The Costs of Political Influence: Firm-Level Evidence from Developing Countries[1]

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Abstract

Arrangements by which politically connected firms receive economic favors are a common feature around the world, but little is known of the form or effects of influence in business-government relationships. We present a simple model in which influence requires firms to provide goods of political value in exchange for economic privileges. We argue that political influence improves the business environment for selected firms, but restricts their ability to fire workers. Under these conditions, if political influence primarily lowers fixed costs over variable costs, then favored firms will be less likely to invest and their productivity will suffer, even if they earn higher profits than non-influential firms. We rely on the World Bank's Enterprise Surveys of approximately 8,000 firms in 40 developing countries, and control for a number of biases present in the data. We find that influential firms benefit from lower administrative and regulatory barriers (including bribe taxes), greater pricing power, and easier access to credit. But these firms also provide politically valuable benefits to incumbents through bloated payrolls and greater tax payments. Finally, these firms are worse-performing than their non-influential counterparts. Our results highlight a potential channel by which cronyism leads to persistent underdevelopment.

Arrangements by which firms with close ties to incumbent political authorities receive favors that have economic value are a pervasive feature of business-government relationships in countries around the world. Despite the prevalence of these arrangements, however, relatively little is known about the precise form firm-level political influence takes, or its consequences. What characterizes the bargain between influential firms and governments? How do influential firms compensate governments, if at all, for any benefits they receive? Recent firm-level analyses have examined various determinants of political influence, and how these connections affect market valuation. Others have detailed the channels through which the benefits accrue. Still other, finally, have explained how "systems" of influence come into being, and why they survive. Much less is known, however, of how these political connections affect decisions within firms or of the strings that may come attached to political influence.

We investigate both the characteristics that define political influence among firms in developing countries as well as the effects of that influence on company behavior and performance. We argue that political influence improves the business environment for selected firms through industrial or quasi-industrial policies, but restricts their ability to fire workers. Influential firms thus relinquish a portion of their control rights—particularly over employment decisions—in order to provide benefits of political value to public officials. If influence lowers fixed operating costs for privileged firms, they may earn higher profits than non-influential firms but they will be less likely to invest or innovate, and their productivity will suffer. Firm-level political influence, therefore, can undermine the performance of politically-powerful firms.

We draw on firm-level surveys in approximately 40 developing countries, consisting of over 8,000 enterprises. We find that politically influential firms do indeed face a more favorable business environment than their non-influential counterparts across several dimensions.

However, influential firms also tend to carry bloated payrolls and report more (hide less?) of their sales to tax authorities, suggesting two mechanisms by which they offer political compensation: employment levels and tax revenues. Influential firms are also less likely to open new product lines or production facilities, or to close obsolete ones; they also report lower real growth in sales, shorter investment horizons, and lower productivity levels than non-influential firms. These results are robust to adjustments for a number of biases in the survey data. Taken together, our results imply that firm-specific industrial policy will be more prone to cronyism than policies that do not target individual firms. Our results can also explain why crony capitalism persists in countries despite its adverse effects on long-term economic performance. Finally, our findings offer some confirmation for the view that politically-devised restrictions that block access to technologies and preserve rents for elites are at the heart of prolonged economic under-development.

Political Influence in Business-State Relations

Three sets of questions must be addressed in order to assess the characteristics and effects of firm-level political influence: (i) what benefits do influential firms receive? (ii) what benefits do politicians receive?; and (iii) what are the economic consequences of political influence? For the first question, arrangements by which political authorities grant favors to influential economic agents that allow these agents to earn above-market returns has been documented in case studies and some cross-national analyses. On the other hand, little empirical investigation has been conducted regarding the last two questions.

Benefits to Firms

The specific nature of relationships of influence varies from country to country. Studies of US campaign finance, political action committees, and the revolving door between lobbying firms and congressional staff offices, have typically identified the ties that politically-influential US firms can forge with specific political figures (Agrawal and Knoeber 2001; Ang and Boyer 2000, Krozner and Stratmann 1998). In developing nations, political influence is usually obtained through a combination of kinship ties, political alliances, ethnic solidarity, or financial dealings between owners and political elites. One effect of these connections is that share values are often linked to individual politicians. Share prices for firms connected to the ruling Suharto family in Indonesia fell when rumors circulated that Suharto was experiencing health problems (Fisman 2001). During the Asian financial crisis, the closure of offshore currency markets benefitted firms with political connections to Malaysian prime minister Mahathir (Johnson and Mitton 2003). Brazilian firms that provided contributions to federal deputies experienced rising share values at election time (Claessen, Feijen, and Laeven 2006).

Favors granted to influential firms have large economic value. In Pakistan, politically connected firms borrow more and have higher default rates than other firms (Khwaja and Mian 2005). These differences in access to credit are all driven by lending practices from government banks, and benefits increase with the strength of the political connections. Cross-national evidence also shows that firms whose controlling shareholders or top managers are members of legislatures or national governments enjoy easier access to debt financing, lower taxation, and greater market shares, and that influential firms also consider the judicial system and tax regulations to be less constraining (Faccio 2006, Chong and Gradstein 2007). Conversely, firms excluded from these privileges may be forced to rely on graft in order to compete with more influential firms.

Influence as Mutual Exchange

A common perspective is that politically-powerful firms manipulate policies and shape legislation in order to give themselves long-term material benefits (e.g. Hellman et al. 2003, Slinko et al. 2005). But these "state-capture" models convey the mistaken impression that governments are unwitting victims of this behavior rather than willing participants in a relationship that is mutually beneficial to politicians and firms alike. Substantial evidence from around the world suggests, however, that political influence is better characterized as an "elite exchange" between firms and politicians, whereby economic rewards are transferred to firms that provide politicians with politically-valuable services in return. In the 1990s influential Russian businesses, for example, were more likely to be subject to price controls and more frequent inspections—both being beneficial to politicians (Frye 2003). In countries such as Mexico and Thailand, companies that acquired concessions during the privatization of state telecoms companies were able to fix prices, restrict the supply of connections, or engage in predatory pricing against would-be competitors while anti-trust authorities looked the other way (Winter 2007; Phongpaichit and Baker 2004). In all cases, specific political parties or public officials benefited directly as a result of elevating these firms to positions of political influence.[1]

One channel by which powerful firms can reward politicians is through employment. Shleifer and Vishny (1994) argue that influential firms receiving public subsidies will, in return, cede some control rights over employment decisions to politicians (who benefit from low unemployment rates). Robinson and Verdier (2002) also emphasize the advantage of control over employment decisions, suggesting that politicians can generate support through selec-

[1] More generally, Choi and Thum (2007) argue that the provision of rent streams from firms to governments is a fundamental part of the influence "bargain" allowing firms to invest in stabilizing the political regime because, in case of a changeover, the firm will lose politically granted benefits. For this reason, crony capitalism is sometimes considered a second-best solution to the government's commitment problem, since politicians share in the above-market returns that economic actors receive over time (Haber 2006).

tive job offers that are contingent on government survival. As long as these jobs pay better than the market rate, potential supporters have a joint stake in keeping incumbents in office. Politicians facing unemployment can also design and implement a range of "hidden" (implicit, off-budget) subsidies or other forms of preferential treatment to keep up employment levels in private firms in order to avoid signaling economic mismanagement (Desai and Olofsgård 2006). Bertrand, et al. (2004), find that politically connected business leaders in France generate "re-election favors" to incumbent politicians by creating more jobs, particularly in more electorally contested areas and around election years.[2]

Although less investigated, a second channel of politically-valuable benefits is the revenue stream from firms to the state. Examining the tax compliance of firms in Eastern Europe and in the former Soviet Union, Gehlbach (2006) finds that the ability of firms to hide revenues from tax authorities accounts for differences in firm-level satisfaction with state-provided goods and services, and in particular, that larger firms are less likely to hide tax revenues and tend to be happier with public goods. In formerly state-socialist economies, the ability of firms to provide revenues is often associated with privileges. In Russia, for example, financial companies that financed the deficit were, in turn, given shares in natural resource companies under the loans-for-shares program in the mid 1990s (Shleifer and Treisman 2000). Alternatively, leaders in Latin America have often targeted tax hikes at politically-powerful businesses, especially during election cycles (Weyland 2002). These examples raise the possibility that influential firms may be more taxable making them, at once, the recipients of tax breaks as well as targets of more stringent monitoring by tax authorities—a possibility

[2]The elite exchange may be more plausible in the context of low- and middle-income countries, where influence-seeking can be dependent on informal ties and cronyism. Alternative perspectives of business-government relations in richer countries focus on objectives other than influence buying. Ansolabehere, Figueiredo, and Snyder (2003), e.g., argue that political contributions by US corporations are a form of political participation. Gordon and Hafer (2005) consider lobbying expenditures a signal of corporations' intent to resist regulatory oversight. We note, however, that others do identify quid-pro-quo arrangements between political contributors and governments in richer countries (e.g., Menozzi, Gutiérrez Urtiaga, and Vannoni 2010; Bonneau and Cann 2009).

suggested by the observation that cronyistic ties between corporations and governments can actually reduce monitoring costs (Kang 2003).[3]

In the next section we provide a simple formal illustration of the effect of political influence on firm investment incentives, and by extension, productivity, when political influence requires firms to cede control rights in return for preferential treatment. Note that the firm-level effects of ceding control rights over hiring and firing are not the same as that of providing direct transfers to politicians. Contributions to incumbents' electoral campaigns, for example, constitute a direct transfer but do not affect investment or production decisions since marginal costs or marginal revenues remain unaffected. By contrast, if politicians impose restrictions on firing (to limit local unemployment) or impose ad hoc taxes (to access revenues that can then be showered on potential voters), firm performance on the margin will be affected.[4]

The Investment Decision

A continuum of firms of size one uses capital (k) and labor(l) in a Leontieff production technology, yielding quantity $Q = \min\{k, l\}$. Some selected firms are protected from competition through monopoly rights, regulatory forbearance, or bureaucratic predation against competitors, and/or are subsidized via budgetary transfers, tax breaks, access to cheap credit, etc.

[3] It is important to emphasize that, although political connectedness is often considered a form of corruption, there are two important differences. First, unlike "administrative" corruption, influence does not necessarily involve bribe-taking by public officials. In fact, influential enterprises or individuals may actually be shielded from predatory public officials. Second, unlike corruption, influence can be perfectly legal—obtained through political financing or lobbying, through favoritism on the part of regulators, through industrial policies, laws or statutes granting special favors, or simply through selective enforcement of existing rules. We focus exclusively on firm-level effects. Arguments have been made, however, that selective protection and subsidy can harm aggregate welfare, e.g., by distorting competition and leading to production of goods of inferior quality, or by increasing costs to state budgets.

[4] Or, political appointees in management positions may be less skilled, driving down productivity both in the aggregate and at the margin.

The efficient per-unit labor and capital costs to firm i are, respectively

$$w_i + I\gamma, \text{ and}$$

$$r - I\lambda,$$

where I is an indicator function taking on the value 1 if the firm is a recipient of privileges that have economic value (protection and subsidies), 0 otherwise. We will refer to these as *influential firms* and *non-influential firms*, respectively. Efficient per-unit labor costs depend on wages w_i and worker effort; γ parameterizes the negative effect on worker effort due to the absence of competition.[5] Additionally, λ represents the capital cost reduction due to subsidization.

In addition to capital and labor, firms face administrative barriers including onerous and costly start-up procedures, bribe taxes, as well as the cost-equivalent of delays in being granted licenses and permits, harassment by police or inspectors, and other methods potentially used by public officials to extract rents. These costs often constitute a significant burden on firms operating in the formal sector in lower- and middle-income countries. We argue that political influence can shield firms from this form of rent extraction. We therefore normalize this cost at $c = 0$ for influential firms, $c > 0$ for non-influential firms.

Firms are price takers, but benefit from higher prices when protected from competition. We denote the price as $p(\delta) = 1 + I\delta$, where δ represents the benefit from protection. Demand for firms' products is uncertain. With probability μ that demand is high, firms sell $\overline{Q}$ at price $p(\delta)$; with probability $(1 - \mu)$ that demand is low, firms sell $\underline{Q}$ at price $p(\delta)$, and $\overline{Q} > \underline{Q}$.

Firms (i) make an investment decision (i.e., whether to augment their capital stock), and

[5] X-efficiency losses due to weak competitive pressures (Leibenstein 1966) typically form the analytical core of microeconomic models that examine how economic agents' efforts are influenced by competitors. Where principals compare the outcome of agents' efforts across competing firms, compensation contracts can be designed with stronger incentives, and agents will thus expend greater effort (Vickers 1995). But without competition, the ability to use such yardsticks is severely limited.

(ii) set employment levels. Finally nature draws demand conditions. The initial employment decision, therefore, is made under uncertainty regarding Q. We assume that firms benefitting from industrial policies have partially ceded control rights over employment decisions, and are prevented from shedding labor. Consequently, non-influential firms can fire workers without cost once Q is realized, whereas influential firms cannot. It follows from the Leontieff production function that once Q is realized, the profit-maximizing employment level is $\min\{k, Q\}$. Influential firms, unable to follow this rule, will have to retain the number of workers decided under uncertainty.

Firms begin with capital and employment at level $\underline{k}$, the optimal level under low-demand conditions. Each firm decides whether to increase its capital stock to $\overline{k}$ as well as the number of additional workers to hire. It follows from the production technology that prior to the realization of Q, $l^* = k$ for all k.[6] We can now compare expected utility with and without investment. The representative firm's expected profit *if it does not invest* can be written as:

$$p(\delta)\,\underline{k} - (r - I\lambda)\,\underline{k} - (w_i + I\gamma)\,\underline{k} - (1 - I)\,c.$$

The same firm's expected profit, *if it invests*, will be

$$\mu\left(p(\delta)\,\bar{k} - (r - I\lambda)\,\bar{k} - (w_i + I\gamma)\,\bar{k} - (1 - I)\,c\right)$$
$$+ (1 - \mu)\left(p(\delta)\,\underline{k} - (r - I\lambda)\,\bar{k} - (w_i + I\gamma)\,l - (1 - I)\,c\right).$$

It follows that the firm will choose to invest if and only if

$$\mu p(\delta)\left(\bar{k} - \underline{k}\right) \geq (r - I\lambda)\left(\bar{k} - \underline{k}\right) + (w_i + I\gamma)\left(l - \underline{k} + \mu\left(\bar{k} - l_j\right)\right). \tag{1}$$

[6] Strictly speaking, when $k = \overline{k}$ influential firms will only employ extra employees such that $l = \bar{k}$ if the benefit of being able to meet the extra demand in good times exceeds the risk of ending up with bloated payrolls in bad times. However, if this condition is not satisfied, then the firm will have no incentive to invest in the higher capital stock at the outset, so it will always be true that $l^* = k$.

Rearranging terms we can rewrite the investment condition (1) as a wage threshold

$$w_i \leq \tilde{w} \equiv \frac{(\mu p(\delta) - (r - I\lambda))}{\tilde{l}} - I\gamma, \tag{2}$$

where $\tilde{l}$ is defined by

$$\tilde{l} \equiv \frac{\left(l - \underline{k} + \mu\left(\bar{k} - l\right)\right)}{\left(\bar{k} - \underline{k}\right)},$$

i.e., the cost of ceding control rights over employment decisions. It follows from the restrictions against firing that $\tilde{l} = 1$ for an influential firm ($l = \bar{k}$), whereas $\tilde{l} = \mu$ for a non-influential firm ($l = \underline{k}$). Firms with a labor unit cost below the wage threshold in (2) will invest, the others will not. An increase in the wage threshold, therefore, increases the likelihood that a randomly-selected firm will invest. If subsidized credits were provided to firms without cost, then the threshold would increase by a factor of λ, suggesting that firms benefiting from industrial policies should be more likely to invest. On the other hand, if these firms are required to cede control over employment decisions, they will be faced with excessive employment (and wage expenditures) in the event of low demand ($\tilde{l}$), reducing their likelihood of investing.

Additionally, protection from competition has two contrasting effects: a price effect and an efficiency effect. On the one hand protection means that the firm can charge higher prices, making new investments more attractive (the price effect is captured by δ). On the other hand, the absence of competition increases the wage needed to obtain an effective unit of labor input (γ), suggesting that influential firms may be less likely to make new investments because of lower labor effort. Finally, expected labor productivity (Q/l) will be lower in influential firms, who will retain excess employees if they invest when demand proves low.

In the section that follows, we test the validity of several assumptions from this framework: that influential firms face a lower cost of doing business (c), have access to cheaper credit

(λ), face fewer competitors (δ), and that influential firms will carry excess labor ($\tilde{l}$). We also test effects of firm-level influence on productivity and investment, which are decreasing in net costs of influence. Note that the cost of doing business is assumed to be fixed and does not vary with k, and therefore only affects firm profits and has no impact on relative incentives to invest. More generally there are likely both fixed and variable cost components in the regulatory and tax environments for businesses–components which cannot easily be identified ex ante. We can, however, determine which component dominates by examining the firms performance; an adverse effect of firm influence on performance would indicate that the fixed component dominates the costs of doing business.

Data and Methodology

We rely on the World Bank's Enterprise Surveys (World Bank 2002, formerly the Productivity and Investment Climate Surveys), which, since its inception in 2000 has collected data from approximately 75,000 manufacturing and service firms in over 100 developing countries. These data, although expansive in their cross-country coverage, do not contain the type of information that would allow us to measure actual political connections, namely, detailed information on owners or officers that could be used to assess their political identities. Instead, the Enterprise Surveys contain several perception-based questions about the political influence of firms in shaping national policies affecting their businesses. Moreover, questions on political influence were dropped from the core questionnaire after 2005. The subset of this total sample of firms who have coded responses for questions of political influence, therefore, is far smaller—but still leaves us with over 8,000 firms surveyed in approximately 40 developing countries between 2000 and 2005.

Addressing Biases in Firm Responses

The use of qualitative or subjective indicators in surveys is subject to measurement error, which introduces three potential biases in the Enterprise Survey data: (i) non-comparability bias, (ii) systemic bias; and (iii) representativeness bias. First, differences across respondents' interpretations of the questions can produce problems of comparability particularly when respondents are asked to use ordinal response categories. Different respondents may interpret concepts such as "influence" in different ways based on unobservable characteristics ("culture," socialization, etc.). Ordinal scales may mean different things to different respondents based on idiosyncratic factors such as mood or overall optimism. Sometimes referred to in educational testing as "differential item functioning" (DIF), the problem is particularly acute in measurements of political efficacy, where the actual level of efficacy may differ from the reported level due to individual-specific proclivities (King and Wand 2007). Firm-level perceptions of influence would similarly be affected by DIF where identical firms may have unequal probabilities of answering questions about their own political influence in the same way.

Explicit "anchoring vignettes" or other hypothetical questions to establish baselines that could normally correct survey responses for inter-firm incomparability, however, are not included in the Enterprise Surveys core questionnaire. Instead, to measure influence we use firm responses to a question related to four categories of businesses:

> *How much influence do you think the following groups actually had on recently enacted national laws and regulations that have a substantial impact on your business?* ***a****: your firm;* ***b****: other domestic firms;* ***c****: dominant firms or conglomerates in key sectors of the economy;* ***d****: individuals or firms with close personal ties to political leaders.*

Each answer ranges from 0 (no impact) to 4 (decisive influence). The distributions of responses to this question are shown in figure 1. Note that the modal response is "none" for

all questions, and in particular, some 68% of firms believe themselves to have no influence. Moreover, it is not the case that firms that rank their own influence lowly tend to rank the influence of other firms highly. Figure 2 breaks down rankings of the other firms' influence by self-rankings of influence. For most categories of self-rankings, the most common response (the dark bars in the graph) is to rank themselves and others as having identical levels of influence–those who think they have no influence also believe that other types of firms have no influence, those who think they are moderately influential also think others are moderately influential, and so on.

We see, then that most firms think that no one has any political influence, and that influence self-ratings are associated with ratings of others. To correct for the strong possibility that DIF is present, we take the sum of the differences between the self-assessment A and the assessments of other groups, i.e., $a-(\frac{b+c+d}{3})$, which yields a measure of the perceived influence "gap" between the responding firm and other types of firms.[7] Our measure of influence ranges from -4 to +4. Figure 3 shows the distrubution of the transformed influence score, which is now more normal than that shown in figure 1.[8] Table 1a shows pairwise correlations among all components of the transformed influence score. We see that most components are positively, and significantly correlated. We also see that the standard deviation is greater than the mean for self-influence responses; the opposite is the case for influence assessments of other types of firms. As with survey "anchors," then, assessments of others are subject to less inter-firm variation than self-assessments, and thus we use responses to questions about other groups to subtract off the DIF from the self-assessment response. Table 1b shows pairwise correlations

[7] We difference firms' self perceptions with their average perceptions regarding three other groups (other firms, other conglomerates, and other politically-connected firms) rather than simply "other domestic firms" to reduce the effect of biased perceptions towards any particular category of firms. Differencing self perceptions solely with perceptions of other firms has no effect on our results.

[8] The normal distribution is even more pronounced when we remove the approximately 1500 observations for which all types of firms are rated as having no influence. Eliminating these observations from subsequent regressions has no effects on the results thus we include them in our core sample.

between the transformed influence score and several more objective firm-level characteristics, including age, whether the firm is an exporter, whether the majority shareholder is domestic, whether the firm is state-owned, and the size of the firm. The relationships generally conform to expectations of the nature of political influence: older firms, state-owned firms, foreign companies, and firms with more employees are influential relative to other types of firms–in line with findings using more objective measures of political connectedness (e.g., Faccio 2006, Bertrand *et al.*, 2004).

Second, that rankings of self and others' influence tend to move together suggests that responses may be affected by systemic bias. Previous analyses of business environment constraints using the Enterprise Surveys have shown that interpretation of responses is complicated by the fact that some managers simply tend to view the world through the same subjective lens, and some firms simply have a higher propensity to complain regardless of the actual constraints their businesses may face (Carlin, Shaffer, and Seabright 2006). The use of country, time, and industry dummy variables can mitigate some of this perception bias, since the variation being examined is within-country, within-survey years, and within-industry, respectively. Consequently, all of our estimations include these fixed effects.

But the inclusion of a variable among regressors that proxies the systemic bias more directly would better correct for bias in perception-based outcomes. We use two approaches to accomplish this. First, we regress responses by managers to a question of the severity of macroeconomic instability on the annual change in the consumer price index (CPI) in the country during the survey year–a proxy for actual macroeconomic instability–plus time and country dummies. The residual from this estimation may be interpreted as the extent to which within-country, within-industry perceptions of macroeconomic instability are not influenced by price instability. We reason that the inclusion of this residual among the regressors in our

main estimations can control for firm-specific systemic bias to the extent that perceptions should reflect actual conditions. Previous research utilizing similar approaches–relying on actual country-specific tax or regulation indicators–has found that subjective responses in the Enterprise Surveys actually do reflect within-country, within-sector objective circumstances measured from within the survey or from outside sources (Hallward-Driemeier and Aterido 2009). Second, we also include responses by managers to questions about the degree to which their firms' activity is constrained by crime. Evidence suggests that, although there is significant variation across countries and sectors, firms within the same country and the same industry are likely to be similarly affected by crime (see, e.g., Amin 2009, Krskoska and Robeck 2006). The distribution of responses to these questions, in equations including country and industry fixed effects, should therefore closely proxy the distribution of the propensity to complain within our sample. The range for each question is 0 (no obstacle) to 4 (very severe obstacle).

Third, firms may refuse to answer certain questions, or they may simply lie, creating a representativeness bias. Despite efforts to minimize non-response during data collection, the World Bank's Enterprise Surveys are characterized by high levels of missing responses. Given that some questions–in particular, those concerning relationships with political authorities–may be highly sensitive, non-response as well as false-response rates can vary across questions. Moreover, non-responses are correlated with certain firm or country characteristics (Jensen, Li, and Rahman 2010). The Enterprise Surveys do not include any set of screening questions that could be used to identify firm "reticence." Our imperfect solution, therefore, is simply to use logistic regression models controlling for baseline information (described below) to estimate the probability of response for each dependent variable; the reciprocals of these probabilities are used as weights in our subsequent analysis.

Specification and Methods

Our basic specifications take the following form:

$$R_i = f\left(\chi_\omega \omega_i, \chi_\theta \theta_i, \chi_x \mathbf{x}_i\right) \tag{3}$$

where R is the hypothesized outcome for firm i specified in the preceding section (firm i faces better business environment; firm i provides politically valuable benefits; firm i invest less), ω is our measure of the relative influence of firm i, θ is the firm-specific systemic bias of firm i as described above, $\mathbf{x}$ is a vector of firm-specific control variables, and χ_ω, χ_θ, andχ_x are vectors of coefficients. The firm-specific characteristics we include are: the age of the firm (in years), the size of the firm (number of permanent employees, log scale, lagged one year), a legal-status effect (identifying whether the firm is publicly listed, privately held, a cooperative, partnership, or sole proprietorship), a location effect (identifying whether the firm is located in the capital city, in a city with more than 1 million, 250,000 to 1 million, 50,000 to 250,000, or less than 50,000 in resident population), dummy variables identifying whether the firm is an exporter, whether the firm is majority-owned by a domestic company or individual (vs. a foreign entity), and whether the firm is a state-owned enterprise. In addition, we include the following sets of dummies in all specifications: industry dummies (ISIC 2-digit), survey-year dummies, and country dummies. Summary statistics for all variables used in our analysis are in table 2.[9] Given that intra-group correlation of errors in survey data can be present even in the presence of fixed effects, we allow errors in (3) to be correlated across firms in a given country-industry, i.e., standard errors are clustered by country-industries in all specifications. Our basic specifications are estimated using OLS or logit regressions depending on whether the outcome of interest is continuous or binary, respectively.

[9] We also included a dummy specifying whether the firms have ever been state-owned, given that newly privatized firms may maintain close political connections while struggling with legacies of state ownership (bloated payrolls and inefficient business practices). The inclusion of this dummy is without consequence for our results.

Estimates of firm-level political influence may, additionally, be affected by selection bias due to the non-random character of "influential" vs. "non-influential" firms, whereby the distribution of covariates ω, θ, and $\mathbf{x}$, may be very different for firms depending on their level of political influence. In the absence of randomization, a common approach is to use matching methods to ensure that different categories of observations (influential vs. non-influential firms) are as similar as possible in terms of relevant covariates–a method analogous to severing the links between explanatory covariates and likelihood of "treatment" in observational data.[10] We therefore correct for observable differences between influential/non-influential firms by pre-processing our data with matching methods, then re-running our parametric analyses on the matched sub-sample of the data as recommended by Ho, *et al.* (2007), and similar to the parametric bias-adjustment for matching by Abadie and Imbens (2006). We compute coefficients on all independent variables after matching rather than reporting the simple difference in means without controlling for potential confounding variables. The purpose of matching here, of course, is to ensure that influential firms are as close as possible to non-influential firms in terms of relevant covariates.

We rely on propensity score matching based on the following model:

$$\Pr\left(Influence_i = 1\right) = \Phi\left(\hat{\beta}_\theta \theta_i + \hat{\beta}_x \mathbf{x}_i + \hat{\beta}_l Lobby_i\right), \tag{4}$$

where $Influence = 1$ [$Influence = 0$] occurs when a firm is [is not] able to influence national policies affecting its business. We designate firms as influential if their transformed influence score as calculated above is greater than zero.[11] Φ is the standard normal distribution function, θ is the firm-specific bias, and $\mathbf{x}$ is a vector of firm-specific indicators–age of the firm, number of permanent workers, dummies specifying whether the firm is an exporter,

[10] This approach does not control for the presence of unobserved heterogeneity, which can only be corrected through the inclusion of all relevant confounding factors in the selection model.

[11] We experimented with different cutoffs, including ≥ 0, >-1, etc., with no major difference in the result. Note that, at a cutoff of >0, approximately 10% of observations are coded as influential; at ≥ 0 it is 30%.

domestically-owned, or state-owned, as well as legal-, location-, sector-, year-, and country dummies. To this we add an additional dummy: whether, in the past two years, the firm has sought to lobby the government or otherwise influence the content of laws or regulations affecting the firm's business. We generate a propensity score derived from a logit regression of (4).[12] All regressions are run on both unmatched and matched subsamples.

Endogeneity

Although a solution to the selection problem, matching does not correct for potential endogeneity. We recognize that the costs that firms face or the benefits they obtain may boost their influence as well as the other way around. For example, it is possible that firms with bloated payrolls are more likely to have the ear of politicians, or that firms that are able to reduce the costs of navigating regulatory barriers are also better at bringing pressure to bear on lawmakers. Alternatively, firms paying high bribes may turn to influence activities to be shielded from rapacious officials, or poorly-performing firms may engage in influence-peddling to compensate for losses.

Finding valid, firm-specific instruments that meet the usual criteria (especially excludability/orthogonality to the outcome of interest) poses a serious challenge. We follow a common approach taken by, among others, Fisman and Svensson (2007), and use grouped averages as instruments to address potential endogeneity. We generate average levels of influence for each country-industry, and use these to instrument firm-level influence. An individual firm's influence level will depend not only on characteristics of that particular firm, but also on characteristics specific to the country and industrial sector in which it operates. At the country

[12] We use local linear regression to construct matched outcomes, with biweight (quartic) kernels and default bandwidths of 0.06 and using the common-support condition. Local linear matching–a generalized version of kernel matching–constructs a match for each influential firm using smoothed local regression over multiple firms in the comparison group, and demonstrates greater robustness to different data densities than alternative pair-matching estimators (see Heckman, Ishimura, and Todd 1997). In our data, local linear matching also improves the balance between influential and non-influential firms better than alternative estimators.

level, the rewards and risks of engaging in elite exchange will depend on the transparency and accountability of the political system, as well as on the distribution of rents in the economy. At the industry level, influence may vary across sectors because of differences in the extent of government regulation, wage- or price-setting (or other existing price distortions), the availability of subsidies, and other forms of state intervention in the sector. Certain sectors may be strategically more important than others, while some industries may be more dependent on public procurement, and so on. We can posit that this variation across countries and sectors is not driven by factors specific to the firm itself, but rather, by factors determined by these country-industry characteristics. It follows that variation in firm-specific influence explained by the country-industry average level of influence should be uncorrelated with unobservable firm-specific factors that are causing endogeneity bias.[13]

Results

Is Life Easier for Influential Firms?

We first examine whether the assumption that influential firms face lower costs of doing business (c) is empirically justified. Table 3 examines three costs typically imposed on businesses in developing countries: bribes, non-payment, and theft (exact wordings of questions used for these and other selected variables can be found in the appendix). Columns (1) to (5) examine bribes as a percentage of sales.[14] We begin with a benchmark regression that is uncorrected for various biases in column (1), then include our CPI-based and crime-based proxies for systemic bias. The inclusion of these terms does not affect the basic result: influential firms pay

[13] To make the uncorrelated errors condition more robust, country-industry average influence is taken from the self-influence indicator, rather than the transformed influence gap score. For a review of the use of group averages as instrumental variables, see Angrist and Krueger (2001).

[14] The Enterprise Survey asks how much "a typical firm like yours" pays in bribes, rather than how much "your firm" pays, in order to minimize under-reporting.

less in bribes than non-influential firms. Similarly, when weighting for non-response bias and clustering errors by country-industry in column (4), and when re-running the estimation on the matched sub-sample in column (5), results do not change.[15] Taking account of systemic bias, non-response weights, and clustering, we examine the effect of influence on government contracts as a percentage of procurement contract value, non-payment of receivables, and losses from crime and theft. As a general robustness check, here and throughout, we run estimations on unmatched and matched samples. As with overall bribes, influential firms also pay fewer bribes for government contracts.[16] With less consistency, we also find that older firms, state-owned companies, and foreign companies are better protected from bribe collectors. We also include workers (our measure of firm size) in quadratic form, and find that firms with more employees pay more in bribes for government contracts but the effect is diminishing. We include, but do not report, legal status, location, industry, survey-year, and country dummies. [17]

These results argue against the view that bribes are an instrument of influence-peddling by private sector elites. Rather, our findings suggest that bribe taxes are used by the public

[15] Given the potential sensitivity of the matched results to specification changes in the propensity score-generating (logit) model for equation (4), we tested the stability of our results as follows: we re-ran the logit specification 12 separate times, each time dropping one covariate or set of dummy variables, then re-estimated our main regression (5) in table 3. The results are essentially identical, with little difference in magnitude, signs, or significance of the covariates in the main results. Moreover, the coefficient of variance (std. deviation ÷ absolute value of the mean × 100%) for the influence beta across these 12 specifications is less than 0.02%.

[16] Using our basic estimation for the matched sub-sample of firms, we further computed the probabilities that influential vs. non-influential firms are forced to pay bribes to various types of inspectors and officials (these results are not reported here). With statistical confidence ($p < 0.01$), we find that the likelihood that non-influential firms will have to pay bribes to building inspectors, health inspectors, and environmental inspectors is, respectively 27%, 29%, and 24% greater than for influential firms. With lower confidence ($p < 0.1$), non-influential firms were also found to be 17% and 24% more likely to have to bribe tax collectors and local police, respectively. Notably, no significant difference in bribe propensity between influential and non-influential firms is found for labor inspectors—perhaps a reflection that, if labor regulations might affect non-influential firms more adversely while labor costs are a problem for influential firms, the bribe tax paid to labor inspectors may be equivalent.

[17] From a simple stochastic simulation of columns (4) and (6), setting all variables at their sample means, an average firm pays 1.8% of sales in bribes, and 2.5% of the value of a government contract in bribes. But for the most influential firms, the amounts drop to 1% and 0.7%, respectively. Meanwhile firms that score below the bottom quintile in influence pay 2% of sales and 3% of contract value in bribes to public officials. It is possible that influential firms pay less bribes because they have less extensive dealings with public officials than non-influential firms. We find no evidence for this disparity. We estimated the percentage of "senior management's time spent in dealing with requirements imposed by government regulations" based on the benchmark specification in table 3, and find that influence has no statistically significant effect.

sector to extort payments from weak or vulnerable enterprises. This is consistent with a bargaining framework for bribe-paying in which political connectedness can increase firms' relative bargaining power in dealing with public officials (Svensson 2003). High-level connections shield firms from predatory behavior by rank-and-file administrators, indicating that the prevalence of corruption and cronyism in an economy are, for non-influential firms, reinforcing.[18]

We turn to instrumental variables regressions in table 4. As indicated above, we cannot discount the possibility that firms which are targets of bribe-taking officials may choose to seek political influence as compensation. Specifically, it is possible that some firms that are paying high bribe taxes will devote greater resources to developing political contacts and relationships, while others do not. Both types of firms would suffer from high bribe payments, but not as a result of political influence. As this potential endogeneity applies to most of our dependent variables, we explore whether the effect of firm influence on bribes changes when we instrument for influence using the approach described in the previous section. Table 4 presents these results.

We replicate our basic regression (table 3, columns 3-4) by estimating the effect of influence on bribes by instrumental variables (IV) regression. Table 4 reports two-stage least squares (2SLS) results for a just-identified model using country-industry averages of influence as instruments for firm-level influence. We identify the effect of firm influence on bribes by the exclusion restriction that country-industry average influence does not appear in the second-

[18] Political connections usually protect firms, but in some notable cases they do not. Columns (8) to (9) estimate the percent of sales that are left unpaid. Firms were asked to report the percent of sales to private customers that involve overdue payments. Firms in developing nations—particularly in the former Soviet-bloc countries—typically suffer from significant unpaid bills from customers, and have often responded by non-payments of their own to creditors, suppliers, tax collectors, and even workers. We find that politically influential firms are less likely to be trapped in these cycles of non-payment. In columns (10) to (11) we examine the effect of political influence on losses from theft, robbery, arson, or vandalism. Losses from theft are unaffected by firm influence, size, or state-ownership (other effects are unstable) suggesting that firms of all stripes are similarly affected by crime, and that the use of a crime-based proxy for systemic bias is valid. For influential firms, governments have less control over criminals than they have over bribe collectors and non-paying customers.

stage regression. In the first stage we see that the impact of country-industry averages has a strong, independent effect on firm-level influence. Tests for under-identification (the Anderson canonical correlation test) reject the null hypothesis that the equation is under-identified. Tests for instrument strength (Cragg-Donald F statistic) are above critical values required to reject inconsistency of the IV estimator. In columns (3) and (4) we use a cluster-robust IV estimator with non-response weights. First-stage results are similar to those of the simple IV estimator: the instrument is correlated with firm influence, and is both valid and above critical values for instrument strength.

Second-stage results in columns (2) and (4) show that instrumented firm influence has a negative impact on bribes, although this is only significant in the cluster-robust estimation. Control variables have signs similar to results in table 3. We also test for the endogeneity of influence in second-stage results. We use the heteroskedasticity-robust version of the Wu-Hausman test implemented by Baum, *et al.* (2003), for which the null hypothesis is that the OLS estimator of the same equation (treating the suspect regressor as exogenous) yields consistent estimates, and a rejection of the null indicates that endogenous regressor's effect on the estimates requires an IV estimator. These tests show that we cannot reject the null of exogeneity. In sum, the use of country-industry influence averages, though a valid instrument, ultimately suggests limited endogeneity bias in our OLS results. Unfortunately, the lack of other valid instruments in the Enterprise Surveys limits our ability to conduct more elaborate explorations of the robustness of our basic claims to potential endogeneity biases. [19]

[19] As a general test of the robustness of our results to possible endogeneity, we instrument firm influence with country-industry, grouped averages, and re-estimate all regressions that follow using the cluster-robust 2SLS estimator, with non-response weights. These results are not reported but are available from the authors. Statistical tests, in all cases, reject under-identification, and reject the inconsistency of the IV estimator given the most stringent criteria (only when replicating results from table 8 below can we reject inconsistency at a slightly less stringent level). With one exception, the coefficients on influence retain their previous signs. In all but one case, however, influence loses statistical significance; this loss of precision is no surprise given the use of an aggregate (as opposed to firm-specific) instrument. In only one instance, finally, is exogeneity rejected with more than 95% confidence, but in this estimation, the sign for firm influence remains the same as in the OLS estimation.

In table 5 we turn to firms' business constraints. In the first several equations, our dependent variables are averages of responses to questions about the severity of five categories of constraints: infrastructure (telecommunications, electricity, and transportation), taxation (both rates and the administration of), regulations (including customs, licensing, and permits), and finance (cost and access). In each case we code these variables 1 if the obstacle was considered "major" or "severe," 0 otherwise. To these four indicators we add a sixth, based on firm responses to a question of how customers would respond were the firm to raise prices of their main product or service by 10%, a proxy for the absence of competitors (δ). We code this outcome 1 if firms state that there would be no change in customer behavior, 0 otherwise.[20] The results of these logit regressions are summarized in equations (1) to (10) in table 5. For simplicity we only report the coefficient on influence across estimations. All outcomes, however, were estimated using the full specification in (3), incorporating bias, non-response weights, and clusters, on both unmatched and matched samples. We also report pseudo R^2 from the full estimations. As with crime, poor infrastructure does not discriminate between influential and non-influential firms. But all other constraints are decidedly more severe for non-influential firms, which are five to eight times more likely to consider tax, regulatory, and financial constraints to be major or severe obstacles than influential firms. These results also confirm the absence of competition for influential firms, for whom price hikes are less likely to affect customer behavior.

As mentioned previously, the allocation of credit to privileged firms on "soft" terms is considered a mainstay of crony capitalism. In equations (11) to (18) we investigate whether influential firms have easier access to credit (λ). Again, we report only the coefficients and standard errors for the influence variable. Here we examine four proxy outcomes: (i)

[20] Firms were given four choices of responses: A: customers would continue to buy at the same quantities; B: customers would continue to buy but at slightly lower quantities; C: customers would continue to buy but at much lower quantities; and D: customers would stop buying.

whether collateral was required for the most recent loan (for firms that obtained loans); (ii) the cost of the collateral (as a percentage of loan value); and the percentage of (iii) working capital and (iv) new investments financed by "informal sources" (money-lenders or other informal financial institutions). For the first outcome—the collateral requirement—we use logit regressions, while for all others we rely on OLS. Consistently and unsurprisingly, influential firms have easier access to credit. Influential firms are less likely to be asked to collateralize loans by lenders. Among firms that do provide collateral or a deposit for their financing, the more influential firms typically have to cover less of their loans than less influential firms. And influential firms are less entwined in the informal financial sector.

Can Influential Firms Benefit Politicians?

In table 6 we examine evidence of the high-employment guarantees ($\tilde{l}$) we have suggested as a source of political rents. Columns (1) and (2) present logit results for unmatched and matched samples, respectively, of estimating the effect of political influence on excess employment. Firms were asked, if they could change the number of full-time workers without restriction or punishment, whether they would shrink their payrolls. We code responses 1 or 0 depending on whether firms reported they would lay off workers. In columns (1) and (2), in addition to the variables included in the basic specification, we also include firms' capacity utilization, on the assumption that use of installed productive capacity can affect firm managers' preferences regarding optimal employment levels. We find that influential firms are more likely to maintain excess labor than non-influential firms.

A second source of potential rents is tax payments, since public expenditures may be used to bolster public support. In columns (3) and (4) the dependent variable is percentage of sales reported for tax purposes. Our results show that influential firms comply with tax reporting

rules to a greater extent than non-influential firms (both in absolute and per-employee terms). Note that we also confirm one of Gehlbach's findings, namely, that firms with more employees hide less taxes. Influential firms may have a harder time evading taxes—possibly because their connections put them under closer scrutiny—but are also more likely to be compensated by explicit exemptions.

Does Influence Affect Investment and Innovation?

Rewards in the form of lowered costs of business, monopoly rents, and other benefits are often justified by developing country governments as a de facto form of targeted industrial policy, on the assumption that most politically-connected firms use these benefits to invest and innovate, and that these influential firms, therefore, are also the most dynamic. However, our model suggests that the opposite could be true, more influential firms are less likely to invest and innovate if the costs of bloated payrolls and x-inefficiency due to lack of competitive pressure dominate the beneficial effects. We examine this relationship in tables 7 and 8.

Firms were asked a series of questions on their restructuring activities and innovation. Table 7 shows the results of estimations in which the dependent variables are a set of innovation/restructuring outcomes: whether, in the past three years, the firm opened a new plant, introduced a new product line, closed an old plant, or closed an obsolete product line. While there are valid concerns regarding the comparability of "newness" or "obsolescence" across firms in different countries and in different industries, the inclusion of industry and country dummies should correct for these differences. In addition to these outcomes, we also examined whether firms engaged in R&D activities in the past year. As in table 5, we only report coefficients and standard errors for the influence variable, for logit estimations using both unmatched and matched samples. Once again, influential firms display a certain consistency:

they are less likely to open or close facilities, introduce or close out product lines, or engage in R&D.

In table 8 we examine real growth in sales over the past three years (log scale), total investment, and the investment planning horizon in months (estimated with a Poisson event-count model). Influential firms suffer from lower real growth in sales over the three-year period. Columns (3) and (4) represent a log-form estimation of investment per worker. Political influence lowers firm-level investment (although the coefficient is significant only in the matched-sample regression). Finally, influential firms have a more myopic investment-planning horizon than non-influential firms.

Political Influence and Productivity

In columns (7) - (10) in table 7, finally, we show results from basic productivity estimations. A Cobb-Douglas production function for firm i in country c can be written as:

$$\log Y_{ic} = \beta_0 + \beta_c + \beta_L \log L_{ic} + \beta_K \log K_{ic} + \mu_{ic}, \tag{5}$$

where Y is output, L and K are labor and capital inputs, and β_0 and β_c are common and country-specific intercepts, respectively. The error term, μ_{ic}, can be interpreted as within-country total-factor productivity (TFP), i.e., productivity after measured inputs have been accounted for. Using equation (5) we estimate firm productivity in two ways. First we estimate an augmented production function where we regress output on L (workers) and K (capital inputs) in addition to the variables in our basic specification (3), including influence and other firm-specific characteristics. We use sales rather than value-added for the dependent variable, given the unavailability of value-added data in the Enterprise Surveys. The augmented production function allows us to estimate the independent effect of political influence on firm productivity. Secondly, we generate the residuals (TFP) from equation

(5) and regress the result using our basic specification, allowing us to gauge the effect of political influence on firm-level TFP. Incorporating these measures shrinks our sample size significantly and therefore must be interpreted with caution, but we do see that the effect of political influence seems to support the notion that connected firms suffer from lower efficiency. In the augmented production functions, political influence is associated with less output (although the significance of the coefficient in the matched sample drops below the 10% level), and influence is also associated with lower TFP. Firms that benefit from preferential treatment are less productive than those that do not.

Conclusion

We have examined the content of firm-state relationships characterized by special privileges granted to favored firms—something that is prevalent across the developing world. Theoretical and empirical analyses of these relationships have generally focused exclusively on the benefits or costs to firms (and to some extent to politicians), without assessing the impact on firm-level decision making. We argue that economic privileges often come with a price, and use a simple framework to show how company performance varies between influential and non-influential firms.

We have characterized political influence as a bargain between firms and politicians whereby the former relinquish a portion of control rights in exchange for subsidies and protection. We model how this bargain affects firm decisions, arguing that protection from competition, combined with the tendency to overstaff dampens incentives to invest, innovate, and lowers productivity. Data from more than 8,000 firms in over 40 developing countries show that influential firms do have easier access to credits, face lower demand elasticity, enjoy regulatory forbearance, pay smaller bribes, and generally face fewer obstacles to doing

business than less-influential firms. In exchange, influential firms provide politicians with politically-valuable benefits in the form of higher employment and revenue.

These constraints—the costs of political influence—mean that influential firms are less likely to restructure operations, invest less in R&D, rely on shorter investment-planning horizons, and report lower real sales growth, investment rates, and productivity than less-influential firms. Despite their access to privileges of economic value, influential firms suffer from sharp disincentives to innovation and long-term investment. We found these results to be robust to simple controls for systemic bias, non-response, intra-group correlation, and selection bias. We also relied on a useful, if imperfect, instrument for firm influence and found that firm influence is generally not subject to strong endogeneity bias, and that in the few cases where there appears to be bias, instrumenting for influence does not alter our main results.

Our findings, finally, suggest some firm-level answers to three separate but related questions on the political-economy of development. First, when does industrial policy lead to adverse economic outcomes and when does it work? There are examples where industrial policy has played an important role in promoting development, just as there are examples where industrial policy has had the opposite effect. The difference may be attributable to the nature of the political institutions implementing these policies (Robinson 2009). Our findings highlight a particular channel: if industrial policy, by picking winners, also endows selected firms with greater influence in public affairs, it is likely that those firms will also provide benefits to incumbent politicians. The underlying political motives for industrial policies are often opaque and the temptation to secure political favors (employment, revenue) in return for selective, targeted supports can be irresistible to political leaders, and

is ultimately harmful to the dynamism and efficiency of beneficiary firms.[21] We show that firms benefiting from distortionary industrial policies are often precisely those that are politically valuable, and that this bargain with the state reduces incentives for investment and innovation, and harms productivity. Non-distortionary interventions, by contrast, such as those that enhance infrastructure or support skill-acquisition by workers, would not directly benefit specific firms. We can speculate, consequently, that governments relying on these broader interventions might avoid cronyism in business-state relations since there would be little grounds for the direct exchange of favors between firms and politicians.[22]

Second, why do some economies remain chronically under-developed? Several authors have argued that differences in barriers to adopting technological innovations account for differing rates of development (e.g., Rosenberg and Birdzell 1986; Mokyr 1992). Others have suggested that that these barriers, far from being exogenously-determined, are deliberately fashioned through restrictive labor practices and restrictions on the import of productivity-enhancing equipment (Parente and Prescott 2002). Our results suggest an additional source of these barriers, namely, the bargains associated with firm-level political influence whereby incentives to invest in advanced equipment and technology (even if available domestically) are weakened, while at the same time, the costs of business are raised for non-influential firms.

Finally, if these firm-state influence relationships are dependent on political incumbents, why do they persist even as political regimes change? For Haber (2006), these bargains are a solution to the government's commitment problem: by sharing a stream of rents with a small group of elites, the bargain is made more credible to income holders and the cronyistic

[21] Even some advocates of experimentation in industrial policy acknowledge that the risks of cronyism can be substantial (e.g. Mukand and Rodrik 2005).

[22] Harrison and Rodriguez-Clare (2010), for example, have argued that these types of "soft" industrial policies, while they are more difficult to implement than tariffs, subsidies, tax breaks, etc., are less vulnerable to political manipulation.

arrangement more durable. For others, limited access to privileges among certain favored groups is a mechanism for maintaining order under conditions of fragile state capacity (North, Wallis, and Weingast 2009). Our results suggest a slightly different logic: both firms and politicians have a strong interest in ensuring that enterprises remain a permanent source of mutual rents. For politicians, control rights in critical sectors of the economy are highly desirable. Meanwhile firms risk losing a series of privileges should politicians be replaced, and thus those that have privileges are encouraged to perpetuate their influence-peddling activities regardless of who is in power.

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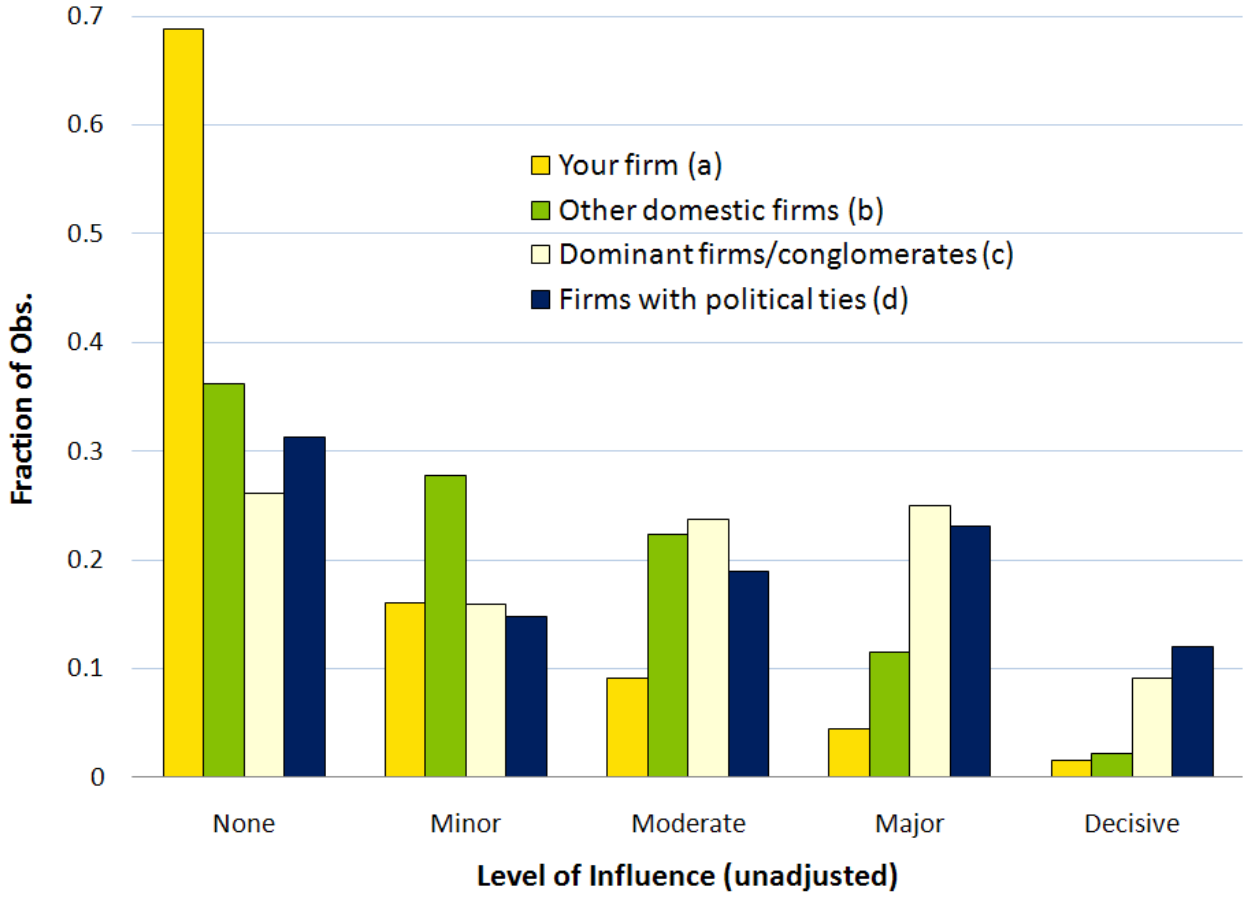
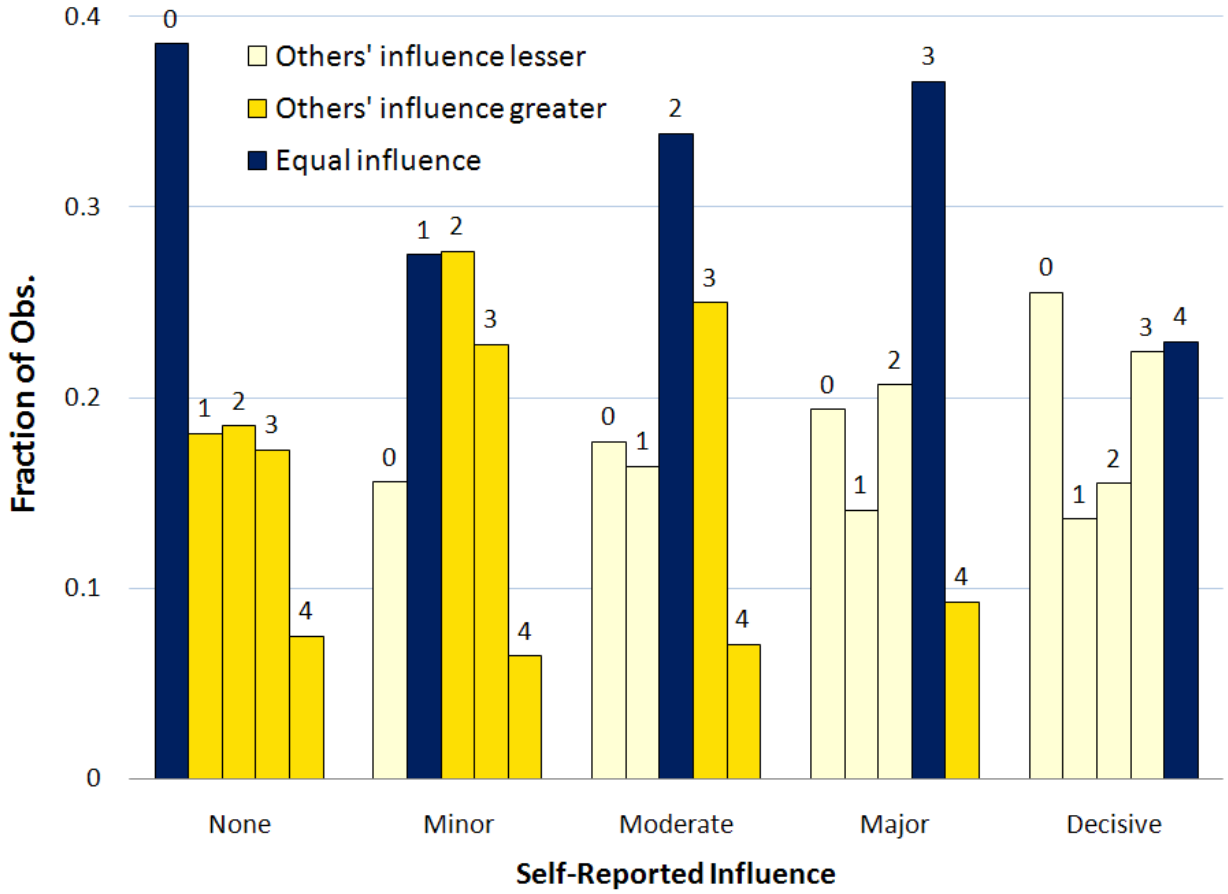
Figure 1: Distributions of influence perceptions



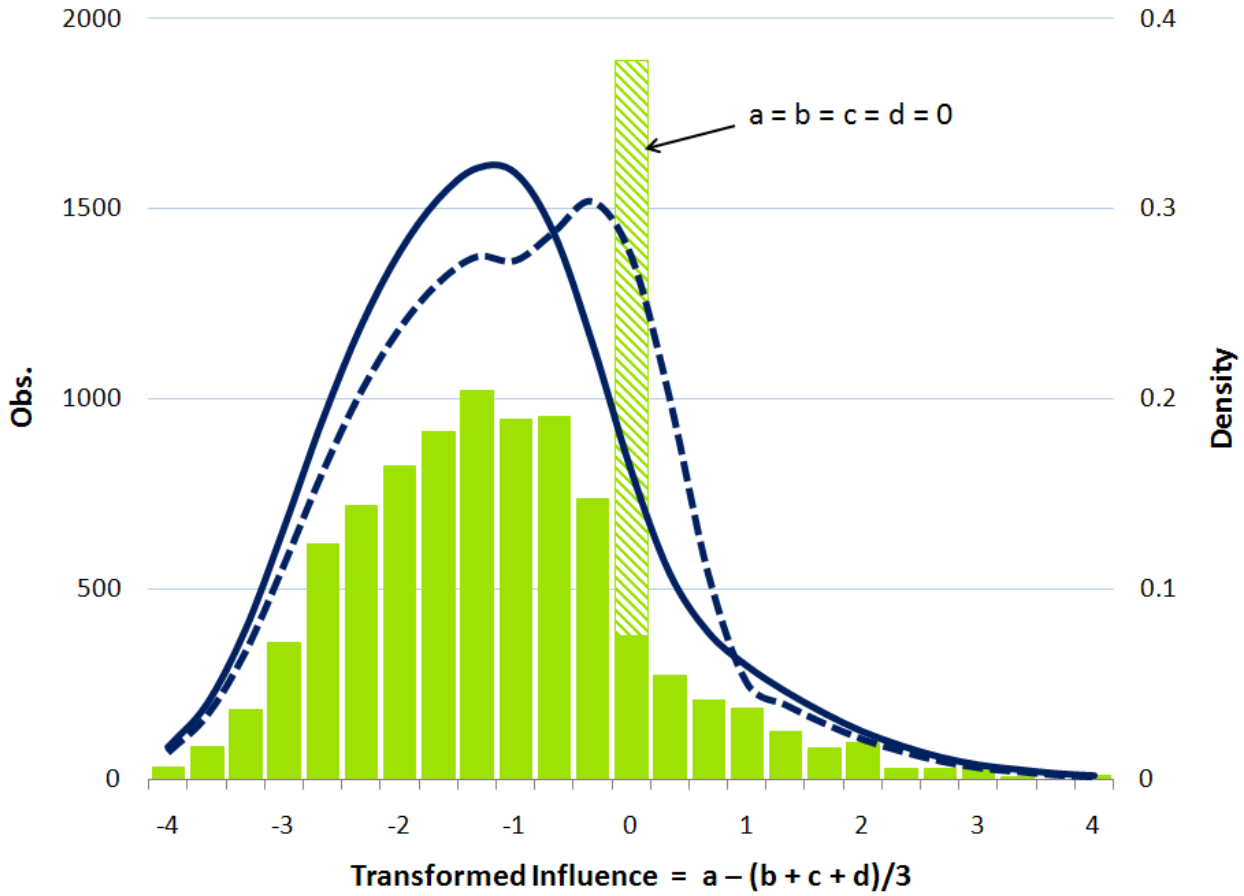
Notes: Graph shows distribution of survey responses to: "How much influence do you think the following groups actually had on recently enacted national laws and regulations that have a substantial impact on your business?"

Figure 2: Self influence of firms relative to perceived influence of other firms



Notes: Graph shows how firms assess influence of three types of other firms (other domestic firms, dominant firms and conglomerates, and firms with personal ties to leaders) based on responding firm assessment of its own influence. Horizontal axis shows response categories for the firm's own influence, histograms show fraction of firms rating the influence of all three other firm types as none (=0), minor (=1), moderate (=2), major (=3), decisive (=4). Dark bars show fraction of observations where firms' own assessment is the same as their assessment of others, shaded and unshaded bars show fractions of observations where others are believed to have greater and lesser influence than the responding firm, respectively.

Figure 3: Transformed influence score



Notes: Section of bar at influence = 0 with cross-hatching shows observations where all categories of firms (including the responding firm) were rated as having no influence. Density functions are plotted with biweight kernels and bandwidths of 1.0; solid line excludes all firms that responded "none" for all categories of firms.

Table 1a: Pairwise correlations of influence components

	(a) Self influence	(b) Influence of other domestic firms	(c) Influence of dominant firms	(d) Influence of politically-connected firms
Self influence (a)	*0.541* *(0.945)*			
Influence of other domestic firms (b)	0.425 (0.000)	*1.160* *(1.106)*		
Influence of dominant firms (c)	0.117 (0.000)	0.424 (0.000)	*1.751* *(1.326)*	
Influence of politically-connected firms (d)	0.012 (0.171)	0.297 (0.000)	0.587 (0.000)	*1.697* *(1.421)*
Transformed influence [a – (b + c + d)/3]	0.569 (0.000)	-0.270 (0.000)	-0.640 (0.000)	-0.680 (0.000)

Table 1b: Correlations of transformed influence with other variables

	a – (b + c + d)/3	
	(1)	**(2)**
Age	0.072 (0.000)	0.086 (0.000)
Exporter	-0.020 (0.061)	-0.039 (0.001)
Domestic	-0.032 (0.004)	-0.035 (0.003)
State-owned	0.114 (0.000)	0.117 (0.000)
Workers (log L)	0.094 (0.000)	0.126 (0.000)
N	8,452	7,349

Notes: Off-diagonal figures in table 1a and figures in table 2b are pairwise correlation coefficients, with significance levels in parentheses. Numbers along the diagonal (in italics) in table 1a are means, with standard deviations in parentheses. Table 1a uses all available observations, while table 1b is restricted to the core sample used in all subsequent regressions. All firms that have responded a = b = c = d = 0 are omitted in column 2 of table 1b.

Table 2: Summary statistics

Variable	N	Mean	Std. Dev.	Min.	Max.
Influence	8,452	-1.02	1.24	-4	4
Age of firm (years)	8,452	19.55	17.72	3	206
Exporter*	8,452	0.20	0.40	0	1
Domestically-owned firm*	8,452	0.86	0.35	0	1
State-owned firm*	8,452	0.07	0.25	0	1
Permanent workers (log, t – 1)	8,452	3.44	1.65	0	9.21
Firm-specific systematic bias (CPI-based)	8,452	0.06	0.02	0.03	0.12
Firm-specific systematic bias (crime-based)	8,452	1.35	1.35	0	4
Lobbied government*	6,919	0.23	0.42	0	1
Country-industry average influence	8,452	0.49	0.36	0	4
Capacity utilization (% of total capacity)	8,060	76.48	20.29	3	120
Total bribes (% sales)	6,220	1.83	3.80	0	50
Bribes for govt. contracts (% of value)	6,580	4.00	8.43	0	100
Overdue receivables (% of sales)	3,020	15.46	22.11	0	100
Losses due to crime (% sales)	7,828	0.95	3.80	0	95
Infrastructure*	8,375	0.09	0.28	0	1
Taxation*	8,452	0.43	0.49	0	1
Regulation*	7,702	0.18	0.39	0	1
Finance*	8,064	0.38	0.48	0	1
Monopoly pricing*	7,818	0.17	0.37	0	1
Collateral requirement*	3,284	0.77	0.42	0	1
Cost of collateral (% of loan value)	2,389	136.25	85.81	1	1000
Informal finance (% of working capital)	8,305	1.44	8.39	0	100
Informal finance (% of new investments)	6,078	0.99	7.44	0	100
Excess labor*	6,647	0.56	0.50	0	1
Tax compliance (% of sales reported)	7,539	77.45	27.81	0	100
Opened new plant or facility (past 3 years)*	7,952	0.15	0.35	0	1
Opened new product line (past 3 years)*	7,958	0.46	0.50	0	1
Closed old plant or facility (past 3 years)*	7,946	0.10	0.30	0	1
Closed obsolete product line (past 3 years)*	7,952	0.26	0.44	0	1
Conducted R&D activities (past year)*	2,475	0.48	0.50	0	1
Output (US$, log)	3,110	7.15	2.64	4.83	18.98
Capital inputs (US$, log)	2,579	5.07	4.09	-9.38	18.24
TFP (US$, log)	2,557	0.01	0.82	-4.21	5.54
Real sales growth (US$, log, 3-year)	2,577	0.24	0.58	-5.99	7.15
Investment (US$, log)	1,650	2.67	2.99	0	18.45
Investment horizon (months)	2,672	9.27	11.03	1	120

Notes: All summary statistics taken from full (unmatched) sample; * denotes dichotomous variable.

Table 3: Firm influence and the costs of doing business

	Total bribe payments (% of sales)					Bribes for procurement (% of contract value)		Unpaid receivables (% of sales)		Losses from crime (% of sales)	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
Influence	-0.184***	-0.184***	-0.158***	-0.152***	-0.152***	-0.340***	-0.387***	-0.697**	-1.003*	-0.039	-0.045
	(0.039)	(0.039)	(0.040)	(0.057)	(0.057)	(0.091)	(0.086)	(0.325)	(0.584)	(0.037)	(0.048)
Age	-0.006*	-0.006*	-0.006**	-0.007**	-0.007**	-0.014***	-0.010***	0.018	-0.001	-0.003	-0.004*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.003)	(0.024)	(0.036)	(0.002)	(0.002)
Exporter	-0.042	-0.043	-0.020	-0.080	-0.080	-0.007	0.063	-2.920*	-4.532**	0.050	0.081
	(0.128)	(0.128)	(0.130)	(0.152)	(0.152)	(0.225)	(0.204)	(1.565)	(2.204)	(0.134)	(0.163)
Domestic	0.327**	0.327**	0.301**	0.310**	0.313**	0.193	0.099	2.044	1.710	0.203*	0.185
	(0.134)	(0.134)	(0.136)	(0.122)	(0.122)	(0.257)	(0.254)	(1.662)	(2.221)	(0.118)	(0.128)
State-owned	-0.643***	-0.641***	-0.596**	-0.607***	-0.613***	-0.637	-0.649*	1.483	3.157	0.125	0.160
	(0.243)	(0.243)	(0.246)	(0.222)	(0.223)	(0.389)	(0.391)	(3.894)	(3.705)	(0.194)	(0.200)
Workers (log L)	0.062	0.062	0.055	0.045	0.046	0.746***	0.515**	2.237	2.415	-0.050	-0.055
	(0.107)	(0.107)	(0.108)	(0.116)	(0.117)	(0.243)	(0.216)	(1.838)	(2.058)	(0.113)	(0.119)
Workers^2	-0.021	-0.021	-0.020	-0.018	-0.019	-0.116***	-0.071***	-0.271	-0.330	0.000	0.001
	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)	(0.029)	(0.025)	(0.204)	(0.238)	(0.013)	(0.013)
Bias (CPI)		-8.553									
		(27.190)									
Bias (crime)			0.318***	0.326***	0.326***	0.536***	0.336***	0.561**	0.786	0.323***	0.401***
			(0.042)	(0.055)	(0.055)	(0.094)	(0.071)	(0.262)	(0.515)	(0.046)	(0.058)
Non-response weighting	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched sub-sample	No	No	No	No	Yes	No	Yes	No	Yes	No	Yes
N	6,531	6,531	6,362	6,046	6,033	6,699	5,699	3,052	1,618	7,952	6,424
k				388	388	413	412	83	77	416	415
Adjusted R^2	0.113	0.112	0.121	0.133	0.133	0.227	0.105	0.250	0.218	0.035	0.036
RMSE	3.594	3.594	3.597	3.696	3.700	7.452	5.797	19.979	23.461	3.813	4.137

Notes: Results from OLS regressions, with legal-status, location, industry, survey-year, and country dummies (not reported). Standard errors are in parentheses in columns 1 – 3; robust standard errors clustered by k country-industry clusters are in parentheses in columns 4 – 11. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Robustness: instrumental variables regressions

	1st stage	2nd stage	1st stage	2nd stage
	Firm influence	**Bribes**	**Firm influence**	**Bribes**
	(1)	**(2)**	**(3)**	**(4)**
Influence		-0.313		-0.469**
		(0.235)		(0.233)
Age	0.003***	-0.006*	0.003***	-0.007**
	(0.001)	(0.003)	(0.001)	(0.003)
Exporter	-0.034	-0.024	-0.020	-0.082
	(0.040)	(0.129)	(0.045)	(0.153)
Domestic	-0.046	0.291**	-0.055	0.289**
	(0.042)	(0.136)	(0.049)	(0.125)
State-owned	0.137*	-0.570**	0.131	-0.556**
	(0.076)	(0.248)	(0.089)	(0.220)
Workers (log L)	-0.026	0.050	-0.012	0.038
	(0.034)	(0.108)	(0.033)	(0.117)
Workers^2	0.009**	-0.018	0.007*	-0.015
	(0.004)	(0.014)	(0.004)	(0.015)
Bias	-0.112***	0.301***	-0.107***	0.292***
	(0.013)	(0.049)	(0.014)	(0.054)
Country-industry average influence	0.806***		0.797***	
	(0.059)		(0.057)	
N	6,362	6,362	6,046	6,046
k			388	388
R^2	0.162	0.130	0.161	0.136
RMSE	1.113	3.579	1.122	3.691
Likelihood ratio	184.627		67.089	
	(0.000)		(0.000)	
Cragg-Donald F	187.784		164.561	
Endogeneity test		0.450		2.059
		(0.502)		(0.151)

Notes: Dependent variable is total bribe payments as a percent of sales. Results are from IV regressions, with legal-status, location, industry, survey-year, and country dummies (not reported). Standard errors are in parentheses in columns 1 and 2; cluster-robust standard errors for k country-industry clusters are in parentheses in columns 3 and 4. All estimations are weighted for non-response bias. R^2 values reported are centered R^2. Likelihood-ratio is Anderson canonical correlation statistic for simple IV, Kleibergen-Paap rank statistic for cluster-robust 2SLS (null hypothesis is that the equation is under-identified). Stock-Yogo critical value (at 95% confidence) for weak-instrument test statistics (Kleibergen-Paap Wald or Cragg-Donald F) is 16.38 for maximum bias of IV estimator to be no more than 10% of the bias (inconsistency) of OLS, i.e., the most stringent criterion. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Political influence and business constraints

Eq.	Dep. Var.	Coeff.	S.E.	*N*	*k*	R^2	Matched sub-sample
(1)	*Infrastructure*	0.005	0.043	8,251	383	0.160	No
(2)		-0.012	0.052	6,713	382	0.199	Yes
(3)	*Taxes*	-0.227***	0.026	8,452	420	0.209	No
(4)		-0.247***	0.029	6,918	419	0.130	Yes
(5)	*Regulations*	-0.227***	0.030	7,623	400	0.178	No
(6)		-0.265***	0.039	6,428	399	0.160	Yes
(7)	*Finance*	-0.145***	0.024	8,163	420	0.215	No
(8)		-0.152***	0.029	6,651	419	0.140	Yes
(9)	*Monopoly*	0.128***	0.027	7,955	411	0.094	No
(10)		0.145***	0.030	6,441	407	0.078	Yes
(11)	*Collateral*	-0.125***	0.040	3,324	360	0.134	No
(12)		-0.124***	0.048	2,792	359	0.142	Yes
(13)	*Collateral*	-3.472**	1.435	2,343	321	0.186	No
(14)	*(% of loan)*	-3.325**	1.617	1,998	320	0.206	Yes
(15)	*Informal finance*	-0.179**	0.081	8,438	419	0.016	No
(16)	*(% of working capital)*	-0.136*	0.076	6,901	418	0.011	Yes
(17)	*Informal finance*	-0.160	0.098	6,173	408	0.002	No
(18)	*(% of investment)*	-0.197*	0.108	4,900	407	0.001	Yes

Notes: Coefficients for "influence" and robust standard errors clustered by k country-industries are reported. All regressions include, in addition to influence, the following variables: age of firm, exporter dummy, domestic dummy, workers (linear and quadratic), firm-specific bias, legal-status, location, industry, time, and country dummies. Equations (1) – (12) show results from logit regressions, (15) – (18) are by OLS. All estimations are weighted for non-response bias. R^2 values reported are pseudo R^2 for logit regressions, adjusted R^2 for OLS. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Political influence, excess labor, and tax compliance

	Excess labor		Tax compliance (% of sales)	
	(1)	**(2)**	**(3)**	**(4)**
Influence	0.090***	0.091***	0.510**	0.565*
	(0.023)	(0.028)	(0.218)	(0.288)
Age	-0.002	0.000	0.049***	0.034*
	(0.002)	(0.002)	(0.018)	(0.019)
Exporter	0.081	0.090	0.438	-0.760
	(0.084)	(0.088)	(0.883)	(0.915)
Domestic	0.172*	0.111	-3.353***	-3.052***
	(0.102)	(0.100)	(0.800)	(0.863)
State-owned	0.171	0.138	3.551**	3.997**
	(0.178)	(0.177)	(1.609)	(1.644)
Workers (log L)	0.215***	0.145*	2.137***	2.609***
	(0.083)	(0.084)	(0.796)	(0.829)
Workers^2	-0.033***	-0.022*	-0.042	-0.121
	(0.011)	(0.012)	(0.090)	(0.094)
Bias	0.092***	0.095***	-1.023***	-0.795***
	(0.024)	(0.032)	(0.224)	(0.250)
Capacity utilization	-0.003**	-0.002		
	(0.002)	(0.002)		
Matched sub-sample	No	Yes	No	Yes
N	6,398	5,054	7,661	6,239
k	400	399	417	416
R^2	0.111	0.092	0.268	0.273
Log likelihood/RMSE	-4,906.254	-3,984.916	23.835	23.914

Notes: All regressions include legal-status, location, industry, time, and country dummies. Robust standard errors clustered by k country-industries are in parentheses. Equations (1) and (2) show results from logit regressions, (2) and (3) are by OLS. All estimations are weighted for non-response bias. R^2 values reported are pseudo R^2 for logit regressions, adjusted R^2 for OLS. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7: Political influence and firm innovation

		Coeff.	S.E.	*N*	*k*	R^2	Matched sub-sample
(1)	*Started new production line*	-0.059**	0.023	7,843	408	0.124	No
(2)		-0.091***	0.026	6,306	407	0.110	Yes
(3)	*Opened new plant*	-0.142***	0.032	7,838	408	0.114	No
(4)		-0.189***	0.040	6,301	407	0.139	Yes
(5)	*Closed obsolete production line*	-0.087***	0.029	7,838	408	0.119	No
(6)		-0.159***	0.031	6,301	407	0.131	Yes
(7)	*Closed old plant*	-0.088***	0.031	7,832	408	0.086	No
(8)		-0.085**	0.038	6,295	407	0.101	Yes
(9)	*Engaged in R&D activities*	-0.083**	0.041	2,256	48	0.078	No
(10)		-0.148***	0.053	783	40	0.164	Yes

Notes: Coefficients for "influence" and robust standard errors clustered by k country-industries are reported using logit estimation. All regressions include, in addition to influence, the following variables: age of firm, exporter dummy, domestic dummy, workers (linear and quadratic), firm-specific bias, legal-status, location, industry, time, and country dummies. All estimations are weighted for non-response bias. R^2 values reported are pseudo R^2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Political influence, productivity, and investment

	Real sales growth (log)		Investment (log)		Investment horizon (months)		Output (log)		Total factor productivity	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Influence	-0.015*	-0.033**	-0.053	-0.129**	-0.054***	-0.058***	-0.028***	-0.026	-0.028***	-0.035*
	(0.009)	(0.016)	(0.041)	(0.064)	(0.005)	(0.008)	(0.009)	(0.016)	(0.009)	(0.017)
Workers (Log L)	0.038	0.003	0.260	0.256	0.129***	0.151***	0.385***	0.301***	-0.829***	-1.012***
	(0.054)	(0.068)	(0.249)	(0.248)	(0.021)	(0.024)	(0.042)	(0.048)	(0.130)	(0.150)
Workers^2	0.001	0.002	0.071***	0.066**	0.004*	-0.002			0.092***	0.096***
	(0.005)	(0.007)	(0.025)	(0.029)	(0.002)	(0.003)			(0.013)	(0.015)
Capital inputs (Log K)							0.370***	0.280***		
							(0.027)	(0.061)		
Age	-0.004***	-0.002	-0.011**	-0.014*	0.001**	0.000	0.001	-0.004	-0.001	-0.006*
	(0.001)	(0.001)	(0.005)	(0.008)	(0.000)	(0.001)	(0.002)	(0.003)	(0.002)	(0.003)
Exporter	-0.035	-0.065	0.047	-0.228	0.074***	0.071***	0.182***	0.031	0.128**	-0.023
	(0.034)	(0.055)	(0.291)	(0.424)	(0.017)	(0.025)	(0.055)	(0.095)	(0.049)	(0.076)
Domestic	-0.015	0.003	-0.546*	-0.573	-0.125***	-0.047*	-0.213**	-0.184*	-0.098	-0.002
	(0.056)	(0.066)	(0.285)	(0.368)	(0.020)	(0.025)	(0.095)	(0.098)	(0.105)	(0.110)
State-owned	-0.052	-0.118	-0.149	-0.040	-0.235***	-0.275***	-1.529***	-1.469***	-1.629***	-1.685***
	(0.111)	(0.121)	(0.363)	(0.386)	(0.084)	(0.089)	(0.518)	(0.517)	(0.510)	(0.490)
Bias					0.004	0.008				
					(0.005)	(0.007)				
Matched sub-subsample	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	2,592	1,183	1,662	881	2,699	1,328	2,580	1,134	2,580	1,134
k	87	80	66	58			72	66	72	66
Adjusted R^2	0.053	0.056	0.325	0.273			0.871	0.901	0.149	0.233
RMSE	0.570	0.636	2.444	2.881			0.782	0.838	0.758	0.819

Notes: Notes: All regressions include legal-status, location, industry, time, and country dummies. Equations (1) – (8) are from OLS, equations (9) and (10) from Poisson event-count regression. Robust standard errors clustered by k country-industries are in parentheses. All estimations are weighted for non-response bias. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix: Coding of selected variables

Variable coding	Wording of source question
Age *(survey year – Q1)*	Q1. In what year did your firm begin operations in this country?
Exporter *(coded 1 if Q2.ii > Q2.i or Q2.iii)*	Q2. What percent of your establishment's sales are: i) sold domestically ii) exported directly iii) exported indirectly (through a distributor)
Domestic *(coded 1 if Q3.i selected)* *State-owned* *(coded 1 if Q3.ii selected)*	Q3. Which of the following best describes the largest shareholder or owner in your firm? (Multiple answers allowed) i) Domestic company ii) Government or government agency iii) [other response categories omitted]
Lobbied government *(coded 1 if Q4 = yes)*	Q4. Think about national laws and regulations enacted in the last two years that have a substantial impact on your business. Did your firm seek to lobby government or otherwise influence the content of laws or regulations affecting it? (yes/no)
Bribes	Q5. We've heard that establishments are sometimes required to make gifts or informal payments to public officials to "get things done" with regard to customs, taxes, licenses, regulations, services, etc. On average, what percent of annual sales value would such expenses cost a typical firm like yours?
Bribes for government contracts	Q6. When establishments in your industry do business with the government, how much of the contract value is typically expected in gifts or informal payments to secure the contract?
Overdue receivables	Q7. What percent of your sales to private customers involve overdue payments?
Losses due to crime	Q8. Please estimate the losses (as a percent of total sales) of theft, robbery, vandalism or arson against your establishment in the last year?
Infrastructure *(coded 1 if Q9.i + Q9.ii + Q9.iii ≥ 9)* *Taxation* *(coded 1 if Q9.iv + Q9.v ≥ 6)* *Regulation* *(coded 1 if Q9.vi + Q9.vii ≥ 6)* *Finance* *(coded 1 if Q9.viii + Q9.ix ≥ 6)* *CPI-based systemic bias* *(residual from regression of Q 9.x on inflation)* *Crime-based systemic bias (Q9.xi)*	Q9. Please tell us if any of the following issues are a problem for the operation and growth of your business. If an issue poses a problem, please judge its severity as an obstacle on a four-point scale where: 0 = No obstacle; 1 = Minor obstacle; 2 = Moderate obstacle; 3 = Major obstacle; 4 = Very severe obstacle i) Telecommunications ii) Electricity iii) Transportation iv) Tax rates v) Tax administration vi) Customs and trade regulations vii) Business licensing and operating permits viii) Access to financing (e.g. collateral) ix) Cost of financing (e.g. interest rates) x) Macroeconomic instability xi) Crime, theft, and disorder
Monopoly *(coded 1 if Q10.i selected)*	Q10. Now I would like to ask you a hypothetical question. If you were to raise your prices of your main product line or main line of services 10% above

	their current level in the domestic market (after allowing for any inflation) which of the following would best describe the result assuming that your competitors maintained their current prices? i) Our customers would continue to buy from us in the same quantities as now ii) Our customers would continue to buy from us, but at slightly lower quantities iii) Our customers would continue to buy from us, but at much lower quantities iv) Our customers would stop buying from us
Collateral *(coded 1 if Q11 = yes)*	Q11. For the most recent loan or overdraft, did the financing require collateral or a deposit? (yes/no)
Excess labor *(coded 1 if Q12 <100%)*	Q12. If you could change the number of regular full-time workers you currently employ without any restrictions (i.e. without seeking permission, making severance payments etc.), what would be your optimal level of employment as a percent of your existing workforce? (E.g. 90% implies you would reduce your workforce by 10%, 110% means you want to expand by 10%)
Tax compliance	Q13. Recognizing the difficulties many enterprises face in fully complying with taxes and regulations, what percentage of total sales would you estimate the typical establishment in your area of activity reports for tax purposes?
New plant *(coded 1 if Q14.i = yes)* *New product* *(coded 1 if Q14.ii = yes)* *Closed plant* *(coded 1 if Q14.iii = yes)* *Closed product* *(coded 1 if Q14.iv = yes)*	Q14. Has your company undertaken any of the following initiatives in the last three years? (yes/no) i) Opened a new plant ii) Developed a major new product line iii) Closed at least one existing plant or outlet iv) Discontinued at least one product (not production line)
Conducted R&D activities *(coded 1 if Q15 > 0)*	Q15. How much did your establishment spend on design or R&D last year? (Spending includes wages and salaries of R&D personnel, such as scientists and engineers; materials, education costs, and subcontracting costs.)

Source: World Bank (2002).