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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 channels resources geographically within countries. We utilize enterprise surveys spanning many low and middle income countries, and exploit transitions of national political leaders for identification. We document strong evidence for regional favoritism among firms located close to current leader's birthplace, but not in other regions, nor in home regions before a leader takes office. Firms in favored regions become substantially larger in terms of sales and employment. They also increase their sales per worker, pay higher wages, and have higher measured total factor productivity. Several mechanisms suggests that leaders divert public resources into their home regions by generating higher demand for firms operating in the non-tradable sector. A simple structural model of resource misallocation that is calibrated to match our empirical estimates implies that favoritism generates aggregate output losses of 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). The literature has hypothesized that lower income and less democratic countries chronically suffer from various types of distortive policies, which presumably widen the income gap between high and low income countries.

Our aim is to shed light on whether regional favoritism should be viewed as a policy failure that necessarily leads to more divergent 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. We remain agnostic regarding the normative mechanisms of favoritism at play. On the one hand, favoritism is likely to diminish welfare if leaders misallocate the factors of production to unproductive firms and regions, 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 survey data from a maximum of 125,000 enterprises in 120 low and middle income countries and utilize transitions of national political leaders for identification (see the geography of the firms and leaders in Figure 1). Our first contribution is to document the existence of strong regional favoritism in firm outcomes. Firms located in the home regions of current political leaders are larger in terms of their sales and the number of employees than firms located in other regions. Exploiting information on the exact geo-location of firms, we show that these effects of favoritism are strongest in a 10 km radius around a leader's birthplace, and that the effects diminish by distance. In our baseline specification, we find that favored firms located within about a 50 km radius of the

leader's birthplace have 22% higher sales and 13% more employees compared to control firms. For an average firm, these effects translate into $1.5 million more in sales and 10 additional employees. Our placebo analysis does not find evidence for pre-trends, suggesting that the causality likely runs from leader changes to firm outcomes.

We then exploit the richness of our enterprise survey data and study the mechanisms that lead to such 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. This evidence is consistent with the interpretation that regional favoritism may be an efficient policy. However, several additional pieces of evidence speak against this hypothesis. First, our results indicate that the effects are driven by the non-tradable sector. In the literature, episodes of rapid expansion of the non-tradable sector, relative to the tradable, are associated with the inflows of funds. These may be driven by natural resource booms, remittances, or borrowing, all of which increase the demand for non-tradable goods (see van der Ploeg 2011 for a comprehensive survey of the literature). By contrast, general productivity improvements should lead to more balanced growth in the two sectors. Second, and relatedly, we do not find evidence for higher export rates among manufacturing firms. Third, we find that the expansion of firms is partly fueled by direct government transfers in the form of more public procurement contracts. Fourth, the effects on firms are temporary, such that they cease almost immediately after a leader leaves office. Fifth, firms located in favored regions do not perceive any improvements in the business environment - if anything, they express an unfavorable view of the available infrastructure and quality of the workforce. Overall, these results are consistent with the interpretation that leaders divert public resources to their home regions, thus 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 third and 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 an economy with spatial wedges driven by favoritism, output is 0.5% lower compared to a distortion free economy. The intuition behind this result is that the redistribution between regions increases incomes in the home region and thus demand. The demand for non-tradable goods can be satisfied only by the local production, while the demand for tradable goods can also be met through imports from the other region. Therefore, factors of production will reallocate towards the non-tradable sector in the leaders' home region and the tradable sector in the non-home region. A higher concentration of labor in sectors decreases the marginal productivity of firms and results in aggregate losses.

This paper is related to two strands of literature. First, we contribute to the evolving literature on regional favoritism. i Miquel et al. (2007) were one of the first to lay down the theoretical framework for favoritism, while Hodler and Raschky (2014) were one of the first to document evidence for it. In particular, Hodler and Raschky (2014) use satellite data from across the globe and find a higher intensity of nighttime light in the birthplaces of the countries' political leaders compared to other regions within countries. In a closely coupled strand of literature, de Luca et al. (2018), Dickens (2018) observe higher nighttime light intensity in political leaders' ethnic homelands. Relatedly, Amodio et al. (2019), Asatryan et al. (2021), Franck and Rainer (2012), Kramon and Posner (2016) find evidence for improved human capital outcomes (e.g. in health, education and labor-markets) among individuals belonging to either the same ethnicity or coming from the same region as those holding political power. Several papers extend this work on ethno-regional favoritism to various sets of policies, such as 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.

Second, our paper relates to an important strand in the literature on how the misallocation of factors of production leads to substantial differences in aggregate total factor productivity. This literature goes back to Hsieh and Klenow (2009, 2010), Restuccia and Rogerson (2008), and is surveyed by Hopenhayn (2014), Restuccia and Rogerson (2017). In this context several studies have used the enterprise survey data to estimate aggregate output losses caused by various institutional frictions (Besley and Mueller 2018, Ranasinghe 2017). Our contribution is to highlight a new source of misallocation that is driven by regional favoritism and which is caused by the endogenous concentration of production factors in opposite 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 two types of firms (private and state-owned). 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.

2 Empirical design

2.1 Data

Firms Our firm-level data are drawn from the World Bank Enterprise Surveys, which have been administered using a global methodology since 2006. The full sample of these surveys spans over 140 countries; however only 98 countries have been surveyed more than once. In these countries, survey waves are typically carried out in two to five year intervals, leading on average to 2.5 survey waves per country. Surveys cover the non-agricultural formal private sector, thus excluding firms which are fully government owned, are informal, or are classified as agricultural firms according to ISIC revision 3.1. Firms are drawn by stratified random sampling, with stratification performed based on firm size, geographic location within the country, and sector of activity. Furthermore, firms are required to have five or more employees.

The number of firms sampled in a given country varies with the size of its economy. Large, medium and small economies host, respectively, 1200-1800, 360 and 150 interviews.[1]

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 favoritism's effects and channels in greater detail.

For the main part of our empirical analysis we consider the sub-sample of surveys carried out since 2009, as they provide us with geocoded data on the location of firms.[2] In additional specifications we use the general sample, and we identify the location of firms according to administrative regions. However, there are occasionally changes in the definition of regions between survey waves. Therefore, 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 larger sample to test the robustness of our baseline findings.

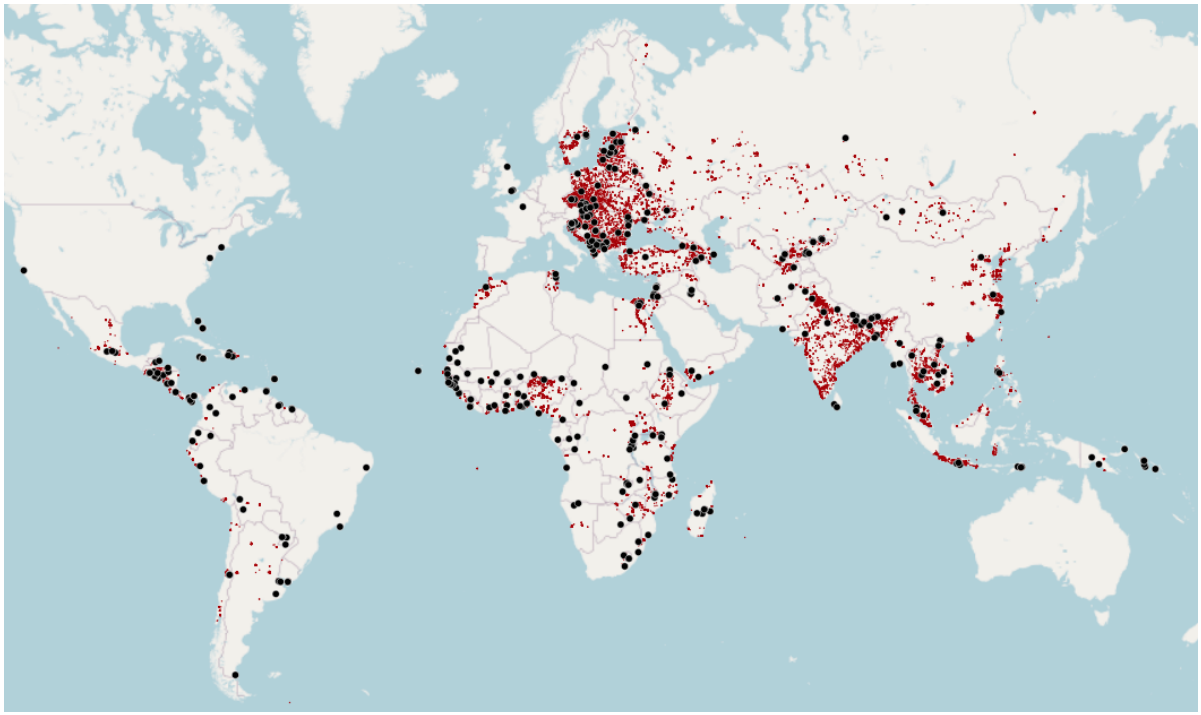
In total there are around 100,000 and 150,000 enterprise surveys carried out in the geocoded and regional samples, respectively. However, the key variables we use have missing values to a varying degree. Additionally, to alleviate bias in our estimates from outliers, we exclude values that are outside three standard deviations of the calculated mean within an industry and country income level. For our baseline analysis this leaves us with 80,000 to 58,000 firm-level observations, depending on the outcome we study. In the regional specification we have between 140,000 to 105,000 observations. Figure 1 presents the geography of firm locations, and Table A1 lists the countries and survey waves in our sample.

Political leaders To identify political leaders in power we use the Archigos database of political leaders (version 4.1). The database includes information on the start and end date of the primary effective leader's time in power. Archigos data are available up to 2015 and we

[1] The size of the economy is determined by gross national income. Further information on the sampling and stratification procedure can be found in the Enterprise Survey and Indicator Surveys sampling methodology available at https://www.enterprisesurveys.org/en/methodology.

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

Figure 1: Leader's Birthplaces and Firms in Our Sample



Notes : This map presents the geography of our sample and of the identifying variation. The small red dots represent firms. The large black dots represent circles of 100 km radii around national leader's birthplaces. Table A1 presents the list of countries and survey waves in our sample.[3]

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 calculate the distance of every firm to each leader in the sample period. In the total sample we generate a dummy indicating whether a firm is within a leader's region. In the general sample we have a total of 250 leaders coming from 120 countries. The leader regions are plotted in Figure 1. Since our empirical strategy builds on leader transitions,

[3]For our main sample there are around 25,000 African, 40,000 Asian, 20,000 European, 6,000 Middle American and 10,500 South American firms available.

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.

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 Polity5 project, as well as data on corruption perception indicators from Transparency International.

Tradable and non-tradable sector On a general level, the enterprise surveys identify whether firms belong to the service or manufacturing sectors. At a more granular level, firms report the ISIC category of their main product or service. We exploit this information to construct a measure of product tradability, so as to categorize firms into either the tradable or non-tradable sector. We 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. This exercise leads to a total of around 26,500 tradable and 75,000 non-tradable firms in our geocoded sample. 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

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

reclassify manufacturing sectors with very high trade costs, such as bricks, as belonging to the non-tradable category.

Sample and summary statistics Table A1 of the appendix presents a detailed description of our sample. The Table lists countries, years, number of firms, and leaders in our sample, as well as which countries contribute to the identification of the favoritism effect. In the Figure 1 map, we also visualize data on firms and leaders' birth regions. Table A2 in the appendix shows the summary statistics of the variables used in this paper.

2.2 Identification

Empirical strategy Our empirical strategy exploits data on leader transitions and firm locations for identification. 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 measures the location of firms either by the exact geocoordinates of the firm or by the administrative region of its 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 the birthplace of the leader. However, this comes at the cost of losing identifying variation from the longer sample period. We start by studying firms whose exact geo-locations is available, where we can identify effects at granular distances. To obtain complementary evidence, we then replicate this exercise with the larger sample.

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

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

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.[5] 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.

The β^{km} is our main coefficient of interest. It is identified by the set of dummy variables $LeaderArea^{km}_{l,c}$, which set firms to be treated if they are located within a km kilometer radius to the birth town 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 $\epsilon_{f,i,r,c,t}$ which we two-way cluster at the level of country-sector-year and leader area 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-level data source and similar time and spatial dimensions as our paper.

[5] 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.

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.

Identifying assumptions Our difference-in-differences model compares firms located within areas or regions around the leader's birthplace before and after the leader comes to power while controlling for firms belonging to the same industries but located further away from leader's birthplace. The main identifying assumption in the 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 validate this assumption in Section 4.2 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 different from zero in the years leading to the nomination of the leader. This absence of 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 so that the average effect on the pre-trends does not mask potentially offsetting trends. This evidence is consistent with previous work that has used regional level data to study patterns of regional favoritism and, similar to our test, finds evidence against the existence of pre-trends.

3 Micro evidence

3.1 Results by distance

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 from 10 km to 100 km around the leader's birthplace in 5 km intervals. In this preliminary 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.

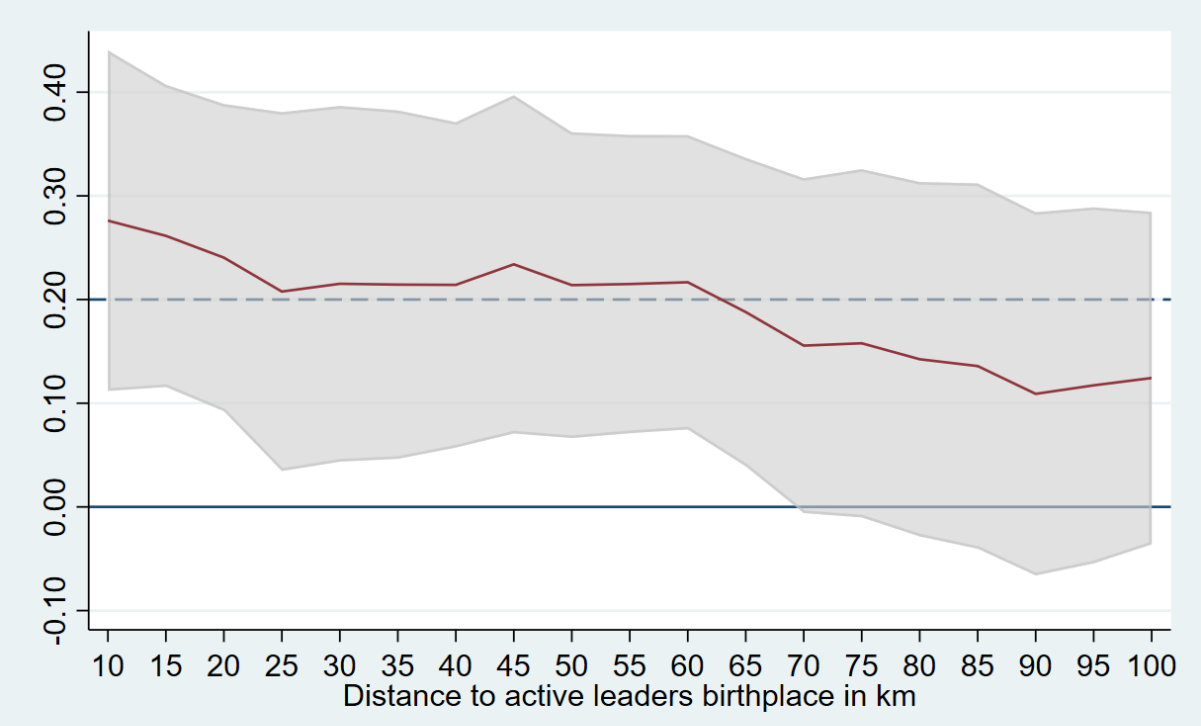
Figure 2 plots the treatment effects of favoritism by distance to the leader's birthplace. The effects are strongest in areas very close, with firms located in a circle of 10 km around the leader's birthplace having on average nearly 30% higher sales than similar firms located further away. These effects decrease by distance and become indistinguishable from zero beyond 70 km from the leader's birthplace.

The magnitudes of these 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 $25 billion (in 2009 nominal USD). Hodler and Raschky (2014) calculates that leader's regions on average have 1% higher GDP in the worldwide sample, but the effects can reach up to 9% in certain subsamples. 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[6] is 11% when transformed into GDP growth values.[7]

[6]To be more comparable with Hodler and Raschky (2014), in this back of the envelope calculation we take the coefficient estimated for regions from Table 2 rather than the coefficient estimated for certain radii around leader's birthplaces, as in Figure 2.

[7]Following Hodler and Raschky (2014) and other papers in this literature, we study whether the effects of favoritism are different across countries with different political institutions. In particular, in Table A4 we interact our treatment effect with the polity score of democracy and a measure of corruption perceptions. To study potential non-linear effects we also interact with the squared values of these indices. Overall, we do not

Figure 2: Treatment Effects by Distance to Leader's Birthplace



$Notes$: 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.

3.2 Baseline results

For ease of presentation, we define a baseline area of treatment around the leader's birthplace and in the rest of the paper present our estimates based on this area, rather than having to estimate dozens of point effects over distance for each outcome variable. We choose the baseline treatment area to include firms located within a 50 km radius around the leader's birthplace. We do not use smaller radius measurements in the baseline given the trade-off that we would lose firm observations and therefore statistical power. Also, focusing on a

find evidence in our sample that democracy or corruption either constrain or exacerbate the effects of regional favoritism. We relay this difference to Hodler and Raschky (2014) on the differences in the sampled countries, as our selection of mostly low- and middle-income countries is closer to Dickens (2018), who similarly does not document significant effects in this dimension.

Table 1: Treatment Effects around Leader's Birthplace

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 leader's birthplace. Dependent variables are specified in logarithms. The mean values of the dependent variables in levels are 6.8 million USD in columns 1-2, 78 employees in column 3, 7423 USD in column 4, and 104,000 USD in columns 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.

small radius may allow us to obtain large estimates, but its aggregate implications on the economy will be relatively inconsequential. On the other hand, we do not use larger circles as, according to Figure 2, the treatment effect would start to decline. This choice of fixing the baseline treated area to a 50 km radius is necessarily a selective one. This choice in general will not matter for the direction of the effects that we identify. While the magnitudes that we identify may be somewhat affected, our results and interpretation are robust to specifying other distances.

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 highly significant and implies that firms located close to the leader's hometown 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 $1.5 million and an employment increase of nearly 10 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 we find that treated firms pay higher wages and produce more output per capita. Finally, column 6 of Table 1 shows that treated firms not only grow in size, but also become more productive, as measured in terms of total factor productivity.

4 Robustness checks

4.1 Region level results

As discussed in Section 2.2, we prefer to work with data containing information on the geolocation of firms. However, for a quite larger sample of firms, our data only indicates 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 region of birth rather than 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 can be replicated.[8]

4.2 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.

[8] In an additional specification we interact the region treatment with the 50 km area treatment. Table A3 of the appendix shows the results. Not surprisingly we find strongest effects on firms that are located within a 50 km radius from leader's birthplace and at the same time belong to the leader's birth regions.

Table 2: Treatment Effects in Leader's Birth Region

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.

For this reason we construct a placebo treatment variable by assuming that the leadership transition took place up to two years earlier than it actually happened. In a similar spirit we also create a treatment variable that takes a value of one for the period covering up to two years after the leadership transition. We then re-estimate equation (1) including these leads and lags. The results are presented in Table 3. As can be seen, neither leads nor lags have significant effects on sales or employment.[9]

Additionally, the fact that the firm growth effect disappears after the leader leaves office implies no evidence for the "big push" hypothesis. According to this hypothesis, large positive shocks and investments can help firms to permanently change their growth trajectories (Murphy et al. 1989), as demonstrated in some recent papers, such as Kline and Moretti (2013),

[9]It has to be stated that our data are not well equipped to investigate this issue in more detail due to the limited frequency with which we obtain the firm-level data. For example, our data also make it difficult to study tenure (the literature has documented increasing effects with longer tenures). Variation in tenure would be across countries or leaders rather than between different levels of tenure for the same leader, thus prohibiting causal interpretation.

Table 3: Treatment Effects Before and After Leader Transitions

VARIABLES	Log Sales	Log Sales	Log Employees	Log Employees
0-2 years before treatment	-0.0697		0.0208	
	(0.2597)		(0.2275)	
0-2 year after treatment		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 leader's birthplace. 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.

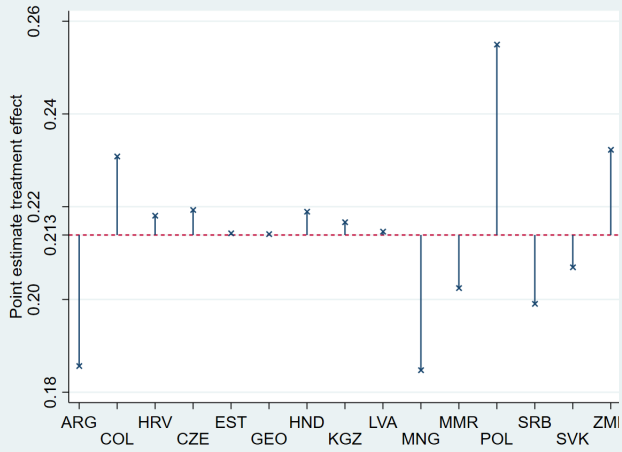
who provide evidence that regional development policies in the US have long term effects, and Lu et al. (2019), who study China's successful implementation of Special Economic Zones.

4.3 Country-by-country dropping

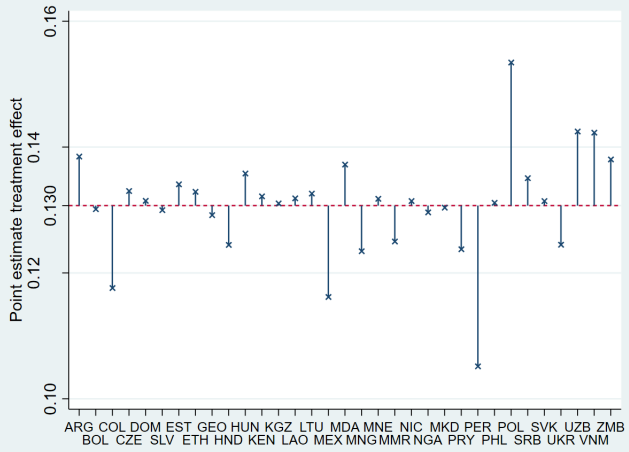
We are interested in determining whether the average effects we find are driven by strong favoritism effects emanating from individual countries. To this end, we re-estimate equations 1 and 2, but successively drop countries with identifying leader transitions one at a time. Decreases in our coefficient of interest indicate that the excluded country experienced a

Figure 3: Changes to the Average Treatment Effect from 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.

stronger than average effect compared to the remaining countries, and vice versa. The results are visualized in Figure 3.

The Figure shows that changes to the average effect are generally small. The largest increase in both samples is driven by Poland being dropped from the regression, indicating that the favoritism effect for Poland is below the other countries' average. On the other hand, the largest decrease in the geocoded sample stems from dropping Mongolia, and in the regional level sample from dropping Peru. Size-wise the largest differential is below five percentage points in specification 1, and below three percentage points in specification 2. Thus, we can comfortably rule out that our findings are driven by individual countries.

5 Mechanisms

5.1 Sectoral results

We specifically investigate how regional favoritism affects the sectors of the economy, in order to shed light on a central mechanism behind our baseline results. To this end, we split firms into the tradable and non-tradable sector. As we discuss in Section 6, we expect redistributive policies implemented by the government to affect these two sectors differently, which is consistent with recent findings by Besley et al. (2021), showing 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 inflows of funds to developing countries from commodity booms, remittances, international aid, or borrowing. Such inflows increase household incomes, thus boosting consumption. The increased demand for tradable goods can be met by imports, while demand for non-tradable goods can only be satisfied with domestic production. Such episodes lead to relative increases in the prices of non-tradable goods (exchange rate appreciation), the reallocation of factors of 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 4 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 tradable sector firms located around the leader's birthplace benefit less from favoritism. Further, the results in column 2 imply that they do not experience any growth in output per worker. Column 3 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 4 we observe that wage growth is similar in both sectors. This is consistent with the idea that there is high level of mobility between

Table 4: Treatment Effects by Sector

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

$Notes$: The regressions are estimated based on equation 1 but including an interaction term between treatment and sectors. The treatment is set to a 50km radius around leader's birthplace. 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.

sectors. And despite the fact that non-tradable firms experience more growth, wage demands faced by firms in both sectors are similar, because both sectors compete for similar workers. In column 5 we document that there are no sectoral differences in employment growth.

5.2 Business environment

Next, we try to identify the policies and tools used by leaders to contribute to firm growth in their region. The enterprise surveys ask questions regarding the constraints that firms face while doing business. Firms are asked to evaluate certain obstacles to their business on a five-point Likert scale. We center and normalize these variables to report the results in terms

of standard deviations in Table 5. In the first column the dependent variable is the average of all business constraints. The estimated coefficient is positive and significant, indicating a worsening business environment. However, this measure does not indicate the specific source of the constraint. Accordingly in the following three columns we study its individual components. The results show that there is no change in the institutional environment around the leader's birthplace. Meanwhile, the estimated coefficients on infrastructure constraints and input constraints are positive and significant. This implies that firms operating in the areas around the leader's birthplace see deficiencies in terms of infrastructure and inputs to a lesser extent as significant 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 5. From these regressions we observe that firms around the leader's birthplace 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.

Taken together, these results imply that leaders divert resources to their home region and generate higher demand for output produced by firms in the area around their birthplace. However, they do not promote sufficient infrastructure development to keep up with the increasing needs of 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 favored firms. 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).

Table 5: Effects on the Business Environment around Leader's Birthtown

VARIABLES	(1) Average	(2) Infrastructure	(3) Institutions	(4) Input	(5) Land	(6) Finance	(7) Workforce
Treated area	0.1133*	0.1558***	0.0316	0.0979**	0.0879***	-0.0493	0.1919***
	(0.0615)	(0.0575)	(0.0787)	(0.0384)	(0.0264)	(0.0336)	(0.0483)
Firm age	-0.0011***	-0.0008***	0.0003	-0.0018***	-0.0025***	-0.0022***	0.0008***
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0003)
% owned foreign	-0.0006***	0.0007***	-0.0009***	-0.0013***	-0.0008***	-0.0024***	0.0005***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
% owned public	-0.0017**	-0.0014**	-0.0021***	-0.0012**	-0.0026***	-0.0005	0.0007
	(0.0007)	(0.0006)	(0.0007)	(0.0005)	(0.0005)	(0.0007)	(0.0007)
Constant	-0.0004	-0.0276**	-0.0080	0.0210*	0.0312***	0.0663***	-0.0590***
	(0.0154)	(0.0138)	(0.0191)	(0.0119)	(0.0102)	(0.0118)	(0.0140)
Observations	65,598	78,826	68,654	76,060	77,954	79,469	79,861
R-squared	0.3969	0.2924	0.3902	0.2806	0.2236	0.1947	0.2354
F	8.105	7.314	9.702	18.79	22.15	34.17	6.004

Notes : The regressions are estimated using equation 1. The treatment is set to a 50km radius around leader's birthplace. Dependent variables are indices that have been centered at zero and normalized with a variance of one. 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.

Regarding input constraints, the regressions 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 outstrips the supply of skilled workers. This is also consistent with increasing wage levels around the leader's birthplace, as presented, for example, 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.

5.3 Further mechanisms

In Table 6 we explore additional channels that may help us to better understand how regional favoritism works. First, we consider whether firms located in proximity to a leader's birthplace are more likely to secure government contracts. Governments may choose to selectively award contracts to favored firms. Our estimations confirm this hypothesis. We observe that firms in the treated area are 2.4% more likely to secure government contracts. In column 2 we observe that sales of firms in which the government has an ownership stake do not experience a differential increase. However, as indicated in column 3, there is a differential increase in employment at these firms. This provides further evidence for another mechanism through which the leader can redistribute resources to his or her home region using national resources. More specifically, those firms engage in hiring despite the fact that their sales are not increasing. This is inconsistent with the behavior of a profit maximizing firm, but since the government has a stake in those firms, it can induce sub-optimal choices. In column 4 of Table 6 we restrict our sample to tradable sector firms and study whether they experience an increase of the share of sales from exports. The results provide no such evidence. A positive and significant coefficient would indicate an improvement in competitiveness among firms located around the leader's birthplace because of improved infrastructure and public goods provision. However, since we did not observe such improvements in Table 5, it is rather intuitive that the exporting prospects of firms in the leader's region do not improve. In the following two columns, we study whether firms have introduced new products or processes. For new products we observe a positive and significant coefficient, while for new processes a negative one. Our interpretation is that higher consumer incomes can generate more demand and increase firms' incentives to introduce new products. However, this horizontal expansion does not necessarily imply improvements in efficiency.[10] As process innovations are more likely to be associated with improved efficiency, these results make intuitive sense.

[10]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.

Table 6: Evidence on Further Mechanisms

VARIABLES	(1) Gov. contract secured?	(2) Log sales	(3) Log employees	(4) Share sales direct exports	(5) New product last 3 years?	(6) New process last 3 years?	(7) Any informal payments?
Treated Area	0.0240***	0.2129***	0.1391**	0.7587	0.0244**	-0.0750***	-0.0504
	(0.0041)	(0.0750)	(0.0588)	(0.5011)	(0.0118)	(0.0031)	(0.0329)
Treated#% owned pub.		0.0062	0.0069*				
		(0.0051)	(0.0035)				
Firm age	0.0016***	0.0251***	0.0192***	0.0281	0.0010***	0.0008***	0.0001
	(0.0002)	(0.0021)	(0.0013)	(0.0231)	(0.0002)	(0.0002)	(0.0001)
% owned public	0.0020***	0.0165***	0.0143***	0.0012	-0.0001	-0.0001	-0.0001
	(0.0006)	(0.0047)	(0.0031)	(0.0322)	(0.0007)	(0.0007)	(0.0003)
% owned foreign	-0.0001	0.0171***	0.0102***	0.2772***	0.0009***	0.0006***	-0.0000
	(0.0001)	(0.0008)	(0.0005)	(0.0240)	(0.0001)	(0.0001)	(0.0001)
Constant	0.1470***	16.4071***	2.8025***	10.1210***	0.3527***	0.3908***	0.3004***
	(0.0033)	(0.0433)	(0.0273)	(0.5343)	(0.0047)	(0.0028)	(0.0085)
Observations	78,635	70,177	79,718	21,294	57,205	55,932	80,810
R-squared	0.1013	0.6660	0.2583	0.2330	0.2113	0.2944	0.2736
F	37.07	119.6	143.5	33.55	37.49	3818	0.707

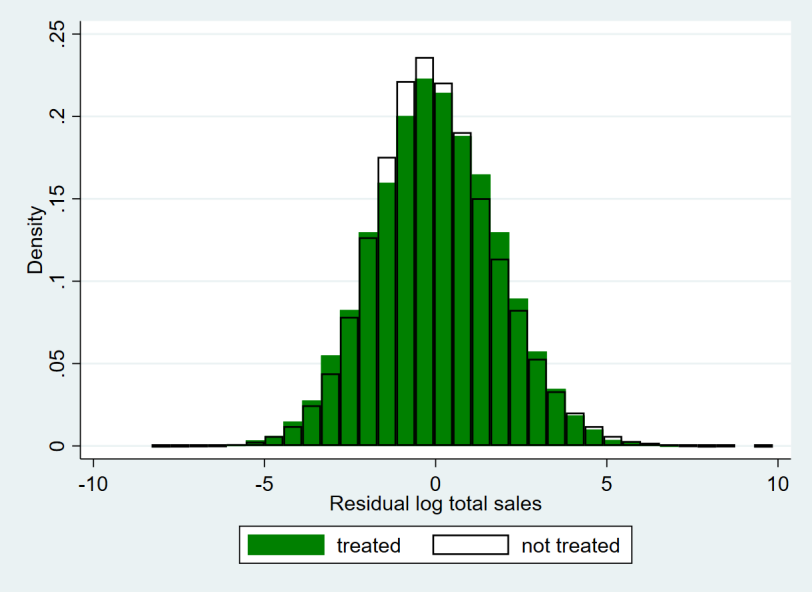
Notes : The regressions are estimated using equation 1. The treatment is set to a 50km radius around leader's birthplace. The mean values of the dependent variables from left to right are 17.8%, 6.8 million USD, 78 employees, 12.9%, 38.2%. 39.4% and 28.9%. All regressions include fixed effects for leader circles, regions, industries, and country-by-years. Standard errors are two-way clustered at the level of country-sector-year and leader region.

In the last column the dependent variable indicates whether firms have reported making informal payments. The estimated coefficient is negative but it is not significant. A negative coefficient would imply that leaders reduce informal tax collection and provide better treatment to firms located in the areas around their birthplaces. These informal payments are one manifestation of policy distortions that have been discussed in the misallocation literature (Hsieh and Klenow 2009, Restuccia and Rogerson 2008).

5.4 Distribution of firm size

In addition to the average effects of favoritism identified thus far, we are also interested in whether favoritism differently affects the distribution of firms. Following Hsieh and Klenow (2009), in Figure 4 we present the histogram of the distribution of firms in terms of total sales by plotting the distribution of residuals using equation (1). We compare treated firms to

Figure 4: Size Distribution of Treated and Untreated Firms



$Notes$: The histogram plots the distribution of firms with respect to log sales for the treatment and control group.

firms in the control group, whereby the green bars represent the former, and the transparent bars represent the latter, in order to ease comparison. If the favoritism effects were to change the distribution of firms, we would expect to observe substantial divergence in the distribution mass of the two groups. As this divergence is minimal, our descriptive evidence does not indicate a differential effect of favoritism across the firm distribution.

To investigate this issue further, we construct a 95% confidence interval of the ratio of the above mentioned residuals standard deviations by bootstrapping. This allows us to detect whether there are statistically significant differences in the distributions between the groups. However, there is no such evidence, as the confident interval ranges from 0.979 to 1.004, critically including the value 1. These results inform our assumptions in the following section, in which we model homogeneous firms.

6 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.

6.1 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 for which we saw some tentative evidence in Table 6. 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 circles 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.[11]

6.1.1 Production

We consider a simple production function

[11] Our estimations include country-year fixed effects and exclude observations for circle-years during which the leader was in office from that circle.

$$Y_{ij} = L_{ij}^{\alpha}. \tag{3}$$

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. We do not introduce capital into the production function because our empirical results do not show any differential frictions in the capital market stemming from regional favoritism. Our empirical results are consistent with a high level of labor mobility between sectors (Table 4), and low mobility between regions (Table 1). Also in Table 5 we observed that firms do not face differential constraints to capital access. Thus, we do not add capital to keep the model more tractable.

The firm's optimization problem can thus be written as

$$(1 - \tau_i)p_{ij}Y_{ij} - w_i L_{ij}, \tag{4}$$

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 ($p_{hm} = p_{am} = 1$).

6.1.2 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

$$p_{is}C_{is} + C_{im} \leq w_i L_i \tag{5}$$

6.1.3 Market clearing

The equilibrium requires clearing in labor and goods markets

$$L_{hs} + L_{hm} = L_h, \quad L_{as} + L_{am} = L_a \tag{6}$$

$$C_{hs} = Y_{hs}, \quad C_{cs} = Y_{cs} \tag{7}$$

$$C_{hm} + C_{am} = Y_{hm} + Y_{am} \tag{8}$$

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

$$\tau_h(p_{hs}Y_{hs} + Y_{hm}) + \tau_a(p_{as}Y_{as} + Y_{am}) = 0. \tag{9}$$

6.2 Model discussion

The model yields several predictions that help us to 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.

$$\frac{\partial L_{is}}{\partial \tau_i} < 0. \tag{10}$$

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.

$$\frac{\partial w_i}{\partial \tau_i} < 0 \implies w_h > wf. \tag{11}$$

6.3 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 22% total output differences between regions and make sure that the government's budget constraint (9) is satisfied. This value is taken from column 2 of Table 1. Notice that the 22% 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 7 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 10%. The following column displays the relative change in prices of non-tradable goods. There is a 15% 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 4, we observed an increase in Y/L ratio only in the services sector. In our data, output is measured

Table 7: 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.

as price times quantity and we do not have information on physical output. However, in Table 5 and 6 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.

In the second row of Table 7 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 14%. 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/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.[12] 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, but without generating additional insights. [13]

7 Conclusions

Regional favoritism - that is, the geographic redistribution of wealth 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 at various levels, 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

[12] 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.

[13] 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.

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. While such productivity improvements could potentially lead to higher growth for the entire country, this conclusion is not supported by our subsequent analysis. In particular, our evidence shows that this evolution of firms in favored regions is driven by a rapid expansion of the non-tradable sector, rather than substantial growth among manufacturing firms. Direct transfers to firms through public procurement contracts are one channel for 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, since 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 generally harmful to the economy as a whole, and should thus be carefully considered.

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 and more careful studies could seek to disentangle the effects of these various policies. Second, we focus on leaders and ignore other systematically important national figures. Our focus is purely attributable to data constraints. It would be potentially interesting to study regional favoritism

in relation to other government figures, e.g. in parliament or the judiciary. 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-difference framework assuages such concerns, our study very much remains a first pass. Fourth, we neglect the potential impact of favoritism on the entry, exit, and relocation of firms. While our survey data are not well equipped to explore this question, it remains an interesting topic for future research, which perhaps is able to consolidate larger data sets from censuses or administrative sources.

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Appendix

Table A1: Sample description

Country	Year	# firms	# leaders
Afghanistan	2008	535	1
	2013	410	
Albania	2006	304	2
	2011	360	
	2017	5	
	2018	372	
Angola	2005	425	1
	2009	360	
Argentina*	2005	1063	3
	2009	1054	
	2016	663	
	2017	328	
Armenia	2007	374	2
	2011	360	
Azerbaijan	2007	380	1
	2011	390	
Bahamas	2009	150	1
Bangladesh	2012	1442	1
Barbados	2009	150	1
Belarus	2007	273	1
	2011	360	
	2017	354	
	2018	246	
Belize	2009	150	1
Benin	2015	150	1
Bhutan	2014	253	1
Bolivia**	2005	613	2
	2009	362	
	2015	11	
	2016	351	
	2017	2	
Botswana	2005	342	2
	2009	268	
Brazil	2007	1802	1
Bulgaria	2006	1015	5
	2007	288	
	2011	293	
	2017	26	
	2018	704	
	2019	42	
Burkina Faso	2008	394	1
Burundi	2005	270	2
	2013	157	
Cambodia	2015	373	1

Continued on next page

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

Table A1 –continued from previous page

Country	Year	# firms	# leaders
Cameroon	2008	363	1
	2015	361	
Chad	2017	153	1
Chile	2005	1017	2
	2009	1033	
China	2011	2700	1
Colombia*	2005	1000	2
	2009	942	
	2016	758	
	2017	235	
Costa Rica	2009	538	1
Croatia†	2006	633	3
	2011	360	
	2017	2	
	2018	402	
Czech Republic*	2007	250	5
	2011	22	
	2012	232	
	2017	9	
	2018	423	
	2019	70	
Côte d'Ivoire	2007	526	2
	2015	361	
DRC	2005	340	1
	2009	359	
	2012	529	
Djibouti	2012	266	1
Dominican Republic**	2009	360	2
	2015	359	
Ecuador	2005	658	3
	2009	366	
	2016	361	
Egypt	2012	2897	3
	2015	1483	
	2016	331	
El Salvador**	2005	693	3
	2009	360	
	2015	719	
Estonia*	2007	273	3
	2011	273	
	2017	71	
	2018	286	
	2019	3	
Eswatini	2005	307	1
	2015	150	
Ethiopia**	2011	644	2
	2014	848	
Gambia	2005	174	2
	2017	150	
	2018	1	
Georgia*	2007	373	4
	2011	360	

Continued on next page

† Identifying variation in geocoded sample only.

Table A1 –continued from previous page

Country	Year	# firms	# leaders
	2018	533	
	2019	48	
Ghana	2006	494	3
	2012	720	
Guatemala	2005	522	4
	2009	590	
	2016	345	
Guinea	2005	223	3
	2015	150	
Guinea Bissau	2005	159	2
Guyana	2009	165	1
Honduras*	2005	436	4
	2009	360	
	2015	332	
Hungary**	2007	291	3
	2011	310	
India	2013	9281	1
Indonesia	2008	1444	2
	2014	1320	
Iraq	2010	756	1
Israel	2012	483	1
Jamaica	2009	376	1
Jordan	2012	573	1
	2017	11	
	2018	590	
Kazakhstan	2007	544	1
	2011	600	
	2018	1446	
Kenya**	2006	657	2
	2012	781	
	2017	727	
	2018	274	
Kosovo	2007	269	4
	2011	202	
	2017	1	
	2018	270	
Kyrgyz Republic*	2007	235	4
	2011	270	
	2017	7	
	2018	353	
Lao PDR**	2008	360	2
	2011	270	
	2015	368	
	2017	314	
	2018	18	
Latvia†	2007	271	6
	2011	336	
	2017	58	
	2018	289	
	2019	12	
Lebanon	2012	561	3
	2017	4	

Continued on next page

Table A1 –continued from previous page

Country	Year	# firms	# leaders
	2018	528	
Lesotho	2015	150	2
Liberia	2016	151	1
Lithuania**	2007	276	2
	2011	270	
	2017	32	
	2018	324	
	2019	2	
Madagascar	2007	445	2
	2012	532	
Malawi	2013	523	1
Malaysia	2014	1000	1
Mali	2006	490	3
	2009	360	
	2015	185	
Mauritania	2005	237	4
	2013	150	
Mexico**	2005	1480	2
	2009	1480	
Moldova**	2007	363	5
	2011	360	
	2018	359	
	2019	1	
Mongolia*	2007	362	3
	2011	360	
	2017	11	
	2018	349	
Montenegro**	2007	116	5
	2011	150	
	2017	2	
	2018	148	
Morocco	2012	407	1
	2018	1096	
Mozambique	2006	479	2
	2017	494	
	2018	107	
Myanmar*	2012	632	2
	2015	198	
	2016	409	
Namibia	2005	329	2
	2013	580	
Nepal	2008	368	3
	2012	482	
Nicaragua**	2005	478	2
	2009	336	
	2016	333	
Niger	2016	151	1
Nigeria**	2006	1891	3
	2013	2676	
North Macedonia**	2007	366	3
	2011	360	
	2017	11	

Continued on next page

Table A1 –continued from previous page

Country	Year	# firms	# leaders
	2018	349	
Pakistan	2012	1247	1
Panama	2005	604	2
	2009	365	
Papua New Guinea	2014	65	1
Paraguay**	2005	613	4
	2009	361	
	2016	364	
Peru**	2005	632	4
	2009	1000	
	2015	10	
	2016	892	
	2017	101	
Philippines**	2008	1326	2
	2014	1335	
Poland*	2007	455	3
	2011	542	
	2018	1369	
Romania	2007	541	2
	2011	540	
Russia	2007	1004	1
	2010	4220	
	2017	1	
	2018	1322	
Rwanda	2005	212	1
	2018	134	
	2019	226	
Senegal	2006	506	2
	2013	601	
Serbia*	2007	388	5
	2011	360	
	2017	13	
	2018	348	
Sierra Leone	2016	152	1
Slovak Republic*	2007	275	4
	2011	54	
	2012	214	
	2017	8	
	2018	415	
	2019	6	
Slovenia	2007	276	6
	2011	270	
	2017	16	
	2018	393	
Solomon Islands	2014	151	2
South Africa	2006	937	1
South Sudan	2013	738	1
Sri Lanka	2010	610	1
Sudan	2013	662	1
Suriname	2009	152	2
	2017	228	
	2018	5	
Sweden	2013	600	1

Continued on next page

Table A1 –concluded from previous page

Country	Year	# firms	# leaders
Tajikistan	2007	360	1
	2011	359	
	2018	352	
Tanzania	2005	419	2
	2012	813	
Thailand	2015	1000	1
Timor-Leste	2014	126	1
Togo	2015	150	1
Trinidad and Tobago	2009	370	1
Tunisia	2012	592	1
Turkey	2007	1152	1
	2011	987	
	2012	357	
	2017	354	
	2018	1309	
Uganda	2005	563	1
	2012	762	
Ukraine**	2007	851	3
	2011	1002	
	2017	2	
	2018	1335	
Uruguay	2005	621	4
	2009	607	
	2015	1	
	2016	257	
	2017	89	
Uzbekistan**	2007	366	2
	2011	390	
	2018	1239	
Venezuela	2009	320	1
Vietnam**	2008	1053	2
	2014	996	
Yemen	2009	477	2
	2012	353	
Zambia*	2006	484	4
	2012	720	
	2018	108	
	2019	491	
	2020	2	
Zimbabwe	2015	540	1
	2016	60	

Table A2: Summary Statistics of Geocoded Sample

	N	Mean	Std. Dev.	p5	p95
Treated area	99965	.19	.39	0	1
Treated region	99965	.16	.37	0	1
0-2 years before treatment	99965	.02	.16	0	0
0-2 year after treatment	99965	.03	.17	0	0
Total sales in 2009 USD	86361	6869380	58078514	12259	22468666
Num. full-time employees	99081	78	217	5	310
Output per employee in 2009 USD	85963	104490	1561689	1185	257811
Wage in 2009 USD	82023	7423	54011	201	23359
TFP residual	71427	-.01	1.34	-1.79	2.33
Log cost of input factors	86677	15.89	3.38	10.8	21.58
Log value machinery/land/buildings	83595	7.21	8.29	0	20.21
Firm age	98676	18.65	15.36	3	48
Firm % owned private foreign	98666	6.84	23.35	0	85
Firm % owned public	98711	.65	6.47	0	0
Average of all constraints	80509	31.64	20.49	1.67	68.33
Infrastructure constraints	97277	33.84	28.16	0	87.5
Institutional constraints	84227	30.32	22.65	0	70
Input constraints	93761	30.19	23.04	0	75
Obstacle land	96199	24.5	31.43	0	100
Obstacle finance	97983	34.15	31.98	0	100
Obstacle inadequately educated workforce	98430	31.86	31.24	0	100
Attempted or secured government contract?	96976	.18	.38	0	1
Firm directly/indirectly exports?	98443	.24	.43	0	1
New product/service over the last 3 years?	93843	.36	.48	0	1
New/improved process over the last 3 years?	92180	.36	.48	0	1
Any informal payments made?	99965	.29	.46	0	1
Polity2 score	97590	4.14	5.68	-7	10
Estimate for control of corruption	99071	-.47	.61	-1.25	.75
Leader tenure in years	99965	7.63	7.24	1	25

Table A3: 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***	0.2246***	0.1516**	0.0939**	0.0917***	0.0561***
	(0.0890)	(0.0799)	(0.0614)	(0.0464)	(0.0177)	(0.0169)
Treated area not in leader admin region	0.0097	0.1277	0.0531	0.0827	0.1253*	-0.0199
	(0.0901)	(0.0847)	(0.0795)	(0.0512)	(0.0744)	(0.1115)
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 government		0.0174***	0.0153***	-0.0001	0.0016	0.0048***
		(0.0042)	(0.0029)	(0.0021)	(0.0014)	(0.0015)
Constant	17.0141***	16.4137***	2.8091***	11.6471***	13.5839***	-0.1286***
	(0.0182)	(0.0429)	(0.0273)	(0.0166)	(0.0203)	(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 A4: Treatment Effects by Institutional Setting

VARIABLES	Log Sales	Log Sales	Log Sales	Log Sales
Treated area	-0.1332	0.3030	0.2129***	0.2098
	(1.1510)	(2.6314)	(0.0809)	(0.1305)
Treated#Polity2	0.4048	-0.6969		
	(1.3118)	(6.9148)		
Treated#Polity2 squared		0.6841		
		(4.6095)		
Treated#Control of Corruption			-0.1032	-0.1076
			(0.1180)	(0.2100)
Treated#Control of Corruption squared				0.0073
				(0.1777)
Firm age	0.0248***	0.0248***	0.0250***	0.0250***
	(0.0021)	(0.0021)	(0.0021)	(0.0021)
% owned foreign	0.0174***	0.0174***	0.0170***	0.0170***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
% owned public	0.0172***	0.0172***	0.0173***	0.0173***
	(0.0042)	(0.0042)	(0.0042)	(0.0042)
Constant	16.4683***	16.4577***	16.4156***	16.4152***
	(0.0543)	(0.0762)	(0.0434)	(0.0445)
Observations	68,375	68,375	69,422	69,422
R-squared	0.6633	0.6633	0.6671	0.6671
F	119.4	99.50	103.2	88.24

$Notes$: The regressions are estimated using equation 1. 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.

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