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# Do Female Mayors Make a Difference? Evidence from Bavaria

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# Do Female Mayors Make a Difference?
# Evidence from Bavaria

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We provide empirical evidence that female mayors do not affect the expenditure allocation or size of local governments. This result is robust across a variety of regression discontinuity and standard multivariate estimations.We also examine whether elected female candidates have an increased likelihood of being reelected in a subsequent election, which is a phenomenon that has previously been interpreted as a sign that female candidates who are elected are particularly able politicians because they won an initial election in spite of the fact that electorates are typically gender-biased against female novice candidates. We confirm this correlation in our sample, but we show that it results from women incumbents being more likely to run for reelection. Moreover, we show that among incumbents who run for reelection, women have a significant electoral disadvantage.

*JEL Classification*: H0, H7, J0

*Keywords*: Local Public Finance, Public Choice, Gender, Regression Discontinuity

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# 1. Introduction

Women remain underrepresented in leading positions in both the private and public sectors, although this pattern has begun to change in recent years. It is therefore important to understand whether an increased proportion of women in politics affects the policies that are implemented by governments. This is important both from a positive viewpoint because the results may help voters with their voting decisions but also potentially from a normative perspective (e.g., in analyzing the question of gender quotas) (Svaleryd, 2009; Ferreira and Gyourko, 2011).

Most developed countries have witnessed profound institutional changes in recent decades, such as changing social norms and values regarding women's role in society, that can be argued to have affected both the desire of women to enter into certain professions (including political office) and the obstacles they face when attempting to do so. These changes are shown in the changing educational accomplishments of women and increasing female labor market participation. In the U.S., Ferreira and Gyourko (2011) showed that the percentage of female mayors has risen from close to zero in 1970 to approximately 15% in recent years. Rigon and Tanzi (2012) highlighted an increase in the share of female mayors in Italian municipalities from 2.5% in the mid 1980s to 10% in 2008. Our data show that similar trends can be observed in Bavaria, although at a slower pace than in the U.S. or Italy: the share of female mayors among Bavarian municipalities increased from below 1% in the mid 1980s to approximately 6% in 2008 (see section 3).

Because institutional changes may affect both candidate selection and local policy outcomes, we cannot interpret the observed correlations between candidate attributes and local policy outcomes as direct causal effects. For example, women's chances for election might increase when the electorate prefers "female" policies because female politicians may be trusted more to implement such measures than male politicians. Our strategy is to identify the causal effects of electing a woman as mayor on local policy outcomes using a regression discontinuity design (RDD); this identification strategy exploits close election outcomes as a source of quasi-randomization in candidate assignment and has previously been employed in a similar context, most notably by Leigh (2008); Lee (2008); Ferreira and Gyourko (2009, 2011).

Recently, scholars have addressed how the personal characteristics of political leaders in general may influence policy outcomes (Jones and Olken, 2005; Ferreira and Gyourko, 2009; Dreher et al., 2009; Ferreira and Gyourko, 2011; Besley et al., 2011; Jochimsen and Thomasius, 2012; Freier and Thomasius, 2012). We will contribute to this literature by examining the effect of politicians' gender at the local policy level in Germany for the first time.

The election data we use were also previously employed by Freier (2011) to estimate the incumbent party effects of mayoral elections on subsequent council elections, and Freier and Thomasius (2012) used the same data to estimate the effects of mayoral education level on investment, debt, and taxation levels. We are the first to link these election data to detailed budget data to analyze the effects of the personal characteristics of mayors on expenditure composition by policy area. Unlike the data used in previous studies concerning the effects of gender in politics, such as Ferreira and Gyourko (2011), these data have the advantage of not being administered by researchers with imperfect response rates. Instead, these are official data and consist of a full sample of all municipalities within one subnational entity. Furthermore, the data contain information about the candidates' professions and party affiliation, which is useful to test certain assumptions of the regression discontinuity approach.[1] The dataset also has a fairly high number of observations: it covers more than $2,000$ municipalities over a timespan of 23 years and $12,000$ elections. However, we face the limitation that there remain very few women in local Bavarian politics, leaving us with only a few hundred cases in spite of the large number of elections.[2]

The aim of this paper is to determine whether women change the size of local government and/or the allocation of expenditures across policy fields. This paper proceeds as follows: first, we will briefly discuss survey evidence about gender differences in public goods preferences. We will then outline relevant public-choice theories and discuss directly related empirical work. Next, we will describe the sample of Bavarian municipalities used in this study with respect to the budget data and the economic and political variables available for our empirical analysis. Finally, we will describe our empirical method and present and interpret our results.

## 2. Background

Early survey studies of gender differences in policy preferences in the U.S. concluded that women - compared to men - tended to disapprove of policies that entail an increased use of physical force, which subsumed fields such as national defense and the regulation of domestic violence. These studies highlighted differences with respect to topics affected by social conservatism, particularly abortion (Shapiro and Mahajan, 1986; Chaney et al., 1998). Regarding policy decisions with more directly visible financial consequences, Welch and Hibbing (1992) provide evidence that women tend to base their voting decisions more on the national economy ("sociotrophic voting")

[1] In other words, tests for the continuity of covariates at the discontinuity threshold (see the discussion in section 4).

[2] This is comparable with Ferreira and Gyourko (2011).

and less on their personal finances ("pocketbook voting").[3] Shapiro and Mahajan (1986), Chaney et al. (1998) and others claim that the gender voting gap in U.S. presidential elections, which has been observed since the Reagan/Carter election in 1980, is based on a divergence in public policy preferences between men and women that emerged from the social changes of the 1970s. Lott et al. (1999) argue that the gender gap is much older and that it merely reappeared in the late 1970s; they posit that it is not caused by differences in policy preferences but that it is instead related to the institutions of marriage, alimony, and the gender division of labor: depending on these institutions, the risk of marital termination because of death or divorce may affect women differently, and may make it more likely that women will benefit disproportionately from most redistributive government activities.[4] Edlund and Pande (2002) argue that rising divorce rates since the end of the 1960s have made women poorer compared to men, which has caused a leftward shift in female voting decisions, particularly with respect to middle-income women.[5] Edlund et al. (2005) use the German Socioeconomic Panel to show that women's preference for left-wing parties increases after a divorce, whereas the political party preferences of recently divorced men do not change; they also utilize evidence from the Eurobarometer survey that women do not have a stronger preference for redistribution per se but for policies that benefit children and single mothers. Svaleryd (2009) provides survey evidence from Swedish municipalities that women prefer expenditures related to childcare and education to those connected to elder care.

If there are gender differences in policy preferences, this does not necessarily indicate that either male or female politicians will act according to their gender-specific preferences (Svaleryd, 2009; Ferreira and Gyourko, 2011). Career-oriented politicians face strong incentives to adopt policies that are preferred by the median voter to increase their chances of being elected ("median voter theorem", Downs (1957)). If the median voter has "female" preferences[6], this might cause a career-minded politician to act accordingly, irrespective of his or her own gender. By contrast, in the "citizen-candidate" framework (Alesina, 1987; Besley and Case, 1995), candidates may also incorporate their personal preferences into their political decisions: in a multi-stage game, candidates who are elected face an incentive not to keep their promises and adopt the policies preferred by the median voter but to

[3]The results in Welch and Hibbing (1992) are based on survey data estimates of gender differences with respect to partial correlations between having voted for the non-incumbent party in presidential elections and a) stated perceptions of the national economy and b) stated individual economic success before the election.

[4]Lott et al. (1999) use variations in the time that women's suffrage was introduced across U.S. states to show that women's suffrage partly explains the growth in government expenditures.

[5]Edlund and Pande (2002) provide empirical evidence from the National Election Studies surveys.

[6]This would not require the median voter to be a woman if we conceive of "male/female"-preferences for public goods as a dichotomy that is not identical with-but merely derived from-biological gender categories

implement their own preferred policies instead.[7]

Candidate selection becomes even more theoretically complicated if the women who decide to enter politics differ not only with respect to preferences but also with respect to other characteristics, such as abilities.[8] Therefore, to attribute effects to gender-partisanship in a regression discontinuity design, it will be instructive to assess whether women who won by narrow margins differ from narrowly elected men with respect to other observed characteristics.[9] This is a feature that has not received much attention in previous studies most likely because of the absence of information about candidate characteristics (compare, for example, Ferreira and Gyourko (2011)).

Thomas (1991) is among the first studies to empirically examine the effect of politicians' sex on policy decisions, finding that a higher share of women in state legislatures makes it more likely that states will pass more bills related to the concerns of women, families, and children.[10] Exploiting female quota legislation that randomly requires certain Indian villages to restrict the mayorship to a woman, Chattopadhyay and Duflo (2004) find that elected mayors make investments in infrastructure in accordance with gender-specific preferences. After having shown that survey evidence for Swedish municipalities suggests that women prefer governmental spending on childcare and education to spending on elder care, Svaleryd (2009) provides empirical evidence that this translates into political decisions by female legislators: controlling for time and municipality-fixed effects, municipalities with a greater share of women in the legislature spend more on these policy fields. By contrast, employing a regression discontinuity design, Ferreira and Gyourko (2011) show that female mayors of U.S. cities do not produce policy results that are significantly different from those of male mayors. Ferreira and Gyourko (2011) find that female mayors remain in office longer, which they attribute to "hidden qualities" of female mayors. Using a sample of Italian municipalities, Rigon and Tanzi (2012) evaluate the effects of female representation in municipal councils using exogenous variation imposed by a federal law that imposed gender quotas and find no effect

[7] However, even in this setting, depending on the politicians' time-horizon and if voters are rational and forward-looking, candidates may be able to credibly commit to policies that differ from their own preferred policies: Besley and Coate (1997) show how maximum-term-length rules affect policy decisions. Moreover, Alesina (1987) theoretically and empirically analyzed a similar setting with respect to (party) preferences for unemployment and inflation, whereas Besley and Case (1995) did so with respect to government size. The importance of commitment was subsequently illustrated by theories that modeled monitoring and selection mechanisms in more complex settings (Persson and Tabellini, 2002).

[8] See Besley (2007), who extended the citizen-candidate framework to integrate ability and partisan issues by analyzing scenarios in which there are both undisputed policy issues ("valence issues", for which only politicians' abilities and / or honesty are relevant) and partisan issues and where the electorate only partly consists of partisan voters.

[9] See section 5.

[10] However, the results are based on a cross-section of states, with a total of only 12.

on the composition of expenditures. In this paper, we will further evaluate the questions raised and results provided by these studies.

Our sample consists primarily of small local jurisdictions. As Ferreira and Gyourko (2009) noted, a central insight from the literature on interjurisdictional competition is that small jurisdictions are relatively restricted in their abilities to implement redistributive policies (Zodrow and Mieszkowski, 1986; Wilson, 1999), which leads to the hypothesis that we should expect less partisanship in local politics. Ferreira and Gyourko (2009) also argue that partisan politics should be expected to be less important at the local level because the preferences of the electorate can be expected to be relatively homogeneous at the local level, since it may be relatively less costly to sort into local jurisdictions according to preferences for public goods ("Tiebout sorting").[11] In the context of this study, however, this argument may not be as cogent because marriage and partnership result in couples being spatially interdependent. Therefore, sorting into jurisdictions according to gender-specific preferences theoretically seems more difficult than, for example, sorting according to age-specific preferences. Moreover, the small size of the jurisdictions might also affect our informational assumptions: if small jurisdictions are characterized by close-tie networks, individuals in small jurisdictions might be better informed about the quality of candidates before the election because they are more likely to know them personally. This would lead to the expectation that ability considerations are less of an issue in local elections, which may make candidate selection in local elections more partisan.[12]

Another issue in determining the policy effects in local jurisdictions may be formal limitations with respect to policy discretion (Svaleryd, 2009; Gerber and Hopkins, 2011).[13] Regarding Bavarian municipalities, we note that a large portion of their responsibilities are shared with higher levels of government. Most importantly, with respect to social assistance, individual municipalities are largely obligated to implement federal policy. Education policies are primarily decided at the state level. However, municipalities have some degree of discretion regarding the financial resources devoted to local schools. They also have some discretion about the quality

[11] Comparing U.S. cities that narrowly elected a Democratic mayor instead of a Republican mayor, Ferreira and Gyourko (2009) find that the mayor's party affiliation seems to have little effect on indicators for the size of government.

[12] However, other factors specific to small jurisdictions might have opposing effects, such as less media coverage or lower average educational attainment of the electorate. See Revelli (2008), who highlighted the importance of media coverage in monitoring politicians.

[13] Also using a regression discontinuity design, Gerber and Hopkins (2011) find that narrowly elected Democratic mayors in U.S. cities spend less on police than narrowly elected Republican mayors and essentially argue that this can be attributed to "ideology" combined with federal institutional rules that make this a policy matter with relatively high local discretion; in other areas with less or overlapping authority, such as social spending and taxation, they do not find significant differences in spending, as expected.

and quantity of local amenities such as parks, sports and recreation facilities, local roads, pedestrian zones, cultural amenities (such as theaters and libraries), and parts of the social budget that concern local services (such as family counseling). Regarding revenue, municipalities are entitled to the revenues from three local taxes and can set the collection rates for these taxes. The first is a local business tax and the other two are property taxes - one for built or buildable land (land zoned for residential housing, "property tax B") and the other for agricultural land (land zoned for agriculture, "property tax A"). Of these, the business tax generates the most revenue (the revenue generated is highly cyclical), followed by property tax B (stable, but much less revenue). Another potentially relevant institutional characteristic is the political power that municipal law grants to the mayor and the effective political power that the mayor has in the specific institutional framework. By contrast to other German states, Bavarian mayors are granted exclusive executive powers de jure. They also hold a one-vote seat on the legislative council ("Gemeinderat") that, depending on the population, consists of between four and 60 members. Because the mayor shares legislative power with the local council, we assume that common pool problems of shared responsibilities may arise, in which the mayor and the other council members do not fully internalize all the consequences of their policy decisions (Buchanan and Tullock, 1962; Jochimsen and Thomasius, 2012). Because the mayor may be held more directly accountable for his/her personal actions in office, he/she may face stronger incentives to respect median voters' preferences than the average council member.

# 3. Data

## 3.1. Elections

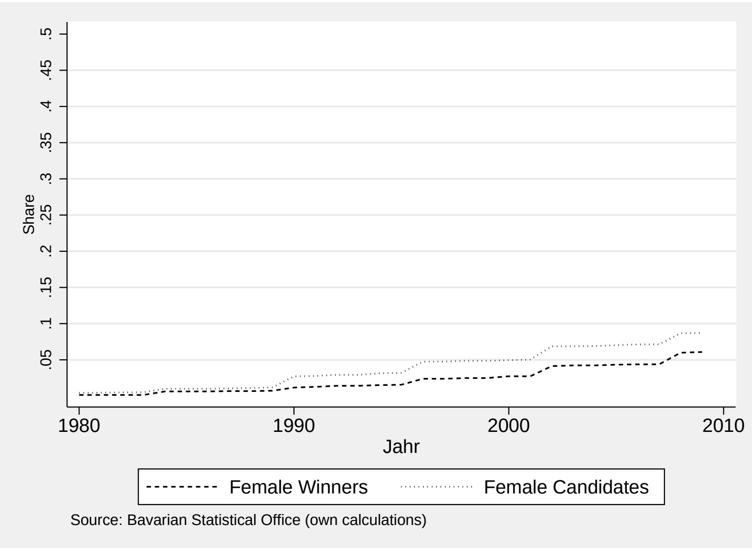
We use Bavarian municipal data on local political candidates, which include their full names, gender, profession, vote share and party affiliations (endorsing parties)[14] from mayoral elections in Bavarian municipalities[15] between 1984 and 2009. Election data are available since 1948, but budget data limitations require our analysis to

[14]Germany currently has five large parties, the CDU/CSU (conservative), of which the CSU, although formally an independent party, can be regarded as the Bavarian "sister-party" of the CDU, the SPD (social democratic), the FDP (liberal), the Green Party (ecological) and the Left Party (socialist). Of these, only the CSU and the SPD play a major role in Bavarian local elections and function alongside a large number of small local parties and voter unions, of which many are formally organized in an umbrella organization called "Free Voters" (FW).

[15]We limit our sample to Bavarian municipalities because first, we could not find mayoral election data from any other German state for a comparable time period. The Bavarian election data cover 1948-2008; the next best data are from Hesse and cover 1994-2008. Second, Bavarian mayoral data include information on candidate professions. Finally, because municipal law differs among German states, limiting the sample to Bavaria helps minimize institutional differences among observational units.

begin in 1984.[16] Our election data consists of all Bavarian municipalities that existed in 2008. Thus, although it includes municipalities that are not constant since 1948 with respect to their jurisdictional boundaries because of mergers (and some re-divisions) during municipal reforms, the identity of municipalities in the dataset exhibits little variation since the completion of the last major municipal reform in 1978 ($2,043$ of the current $2,056$ have not changed boundaries since 1978). Elections are regularly and uniformly held, nearly always on the same day in March every six years.[17] The total number of elections in our dataset since 1952 is $23,761$, the number of elections since 1978 is $12,692$.

**Figure 1:** Share of Women in Bavarian Mayoral Elections, 1980-2008




As figure 1 shows, since 1980, the number of female mayoral candidates and female mayors, beginning from a level well below 1%, slowly rose to approximately 6% in 2008, with each regular election year resembling a small jump upwards.[18]

[16]Note that there are few women in the sample previously; thus, the limited time frame of the budget data is not a problematic restriction, which becomes apparent in robustness estimations that we performed for those variables that are available for years prior to the mid-1980s, such as debt (since 1978) and tax collection rates (since 1970): the results are not meaningfully altered by including these years.

[17]There are certain exceptions, such as resignations before the next regular election year. We partly control for this by the use of calendar year-fixed effects. Moreover, we reestimated all specifications without the few off-cycle elections, which did not change our basic results.

[18]According to our data, the first mayoral election with a listed female candidate in Bavaria occurred in 1960, when Paula Riegel in the village of *Genderkingen* challenged incumbent Andreas Voag and lost by receiving only two of the valid 501 votes (Mr. Voag got 499). The first female mayor in Bavaria was the small business owner and liberal independent candidate Käthe Winkelmann, who was elected mayor of *Neufahrn bei Freising* in 1964; however, she was the only candidate in that election. In the subsequent election in 1966, Ms. Winkelmann successfully defeated her male challengers Ulrich Karg (CSU) and Adam Lamprecht (SPD) (she received 67.4% of the vote). She remained in office until 1977. She is most known for investment in local infrastructure. She is also known for her (unsuccessful) resistance to plans to build an international airport near the town (information from our data and the newspaper article "Nun bringt Käthe Zug ins Dorf" from the daily "Die Zeit", 02/21/1975).

Note that, compared to the U.S. (approximately 15%, Ferreira and Gyourko (2011)) and Italy (approximately 10%, Rigon and Tanzi (2012)), this 6% share is low. Compared to neighboring German Länder for which we have corresponding figures, however, it is approximately identical.[19]

Another remarkable observation from the election data is the high share of practically unopposed candidates. Since 1978, $5,046$ of $12,692$ - approximately 39% - of all elections[20] had only one listed candidate ("sample 1" in table 1).

**Table 1:** Bavarian Mayoral Elections, Subsamples, 1978-2009

| Subsamples | N | Municipal Characteristics (Means) | | | |
|---|---|---|---|---|---|
| | Elections | Pop. | Protestants'87 | Agric.Empl.'87 | Serv.Empl.'87 |
| 1: one candidate | 5,046 | 2,601 | 21.1% | 12.9% | 13.3% |
| 2a: two candidates | 5,561 | 5,353 | 21.8% | 9.2% | 14.4% |
| 2b: two cand., one female | 547 | 6,821 | 23.0% | 7.8% | 14.8% |
| 3: 3+ candidates | 1,538 | 16,389 | 21.8% | 6.7% | 15.2% |
| **Total** | 12,692 | 5,659 | 21.5% | 10.3% | 14.1% |

*Source:* Bavarian Statistical Office, own calculations

With few exceptions, these (formally) uncontested candidates were all elected because all unlisted candidates altogether seldom receive more than a few percentage points.[21] In approximately 44% of all elections, exactly two listed candidates participated (sample 2a in table 1).[22] In 547 - or approximately 9.8% - of these 2-candidate elections, one of the candidates was a woman (sample 2b), which con-

[19]The corresponding values for Hesse and Baden-Wuerttemberg are 8% and 5%. The data for Hesse are from the Hessian statistical office. The statistical office of Baden-Wuerttemberg does not centrally collect mayoral election data. Consequently, there are no official data available for Baden-Wuerttemberg. The figure of 5% is an estimate that is based on an unofficially collected Baden-Wuerttemberg mayoral election dataset that was kindly provided by a local treasurer of the Social Democrat Party (SPD) (further information is available from the author upon request).

[20]We define elections as "decisive" elections, meaning that we exclude elections that, due to no candidate receiving an absolute majority, lead to a subsequent runoff election. This means that we include runoff elections in these cases, effectively treating elections that lead to a runoff as a formal but equivalent extension of any informal pre-selection processes that typically precede elections.

[21]In addition to marking a cross next to a listed candidate, voters may write-in any citizens' name on the ballot instead (the citizen must be at least the legal passive voting age, which is 21). Typically, a resident citizen who wishes to be seriously considered as a candidate will face only modest difficulties in being listed on the ballot, as she must merely find a group of 6 people to nominate her to be listed.

[22]To (practically) ensure clear assignment using the vote share in a wide bandwidth around the regression discontinuity threshold, we classified the (very rare) elections with two listed candidates in which all unlisted candidates together received more than 10% of the vote as elections with more than two candidates.

stitutes the sample we can use for our sharp regression discontinuity design (see section 4). Approximately 12% of all elections had more than two listed candidates (sample 3). In the event that neither of the candidates receives more than 50% of the vote, Bavarian Municipal Election law prescribes runoffs.[23] In these cases, we take the results from the runoff election to ensure that we can calculate the voting margin with unambiguous assignment (see the discussion in section 4).

## 3.2. Budget and Tax Rates

We derive indicators for the allocation of expenditures from municipal budget data and the indicators of local government size from budget data and data on tax collection rates. Budget data for Bavaria are available from the Bavarian Statistical Office from 1984-2009 for general budget categories, aggregated by item type ("total investment in fixed assets", "total operating expenditures", "total wages", etc.). Municipal debt data are available from 1978-2009. Municipal tax collection rate data cover the years 1970-2010. We provide mean statistics for these general budget items along with tables that present our estimates in section 6.

Detailed budget data that allow us to examine expenditure allocations across policy fields are available from 1987-2009. These detailed data list single budget items according to both the field of activity and expenditure type. The budgetary system in place during the time span covered by our data (the *Haushaltssytematik*[24]) lists expenditures and revenues by type and policy field. Regarding policy fields, it distinguishes the following: (0) general administration; (1) public safety (police and fire); (2) schools; (3) culture and science; (4) social; (5) health, sports and recreation; (6) roads and public housing; and (7)-(9) which comprise local business development, municipal utility companies, and financial administration. Public utilities (water, electricity, waste disposal, etc.) account for approximately half of the entire budget, on average.[25] Except for the larger municipalities, public utility companies are often run in cooperation with other municipalities and are therefore not generally directly and exclusively controlled by a local government, which is why we exclude them from further analysis. During the period considered, the same is true for the majority of the social budget (social assistance, in particular).

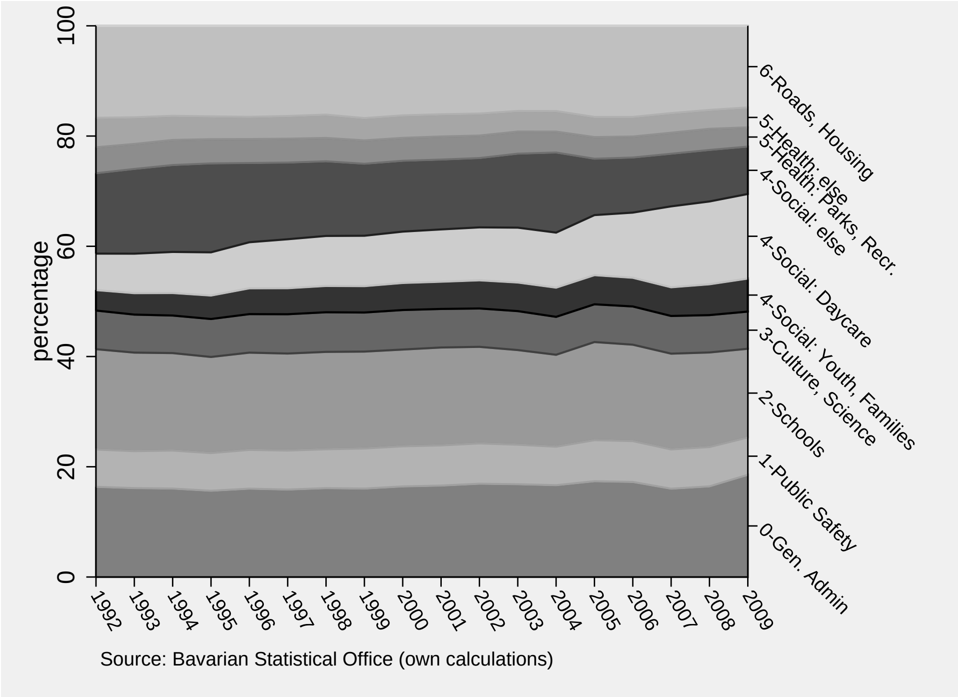
Figure 2 shows a graphic illustration of how policy fields (1) through (6) are represented in local budgets and how their shares have evolved over time, as measured by

[23] Gesetz über die Wahl der Gemeinderäte, der Bürgermeister, der Kreistage und der Landräte (Gemeinde- und Landkreiswahlgesetz - GLKrWG).

[24] The *Haushaltssytematik* is legally based on *Bayerische Gemeindeordnung* (Bavarian municipal law) and *Kommunalhaushaltsverordnung*, a state government decree on local public budget plans (*"Verordnung über das Haushalts-, Kassen- und Rechnungswesen der Gemeinden, der Landkreise und der Bezirke" (KommHV).*)

[25] Calculated from our data. The respective statistics are available from the author upon request.

**Figure 2:** Development of Policy Areas' Shares in Municipal Budgets (Operating Expenditures, Wages, and Transfers, w/o (7)-(9))




their shares in the sum of operating expenditures, transfers, and wages (excluding policy fields (7)-(9)).[26] A notable aspect of the development depicted in figure 2 is that the calculated share of "Social: else", which is primarily social assistance, declines in favor of the other policy fields in 2005. This is most likely because of a large federal social policy reform that occurred that year (the "Hartz IV-Reform"). Under this reform, responsibility for social assistance was shifted to the federal level in exchange for the relatively smaller task of housing assistance (IW Dienst, 2011). Another feature revealed by this figure is a steady increase in the share of childcare expenditures since 1992. We believe that an important driving factor of this development is the steady increase in female labor market participation that occurred during the same period and (likely related to this factor) changes in legislation at the federal level.[27]

[26] We include transfers because some of the smallest municipalities also share smaller facilities such as schools, libraries, and kindergartens and some municipalities have delegated these tasks to other providers (mostly churches, in the case of kindergartens), which then leads to operating expenditures, wages, and investments for these categories being zero and replaced by transfers to some other entity. We exclude investment expenditures from this figure because, given the time frame of our data, we consider investment at the disaggregated level excessively noisy for our empirical analyses.

[27] There were several efforts at the federal level to increase daycare services for children between one and three years old, for instance *"Gesetz zum qualitätsorientierten und bedarfsgerechten Ausbau der Tagesbetreuung (Tagesbetreuungsausbaugesetz, TAG)"* passed in 2005. We are also open to the explanation that increasing female participation in local politics played a role, which is precisely the research question in this paper. However, given the small share of female mayors in Bavaria, we are skeptical that even a large local effect of female leadership on expenditure

Finally, we also collect structural and economic data for the municipalities in our dataset, which we use to assess covariate continuity assumptions that are necessary for a regression discontinuity design.[28]

# 4. Method

To determine whether women increase the size of local government and/or affect the allocation of expenditures by policy area, we measure changes[29] in the per capita values of the (sum of) budget items from before the election to the end of the election period. Because the budget data are noisy (i.e., they fluctuate between years), we compare smoothed values from before and after the election. More precisely, to calculate changes, we take the mean of the respective budget item in years 4, 5, and 6 of the subsequent election term and subtract the mean of the respective budget item in years $-1$, $-2$, and $-3$ before the election, which are in most cases equal to years 4, 5, and 6 of the preceding election term.[30]

Therefore, the dependent variables comprise changes in the deflated[31] values of debt per capita, investments in fixed assets, operating expenditures, and total wages.[32] To measure the effects regarding the allocation of public expenditures according to female preferences for public goods, we assign a variable to each expenditure category (by policy field) that indicates female preferences, based on conclusions that can be drawn from the relevant literature. For the sake of simplicity, we constrain the gender assignment of policy fields to be bivariate, which means that we simply categorize all budget items into two categories: (1) "female" or (2) "non-female". Because previous studies provide theoretical indications but are not conclusive with respect to the policy fields that should be classified as "female", we test alternative gender assignment combinations.

We wish to identify the causal effects of electing a woman mayor using observational data. In so doing, we must consider the possibility that there are unobserved covariates that affect both our outcome variable and our explanatory variable. For example, it might be that a woman is elected because the electorate prefers that

composition would have had a notable aggregate effect on state-wide municipal averages as they are described here.

[28] These data include data on the number of residents (1978-2010) and other covariates available from the Statistical Office of the State of Bavaria.

[29] In the case of municipal fixed effects estimations, we measure levels at the end of the election period.

[30] Note that budget years are equivalent to calendar years. Moreover, as mentioned above, as a robustness check we reestimated all of our specifications excluding elections that were not held at the usual time in March in the years 1984, 1990, 1996, and 2002, and this did not change any of our basic results (which is not surprising, as the bulk of elections were held at these regular dates).

[31] The base year for deflation is 2005.

[32] Wages exclude pensions and allowances for voluntary work.

more resources be allocated to female public goods. Although time-constant differences in local preferences for female public goods might be addressed using municipal fixed-effects estimations, these preferences may change.[33] Majority election systems allow us to address this identification problem using a regression discontinuity design (Lee, 2008; Ferreira and Gyourko, 2009, 2011): with two electoral candidates, one of which possesses a particular property that the other does not, simple majority rule leads the treatment covariate $W$ to take the value 1 if the respective candidate's vote share ($X$: the "assignment variable" or "forcing variable") takes a value larger than a threshold value of $c = 50\%$ and 0 otherwise. This rule thus creates a known discontinuity in the treatment covariate $W$ at the threshold of $c = 50\%$ of the vote share ($X$) (Imbens and Lemieux, 2008).

In the potential outcomes framework, for each unit of observation (budget years within jurisdictions in our case), one can term $Y(W = 1)$ the potential outcome if that unit were treated and $Y(W = 0)$ if that unit were not treated, making $Y_i(1) - Y_i(0)$ the treatment effect for a given unit $i$. The identification assumption of a regression discontinuity design builds on the reasoning that, a priori, there seems to be no compelling reason to assume that (in the absence of the treatment assignment rule) future outcomes $Y$ (covariates realized after any potential treatment has occured, i.e., future fiscal variables, in our case) would have shown a discontinuity at the threshold ($c$), which is equivalent to arguing that the conditional distribution functions of $Y$ to the left of the threshold, $F_{Y(0)|X)}(Y|X)$, and to right of the threshold, $F_{Y(1)|X)}(Y|X)$, are continuous in $X$ for all $Y$ (Imbens and Lemieux, 2008).

Because we can never observe the same unit in both a treated and in an untreated state (i.e., we do not observe the counterfactual outcome), we can only estimate average treatment effects for groups of units (subpopulations) that are either treated or untreated.[34] However, by contrast to other identification methods (such as standard multivariate regression), we can also never observe both treated and untreated units *for the same value of the covariate* $X$ (the assignment variable). Therefore, we can only observe average outcomes for untreated units to the left of the threshold $c$, $E[Y_i(0)|X_i < c]$ and the average outcomes for treated units to the right of this threshold, $E[Y_i(1)|X_i > c]$ (Imbens and Lemieux, 2008; Lee and Lemieux, 2010).

From the continuity assumption and the assumption that individuals cannot sort precisely around the threshold, however, it follows that we are allowed to extrapolate across the threshold at least within an interval small enough such that our observed $Y$ are not confounded by other relationships with the assignment variable $X$,[35] and

[33] For example, changes in the cultural and socioeconomic characteristics of the electorate resulting from migration or urbanization processes.

[34] Note that this is the case for any causal identification method.

[35] For example, it might be that a large value of the assignment variable, such as the vote share of a female candidate, is confounded by some other variable that is correlated with the outcome

we compare outcomes to the left and right of the threshold that are sufficiently close to the threshold, $\lim_{\epsilon\to\infty} E[Y_i|X_i = c+\epsilon] - \lim_{\epsilon\to\infty} E[Y_i|X_i = c-\epsilon]$, and interpret this as the local average treatment effect (LATE) at the threshold, $E[Y_i(1) - Y_i(0)|X_i = c]$.

It is important to note that this is a *local* average treatment effect, and therefore only allows inferences regarding causal relationships for units that are within the sufficiently small[36] bandwidth $\epsilon$ around the threshold (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In our case, this means we can only make inferences for the subsample of close-outcome elections with exactly two candidates, of which one is a woman. Comparing the descriptive statistics of this subsample of elections with other elections (as shown in table 1) suggests that municipal characteristics are not markedly different for this subsample. However, it seems plausible that at least with respect to time-varying variables, such as increased political competition during or before controversial long-term (investment) decisions, our identification strategy comes at the cost of limited sample representativity.

It may also be important that, as Ferreira and Gyourko (2011) note, electing a woman as mayor may have multiple direct or indirect effects on policy outcomes: for example, electing a woman could encourage (or discourage) other women to run in the next election, which might cause political competition in the next election to depend on the incumbent's gender. The multitude of possible theoretically plausible channels of causation - meaning the channels through which electing a woman affects policy outcomes such as budget decisions or term lengths - cannot be disentangled with our data, because we have no way of observing all possible channels. All the observed effects should therefore be interpreted as "intent-to-treat effects" (Ferreira and Gyourko, 2011).[37]

As Lee and Lemieux (2010) note, randomization of treatment assignment is a necessary consequence if agents are unable to precisely control the assignment variable to sort themselves on either side of the cutoff.[38] One can then measure the effect of treatment at the threshold by estimating local linear regressions to the left and right of the threshold within the bandwidth $\epsilon$, and compare the estimates as they

variable.

[36]The procedure for determining the value that $\epsilon$ should take cannot be discussed here; we refer to Lee (2008). For the justification of the thresholds chosen in our analysis, see below.

[37]Although this limitation to intent-to-treat effects seems particularly evident in the given context, it may actually prove difficult to find any applied study that is both suitable for an RDD and in which the treatment effects theoretically only operate through a single channel (for example, imagine admission to a scholarship program based on being above a certain test score threshold; the scholarship will provide funding, but it may also provide encouragement through social recognition or lead to different treatment by fellow students).

[38]In our case, for randomization around the 50% cutoff to be successful, this means that the equivalent necessary assumption is that, although candidates may be able to influence their vote shares by increasing or decreasing their effort levels during their election campaigns, they cannot precisely control whether they are above or below a given vote share $a$, given that they can be expected to end up near that certain vote share.

approach the cutoff. If one is willing to make assumptions regarding the functional form of the continuous relationship between the assignment variable (vote share) and future outcomes (i.e., fiscal variables) over a range of the assignment variable that is larger than $\epsilon$, this discontinuous relationship may be modeled by imposing a functional form on the relationship between the assignment variable and the outcome that allows the use of information from realizations further from the cutoff value. In practice, along with a treatment dummy that takes the value of 1 whenever the assignment variable is larger than 50% and 0 otherwise, a higher degree polynomial should then be imposed on the relationship, which is allowed to differ to the left and right of the threshold (Imbens and Lemieux, 2008; Lee and Lemieux, 2010).

Finally, calculating per capita values often leads to rather noisy data because we are considering small municipalities. This indicates that it is necessary to perform at least basic outlier analysis. We do so by excluding observations with an absolute studentized residual value larger than 3 (Huber, 2004).[39] In our graphical RDD representations of the effect estimates on per capita values, such defined outliers are marked in blue; a second (broken, blue) regression line depicts the estimates if we *include* these outliers, whereas a solid (black) line shows the estimates if we *exclude* the outliers. We retain a consistent outlier definition throughout.

# 5. Validity

As discussed in the previous section, the regression discontinuity design relies on a set of theoretically plausible assumptions. The appropriateness of these assumptions cannot be directly tested, but we can perform empirical tests for at least some of these assumptions that may improve our confidence that these assumptions are satisfied for our particular research question and data sample. We performed a series of tests for jumps in municipal characteristics measured in the year preceding the election, and these results are presented in table 6. Figure 9 in section A.1 shows the corresponding estimates; these estimates provide no indication that municipalities to the left and right of the thresholds are markedly different. Note that the t-values for population and population density are above standard significance thresholds. However, our outlier identification rule states that this is because of the municipalities in only two or three observations that happen to be both outliers and influential (because they are near the cutoff).[40]

[39] We recalculated all of our estimations with an outlier definition of the studentized residual being larger than 2 or 4. Neither of these robustness checks led to changes in our basic results.

[40] Excluding the elections in Augsburg in 2002, Würzburg in 2002 and 2008, Regensburg in 1990, and Ottobrunn in 1989 and 2001 causes population and population density to turn insignificant (see figure 9), but does not change any of our basic results.

At the candidate level, if the political decision only concerns partisanship, a similar vote share guarantees that these male and female candidates are equal, on average, except for their sex. If men and women also differ with respect to other characteristics that are unobserved by us but are observed by the electorate, the election may concern other aspects as well, such as ability; thus, a similar vote share would no longer signal similar candidates. We are therefore interested in comparing codetermined variables at the candidate level to examine the extent to which there are gender differences between individuals near the threshold, and we could then attempt to control for the differences that might remain. Table 7 provides estimates for jumps in candidate personal characteristics at the cutoff.[41]

The estimates presented in table 7 in section A.1 below show that narrowly elected women are not markedly different from men. None of the tested characteristics is robustly significant across several regression discontinuity specifications. Potentially most important for our research question, women are equally likely to hold a college degree. OLS and FE estimations indicate that women are significantly less likely to be incumbents, but according to the RDD specifications, this does not seem to hold to the same extent in the case of close elections. This was to be expected, given that women are in the process of entering local politics. The same is true for election experience, of which women have less on average and even less with respect to close elections. Moreover, we note that women seem slightly more likely to be nominated by the SPD, although this relationship is significant only for OLS, FE and for one RDD specification. Note that OLS and FE estimates indicate that women are less likely to be nominated by the CSU, but the RDD specifications' t-values for CSU nomination are close to zero. Given these results, we reestimated all our specifications with added controls for all the codetermined covariates that were significant in at least one of our RDD specifications: "Social Democrat Nomination", "Incumbent", and "Number of Elections participated in". These alternative specifications did not yield qualitatively different results.[42]

# 6. Results

We test the effect of electing a female mayor on the size of the local government and the allocation of expenditures to policy areas that are presumed to be characterized as providing benefits particularly valued by women. Previous empirical studies have led to ambiguous results concerning the effects of gender, particularly with respect to expenditure composition (Svaleryd, 2009; Ferreira and Gyourko, 2011; Rigon and

[41] Figure 10 in section A.2 shows the corresponding graphical representations.

[42] As another robustness check, we also excluded all SPD-nominated candidates from our estimations, which does not lead to many cases being lost because the share of SPD-nominated candidates is small. This, too, did not change the results.

Tanzi, 2012). We also wish to empirically test the claim made by Ferreira and Gyourko (2011) that women tend to remain in office longer because, once elected, their true ability emerges that enabled them to be elected even when the electorate was gender-biased.

As discussed in section 4, regression discontinuity designs only provide estimates of local average treatment effects. The causal interpretation is only valid with respect to municipal elections with close outcomes. These are likely to be elections with an above average degree of political competition and / or a high degree of partisanship (controversial policy issues). Therefore, we complement the RDD specifications with standard multivariate regression estimates.

## 6.1. Budget Size

We first report our results for various government budgetary size indicators. Table 2 provides estimates of the effects on our budget indicators for the size of government: changes in per capita values of public debt, investment in fixed assets, operating expenditures, and total wages.

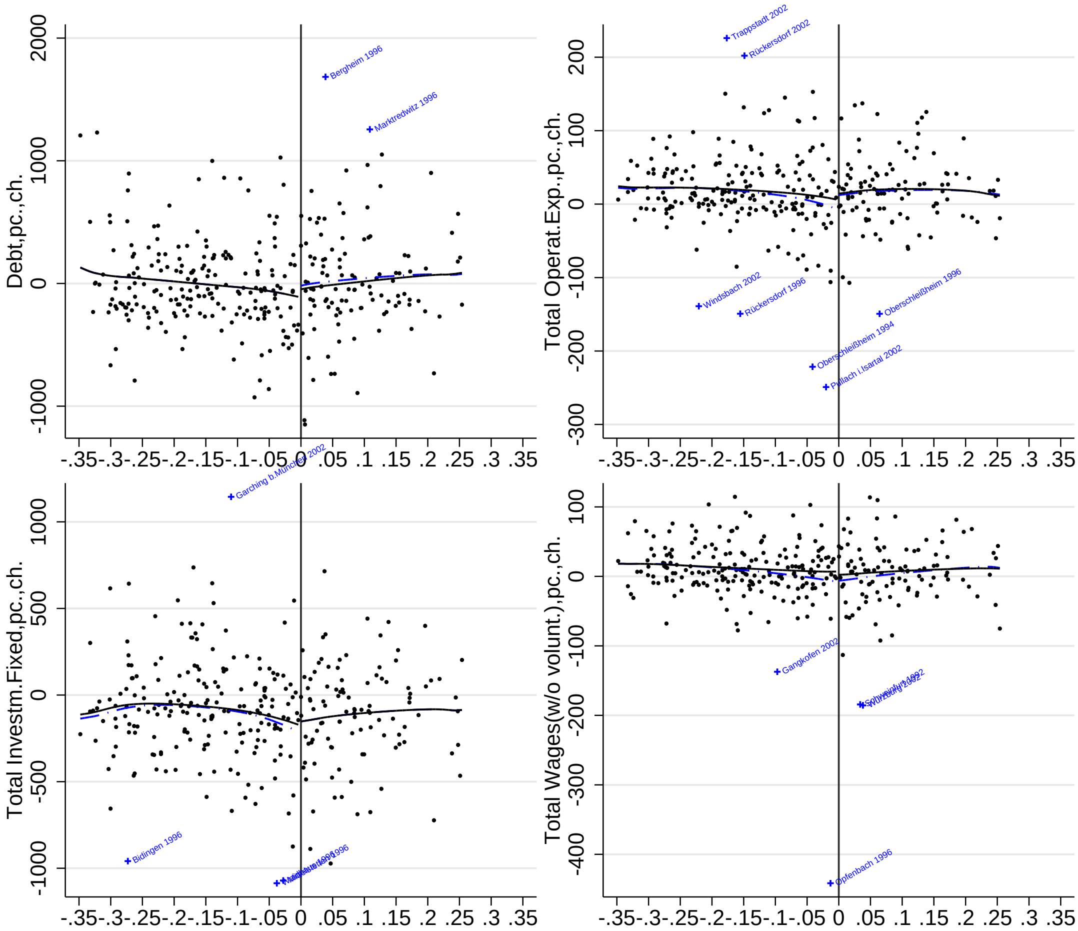
**Table 2:** Main Government Budget Size Indicators, Elections 1984-2009

| | | ATE | | | Param. RDD | | Local Lin. RDD | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | mean | OLS | FE | mean-RD | sqr | cub | bw0.1 | bw0.075 |
| Debt,p.c. | 705.8 | 28.54 | 46.71 | 709.8 | -18.54 | 60.96 | 4.900 | 36.79 |
| | | (0.83) | (0.95) | | (-0.13) | (0.33) | (0.03) | (0.21) |
| Operat. exp.,p.c. | 240.4 | 0.155 | -6.792 | 257.1 | 17.92 | 36.39 | 45.01 | 47.47 |
| | | (0.04) | (-1.12) | | (0.88) | (1.32) | (1.47) | (1.35) |
| Investm.,p.c. | 390.1 | 7.827 | 20.69 | 330.2 | 22.74 | 105.4 | -17.84 | -39.38 |
| | | (0.35) | (0.82) | | (0.22) | (0.75) | (-0.13) | (-0.25) |
| Total wages,p.c. | 196.0 | -3.363 | -2.253 | 217.3 | 7.228 | 27.83 | 23.38 | 25.85 |
| | | (-1.06) | (-0.49) | | (0.34) | (0.87) | (0.85) | (0.88) |
| Observations | 6832 | 6188 | 629 | 311 | 306 | 306 | 134 | 109 |
| Sample Window | | Full | Full | | Full | Full | +/- 10% | +/- 7.5% |

$t$ statistics in parentheses (clustered by individial municipal election). $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Specifications (1) to (6) report coefficients from estimations (specifications as indicated in the column heading) of the effect of electing a female mayor on the variable denoted in the leftmost column. For the OLS and RDD specifications, the dependent variable is the absolute change from the average deflated per capita value in the three years preceding the election (typically years 4 to 6 of the preceeding six-year term) to the average deflated per capita value of (typically) years 4 to 6 of the following six-year term (year 1 being defined as the election year). FE specifications refer to municipal-fixed effects, and the dependent variable is the average deflated per capita value of years 4 to 6 of the following six-year term. All specifications include year-fixed effects and population in $t-1$ of the election year (population in the last year of the preceding election term) and population in $t-1$ squared.

**Figure 3:** Main Government Budget Size Indicators, 1984-2009




The OLS estimates provide information about the partial correlation between electing a female mayor and our dependent variable that is conditional on a set of time-varying structural control variables.[43] The fixed-effects specification removes bias in the coefficient estimates because of unobservable constant municipal characteristics - such as the share of variation in preferences for public goods - that are constant over time.[44]

RD specifications (5), (6), (7), and (8) are restricted to cases with exactly two candidates, one of whom is a woman. RDD specifications (5) and (6) use observations from the entire range of victory margins, controlling for the female margin of victory using second degree (5) or third degree (6) polynomials of this margin (the

[43] As discussed in section 4, because we assume that there are other relevant covariates that we do not observe and cannot control for, and that some of these covariates may be correlated with both the gender of the mayor and the dependent variable, we generally expect OLS estimates to be biased, i.e., not to measure the true causal effect.

[44] The fixed-effects specification does not ensure that unobserved, time variant variables that are correlated with both the female vote margin and the dependent variable do not bias the coefficient estimates. For example, this could occur when preferences for public goods change within the time window examined here because of changes in the population composition due to suburbanization.

polynomials are allowed to take different functional forms to the left and right of the threshold). RDD specifications (7) and (8) only compare observations within a window of +/- 10% and +/- 7.5% to the left and right of the 50% victory cutoff by using local linear regressions, which ensures that under the assumptions stated in section 4, municipalities to the left and right of the threshold are equal, on average. Because rectangular kernels are relatively sensitive to observations at the border of the employed bandwidth region, a triangular kernel is used.

Table 2 shows that neither of the main budgetary indicators for local government size in Bavarian municipalities seems affected by electing a woman as mayor.

The estimates are insignificant with signs that vary across all specifications. A visual inspection of the RDD estimation is presented in figure 3, which plots the vote margin of the female candidate on the x-axis and the outcome - the absolute change in the respective dependent variable in deflated per capita terms - on the y-axis. To the left of the 0%-value on the x-axis ("election victory threshold"), the female candidate has a negative vote margin, indicating that the male candidate won. To the right of the threshold, the female candidate has won the majority of votes, thus all points to the right of the threshold indicate municipalities that (in varying years) have had a female mayor. If narrowly electing a woman instead of a man as mayor had a measurable and sufficiently strong causal effect on the size of the local government, we would see the points to the right of the threshold systematically shifted either upwards or downwards, of which the figures and the estimates presented estimates offer no indication. We can conclude that, five years later, for the sample of municipal elections studied here, electing a woman does not exert a significant effect on debt per capita, operating expenditures per capita, investment per capita, or total wages per capita.

Regarding outlier sensitivity, note that excluding outliers tends to further reduce elevated t-values. For example, considering operating expenditures, we note that the t-values are slightly elevated. However, our outlier analysis reveals that the t-values for operating expenditures are close to zero if reestimated without outliers.[45]

Regarding figure 3, we also note a slightly V-shaped form of the regression line in the upper left panel. Some elections are above the line of zero change in per capita debt, and some are below the line. The municipalities close to the threshold tend to be below the line, on average. This may be a sign of political competition, which is stronger close to the threshold, and makes politicians devote greater effort to reducing debt. However, the regression discontinuity design only ensures that municipalities close to the threshold are equal, on average, and here we are comparing municipalities close to the threshold to municipalities far from the threshold;

[45] Compare figure 3. Note that there are only two observations causing the slightly elevated t-values in the upper right panel: Pullach 2002 and Oberschleißheim 1994.

thus, we cannot attribute a causal interpretation to this finding.[46]

## 6.2. Tax Setting

Regarding the revenue side, we examine tax-setting decisions, which may be argued to be a more direct measure of the politicians' intent than actual revenues. Although it is not the mayor alone but the municipal council that sets municipal taxes, we argue that the mayor is likely to have above-average effective voting power in the council because he/she has exclusive executive powers elsewhere (see the brief discussion above). Table 3 shows that coefficient estimates of electing a woman as mayor are positive for all three municipal tax collection rates but insignificant. Only the average of all three local tax collection rates, as a measure of overall tax-rate setting, exhibits t-values above a 10% significance level - but only for the local linear regression specifications, i.e., for RDD specifications that do not attempt to use information from observations outside a certain bandwidth around the threshold.

**Table 3:** Local Tax Collection Rates, 1984-2009

| | | ATE | | | Param. RDD | | Local Lin. RDD | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | mean | OLS | FE | mean-RD | sqr | cub | bw0.1 | bw0.075 |
| Coll. rate business tax | 319.8 | -1.441 | 0.293 | 322.9 | 2.324 | 4.435 | 7.548 | 8.074 |
| | | (-1.62) | (0.19) | | (0.59) | (0.93) | (1.63) | (1.58) |
| Coll. rate prop. tax A | 319.9 | 2.500 | 3.803 | 313.5 | 3.078 | 9.602 | 12.39 | 13.35 |
| | | (1.45) | (1.57) | | (0.44) | (1.07) | (1.43) | (1.36) |
| Coll. rate prop. tax B | 314.2 | 2.044 | 3.424 | 313.7 | 3.750 | 11.95 | 13.76 | 14.19 |
| | | (1.20) | (1.44) | | (0.50) | (1.29) | (1.52) | (1.45) |
| Coll. rates (mean) | 318.0 | 1.034 | 2.507 | 316.7 | 3.051 | 8.662 | 11.23* | 11.87* |
| | | (0.83) | (1.34) | | (0.56) | (1.29) | (1.72) | (1.68) |
| Observations | 8549 | 6547 | 792 | 346 | 346 | 346 | 152 | 126 |
| Sample Window | | Full | Full | | Full | Full | +/- 10% | +/- 7.5% |

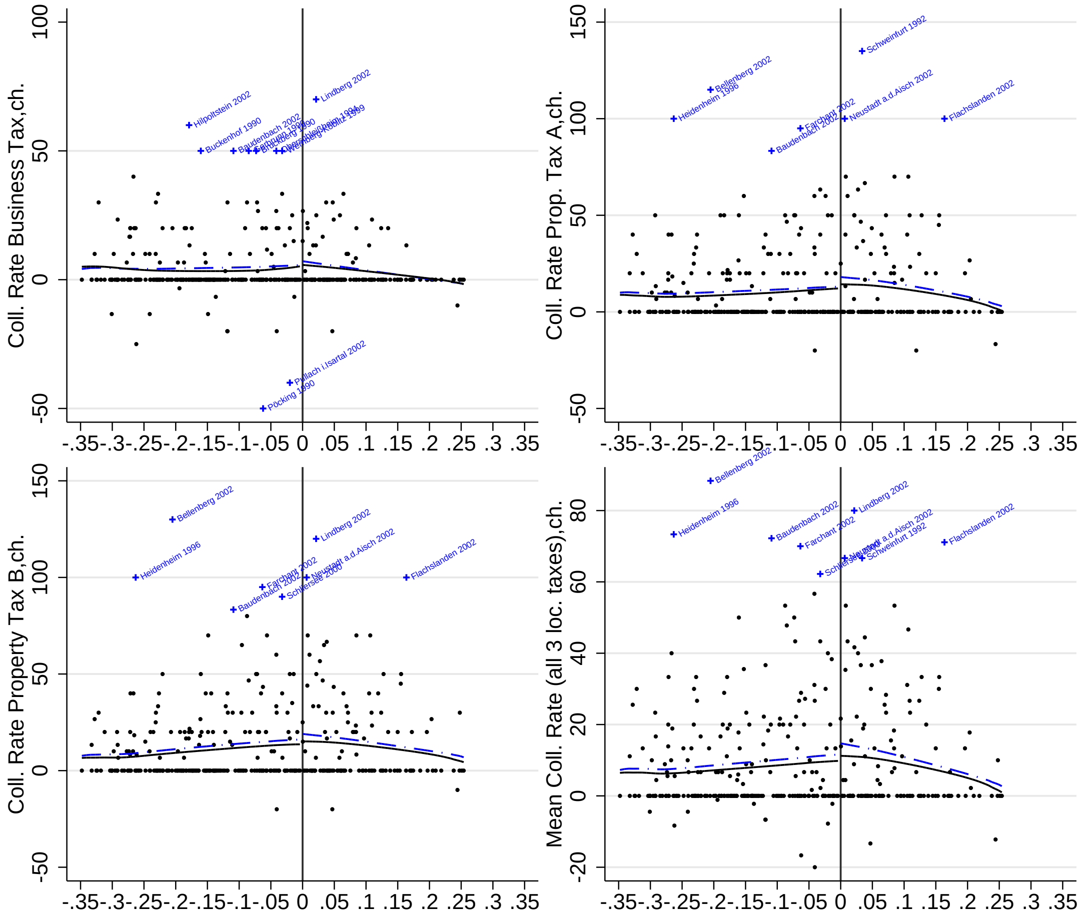
$t$ statistics in parentheses (clustered by individial municipal election). $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Figure 4 again presents a graphic representation of these estimates. We observe a small jump in the dependent variable at the threshold; however, the figure also reveals that tax collection rates remain unchanged for most of the observations. This indicates that our dependent variable is not normally distributed, which is likely to cause the reported standard error to be unreliable. Consequently, although these estimates may provide some indication that women mayors tend to raise local tax collection rates compared to levels set by male mayors, we do not wish to overemphasize this result.[47]

[46] For example, municipalities with higher political competition may also have higher population growth, which could explain part of the difference in per capita debt reduction between municipalities nearer and farther from the threshold.

[47] Note in Figure 4 that coefficients become smaller once we exclude outliers, as defined above.

**Figure 4:** Local Tax Collection Rates, 1984-2009




## 6.3. Expenditure Allocation

We also wish to determine whether electing a woman has any effect on the allocation of expenditures. To do so, we must define which policy areas we should classify as "female", based on results from the literature on gender-specific preferences for public goods. We refer to Edlund et al. (2005), who conclude that women do not prefer redistribution per se but have a relative preference for policies that benefit children and single mothers, and to Svaleryd (2009), who finds that women prefer expenditures on childcare and education to elder care expenditures (see section 2). Given the institutional characteristics of Bavarian jurisdictions described in section 2, we suggest that this is best captured by examining expenditures for schools, parks and recreation (essentially the health budget without hospitals), daycare (a part of the social budget), and expenditures on family and youth services (likewise a part of the social budget).

Because our research question ultimately concerns the dichotomy between female and non-female public goods, we also test sums of these (4) categories, which means that we add all presumed female expenditure types together in different combina-

**Table 4:** Sum of Operating Expenditures, Transfers and Wages, by Policy Field, 1992-2009

| | | ATE | | | Param. RDD | | Local Lin. RDD | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | mean | OLS | FE | mean-RD | sqr | cub | bw0.1 | bw0.075 |
| 2-Schools | 79.66 | -1.512 | -1.358 | 78.57 | -2.826 | -2.881 | 0.257 | 1.467 |
| | | (-1.24) | (-0.64) | | (-0.52) | (-0.42) | (0.04) | (0.21) |
| 4-Youth & Fam. | 4.433 | 0.319 | -0.435 | 6.266 | 1.214 | 1.702 | 0.470 | 0.651 |
| | | (1.08) | (-0.72) | | (0.95) | (0.97) | (0.31) | (0.37) |
| 4-Daycare | 73.84 | -0.943 | -4.012 | 78.19 | -9.774 | -9.883 | -12.99 | -15.28 |
| | | (-0.57) | (-1.53) | | (-1.21) | (-0.99) | (-1.33) | (-1.44) |
| 5-Parks & Recr. | 16.56 | -2.330 | 0.764 | 22.57 | -2.822 | -3.608 | -5.661 | -7.706 |
| | | (-1.27) | (0.74) | | (-0.82) | (-0.82) | (-1.10) | (-1.25) |
| Observations | 4821 | 4630 | 445 | 264 | 251 | 251 | 111 | 89 |
| Sample Window | | Full | Full | | Full | Full | +/- 10% | +/- 7.5% |

$t$ statistics in parentheses (clustered by individial municipal election). $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

tions. For some subcategories of the 10 main policy fields, the *Haushaltssystematik* changed profoundly in 1992. Therefore, we must exclude the years 1987 – 1991, which causes the number of cases to shrink somewhat. Table 4 shows that none of the four proposed "female" categories show a significant coefficient. Figure 5 presents the regression discontinuity estimates graphically.

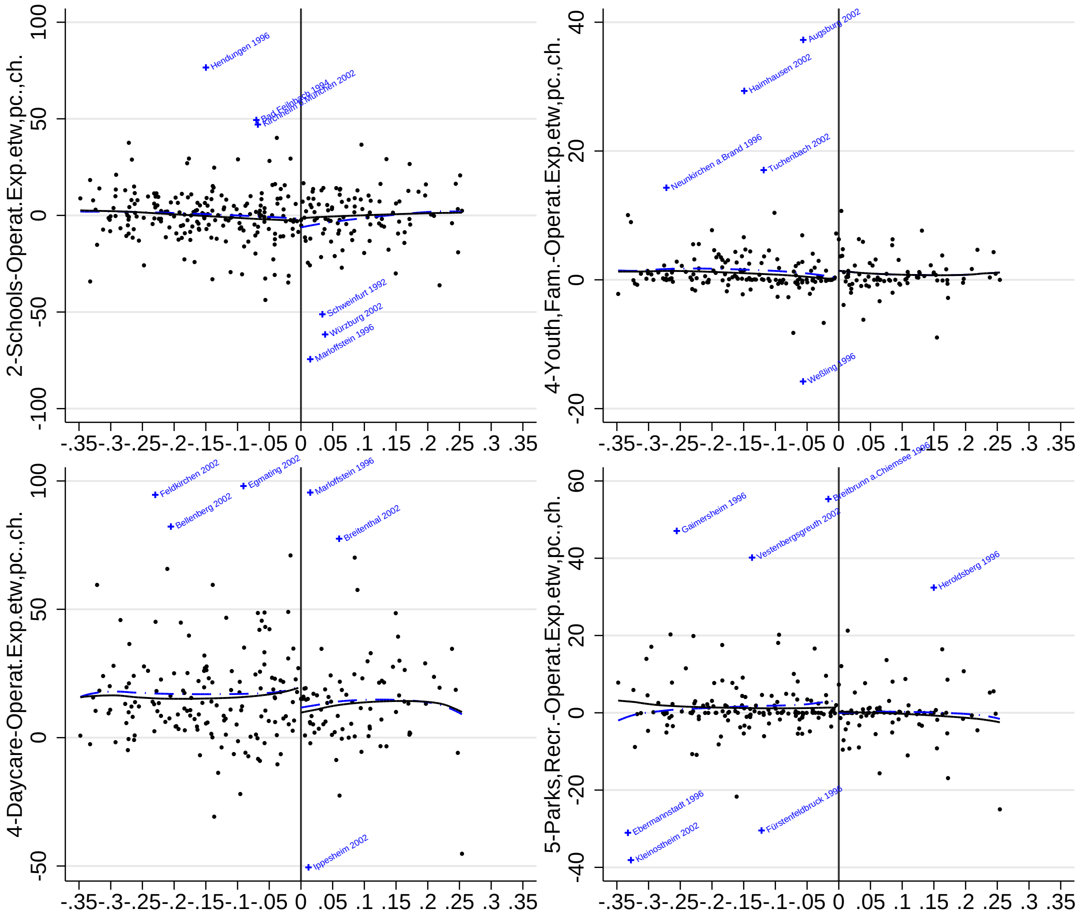
First, note that the shares of the policy fields in total operating expenditures, transfers, and wages are nearly constant for schools, youth and family services, and parks and recreation facilities, but the share of daycare shows a marked increase, on average. However, there is no evidence that the changes in policy area shares depend on the sex of the mayor. Relative to men, women do not seem to increase expenditures on schools, daycare, youth and family services, or parks and recreation facilities. For daycare, it may be noteworthy that the coefficients are negative although the t-values are only slightly elevated and insignificant.[48] Table 5 in section A.1 indicates that this also holds for all possible sums of these four categories.

## 6.4. Incumbent Effects by Gender

An incumbent effect describes the phenomenon that, on average, the candidate who won the previous election (the incumbent) has a much higher likelihood of winning the next election than a candidate who has not won the previous election (the challenger). This effect can be explained by voters' uncertainty regarding candidate quality: an incumbent, compared to any losing candidate, has the relative advantage of having one election period to signal ability by supplying effort (Lee, 2008). Ferreira and Gyourko (2011) find that the incumbent effect is significantly stronger for women incumbents than for male incumbents, as measured by the (unconditional)

[48]Excluding outliers slightly increases t-values but does not change significance levels. Note that in the lower left panel of figure 5, the outliers of Ippersheim 2002 and Marloffstein 1996 almost cancel one another out.

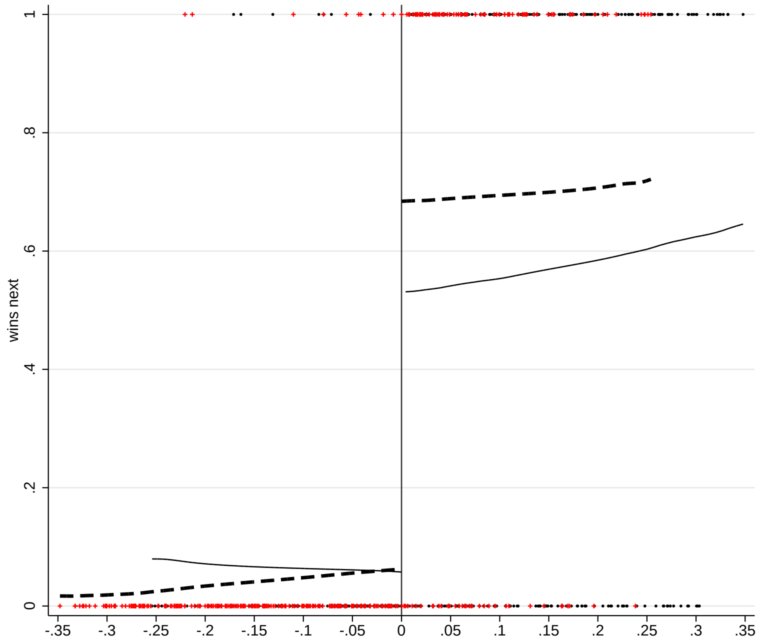
**Figure 5:** Sum of Operating Expenditures, Transfers and Wages, by Policy Fields, 1992-2009




probability of winning the following election. They essentially argue that the most plausible explanation for this is that women, before being first elected, face an electorate that is gender-biased, i.e., prejudiced in its judgment of new, unknown female candidates as opposed to male candidates. Once in office, the difference between the expected and true quality of the candidate is thus relatively large for women: women must be better than men to be elected in the first place, making a narrowly elected female candidate better than a narrowly elected male candidate, which leads to higher reelection rates for women (Ferreira and Gyourko, 2011).

We can replicate the same effect in our data as in Ferreira and Gyourko (2011): figure 6 shows that the likelihood of winning the following election changes with the vote share, by gender, as depicted in Ferreira and Gyourko (2011). The red dots represent female candidates and the black dots male candidates. Note that this figure differs from the previous regression discontinuity graphs: while the previous graphs had the female candidates' vote shares on the x-axis, the incumbency effects graphs place all candidates on the x-axis in the same direction, meaning candidates with a low vote share are to the left and candidates with a high share are to the right.

**Figure 6:** Incumbent Effects by Gender (I): Likelihood of Serving Another Term



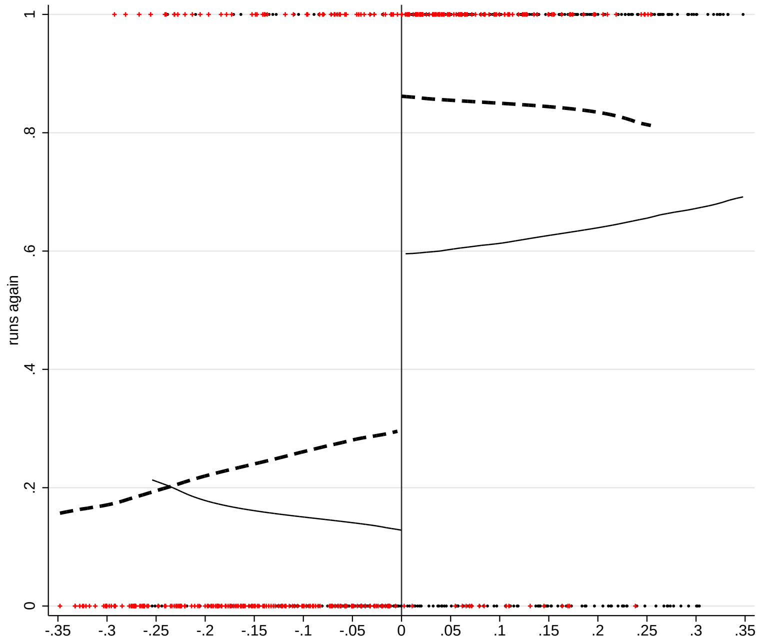

The solid line represents the smoothed average that at the threshold estimates the discontinuity for men, whereas the broken line is the smoothed average for women. The jumps at the threshold correspond to the regression discontinuity estimates of the effect that being elected before has on the likelihood of winning the subsequent election. Essentially, we observe the same effect in our data as in Ferreira and Gyourko (2011): first, we observe large and highly significant jumps in the odds of winning the subsequent election at the threshold for both men and women.[49] Second, the jump at the threshold of the likelihood of serving another term is significantly larger for women.[50]

We attempt to disentangle this effect further by examining the likelihood of running for reelection by gender. Figure 7 depicts how winning an election affects the likelihood of running again in the next election by gender. The picture reveals that women are also much more likely to run again than men. This is true for both first- and second-place candidates. Ultimately, these differences may explain some of the higher likelihood of women to serve another term. Indeed, if we relate the odds of recandidacy by gender and those of reelection by gender, we note that narrowly elected men decide to run again approximately 60% of the time, while they serve another term approximately 52% of the time, leading to a success rate conditional

[49]In spite of the small number of cases left of the threshold, particularly for men, the incumbent effects are significant for both men and women separately, and also hold for alternative bandwidths including the 10% and 7.5% bandwidths used above.

[50]The difference between both lines on the right side amounts to about 15 percentage points (the difference is significant at the 1%-level). The corresponding estimate in Ferreira and Gyourko (2011) amounts to 10 percentage points.

**Figure 7:** Incumbent Effects by Gender (II): Likelihood of Running Again




on recandidacy of almost 90%. Women, by contrast, decide to run again approximately 85% of the time, while they serve another term approximately 67% of the time, yielding a success rate conditional on recandidacy of approximately 78%.

Before we draw further conclusions, it may also be instructive to determine whether women actually fare worse in the subsequent election in terms of vote share, which we should expect given the calculations above.

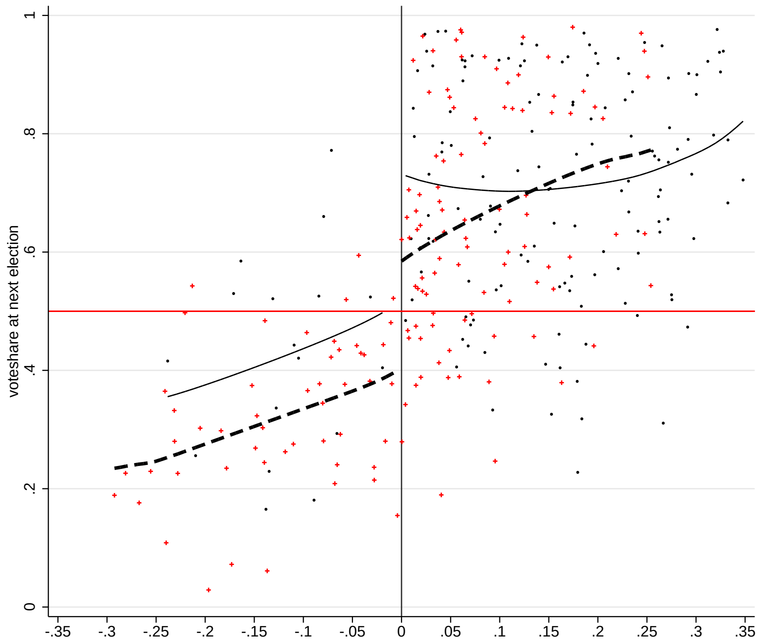
Figure 8 confirms that this is indeed the case: the graph is similar to figure 6, except that now, instead of reelection likelihood, the y-axis depicts the vote share in the next election. Women who have narrowly won an election, compared to their male counterparts, have an electoral disadvantage in the subsequent election of 15.4% of the entire vote.[51]

With respect to the likelihood of serving another term, our results are consistent with the findings of Ferreira and Gyourko (2011). However, our results are more differentiated because they divide the effect into two effects: we show that women have a higher likelihood of self-selecting into (re-)candidacy, but we also show that women face a significant electoral disadvantage when running again, and this latter effect seems to be outweighed by the former effect.

How should this finding be interpreted? We suggest two possible explanations: first, the process of turning undesirable incumbents out of office after their first term, when voters have the first opportunity to do so, may affect women more than men because the share of first-time incumbents is larger among female incumbents

[51]This is the result of a t-test of the differences in means between men and women who are within a bandwidth of 10% to the right of the cutoff. This difference is significant at the 1%-level.

**Figure 8:** Incumbent Effects by Gender (III): Vote Share in the Following Election




than among male incumbents.[52]

Second, women may have lower opportunity costs of political candidacy than men, which may cause the quality of the average female re-runner to be lower than average quality of male re-runners, which will cause lower vote shares of female re-runners.[53]

# 7. Conclusion

We find that electing a female mayor does not have a measurable effect on the size of the government budget, tax setting, or on the allocation of expenditures. This result is robust across a variety of different specifications that all include time-fixed effects and consist of OLS with structural control variables, municipal fixed-effects estimations and different forms of regression discontinuity specifications. This result is surprising given the survey evidence on gender differences in preferences for public goods and the strong de jure position that Bavarian mayors enjoy. However, the result is to be expected if, at the local policy level, gender partisanship is weak and/or political competition is strong.

We also examined differences in incumbency effects by gender. Here, we confirm that women have a significantly higher likelihood of being elected again in the next

[52] Our validity tests in section 5 reveal that narrowly elected women may have slightly less election participation experience than narrowly elected men, although this result is not robust across different RDD specifications.

[53] Opportunity costs could differ, for example, because of different earnings opportunities in the labor market (that need not be fully captured by "college education") or because women receive different social rewards for running for office.

election, as previously found by Ferreira and Gyourko (2011). However, our results do not support the interpretation that Ferreira and Gyourko (2011) provide for this effect; instead, our results show that this effect is caused by the higher propensity of women to self-select into recandidacy. Moreover, if we account for the fact that women are more likely to run again, women's chances in the subsequent election are actually significantly worse than those of men. We suggested two possible explanations for this latter effect: a relatively larger share of first-time re-runners among female re-runners and different opportunity costs of re-candidacy.

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# A. Appendix

## A.1. Tables

**Table 5:** Sum of Operating Expenditures, Transfers and Wages, by Different Combinations of "Female" Policy Fields, Elections 1988-2009

| | | ATE | | | Param. RDD | | Local Lin. RDD | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | mean | OLS | FE | mean-RD | sqr | cub | bw0.1 | bw0.075 |
| Schools,YthFam,Dayc,Parks | 174.8 | -4.824* | -4.297 | 185.5 | -15.65 | -12.96 | -18.19 | -20.76 |
| | | (-1.74) | (-1.23) | | (-1.50) | (-0.98) | (-1.29) | (-1.27) |
| Schools,YthFam,Dayc | 158.3 | -2.494 | -5.061 | 163.0 | -12.82 | -9.349 | -12.53 | -13.05 |
| | | (-1.26) | (-1.54) | | (-1.50) | (-0.88) | (-1.20) | (-1.11) |
| School,YthFam,Parks | 101.0 | -3.881* | -0.285 | 107.4 | -5.872 | -3.074 | -5.202 | -5.474 |
| | | (-1.65) | (-0.12) | | (-0.75) | (-0.30) | (-0.49) | (-0.46) |
| School,Dayc,Parks | 170.4 | -5.144* | -3.862 | 179.3 | -16.86* | -14.66 | -18.66 | -21.41 |
| | | (-1.91) | (-1.15) | | (-1.66) | (-1.15) | (-1.34) | (-1.31) |
| School,YthFam | 84.43 | -1.551 | -1.049 | 84.79 | -3.050 | 0.534 | 0.459 | 2.232 |
| | | (-1.10) | (-0.49) | | (-0.48) | (0.06) | (0.06) | (0.28) |
| School,Dayc | 153.8 | -2.813 | -4.626 | 156.7 | -14.04* | -11.05 | -13.00 | -13.70 |
| | | (-1.46) | (-1.46) | | (-1.71) | (-1.09) | (-1.26) | (-1.18) |
| School,Parks | 96.56 | -4.200* | 0.150 | 101.1 | -7.086 | -4.776 | -5.672 | -6.125 |
| | | (-1.84) | (0.07) | | (-0.94) | (-0.48) | (-0.55) | (-0.52) |
| YthFam,Dayc,Parks | 94.83 | -2.954 | -3.683 | 107.0 | -11.38 | -11.79 | -18.18 | -22.34 |
| | | (-1.15) | (-1.23) | | (-1.16) | (-0.97) | (-1.44) | (-1.57) |
| YthFam,Dayc | 78.27 | -0.624 | -4.447 | 84.46 | -8.560 | -8.180 | -12.52 | -14.63 |
| | | (-0.36) | (-1.61) | | (-1.04) | (-0.81) | (-1.28) | (-1.38) |
| YthFam,Parks | 20.99 | -2.011 | 0.329 | 28.83 | -1.607 | -1.906 | -5.190 | -7.055 |
| | | (-1.07) | (0.27) | | (-0.42) | (-0.39) | (-0.95) | (-1.10) |
| Dayc,Parks | 90.40 | -3.274 | -3.248 | 100.8 | -12.60 | -13.49 | -18.65 | -22.99 |
| | | (-1.30) | (-1.13) | | (-1.31) | (-1.12) | (-1.49) | (-1.62) |
| Observations | 4308 | 4097 | 400 | 249 | 234 | 234 | 104 | 83 |
| Sample Window | | Full | Full | | Full | Full | +/- 10% | +/- 7.5% |

$t$ statistics in parentheses (clustered by individial municipal election). $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Table 6:** Tests for Jumps in Predetermined Municipal Characteristics, 1984-2003

| | | ATE | | | Parametric RDD | | Local Lin. RDD | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | mean | OLS | FE | mean-RD | sqr | cub | bw0.1 | bw0.05 |
| Pop. t-1 | 5615.6 | 1.29e-13 | -4.32e-14** | 7022.0 | -2.44e-12 | 5.42e-12 | 3.14e-12* | 2.50e-11*** |
| | | (1.03) | (-2.18) | | (-0.91) | (1.25) | (1.85) | (2.96) |
| Pop. Dens. | 169.0 | 88.97*** | 1.682 | 295.4 | 144.0 | 367.4* | 234.1 | 36.94 |
| | | (2.61) | (0.83) | | (1.00) | (1.91) | (1.19) | (0.19) |
| Share 6 to 15 | 11.03 | -0.00963 | 0.224* | 10.78 | 0.506 | 0.0547 | -0.0315 | 0.861 |
| | | (-0.31) | (1.76) | | (1.14) | (0.09) | (-0.06) | (0.77) |
| Share over 65 | 14.48 | 0.0380 | 0.00892 | 14.47 | -0.960 | -0.416 | -0.496 | -0.743 |
| | | (1.35) | (0.06) | | (-1.08) | (-0.38) | (-0.42) | (-0.40) |
| CSU shr.counc. | 23.97 | -1.741 | 0.0990 | 26.26 | 1.776 | -0.800 | 0.204 | -4.105 |
| | | (-1.16) | (0.07) | | (0.27) | (-0.09) | (0.03) | (-0.40) |
| SPD shr.counc. | 12.21 | -1.645 | 0.472 | 14.43 | -0.955 | -0.933 | -1.134 | -4.640 |
| | | (-1.57) | (0.53) | | (-0.24) | (-0.18) | (-0.24) | (-0.86) |
| Observations | 7681 | 6348 | 707 | 316 | 316 | 316 | 243 | 130 |
| Sample Window | | Full | Full | | Full | Full | +/- 10% | +/- 7.5% |

$t$ statistics in parentheses (clustered by individial municipal election). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 7:** Tests for Jumps in Codetermined Candidate Characteristics, 1984-2003

| | | ATE | | | Parametric RDD | | Local Lin. RDD | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | mean | OLS | FE | mean-RD | sqr | cub | bw0.1 | bw0.05 |
| Cons.Party | 0.476 | -0.133*** | -0.220*** | 0.427 | -0.0634 | -0.0795 | -0.0346 | -0.199 |
| | | (-3.67) | (-3.19) | | (-0.41) | (-0.37) | (-0.17) | (-0.64) |
| Soc.Dem. | 0.144 | 0.0734** | 0.0998* | 0.207 | 0.202 | 0.233 | 0.304** | 0.192 |
| | | (2.32) | (1.89) | | (1.55) | (1.40) | (2.19) | (0.89) |
| Free Voters | 0.268 | -0.00545 | 0.0613 | 0.241 | -0.0877 | 0.0465 | -0.00549 | 0.138 |
| | | (-0.17) | (1.45) | | (-0.70) | (0.29) | (-0.04) | (0.54) |
| College Degr. | 0.447 | -0.0222 | 0.0542 | 0.533 | 0.00138 | -0.0945 | -0.135 | -0.0881 |
| | | (-0.49) | (0.64) | | (0.01) | (-0.42) | (-0.62) | (-0.22) |
| Inc. reelected | 0.663 | -0.189*** | -0.469*** | 0.489 | -0.306** | -0.260 | -0.230 | -0.102 |
| | | (-4.89) | (-8.88) | | (-2.43) | (-1.50) | (-1.42) | (-0.38) |
| Elect. partic. | 1.480 | -0.615*** | -1.281*** | 1.108 | -0.791* | -0.790 | -0.630 | 0.282 |
| | | (-8.25) | (-8.75) | | (-1.87) | (-1.28) | (-1.10) | (0.34) |
| Observations | 8010 | 6101 | 735 | 307 | 307 | 307 | 234 | 126 |
| Sample Window | | Full | Full | | Full | Full | +/- 10% | +/- 7.5% |

$t$ statistics in parentheses (clustered by individial municipal election). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

## A.2. Figures

**Figure 9:** Tests for Jumps in Predetermined Municipal Characteristicss

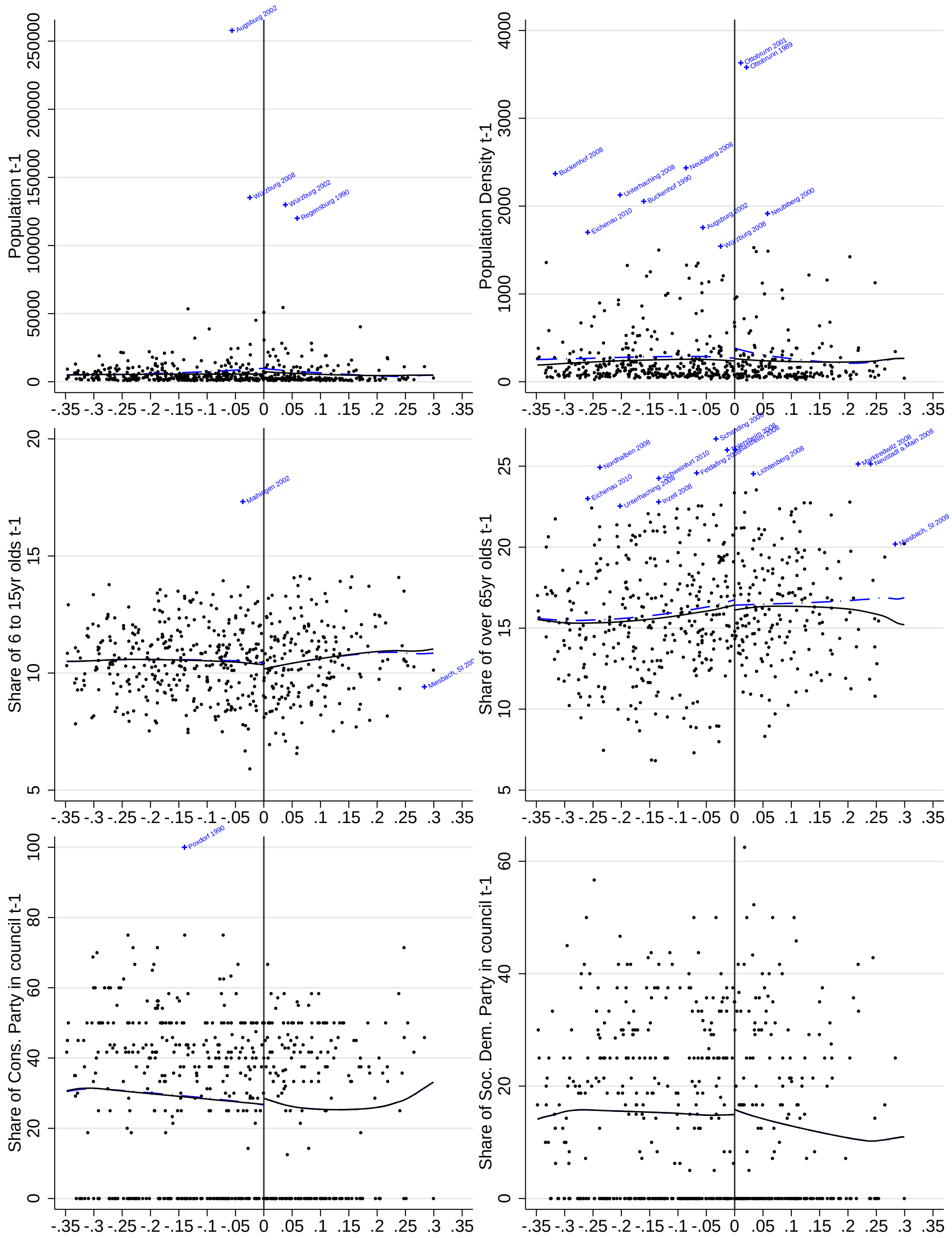


**Figure 10:** Tests for Jumps in Codetermined Candidate Characteristics

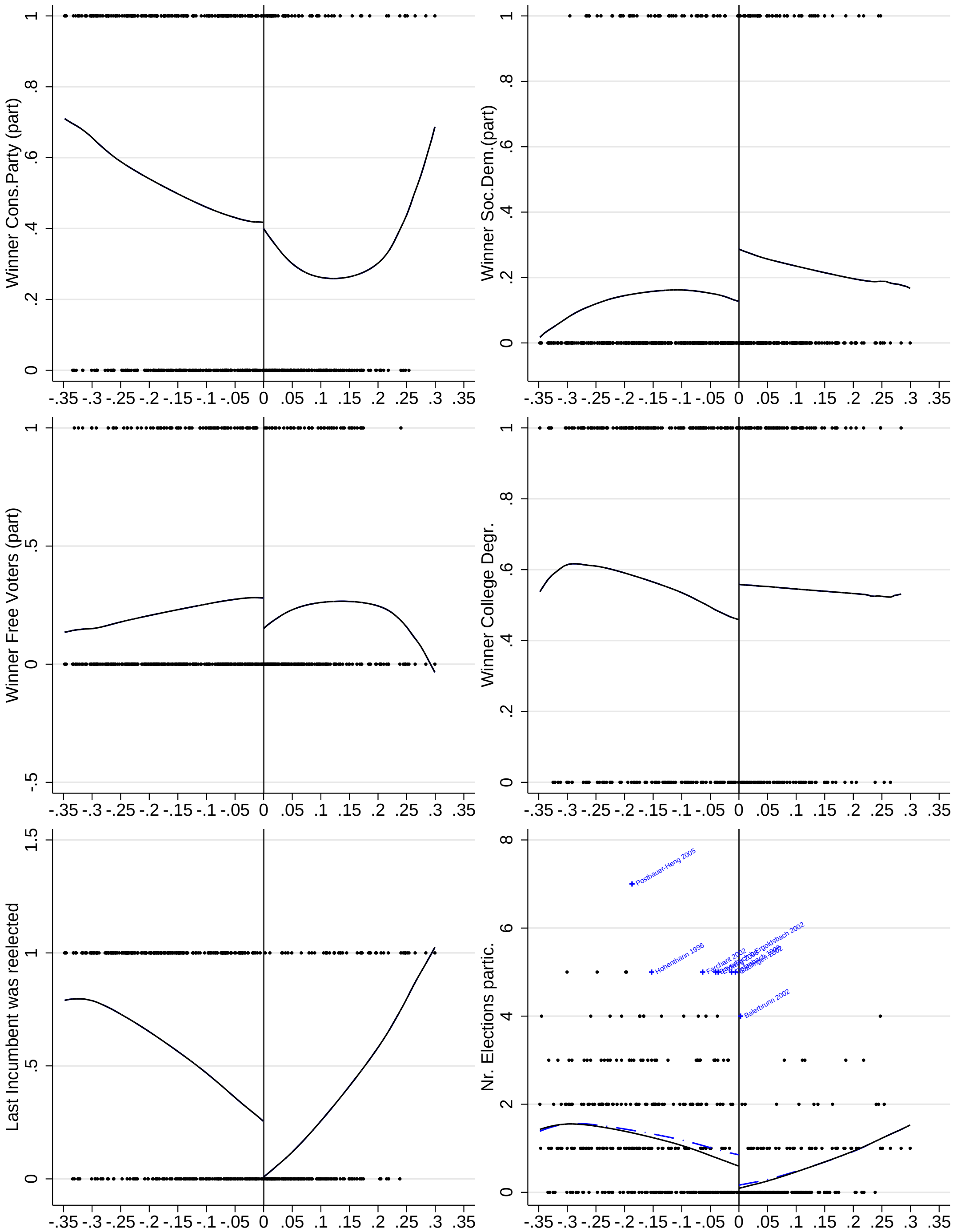
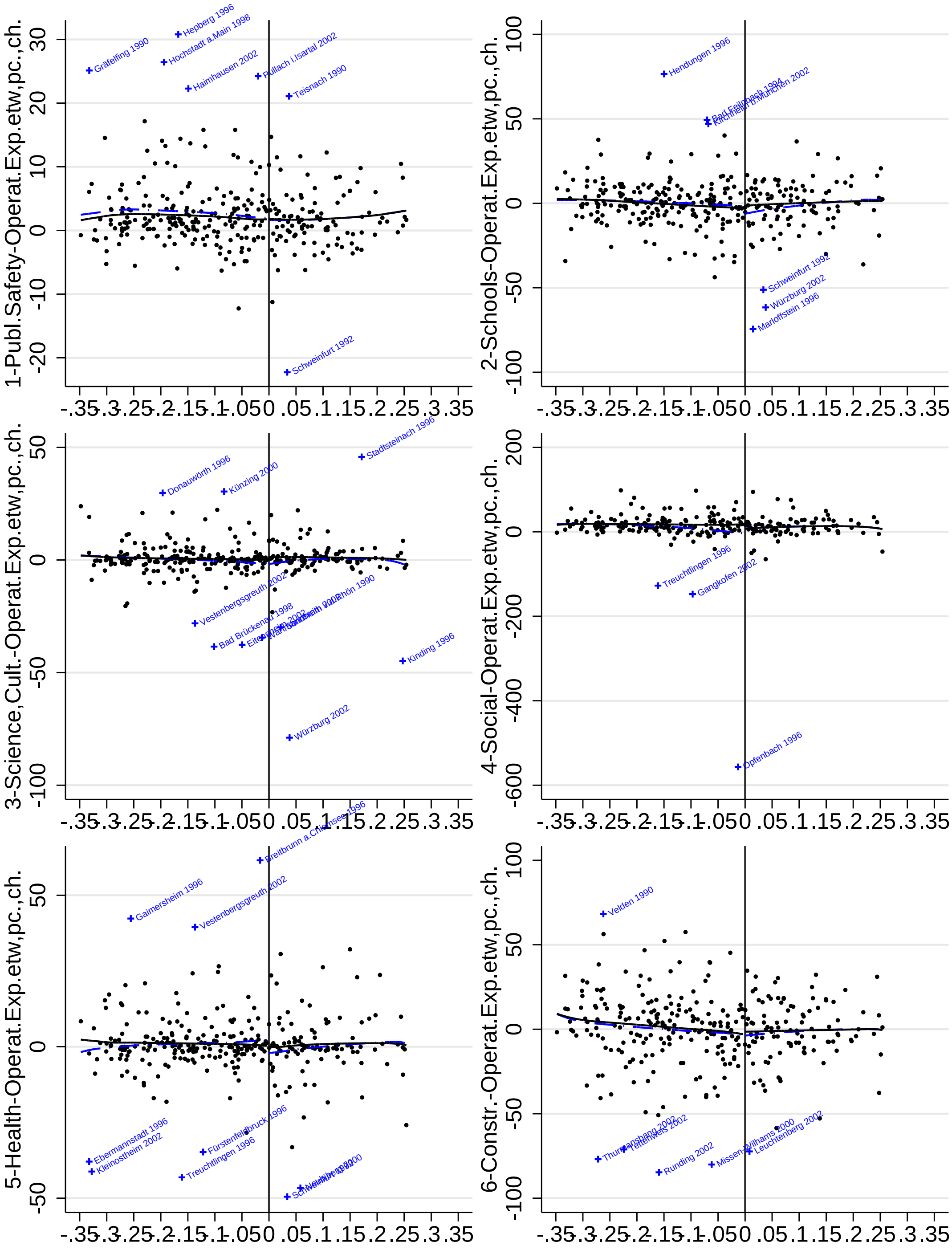


**Figure 11:** Operating Expenditures, Transfers and Wages, by Policy Fields, 1988-2009



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