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FAVORITISM AND FIRMS: MICRO EVIDENCE AND MACRO IMPLICATIONS

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Favoritism and Firms:

Micro Evidence and Macro Implications

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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 (public) demand for the non-tradable sector. We calibrate a simple structural model of resource misallocation on our estimates. This exercise implies that favoritism reduces output by 0.5% annually.

JEL codes: D22, D72, O43, R11.

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

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1 Introduction

Regional favoritism - that is, the geographic redistribution of resources within countries based on preferential political treatment - is a large phenomenon observed in many parts of the world (Hodler and Raschky 2014). Economists have long studied the question of whether and how distributive politics - including political and regional favoritism - lead to distortionary economic policies (Golden and Min 2013). Many authors have claimed that lower income and less democratic countries chronically suffer from such distortive policies, which possibly contribute to widening the income gap between high and low income countries.

We ask whether regional favoritism should be viewed as a policy failure that necessarily impedes economic outcomes, or whether it can be thought of as a type of industrial policy that may potentially improve economic development. To answer this question, we examine whether regional favoritism impacts firm performance. On the one hand, favoritism is likely to diminish welfare if leaders divert too many resources to their home region without improving the productive capacities of firms, for example due to political connections and corrupt motives. On the other hand, favoritism can improve welfare if leaders can provide at least a selected set of firms and regions the push necessary to grow, become more productive and enter international markets.

To study this trade-off, we employ cross-sectional survey data from at most 125,000 enterprises in 120 low and middle income countries, and utilize transitions of national political leaders for identification. 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 after the leaders assume office. Exploiting information on the exact geo-location of firms, we show that these effects of favoritism are strongest in a 10 km radius around leaders' birthplaces, and that the effects diminish by distance. In our baseline specification, we find that firms located within about a 50 km radius of the leaders' birthplaces have 22% higher sales and 13% more employees compared to control firms.

For an average firm, these effects translate into $1.6 million higher sales and 11 additional employees. Our placebo analysis does not find evidence for the existence of pre-trends in firm outcomes, suggesting that the causality likely runs from leader changes to firm outcomes. One robustness exercise uses propensity score weights from random forest classification to balance out differences in many observable characteristics between treated and control firms in our cross-sectional data, and confirms our baseline findings.

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 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 the two 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 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. 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 0.5% lower compared to a distortion free economy. The intuition behind this result is as follows. Redistribution between regions increases the level of income in the home 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 leaders' home region and towards the tradable sector in the non-home region. This higher concentration of labor in the two sectors decreases the marginal productivity of firms and results in aggregate losses.

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), Martinez-Bravo and Wantchekon (2021), Restuccia and Rogerson (2017). In this context several studies have used enterprise survey data to estimate aggregate output

[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.

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 and a number of extensions. Section 4 sets up the quantitative model and calibrates it to arrive at aggregate implications. Section 5 concludes.

2 Empirical design

2.1 Data

2.1.1 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

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

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.

We categorize firms into either the tradable or non-tradable sector. To this end we exploit information reported by firms regarding the ISIC category of their main product or service. We then rely on the micro-founded approach of Chen and Novy (2011) that ranks the trade costs of 163 industries at the four-digit NACE level.[4] We use this classification and categorize firms ranking 50 or higher as tradable. We prefer this approach because, as noted by Holmes and Stevens (2014), many product categories that are considered manufacturing tend to be sold only locally. For this reason we reclassify manufacturing sectors with very high trade costs, such as bricks, as belonging to the non-tradable category.

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.[5] In additional specifications we use the general sample, where we can identify the location of firms according to administrative regions. We give priority to the smaller sub-sample of geocoded data to achieve greater 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.

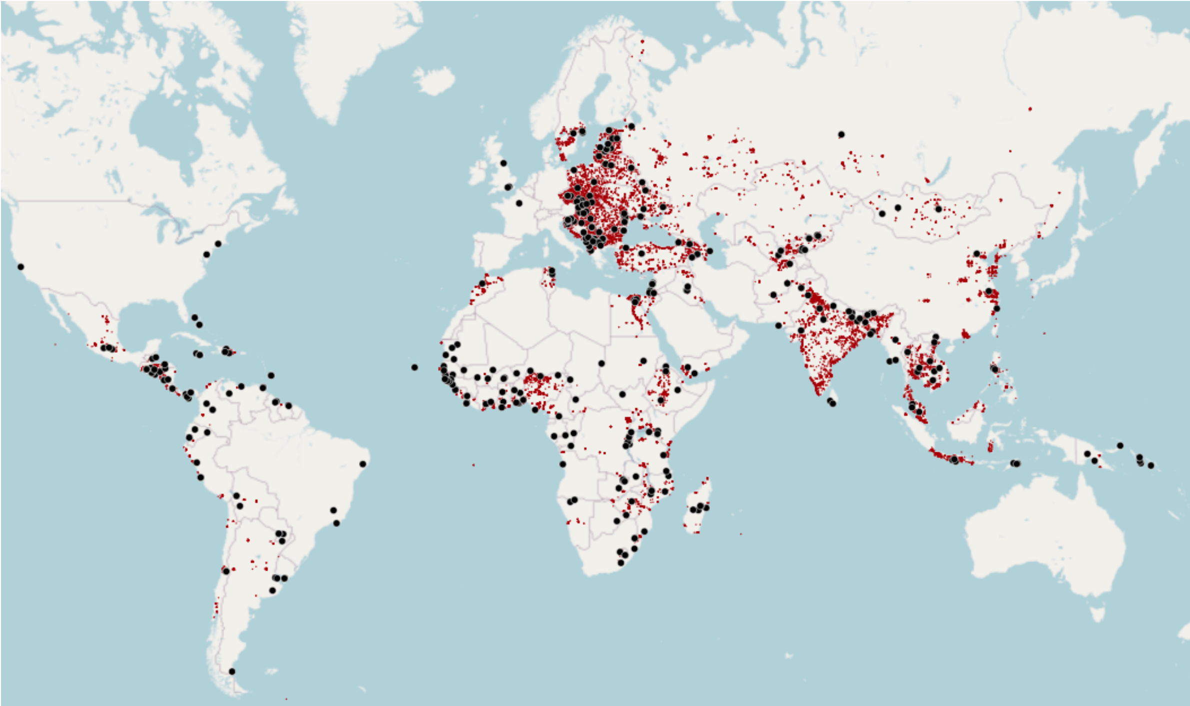
2.1.2 Political leaders

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

[4]We utilize conversion tables to translate our ISIC rev 3.1 classification to the 4-digit NACE rev.1 classification of industries.

[5]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.

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 subsample 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 15 countries in the baseline sample, and from 33 countries in the regional sample.

2.1.3 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.

2.1.4 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 58,000 to 80,000 firm-level observations, depending on the outcome we study. In the regional specification we have between 105,000 to 140,000 observations.

Table A1 of the appendix lists the countries and survey waves in our sample, with information on the number of firms and leaders per country and survey wave. We note the countries that contain 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 region, 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.

As discussed in Section 2.1, our data measure the location of firms either by the exact geocoordinates of the firm, or by the administrative region of their location as reported in the enterprise surveys. The geocoded specification is preferred over the regional specification, as the former is more precise, and allows us to study spatial effects around leaders' birthplaces. However, this comes at the cost of losing identifying variation due to the shorter period of the geocoded sample. We start by studying firms whose exact geolocations are available, where we can identify treatment effects over granular distances. We then replicate this exercise on the larger sample to obtain complementary evidence.

2.2.1 Geocoded data

We estimate a difference-in-differences model of the following form:

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

where $Outcome_{f,i,r,c,t}$ is the logarithm of either of the following five main outcome variables: total sales, number of permanent employees, output per worker, wage 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.[6] We then study the residual from this regression as an outcome in Equation (1). Our unit of observation is the firm f belonging to industry i located in region r of country c in year t.

β^{km} is our coefficient of main interest. It is identified by the set of dummy variables $LeaderArea^{km}_{l,c}$, which turn on if a firm is located within a km kilometer radius to the birthplace of leader l in country c. The superscript km ranges from 10 to 100 km around the leader's birthplace in 5 km intervals. Firms located in country c but outside a 150 km radius of the leader l's birthplace serve as our control group. To get at the average treatment effect, we interact $LeaderArea^{km}_{l,c}$ with $Term_{l,c,t}$ which is a dummy indicating whether leader l is currently in office.

$Controls_f$ 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, μ^{km}_f, 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 two-way cluster the error term at country-sector-year and leader-area levels following the arguments laid out by Abadie et al. (2017) to cluster based on the assignment to the treatment. This clustering strategy is also in line with the design used by De Haas and Poelhekke (2019), who study the effects of mining activity with the same firm data, and similar time and spatial dimensions as our paper.[7]

[6]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.

[7]In the Appendix Table A3 we show how the estimated standard errors change under alternative clustering strategies. Our results are robust to broader clustering approaches, with the exception of the statistical significance of one outcome variable, total factor productivity, relying on clustering at the level of industries (columns (1) and (5) of Table A3).

2.2.2 Regional data

As discussed above, 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:

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

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.

2.2.3 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 3.2.2 by conducting an analysis that tests for effects in leads and lags of the treatment variable. We do not find evidence that any of the several outcome variables between treated and control firms are statistically significantly different from zero in the years leading to 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 homogenous across regions, such that the average effect of the pre-trends does not mask potentially offsetting trends. Due to the short time horizon of our data we can perform the pre-trend test for only one pre-treatment period, however this evidence is consistent with previous work that has used regional level data to study regional favoritism and, similar to our test, 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 3.2.3. 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 3.2.4, 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 heterogenous 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) on countries that provide identifying variation to investigate the issue of potentially problematic comparisons of early and late treated groups. This test suggests that only 3 of the 40 average treatment on treated effects received a negative weight,[8] which reassures the use of the standard two-way fixed effects estimation.

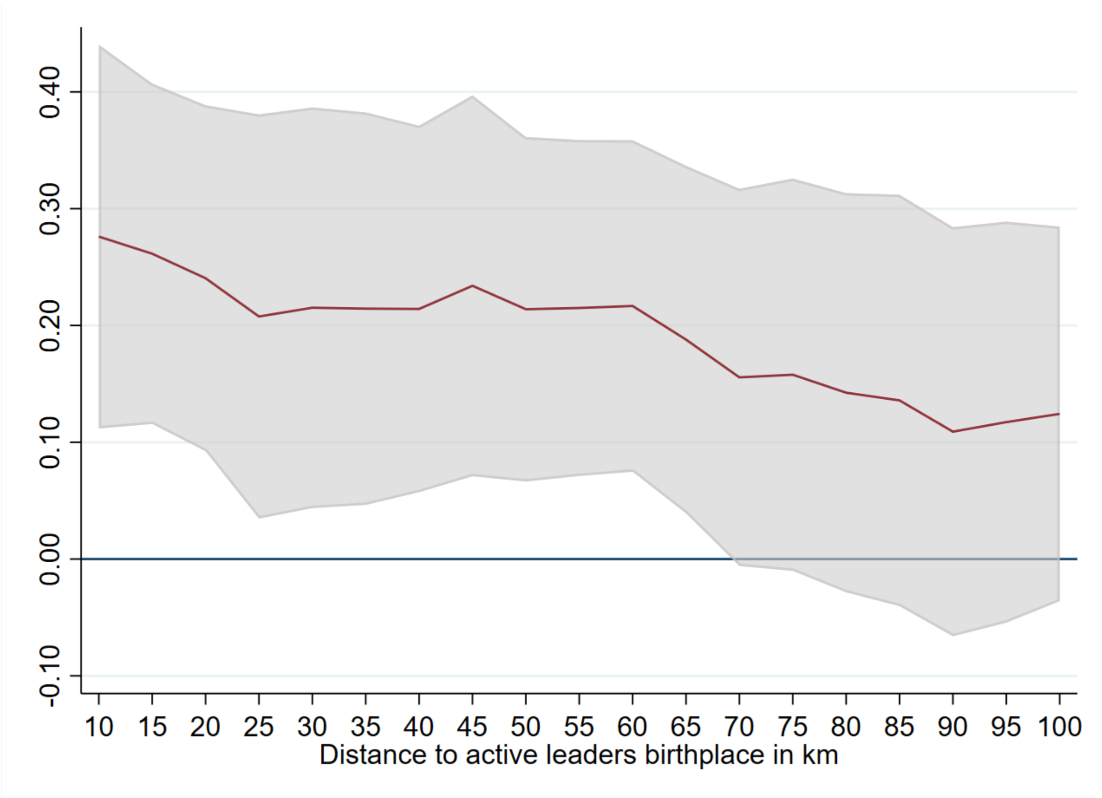
3 Empirical results

3.1 Baseline results

We start by studying the treatment effects of favoritism using the detailed geolocation of firms. We are agnostic about the area around the birthplace, which is potentially affected by favoritism. Therefore, we exploit information on the exact location of firms and, as specified in Equation 1, estimate the treatment effects of favoritism on firm outcomes in a radius going

[8]See the list of these countries with identifying variation in Section 3.2.5. Given the relatively small number of countries, in that section we also perform a jackknife-type exercise to test the sensitivity of our results to the inclusion of individual countries.

Figure 2: Treatment Effects on Log Sales by Distance to Leaders' Birthplaces



Notes : The regression is estimated using Equation 1. The red line plots the coefficient β^{km} estimated for each radius separately. 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 two-way clustered at the level of country-sector-year and leader area.

from 10 km to 100 km around leaders' birthplaces in 5 km intervals. In this exercise, which aims to reveal the spatial dimension of our potential treatment effect, we use the logarithm of total sales as the main firm level outcome.

In Figure 2 we plot the treatment effects of favoritism by distance to leaders' birthplaces. The effects are strongest in areas very close to leaders' birthplaces, with firms located in a circle of 10 km around the birthplaces having 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.

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

VARIABLES	Log Sales	Log Sales	Log Employees	Log Wage	Log Output per Worker	TFP Residual
Treated area	0.2828***	0.2139***	0.1404**	0.0927**	0.0954***	0.0479***
	(0.0892)	(0.0749)	(0.0588)	(0.0436)	(0.0173)	(0.0080)
Firm age		0.0251***	0.0192***	0.0030***	0.0049***	0.0067***
		(0.0021)	(0.0013)	(0.0007)	(0.0009)	(0.0007)
% owned foreign		0.0171***	0.0102***	0.0038***	0.0065***	0.0050***
		(0.0008)	(0.0005)	(0.0004)	(0.0005)	(0.0004)
% owned public		0.0174***	0.0153***	-0.0001	0.0016	0.0048***
		(0.0042)	(0.0029)	(0.0021)	(0.0014)	(0.0015)
Constant	16.9923***	16.4067***	2.8020***	11.6463***	13.5864***	-0.1344***
	(0.0217)	(0.0433)	(0.0273)	(0.0167)	(0.0189)	(0.0130)
Observations	70,177	70,177	79,718	66,262	69,524	57,840
R-squared	0.6369	0.6660	0.2582	0.8286	0.7796	0.2995
F	10.06	129.0	148.4	33.30	45.85	785.0

Notes : The regressions are estimated using Equation 1. The treatment is set to a 50km radius around leaders' birthplaces. Dependent variables are specified in logarithms. The mean values of the dependent variables in levels are 7.6 million USD in columns 1-2, 80 employees in column 3, 7420 USD in column 4, and 107,000 USD in column 5. USD is measured in 2009 nominal values. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and leader area.

This exercise informs our choice of defining a baseline treatment area of 50 kilometers around leaders' birthplaces, based on which we present the remaining estimates. When fixing this baseline area, we face a necessary trade-off: At smaller radii we capture the most strongly affected firms, and potentially miss less strongly affected but still relevant firms, while at larger radii we capture a larger population of firms but which potentially include irrelevant firms leading to smaller and less precise estimates.

We present our baseline results in Table 1. The first column regresses log sales on the treatment variable and fixed effects. In the second column we include key firm characteristics as control variables. The estimated coefficient is significant and implies that firms located close to leaders' birthplaces experience a 21% increase in sales relative to firms in the other parts of the country. In the third column our dependent variable is the log total number of employees. Again, we observe highly significant positive effects of 14% on average. These effects represent a sales increase of $1.6 million, and an employment increase of 11 workers for an average firm.

The size of the estimated coefficient for employment is smaller than the coefficient for sales. Consistent with this, in columns 4 and 5 of Table 1 we find that firms in the treated area pay higher wages, and produce more output per capita. Finally, column 6 shows that these firms not only grow in size, but also become more productive, as measured in terms of total factor productivity.

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. The favoritism effect leads to an estimated aggregated sales increase of $28 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.[9] 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.

3.2 Robustness tests

3.2.1 Definition of treated areas

As discussed in Section 2.2, we prefer to work with data containing information on the geolocation of firms. However, for a substantially larger sample of firms our data only indicate

[9] 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 A6 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 2: Treatment Effects in Leaders' Birth Regions

VARIABLES	Log Sales	Log Sales	Log Employees	Log Wage	Log Output per Worker	TFP Residual
Treated region	0.1543***	0.1308**	0.0609**	0.1013***	0.0662**	0.0190*
	(0.0581)	(0.0512)	(0.0290)	(0.0343)	(0.0280)	(0.0111)
Firm age		0.0257***	0.0195***	0.0032***	0.0051***	0.0060***
		(0.0010)	(0.0006)	(0.0004)	(0.0006)	(0.0004)
% owned foreign		0.0173***	0.0103***	0.0041***	0.0067***	0.0045***
		(0.0006)	(0.0004)	(0.0003)	(0.0004)	(0.0002)
% owned public		0.0176***	0.0157***	0.0011	0.0011	0.0034***
		(0.0015)	(0.0011)	(0.0009)	(0.0009)	(0.0008)
Constant	16.8800***	16.2709***	2.7792***	11.5343***	13.4884***	-0.1447***
	(0.0129)	(0.0238)	(0.0135)	(0.0103)	(0.0139)	(0.0097)
Observations	126,359	126,359	142,710	121,357	125,191	107,439
R-squared	0.6319	0.6643	0.2626	0.8382	0.7800	0.2741
F	7.048	388.6	499.3	62.28	90.31	149.6

Notes : The regressions are estimated using Equation 2. The treatment is set equal to the administrative region where the leader was born. Dependent variables are specified in logarithms. All regressions include fixed effects for regions, industries and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and leader region.

location at the regional level. This larger sample also uses twice as many leader transitions for identification than the geolocated sample. Therefore, as a complementary exercise to our baseline results, we run regressions in which the treatment is defined by the region of leaders' birth rather than their exact birthplace. Table 2 shows these estimates using our five main outcome variables of interest. As expected, the treatment effects become somewhat smaller and less precise. However, in all cases the evidence for positive and statistically significant effects is replicated.[10]

In a further test, we show that our results are robust to a more granular classification of birthplaces than the birth regions. We overlay countries' geographies with a fine grid layer of 0.5 x 0.5 degree pixels.[11] This allows us to introduce pixel fixed effects to control for sub-regional time invariant confounding effects. Figure A1 of the Appendix visualizes the

[10] 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.

[11] At the equator 0.5 degree corresponds to roughly 55km. Results are also robust to a 1 degree specification.

Table 3: Treatment Effects Before and After Leader Transitions

VARIABLES	Log Sales	Log Sales	Log Employees	Log Employees
0-2 years before leader	-0.0697		0.0208	
	(0.2597)		(0.2275)	
0-2 year after leader		0.0248		0.0152
		(0.1190)		(0.0814)
Treated area	0.1953*	0.2156***	0.1456**	0.1413**
	(0.0992)	(0.0766)	(0.0721)	(0.0609)
Firm age	0.0251***	0.0251***	0.0192***	0.0192***
	(0.0021)	(0.0021)	(0.0013)	(0.0013)
% owned foreign	0.0171***	0.0171***	0.0102***	0.0102***
	(0.0008)	(0.0008)	(0.0005)	(0.0005)
% owned public	0.0174***	0.0174***	0.0153***	0.0153***
	(0.0042)	(0.0042)	(0.0029)	(0.0029)
Constant	16.4122***	16.4060***	2.8004***	2.8015***
	(0.0493)	(0.0436)	(0.0322)	(0.0277)
Observations	70,177	70,177	79,718	79,718
R-squared	0.6660	0.6660	0.2582	0.2582
F	105.8	103.4	118.6	120.8

Notes : The regressions are estimated based on Equation 1, but adding the leads and lags of the treatment variable. The treatment is set to a 50km radius around leaders' birthplaces. Dependent variables are specified in logarithms. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and leader region.

grid, while Table A5 shows the estimation results. The estimated effects are similar to those from the baseline exercise, only the effect on wages drops in size, and becomes imprecisely estimated.

3.2.2 Effects before and after leader transitions

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 up to two years earlier than it actually happened. We also create a post-treatment variable that captures the period covering up to two years after the leader leaves office. We then re-estimate Equation (1) including these leads and lags. The results are presented in Table 3. Neither pre-treatment nor post-treatment variables significantly correlate with firm sales or employment.[12]

Table 3 additionally shows that the effects of favoritism on firm growth disappear after leaders leave office. This evidence is inconsistent with the 'big push' hypothesis, according to which large positive shocks can help firms to permanently change their growth trajectories (Murphy et al. 1989). Such evidence is demonstrated by Kline and Moretti (2013), who provide evidence that regional development policies in the US have long term effects, or Lu et al. (2019), who study China's successful implementation of Special Economic Zones.

3.2.3 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 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

[12] Due to the limited frequency of the firm-level data, we are unable to identify these dynamic effects annually in years before and after leader transitions. Our data also constraints 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.

[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 4: Comparison of Baseline Estimates with Propensity Score Weighting Estimates

VARIABLES	Treated Area		Observations	R-squared		F	
	Weighted	Unweighted		Weighted	Unweighted	Weighted	Unweighted
(1) Log Sales	0.3277***	0.3376***	56,804	0.6453	0.6777	7.911	143.6
	(0.1165)	(0.0877)					
(2) Log Employees	0.2077***	0.2112***	65,167	0.1120	0.2506	20.40	203.5
	(0.0460)	(0.0629)					
(3) Log Wage	0.0928*	0.1017**	53,440	0.8519	0.8348	3.748	33.78
	(0.0479)	(0.0478)					
(4) Log Output per Employee	0.1352***	0.1430***	56,233	0.7934	0.7873	157.0	43.59
	(0.0108)	(0.0284)					
(5) TFP Residual	0.1276***	0.1089***	46,865	0.2650	0.3036	134.6	1073
	(0.0110)	(0.0049)					

Notes : This table compares the treatment effects on our five 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 3 are applied. The sample is trimmed to restrict the observations to the area of common support. The treatment is set to a 50km radius around leaders' birthplaces. Dependent variables are specified in logarithms. All regressions include fixed effects for leader circles, regions, and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and leader region.

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 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 B1. 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 4 reports 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 all five 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.

3.2.4 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 A2 of the Appendix. The red line indicates the estimate of the correct treatment assignment on sales 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 A2 shows that, also in this non-parametric setting, the null hypothesis can be rejected at the 10% significance level.

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

3.2.5 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, that 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 A3 of the Appendix shows that changes to the average effects are small, and that they never lead to the average effect to become indistinguishable from zero. In specification 1 the largest change in the point estimate is not larger than five 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.

3.3 Mechanisms

3.3.1 Sectoral results

In order to shed light on the mechanisms behind our baseline results, we start by investigating how regional favoritism affects the main sectors of the economy. 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.

We split firms into the tradable and non-tradable sector. As we discuss in Section 4, 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 in-

Table 5: Treatment Effects by Sector

VARIABLES	Log Sales	Log Employees	Log Wage	Log Output per Worker	TFP Residual
Treated area	0.2554***	0.1239**	0.1010***	0.1455***	0.1018***
	(0.0692)	(0.0487)	(0.0387)	(0.0201)	(0.0145)
Tradable	0.1194**	0.2976***	-0.0942***	-0.1777***	-0.0805*
	(0.0473)	(0.0371)	(0.0167)	(0.0614)	(0.0451)
Treated#Tradable	-0.1386**	0.0335	-0.0160	-0.1504**	-0.1281**
	(0.0670)	(0.0583)	(0.0291)	(0.0731)	(0.0522)
Firm age	0.0257***	0.0199***	0.0030***	0.0048***	0.0062***
	(0.0021)	(0.0014)	(0.0007)	(0.0009)	(0.0007)
% owned foreign	0.0174***	0.0104***	0.0039***	0.0065***	0.0051***
	(0.0008)	(0.0005)	(0.0004)	(0.0006)	(0.0004)
% owned public	0.0177***	0.0153***	-0.0000	0.0020	0.0050***
	(0.0042)	(0.0028)	(0.0020)	(0.0015)	(0.0017)
Constant	16.3595***	2.7106***	11.6720***	13.6339***	-0.1127***
	(0.0395)	(0.0328)	(0.0162)	(0.0212)	(0.0126)
Observations	70,177	79,718	66,262	69,524	57,840
R-squared	0.6585	0.2374	0.8269	0.7731	0.2615
F	100.00	112.3	31.65	470.8	265.6

Notes : The regressions are estimated based on Equation 1, but include an interaction term between treatment and sectors. The treatment is set to a 50km radius around leaders' birthplaces. Dependent variables are specified in logarithms. All regressions include fixed effects for leader circles, regions, and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and leader region.

creased 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 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 additional interaction term between the treatment variable and a dummy variable for firms in the tradable sector. Section 2.1 describes how we construct this dummy variable. The results in column 1 show that firms in the tradable sector located around leaders' birthplaces benefit less from favoritism. Further, the results in column 4 imply that they do not experience any growth in output per worker. Column 5 yields similar results for TFP. In favored areas, productivity growth and growth in output per worker are completely driven by firms in the non-tradable sector. In column 3 we observe that wage growth is similar in both sectors. This is consistent with the idea that there is high level of mobility of labor between the two sectors: Despite the fact that non-tradable 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 sectoral differences in employment growth.

3.3.2 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 2.2% 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 in particular by firms in the non-tradable sector. In columns 3 and 4, we then study whether the sales and employment grow more in firms where the government has a partial ownership stake compared to fully private 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 more strongly present in firms fully rather than partially owned by the government.

Table 6: Government Demand

VARIABLES	(1) Gov. contract secured?	(2) Gov. contract secured?	(3) Log sales	(4) Log employees
Treated Area	0.0219***	0.0316***	0.2092***	0.1323**
	(0.0050)	(0.0033)	(0.0750)	(0.0591)
Tradable		-0.0196*		
		(0.0112)		
Treated#Tradable		-0.0212		
		(0.0132)		
Log employees	0.0293***	0.0311***		
	(0.0038)	(0.0039)		
Share public ownership			0.8056***	0.7498***
			(0.2979)	(0.1595)
Treated#Share public ownership			0.3927	0.4926***
			(0.3524)	(0.1894)
Firm age	0.0010***	0.0010***	0.0251***	0.0192***
	(0.0001)	(0.0002)	(0.0021)	(0.0013)
% owned private foreign	-0.0004***	-0.0004***	0.0170***	0.0101***
	(0.0001)	(0.0001)	(0.0008)	(0.0005)
% owned public	0.0015***	0.0014***		
	(0.0005)	(0.0005)		
Constant	0.0647***	0.0637***	16.4069***	2.8029***
	(0.0123)	(0.0104)	(0.0436)	(0.0275)
Observations	78,231	78,231	70,190	79,738
R-squared	0.1094	0.0985	0.6659	0.2588
F	31.49	4225	112.2	148.6

$Notes$: The regressions are estimated using Equation 1, with logarithm of employees as an additional control variable to account for firm size. The treatment is set to a 50km radius around leaders' birthplaces. 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. 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 two-way clustered at the level of country-sector-year and leader region.

3.3.3 Business environment

Next, we study whether leaders use government regulatory policies to contribute to firm growth in their birth regions. Thus, we are interested in the supply side, rather than the demand side as studied in the previous section. The Enterprise Surveys ask questions regarding the constraints that firms face while doing business. Firms are asked to evaluate certain obstacles

Table 7: Perceived Business Constraints

VARIABLES	Treated Area	Observations	R-squared	F
(1) Average	0.1101*	65,287	0.3984	7.552
	(0.0621)			
(2) Infrastructure	0.1482**	78,393	0.2945	10.27
	(0.0586)			
(3) Institutions	0.0299	68,297	0.3911	7.917
	(0.0784)			
(4) Inputs	0.0959**	75,674	0.2814	15.38
	(0.0383)			
(5) Land	0.0854***	77,520	0.2240	17.90
	(0.0257)			
(6) Finance	-0.0451	79,037	0.1979	31.30
	(0.0126)			
(7) Workforce	0.1866***	79,437	0.2405	11.40
	(0.0493)			

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. The treatment is set to a 50km radius around leaders' birthplaces. 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 two-way clustered at the level of country-sector-year and leader region.

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.

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. Meanwhile, the estimated coefficients on infrastructure and input constraints suggest that firms operating in the areas around the leaders' birthplaces see deficiencies in terms of infrastructure and inputs as constraints to their businesses. The input constraint concept itself combines three components, the results for which are displayed in the last three columns of Table 7. From these regressions we observe that firms around leaders' birthplaces complain about the lack of land and educated workforce, while the coefficient on the access to finance measure is not significantly different from zero. In terms of relative magnitudes, among the several types of business constraints, firms are most concerned about the quality of the workforce.

These results imply that leaders divert resources to their home region such as by generating higher government demand for output produced by firms in the area around their birthplaces. However, they do not promote sufficient infrastructure development to keep up with the increasing needs of these firms. 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 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).

Regarding input constraints, our results indicate that leaders do not directly affect the capital market. The increasing complaints about lack of land are rather intuitive because this factor has a fixed supply, and does not increase proportionately with output. Finally, the result in the last column indicates that the demand for labor exceeds the supply of skilled workers. This is also consistent with increasing wage levels around leaders' birthplaces, as presented in Table 1. 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.

3.3.4 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 technologies, 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 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. Row 6 does not find that firms in treated areas spend more on R&D investments 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,[15] while for new processes a negative one. Our interpretation is that higher demand in the treated regions increases firms'

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

Table 8: Drivers of Firm Productivity

VARIABLES	Treated Area	Observations	R-squared	F
Management				
(1) Log Years of Manager's Experience	-0.0129	78,909	0.2621	87.41
	(0.0196)			
Quality of Inputs				
(2) % Workers with High School Degree	-0.0008	59,486	0.3699	15.81
	(0.0148)			
(3) Formal Training	-0.0272	79,835	0.2728	228.8
	(0.0126)			
ICT Adoption				
(4) Own Website	-0.0100	80,273	0.2959	178.6
	(0.0138)			
(5) E-Mail Communication	-0.0087	63,357	0.3703	27.01
	(0.0095)			
Innovation				
(6) R&D	0.0183	63,998	0.2383	94.71
	(0.0126)			
(7) New Processes	-0.0796***	55,641	0.3083	1756
	(0.0038)			
(8) New Products	0.0195**	56,904	0.2211	121.7
	(0.0093)			
(9) Technology Licensed from Abroad	-0.0105	59,556	0.0824	30.00
	(0.0772)			
Competition				
(10) Share of Exports in Sales	-0.3021	21,207	0.2969	57.45
	(0.4450)			

Notes : This table reports the treatment effects on firms' internal drivers of productivity. The regressions are estimated using Equation 1, with logarithm of employees as an additional control variable to account for firm size. The treatment is set to a 50km radius around leaders' birthplaces. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and leader region.

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.[16] In row 9, we do not find that firms in the treated area are more likely to adopt licensed technologies from abroad, which is another measure

[16] 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.

of innovation activity. Finally, in row 10 of Table 8, we restrict our sample to firms in the tradable sector, 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.

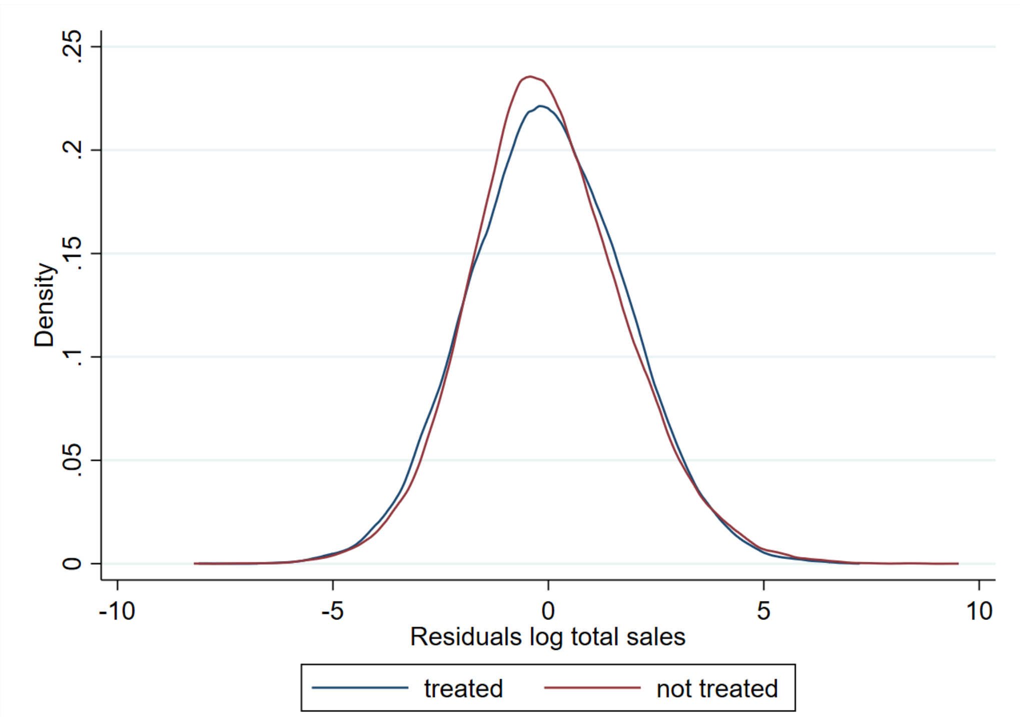
Given these null effects on this fairly comprehensive set of ten correlates of productivity, the explanation most consistent with our findings is that, despite the increases in measured TFP, firms in fact do not become more productive. Instead, the treatment effects on our productivity measures rather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

3.3.5 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 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.[17] This result supports our assumptions in the following section, in which we model homogeneous firms.

[17] 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 confident interval of the ratio ranges between 0.979 and 1.004, thus suggesting that there are no statistically significant differences in the distributions between the control and treatment groups.

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.

4 Aggregate implications

In this section we introduce a simple theoretical framework to facilitate the interpretation of our empirical findings. We also use this framework to estimate the size of the distortions caused by regional favoritism, and to quantify the aggregate welfare losses generated by such policies.

4.1 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), including informal payments. Firms in both regions produce manufacturing goods (m) and services (s) $j \in \{m, s\}$. Manufacturing goods are traded across regions and internationally; they correspond to the tradable sector in our empirical analysis. On the other hand, services are only produced and consumed locally, and thus match the definition of the non-tradable sector given above. We will assume that both regions are symmetric. Our data provide evidence in support of this assumption. We run regressions on outcomes that can proxy the average level of development (output per worker and wage) and include an indicator variable for areas which produced national leaders during the study period. The estimated coefficient for this indicator variable turns out to be very close to 0 and statistically insignificant, which implies that the leader circles are not systematically wealthier or poorer compared to other places.[18]

4.1.1 Production

We consider 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). 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.

[18]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

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

where p_{ij} is the price in region i and sector j and w_i the wage in region i. Perfect mobility between sectors implies that firms in both sectors face the same wage demands. We also set a uniform price for manufacturing goods ($p_{hm} = p_{am} = 1$).

4.1.2 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 maximize their utility subject to budget constraints

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

4.1.3 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 regions should equal to the subsidies provided in the home region

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

4.2 Model discussion

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.

$$\frac{\partial L_{is}}{\partial \tau_i} < 0. \tag{11}$$

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, the implementation of taxes will 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 1, wages decrease with taxes.

$$\frac{\partial w_i}{\partial \tau_i} < 0 \implies w_h > w_a. \tag{12}$$

4.3 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 follow the macroeconomic literature and set the labor share at $\alpha = 2/3$, and the parameter governing the share of manufacturing goods consumption in developing economies to $\gamma = 0.31$. We will assume that each region is endowed with one unit of labor. Our key objective is to choose parameters τ_h and τ_a such that we can match the 21% total output differences between regions, and make sure that the government's budget constraint (10) is satisfied. This value is taken from column 2 of Table 1. Notice that the 21% target is not relative to the distortion free economy but relative to the other region because our empirical estimates capture this effect.

Since both regions are symmetric, in the absence of wedges both regions produce and consume exactly the same quantities. In the first row of Table 9 we present the relative changes in some key estimates relative to values for the tax free economy. As already discussed the relative share of labor allocated to the services sector in the home region increases. Quantitatively this change is about 12%, while in the non-home region the corresponding figure goes down by 11%. The following column displays the relative change in prices of non-tradable goods. There is a 16% increase in prices in the home region. In the data we do not observe these quantities 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 do 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 aggregate labor. By assumption, this measure does not change, because labor is assumed to be immobile between regions. The fifth column displays the net loss in total real output, which amounts to 0.5%. In the last column we also report aggregate welfare changes, as measured in consumption equivalents. The decline in welfare is larger than in output because of the concavity of the utility function for specific goods.

Table 9: The Effect of Distortions on Factors and Output

	(1) L_{hs}	(2) L_{as}	(3) p_{hs}	(4) L_h	(5) Y	(6) W
Immobile labor %	12.00	-11	16	0	-0.5	-0.7
Mobile labor %	16.00	-14	2	13	-0.25	-0.3

$Notes$: The table displays the changes in percentages relative to the distortion-free economy. In column (6) Y refers to total output in the economy and in column (7) W refers to aggregate welfare in terms of consumption equivalents. In the first row labor is immobile across regions. In the second row labor is perfectly mobile.

In the second row of Table 9 we consider a specification with perfect labor mobility. In this environment workers will flow to the home region until wages are equalized between regions. Thus, in column 4 we observe that total labor in the home region increases by 13%. The flow of workers between regions is also reflected in a larger increase in employment in the services sector in the home region, and corresponding decline in the non-home region. This mitigates the effect on prices, such that we observe only a small increase in prices. Perfect mobility of labor also mitigates aggregate losses. In terms of output, these losses are halved compared to the specification that has no labor mobility between regions.

The reality lies between these two extreme cases. The specification with immobile labor between regions is inconsistent with the data because it cannot generate an increase in total employment in the leader's region, while the specification with mobile labor is inconsistent with the data because it generates very small price changes and an equalization of wages. Probably, the proper specification involves some frictions on labor mobility that only lead to partial wage equalization. These frictions may involve direct utility costs, or time or efficiency losses for migrant workers. We do not take a stand on the specific formulation of these losses and their parametrization. But as the specification with perfectly mobile labor demonstrates, even under very loose assumptions regional favoritism generates aggregate output and welfare losses.

Finally, we would like to note that the decreasing returns to scale in the production function is a key driver of our results. This assumption is widely used in the misallocation literature with heterogeneous firms. For example, we can obtain qualitatively similar results if we introduce firm heterogeneity and adopt a decreasing returns production function similar to Restuccia and Rogerson (2008), who consider both capital and labor.[19] Since our empirical estimates do not provide any evidence that leader transitions have a differential impact on firm-level productivity or access to finance, expanding the model with these additional layers of detail will reduce the analytical tractability of the model, without generating additional insights.[20]

5 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

[19] In a heterogeneous firm framework we will need to model market entry with fixed costs. Since we have two regions and two sectors, we will need to assume that firms draw their region- and sector-specific productivities from a known distribution, and decide where and what to produce.

[20] Adding capital to the model can magnify welfare losses if one properly models the transitional dynamics with capital adjustment costs. Leader transitions create incentives for adjusting the levels of capital stock in regions and sectors, which in the presence of capital adjustment costs will come at the expense of decreased consumption.

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 0.5% of their output each year. One policy implication of this finding is that countries can become substantially better off if they manage to constrain the regional redistributive policies of their leaders. However, our paper is less clear on how such constraints could be achieved. For example, our evidence on the role of democratic institutions as a mitigating factor is rather weak. Another lesson from our finding is that while the re-allocation of resources towards certain firms can improve their outcomes substantially, such policies are in general harmful to the economy as a whole, and should thus be considered more carefully.

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. While 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.

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Appendix A: Additional tables and figures

Table A1: Sample description

Country	Year	# firms	# leaders
Afghanistan	2008	535	1
	2014	410	
Albania	2007	304	2
	2013	360	
	2019	377	
Angola	2006	425	1
	2010	360	
Argentina*	2006	1063	3
	2010	1054	
	2017	991	
Armenia	2009	374	2
	2013	360	
Azerbaijan	2009	380	1
	2013	390	
Bahamas	2010	150	1
Bangladesh	2013	1442	1
Barbados	2010	150	1
Belarus	2008	273	1
	2013	360	
	2018	600	
Belize	2010	150	1
Benin	2016	150	1
Bhutan	2015	253	1
Bolivia**	2006	613	2
	2010	362	
	2017	364	
Botswana	2006	342	2
	2010	268	
Brazil	2009	1802	1
Bulgaria	2007	1015	5
	2009	288	
	2013	293	
	2019	772	
Burkina Faso	2009	394	1
Burundi	2006	270	2
	2014	157	
Cambodia	2016	373	1
Cameroon	2009	363	1
	2016	361	
Chad	2018	153	1

Continued on next page

Table A1 –continued from previous page

Country	Year	# firms	# leaders
Chile	2006	1017	2
	2010	1033	
China	2012	2700	1
Colombia*	2006	1000	2
	2010	942	
	2017	993	
Costa Rica	2010	538	1
Croatia†	2007	633	3
	2013	360	
	2019	404	
Czech Republic*	2009	250	5
	2013	254	
	2019	502	
Côte d'Ivoire	2009	526	2
	2016	361	
DRC	2006	340	1
	2010	359	
	2013	529	
Djibouti	2013	266	1
Dominican Republic**	2010	360	2
	2016	359	
Ecuador	2006	658	2
	2010	366	
	2017	361	
Egypt	2013	2897	3
	2016	1814	
El Salvador**	2006	693	3
	2010	360	
	2016	719	
Estonia*	2009	273	3
	2013	273	
	2019	360	
Eswatini	2006	307	1
	2016	150	
Ethiopia**	2011	644	2
	2015	848	
Gambia	2006	174	2
	2018	151	
Georgia*	2008	373	4
	2013	360	
	2019	581	
Ghana	2007	494	2
	2013	720	
Guatemala	2006	522	4
	2010	590	
	2017	345	
Guinea	2006	223	3
	2016	150	
Guinea Bissau	2006	159	1
Guyana	2010	165	1

Continued on next page

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

Table A1 –continued from previous page

Country	Year	# firms	# leaders
Honduras*	2006	436	4
	2010	360	
	2016	332	
Hungary**	2009	291	3
	2013	310	
India	2014	9281	1
Indonesia	2009	1444	1
	2015	1320	
Iraq	2011	756	1
Israel	2013	483	1
Jamaica	2010	376	1
Jordan	2013	573	1
	2019	601	
Kazakhstan	2009	544	1
	2013	600	
	2019	1446	
Kenya**	2007	657	2
	2013	781	
	2018	1001	
Kosovo	2009	269	4
	2013	202	
	2019	271	
Kyrgyz Republic*	2009	235	4
	2013	270	
	2019	360	
Lao PDR**	2009	360	2
	2012	270	
	2016	368	
	2018	332	
Latvia†	2009	271	6
	2013	336	
	2019	359	
Lebanon	2013	561	3
	2019	532	
Lesotho	2016	150	1
Liberia	2017	151	1
Lithuania**	2009	276	2
	2013	270	
	2019	358	
Madagascar	2009	445	2
	2013	532	
Malawi	2014	523	1
Malaysia	2015	1000	1
Mali	2007	490	3
	2010	360	
	2016	185	
Mauritania	2006	237	4
	2014	150	
Mexico**	2006	1480	2
	2010	1480	
Moldova**	2009	363	5
	2013	360	
	2019	360	

Continued on next page

Table A1 –continued from previous page

Country	Year	# firms	# leaders
Mongolia*	2009	362	3
	2013	360	
	2019	360	
Montenegro**	2009	116	5
	2013	150	
	2019	150	
Morocco	2013	407	1
	2019	1096	
Mozambique	2007	479	2
	2018	601	
Myanmar*	2014	632	2
	2016	607	
Namibia	2006	329	1
	2014	580	
Nepal	2009	368	3
	2013	482	
Nicaragua**	2006	478	2
	2010	336	
	2016	333	
Niger	2017	151	1
Nigeria**	2007	1891	3
	2014	2676	
North Macedonia**	2009	366	3
	2013	360	
	2019	360	
Pakistan	2013	1247	1
Panama	2006	604	1
	2010	365	
Papua New Guinea	2015	65	1
Paraguay**	2006	613	4
	2010	361	
	2017	364	
Peru**	2006	632	4
	2010	1000	
	2017	1003	
Philippines**	2009	1326	2
	2015	1335	
Poland*	2009	455	3
	2013	542	
	2019	1369	
Romania	2009	541	1
	2013	540	
Russia	2009	1004	1
	2012	4220	
	2019	1323	
Rwanda	2006	212	1
	2019	360	
Senegal	2007	506	2
	2014	601	
Serbia*	2009	388	5
	2013	360	
	2019	361	
Sierra Leone	2017	152	1

Continued on next page

Table A1 –concluded from previous page

Country	Year	# firms	# leaders
Slovak Republic*	2009	275	4
	2013	268	
	2019	429	
Slovenia	2009	276	5
	2013	270	
	2019	409	
Solomon Islands	2015	151	1
South Africa	2007	937	1
South Sudan	2014	738	1
Sri Lanka	2011	610	1
Sudan	2014	662	1
Suriname	2010	152	2
	2018	233	
Sweden	2014	600	1
Tajikistan	2008	360	1
	2013	359	
	2019	352	
Tanzania	2006	419	2
	2013	813	
Thailand	2016	1000	1
Timor-Leste	2015	126	1
Togo	2016	150	1
Trinidad and Tobago	2010	370	1
Tunisia	2013	592	1
Turkey	2008	1152	1
	2013	1344	
	2019	1663	
Uganda	2006	563	1
	2013	762	
Ukraine**	2008	851	3
	2013	1002	
	2019	1337	
Uruguay	2006	621	3
	2010	607	
	2017	347	
Uzbekistan**	2008	366	2
	2013	390	
	2019	1239	
Venezuela	2010	320	1
Vietnam**	2009	1053	2
	2015	996	
Yemen	2010	477	2
	2013	353	
Zambia*	2007	484	4
	2013	720	
	2019	601	
Zimbabwe	2016	600	1

Table A2: Summary Statistics

Geocoded sample	N	Mean	Std. Dev.	p5	p95
Treated area	101350	0.19	0.39	0	1
0-2 years before treatment	101350	0.025	0.16	0	0
0-2 year after treatment	101350	0.031	0.17	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
Obstacle practices informal competitor	95590	35.8	33.4	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
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
V-Dem electoral democracy index	148293	0.52	0.22	0.2	0.9
Scaled WB Control of Corruption percentile	147690	0.37	0.22	0.07	0.8

Table A3: Overview of Results using Alternative Clustering Approaches

	(1) CSY & LA	(2) CY & LA	(3) C & Y	(4) R & Y	(5) S & Y	(6) LA & Y
Log(Sales)	.07489	.06894	.05341	.06036	.03978	.06240
Log(Employees)	.05878	.04851	.04309	.05091	.04122	.05138
Log(Wage)	.04358	.04333	.03606	.03137	.00842	.02954
Log(Output per Worker)	.01730	.03139	.02220	.02078	.00833	.02139
Total Factor Productivity	.00801	.03534	.03473	.03325	.00491	.03121
# of Cluster 1	863	194	105	591	46	261
# of Cluster 2	261	261	12	12	12	12

$Notes$: 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 two way clustering of country-sector-year and leader area - our main specification for comparability.

Table A4: Spatial versus Regional Treatment Effects

VARIABLES	Log Sales	Log Sales	Log Employees	Log Wage	Log Output per Worker	TFP Residual
Treated area in leader admin region	0.3165*** (0.0890)	0.2246*** (0.0799)	0.1516** (0.0614)	0.0939** (0.0464)	0.0917*** (0.0177)	0.0561*** (0.0169)
Treated area not in leader admin region	0.0097 (0.0901)	0.1277 (0.0847)	0.0531 (0.0795)	0.0827 (0.0512)	0.1253* (0.0744)	-0.0199 (0.1115)
Firm Age		0.0251*** (0.0021)	0.0192*** (0.0013)	0.0030*** (0.0007)	0.0049*** (0.0009)	0.0067*** (0.0007)
% owned foreign		0.0171*** (0.0008)	0.0102*** (0.0005)	0.0038*** (0.0004)	0.0065*** (0.0005)	0.0050*** (0.0004)
% owned government		0.0174*** (0.0042)	0.0153*** (0.0029)	-0.0001 (0.0021)	0.0016 (0.0014)	0.0048*** (0.0015)
Constant	17.0141*** (0.0182)	16.4137*** (0.0429)	2.8091*** (0.0273)	11.6471*** (0.0166)	13.5839*** (0.0203)	-0.1286*** (0.0165)
Observations	70,177	70,177	79,718	66,262	69,524	57,840
R-squared	0.6369	0.6660	0.2582	0.8286	0.7796	0.2995
F	7.190	105.0	123.9	26.70	36.28	722.5

$Notes$: 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. All regressions include fixed effects for leader circles, regions, industries and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and leader area.

Table A5: Treatment Effect: Pixel Level Analysis

VARIABLES	Log Sales	Log Sales	Log Employees	Log Wage	Log Output per Worker	TFP Residual
Treated pixel	0.2835***	0.2378***	0.1667***	0.0532	0.0749*	0.0413***
	(0.0831)	(0.0752)	(0.0583)	(0.0507)	(0.0394)	(0.0134)
Firm age		0.0251***	0.0192***	0.0032***	0.0050***	0.0068***
		(0.0014)	(0.0008)	(0.0005)	(0.0007)	(0.0006)
% owned foreign		0.0169***	0.0100***	0.0037***	0.0065***	0.0050***
		(0.0007)	(0.0005)	(0.0003)	(0.0005)	(0.0003)
% owned public		0.0180***	0.0157***	0.0002	0.0018	0.0049***
		(0.0022)	(0.0015)	(0.0014)	(0.0012)	(0.0011)
Constant	17.0065***	16.4277***	2.8116***	11.6610***	13.6021***	-0.1285***
	(0.0169)	(0.0319)	(0.0187)	(0.0136)	(0.0179)	(0.0133)
Observations	70,586	69,298	78,838	65,405	68,643	57,005
R-squared	0.6492	0.6784	0.2833	0.8346	0.7879	0.3190
F	11.63	217.1	296.5	35.60	50.79	105.1

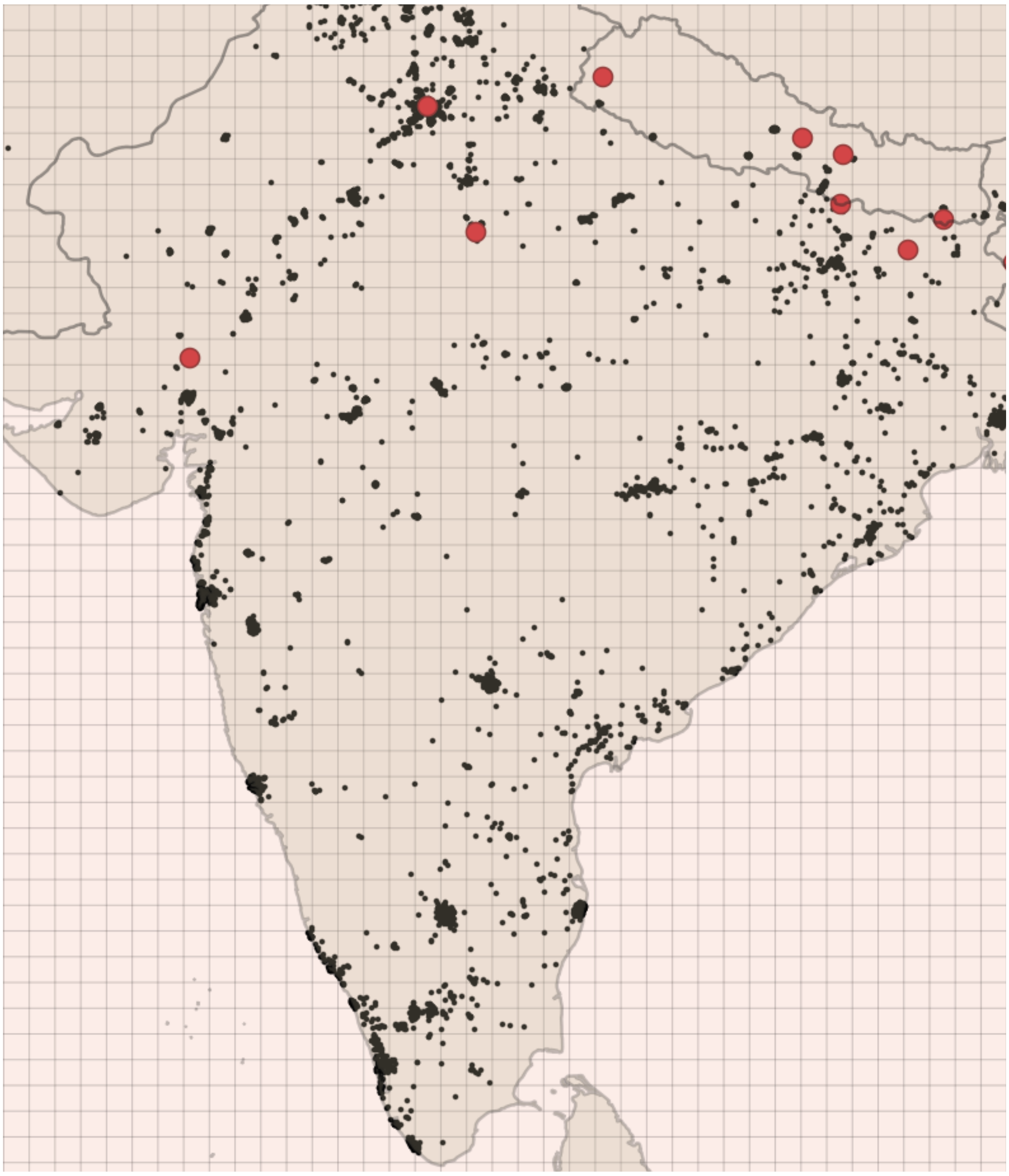
Notes : The regressions are estimated using the grid-level specification. The grid is spanned by 0.5 x 0.5 degree pixels. Dependent variables are specified in logarithms. All regressions include fixed effects for individual pixels, industries and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and pixel area.

Table A6: Treatment Effects by Institutional Setting

VARIABLES	(1) Log Sales	(2) Log Sales	(3) Log Sales	(4) Log Sales
Treated region	-0.1180	-0.5968**	0.1708	-0.4291
	(0.1518)	(0.2936)	(0.1137)	(0.2703)
Treated#V-Dem electoral democracy index	0.4193	2.4072**		
	(0.2711)	(1.2106)		
Treated#(V-Dem electoral democracy index)^2		-1.8290		
		(1.1542)		
Treated#Control of Corruption Percentile			-0.0822	3.1177**
			(0.2991)	(1.3308)
Treated#(Control of Corruption Percentile)^2				-3.6183**
				(1.4375)
Firm age	0.0257***	0.0257***	0.0257***	0.0257***
	(0.0010)	(0.0010)	(0.0010)	(0.0010)
% owned foreign	0.0173***	0.0173***	0.0172***	0.0172***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
% owned public	0.0176***	0.0176***	0.0176***	0.0176***
	(0.0015)	(0.0015)	(0.0015)	(0.0015)
Constant	16.2783***	16.2845***	16.2779***	16.2970***
	(0.0237)	(0.0235)	(0.0243)	(0.0243)
Observations	126,091	126,091	125,527	125,527
R-squared	0.6641	0.6641	0.6650	0.6651
F	309.1	258.4	307.2	261.4

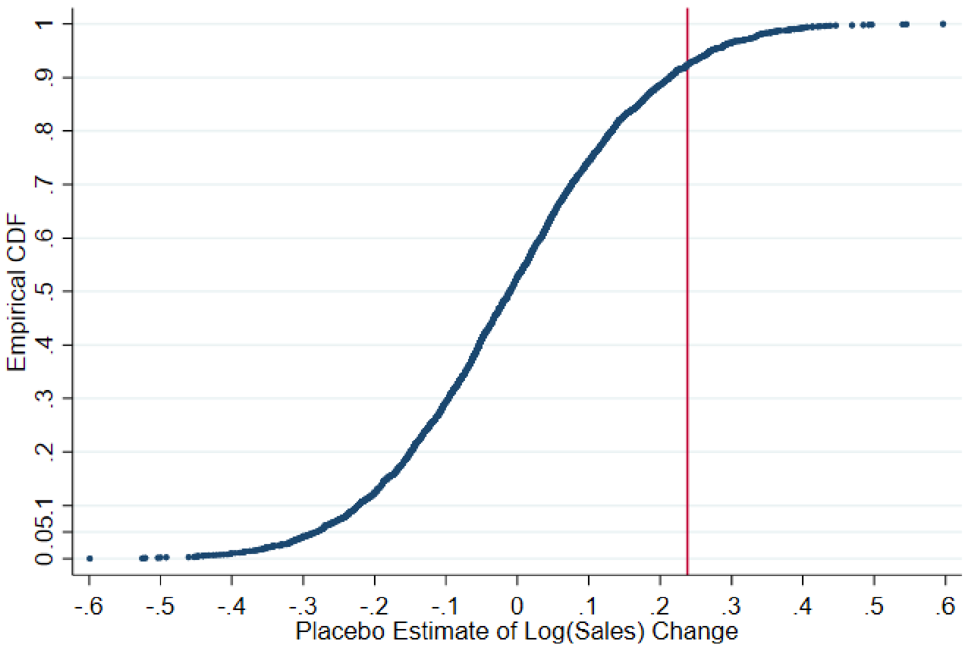
Notes : The regressions are estimated using Equation 2 augmented by interacting the treatment variable with the V-Dem electoral democracy index and the control of corruption index from the World Banks Worldwide Governance Indicators. The former index seeks to answer the question 'to what extent is the 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 regions, industries and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and region.

Figure A1: 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.

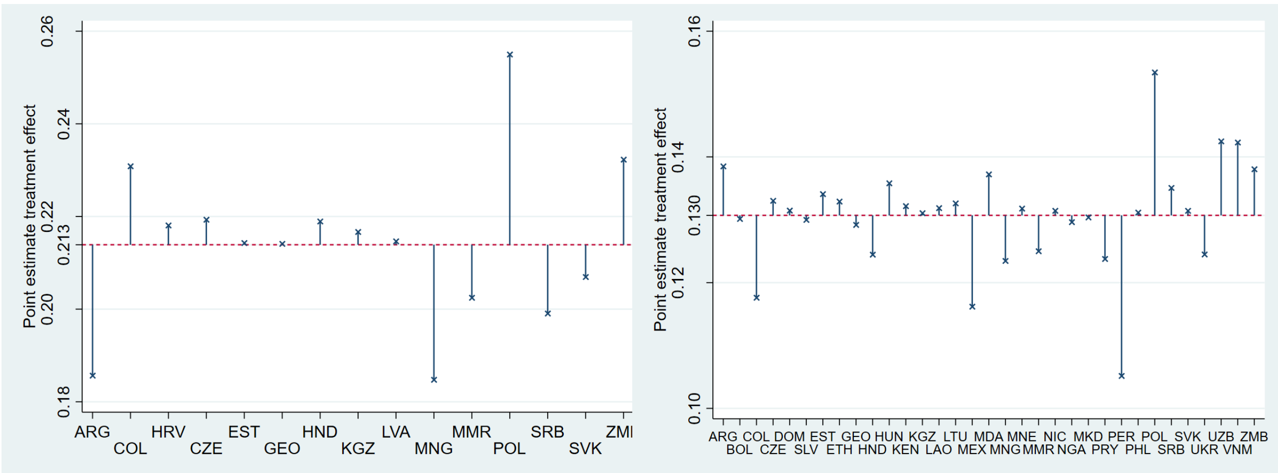
Figure A2: Permutation Test: Effect of Placebo Treatment on Log Sales



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 x 0.5 degrees, where in each country on permutation a random grid cell receives treatment status. The vertical red line shows the magnitude of the actual treatment effect from Table A5.

Figure A3: 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.

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 250 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 1, and showing in incremental steps the response of the prediction error rate. After a value of 10 the error rate improves by less than 0.01, and has virtually converged to a stable value after 15. Informed by the graph we pick a value of 20 for 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 four groups: the not yet treated, the treated and the never treated separated by survey waves. We do this to adopt the weighting scheme 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:

$$w_i = \frac{p_1(X_i)}{p_g(X_i)} \tag{3}$$

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 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 following the minima and maxima criterion explained in Caliendo and Kopeinig (2008), which is appropriate given the continuous distribution of predicted probabilities for all groups.

Figure B1: Distribution of Standardized % Bias across Covariates between Treated and Untreated Observations

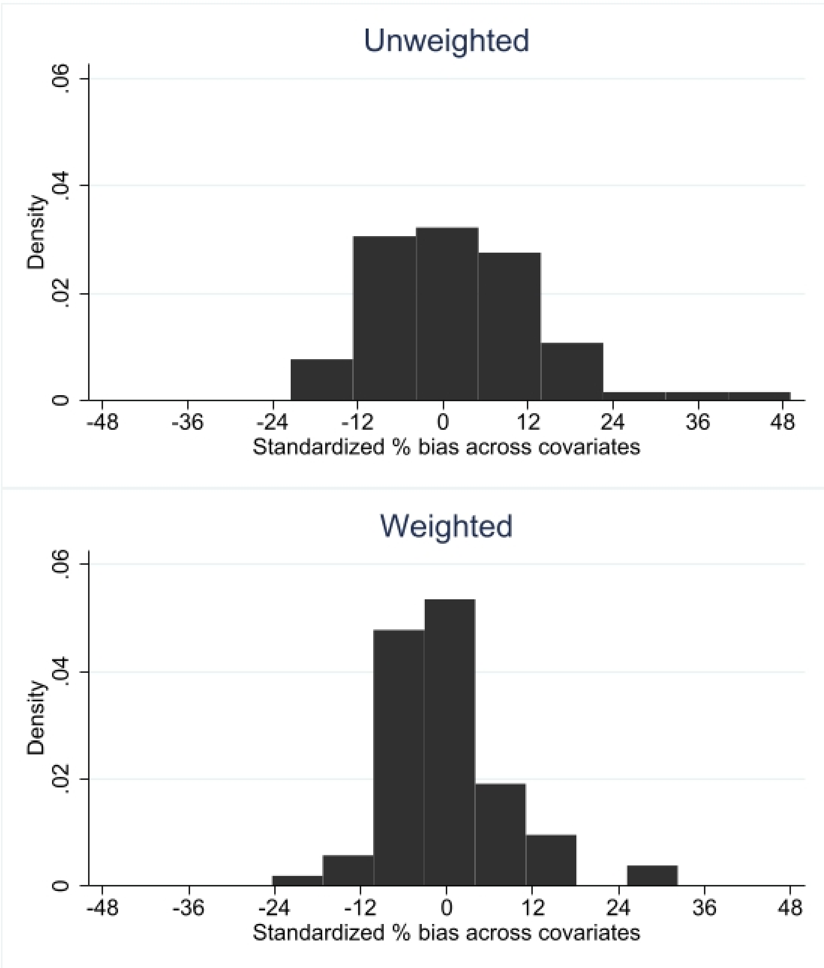


Figure B2: Random Forest Accuracy Over the Number of Trees Grown

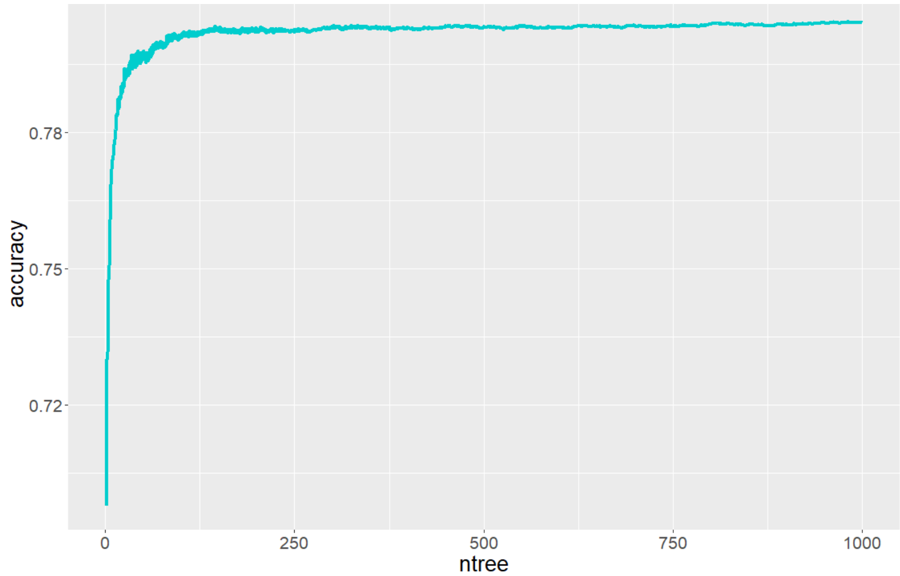


Figure B3: Random Forest OOB Error Rate over Number of Variables Used to Split at Each Tree Node

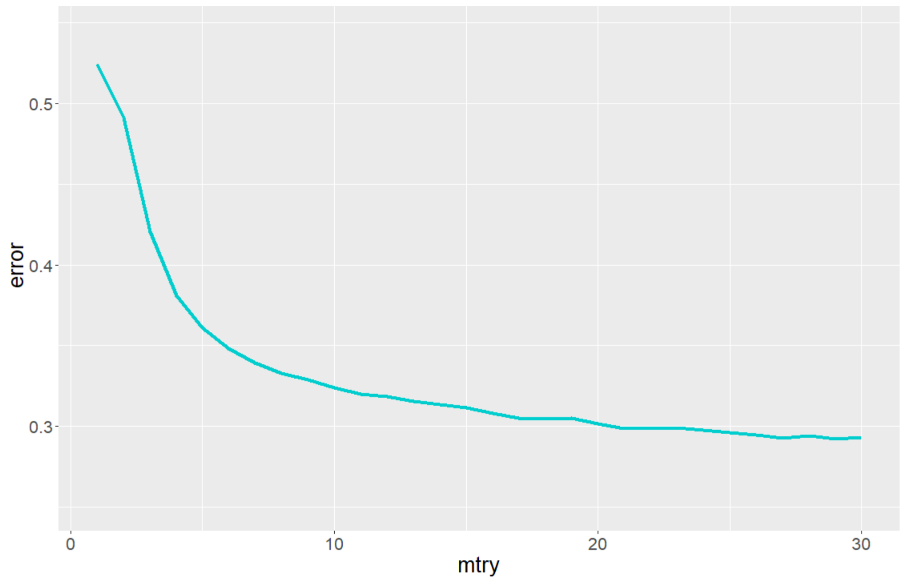
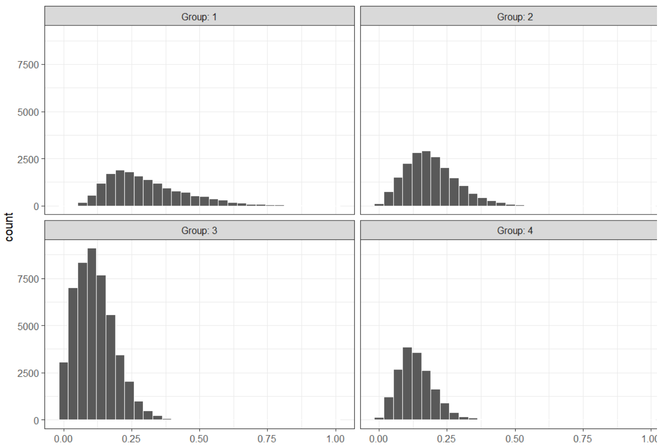
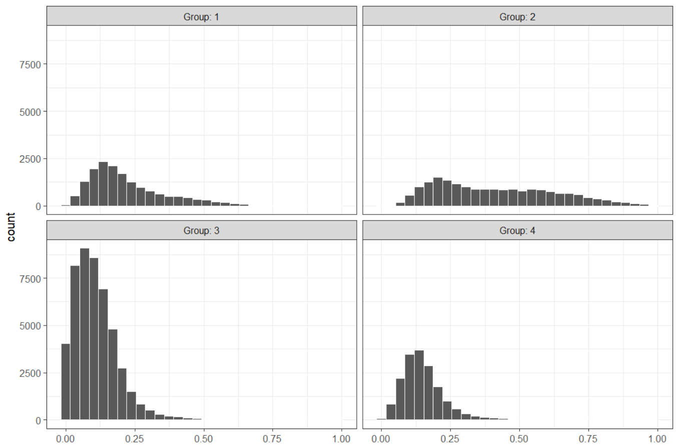


Figure B4: Distribution of the Predicted Probability to Belong to Groups 1 to 4 Given the Observed Covariates by Group

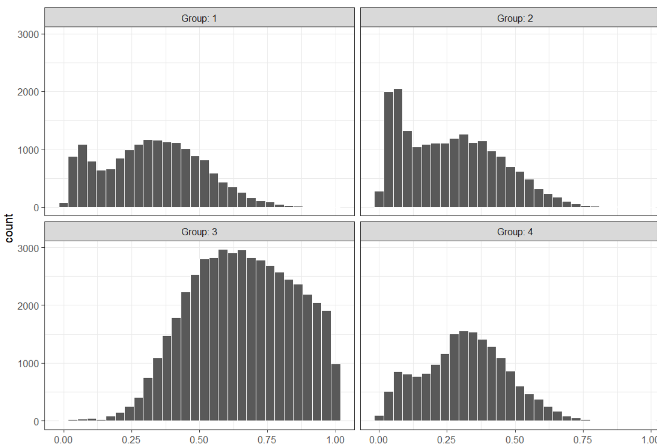
(a) Predicted probability to belong to group 1



(b) Predicted probability to belong to group 2



(c) Predicted probability to belong to group 3



(d) Predicted probability to belong to group 4