Local State Capacity and Economic Development of Regions

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Abstract

This paper studies the impact of local state capacity on the economic development of regions. We exploit a change in the municipal structure of Turkey in 2012 that converted 14 municipalities to metropolitan municipalities. Metropolitan municipalities have more control over local county municipalities and a larger budget. Using credit registry and firm balance sheet data, we show that local firms' long-term loans from privately-owned banks and employment increased in the new metropolitan provinces despite no effect on productivity. The effects on both outcomes emerge when the legislation was enacted and the size of the effects grow over time after the implementation of the law.

JEL classifications: J21, G18, O23
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1 Introduction

What role does local governance play in economic development? This is a central question particularly in the developing world where state capacity visibly plays an important role in overall welfare. Standard models of fiscal federalism predict that increasing state capacity for local governments and decentralization will benefit economic development because local governments will better match local preferences in public goods provision and costs of public services will be internalized through local taxation.[1] On the other hand, if there are significant economies of scale in governance and adminstrative efficiency is higher at the central level, decentralization or investing in local governance may actually have null or adverse effects by generating deadweight loss. Whether an expansion of local state capacity will lead to economic development is therefore an empirical question. Furthermore, an expansion in local state capacity can be considered to be a place-based policy where the economic evidence is often unclear as to whether external policy interventions can result in local economic development (Kline and Moretti, 2014). Estimating the effects of the local state capacity on the economy is challenging since major reforms in governance are rare and often too involved for the identification of causal effects.

In this paper, we estimate the causal effects of increasing local state capacity on local businesses and employment using a unique reform in Turkey. In 2012 there was a change in the local governance system of Turkey that resulted in metropolitan designation of 14 provinces that increased the number of metropolitans to 30. New metropolitan municipalities were determined based on a population threshold. According to the 2012 law that went into effect following the local elections in 2014, any province that had a population over 750,000 became a metropolitan. Metropolitan designation brought about by the 2012 law represents a unique opportunity to causally estimate the impact of increasing local state capacity.

Local state capacity increased through two main channels as a result of metropolitan

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[1]See Gadenne and Singhal (2014) for an overview of the literature on fiscal decentralization and developing economies.

designation. Unlike other provinces, metropolitan municipalities receive a portion of the tax revenue within the province border, which leads to an increase in the municipal budget.[2] The increased budget implies an increase in the local government spending and hence an increase in the total GDP of a region.[3] Data from the Ministry of Treasury and Finance show that local incomes and central government transfers of newly designated metropolitan province municipalities rose along with their expenses in investments and service provision. In metropolitan municipalities, the role of central government is reduced, shifting oversight and monitoring of lower level county municipalities to metropolitan municipalities. Metropolitan municipalities also take on the responsibility of coordination of investments and services in their provinces, which imply an increase of local state activity.

The new metropolitan status can affect local firms in several ways. If economic actors expect local state capacity to increase in a way that improves the environment for conducting business, this can spur investment by firms and lead banks to form new credit lines to affected provinces. In addition, the additional spending by the local municipality can generate a multiplier effect in local economies if government purchases from local firms increase. The resulting expansion in the local economy would then be expected to lead to employment growth in the new metropolitan provinces. On the other hand, if the increase in the budget is spent (or is expected to be spent) inefficiently, response from the private sector is likely to be small. In the absence of a response from local businesses, effect of increased local state capacity would be muted rather than having a multiplier effect. Local government activity can even crowd out local private sector activity if larger municipalities are more likely to make purchases from firms located outside their province.

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[2]The tax rates by the central government does not vary by metropolitan status, hence the local tax burden remains the same. In our setting although revenues increase, autonomy over the tax rates remain the authority of the central government. This setting differs from other cases of decentralization that give some authority to local governments for raising extra tax revenues. A related literature studying decentralization and the optimal organization of local governments considers the role of taxes (e.g. Tiebout (1956) and Calabrese et al. (2012)). Various studies consider the behavioral effects of transfers to local governments (e.g, Hines and Thaler (1995)).

[3]There is a large literature finding generally positive and significant multipliers from regional government spending, see Chodorow-Reich (2019) for a review of the empirical literature.

As such, the overall impact of local state capacity on welfare and economic development hinges on both the activities of local government and local businesses. In this paper, we study changes in the credit market outcomes and performance of local firms and investigate whether metropolitan status after announcement and implementation leads to a financial and business response that is conducive to regional development and employment growth.

Using bank-firm level administrative credit registry and firm level administrative balance sheet data, we estimate the effects of metropolitan municipality designation on bank loans and employment. The bank-firm level credit registry data covers the entire population of bank loans to firms while the balance sheet dataset covers all incorporated firms in Turkey. Loan data allows us to test whether there is an increase in loan and therefore investment demand by firms and whether they can find the necessary credit lines from banks. Firm level employment outcomes provide a direct test of whether firms grow and new jobs are created. For all outcomes, we use a difference-in-differences approach to estimate the effects while controlling for firm heterogeneity and time-varying regional shocks by including firm and NUTS-2 level region-year fixed effects. We further use an event study approach to estimate both pre- and post-treatment effects of metropolitan status. This allows us to test the presence of pre-existing trends in outcomes and estimate the short and medium-term response to the passing of the law.

We use data from the Ministry of Treasury and Finance to demonstrate the effects of metropolitan designation on local and central government spending at the province level. We find that both local incomes and central government transfers to municipalities rose after the metropolitan designation in 2014. There is no change in central government spending in these provinces over the same period. With two years lag, we further observe a large increase in treated municipalities' spending on services and investments. Based on these changes, the reform represents both an increase and a decentralization in government spending, since the share of local government spending in total government spending rises.

The firm level loan estimations reveal an increase in long-term bank loans to firms in

treated provinces. The effect is statistically significant starting for the 2012-2014 period when the law was passed but not implemented and further grows in size up to 20% in the 2015-2019 period. There is a smaller and statistically less significant negative effect on short-term loans and no effect on medium term loans. When we estimate the effects for privately-owned and state-owned banks, we find that the increase in long-term loans is driven by privately-owned banks. This indicates that the increase in bank loans is not due to a targeting policy by state-owned banks. These effects are robust to firm-bank level estimations, which allow for bank-specific year fixed effects to control for banks' loan supply.

Firm level evidence shows that firms increased their employment in affected provinces. While this effect is negligible between 2012-2014, it reaches around 4% once the metropolitan status is implemented. When we estimate the employment effects by sector, we find that the growth in firm employment is driven by manufacturing firms. Employment growth in tradable manufacturing sectors instead of services suggests that the effects are as a result of rising investment and growth expectations rather than being driven by additonal local government spending. In addition, we find no effect on net migration to new metropolitan provinces, indicating that the benefits are accrued to the local population and do not generate increased worker flows in the short term.

One way municipal activities can benefit local firms is through infrastructure projects that may increase firm productivity. For example, new metropolitans may improve transportation infrastructure that reduces costs of transportation and allow easier access to markets. Municipalities may also engage in development of industry zones that may result in positive spillovers across firms. We test presence of such effects by estimating the impact on firm TFP. Overall, we find no effect on firm level TFP. This result is not surprising since productivity effects are likely to materialize over longer term. Since there appear to be no immediate effects on productivity, the expansion in bank loans and employment is more likely to be due to an expectation of long-term economic growth in treated provinces.

In sum, our results provide evidence of anticipation effects where firms increase investment

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and banks provide necessary loans with the passage of the new law in 2012. These effects are in line with the effects from the annoucements of public infrastructure improvements reported in the literature (McDonald and Osuji, 1995; Agostini and Palmucci, 2008; Billings, 2011). The effects on employment become visible two years later starting in 2014 when the legislation is implemented. The effects on both bank loans and employment grow over time, indicating that the effect is not limited to a short-term boost to economic activity. Our results therefore indicate that expectations about increased local state capacity play an important role in driving local economic growth.

Our results contribute to two strands of the literature. The first is the literature on state capacity and economic development where there are few quasi-experimental studies. These studies make use of the variation in historical trends and origins of state capacity. Using the presence of post offices as an indicator for state capacity, Acemoglu et al. (2016) finds a positive link between 19th century state capacity in US counties and innovation measured by patent applications. Dincecco and Katz (2016) use the variation in the political transformation of European countries following WW1 to document a link between long-run economic growth and state capacity. Dell et al. (2018) use a regression discontinuity design to show that Vietnamese areas that were more exposed to a bureaucratic regime historically have higher living standards in the present. On the other hand, Michalopoulos and Papaioannou (2014) find no effects from national institutions on development in Africa, though this is due to a diminishing effect in regions away from national capitals. Our paper contributes to the literature on state capacity by studying a more contemperenous case of local state capacity expansion and shows that the economic benefits can be elicited in the (relative) short-term.[4]

Our study also contributes to the literature that analyzes the effects of place-based poli-

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[4]In a related line of inquiry, researchers analyzed the effect of government re-organization on economic performance in China. Li et al. (2016) study the effect of flattening the government hierarchy on economic growth in Chinese counties and find a negative effect. Using an earlier reform, Bo (2020) finds a positive effect on economic growth from a Chinese reform that re-organized counties into more centralized cities. These studies analyze the effect of government re-organization and not the expansion of state capacity at the local or central level.

cies. There is a large quasi-experimental literature analyzing the effects of various geographic initiatives focused on job creation, economic development, support of lagging areas and transportation projects. The estimated effects of place-based policies are mixed (Neumark and Simpson, 2015) and suggest that the effects are context and policy specific. In the developing country contexts the estimated effects are usually positive,[5] while evidence from place-based policies in developed countries indicate more modest effects.[6] Metropolitan designation can be considered as a place-based policy that aims to improve local services and enhance development of regions by increasing state capacity. In this aspect, it differs from other examples of place-based policies that target economically backward areas with fiscal interventions such as tax exemptions and grants or aim to create special economic zones. Our study is the first in the literature to estimate the effects of a place-based policy that aims to increase local state capacity. We also extend the set of outcomes studied in the literature by estimating, for the first time, the effects on credit markets.

The remainder of the paper is organized as follows. Section 2 explains the 2012 law and the benefits of the metropolitan status for municipalities. Section 3 introduces the methodology and empirical specifications. Section 4 presents the data. Section 5 presents the results of the empirical analysis. Section 6 concludes.

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[5]For example, Lu et al. (2019) analyze the impact of Special Economic Zones in China using administrative firm level data and find positive effects on investment, productivity, employment and output. Studying the Great Western Development Programme in China, Jia et al. (2020) find an increase in GDP of targeted regions but find no effects on non-agricultural employment or wages. In India, Hasan et al. (2021) find a positive effect on economic activity from a tax exemption programme for new firms in economically backward districts. A more general tax exemption and infastructure investment programme at the new Indian state of Uttarakhand similarly resulted in a significant increase in economic output (Chaurey, 2017; Shenoy, 2018). Studying Special Economic Zones in India, Görg and Mulyukova (2022) shows that the economic effects of place-based policies can further include significant heterogeneity across firms depending on size, productivity and sector of the firm.

[6]Studying French Enterprise Zones, researchers have found relatively modest effects on employment (Gobillon et al., 2012; Briant et al., 2015). Among studies from the USA, Neumark and Kolko (2010) find no significant employment effects from California's Enterprise Zone Programme while Busso et al. (2013) find significant employment gains and wage increases from the federal urban Empowerment Zone programme. In a study of a local government level policy in Sweden, Lundqvist et al. (2014) report that general grants to struggling municipalities in Sweden had little effect on public employment except for administrative personnel.

2 Institutional framework

2.1 History of Metropolitan Legislation

Structural transformation in the Turkish economy from agriculture to manufacturing to services since 1960s was accompanied by large scale rural to urban migration. This resulted in rapid increase in rural populations and created large demand for local public services. To deal with the growing challenges legislation number 3030 was adopted in 1984 that introduced metropolitan municipalities for the first time. Decision on the metropolitan status of a province could only be made through a decree law or new legislation through the parliament. The first provinces that attained metro status as a result of the 1984 law were the most populated three provinces in Turkey, namely Istanbul, Ankara and İzmir. An additional 12 provinces received metropolitan status until 1993.

Expectations of increased revenues and better governance in metropolitan municipalities fueled large pressure on the political parties and the central government in the following years for further metropolitan designations. These demands originated from provinces with varying population sizes since the law did not stipulate a population threshold for the metropolitan status. To deal with growing pressures for metro designation and improve the functioning of metropolitan and lower-level county municipalities law number 5216 was passed in 2004 that introduced the population threshold of 750,000 for metro designation. According to the law 5216, the population threshold is a necessary but not sufficient condition for metro designation. The law indicates that bill of law for new metropolitan designations needs to take into account physical settlement and economic development level of the provinces, hence giving some leeway to decision makers as to how they implement the legislation.

The law in 2012 (law number 6360) established new metropolitans and introduced further refinements to strengthen the coordination between metropolitan and county municipalities. The law 6360 provided metropolitan status to all provinces above the 750,000 threshold that were not yet metropolitans and stipulated that this designation would take effect after the

2014 municipal elections. Thus, the 2012 metropolitan designations we study in this paper were based on the pre-existing population threshold introduced in 2004. As a result of the law, 14 municipalities gained the metropolitan designation. The timing of the law was partly politically motivated, appeasing voters in the upcoming general and municipal elections in 2014, and allowed the government to meet the existing demands for metropolitan status.

The bill of laws referred to the parliament both in 2004 and 2012 stressed the need for stronger local governance and the expectation that the services will be delivered more efficiently and effectively due to economies of scale (page 86, proposed bill to the parliament in 2004). As a result of these laws central governments control and audit on municipalities in metropolitan areas were reduced. Proposed bills also made reference to the importance of infrastructure investments and transportation services, such as the law in 2012 stipulating that infrastructure investment and transportation coordination centers to be established for planning and coordination of these services and to be presided by metropolitan mayors. In addition, the share that all metropolitan municipalities received from taxes collected within their borders was increased from 5% to 6%.

2.2 Present Local Governance Structure

Turkey currently consists of 81 provinces and each province is made-up of counties, of which there are 972 in total. Each province has its own municipality, either a standard municipality or a metropolitan one–the latter serving provinces with larger populations. The primary responsibilities of province municipalities are city planning, i.e. the issuing and inspection of construction and workplace permits, building and maintenance of province level infrastructure including water and sewage systems, road infrastructure, and environmental planning and protection. Budgets are set differently for standard and metropolitan municipalities. A pre-determined amount of the central budget (2.85%) is distributed across municipalities based on their geographical size and population. On top of the amount received through this channel, metropolitan municipalities also receive 6% of all taxes collected

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within their borders. In practice, this amounts to a 20-30% increase in the municipality budget for provinces with a metropolitan status.[7]

There are several other differences between standard and metropolitan province municipalities. In standard municipalities, the province municipality is limited to servicing the province center while county municipalities are responsible for services within their own borders. The legislation on metropolitan municipalities, on the other hand, gives the metropolitan municipality oversight and monitoring responsibilities over the activities of county municipalities which implies a greater degree of coordination and planning in municipal functions.

The population counts of non-metropolitan provinces prior to the law change is presented by Figure 1 as of 2013 which shows a remarkable continuity in this distribution. While 14 provinces above the threshold received metropolitan status, some provinces with relatively large populations –such as Afyonkarahisar, Sivas, Tokat– could not attain metropolitan status as they fell short of the 750,000 threshold.[8] Figure 2 shows the map of provinces by their metropolitan status. The new metropolitan provinces are not concentrated in a specific region but are scattered across the country. Prior to Law 6360, 57% of the country's population lived in 16 metropolitan provinces in 2013. This share increased by a further 24 percentage points as 14 provinces became metropolitans reducing the population share of standard municipality provinces to 20%.

The populations of the provinces had little relation to economic growth in the years preceding the new metropolitan law in 2012. Figure 3 shows scatter plots of GDP per capita growth between 2006 and 2010 and population in 2013 at the province level.[9] There is no visible positive relation between the growth rate of provinces and their population. This applies to figures in both panel (a) which includes all (non-metropolitan) provinces prior to Law 6360, as well as, panel (b) which excludes provinces with populations below 400,000.

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[7]Municipality budgets can benefit from fees for services (like water and parking) they provide, but this plays a limited role compared to the allocation from the central budget.

[8]Ordu was not in the initial list of new metropolitan municipalities published in 2012 and was added later in 2013. Its population first exceeded the 750,000 threshold in 2013.

[9]This figure excludes provinces that had a metropolitan status prior to the 2012 legislation.

The effect of the metropolitan status on municipality incomes is evident in 4, which shows the relative evolution in local incomes and central government transfers of new metropolitan status provinces (treatment group) and remaining non-metropolitan provinces (control group). Both groups' values are normalized to 1 in 2007 and we exclude election years (2009, 2014, 2019) from the graph since the available budgets in these years only reflect the period after the election. There is a significant rise in both sources of municipal income following the implementation of the law in 2014. As shown in Figure 5, there is no accompanying increase in the share of central government spending to the new metropolitan municipalities.

We explore whether the metropolitan status led to changes in local government expenditures using annual data on municipal expenditures by province. We compute the province level change in expenditures relative to 2006 by expenditure category. Figure 6 plots the difference in mean changes between standard provinces and treated provinces that became metropolitans in 2012. Prior to the 2012 law change two major expenditure categories included personnel costs that accounted for about 32% and purchases of goods, services and capital investments that accounted for about 61% of all local government expenditures.[10] The figure illustrates that there were no differences in personnel costs over the 2006-2021 period, implying that standard and metropolitan provinces did not expand differentially in terms of municipal employees. In terms of services and investments, however, there is a clear difference that emerges after 2014 that indicates higher growth for new metropolitan provinces. This is consistent with the expectation of the law that service delivery and infrastructure investments improve with the metropolitan designation. Both the expectation and the realization of such improvements may have increased the investment prospects of private-sector firms in the new metropolitan areas.

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[10]There is no data that allows seperately identifying service and investment expenditures.

3 Methodology

3.1 Baseline difference-in-differences

We estimate the effects of metropolitan designation (i.e. converting standard municipalities to metropolitan municipalities) by setting up an empirical model following the difference-in-differences methodology. The treatment group includes all provinces (or firms in provinces) that are converted to metropolitan municipalities in 2012. We exclude provinces that were already designated as metropolitan prior to 2012 from the analysis.[11] Thus, our control group is composed of provinces with a population below the threshold value of 750,000. Our baseline specification estimates the effect of the legislation for two subperiods between 2012-2014 and the implementation of the new metropolitan status between 2015 and 2019. As an alternative estimator, we use an event study type specification and estimate the effect of the metropolitan status for each pre- and post-treatment year seperately with 2011 as the baseline year. Our analyses comprises estimation of the effects for various outcomes at three levels: province, bank-firm and firm. In all regressions, we cluster standard errors at the province level to avoid over-rejection in line with the difference-in-differences literature (Bertrand et al., 2004; Cameron and Miller, 2015).

At the aggregate level, we use province level data covering the period between 2010 and 2019 and study effects of metropolitan designation on government spending (local and central), net migration, employment, and number of establishments. The empirical specification for these province-level outcomes is given by equation (1). The outcome is denoted by Y_{pt} for province p, at time t. Our first treatment variable $L_{pt}^{2012-2014}$ is a dummy variable that takes the value of 1 for the years 2012 to 2014 if province p received metropolitan status in 2012 based on Law 6360; 0 otherwise. The coefficient β_1 captures the effect of passing the legislation but not the implementation of the new metropolitan status of provinces. Thus,

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[11]We tested this group as an alternative control group and found that there are significant differences from the treatment group in the pre-treatment trends of virtually all variables. In addition, in 2012, there were changes made to the revenues of all metropolitan municipalities, implying that municipalities designated as metropolitan prior to 2012 were themselves treated to a lesser extent.

β_1 captures the anticipation effects. The second treatment variable, $I_{pt}^{2015-2019}$ is similarly defined, and is equal to 1 for the years between 2015 and 2019 for provinces that received the metropolitan status in 2012; 0 otherwise. The corresponding coefficient β_2 captures the estimate for the effect of the implementation of the metropolitan status.

The treatment and control provinces can be located in different regions. Since these differences can be large, we control for regional differences by including a set of region-specific fixed effects. In our preferred specification, we use NUTS-2 region level year-specific fixed effects: $NUTS2_p \times Year_t$.[12] Any shocks common to a region are therefore controlled for, including for example the arrival of Syrian refugees to Southeastern Turkey beginning in 2013.[13] In other words, effect is identified by comparing new metropolitans to other standard provinces in the same NUTS-2 region. We also provide estimates where NUTS-1 level year-specific fixed effects or no region-specific fixed effects are controlled for. Besides the region-year specific fixed effects, we include a set of province specific fixed effects, σ_p, as is standard in two-way fixed effects models.[14]

$$Y_{pt} = \beta_0 + \beta_1 L_{pt}^{2012-2014} + \beta_2 I_{pt}^{2015-2019} + \sigma_p + NUTS2_p \times Year_t + e_{pt} \tag{1}$$

To estimate the effect of metropolitan municipalities on firm-level loans and balance sheet outcomes, we use the specification shown by equation (2). Y_{ispt} denotes the outcome for firm i, in sector s, region p, at time t. The specification includes firm fixed effects, ϕ_i, NUTS-2 region-year fixed effects, $NUTS2_p \times Year_t$. Since sector information is available in the balance sheet dataset, we further include sector-year fixed effects, $Sector_s \times Year_t$, when estimating the effects on employment, sales and TFP. Consider as an example employment as the outcome variable. Equation (2), by controlling firm fixed effects, exploits variation

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[12]There are 81 provinces (NUTS-3 level designation), 26 NUTS-2 regions and 12 NUTS-1 regions in Turkey.

[13]In fact, previous studies on the labour market impact of the arrival of Syrian refugees use variation at the NUTS-2 level.

[14]Year fixed effects are included in the specification when region-year fixed effects are omitted.

in firm level employment over time.[15] Therefore, the identification of employment effects come from differences in changes in employment over time between firms located in new metropolitan and standard provinces that share the same time-varying region and sector-specific shocks. Loan outcomes differ slightly from other balance sheet outcomes because the data is available at a monthly rather than annual frequency. In these models, we replace region-year specific fixed effects with region-year-month fixed effects.

$$Y_{ispt} = \beta_0 + \beta_1 L_{pt}^{2012-2014} + \beta_2 I_{pt}^{2015-2019} + \phi_i + NUTS2_p \times Year_t + Sector_s \times Year_t + e_{isrt} \quad (2)$$

We also estimate the effect of the reform for firm loans at the bank-firm level. The empirical specification is given by equation (3). $ln(Loan)_{ijpt}$ is the natural log of the amount of loan lent to firm i by bank j at province p in year t. The primary difference from the firm level estimation is the inclusion of fixed effects at bank-firm, γ_{ij}, and bank-year-month, $Bank_j \times YM_t$, levels.[16] Bank-firm fixed effects control for time invariant differences in loan amounts lent to firms by banks, capturing the credit history a firm might have established with a particular bank. Bank-year-month fixed effects control for credit supply at the bank level. These fixed effects control for credit expansions and contractions over time at bank level driven by national level trends. Hence, the estimated effects can be interpreted as the change in credit demand over time by firms in new metropolitan provinces compared to those in standard provinces.

$$ln(Loan)_{ijpt} = \beta_0 + \beta_1 L_{pt}^{2012-2014} + \beta_2 I_{pt}^{2015-2019} + \gamma_{ij} + NUTS2_p \times YM_t + Bank_j \times YM_t + e_{ijrt}$$

$$(3)$$

In order to estimate the effects on productivity, we estimate the TFP at the firm level.

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[15]Including firm level fixed effects has the additional upside of controlling for a change in firm composition due to changes in the balance sheet reporting rates of firms after the switch to the metropolitan status.

[16]We do not include sectoral controls since the credit registry does not include firm level sector information.

For TFP estimation we use the following steps. We first estimate a firm-level production model following Levinsohn and Petrin (2003) based on a Cobb-Douglas specification. This involves estimation of separate production functions, depicted by equation (4), at two-digit NACE sectors in log form to retrieve the output elasticities of capital and labor. In equation (4), $lnRev$ is the natural logarithm of the CPI deflated sales of firm i at time t. The log of firm fixed assets, lnk, and labor, lnl, as well as a vector of first-, second-, and third-degree polynomials of lagged fixed assets and labor are the independent variables, which are denoted by , X_{it-1}.

$$lnRev_{it} = \beta_0 + \beta_1 lnk_{it} + \beta_2 lnl_{it} + \sigma X_{it-1} + Year_t + e_{it} \tag{4}$$

Equation (4) is estimated using a one-step GMM where lagged values of labour are used as instruments for its contemporary values following Wooldridge (2009). The output elasticities with respect to labor and capital at the two digit sector level can then be used to estimate firm level TFP by subtracting elasticity multiplied by capital and labor from total sales as shown by equation (4).

$$TFP_{it} = lnRev_{it} - \beta_1 lnk_{it} - \beta_2 lnl_{it} \tag{5}$$

3.2 Identifying assumptions and event study specifications

For all our estimations, whether at the aggregate, firm-bank or firm level, the key assumption is that trends in economic outcomes in new metropolitan provinces are parallel to standard provinces. In order to fulfill the parallel trends assumption in our baseline analysis, we include NUTS-2 region-time fixed effects, which implies that the parallel trends assumption has to hold for new metropolitan and standard provinces within the same NUTS-2 region rather than across all regions. Distribution of provinces across NUTS-2 regions is given by Table 1. Including region-time fixed effects implies that regions where all provinces

became metropolitan in 2012 (NUTS-2 region 5 including Aydın, Denizli, Muğla) or where all provinces already had the metropolitan status prior to 2012 (NUTS-2 region 25 including Şanlıurfa and Diyarbakır) do not contribute to the identifying variation. As such we also show results where the region-year fixed effects are excluded or are replaced by the more aggregate NUTS-1 level region-year fixed effects.

We check the validity of the parallel trends assumption by estimating an event study type specification where we augment the baseline difference-in-differences set-up with year-specific estimates of the treatment variable, $Treat_p$, which indicates whether a province received the metropolitan designation in 2012. The estimates for the years prior to 2012 can then be treated as testing for the parallel trends assumption in the pre-treatment period.[17] Since we estimate a coefficient for each year after the legislation is passed, the event study specification is also more flexible in estimating short- and medium-run effects. The baseline year is set to 2011, the year prior to the passing of the new metropolitan designation. The event study specification is shown by equation (6) where the coefficients of interest are β_k.

$$
y_{ispt} = \beta_0 + \sum_{k=2006}^{2010} \beta_k Treat_p x Year_k + \sum_{k=2012}^{2019} \beta_k Treat_p x Year_k
$$
$$
+ \phi_i + NUTS2_p \times Year_t + Sector_s \times Year_t + e_{isrt}
$$

(6)

While we can acount for region-specific shocks by including region-year level fixed effects in our specifications, the treatment and control provinces also differ in their population size due to the nature of the legislation. If high population provinces experience shocks that are systematically different in the post-treatment period, our estimates will be biased even if there are no differences in pre-treatment trends. To test whether our results are driven by the choice of the control group, we provide results from event studies where we exclude all 34 provinces with a 2013 population below 400,000. This results in a control group that is more

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[17]The credit data are available starting in 2006 while balance sheet and province employment data are available starting in 2009.

similar in terms of population size to the treatment provinces. These results are presented in Appendix A. In the same Appendix, we present results from event studies where we exclude Hatay and Şanlıurfa, both converted to metropolitan municipalities, because they had a rapid increase in the share of Syrian refugee to native ratio during the post-treatment period. Şanlıurfa and Hatay are also the largest provinces in the treatment group in terms of population. Excluding them allows us to test whether our results are driven by large provinces.

4 Data

We draw outcomes from several data sets. To measure effects on loans we use the Credit Registry dataset that covers the entire population of bank loans to firms. For firm level outcomes, firm balance sheets and social security records are used which covers the full population of incorporated firms in Turkey. At the aggregate province level, we use data on the number of workplaces and internal migration. The former is obtained from the Social Security Institute while the latter is from the Turkish Statistical Institute. Credit registry data are available beginning in 2007 while balance sheet and province level data are available starting in 2009. We use data up to year 2019 in all cases. Given that the legislation regarding the metropolitan status was passed in 2012 and took effect in 2014, data from 2007 to 2011 correspond to the pre-treatment phase, 2012-2014 to the phase where anticipation effects due to legislation can emerge, and 2015-2019 to the implementation phase.

Table 2 presents descriptive statistics of variables derived from credit registry, balance sheet and aggregate province level indicators for control and treatment groups. The mean and median values for variables from credit registry and balance sheet data in control and treatment groups are similar and there are slight differences across province level aggregate indicators.

4.1 Credit Registry Data

The Credit Registry data made available at the Central Bank of the Republic of Turkey (CBRT) covers all corporate bank loans at the bank-firm-month level. For our analysis, we use data from the period between 2007 and 2019.[18] The loans are divided into three subcategories of short-term (less than a year), medium-term (1 to 3 years) and long-term (more than 3 years) depending on the maturity. As of 2010, 40% of the credit volume in our sample was long-term, 18.5% short-term and 41.5% medium-term. In the final year, 2019, there were 3 state-owned and 27 privately-owned banks, which provided around 86% of total credit supply. There were further 6 participation and 14 development banks, which provided the remaining 14% of credit supply. These are practically all loans to firms as 95% of all outstanding loans are provided by banks in Turkey.

The credit registry covers loans to firms at the bank branch level and the location of the bank branch is available in the data set used in this analysis. Since our treatment is based on the province of the firm, we assume that each firm is located in the province of the bank branch they work with. For firms with loans from multiple bank branches in different provinces, we assign the province with the highest share of loans as the province of the firm. This is important because some firms receive loans from bank branches from multiple provinces. The number of unique firm-province level loans is nearly 20% higher than the number of unique firms with loans in the data. Data contain 180,532 unique firms.

We use several variables as outcomes from the credit registry data and provide results from analysis at three levels. At the firm-bank level, we estimate effects on log-transformed short, medium and long term loans. Note that data at the bank-firm level can only be used to estimate the effects on the internal margin of loans for bank-firm relationships that exist before and after the reform. To further include effects on the number of firm-bank relationships, we estimate the effects on loan outcomes at the firm level. Finally, we aggregate

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[18]Data starts in 2006 but only the last 3 months are available for 2006. We therefore exclude the year 2006 from the sample.

both the number of firms with bank loans and firm-bank links to the province and province-bank levels to test for effects on the extensive margin of firm level bank loan take-out.

4.2 Balance sheet data

Balance sheet data are drawn from CBRT data for the population of incorporated firms. These balance sheets are reported yearly to the Turkish Revenue Administration for tax purposes and include information on firm sales, fixed assets and sectors. Employment information for all firms is drawn from the Social Security Institute (SSI) registry and the available employment information is based on the number of employees firms had registered to the SSI in December of each year. Information on the location (and therefore the province) of each firm is based on the tax administration office that the firm is registered to. Balance sheet data cover the period between 2009 and 2019. Since inactive firms can still appear in the tax registry, we excluded all firms with no registered employment or annual (2017 CPI deflated) sales less than 20,000 TL. We selected this threshold because 20,000 TL is less than the employer cost of a single minimum wage employee (24,000 TL) for a year. We estimate effects on firm level log-transformed employment and TFP for this remaining sample. In total, there are 151,687 unique firms in our sample. 23,341 of these firms operate in manufacturing sector and 103,312 in service sector. The remainder are in construction, mining and agriculture sectors.

4.3 Province level variables

We use several sources of aggregate, province level data. Local and central government spending data for each province are drawn from the database of the Ministry of Treasury and Finance which is available between 2006 and 2019. We deflate these figures using CPI. For election years (2009, 2014, 2019), only post-election spending appears to be reported at the end of the year. We therefore exclude these three years from the government spending dataset.

Data on the number of workplaces are drawn from the public database of the Social Security Institute (SSI). SSI data are available for the period between 2009 and 2019. It is worth noting that the administrative definition of workplaces in this data differs from firms. A firm can have multiple workplaces in different locations. Each location that a firm has registered employment is counted as a workplace by the Social Security Institute. The advantage of using the publicly available SSI data instead of constructing these variables from micro level balance sheets is that non-incorporated firms can also be included in the aggregate numbers. Finally, we obtain information on internal migration and population from the Turkish Statistics Institute website for the years between 2009 and 2019. The internal migration data includes both in migrants and out migrants and we use this information to calculate province level net migration rates.

5   Results

5.1   Impact on bank loans

Table 3 shows the estimation results for short, medium and long-term bank loans at the firm level based on specification 3 in panel A. The preferred specifications with NUTS-2 region-year effects are presented by columns 3, 6 and 9. For short-term loans with less than a year in maturity, we find no effects. For medium-term loans with maturities between 1 and 3 years, the effects are positive during the implementation period, but are not statistically significant. The largest effects are found for long-term loans with maturity of more than 3 years. The effects are immediately positive at around 12% starting in the legislation period when the law was first passed, and the size of the positive effect reaches 19% during the implementation period. Thus, these results suggest that following the conversion of municipalities to metropolitans long-term loans increased significantly, while the effect on loans with maturities less than 3 years is smaller in size.

A potential explanation for the increase in bank loans to firms in affected provinces

could be an increase in supply of loans to these provinces by state-owned banks. State-owned banks are generally assumed to have different objective functions (Yeyati et al., 2007) and may have aimed to boost development in the newly designated metropolitan provinces. We test whether this is the case in panels B and C, where we estimate the effects on firm-bank loans for subsamples of loans by privately-owned and state-owned banks. In panel B, the estimated effects on long term loans provided by private banks are about the same size as in panel A, but there is also a statistically significant negative effect for short-term loans during the legislation period. In panel C, on the other hand, we find a statistically significant positive effect on long-term loans from state-owned banks, but these are smaller in size than the effect estimates for private bank loans. Hence, results in panels B and C suggest that state banks are not responsible for the expansion in loans and the effect in the full sample is mainly driven by privately-owned banks. These results suggest that firms in the new metropolitan provinces are shifting their demand from short-term loans to long term loans. The increase in loans from privately-owned banks is consistent with the effects being driven by an expectation of economic growth in affected provinces and that there is a complementarity between state capacity and private investment.

To complement the estimates in Table 3 where we present the effects at the firm level, we estimate the effects on bank-firm level loans and present the results in Table 4. At the bank-firm level, we can further control for bank-firm relationships by including bank-firm level fixed effects as well as bank composition and supply by including bank-year-month fixed effects. Overall, the results are similar, with significant effects on long-term loans and no effects on medium-term loans. The estimates for long-run loans are smaller for both periods, suggesting that the increase in long-term loans at the firm level are partly due to loans from new firm-bank relationships. On the other hand, the negative effects on the short-term loans are statistically significant, which suggests a clear switch from short- to long-term loans. Similar to firm level loan results, the positive effects appear to be driven by privately-owned banks and the effect on long-term loans from state-owned banks is no longer statistically

significant.

Event study estimates for firm-level loans are shown by Figure 7. The results for short-term loans are shown in panel (a), for medium-term loans in panel (b), and for long-term loans in panel (c). Consistent with the results in Table 3, the strongest and most statistically significant results are found in panel (c) for long-term loans. While none of the pre-treatment period coefficients are statistically significant, we find statistically significant and positive effects already in 2012 and the size of these effects grow, particularly starting after the implementation of the reform in 2015. The event study estimates also provide a more nuanced view of the effects on short and medium-term loans. Once again, the pre-treatment years suggest that the parallel trends assumption is satisfied as all estimates are statistically insignificnat and close to 0. There is an immediate, though small, decline in short-term loans after the legislation is passed in 2012, 2013 and 2014. However, this effect is not statistically significant and disappears after the implementation of the reform. For medium term loans, while marginally statistically insignificant, we find sizeable effects reaching to 10% during the latter years of our sample. In Appendix Figures A1 to A3, we replicate the event study analysis for loan outcomes using alternative samples. Panel (a) of each figure shows the results when control group provinces with a population of less than 400,000 are excluded and panel (b) shows the results when two treatment group provinces, Hatay and Şanlıurfa, with high refugee to native ratios during the treatment years are excluded. The results remain largely similar, one difference is the disappearance of the significant negative effect for short-term loans during the period after the legislation and prior to implementation.

Given that our specifications so far include either firm or bank-firm fixed effects, the estimated impacts refer to the intensive margin. To assess the impacts on the extensive margin, we estimate the impact on the number of firms with bank loans at the province and bank-province levels. The results of this estimation are presented in Table 5.[19] While the estimated effects are generally positive and substantial, none of them are statistically

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[19]Province level estimations use the specification shown by equation (1). For the bank-province level estimates, we further include bank-province and bank-year-month fixed effects.

significant. The event study estimates for the effect on the number of firms with bank loans at the province level shown by Figure 8 suggest a statistically significant increase in the number of firms with bank loans after the legislation is passed between 2012 and 2016. However, in later years, this effect disappears.

5.2   Impact on sales, employment and TFP

Our findings in the previous section indicate that long-term loans to banks increased as a result of metropolitan status. An increase in bank loans to firms will have limited impact on welfare unless firms expand and increase their employment. In this section, we estimate the effects on three balance sheet outcomes: sales, employment and TFP. The difference-in-differences estimates for all three outcomes are presented in Table 6 with the first three columns presenting the results for the full sample, columns 4 to 6 the sample of manufacturing firms and columns 7 to 9 the sample of firms in the service sector.

The results for the effects on firm level employment are shown by panel A of Table 6. According to our preferred estimate for the full sample in column 3, there is a 2.5% increase in employment during the period after the legislation is passed. This effect increases to 4% during the implementation period, but the estimate is less precisely estimated. Columns 6 and 9 demonstrate that the effects are driven largely by the manufacturing sector, where the effect size is triple that of the average effect during both the legislation and implementation period.

The estimated effects on sales shown by Panel B are consistent with the effects on employment.[20] We find that sales of firms in treatment provinces rose by 3.4% during the implementation period. Similar to the employment results, the increase in sales appears to be largely due to the manufacturing sector. Unlike the employment results, there is a marginally significant effect on service sector firm sales as well.

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[20]The consistency of the effects on sales and employment is important because the rise in employment may also be due to a switch from informal to formal employment in the Turkish setting where non-agricultural employment has an informality rate around 20% to 30% during the period of analysis.

A potential explanation for the increase in the size of manufacturing firms would be an accompanying increase in productivity. Local government spending on infastructure can affect the productivity of firms. We estimate the effect on firm TFP and present the results in panel C of Table 6. The results suggest that firm productivity was not affected by the 2012 reform. This result holds across manufacturing and service sectors. Effects observed on employment and sales are more likely to be driven by the expectations of economic growth and the increase in investment through long-term bank loans presented in the previous subsection.

The event study estimates for firm employment, sales and TFP are presented by Figure 9. The event study estimates show that none of the outcomes had significantly different trends during the pre-treatment years. The employment increase occurs already in 2014 and this effect remains in the following years. The increase in sales coincides with the implementation of the reform and becomes larger starting in 2016. The most striking difference is in the results for TFP, where there appear to be an increase particularly after the implementation of the reform. However, even in 2019 where the estimated coefficient is largest, the effect is marginally statistically significant. The estimated event studies using alternative samples where we exclude provinces with fewer than 400,000 population and Şanlıurfa and Hatay are presented by the Appendix Figures A4 to A6. The results are largely in line with the baseline event study results. The observed effect on TFP becomes less pronounced when provinces with fewer than 400,000 population are excluded and more pronounced when Şanlıurfa and Hatay are excluded. Overall, while there is some suggestive evidence of an increase in TFP, the estimates are inconclusive.

5.3   Impact on internal migration and firm entry

Our analysis on firm performance focused only on existing firms. If incumbents benefited, we may also see a rise in firm entry. In panel A of Table 7, we estimate the effects on the number of workplaces. While the point estimates are again positive, they are not statistically

significant. Benefits appear to be therefore accrued by existing private sector firms rather than new entrants. Effects may further be moderated through internal migration. A rise in the economic growth prospects of a particular province may draw migrants from other provinces. Specifically, if there is an internal migration response, there may be an upward bias in our estimates particularly if exits from other provinces result in a negative labor supply shock in the control group provinces. We test the presence of internal migration effects by estimating the effects of the reform on net migration rates at the province level. The results are presented by panel B of Table 7. There is no evidence of an increase in net migration to new metropolitan provinces as none of the estimated coefficients appear to be statistically insignficant and the point estimates are close to zero.

6   Discussion and Conclusion

This study contributes to the literature by providing quasi-experimental evidence on the effects of increasing local state capacity using detailed administrative data. The results indicate positive effects on several outcomes. Even before full implementation of the law, firms in provinces with the new metropolitan status started increasing their long-term bank loans. This suggests that even the expectation of an increase in local state capacity was enough to spur economic development. Following the implementation of the law, we find firms had a significant increase in their loans, employment and sales. The increase in sales and employment are driven by the manufacturing sector, indicating that the effects are not due to increasing government spending in non-tradable sectors. The results are robust to alternative samples and placebo tests for outcomes show no sign of the effect being driven by pre-reform trends.

If viewed from the lens of local state capacity building, the positive effects confirm the previous findings on the link between (central) state capacity and economic growth of countries. On the other hand, when compared to many studies in the literature on place-based

policies, the results of the policy we study deliver more positive effects. One explanation may be that most region-based policies are known to be temporary measures to assist lagging regions. A promise for a perpetual increase in local state capacity, government spending and the expectations it raises for sustained future growth in a region appears to be more effective for spurring private sector investment.

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Figures and Tables

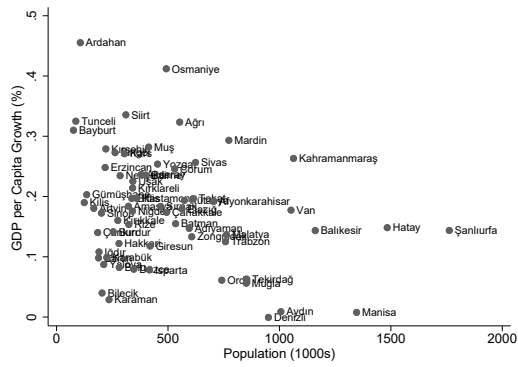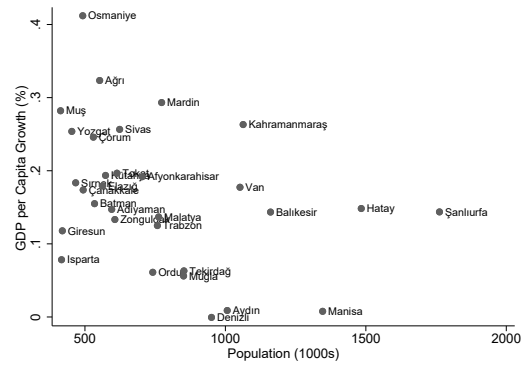**Figure 1.** Population and metropolitan designation

Figure 2. Treatment by province
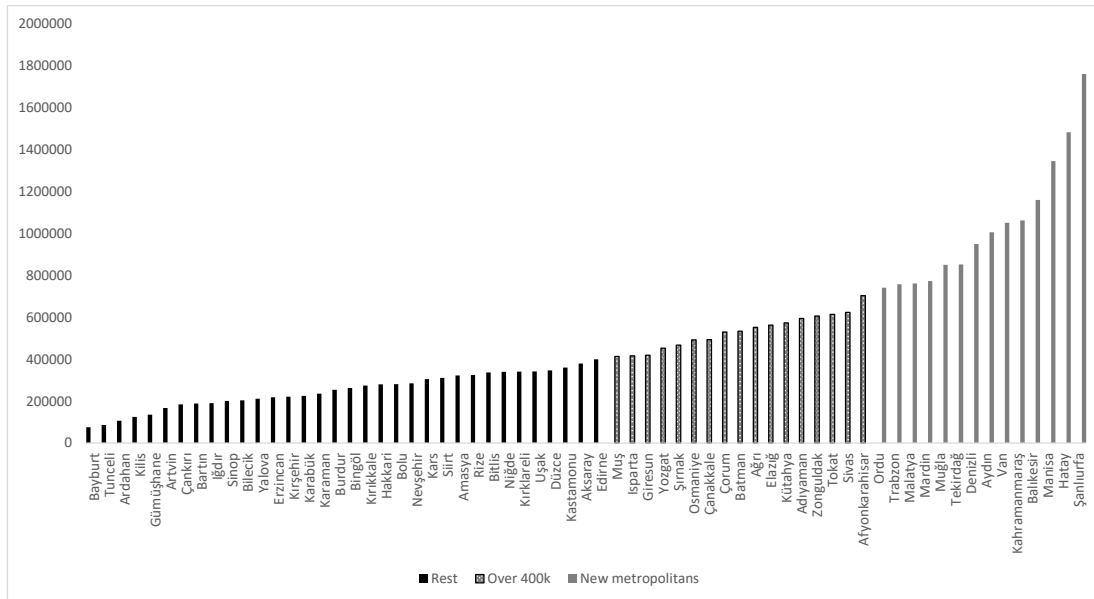
Notes: Authors' calculations.

Figure 3. Province level GDP growth and population 2006-2010

(a) All provinces

(b) Population greater than 400,000

Figure 4. Municipality income

(a) Local income of municipalities

(b) Central government transfers to muncipalities
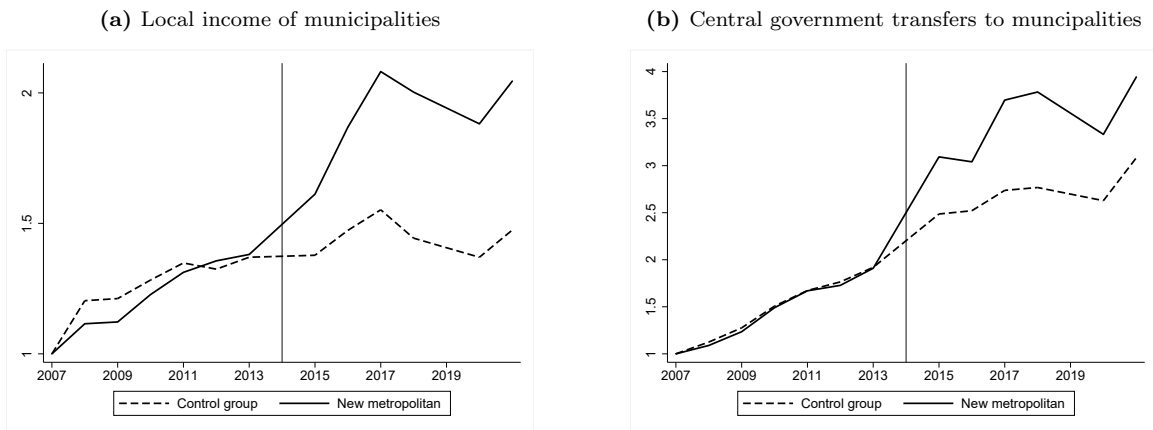
Notes: The figure is based on CPI adjusted annual data on municipal incomes. Values are normalized such that the spending in 2007 is equal to 1. We remove the local election years of 2009, 2014 and 2019 from the data to remove the effects of elections.

Figure 5. Central government spending at the province level

Figure 6. Spending of new municipalities by type

Figure 7. Event study estimates for bank loans

(a) Short-term loans

(b) Medium-term loans

(c) Long-term loans

Notes: The dependent variables in each panel are log-transformed loans. Panel (A) shows the results for short-term loans with a maturity less than a year, panel (B) medium-term loans with a maturity between one and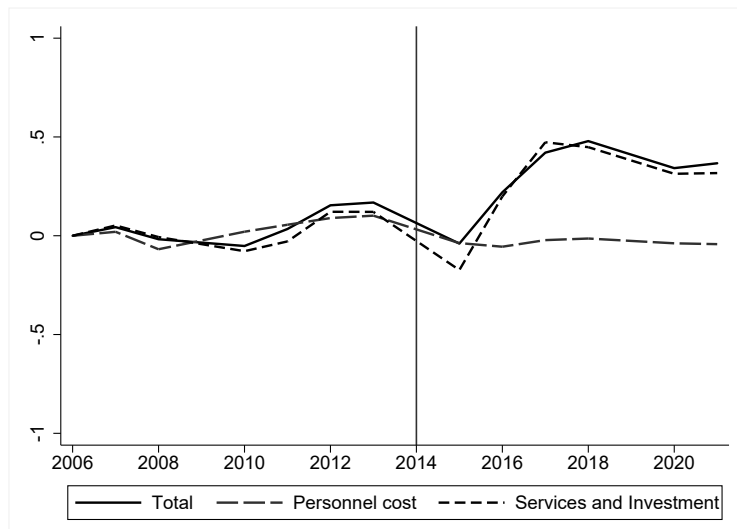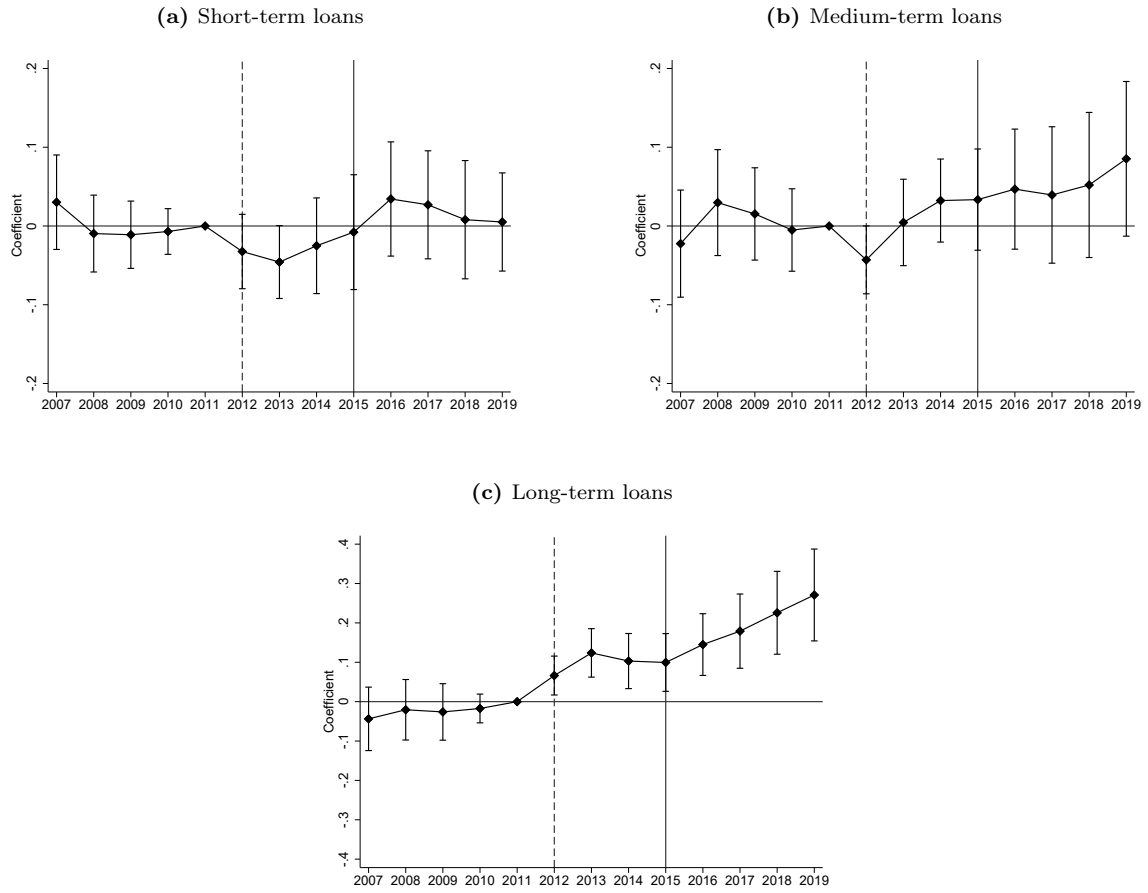 three years and panel (C) long-term loans with a maturity longer than three years. The treatment variable of interest is whether the province of the firm was made a metropolitan by the 2012 legislation, and the figure shows yearly coefficient estimates for this treatment variable. The estimates come from a regression where treatment variable is interacted with year dummies. The baseline year is 2011. Other control variables in this regression include firm, NUTS-2 region-year and year-month fixed effects. Province fixed effects are omitted due to collinearity. The vertical bars illustrate the 95% confidence intervals. The dashed vertical line in 2012 indicates the year the legislation was passed and the solid vertical line on 2015 indicates the year the legislation took full effect.

Figure 8. Event study estimates for the number of firms with bank loans

Figure 9. Event study estimates for firm sales, employment and TFP

(a) Sales
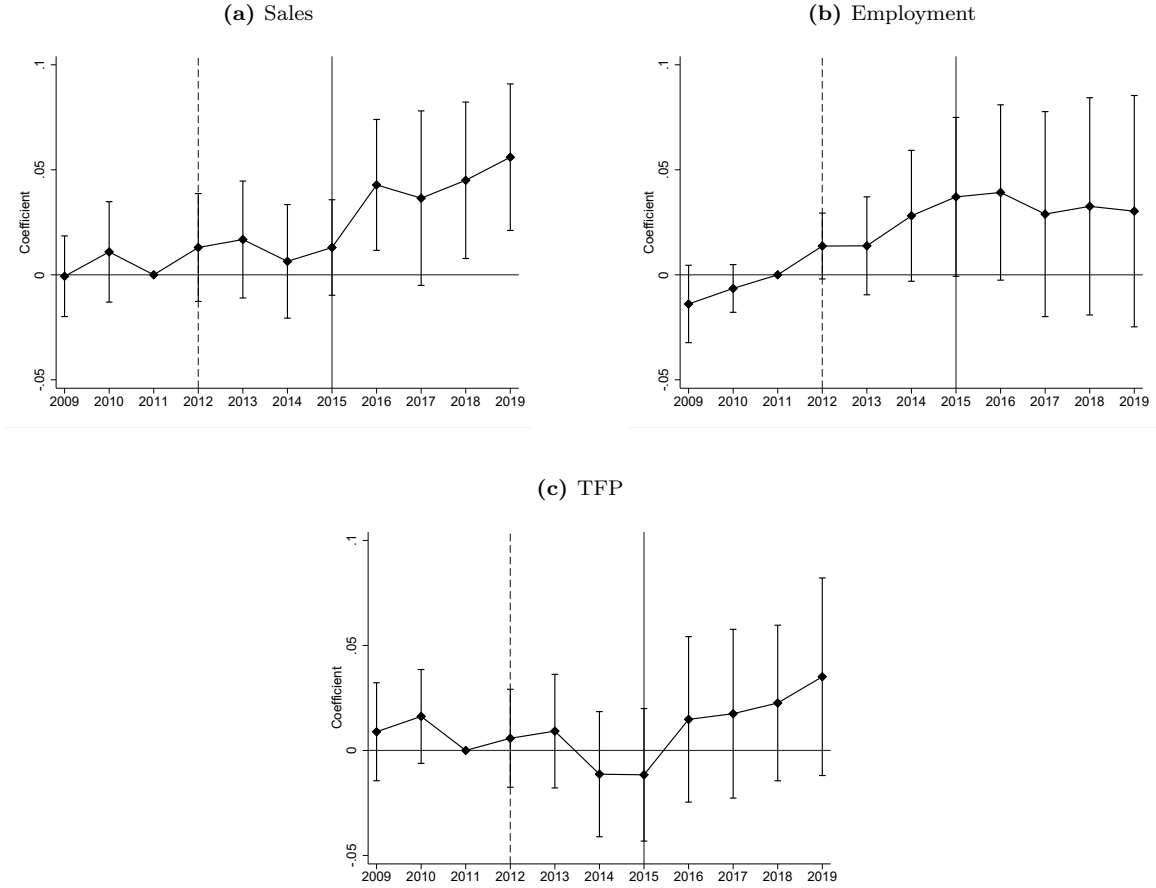
(b) Employment

(c) TFP

Notes: The dependent variable in Panel (A) is log-transformed and CPI adjusted sales, panel (B) log-transformed employment and panel (C) TFP. The treatment variable of interest is whether the province of the firm was made a metropolitan by the 2012 legislation, and the figure shows yearly coefficient estimates for this treatment variable. The estimates come from a regression where treatment variable is interacted with year dummies. The baseline year is 2011. Other control variables in this regression include firm, province 2-digit sector-year and NUTS-2 region-year. Sector and year fixed effects are omitted due to collinearity. The vertical bars illustrate the 95% confidence intervals. The dashed vertical line in 2012 indicates the year the legislation was passed and the solid vertical line on 2015 indicates the year the legislation took full effect.

Table 1. Nuts-2 regions of Turkey and municipality status.

NUTS-2	Provinces
1	*İstanbul*
2	**Tekirdağ**, Edirne, Kırklareli
3	**Balıkesir**, Çanakkale
4	*İzmir*
5	**Aydın**, **Denizli**, **Muğla**
6	**Manisa**, Afyon, Kütahya, Uşak
7	*Bursa*, *Eskişehir*, Bilecik
8	*Kocaeli*, Sakarya, Düzce, Bolu, Yalova
9	*Ankara*
10	Konya, Karaman
11	*Antalya*, Isparta, Burdur
12	*Adana*, *Mersin*
13	**Hatay**, **Kahramanmaraş**, Osmaniye
14	Kırıkkale, Aksaray, Niğde, Nevşehir, Kırşehir
15	*Kayseri*, Sivas, Yozgat
16	Zonguldak, Karabük, Bartın
17	Kastamonu, Çankırı, Sinop
18	*Samsun*, Tokat, Çorum, Amasya
19	**Trabzon**, **Ordu**, Giresun, Rize, Artvin, Gümüşhane
20	*Erzurum*, Erzincan, Bayburt
21	Ağrı, Kars, Iğdır, Ardahan
22	**Malatya**, Elazığ, Bingöl, Tunceli
23	**Van**, Muş, Bitlis, Hakkari
24	*Gaziantep*, Adıyaman, Kilis
25	**Şanlıurfa**, *Diyarbakır*
26	**Mardin**, Batman, Şırnak, Siirt

Notes: Italic provinces are excluded from the analysis since they were already metropolitan in 2012. **Bold** provinces are made metropolitans by the 2012 legislation.

Table 2. Summary statistics

	Mean	p50	SD	N
Control				
Balance sheet				
Sales	13.8652	13.7795	1.6277	508,852
Labor	1.6774	1.6094	1.3533	508,852
TFP	12.7104	12.6311	1.2556	508,852
Credit registry				
Short-term loans	10.8904	10.9134	2.1122	4,038,547
Medium-term loans	11.0966	11.0070	1.6107	2,498,383
Long-term loans	11.5027	11.4046	1.8488	2,803,115
Province level				
Number of workplaces	8.3198	8.4632	0.6932	561
Treatment				
Balance sheet				
Sales	13.9309	13.8220	1.3082	506,851
Labor	1.6966	1.6094	1.3697	506,851
TFP	12.7548	12.6609	1.3082	506,851
Credit registry				
Short-term loans	10.9598	10.9404	2.2357	4,202,298
Medium-term loans	11.1628	11.0635	1.6910	2,462,044
Long-term loans	11.5928	11.4685	1.9333	2,813,610
Province level				
Number of workplaces	9.6233	9.7265	0.5775	154

Notes: All variables are log-transformed. Short term loans have a maturity of less than a year. Medium term loans have a maturity between 1 and 3 years. Long term loans have a maturity of more than 3 years.

Table 3. Impact on bank loans - firm level

	(1)	(3)	(4)	(4)	(5)	(6)	(7)	(8)	(9)
	Short-run loans			Medium-run loans			Long-run loans		
A- All									
Legislation (2012-2014)	-0.0575	-0.0260	-0.0335	-0.0015	0.0122	-0.0078	0.0743**	0.1276***	0.1171***
	(0.0387)	(0.0298)	(0.0234)	(0.0214)	(0.0225)	(0.0199)	(0.0305)	(0.0284)	(0.0217)
Implementation (2015-2019)	-0.0538	0.0187	0.0130	0.0378	0.0713	0.0410	0.1474***	0.1998***	0.1932***
	(0.0507)	(0.0434)	(0.0354)	(0.0382)	(0.0454)	(0.0396)	(0.0469)	(0.0503)	(0.0356)
N	8,239,301	8,239,301	8,239,301	4,958,064	4,958,064	4,958,064	5,614,777	5,614,777	5,614,777
B- Privately-owned banks									
Legislation (2012-2014)	-0.1078***	-0.0752***	-0.0802***	0.0104	0.0156	0.0048	0.0810**	0.0876***	0.0787***
	(0.0382)	(0.0263)	(0.0239)	(0.0222)	(0.0239)	(0.0179)	(0.0318)	(0.0324)	(0.0251)
Implementation (2015-2019)	-0.1137**	-0.0298	-0.0342	0.0170	0.0401	0.0083	0.1482***	0.1958***	0.1841***
	(0.0512)	(0.0486)	(0.0455)	(0.0412)	(0.0510)	(0.0403)	(0.0488)	(0.0583)	(0.0475)
N	6,829,537	6,829,537	6,829,537	3,860,721	3,860,721	3,860,721	3,916,955	3,916,955	3,916,955
C- State-owned banks									
Legislation (2012-2014)	-0.0462	-0.0539	-0.0501*	-0.0601	-0.0538	-0.0425	0.0099	0.0802**	0.0973**
	(0.0376)	(0.0369)	(0.0273)	(0.0395)	(0.0413)	(0.0436)	(0.0362)	(0.0387)	(0.0382)
Implementation (2015-2019)	-0.0401	-0.0390	-0.0319	-0.0284	-0.0038	0.0181	0.0120	0.0468	0.0716**
	(0.0447)	(0.0485)	(0.0399)	(0.0355)	(0.0392)	(0.0436)	(0.0414)	(0.0416)	(0.0342)
N	3,264,962	3,264,962	3,264,962	1,436,284	1,436,284	1,436,284	2,469,042	2,469,042	2,469,042
Firm	+	+	+	+	+	+	+	+	+
Province	+	+	+	+	+	+	+	+	+
Year-month	+	+	+	+	+	+	+	+	+
Nuts-1 x Year		+			+			+	
Nuts-2 x Year			+			+			+

*** p<0.01, ** p<0.05, * p<0.1
Notes: Standard errors clustered at the (65) province level. The dependent variable is the log of outstanding loans at the firm level. Short-term loans have a maturity of less than a year. Medium-term loans have maturity between 1 to 3 years. Long-term loans have a maturity of more than 3 years.

Table 4. Impact on bank loans - firm-bank level

	(1)	(3)	(4)	(4)	(5)	(6)	(7)	(8)	(9)
	Short-run loans			Medium-run loans			Long-run loans		
A- All									
Legislation (2012-2014)	-0.0678***	-0.0523**	-0.0626***	-0.0027	0.0101	0.0044	0.0293	0.0619***	0.0734***
	(0.0238)	(0.0197)	(0.0163)	(0.0152)	(0.0182)	(0.0168)	(0.0216)	(0.0221)	(0.0197)
Implementation (2015-2019)	-0.0693**	-0.0397*	-0.0493**	0.0131	0.0273	0.0234	0.0292	0.0699**	0.0882***
	(0.0304)	(0.0227)	(0.0202)	(0.0257)	(0.0338)	(0.0349)	(0.0324)	(0.0328)	(0.0292)
N	17,330,600	17,330,600	17,330,600	7,180,609	7,180,609	7,180,609	8,581,358	8,581,358	8,581,358
B- Privately-owned banks									
Legislation (2012-2014)	-0.0801***	-0.0542**	-0.0637***	0.0078	0.0173	0.0077	0.0483*	0.0687***	0.0755***
	(0.0273)	(0.0220)	(0.0207)	(0.0180)	(0.0219)	(0.0189)	(0.0257)	(0.0255)	(0.0217)
Implementation (2015-2019)	-0.0784**	-0.0317	-0.0403	0.0129	0.0209	0.0076	0.0664	0.1154**	0.1203***
	(0.0361)	(0.0270)	(0.0267)	(0.0297)	(0.0420)	(0.0429)	(0.0442)	(0.0458)	(0.0425)
N	12,320,013	12,320,013	12,320,013	5,059,640	5,059,640	5,059,640	5,274,643	5,274,643	5,274,643
C- State-owned banks									
Legislation (2012-2014)	-0.0397	-0.0590**	-0.0676***	-0.0467	-0.0460	-0.0193	-0.0102	0.0374	0.0592
	(0.0278)	(0.0265)	(0.0251)	(0.0384)	(0.0436)	(0.0451)	(0.0300)	(0.0353)	(0.0402)
Implementation (2015-2019)	-0.0566*	-0.0729**	-0.0785**	-0.0331	-0.0154	0.0149	-0.0485	-0.0221	0.0215
	(0.0301)	(0.0330)	(0.0333)	(0.0337)	(0.0426)	(0.0452)	(0.0338)	(0.0347)	(0.0360)
N	3,995,977	3,995,977	3,995,977	1,555,487	1,555,487	1,555,487	2,820,536	2,820,536	2,820,536
Firm	+	+	+	+	+	+	+	+	+
Province	+	+	+	+	+	+	+	+	+
Year-month	+	+	+	+	+	+	+	+	+
Nuts-1 x Year		+			+			+	
Nuts-2 x Year			+			+			+

*** p<0.01, ** p<0.05, * p<0.1
Notes: Standard errors clustered at the (65) province level. The dependent variable is the log of outstanding loans at the firm-bank level. Short-term loans have a maturity of less than a year. Medium-term loans have maturity between 1 to 3 years. Long-term loans have a maturity of more than 3 years.

Table 5. Impact on number of firms with bank loans

	(1)	(3)	(4)	(4)	(5)	(6)
	Province level			Bank-province level		
Legislation (2012-2014)	0.0421	0.0573	0.0509*	0.0375	0.0893	0.0967
	(0.0514)	(0.0402)	(0.0293)	(0.0656)	(0.0734)	(0.0725)
Implementation (2015-2019)	0.0252	0.0498	0.0373	0.0179	0.1094	0.1176
	(0.0713)	(0.0596)	(0.0583)	(0.1152)	(0.1172)	(0.1042)
N	10,140	9,984	9,672	207,729	207,729	207,729
Province	+	+	+			
Bank-province				+	+	+
Year-month	+	+	+	+	+	+
Nuts-1 x Year		+			+	
Nuts-2 x Year			+			+

*** p<0.01, ** p<0.05, * p<0.1
Notes: Standard errors clustered at the (65) province level. The dependent variable is the log of the number of firms with bank loans at the province level in columns 1-3 and the log of the number of bank-firm relationships at the bank-province level in columns 4-6.

Table 6. Impact on firm balance sheet outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		All			Manufacturing			Services	
A- Employment									
Legislation (2012-2014)	0.0229*	0.0323**	0.0246*	0.0817***	0.0842***	0.0859***	0.0143	0.0212	0.0090
	(0.0125)	(0.0125)	(0.0131)	(0.0163)	(0.0137)	(0.0119)	(0.0140)	(0.0141)	(0.0152)
Implementation (2015-2019)	0.0338**	0.0420*	0.0397	0.1107***	0.1037***	0.1262***	0.0277	0.0351	0.0221
	(0.0162)	(0.0211)	(0.0247)	(0.0279)	(0.0280)	(0.0296)	(0.0185)	(0.0212)	(0.0241)
B- Sales									
Legislation (2012-2014)	0.0095	0.0165	0.0088	0.0403*	0.0475**	0.0542**	0.0080	0.0080	-0.0066
	(0.0186)	(0.0124)	(0.0120)	(0.0205)	(0.0237)	(0.0235)	(0.0197)	(0.0121)	(0.0111)
Implementation (2015-2019)	0.0167	0.0327	0.0343**	0.0381	0.0449	0.0671**	0.0260	0.0332*	0.0209*
	(0.0277)	(0.0196)	(0.0160)	(0.0327)	(0.0382)	(0.0299)	(0.0309)	(0.0185)	(0.0117)
C- TFP									
Legislation (2012-2014)	-0.0063	-0.0053	-0.0065	-0.0146	-0.0081	-0.0026	-0.0024	-0.0066	-0.0116
	(0.0156)	(0.0111)	(0.0117)	(0.0194)	(0.0221)	(0.0216)	(0.0162)	(0.0110)	(0.0119)
Implementation (2015-2019)	-0.0069	0.0033	0.0069	-0.0372	-0.0243	-0.0166	0.0060	0.0080	0.0051
	(0.0257)	(0.0190)	(0.0193)	(0.0318)	(0.0366)	(0.0295)	(0.0266)	(0.0175)	(0.0186)
N	990,133	990,133	990,133	7,289,179	7,289,179	7,289,179	683,193	683,193	683,193
Firm	+	+	+	+	+	+	+	+	+
Province	+	+	+	+	+	+	+	+	+
Year	+	+	+	+	+	+	+	+	+
2-digit NACE x Year	+	+	+	+	+	+	+	+	+
Nuts-1 x Year		+				+		+	
Nuts-2 x Year			+				+		+

*** p<0.01, ** p<0.05, * p<0.1
Notes: Standard errors clustered at the (65) province level. The dependent variable in panel (A) is log transformed firm employment.The dependent variable in panel (B) is log transformed and CPI corrected firm sales.

Table 7. Impact on province level outcomes

	(1)	(2)	(3)
A- Number of workplaces			
Legislation (2012-2014)	0.0277	0.0292	0.0217
	(0.0254)	(0.0188)	(0.0263)
Implementation (2015-2019)	0.0388	0.0501	0.0360
	(0.0429)	(0.0312)	(0.0428)
B- Net entry			
Legislation (2012-2014)	-0.0011	-0.0017	-0.0018
	(0.0029)	(0.0033)	(0.0035)
Implementation (2015-2019)	-0.0015	-0.0005	0.0007
	(0.0026)	(0.0029)	(0.0031)
N	715	704	682
Province	+	+	+
Year	+	+	+
Nuts-1 x Year		+	
Nuts-2 x Year			+

*** p<0.01, ** p<0.05, * p<0.1
Notes: Standard errors clustered at the (65) province level. Variables in panels (A), (B) and (C) are at the province-level and are log-transformed. Net entry in panel (D) is defined as the number of in-migrants minus out-migrants divided by province population in 2013.

Appendix A - Event studies with alternative samples

Figure A1. Alternative samples - event study for short-term loans

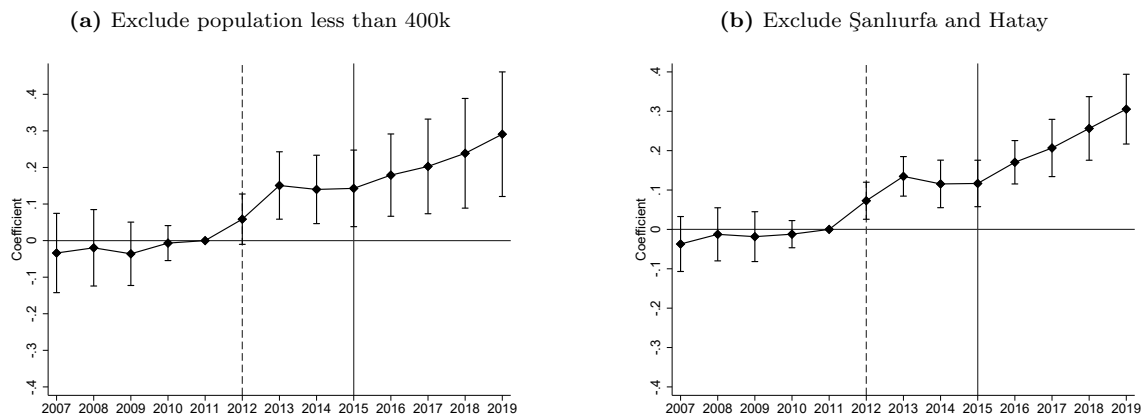**(a)** Exclude population less than 400k   **(b)** Exclude Şanlıurfa and Hatay
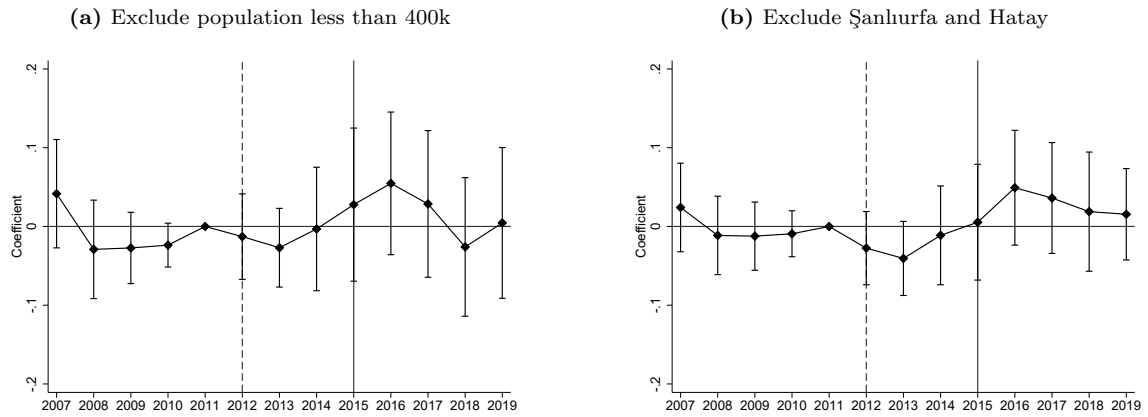
Notes: The dependent variable is firm level short-term loans. Panel (a) excludes all firms in provinces with a population below 400,000 in 2013. Panel (b) excludes Hatay and Şanlıurfa, two provinces with high shares of Syrian refugees during the treatment years. The treatment variable of interest is whether the province was made a metropolitan by the 2012 legislation, and the figure shows yearly coefficient estimates for this treatment variable. The estimates come from a regression where treatment variable is interacted with year dummies. The baseline year is 2011. Other control variables in this regression include firm, province, year-month and NUTS-2 region-year fixed effects. The vertical bars illustrate the 95% confidence intervals. The dashed vertical line in 2012 indicates the year the legislation was passed and the solid vertical line on 2015 indicates the year the legislation took full effect.

Figure A2. Alternative samples - event study for medium-term loans

(a) Exclude population less than 400k
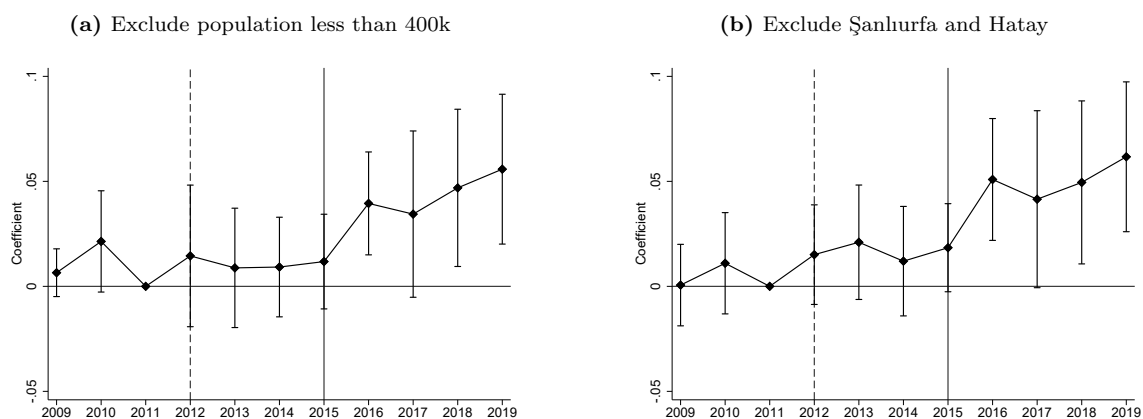
(b) Exclude Şanlıurfa and Hatay
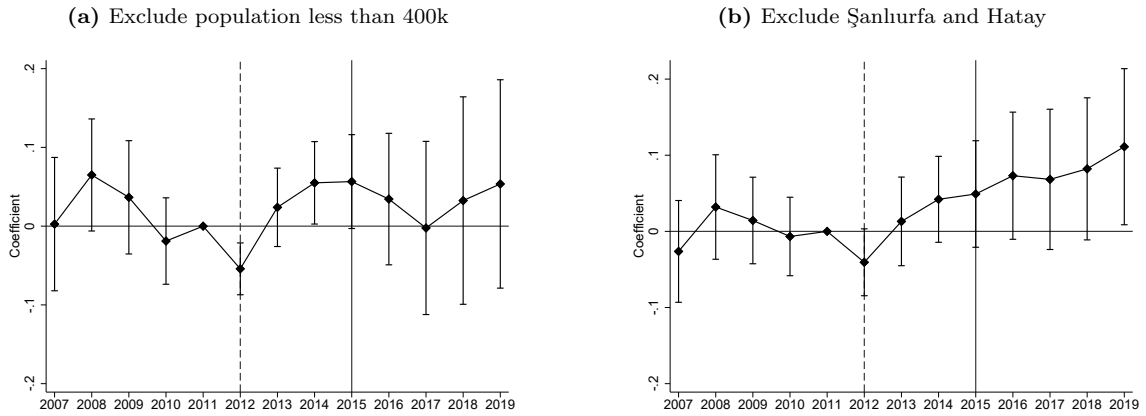
Notes: The dependent variable is firm level medium-term loans. Panel (a) excludes all firms in provinces with a population below 400,000 in 2013. Panel (b) excludes Hatay and Şanlıurfa, two provinces with high shares of Syrian refugees during the treatment years. The treatment variable of interest is whether the province was made a metropolitan by the 2012 legislation, and the figure shows yearly coefficient estimates for this treatment variable. The estimates come from a regression where treatment variable is interacted with year dummies. The baseline year is 2011. Other control variables in this regression include firm, province, year-month and NUTS-2 region-year fixed effects. The vertical bars illustrate the 95% confidence intervals. The dashed vertical line in 2012 indicates the year the legislation was passed and the solid vertical line on 2015 indicates the year the legislation took full effect.

Figure A3. Alternative samples - event study for long-term loans

(a) Exclude population less than 400k
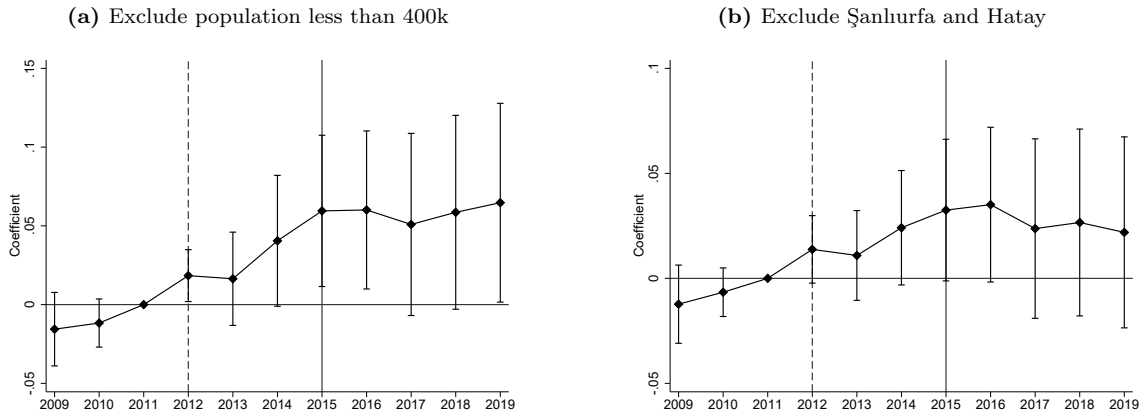

(b) Exclude Şanlıurfa and Hatay

Notes: The dependent variable is firm level long-term loans. Panel (a) excludes all firms in provinces with a population below 400,000 in 2013. Panel (b) excludes Hatay and Şanlıurfa, two provinces with high shares of Syrian refugees during the treatment years. The treatment variable of interest is whether the province was made a metropolitan by the 2012 legislation, and the figure shows yearly coefficient estimates for this treatment variable. The estimates come from a regression where treatment variable is interacted with year dummies. The baseline year is 2011. Other control variables in this regression include firm, province and NUTS-2 region-year fixed effects. Year fixed effects are omitted due to collinearity. The vertical bars illustrate the 95% confidence intervals. The dashed vertical line in 2012 indicates the year the legislation was passed and the solid vertical line on 2015 indicates the year the legislation took full effect.

Figure A4. Alternative samples - event study for firm sales

(a) Exclude population less than 400k

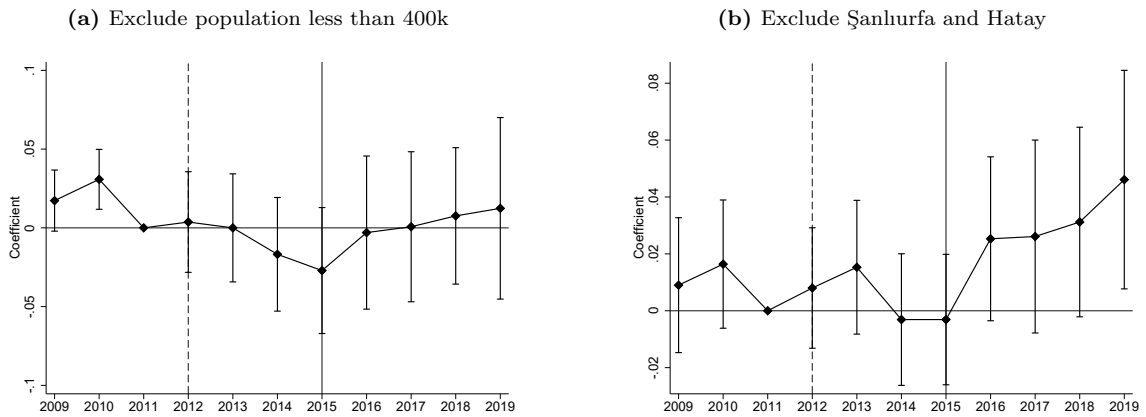**(b)** Exclude Şanlıurfa and Hatay

Notes: The dependent variable is firm level sales. Panel (a) excludes all firms in provinces with a population below 400,000 in 2013. Panel (b) excludes Hatay and Şanlıurfa, two provinces with high shares of Syrian refugees during the treatment years. The treatment variable of interest is whether the province was made a metropolitan by the 2012 legislation, and the figure shows yearly coefficient estimates for this treatment variable. The estimates come from a regression where treatment variable is interacted with year dummies. The baseline year is 2011. Other control variables in this regression include firm, 2-digit sector-year, province and NUTS-2 region-year fixed effects. Year fixed effects are omitted due to collinearity. The vertical bars illustrate the 95% confidence intervals. The dashed vertical line in 2012 indicates the year the legislation was passed and the solid vertical line on 2015 indicates the year the legislation took full effect.

Figure A5. Alternative samples - event study for firm employment

(a) Exclude population less than 400k

(b) Exclude Şanlıurfa and Hatay

Notes: The dependent variable is firm level employment. Panel (a) excludes all firms in provinces with a population below 400,000 in 2013. Panel (b) excludes Hatay and Şanlıurfa, two provinces with high shares of Syrian refugees during the treatment years. The treatment variable of interest is whether the province was made a metropolitan by the 2012 legislation, and the figure shows yearly coefficient estimates for this treatment variable. The estimates come from a regression where treatment variable is interacted with year dummies. The baseline year is 2011. Other control variables in this regression include firm, 2-digit sector-year, province and NUTS-2 region-year fixed effects. Year fixed effects are omitted due to collinearity. The vertical bars illustrate the 95% confidence intervals. The dashed vertical line in 2012 indicates the year the legislation was passed and the solid vertical line on 2015 indicates the year the legislation took full effect.

Figure A6. Alternative samples - event study for TFP

(a) Exclude population less than 400k

(b) Exclude Şanlıurfa and Hatay

Notes: The dependent variable is firm level TFP. Panel (a) excludes all firms in provinces with a population below 400,000 in 2013. Panel (b) excludes Hatay and Şanlıurfa, two provinces with high shares of Syrian refugees during the treatment years. The treatment variable of interest is whether the province was made a metropolitan by the 2012 legislation, and the figure shows yearly coefficient estimates for this treatment variable. The estimates come from a regression where treatment variable is interacted with year dummies. The baseline year is 2011. Other control variables in this regression include firm, 2-digit sector-year, province and NUTS-2 region-year fixed effects. Year fixed effects are omitted due to collinearity. The vertical bars illustrate the 95% confidence intervals. The dashed vertical line in 2012 indicates the year the legislation was passed and the solid vertical line on 2015 indicates the year the legislation took full effect.