Favoritism and Firms:

Micro Evidence and Macro Implications

Zareh Asatryan*†   Thushyanthan Baskaran*‡

Carlo Birkholz*§   David Gomtsyan*¶

August 2024

Abstract

We study the economic implications of regional favoritism, a form of distributive politics that redistributes resources geographically within countries. Using enterprise surveys from low- and middle-income countries, we document that firms located close to leaders' birthplaces grow substantially in sales and employment after leaders assume office. Firms in favored areas also experience increases in sales per worker, wages, and measured total factor productivity. These effects are short-lived, and operate through rising government demand in the non-tradable sector. We calibrate a simple structural model of resource misallocation in a two-sector and two-region economy on our estimates. This exercise implies that, despite large firm-level effects, output losses caused by favoritism are small because leaders do not tend to redistribute funds towards less productive regions.

JEL codes: D22, D72, O43, R11.

Keywords: Regional favoritism, firm performance, enterprise surveys, resource misallocation.

1   Introduction

Regional favoritism – defined as the geographic redistribution of resources within countries based on preferential political treatment – is a widespread phenomenon observed in many parts of the world (Hodler and Raschky 2014). Many authors have claimed that such distributive politics, particularly prevalent in lower income and less democratic countries (Golden and Min 2013), lead to distortionary economic policies that sustain or even exacerbate the income gap between high- and low-income countries.

However, the magnitude of the effect and the mechanisms through which distributive policies between regions generate aggregate inefficiencies have not been explored rigorously. The ultimate answer hinges on the productivity levels of recipient regions compared to the rest of the economy, as well as the efficiency of the firms that benefit most within the recipient regions. If leaders divert resources excessively to beneficiaries in their home regions that are unproductive, for example due to the distributive policy being driven by private rent-seeking motives or a desire to hand out benefits solely to secure political support, favoritism can result in sizeable aggregate efficiency losses. Conversely, if the redistribution targets productive firms, for example because leaders leverage informational advantages about their home regions, favoritism will not substantially diminish welfare.

To understand the aggregate implication of regional favoritism, we employ cross-sectional survey data from up to around 150,000 enterprises in 120 low- and middle-income countries, and utilize transitions of national political leaders which provide us with identifying variation in up to 33 countries. With this data and identification at hand, we document the firm level effects of regional favoritism, trace the channels leading to these effects, and calibrate a simple structural model of resource misallocation in a two-sector and two-region economy to estimate the aggregate effects of favoritism.

Our first contribution is to document the existence of strong regional favoritism in firm outcomes using a difference-in-differences approach. Firms located around the birthplaces of political leaders are larger in terms of their sales and number of employees than firms located

in other regions during the leaders' term in office. Exploiting information on the exact geo-location of firms, we show that these effects of favoritism are strongest in very close proximity to the leaders' birthplaces and gradually diminish with increasing distance. In our baseline specification, we employ a distance measure tailored to countries' specific sizes and shapes to define treatment. We find that treated firms have 14% higher sales and 8% more employees compared to control firms. For the average firm, these effects correspond to an increase of $1.1 million in sales and 6 additional employees. We show that these results are robust to several alternative definitions of the treatment area. Our placebo analysis does not find evidence for the existence of pre-trends in firm outcomes, suggesting that causality likely runs from leader changes to firm outcomes. While we acknowledge that the cross-sectional nature of our data is not ideal for testing such dynamic effects, the very local effects in close proximity to leaders' birthplaces that we identify make reverse causality – where local growing firms affect the choice of leaders – unlikely. Additionally, using yearly nighttime light intensity as a proxy of economic activity, we demonstrate the absence of pre-trends in this measure as well. A further robustness exercise uses propensity score weights from random forest classification to balance out differences in many observable characteristics between treated and control firms, and confirms our baseline findings.

As a second contribution, we exploit the richness of our enterprise survey data, and study the mechanisms that lead to these outcomes. We find that firms located in favored regions are not only larger in size, but that they also produce more output per worker, pay higher wages, and have higher total factor productivity compared to other firms. Prima facie, this evidence suggests that regional favoritism may be considered as an efficiency enhancing policy. However, our further results indicate that the effects are driven by the non-tradable sector only, partly fueled by direct government transfers, and that they are temporary, fading away almost immediately after leaders leave office. This evidence goes in contrast to the hypothesis that favoritism induces general productivity improvements, since these should lead to more balanced growth in both the tradable and non-tradable sectors, as well as extend to the longer-term (van der Ploeg 2011). Additionally, we do not find evidence that any of

the important correlates of productivity – such as exports, management practices, quality of inputs, or research and development activities – improve in firms located in favored regions, nor that the general business and regulatory environment – as measured by firms' perceptions on business constraints – improves among these firms. Overall, these results are consistent with the interpretation that leaders divert public resources to their home regions, thereby generating higher demand for output produced by firms operating in the non-tradable sector. This redistribution comes at the cost of other regions, and is thus indicative of misallocation of resources.

As a final step, we set up a simple misallocation model in the spirit of Restuccia and Rogerson (2008). We use the model to quantify the aggregate implications of regional favoritism. We consider an economy with two regions and two sectors, where firms face wedges driven by favoritism. In our setting redistribution between regions increases the level of income in the leader's region and thus demand. Since demand for non-tradable goods can be satisfied only by local production, factors of production reallocate towards the non-tradable sector in the leader's region and towards the tradable sector in the other region. This higher concentration of resources in the two sectors decreases the marginal productivity of firms and results in aggregate losses. We calibrate the model to match the moments that we estimate empirically. Our counterfactual exercise shows that in a country with spatial wedges driven by favoritism, output is 0.07% lower compared to a distortion free economy. The fact that output losses are only small despite large effects at the firm level is primarily driven by the fact that the distribution of firms within regions is not affected by favoritism. Moreover, we identify only small differences in productivity levels between the the leaders' region and the rest of the county. Thus, there is no evidence of reallocation of resources towards inefficient firms or regions.

Our paper is related to two strands of literature. First, we contribute to the evolving literature on regional favoritism. Miquel et al. (2007) were one of the first to develop a theoretical framework for favoritism, and Hodler and Raschky (2014) were one of the first to document evidence for it. In particular, they use satellite data from across the globe and

find higher intensity of nighttime light in the birthplaces of the countries' political leaders compared to other regions within countries. A closely related literature documents similar favoritism effects in political leaders' ethnic homelands.[1] Several papers extend the work on ethno-regional favoritism to specific sets of policies.[2] Our contribution is to study the effects of favoritism on firms, which allows to better understand the productivity implications of such distributional polices.

Second, our paper relates to the literature on how the misallocation of factors of production leads to differences in aggregate total factor productivity. This literature goes back to Hsieh and Klenow (2009, 2010), Restuccia and Rogerson (2008), and is surveyed by Hopenhayn (2014), Restuccia and Rogerson (2017) and Martinez-Bravo and Wantchekon (2021). In this context several studies have used enterprise survey data to estimate aggregate output losses caused by various institutional frictions (Besley and Mueller 2018, Ranasinghe 2017). Our contribution is to highlight a new source of misallocation that is driven by regional favoritism, which is caused by the endogenous concentration of production factors in tradable and non-tradable sectors in each region. Several related papers study efficiency losses caused by policy distortions in spatial contexts. Brandt et al. (2013) study China's economy in a model with multiple provinces and private and state-owned types of firms. Desmet and Rossi-Hansberg (2013) introduce labor wedges to a model with cities to asses efficiency losses in the US and China. Fajgelbaum et al. (2018) use an economic geography model to estimate welfare losses caused by heterogeneity in tax systems across US states.

The remainder of the paper is structured as follows: Section 2 presents the data and our identification approach. Section 3 discusses our baseline empirical results as well as the robustness tests. In Section 4 we develop the mechanisms that drive our main findings.

---

[1]De Luca et al. (2018), Dickens (2018) observe higher nighttime light intensity in political leaders' ethnic homelands, and Amodio et al. (2019), Asatryan et al. (2021), Franck and Rainer (2012), Kramon and Posner (2016) find evidence for improved human capital outcomes among individuals belonging to either the same ethnicity, or coming from the same region as those holding political power.

[2]These policies include road building in Kenyan districts (Burgess et al. 2015) and Sub-Saharan Africa more broadly (Bandyopadhyay and Green 2019), infrastructure projects in Vietnam (Do et al. 2017), school construction in Benin (André et al. 2018), enforcement of audits (Chu et al. 2021) and taxes (Chen et al. 2019) in China, mining activities in Africa (Asatryan et al. 2021), and the allocation of foreign aid in Africa (Anaxagorou et al. 2020, Dreher et al. 2019), among others.

Section 5 sets up the quantitative model and calibrates it to arrive at aggregate implications. Section 6 concludes.

2 Empirical design

2.1 Data

Firms Our firm-level data are a repeated cross-section drawn from the World Bank Enterprise Surveys. The surveys have been conducted since 2006, and they span over 140 countries, of which 98 countries have been surveyed more than once. Among these countries, the survey is typically repeated in two to five year intervals, leading to an average of 2.5 survey waves per country. Firms are drawn by stratified random sampling, with stratification performed based on firm size, geographic location within the country, and sector of activity.[3] The surveys cover non-micro formal firms in the non-agricultural private sector. Thus, by design, they exclude firms which are fully government owned, are informal, have less than five employees, or are classified as agricultural firms. In general, our data will be representative of the manufacturing and service sectors, but not for the above-mentioned sectors or firms.

The enterprise surveys contain information on general firm characteristics such as their age, ownership structure, and sector, as well as indicators of their performance in terms of sales, employment, and input factors. In addition, firms are asked about their management practices, relations to the government, crime and corruption, and the business environment. These latter aspects allow us to study the channels of how favoritism operates in greater detail.

For the main part of our empirical analysis, we consider the sub-sample of surveys carried out since 2009, as they provide us with the geocoded location of firms.[4] In additional specifications we use the general sample, where we can identify the location of firms on a regional level. We give priority to the smaller sub-sample of geocoded data to achieve greater

---

[3]Further information on the sampling and stratification procedure can be found at https://www.enterprisesurveys.org/en/methodology.

[4]For data privacy reasons the latitudes and longitudes are precise within 0.5 to 2 kilometers.

Figure 1: Birthplaces of Leaders and Locations of Firms in the Sample
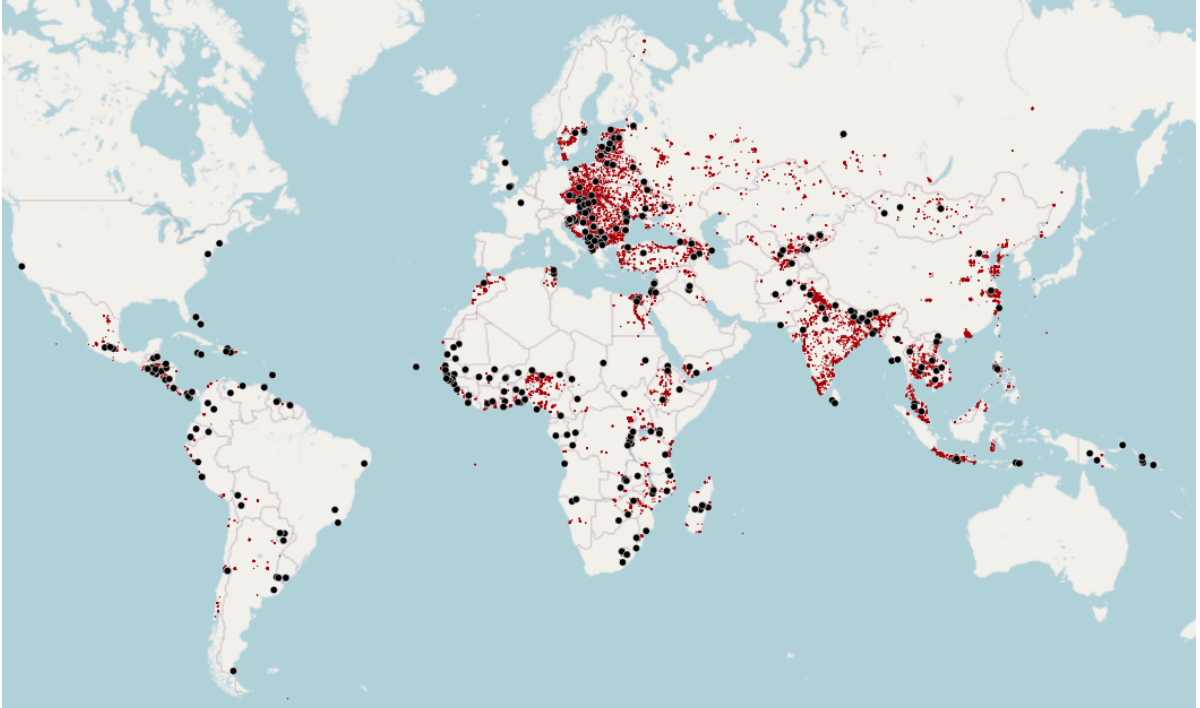
$Notes$: This map shows the geography of our sample. The small red dots represent firms, the large black dots leaders' birthplaces. Table A1 presents the list of countries and survey waves in our sample. There are around 25,000 African, 40,000 Asian, 20,000 European, 6,000 Middle American and 10,500 South American firms available in our main sample.

precision, and to perform detailed spatial analysis, while we rely on the latter sample to test the robustness of our baseline findings on a larger sample.

Political leaders   To identify political leaders in power we use the Archigos database of political leaders (version 4.1). The database includes information on the start and end date of the primary effective leader's time in power. Archigos data are available up to 2015 and we manually extend these data by including leaders from 2016 to 2020. We then utilize a plug-in that automatically parses a leader's birthplace to Google Maps' API, and retrieves the latitude and longitude of the city or town. We manually validate no matches or faulty matches, which can arise due to cities sharing the same names, special characters in city names, or other reasons. We exclude any leader with less than a year of tenure.

We merge this data on leaders to the enterprise data by country. In the geocoded sub-sample we can calculate the distance of every firm to each leader's birthplace in the sample period. In the larger sample with regions as the spatial dimension, we generate a dummy indicating whether a firm is within a leader's birth region. In total we have 250 leaders coming from 120 countries. Figure 1 plots leaders' birthplaces and firms in a map. Since our empirical strategy builds on leader transitions, our identifying variation comes from a much smaller sample than the 250 leaders. First, as discussed above, the enterprise surveys have only been carried out 2.5 times within each country on average. Second, in many countries, especially in less democratic ones, we do not observe leader transitions within our relatively short sample. Third, in cases when leaders were born in foreign countries, we do not identify any favored region. Taking into account these restrictions, our identifying variation comes from 25 countries in the baseline sample and from 33 countries in the regional sample.

Country characteristics In order to allow for comparisons across countries, and for the interpretation of mean and aggregate values of monetary variables, we transform variables from local currency units to 2009 USD. For this transformation, we use period average exchange rates and GDP deflators from the World Bank's World Development Indicators. To study whether the effects of favoritism differ with respect to the political and institutional features of countries, we collect democracy index data from the V-Dem electoral democracy index, as well as data on perception of corruption from the World Banks Worldwide Governance Indicators.

Sample and summary statistics In total there are around 100,000 and 150,000 enterprise surveys carried out in the geocoded and regional samples, respectively. However, the key variables we use have missing values to a varying degree. Additionally, to alleviate bias in our estimates from outliers, we exclude values that are outside three standard deviations of the calculated mean within an industry and country income level. For our baseline analysis this leaves us with 82,000 to 94,000 firm-level observations, depending on the outcome we study. In the regional specification we have between 126,000 to 142,000 observations.

Table A1 of the appendix lists the countries and survey years in our sample, with information on the number of firms per country and survey wave. We note the countries that contribute identifying variation in our two samples. Table A2 in the Appendix shows the summary statistics of the variables used in this paper.

2.2 Identification

Our empirical strategy exploits data on leader transitions and firm locations for identification in a difference-in-differences setup. We compare firms located in 'favored' areas in the sense of the current national leader being born in that area, to firms in the same area but in a time period when the current leader was not in office. Firms located in other non-favored areas but having similar observable characteristics, such as being in the same industry, serve as our control group.

2.2.1 The definitions of regions

A central question of our empirical design relates to the spatial extent at which favoritism takes place. As we discussed in Section 2.1, our data measure the location of firms either by the geocoordinates of the firm, or by the region of their location as reported in the enterprise surveys. This lets us define treatment based on spatiality in a number of ways. The first conceptual choice regards the use of regional boundaries versus the use of distances. We prefer the latter approach utilizing the geocoded sample, as it allows us to study spatial effects around leaders' birthplaces at higher granularity and precision. This higher precision stems from two facts: First, depending on the shape and size as well as the location of the leader's birthplace within a region, treatment assignment based on regional boundaries will capture different firms. As an example, defining regional treatment for a leader born at the edge of an elongated region will assign firms in close proximity just across the border to the control group, whereas firms potentially far away on the other side of the region get assigned treatment status. Second, the region definitions within our data are not always consistent across time, and do not always coincide with administrative regions - which themselves might

shift over time.[5] There are however two upsides to defining treatment based on regions. Since we don't require information on the geocoordinates of the firms, we can utilize the full sample. Furthermore, if favoritism does take place through regional policies that are channeled at the level of administrative regions, this definition would capture the effect most precisely. Given these tradeoffs, we start by studying firms whose exact geolocations are available, where we can identify treatment effects over granular distances. We then replicate our results on the larger sample with the regional specification to obtain complementary evidence.

Within the distance based specification another conceptual question pertaining to the spatial extent of favoritism arises. Does favoritism travel equally far in countries of different sizes and shapes? For example, using larger distances for the treatment assignment might be adequate in a bigger or less compactly shaped country, while the same distance might cover the majority of a smaller or more compact country. To address this issue our baseline specification uses a country-specific distance measure that incorporates a country's shape and size. Inspired by Harari (2020), we construct the measure by overlaying each country's shape with a fine-grained point layer, and calculate the average Euclidean distance between a random selection of 10% of these points over 100 repetitions. The measure will vary across countries for two reasons: A larger country has a larger point layer, which on average leads to the randomly drawn points being further apart, thereby increasing the measure. The measure also increases the more a country's shape diverges from the most compact shape - a circle. We visualize these concepts in Figure A1.[6] An alternative approach to this is to define a fixed distance across countries, which we conduct as a robustness check.

---

[5]We can also manually map firms based on their geocoordinates into regional boundaries. We employ this technique in Section 3.3.1, where we overlay the world with a 0.5×0.5 degree grid layer and allocate the firms into cells. This allows to employ a stable granular region fixed effect, and mirrors the geographic boundaries used in the prior favoritism literature with night lights. In the same vein we could map firms consistently into administrative regions. However both approaches do not alleviate the first drawback we mention, while removing the largest upside of the regional treatment definition, namely the larger sample.

[6]This measure can be interpreted as the average length of all hypothetical journeys through the country. We incorporate neither the degree of urbanization nor ruggedness of the country's terrain in the calculation of the measure to maintain clarity on what is being captured.

2.2.2 Identification with geocoded firm data

In our baseline we estimate a difference-in-differences model of the following form:

$$
\begin{aligned}
log(Outcome_{f,i,r,c,t}) = & \alpha + \beta^{km_c} \cdot LeaderArea_{l,c}^{km_c} \times Term_{l,c,t} + \\
& \gamma \cdot Controls_{f,t} + \tau_i + \mu_l^{km_c} + \lambda_r + \eta_{c,t} + \epsilon_{f,i,r,c,t}
\end{aligned}
\tag{1}
$$

where $Outcome_{f,i,r,c,t}$ is the logarithm of either total sales, or the number of permanent employees. Our unit of observation is the firm f belonging to industry i located in region r of country c in year t.

β^{km_c} is our coefficient of main interest. It identifies the average treatment effect as the interaction of the dummy variables $LeaderArea_{l,c}^{km_c}$, which turn on if a firm is located within a country-specific kilometer radius km_c to the birthplace of leader l in country c, and the $Term_{l,c,t}$ dummy that indicates whether leader l is currently in office. We described the construction of our country-specific distance measure in the previous section. It is scaled such that in the median we match the area covered by the 0.5×0.5 degree pixels commonly used in the favoritism literature.[7] This is the case for a radius of approximately 31km, which amounts to $\frac{1}{11}$ of our measure. Our results do not rely on choosing this particular share, as we demonstrate in the Appendix C.

Since favoritism might directly, or indirectly through spill-overs, affect firms farther away than the specified distance, we choose a cut-off distance between the treatment and control area. Firms falling into this area are excluded, which allows us to minimize diluting the control group with firms close enough to still be somewhat affected by the treatment. We again utilize the country-specific distance measure, and exclude firms between the treatment distance of $\frac{1}{11}$ and $\frac{1}{7}$ of the measure. Around 3% of observations fall into this area. We again verify robustness to alternative choices of the cut-off distance in the Appendix C.

---

[7]We match on the median not the mean to not overweight particularly large or small countries. Each individual country's value is reported in the Appendix Table A1.

$Controls_f$ is a vector of firm specific control variables including the age of the firm, and its ownership shares belonging to foreigners, or to the public sector. τ_i, $\mu_l^{km_c}$, λ_r and $\eta_{c,t}$ are industry, leader area, region and country-by-time fixed effects, respectively. The error term is captured by $\epsilon_{f,i,r,c,t}$. We cluster the error term at the level of treatment following the arguments laid out by Abadie et al. (2017), which in the baseline estimation amounts to leader area by year. In the Appendix Table A3 we show the robustness of the estimated standard errors under alternative clustering strategies.

2.2.3 Identification with regional firm data

We also estimate a version of Equation (1), where the treatment is defined based on the birth region of the leader. The equation is as follows:

$$log(Outcome_{f,i,r,c,t}) = \alpha + \beta \cdot LeaderRegion_{r,c} \times Term_{l,c,t} + \quad\quad (2)$$
$$\gamma \cdot Controls_{f,t} + \tau_i + \lambda_r + \eta_{c,t} + \epsilon_{f,i,r,c,t}$$

where the treatment status of a firm is defined by $LeaderRegion_{r,c}$ which is a dummy variable indicating whether any national leader was born in region r or not.

2.2.4 Identifying assumptions

Our model compares firms located within areas or regions around leaders' birthplaces before and after leaders assume power, while controlling for firms belonging to the same industries but located further away from leaders' birthplaces. The main identifying assumption in this difference-in-differences setting is that the treatment and control groups follow parallel trends prior to the treatment. In our case, this will be violated if, for example, faster developing regions are more likely to nominate a national leader.

We test this assumption in Section 3.1 by conducting an analysis that tests for effects in leads and lags of the treatment variable. We do not find evidence that the outcome variables between treated and control firms are statistically significantly different from zero

11

in the year preceding the nomination of the leader. This absence of significant pre-trends suggests no systematic bias coming from selection as long as the selection effect is captured by the observables, and assuming that the selection effect is homogeneous across regions, such that the average effect of the pre-trends does not mask potentially offsetting trends. Our evidence from this test is consistent with previous work that has used regional level data to study regional favoritism and does not find evidence for the existence of pre-trends (see, for example, Hodler and Raschky 2014).

We further validate our baseline results by augmenting the baseline difference-in-differences design with a propensity score approach in Section 3.3.2. This exercise suggests that our results are neither driven by differential firm characteristics across treatment and control groups that potentially affect firm outcomes, nor by changes in the composition of groups over time in our repeated cross-sectional data. We also implement a permutation test in Section 3.3.3, which suggests that assigning placebo treatments randomly to areas across time and space only very rarely leads to similarly large treatment effects as the ones we find in our baseline.

Finally, we follow the literature on difference-in-differences design with heterogeneous treatment effects (de Chaisemartin and D'Haultfœuille 2022, Roth et al. 2022), to verify the validity of our setup which involves multiple periods and variation in treatment timing. Given the inclusion of country-by-time fixed effects, and the availability of only few survey waves per country, our results are almost always obtained from comparing treated, and never or not yet treated groups within countries, rather than by making 'forbidden' comparisons between already-treated units. More formally, we execute the diagnostics command *twowayfeweights* by de Chaisemartin and D'Haultfœuille (2020) to investigate the issue of potentially problematic comparisons of early and late treated groups. The test reveals that the large majority of ATTs receive positive weights, which sum to 1.08, a large multiple of the sum of negative weights of -0.08. This reassures the use of the standard two-way fixed effects estimation.

3 Empirical results

3.1 Baseline results

We start by studying the treatment effect of favoritism using the geolocation of firms. We present our baseline results in Table 1. The first column regresses log sales on the treatment variable and the fixed effects as well as key firm characteristics as control variables. The estimated coefficient is statistically significant and implies that firms located close to leaders' birthplaces experience a 14% increase in sales relative to firms in the other parts of the country. In the second column our dependent variable is the log total number of employees. Again, we observe highly significant positive effects of 8% on average. These effects represent a sales increase of $1.1 million, and an employment increase of 6 workers for an average firm.

The magnitudes of the effects are substantial. Taking into account the number of firms operating in these areas, and the sum of their sales, we can calculate the aggregate effects of favoritism in our sample. The favoritism effect leads to an estimated aggregate sales increase of $19.5 billion (in 2009 nominal USD). Hodler and Raschky (2014) calculate that leaders' regions have on average 1% higher GDP in the worldwide sample, but the effects can reach up to 9% in certain subsamples, such as in countries with weak political institutions.[8] We take their approach of mapping the effects on nighttime light to GDP growth using the correlation coefficient of 0.8 between firm revenues and GDP growth, as estimated by Cravino and Levchenko (2017). In our case, the corresponding effect on the favored regions is 11% when transformed into GDP growth values. This estimate is larger than that of Hodler and

---

[8]Following Hodler and Raschky (2014), we study whether the effects of favoritism on firm sales are different across countries with different political institutions. In Table A5 we interact our treatment variable with the electoral democracy score from V-Dem, and with the measure of corruption control from the World Bank. We do not find a linear relation between these institutional measures and our treatment effect. However, when allowing for a quadratic relation, we find suggestive evidence for a concave relation. In autocratic settings, leaders with a very strong grip on power have little incentive to seek support through regional favoritism. Such incentives increase with more democratization, but eventually, as the level of democratic institutions are sufficiently developed to impose the necessary constraints, possibilities of excessive regional redistribution are eliminated. This result should be interpreted with caution, given that the identification of this interaction effect comes from variation across countries.

Table 1: Baseline Results: Treatment Effects around Leaders' Birthplaces

	(1) Log Sales	(2) Log Employees	(3) Log Sales	(4) Log Employees	(5) Log Sales	(6) Log Employees
Treated area	0.1434*** (0.0506)	0.0817** (0.0366)	0.1657*** (0.0513)	0.0874** (0.0390)	0.1349*** (0.0504)	0.0757** (0.0363)
Year before treatment start			0.0462 (0.1598)	-0.1080 (0.1081)		
Year after treatment end					-0.2762*** (0.0836)	-0.2111*** (0.0424)
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	82,527	94,093	82,377	93,921	82,377	93,921
R-squared	0.6621	0.2647	0.6622	0.2648	0.6622	0.2648
F	200.2	228.6	160.5	181.9	163.3	189.3

Notes: The regressions are estimated using Equation 1. Treatment area is defined as $\frac{1}{11}$ of the country-specific distance measure. Dependent variables are specified in logarithms. The mean values of the dependent variables in levels are 7.6 million USD for sales, and 80 employees. USD is measured in 2009 nominal values. Firm level controls include firm age, the share owned by foreigners and the share owned by the public sector. Columns (3) and (4) include a dummy that identifies the year before the start of treatment, and columns (5) and (6) a dummy for the year following the treatment end. Sample size changes are due to varying availability of the outcomes as well as the fact that we exclude rare cases in which observations are prior to and at the same time post treatment, which can occur if of three consecutive leaders the first and third are born in close proximity. The results hold when we fix the sample. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.

Raschky (2014), but not implausible considering that our sample consists of many countries with weaker political institutions.

We conduct placebo estimations to ensure that our results are driven by leader transitions rather than existing trends in regions. Since we are using a difference-in-differences specification, we want to make sure that there are no pre-trends that potentially drive our results. We construct a placebo pre-treatment variable by assuming that the leadership transition took place the year prior to when it actually happened. We also create a post-treatment variable in the same fashion. We then re-estimate Equation (1) including these leads and lags. The results are presented in columns 3 to 6 in Table 1. The pre-treatment dummy does not corre-

late with firm sales or with employment in a significant way, confirming the prior literature's notion of a lack of systematic anticipation or other sources of pre-trends.

Due to the limited frequency of the firm-level data, we are however unable to identify these dynamic effects on an annual basis for longer periods.[9] We therefore go one step further and reassess whether the prior literature's finding of a lack of pre-trends, which is established with nightlight data, also holds for our setting and sample. We present the treatment effect of leader changes on nighttime luminosity for exactly the 25 countries and the time period that lends identifying variation in our main specification. The yearly frequency of the nightlight data enables us to plot an event study in Figure A3. The figure confirms the existing picture. There is no evidence of systematic pre-trends, but a sizeable significant treatment effect three years after the new leader first comes into power.

Table 1 additionally shows large negative effects in the year after treatment ends, indicating that the effects of favoritism on firm growth are not persisting after leaders leave office. This evidence suggests that regional favoritism does not serve as a 'big push' industrial policy, according to which large positive shocks can help firms to permanently change their growth trajectories (Murphy et al. 1989).

3.2   The spatial gradient of the favoritism effect

While we are agnostic about the exact area around the leaders' birthplaces which is affected by favoritism, we postulate the following hypothesis: The closer a firm is located to the leader's birthplace, the more likely it is going to receive favorable treatment. This means that for smaller distances we should estimate larger absolute point estimates at higher precision. However, documenting strong effects on only a small share of the economy might hold little aggregate implications. On the other hand, capturing favoritism at a greater distance means a bigger share of the economy is implicated, but it will lead to more noisy results according to the hypothesis above.

---

[9]Our data similarly constrain us from studying the question of whether favoritism increases with the years a leader is in office. In our case, variation in tenure would come from across rather than within leaders' tenure.

Figure 2: Treatment Effects by Distance to Leaders' Birthplaces
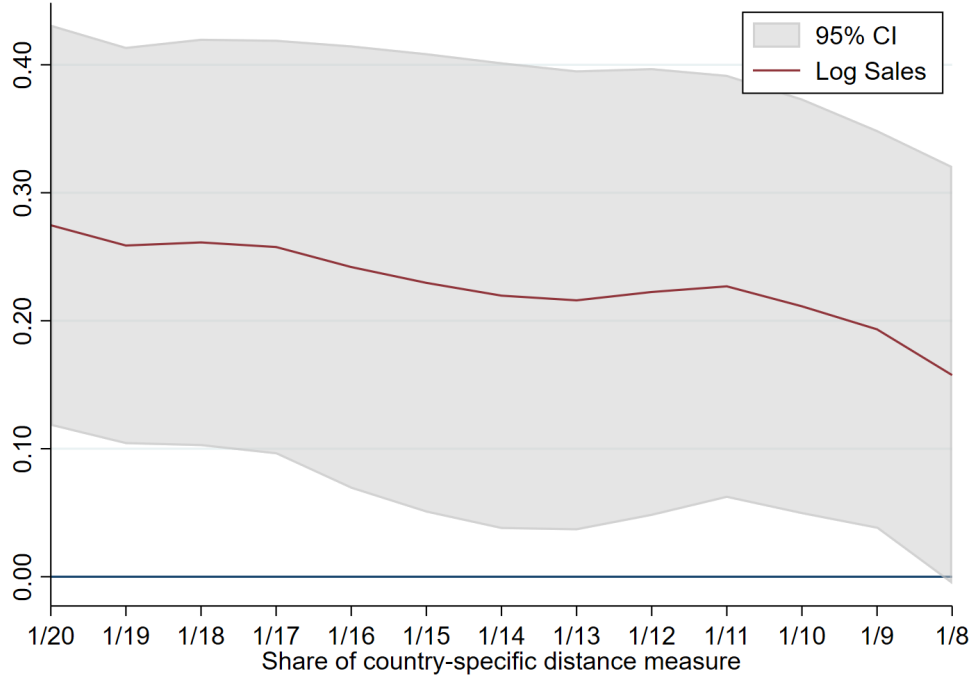
Notes: The regression is estimated using Equation 1. The red line plots the coefficient β^{km_c} estimated for each radius separately. The shaded area represents 95% confidence intervals. To keep the countries contributing to identification stable across the estimates we exclude five countries, which are marked in Table A1. The dependent variable is logarithm of total sales. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.

We aim to test our hypothesis and reveal the spatial dimension of the treatment effect by running the estimation specified in Equation 1 over varying distances.[10] In Figure 2 we plot these treatment effects of favoritism over the varying distances to leaders' birthplaces for the logarithm of sales. As we hypothesized, we measure the strongest effects for areas very close to leaders' birthplaces, they decrease over distance, and become statistically not distinguishable from zero at a share of $\frac{1}{8}$ of our country-specific distance measure. Figure A2, which defines fixed distances starting from a 10km radius around the birthplace of the leader

---

[10]The range of distances we display is informed by the necessity to keep the amount of countries contributing to the identification stable, such that changes in the effect size over distance can be interpreted as due to the spatial spread of favoritism, and not simply due to the sample composition changing.

16

and going until a 100km radius for all countries, documents a similar pattern of a decreasing gradient in the estimated treatment effect.

3.3 Robustness tests

3.3.1 Definition of treated areas

As we discussed in Section 2.2.1, alternative choices in the definition of the treatment area are possible. In this section we assess the robustness of our baseline results by employing a number of these alternative specifications. We start by defining treatment by the region of leaders' birth rather than their exact birthplace. This allows us to utilize a substantially larger sample of firms, for which our data only indicate their location at the regional level. For the second alternative, we overlay countries' geographies with a fine grid layer of 0.5 x 0.5 degree pixels, and map firms into these grid cells.[11] Figure A4 of the Appendix visualizes this grid approach, which allows us to introduce granular pixel fixed effects to control for sub-regional time invariant confounding effects. The third approach we offer, fixes the radius of the treatment area to 50km for all countries, instead of relying on the country-specific measure. Table 2 collects the results. In all cases the evidence for positive and statistically significant effects is replicated.[12]

3.3.2 Propensity score weighting

Our difference-in-differences design leads to the identification of causal effects assuming that the group-specific pre-trends are parallel. Our analysis in the previous section did not find evidence for the existence of differential pre-trends. In this section, we provide a further robustness test by augmenting our difference-in-differences design with a propensity score approach. This exercise allows us to balance out observable differences between the treatment and control groups, thereby ruling out the possibility that the growth of firms in the treated

---

[11]At the equator 0.5 degree corresponds to roughly 55km. Results are also robust to a 1 degree specification.
[12]In an additional specification we interact the region treatment with the 50 km area treatment. Table A4 of the appendix shows the results. We find the strongest effects on firms that are located within a 50 km radius from the leader's birthplace, and at the same time belong to the leader's birth region.

Table 2: Alternative Definitions of Treated Areas: Regions, Pixels, Fixed Radius

	(1) Log Sales	(2) Log Employees	(3) Log Sales	(4) Log Employees	(5) Log Sales	(6) Log Employees
Treated area	0.1308***	0.0609***	0.2378***	0.1667***	0.2139***	0.1404**
	(0.0389)	(0.0221)	(0.0447)	(0.0372)	(0.0749)	(0.0588)
Treatment Area defined by	Regions	Regions	Pixels	Pixels	50km Radius	50km Radius
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	126,359	142,710	69,298	78,838	70,177	79,718
R-squared	0.6643	0.2626	0.6784	0.2833	0.6660	0.2582
F	654.4	826.9	34.97	347.1	129.0	148.4

Notes: The regressions are estimated with alternative definitions of the treatment area, each indicated at the bottom of the table. Dependent variables are specified in logarithms. Firm level controls include firm age, the share owned by foreigners and the share owned by the public sector. All regressions include fixed effects at the level of the respective treatment definition, regions, industries and country-by-years. Standard errors are clustered at the level of treatment.

area is driven by firm characteristics which differ systematically from the characteristics of control firms (Imbens 2015).[13] This exercise also helps alleviate a second potential concern related to firm outcomes being driven by changes in the composition of the treatment and control groups over time. The sampling strategy of firm surveys is designed to make the data representative at the region level, such that, in principle, any compositional differences across the treatment and control groups over time would be the result of our treatment. However, given small sample sizes at the regional level, we nevertheless carry out this exercise.

One common shortcoming of this approach is that the choice of variables, as well as the functional form of the model used to calculate the propensity scores is under the discretion of the researcher. For this reason, we utilize the many firm characteristics available in our dataset in a data-driven machine learning approach. More specifically, we use random forests, an ensemble learning technique that averages the predictions of many individual decision

---

[13]An alternative approach is to include a long list of covariates. The advantage of our approach is that it is more data driven such that we do not need to take a stance on the importance of specific variables. Moreover, it allows for non-linear relationships between firm characteristics and outcome variables.

Table 3: Comparison of Baseline Estimates with Propensity Score Weighting Estimates

	Treated Area Weighted	Treated Area Unweighted	Observations	R-squared Weighted	R-squared Unweighted	F Weighted	F Unweighted
(1) Log Sales	0.2488*** (0.0836)	0.2334*** (0.0707)	69,352	0.6366	0.6639	8.846	193.6
(2) Log Employees	0.1096** (0.0523)	0.1405*** (0.0441)	79,160	0.1145	0.2591	4.395	263.0

Notes : This table compares the treatment effects on the main outcomes estimated with unweighted (i.e. baseline) and weighted (propensity score) specifications of Equation 1. We restrict both specifications to the same sample. For the weighted specification, control variables are dropped, and instead the weights calculated according to Equation **??** are applied. The sample is trimmed to restrict the observations to the area of common support. Treatment area is defined as $\frac{1}{11}$ of the country-specific distance measure. Dependent variables are specified in logarithms. All regressions include fixed effects for leader circles, regions, and country-by-years. Standard errors are clustered at the level of treatment.

trees, to calculate propensity scores (Lee et al. 2010, Zhao et al. 2016). We discuss the technical implementation of the random forest and calculation of the propensity score weights in Appendix B. These weights help us make our treatment and control groups more similar in terms of the observable firm characteristics. Figure B1 shows the distribution of the standardized bias between the two groups before and after the application of the propensity score weights. The weighting shifts the distribution mass towards the center, indicating a substantial reduction in bias between the groups as captured by the observables.

In Table 3 we report the results of our difference-in-differences specification augmented by the propensity score weights. In order to draw comparisons to our baseline results, we re-estimate the baseline specification but restrict it to the same sample on which we run the weighted regressions. The two estimates are very similar in both size and precision for both outcome variables. These results reassure that our baseline results are neither driven by changes in the group composition across time, nor by differences in observable characteristics between the treatment and control groups.

3.3.3 Permutation test

We further address the direction of causality originating from leader transitions by conducting a placebo permutation analysis. Following Chetty et al. (2009), we perturb treatments randomly both across time and spatially. If leader transitions do drive the effects, we must see that they are a statistical rarity compared to the effects generated by the random permutations. To this end, we generate an empirical cumulative distribution function utilizing the grid-level estimation specification, and randomly assign each country with a treated pixel-year.[14] Originally treated observations and pixels with very few observations are dropped. We repeat this process to generate 5000 distinct estimates, and plot these in Figure A5 of the Appendix. The red line indicates the estimates of the correct treatment assignment on sales and employment for the grid-level specification. This exercise confirms that the result we find is indeed statistically rare. Furthermore, this test allows us to speak to the issue of serial correlation in difference-in-differences estimates raised by Bertrand et al. (2004). They state that, if uncorrected, serial correlation can lead to over-rejection of the null hypothesis in standard t-tests of difference-in-differences estimates. However, Figure A5 shows that, also in this non-parametric setting, the null hypothesis can be rejected at the 10% significance level.

3.3.4 Sensitivity of results to individual countries

We perform a jackknife-type exercise to test whether the average treatment effects we find are driven by strong favoritism effects emanating from individual countries. We re-estimate Equations 1 and 2, which are the regressions using geocoded and regional data, but successively dropping individual countries which provide identifying variation. Decreases (increases) in our coefficient of interest would indicate that the excluded country experienced a stronger (weaker) effect compared to the average country. Figure A6 of the Appendix shows that changes to the average effects are small, and that they never lead to the average effect becoming statistically indistinguishable from zero. In specification 1 the largest change in the

---

[14]Using the grid-level estimation has the upside of capturing equal sized areas for control and treatment groups over each permutation.

point estimate is not larger than four percentage points relative to the baseline effect, and in specification 2 this change is not larger than three percentage points relative to the baseline effect. Thus, we rule out that our findings are driven by individual countries.

4   Mechanisms

In order to shed light on the mechanisms behind our baseline results, we start by investigating whether the measured increases in sales and employment are accompanied by increases in productivity measures. We then assess whether regional favoritism affects the main sectors of the economy differentially. In the following sub-sections we study the role of government demand, of government regulatory policies, and of firm-level drivers of productivity in explaining our baseline favoritism effect.

4.1   Effects on productivity

From the information in the firm surveys, we construct three measures of firm productivity: Wage per worker, output per worker, and total factor productivity (TFP). We estimate TFP by regressing output in terms of sales on input factor costs and the net book value of land, buildings and machinery.[15] We then run Equation 1 with the residual from this regression and the other productivity measures as outcomes. Table 4 presents the results.

In Table 1 we found the size of the estimated coefficient for employment to be smaller than the coefficient for sales. Consistent with this, in columns (1) and (2) of Table 4 we find that firms in the treated area pay higher wages, and produce more output per capita. Column (3) shows that these firms not only grow in size, but also become more productive, as measured in terms of our revenue based total factor productivity measure. We are cautious of this last result, given that the estimate becomes statistically insignificant for many alternative choices of our distance measure as we show in Figure C1. Our further analysis of the mechanisms of

---

[15]We sum up the costs for various input factors such as labor, raw materials, and intermediate goods, or electricity. As we use total sales as output in this regression, it constitutes as a revenue based TFP measure.

Table 4: Treatment Effect on Productivity Outcomes

	(1) Log Wage	(2) Log Output per Worker	(3) TFP Residual
Treated area	0.0904***	0.0795***	0.0489*
	(0.0249)	(0.0236)	(0.0261)
Firm level controls	Yes	Yes	Yes
Observations	77,946	81,735	68,318
R-squared	0.8293	0.7748	0.2988
F	40.38	61.36	77.50

Notes: The regressions are estimated using Equation 1. Treatment area is defined as $\frac{1}{11}$ of the country-specific distance measure. The mean values in levels are 7,420 USD in column (1), and 107,000 USD in column (2). USD is measured in 2009 nominal values. Firm level controls include firm age, the share owned by foreigners and the share owned by the public sector. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.

favoritism in this section corroborates this caution, as we do not find patterns consistent with a productivity increase.

4.2 Sectoral results

We divide firms into the manufacturing and service sector. As we will discuss in Section 5, we expect redistributive policies implemented by the government to affect these two sectors differently. This is consistent with recent findings by Besley et al. (2021) who show that governments have less leverage to affect firms in the tradable versus the non-tradable sector. In particular, our model predicts that the non-tradable sector is likely to benefit more from redistributive policies. This prediction is similar and in line with the literature on the inflows of funds to developing countries from commodity booms, remittances, international aid, or borrowing. Such inflows increase household incomes, thus boosting consumption. The increased demand for tradable goods can be met by imports, while demand for non-tradable goods can only be satisfied with domestic production. Such episodes lead to relative increases in the prices of non-tradable goods (exchange rate appreciation), the reallocation of factors of

Table 5: Treatment Effects by Sector: Manufacturing vs Services

	(1) Log Sales	(2) Log Employees	(3) Log Wage	(4) Log Output per Worker	(5) TFP Residual
Treated area	0.2573***	0.1198**	0.0984***	0.1498***	0.1216***
	(0.0754)	(0.0546)	(0.0310)	(0.0478)	(0.0438)
Manufacturing	0.1602***	0.4058***	-0.1335***	-0.2403***	-0.2066***
	(0.0570)	(0.0363)	(0.0172)	(0.0703)	(0.0475)
Treated#Manufacturing	-0.1974*	-0.0772	-0.0111	-0.1089	-0.1364**
	(0.1031)	(0.0758)	(0.0334)	(0.0775)	(0.0673)
Firm level controls	Yes	Yes	Yes	Yes	Yes
Observations	82,527	94,093	77,946	81,735	68,318
R-squared	0.6558	0.2500	0.8279	0.7696	0.2697
F	164.0	172.6	38.10	45.96	65.48

Notes: The regressions are estimated based on Equation 1, but include an interaction term between treatment and sectors. Treatment area is defined as $\frac{1}{11}$ of the country-specific distance measure. Dependent variables are specified in logarithms. Firm level controls include firm age, the share owned by foreigners and the share owned by the public sector. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.

production to the non-tradable sector, and deindustrialization. van der Ploeg (2011) provides a review of the resource curse literature and its implications. In a more recent study, De Haas and Poelhekke (2019) investigate the implications of natural resource booms and sectoral reallocation patterns while also using firm data from the Enterprise Surveys.

In Table 5 we include an interaction term between the treatment variable and a dummy variable for firms in the manufacturing sector. The results in column (1) show that manufacturing firms located around leaders' birthplaces benefit less from favoritism. Column (5) implies the same for measured TFP, in fact in favored areas the measured productivity growth is completely driven by service sector firms. Likewise we observe a large negative coefficient for output per worker in column (4), however it lacks the statistical precision to be deemed significantly different from zero. In column (3) we observe that wage growth is similar in both sectors indicated by the close to zero coefficient with a relatively small standard error. This result is consistent with the idea that there is high level of mobility of labor between the two sectors: Despite the fact that service sector firms experience higher growth, wage demands

faced by firms in both sectors are similar, because both sectors compete for similar workers. In column (2) we document that there are no statistically significant sectoral differences in employment growth.

4.3 Government demand

In Table 6 we explore whether our baseline effect operates through the diversion of government demand towards firms in the favored regions. We consider the generation of additional government demand either through the public procurement system or through government owned firms more directly. Column (1) shows that firms located in proximity to leaders' birthplaces are 1.8% more likely than other firms to secure government contracts. The magnitude of this effect is substantial when compared to the mean probability of 17.8% of securing government contracts in our sample. In line with our sectoral results, column (2) presents evidence that this is driven by firms in the services sector. In columns (3) and (4), we then study whether sales and employment grow more in firms where the government has a partial ownership stake compared to privately owned firms. Our data provides weak evidence in support of this hypothesis. However, given that the enterprise surveys exclude firms which are fully government owned, we think about these estimates as lower bound effects. This interpretation will hold true as long as the government demand effect is at least as strongly present in (omitted) firms which are fully rather than partially owned by the government.

4.4 Business environment

Next, we shift our attention to the supply rather than the demand side studied in the previous section, and investigate whether leaders use government regulatory policies to contribute to firm growth in their birth regions. The Enterprise Surveys ask questions regarding the constraints that firms face while doing business. Firms are asked to evaluate certain obstacles to their business on a five-point Likert scale. We center and normalize these variables and report the results in terms of standard deviations in Table 7.

Table 6: Government Demand

	(1) Gov. contract secured?	(2) Gov. contract secured?	(3) Log Sales	(4) Log Employees
Treated Area	0.0179** (0.0077)	0.0337*** (0.0101)	0.1387*** (0.0507)	0.0745** (0.0370)
Manufacturing		-0.0181** (0.0079)		
Treated#Manufacturing		-0.0257** (0.0121)		
Log employees	0.0285*** (0.0025)	0.0302*** (0.0026)		
Partial public ownership			0.8401*** (0.2302)	0.7809*** (0.1232)
Treated#Partial public ownership			0.3879 (0.2995)	0.4763*** (0.1745)
Firm level controls	Yes	Yes	Yes	Yes
Observations	91,370	91,370	82,544	94,120
R-squared	0.1077	0.0975	0.6620	0.2650
F	49.68	44.84	173.5	206.5

Notes: The regressions are estimated using Equation 1, with logarithm of employees as an additional control variable to account for firm size. Treatment area is defined as $\frac{1}{11}$ of the country-specific distance measure. The mean values of the dependent variables in column (1) and (2) are 17.8%, in column (3) 7.6 million USD, and in column (4) 80 employees. Firm level controls include firm age, the share owned by foreigners and the share owned by the public sector. All regressions include fixed effects for leader circles, regions, and country-by-years, while (1), (3), and (4) also include industry fixed effects. Standard errors are clustered at the level of treatment.

In the first column, the dependent variable is the average of all business constraints. The estimated coefficient is positive and significant, indicating a worsening, not improving, business environment. In the following three columns, we study the more specific sources of business constraints. The results suggest that there is no change in the perceived institutional environment around leaders' birthplaces, but that the worsening business environment is driven by deficiencies in infrastructure and inputs.

On infrastructure, the result suggests that while leaders do divert resources to their home region for example through generating higher government demand (Section 4.3), they

Table 7: Perceived Business Constraints

	Treated Area	Observations	R-squared	F
(1) Average	0.0850** (0.0371)	76,394	0.3959	11.41
(2) Infrastructure	0.1190*** (0.0372)	91,590	0.2907	17.50
(3) Institutions	0.0140 (0.0436)	79,775	0.3812	8.456
(4) Inputs	0.0586*** (0.0226)	88,522	0.2826	20.76
(5) Land	0.0514** (0.0200)	90,616	0.2303	26.04
(6) Finance	-0.0339 (0.0222)	92,329	0.2022	49.78
(7) Workforce	0.1251*** (0.0257)	92,744	0.2330	26.82

Notes: This table reports the treatment effect on firms' perceived business constraints. The regressions are estimated using Equation 1, with logarithm of employees as an additional control variable to account for firm size. Treatment area is defined as $\frac{1}{11}$ of the country-specific distance measure. Dependent variables are indices that have been centered at zero and normalized with a variance of one, with larger values indicating higher constraints. Average constraints in row 1 average the variable over business constraints related to infrastructure (2), institutions (3) and inputs (4). Input constraints are in turn an average over the constraints on land (5), finance (6), and workforce (7). All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.

do not promote sufficient infrastructure development to keep up with the increasing needs of the firms in these areas. This result is intuitive because infrastructure investments require planning and proper project implementation. Such activities require longer time horizons and more effort than, for example, simply awarding contracts to services firms in the favored areas. In this way, our results indicate that leaders are more likely to choose the latter option, or similar mechanisms to promote development in their home region. Infrastructure investments themselves can increase the incomes of local firms and workers, but do little to expand the infrastructure stock. Studies have shown that in the presence of limited absorptive capacity – in terms of skills, institutions, and management – countries are unable to translate every dollar of public investment into an additional dollar of capital stock (Presbitero 2016).

On input constraints, the concept itself combines three components, the result for each of which are displayed in the last three columns of Table 7. From these regressions we observe that firms around leaders' birthplaces complain in particular about the lack of land and educated workforce, while the coefficient on the measure for access to finance is not significantly different from zero suggesting that leaders do not directly affect the capital market. The increasing complaints about lack of land make sense because this factor has a fixed supply and does not increase proportionately with output, while the deteriorating perceptions about educated workforce suggest that the demand for labor exceeds the supply of skilled workers. This is consistent with increasing wage levels around leaders' birthplaces, as presented in Table 4. It is also worthwhile to note that, in the context of ethnic favoritism, Dickens (2018) shows that there is no increase in migration to the leader's ethnic region. It would therefore appear that adjustment is impaired by frictions to labor mobility. Specifically, tensions between ethnicities can be one factor hindering labor mobility within countries.

4.5 Drivers of firm productivity

Our baseline results show that firms located around leaders' birthplaces do not only grow in size, but that they also become more productive in terms of output per worker and measured TFP. However, given that both of these measures are based on nominal revenues, these measured productivity increases could be alternatively explained by increasing prices which we do not observe. Therefore, in order to better understand the question of whether, and if so how, favoritism leads to improvements in productivity, we adopt various drivers of firm productivity as comprehensively as possible, and test if firms located in favored areas improve on these measures.

We base our analysis on the review by Syverson (2011), and adopt ten measures from five broad categories of drivers of productivity. These are management practices, quality of inputs, adoption of ICT, research and development activities, and exports. Syverson (2011) also mentions that firm structure, and learning by doing effects can improve firm productivity, but we are unable to measure these components in our data.

Table 8: Drivers of Firm Productivity

	Treated Area	N		Treated Area	N
Management			**Innovation**		
(1) Log Years of Manager's Experience	-0.0154 (0.0173)	92,104	(6) R&D	0.0406*** (0.0151)	74,303
			(7) New Processes	-0.0480* (0.0262)	63,214
Quality of Inputs					
(2) % Workers with High School Degree	0.0101 (0.0133)	68,945	(8) New Products	0.0613* (0.0369)	64,569
			(9) R&D controlling for new products	0..0225 (0.0171)	69,136
(3) Formal Training	-0.0205* (0.0116)	93,450			
			(10) Technology Licensed from Abroad	0.0027 (0.0085)	68,652
ICT Adoption					
(4) Own Website	0.0089 (0.0089)	93,698			
(5) E-Mail Communication	0.0069 (0.0122)	73,031	**Competition** (11) Share Exports in Sales	0.0696 (0.5695)	51,067

Notes: This table reports the treatment effects on firm's internal drivers of productivity. The regressions are estimated using Equation 1, with logarithm of employees as an additional firm level control variable to account for firm size. Treatment area is defined as $\frac{1}{11}$ of the country-specific distance measure. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.

Table 8 shows our estimates. Row (1) does not find evidence that firms in treated areas are managed by more experienced managers measured by the years of experience of working in the industry of the respective firms. Rows (2) and (3) study the role of firms' quality of inputs. There is no indication that firms in treated areas have a more educated workforce in terms of the share of workers with secondary school degrees, nor that these firms conduct formal training of their workforce. Rows (4) and (5) do not find evidence that firms in treated areas are more likely to adopt ICTs, as measured by firms having their own websites, or their use of emails when communicating with clients or suppliers. We then test the role of several variables measuring potential productivity improvements through innovation activity or adoption. In Row (6) we take note that firms in treated areas are significantly more likely to report any R&D expenditures than control firms. In rows (7) and (8), we study whether firms have introduced new products or processes. For new products we observe a positive and significant

coefficient,[16] while for new processes a negative significant one. Our interpretation is that higher demand in the treated regions increases firms' incentives to introduce new products. However, this horizontal expansion does not necessarily imply improvements in efficiency, as process rather than product innovations are more likely to be associated with improved efficiency.[17] We then test in row (9) whether the increase in the likelihood to have reported any R&D expenses is driven by this vertical expansion of the firms' product portfolios. Indeed we find that controlling for the introduction of products that are new to the firm leads to an insignificant treatment effect on R&D.[18] In row (10), we do not find that firms in the treated area are more likely to adopt licensed technologies from abroad, which captures productivity improvements through technological diffusion from foreign countries. Finally, in row (11) of Table 8, we restrict our sample to manufacturing firms, and study whether they experience an increase in the share of sales coming from exports. Syverson (2011) warns that propensity of exporting is not necessarily a causal driver of productivity, but that it has been shown to be one of the most robust correlates of it. The direction of causality is not very important in our context, what is important is that this result, once again, does not show that firms in the treated area are more productive as far as productivity is correlated with export activity.

Given these null effects on this fairly comprehensive set of correlates of productivity, the explanation most consistent with our findings is that, despite the increases in measured TFP, firms in fact do not become more productive. Instead, the treatment effects on our productivity measures rather reflect the change in local prices driven by the demand shock.

4.6   Size distribution of firms

In addition to the average effects of favoritism identified thus far, we are also interested in whether favoritism differently affects the size distribution of firms. Following Hsieh and

---

[16]This variable measures the introduction of products that are new to the firm, but not new to the market.

[17]For example, in the multi-product firm framework posited by Mayer et al. (2014) an exogenous increase in demand can lead the firm to expand its product scope without any improvement in productivity.

[18]This is not driven by the sample composition changing, as the treatment effect remains significant when restricting the sample to the subset with non-missing information on the introduction of new products but without including it as a control.

Figure 3: Size Distribution of Treated and Untreated Firms
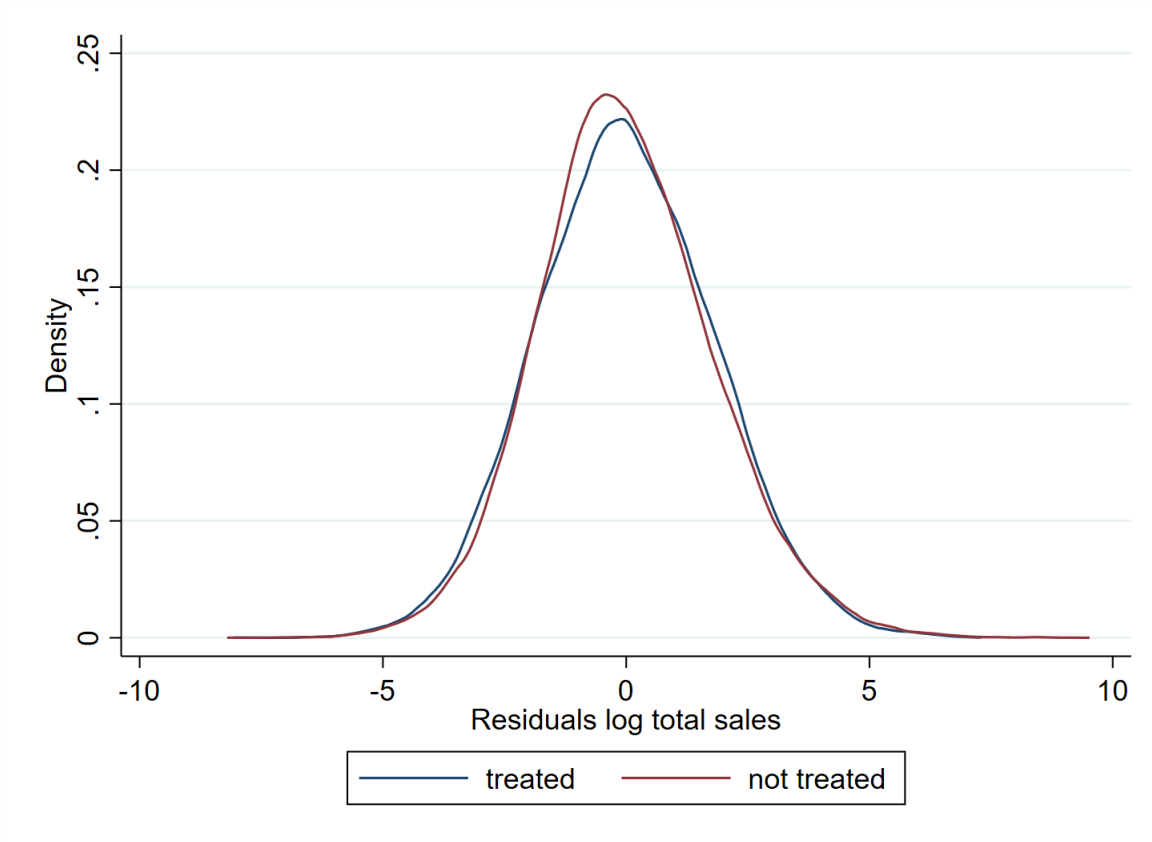
Notes: The figure plots the approximated density of residuals from Equation (1) with respect to logarithm of sales for the treatment and control group using Epanechnikov kernel estimator.

Klenow (2009), in Figure 3 we present the distribution of firms in terms of total sales by plotting the approximated density of residuals from Equation 1 using Epanechnikov kernels. We separately plot the distribution of control and treated firms. If the favoritism effects were to change the distribution of firms, we would expect to observe substantial divergence in the density distribution of the two groups. This divergence is minimal, and therefore does not indicate a differential effect of favoritism across the size distribution of firms.[19] This result supports our assumptions in the following section, in which we model homogeneous firms.

---

[19]To test this hypothesis more formally, we use bootstrapping to construct a confidence interval of the ratio of the above mentioned residuals' standard deviations. The 95% confident interval of the ratio ranges between 0.980 and 1.001, thus suggesting that there are no statistically significant differences in the distributions between the control and treatment groups.

Given that we have identified differential treatment effects for firms in the services and manufacturing sector in Table 5, we plot the distributions additionally for these sectors in Figure A7.[20]

5 Aggregate implications

Our empirical results are based on difference-in-differences estimations thus representing changes relative to the control group of firms in non-favored regions. As such we cannot draw conclusions regarding the aggregate effects of regional favoritism from these estimates directly. In this section, we propose a simple model that fits the patterns we detected in the empirical section. In developing the model, we make a number of assumptions which are based on our empirical results, and as we describe the model, we motivate these assumptions by linking them to the related empirical findings. Then we calibrate the model to obtain a potential quantitative outcome for the aggregate economy resulting from observed patterns of regional favoritism. Despite the fact that regional favoritism involves substantial reallocation of resources, we find that the aggregate impact is small. Our model includes minimal ingredients, but as we argue along the discussion of the quantitative results of the model, adding additional details will further mitigate, rather than exacerbate, the negative effect of favoritism.

5.1 Framework

We consider a two-region and two-sector economy with perfectly competitive firms. Regions denoted $i \in \{h, a\}$ are the home region which receives subsidies τ_h, and the rest of the country a which pays taxes τ_a to finance these subsidies. Positive values of τ_i denote taxes and negative values subsidies. We use the term taxes to refer to τ_i but this should not be taken literally because these taxes capture various wedges discussed by Restuccia and Rogerson (2008). Firms in both regions produce manufacturing goods (m) and services (s) $j \in \{m, s\}$.

---

[20]Likewise the 95% confidence intervals of the bootstrapped ratios range from 0.960 to 1.006 for service sector firms and from 0.979 to 1.015 for manufacturing sector firms.

Manufacturing goods are traded across regions, whereas services are produced and consumed locally only. We assume that both regions have the same levels of productivity. Our data provide evidence in support of this assumption. We run regressions on outcomes that can proxy the average level of productivity (TFP, output per worker and wage), and include an indicator variable for areas which produced national leaders during the study period.[21] Across all specifications the estimated coefficients for the indicator variable turn out to be less than 10% and statistically only marginally significant at the 10% level, which implies that leader areas are if at all only slightly more productive compared to the national average. At the end of this section we provide intuition on the outcomes if there were large differences between treated and non-treated areas in terms of productivity.

Production. We consider a representative firm that operates a simple production function

$$Y_{ij} = L_{ij}^{\alpha} \tag{4}$$

such that output Y_{ij} is produced by using labor L_{ij}. Both regions are endowed with a fixed amount of homogenous labor L_i which is allocated across sectors competitively. Labor is perfectly mobile across sectors but immobile across regions. Our empirical results are consistent with a high level of labor mobility between sectors (Table 5), and low mobility between regions (Table 1). The mass of firms is proportional to the labor endowment in each region. We do not introduce capital into the production function because our empirical results in Table 7 do not show any differential frictions in the capital market stemming from regional favoritism. Thus, to keep the model more tractable we do not add capital. We will assume that the production function exhibits decreasing returns to scale ($\alpha < 1$), as in models with span of control. Another motivation for the decreasing returns to scale assumption is that there are some fixed factors used in the production which do not adjust. In Table 7 we showed that firms in favored regions perceive a worsening of the infrastructure and availability of land.

---

[21]Our estimations include country-year fixed effects, and exclude observations for years and areas during which the respective leader was in office.

The firm's optimization problem can therefore be written as

$$\pi_{ij} = (1 - \tau_i)p_{ij}Y_{ij} - w_i L_{ij}, \tag{5}$$

where π_{ij} is the profit of the firm in region i and sector j, p_{ij} is the corresponding price and w_i the wage in region i. Perfect mobility between sectors implies that firms in both sectors face the same wage, for which we observe empirical evidence in Table 5 column (3). Since manufacturing goods are perfectly tradable between regions, their prices are the same in both regions and we normalize them to one ($p_{hm} = p_{am} = 1$)

Consumption. Both regions are populated by representative agents who derive utility by combining services (C_{is}) and manufacturing goods (C_{im}) given by $U_i = C_{im}^{\gamma} C_{is}^{1-\gamma}$. Agents earn wages by supplying labor inelastically, and receive the profits of the firms located in their region. The budget constraint is given by:

$$p_{is}C_{is} + C_{im} \leq w_i L_i + \pi_i, \tag{6}$$

where π_i denotes total profits of firms in both sectors.

Market clearing. The equilibrium requires clearing in labor and goods markets

$$L_{hs} + L_{hm} = L_h, \quad L_{as} + L_{am} = L_a \tag{7}$$

$$C_{hs} = Y_{hs}, \quad C_{as} = Y_{as} \tag{8}$$

$$C_{hm} + C_{am} = Y_{hm} + Y_{am} \tag{9}$$

Finally, the government balances its books, which requires that the amount of tax collected in the non-home region equals the subsidies provided to the home region

$$\tau_h(p_{hs}Y_{hs} + Y_{hm}) + \tau_a(p_{as}Y_{as} + Y_{am}) = 0. \tag{10}$$

5.2   Mechansims

The model yields several predictions that help us understand the empirical results observed in Section 3. The key outcome of the model concerns the relationship between the tax rate and the relative allocation of labor between sectors. The model implies that the share of labor allocated to the services sector decreases with the tax rate. Given that the home region receives a subsidy, and the non-home region pays taxes, this implies that a relatively larger share of labor in the home region will be allocated to the services sector. The intuition behind this result is rather simple. Since only the tradable good can be transferred across regions, the wedges introduced by the government require transfers from the non-home region. The relative supply of the tradable good in the home region increases because it receives transfers. As a result, it becomes optimal for firms in the home region to allocate relatively more resources to production in the services sector to meet consumer demand. Consequently, both regions will have relatively more resources allocated to one of the sectors compared to the economy without wedges. A concentration of resources in any of the sectors implies a lower level of marginal physical output in the presence of decreasing returns to scale. As a result, wedges generate aggregate losses in the economy.

Another prediction of the model concerns the effect of taxes on wages. Consistent with the empirical results documented in Table 4, wages decrease with taxes.

In Section 3.1 we mentioned the possibility that regional favoritism can have long term effects in line with the 'big push' hypothesis. However, our model does not allow for such a possibility. The main reason for this is that our empirical results do not support this idea. It should also be mentioned that quantitative models of 'big push' are in their infancy (see Buera et al. 2021). Another important point is that we fit the data by varying wedges (τ) rather than the productivity terms of the production function which are fixed. In the latter case it would be possible to obtain positive aggregate effects from regional favoritism. Again, modeling choices are substantiated by the empirical results. Although, in Table 4 we found some evidence for increases in revenue based TFP, the results in Table 5 show that this is completely driven by the non-tradable sector. Furthermore, in Section 4.2 we presented a

series of results showing that the increased sales in favored regions are primarily driven by demand and not by productivity enhancing activities. To be consistent with this evidence and fit the data, we model regional favoritism through changes in wedges rather than productivity terms.

5.3 Calibration

The qualitative discussion of the model's predictions concluded that taxes generate net losses. In this section, we use standard parameter values from the literature, and target some key moments from the empirical section to quantitatively asses the magnitude of taxation required to generate observed output differences, and to quantify associated output and welfare losses. We set the parameter governing the share of manufacturing goods consumption in developing economies to $\gamma = 0.30$ to generate an employment share of 30% in the manufacturing sector. As mentioned above we assume that firms operate a decreasing returns to scale technologies and set $\alpha = 0.85$ as in Restuccia and Rogerson (2008). We set the size of labor force in the home region (equivalently output in the undistorted economy) to 32% of total labor. This figure corresponds to the share of output produced by firms in the leader's region across our sample. Our key objective is to choose parameters τ_h and τ_a such that we can match the 14% total output increase in the home region, and make sure that the government's budget constraint (10) is satisfied. This value is taken from column (1) of Table 1.

Since both regions operate same technologies, in the absence of wedges both regions produce and consume exactly the same quantities per capita. In Table 9 we present the relative changes in some key estimates relative to values for the economy without wedges. As already discussed the share of labor allocated to the services sector in the home region increases. Quantitatively this change is 10% (compared to the 8% in our empirical results), while in the non-home region the corresponding figure goes down by -4.90%. Because in both regions labor is in fixed supply the expansion of the services sector implies a decline in labor employed in the manufacturing sector, which is not consistent with our estimates in Table 5, where we did not find a decline in labor employed in the manufacturing sector. Introducing

Table 9: The Effect of Distortions on Factors and Output

	(1) L_{hs}	(2) L_{as}	(3) p_{hs}	(4) w_h	(5) Y	(6) W
Changes in %	10.00	-4.90	5.56	14.47	-0.07	-0.04

Notes: The table displays the changes in percentages relative to the distortion-free economy. In column (5) Y refers to total output in the economy and in column (6) W refers to aggregate welfare in terms of consumption equivalents.

frictional labor mobility across regions, elastic labor supply or rural-urban migration can allow us to address this issue. A model with these features will mitigate the aggregate negative consequences of regional favoritism.

The following column displays the relative change in prices of non-tradable goods in the home region. There is a 5.56% increase in prices in the home region. In the data we do not observe prices and cannot compare them but there was strong suggestive evidence that the price of non-tradable goods increases in treated circles. For example, in Table 5, we observed an increase in Y/L ratio only in the services sector. In our data, output is measured as price times quantity, and we do not have information on physical output. However, in Table 7 and 8 we did not find any supporting evidence for improvements in efficiency, so it is very likely that the Y/L ratio is driven by the increasing price of non-tradable goods. Column (4) displays the change in wages in the home region, which increase by about 14.5%. This figure exceeds our empirical estimate in column (1) of Table 4 but it is not far away. Additional features related to labor mentioned in the previous paragraph can improve the performance in this dimension. Overall, we find that this simple model performs relatively well in matching some key non-targeted moments. The fifth column displays the net loss in total real output, which amounts to 0.07% of annual output. In the last column we also report aggregate welfare changes, as measured in consumption equivalents.

5.4 Discussion

Overall, despite substantial changes in output at the firm level, our model implies relatively small aggregate losses. Of course, our model is simple but adding more features will not

increase these losses because we have made a number of assumptions that work in the direction of generating larger rather than smaller output losses due to regional favoritism. For example, we assume a decreasing returns to scale technology, immobile labor across regions and inelastic labor supply. The relaxation of these assumptions will further shrink the negative effect of distortions on output and welfare.

Another simplification is that we modeled an economy with a representative firm. Adding firm heterogeneity similar to Restuccia and Rogerson (2008) will not increase aggregate losses. As we documented in Section 4.6, the firm distribution within regions is unaffected by regional favoritism. If we model firm heterogeneity with entry that will make aggregate losses even smaller. The reason is that in our model all demand generated by regional favoritism is met by the expansion on the intensive margin which generates losses due to decreasing returns. If there was firm entry, then extensive margin responses will also play a role and will mitigate losses. The overall conclusion is that our simple model generates very small aggregate losses, however given our empirical results, adding any of the additional features discussed will work in the direction of further dampening the aggregate losses.

It is also worthwhile to discuss our findings in the context of the misallocation literature. In this literature most papers that generate large quantitative losses rely on a negative correlation between wedges and firm productivity. In our sample, the Y/L ratio of the top 10% of firms is larger by a factor of 22 compared with the bottom 10% of firms. Obviously, if we impose a negative wedge on the top firms, then those firms will decrease their output and given their size in the initial economy the aggregate effect will be sizable. At the same time, the positive effect from unproductive firms will be too small to compensate the loss. However, our empirical results do not allow us to exploit this kind of mechanism. In Section 4.6 we documented that the distributions of firms within regions do not significantly change due to regional favoritism, and we observe only a small difference in the average productivity. According to our analysis, productivities differ at most by a factor of 1.1, which is very small compared to the figure of 22. Moreover, the redistribution is towards more productive regions, i.e., the correlation is positive not negative. Hence, our paper is more comparable to papers

on spatial misallocation rather than firm misallocation, and in this literature magnitudes in terms of output are small (Fajgelbaum et al. 2018).

6 Conclusions

Regional favoritism - that is, the geographic redistribution of resources within countries in favor of a political leader's home region - is a widespread phenomenon that is particularly prevalent in low and middle income countries. While evidence for regional favoritism has been extensively documented, its implications are not clearly understood. A commonly held normative view is that favoritism is necessarily a negative phenomenon that is fueled by corruption and other forms of rent seeking. However, preferential treatment of a region can also lead to higher welfare in the aggregate if, for example, leaders are well informed and are able to subsidize productive activities in the economy at the expense of more wasteful ones.

In this paper we sought to solve this normative tradeoff by first identifying the micro effects of favoritism within a global sample of firms. We then quantified the macro effects of favoritism by feeding the estimated empirical parameters into a revised model of resource misallocation. Our empirical results suggest that firms located closer to leaders' birthplaces not only grow in size, but also become relatively more productive when measured by sales per worker, wages and total factor productivity. While such improvements could potentially lead to higher growth for the entire country, this conclusion is not supported by our subsequent analysis. In particular, our evidence shows that this evolution of firms in favored regions is driven by a rapid expansion of the non-tradable sector, rather than substantial growth among manufacturing firms. Direct transfers to firms through public procurement contracts are one channel behind this effect. Importantly, these positive and economically substantial effects on firms are not sustainable, and vanish after the leaders leave office.

We quantify that the net aggregate effects of the favoritism-based redistribution of resources between regions and sectors cost countries on average 0.07% of their output each year. We obtain a relatively small effect because on average leaders' home regions have similar

levels of efficiency as the rest of the country. This means that resources are not redistributed towards less productive regions, which, if it was the case, would lead to larger aggregate losses.

Our results require several caveats. First, the regional favoritism we study may be an expression of various intentional and unintentional policies, including policies working on other forms of societal divides along ethnic, religious, or cultural lines. Future research could seek to disentangle the effects of these various policies. Second, owing to data constraints, we focus on leaders and ignore other systematically important national figures. It would be potentially interesting to study regional favoritism in relation to other government figures. Third, future research could devote additional attention to the endogeneity of regions. Political leaders gain power often as a result of battles between complicated power structures, which may or may not reflect the underlying economic trends within specific regions. Although the evidence from our difference-in-differences framework assuages such concerns, our study remains a first pass. Fourth, we neglect the potential impact of favoritism on the entry and exit of firms, as well as its implications for firms in the informal and agricultural sectors. Since our survey data are not well equipped to explore these margins, future research may try to consolidate larger datasets, for example from censuses or administrative sources, to better understand firm dynamics in general, and movements of firms and workers from informal and agricultural sectors more specifically.

References

Abadie, A., S. Athey, G. W. Imbens, and J. Wooldridge (2017). When should you adjust standard errors for clustering?

Amodio, F., G. Chiovelli, and S. Hohmann (2019). The employment effects of ethnic politics. *CEPR Discussion Paper No. DP-14170*.

Anaxagorou, C., G. Efthyvoulou, and V. Sarantides (2020). Re-election incentives, ethnic favoritism and the subnational allocation of foreign aid. *European Economic Review 127*, 1–32.

André, P., P. Maarek, F. Tapo, et al. (2018). Ethnic favoritism: Winner takes all or power sharing? evidence from school constructions in Benin. *THEMA Working Paper No 2018-03*.

Asatryan, Z., T. Baskaran, C. Birkholz, and P. Hufschmidt (2021). Regional redistribution of mineral resource wealth in Africa. ZEW Discussion Paper 21-032.

Asatryan, Z., T. Baskaran, P. Hufschmidt, and A. Stöcker (2021). Regional favoritism and human capital accumulation in Africa. ZEW Discussion Paper 21-030.

Bandyopadhyay, S. and E. Green (2019). Roads and regional favoritism in Sub-Saharan Africa. Technical report, LSE Working Paper No.19-195.

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How Much Should We Trust Differences-In-Differences Estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Besley, T., N. Fontana, and N. Limodio (2021). Antitrust policies and profitability in non-tradable sectors. *American Economic Review: Insights 3*(2), 251–65.

Besley, T. and H. Mueller (2018). Predation, protection, and productivity: A firm-level perspective. *American Economic Journal: Macroeconomics 10*(2), 184–221.

Brandt, L., T. Tombe, and X. Zhu (2013). Factor market distortions across time, space and sectors in China. *Review of Economic Dynamics 16*(1), 39 – 58.

Buera, F. J., H. Hopenhayn, Y. Shin, and N. Trachter (2021, March). Big push in distorted economies. Working Paper 28561, National Bureau of Economic Research.

Burgess, R., R. Jedwab, E. Miguel, A. Morjaria, and G. Padró i Miquel (2015). The value of democracy: Evidence from road building in Kenya. *American Economic Review 105*(6), 1817–51.

Chen, Y., J. Huang, H. Liu, and W. Wang (2019). Regional favoritism and tax avoidance: Evidence from China. *Accounting & Finance 58*(5), 1413–1443.

Chetty, R., A. Looney, and K. Kroft (2009). Salience and taxation: Theory and evidence. *American Economic Review 99*(4), 1145–77.

Chu, J., R. Fisman, S. Tan, and Y. Wang (2021). Hometown favoritism and the quality of government monitoring: Evidence from rotation of chinese auditors. *American Economic Journal: Economic Policy, forthcoming*.

Coppedge, M., J. Gerring, C. H. Knutsen, S. I. Lindberg, J. Teorell, D. Altman, M. Bernhard, A. Cornell, M. S. Fish, L. Gastaldi, H. Gjerløw, A. Glynn, A. Hicken, A. Lührmann, S. F. Maerz, K. L. Marquardt, K. McMann, V. Mechkova, P. Paxton, D. Pemstein, J. von Römer, B. Seim, R. Sigman, S.-E. Skaaning, J. Staton, A. Sundtröm, E. Tzelgov, L. Uberti, Y.-t. Wang, T. Wig, and D. Ziblatt (2021). V-dem codebook v11.1.

Cravino, J. and A. A. Levchenko (2017). Multinational firms and international business cycle transmission. *The Quarterly Journal of Economics 132*(2), 921–962.

de Chaisemartin, C. and X. D'Haultfœuille (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review 110*(9), 2964–96.

De Chaisemartin, C. and X. d'Haultfoeuille (2022). Difference-in-differences estimators of intertemporal treatment effects. Technical report, National Bureau of Economic Research.

de Chaisemartin, C. and X. D'Haultfœuille (2022). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *National Bureau of Economic Research, Working Paper 29691*.

De Haas, R. and S. Poelhekke (2019). Mining matters: Natural resource extraction and firm-level constraints. *Journal of International Economics 117*, 109–124.

De Luca, G., R. Hodler, P. A. Raschky, and M. Valsecchi (2018). Ethnic favoritism: An axiom of politics? *Journal of Development Economics 132*, 115–129.

Desmet, K. and E. Rossi-Hansberg (2013). Urban accounting and welfare. *American Economic Review 103*(6), 2296–2327.

Dickens, A. (2018). Ethnolinguistic favoritism in African politics. *American Economic Journal: Applied Economics 10*(3), 370–402.

Do, Q.-A., K.-T. Nguyen, and A. N. Tran (2017). One mandarin benefits the whole clan: Hometown favoritism in an authoritarian regime. *American Economic Journal: Applied Economics 9*(4), 1–29.

Dreher, A., A. Fuchs, R. Hodler, B. C. Parks, P. A. Raschky, and M. J. Tierney (2019). African leaders and the geography of China's foreign assistance. *Journal of Development Economics 140*, 44–71.

Fajgelbaum, P. D., E. Morales, J. C. Suarez Serrato, and O. Zidar (2018). State Taxes and Spatial Misallocation. *The Review of Economic Studies 86*(1), 333–376.

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

Golden, M. and B. Min (2013). Distributive politics around the world. *Annual Review of Political Science 16*(1), 73–99.

Harari, M. (2020). Cities in bad shape: Urban geometry in india. *American Economic Review 110*(8), 2377–2421.

Heckman, J. J., H. Ichimura, and P. E. Todd (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *The review of economic studies 64*(4), 605–654.

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

Hopenhayn, H. A. (2014). Firms, misallocation, and aggregate productivity: A review. *Annual Review of Economics 6*(1), 735–770.

Hsieh, C.-T. and P. J. Klenow (2009). Misallocation and manufacturing TFP in China and India. *The Quarterly Journal of Economics 124*(4), 1403–1448.

Hsieh, C.-T. and P. J. Klenow (2010). Development accounting. *American Economic Journal: Macroeconomics 2*(1), 207–223.

Imbens, G. W. (2015). Matching methods in practice: Three examples. *Journal of Human Resources 50*(2), 373–419.

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

Lee, B. K., J. Lessler, and E. A. Stuart (2010). Improving propensity score weighting using machine learning. *Statistics in medicine 29*(3), 337–346.

Martinez-Bravo, M. and L. Wantchekon (2021). Political economy and structural transformation: Democracy, regulation and public investment. STEG Pathfinding Paper.

Mayer, T., M. J. Melitz, and G. I. P. Ottaviano (2014). Market size, competition, and the product mix of exporters. *American Economic Review 104*(2), 495–536.

Miquel, i. G. P. et al. (2007). The control of politicians in divided societies: The politics of fear. *Review of Economic Studies 74*(4), 1259–1274.

Murphy, K. M., A. Shleifer, and R. W. Vishny (1989). Industrialization and the big push. *Journal of Political Economy 97*(5), 1003–1026.

Presbitero, A. F. (2016). Too much and too fast? public investment scaling-up and absorptive capacity. *Journal of Development Economics 120*, 17–31.

Ranasinghe, A. (2017). Property rights, extortion and the misallocation of talent. *European Economic Review 98*, 86–110.

Restuccia, D. and R. Rogerson (2008). Policy distortions and aggregate productivity with heterogeneous establishments. *Review of Economic Dynamics 11*(4), 707–720.

Restuccia, D. and R. Rogerson (2017). The causes and costs of misallocation. *Journal of Economic Perspectives 31*(3), 151–74.

Roth, J., P. H. Sant'Anna, A. Bilinski, and J. Poe (2022). What's trending in difference-in-differences? A synthesis of the recent econometrics literature. *arXiv preprint*.

Stuart, E. A., H. A. Huskamp, K. Duckworth, J. Simmons, Z. Song, M. E. Chernew, and C. L. Barry (2014). Using propensity scores in difference-in-differences models to estimate the effects of a policy change. *Health Services and Outcomes Research Methodology 14*(4), 166–182.

Syverson, C. (2011). What determines productivity? *Journal of Economic literature 49*(2), 326–65.

van der Ploeg, F. (2011). Natural resources: Curse or blessing? *Journal of Economic Literature 49*(2), 366–420.

Zhao, P., X. Su, T. Ge, and J. Fan (2016). Propensity score and proximity matching using random forest. *Contemporary clinical trials 47*, 85–92.

Appendix A: Additional tables and figures

Table A1: Sample description

Country	Year	# firms	Dist. measure
Afghanistan	2008	535	617
	2014	410	
Albania†	2007	304	125
	2013	360	
	2019	377	
Angola	2006	425	730
	2010	360	
Argentina*	2006	1063	1286
	2010	1054	
	2017	991	
Armenia	2009	374	143
	2013	360	
Azerbaijan	2009	380	201
	2013	390	
Bahamas	2010	150	242
Bangladesh	2013	1442	266
Barbados	2010	150	12
Belarus	2008	273	307
	2013	360	
	2018	600	
Belize	2010	150	104
Benin	2016	150	273
Bhutan	2015	253	137
Bolivia**	2006	613	728
	2010	362	
	2017	364	
Botswana	2006	342	517
	2010	268	
Brazil	2009	1802	1948
Bulgaria	2007	1015	213
	2009	288	
	2013	293	
	2019	772	
Burkina Faso	2009	394	391
Burundi	2006	270	115
	2014	157	
Cambodia	2016	373	267
Cameroon	2009	363	552
	2016	361	
Chad	2018	153	773
Continued on next page			

† Identifying variation in geocoded sample only.
* Identifying variation in both samples.
** Identifying variation in region sample only.

Table A1 –continued from previous page

Country	Year	# firms	Dist. measure
Chile	2006	1017	1330
	2010	1033	
China	2012	2700	1327
Colombia*	2006	1000	810
	2010	942	
	2017	993	
Costa Rica	2010	538	153
Croatia†	2007	633	225
	2013	360	
	2019	404	
Czech Republic*	2009	250	205
	2013	254	
	2019	502	
Côte d'Ivoire	2009	526	347
	2016	361	
DRC	2006	340	1087
	2010	359	
	2013	529	
Djibouti	2013	266	92
Dominican Republic**	2010	360	133
	2016	359	
Ecuador	2006	658	383
	2010	366	
	2017	361	
Egypt	2013	2897	539
	2016	1814	
El Salvador*	2006	693	101
	2010	360	
	2016	719	
Estonia*	2009	273	136
	2013	273	
	2019	360	
Eswatini	2006	307	74
	2016	150	
Ethiopia* §	2011	644	752
	2015	848	
Gambia	2006	174	109
	2018	151	
Georgia*	2008	373	197
	2013	360	
	2019	581	
Ghana	2007	494	300
	2013	720	
Guatemala	2006	522	226
	2010	590	
	2017	345	
Guinea	2006	223	349
	2016	150	
Guinea Bissau	2006	159	123
Guyana	2010	165	336
Continued on next page			

§ Dropped in Figure 2.

Country	Year	# firms	Dist. measure
Honduras** §	2006	436	272
	2010	360	
	2016	332	
Hungary**	2009	291	220
	2013	310	
India	2014	9281	1350
Indonesia	2009	1444	1713
	2015	1320	
Iraq	2011	756	479
Israel	2013	483	480
Jamaica	2010	376	53
Jordan	2013	573	226
	2019	601	
Kazakhstan	2009	544	225
	2013	600	
	2019	1446	
Kenya*	2007	657	500
	2013	781	
	2018	1001	
Kosovo† §	2009	269	499
	2013	202	
	2019	271	
Kyrgyz	2009	235	373
Republic*	2013	270	
	2019	360	
Lao PDR*	2009	360	457
	2012	270	
	2016	368	
	2018	332	
Latvia†	2009	271	182
	2013	336	
	2019	359	
Lebanon†	2013	561	70
	2019	532	
Lesotho	2016	150	120
Liberia	2017	151	219
Lithuania**	2009	276	173
	2013	270	
	2019	358	
Madagascar	2009	445	487
	2013	532	
Malawi	2014	523	330
Malaysia	2015	1000	889
Mali	2007	490	890
	2010	360	
	2016	185	
Mauritania	2006	237	663
	2014	150	
Mexico**	2006	1480	1152
	2010	1480	
Moldova** §	2009	363	159
	2013	360	
	2019	360	
		Continued on next page	

Country	Year	# firms	Dist. measure
Mongolia*	2009	362	975
	2013	360	
	2019	360	
Montenegro*	2009	116	83
	2013	150	
	2019	150	
Morocco	2013	407	720
	2019	1096	
Mozambique	2007	479	710
	2018	601	
Myanmar*	2014	632	676
	2016	607	
Namibia	2006	329	663
	2014	580	
Nepal	2009	368	337
	2013	482	
Nicaragua**	2006	478	227
	2010	336	
	2016	333	
Niger	2017	151	784
Nigeria**	2007	1891	608
	2014	2676	
North Macedonia*	2009	366	101
	2013	360	
	2019	360	
Pakistan	2013	1247	781
Panama	2006	604	248
	2010	365	
Papua New Guinea	2015	65	526
Paraguay** §	2006	613	469
	2010	361	
	2017	364	
Peru**	2006	632	804
	2010	1000	
	2017	1003	
Philippines**	2009	1326	642
	2015	1335	
Poland*	2009	455	339
	2013	542	
	2019	1369	
Romania	2009	541	323
	2013	540	
Russia	2009	1004	2918
	2012	4220	
	2019	1323	
Rwanda	2006	212	105
	2019	360	
Senegal	2007	506	281
	2014	601	
Serbia*	2009	388	203
	2013	360	
	2019	361	
Sierra Leone	2017	152	168
		Continued on next page	

Country	Year	# firms	Dist. measure
Slovak Republic*	2009	275	167
	2013	268	
	2019	429	
Slovenia†	2009	276	101
	2013	270	
	2019	409	
Solomon Islands	2015	151	281
South Africa	2007	937	756
South Sudan	2014	738	595
Sri Lanka	2011	610	139
Sudan	2014	662	849
Suriname	2010	152	236
	2018	233	
Sweden	2014	600	564
Tajikistan	2008	360	310
	2013	359	
	2019	352	
Tanzania	2006	419	623
	2013	813	
Thailand	2016	1000	579
Timor-Leste	2015	126	106
Togo	2016	150	206
Trinidad and Tobago	2010	370	49
Tunisia	2013	592	299
Turkey	2008	1152	608
	2013	1344	
	2019	1663	
Uganda	2006	563	323
	2013	762	
Ukraine**	2008	851	558
	2013	1002	
	2019	1337	
Uruguay	2006	621	261
	2010	607	
	2017	347	
Uzbekistan*	2008	366	624
	2013	390	
	2019	1239	
Venezuela	2010	320	692
Vietnam**	2009	1053	606
	2015	996	
Yemen	2010	477	476
	2013	353	
Zambia*	2007	484	616
	2013	720	
	2019	601	
Zimbabwe	2016	600	415

Table A2: Summary Statistics

Geocoded sample	N	Mean	Std. Dev.	p5	p95
Treated area	101350	0.16	0.37	0	1
Year before treatment start	101350	0.019	0.14	0	0
Year after treatment start	101350	0.011	0.10	0	0
Total sales in 2009 USD	87218	7597616	63214844	12045	24197024
Num. full-time employees	99707	79.6	223	5	320
Output per employee in 2009 USD	86300	106982	1622484	1154	258941
Wage in 2009 USD	82360	7420	53922	195	23362
TFP residual	72333	0.0095	1.38	-1.8	2.4
Firm age	100047	18.7	15.5	3	49
Firm share owned private foreign	100025	7.00	23.6	0	90
Firm share owned public	100070	0.68	6.61	0	0
Government contract secured?	98287	0.18	0.38	0	1
Avgerage of constraints	81644	31.6	20.5	1.7	68.3
Infrastructure constraints	98627	33.8	28.2	0	87.5
Institutional constraints	85401	30.3	22.6	0	70
Input constraints	95075	30.2	23.0	0	75
Obstacle land	97548	24.5	31.4	0	100
Obstacle finance	99345	34.1	32.0	0	100
Obstacle inadequately educated workforce	99788	31.9	31.2	0	100
Years of experience top manager	98826	18.0	11.2	3	40
Share employees completed high school	73101	0.65	0.35	0.02	1
Formal Training for employees	100383	0.38	0.48	0	1
Firm has own website	100995	0.53	0.50	0	1
Firm communicates via email	78932	0.75	0.43	0	1
Firm spent on R&D excl. market research	80057	0.22	0.41	0	1
New product / service last 3 years?	95133	0.36	0.48	0	1
New / improved process last 3 years?	93444	0.36	0.48	0	1
Firm licensed technology from foreign firm	74001	0.15	0.36	0	1
Share of sales: direct exports	99605	7.64	21.9	0	70
V-Dem electoral democracy index	101350	0.49	0.22	0.09	0.92
Scaled WB Control of Corruption percentile	100447	0.36	0.21	0	1

Region sample	N	Mean	Std. Dev.	p5	p95
Treated region	148593	0.16	0.37	0	1
Total sales in 2009 USD	129050	8121428	172838953	11797	23715758
Num. full-time employees	146365	77.6	214.5	5	306
Output per employee in 2009 USD	127761	129963	4156877	1187	246908
Wage in 2009 USD	123875	7475	63795	207	22143
TFP residual	109796	0.0084	1.31	-1.6	2.3

**Table A3:** Overview of Results using Alternative Clustering Approaches

	(1) LA-Y	(2) C-Y & LA-Y	(3) R-Y & LA-Y	(4) S & LA-Y	(5) C-S-Y & LA-Y
Log(Sales)	.0506	.0523	.0510	.0130	.0548
Log(Employees)	.0366	.0371	.0370	.0162	.0437
# of Cluster 1	556	198	890	46	877
# of Cluster 2		556	556	556	556

Notes: The table showcases changes to the main estimates' standard errors from Equation 1 using other clustering approaches. The nomenclature is as follows: 'C' stands for 'Country', 'S' for 'Sector', 'Y' for 'Year', 'R' for 'Region' and 'LA' for 'Leader Area'. Column (1) thus lists standard errors for clustering of leader area by year - our main specification for comparability.

Figure A1: Example how the Country-Specific Distance Measure Varies with Size and Shape

(a) Circle, area ≈ 36   **(b)** Circle, area ≈ 72   **(c)** Rectangle, area = 36

Distance measure = 278   Distance measure = 393   Distance measure = 310
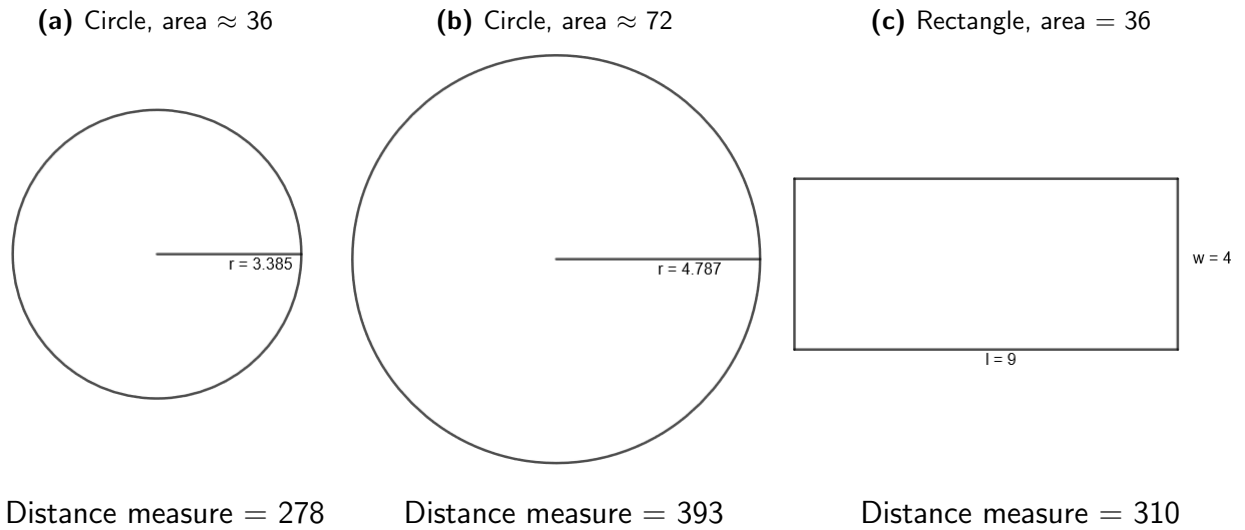
Notes: The figure showcases conceptually how our country-specific distance measure varies across countries of different sizes and geographic outlines. We created stylized geographic forms to which we apply the same algorithm as described in Section 2.2.1 to calculate the measure. Moving from figure (a) to figure (b) we keep the same circular shape, but double the area, and consequently the distance measure increases substantially. On the other hand going from figure (a) to figure (c) we keep the area constant, but change the shape to a rectangle. The larger distance measure of figure (c) reflects the decrease in compactness.

Figure A2: Treatment Effects by Fixed Distance to Leader's Birthplace

Logarithm total sales
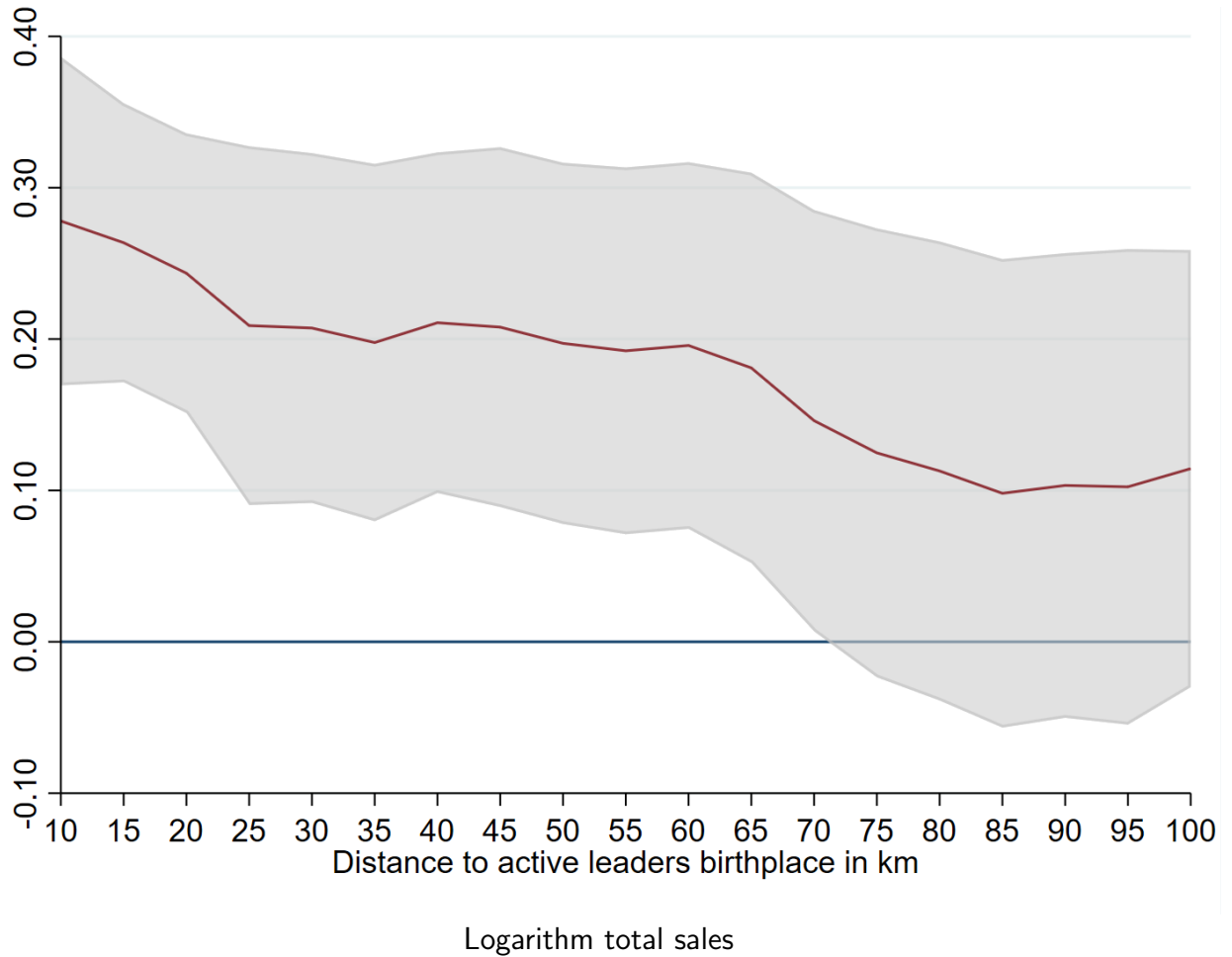
Notes: In the figure, the red line plots the coefficient β^{km} estimated for each radius around the leaders' birthplaces' stated on the x-axis separately. Firms located in a circle of 10 km have on average nearly 30% higher sales than similar firms located further away. These effects decrease by distance, and become indistinguishable from zero beyond 70 km from leaders' birthplaces. The regression is estimated using equation 1. We drop eleven countries such that the estimates are identified by a stable set of countries over all distances. The shaded area represents 95% confidence intervals. The dependent variable is total sales and is specified in logarithm. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.

Table A4: Spatial versus Regional Treatment Effects

	(1) Log sales	(2) Log employees
Treated area in leader region	0.1658*** (0.0533)	0.0967** (0.0389)
Treated area <u>not</u> in leader region	0.0390 (0.0817)	0.0126 (0.0640)
Firm level controls	Yes	Yes
Observations	82,527	94,093
R-squared	0.6621	0.2647
F	161.3	186.1

Notes: The regressions are estimated using Equation 1. In this specification we interact the spatial and regional definition of treatment. Dependent variables are specified in logarithms. Firm level controls include firm age, the share owned by foreigners and the share owned by the public sector. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.

Table A5: Treatment Effects by Institutional Setting

	(1) logsales	(2) logsales	(3) logsales	(4) logsales
Treated Area	-0.0565	-1.0897**	0.0401	-0.3871***
	(0.1701)	(0.4842)	(0.1146)	(0.1365)
Treated#V-Dem electoral democracy index	0.3293	4.0786**		
	(0.2890)	(1.7230)		
Treated#(V-Dem electoral democracy index)^2		-3.1328**		
		(1.4590)		
Treated#Control of Corruption			0.2252	2.3898***
			(0.2359)	(0.6027)
Treated#(Control of Corruption)^2				-2.2891***
				(0.6376)
Firm level controls	Yes	Yes	Yes	Yes
Observations	82,527	82,527	81,697	81,697
R-squared	0.6621	0.6621	0.6632	0.6632
F	160.7	135.9	155.6	133.2

Notes: The regressions are estimated using Equation 1 augmented by interacting the treatment variable with the V-Dem electoral democracy index and the control of corruption index from the World Banks Worldwide Governance Indicators. The former index seeks to answer the question 'to what extent is the ideal of electoral democracy in its fullest sense achieved' by aggregating a number of relevant sub-indices. It ranges from 0 (low) to 1 (high). The aggregation encompasses both the idea of a weakest link argument and partial compensation between the sub-indices (Coppedge et al. 2021). The latter index is also an aggregate of a number of sources' perception of corruption. It is expressed as a percentile rank and scaled to the 0 (worst rank) to 1 (best rank) interval. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.

Figure A3: Treatment Effect on Nightlight Luminosity - Difference-in-Difference as introduced in De Chaisemartin and d'Haultfoeuille (2022)
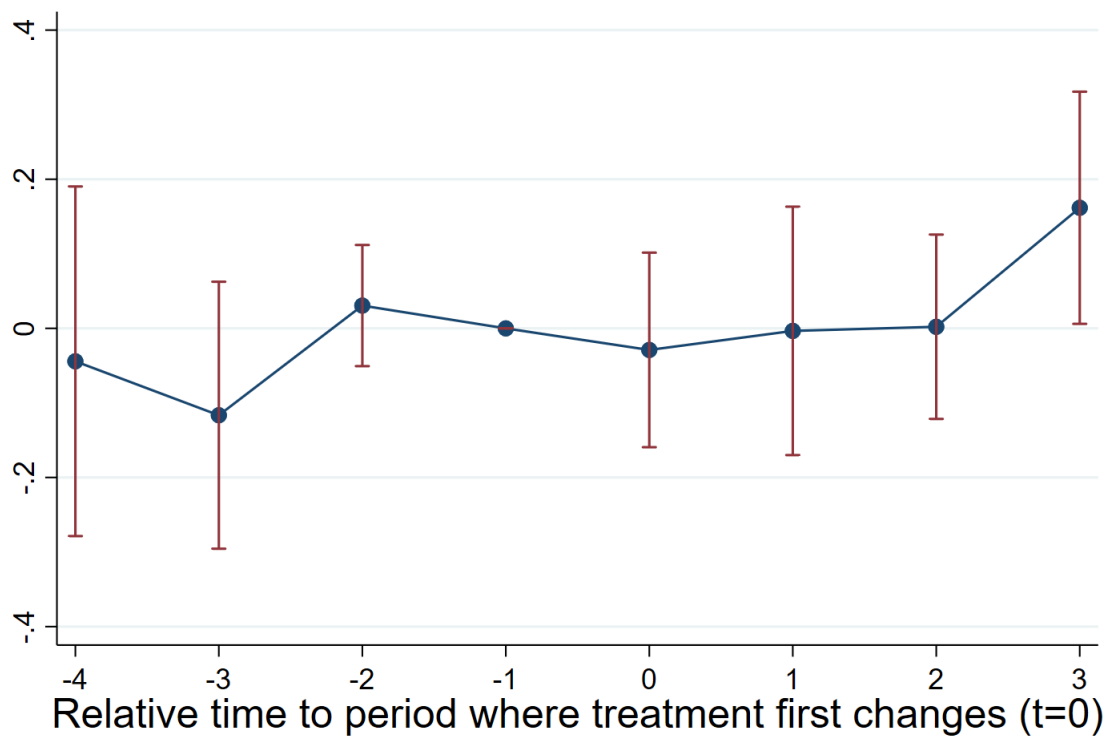

Notes: We construct shape files of the treatment and control areas based on the country specific distance measures for the countries with identifying variation in our baseline specification. Control areas are constructed by utilizing admin-1 region shape files and subtracting the treated and exclusion areas from them. We then extract average night lights for these areas from the Earth Observation Group's extended annual DMSP nighttime lights time series using R's *exact_extract* function. The figure presents the estimated treatment effects relative to the year before treatment status switches for the first time, comparing treatment status switchers to non-switchers (De Chaisemartin and d'Haultfoeuille 2022). The estimates are unbiased under heterogeneous and dynamic effects, which is a potentially more prominent issue given the yearly frequency of the nightlight data. We include region fixed effects as control variables and cluster at the group level.

Figure A4: Example of a 0.5 x 0.5 Degree Grid Layer over India
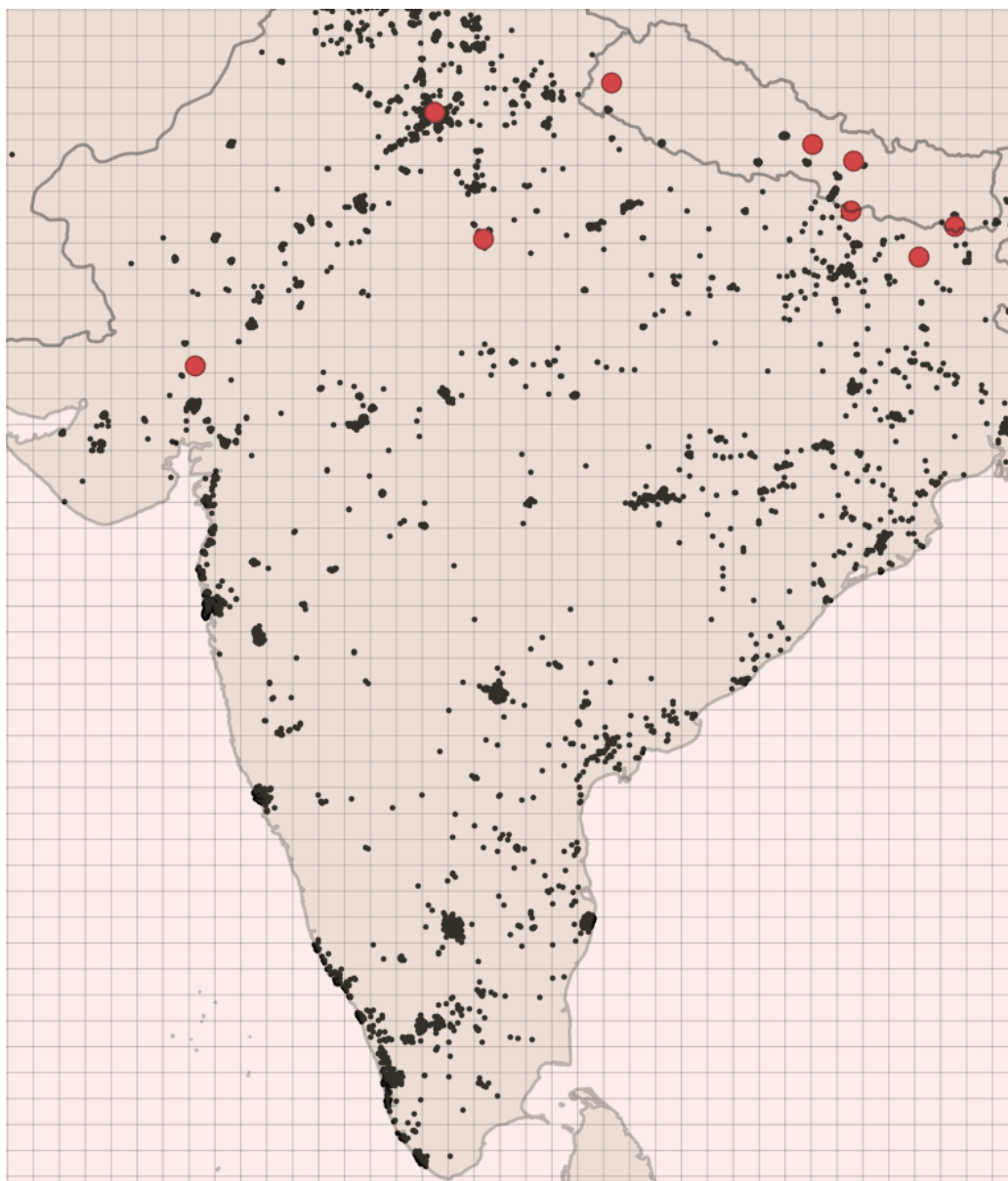
Notes: This map serves as a visual example of the grid-layer over India. The grid is spanned by 0.5 x 0.5 degree pixels across the world. The small black dots represent firms. The large red dots represent leader birthplaces.

Figure A5: Permutation Test: Effect of Placebo Treatment

(a) Log sales

(b) Log employees
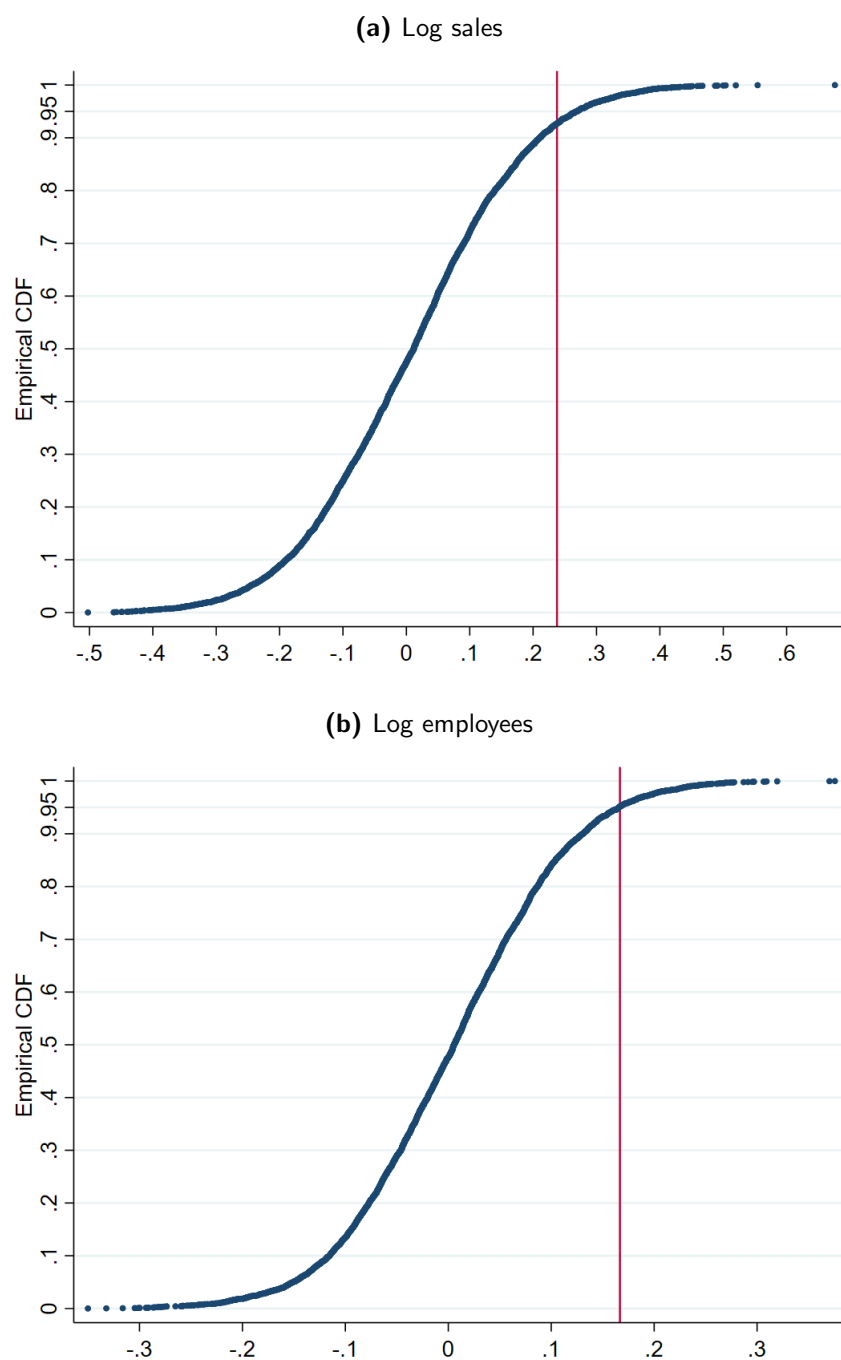
Notes: The figure depicts the cumulative distribution of 5000 placebo estimates of the permuted treatment effect. The estimates are derived from the grid-level specification with size 0.5 × 0.5 degrees, where in each country on permutation a random grid cell receives treatment status. The vertical red lines show the magnitude of the actual treatment effects from columns (3) and (4) of Table 2.

Figure A6: Changes to the Average Treatment Effect when Dropping Countries with Identifying Variation One-by-One

(a) Specification 1, geocoded data

(b) Specification 2, regional data
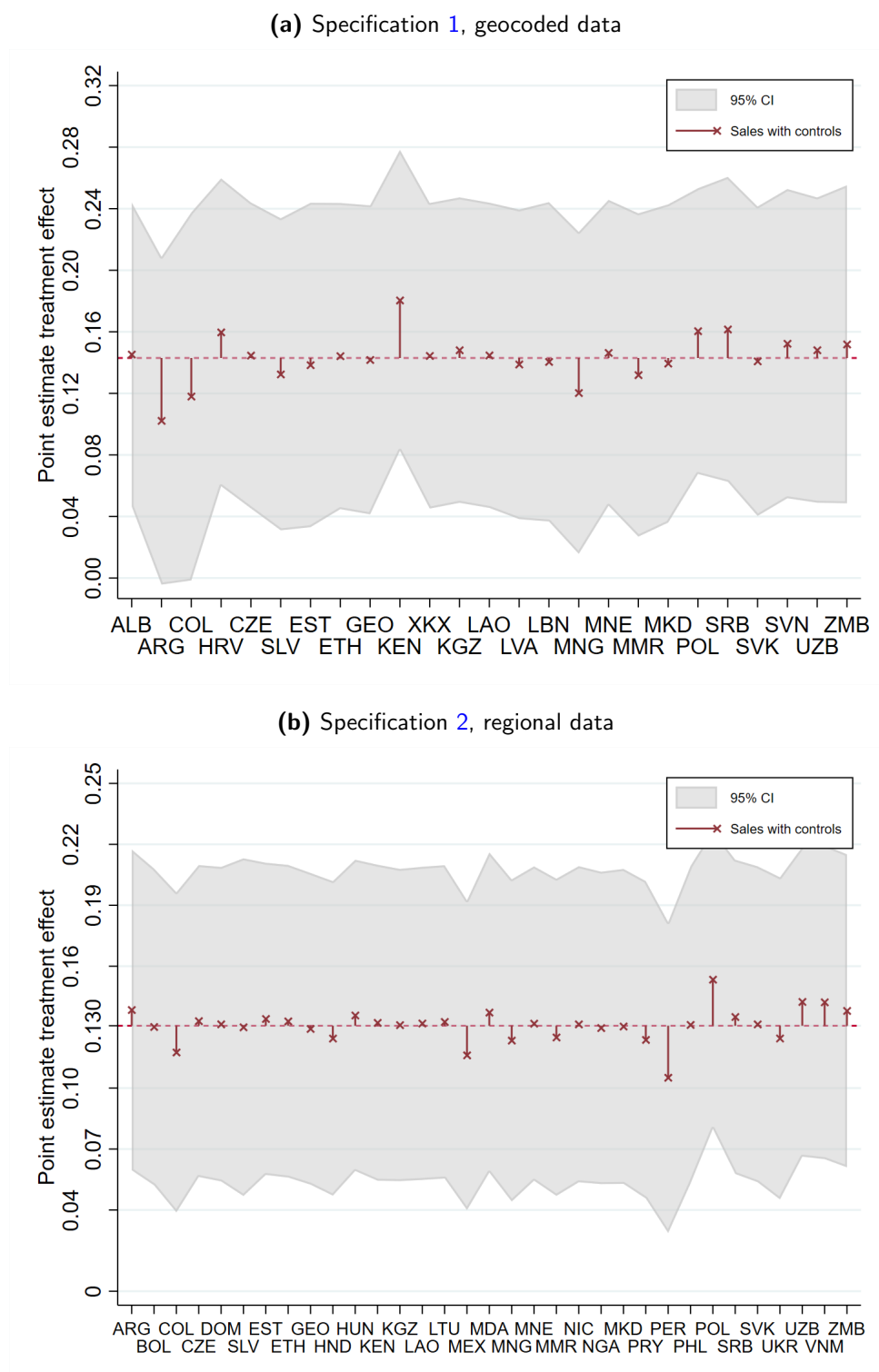
Notes: The x-axis lists the 3-letter ISO 3166 country code of the country that is dropped from the estimation for the respective estimate. The red line depicts the average effect of the corresponding unrestricted samples from Tables 1 and 2.

Figure A7: Size Distribution of Treated and Untreated Firms by Sector
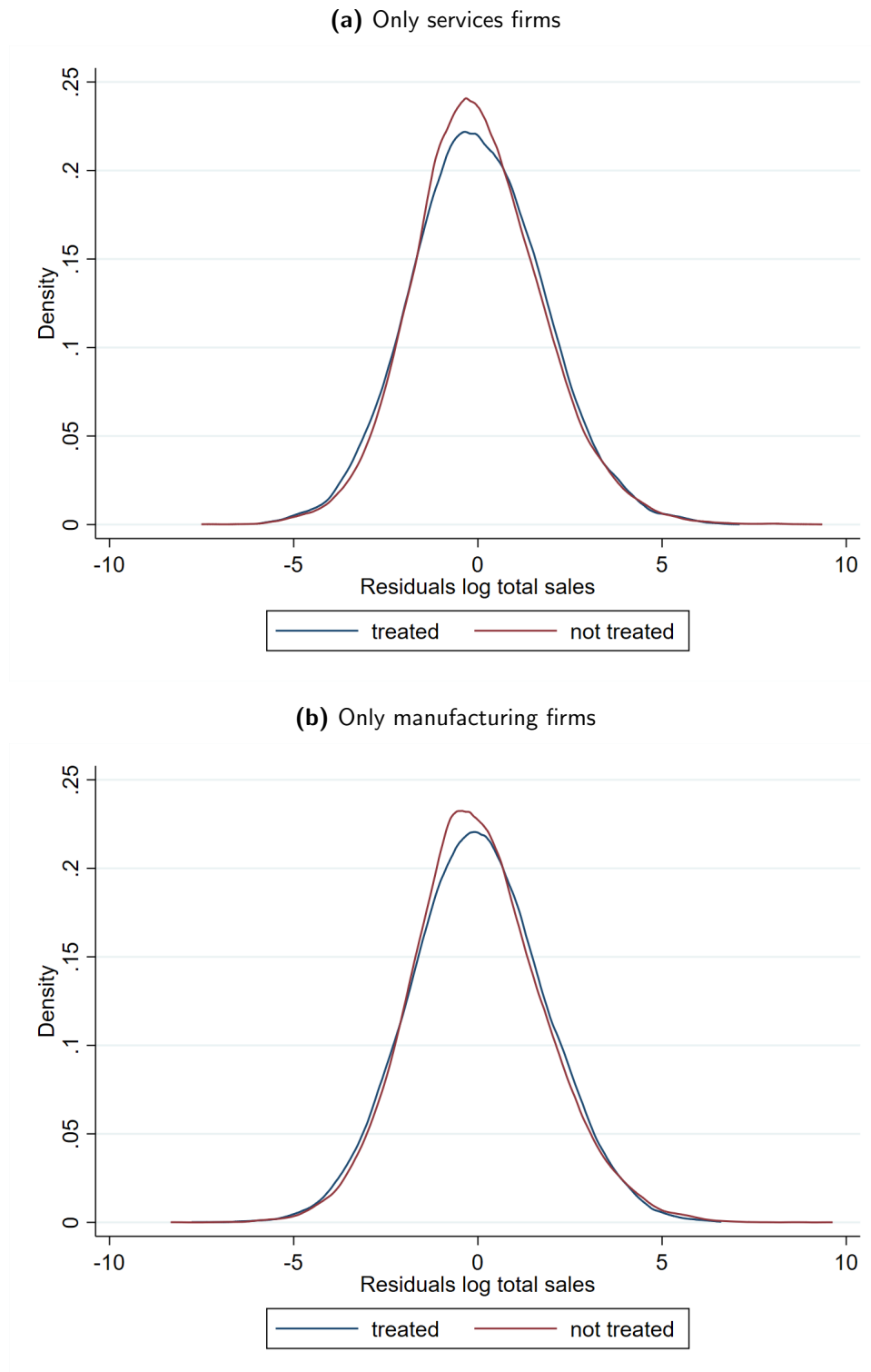
(a) Only services firms

(b) Only manufacturing firms

Notes: The figure plots the approximated density of residuals from Equation (1) with respect to logarithm of sales for the treatment and control group restricting the sample to service sector firms in panel (a) and to manufacturing sector firms in panel (b) using Epanechnikov kernel estimator.

Appendix B: Implementation of the Random Forest

Random forests operate by averaging over a number of unique uncorrelated decision trees. Each individual decision tree splits the data based on a number of randomly selected variables at each node looking to purify the data. That is, at each node the data is partitioned into groups based on the observations' similarity in terms of the randomly selected variables. Decision trees reach their terminal nodes once no further purification of a given data partition can be reached. These terminal nodes then determine our estimated propensity scores as the share of observations belonging to the treatment group at that node for the subjects present.

There are two main parameters that establish the generation of the random forest. The first is the number of trees to be grown. Figure B2 shows that the prediction error rate of our forest is stable after 100 trees, however to be extra diligent we grow 500 trees. The second parameter is the number of randomly sampled variables available for splitting the data at each tree node. In Figure B3 we investigate its optimal value by starting from a value of 2, and gradually showing the response of the prediction error rate. At 20, the error rate has virtually converged to a stable value, to which we therefore set this parameter.

All firm level variables with less than 20% missing values that are not our regression outcomes are fed into the random forest algorithm. Zhao et al. (2016) demonstrate that random forests can perform well with variables missing even up to 40% of values. We let the algorithm classify firms into the following groups: the not yet treated, the treated and the never treated. We do this to adopt a weighting scheme similar to the one suggested by Stuart et al. (2014) that accounts specifically for a difference-in-differences design with cross-sectional data. The weights are calculated as follows:

$$w_i = \frac{p_1(X_i)}{p_g(X_i)}$$

where firms' weight w_i is equal to the predicted probability to be in group 1 given the observed covariates X_i over the predicted probability to be in the group they are actually in. Group 1 consists of the not yet treated. Firms in the other groups receive a weight that is proportional

to the predicted probability of them being in group 1, relative to the predicted probability of them being in the group they actually belong to.

In figure B4 we visualize the distribution of the predicted probabilities to belong to any of the groups for the not yet treated, the treated, and the never treated by kernel density estimation. This serves to evaluate the overlap and common support hypothesis. First we exclude observations with probabilities close to 0 or 1 of belonging to any group to avoid perfect predictability given a set of covariates. Then we trim observations to the area of common support following the approach of Heckman et al. (1997). We drop areas where the estimated densities of the kernel estimator are below a threshold of 0.01.
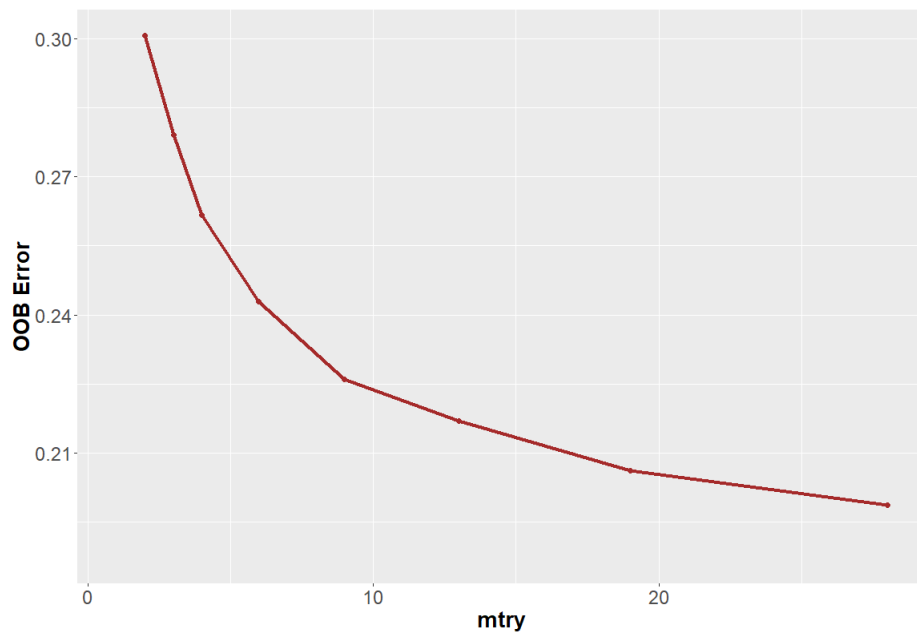
Figure B1: Distribution of Standardized % Bias across Covariates between Treated and Untreated Observations
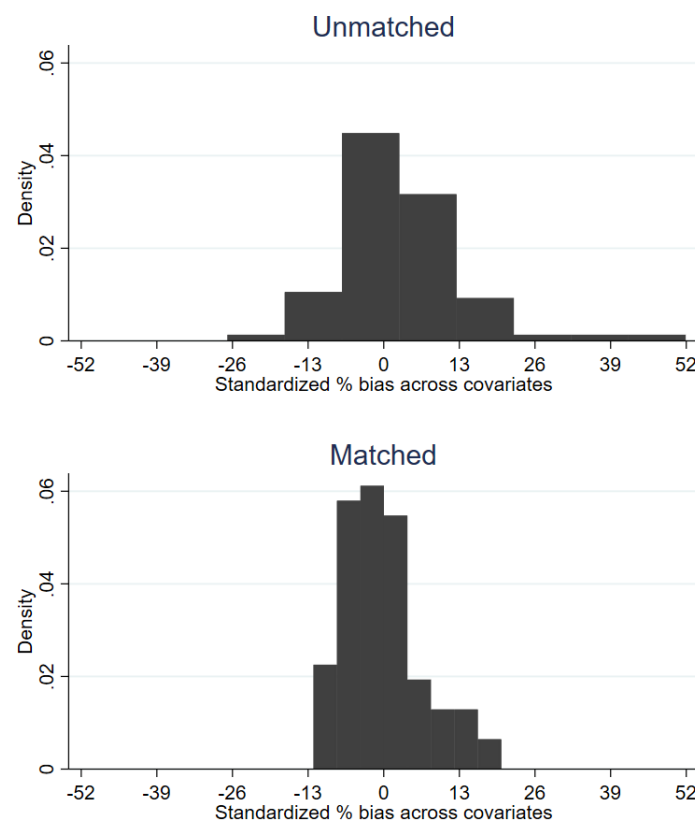
**Figure B2:** Random Forest Accuracy Over the Number of Trees Grown
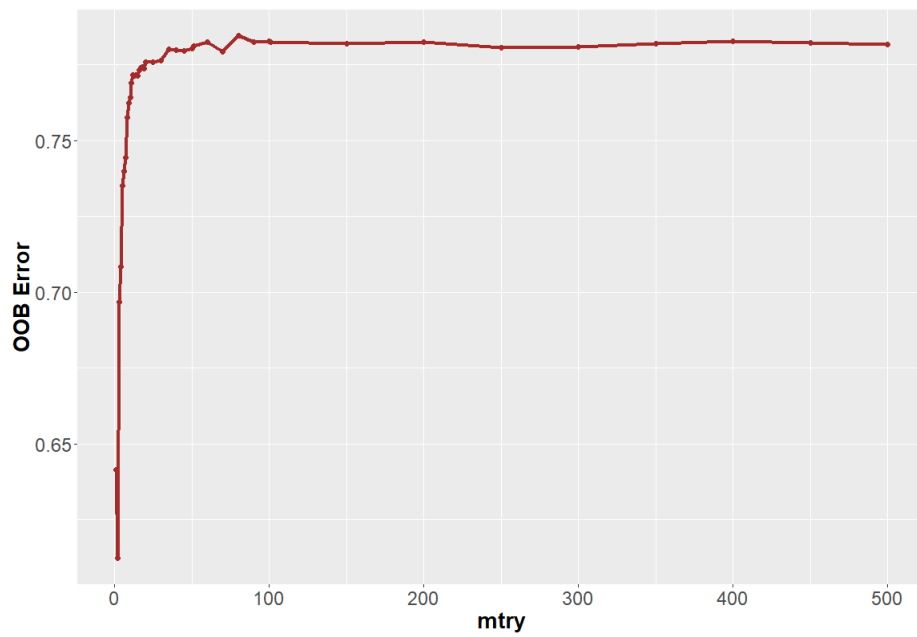
Figure B3: Random Forest OOB Error Rate over Number of Variables Used to Split at Each Tree Node
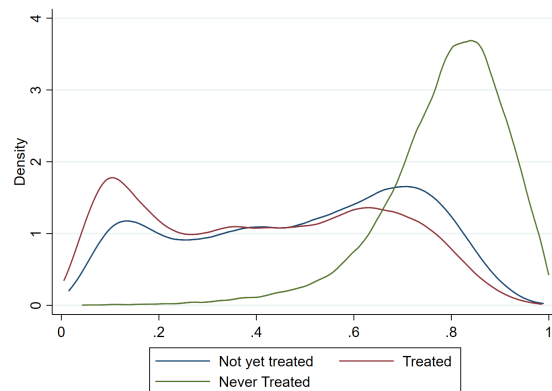
Figure B4: Distribution of the Predicted Probability to Belong to Groups 1 to 3 Given the Observed Covariates by Group

(a) Predicted probability to belong to "Not yet treated"

(b) Predicted probability to belong to "Treated"
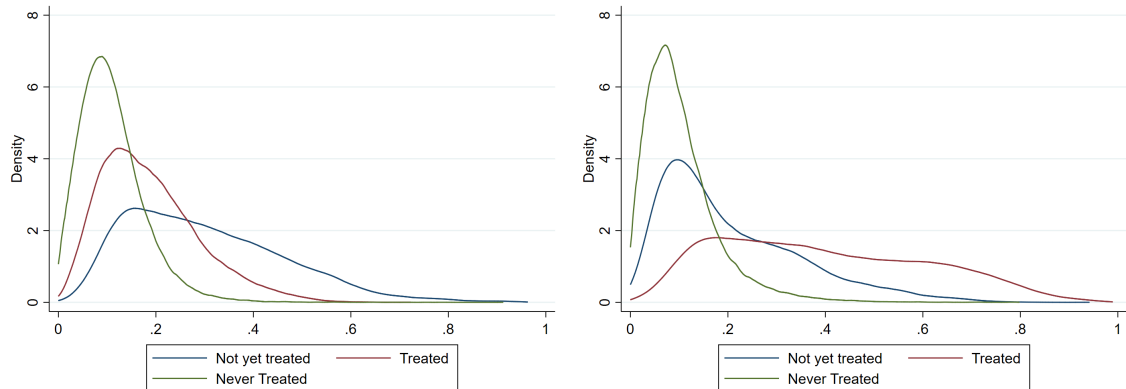
**(c)** Predicted probability to belong to "Never treated"

Notes: The figure shows distributions of predicted probabilities to belong to the group of "not yet treated" observations in panel (a), the group of "treated" observations in panel (b) and the group of "never treated" observations in panel (c) given the observed covariates and actual group status. The red line indicates the distributions for the "treated" group, the blue line for the "not yet treated" group and the green line for the "never treated" group.

Appendix C: Robustness of the Results to Alternative Specifications

In this section we showcase the robustness of our results to a host of alternative choices for the share of the distance measure as well as the distance cut-off. We plot the distribution of all 90% confidence intervals of the treatment effects for distances from $\frac{1}{8}$ to $\frac{1}{20}$ and for cutoffs of $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{8}$ and $\frac{1}{9}$ for our main outcomes sales and employment, as well as the productivity outcomes. We also re-estimate our baseline table fixing the sample size such that the number of observations is stable.

Figure C1: Alternative Choices of the Distance Measure and Cutoff Threshold
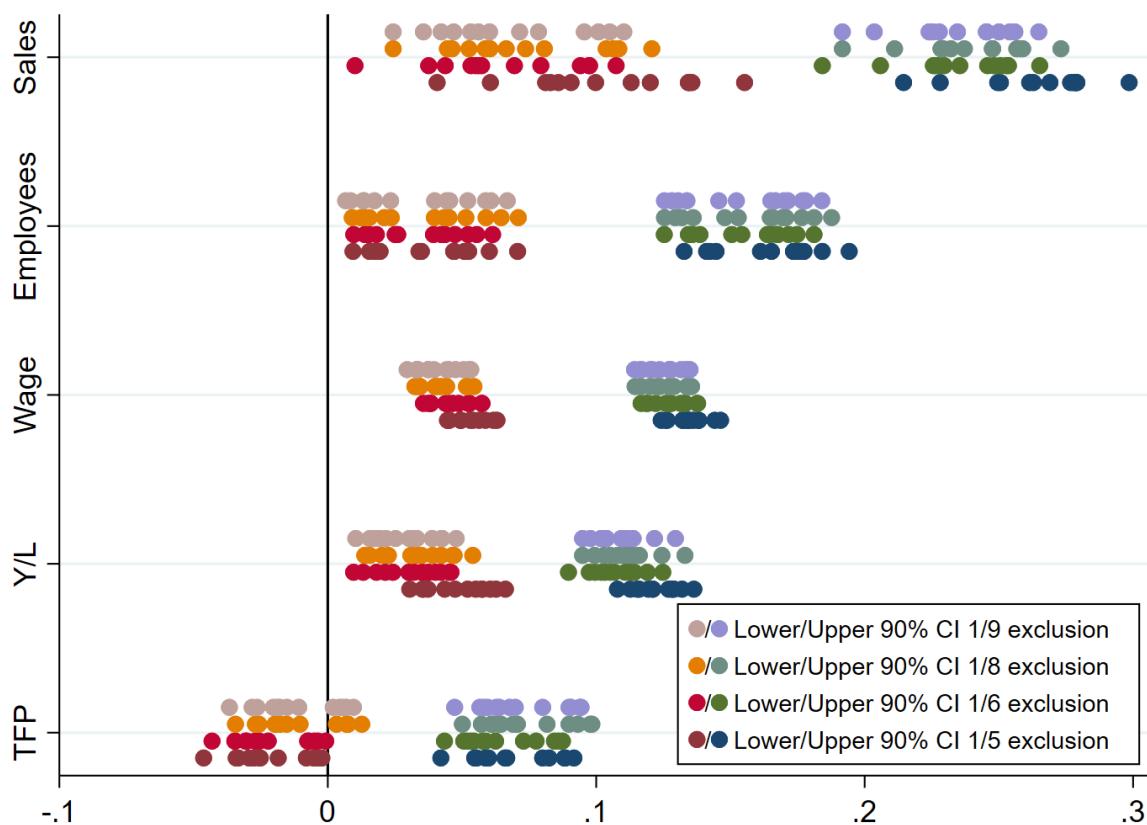
Notes: The figure shows the upper and lower bounds of 90% confidence intervals for alternative specifications of the baseline specification. Each pair of points represents a pairing of an alternative treatment distance, going from $\frac{1}{8}$ to $\frac{1}{20}$ of the country-specific distance measure, with an alternative cutoff distance of $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{8}$, or $\frac{1}{9}$ of the country-specific distance measure. Outcomes are noted on the y-axis, the vertical black line marks zero.

Table C1: Baseline Results: Fixed Sample Size

	(1) Log Sales	(2) Log Employees	(3) Log Sales	(4) Log Employees	(5) Log Sales	(6) Log Employees
Treated area	0.1811***	0.0969**	0.1839***	0.0873**	0.1562***	0.0747*
	(0.0524)	(0.0387)	(0.0536)	(0.0415)	(0.0533)	(0.0392)
Year before treatment start			0.0317	-0.1094		
			(0.1600)	(0.1170)		
Year after treatment end					-0.2579***	-0.2298***
					(0.0881)	(0.0485)
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	81,587	81,587	81,587	81,587	81,587	81,587
R-squared	0.6661	0.2719	0.6661	0.2719	0.6661	0.2720
F	189.5	198.3	151.7	158.4	154.2	166.4

Notes: The regressions are estimated using Equation 1 replicating Table 1, but restricting the sample such that the number of observations is stable. Treatment area is defined as $\frac{1}{11}$ of the country-specific distance measure. Dependent variables are specified in logarithms. Firm level controls include firm age, the share owned by foreigners and the share owned by the public sector. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are clustered at the level of treatment.