Appointed public officials and local favoritism:

Evidence from the German States

Thushyanthan Baskaran[†*]   Mariana Lopes da Fonseca[††]

[†]University of Siegen

[††]MPI Tax Law and Public Finance

Abstract

We study local favoritism by appointed government officials in Germany using hand-collected data on members of state cabinets in the West-German states. Relying on a sample of more than 8,000 West-German municipalities during the period 1994–2013, we find that the home municipalities of state ministers experience higher employment growth rates than control municipalities. We show further, that employment growth is driven partly by higher state public employment. Given the institutional context, this effect is driven by home bias rather than electoral considerations. This finding shows that favoritism influences the behavior of politicians even in established democracies.

Keywords: Distributive politics, Favoritism, Employment growth

JEL codes: D73, H70

[*]Corresponding author: Thushyanthan Baskaran, Department of Economics, University of Siegen, Unteres Schloß 3, 57072 Siegen, Germany. Tel: +49(0)-271-740-3642. Email: baskaran@vwl.uni-siegen.de.

"Politicians are the same everywhere."

Nikita Khrushchev

1 Introduction

One of the perks of holding office is the opportunity to influence policy (Wittman, 1983). Policy choices, in turn, involve the allocation of public resources. Politicians may use this opportunity to allocate resources to promote their parochial interests; the political economy literature identifies two primary motivations for this behavior: re-election concerns and outright favoritism.

In general, empirical research on established democracies tends to focus on the rational political calculus of officeholders while research on autocracies emphasizes patronage. Two reasons for this divergence in focus may reside in the institutional context and the degree of electoral competition. The quality of institutions may prevent favoritism in democracies but not in autocracies, and electoral competition may enforce the targeting of politically decisive groups in democracies but be too muted to motivate distributive politics in dictatorships.

Yet, is favoritism really absent in democratic systems? If a hometown or group bias is a persistent cognitive trait of politicians across space and time, i.e., if politicians are indeed the same everywhere, they may always engage in favoritism irrespective of whether they hold office in a democratic or autocratic country. We study this question using data from Germany, a well established democratic country with strong institutions. Specifically, we explore whether German state ministers dispense benefits, in the form of jobs, to residents of their hometowns for non-electoral reasons. To this end, we combine data on employment growth over the period 1994–2013 with

hand-collected data on the home municipality of state ministers for all (non-city) states in Western Germany.

Our identification strategy takes advantage of the fact that German state ministers are appointed and thus face no direct electoral incentives – which makes this an ideal context to study favoritism –, and that the treatment assignment mechanism, i.e., ministerial appointments, and timing are exogenous from the perspective of the municipalities. To check our identifying assumptions, we examine the spatial reach of the treatment effect and test for reverse causality.

We find that treated municipalities experience a 0.4–0.5% significantly higher growth rate of employment than control. For a municipality of 10,000 inhabitants, this represents a disproportionate yearly increase in employment of about 40–50 employees. The timing of ministerial appointments coincides with the rise in the growth rate of employment, which is stronger for municipalities that have been the long-term residence of ministers. These findings suggest that state ministers facilitate hiring opportunities for co-residents, which we interpret as evidence of favoritism.

Consistent with the lack of electoral incentives we find that the effect is targeted at the municipal level and persistent over time as we do not see significant treatment effects on neighboring jurisdictions and no decline in employment upon the dismissal of ministers. We also show that the effect is more pronounced for prime ministers and ministers responsible for portfolios with large budgets, but standard indicators of pork, such as intergovernmental transfers do not increase to treated municipalities. Finally, in line with the claim that politicians are the same everywhere, we find no differences across gender or party affiliation; hometown favoritism is rife across state cabinet members.

In sum, this article makes three main contributions. First, our findings add to previous empirical research by Hodler and Raschky (2014) showing that regional favoritism is widespread even if more prevalent in countries with weak institutions. Similarly, Franck and Rainer (2012), and Kramon and Posner (2016) show, in contrast to Burgess et al. (2015), that ethnic favoritism is present both in autocratic and democratic settings. According to our results, public officials can dispense favors to co-residents even in contexts with strong democratic institutions. Unlike in autocracies, however, they may refrain from highly visible forms of favoritism opting instead for more subtle means as the mediation of employment opportunities.

Second, to our knowledge, this is the first article that can identify hometown favoritism as the motivation for a regional bias in an advanced democracy. Many studies observe similar distortions but interpret these in light of the standard neoclassical model of politics as transfers directed to a subset of the electorate that is decisive for re-election purposes (Weingast et al., 1981). Closest to our study are the articles by Fiva and Halse (2016) and Carozzi and Repetto (2016). Fiva and Halse (2016) identify a hometown bias in public road construction by regional governments in Norway elected within an at-large proportional representation (PR) system of closed lists, while Carozzi and Repetto (2016) find a birth town bias in the allocation of central government transfers driven by Italian members of parliament (MPs) whose birthplace does not belong to their electoral district. Despite the fact that re-election concerns are unlikely to be driving these regional biases, both studies associate the effect with political considerations. Specifically, they argue that politicians either came from or aim to, in the future, pursue a career in local politics. The fundamental differences in our setting are that state ministers in Germany are not elected but appointed, which

mutes any electoral concerns, and have no interest in pursuing a career in local politics after their tenure in the cabinet.

Finally, we also contribute to the literature on appointed versus elected officials and on the determinants of regional economic development. We show that appointed politicians may also cause economic distortions despite the absence of electoral concerns, whereas most of the literature on the topic focuses on the distorting incentives of elections (Rogoff, 1990). And we complement the results in Asher and Novosad (2017) showing that constituencies aligned with the state government in India experience higher employment growth. In Germany, instead of alignment effects, we find that local employment growth is higher if a municipality is the hometown of a state minister.

2 Empirical Setting

2.1 State politics in Germany

The Federal Republic of Germany consists of sixteen states. Ten states are in the former West- and six in the former East-Germany.[1] Three states are so called city-states, Berlin, Bremen, and Hamburg; these are mainly big cities.

All states are parliamentary democracies and have their separate elected government. Voting rules vary across states, but all use some variant of PR. In most states, the electoral rule follows a mixed-member PR system – also know as personalized PR, *personalisiertes Verhaeltniswahlrecht*

---

[1] The city-state of Berlin was divided into a western and an eastern part before reunification. Given its location in the East, we count it as an Eastern state.

– that awards a seat to individual candidates in first-past-the-post elections while ensuring that the seat share of each party in the state parliament matches its vote share across the entire state.[2]

The state parliament elects a state cabinet, its executive counterpart for the entire legislative period. A legislature usually lasts for five years.[3] Every cabinet requires the support of at least 50% of the MPs. In practice, either a single party wins more than 50% of the seats in the parliament and can thus form the cabinet by itself or several parties, which jointly surpass the 50% threshold, agree to form a coalition cabinet.

In both cases, internal party politics determine how to staff key positions in the cabinet. In particular, who gets appointed to what ministry is under the purview of the party leadership. Ministerial appointments do not depend on how politicians perform in their districts, i.e., in the first-past-the-post election, nor on how the party performs in their municipality or region. Instead, the personal prominence of politicians in a state, their expertise, and connections to the party leadership are the decisive factors. Often ministers do not even run in the first-past-the-post elections, nor do they have to be MPs.

The structure of state cabinets, notably the size of the cabinet, varies across states and over time; each state government determines the structure at its discretion. Cabinet size typically ranges from

---

[2] Typically voters have two votes: the "first vote" is used to elect a candidate in a single-member constituency, and the "second vote" is used to vote for a party list. The total number of seats to which a party is entitled depends on the distribution of "second votes". All candidates who won in their districts receive a seat, and candidates from the party lists fill any remaining seats. Exceptions to this rule are the states of Baden-Württemberg and Saarland and the city-states of Hamburg and Bremen. For more details on the electoral rules for state parliaments in Germany see http://www.wahlrecht.de/landtage/index.htm (in German).

[3] The only exception is the city-state of Bremen with a four-years parliamentary term.

10 to 20 ministers. Each minister is responsible for many distinct policy areas. Which ministry is responsible for which policy areas also varies across states and over time. Still, each state cabinet has some core ministers, typically staffed by relatively prominent state politicians, and several further ministries with varying denominations. The core ministers are the prime minister, the finance minister, and the interior minister.

The prime minister is the most powerful member of the cabinet and usually the most prominent politician of a party in the state. The power of prime ministers originates from a number of institutional rules. Notably, in most states, the prime ministers are the only members of the cabinet that are directly elected by the state parliament. Once confirmed by the parliament, the prime ministers typically appoint the remaining members of the cabinet, which may or may not be subject to parliamentary approval. Cabinets cannot reach decisions that the prime ministers oppose, i.e., the prime ministers can overrule a majority of cabinet members. If in theory, prime ministers are only answerable to the parliament, in practice they are subject to various restrictions. In particular, prime ministers have to ensure that they retain the support of their party, and of other parties in the case of coalition governments.

The second core minister is the finance minister. Finance ministers ultimately control the budgets of all other ministries. In particular, no expenditure can be undertaken if the finance minister objects. This power over the budget gives finance ministers substantial influence within the cabinet.

The third core minister is the interior minister, which traditionally also has a prominent position in the cabinet. Interior ministers directly control a large administration since the responsibility for policing is under the purview of the states in German federalism. Moreover, interior ministers are

also responsible for supervising local governments, which gives them added visibility in the state and importance in the cabinet.

The other ministries cover a broad range of policy areas such as education and culture, economy and infrastructure, social policy and health, and environmental issues. The precise delineations of policy areas and corresponding denominations vary across states. Thus, the budgets of individual ministries cannot be compared across states or over time. However, the Federal Statistical Office offers harmonized expenditure data for specific policy areas. According to these data, the most important policy areas for state cabinets are education, culture, and science (around 30%–35% of total state expenditure), social policy and health (around 10%–15%), and economic promotion and infrastructure (around 5%–10%). State ministers, in general, wield influence over the allocation of substantial financial resources given the extensive expenditure decentralization in Germany; the state tier in Germany accounts for roughly 50% of total public spending.

2.2 Theoretical Considerations

An extensive literature on the distribution of public resources focuses on its geographical bias. Two main theoretical channels motivate this bias. First, and according to Weingast et al. (1981), this bias is due to the geographical basis of political constituencies. Different models predict that, in an attempt to increase their probability of reelection, politicians target different constituencies, from their core supporters (Cox and McCubbins, 1986), to swing voters (Lindbeck and Weibull, 1987), or minorities and special interest groups (Myerson, 1993; Coate and Morris, 1995), creating geographic patterns in the distribution of public resources.

Empirical studies usually focus on the distribution bias towards core supporters by exploring alignment effects in distributive policies. Studies show that sub-national units belonging to the ruling party often receive higher federal outlays, e.g., higher discretionary transfers for infrastructure in Brazil (Brollo and Nannicini, 2012), larger federal grants for transport and defense spending in the U.S. (Albouy, 2013) and higher allocation of EU Structural and Cohesion Funds in Hungary (Murakozy and Telegdy, 2016). Johansson (2003), on the other hand, provides evidence of targeting toward municipalities with a large number of swing voters using a panel data of 225 municipalities in Sweden over the years 1981–1995. And Cascio and Washington (2014) study the enfranchisement of black communities in the south of the US following the Voting Rights Act of 1965 to find evidence on targeting towards special interest groups.

Yet, electorally motivated favoritism is an unlikely reason for German state ministers to favor their home municipalities. Targeting specific areas is not an effective electoral strategy within the context of an essentially PR system (Lizzeri and Persico, 2001; Milesi-Ferretti et al., 2002). Moreover, individual ministers do not have to be successful or even run in the first-past-the-post-election thus they do not need to cultivate a "personal vote". Even if state governments decide to target specific constituencies, there are no electoral incentives to concentrate pork in the municipalities of state ministers as the first-past-the-post constituencies typically cover several municipalities – any votes gained in any single municipality will ultimately have a negligible effect on the outcome within a constituency.

The second and more plausible theoretical mechanism comes from models of identity politics. Rather than political considerations, politicians may favor their home towns for personal reasons.

There is a well-established literature demonstrating that the identity of politicians affects the allocation of public resources. Politicians typically favor members of their own group identity, which has been proxied in the literature by gender (Chattopadhyay and Duflo, 2004), castes and tribes (Pande, 2003), or, more generally, ethnicity (Franck and Rainer, 2012). Anecdotal evidence for regional favoritism exists for many developing and emerging countries. Do et al. (2016) and Besley et al. (2011) provide systematic evidence of this phenomenon for Vietnam and India. In these contributions, geographical biases are not explained by the leaders' political calculus, but by an innate preference for helping their home towns. This preference is also the most plausible explanation for favoritism in German state politics.

Even though this mechanism is more often associated with developing countries and weak institutional settings, Hodler and Raschky (2014) show, using a sample of 126 countries and over 38,000 regions, that regional favoritism is widespread. Similarly, Franck and Rainer (2012) find for a sample of 18 African countries that ethnic favoritism is equally present in democracies and autocracies. Burgess et al. (2015) and Kramon and Posner (2016) in turn, using Kenya as a case study, show that visible forms of favoritism, such as increases in infrastructure investment, cease in periods of democracy, but more subtle forms, such as the supply of extra inputs in the education sector, persist. These findings suggest that the difference between countries with strong and weak institutions might not lie in the prevalence of favoritism but in its visibility.

3 Methodology

3.1 Data

We rely on hand-collected data on the composition of German state cabinets in the eight West-German non-city states from 1994 to 2013. Using various sources, we were able to collect information on the place of residence of 292 of the 365 state ministers who were in office in the eight states during the 1994–2013 period (about 80%).[4] We add to this data information on the municipalities where ministers went to school, the specific ministries headed, the entry and exit dates into and out of office, and further characteristics of the ministers.

Panel A of Table 1 reports basic summary statistics on the characteristics of ministerial appointees. The average minister enters a cabinet at about 51 years of age and stays in office for about four years. Around 29% of the ministers are female. Regarding their political affiliation, the majority of the ministerial appointees are either from the CDU (29%) or SPD (43%). For the ministers for which we have information on the career path following the dismissal from office, about 55% remain in politics, while 20% pursue careers in the private sector, and 10% represent the interests of different associations. About 23% of the ministers for whom we have data on both places of residence and of schooling continue living in the town where they went to school.

Our primary outcome variable is the annual growth rate of social-security covered employees that reside in a municipality, defined as follows:

---

[4] We identify ministers during the early sample period using information provided by Schnapp (2006). For more recent years, we rely on information available from official state government or state parliament websites.

$$y_{i,t} = \Delta log(employment)_{i,t} = log(employment)_{i,t} - log(employment)_{i,t-1}. \qquad (1)$$

Social-security covered employment data are available from the German Employment Agency from 1993 onward. Even though social-security covered employment does not include the entire universe of employment, it allows us to assess the employment prospects of most of the working-age population as it is the default form of work in Germany. The majority of private and public sector employees belong to this category. The main groups that are outside of this category are the self-employed and certain types of public servants. While conditions and wages of social-security covered jobs vary, they come with many benefits. Among them the fact that they award relatively high job security, especially in the public sector.

Data for all German states are available; however, we make three sample restrictions. First, we drop municipalities located in the former East-German states. Municipalities in the former East-Germany were subject to various boundary reforms, which makes it difficult to track a sufficient number of municipalities over time. Moreover, the former East-German states were hit by various idiosyncratic employment shocks, such as the transition from the socialist to the market economy in the early nineties and massive outmigration. Second, we drop the two city-states given that they have no subordinate municipalities. Thus, the sample covers municipalities in the eight West-German non-city states. Finally, we drop municipalities for which we have missing employment observations.[5] Panel B of Table 1 reports summary statistics on municipalities, including

---

[5] Municipalities may lack data on employment for some years either due to boundary reforms or because of generic missing data.

the growth rate of employment. On average, the municipal-level annual employment growth in Germany during the sample period is about 0.6%.

We use the data on state ministers and the data on employment to create a municipal-level dataset for the period 1994–2013. Specifically, we use the available information on the place of residence of a minister and the entry date to the cabinet to generate a dummy variable that turns to one for a given municipality in the year a resident is appointed state minister. More formally, let $Minister_{i,t}$ be the dummy variable indicating treatment, where i indexes municipalities and t time. For each municipality i, treatment occurs if during some year t, between 1994–2013, a resident is appointed state minister. The $Minister_{i,t}$ is then equal to one for every year after the appointment.

Overall, our sample covers 8,362 municipalities. The total number of municipalities in Western Germany in 2013 was 8,459. Figure 1 shows a map that indicates all included municipalities. Altogether, there are 184 municipalities that at some point during the sample period were the residence of a minister. We refer to these municipalities in the following as "minister municipalities". Panel B of Table 1 shows that of the 167,240 municipality-year pairs in the dataset, around 1.4% (2,314 observations) assume the value one, i.e., are the residence of a minister.

Finally, we use additional data to extend and validate the baseline results. We use data on investment grants paid by the state government to municipalities; investment grants are the largest discretionary transfer program in Germany and the most obvious fiscal channel through which ministers may try to favor their home municipalities. Data on investment grants is available from the Federal Statical Office for all German municipalities, albeit only for the period 2008–2013. We also use data on public employment available from the different states statistical offices, to test

whether the changes in social-security covered employment are the result of a creation of public employment opportunities.

3.2 Empirical strategy

We study whether employment growth in a municipality increases when a resident is appointed to the state cabinet. For this purpose, we estimate the following general difference-in-differences (diff-in-diff) regression model:

$$y_{i,t} = \alpha_i + \gamma_t + \beta Minister_{i,t} + \varepsilon_{i,t}, \tag{2}$$

where $y_{i,t}$ is the annual growth rate of social security covered employment in a municipality as defined in Equation (1) and $Minister$ is the dummy variable indicating whether a municipality is the place of residence of at least one minister. Since we have a panel of municipalities, we follow Islam (1995) and control for municipality fixed effects (α_i) to account for municipality-specific factors that may lead to persistently higher or lower employment growth rates. In addition, we control for common shocks (γ_t) using various strategies: by including year fixed effects, state-specific trends, and state-specific year fixed effects. Standard errors are clustered at the municipal level and robust to heteroscedasticity.

Two assumption must hold for $\hat{\beta}$ to retrieve a causal effect. First, there must be no municipality-level omitted variables correlated with the timing of ministerial appointment. And second, there must be no reverse causality between employment growth in a minister municipality and the propensity of a resident to be appointed to office. We validate both assumptions in robustness

14

checks. However, they are plausible in our setting. As discussed, political careers at the state-level and, in particular, ministerial appointments are independent of municipality-level developments given the institutional details of German state politics. To advance at the state-level, politicians have to gain support of party elites from across the state. Though party elites may factor in from which region a minister originates, the specific municipality is most likely unimportant. Moreover, while implicit regional quotas for ministerial appointments may exist, their relevance is unlikely to change over time, and will not depend on any short-term developments. Finally, the party of a minister has to actually be part of the ruling coalition after a state-election, which is, in general, an uncertain event.

4 Results

4.1 Baseline results

We collect the baseline results in Table 2. All models include municipality fixed effects and rely on heteroscedasticity- and cluster-robust standard errors. Model (1) includes year fixed effects; model (2) relies, in addition to year fixed effects, on state-specific trends; and model (3) includes state-specific year fixed effects.[6]

The minister dummy is positive and significant (p-value=0.000) in all models. Coefficient estimates indicate that the growth rate of employment is, on average, higher in minister relative to control municipalities. The magnitude of the coefficients suggests a 0.4–0.5 percent significantly

---

[6] Including state-specific fixed effects consists of the most conservative version of Equation (2). State-specific fixed effects subsume the year fixed effects and state-specific trends.

higher gorwth rate of social security covered employment in minister municipalities. For a municipality with 10,000 inhabitants, this estimate implies that employment growth is on average higher by 40–50 individuals in minister municipalities annually.

4.2 Robustness tests

4.2.1 Different Specifications

In Table 3, we collect various robustness tests that explore the sensitivity of the results to different samples and specifications. All models include municipality and state-specific year fixed effects, and rely on heteroscedasticity- and cluster-robust standard errors.

The first robustness check, in model (1), tests whether results are robust to the definition of the dependent variable. Instead of the growth rate of employment, we use the annual change in the level of employment as the dependent variable. The result is consistent with the baseline estimates; the average growth in the level of employment in a minister municipality is positive and significant at around 265 individuals.

Models (2), (3), and (4) test the robustness of the results to the following samples adjustments: the exclusion of small municipalities, with on average below 500 social-security covered employees, due to the high variability of employment growth in these municipalities; the exclusion of state capitals as these are almost continuously the place of residence of at least one minister; and the exclusion of extreme outliers defined as those municipalities with employment growth rates below the 1st and above the 99th percentile, these include obvious data entry errors. Coefficient

estimates remain in line with the baseline results indicating a 0.4–0.5 percent higher growth rate of employment in minister municipalities.

Finally, models (5) and (6) test the spatial reach of the treatment effect. We create two dummy variables identifying, first, the municipalities belonging to the same counties as the minister municipalities – excluded of the last –, and second, the jurisdictions neighboring the minister municipalities. Both robustness checks allow us to test whether we identify an at-large or targeted treatment effect. The insignificance of the coefficient estimates indicates that the increase in the growth rate of employment is restricted to minister municipalities.

4.2.2   Omitted Variables

To address concerns regarding omitted variable bias we restrict the sample to minister municipalities and their immediate neighbors. We use four definitions of neighborliness based on the following fixed critical distances: 50km, 40km, 30km, and 20km. We define as neighbors municipalities whose centroid lies within each of the different distances from the centroid of a minister municipality. We also create neighborhood-specific dummy variables to delineate neighborhoods, and neighborhood-specific trends, i.e., a separate trend variable for each minister municipality and all other municipalities located in its neighborhood.

Restricting the sample to immediate neighbors increases the comparability between treatment and control municipalities. Coefficient estimates measure the effect of ministerial appointments for a minister municipality compared to its immediate neighbors while accounting for neighborhood-specific developments. The underlying assumption is that neighborhood-specific trends account

for any neighborhood-specific variables that determine employment growth and the propensity of a politician from that neighborhood to be appointed to office.

In addition to addressing omitted variables, the results also implicitly asses whether ministers target their home municipality or the surrounding broader region – and in particular the electoral district. If ministers target larger regions, the coefficient estimate of the *Minister* dummy should approximate zero once we limit the sample to the immediate neighborhood of a minister municipality.

We collect the results in Table 4. All models rely on state-specific year fixed effects, neighborhood fixed effects, and neighborhood-specific trend, as well as heteroscedasticity- and cluster-robust standard errors. Coefficient estimates remain positive and significant, and vary between 0.2–0.4 percent; the more restricted the neighborhood, the higher is the treatment effect. These results are in line with the baseline estimates and show that a municipality benefits to a significant extent compared to its immediate neighbors from the appointment of a resident to the state cabinet.

4.2.3 Measurement error

When we cannot find information on the place of residence of ministers during their tenure, we rely on the current residence. To address issues of measurement error that can result from this approach, we collect information on the school-town of ministers. We hypothesize that ministers whose current place of residence is the same as the municipality where they went to school also lived in that municipality when they were in office. We estimate models where we distinguish between ministers who live in the municipality where they went to school and ministers who no

longer live in the same municipality or for whom we do not have information on the place of schooling.

We collect the results in Table 5. All models include municipality and state- specific year fixed effects, and rely on heteroscedasticity- and cluster-robust standard errors. Model (1) includes the dummy indicating municipalities that are both the schooling place and residence of a minister. Model (2) includes the dummy indicating municipalities that are the residence of a minister but not their place of schooling or for whom we do not have information on the place of schooling. Model (3) includes both dummy variables.

Coefficient estimates are positive and significant across the different models. Model (3), where we include both dummy variables, suggests that the growth rate of employment is higher in magnitude for municipalities that are both residence and place of schooling of a minister. However, we cannot reject the equality hypothesis between coefficients at any of the standard significance levels. Overall, these results confirm that the growth rate of employment is higher in minister municipalities; testing for measurement error has no significant effect on the estimates.

4.2.4 Reverse causality

To study reverse causality, we re-run Equation (2) using placebo appointment years. For this purpose, we create fake treatment dummy variables indicating one, three, and five years before the ministerial appointments. If employment growth in a municipality is not related to the propensity of a politician from that municipality to be appointed state minister, the growth rate of social security covered employment should not be higher in future minister municipalities already in the pre-treatment period.

19

We collect the results in Table 6. In models (1)–(3), we test each of the placebo dummy variables. In models (4)–(6), we replicate the regressions but also include our true treatment variable. All specifications include municipality and state-specific year fixed effects, and rely on heteroscedasticity- and cluster-robust standard errors.

The coefficient estimate for the placebo treatment indicating the year preceding ministerial appointments is statistically insignificant, while the placebo treatments corresponding to three and five years before the appointments are significant but negative. These results suggest that minister municipalities appear to, in the past, have suffered from a lower growth rate of employment. However, one year before a resident is appointed minister, the growth rate of employment in soon to be minister municipalities is not significantly different from control municipalities.

In models (4)–(6), when we include the correct ministerial appointment, placebo dummy variables turn insignificant. The previous significance of the placebos thus appears to derive from the within-variation in the growth rate of employment before and after the ministerial appointments; compared to the later years when a resident is appointed a state minister, in the pre-treatment period the growth rate of employment was lower in minister municipalities. These results mitigate concerns regarding reverse causality.

4.3   Timing

To study the timing of the treatment effects, we analyze the temporal pattern of employment growth during ministerial appointments and after the dismissal of appointees. We divide the tenure of

ministers into the different periods in office and create dummy variables for the years after the dismissal of the ministers.

We collect the coefficient estimates in Table 7. Model (1) focuses on the full tenure of the ministers and the period after dismissal from office; both coefficient estimates are positive and significant. These results suggest that minister municipalities benefit especially in the long run from being the residence of a (former) state minister. Also, these results show that there is no reversal in employment after the dismissal of ministers from office; the jobs created during the tenure of ministers persist. This finding is plausible given the high job security of social-security covered employment.

Likewise, models (3) and (4) provide the same insights. In model (3) we divide the tenure of the ministers into five years in office and all years thereafter, and, in model (4), we divide it into the first three years in office, four to five, and six to ten years in office, all years after until dismissal, and after dismissal. All coefficient estimates show a positive and significant treatment effect in both models. Model (3) suggests, and model (4) further shows that the growth rate of employment in a minister municipality increases over the tenure of the minister. While the minister is in office, the growth rate of employment is 0.3–0.8 percent higher in minister municipalities, and, after the dismissal from office, the growth rate of employment remains about 0.8 percent higher.

5 Mechanisms

5.1 Public Employment

The rate of employment growth increases significantly in a municipality upon the appointment of a resident to the state cabinet. However, it remains unclear how ministers promote this growth in employment. Ministers have, in general, several means of creating employment opportunities for the residents in their municipalities. First, they can provide additional financial transfers to their home towns. Most ministries run discretionary grant programs to which municipalities can submit projects for funding. Ministers may ensure that applications from their municipalities are treated favorably by the ministerial administrations. Second, ministers can treat their home municipalities favorably in the distribution of public projects, such as road construction, cultural venues, or other infrastructure projects. Finally, ministers can use their influence to directly obtain jobs for co-residents. They can put pressure on public agencies that are directly subordinate to them to hire co-residents or they can provide informal recommendations to private sector firms or public sector employers that are not their subordinates. They can also ask colleagues in other ministries for favors.

To explore the underlying mechanism, we study data on the growth rate of public employment at different levels, as well as on the growth rate of investment grants to municipalities. We collect the results in Table 8. All models include municipality and state-specific year fixed effects, and rely on heteroscedasticity- and cluster-robust standard errors. Model (1) through (6) study the growth rate of state employment, local government employment, special purpose organization employment – e.g., employment in organizations set up by a collective of municipalities –, employment

in social security services, and in legally independent state and local organizations in minister municipalities. Model (7) in turn, uses as the dependent variable the growth rate of investment grants attributed to municipalities.

Results show a positive and significant (p=0.002) effect of ministerial appointments on the growth rate of state employment. Having a resident appointed to the state cabinet increases state public employment in the municipality by around 1 percent. The coefficient estimate for the effect of ministerial appointment on the growth rate of employment in social security services is also positive and significant (p=0.068) with a magnitude of about 6 percent. We find no effect of ministerial appointments on the remaining forms of public employment, nor on the growth rate of investment grants.

To estimate how the effect of ministerial appointments on employment growth works through public employment we calculate the local average treatment effects at the mean. Back of the envelope calculations show that social-security covered employment grows by 9.88 persons per year while average state public employment increases by 2.46 and employment in social security services by 0.8 persons. Thus, state public employment and employment in social security services explain about 25% and 8% of the growth in social-security covered employment in minister municipalities.

5.2 Heterogeneous Effects

In this section, we explore how different characteristics of ministers affect the growth rate of employment. Specifically, we test for heterogeneous effects depending on the ministry headed and

the post-office career path to understand the circumstances under which ministers promote employment growth in their home municipalities. Also, we test for heterogeneous effects hinging on partisan affiliation and the gender of the ministers to assess whether there are intrinsic characteristics that lead some ministers to promote higher employment growth than others.

The ministry headed provides information on whether ministers rely on their bureaucracies or personal influence. If on the one hand, ministers use their standing to create jobs, more visible positions in the cabinet, such as prime minister, finance minister, or interior minister should promote higher employment growth. On the other hand, if ministers use their immediate leverage over their bureaucracies, ministers that have portfolios with large budgets and oversee a large workforce may be more efficient in promoting employment growth.

We estimate separate treatment effects for different types of ministers. As ministries are not standardized across states and differ in the delineations of policy areas we classify them into seven main categories: the prime minister, the finance minister, the interior minister, ministries dealing with economy and infrastructure, ministries dealing with social policy and health, ministries dealing with culture and education, and ministries dealing with environmental protection.

We collect the results in Table 9. We find a large positive and significant effect for the prime-minister, followed by the ministries for economy and infrastructure, and social policy and health. Coefficient estimates are also positive and significant, albeit to a smaller degree, for the ministries of culture and education and environmental protection.

The ministries for economy and infrastructure, social policy and health, and culture and education are responsible for a large budget and a high number of public sector jobs. Overall, these

results indicate that ministries that come with direct leverage are more beneficial for a municipality. The only exception is the prime minister; the prime minister does not have a large budget and is not directly responsible for many jobs. Thus, it is most likely the prominence of prime ministers and their influence that leads to higher employment growth in their home municipalities.

Also, we gather data on the career path of ministers following their tenure in the state cabinet. We have this information for around 70% of the ministers in our sample. To study whether the post-office career path has an influence on the growth rate of social security employment in a minister municipality, we re-estimate Equation (2) including a dummy variable indicating minister municipalities during the post-office period interacted with each of the following post-office occupations: political career, career in the public administration, private sector, member of associations, and academia. We run a separate regression for each of the categories. A career in politics most often means that ministers are repeatedly appointed to the state cabinet, or become state or federal MPs. Careers in the public administration are rare and often entail being the chairman or a board or council member of a public agency or institution. Ministers pursuing careers in the business sector often practice law or become consultants. Around ten percent of the the ministers chose to represent the interests of some association following their political career; these associations may be at different levels representing from local to national interests. Another three percent of the ministers followed an academic career after leaving the cabinet.

In Table 10, we collect the different coefficient estimates. We find a positive and significant effect in the range of 0.5–0.7 percent for ministers that make a career in politics, in the public administration, and in the private sector. These results are consistent with previous findings show-

25

ing that long tenures promote higher growth rates of employment. Following a political career or a career in the public administration may involve some leverage in creating public jobs; these together with employment in social security services account for over 30% of the growth rate of employment in minister municipalities. Also, that municipalities whose ministers transition into the private sector see their growth rate of employment continue to increase after the minister leaves office suggests that some of jobs may be created in the private sector.

Finally, ministers have different partisan affiliations. As shown in Panel A of Table 1 around 70% of the ministers belong to either of the two biggest parties, CDU or SPD. Around 8% of ministers are from the FDP and 7% from the Green Party with the remaining 13% belonging to other smaller parties. Also, minister differ in gender, with 29% of them being female. We test for heterogeneity across parties and gender and collect the results in Tables 11 and 12. Coefficient estimates are positive and significant for the four main parties and for both genders. Additional test show that both the coefficient estimates across parties and across genders are not statistically different from each other; on average politicians engage in home town favoritism. We interpret these results as evidence for the claim that politicians are the same everywhere.

6 Conclusion

We study local favoritism by appointed government officials in Germany using hand-collected data on members of state cabinets in the West-German states. Relying on a sample of more than 8,000 municipalities during the period 1994–2013, we find that the home municipalities of state ministers experience higher growth rates of social security covered employment than control municipalities.

Given the institutional features of state politics in Germany, this effect is more likely to be driven by home bias rather than electoral considerations. In line with this hypothesis, we find that ministers target their home municipalities in particular instead of the whole constituency and that the effect is more pronounced after the dismissal of the minister from the state cabinet.

In extensions, we provide further evidence indicating that the increase in employment growth is partially achieved through the creation or mediation of job opportunities in the public sector rather than through targeted redistribution of pork. Moreover, the effect appears to be driven by ministers in control of policy areas with large budgets, that after dismissal continue in politics or pursue a career in public administration or the private sector. The effects are not distinct across parties nor do they depend on the gender of the minister.

Overall, these findings indicate that state ministers at large engage in relatively mild forms of favoritism, leveraging their influence and bureaucracies to create employment opportunities for co-residents. Thus, this paper adds to the recent literature showing regional favoritism to be widespread, present in both autocracies and democracies. Moreover, it also reveals that democratic institutions can prevent visible forms of geographical targeting but not more subtle mechanisms available to officeholders.

References

Albouy, D. (2013). Partisan representation in congress and the geographic distribution of federal funds. *Review of Economics and Statistics 95*(1), 127–141.

Asher, S. and P. Novosad (2017). Politics and local economic growth: Evidence from India. *American Economic Journal: Applied Economics 9*(1), 229–273.

Besley, T., R. Pande, and V. Rao (2011). Just rewards? Local politics and public resource allocation in south India. *The Wrold Bank Economic Review 26*(2), 191–216.

Brollo, F. and T. Nannicini (2012). Tying your enemy's hands in close races: The politics of federal transfers in Brazil. *American Political Science Review 106*, 742–761.

Burgess, R., R. Jedwab, E. Miguel, A. Morjaria, and G. Padró i Miquel (2015). The value of democracy: Evidence from road building in Kenya. *American Economic Review 105*(6), 1817–1851.

Carozzi, F. and L. Repetto (2016). Sending the pork home: Birth town bias in transfers to Italian municipalities. *Journal of Public Economics 134*, 42–52.

Cascio, E. U. and E. Washington (2014). Valuing the vote: The redistribution of voting rights and state funds following the voting rights act of 1965. *Quarterly Journal of Economics 129*(1), 379–433.

Chattopadhyay, R. and E. Duflo (2004). Women as policy makers: Evidence from a randomized policy experiment in India. *Econometrica 72*(5), 1049–1443.

Coate, S. and S. Morris (1995). On the form of transfers to special interests. *Journal of Political Economy 103*, 1210–1235.

Cox, G. and M. McCubbins (1986). Electoral politics as a redistributive game. *Journal of Politics 48*, 370–389.

Do, Q.-A., K.-T. Nguyen, and A. N. Tran (2016). One mandarin benefits the whole clan: Hometown favoritism in an authoritarian regime. forthcoming *American Economic Journal: Applied Economics*.

Fiva, J. and A. Halse (2016). Local favoritism in at-large proportional representation systems. *Journal of Public Economics 143*, 15–26.

Franck, R. and I. Rainer (2012). Does the leader's ethnicity matter? Ethnic favoritism, education, and health in Sub-Saharan Africa. *American Political Science Review 106*(2), 294–325.

Hodler, R. and P. Raschky (2014). Regional favoritism. *Quarterly Journal of Economics 129*(2), 995–1033.

Islam, N. (1995). Growth empirics: A panel data approach. *Quarterly Journal of Economics 110*(4), 1127–1170.

Johansson, E. (2003). Intergovernmental grants as a tactical instrument: empirical evidence from Swedish municipalities. *Journal of Public Economics 87*, 883–915.

Kramon, E. and D. Posner (2016). Ethnic favoritism in education in Kenya. *Quarterly Journal of Political Science 11*, 1–58.

Lindbeck, A. and J. Weibull (1987). Balanced budget redistribution as the outcome of political competition. *Public Choice 52*, 273–297.

Lizzeri, A. and N. Persico (2001). The provision of public goods under alternative electoral incentives. *American Economic Review 91*(1), 225–239.

Milesi-Ferretti, G. M., R. Perotti, and M. Rostagno (2002). Electoral systems and public spending. *Quarterly Journal of Economics 117*(2), 609–657.

Murakozy, B. and A. Telegdy (2016). Political incentives and state subsidy allocation: Evidence from Hungarian municipalities. *European Economic Review 89*, 324–344.

Myerson, R. (1993). Incentives to cultivate favored minorities under alternative electoral systems. *American Political Science Review 87*, 856–869.

Pande, R. (2003). Can mandate political representation increase policy influence for disadvantaged minorities? Theory and evidence from India. *American Economic Review 93*(4), 1132–1151.

Rogoff, K. (1990). Equilibrium political business cycles. *American Economic Review 80*, 21–36.

Schnapp, K. U. (2006). Einfluss der Bundespolitik auf Landtagswahlen. Eine Analyse des Waehlerverhaltens auf Landesebene unter besonderer Beruecksichtigung der Bundespolitik. Research Project of the German Research Foundation.

Weingast, B., K. Shepsle, and C. Johnsen (1981). The political economy of costs and benefits: A neoclassical approach to distributive politics. *Journal of Political Economy 89*(4), 642–664.

Wittman, D. (1983). Candidate motivation: A synthesis of alternative theories. *American Political Science Review 72*, 142–157.

Table 1: Summary statistics

Variable	Count	Mean	SD	Min	Max
Panel A: Appointees					
Data on place of residence available	713	0.835	0.372	0.000	1.000
Age of entry into office	713	51.555	6.989	29.000	68.000
Age of exit from office	713	54.346	7.026	33.000	69.000
Tenure	713	3.791	1.491	1.000	7.000
Female	713	0.286	0.452	0.000	1.000
CDU	713	0.290	0.454	0.000	1.000
SPD	713	0.425	0.495	0.000	1.000
FDP	713	0.081	0.274	0.000	1.000
Green Party	713	0.072	0.258	0.000	1.000
Other party	713	0.132	0.339	0.000	1.000
Politics	505	0.552	0.498	0.000	1.000
Public Administration	505	0.050	0.217	0.000	1.000
Business	505	0.198	0.399	0.000	1.000
Associations	505	0.105	0.307	0.000	1.000
Academia	505	0.030	0.170	0.000	1.000
Retired	505	0.059	0.237	0.000	1.000
Place of residence and schooling identical	593	0.229	0.421	0.000	1.000
Panel B: Municipalities					
Employment growth	169653	0.621	5.627	-592.613	327.287
Minister	169653	0.014	0.117	0.000	1.000
Minister CDU	169653	0.007	0.083	0.000	1.000
Minister SPD	169653	0.007	0.081	0.000	1.000
Minister FDP	169653	0.002	0.042	0.000	1.000
Minister Green	169653	0.001	0.033	0.000	1.000
Prime minister	169653	0.002	0.047	0.000	1.000
Finance	169653	0.002	0.050	0.000	1.000
Interior	169653	0.003	0.051	0.000	1.000
Infrastructure	169653	0.003	0.056	0.000	1.000
Social Policy	169653	0.003	0.055	0.000	1.000
Culture & Education	169653	0.003	0.056	0.000	1.000
Environment	169653	0.003	0.052	0.000	1.000

[a] This table presents summary statistics on ministerial appointees and municipal characteristics.

Table 2: Ministerial Appointments and Local Employment Growth: Baseline Results

	(1)	(2)	(3)
Minister	0.518***	0.436***	0.408***
	(0.092)	(0.090)	(0.087)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State-specific Trends	No	Yes	Yes
State-specific year FE	No	No	Yes
Observations	167240	167240	167240
Municipalities	8362	8362	8362

[a] This table collects difference-in-differences regression results from the estimation of Equation (2) evaluating the benefits of living in a minister municipality.
[b] The dependent variable is the growth rate of social security covered employees in a municipality as defined in Equation (1).
[c] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).
[d] Heteroscedasticity robust standard errors in parentheses.

Table 3: Ministerial Appointments and Local Employment Growth: Robustness Tests

	(1: Level specification)	(2: W/o small municipalities)	(3: W/o capitals)	(4: W/o outliers)	(5: Minister County)	(6: Minister Neighbors)
Minister	265.356***	0.414***	0.411***	0.461***		
	(47.227)	(0.081)	(0.087)	(0.083)		
Minister county					-0.003	
					(0.056)	
Minister neighbor						-0.063
						(0.078)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	167240	94300	167100	163906	167240	165886
Municipalities	8362	4715	8355	8362	8362	8295

[a] This table collects difference-in-differences regression results from the estimation of variations of Equation (2) evaluating the robustness of the baseline results to different samples and specifications.

[b] The dependent variable is the annual change in the level of social security covered employees in a municipality in model (1) and the growth rate of social security covered employees in a municipality as defined in Equation (1) in models (2)–(6),

[c] In models (5) and (6) Minister County and Minister Neighbors indicate municipalities in the same county as the minister municipality and neighbors of minister municipalities.

[d] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).

[e] Heteroscedasticity robust standard errors in parentheses.

Table 4: Ministerial Appointments and Local Employment Growth: Minister Municipalities and Neighbors

	(1: 50km)	(2: 40km)	(3: 30km)	(4: 20km)
Minister	0.247**	0.351***	0.408***	0.412***
	(0.099)	(0.094)	(0.090)	(0.092)
Municipality FE	Yes	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes	Yes
Neighborhood FE	Yes	Yes	Yes	Yes
Neighborhood-specific trends	Yes	Yes	Yes	Yes
Observations	161040	153840	134440	96000
Municipalities	8052	7692	6722	4800

[a] This table collects difference-in-difference regression results evaluating the benefits of living in a minister municipality compared to its neighbors. We only include non-minister municipalities whose centroids are at most either (1) 50km, (2) 40km, (3) 30km, or (4) 20km away from the centroid of a minister municipality.
[b] The dependent variable is the growth rate of social security covered employees in a municipality as defined in Equation (1).
[c] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).
[d] Heteroscedasticity robust standard errors in parentheses.

Table 5: Ministerial Appointment and Local Employment Growth: Schooling in Town of Residence

	(1)	(2)	(3)
Minister$_{Schooltown}$	0.304**		0.473***
	(0.137)		(0.126)
Minister$_{Non-schooltown}$		0.320***	0.393***
		(0.094)	(0.098)
Municipality FE	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes
Observations	167240	167240	167240
Municipalities	8362	8362	8362

[a] This table collects difference-in-differences regression results from the estimation of variations of Equation (2) evaluating the robustness of the baseline results to measurement error.
[b] The dependent variable is the growth rate of social security covered employees in a municipality as defined in Equation (1).
[c] The dummies Minister$_{Schooltown}$ and Minister$_{Non-schooltown}$ indicate ministers that reside in the same municipalitiy in which they went to school and ministers that do not or for which we have no informations on the place of chooling.
[d] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).
[e] Heteroscedasticity robust standard errors in parentheses.

Table 6: Ministerial Appointments and Local Employment Growth: Placebo Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Five years before appointment			-0.155**			0.170
			(0.071)			(0.109)
Three years before appointment		-0.170*			0.081	
		(0.094)			(0.124)	
Year before appointment	-0.165			0.052		
	(0.123)			(0.140)		
Minister				0.416***	0.440***	0.507***
				(0.096)	(0.113)	(0.126)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	167240	167240	167240	167240	167240	167240
Municipalities	8362	8362	8362	8362	8362	8362

[a] This table collects placebo difference-in-differences regression results from the estimation of variations of Equation (2) evaluating the benefits of living in a minister municipality. The period of ministerial appointment is defined as (1) one, (2) three and (3) five years before the actual appointment.
[b] The dependent variable is the growth rate of social security covered employees in a municipality as defined in Equation (1).
[c] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).
[d] Heteroscedasticity robust standard errors in parentheses.

Table 7: Ministerial Appointment and Local Employment Growth: Timing

	(1)	(2)	(3)
Minister, in office	0.274***		
	(0.092)		
Up to five years after appointment		0.316***	
		(0.102)	
Five years after appointment onwards		0.704***	
		(0.133)	
Up to three years after appointment			0.306***
			(0.108)
Fourth and fifth years after appointment			0.349***
			(0.127)
Five to ten years after appointment			0.374***
			(0.142)
Ten years after appointment until dismissal			0.802***
			(0.161)
After dismissal	0.706***		0.825***
	(0.122)		(0.150)
Municipality FE	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes
Observations	167240	167240	167240
Municipalities	8362	8362	8362

[a] This table collects difference-in-differences regression results from the estimation of variations of Equation (2) evaluating the benefits of living in a minister municipality during and after the tenure of state ministers.
[b] The dependent variable is the growth rate of social security covered employees in a municipality as defined in Equation (1).
[c] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).
[d] Heteroscedasticity robust standard errors in parentheses.

Table 8: Ministerial Appointments and Local Employment Growth: Employment Sources

	(1: State employment)	(2: Local employment)	(3: Special purpose authority)	(4: Social security)	(5: Independent state organization)	(6: Independent local organization)	(7: Investment grants)
Minister	1.273***	0.250	-1.930	5.979*	2.346	0.159	-3.406
	(0.419)	(0.549)	(1.440)	(3.270)	(3.483)	(1.730)	(15.702)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	116686	112783	117274	117230	117463	111917	25565
Municipalities	7095	6752	7099	7104	7104	7078	5816

[a] This table collects difference-in-differences regression results from the estimation of variations of Equation (2) to identify the source of the benefits of living in a minister municipality.

[b] The dependent variables are the log growth rates of (1) state employment, (2) local employment, (3) employment in special purpose authorities, (4) social security services employment, (5) employment in independent state organizations, (6) employment in independent local organizations, and (7) municipal investment grants.

[c] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).

[d] Heteroscedasticity robust standard errors in parentheses.

Table 9: Ministerial Appointments and Local Employment Growth: Heterogeneous Effects Across Ministries

	(1: Prime Minister)	(2: Finance)	(3: Interior)	(4: Economy and Infrastructure)	(5: Social policy and Health)	(6: Culture and Education)	(7: Environment)
Minister	1.171***	0.215	0.365	0.729***	0.668***	0.441**	0.446*
	(0.202)	(0.203)	(0.227)	(0.189)	(0.145)	(0.189)	(0.263)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	167240	167240	167240	167240	167240	167240	167240
Municipalities	8362	8362	8362	8362	8362	8362	8362

[a] This table collects difference-in-differences regression results from the estimation of Equation (2) evaluating the benefits of living in a minister municipality for different ministries.
[b] The dependent variable is the growth rate of social security covered employees in a municipality as defined in Equation (1).
[c] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).
[d] Heteroscedasticity robust standard errors in parentheses.

Table 10: Ministerial Appointments and Local Employment Growth: Heterogeneous Effects
Across Career Paths

	(1: Politics)	(2: Public Administration)	(3: Business)	(4: Association)	(5: Academia)
Minister	0.334***	0.421***	0.389***	0.420***	0.425***
	(0.091)	(0.087)	(0.086)	(0.087)	(0.086)
Post-office employment	0.532***	0.669***	0.645**	0.245	0.111
	(0.162)	(0.210)	(0.263)	(0.235)	(0.937)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State-specific trend	Yes	Yes	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes	Yes	Yes
Observations	169645	169645	169645	169645	169645
Municipalities	8552	8552	8552	8552	8552

[a] This table collects difference-in-differences regression results from the estimation of Equation (2) evaluating the benefits of living in a minister
     municipality for the different career paths ministers follow after holding office.
[b] The dependent variable is the growth rate of social security covered employees in a municipality as defined in Equation (1).
[c] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).
[d] Heteroscedasticity robust standard errors in parentheses.

Table 11: Ministerial Appointments and Local Employment Growth: Heterogeneous Effects Across Parties

	(1: CDU)	(2: SPD)	(3: FDP)	(4: Greens)
Minister	0.429***	0.401**	0.521***	0.674**
	(0.107)	(0.175)	(0.193)	(0.284)
Municipality FE	Yes	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes	Yes
Observations	167240	167240	167240	167240
Municipalities	8362	8362	8362	8362

[a] This table collects difference-in-difference regression results from the estimation of Equation (2) evaluating the benefits of living in a minister municipality for different parties.
[b] The dependent variable is the growth rate of social security covered employees in a municipality as defined in Equation (1).
[c] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).
[d] Heteroscedasticity robust standard errors in parentheses.

Table 12: Ministerial Appointments and Local Employment Growth: Heterogeneous Effects Across Gender

	(1)	(2)	(3)
Male	0.468***		0.461***
	(0.091)		(0.091)
Female		0.417**	0.397**
		(0.174)	(0.172)
Municipality FE	Yes	Yes	Yes
State-specific year FE	Yes	Yes	Yes
Observations	167240	167240	167240
Municipalities	8362	8362	8362

[a] This table collects difference-in-differences regression results from the estimation of Equation (2) evaluating the benefits of living in a minister municipality depending on the gender of the minister.
[b] The dependent variable is the growth rate of social security covered employees in a municipality as defined in Equation (1).
[c] Stars indicate significance levels at 10%(*), 5%(**) and 1%(***).
[d] Heteroscedasticity robust standard errors in parentheses.