DISCUSSION PAPER

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Distance Matters:
Geographical Proximity and
Fiscal Rules Enforcement

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Geographical Proximity and Fiscal Rules Enforcement

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Abstract

A reform in the German state of Hesse selectively shifted the responsibility for overseeing and enforcing a balanced budget rule at the municipal level. Oversight was transferred from politically affiliated county administrators to non-political and potentially more impartial fiscal supervisors at a higher administrative layer. We use this institutional change to empirically examine whether enforcement under the former regime was affected by hidden bias related to geographic proximity. We find that municipalities located closer to the former political supervisors reduced their cash loans more strongly under the subsequent neutral regime, consistent with the existence of proximity-related favoritism prior to the reform. This spatial pattern is most evident for smaller municipalities, when supervisors oversee more municipalities, and when mayors and supervisors are not politically aligned.

JEL Codes: D73; H71; H77; H83

Keywords: fiscal oversight, fiscal rules, balanced budget rule, local government debt

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1 Introduction

Fiscal decentralization carries the inherent risk that subnational governments over-borrow when there are implicit or explicit bailout guarantees. Fiscal restrictions are therefore an important feature for monetary unions but also for states characterized by a high degree of vertical fiscal imbalance (von Hagen and Eichengreen, 1996; Yared, 2019). However, ensuring compliance with such fiscal rules is not necessarily automatic, and effective enforcement matters (Debrun et al., 2008; Christofzik and Kessing, 2018; Dovis and Kirpalani, 2020). This is a general challenge, as a principal–agent problem arises when regulations set at a higher level of government need to be enforced at a lower level (Axbard and Deng, 2024).

Determining who should oversee and enforce fiscal rules raises questions about the suitable government level for these responsibilities and about political independence. The delegation literature emphasizes that institutional design shapes discretion, political incentives, and accountability in rule enforcement (e.g., Huber and Shipan, 2002; Alesina and Tabellini, 2008; Gailmard and Patty, 2007). These considerations are reflected in the recent debate on reforming European fiscal rules. Presently, the European Commission monitors compliance with fiscal rules, and responsible commissioners are politically affiliated. As shown by Killermann (2016), they tend to align with governments led by members from the same European party group, and national linkages play a role. Complementary evidence at the national level suggests that independent fiscal councils are linked to higher compliance with fiscal rules (Beetsma et al., 2019; Reuter, 2019).

The relevance of these questions extends to the municipal level, which is the focus of our paper. We study whether enforcement of fiscal rules at the municipal level is affected by geographical proximity between municipalities and their supervisory authorities. We exploit an institutional reform in the German state of Hesse, which selectively delegated fiscal oversight to a non-political supervisor at a higher tier of local administration. This centralizing of supervision for a number of municipalities allows us to detect previously hidden biases in the enforcement of fiscal rules. In particular, we examine whether spatial proximity is associated with systematic differences in fiscal oversight. While geographical distance has been discussed in the context of intergovernmental transfers (Borck and Owings, 2003) and spatial favoritism related to birthplace (Carozzi and Repetto, 2016; Fiva and Halse, 2016; Hodler and Raschky, 2014) or residence (Jones et al., 2025), we provide evidence on previously unexplored spatial biases in subnational fiscal rule enforcement.[1]

The background for the reform is that despite strict fiscal rules set by the states, municipalities in several German states have persistently exhibited high deficits and cash

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[1]Geographical distance has also been studied in related enforcement contexts, including administrative enforcement in environmental regulation (He et al., 2020; Axbard and Deng, 2024), tax compliance (Kubick et al., 2017; Dong et al., 2025), and auditor performance in municipal internal control reporting (López and Rich, 2017).

loans that conflict with these regulations. Consequently, in some states discussions have emerged regarding the actual enforcement of the rules, which is delegated by the state to supervisory authorities at the local level. Hesse is a prominent case in this debate. The Hessian Court of Auditors (2015) documents that supervisory authorities partly did not fulfill their legal obligations and identifies uneven audit standards in municipal financial supervision, a finding that is corroborated by a municipal survey (Geißler et al., 2017). Yet, there is limited empirical evidence on the extent of such enforcement biases and the forces driving them. In this setting, we focus on cash loans per capita as a primary outcome. Institutionally, cash loans are intended to bridge short-term liquidity needs. Persistent reliance on them is widely viewed as an indicator of structural fiscal distress and is inconsistent with the fiscal rules. Moreover, cash loans are closely monitored by supervisory authorities through reporting obligations, making them a salient margin of enforcement.

To motivate our empirical design and guide the interpretation of the results, we outline a simple conceptual framework that highlights the incentives of municipalities and supervisors and the role of geographical proximity. It is important to note that fiscal oversight is not a mechanical process but involves substantial discretion. When a municipality fails to balance its budget, the supervisor must enforce consolidation measures tailored to the specific case in order to restore fiscal balance. This requires evaluating detailed consolidation plans submitted by the municipality and monitoring their implementation. To ensure compliance, supervisors have several instruments at their disposal, ranging from enhanced information and reporting requirements to, in extreme cases, appointing a commissioner who temporarily assumes some or all municipal functions at the municipality's expense.

Geographical distance may shape how such discretion is exercised. To illustrate the incentives on the side of municipalities, we draw on a key mechanism in the model developed by Borck and Owings (2003). While this model focuses on intergovernmental grants, it captures a general interaction between local governments and higher tiers of government. In the model, local governments lobby the central government for transfers, and the costs of lobbying depend on distance. In our setting, the object of interaction is not transfers but the enforcement of burdensome fiscal consolidation measures by a supervisor, and municipalities have incentives to limit the scope of these measures. Analogously, municipalities may attempt to influence the supervisor in their favor, and the costs of doing so can depend on distance in a broad sense – be it geographical, political, or social.

Distance also matters on the supervisor's side of the interaction. First, supervisors are accountable to a higher-level authority and may therefore have incentives to apply more conservative standards when fiscal risks are difficult to assess, to avoid being judged as having failed in their oversight duties. Distance can raise monitoring and information

costs, so that supervisors may be less willing to grant leeway to municipalities about which they have less precise information. Under this information channel, differential treatment may be efficient. Second, distance may shape personal incentives. Supervisors may be more personally affected when strict consolidation measures are imposed on municipalities in their vicinity, a consideration related to the literature on politicians' bias toward their birthplace or place of residence. These channels may also operate through non-geographic forms of proximity, such as political closeness or repeated interaction.

Taken together, these channels are therefore more likely to translate into leniency rather than stricter enforcement for proximate municipalities. In addition, proximity effects should vary systematically with factors that shape (i) municipalities' capacity to interact with and influence supervisors, (ii) supervisors' monitoring constraints and information frictions, (iii) supervisors' personal incentives, and (iv) other forms of proximity that may substitute for geography. In particular, political alignment and repeated interaction may reduce uncertainty and facilitate communication even at greater geographic distance, so that geographical proximity should play a smaller role when such substitutes are present. We therefore examine heterogeneous effects across a set of subgroups that proxy these mechanisms: municipal population size captures differences in administrative capacity and interaction costs; the number of municipalities under a county's supervision proxies supervisory workload and monitoring constraints; the supervisor's place of birth captures a narrow form of local attachment emphasized in the birthplace favoritism literature; political alignment captures political proximity; and long-term overlap in office captures repeated interaction and working routines.

We find that municipalities geographically closer to the former political supervisor experienced larger post-reform reductions in cash loans under the subsequent neutral supervisory regime. This spatial pattern is consistent with proximity-related favoritism under politically embedded oversight. In heterogeneity analyses, the proximity gradient is most evident among smaller municipalities, in counties where supervisors oversee a larger number of municipalities, and when mayors and supervisors are not politically aligned, consistent with the role of interaction costs, capacity constraints, and political proximity as a potential substitute for geographic proximity. By contrast, we find no meaningful differences across subgroups based on long-term overlap in office between mayor and supervisor or whether the county administrator was born locally. Complementary robustness checks indicate that the results are not driven by general differences between urban and rural municipalities, broader spatial characteristics, or municipality size, but are specifically related to geographical proximity to the prior supervisor.

These results contribute to various strands of literature. They shed light on the effectiveness of fiscal rules at the subnational level, see Heinemann et al. (2018), Potrafke (2025), and Blesse et al. (2026) for overviews. In particular, we add to the empirical research on the role of supervision and enforcement. Vannutelli (2022) studies changes

in Italy's fiscal monitoring system and finds that when mayors have less discretion in appointing municipal auditors, net surpluses increase. Grembi et al. (2016) focus on the effects of fiscal rules on municipal deficits and taxes and demonstrate that municipal debt can be reduced through fiscal rules when enforced by the national government. Consequently, institutions responsible for enforcing these rules have an important role (Christofzik and Kessing, 2018; Borge and Hopland, 2020; Nakazawa, 2024). Baskaran et al. (2016) as well as Bonfatti and Forni (2019) show that stringent fiscal supervision can limit political budget cycles.

While geographical distance has rarely been examined in the context of fiscal policy decisions, political distance and ideology has been a more prominent focus in research. The effects of shared party alignment were empirically examined under various conditions and for different countries (Solé-Ollé and Sorribas-Navarro, 2008; Berry et al., 2010; Sengupta, 2011; Brollo and Nannicini, 2012; Bracco et al., 2015; Baskaran and Hessami, 2017; Curto-Grau et al., 2018; Bernardo and Toro M., 2019; Bury et al., 2025). These studies demonstrate that political alignment can influence financial grants and transfer payments to municipalities under different specifications. Party ideology can also be a crucial factor. Rösel (2017) bridges the literature on fiscal oversight and the political orientation of politicians and finds that party ideology influences the strength of fiscal supervision. Similarly, Pettersson-Lidbom (2001), Mulas-Granados (2003), and Tavares (2004) show that partisanship influences debt levels and fiscal adjustments.

The paper is structured as follows. In Section 2, we describe the institutional setting, the reform and our data. Section 3 explains our identification strategy and the econometric framework. Section 4 presents the results. Section 5 concludes.

2 Institutional setting and data

2.1 Institutional environment in Hesse

We study the role of proximity for the enforcement of fiscal rules at the municipal level in Hesse. It is one of the 16 federal states in Germany, has approximately six million residents, and is one of the economically strongest states in Germany. The state is administratively structured into the three districts (*Regierungsbezirke*) Kassel, Darmstadt, and Gießen. They are further segmented into 21 counties (*Landkreise*). The counties are led by a county administrator (*Landrat*). He or she is directly elected by the citizens for a term of six years. County administrators operate in a dual role. On one hand, they serve as heads of the county administrations, shaping and overseeing the entire county administrations. Typical tasks of a county include waste disposal, provision of emergency services, and maintenance of district roads. On the other hand, they also serve as local state administrative authorities. In this context, various tasks (such as agricul-

ture, rural, and regional development) are delegated from the state level to the county administrators. One part of these delegated tasks is municipal supervision. In general, the county administrators are responsible for supervising the municipalities that belong to their county (§136(3) of the Hessian Municipal Code). This applies to 409 municipalities.[2] This delegation of authority gives rise to a principal-agent problem. We focus on a reform that shifted the responsibility for fiscal oversight to the three districts as a higher layer of local administration, see Section 2.2.

Municipalities have the right to self-government, enshrined in Article 28(2) of the German Basic Law. This right includes fiscal autonomy within the limits of state law, notably the authority to set the tax rates of two major municipal taxes, the business tax and the property tax, as well as discretion over certain expenditure decisions. However, this autonomy is constrained, for instance by a strict fiscal rule set by the state. Municipalities have to balance their budget.[3] They are permitted only to acquire loans for investment purposes and short-term liquidity needs. Notably, in Hesse, the use of these cash loans (*Kassenkredite*) has significantly increased over time, from EUR 166 per capita on average in 2006 to approximately EUR 520 per capita on average in 2012 (in prices of 2020). Although intended solely to address short-term liquidity needs, the observed surge suggests that municipalities have increasingly relied on these loans to offset deficits (Borck et al., 2015). In accordance with this, many municipalities produced persistent deficits. These trends played a role in motivating the reform under examination that targeted those municipalities with high deficits and high cash loans per capita.

The fiscal oversight of municipalities is designed to ensure that municipal budgets are planned and managed in a manner that guarantees the sustained fulfillment of municipal duties. The maximum amount of investment loans as well as cash loans must be approved by the supervisory authorities. When municipalities are unable to achieve a balanced budget, they are required to present a consolidation plan for approval by the supervisor. The supervisor has to impose appropriate conditions tailored to the specific case in order to work towards a balanced budget. However, at the onset of the 2000s, an increasing number of municipalities did not balance their budgets. In response, the state opted for a reform that also impacted fiscal oversight.

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[2]Five county-free cities and seven cities with more than 50,000 inhabitants are under the supervision of the districts.

[3]By 2009, all municipalities under consideration had transitioned to accrual accounting. Within this system, the budget is considered to be balanced if the result budget is balanced in the ordinary result, factoring in carried-over deficits or reserves, and in the cash flow statement where specific inflows and outflows are explicitly specified by law.

2.2 The reform

The "Municipal Protection Shield" (*Kommunaler Schutzschirm*) in Hesse was a debt relief program for municipalities and counties. The program for highly indebted municipalities and counties was officially adopted in 2012 through legislation and an executive order. It had a funding volume of EUR 2.8 billion. The underlying concept of the reform was for the state to assume responsibility for a share of municipal debt of participating municipalities and counties. The corresponding law determines a debt relief of 46% of the total loans that existed as of December 31, 2009.[4]

The program's objective was to enable municipalities to achieve a balanced budget by the end of the program, ensuring their solvency and future fiscal capacity. Participation in the municipal protection shield was possible through an application of the municipalities and counties. To be eligible to apply as a municipality, one of three criteria regarding the budget balance and/or the cash loans had to be met.[5] To be part in the program, municipalities had to conclude agreements in which the measures to be implemented in order to achieve budgetary balance are described (Hessami et al., 2025). Of the 86 municipalities that met the criteria, four chose not to apply for the program, and two did not sign the agreement. In the end, 80 participated in the protection shield (Merlo et al., 2023).[6] Figure 1 depicts the geographical distribution of these municipalities.

Participation in the municipal protection shield brought about not only financial support but also another change for the municipalities. We are interested in one specific element of this reform, the "upzoning" of fiscal oversight. For the participating municipalities, fiscal oversight was shifted to a higher administrative level.[7] Prior to participation in the support program, fiscal oversight over the municipalities was the responsibility of the county administrator. During the program period, fiscal oversight was the responsibility of the respective district administration *(Bezirksregierung)*. They constitute a local administrative tier above the counties. The concept of "upzoning" fiscal oversight is based on the idea that effective and entirely independent oversight might not always be ensured by the counties. Inconsistent examination and inadequate enforcement has been attributed to the counties according to the Hessian Court of Auditors (2015). This

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[4]To ensure that we are not capturing a purely mechanical effect, we control for the received payments and confirm that our results remain robust when total loans are used as the outcome variable, see Figure A3 in the Appendix.

[5]The criteria use indicators from previous years, making selection into treatment unlikely. (i) Either the mean cash loans exceeded 1,000 EUR per capita in 2009 and 2010, or (ii) the budget balance was negative and the cash loans exceeded 470 EUR per capita averaged over the years 2005 to 2009, or (iii) the municipality had no cash loans at all, but the budget deficit exceeded 200 EUR per capita averaged over the years 2005 to 2009. The participation criteria and a detailed chronology of the reform are described in detail by Keilmann et al. (2013).

[6]Our sample covers 79 municipalities as one municipality was affected by a territorial reform.

[7]With the exception of two municipalities, whose financial oversight was upzoned in December 2012, the change occurred for all other participants in 2013 (the last municipality was upzoned on March 18, 2013). Since the date rarely varies, we define the treatment date for all municipalities as 2013.

Figure 1: Participating municipalities in the municipal protection shield
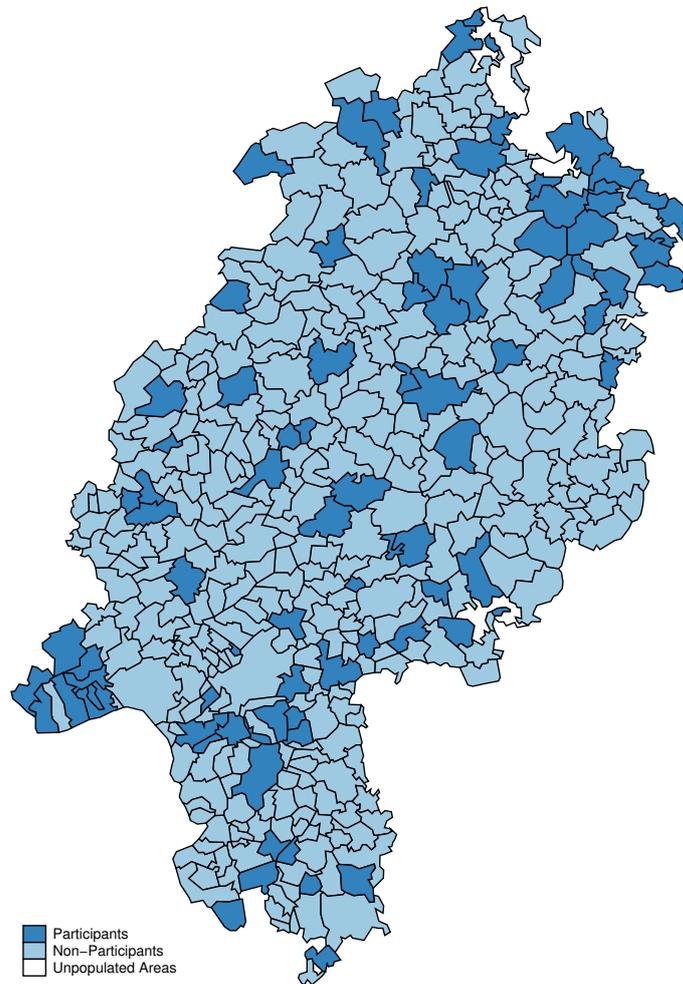
Notes: This figure shows the municipalities that were participating in the municipal protection shield in dark blue.

argument has also played a crucial role in the parliamentary procedure (Dressler, 2012). Additionally, within a survey, municipalities have described their perception that disparities occur during the monitoring process (Geißler et al., 2017).[8] These aspects were intended to be addressed through an arguably more professional and independent oversight carried out by the districts. Furthermore, the supervisory districts were intended to benefit from modified reporting obligations of the municipalities. Arguably, this should have increased the neutrality of the supervision process.

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[8]In one of the four municipalities that decided against participation, the political opposition accused the municipal government of rejecting participation due to the required shift of supervision from the politically aligned county administrator. The mayor confirmed this stance against the district administration as the supervisory authority, as reported in the local newspaper (https://www.wetterauer-zeitung.de/wetterau/florstadt-ort848700/florstadt-will-nicht-unter-schutzschirm-12096731.html).

2.3   Data and descriptive statistics

The basic balanced panel data set consists of all 406 municipalities and cities in Hesse that belong to counties over eleven years (2006 - 2016).[9] We conclude our sample in 2016 as another debt relief program called *Hessenkasse* was announced and introduced in Hesse in 2017. Under this program, additional municipalities underwent significant debt relief. Descriptive statistics are summarized in Table A1 in the Appendix.

The sample is based on administrative municipal data which is published annually by the Statistical Office of Hesse and the Federal Statistical Office. These statistics include population data, cash loans, municipal tax rates, and revenues. Furthermore, structural data such as labor market and infrastructure indicators, the migration balance and taxing capacity were extracted from the INKAR database of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR). Additional information, including the budget balance (adjusted ordinary balance) was obtained through a special evaluation conducted by the Hessian Statistical Office. This budget balance was the key indicator for a successful completion of the municipal stabilization program. By examining the budget balance, one can also anticipate the heterogeneity among Hessian municipalities. The results range from a deficit of EUR -2,657.11 to a surplus of EUR 4,384.53 per capita, highlighting the diverse financial situation of Hessian municipalities. This also corresponds to the different cash loans per capita ranging from no loans to EUR 6,800 per capita.

Before 2009, Hessian municipalities transitioned from cash accounting to accrual accounting in a staggered manner. Given the potential impact on fiscal decisions, we consider the date of this transition in our estimations.[10] The data was compiled using information by the Hessian Ministry of the Interior.

To study the influence of geographical proximity detailed distance data were collected. The calculations of travel routes in kilometers and travel time in minutes between the different institutions is based on the official address directory, which is published by the Hessian Statistical Office. Length of the route and travel time were calculated relying on the HERE API by using the Stata georoute module (Ozimek and Miles, 2011; Weber et al., 2022). We select the fastest route by car.[11] The travel duration and the length of the route between the municipalities and the responsible county administrator vary

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[9]Due to these territorial reforms, ten municipalities are not comparable across our study period. Therefore, these municipalities were excluded from the sample.

[10]As a robustness check, we narrow our sample to years after 2009, see Table A4 in the Appendix. This decision stems from the fact that before 2010, municipalities in Hesse transitioned to accrual accounting (Christofzik, 2019). While this change does not impact the comparability of data on cash loans over time, it could potentially affect other financial variables during the transition period, thus warranting caution in our analysis.

[11]Since the historical route calculations of the HERE API do not extend beyond January 2020, Wednesday, March 1, 2023, was chosen for the route calculation. The median value from five calculations was selected as the travel route or travel time.

Figure 2: Distribution of travel time from the municipality to the county administration
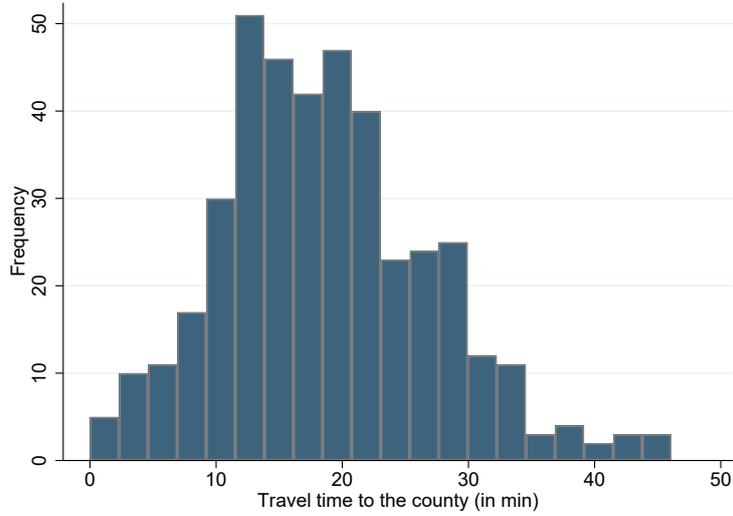
to a large extent. While the shortest route is only 0.07 km, the longest route measures 51.74 km. A similar pattern naturally emerges when measuring travel time. Travel times range from less than a minute to just over 45 minutes, with a mean travel time of 18.64 minutes (see Figure 2). Even when distances within a state appear small, practitioners cite spatial distance as affecting the availability of detailed information for fiscal oversight (Geißler et al., 2017). To facilitate the interpretation of the results, we use the inverted distance (distance multiplied by -1) in our specifications.

We enriched the dataset with structural and political data as well as information on the relationship between the mayors and their supervisor. We use political variables to examine heterogeneous effects. The Hessian Statistical Office provides information on all mayor elections. It is important to note that mayoral elections in different municipalities do not occur simultaneously but independently of each other. At the local level, some mayors are elected who run as non-partisan. Nevertheless, these politicians can still be members of a political party. When they run without the official support of their own party, they were classified as independent in the official database. We therefore hand-collected additional information to determine whether non-partisan elected mayors might still be members of a certain political party, which could be considered as an indication of their political orientation and connections. Therefore, a binary variable was created to indicate whether the county administrator and mayor belong to the same political party. Moreover, we looked at the duration of collaboration between them. We identified those municipalities where the mayor and county administrator collaborated consistently between the years 2006 and 2012. We also collected the county administrator's birthplaces.

3 Empirical strategy

3.1 Estimation model

We analyze if municipalities were affected differently by the shift of oversight to an arguably more neutral layer. Before, the political county administrator was responsible for enforcing the fiscal rule set by the state. The upzoning to the non-political districts should ensure higher neutrality in the control process. Additionally, this is reinforced by tighter municipal reporting obligations, which are intended to standardize the information available to the supervisors. Our hypothesis is that structural distortions due to geographical proximity to the former supervisor no longer occur due to the centralization of fiscal oversight. This means that if, after the reform and under the neutral supervisor, municipalities have to consolidate their budget to varying degrees, it is due to unequal treatment by the county administrator before the upzoning.

We implement a generalized difference-in-differences (DiD) design that compares the pre- to post-reform evolution of cash loans in municipalities with different distances to their county supervisor. In addition, we exploit the fact that not all municipalities were affected by the reform[12].

Our baseline results on the impact of geographical proximity on the effectiveness of fiscal supervision are based on the following equation.

$$y_{it} = \alpha_i + \beta_1 treat_{it} + \beta_2 treat_{it} \times proximity_i + \sum_k \beta_k X_{kit} + \gamma_t + \varepsilon_{it}, \qquad (1)$$

where y_{it} denotes the cash loans per capita of a municipality i in year t in prices of 2020. α_i and γ_t are municipality and year fixed effects. The binary variable $treat_{it}$ indicates whether a municipality is supervised by the district in year t. It is one for the participating municipalities in 2013 and all following years. This variable is interacted with $proximity_i$ which is measured as the inverted travel time in minutes or the inverted travel route in kilometers between the municipal administration and the (former) county administrator, and β_2 is our parameter of interest. X_{kit} represents additional explanatory variables. We include the population shares of elderly, employed, and unemployed, the migration balance, a binary variable for election years, and a binary variable indicating the transition to accrual accounting. As the reform also included payments to participating municipalities, we account for these payments. We cluster standard errors at the municipal level.[13]

To examine the dynamic effects of the shift in supervision on municipal cash loans, we estimate dynamic generalized DiD specifications for the sample of participating municipalities using the following equation.

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[12]In addition to a sample that includes only the participating municipalities, we also use a sample with all municipalities in Hesse.

[13]In a robustness check, we cluster standard errors at the county level, see Table A10 in the Appendix.

$$y_{it} = \alpha_i + \sum_{m=-2}^{2} \beta_m z_{i,t-m} + \sum_{k} \beta_k X_{kit} + \gamma_t + e_{it}, \qquad (2)$$

where y_{it} denotes again the municipal cash loans per capita. We include municipality and year fixed effects (α_i and γ_t). The treatment is represented by $z_{i,t-m}$, which equals one after the reform for those municipalities closer than the median distance to the supervisor.[14] The parameters β_m show the dynamic effects of the policy. In our baseline, we use the year before the shift of the supervisor and municipalities located farther away than the median value as reference categories. The baseline estimation window is limited to two years before and after the implementation of the reform. We bin the endpoints of the effect window. Additionally, control variables are again accounted for in the estimations through X_{kit}. The term e_{it} describes the error term. To test for heterogeneity, we estimate the equation for several subgroups.

3.2 Identifying assumptions and threats to identification

The key identifying assumption is that, absent the reform, post-reform counterfactual trends would have been parallel across municipalities. Examining pre-trends usually provides a check of this assumption's plausibility. As our aim is to investigate whether uneven treatment by the supervisor occurred before the reform, differing pre-trends could also be plausible. However, if biases are structural and persistent, variations in trends are unlikely in the periods before the reform. Hence, we test for differences in trends and levels in observables in various ways.

To gain a first impression of the evolution of cash loans before and after the reform, we split the group of participating municipalities into two groups based on the median travel time to the supervisor. The left panel of Figure 3 illustrates the mean cash loans per capita over time for those two groups. Prior to the reform, not only did the trends run parallel, but there was also little disparity between the two groups in terms of levels.[15] This does not necessarily imply that the groups were treated impartially by the supervisor. It is quite possible that the consolidation potential and the conditions established by the supervisor vary. Given the complexity of the assessment, which must account for the individual circumstances of each municipality, drawing conclusions from the available observable data is challenging. Therefore, we base our analysis on the changes brought about by the reform.[16] The average amount of cash loans increased in both groups from

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[14]We also explore a continuous variant by using travel distance as a robustness check, see Figure A2 in the Appendix.

[15]This is also supported by the results of the dynamic generalized DiD approach. A joint significance test of all pre-treatment coefficients is reported in Table A5 in the Appendix. The Wald p-value based on a F-statistic provides no indication of differential pre-trends in the setting, as the test fails to reject the joint null hypothesis that all pre-treatment coefficients are equal to zero.

[16]While the levels of cash loans are very similar across the two groups, municipalities closer to the

Figure 3: Cash loans per capita in municipalities closer and farther from the supervisor
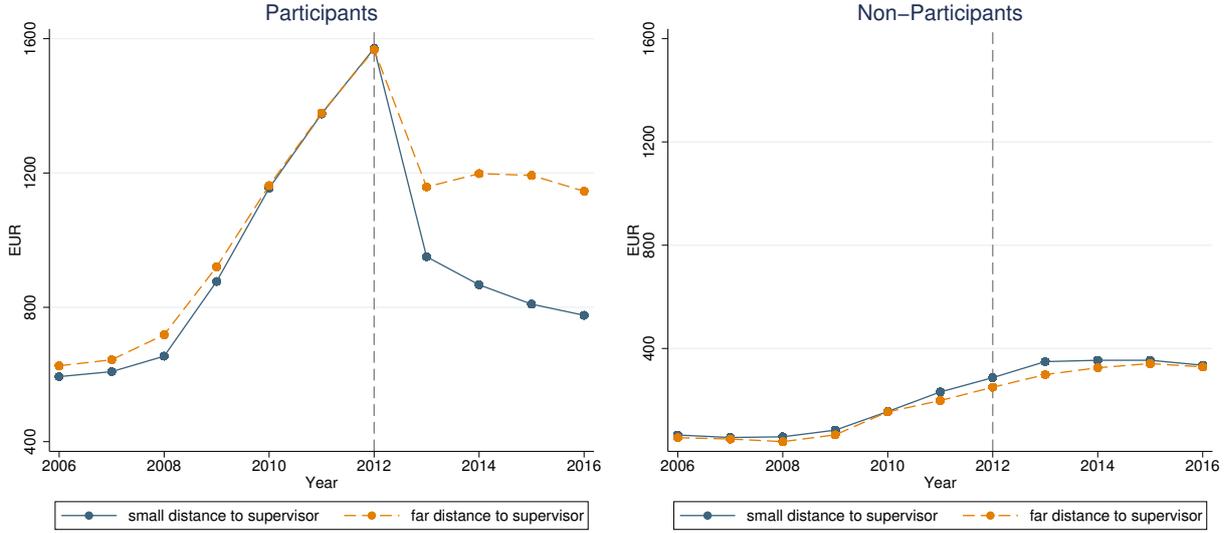
Notes: This figure shows the average cash loans (EUR per capita, in prices of 2020) of the participants (left panel) and the non-participants (right panel) in the municipal protection shield. We split both groups based on the median travel time to the county administration.

around EUR 600 per capita in 2006 to almost EUR 1,600 in 2012. Subsequently, the cash loans per capita declines in both groups, with the decrease being more pronounced in the group closer to the (former) supervisor. We conduct the same analysis for the non-participating municipalities. The right panel of Figure 3 reveals no significant differences, neither before nor after the reform. These findings do not appear to be driven by a mechanical effect or substitution between cash loans and other loans, as we find no significant effect on investment loans, but do find a significant effect on total loans (see Figure A1, Figure A3, and Table A2 in the Appendix).

In a next step, we assess if distance to the supervisor or other municipal characteristics can predict a participation in the program. We estimate binary probit regression models to study the relationship between a municipality's participation in the program and a set of variables measured in the pre-treatment year 2012. Across all model specifications, proximity to the county supervisor does not significantly predict participation, as shown in Table A3 in the Appendix. Similarly, population size does not appear to be a relevant explanatory factor. However, participating municipalities are characterized by a significantly higher share of elderly residents, higher unemployment rates, and higher property

former supervisor exhibit, on average, higher investment loans in 2012 (1,992 EUR per capita in closer municipalities versus 1,874 EUR per capita in more distant ones), resulting in higher total debt. They also display higher consumption expenditures (1,630 EUR per capita versus 1,597 EUR per capita). Based on these observable indicators, closer municipalities appear to have had weaker fiscal positions, despite having access to comparable levels of cash loans.

tax rates. When we include cash loans in the estimation, the explanatory power of the other variables diminishes, while cash loans emerge as a strong predictor of participation. This is not surprising given its role as a selection criterion. Moreover, this suggests that cash loans serve as a meaningful proxy for different structural fiscal challenges faced by municipalities. Overall, these findings suggest that participation in the program cannot be explained by the observable characteristics central to our analysis.[17] To further assess whether distance drives our main result, we conduct a series of robustness checks in Section 4.2.

We assess spatial dependence by computing Moran's I for residuals from our baseline fixed effects specification, averaged over time at the municipality level. We use several distance thresholds to define neighborhoods. For our main sample of municipalities participating in the program, we find no statistically significant spatial autocorrelation, see Table A7 in the Appendix. Repeating the analysis for the full sample yields statistically significant but very small autocorrelation. Moran's I ranges between 0.012 and 0.035, depending on the distance threshold. Values close to zero suggest limited residual spatial dependence (Nakamura et al., 2022).

Another potential concern arises from the possibility of anticipatory behavior. Although in 2010 the Prime Minister of Hesse announced a municipal protection shield, at that time no details regarding a possible upzoning of financial supervision were known. This emerged during meetings of various parties or associations involved in the process, which worked out the program details in the years 2010 and 2011. Accordingly, any announcement effects should, if anything, be confined to the period shortly before implementation. Consistent with this, we find no evidence of differential pre-trends.

The change in the oversight procedure was only one element of the reform. Hence, there may be concerns regarding the potential impact of the received payments on municipal finances in ways beyond the supervisor change. We cannot fully separate the two effects because debt relief and change of oversight happened simultaneously which is a limitation of the paper. We address this by controlling for the debt relief in our estimations and by restricting our sample in the baseline estimations to the participating municipalities. The maximum debt relief was established by law for each municipality and is based on their debt level in 2009. It is not correlated with our measures for distance. Alternatively, a more direct approach would be to examine cities with more than 50,000 inhabitants that participated in the reform. These cities were already supervised by districts before the reform, providing a potential triple difference-in-difference setting. Unfortunately, only 6 municipalities meet this criterion, which is why we refrain from pursuing this approach.

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[17]To address potential selection concerns arising from systematic differences between the treatment and control groups, our main analysis focuses on the restricted sample of participating municipalities. As an additional check, we estimate our baseline specifications for the full sample using weights based on various propensity scores. Across all approaches, the effect of proximity remains statistically significant and robust. Results are available upon request.

4 Results

4.1 Baseline results

We first assess the effects of the reform on the cash loans per capita using our baseline specification of the DiD setting as laid out in Equation (1). We estimate it for our sample which consists of the municipalities that participated in the program and experienced a centralization of oversight. In addition we use a sample with all municipalities in Hesse.[18] The corresponding estimates are provided in Table 1. In Columns (1) - (4), proximity is measured as the reverse-scaled travel time in minutes, while in Columns (5) - (8), it is measured as the reverse-scaled distance in kilometers. The results consistently reveal a negative relationship between the proximity from a municipality to the formerly responsible county administration and the cash loans per capita across all specifications.

Depending on the model specification, a one-minute shorter travel time corresponds to a relatively more substantial decline in cash loans per capita within a range of approximately EUR 10.82 to EUR 11.87. The estimations yield an effect which is statistically significantly different from zero at the 0.05 level. When considering the distance in kilometers instead of travel time, the outcomes are qualitatively similar, as shown in Columns (5) - (8). This pattern is consistent with the interpretation that municipalities closer to the supervisor were treated more leniently prior to the reform, and that the more neutral supervisory regime counteracted this favorable treatment.

Table 1: Effects of travel time and travel route on cash loans

	Specification							
	Travel time in minutes				Travel route in kilometers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatment×proximity	-10.82**	-11.87**	-10.82**	-11.18**	-10.86**	-11.80**	-10.86**	-11.25**
	(5.217)	(5.143)	(5.162)	(5.122)	(4.595)	(4.542)	(4.546)	(4.506)
treatment			-241.4	-254.0			-240.4	-253.5
			(180.4)	(177.5)			(173.4)	(170.3)
sample	participants	participants	full	full	participants	participants	full	full
controls	no	yes	no	yes	no	yes	no	yes
N	869	869	4466	4466	869	869	4466	4466

Notes: The dependent variable is cash loans per capita. Proximity is measured as the reverse-scaled travel time in minutes in Columns (1)-(4) and as the reverse-scaled distance in kilometers in Columns (5)-(8). Robust standard errors are reported in parentheses and are clustered at the municipal level. In all specifications, municipality fixed effects and year fixed effects are taken into account. The observation period covers the years 2006 to 2016. The sample varies across the specifications from *participants* (only the municipalities which participated in the municipal protection shield) in Columns (1), (2), (5), and (6) to *full* (all municipalities) in Columns (3), (4), (7), and (8). All financial data are in prices of 2020. The following control variables are included in Columns (2), (4), (6), and (8): the population share of elderly, the population share of employed, the population share of unemployed, the migration balance, debt relief, a dummy variable if there was an election in that year, and a dummy variable if there was a transition to accrual accounting. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

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[18]Excluding the six municipalities that did not participate in the program, even though they would have been eligible to apply, does not alter the main results. Selection into treatment was not possible in other forms.

Figure 4: Dynamic effect of geographical proximity on cash loans per capita
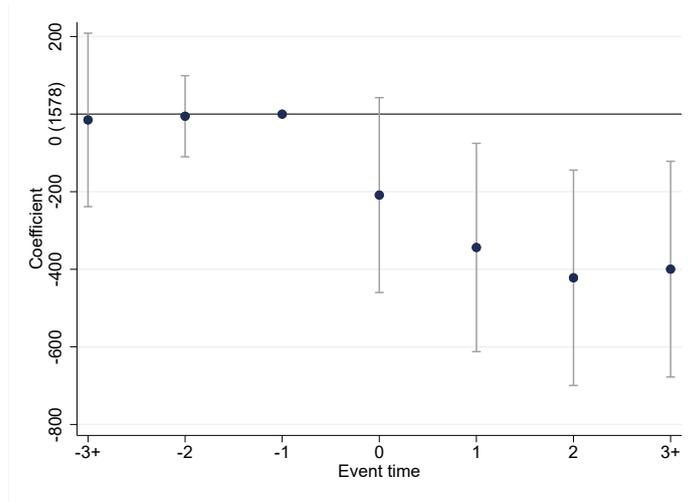
Notes: This figure shows the influence of geographical proximity on cash loans per capita (in prices of 2020) for participating municipalities located nearer to the supervisor in a dynamic generalized DiD estimation. We use the year before the shift of the supervisor and municipalities located farther away than the median value as reference categories. The effect window covers two years before and after the implementation of the reform. The binned endpoints show the remaining cumulative estimated effects. Vertical bands represent 95% confidence intervals. The estimation coefficients are reported in Table A5 in the Appendix.

Figure 4 illustrates the outcomes of the dynamic DiD estimation of Equation (2) for the participating municipalities.[19] In line with the findings presented in Table 1, we observe a notable decrease in cash loans following the change of the supervisor for municipalities situated closer to the political supervisor. This effect increases in the three years after the reform and levels off afterwards. We find an decrease by about EUR 400 per capita for those municipalities located closer, or 25% relative to the pre-treatment level. This corresponds to roughly 20% of their average total expenditure in 2012. Examining pre-treatment periods, we find no coefficients that statistically significantly deviate from zero. This finding is further supported by a joint significance test of all pre-treatment coefficients, see Table A5 in the Appendix.

4.2   Robustness checks

We subject these results to several robustness checks. First, we address concerns that our proximity measure might capture other municipal characteristics, such as general remoteness, which could impact the strictness of supervision by the county administrator. We address these concerns in five ways: (i) We analyze the effect of proximity to the nearest non-responsible county administration. In 43 municipalities, a county administration that is not responsible for a municipality is geographically closer than the actual

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[19]In Table A5 in the Appendix, we report coefficient estimates for pre- and post-treatment years.

responsible authority, illustrating that greater distance from the supervisory authority does not necessarily imply geographic remoteness.[20] While proximity to the responsible county administration has a measurable impact, we find no such effect for the nearest non-responsible county, as reported in Table A8 in the Appendix. (ii) We check whether the travel time to the non-political district supervisor has an effect on the cash loans. Proximity should not play a role due to the neutrality of the non-political supervisor. As shown in Table A8 in the Appendix, we find no effect for this case.[21] (iii) In some cases, the county administrator's office is located directly in a city that he or she supervises. We check whether these administrative centers drive the results, which they do not, as shown in Table A9 in the Appendix. (iv) Furthermore, we exclude extreme cases, specifically the nearest 5% and farthest 5% of municipalities in each specification. (v) To further account for unobserved features such as urban centrality, infrastructure quality, and administrative capacity, we include a set of additional control variables in a robustness check. As demonstrated in Table A9 in the Appendix, the results remain statistically significant. Overall, these checks indicate that the observed effect is not solely due to geographical location, but rather arises from proximity to the former supervisor. However, our heterogeneity analysis suggests that the effect may be more pronounced in smaller municipalities.

In addition, we explore the role of counties through three robustness checks. First, we introduce county-specific year fixed effects. This approach limits identification to within-county variation and nets out general differences in leniency of enforcement across county administrators. Simultaneously, it accounts for heterogeneity across counties in terms of economic or socio-demographic development. The results remain robust, with the coefficients even larger than in our baseline estimations, see Columns (1) and (2) in Table A10 in the Appendix. Secondly, some counties also participated in the program. One might be concerned that municipalities located in these counties are treated not only because of their own participation but also due to the involvement of their county. To address this concern, we include an additional interaction term in our specification. Our results show no significant differences in effects, as indicated in Columns (3) and (4) of Table A10 in the Appendix. Thirdly, we clustered standard errors at the county level to account for potential correlation among municipalities sharing the same supervisory authority. The results remain robust and the main effect stays statistically significant, see Columns (5) and (6) in Table A10 in the Appendix.

We also substitute the dependent variable and examine the budget balance per capita instead of the cash loans per capita, the second variable used as a criterion for participation in the program. Consistent with our baseline results, we find a positive relationship between proximity and the budget balance. Closer municipalities not only experience larger post-reform reductions in cash loans but also also exhibit improvements in their

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[20]This also applies to 7 municipalities participating in the program.
[21]The result also remains robust when using the logarithm of distance instead.

budget balances, as shown in Table A11 in the Appendix. Given that cash loans may also reflect short-term liquidity needs, this pattern is consistent with tighter enforcement rather than cyclical liquidity management.

In our dynamic generalized DiD setting, even with an extension of the effect window to five years before the reform, no discernible pre-trend is evident, see the left panel of Figure A2 in the Appendix. This is further supported by a joint significance test of all pre-treatment coefficients, reported in Table A5 in the Appendix. We also conduct the same analysis using the continuous proximity instead of a binary variable for nearer municipalities in the dynamic setting. In this specification as well, we observe that nearer municipalities experience a persistent decrease in cash loans (right panel of Figure A2 in the Appendix). Furthermore, we conduct Placebo tests using fiscal outcomes that municipalities cannot directly influence in the short run and that should therefore be largely unaffected by supervisory enforcement. We find no statistically significant effects on the municipal VAT share per capita or on equalization transfers per capita, see Figure A4 in the Appendix.

The volume of debt relief is based on total loans in 2009. To exclude the possibility of capturing only a compositional or mechanical effect, instead of controlling for the received debt relief as in our baseline estimations, we use total loans as the outcome variable. As shown in Figure A3 in the Appendix, the same pattern emerges for this variable, whether measured in per capita terms or in logarithmic form.

4.3 Heterogeneity analysis

Our conceptual framework implies that the role of geographical proximity should vary systematically with factors that shape (i) municipalities' capacity to interact with and influence supervisors, (ii) supervisors' monitoring constraints and information frictions, (iii) supervisors' personal incentives, and (iv) non-geographic forms of proximity that may substitute for geography. We therefore examine heterogeneous effects across a set of municipal and supervisory characteristics that proxy these mechanisms.

We proxy differences in municipal administrative capacity and interaction costs using population size. Supervisory workload and monitoring constraints are operationalized by the number of municipalities under a county's supervision. For personal incentives we draw on local attachment, proxied by the supervisor' place of birth. Political alignment between mayor and supervisor captures political proximity, while a long-term overlap in office between mayor and county administrator captures repeated interaction and established working routines.

While our sample size limits precision and does not allow for sharp discrimination between all channels, the heterogeneity patterns provide structured evidence on which mechanisms are more consistent with the data. To implement this analysis, we estimate

our dynamic generalized DiD specification separately by subgroup. Figure 5 reports the resulting event-study estimates. In Table A6 in the Appendix, we additionally report the corresponding coefficient estimates for the subgroups and specifications for all participating municipalities with explicit interaction terms between geographical proximity and the subgroup indicators.

Population. We divide the sample of participating municipalities into two subgroups based on the median population size. Municipal population size can capture differences in administrative capacity and the ability to engage with supervisors. Smaller municipalities typically have fewer specialized staff and may face higher transaction costs in preparing, communicating, and defending consolidation plans, implying a stronger role for proximity if interaction costs matter. Apart from that, from the supervisor's perspective, enforcement decisions may also be more salient for larger municipalities, whereas stricter consolidation requirements imposed on small municipalities may entail lower expected reputational, personal or political costs.

For larger municipalities, geographical distance does not appear to influence outcomes. Although the difference between size groups is not statistically significant (see Table A6 in the Appendix), the estimates for smaller municipalities are strong and statistically significant: those located farther from the county supervisor face lower consolidation requirements after the reform, suggesting they were subject to more stringent requirements beforehand. This pattern is consistent with two complementary mechanisms. From the municipality's perspective, greater distance raises the costs of interaction and persuasion when documenting that consolidation options have been exhausted. From the supervisor's perspective, distance may exacerbate information and communication frictions and, at the same time, lower expected backlash, making supervisors less inclined to grant leeway to small, remote municipalities.

Number of municipalities in county. The number of municipalities under a county's supervision proxies supervisory workload and monitoring constraints. When supervisory capacity is limited, gathering and processing information on municipal fiscal conditions becomes more difficult, and information asymmetries are harder to resolve. In such settings, supervisors may rely more on accessible information and apply more cautious standards in cases of uncertainty, which can translate into stricter enforcement for municipalities that are farther away.

Consistent with this logic, the results indicate that municipalities in counties with a larger number of municipalities face stricter oversight by the county supervisor when they are farther away. By contrast, geographical proximity is not a significant factor when the number of municipalities in a county is low. This pattern suggests that limited administrative capacity may require prioritization.[22]

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[22]One way to explore this further is to consider the financial resources allocated to fiscal oversight within the counties. We requested this information from the counties. However, due to differing delineations, it is not possible to use these data.

Political alignment with supervisor. Political alignment between the mayor and the supervisor captures a form of political proximity that may facilitate communication and increase trust, thereby reducing uncertainty. If political proximity substitutes for geographical proximity in this way, distance should matter less when mayors and supervisors are aligned. Consistent with this interpretation, we find no statistically significant effect of geographical proximity within the group of politically aligned mayors and supervisors, whereas proximity is associated with differential oversight in the non-aligned subgroup.

Long-term mayor-supervisor collaboration. Another dimension of proximity is the continuity of working relationships, which can foster routines and familiarity beyond mere geography. The length of the working relationship between a mayor and the county supervisor may therefore attenuate the role of geographic distance: established routines can reduce interaction costs and uncertainty even when municipalities are physically farther away. We define a long-term collaboration between mayor and supervisor as a continuous overlap in office during the pre-reform period from 2006 to 2012. We find that geographical distance has a statistically significant effect in both subgroups, and the estimated effects are not statistically distinguishable between them. One possible explanation is that oversight depends not only on the two political actors, the mayor and the county supervisor, but also on the respective administrative staff, in particular the municipal treasurer. We do not observe or measure these administrative relationships or other social ties in the data and therefore cannot explore this channel directly.

Administrator born in region. The supervisor's birthplace in a municipality's vicinity captures a narrow form of local attachment emphasized in the birthplace favoritism literature. Drawing on this perspective, one might expect that when the county administrator is born in or near a municipality, oversight is shaped more by local attachment than by geographical proximity per se. To examine this, we collected data on the birthplaces of county administrators. We define an indicator that equals one if a municipality is either the administrator's place of birth or directly borders it, which applies to about 15% of the sample. The results, however, indicate no statistically significant differences in the effect of distance between municipalities in regions with a locally born administrator and those without.

County as participant. We also assess whether the results are driven by whether the county itself participated in the program. Our analysis focuses on municipalities that are part of counties and were reassigned to the higher-tier supervisor. Some of these municipalities are located in counties that also participated in the program. To examine whether this institutional environment matters, we split the sample into municipalities in participating versus non-participating counties. The estimated proximity effect is qualitatively similar across the two subgroups and does not exhibit a clear differential pattern.

Figure 5: Heterogeneity analysis: Dynamic effect of geographical proximity on cash loans per capita for subgroups
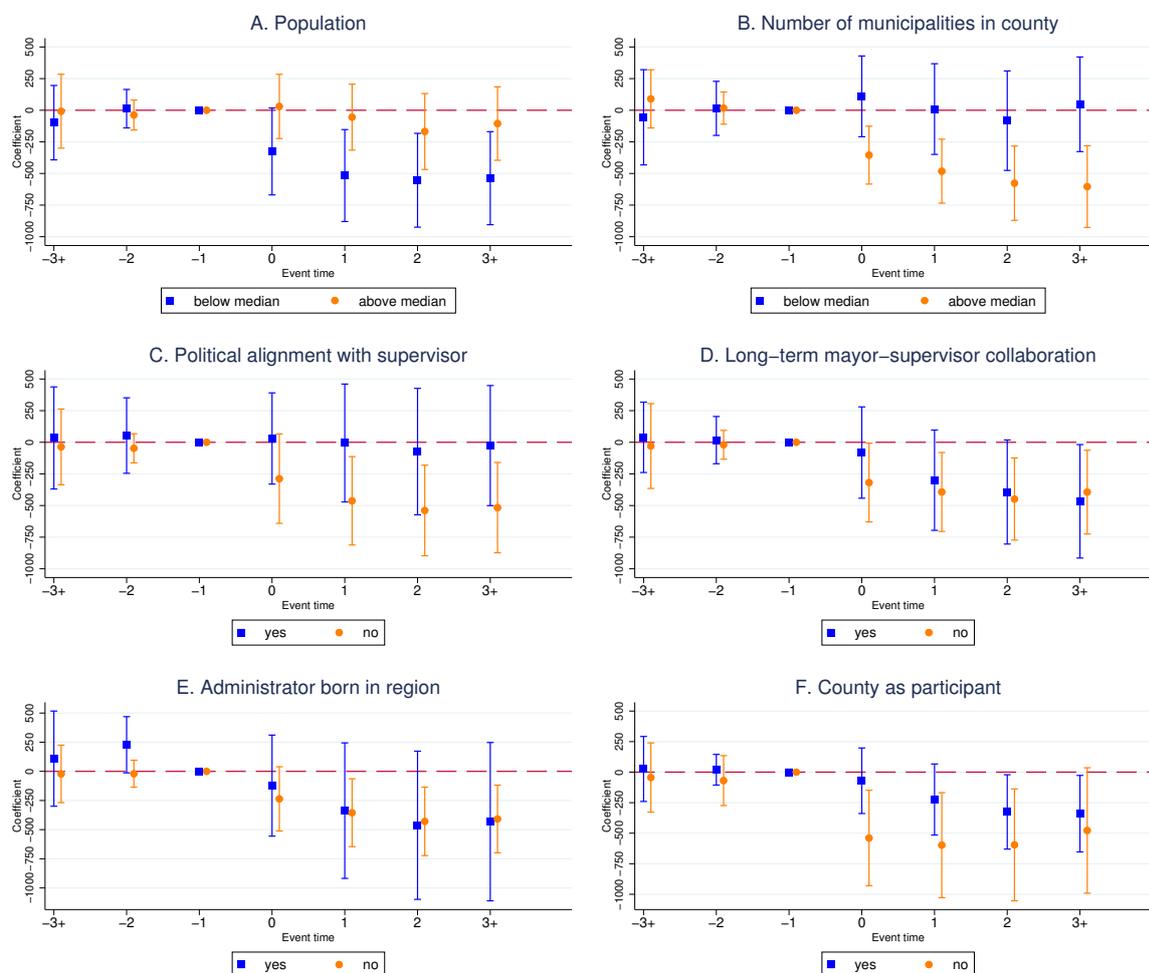
Notes: This figure shows the influence of geographical proximity on cash loans per capita (in prices of 2020) for participating municipalities located nearer to the supervisor in a dynamic generalized DiD estimation for different subgroups. Grouping in Panels A and B follows a median split. Panel C groups municipalities by political alignment, defined as the mayor belonging to the same political party as the supervisor. A long-term mayor-supervisor collaboration, which is used for grouping in Panel D, is assumed when mayor and county administrator do not change during the period from 2006 to 2012. Panel E groups the municipalities based on whether they are the birthplace of the county administrator or border the birthplace. In Panel F, municipalities were distinguished based on whether their county participated in the municipal protection shield or not. We use the year before the shift of the supervisor and municipalities located farther away than the median value as reference categories. The effect window covers two years before and after the implementation of the reform. The binned endpoints show the remaining cumulative estimated effects. Vertical bands represent 95% confidence intervals.

5 Conclusions

We provide empirical evidence on the influence of geographical proximity on the enforcement of fiscal rules. To do so, we exploit a reform in the German state of Hesse that partly reassigned responsibility for supervising and enforcing a balanced budget rule from politically affiliated county administrators to non-political fiscal supervisors at a higher administrative tier. A shorter travel time is associated with a higher consolidation requirement afterwards, as measured by cash loans per capita and the budget balance. This pattern is consistent with proximity-related favoritism under politically embedded oversight. Heterogeneity analyses indicate that the spatial gradient is most evident among smaller municipalities, in counties where supervisors oversee more municipalities, and when supervisors and mayors are not politically aligned. Taken together, these findings suggest that proximity matters when interaction is relatively costly and monitoring capacity is constrained, while political alignment may substitute for geographic proximity.

Importantly, the reform involved both depoliticization and centralization of oversight. Before and after the reform, the state delegated enforcement to local supervisory bodies, but at different administrative levels and under different incentive structures: oversight shifted from politically affiliated county administrators to non-political supervisors at a higher administrative tier. Viewed through the lens of the delegation literature, this represents a shift from political to bureaucratic actors, which plausibly reduced direct electoral and partisan incentives. At the same time, centralizing oversight may come at the cost of lower local information and responsiveness. Our results suggest that geographic proximity facilitated favoritism in a setting where supervisors were politically embedded. While local ties can generate information advantages, such advantages may also enable selective enforcement in particular when political incentives are present.

Distinguishing between depoliticization and centralization is therefore central for interpretation and policy implications. An effective institutional design could aim to limit political incentives in enforcement while preserving equal access to relevant local information. Improving reporting standards, transparency, and information-sharing may mitigate proximity-related biases, while greater independence or depoliticization may be more effective when electoral and partisan incentives drive leniency. These insights are relevant for current European debates on independent fiscal institutions and the implementation of the reformed EU fiscal framework, where policymakers face similar trade-offs between impartial enforcement, access to information, and accountability.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used ChatGPT in order to improve the readability and language. After using this tool, the authors reviewed and edited the text as needed and take full responsibility for the content of the publication.

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Appendix

## Additional figures

Figure A1: Investment loans per capita in municipalities closer and farther from the supervisor
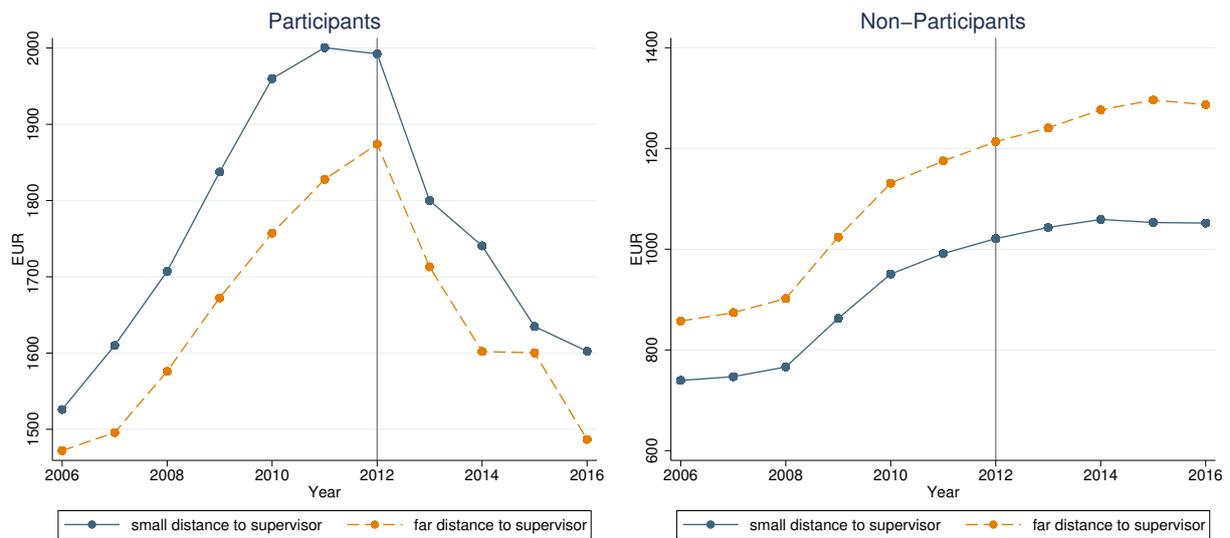
Notes: This figure shows the average investment loans (EUR per capita, in prices of 2020) of the participants (left panel) and the non-participants (right panel) in the municipal protection shield. We split both groups based on the median travel time to the county administration.

Figure A2: Robustness check: Extension of effect window and continuous proximity measure
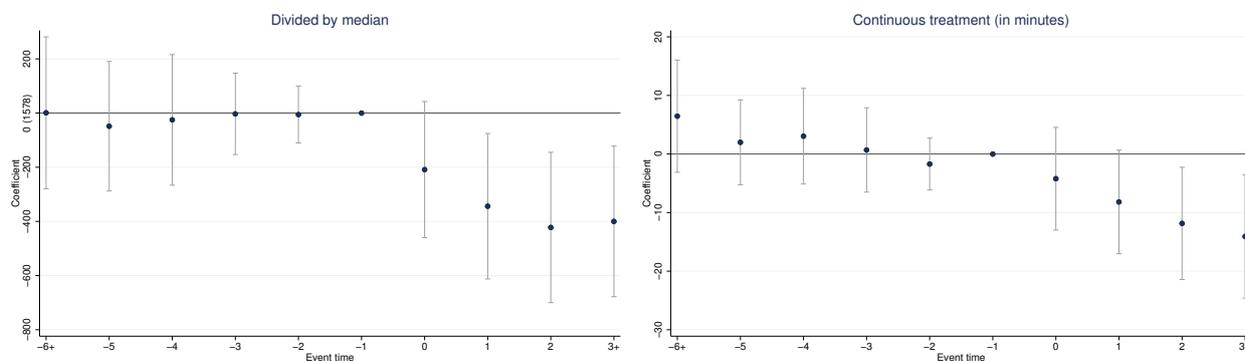
Notes: This figure shows the influence of geographical proximity on cash loans per capita (in prices of 2020) for participating municipalities in a dynamic generalized DiD estimation. In the left panel, we use the year before the shift of the supervisor and municipalities located farther away than the median value as reference categories. The estimation coefficients are reported in Table A5 in the Appendix. In the right panel, proximity is measured continuously as the reverse-scaled travel time in minutes. The effect window covers five years before and two years after the implementation of the reform. The binned endpoints show the remaining cumulative estimated effects. Vertical bands represent 95% confidence intervals.

Figure A3: Robustness check: Total loans
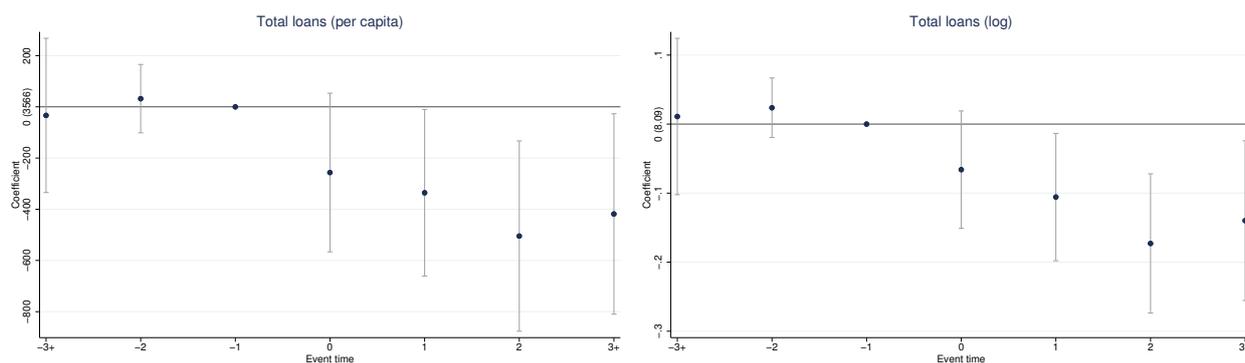
Notes: This figure shows the influence of geographical proximity on total loans for participating municipalities in a dynamic generalized DiD estimation. In the left panel, total loans are measured as the sum of cash loans and investment loans per capita (in prices of 2020). For the right panel, the same variable is expressed in logarithms. We use the year before the shift of the supervisor and municipalities located farther away than the median value as reference categories. The effect window covers two years before and two years after the implementation of the reform. The binned endpoints show the remaining cumulative estimated effects. Vertical bands represent 95% confidence intervals.

Figure A4: Robustness check: Placebo test - Effects of travel time on VAT share and equalization transfers
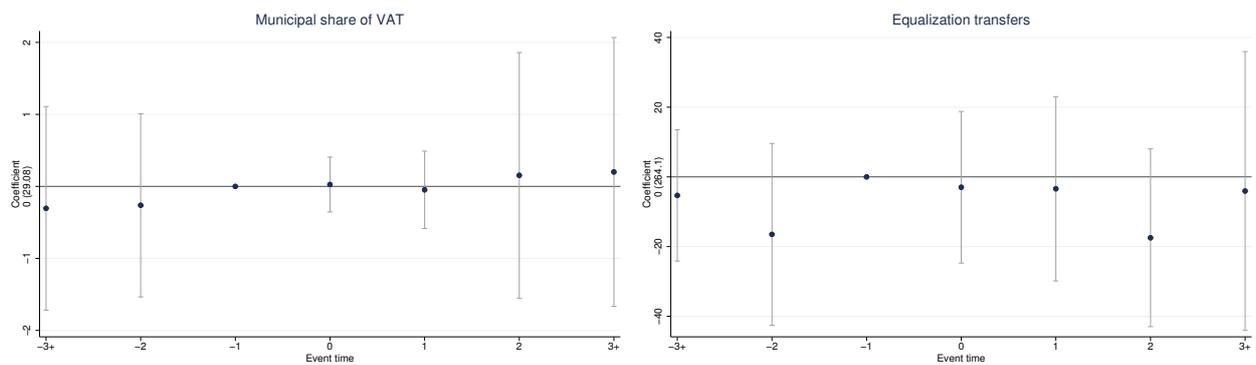
Notes: This figure shows the influence of geographical proximity on two different fiscal outcomes for participating municipalities in a dynamic generalized DiD estimation. In the left panel, the municipal share of VAT per capita (in prices of 2020) is the dependent variable. The right panel presents the influence of geographical proximity on the equalization transfers per capita (in prices of 2020). We use the year before the shift of the supervisor and municipalities located farther away than the median value as reference categories. The effect window covers two years before and two years after the implementation of the reform. The binned endpoints show the remaining cumulative estimated effects. Vertical bands represent 95% confidence intervals.

Additional tables

Table A1: Summary Statistics (2006–2016)

Variable		Mean	Std. Dev.	Min.	Max.
Population	Metric	10,247.27	7,923.68	1,037	45,849
Population share over age of 64	Percent	0.21	0.02	0.14	0.32
Population share of employed	Percent	0.35	0.03	0.26	0.47
Population share of unemployed	Percent	0.03	0.01	0.01	0.08
Population density^a	Metric	317.33	352.11	38.94	2,394.55
Full-time equivalents in the public sector	Metric	81.38	74.62	5	460
Migration balance	Per 1,000 capita	1.42	10.89	-162.12	172.8
Route to county administration	Kilometers	18.87	10.1	0.07	51.74
Time to county administration	Minutes	18.64	8.27	0.83	46.08
Route to district administration	Kilometers	49.4	27.07	4.55	136.26
Time to district administration	Minutes	38.91	16.15	8.17	85.25
Time to non-resp. county admin.^{b}	Minutes	31.59	10.05	12.37	75.42
Debt relief^c	EUR per capita	193.49	454.22	0	2,595.04
Number of municipalities in county	Metric	22.44	4.98	13	31
Cash loans	EUR per capita	354.57	541.88	0	6,800.67
Budget balance	EUR per capita	-60.81	323.12	-2,657.11	4,384.53
Investment loans	EUR per capita	1,148.46	919.81	0	7,796.37
Business tax multiplier	Percent	340.76	32.64	250	480
Property tax multiplier	Percent	304.88	81.48	140	960
Municipal share of VAT	EUR per capita	29.49	22.91	3.12	309.52
Taxing capacity^d	EUR per capita	851.73	533.73	-358	9,728
Equalization transfers	EUR per capita	230.75	130.57	0	1,485.81
Building permits	Per 1,000 capita	1.71	1.8	0	30.4
Car density in county	Per 1,000 capita	591.66	39.45	490.94	758.34
Political alignment^e	Binary	0.4	0.49	0	1
Long-term collaboration^f	Binary	0.32	0.47	0	1
Supervisor birthplace^g	Binary	0.16	0.37	0	1
Participating municipality	Binary	0.19	0.39	0	1
County participant^h	Binary	0.67	0.47	0	1
Accrual accounting	Binary	0.78	0.42	0	1

Notes: All financial data are in prices of 2020. ^{a}Population density is based on the 2018 municipal area (in km^2). ^{b}Travel time from a municipality to the closest non-responsible county administration. ^{c}Maximum debt relief according to the Annex of the Municipal Protection Shield Law. ^{d}To calculate municipal tax capacity, standardized tax multipliers are applied. In three cases within our sample, tax capacity is negative, as reimbursements of business tax payments exceeded business tax revenues. ^{e}The binary variable takes the value 1 if in 2012 the county administrator and the mayor belong to the same political party. ^{f}The binary variable takes the value 1 if the mayor and the supervisor do not change during the years 2006 to 2012. ^{g}The binary variable takes the value 1 for municipalities where the supervisor was born or those that border the supervisor's birthplace. ^{h}The binary variable takes the value 1 for municipalities located in counties that participated in the municipal protection shield.

Table A2: Effects of travel time and travel route on investment loans

	Specification							
	Travel time in minutes				Travel route in kilometers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatment×proximity	-5.058	-4.264	-5.058	-3.967	-6.824	-5.942	-6.824	-5.624
	(8.904)	(9.129)	(8.810)	(9.003)	(8.231)	(8.514)	(8.144)	(8.364)
treatment			-78.26	-44.25			-109.0	-73.45
			(248.7)	(246.1)			(233.9)	(232.0)
sample	participants	participants	full	full	participants	participants	full	full
controls	no	yes	no	yes	no	yes	no	yes
N	869	869	4466	4466	869	869	4466	4466

Notes: The dependent variable is investment loans per capita. Proximity is measured as the reverse-scaled travel time in minutes in Columns (1)-(4) and as the reverse-scaled distance in kilometers in Columns (5)-(8). Robust standard errors are reported in parentheses and are clustered at the municipal level. In all specifications, municipality fixed effects and year fixed effects are taken into account. The sample varies across the specifications from *participants* (only the municipalities which participated in the municipal protection shield) in Columns (1), (2), (5), and (6) to *full* (all municipalities) in Columns (3), (4), (7), and (8). All financial data are in prices of 2020. The following control variables are included in Columns (2), (4), (6) and (8): the population share of elderly, the population share of employed, the population share of unemployed, the migration balance, debt relief, a dummy variable if there was an election in that year, and a dummy variable if there was a transition to accrual accounting. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A3: Determinants of the participation of municipalities in the debt relief program

	Specification					
	(1)	(2)	(3)	(4)	(5)	(6)
proximity	-0.00105	0.00125	0.00283	0.00401	0.00211	0.0250
	(0.00833)	(0.00882)	(0.00904)	(0.00934)	(0.00961)	(0.0160)
population (log)		-0.0825	0.0439	-0.157	-0.205	-0.406
		(0.103)	(0.114)	(0.130)	(0.161)	(0.277)
elderly (population share)			15.62***	14.59***	9.198**	8.104
			(3.269)	(4.075)	(4.435)	(7.665)
migration balance (population share, per 1,000)			-0.00971	-0.00695	-0.00463	-0.0202
			(0.00907)	(0.00935)	(0.00955)	(0.0160)
employed (population share)				-0.557	-1.744	-6.468
				(4.592)	(4.790)	(6.943)
unemployed (population share)				47.51***	39.64***	32.90
				(12.65)	(13.27)	(21.96)
tax multiplier business (log)					-0.889	-0.0838
					(1.056)	(1.884)
tax multiplier property (log)					2.666***	0.498
					(0.731)	(1.294)
tax capacity (log)					-0.197	0.170
					(0.288)	(0.537)
cash loans (level)						0.00301***
						(0.000348)
N	406	406	406	406	406	406
pseudo R^2	0.000	0.002	0.063	0.105	0.145	0.668

Notes: The table shows regression coefficients of probit models. The dependent variable is being participant in the program. The independent variables are measured in the year prior to the reform. Column (1) includes proximity, measured as the reverse-scaled travel time in minutes. Column (2) adds population size (log-transformed). Column (3) introduces the population share aged 65 and over, as well as the migration balance per 1,000 inhabitants. Column (4) incorporates the share of social security-contributing employees by place of residence and the unemployment rate. Column (5) adds three variables related to local tax policy: the tax multipliers for business and property taxes, and the taxing capacity per inhabitant. Column (6) includes the level of cash loans per inhabitant. Standard errors are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A4: Effects of travel time and travel route on cash loans in a shorter sample

| | Specification | | | | | | | |
| | Travel time in minutes | | | | Travel route in kilometers | | | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatment×proximity	-8.890*	-9.704*	-8.890*	-9.087*	-8.525*	-9.106**	-8.525**	-8.695**
	(4.875)	(4.878)	(4.822)	(4.807)	(4.377)	(4.394)	(4.330)	(4.321)
treatment			-351.7*	-353.4*			-343.7*	-344.7*
			(201.5)	(200.0)			(194.2)	(192.7)
observation period	2010-2016	2010-2016	2010-2016	2010-2016	2010-2016	2010-2016	2010-2016	2010-2016
sample	participants	participants	full	full	participants	participants	full	full
controls	no	yes	no	yes	no	yes	no	yes
N	553	553	2842	2842	553	553	2842	2842

Notes: The dependent variable is cash loans per capita. Proximity is measured as the reverse-scaled travel time in minutes in Columns (1)-(4) and as the reverse-scaled distance in kilometers in Columns (5)-(8). Robust standard errors are reported in parentheses and are clustered at the municipal level. In all specifications, municipality fixed effects and year fixed effects are taken into account. The shorter observation period covers the years 2010 to 2016. The sample varies across the specifications from *participants* (only the municipalities which participated in the municipal protection shield) in Columns (1), (2), (5), and (6) to *full* (all municipalities) in Columns (3), (4), (7), and (8). All financial data are in prices of 2020. The following control variables are included in Columns (2), (4), (6), and (8): the population share of elderly, the population share of employed, the population share of unemployed, the migration balance, debt relief, a dummy variable if there was an election in that year, and a dummy variable if there was a transition to accrual accounting. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A5: Dynamic effect of geographical proximity on cash loans per capita, coefficient estimates over time

| | Specification | |
| | estimates | estimates (extended time period) |
	(1)	(2)
Periode -6+		0.992
		[-279.4,281.3]
Periode -5		-48.56
		[-287.8,190.7]
Periode -4		-25.11
		[-266.2,216.0]
Periode -3(+)	-14.92	-3.058
	[-238.5,208.7]	[-153.3,147.2]
Periode -2	-5.432	-5.599
	[-110.0,99.14]	[-110.4,99.19]
Periode 0	-208.7	-208.8
	[-459.8,42.48]	[-460.1,42.48]
Periode 1	-343.7**	-344.0**
	[-612.3,-75.19]	[-612.7,-75.36]
Periode 2	-421.9***	-422.3***
	[-699.5,-144.3]	[-699.9,-144.7]
Periode 3	-399.6***	-400.0***
	[-677.6,-121.6]	[-678.0,-122.1]
Pre-trend F-test (p-value)	0.01	0.19
	(0.99)	(0.96)
sample	participants	participants
controls	yes	yes
N	869	869

Notes: The table shows the corresponding coefficients to the results of the dynamic generalized DiD estimation of Figure 4 (Column 1) and of the left panel of Figure A2 (Column 2). It shows the influence of geographical proximity on cash loans per capita (in prices of 2020) for participating municipalities. In the dynamic generalized DiD estimation the year before the shift of the supervisor and municipalities located farther away than the median value as reference categories. In Column 1, the effect window covers three years before and two years after the implementation of the reform. In Column 2, the effect window covers five years before and two years after the implementation of the reform. The binned endpoints show the remaining cumulative estimated effects. 95% confidence intervals are reported in brackets. The pre-trend F-test shows the results of the joint significance test of all pre-treatment coefficients. The Wald p-value based on a F-statistic is reported in parentheses. An insignificant result indicates that there is no evidence of differential pre-trends. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A6: Effect of geographical proximity on cash loans per capita, heterogeneity analysis

	Specification		
	subsample 1	subsample 0	interaction coefficient
	(1)	(2)	(3)
A. Population	-413.1***	-62.53	-246.3
1=below median; 0=above median	(146.6)	(123.4)	(190.7)
B. Number of municipalities in county	-569.4***	60.08	-560.4***
1=above median; 0=below median	(121.2)	(146.9)	(186.7)
C. Political alignment with supervisor	-49.40	-417.9***	279.4
1=aligned; 0= not aligned	(156.2)	(134.8)	(223.1)
D. Long-term mayor-supervisor collaboration	-339.7*	-365.2**	83.79
1=long-term collaboration; 0=no long-term collaboration	(166.1)	(149.1)	(231.9)
E. Administrator born in region	-435.4	-337.8***	73.24
1=born in region; 0=not born in region	(328.5)	(117.3)	(339.6)
F. County as participant	-260.0**	-511.9**	206.5
1=county is participant; 0=county is no participant	(125.6)	(225.3)	(235.5)

Notes: This table shows the influence of geographical proximity on cash loans per capita (in prices of 2020) for participating municipalities located nearer to the supervisor in a dynamic generalized DiD estimation for different subgroups. Columns (1) and (2) report subgroup-specific estimates. Column (3) reports the coefficient on the interaction term between geographical proximity and the subgroup indicator from Column (1) from a specification estimated on the full sample of program participants. Grouping in Panels A and B follows a median split. Panel C groups municipalities by political alignment, defined as the mayor belonging to the same political party as the supervisor. A long-term mayor-supervisor collaboration, which is used for grouping in Panel D, is assumed when mayor and county administrator do not change during the period from 2006 to 2012. Panel E groups the municipalities based on whether they are the birthplace of the county administrator or border the birthplace. In Panel F, municipalities were distinguished based on whether their county participated in the municipal protection shield or not. Robust standard errors are reported in parentheses and are clustered at the municipal level. In all specifications, municipality fixed effects and year fixed effects are taken into account. The following control variables are included in all specifications: the population share of elderly, the population share of employed, the population share of unemployed, the migration balance, debt relief, a dummy variable if there was an election in that year, and a dummy variable if there was a transition to accrual accounting. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A7: Moran's I for different distances

distance (in km)	Specification	
	restricted sample	full sample
	(1)	(2)
10.723	0.023	0.035
	(0.150)	(0.032)
20	-0.075	0.028***
	(0.098)	(0.018)
30	-0.087	0.022***
	(0.066)	(0.012)
40	-0.077	0.023***
	(0.055)	(0.01)
50	-0.065	0.017***
	(0.046)	(0.008)
60	-0.057	0.014***
	(0.04)	(0.007)
70	-0.04	0.012***
	(0.036)	(0.006)

Notes: This table reports results for Moran's I, an index bounded between minus one and one, where a value of zero indicates the absence of spatial autocorrelation. The distance threshold defining neighboring municipalities is reported in the first column. The first distance threshold of 10.723 kilometers corresponds to the minimum distance at which each municipality can be assigned at least one neighboring municipality. Column (1) presents the results for a *restricted sample* (only the municipalities which participated in the municipal protection shield). Column (2) shows the results for the *full sample* of municipalities. Moran's I is computed using the mean residuals aggregated over the entire sample period at the municipality level from our baseline estimation with year and municipality fixed effects. The following control variables are included in all specifications: the population share of elderly, the population share of employed, the population share of unemployed, the migration balance, debt relief, a dummy variable if there was an election in that year, and a dummy variable if there was a transition to accrual accounting. Standard errors are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A8: Robustness check: Effects of travel time to the non-political district and the closest non-responsible county administration on cash loans

| | Specification | | | | | | | |
| | non-responsible county | | | | non-political district | | | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatment×proximity$_{non-resp}$	-6.082	-5.491	-3.472	-2.841				
	(6.637)	(6.536)	(5.974)	(5.986)				
treatment×proximity$_{non-pol}$					1.318	1.418	5.079	5.119
					(3.472)	(3.491)	(4.012)	(4.028)
treatment×proximity			-11.05**	-10.44**			-14.49**	-13.93**
			(4.830)	(4.827)			(5.557)	(5.630)
treatment		-188.8		-311.7		0.193		-117.9
		(218.4)		(215.3)		(195.7)		(190.8)
sample	participants	full	participants	full	participants	full	participants	full
controls	yes	yes	yes	yes	yes	yes	yes	yes
N	869	4466	869	4466	869	4466	869	4466

Notes: The dependent variable is cash loans per capita. Proximity is measured as the reverse-scaled travel time in minutes. In Columns (1)-(4), the variable $proximity_{non-resp}$ represents the reverse-scaled travel time to the closest non-responsible county administration. In Columns (5)-(8), the variable $proximity_{non-pol}$ represents the reverse-scaled travel time to the non-political supervisor. Robust standard errors are reported in parentheses and are clustered at the municipal level. In all specifications, municipality fixed effects and year fixed effects are taken into account. The sample varies across the specifications from *participants* (only the municipalities which participated in the municipal protection shield) in Columns (1), (3), (5), and (7) to *full* (all municipalities) in Columns (2), (4), (6), and (8). All financial data are in prices of 2020. The following control variables are included in all specifications: the population share of elderly, the population share of employed, the population share of unemployed, the migration balance, debt relief, a dummy variable if there was an election in that year, and a dummy variable if there was a transition to accrual accounting. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A9: Robustness check: Effects of travel time on cash loans, county cities, outliers and additional control variables

| | Specification | | | | | |
| | county city | | outliers | | additional controls | |
	(1)	(2)	(3)	(4)	(5)	(6)
treatment×proximity	-13.23**	-12.45**	-18.70*	-16.87*	-13.16**	-11.85**
	(5.841)	(5.783)	(9.648)	(9.197)	(4.998)	(5.085)
treatment×proximity×county city	-41.61	-39.11				
	(62.44)	(60.94)				
treatment		-285.3		-279.8		-269.1
		(186.7)		(246.7)		(175.9)
sample	participants	full	participants	full	participants	full
controls	yes	yes	yes	yes	yes	yes
N	869	4466	693	4037	869	4466

Notes: The dependent variable is cash loans per capita. Proximity is measured as the reverse-scaled travel time in minutes. In Columns (1) and (2), the variable county city is one if the municipality is the administrative center of a county. In Columns (3) and (4), the 5% of all Hessian municipalities that are located nearest or farthest from the supervisor are excluded. In Columns (5) and (6), the following additional control variables are included: population density, building permits, car density, and full-time equivalents in the public sector. Robust standard errors are reported in parentheses and are clustered at the municipal level. In all specifications, municipality fixed effects and year fixed effects are taken into account. The sample varies across the specifications from *participants* (only the municipalities which participated in the municipal protection shield) in Columns (1) and (3) to *full* (all municipalities and cities in the sample) in Columns (2) and (4). All financial data are in prices of 2020. The following control variables are used in all specifications: the population share of elderly, the population share of employed, the population share of unemployed, the migration balance, debt relief, a dummy variable if there was an election in that year, and a dummy variable if there was a transition to accrual accounting. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A10: Robustness check: Effects of travel time on cash loans, the role of counties

	Specification					
	county×year fixed effects		county as participant		cluster on county level	
	(1)	(2)	(3)	(4)	(5)	(6)
treatment×proximity	-14.08**	-12.87**	-15.08**	-14.01**	-11.87**	-11.18*
	(5.689)	(5.082)	(7.530)	(7.009)	(5.587)	(5.575)
treatment×proximity×county participant			4.095	3.712		
			(7.082)	(6.699)		
treatment		-286.6*		-247.0		-254.0
		(168.7)		(178.5)		(154.6)
sample	participants	full	participants	full	participants	full
controls	yes	yes	yes	yes	yes	yes
N	869	4466	869	4466	869	4466

Notes: The dependent variables is cash loans per capita. Proximity is measured as the reverse-scaled travel time in minutes. In Columns (1) and (2), additional county-specific year fixed effects are used. In Columns (3) and (4), the variable county participant is one if the county participated in the municipal protection shield. Robust standard errors are reported in parentheses. In Columns (1) to (4) they are clustered at the municipal level. In Columns (5) and (6) the standard errors are clustered at the county level. In all specifications, municipality fixed effects and year fixed effects are taken into account. The sample varies across the specifications from *participants* (only the municipalities which participated in the municipal protection shield) in Columns (1) and (3) to *full* (all municipalities and cities in the sample) in Columns (2) and (4). All financial data are in prices of 2020. The following control variables are used in all specifications: the population share of elderly, the population share of employed, the population share of unemployed, the migration balance, debt relief, a dummy variable if there was an election in that year, and a dummy variable if there was a transition to accrual accounting. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A11: Robustness check: Effects of travel time and travel route on the budget balance

	Specification							
	Travel time in minutes				Travel route in kilometers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatment×proximity	4.514**	4.638**	4.514**	4.541**	3.859*	3.969*	3.859*	3.940**
	(2.133)	(2.179)	(2.110)	(2.107)	(1.997)	(2.062)	(1.976)	(1.972)
treatment			165.7**	167.4**			153.2**	155.9**
			(70.36)	(70.66)			(67.04)	(67.43)
sample	participants	participants	full	full	participants	participants	full	full
controls	no	yes	no	yes	no	yes	no	yes
N	869	869	4466	4466	869	869	4466	4466

Notes: The dependent variables is the budget balance per capita. Proximity is measured as the reverse-scaled travel time in minutes in Columns (1)-(4) and as the reverse-scaled distance in kilometers in Columns (5)-(8). Robust standard errors are reported in parentheses and are clustered at the municipal level. In all specifications, municipality fixed effects and year fixed effects are taken into account. The sample varies across the specifications from *participants* (only the municipalities which participated in the municipal protection shield) in Columns (1), (2), (5), and (6) to *full* (all municipalities and cities in the sample) in Columns (3), (4), (7), and (8). All financial data are in prices of 2020. The following control variables are included in Columns (2), (4), (6), and (8): the population share of elderly, the population share of employed, the population share of unemployed, the migration balance, debt relief, a dummy variable if there was an election in that year, and a dummy variable if there was a transition to accrual accounting. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

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