the ratio between the collected tax and transfer revenues and the total amount of assessed revenues that a municipality should collect. On the other hand, the speed of payment is the ratio between the outlays paid and the outlays committed in the municipality budget. Specifically, the speed of revenue collection is expected to increase if the local government successfully combats tax avoidance and implements centrally funded public works to obtain new blocks of capital transfers. If the local government effectively meets its promises of payment to private contractors, the speed at which payments are made is expected to increase (Drago et al., 2014). These measures are particularly suitable because they infer about the administrative capacity of the politicians without being influenced by policy preferences (Gagliarducci and Nannicini, 2010) and because efficient financial management is crucial to implement policies. Thus, it is plausible that an increase in efficiency leads to an increase in the perceived performance of the politicians. As explained in the literature review, Baltrunaite et al. (2012), Braga and Scevini (2017) and De Paola et al. (2010) show evidence of an increase in quality or performance thanks to the gender quotas. The result of De Paola et al. (2010) is particularly interesting since it shows that the rise in female representation after the gender quota reform in municipalities that voted under the quota system persisted over time since abolishing the law. Since a temporary affirmative action program results in permanent benefits for women in representation, the evidence shows that gender quotas break down negative stereotypes. Thus, it is plausible that the break down of negative stereotypes is due to an increase in the perceived performance of politicians.

These findings from the literature suggest that gender quotas may also lead to a rise in efficiency, increasing the speed of revenues collected and the speed of payment. Thus, I can formally state the null hypothesis, which indicates no effect:
**$H_0(1)$**: there are no differences in the speed of revenues collected and the speed of payment between municipalities in the treatment and the control group.
On the other hand, the alternative hypothesis states:
**$H_1(1)$**: there are differences in the speed of revenues collected and the speed of payment between municipalities in the treatment and the control group in favour of the treatment group.

### 3.1.2 Diffusion mechanism and geographical spillovers

This subsection argues that gender quotas can produce cross-unit geographical spillover effects in female political representation. This research question is based on the diffusion literature, which states that political processes and outcomes in one geographical unit are influenced by those of other units (Dobbin et al., 2007; Gilardi, 2012; Graham et al., 2013). This process does not occur only at the international level, but it can also occur within countries (Gilardi, 2012).

In the context considered by this analysis, learning is a crucial mechanism for diffusion and consist of a process in which political actors change their beliefs about the consequences of an action based on the outcomes observed elsewhere (Gilardi, 2012). Learning is defined as the process whereby people use the experience of others to estimate the likely consequences of an action. Before a policy is introduced or action is undertaken, its outcomes are by definition uncertain. People may rely on expert reports and other assessments, but other units can also be valuable information sources. Looking at the effects in the units that have already undertaken the action can be a way for an individual to evaluate what will likely happen if they choose to pursue the new action (Gilardi, 2015).

However, although people intend to learn from the experience of others, they are inherently limited by how the human brain processes new information (Gilardi, 2012). Cognitive psychologists' theories state that people rely on cognitive shortcuts like availability and representativeness when trying to make sense of information in uncertain circumstances (Tversky and Kahneman, 1974; McDermott, 2001). As Kahneman and Tversky (1982) synthesised, people replace the laws of chance with heuristics, which sometimes yield reasonable estimates and often do not (Gilardi, 2012). Because of that, the upgrade in belief can favour or not favour spillover depending on whether the upgrade in belief is positive or negative. Even if the literature shows a reduction in the gender stereotypes and an increase in the quality and in the performance of the politicians, it is not possible to be sure that this reduction will spill over to other units, also because individuals are more likely to be aware of successful female politicians in their community than in others. They also tend to consider the experiences in their municipality as more relevant to them than female candidates' experiences in the neighbourhoods (Gilardi, 2015). Furthermore, the nature of the diffusion process changes over time. Diffusion theory states that as certain practices gain normative acceptance and become progressively internalised, learning loses significance as a diffusion mechanism (Gilardi, 2012, 2015). Thus, if there are geographical spillovers, they are expected to weaken over time.

All these studies suggest that gender quotas can spill over to neighbourhood units through diffusion mechanisms and learning. People observing the outcomes in other geographical units upgrade their belief in female political representation, and the efficacy of the spillover effects depend on whether the upgrade in belief is positive or negative. Since the literature shows an increase in the quality and the performance of politicians and a decrease in gender stereotypes (De Paola et al., 2010; Baltrunaite et al., 2012; Braga and Scevini, 2017) and considering the expectations on hypothesis 1, it is plausible that there is an upgrade in belief in neighbourhoods with consequent spillovers. Thus, I can formally state the null hypothesis, which indicates no effects:
$\mathbf{H_0(2)}$: the share of elected female in a given geographical unit is not affected by the number of women elected in other geographical units.
In contrast, the alternative hypothesis states:
$\mathbf{H_1(2)}$: the share of elected female in a given geographical unit increases with the number of women elected in other geographical units.
Gilardi (2012, 2015) suggested that when a specific practice becomes internalised, the diffusion mechanism loses power. Because of that, I expect a decreasing impact of spillover effects, and I can state the null hypothesis, which indicates no effects:
$\mathbf{H_0(3)}$: the positive relationship between the number of female elected in a given unit and the number of women elected in other units do not change over time.
On the other hand, the alternative hypothesis states:

**$H_1$(3)**: the positive relationship between the number of female elected in a given unit and the number of women elected in other units weakens over time.

## 3.2 Spillover effects of municipalities gender quotas in the labour market

The last research question regards the spillover effects of the gender quotas on the labour market and specifically on the female unemployment rate and the share of housewives over the total female population aged 15 or older. Also, in this case, it involves two macro-themes: the first one regards the impact of women on policy outcomes. In contrast, the other section considers the breakdown of the barriers to female engagement across sectors.

### 3.2.1 The impact of women in policy outcomes

In this subsection, I argue that gender quotas can impact the female unemployment rate and the share of housewives by impacting policy outcomes. Gender quotas may improve the representation of women's preferences. If these preferences favour lower unemployment and enhance labour force participation, more female politicians will reduce unemployment or enhance labour force participation. Such policy effects require that female politicians should share other women's policy preferences. This condition is critical because, according to citizen-candidate models, if men and women differ in their policy preferences, the gender of policy-makers may be relevant (Osborne and Slivinski, 1996; Besley and Coate, 1997). According to Rigon and Tanzi (2011), there are gender-specific preferences in the society in the Italian context considered by this thesis. Women were more interested in unemployment, crime, health services, the education system, immigration and poverty. Nevertheless, there is no evidence that the allocation of resources among different spending categories is affected by the gender of politicians in the framework considered by this thesis (Rigon and Tanzi, 2011). It is consistent with the Median Voter Behaviour, which suggests that gender is not a determinant of politicians' voting behaviour, meaning that the preferences of female politics are similar to those of their male colleagues because politicians consider only citizens' preferences when they design policies. However, as Bagues and Campa (2017) claim, it is still possible that quotas had an impact on the economic situation of women, even if they do not impact public spending. Furthermore, it may be that also preferences and policies related to family or households can impact the results. For example, in the framework considered by this thesis, Braga and Scevini (2017) showed that gender quotas increase the efficacy of policies targeted to women and households, increasing the fertility rate. However, an increase in the fertility rate may produce a lower female labour force participation, as shown by Bloom et al. (2009).

All these studies show that the impact of women in policy outcomes depends on whether politicians share other women's policy preferences, on whether politicians preferences matter for policy outcomes and on the preferences themselves. Based on this literature, it is unclear whether there will be an effect and also the direction of the effect. The null hypothesis states:
**$H_0$(4)**: There are no differences in the temporal variations of female unemployment rate and the share of housewives over the total female population aged 15 or older between the treatment and control group.
On the other hand, the alternative hypothesis states:
**$H_1$(4)**: There are no differences in the temporal variations of female unemployment rate and the share of housewives over the total female population aged 15 or older between the treatment and control group.

### 3.2.2 Breakdown of barriers to female engagement across sectors

Even if women may not impact policy outcomes, through policy-making, female politicians can impact the female unemployment rate and the share of housewives easing the existing barriers to the participation of women in society in different areas and at different levels (Schwindt-Bayer and Mishler, 2005). As stated in the previous section, gender quotas can break down stereotypes, and they can influence other sectors because of the diffusion mechanism. Furthermore, gender quotas may improve investments by women. Indeed, women may under-invest in their human capital if they believe leadership opportunities are unavailable. Through providing incentives for political advancement, gender quota can encourage women to invest more in their education, career and leadership potential (Pande et al., 2011). It can have a positive impact in the female labour market participation rate and a negative impact in the female unemployment rate. However, it may be that gender quotas lead to a breakdown in stereotypes in politics but not in other sectors not directly related to politics. Also, in this case, it is unclear whether there will be an effect on hypothesis 4.

# The Italian institutional framework

## 4.1 The institutional framework

The Italian Constitution divides local government into three levels: regions, provinces, and municipalities. In the period considered, in Italy, there were 20 regions, about 110 provinces and about 8.100 municipalities. The number of provinces and municipalities is subject to slight changes over time due to the merger or creation of provinces and municipalities. Municipal governments are responsible for providing many key local services, such as nursery schools, assistance to elders, public housing, transportation, and waste collection. Furthermore, municipalities provide several facilities, such as libraries, sports facilities and recreation centres.

Municipal administration is organised in three bodies: the mayor, the Municipal Council and the Municipal Executive Committee. The Municipal Council is the representative assembly, and it has legislative power. It supervises the legislative activity of the mayor and endorses the proposed policies, including the annual budget, with majority rules. The Municipal Council's size is determined by national law, and it has been decreased in recent years for budgetary reasons. Table (4.1) shows the number of members in the period considered in this paper.

Table 4.1: Seats in Municipal Council 1946-2011 by population levels

| Population | 1946 | 1993 | 2010 | 2011 |
|---|---|---|---|---|
| 0 - 3000 | 15 | 12 | 9 | 6 |
| 3001 - 5000 | 20 | 16 | 12 | 7 |
| 5001-10000 | 20 | 16 | 12 | 10 |
| 10001-30000 | 30 | 20 | 16 | 16 |
| 30001-100000 | 40 | 30 | 24 | 24 |
| 100001-250000 | 50 | 40 | 32 | 32 |
| 250001-500000 | 60 | 46 | 36 | 36 |
| 500001-1000000 | 80 | 50 | 40 | 40 |
| 1000000 and over | 80 | 60 | 48 | 48 |

The mayor holds the executive power, and she/he is responsible for the municipality administration and the supervision of all the functions proper to the municipality. She/he heads the Executive Committee, and she/he is one of the Municipal Council members. Since 1993, the mayor is elected by citizens with plurality rule together with Municipal Council[1], and she/he is subject to a two-term limit. The Municipal Executive is nominated by the mayor, who coherently with its jurisdiction can appoint or withdraw mandates to its member and attribute specific powers to them. The primary function of the Executive is the cooperation with the mayor in the governance of the

[1]Individual ballots and plurality criteria are used to distribute seats on the Municipal Council. This method, which replaced a party ballot system, was implemented in 1993. Elections are held with a single ballot in cities with a population of less than 15,000 people, and the successful candidate receives a majority premium of at least two-thirds of the seats in the Council; for municipalities with populations greater than 15,000, elections are held using a dual ballot (the second ballot is held only if none of the candidates receives an absolute majority of votes in the first ballot), with the winning candidate receiving a majority premium of at least 60% of the council seats.

municipality. The law assigns to the Municipal Executive the functions not attributed to the mayor or the Municipal Council. The Municipal Executive is smaller than the Council, and its size is delegated to the mayor. The latter ultimately decides the number of its cabinet within a maximum limit designed by the law. Table (4.2) summarises the maximum size of the Municipal Executive in the period considered in this thesis.

Table 4.2: Seats in Executive Committee 1990-2009 by population level

| Population | 1990 | 1993 | 2000 | 2008 | 2009 |
|---|---|---|---|---|---|
| 0 - 3000 | $\leq 4$ | $\leq 2$ | $\leq 4$ | $\leq 4$ | $\leq 3$ |
| 3001 - 10000 | $\leq 6$ | $\leq 4$ | $\leq 5$ | $\leq 5$ | $\leq 4$ |
| 10001-30000 | $\leq 6$ | $\leq 6$ | $\leq 7$ | $\leq 7$ | $\leq 5$ |
| 30001-100000 | $\leq 8$ | $\leq 6$ | $\leq 10$ | $\leq 10$ | $\leq 8$ |
| 100001-250000 | $\leq 12$ | $\leq 8$ | $\leq 13$ | $\leq 12$ | $\leq 10$ |
| 250001-500000 | $\leq 12$ | $\leq 8$ | $\leq 15$ | $\leq 12$ | $\leq 12$ |
| 500001 and over | $\leq 16$ | $\leq 8$ | $\leq 16$ | $\leq 12$ | $\leq 12$ |

In 1993, the duration of a legislative term was shortened from five to four years, then restored to five years in 2000. However, various reasons could lead a municipality to vote before the natural end. Early termination of the legislature can occur because of the mayor's resignation, the resignation of the majority of the Council or a vote of no-confidence in the Council, the mayor's death, ex-post incompatibility, or the mayor is accused of a crime. If it is impossible to build a new government coalition in these circumstances, anticipated elections are called.

## 4.2 The gender quota law in municipality elections

A relevant institutional change in the municipal election is represented by the gender quotas system introduced in March 1993. Law No. 81 stated that no gender could represent more than two-thirds of candidates' municipality council list. The maximum quota for either sex was set at three quarters in municipalities with up to fifteen thousand people[2]. This law established a minimum number of female candidates through the reservation of candidacies. Unlike quota systems based on the reservation of seats did not guarantee that a minimum number of women would be elected. This law was enforced in all the elections that took place between 1993 and 1995. On September 12, 1995 (Sentence no. 422), the Constitutional Court abrogated the reservation quotas declaring illegitimacy for the Constitution [3]. The law was repealed under article 51 of the Italian Constitution, which states: *"All citizens of either sex are qualified for public offices and elective positions on equal terms, subject to the conditions specified by law"*. The constitutional right to fair access to elective offices, as defined by Art. 3 and Art. 51 of the Constitution cannot be subject to special treatment based on sex.

Because of that, only some municipalities were affected by the gender quotas law. Specifically, 7.703 municipalities voted during the reform, while 397 municipalities never voted during the reform. Figure (4.1) maps the geographical location of the municipalities in the treatment and the control group, showing that control municipalities are well distributed between the entire country.

[2]Provincial Councils and the Chamber of Deputies were also subjected to the same procedures. In February 1995 (Law No. 43), Regional Council elections were also subject to the gender quota system.

[3]Since legislative gender quotas were repealed, certain political parties have adopted, voluntarily, regulations requiring a particular number of women to be presented as candidates


Figure 4.1: Geographical location of the treatment and control groups

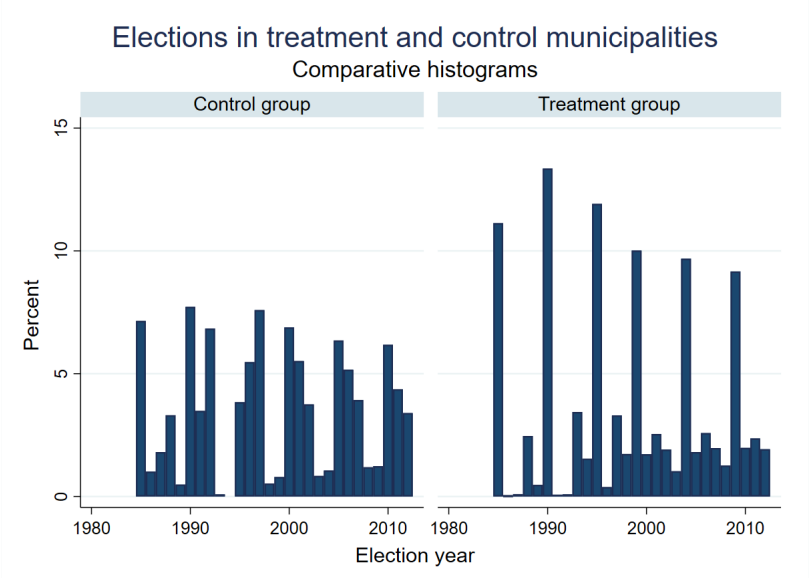



Figure 4.2: Elections in treatment and control municipalities

Given the fixed term of local elections, when voting occurs in a municipality and hence whether a municipality voted under the gender quota is decided by historical events unrelated to the gender quota law. As shown by Figure (4.2), the timing of the elections in both groups differs by construction. Because of the election's fixed term, the data shows a cyclical trend, with several elections in some years and few in others. As a result, the trend is not a specific feature of the elections around the time of adopting the reform since it is visible as early as in the mid-eighties. Thus, the

difference in the timing of elections across municipalities can be explained by the early termination of certain local councils. These results support the claim that the scheduling of elections around the introduction of gender quotas is not due to local officials manipulating election dates with the explicit purpose of conducting elections with or without gender quotas. Thus, the division between the treatment and control group is assumed to be completely random. De Paola et al. (2010) confirmed this claim, proving no relevant differences in female representation between municipalities belonging to the treatment and the control groups before 1993. Thus, it is possible to assume that the difference in the outcome between the treatment and the control group would have remained the same in the absence of the reform. It means:

$$E[\epsilon_{i,t}/Treated_i, Postquota_t, X_{i,t}] = 0 \tag{4.1}$$

where i indexes the municipality and t the election year. Treated$_i$ is a dummy variable for municipalities that voted under the gender quota; Postquota$_t$ is a dummy equal to one for elections taking place after introducing the law; $X_{i,t}$ is a vector of municipal characteristics, and $\epsilon_{i,t}$ is the error term. Hence, as has been done by several scholars (De Paola et al., 2010; Rigon and Tanzi, 2011; Baltrunaite et al., 2012; De Paola et al., 2012; Braga and Scevini, 2017), this paper uses this random variation in whether a municipality voted under the law or not.

Furthermore, this thesis is perfectly aware of the potential mixing between the two groups, which would happen when the electoral campaign is run right before adopting or right after abolishing gender quotas. It means that there could be some municipalities in the treatment group that ran their campaign without gender quotas. Similarly, some control group municipalities could have their electoral lists prepared and promoted during the period when gender quotas were in place but voted in their absence. However, given that electoral campaigns officially last 30 days, there is no such mixing in the sample because no municipalities voted during the 30 days after March 25, 1993, and 30 days after September 12, 1995 (Baltrunaite et al., 2012).

# Data

## 5.1 Data

This section aims to give a brief overview of the information contained in the data and how the data are used to address the research questions. The complete collected dataset is a panel data set containing data for all the Italian municipalities. The sample size is around 8.100, which corresponds to the number of municipalities. While the time covered by the analysis changes depending on the hypothesis considered[1]. The primary data source is the Italian Ministry of Internal Affairs. Indeed, on their website, there is information on the characteristics of elected candidates[2] and yearly balance sheets[3]. The dataset regarding the characteristics of elected candidates also carries information on the size of the resident population. Furthermore, 1981, 1991, 2001 and 2011 Italian Census of Population are used to obtain data, at the municipality level on the labour market outcomes of the resident population and the average education attained by the population[4]

Since the different research questions use different data sets, I created three separate panels on the previously illustrated data. I divide the section into three subsections to explain each data set. While a complete list of all the variable used in the analysis, comprehensive information about their type, their description is available in Appendix A.

### 5.1.1 Hypothesis 1: difference in efficiency

To evaluate whether increased competition of possible female entrants impacts the performance of the elected politicians, the speed of revenues collected and the speed of payments are used as a measure for efficiency. All these data are available in the financial indicators balance sheet. Unfortunately, the availability of these data is limited from 1998 to 2008. From 2009 the variables that made up the formulas are inexact. Because of that, they were not computed by the local finance office of the Ministry of the Internal Affairs. Also, the data are available from 1998 because the new model of final certificates of municipalities and the corresponding financial indicators were started in 1998. However, since the elections are held every four years[5], it is possible to evaluate the efficiency of a set of Municipal Councils elected from 1995. For this hypothesis, the total number of observations is equal to 83.826.

### 5.1.2 Hypothesis 2 & 3: geographical spillovers

Then, I assess the spillover effects of gender quotas in politics, considering the share of female winners. To do so, three main dependent variables are considered: the percentage of female politicians in a

[1] Further details are provided below

[2] Data are available at the following link: https://dait.interno.gov.it/elezioni/anagrafe-amministratori

[3] Data are available at the following link: https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/8

[4] For the 2011 Italian Census data are available at the following link http://dati-censimentopopolazione.istat.it/Index.aspx?lang=it.
For the other years, it is possible to download the database containing all the data used at the following link: https://www.istat.it/it/archivio/113712

[5] In 1993, the duration of a legislative term was shortened from five to four years, then restored to five years in 2000.

Municipal Council, the percentage of female politicians in the Executive Committee and a dummy variable that indicates whether the mayor is female or male. These variables are computed using data on the characteristics of elected candidates. The data set contains 40.649 observations, and it is a panel from 1985 to 2012. The choice of 2012 is dictated by a new law for gender quotas introduced in 2013 at the municipal level. To investigate spillovers among neighbourhood units, I focus on Italy's 610 Labour Market Area (LMAs)[6]. These territorial units are well-suited because they capture small and homogeneous zones. In the dataset of this thesis, the average number of municipalities in LMAs is equal to 32,46.

### 5.1.3 Hypothesis 4: cross-sector spillovers

To evaluate the effects of gender quotas in the labour market, the female unemployment rate and the share of housewives over the total female population aged 15 or older[7] are used as dependent variables. Data on female unemployment and housewives at the municipal level are available only for the Census years. Thus, the unemployment rate will be analysed only for 1981, 1991, 2001 and 2011. Regarding the share of housewives, the data will be analysed from 1991 because of data unavailability. For this hypothesis, the dataset contains 32.372 observations for the female unemployment rate and 24.286 observations for the share of housewives.

## 5.2 Descriptive statistics

The descriptive statistics of all the variables used in the analysis are presented in Table (5.1) divided according to the different hypotheses.

Table 5.1: Descriptive statistics of all the variables used in the analysis

| | N | Mean | Median | S.d. | Min | Max |
|---|---|---|---|---|---|---|
| | | | Hypothesis 1 | | | |
| Speed of Rev. | 83826 | 59.334 | 60.39 | 15.811 | 0 | 100 |
| Speed of Pay. | 83826 | 78.275 | 78.94 | 7.506 | 0 | 100 |
| treated | 83826 | .952 | 1 | .214 | 0 | 1 |
| population | 83826 | 6371.595 | 2327 | 24777.09 | 31 | 2733908 |
| employed/total population | 83826 | .409 | .414 | .093 | .112 | .714 |
| average education | 83826 | 7.402 | 7.501 | .951 | 2.696 | 12.622 |
| share empl. in the Council | 83826 | .996 | 1 | .02 | .235 | 1 |
| average education in the Council | 83826 | 12.234 | 12.428 | 1.885 | .929 | 17.167 |
| early termination | 83826 | .05 | 0 | .218 | 0 | 1 |
| share members of the Council with previous political experience | 83826 | .602 | .611 | .152 | 0 | 1 |
| | | | Hypothesis 2 & 3 | | | |
| female mayor | 40649 | .071 | 0 | .258 | 0 | 1 |
| share female Ex. Committee | 40649 | .141 | 0 | .182 | 0 | 1 |

[6]Labour market Areas are 610 sub-regional geographical areas where the bulk of the labour force lives and works, and where companies can find most of the labour force necessary to occupy jobs

[7]This paper uses the Census definition of housewives, i.e. people aged 15 and above who are mainly dedicated to housework. The threshold of 15 years old is because 15 is the legal working age

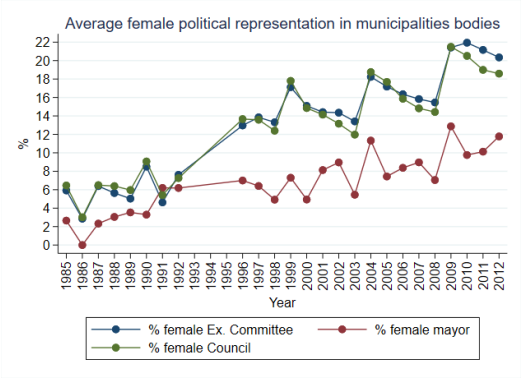
Table 5.1: Descriptive statistics of all the variables used in the analysis

| | N | Mean | Median | S.d. | Min | Max |
|---|---|---|---|---|---|---|
| share female Council | 40649 | .142 | .125 | .11 | 0 | .818 |
| treatment | 40649 | .944 | 1 | .229 | 0 | 1 |
| share treated LMA | 40649 | .95 | .978 | .078 | .333 | 1 |
| postquota | 40649 | .667 | 1 | .471 | 0 | 1 |
| population | 40649 | 6984.703 | 2309 | 41600 | 31 | 2840259 |
| average education | 40649 | 6.708 | 6.76 | 1.376 | 2.628 | 12.622 |
| employed/ total population | 40649 | .386 | .388 | .09 | .112 | .74 |
| | | | Hypothesis 4 | | | |
| share female unempl | 32372 | .171 | .125 | .128 | 0 | .854 |
| share housewives | 24286 | .242 | .233 | .092 | 0 | .755 |
| treated | 32372 | .951 | 1 | .216 | 0 | 1 |
| postquota | 32372 | .5 | 0 | .5 | 0 | 1 |
| share male unempl | 32372 | .1 | .063 | .089 | 0 | .634 |
| share unempl | 32372 | .125 | .084 | .099 | 0 | .708 |
| pop +15 | 32372 | 6597.896 | 2180 | 39462.27 | 29 | 2840259 |
| pop +15 female | 32372 | 3413.968 | 1114 | 20814.16 | 15 | 1480868 |
| share female active pop | 32372 | .33 | .323 | .089 | .055 | .717 |
| female average education | 32372 | 6.627 | 6.699 | 1.452 | 2.117 | 12.583 |
| average education | 32372 | 7.039 | 7.106 | 1.502 | 2.628 | 12.622 |
| share househusbands | 24286 | .002 | 0 | .003 | 0 | .101 |

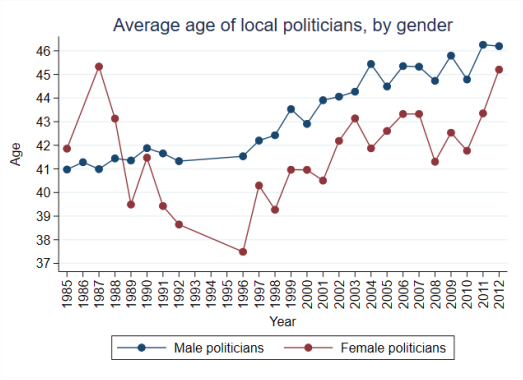
Considering the entire data set and looking at the municipalities characteristics, the average population size of Italian municipalities is 6.985, but only 7,8% of municipalities have a population greater than 15.000 inhabitants. In the data indicated by the Census (1981-2011), the average education of the population is 7,04 years, and it is higher for males (7,31) than for females (6,63). The average unemployment rate is at 12%, and it is higher for females (17%) than males (10%). Furthermore, women are more likely to be housewives and their labour force participation is lower. Regarding the municipalities' bodies characteristics from 1985 to 2012, the data set reveals that women are strongly underrepresented. Although the share of women in the Executive Committee and the Municipal Council has been increasing over time, it reached respectively only 20% and 18% in 2012. Also, the share of female mayors have increased over time, and in 2012 the female mayors were 12% of the sample. As shown in Figure (5.1a), a substantial rise had occurred when the gender quota law was implemented, especially for the share of women in the Municipal Council and the Executive Committee[8]. On average, female politicians[9] are more educated and younger than male ones, but they are more likely to be unemployed. Indeed, women politicians had spent around 13,7 years of education, while the male counterpart had spent 11,8 years in education. The fact that female politicians in our sample are, on average, more educated than men is consistent with the existence of a bias in favour of male candidates. Furthermore, the average female age was 42 years, one and a half years younger than the politicians' average male age. However, the share of unemployed female politicians was on average 0.5%, while the male average was 0.2%. These gender gaps have kept almost constant over time, as shown by Figure (5.1b),(5.1c), (5.1d).

[8]Note that the % in the labels of Figure (5.1a) indicates the share of female members over the total.

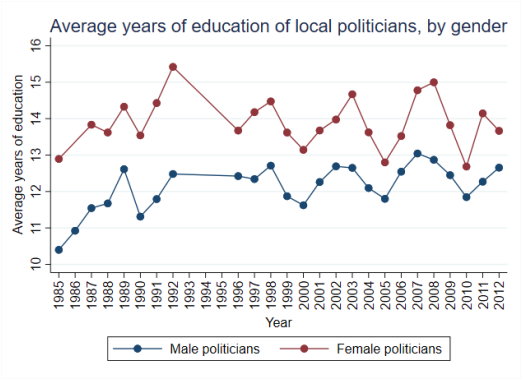
[9]Female politicians, in this case, indicates the members of the Executive Committee, the members of the Municipal Councils and the mayors



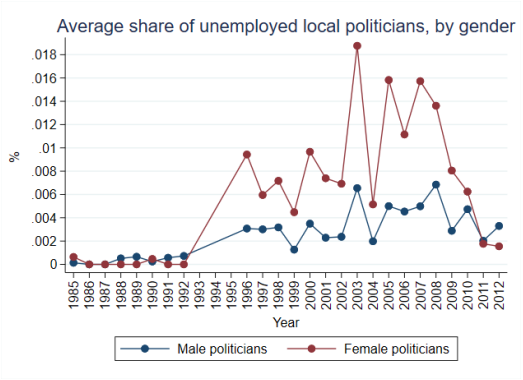

(a) Average female political representation in Municipalities bodies




(b) Average age of local politicians by gender




(c) Average years of education of local politicians by gender




(d) Average share of unemployed local politicians by gender

Figure 5.1: Female representation in municipal bodies and characteristics of elected politicians (1985-2012)

The population size is shown for the treatment and the control group before the reform in Table (5.2) to provide further proof that there were no consistent differences between the treatment and the control group before the introduction of the reform. In the tables, the t-tests are reported comparing the means between the treatment and the control group. The null hypothesis is that the difference between the means is zero.

Table 5.2: Average municipality population in the treatment and in the control group before the reform

| | Before |
|---|---|
| Treatment group | 5926.237 |
| Standard error | 341.647 |
| N | 12788 |
| Control group | 5435.527 |
| Standard error | 436.142 |
| N | 742 |
| t-stat | -0.345 |
| p-value | 0.7301 |
| Total N | 13530 |

The t-tests are reported comparing the means between the treatment and the control

The null hypothesis states that the difference
between the means is zero.

Table (5.2) shows that there are no differences in the population between the treatment and control groups before introducing the reform. Thus, it confirms an exogenous variation between the two groups. In addition, in Tables (5.3), (5.4), (5.5), the means of the dependent variables are shown for the treatment and the control group and in Tables (5.4), (5.5) the means are calculated dividing the data before and after the introduction of the gender quotas. The idea behind these tables is to provide a descriptive insight of the change of the dependent variable between the two groups and between the time threshold. Table (5.3) shows that the speed of revenues collected and the speed of payment are higher in the treatment group. Since the data starts from 1998, it is not possible to compare before and after, but the difference between the treatment and the control group is statistically significant at 1% level for both the dependent variables.

Table 5.3: Average speed of revenues collected and speed of payment in the treatment and in the control group after the reform (1998-2008)

| | Average speed of revenues collected | Average speed of payment |
|---|---|---|
| Treatment group | 59.6 | 78.306 |
| Standard error | 0.056 | 0.265 |
| N | 79791 | 79791 |
| Control group | 54.085 | 77.672 |
| Standard error | 0.267 | 0.125 |
| N | 4035 | 4035 |
| t-stat | -21.676 | -5.235 |
| p-value | 0.0000 | 0.0000 |
| Total N | 83826 | 83826 |

The t-tests are reported comparing the means between the treatment and the control
The null hypothesis states that the difference between the means is zero.

Table (5.4) shows how female representation evolved in the two groups of municipalities, before and after the adoption of the gender quotas. It emerges that the temporal change in female representation was positive for both groups; the increase in municipalities that voted with gender quotas is more pronounced than in municipalities that did not. The differences between the treatment and the control group are statistically significant at 5% level, except for the difference between the two groups after the reform for the share of female mayors. The fact that the differences in the two groups were statistically significant also before the introduction of the gender quotas does not question the claims of random assignment between the two groups because the key identification assumption is that in the absence of the policy intervention, the differences in female political involvement between treatment and control group would have been the same over time. Thus, it is possible that there were some descriptive differences between the groups before introducing the reform.

Table 5.4: Share of female politicians in the treatment and in the control group before and after the reform

| | Before(1985-1992) | After(1996-2012) | t-stat | p-value |
|---|---|---|---|---|
| Average share female politicians in the Municipal Council | | | | |
| Treatment group | 0.077 | 0.176 | -90.771 | 0.0000 |
| Standard error | 0.001 | 0.001 | | |
| N | 12788 | 25608 | | |
| Control group | 0.063 | 0.146 | -18.544 | 0.00000 |
| Standard error | 0.003 | 0.003 | | |
| N | 742 | 1511 | | |

| | Before (1985-1992) | After (1996-2012) | t-stat | p-value |
|---|---|---|---|---|
| t-stat | -5.282 | -10.1291 | | |
| p-value | 0.0000 | 0.0000 | | |
| Total N | 13530 | 27119 | | |
| Average share female politicians in the Executive Committee | | | | |
| Treatment group | 0.072 | 0.178 | -55.801 | 0.0000 |
| Standard error | 0.001 | 0.001 | | |
| N | 12788 | 25608 | | |
| Control group | 0.057 | 0.146 | -18.201 | 0.00000 |
| Standard error | 0.004 | 0.005 | | |
| N | 742 | 1511 | | |
| t-stat | -3.137 | -6.113 | | |
| p-value | 0.0017 | 0.0000 | | |
| Total N | 13530 | 27119 | | |
| Average share female mayors | | | | |
| Treatment group | 0.030 | 0.093 | -22.662 | 0.0000 |
| Standard error | 0.001 | 0.002 | | |
| N | 12788 | 25608 | | |
| Control group | 0.047 | 0.08 | -2.796 | 0.0052 |
| Standard error | 0.008 | 0.007 | | |
| N | 742 | 1511 | | |
| t-stat | 2.666 | -1.8238 | | |
| p-value | 0.0077 | 0.0682 | | |
| Total N | 13530 | 27119 | | |

The t-tests are reported comparing the means between the treatment and the control group and between before and after. The null hypothesis states that the difference between the means is zero.

Regarding the labour market, Table (5.5) shows that after the reform, the average female unemployment rate decreased. All differences are highly statistically significant. On the other hand, the share of housewives shows a slightly greater average value in the treatment group than the control group before introducing the gender quotas. At the same time, the trend is reverted after the law, even if the difference is minimal. Indeed, no difference between the treatment and the control group is statistically significant at 5% level. In contrast, the differences between before and after are highly significant for both groups.

Table 5.5: Average female unemployment rate and share of housewives in the treatment and in the control group before and after the reform

| | Before (1981, 1991) | After (2001, 2011) | t-stat | p-value |
|---|---|---|---|---|
| Average female unemployment rate | | | | |
| Treatment group | 0.203 | 0.132 | 51.394 | 0.0000 |
| Standard error | 0.001 | 0.001 | | |
| N | 15395 | 15387 | | |
| Control group | 0.278 | 0.179 | 14.151 | 0.0000 |
| Standard error | 0.006 | 0.004 | | |
| N | 794 | 796 | | |
| t-stat | 14.693 | 12.57 | | |
| p-value | 0.0000 | 0.0000 | | |
| Total N | 16189 | 16183 | | |
| Average share of housewives | | | | |
| Treatment group | 0.276 | 0.225 | 41.998 | 0.0000 |
| Standard error | 0.001 | 0.001 | | |
| N | 7705 | 15387 | | |

| | Before (1981, 1991) | After (2001, 2011) | t-stat | p-value |
|---|---|---|---|---|
| Control group | 0.27 | 0.23 | 6.738 | 0.00000 |
| Standard error | 0.005 | 0.003 | | |
| N | 398 | 796 | | |
| t-stat | -1.27 | 1.76 | | |
| p-value | 0.2044 | 0.0785 | | |
| Total N | 8103 | 16183 | | |

The t-tests are reported comparing the means between the treatment and the control group and between before and after. The null hypothesis states that the difference between the means is zero.

# Methodology

## 6.1 Hypothesis 1: difference in efficiency

The hypothesis $H_1(1)$ states: "there are differences in the speed of revenues collected and the speed of payment between the municipalities in the treatment and the control group in favour of the treatment group."
The model to test against $H_0(1)$ is estimated using OLS. It compares the treatment to the control group in terms of the speed of revenues collected and speed of payment. With the data at hand, it is impossible to compare the before and after gender quotas. However, since there is random assignment between the treatment and the control group, as shown in section (4.2), there should be no difference between the treatment and the control group except for the treatment effect. Thus, it is possible to make causal claims about the differences between the two groups, but it is not possible to claim how much the treatment has changed over time relative to the control.

$$y_{i,t} = \beta_0 + \beta_1 treated_i + \beta_2 X_{i,t} + fixed\_effects + year\_fixed\_effects + \epsilon_{i,t} \tag{6.1}$$

where i indexes the municipality and t the year. $y_{i,t}$ refers to the dependent variables used in this regression, i.e. the speed of revenue collection and the speed of payment. $treated_i$ is a dummy variable equal to one if the municipality i is in the treatment group. The model is estimated with a set of controls for the municipalities and municipality's Council characteristics[1]; $X_{i,t}$ includes the population size, the average education in the population of the municipality, the ratio between the employed and the total population, the share of Municipal Council members that were employed, the average education of the Municipal Council, the share of council members with a previous political experience, and a dummy variable equal to one if there is earlier termination before the mandate's natural end. $fixed\_effects$ is a vector of provincial dummies, which allow taking into account that municipalities in the control group may differ from those included in the treatment group. Furthermore, $year\_fixed\_effects$ is a vector of year dummies, and it is added to control for factors changing over years that are common to all municipalities, affecting the dependent variable. In this way, the bias from unobservables that change over time is eliminated. $\epsilon_{i,t}$ is an error term. Standard errors are cluster at the municipality level to account for within-group residual correlations. The errors are also are robust to heteroskedasticity.

## 6.2 Hypothesis 2 & 3: geographical spillovers

Two hypotheses are stated to investigate the spillover effects on other units not affected by the quota system:

- $H_1(2)$: "the share of elected female in a given unit increases with the number of women elected in other units."
- $H_1(3)$: "the positive relationship between the number of female elected in a given unit and the number of women elected in other units weakens over time."

[1]The choice to controls only for council characteristics is because it is responsible for the endorsement of the annual budget. Further details are provided in section (4.1)

The analysis starts with regression (6.2) to test against these hypotheses. This first regression is the baseline for the other models, and it replicates the analysis of De Paola et al. (2010), extending the time period to 2012[2]. A Difference-in-Difference (DiD) approach is applied for this regression, and results are estimated using OLS. The reasons behind this choice are based on the fact that DiD measures the impact of the program intervention by the difference in the before-after change in outcomes between participants and nonparticipants, i.e. the treatment and the control group.

$$y_{i,t} = \beta_0 + \beta_1 treated_i + \beta_2 postquota_t + \beta_3 (treated_i)(postquota_t) + \beta_4 X_{i,t} + fixed\_effects + \epsilon_i \quad (6.2)$$

where i indexes the municipality and t the election year. $y_i$ represents the dependent variables used in this regression, i.e. the share of women in the Municipal Council over the total number of seats, the share of women in the Executive Committee over the total number of seats and a dummy equal to one if the mayor is female and equal to zero if the mayor is male. $postquota_t$ is a dummy variable equal to zero for the period before introducing the quota (1985-1992) and equal to one for the period after abolishing the quota (1996-2012). $(Treated_i)(Postquota_t)$ is the interaction term, and its coefficient measures the difference in the temporal variations of female representation between the municipalities that voted under the gender quotas and the municipalities that do not vote. Also, this model is estimated with a set of controls for the municipalities; $X_{i,t}$ is a vector of municipality characteristics, including the population size, the share of the employed resident population and the average education of the population. $fixed\_effects_{i,t}$ is a vector of municipalities or provincial dummies. The inclusion of a vector for provincial dummies considers that non-gender quota municipalities might differ from those included in the treatment group. In addition, I experiment with municipal fixed effects to see whether the results hold. Indeed, even though there are no reason to think of selection problems driving our results, fixed effect estimates ensure that the effect of gender quotas on female representation is not due to unobservable municipal characteristics which, for some reason, are related both to inclusion in the treatment group and greater female involvement in political activity. Furthermore, the Hausman test, which is conducted to differentiate between the fixed effects model and the random effects model in panel analysis, shows that controlling for fixed effects is appropriate. Indeed, under the null hypothesis that the random effects model is appropriate, the test shows that it is possible to reject the null hypothesis at 5% level for the dependent variables considered[3]. Also, in this regression, standard errors are cluster at the municipality level, and they are robust to heteroskedasticity.

New models are developed to test against $H_0(2)$. These models are based on Romarri (2020) but differ from its models because Romarri (2020) uses a standard dummy variable to measure the effects of the treatment group, while this study uses (*share treated in LMA*), a variable that defines the continuous treatment, measured by the share of municipalities treated in a LMA over the total number of municipalities for each LMA. First of all, a OLS model is run:

$$y_{i,t} = \beta_0 + \beta_1 (share\_treated\_in\_LMA)_i + \beta_2 X_{i,t} + fixed\_effects + \epsilon_i \quad (6.3)$$

where $y_i$, $X_{i,t}$ and the $fixed\_effects_{i,t}$ are the same as the one specifies in regression (6.2). The idea of this regression is to see how the dependent variables are affected by the share of treated in a LMA.

Having run this regression, a Difference-in-Difference strategy is implemented.

$$y_{i,t} = \beta_0 + \beta_1 (share\_treated\_in\_LMA)_i + \beta_2 postquota_t + \beta_3 (share\_treated\_in\_LMA)_i (postquota)_t + \beta_4 X_{i,t} + fixed\_effects + \epsilon_i \quad (6.4)$$

This model is analogous to model (6.2), with the unique difference that *treated* is substituted with *(share treated in LMA)*.

[2]The time period analysed by De Paola et al. (2010) goes from 1985 to 2007

[3]The results of the Hausman test are reported in Appendix B

Then, a related analysis is carried out to check if the potential spillovers are heterogeneous over time. The aim of regression (6.5) is to test against $H_0(3)$

$$y_{i,t} = \beta_0 + \beta_1(sharetreatedinLMA)_i + \beta_2(election\_postquota)_t + \beta_3(share\_treated\_in\_LMA)_i \\ (election\_postquota)_t + \beta_4 X_{i,t} + fixed\_effects + \epsilon_i \tag{6.5}$$

where the generic variable *postquota* is substituted with *election_postquota*, that represents the exact number of election elapsed since 1995, i.e. when quota were abolished.

## 6.3 Hypothesis 4: cross-sector spillovers

$H_0(4)$ states: "there are no differences in the temporal variations of female unemployment rate and the share of housewives over the total female population aged 15 or older between the treatment and control group."
Equation (6.6) shows the regression modelled to test against the hypothesis $H_0(4)$. Also in this case, the model used is a Difference-in-Difference and results are estimated using OLS.

$$y_{i,t} = \beta_0 + \beta_1 treated_i + \beta_2 postquota_t + \beta_3(treated_i)(postquota_t) + \\ \beta_4 X_{i,t} + fixed\_effects + \epsilon_{i,t} \tag{6.6}$$

where i indexes the municipality and t the Census year. $y_{i,t}$ represents the the two dependent variables considered: the female unemployment rate and the share of housewives over the total female population. The female unemployment rate is computed as the ratio between the number of female unemployed and the active female population. Housewives' share is computed as the ratio between female housewives and the total female population over 15 years old. $X_{i,t}$ includes the population size after 15 years old, the share of the active female population, male unemployment rate, average female education, population average education. $fixed\_effects$ is a vector for municipalities or provincial dummies. This inclusion has already been explained in section (6.1), and the Hausman test confirms this choice [4]. In this regression, standard errors are cluster at the municipality level, and they are robust to heteroskedasticity.

[4]The results of the Hausman test are reported in Appendix B

# Analysis & Results

## 7.1 Hypothesis 1: difference in efficiency

The results from the regressions (6.1) testing against $H_0(1)$ are presented in Table (7.1). Full results from the analysis of all the hypothesis, including coefficients from control variables, are presented in Appendix C.

Table 7.1: Regression results $H_0(1)$ Eq. (6.1).

| | (1) Speed of Rev. | (2) Speed of Rev. | (3) Speed of Pay. | (4) Speed of Pay. |
|---|---|---|---|---|
| treated | 3.161*** | 0.931* | 0.652* | 0.155 |
| | (0.591) | (0.467) | (0.301) | (0.273) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES |
| YEAR FIXED EFFECTS | NO | YES | NO | YES |
| $N$ | 83826 | 83826 | 83826 | 83826 |
| $R^2$ Within | 0.104 | 0.174 | 0.028 | 0.059 |
| $R^2$ Overall | 0.262 | 0.428 | 0.029 | 0.158 |
| $R^2$ Between | 0.357 | 0.563 | 0.037 | 0.244 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression includes controls for: share empl. in the Council, average education in the Council, early termination of the mandate, share members of the Council with previous political experience, pop, $(pop)^2$, employed/population and average education

Full results, including coefficients from control variables, are available in Appendix C Table (9.12)

The results in Table (7.1), column (1) suggest that being in the treatment group has a statistically significant impact on the speed of revenue collected. This effect is robust to the inclusion of provincial and year fixed effects (column (2)). Looking at column (2), the speed of revenues collected is 0,93 percentage points higher in gender quota municipalities. The relationship between the speed of revenue collection and the controls shows that population has a marginal but positive effect. In addition, the average education of the population has a positive impact on the speed of revenues collected, as well as the average education of the Council and the share of members with previous political experience.

On the other hand, Table (7.1) column (3) shows that being in the treatment group has a positive effect on the speed of payment. Indeed, the speed of payment is 0,65 percentage points higher in the gender quota municipalities. However, the result is not statistically significant, controlling for provincial and year fixed effects. Observing the controls shown in column (4), the municipal-

ity's population has a marginal but negative effect, and also, the average education has a negative impact on the speed of payment. On the other hand, the share of employed over the total population has a positive effect as well as the percentage of an employed member of the Municipal Council.

In summary, the results indicate overall a positive relationship between being in the treatment group and the two efficiency indicators, but only the speed of revenues collection is robust to the inclusion of provincial and year fixed effects. It seems plausible that there were some unobservable provincial or time characteristics related to the inclusion in the treatment group and the speed of payment. Controlling for them, the coefficient of *treated* becomes insignificant. On the other hand, it is possible to conclude that being a gender quota municipalities has a positive impact on the speed of revenues, even if it is not possible to make causal claims about how much the treatment group has changed over time relative to the control. Thus, the first hypothesis $H_0(1)$ is partly rejected: there are differences between being in the treatment and the control group in terms of speed of revenues collected, but there are not in terms of the speed of payment. Since there was an increase in the speed of revenues, gender quotas lead to a rise in the efficiency of the politicians. Furthermore, the finding of Baltruinaite et al. (2012), Braga and Scevini (2017) and De Paola et al. (2010) suggest respectively an increase in the overall quality or performance of the politicians and persistence of female politicians even after the abolishment of gender quotas. Because of that results, it is possible to proceed with the analysis, assuming a perceived increase in the performance of the politicians.

## 7.2 Hypothesis 2 & 3: geographical spillovers

In order to test against $H_0(2)$ and $H_0(3)$, a preliminary regression is run using regression (6.2). The results are presented in Table (7.2)

Table 7.2: Regression results Eq. (6.2)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| treated | 0.000 | -0.006 | 0.000 | -0.004 | 0.000 | -0.030*** |
| | (.) | (0.003) | (.) | (0.005) | (.) | (0.009) |
| postquota | 0.065*** | 0.072*** | 0.063*** | 0.069*** | 0.002 | 0.010 |
| | (0.004) | (0.004) | (0.008) | (0.007) | (0.012) | (0.012) |
| (treated)(postquota) | 0.014** | 0.014*** | 0.015* | 0.015* | 0.027* | 0.028* |
| | (0.004) | (0.004) | (0.008) | (0.007) | (0.012) | (0.012) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 40649 | 40649 | 40649 | 40649 | 40649 | 40649 |
| $R^2$ | 0.310 | | 0.117 | | 0.024 | |
| $R^2$ Adj. | 0.310 | | 0.117 | | 0.024 | |
| $R^2$ Within | 0.310 | 0.311 | 0.117 | 0.117 | 0.024 | 0.025 |
| $R^2$ Overall | 0.210 | 0.312 | 0.059 | 0.130 | 0.018 | 0.033 |
| $R^2$ Between | 0.030 | 0.315 | 0.005 | 0.161 | | |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression includes controls for: pop, $(pop)^2$, average educ, employed/total population

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Full results, including coefficients from control variables, are available in Appendix C Table (9.13)

The results are in line with the findings of De Paola et al. (2010), and they show that the municipalities affected by the reform continued to have a higher female political representation compared to the control municipalities, even extending the dataset to 2012. It is a further confirmation of the politicians' perceived increase in performance thanks to gender quotas. Looking at Table (7.2), column (2), the proportion of women in the Council increased by 7,2 percentage points after 1995, but it increased by 1,4 percentage points more in municipalities directly affected by the reform. The results are highly statistically significant, and they are robust to municipalities fixed effect, as shown in column (1). Column (4) shows that after the reform, the proportion of women in the Executive Committee increased by 1,5 percentage points more in gender quotas municipalities. This result is robust controlling for municipalities gender quotas, and the impact is the same, as shown in column (3). Looking at column (6), the gender quota law has increased the probability of electing a female mayor by 2,8 percentage points. The results are consistent controlling for municipalities fixed effects (column (5)). In all the specifications in Table (7.2), I control for municipal population size and its square, education and employment levels. From the controls, it emerges that municipalities with higher average education and a higher level of employment are more likely to elect female politicians.

Then, I test the potential spillover effects by focusing on Italy's Labour Market Area, using regression (6.3).

Table 7.3: Regression results $H_0(2)$ Eq. (6.3)

| share female | (1)<br>share female Council | (2)<br>share female Ex. Committee | (3)<br>female mayor |
|---|---|---|---|
| share treated LMA | -0.004 | 0.003 | -0.012 |
| | (0.010) | (0.015) | (0.020) |
| PROVINCIAL FIXED EFFECTS | YES | YES | YES |
| $N$ | 40649 | 40649 | 40649 |
| $R^2$ Within | 0.270 | 0.103 | 0.024 |
| $R^2$ Overall | 0.256 | 0.111 | 0.030 |
| $R^2$ Between | 0.257 | 0.140 | 0.047 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

$^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

The regression includes controls for: pop, $(\text{pop})^2$, average educ, employed/total population

Full results, including coefficients from control variables, are available in Appendix C Table (9.14)

Having compared the treatment and the control group, I do not find any significant difference. These results may be driven by the null influence of the pre-quota years. Thus, to control before and after the quota, a DiD strategy is used, as shown in regression (6.4). Results are shown in Table (7.4), and they confirm the intuition that the results of Table (7.3) were driven by the null influence of pre-quota years.

Table 7.4: Regression results $H_0(2)$ Eq. (6.4)

| sahre female | (1)<br>Council | (2)<br>Council | (3)<br>Ex. Committee | (4)<br>Ex. Committee | (5)<br>Mayor | (6)<br>Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.042*** | 0.000 | -0.027 | 0.000 | -0.055* |
| | (.) | (0.010) | (.) | (0.016) | (.) | (0.022) |
| postquota | 0.022* | 0.032** | 0.032 | 0.041* | -0.030 | -0.024 |
| | (0.011) | (0.010) | (0.019) | (0.019) | (0.029) | (0.027) |

Table 7.4: Regression results $H_0(2)$ Eq. (6.4)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| (share treated LMA)(postquota) | 0.059*** | 0.056*** | 0.047* | 0.044* | 0.060* | 0.063* |
| | (0.011) | (0.011) | (0.020) | (0.020) | (0.031) | (0.029) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 40649 | 40649 | 40649 | 40649 | 40649 | 40649 |
| $R^2$ | 0.311 | | 0.117 | | 0.024 | |
| $R^2$ Adj. | 0.311 | | 0.117 | | 0.024 | |
| $R^2$ Within | 0.311 | 0.311 | 0.117 | 0.117 | 0.024 | 0.025 |
| $R^2$ Overall | 0.212 | 0.312 | 0.060 | 0.130 | 0.018 | 0.033 |
| $R^2$ Between | 0.036 | 0.315 | 0.005 | 0.161 | 0.011 | 0.052 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression includes controls for: pop, $(pop)^2$, average educ, employed/total population

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Full results, including coefficients from control variables, are available in Appendix C Table (9.15)

In this case, the interaction terms are significant for almost every model's specification, suggesting the presence of geographical spillover effects. Focusing on the share of female in the Municipal Council, Table (7.4) column (2) shows that the proportion of women in Municipal Council after the reform increased by 5,6 percentage points more in municipalities with a high share of treated municipalities in the neighbourhoods. This effect is slightly smaller than the estimated in column (1) using municipalities fixed effects, but both are highly significant. In column (3) and (4), I analysed the share of women in the Executive Committee. In both specifications, the coefficient of the interaction term is statistically significant and positive, and it has an impact of 4,4 percentage points, controlling for provincial fixed effects (column (4)), and 4,7 percentage points controlling for municipalities fixed effects (column (3)). On the other hand, the coefficient of the interaction effect for the probability of observing a female mayor is statistically significant, controlling for provincial fixed effects, where the probability of observing a female mayor after the reform increased by 6,3 percentage points in municipalities in a LMA with a high share of treated municipalities (column (6)). The effect is robust to the inclusion of municipality fixed effects (column (5)), but it is lower (6 percentage points).

In conclusion, to check that spillover effects are heterogeneous over time, regression (6.5) is run. Since only 106 out of 40649 observations refers to the fifth or the sixth election after the reform (respectively 104 and 2 observations), I consider only until the fourth election after the law to avoid that results are driven by the small sample size. Table (7.5) shows the results.

Table 7.5: Regression results $H_0(3)$ Eq. (6.5)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.048*** | 0.000 | -0.035* | 0.000 | -0.061** |
| | (.) | (0.010) | (.) | (0.016) | (.) | (0.022) |

Table 7.5: Regression results $H_0(3)$ Eq. (6.5)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| (share treated LMA)(election 1) | 0.043** | 0.039** | 0.027 | 0.024 | 0.053 | 0.063* |
| | (0.014) | (0.014) | (0.030) | (0.030) | (0.032) | (0.031) |
| (share treated LMA)(election 2) | 0.075*** | 0.070*** | 0.051 | 0.048 | 0.073 | 0.088* |
| | (0.015) | (0.014) | (0.028) | (0.028) | (0.043) | (0.042) |
| (share treated LMA)(election 3) | 0.093*** | 0.086*** | 0.101*** | 0.089** | 0.047 | 0.047 |
| | (0.015) | (0.015) | (0.027) | (0.027) | (0.049) | (0.047) |
| (share treated LMA)(election 4) | 0.091*** | 0.078*** | 0.082* | 0.065 | 0.105* | 0.109* |
| | (0.021) | (0.020) | (0.039) | (0.038) | (0.053) | (0.052) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 40543 | 40543 | 40543 | 40543 | 40543 | 40543 |
| $R^2$ | 0.320 | | 0.122 | | 0.025 | |
| $R^2$ Adj. | 0.320 | | 0.122 | | 0.025 | |
| $R^2$ Within | 0.320 | 0.321 | 0.122 | 0.122 | 0.025 | 0.025 |
| $R^2$ Overall | 0.188 | 0.320 | 0.061 | 0.133 | 0.018 | 0.034 |
| $R^2$ Between | 0.001 | 0.319 | 0.001 | 0.161 | 0.008 | 0.055 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression includes controls for: pop, $(pop)^2$, average educ, employed/total population

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Full results, including coefficients from control variables, are available in Appendix C Table (9.16)

The results show that the spillover effects are particularly strong for the Municipal Council and the Executive Committee after three elections after the reform, and then they seems to decay. On the other hand, the spillovers regarding the mayor are particularly strong four elections after the law, but the effects are fluctuating.

In summary, this analysis shows that there were geographical spillover effects for the three dependent variables. Thus, it is possible to reject $H_0(2)$ because the percentage of elected female in a given geographical unit increases with the number of women elected in other geographical units. Controlling for the timing, the spillover effects seem to increase until a point and then decreases for the share of women in the Executive Committee and the Municipal Council, while the spillover effects for the mayor are fluctuating over time. The findings reject $H_0(3)$, which states that the positive relationship between geographical units does not change over time.

## 7.3 Hypothesis 4: cross-sector spillovers

Table (7.6) presents the results from the regression (6.6), testing the differences in the change in female unemployment and the share of housewives between the treatment and the control group. The results are shown controlling for municipalities fixed effects or provincial fixed effects.

Table 7.6: Regression results $H_0(4)$ Eq. (6.6)

| | (1) share female unempl. | (2) share female unempl. | (3) share housewives | (4) share housewives |
|---|---|---|---|---|
| treated | 0.000 | -0.001 | 0.000 | 0.002 |
| | (.) | (0.003) | (.) | (0.003) |
| postquota | -0.013*** | -0.015*** | 0.033*** | 0.009** |
| | (0.004) | (0.003) | (0.003) | (0.003) |
| (treated)(postquota) | 0.003 | 0.003 | 0.001 | 0.001 |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES |
| MUNICIPALITIES FIXED EFFECTS | YES | NO | YES | NO |
| $N$ | 32372 | 32372 | 24286 | 24286 |
| $R^2$ | 0.573 | | 0.580 | |
| $R^2$ Adj | 0.573 | | 0.580 | |
| $R^2$ Within | 0.573 | 0.574 | 0.580 | 0.563 |
| $R^2$ Overall | 0.799 | 0.829 | 0.246 | 0.563 |
| $R^2$ Between | 0.890 | 0.919 | 0.131 | 0.565 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression (column (1)-(2)) includes controls for: pop. +15, (pop. +15)$^2$, share of female active pop. male unempl. rate, female average educ., average educ.

The regression (column (3)-(4)) includes controls for: pop. +15, (pop. +15)$^2$, share of female active pop. female average educ., average educ., female unempl. rate, unempl. rate, share of households, female pop. +15

In column (1),(3) time-invariant variables have been omitted because of fixed effects, e.g. treated

Full results, including coefficients from control variables, are available in Appendix C Table(9.17)

Regarding the female unemployment rate, the coefficients of the variable *treated* and the interaction term *(treated)(postquota)* are not statistically significant for both municipalities and provincial fixed effects. On the other hand, the coefficient of the variable *postquota* is highly statistically significant, and it shows a decrease in unemployment after the abolishment of the reform. Considering column (2), the proportion of female unemployment decreased by 1,5 percentage points after abolishing the reform. Looking at the full results, the female active population's share and average education of the population tend to negatively affect female unemployment; while, the male unemployment rate positively impacts the dependent variable.

Table (7.6) column (3), (4) presents the results from the regression (6.6), testing the differences in the temporal variation of the share of female housewives between the treatment and the control group. Also, in this case, the coefficient of the interaction term *(treated)(postquota)* and the variable *treated* are not significant. On the other hand, considering column (4), the share of housewives increased by 0,9 percentage points after 1995. The increase in the percentage of housewives is higher (3,3 percentage points) considering municipalities fixed effects (column (3)), but the results are statistically significant for both specifications. Furthermore, the share of housewives appears to be slightly higher in more populated municipalities and municipalities with higher average education and a higher female unemployment rate. It is also negatively related to the percentage of the active female population, to the average female education, to the unemployment rate and the female population in working age.

In conclusion, there is no evidence of spillover effects of municipalities gender quotas on the labour

market. Indeed, gender quotas do not impact the female unemployment rate and the share of housewives. Thus, it is not possible to reject $H_0(4)$. The potential reasons for these outcomes may be that there is no impact of genders preferences in policy outcomes. Indeed, as the Median voter theorem states, only citizens' preferences matter for policy-making. Another possibility is that female politicians have preferences closer to the male ones than women not involved in politics. Regarding the share of housewives, it may also be that there are no gender preferences regarding participation in the labour force. Furthermore, gender quotas may not impact the breakdown of gender stereotypes in other areas of society and it can explain the null effects on the labour market.

# Robustness

## 8.1 Influence of the differences between North and South Italy

De Paola et al.(2010) pointed out that the South of Italy has a lower degree of economic and social development than the North. It may lead to a different temporal trend in the representation of women in politics and the labour market. The differences in the regions may also have effects on the efficiency of municipality administration. Thus, I undertake a robustness check to ensure that different geographical regions' trends do not drive the results. For the first hypothesis, I interact *treated* with the variable *south*, a dummy variable taking the value 1 for municipalities located in the South and zero otherwise [1].For the last three hypotheses, I interact *postquota*, *treated*, *(postquota)(treated)* with the variable *south*. Full results of the OLS estimates from the analysis of all the hypotheses, including coefficients for the control variables, are presented in Appendix C.

### 8.1.1 Hypothesis 1: difference in efficiency

Table (8.1) shows the results of the model presented in Table (7.1), including the variable *south*. The findings show that treated municipalities in the South are not statistically and significantly different from those located in the North for the speed of revenues. Thus, the differences between the regions do not drive the results. However, geographical differences affect the results for the speed of payment. Being a treated municipality in the South of Italy leads to an average lower speed of payment compared to the North. However, this finding does not affect the claims regarding the change in administration efficiency due to the gender quotas because I do not detect an impact of gender quotas on the speed of payment.

Table 8.1: Regression results $H_0(1)$ Eq. (6.1) with interaction North-South

| | (1)<br>Speed of Rev. | (2)<br>Speed of Pay. |
|---|---|---|
| treated | 0.794 | 0.766 |
| | (0.685) | (0.416) |
| (treated)(south) | 0.244 | -1.097* |
| | (0.935) | (0.550) |
| PROVINCIAL FIXED EFFECTS | YES | YES |
| YEAR FIXED EFFECTS | YES | YES |
| *N* | 83826 | 83826 |
| $R^2$ Within | 0.174 | 0.059 |
| $R^2$ Overall | 0.428 | 0.158 |
| $R^2$ Between | 0.563 | 0.245 |

[1]The following regions are included in the South of Italy: Abruzzo, Basilicata, Calabria, Campania, Molise, Apulia, Sicily, Sardinia

Table 8.1: Regression results $H_0(1)$ Eq. (6.1) with interaction North-South

| | (1) Speed of Rev. | (2) Speed of Pay. |
|---|---|---|

Robust standard errors in parentheses. Standard errors are clustered at municipality level
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
The regression includes controls for: share empl. in the Council, average education in the Council, early termination of the mandate, share members of the Council with previous political experience, pop, $(pop)^2$, employed/population and average education
Full results, including coefficients from control variables, are available in Appendix C Table (9.18)

### 8.1.2 Hypothesis 2 & 3: geographical spillovers

For the spillover effects in other geographical unit, results from regression (6.2) including the variable *south* are presented in Table (8.2). They show that female representation on Municipal Councils has increased less in the South and the result is robust to the inclusion of provincial fixed effects. Overall, it is not possible to state that southern treated municipalities drive the results because the coefficient of the interaction *(treated)(postquota)(south)* is not statistically significant in almost every case. Column (2) and (4) are the only exceptions. However, since the results are not robust for municipalities' fixed effects, there is not enough evidence to state that being in the South of Italy drives the results.

Table 8.2: Regression results Eq. (6.2) with interaction North-South

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| treated | 0.000 | -0.014*** | 0.000 | -0.015* | 0.000 | -0.030* |
| | (.) | (0.004) | (.) | (0.007) | (.) | (0.013) |
| postquota | 0.079*** | 0.072*** | 0.075*** | 0.065*** | 0.002 | 0.018 |
| | (0.007) | (0.007) | (0.013) | (0.012) | (0.022) | (0.020) |
| (treated)(postquota) | 0.014* | 0.024*** | 0.020 | 0.029* | 0.027 | 0.024 |
| | (0.007) | (0.007) | (0.013) | (0.012) | (0.022) | (0.020) |
| (treated)(south) | 0.044*** | 0.022*** | 0.053*** | 0.025*** | 0.002 | 0.014 |
| | (0.006) | (0.004) | (0.012) | (0.007) | (0.017) | (0.012) |
| (postquota)(south) | -0.017 | 0.001 | 0.001 | 0.019 | -0.009 | -0.020 |
| | (0.009) | (0.008) | (0.016) | (0.015) | (0.025) | (0.022) |
| (treated)(postquota)(south) | -0.005 | -0.023** | -0.013 | -0.033* | -0.020 | -0.008 |
| | (0.009) | (0.008) | (0.017) | (0.015) | (0.026) | (0.023) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 32548 | 32548 | 32548 | 32548 | 32548 | 32548 |
| $R^2$ | 0.319 | | 0.111 | | 0.024 | |
| $R^2$ Adj. | 0.319 | | 0.110 | | 0.023 | |
| $R^2$ Within | 0.319 | 0.319 | 0.111 | 0.111 | 0.024 | 0.024 |
| $R^2$ Overall | 0.122 | 0.308 | 0.045 | 0.118 | 0.009 | 0.032 |
| $R^2$ Between | 0.037 | 0.292 | 0.003 | 0.130 | 0.003 | 0.048 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8.2: Regression results Eq. (6.2) with interaction North-South

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|

The regression includes controls for: pop, $(pop)^2$, average educ, employed/total population
In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated
Full results, including coefficients from control variables, are available in Appendix C Table (9.19)

Table (8.3) shows the results for regression (6.4) including the variable *south*. Also, in this case, the interaction term does not appear statistically significant, except for the share of female in the Municipal Council controlling for municipalities fixed effects. However, since this result is not robust to both specification, it is not possible to state that the different trends between the South and the Center-North of Italy drive the results.

Table 8.3: Regression results $H_0(2)$ Eq. (6.4) with interaction North-South

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.034** | 0.000 | -0.039* | 0.000 | -0.011 |
| | (.) | (0.011) | (.) | (0.018) | (.) | (0.024) |
| postquota | 0.135*** | 0.088*** | 0.137** | 0.103* | -0.013 | 0.008 |
| | (0.027) | (0.024) | (0.048) | (0.044) | (0.076) | (0.065) |
| (share treated LMA)(postquota) | -0.053 | 0.004 | -0.059 | -0.017 | 0.049 | 0.039 |
| | (0.028) | (0.025) | (0.050) | (0.046) | (0.079) | (0.067) |
| (share treated LMA)(south) | 0.052*** | 0.041*** | 0.059*** | 0.051*** | 0.005 | 0.005 |
| | (0.006) | (0.005) | (0.012) | (0.010) | (0.016) | (0.014) |
| (postquota)(south) | -0.098*** | -0.025 | -0.109* | -0.050 | 0.039 | 0.020 |
| | (0.029) | (0.026) | (0.052) | (0.047) | (0.082) | (0.067) |
| (share treated LMA)(postquota) (south) | 0.074* | -0.001 | 0.098 | 0.036 | -0.077 | -0.058 |
| | (0.030) | (0.027) | (0.055) | (0.049) | (0.085) | (0.070) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 40649 | 40649 | 40649 | 40649 | 40649 | 40649 |
| $R^2$ | 0.315 | | 0.118 | | 0.025 | |
| $R^2$ Adj. | 0.315 | | 0.117 | | 0.025 | |
| $R^2$ Within | 0.315 | 0.315 | 0.118 | 0.118 | 0.025 | 0.026 |
| $R^2$ Overall | 0.149 | 0.314 | 0.045 | 0.130 | 0.020 | 0.033 |
| $R^2$ Between | 0.027 | 0.315 | 0.001 | 0.161 | 0.014 | 0.052 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
The regression includes controls for: pop, $(pop)^2$, average educ, employed/total population
In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated
Full results, including coefficients from control variables, are available in Appendix C Table (9.20)

In conclusion, the robustness check proves that different trends in female representation among geographical areas do not drive the results for female representation in politics.

### 8.1.3 Hypothesis 4: cross-sector spillovers

Table (8.4) shows the results of the specifications presented in Table (7.6) including the variable *south*.

Table 8.4: Regression results $H_0(4)$ Eq. (6.6) with interaction North-South

| | (1) share female unempl. | (2) share female unempl. | (3) share housewives | (4) share housewives |
|---|---|---|---|---|
| treated | 0.000 | 0.000 | 0.000 | -0.008 |
| | (.) | (0.003) | (.) | (0.005) |
| postquota | -0.013*** | -0.015*** | 0.029*** | 0.006 |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| (treated)(postquota) | 0.003 | 0.003 | 0.006 | 0.006 |
| | (0.003) | (0.003) | (0.004) | (0.004) |
| (treated)(south) | 0.000 | -0.003 | 0.000 | 0.018** |
| | (.) | (0.005) | (.) | (0.006) |
| (postquota)(south) | 0.001 | -0.000 | 0.002 | 0.001 |
| | (0.006) | (0.006) | (0.005) | (0.005) |
| (treated)(postquota)(south) | 0.001 | 0.000 | -0.010 | -0.011* |
| | (0.006) | (0.006) | (0.005) | (0.005) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO |
| $N$ | 32372 | 32372 | 24286 | 24286 |
| $R^2$ | 0.573 | | 0.581 | |
| $R^2$ Adj | 0.573 | | 0.581 | |
| $R^2$ Within | 0.573 | 0.574 | 0.581 | 0.565 |
| $R^2$ Overall | 0.800 | 0.829 | 0.249 | 0.564 |
| $R^2$ Between | 0.890 | 0.919 | 0.132 | 0.565 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression (column (1)-(2)) includes controls for: pop. +15, (pop. +15)$^2$, share of female active pop. male unempl. rate, female average educ., average educ.

The regression (column (3)-(4)) includes controls for: pop. +15, (pop. +15)$^2$, share of female active pop. female average educ., average educ., female unempl. rate, unempl. rate, share of households, female pop. +15

In column (1),(3) time-invariant variables have been omitted because of fixed effects, e.g. treated

Full results, including coefficients from control variables, are available in Appendix C Table (9.21)

After the reform, the female unemployment rate has decreased almost the same in the South and the Center-North. Southern gender quota municipalities are not statistically and significantly different from those located in the North. The coefficient on the interaction *(treated)(postquota)(south)* does not appear statistically significant, implying that the reform has led to effects on unemployment both in Northern and Southern municipalities. These results hold also for provincial fixed effects (columns (1)). On the other hand, for the share of housewives, the interaction *(treated)(postquota)(South)* is statistically significant in column (4). However, the results are not robust to provincial fixed effects (column (3)). Because of this, it is not possible to state that social and economic differences between

the South and the Center-North drive the results.

## 8.2 Hypothesis 2 & 3: Influence of being in the treatment group for geographical spillovers

The treatment group may drive the finding regarding the geographical spillovers, being the treated municipalities (7.703) much more than the control municipalities (397) and the control municipalities well-distributed across the country. In other words, since the outcome variables are measured at the municipality level and there are a lot of neighbourhood treated municipalities, it may be that the effects are driven by being in the treatment group and not by the geographical spillover effects.

Thus, regressions (6.3) and (6.4) are run, excluding the treated municipalities from the analysis. In other words, the following analysis will consider only municipalities that do not vote under the reform and the sample size is now made by 2.253 observations. In this way, it is possible to check whether the results are driven by the effect of being in the treatment group. Considering regression (6.3), Table (8.5) shows that the treated municipalities drive the results in most of the cases. The coefficient of the interaction term *(share treated LMA)(postquota)* is not statistically significant for the share of women in the Executive Committee and the probability of encounter a female mayor. However, it is statistically significant for the share of female politicians in the Municipal Council. After the reform, the proportion of female politicians in the Municipal Council increased by 4,7 percentage points more in control municipalities with a high share of treated municipalities in the neighbourhood (column (2)). The result is robust to municipalities fixed effects (column (1)), where it is slightly bigger (4,9 percentage points).

Table 8.5: Regression results $H_0(2)$ Eq. (6.4) excluding treated municipalities

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.064* | 0.000 | -0.040 | 0.000 | 0.006 |
| | (.) | (0.027) | (.) | (0.046) | (.) | (0.054) |
| postquota | 0.018 | 0.022 | 0.104** | 0.098* | 0.034 | 0.010 |
| | (0.021) | (0.020) | (0.039) | (0.040) | (0.061) | (0.056) |
| (share treated LMA)(postquota) | 0.049* | 0.047* | -0.048 | -0.038 | -0.041 | -0.005 |
| | (0.024) | (0.024) | (0.045) | (0.046) | (0.074) | (0.071) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 2253 | 2253 | 2253 | 2253 | 2253 | 2253 |
| $R^2$ | 0.240 | | 0.088 | | 0.010 | |
| $R^2$ Adj | 0.238 | | 0.086 | | 0.008 | |
| $R^2$ Within | 0.240 | 0.244 | 0.088 | 0.094 | 0.010 | 0.017 |
| $R^2$ Overall | 0.175 | 0.322 | 0.059 | 0.152 | 0.006 | 0.083 |
| $R^2$ Between | 0.107 | 0.457 | 0.016 | 0.303 | 0.005 | 0.236 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression includes controls for: pop, $(pop)^2$, average educ, employed/total population

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Full results, including coefficients from control variables, are available in Appendix C Table (9.22)

Table (8.6) shows the results for regression (6.4), including only control municipalities. From the table, it emerges that the geographical spillover effects in the Council female representation are particularly strong in the fourth election after the abolishment of the reform.

Table 8.6: Regression results $H_0(2)$ Eq. (6.5) excluding treated municipalities

| sahre female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.062* | 0.000 | -0.042 | 0.000 | 0.012 |
| | (.) | (0.026) | (.) | (0.046) | (.) | (0.054) |
| (share treated LMA)(election 1) | 0.023 | 0.026 | -0.098 | -0.080 | 0.045 | 0.086 |
| | (0.033) | (0.033) | (0.066) | (0.066) | (0.066) | (0.062) |
| (share treated LMA)(election 2) | 0.016 | 0.019 | -0.105 | -0.086 | -0.074 | -0.033 |
| | (0.034) | (0.034) | (0.070) | (0.072) | (0.108) | (0.107) |
| (share treated LMA)(election 3) | 0.034 | 0.031 | -0.002 | 0.010 | -0.202 | -0.170 |
| | (0.033) | (0.033) | (0.074) | (0.076) | (0.130) | (0.129) |
| (share treated LMA)(election 4) | 0.135** | 0.126** | 0.010 | 0.000 | 0.106 | 0.128 |
| | (0.041) | (0.042) | (0.089) | (0.091) | (0.104) | (0.107) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 2236 | 2236 | 2236 | 2236 | 2236 | 2236 |
| $R^2$ | 0.260 | | 0.094 | | 0.015 | |
| $R^2$ Adj. | 0.256 | | 0.089 | | 0.009 | |
| $R^2$ Within | 0.260 | 0.264 | 0.094 | 0.100 | 0.015 | 0.022 |
| $R^2$ Overall | 0.164 | 0.336 | 0.058 | 0.157 | 0.008 | 0.087 |
| $R^2$ Between | 0.047 | 0.461 | 0.002 | 0.298 | 0.003 | 0.239 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression includes controls for: pop, $(pop)^2$, average educ, employed/total population

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Full results, including coefficients from control variables, are available in Appendix C Table (9.23)

Thus, it is possible to conclude that most of the spillover effects detected in the analysis were driven by the belonging of the municipalities to the treatment group and not by geographical spillovers. The representation in the Municipal Council seems the only subject to geographical spillover effects, and this effect appears to be stronger four elections after the introduction of the gender quotas. However, considering that the sample size is equal to 2.253 observations, the small sample size may affect the results, particularly in Table (8.6). Furthermore, adding multiple interaction effects, as in regression (6.5), lowers the power of the analysis because the power of detecting interaction effects is lower than the power to detect main effects.

## 8.3 Hypothesis 2: Investigating geographical spillover effects using a spatial model

To overcome the small sample size limitation, it is possible to investigate the spillover effects using a spatial model. Spatial models identify spatial interaction and dependence, distinguishing between own-unit direct effects and across-unit direct effects. Because of that, there is no need to drop the treated municipalities.

The data set considered by this thesis is an unbalanced panel. Indeed, not all the municipalities voted the same amount of times because of the early termination of mandates. However, spatial analysis requires the panel data set to be strongly balanced. To overcome this problem, the data set is treated as pooled cross-sectional, as done by Baltagi et al. (2007) and Egger et al. (2005). Because of this choice, only the post-quota years are considered. Thus, the data set is a panel from 1996 to 2012. It considers only the first four elections after abolishing the law because only 106 out of 27.104 observations refers to the fifth or the sixth election after the reform (respectively 104 and 2 observations) and generating a spatial matrix with this high amount of missing values would not be convenient.

The analysis starts with regression (8.1) to perform the Moran's I test for residuals in order to understand whether there is spatial dependence

$$y_i = \beta_0 + \beta_1 treated_i + \beta_2 X_i + fixed\_effects + \epsilon_i \tag{8.1}$$

where $y_i$ represents the dependent variables, i.e. the share of women in the Municipal Council over the total number of seats, the share of women in the Executive Committee over the total number of seats and a dummy equal to one if the mayor is female and equal to zero if the mayor is male. The controls $X_{i,t}$ are the same as the main model and in the model there are time and provincial fixed effects. To perform the Moran's I test and to conduct the analysis, I consider an inverse distance spatial matrix W. Indeed, it is not common for two municipalities to share a border, but they are not close to one another, for instance, when the border runs through a natural barrier. The null hypothesis of the test states that there are no spatial effects in the residuals[2]. It is possible to strongly reject the null hypothesis (p-value=0.0000) for the share of women in the Municipal Council that for the percentage of women in the Executive Committee, but it is not possible for the probability of encountering a female mayor (p-value=0.2366). Since the null hypothesis is strongly rejected for the share of women in the Municipal Council and the Executive Committee, it is possible to run models with a spatial component, where the spatial weighting matrix W represents the spatial dependence. Since the data set is treated as cross-sectional, following the strategy of Baltagi et al. (2007), the spatial weighting matrix W is a block diagonal matrix. W has row normalised $W_t$ matrices as diagonal elements. In this case, t, the time component, represents the election after abolishing the quota system.

To test against $H_0(2)$ a Spatial Durbin linear (SLX, i.e. spatially lagged X model) is used. This model includes a spatial lag of the explanatory variable *treated*:

$$y_{i,t} = \beta_0 + \beta_1 treated_i + \beta_2 Wtreated_i + \beta_3 X_i + fixedeffects_i + \epsilon_i \tag{8.2}$$

where $Wtreated_i$ represents the spatially lagged explanatory variables and it shows the effect of having treated municipalities in the neighbourhoods on the dependent variable. The regression is estimated using the generalised method-of-moments estimator.

[2]Results from Eq (8.1) are shown in Table (9.26) in Appendix D, together with the outcomes of the test.

Table 8.7: Regression results Eq. (8.2)

| share female | (1) Council | (2) Ex. Committee |
|---|---|---|
| main | | |
| treated | 0.009** | 0.012* |
| | (0.003) | (0.005) |
| W | | |
| treated | 0.172 | 0.466* |
| | (0.108) | (0.194) |
| PROVINCIAL FIXED EFFECTS | YES | YES |
| TIME FIXED EFFECTS | YES | YES |
| $N$ | 26998 | 26998 |
| Pseudo $R^2$ | 0.1967 | 0.0671 |

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression includes controls for: pop, $(pop)^2$, average educ, employed/total population

Full results, including coefficients from control variables are available in Appendix C Table (9.24)

Results of regression (8.2) are shown in Table (8.7), and they show a significant impact of across-unit spillover effects for the female in the Executive committee, but not for the Municipal Council. Thus, the analysis shows that the share of women in the Executive Committee is affected directly and indirectly by the variable *treated*. The own-municipality direct effect of being in the treatment group is to increase female representation by 1,2% percentage points. The across-municipalities spillover effect is to increase female representation by 46,6% on average.

However, treating the results as pooled cross-sectional leads the coefficient to be several biased, as visible by the extremely high geographical spillover effect. Because of that, this analysis is conducted mainly to check whether the impacts share the same directions using different models. The spatial model shows geographical spillover effects of female representation for the Executive Committee members, even if it is plausible that the effect is not as large as estimated. Since the results are biased, there may also be geographical spillovers in the Municipal Council since the results are statistically significant at 10%. Thus, the results of this analysis seem to be in favour of geographical spillover effects. However, considering the high bias, the results are only suggestive, and they should be interpreted with caution.

## 8.4 Hypothesis 4: Check whether spillover effects are heterogeneous over time

This section shows whether the timing influences the results of regression (6.6). It is possible that there were effects that disappear quickly, mainly because the available data have a time interval of ten years. Furthermore, the observations from 2011 are affected by the economic crisis subsequent to the 2007-2008 financial crisis. The analysis is implemented considering multiple timing interaction effects and Eq. (6.6) becomes:

$$y_{i,t} = \beta_0 + \beta_1 treated_i + \beta_2(year\_postquota) + \beta_3(treated_i)(year\_postquota) + \beta_6 X_{i,t} + fixed\_effects_{i,t} + \epsilon_{i,t} \tag{8.3}$$

where the generic variable *postquota* is substituted with *year_postquota*, that represents the exact census year after the abolition of the gender quotas. Thus, *year_postquota* can be equal to 2001 or 2011 whether the observations are respectively from the Census 2001 or Census 2011. In this way, it would be possible to assess how effects change over time and whether they accumulate or deteriorate as time progresses.

Table (8.8) column (1), (2) shows that both the 2001's dummy and the 2011's dummy are statistically significant. While the female unemployment rate decreased by 0,9 percentage points in 2001 compared to the other periods, it fell by 3,6 percentage points in 2011 (column (1)). The results are robust to provincial fixed effects. On the other hand, the difference between treated municipalities and control municipalities is not statistically significant in 2001. Still, the difference is statistically significant in 2011: in 2001, the female unemployment rate increased by 0,8 percentage points more in treated municipalities. The results are robust under municipalities and provincial fixed effects. Since there is a large time period gap, it is not possible to understand what leads to an increase in the female unemployment rate. The financial crisis is the most significant event during the time period considered and it could have an impact on the results. However, there could be other factors that may have occurred over time.

On the other hand, in treated municipalities, housewives' share increased by 0,8 percentage points more in 2001, and it decreased by 0,7 percentage points more in 2011. The results hold with provincial and municipalities fixed effects. Also, in this case, the financial crisis could have impacted the observations of 2011, but because of the large time period gap, other things could have occurred. However, while the effect for treated municipalities in 2011 shares the same direction as 2011's dummy, the effect for treated municipalities in 2001 has a different direction compared to 2001's dummy. Thus, gender quotas inverted the trends.

Table 8.8: Regression results $H_0(5)$ Eq. (8.3)

| | (1)<br>share female unempl. | (2)<br>share female unempl. | (3)<br>share housewives | (4)<br>share housewives |
|---|---|---|---|---|
| (treated)(2001) | -0.003 | -0.003 | 0.008** | 0.008** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| (treated)(2011) | 0.008* | 0.008* | -0.007* | -0.007* |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES |
| MUNICIPALITIES FIXED EFFECTS | YES | NO | YES | NO |
| $N$ | 32372 | 32372 | 24286 | 24286 |
| $R^2$ | 0.578 | | 0.623 | |
| $R^2$ Adj | 0.577 | | 0.623 | |
| $R^2$ Within | 0.578 | 0.579 | 0.623 | 0.619 |
| $R^2$ Overall | 0.795 | 0.830 | 0.372 | 0.627 |
| $R^2$ Between | 0.889 | 0.919 | 0.260 | 0.631 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The regression (column (1)-(2)) includes controls for: pop. +15, (pop. +15)$^2$, share of female active pop. male unempl. rate, female average educ., average educ.

The regression (column (3)-(4)) includes controls for: pop. +15, (pop. +15)$^2$, share of female active pop. female average educ., average educ., female unempl. rate, unempl. rate, share of households, female pop. +15

Full results, including coefficients from control variables, are available in Appendix C Table (9.25)

To directly compare between 2001 and 2011 effects, a linear combination test of parameters is conducted to test for the difference between the coefficient of *(treated)(2001)* and *(treated)(2011)*. It tests for the difference in the point estimate across coefficients. Results are shown in Table (8.9). In the table are reported the point estimates, the standard errors, the z score and the p-values for the differences between the coefficients *(treated)(2001)* and *(treated)(2011)* for each model of Table (8.8). The z score and the p-values are computed testing if the estimated parameters considered are statistically different, under the null hypothesis that they are not.

Table 8.9: Difference between the coefficients of *(treated)(2011)* and *(treated)(2001)*

| Model Table (8.8) | Coeff. point estimate | Std. Err. | z | p-value |
|---|---|---|---|---|
| (1) | 0,011 | 0,003 | 3,82 | 0,000 |
| (2) | 0,012 | 0,003 | 3,94 | 0,000 |
| (3) | -0,015 | 0,003 | -5,20 | 0,000 |
| (4) | -0,015 | 0,003 | -5,35 | 0,000 |

In the table are reported point estimates, standard errors, z statistics and p-values for the differences between the coefficients of *(treated)(2011)* and *(treated)(2001)* The z statistics and the p-values are computed testing if the estimated parameters considered are statistically different, under the null hypothesis that they are not.

The differences for the two interaction coefficients are all statistically different from zero. Regarding the female unemployment rate, the difference between the two years in the treatment group shows an increase in the female unemployment rate in 2011 of 1,1 percentage points compared to 2001. The result is robust considering provincial fixed effects, but it is slightly higher (1,2 percentage points). On the other hand, the share of female housewives in treated municipalities decreased by 1,5 percentage points in 2011 compared to 2001. Again, the results are robust, including provincial fixed effects. Thus, these findings indicate an increase in female unemployment rate and a decrease in the share of housewives in the treatment group between 2011 and 2001.

Interpreting the results, it is critical to consider that the variables used to test against $H_0(4)$ are recorded every ten years. Because of that, it is challenging to investigate the short-run effects of gender quotas in the labour market and understand the path of the phenomenon.

# Discussion & Conclusion

This paper seeks to understand the potential spillover effects that gender quotas may generate. I first asked whether gender quotas produce an increase in the efficiency of municipalities governance because potential spillover may happen only if the citizens perceive an increase in the performance of the politicians. This analysis used two performance measures: the speed of revenues collected and the speed of payment. Testing for them, there were no significant differences between the treatment and the control group regarding the speed of payment, but there were in terms of the speed of revenues collected. Thus, gender quotas lead the local government to effectively fight tax evasion and implement centrally financed public works to receive new capital transfers. Since efficient financial management is pivotal to enforce local policies, it is plausible that citizens perceive an increase in performance derived from the increase in the speed of revenues collected. Furthermore, as proved by De Paola et al. (2010) and confirmed by this thesis, it is still possible to claim the increase in politicians' perceived ability since the effects of the reform last until 2012. However, even if this result is consistent with the previous findings, it is pivotal to underline that, starting from 1998, the analysis does not measure the efficiency of the municipal government elected in 1993 and 1994. Furthermore, since it is impossible to compare the before and after gender quotas, it is not possible to make claims about how the treatment improved over time relative to the control.

I then asked whether gender quotas may produce geographical spillover effects to municipalities not affected by the reform. The results show that the positive impact of gender quotas are expected in the treated units and the neighbourhoods. Furthermore, this paper found that the spillover effects are expected to increase and then decrease after the third election for the Municipal Council and the Executive Committee. On the other hand, there is no evidence of a decrease over time for the probability of encountering a female mayor. However, a robustness check shows that the results are affected by the higher presence of treated municipalities in the sample. Excluding them from the analysis, the evidence seems to reject the existence of spillover effects, except for the Municipal Council. It is plausible that the Municipal Council is the unique body to be affected by the geographical spillover effects of the gender quotas. Together with the mayor, it is the only elected body in a municipality. In addition, the gender quotas law regarded the Municipal Council directly because there are no gender restrictions on the candidates for the mayor's office, and the Municipal Executive is appointed directly by the mayor. Since the effects are mainly detected four elections after the reform, it may show a progressive decrease in stereotypes in the neighbourhoods, driven by a prolonged observation of female politicians and their performance. These results are in contrast with Gilardi and Dablac (2019), which claim a decreasing effect. However, this result may be driven by the small sample size. Indeed, having excluded the treatment from the regression, the sample size decreases consistently, being equal to 2.253 observations. Adding multiple interaction effects lowers the power of the analysis since the power of detecting interaction effects is lower than the power to detect main effects. To overcome the small sample size problem, results are tested using a spatial lagged X model. However, it is essential to remind that the estimation strategy used for this model produces biased results. Thus, also, in this case, results should be interpreted with caution. The analysis shows that the share of women in the Executive Committee is positively affected by geographical spillover. Still, this result does not hold for the other dependent variables, even if the geographical spillovers are statistically significant at 10% level for the Municipal Council. However, the spatial analysis offers a suggestive but not conclusive insight into the potential geographical

spillover effects in the municipalities bodies, considering the bias of the results. In conclusion, it is not entirely possible to provide a definitive claim regarding the spillover effects of gender quotas because both the analysis are biased and they present opposite results.

I then asked whether gender quotas may produce spillover effects in the labour market, decreasing female unemployment and decreasing the share of housewives over the total working-age female population. The results do not show a difference between the treatment and the control group regarding the female unemployment rate and housewives' share. The absence of gender effects on the female unemployment rate may be due to the missing impact of women's preferences in policy outcomes. It supports the Median voter theorem, according to which politicians consider only citizens' preferences when they design policies. However, even if politicians vote according to their preferences, it could also be that gender is not a determinant of their voting behaviour. A possible interpretation of such behaviour can be that female politicians have preferences closer to the males' ones, comparing to the women not involved in politics. Another possible explanation that does not exclude the previous is that gender quotas do not impact the breakdown of gender stereotypes in other areas of society. These results are consistent with Bagues and Campa (2017) that do not find any significant effect of gender quotas on female unemployment. A robustness check is implemented to understand the timing of the effects. While there were no statistically significant differences in 2001 in the female unemployment rate between the two groups, the findings show that being in the treatment group in 2001 leads to a higher share of housewives, while the effect of not being in the treatment group in 2001 has an opposite effect. A possible explanation for this finding is the impact of gender quotas on policy-making, disincentivise women to enter the labour force. This claim is consistent with the findings of Braga and Scevini (2017). The authors showed that gender quotas increase the efficacy of policies targeted to women and households, increasing the fertility rate. Furthermore, the results show that being in the treatment group in 2011 leads to a higher female unemployment rate and a decrease in the share of housewives. This result could be imputable to the 2007-2008 financial crisis that leads Italy into an economic crisis with clear consequences on the labour market. The interaction could be driven by a different municipal government reaction connected to a higher presence of female politicians. Another possibility could be that the crisis affected more a part of the treated municipalities for some interference factors, and it impacted the results. However, since there is a significant time period gap between an observation and another, it is not possible to understand what leads to an increase in the dependent variables because there could be many other factors imputable to this. In interpreting the results, it is worth noticing that most of the variables used to test against this hypothesis are recorded every ten years. Because of that, it was impossible to detect spillover effects in the short run or clearly understand the pattern of the phenomenon. Furthermore, the observations from 2011 may be biased by the financial crisis and its effects on the Italian labour market.

Overall, the results suggest that gender quotas did not produce all the results expected. Excluding the inconsistent results, a possible explanation for the prevalence of the null findings may be the low representation of women in politics. Indeed, the effects I am investigating may require a higher female representation. It means that women's representation must achieve a certain degree to matter for the decision process or ease the existing barriers in neighbourhoods and between different areas (Rigon and Tanzi, 2011). Future research should address whether, in countries where gender quotas produce a higher representation, there is evidence of spillover effects in politics and other society's areas.

It is also worth noticing that this thesis does not consider the potential impact of the general equilibrium effects or other interference effects. When allowing for behavioural responses among the control group, the outcome may change because of the group's interactions induced by the policy considered. Considering the consistent amount of treated municipalities, general equilibrium interactions may impact the partial equilibrium conclusions (Heckman et al., 1998). Several papers provide examples where sensible conclusions made in partial equilibrium are offset by general equilibrium effects (Heckman et al., 1998; Yanagizawa-Drott and Svensson, 2012). Future research would benefit

from taking these effects into account.

Because of the inconsistent results and the limitations of this thesis, it is hard to make conclusions and provide insightful policy implications. Nonetheless, it is worth noticing that a better understanding of the diffusion mechanism of affirmative action is pivotal to design appropriate policy interventions, and it is a task left for future research.

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# Appendix A

Table 9.1: Variables used

| **Variable** | **Description** | **Type** |
|---|---|---|
| Speed of Rev. | Speed of revenues collected [1] | Continuous |
| Speed of Pay. | Speed of payment [2] | Continuous |
| $\text{treated}_i$ | 1 if municipality i is in the treatment group, 0 otherwise | Dummy |
| $\text{postquota}_t$ | 1 if t $\in$ [1996,2012], 0 $\in$ [1985,1992] for $H_0(2)$-$H_0(3)$ and 1 if t $\in$ [2001,2011], 0 $\in$ [1981,1991] for $H_0(4)$ | Dummy |
| population | Population of the municipality, used as control | Discrete |
| employed/total population | Employed population over the total population of a municipality, used as control | Continuous |
| average education | Average education of the population of a municipality, used as control | Continuous |
| share empl. in the Council | Ratio of employed members over the total members of a Municipal Council, used as control | Continuous |
| average education in the $\text{Council}_i, t$ | Average education of the members of the Municipal Council, used as control | Continuous |
| early termination | 1 if the administration considered end before the natural end of the mandate, 0 otherwise, used as control | Dummy |
| share members of the Council with previous political experience | Ratio between the members of the Municipal Council with previous political experience and the total number of members of the municipal council, used as control | Continuous |
| fixed effects | Vector of provincial or municipalities dummies | Dummies |
| year fixed effects | Vector of year dummies | Dummies |
| female mayor | 1 if the mayor is female,0 otherwise | Dummy |
| share female Ex. Committee | Ratio between female members of the Ex. Committee and total members of the Executive Committee | Continuous |
| share female Council | Ratio between female members of the Municipal Council and total members of the Council | Continuous |
| share treated LMA | Ratio between the number of treated municipalities in a Labour Market Area (LMA) and the total number of municipalities in a LMA [3] | Continuous |
| election postquota | Dummies for the exact number of election after 1995 | Dummies |
| share female unempl. | Female unemployment rate, i.e. the number of women unemployed over the number of women in the labour force | Continuous |

[1] The ratio between the collected tax and transfer revenues and the total amount of assessed revenues that a municipality should collect.

[2] The ratio between the outlays paid, and the outlays committed in the municipality budget

[3] Labour market Areas are 610 sub-regional geographical areas where the bulk of the labour force lives and works, and where companies can find most of the labour force necessary to occupy jobs

| Variable | Description | Type |
|---|---|---|
| share housewives | Ratio between the number of housewives [4] and the total female population aged 15 and above [5] | Continuous |
| share male unempl. | Male unemployment rate[6], used as control | Continuous |
| share unempl. | Unemployment rate[7], used as control | Continuous |
| pop +15 | Population of the municipality aged 15 or older, used as a control | Discrete |
| pop +15 female | Female population of the municipality aged 15 or older, used as control | Discrete |
| share female active pop | Ratio between the female active population [8] and the total female population aged 15 and above, used as control | Continuous |
| female average educ. | Female average education of the population used as control | Continuous |
| share house husband | Ratio between the number of househusband [9] and the total male population aged 15 and above, used as control | Continuous |
| year_postquota | Dummies for the census postquota year | Dummies |
| south | 1 if the municipality is in the south of Italy 0 otherwise[10] | Dummy |

[4]Housewives are defined as female people aged 15 and above who are mainly dedicated to housework.

[5]The legal working age in Italy is from 15 years old

[6]Male unemployment rate is defined as the number of men unemployed over the number of men in the labour force

[7]Unemployment rate is defined as the number of unemployed over the population in the labour force

[8]Active population includes both employed and unemployed people, but not the economically inactive, such as pre-school children, school children, students, housewives/househusbands and pensioners.

[9]Househusbands are defined as male people aged 15 or older who are mainly dedicated to housework.

[10]The following regions are included in the South of Italy: Abruzzo, Basilicata, Calabria, Campania, Molise, Apulia, Sicily, Sardinia

# Appendix B

Table 9.2: Regression results random effects vs fixed effects having the share of female in the Municipal Council as dependent variable

| share female | (1) Council | (2) Council |
|---|---|---|
| treated | 0.005 | 0.000 |
| | (0.004) | (.) |
| postquota | 0.068*** | 0.065*** |
| | (0.004) | (0.004) |
| (treated)(postquota) | 0.015*** | 0.014*** |
| | (0.004) | (0.004) |
| population | -0.000*** | -0.000 |
| | (0.000) | (0.000) |
| population$^2$ | 0.000*** | 0.000* |
| | (0.000) | (0.000) |
| average education | 0.002** | 0.011*** |
| | (0.001) | (0.001) |
| employed/total population | 0.265*** | 0.064*** |
| | (0.009) | (0.015) |
| constant | -0.031*** | -0.005 |
| | (0.005) | (0.004) |
| FIXED EFFECTS | NO | YES |
| $N$ | 40649 | 40649 |
| $R^2$ | | 0.310 |
| $R^2$ Adj. | | 0.138 |
| $R^2$ Within | 0.307 | 0.310 |
| $R^2$ Overall | 0.239 | 0.210 |
| $R^2$ Between | 0.118 | 0.030 |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.3: The Hausman test related to Table (9.2)

| | (b) (2) | (B) (1) | (b-B) Difference | S.E. |
|---|---|---|---|---|
| postquota | 0.065 | 0.068 | -0.003 | 0.001 |
| (treated)(postquota) | 0.014 | 0.015 | -0.001 | 0.000 |
| $(\text{population})^2$ | -3.08e$^{-7}$ | -4.15e$^{-7}$ | 1.07e$^{-7}$ | 2.69e$^{-7}$ |
| population | 2.01e$^{-13}$ | 1.75e$^{-13}$ | 2.58$^{-14}$ | 8.43e$^{-14}$ |
| average education | 0.010 | 0.002 | 0.009 | 0.001 |
| employed/ total | 0.064 | 0.264 | -0.200 | 0.012 |

Hausman test:
$H_0$: the preferred model is random effects
$H_1$: the preferred model is fixed effects

$$\chi^2(4) : 1197.09$$
$$p - value : 0.0000$$

Table 9.4: Regression results random effects vs fixed effects having the share of female in the Executive Committee as dependent variable

| share female | (1) Ex. Committee | (2) Ex. Committee |
|---|---|---|
| treatment | 0.005 | 0.000 |
| | (0.007) | (.) |
| postquota | 0.067*** | 0.063*** |
| | (0.007) | (0.008) |
| (treated)(postquota) | 0.016* | 0.015* |
| | (0.007) | (0.008) |
| population | -0.000 | -0.000** |
| | (0.000) | (0.000) |
| population$^2$ | 0.000 | 0.000 |
| | (0.000) | (0.000) |
| average education | 0.004*** | 0.011*** |
| | (0.001) | (0.002) |
| employed/total population | 0.287*** | 0.152*** |
| | (0.015) | (0.030) |
| constant | -0.056*** | -0.033*** |
| | (0.009) | (0.008) |
| FIXED EFFECTS | NO | YES |
| $N$ | 40649 | 40649 |
| r2 | | 0.117 |
| r2_a | | -0.104 |
| r2_w | 0.116 | 0.117 |
| r2_o | 0.098 | 0.059 |
| r2_b | 0.058 | 0.005 |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.5: The Hausman test related to Table(9.4

| | (b)<br>(2) | (B)<br>(1) | (b-B)<br>Difference | S.E. |
|---|---|---|---|---|
| postquota | 0.063 | 0.067 | -0.004 | 0.002 |
| (treated)(postquota) | 0.015 | 0.016 | -0.001 | 0.001 |
| $(\text{population})^2$ | $-1.52e^{-6}$ | $-2.60e^{-9}$ | $-1.52e^{-6}$ | $5.26e^{-7}$ |
| population | $3.22e^{-13}$ | $1.28e^{-14}$ | $3.09e^{-13}$ | $1.66e^{-13}$ |
| average education | 0.011 | 0.004 | 0.007 | 0.001 |
| employed/ total | 0.152 | 0.287 | -0.134 | 0.026 |

Hausman test:
$H_0$: the preferred model is random effects
$H_1$: the preferred model is fixed effects

$$\chi^2(4) : 50.04$$
$$p - value : 0.0000$$

Table 9.6: Regression results random effects vs fixed effects having the share of female mayors as dependent variable

| share female | (1) Council | (2) Council |
|---|---|---|
| treated | -0.026* | 0.000 |
| | (0.010) | (.) |
| postquota | 0.010 | 0.002 |
| | (0.011) | (0.011) |
| (treated)(postquota) | 0.029** | 0.027* |
| | (0.011) | (0.011) |
| population | -0.000 | -0.000 |
| | (0.000) | (0.000) |
| population$^2$ | 0.000 | 0.000 |
| | (0.000) | (0.000) |
| average education | 0.005** | 0.010*** |
| | (0.002) | (0.003) |
| employed/total population | 0.268*** | 0.242*** |
| | (0.021) | (0.044) |
| constant | -0.065*** | -0.106*** |
| | (0.013) | (0.012) |
| FIXED EFFECTS | NO | YES |
| $N$ | 40649 | 40649 |
| $R^2$ | | 0.024 |
| $R^2$ Adj. | | -0.219 |
| $R^2$ Within | 0.024 | 0.024 |
| $R^2$ Overall | 0.022 | 0.018 |
| $R^2$ Between | 0.018 | 0.010 |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.7: The Hausman test related to Table(9.6

| | (b)<br>(2) | (B)<br>(1) | (b-B)<br>Difference | S.E. |
|---|---|---|---|---|
| postquota | 0.002 | 0.010 | -0.008 | 0.003 |
| (treated)(postquota) | 0.027 | 0.029 | -0.002 | 0.002 |
| $(\text{population})^2$ | -6.54e$^{-7}$ | -1.22e$^{-7}$ | -5.32e$^{-7}$ | 7.81e$^{-7}$ |
| poppulation | 7.88e$^{-14}$ | 4.60e$^{-14}$ | 3.28$^{-14}$ | 2.46e$^{-13}$ |
| average education | 0.010 | 0.005 | 0.005 | 0.002 |
| employed/ total | 0.242 | 0.268 | -0.026 | 0.039 |

Hausman test:
$H_0$: the preferred model is random effects
$H_1$: the preferred model is fixed effects

$$\chi^2(4) : 16.02$$
$$p - value : 0.0030$$

Table 9.8: Regression results random effects vs fixed effects having female unemployment rate as dependent variable

| | (1)<br>share female unempl. | (2)<br>share female unempl. |
|---|---|---|
| treated | -0.001 | 0.000 |
| | (0.002) | (.) |
| postquota | 0.006* | -0.013*** |
| | (0.003) | (0.003) |
| (treated)(postquota) | 0.001 | 0.003 |
| | (0.003) | (0.003) |
| pop + 15 | 0.000* | -0.000 |
| | (0.000) | (0.000) |
| (pop +15)$^2$ | -0.000* | 0.000 |
| | (0.000) | (0.000) |
| share female active pop | -0.225*** | -0.086*** |
| | (0.006) | (0.008) |
| share female unempl | 1.138*** | 0.965*** |
| | (0.004) | (0.008) |
| female average education | 0.005** | 0.003 |
| | (0.002) | (0.002) |
| average education | -0.008*** | -0.009*** |
| | (0.002) | (0.002) |
| constant | 0.157*** | 0.152*** |
| | (0.003) | (0.003) |
| FIXED EFFECTS | NO | YES |
| $N$ | 32372 | 32372 |
| $R^2$ | | 0.573 |
| $R^2$ Adj. | | 0.430 |
| $R^2$ Within | 0.567 | 0.573 |
| $R^2$ Overall | 0.808 | 0.799 |
| $R^2$ Between | 0.896 | 0.890 |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.9: The Hausman test related to Table(**??**

| | (b)<br>(2) | (B)<br>(1) | (b-B)<br>Difference | S.E. |
|---|---|---|---|---|
| postquota | 0.013 | 0.006 | -0.018 | 0.001 |
| (treated)(postquota) | 0.003 | 0.001 | 0.002 | |
| pop +14 | -6.69e$^{-8}$ | 4.70e$^{-8}$ | -1.14e$^{-7}$ | 9.51e$^{-8}$ |
| (pop +15)$^2$ | 1.35e$^{-14}$ | -2.45e$^{-14}$ | 3.80$^{-14}$ | 2.72e$^{-14}$ |
| share female active pop | -0.086 | -0.225 | 0.139 | 0.006 |
| share male unempl | 0.964 | 1.138 | -0.173 | 0.007 |
| female average education | 0.003 | 0.004 | -0.001 | 0.001 |
| average education | -0.009 | -0.008 | -0.001 | 0.001 |

Hausman test:
$H_0$: the preferred model is random effects
$H_1$: the preferred model is fixed effects

$$\chi^2(6) : 838.08$$
$$p - value : 0.0000$$

Table 9.10: Regression results random effects vs fixed effects having the share of housewives over the total female population aged 15 or over as dependent variable

| | (1)<br>share housewives | (2)<br>share housewives |
|---|---|---|
| treatment | 0.005 | 0.000 |
| | (0.004) | (.) |
| postquota | 0.009** | 0.033*** |
| | (0.003) | (0.003) |
| (treated)(postquota) | 0.003 | 0.001 |
| | (0.003) | (0.003) |
| pop +15 | 0.000*** | 0.000*** |
| | (0.000) | (0.000) |
| (pop +15)$^2$ | -0.000*** | -0.000*** |
| | (0.000) | (0.000) |
| share female active pop | -0.620*** | -0.653*** |
| | (0.008) | (0.010) |
| share female unempl | 0.311*** | 0.159*** |
| | (0.012) | (0.012) |
| share unempl | -0.439*** | -0.236*** |
| | (0.015) | (0.016) |
| share house husbands | -1.030*** | -0.124 |
| | (0.124) | (0.125) |
| female average education | -0.022*** | -0.027*** |
| | (0.002) | (0.002) |
| average education | 0.018*** | 0.009*** |
| | (0.002) | (0.002) |
| pop +15 female | -0.000*** | -0.000*** |
| | (0.000) | (0.000) |
| constant | 0.469*** | 0.573*** |
| | (0.006) | (0.005) |
| FIXED EFFECTS | NO | YES |
| $N$ | 24286 | 24286 |
| $R^2$ | | 0.580 |
| $R^2$ Adj. | | 0.369 |
| $R^2$ Within | 0.563 | 0.580 |
| $R^2$ Overall | 0.321 | 0.246 |
| $R^2$ Between | 0.218 | 0.131 |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.11: The Hausman test related to Table(9.11

| | (b)<br>(2) | (B)<br>(1) | (b-B)<br>Difference | S.E. |
|---|---|---|---|---|
| postquota | 0.033 | 0.008 | 0.024 | |
| (treated)(postquota) | 0,001 | 0.003 | -0.002 | |
| pop +15 | $8.84e^{-6}$ | 0.003 | $-2.00e^{-6}$ | $1.38e^{-6}$ |
| $(pop + 15)^2$ | $-2.40e^{-13}$ | $-3.19e^{-13}$ | $7.84^{-14}$ | $3.63e^{-14}$ |
| share female active pop | -0.653 | -0.619 | 0.034 | 0.006 |
| share female unempl | 0.159 | 0.311 | -0.152 | 0.001 |
| share unempl | -0.236 | -0.439 | 0.023 | 0.006 |
| share house husbands | -0.124 | -1.030 | 0.906 | 0.019 |
| female average education | -0.027 | -0.023 | -0.008 | 0.001 |
| average education | 0.009 | 0.018 | -0.008 | 0.001 |
| pop + 15 female | -0.000 | -0.000 | $2.77e^{-6}$ | $2.75e^{-6}$ |

Hausman test:
$H_0$: the preferred model is random effects
$H_1$: the preferred model is fixed effects

$$\chi^2(9) : 2839.25$$
$$p - value : 0.0000$$

# Appendix C

Table 9.12: Full results Table (7.1)

| | (1) Speed of Rev. | (2) Speed of Rev. | (3) Speed of Pay. | (4) Speed of Pay. |
|---|---|---|---|---|
| treated | 3.161*** | 0.931* | 0.652* | 0.155 |
| | (0.591) | (0.467) | (0.301) | (0.273) |
| population | 0.000** | 0.000*** | -0.000*** | -0.000*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| population$^2$ | -0.000*** | -0.000*** | 0.000*** | 0.000*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| share empl. in the Council | 5.228 | 1.194 | 9.525*** | 5.405* |
| | (3.243) | (3.193) | (2.253) | (2.225) |
| average education in the Council | 0.093* | 0.103* | -0.088*** | -0.018 |
| | (0.044) | (0.043) | (0.025) | (0.025) |
| early termination | 0.100 | 0.069 | -0.192 | -0.185 |
| | (0.234) | (0.224) | (0.134) | (0.133) |
| share members of the Council with previous political experience | -2.333*** | 0.940* | 0.791** | 0.431 |
| | (0.432) | (0.447) | (0.251) | (0.268) |
| employed/total population | 53.848*** | 2.438 | 7.328*** | 3.049* |
| | (1.756) | (2.323) | (0.905) | (1.280) |
| average education | 2.088*** | 1.531*** | -1.674*** | -0.768*** |
| | (0.132) | (0.184) | (0.073) | (0.107) |
| constant | 13.596*** | 41.427*** | 78.318*** | 76.685*** |
| | (3.372) | (3.647) | (2.297) | (2.413) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES |
| YEAR FIXED EFFECTS | NO | YES | NO | YES |
| $N$ | 83826 | 83826 | 83826 | 83826 |
| $R^2$ Within | 0.104 | 0.174 | 0.028 | 0.059 |
| $R^2$ Overall | 0.262 | 0.428 | 0.029 | 0.158 |
| $R^2$ Between | 0.357 | 0.563 | 0.037 | 0.244 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.13: Full results Table (7.2)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| treated | 0.000 | -0.006 | 0.000 | -0.004 | 0.000 | -0.030*** |
| | (.) | (0.003) | (.) | (0.005) | (.) | (0.009) |
| postquota | 0.065*** | 0.072*** | 0.063*** | 0.069*** | 0.002 | 0.010 |
| | (0.004) | (0.004) | (0.008) | (0.007) | (0.012) | (0.012) |
| (treated)(postquota) | 0.014** | 0.014*** | 0.015* | 0.015* | 0.027* | 0.028* |
| | (0.004) | (0.004) | (0.008) | (0.007) | (0.012) | (0.012) |
| population | -0.000 | -0.000*** | -0.000*** | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| population$^2$ | 0.000* | 0.000*** | 0.000** | 0.000* | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| average education | 0.011*** | 0.007*** | 0.011*** | 0.009*** | 0.010** | 0.008*** |
| | (0.001) | (0.001) | (0.002) | (0.001) | (0.003) | (0.002) |
| employed/total population | 0.064*** | 0.075*** | 0.152*** | 0.130*** | 0.242*** | 0.168*** |
| | (0.017) | (0.013) | (0.032) | (0.022) | (0.051) | (0.035) |
| constant | -0.005 | 0.003 | -0.033*** | -0.052*** | -0.106*** | -0.056* |
| | (0.004) | (0.009) | (0.009) | (0.014) | (0.013) | (0.022) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 40649 | 40649 | 40649 | 40649 | 40649 | 40649 |
| $R^2$ | 0.310 | | 0.117 | | 0.024 | |
| $R^2$ Adj. | 0.310 | | 0.117 | | 0.024 | |
| $R^2$ Within | 0.310 | 0.311 | 0.117 | 0.117 | 0.024 | 0.025 |
| $R^2$ Overall | 0.210 | 0.312 | 0.059 | 0.130 | 0.018 | 0.033 |
| $R^2$ Between | 0.030 | 0.315 | 0.005 | 0.161 | | |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Table 9.14: Full results Table (7.3)

| share female | (1)<br>share female Council | (2)<br>share female Ex. Committee | (3)<br>female mayor |
|---|---|---|---|
| share treated LMA | -0.004 | 0.003 | -0.012 |
| | (0.010) | (0.015) | (0.020) |
| population | -0.000*** | -0.000*** | -0.000* |
| | (0.000) | (0.000) | (0.000) |
| population$^2$ | 0.000*** | 0.000*** | 0.000* |
| | (0.000) | (0.000) | (0.000) |
| average education | 0.036*** | 0.036*** | 0.021*** |
| | (0.001) | (0.001) | (0.001) |
| employed/ | | | |
| total population | -0.032* | 0.046* | 0.127*** |
| | (0.013) | (0.023) | (0.034) |
| constant | -0.099*** | -0.153*** | -0.116*** |
| | (0.013) | (0.019) | (0.027) |
| PROVINCIAL | | | |
| FIXED EFFECTS | YES | YES | YES |
| $N$ | 40649 | 40649 | 40649 |
| $R^2$ Within | 0.270 | 0.103 | 0.024 |
| $R^2$ Overall | 0.256 | 0.111 | 0.030 |
| $R^2$ Between | 0.257 | 0.140 | 0.047 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.15: Full results Table (7.4)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.042*** | 0.000 | -0.027 | 0.000 | -0.055* |
| | (.) | (0.010) | (.) | (0.016) | (.) | (0.022) |
| postquota | 0.022* | 0.032** | 0.032 | 0.041* | -0.030 | -0.024 |
| | (0.011) | (0.010) | (0.019) | (0.019) | (0.029) | (0.027) |
| (share treated LMA)(postquota) | 0.059*** | 0.056*** | 0.047* | 0.044* | 0.060* | 0.063* |
| | (0.011) | (0.011) | (0.020) | (0.020) | (0.031) | (0.029) |
| population | -0.000 | -0.000*** | -0.000*** | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| population$^2$ | 0.000* | 0.000*** | 0.000** | 0.000* | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| average education | 0.011*** | 0.007*** | 0.011*** | 0.009*** | 0.011*** | 0.009*** |
| | (0.001) | (0.001) | (0.002) | (0.001) | (0.003) | (0.002) |
| employed/ total population | 0.060*** | 0.073*** | 0.148*** | 0.129*** | 0.235*** | 0.164*** |
| | (0.017) | (0.013) | (0.032) | (0.022) | (0.051) | (0.035) |
| constant | -0.005 | 0.037** | -0.033*** | -0.029 | -0.107*** | -0.033 |
| | (0.004) | (0.012) | (0.009) | (0.020) | (0.013) | (0.028) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 40649 | 40649 | 40649 | 40649 | 40649 | 40649 |
| $R^2$ | 0.311 | | 0.117 | | 0.024 | |
| $R^2$ Adj. | 0.311 | | 0.117 | | 0.024 | |
| $R^2$ Within | 0.311 | 0.311 | 0.117 | 0.117 | 0.024 | 0.025 |
| $R^2$ Overall | 0.212 | 0.312 | 0.060 | 0.130 | 0.018 | 0.033 |
| $R^2$ Between | 0.036 | 0.315 | 0.005 | 0.161 | 0.011 | 0.052 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.16: Full results Table (7.5)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.048*** | 0.000 | -0.035* | 0.000 | -0.061** |
| | (.) | (0.010) | (.) | (0.016) | (.) | (0.022) |
| election 1 | 0.051*** | 0.054*** | 0.068* | 0.066* | -0.020 | -0.022 |
| | (0.014) | (0.013) | (0.028) | (0.028) | (0.030) | (0.029) |
| (share treated LMA)(election 1) | 0.043** | 0.039** | 0.027 | 0.024 | 0.053 | 0.063* |
| | (0.014) | (0.014) | (0.030) | (0.030) | (0.032) | (0.031) |
| election 2 | 0.030* | 0.031* | 0.049 | 0.042 | -0.029 | -0.021 |
| | (0.014) | (0.014) | (0.027) | (0.026) | (0.042) | (0.040) |
| (share treated LMA)(election 2) | 0.075*** | 0.070*** | 0.051 | 0.048 | 0.073 | 0.088* |
| | (0.015) | (0.014) | (0.028) | (0.028) | (0.043) | (0.042) |
| election 3 | 0.035* | 0.038** | 0.028 | 0.030 | 0.006 | 0.029 |
| | (0.015) | (0.014) | (0.027) | (0.026) | (0.048) | (0.045) |
| (share treated LMA)(election 3) | 0.093*** | 0.086*** | 0.101*** | 0.089** | 0.047 | 0.047 |
| | (0.015) | (0.015) | (0.027) | (0.027) | (0.049) | (0.047) |
| election 4 | 0.051** | 0.056** | 0.069 | 0.068 | -0.055 | -0.027 |
| | (0.020) | (0.019) | (0.037) | (0.036) | (0.050) | (0.048) |
| (share treated LMA)(election 4) | 0.091*** | 0.078*** | 0.082* | 0.065 | 0.105* | 0.109* |
| | (0.021) | (0.020) | (0.039) | (0.038) | (0.053) | (0.052) |
| population | -0.000 | -0.000*** | -0.000*** | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| population$^2$ | 0.000* | 0.000*** | 0.000* | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| average education | -0.002 | -0.001 | -0.003 | 0.002 | 0.006 | -0.000 |
| | (0.002) | (0.001) | (0.003) | (0.002) | (0.005) | (0.002) |
| employed/total population | 0.019 | 0.024 | 0.126*** | 0.097*** | 0.181** | 0.081* |
| | (0.019) | (0.014) | (0.035) | (0.025) | (0.056) | (0.038) |
| constant | 0.076*** | 0.097*** | 0.050** | 0.023 | -0.062* | 0.041 |
| | (0.010) | (0.013) | (0.019) | (0.021) | (0.027) | (0.031) |

Table 9.16: Full results Table (7.5)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 40543 | 40543 | 40543 | 40543 | 40543 | 40543 |
| $R^2$ | 0.320 | | 0.122 | | 0.025 | |
| $R^2$ Adj. | 0.320 | | 0.122 | | 0.025 | |
| $R^2$ Within | 0.320 | 0.321 | 0.122 | 0.122 | 0.025 | 0.025 |
| $R^2$ Overall | 0.188 | 0.320 | 0.061 | 0.133 | 0.018 | 0.034 |
| $R^2$ Between | 0.001 | 0.319 | 0.001 | 0.161 | 0.008 | 0.055 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Table 9.17: Full results Table (7.6)

| | (1)<br>share female unempl. | (2)<br>share female unempl. | (3)<br>share housewives | (4)<br>share housewives |
|---|---|---|---|---|
| treated | 0.000 | -0.001 | 0.000 | 0.002 |
| | (.) | (0.003) | (.) | (0.003) |
| postquota | -0.013*** | -0.015*** | 0.033*** | 0.009** |
| | (0.004) | (0.003) | (0.003) | (0.003) |
| (treated)(postquota) | 0.003 | 0.003 | 0.001 | 0.001 |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| pop +15 | -0.000 | -0.000 | 0.000*** | 0.000** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| (pop +15)$^2$ | 0.000 | -0.000 | -0.000*** | -0.000*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| share female active pop | -0.086*** | -0.132*** | -0.653*** | -0.622*** |
| | (0.012) | (0.009) | (0.013) | (0.011) |
| share male unempl. | 0.965*** | 0.961*** | | |
| | (0.013) | (0.011) | | |
| female average educ. | 0.003 | 0.004* | -0.027*** | -0.022*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| average education | -0.009*** | -0.007*** | 0.009*** | 0.020*** |
| | (0.003) | (0.002) | (0.003) | (0.003) |
| share female unempl. | | | 0.159*** | 0.303*** |
| | | | (0.019) | (0.017) |
| share unempl. | | | -0.236*** | -0.377*** |
| | | | (0.026) | (0.024) |
| share house husbands | | | -0.124 | -0.971*** |
| | | | (0.182) | (0.175) |
| pop +15 female | | | -0.000*** | -0.000** |
| | | | (0.000) | (0.000) |
| constant | 0.152*** | 0.186*** | 0.573*** | 0.475*** |
| | (0.003) | (0.006) | (0.006) | (0.009) |

Table 9.17: Full results Table (7.6)

| | (1) share female unempl. | (2) share female unempl. | (3) share housewives | (4) share housewives |
|---|---|---|---|---|
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES |
| MUNICIPALITIES FIXED EFFECTS | YES | NO | YES | NO |
| $N$ | 32372 | 32372 | 24286 | 24286 |
| $R^2$ | 0.573 | | 0.580 | |
| $R^2$ Adj | 0.573 | | 0.580 | |
| $R^2$ Within | 0.573 | 0.574 | 0.580 | 0.563 |
| $R^2$ Overall | 0.799 | 0.829 | 0.246 | 0.563 |
| $R^2$ Between | 0.890 | 0.919 | 0.131 | 0.565 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column (1),(3) time-invariant variables have been omitted because of fixed effects, e.g. treated

Table 9.18: Full results Table (8.1)

| | (1)<br>Speed of Rev. | (2)<br>Speed of Pay. |
|---|---|---|
| treated | 0.794 | 0.766 |
| | (0.685) | (0.416) |
| (treated)(south) | 0.244 | -1.097* |
| | (0.935) | (0.550) |
| population | 0.000*** | -0.000*** |
| | (0.000) | (0.000) |
| population$^2$ | -0.000*** | 0.000*** |
| | (0.000) | (0.000) |
| share empl. in the Council | 1.195 | 5.399* |
| | (3.193) | (2.225) |
| average education in the Council | 0.103* | -0.018 |
| | (0.043) | (0.025) |
| early termination | 0.069 | -0.185 |
| | (0.224) | (0.133) |
| share members of the Council with previous political experience | 0.940* | 0.432 |
| | (0.447) | (0.268) |
| employed/total population | 2.441 | 3.033* |
| | (2.323) | (1.280) |
| average education | 1.531*** | -0.769*** |
| | (0.184) | (0.107) |
| constant | 41.553*** | 76.121*** |
| | (3.683) | (2.432) |
| PROVINCIAL FIXED EFFECTS | YES | YES |
| YEAR FIXED EFFECTS | YES | YES |
| $N$ | 83826 | 83826 |
| $R^2$ Within | 0.174 | 0.059 |
| $R^2$ Overall | 0.428 | 0.158 |
| $R^2$ Between | 0.563 | 0.245 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.19: Full results Table (8.2)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| treated | 0.000 | -0.014*** | 0.000 | -0.015* | 0.000 | -0.030* |
| | (.) | (0.004) | (.) | (0.007) | (.) | (0.013) |
| postquota | 0.079*** | 0.072*** | 0.075*** | 0.065*** | 0.002 | 0.018 |
| | (0.007) | (0.007) | (0.013) | (0.012) | (0.022) | (0.020) |
| (treated)(postquota) | 0.014* | 0.024*** | 0.020 | 0.029* | 0.027 | 0.024 |
| | (0.007) | (0.007) | (0.013) | (0.012) | (0.022) | (0.020) |
| (treated)(south) | 0.044*** | 0.022*** | 0.053*** | 0.025*** | 0.002 | 0.014 |
| | (0.006) | (0.004) | (0.012) | (0.007) | (0.017) | (0.012) |
| (postquota)(south) | -0.017 | 0.001 | 0.001 | 0.019 | -0.009 | -0.020 |
| | (0.009) | (0.008) | (0.016) | (0.015) | (0.025) | (0.022) |
| (treated)(postquota)(south) | -0.005 | -0.023** | -0.013 | -0.033* | -0.020 | -0.008 |
| | (0.009) | (0.008) | (0.017) | (0.015) | (0.026) | (0.023) |
| population | -0.000 | -0.000*** | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| population$^2$ | 0.000 | 0.000*** | 0.000 | 0.000** | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| average education | 0.004** | 0.002 | -0.000 | 0.002 | 0.016*** | 0.010*** |
| | (0.001) | (0.001) | (0.003) | (0.002) | (0.004) | (0.002) |
| employed/total population | 0.009 | 0.029* | 0.115** | 0.086*** | 0.123* | 0.105** |
| | (0.019) | (0.013) | (0.037) | (0.024) | (0.058) | (0.036) |
| constant | 0.035*** | 0.045*** | 0.018 | 0.006 | -0.088*** | -0.045 |
| | (0.005) | (0.009) | (0.011) | (0.016) | (0.016) | (0.025) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 32548 | 32548 | 32548 | 32548 | 32548 | 32548 |
| $R^2$ | 0.319 | | 0.111 | | 0.024 | |
| $R^2$ Adj. | 0.319 | | 0.110 | | 0.023 | |
| $R^2$ Within | 0.319 | 0.319 | 0.111 | 0.111 | 0.024 | 0.024 |
| $R^2$ Overall | 0.122 | 0.308 | 0.045 | 0.118 | 0.009 | 0.032 |
| $R^2$ Between | 0.037 | 0.292 | 0.003 | 0.130 | 0.003 | 0.048 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Table 9.20: Full results Table (8.3)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.034** | 0.000 | -0.039* | 0.000 | -0.011 |
| | (.) | (0.011) | (.) | (0.018) | (.) | (0.024) |
| postquota | 0.135*** | 0.088*** | 0.137** | 0.103* | -0.013 | 0.008 |
| | (0.027) | (0.024) | (0.048) | (0.044) | (0.076) | (0.065) |
| (share treated LMA) (postquota) | -0.053 | 0.004 | -0.059 | -0.017 | 0.049 | 0.039 |
| | (0.028) | (0.025) | (0.050) | (0.046) | (0.079) | (0.067) |
| (share treated LMA)(south) | 0.052*** | 0.041*** | 0.059*** | 0.051*** | 0.005 | 0.005 |
| | (0.006) | (0.005) | (0.012) | (0.010) | (0.016) | (0.014) |
| (postquota)(south) | -0.098*** | -0.025 | -0.109* | -0.050 | 0.039 | 0.020 |
| | (0.029) | (0.026) | (0.052) | (0.047) | (0.082) | (0.067) |
| (share treated LMA) (postquota)(south) | 0.074* | -0.001 | 0.098 | 0.036 | -0.077 | -0.058 |
| | (0.030) | (0.027) | (0.055) | (0.049) | (0.085) | (0.070) |
| population | -0.000 | -0.000*** | -0.000*** | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| population$^2$ | 0.000* | 0.000*** | 0.000** | 0.000* | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| average education | 0.013*** | 0.008*** | 0.012*** | 0.009*** | 0.014*** | 0.010*** |
| | (0.001) | (0.001) | (0.002) | (0.001) | (0.003) | (0.002) |
| employed/total population | 0.020 | 0.053*** | 0.127*** | 0.120*** | 0.183*** | 0.138*** |
| | (0.018) | (0.013) | (0.033) | (0.023) | (0.053) | (0.035) |
| constant | -0.018*** | 0.024 | -0.050*** | -0.022 | -0.105*** | -0.081** |
| | (0.005) | (0.013) | (0.010) | (0.021) | (0.014) | (0.031) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 40649 | 40649 | 40649 | 40649 | 40649 | 40649 |
| $R^2$ | 0.315 | | 0.118 | | 0.025 | |
| $R^2$ Adj. | 0.315 | | 0.117 | | 0.025 | |
| $R^2$ Within | 0.315 | 0.315 | 0.118 | 0.118 | 0.025 | 0.026 |
| $R^2$ Overall | 0.149 | 0.314 | 0.045 | 0.130 | 0.020 | 0.033 |
| $R^2$ Between | 0.027 | 0.315 | 0.001 | 0.161 | 0.014 | 0.052 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Table 9.21: Full results Table (8.4)

| | (1)<br>share female unempl. | (2)<br>share female unempl. | (3)<br>share housewives | (4)<br>share housewives |
|---|---|---|---|---|
| treated | 0.000 | 0.000 | 0.000 | -0.008 |
| | (.) | (0.003) | (.) | (0.005) |
| postquota | -0.013*** | -0.015*** | 0.029*** | 0.006 |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| (treated)(postquota) | 0.003 | 0.003 | 0.006 | 0.006 |
| | (0.003) | (0.003) | (0.004) | (0.004) |
| (treated)(south) | 0.000 | -0.003 | 0.000 | 0.018** |
| | (.) | (0.005) | (.) | (0.006) |
| (postquota)(south) | 0.001 | -0.000 | 0.002 | 0.001 |
| | (0.006) | (0.006) | (0.005) | (0.005) |
| (treated)(postquota)(south) | 0.001 | 0.000 | -0.010 | -0.011* |
| | (0.006) | (0.006) | (0.005) | (0.005) |
| pop +15 | -0.000 | -0.000 | 0.000*** | 0.000** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| (pop +15)$^2$ | 0.000 | -0.000 | -0.000*** | -0.000*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| share female active pop | -0.084*** | -0.133*** | -0.657*** | -0.627*** |
| | (0.012) | (0.009) | (0.013) | (0.011) |
| share male unempl. | 0.969*** | 0.961*** | | |
| | (0.014) | (0.012) | | |
| female average educ. | 0.003 | 0.004* | -0.027*** | -0.022*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| average education | -0.010*** | -0.007*** | 0.010*** | 0.021*** |
| | (0.003) | (0.002) | (0.003) | (0.003) |
| share female unempl. | | | 0.166*** | 0.310*** |
| | | | (0.019) | (0.017) |
| share unempl. | | | -0.263*** | -0.407*** |
| | | | (0.027) | (0.025) |
| share house husband | | | -0.146 | -0.973*** |
| | | | (0.182) | (0.175) |
| pop +15 female | | | -0.000*** | -0.000** |
| | | | (0.000) | (0.000) |
| constant | 0.152*** | 0.184*** | 0.573*** | 0.484*** |
| | (0.003) | (0.006) | (0.006) | (0.009) |

Table 9.21: Full results Table (8.4)

| | (1) share female unempl. | (2) share female unempl. | (3) share housewives | (4) share housewives |
|---|---|---|---|---|
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO |
| $N$ | 32372 | 32372 | 24286 | 24286 |
| $R^2$ | 0.573 | | 0.581 | |
| $R^2$ Adj | 0.573 | | 0.581 | |
| $R^2$ Within | 0.573 | 0.574 | 0.581 | 0.565 |
| $R^2$ Overall | 0.800 | 0.829 | 0.249 | 0.564 |
| $R^2$ Between | 0.890 | 0.919 | 0.132 | 0.565 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column (1),(3) time-invariant variables have been omitted because of fixed effects, e.g. treated

Table 9.22: Full results Table (8.5)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.064* | 0.000 | -0.040 | 0.000 | 0.006 |
| | (.) | (0.027) | (.) | (0.046) | (.) | (0.054) |
| postquota | 0.018 | 0.022 | 0.104** | 0.098* | 0.034 | 0.010 |
| | (0.021) | (0.020) | (0.039) | (0.040) | (0.061) | (0.056) |
| (share treated LMA)(postquota) | 0.049* | 0.047* | -0.048 | -0.038 | -0.041 | -0.005 |
| | (0.024) | (0.024) | (0.045) | (0.046) | (0.074) | (0.071) |
| population | -0.000 | -0.000*** | -0.000 | -0.000 | -0.000 | -0.000* |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| population$^2$ | 0.000 | 0.000*** | 0.000 | 0.000 | 0.000 | 0.000* |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| average education | 0.016*** | 0.014*** | 0.010 | 0.011* | 0.020* | 0.018* |
| | (0.004) | (0.003) | (0.007) | (0.005) | (0.010) | (0.007) |
| employed/total population | 0.016 | 0.008 | 0.168 | 0.083 | -0.007 | -0.018 |
| | (0.070) | (0.053) | (0.112) | (0.082) | (0.182) | (0.142) |
| constant | -0.006 | 0.009 | -0.028 | -0.022 | -0.036 | 0.056 |
| | (0.021) | (0.047) | (0.039) | (0.056) | (0.051) | (0.158) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 2253 | 2253 | 2253 | 2253 | 2253 | 2253 |
| $R^2$ | 0.240 | | 0.088 | | 0.010 | |
| $R^2$ Adj | 0.238 | | 0.086 | | 0.008 | |
| $R^2$ Within | 0.240 | 0.244 | 0.088 | 0.094 | 0.010 | 0.017 |
| $R^2$ Overall | 0.175 | 0.322 | 0.059 | 0.152 | 0.006 | 0.083 |
| $R^2$ Between | 0.107 | 0.457 | 0.016 | 0.303 | 0.005 | 0.236 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Table 9.23: Full results Table (8.6)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| share treated LMA | 0.000 | -0.062* | 0.000 | -0.042 | 0.000 | 0.012 |
| | (.) | (0.026) | (.) | (0.046) | (.) | (0.054) |
| election 1 | 0.046 | 0.043 | 0.149** | 0.132* | -0.030 | -0.062 |
| | (0.027) | (0.027) | (0.056) | (0.056) | (0.052) | (0.044) |
| (share treated LMA)(election 1) | 0.023 | 0.026 | -0.098 | -0.080 | 0.045 | 0.086 |
| | (0.033) | (0.033) | (0.066) | (0.066) | (0.066) | (0.062) |
| election 2 | 0.052 | 0.048 | 0.161** | 0.143* | 0.058 | 0.032 |
| | (0.030) | (0.030) | (0.061) | (0.062) | (0.089) | (0.087) |
| (share treated LMA)(election 2) | 0.016 | 0.019 | -0.105 | -0.086 | -0.074 | -0.033 |
| | (0.034) | (0.034) | (0.070) | (0.072) | (0.108) | (0.107) |
| election 3 | 0.064* | 0.064* | 0.089 | 0.078 | 0.182 | 0.170 |
| | (0.029) | (0.029) | (0.065) | (0.067) | (0.114) | (0.109) |
| (share treated LMA)(election 3) | 0.034 | 0.031 | -0.002 | 0.010 | -0.202 | -0.170 |
| | (0.033) | (0.033) | (0.074) | (0.076) | (0.130) | (0.129) |
| election 4 | -0.006 | -0.002 | 0.100 | 0.104 | -0.068 | -0.068 |
| | (0.036) | (0.035) | (0.078) | (0.079) | (0.086) | (0.086) |
| (share treated LMA)(election 4) | 0.135** | 0.126** | 0.010 | 0.000 | 0.106 | 0.128 |
| | (0.041) | (0.042) | (0.089) | (0.091) | (0.104) | (0.107) |
| population | -0.000 | -0.000*** | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| population$^2$ | 0.000 | 0.000*** | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| average education | 0.007 | 0.007* | 0.003 | 0.006 | 0.015 | 0.012 |
| | (0.004) | (0.003) | (0.007) | (0.005) | (0.011) | (0.008) |
| employed/total population | -0.090 | -0.074 | 0.062 | 0.003 | -0.021 | -0.068 |
| | (0.082) | (0.060) | (0.123) | (0.088) | (0.189) | (0.156) |
| constant | 0.069* | 0.068 | 0.037 | 0.032 | -0.011 | 0.096 |
| | (0.029) | (0.048) | (0.052) | (0.057) | (0.070) | (0.165) |

Table 9.23: Full results Table (8.6)

| share female | (1) Council | (2) Council | (3) Ex. Committee | (4) Ex. Committee | (5) Mayor | (6) Mayor |
|---|---|---|---|---|---|---|
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES | NO | YES |
| MUNICIPAL FIXED EFFECTS | YES | NO | YES | NO | YES | NO |
| $N$ | 2236 | 2236 | 2236 | 2236 | 2236 | 2236 |
| $R^2$ | 0.260 | | 0.094 | | 0.015 | |
| $R^2$ Adj. | 0.256 | | 0.089 | | 0.009 | |
| $R^2$ Within | 0.260 | 0.264 | 0.094 | 0.100 | 0.015 | 0.022 |
| $R^2$ Overall | 0.164 | 0.336 | 0.058 | 0.157 | 0.008 | 0.087 |
| $R^2$ Between | 0.047 | 0.461 | 0.002 | 0.298 | 0.003 | 0.239 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column (1),(3),(5) time-invariant variables have been omitted because of fixed effects, e.g. treated

Table 9.24: Full results Table (8.7)

| share female | (1) Council | (2) Ex. Committee |
|---|---|---|
| main | | |
| treated | 0.009** | 0.012* |
| | (0.003) | (0.005) |
| population | -0.000*** | -0.000 |
| | (0.000) | (0.000) |
| population$^2$ | 0.000*** | 0.000 |
| | (0.000) | (0.000) |
| average education | -0.004*** | 0.003 |
| | (0.001) | (0.002) |
| employed/total population | -0.002 | 0.059* |
| | (0.016) | (0.030) |
| constant | 0.048 | -0.310 |
| | (0.098) | (0.176) |
| W | | |
| treated | 0.172 | 0.466* |
| | (0.108) | (0.194) |
| PROVINCIAL FIXED EFFECTS | YES | YES |
| TIME FIXED EFFECTS | YES | YES |
| $N$ | 26998 | 26998 |
| Pseudo $R^2$ | 0.1967 | 0.0671 |

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9.25: Full results Table (8.8)

| | (1) share female unempl. | (2) share female unempl. | (3) share housewives | (4) share housewives |
|---|---|---|---|---|
| treated | 0.000 | -0.001 | 0.000 | 0.003 |
| | (.) | (0.003) | (.) | (0.003) |
| 2001 | -0.009* | -0.008* | -0.010** | -0.013*** |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| (treated)(2001) | -0.003 | -0.003 | 0.008** | 0.008** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| 2011 | -0.036*** | -0.034*** | -0.057*** | -0.068*** |
| | (0.004) | (0.004) | (0.005) | (0.004) |
| (treated)(2011) | 0.008* | 0.008* | -0.007* | -0.007* |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| pop +15 | -0.000 | -0.000* | 0.000*** | 0.000** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| (pop +15)$^2$ | 0.000 | 0.000 | -0.000*** | -0.000*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| share female active pop | -0.060*** | -0.121*** | -0.596*** | -0.585*** |
| | (0.012) | (0.009) | (0.012) | (0.010) |
| share male unempl. | 0.954*** | 0.961*** | | |
| | (0.013) | (0.011) | | |
| female average educ. | 0.014*** | 0.013*** | 0.007* | 0.018*** |
| | (0.003) | (0.002) | (0.003) | (0.002) |
| average education | -0.016*** | -0.013*** | 0.014*** | 0.009*** |
| | (0.003) | (0.002) | (0.002) | (0.002) |
| share female unempl. | | | 0.121*** | 0.228*** |
| | | | (0.019) | (0.017) |
| share unempl. | | | -0.143*** | -0.233*** |
| | | | (0.025) | (0.023) |
| share house husbands | | | 1.293*** | 1.576*** |
| | | | (0.158) | (0.144) |
| share female active pop | | | -0.000*** | -0.000** |
| | | | (0.000) | (0.000) |
| constant | 0.126*** | 0.171*** | 0.310*** | 0.286*** |
| | (0.004) | (0.006) | (0.012) | (0.009) |
| PROVINCIAL FIXED EFFECTS | NO | YES | NO | YES |
| MUNICIPALITIES FIXED EFFECTS | YES | NO | YES | NO |
| $N$ | 32372 | 32372 | 24286 | 24286 |
| $R^2$ | 0.578 | | 0.623 | |
| $R^2$ Adj | 0.577 | | 0.623 | |
| $R^2$ Within | 0.578 | 0.579 | 0.623 | 0.619 |
| $R^2$ Overall | 0.795 | 0.830 | 0.372 | 0.627 |
| $R^2$ Between | 0.889 | 0.919 | 0.260 | 0.631 |

Robust standard errors in parentheses. Standard errors are clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column (1),(3) time-invariant variables have been omitted because of fixed effects, e.g. treated

# Appendix D

Table 9.26: Results Eq.(8.1)

| share female | (1) Council | (2) Ex. Committee | (3) Mayor |
|---|---|---|---|
| treated | 0.009** | 0.011* | -0.002 |
| | (0.003) | (0.005) | (0.007) |
| population | -0.000*** | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) |
| population$^2$ | 0.000*** | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) |
| average education | -0.004*** | 0.003 | -0.004 |
| | (0.001) | (0.002) | (0.003) |
| employed/total population | -0.001 | 0.061* | 0.051 |
| | (0.016) | (0.030) | (0.044) |
| constant | 0.202*** | 0.108*** | 0.108*** |
| | (0.012) | (0.021) | (0.031) |
| PROVINCIAL FIXED EFFECTS | YES | YES | YES |
| TIME FIXED EFFECTS | YES | YES | YES |
| $N$ | 26998 | 26998 | 26998 |
| $R^2$ | 0.197 | 0.067 | 0.028 |
| $R^2$ Adj. | 0.193 | 0.063 | 0.023 |

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Moran test for spatial dependence:
$H_0$: error is i.i.d (there is no spatial effect in the residual considering the spatial matrix W)
$H_1$: there is a spatial effect in the residual (there is spatial effect in the residual considering the spatial matrix W)

Table 9.27: Moran test for spatial dependence

| Model | $\chi^2$ | p-value |
|---|---|---|
| (1) | 157,42 | 0.0000 |
| (2) | 22,63 | 0.0000 |
| (3) | 1,4 | 0.2366 |