The Price of Doing Business: Why Replaceable Foreign Firms Get Worse Government Treatment*

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Abstract

We argue that a host government treats foreign firms better if those foreign firms have fewer replacements. We identify a key structural determinant of replaceability: the startup costs that foreign firms must incur to begin production. Since the host government can only take from foreign firms that actually produce in its market, it must treat foreign firms better when their startup costs are high, lest the government drive all foreign firms out. Our theoretical model applies contemporary trade theory to foreign direct investment and provides insights about the understudied relationship between foreign and domestic firms. Most importantly, it endogenizes market entry and exit, establishing the importance of entry despite scholars' long-time focus on exit. Our analysis uses cross-national firm-level data on taxes and production outcomes, and we provide a new industry-level measure of government treatment of foreign firms.

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1 Introduction

A canonical argument in political economy is that an individual who can more profitably exit from an institution has more power to secure her preferred outcome within that institution (Hirschman, 1970). This leverage is particularly important in international relations because the anarchic nature of the international system allows states to act independently and violate existing cooperative agreements (Johns, 2007; Voeten, 2001). Yet the role of exit may be overstated when considering the power of firms within the global economy. Unlike states, which cannot be easily replaced in the international system, the exit of one multinational corporation can often be offset by the entry of a new one.

Consider the Trump International golf course in Aberdeen, Scotland. After a large offshore windfarm within sight of the course was proposed in 2012, Donald Trump wrote to the Scottish Prime Minister that the "monstrous turbines" would turn the country into "a third world wasteland that global investors will avoid."[1] Trump has tweeted about this at least sixty times, complained to the Scottish Parliament, taken a court case to the UK Supreme Court, and pressed onetime UKIP leader Nigel Farage to cancel the project.[2] Nonetheless, the Scottish government approved the plans, and the first of eleven planned turbines went up in April 2018. Why have Trump's complaints gone unaddressed? Shouldn't Scotland fear that Trump will pull his investment, leaving the Scottish economy in the lurch?

Conventional political economy accounts might emphasize that Trump cannot take his investment and leave: the golf course cannot be packed up and moved. Yet Trump spent relatively little money developing the golf course in the first place, and the equipment used to maintain the course can be easily moved to another location. We argue that the central feature of Trump's dilemma is not that he has sunk a lot of money into assets that cannot be moved, but rather that he is easily replaceable (especially in the home of golf). The costs of building a golf course are relatively trivial; it would be relatively easy for a new investor to start her own golf course, even if she had to purchase new equipment and rebuild the golf course from scratch. We suggest that low startup costs—which lead to high replaceability—are a key explanation for Trump's inability to get his way. There would be another firm waiting in the wings to replace the Trump Organization were it to exit—which it has not.

Our key contribution is to consider the political effects of *startup costs*: the one-time costs

[1] Drury, Colin. "World's Most Powerful Wind Turbine Goes Up Off Scottish Coast—Despite Trump's Opposition." *The Independent*. 11 April 2018.

[2] Griffiths, Brent. "Trump Tweeted About Scottish Wind Farm 60 Times." *Politico*. 22 November 2016.

a firm must pay to enter a market. Think of the minimum amount a firm needs to invest to begin production, such as building a factory, renting office space, acquiring basic machinery, or otherwise. While quantifying this concept requires considerable humility, we intend the reader to share our baseline intuition that such a concept exists: a firm clearly incurs costs in order to make its first dollar in a new market. We also intend the reader to share two key intuitions about how this concept varies. First, startup costs vary as a result of exogenous, structural aspects of the firm's industry: the costs necessary to earn one dollar in revenue in a new market differ depending on whether that revenue comes from an oil refinery, a management consulting business, a retail store, or a lumber mill.[3] Second, and crucially, aspects of the host state market that are exogenous to the firm also generate variation.[4] The costs necessary to earn one dollar of revenue from operating a 5G network vary as a result of geography, population, GDP per capita, transportation, infrastructure, the depth of local financial markets, the availability of predecessor technologies, etc. Our aim here is to conceptualize these exogenous startup costs in a host state independent of costs at entry that are endogenous to politics. Thus, one contribution of our approach is to elucidate how firm decision-making is shaped by the exogenous, structural aspects of the host state market, upon which government actions at entry—from investment incentives to FDI restrictions—are layered.

Startup costs affect how host a government treats foreign firms via a mechanism of replaceability, or how easily an existing firm can be replaced by a new firm. When an industry has low startup costs, new firms can more easily replace existing firms that choose to exit a market. The host government can therefore take more from foreign firms that face low startup costs, and be less concerned that such takings will deter future economic activity. Conversely, when firms in a given industry must pay higher startup costs to begin producing goods and services in a given host state market, entry is more cost-prohibitive. Thus, foreign firms that exit the market are less likely to be replaced by new foreign firms, so high government takings are more likely to deter future economic activity. As a result, the host government attempts to offset the burden of startup costs in these industries by offering more favorable treatment to foreign firms. This mechanism of replaceability generates our main, novel hypothesis: for foreign firms, higher startup costs are associated with better government treatment. Moreover, our account establishes that the association between high startup costs and favorable treatment remains year-in and year-out, not simply at entry.

[3] Our consideration of this variation expands on the well-known result that the costs of redeploying assets is a key, structural determinant of government treatment via "obsolescing bargains" (Vernon, 1971; Kobrin, 1987; Frieden, 1994).

[4] To be precise, there exist characteristics of the host state market that are plausibly exogenous to the firm in the short-run relevant to firm decision-making.

To establish the political importance of startup costs, we examine government treatment of foreign firms using a firm-level political economy model of foreign direct investment in a host state with multiple industries. Importantly, our model accounts for not just foreign but also for understudied domestic firms in a host state. We model a host government that regulates its own market, and can take rents from foreign firms using regulatory policies that discriminate between domestic and foreign firms. In equilibrium, individual domestic and foreign firms enter and exit the market over time in response to changes in their underlying productivity, thus reflecting our mechanism of replaceability. Yet the market is stationary, meaning that aggregate features of the market—such as the share of domestic/foreign firms—remain constant over time. Additionally, the host government's optimal strategy is stationary: government treatment can vary across industries, but it is stable over time for each given industry. Our model therefore differs from the existing literature, in which firms learn new information from government policies and governments develop reputations over time. In contrast, our model isolates an alternative causal mechanism: that government actions are constrained by the entry and exit of firms over time, irrespective of information about a government's preferences or reputational concerns.

We aim to creatively test observable implications of our theory, given empirical limitations in matching our theoretical concepts to data. We develop a novel measure of industry-state startup costs using firm-level data in up to 96 disaggregated industries in up to 150 states (2008–2016).[5] We also use tax information as reported in firms' income statements to construct novel measures of government treatment.[6]

First, we present evidence that higher foreign tax burdens increase the divergence between observed foreign and domestic firm productivity. Then we present results about our key substantive interest: the relationship between startup costs and government treatment. While this evidence does not reach conventional levels of statistical significance, it consistently matches our expectations across many specifications and robustness checks. Finally, we provide extensive indirect evidence for our theoretical argument. This indirect evidence leverages the impact of selection—namely, the entry and exit of firms over time—on observable firm attributes, including the productivity and revenues of foreign firms. When this evidence is assessed holistically, we have compelling evidence for our theoretical argument.

[5] We measure industry at the NAICS 3-digit level. Bureau van Dijk Osiris databases. `bvdinfo.com`. Accessed July 2017.

[6] Our article does not offer a theory of optimal taxation. Rather, we introduce firm-level tax data as a creative proxy for differential government treatment of FDI, a key research topic in international political economy. Nonetheless, we hope that by employing tax data, we might encourage further scholarship on the role of taxation in foreign investor-host state relation in the vein of Wallerstein and Przeworski (1995) and Hallerberg and Basinger (1998).

This paper makes key theoretical and empirical contributions to political science. Our theoretical model adapts existing formal models of international trade with firm heterogeneity (Melitz, 2003; Melitz and Redding, 2014), which a spate of important political science scholarship has tied to the politics around trade policy (Gulotty, 2017; Baccini, Pinto and Weymouth, 2017; Kim, 2017; Osgood et al., 2017; Owen and Quinn, 2016; Queralt, 2017). Our model substantially expands on prior trade models by modeling government policies and market entry and exit as strategic decisions. We expect these modeling innovations to be applicable to a variety of work in political economy. Further, this paper makes an important empirical contribution by identifying, measuring, and disseminating a previously unexamined determinant of cross-industry and cross-state variation in government treatment: the (exogenous) startup costs paid by firms that enter a market. Together, our theoretical and empirical innovations allow us to establish how structural constraints on producing abroad shape the extent to which host governments can pursue their domestic agendas while still attracting foreign capital.

2 Determinants of Government Treatment of Foreign Firms

A rich literature in international political economy examines the determinants of government treatment of foreign firms. The first strand of this literature emphasizes an important firm- and industry-level attribute: mobility, which is the share of startup costs that a firm can take when it exits a market (Kobrin, 1987; Frieden, 1994; Jensen and Johnston, 2011; Hajzler, 2012). The concept of mobility is thus fundamentally linked to firm decisions about whether to exit a market.

Previous scholars argue that when firms invest abroad, they expose themselves to poor treatment by the government of the host (receiving) state. This problem is thought to be most acute for firms that make longer-term investments and acquire at least some managerial control over operations abroad through foreign direct investment (FDI). Host governments that are eager for the positive developmental effects of FDI have reason to lure investors in with the promise of favorable regulatory treatment (Pandya, 2014; Jensen and Malesky, 2018; Jensen, Malesky and Walsh, 2015). Yet even if a host government is genuine when making initial promises, sometimes it may later wish to retract incentives, increase environmental or labor standards, or increase corporate taxes. The expectation born of the "obsolescing bargain" theory is that when firms cannot profitably exit their existing investments in response to such policy changes, the host government can continue to benefit from their FDI even as it changes the regulatory environment (Vernon, 1971). To explain variation in firms' ability to exit, scholars have converged on industry-level mobility, or the extent

to which a foreign firm can recoup and redeploy its initial capital outlay when it exits a market (Kobrin, 1987; Frieden, 1994; Jensen and Johnston, 2011; Hajzler, 2012). When a foreign firm in a low-mobility industry invests, it cannot easily recoup its initial investment in response to changes in the regulatory environment.

This dominant literature comes with several limitations. First, arguments about the effects of mobility and exit often focus on extreme forms of treatment, particularly direct expropriation and government breach of contract (Li, 2009; Wellhausen, 2015*a*; Jensen et al., 2012; Graham, Johnston and Kingsley, 2016). We do not presume that adverse government treatment necessarily results in such severe property rights violations. Instead, we widen our focus to the relationship between attributes of firms and the array of government policies relevant to them, such as subsidies, environmental and labor policies, and taxation. In doing so, we highlight that government treatment of foreign firms can be marked not just by extreme, unlawful events, but also by relative stability.

Second, the mobility literature overlooks the role of domestic firms in the market, and specifically the host government's priorities when it comes to domestic firms.[7] As economic globalization has deepened, perceptions that foreign firms are privileged over domestic firms have contributed to frustration with FDI. For example, critics point to the fact that modern international investment law designed to protect property rights applies only to foreign, and not to domestic, firms (Waibel, 2010). Host governments are engaged in a delicate balance as they work to attract FDI while nonetheless promoting domestic entrepreneurship, especially in middle-income and developing countries. For example, Hungary has famously welcomed foreign ownership since it underwent economic transition in the 1990s, which has led to foreign dominance in high-profile industries. In the 2010s, a key part of the far-right Orban regime's rhetoric has been to "politicize dependency" and prioritize domestic firms. Yet such rhetoric exists alongside the "quiet politics" of continued subsidies to FDI in manufacturing (Bohle and Greskovits, 2018). We take the host government's interest in balancing between foreign and domestic firms seriously, particularly by examining how the host government's treatment of foreign firms affects economic conditions for domestic firms.

Third, and most importantly, the mobility literature, which focuses on exit, has largely ignored the potential role of entry in shaping government regulation. Literature focused on exit typically rests on implications of exit for entry. For example, scholars who theorize around exit often invoke reputation-based arguments, such that mistreatment of one foreign firm impacts entry

[7] For notable exceptions, see Kosova (2010) and Betz and Pond (2019).

by other foreign firms (Tomz, 2007; Li, 2009; Allee and Peinhardt, 2011; Albertus and Menaldo, 2012). Yet even if reputation effects exist among current and potential foreign investors, prior adverse government treatment need not drive future FDI to zero (Johns and Wellhausen, 2016), nor does it appear to do so in practice (Wellhausen, 2019). By taking entry as our starting point, we engage with the reality that the deterrent effect of (the threat of) exit is probabilistic: when one firm exits a market, another sometimes enters the market and replaces it. This allows us to take seriously the proposition that a host government's ability to set its preferred regulatory policies is affected by explanations for entry beyond those derived from the implications of exit.

A few other strands of literature are relevant to our analysis, although they are not our main focus. First, many scholars have examined an attribute of consumer preferences: the elasticity of substitution, which captures how easily a consumer can substitute different varieties of goods within a given industry. Many theoretical scholars have examined the complex impact of this attribute on international trade flows and barriers (Krugman, 1980; Chaney, 2008). This literature has in turn spawned a rich empirical literature on international trade. Because our theoretical focus is on firm startup costs, we treat the elasticity of substitution as a control variable. All of our theoretical results are ceteris paribus claims; that is, they hold the elasticity of substitution constant. We also control for this variable in robustness checks of our empirical analysis. Interested readers can extend our model to examine how changes in consumer preferences affect FDI.

Second, a large literature examines the relationship between trade and FDI. Many scholars posit that firms tariff jumpthat is, when tariffs are high, firms may shift production to foreign market rather than exporting their goods abroad (Helpman, Melitz and Yeaple, 2004). Similarly, many scholars have examined how industry attributes affect the decision by firms about how to structure their supply chains (Antras and Helpman, 2004). This literature thus posits a key relationship between trade policy and FDI. However, it does not address the focus of our analysis: how foreign firms are treated by a government once they engage in FDI. Future research might incorporate our insights into the literature on the relationship between trade and FDI.

Finally, many scholars have examined the influence of political institutions on government treatment of foreign firms. Scholars have demonstrated the influence of many such factors, including: regime type; federalism; government turnover (especially between capital- or labor-friendly parties); benefits for unskilled workers; and dependence on international institutions like the IMF or the World Bank (Jensen, 2006; Li, 2009; Pandya, 2010; Pinto, 2013; Biglaiser, Lee and Staats, 2016; Jensen et al., 2012). As described below in our discussion of model extensions, we can

extend our results to utility functions in which a government trades off the welfare of various constituency groups. This might be a profitable way for future research to examine the role of political institutions.

In theorizing around entry, our explanatory variable is a previously-ignored industry attribute: startup costs, which are the one-time costs a firm must pay to enter a market. Startup costs can include the costs of buying necessary machinery at the prevailing world price and shipping it to the new location. They can also include things like the construction costs of building new facilities or the rental costs of acquiring office space. If a firm finds the local infrastructure insufficient to facilitate transport of produced goods, startup costs can include the cost of activities like cutting and paving roads. We imagine that executives can and do know the costs of establishing facilities in a given industry and a given physical environment. We conceptualize startup costs as exogenous, although we consider at length below aspects of startup costs that are endogenous to government treatment at entry.

We argue that startup costs influence our outcome variable: a host government's takings rate, which is the amount per unit of production that the government takes from each foreign firm. Such takings can be achieved via regulatory policies that transfer utility from foreign firms to the host government. Such policies can include confiscatory takings, as well as perfectly legal and legitimate forms of regulation, such as environmental rules, labor protections, and taxation. Any regulatory policy that raises the cost of production for a foreign-owned firm and provides utility to the host state matches our conception of government takings.

The causal mechanism that links startup costs to the takings rate is replaceability, which is how easily an existing firm can be replaced by a new firm. Like the obsolescing bargain literature, we allow firms to exit a market in response to changes in government policy. However, unlike the obsolescing bargain literature, we allow such firms to be replaced by new foreign firms that choose whether to enter the market. Such entry and exit decisions are driven endogenously by government policies that determine the takings rate. We assume that a host government cares about its ability to seize rents in both the short- and long-term. Higher government takings increase the amount that the government receives from each unit of foreign production, but reduces the overall amount of foreign production, because higher takings drive existing firms from the market and make it less attractive for new firms to enter. As startup costs for foreign firms increase, the entry problem becomes exacerbated: new foreign firms are less likely to replace departing firms, meaning that a government must lower its takings rate to maximize its overall rents. Therefore, startup costs affect

government takings via the mechanism of replaceability. Market forces implicitly and endogenously affect the host government's treatment of foreign firms. We include both mobility and the elasticity of substitution as control variables in our theoretical model and empirical tests to demonstrate that our argument is a complement, rather than a competitor, to existing research.

By modeling interactions at the firm-level, we can provide the theoretical microfoundations for why some firms select into participation in the global economy through FDI and others do not. This approach also comes with empirical benefits. As we describe below, we take a novel and, in our judgment, compelling approach to measuring government treatment via tax burdens. Yet we cannot be confident that tax burdens characterize the full spectrum of government treatment of foreign firms. However, our model allows us to derive indirect tests of our causal mechanism by examining the attributes of firms that select into FDI, including the productivity of foreign and domestic firms, and foreign firm revenues. Thus, we can use relationships between our variables of interest and standard measures of financial concepts to provide indirect evidence in support of our political economic theory.

3 Theory

Our model of FDI is based on the economic microfoundations of contemporary trade theory, as established in Melitz (2003) and subsequently extended to economies with multiple industries by Melitz and Redding (2014).[8] In these trade models, firms decide whether to produce goods that can be sold in the firm's domestic market and/or exported abroad for sale in foreign markets. Firms differ from one another based on both the unique goods that they produce,[9] and their inherent productivity in producing their good. In every period, a small portion of firms experience an exogenous shock that causes them to "die", or go out of business. Melitz (2003) and subsequent follow-on papers assume that the market has a stationary structure, as the firms that exogenously exit the market are replaced by new firms that endogenously decide to start new production.[10] The main result in Melitz (2003) is that exporting firms must be more productive than firms that just produce for the domestic market, because they must overcome the added exogenous transportation costs for exporting goods to foreign markets.

Rather than modeling trade across countries, we instead model decisions by both domestic

[8] These microfoundations are used in almost all contemporary trade theory models that introduce firm-level heterogeneity.

[9] That is, firms engage in monopolisitic competition, per Dixit and Stiglitz (1977).

[10] This concept of market stationarity with firm-level entry and exit was earlier developed in Hopenhayn (1992).

and foreign-owned firms about whether to invest in the production of goods within a single market. Just as Melitz (2003) assumes that exporters face added transportation costs, we assume that foreign-owned firms face the potential for discriminatory treatment, in which government takings increase the marginal cost of production for foreign-owned firms.[11] Our theory includes two major innovations that accord with our substantive focus on FDI. First, we assume that government takings are endogenously chosen by a strategic host government (and hence are not exogenous, like Melitz's transportation costs). Second, we assume that firms endogenously choose whether to exit the market (unlike Melitz, which assumes that a small portion of firms exogenously dies). Domestic and foreign-owned firms thus both enter and exit the market over time in response to changes in their firm-level productivity, which we allow to fluctuate over time. Other factors that affect entry and exit decisions are: the startup cost of beginning production, the mobility of capital that has previously been invested in production, and the treatment provided by the host government to foreign investors.

3.1 Model Primitives and Structure

We focus on the unique stationary equilibrium of an economy of a single country that has $J + 1$ industries and a labor force of size L. We assume that industry $j = 0$ produces a homogenous good, which serves as our numeraire good. We assume that all other sectors ($j = 1, \ldots, J$) produce differentiated goods. Firms can be either domestically- or foreign-owned, and each firm can produce a unique good from a set of industry-level varieties, $v \in V_j$. Whether a firm actually produces its good is an attribute of equilibrium behavior. At any given point in time, there are both domestic and foreign firms that are currently producing for the market; we describe these producing firms as being "in" the market. Similarly, there are also domestic and foreign firms that are not currently producing for the market; we describe these latent firms as being "out" of the market.

We assume that consumers have a preference for a variety of goods within an industry, and let $\sigma > 1$ denote the constant elasticity of substitution across goods within an industry. These consumers both buy goods and serve as the labor force that produces these goods. We let $q_j(v)$ denote the quantity of consumption of a specific variety v in industry j, and we let w_j denote the relative weight that consumers place on goods across industries, such that $\sum_j w_j = 1$. Consumer

[11] Here we focus on the treatment of foreign firms for substantive reasons, in keeping with a substantial body of work that examines adverse government treatment of foreign firms relative to domestic firms (for an overview, see Graham, Johnston and Kingsley 2016). However, as discussed below, our framework can also be extended to examine the treatment of domestic firms as well.

utility from aggregate consumption (across all industries) is:

$$U = \sum_{j=0}^{J} w_j \log Q_j \quad \text{where: } Q_j \equiv \left[\int_{v \in V_j} q_j(v)^{\frac{\sigma-1}{\sigma}} \, dv \right]^{\frac{\sigma}{\sigma-1}} \tag{1}$$

The index Q_j represents consumer utility from consuming the goods produced by industry j using the standard functional form in the monopolistic competition literature, as first introduced by Dixit and Stiglitz (1977). Consumers must optimize their utility subject to the budget constraint:

$$\sum_{j=0}^{J} \int_{v \in V_j} p_j(v) \, q_j(v) \, dv \leq R \tag{2}$$

where $p_j(v)$ is the price of good v in industry j, and R is aggregate revenue.

The game takes place over discrete time periods. At the start of every period, there are four different groups of firms in each industry. First, there are both foreign and domestic firms that are already "in" the market because they produced goods in the previous period. Second, there are both foreign and domestic firms that are "out" of the market because they did not produce goods in the previous period. In each period t, the game begins when each firm decides whether to pay a small informational cost, $\beta > 0$, to learn its type for that period, φ. This type variable corresponds to the firm's productivity in producing its unique good. Each firm's type variable is independently and identically distributed across both players and times. We assume that Nature chooses a firm's type (i.e. productivity) according to the Pareto distribution.[12] A firm cannot produce without first learning its type.[13]

The government then announces a takings rate τ. This rate corresponds to the amount per unit of production that the government takes from each foreign firm.[14] We allow it to vary across industries.[15] After hearing the government's announcement, each firm decides whether to produce its good in that period. Those firms that are currently "out" of the market (meaning that they

[12] This is a standard assumption in firm-level models because of the Pareto distribution's analytical tractability, and because it closely matches the empirical distribution of FDI and trade data (Chaney, 2008; Helpman, Melitz and Yeaple, 2004).

[13] The cost of learning type can vary across foreign and domestic firms, across firms that were "in" or "out" of the market in the previous period, and across industries. If the information cost varies across firms that are "in" and "out", the magnitude of this difference must be limited, as detailed in the Appendix. This informational cost can be arbitrarily small, but is necessary in models of market competition to ensure that there is stability in a market's size over time.

[14] To simply our presentation, we assume that this taking does not apply to domestic firms. Empirically, we measure the takings rate as the amount taken in tax per production as accounted for by pretax income.

[15] Throughout this discussion, we suppress the notation for different industries for the sake of clarity.

did not produce in the previous period) can choose to either remain out—without incurring any additional costs or generating any revenue in the market—or enter the market and begin producing goods for sale. As shown in Figure 1, firms that are "out" of the market must pay a startup cost, κ_i, in order to enter the market and establish production facilities. We allow the startup costs faced by domestic firms, κ_d, to differ from the startup costs faced by foreign firms, κ_f.[16] In contrast, firms that are "in" the market at the beginning of the time period (because they established production facilities in prior periods) can decide either to stay in the market and produce goods in period t, or to take their mobile capital and leave the market. We measure mobility as the share $\mu_i \in [0, 1]$ of startup costs that a firm can take when it leaves the market. We allow the mobility of domestic firms, μ_d to differ from the mobility of foreign firms μ_f.[17] We assume that this decision about whether to stay or leave the market must be made prior to the actual production of goods in any given period.[18] Over time, we allow firms to move both in and out of the market multiple times; that is, we do not assume that firms "die" based on exogenous and unexplained shocks, as in Melitz (2003). A firm's decision to exit a market can always be reversed in a future period, albeit after paying the informational cost (to learn its productivity for that period) and the startup cost (to re-enter the market).

[Figure 1 goes here.]

Because each firm produces a unique good, we can refer to each good by the productivity of the firm that produces it. That is, if a firm of type φ' produces good v', we can use the terms $p(v')$ and $p(\varphi')$ interchangeably. We can now consider the production decisions by firms. Since these decisions are driven by productivity levels, we accordingly use φ as our relevant parameter, rather than v. We assume that production uses only one input, domestic labor, and there is a fixed production cost in each period, $c > 0$, which is measured in terms of a unit of labor.

For a firm with a productivity φ, we let $p(\varphi)$ denote the price and $q(\varphi)$ denote the quantity of the differentiated good produced by the firm. The profit function for a firm is its interim utility after learning its type and deciding to produce. For a domestic firm (which does not pay a taking

[16] For the results we present here, we do not need to make any assumptions about which type of investor has higher startup costs.

[17] For the results we present here, we do not need to make any assumptions about which type of investor has higher mobility.

[18] So if a firm produces goods in a given period, it must wait until the next period before it can again decide whether to exit. This accords with the definition of startup costs as the fixed assets necessary to produce goods.

to the government), the profit function is:

$$\pi_d(\varphi) = p_d(\varphi)q_d(\varphi) - \left[\frac{q_d(\varphi)}{\varphi} + c\right]$$

Higher levels of productivity therefore correspond to lower unit production costs. Since a foreign firm must pay an additional per unit taking to the government, its profit function is:

$$\pi_f(\varphi) = p_f(\varphi)q_f(\varphi) - \left[\frac{q_d(\varphi)(1+\tau)}{\varphi} + c\right]$$

Note that this profit function assumes that more productive firms can both produce goods and pay the government takings rate at a lower cost in units of labor. These profit functions represent the interim utility of a firm that has already paid the information cost (β) to learn its type and the startup cost (κ_d or κ_f) to enter the market.

3.2 Equilibrium Behavior

The full derivation of equilibrium behavior is included in the Appendix. We begin by examining market behavior after the government has announced its takings rate for each industry:

Proposition 1. *For any given takings rate, $\tau \geq 0$, there exist types x_i and y_i, for $i = d, f$, such that $0 < x_i < y_i$. Firms that are in the market decide to exit if $\varphi < x_i$, and stay and produce if $x_i \leq \varphi$. Firms that are out of the market decide to stay out if $\varphi < y_i$, and enter and produce if $y_i \leq \varphi$.*

As shown in Figure 2, those firms that are already "in" the market will find it profitable to stay and produce as long as they have moderate or high levels of productivity ($x_i < \varphi$). If a firm that is already in the market has low productivity for the period, it cannot compete profitably against the other firms in the market; accordingly, it will exit, taking its mobile capital with it. However, those firms that are "out" of the market will only enter and pay the accompanying startup cost if they have high levels of productivity ($y_i < \varphi$). If their productivity is either low or moderate, they cannot profitably pay the startup cost to enter the market and compete against other firms.

[Figure 2 goes here.]

To understand strategic behavior by the government, we must first understand how changing the takings rate for an industry affects economic outcomes. When the government increases the

takings rate, it increases the unit cost of production for foreign firms. This increase in production cost means that each foreign firm produces less and earns lower revenue. Since production is less lucrative, existing foreign firms are more likely to leave the market, and potential foreign firms are less likely to enter. The aggregate effect of these changes is that there is less aggregate production by foreign firms, but those foreign firms that do survive in the market are more productive. Simply put, higher government takings drives less productive foreign firms out of the market by raising cutpoints x_f and y_f. This selection effect raises the average productivity of those foreign firms that choose to produce.

While the takings rate does not directly affect domestic firms, the changing behavior of foreign firms affects domestic firms. Since a higher takings rate reduces the number of foreign firms in the market (by increasing x_f and y_f), it also reduces the variety of goods that are produced by foreign-owned firms. The elasticity of substitution ensures that consumers will accordingly increase their purchases of the goods produced by domestic firms. A higher takings rate therefore allows less productive domestic firms to enter and survive in the market. This corresponds to a decrease in cutpoints x_d and y_d. This selection effect lowers the average productivity of domestic firms that produce in the market. Both of these implications—about average foreign productivity and average domestic productivity—explicitly take into account what is observable by researchers, given the strategic behavior of firms in the market.[19]

Proposition 2. *A higher government takings rate from foreign firms is associated with higher average foreign productivity and lower average domestic productivity.*

Given these market effects, we can now consider the host government's decision about how much to take from foreign firms. Since the takings rate applies to each unit of foreign production, the utility to the host government of the takings rate for an industry is simply:

$$W(\tau) = \tau Q_f$$

When choosing the optimal rate, the government must balance the benefit of increasing the takings rate against the cost of decreasing the number of units produced by foreign firms. This balancing process takes into account the impact of the takings rate on firm-level decisions about whether to enter the market, how much to produce, and whether to exit the market, which in turn affect the

[19] It is possible that these selection effects change the dynamics of collective action among and between foreign and domestic firms that produce in the market, which is an important topic for future research.

productivity of firms in the market.[20] The host government can find a unique takings rate that balances these two competing factors to maximize its own utility.

Proposition 3. *There exists an equilibrium in which the host government chooses an optimal takings rate from foreign firms, and foreign and domestic firms operate in the resulting market equilibrium.*

3.3 Comparative Statics

Our model yields a wealth of possible comparative statics.[21] However, our main interest lies in the effect of startup costs on government takings:

Proposition 4. *For foreign firms, higher startup costs are associated with a lower average government takings rate.*

The magnitude of foreign startup costs affects both entry and exit decisions by foreign firms. Holding mobility constant, when startup costs are low, it is relatively easy for new foreign firms to enter, and existing foreign firms have relatively little incentive to leave. Accordingly, cutpoints x_f and y_f are relatively low, and the government has a broad set of foreign firms from which it can take. As foreign startup costs increase, entry becomes less desirable for foreign firms that are out of the market: new foreign firms must be more productive to pay the higher startup costs, meaning that cutpoint y_f increases. At the same time, exit becomes more desirable for foreign firms that are already in the market. These firms must be more productive to be willing to stay, meaning the cutpoint x_f increases. This leads to an overall reduction in the set of foreign firms from which the government can take. To offset this decrease, the rent-seeking government is best off if it lowers its takings rate in order to keep more foreign firms in the market.[22] These dynamics ensure that high startup costs indirectly protect foreign firms: since it is more difficult to replace foreign firms when startup costs are higher, the government will treat them more favorably by taking less.

Unfortunately, it is difficult to accurately observe and measure the full spectrum of government treatment of foreign firms, which means we cannot avoid tradeoffs in empirical testing. It is therefore of paramount importance that our theoretical model allows us to state the implications

[20] As discussed below, in model extensions we adjust the government's objective function to take into account other additional factors, like domestic production, domestic productivity, and consumer welfare.

[21] For example, interested readers can easily examine the impact of mobility and the elasticity of substitution on various model outcomes.

[22] As discussed below, if the government also cares about consumer welfare, its choice of a taking rate will also take into consideration the impact on domestic production.

of our theory for other standard, measurable economic outcomes. We next consider the average productivity of foreign firms that have selected into producing in the host country and, hence, are observable to researchers:

Proposition 5. *For foreign firms, higher startup costs are associated with higher average productivity when foreign mobility is high.*

Startup costs have both a direct economic effect and an indirect political effect on which foreign firms decide to produce. The direct economic effect of high startup costs is to deter low productivity foreign firms from entering the market. Simply put, a firm must be more productive in order to recoup the initial cost of entering the market. However, since governments can only take from those foreign firms that actually produce, high startup costs also cause the government to take less, per Proposition 4. So high startup costs have an indirect political effect by lowering government takings, which in turns allows less productive firms to produce, per Proposition 2. Which effect is stronger—the direct economic effect or the competing indirect political effect—depends on assumptions about the basic characteristics of the market. However, when mobility is relatively high, the level of government takings has a relatively small effect on firm decision-making. This means that the direct economic effect outweighs the indirect political effect of high startup costs. In industries with high foreign mobility, higher startup costs will be associated with higher levels of productivity for those foreign firms that choose to produce in the host economy.

We can additionally indirectly assess our theory using firm revenues, which are observable in our data. The overall impact of startup costs on firm-level revenues is positive for those foreign firms that are willing to produce:

Proposition 6. *For foreign firms, higher startup costs are associated with higher revenues.*

Since high startup costs deter new foreign firms from entering a market, they increase the prices of those goods that are produced. However, startup costs are only paid when a firm enters a market, meaning that they are sunk costs by the time that a foreign firm begins actual production: they do not affect production costs after a foreign firm has entered the market. By increasing prices without increasing the production costs for those firms that have already entered the market, higher startup costs directly lead to higher revenues for foreign firms. Additionally, foreign startup costs indirectly increase firm revenues even further by pressuring the government to provide more favorable treatment. Both the direct and indirect effects of startup costs therefore lead to higher revenue for foreign firms.

3.4 Robustness

How robust are our results? We should begin by noting that the model above explicitly includes mobility and allows foreign firms to exit the market in response to alleged mistreatment by the host government. It also includes the elasticity of substitution. As such, our theoretical account is a complement to the existing theoretical literature, not a substitute for it. Our model highlights that while the previous literature has yielded important insights, it has also caused us to overlook the equally important impact of startup costs and market entry. Readers who are substantively interested in mobility and the elasticity of substitution can use our modeling framework to derive implications that are consistent with prior research. Here we have chosen to emphasize our new findings, rather than simply restating logic that has been well-explored previously.

Readers might note that when we endogenized government behavior, we adopted a very simple objective function for the host government: we assumed that the host government seeks to maximize takings from foreign firms. In a model extension, we allow the host government to trade-off the direct benefits it receives from its takings against the indirect impact of these takings on consumer welfare.[23] Not surprisingly, when the host government places more weight on consumer welfare, it extracts less from foreign firms. However, all of the basic results in our model continue to hold, provided that the government places sufficient weight on takings. We are careful in our empirical analysis below to account for possible variation across countries in their responsiveness to consumer welfare. As detailed below, host country and year fixed effects account for host country- and time-specific characteristics. Other state-level controls, particularly regime type and commitments to international investment law speak to within-country over-time variation in the host government's weighting of consumer welfare.

We also simplified our main analysis by assuming that the government only takes from foreign firms. However, the model can be expanded to allow for takings from both domestic and foreign firms. We need not assume that the government is perfectly constrained in its treatment of domestic firms—just as the model above allows the government to choose takings from foreign firms, we can also allow the government to take from domestic firms. In the model extension with domestic takings, Propositions 1-3 always hold. Additionally, Proposition 4-6 hold whenever foreign mobility is relatively high or domestic takings are relatively low. These conditions have the effect of biasing of empirical tests away from the effects that we are trying to identify, making the task of identifying empirical effects even harder.

[23] Every model extension discussed in this section is available upon request.

Our main substantive interest is in Proposition 4, which shows that for foreign firms, higher startup costs should be associated with lower government takings. The argument that supports this logic is contingent on both (1) changes in startup costs in the country being observed, and (2) existing investors being able to recover a portion of their capital and redeploy it elsewhere. That is, when we consider the impact of increasing startup costs in a given country, we assume that investors have a credible exit option: they can recover a portion of their initial capital and engage in other profitable activities. These dynamics should be different if we consider the impact of startup costs in alternative markets or economic activities. Imagine an investor who has deployed her capital in a given country A. If startup costs increase in a different country B, then the real value of her mobile capital should decrease: the foreign investor will have a less credible exit option, which means that her firm will be a more attractive target for mistreatment. While high startup costs at home can discipline a host government, high startup costs in other markets may allow a host government to increase takings at home (since exit is a less desirable option). This suggests that there may be important competitive dynamics across countries that are currently missing in our model.[24] These kinds of competitive dynamics lie outside the framework of our current work, but pose an interesting possibility for future research.

Another limitation of our modeling framework is that we focus on government takings at the industry-level. We do not, for example, allow the host government to microtarget its treatment at the firm-level. It is unclear how relaxing this assumption would affect our results. A sophisticated government could ameliorate some of the entry and exit dynamics that drive our results by targeting firms for mistreatment based on their productivity. From a substantive perspective, it is unclear to us whether such behavior would be feasible because a host government is unlikely to know the precise productivity of individual firms, which can change over time. But a host government could target firms based on production levels, revenues, or other observable attributes. We have chosen not to pursue the line of inquiry because our intuition is that forward-looking firms could anticipate possible microtargeting and adjust their production accordingly. While the distortions that would be created by such a scenario would be important for understanding economic outcomes, we do not have any reason to believe that they would invalidate our substantive interest in political outcomes; namely, the impact of startup costs on government treatment of foreign firms.

One final element that is missing from our model is political action by foreign firms through campaign contributions, corruption, lobbying, etc. A huge literature has demonstrated—both

[24] We thank Iain Osgood for highlighting this point.

theoretically and empirically—that political action for firms matters in shaping government policies, particularly in democracies (Grossman and Helpman, 1994; Kim and Osgood, 2019). Perhaps industries with higher startup costs can more effectively use political action to secure beneficial treatment because these industries have fewer foreign firms, allowing them to more easily overcome collective action problems.[25] While the absence of political action may make our model less realistic, we believe that this absence is a virtue because our theoretical model shows that such political action is not necessary for firms to secure protection from mistreatment. Basic economic fundamentals can constrain host governments, even when firms cannot engage in political action. Additionally, any account of political action by foreign firms would need to also consider countervailing pressure by domestic firms, making the expectations from such an alternative model unclear. Absent cross-national time-series data on political action at the firm- or industry-level, we cannot control for political action in our empirical analyses. However, our empirical analysis controls for regime type and includes country fixed effects, which control for variation across countries in government responsiveness to firm concerns.

4 Empirics

Our formal results allow us to construct a set of hypotheses. The first two hypotheses are direct claims about government behavior.

Hypothesis 1. *A higher government takings rate from foreign firms will increase average foreign productivity and decrease average domestic productivity within each industry. (Proposition 2)*

Hypothesis 2. *For foreign firms, higher startup costs will be associated with a lower government takings rate within each industry. (Proposition 4)*

We do our best to measure government takings so as to provide evidence consistent with Hypotheses 1 and 2; yet proxy measures of government treatment can only go so far. Therefore, our next two hypotheses involve standard, observable attributes of firms that select into FDI, which we can use to indirectly test our political-economic theory.

Hypothesis 3. *For foreign firms that are mobile, higher startup costs will be associated with higher average firm productivity within each industry. (Proposition 5)*

[25] We thank an anonymous referee for suggesting this possibility.

Hypothesis 4. *For foreign firms, higher startup costs will be associated with higher revenues at the firm-level. (Proposition 6)*

These hypotheses, their related propositions, and the tables containing their empirical tests are summarized in Table 1.

[Table 1 goes here.]

To empirically assess our theoretical argument, we must measure multiple outcomes of interest, including startup costs, government takings, and firm- and industry-level financials. To do so, we use financial data from the Bureau van Dijk (BVD) Osiris Industrials database, which records income statements (P/L statements) for 74,270 unique firms (2008-2016).[26] Osiris intends to cover all publicly listed companies that report at least one year of financial accounts; firms are listed on 200 stock exchanges worldwide. The data also include a less well-defined set of private, non-listed companies.[27] We include these firms in our sample so as to leverage BVD's expertise in limiting the issue of selection on listing, while nonetheless marking them with a dummy for Unlisted firm.

One potential weakness of the Osiris data is that it may introduce sampling bias into our evidence. If larger, more productive firms are be more likely to be publicly-listed on stock exchanges and to provide their financial information, then our data does not the true underlying population of firms that operate in each state. Such sampling bias should make it harder for us to achieve statistical significance in some of our tests. For Hypotheses 1 and 3, sampling bias should push our statistical tests towards null findings because we are less likely to have data on those less-productive firms that are indifferent about whether to produce in a market, thereby underestimating the true underlying variation in average productivity levels across industries. Possible sampling bias should not affect our test of Hypothesis 2, which focuses on industry attributes that are not related to sampling. Finally, because Hypothesis 4 holds at the firm-level, our theoretical model suggests that the impact of startup costs on revenues hold for all firms in a market. While sampling bias might affect the magnitude of this statistical effect, it should not affect the significance of the effect. Regardless of this possible bias, Osiris is at this time the best-available resource for cross-national financial data at the firm-level.

[26] Bureau van Dijk Osiris databases. `bvdinfo.com`. Accessed July 2017. Codebook as of 2007.

[27] BVD includes non-listed companies when they are "primary subsidiaries of publicly listed companies," companies with listed bonds, or "in certain cases...at special request of clients." Osiris Data Guide 2007: p 2.

Consistent with our theoretical model's focus on the firm-level, each observation in our data is at the firm-state-year level.[28] We must measure several concepts separately based on whether a firm that reports financials in a given state is foreign or domestic. To determine this, we match firms in the Osiris Industrials financial database to the Ultimate Owners database. We code as FOREIGN those firms with an ultimate owner originating in a state other than the one for which the firm reports financials. This results in 852 unique foreign firms with ultimate owners originating in 66 different states, investing in 90 different host states; each firm has on average a presence in two different foreign host states. We use two strategies to identify domestic firms. First, a domestic firm is one in which the ultimate owner originates in the same state for which the firm reports financials, netting 6,771 domestic firms. However, it is unlikely that the sample of domestic firms with matched ultimate owners is randomly drawn from the population of domestic firms. Therefore, we also code as domestic those firms in the Industrials database for which there is no match in the Ultimate Owner database. In sum, we identify a total of 74,259 firms that are DOMESTIC.

Our theoretical model generates predictions based on a firm's industry classification. Thus, industry is an important identifier recorded for each firm-state-year observation. When coding a firm's INDUSTRY, we use the 3-digit NAICS code, and we include fixed effects for the overarching 2-digit NAICS code. For example, we code firms in the industries of crop production (NAICS 111), animal production and aquaculture (NAICS 112), and forestry and logging (NAICS 113), with a fixed effect for agriculture (NAICS 11).[29] In the data, foreign firms operate in 70 different industries, and domestic firms operate in 96 different industries.[30]

A conservative empirical strategy requires us to control for potential confounding variation at the firm-, industry-, state-, and year-level. Because these sources of variation are not of theoretical interest, our overall strategy is to use extensive controls and fixed effects to fully specify models. Given the complexity of our data and approach, Table 2 summarizes the variables used in our regression analyses. Note that for all logged (ln) variables, we first shift the data to avoid logging negative values. We explain each of the variables in turn below.

[Table 2 goes here.]

[28] Thus, one unique overarching firm can be located in, and report separate financials for, its investments in different states. It can be domestic in one state and foreign in others.

[29] Unfortunately, data limitations preclude us from confidently examining variation at the oft-used 4-digit NAICS level; we include 4-digit NAICS codes in replication files for the readers' interest.

[30] In the data, each firm invests in only one 3-digit industry in each state.

4.1 Dependent Variables

Among our dependent variables, Firm Revenue used in tests of Hypothesis 4 is the most straightforward. This variable is equivalent to (ln) revenue (sales), or the amount earned from a firm's main activities. It directly measures a firm-level outcome in a given state, consistent with our theoretical model, and therefore is a precise operationalization of Hypothesis 4's firm-level implication of our political-economic theory.

Our other dependent variables in Hypotheses 1, 2, and 3 must measure the industry-level predictions of our political-economic theory. In order to measure outcomes at the industry-level, we take the average of the variable in question across firms in the same (3-digit NAICS) industry in a given state. When the outcome is time-varying, we take the average of the variable in question across firms in a given industry-state-year. Overall, this averaging strategy accomplishes a few things. First, averages allow us to be more confident in using industry-level outcome measures that match industry-level predictions. They allow us to minimize the influence of outliers and thus avoid embedding confounding heterogeneity in our most theoretically important variables. Second, an average measure is appropriate, because industry and host state variation are important components of our theory that are properly included in our empirical measures rather than relegated to an atheoretical approach that accounts for them only via controls or fixed effects.[31]

We rely on standard accounting practices to measure productivity in the dependent variables ROA: Foreign and ROA: Domestic. Return on Assets (ROA) is a firm's net income divided by its total assets. We see it as a benefit that our income-statement data allow us to capture a common productivity measure that accountants calculate—and executives (and tax collectors) see. Because ROA is time-varying, we average by industry-state-year. ROA: Foreign averages across foreign firms by industry-state-year; ROA: Domestic averages across domestic firms by industry-state-year.

Our most explicitly political dependent variable must measure government takings from foreign firms (Hypothesis 2). Government takings are also a key explanatory variable of interest (Hypothesis 1). Consistent with our theory's industry-level implications, we average takings variables at the industry-state-year level. We rely on a creative strategy to capture our quantities of interest as closely as possible, as spelled out in the next section.

[31] Our use of averages necessitates our weighted least squares estimation strategy, explained below.

4.1.1 Measuring Government Treatment: Taxes

Our main political variable of interest is the government takings rate, which in our formal model is a transfer from the firm to the government. Our best approximation is to focus on taxes reported in the Osiris Industrials database of firm income statements. We note that by choosing to measure taxes, we are measuring indirect takings but not takings in which the government gains benefits from the firm directly. For example, instead of collecting taxes, the government could expropriate foreign property and, as owner, earn direct returns on production.[32] As we fully acknowledge, our theory is best tested with a measure of the full set of transfers—indirect and direct, monetary and otherwise—from foreign firms to a host government. Nonetheless, in the absence of such a measure, we see taxes as a useful second-best for both empirical and theoretical reasons. Empirically, if tax rates are indeed relatively unimportant in explaining aggregate FDI flows (Jensen, 2012), this operationalization will make it more difficult to link government treatment to firm production decisions as predicted. Theoretically, tax burdens are a key component of differential government treatment, in that political decisions over taxes clearly shape expected returns, yet their examination has been largely excluded from the literature on FDI in international political economy. To be clear, our use of tax data does not mean that this article is about optimal taxation.[33] Nonetheless, we hope our use of firm-level tax data might reinvigorate scholarship in the vein of important, earlier work on the role of taxation in foreign investor-host state relations (e.g. Wallerstein and Przeworski 1995; Hallerberg and Basinger 1998).

While firms report taxes in a variety of ways, our theory is based on actual takings by the government, or the cash tax expense. Cash tax expense is effectively the amount paid out of the firm's "checking account" and into the government's coffers in a given year. Unfortunately, cash tax expense is not recorded in standard income statements, and backing it out requires time-series data on tax expenses that carry over from the previous year, which is problematic given our short panel (2008-2016).[34] This data constraint further underscores the importance of the tests of our theoretical model that rely on measures directly reported in income statements. Our concession given the missingness created by time-series limitations is to systematically underestimate cash tax expense, by assuming that no taxes carry over from the previous year. In exchange for this

[32] One driver of the government's decision to take via taxes or take via ownership is its expectation that it has the technology and intangible assets necessary to produce efficiently and profitably absent foreign ownership.

[33] Recall that in endogenizing government behavior, we assume the host government seeks only to maximize takings from foreign firms. See again Section 3.4 Robustness for discussion of this assumption as well as model extensions that incorporate distributional effects on consumer welfare (available upon request).

[34] Cash tax expense = (income tax expense$_t$ + tax payable$_{t-1}$ + deferred tax$_{t-1}$) - (taxpayable$_t$ + deferred tax$_t$).

concession, we recover cash tax expense for 96 percent of foreign firms.[35]

Our ultimate political measure of interest is not simply the cash tax expense but the tax rate. The effective cash tax rate measures the tax rate based, again, on the amount paid into a government's "checking account." This is cash tax expense divided by the firm's taxable income. The denominator, pretax income, is a firm's revenue minus the costs of goods sold. Our dependent variable TAX RATE: FOREIGN is the effective cash tax rate for foreign firms in a given industry-state-year (Hypothesis 3). Again, note that this is an averaged industry-state-year measure, in this case across foreign firms.

One of our theoretical model's key insights into domestic firms relies on takings from foreign firms. For simplicity, our presentation of our theoretical model here assumes that there are no takings from domestic firms. As discussed in Robustness above, extending the model to allow domestic takings as well has the effect of biasing empirical tests away from what we are trying to identify, making the task of identifying empirical effects even harder.[36] Usefully, the data allow us to conduct these hard tests, deriving from the real-world situation in which governments can and do take from foreign and domestic firms. Specifically, with a relative takings measure, we can empirically capture foreign takings relative to domestic takings rather than to the counterfactual of zero domestic takings. We follow the same procedure for domestic firms to measure the effective cash tax rate TAX RATE: DOMESTIC, and we average it across domestic firms in a given industry-state-year. The variable RELATIVE FOREIGN TAKINGS is the difference between the two. This is not a dependent variable but rather a key variable of interest in testing Hypothesis 1.

Tax variables based on averages are appropriate for the reasons laid out above regarding our general use of averages. Industry-specific taxes, and industry-specific tax planning strategies, clearly affect the bottom line of cash tax expense. Additionally, state characteristics clearly shape firms' abilities to manipulate cash tax expense, for example through profit-shifting, amortization, transfer pricing, and the like (Rixen, 2011). Including industry and state in our variables of interest aid us in theoretically accounting for these sources of tax variation, rather than only atheoretically controlling for them. Moreover, there is certainly heterogeneity in MNCs' abilities to minimize their tax burdens within a given industry-state. Averaging across firms allows us to mitigate the effect of outliers, in either direction.

[35] Negative values are replaced with 0, as we make the conservative assumption that a firm with a negative cash tax expense does not expect the government to pay funds back into the firm's "checking account," or at least not in a timely way.

[36] See the Online Appendix for detail.

4.2 Variables of Interest

In addition to RELATIVE FOREIGN TAKINGS, our explanatory variables of interest include whether a firm is in an industry that is MOBILE. Our theoretical model explicitly incorporates expectations about mobility and government treatment, born of the obsolescing bargain logic. In our empirical tests, we aim to validate the importance of mobility, consistent with its role in our theoretical model. At the same time, we aim to establish that a major contribution of our theory is to explain outcomes that mobility cannot. In the absence of a continuous measure of mobility, we use the standard dichotomous measure. This measure constrains scholars' (including our) ability to fully test theoretical implications.[37] Nonetheless, we expect coefficients on even an imprecise measure to track the logic of the obsolescing bargain. MOBILE codes industries at the NAICS 2-digit level as either mobile or not. Mobile industries include manufacturing, wholesale and retail trade, information, finances, technical and other services, education, waste management, health care, entertainment, and construction. Firms in these industries are assumed to own a non-negligible amount of mobile capital that they can move out of a state should they choose to exit (per Figure 1). Immobile industries include agriculture, mining, utilities, transportation, real estate, hotel and food, and public administration. Firms in these industries are assumed to be effectively immobile, in that they own a negligible amount of mobile capital capable of being moved and redeployed outside the state.

4.2.1 Measuring Startup Costs

Our main variable of interest is startup costs. The underlying concept we aim to measure is the one-time costs a firm must pay to enter a market. We proxy for this using the (ln) dollar value of fixed assets in the first year a firm operates in a given industry-state. One reason we choose to use fixed assets as our measure of startup costs is that we see it as the measure of a firm's initial investment that is least vulnerable to endogeneity. Contrast choices over fixed assets with choices firms make over incurring variable costs at entry. Firms have an interest in responding flexibly to expected government treatment, because government treatment can vary over time. Firms can more flexibly respond to variation in government treatment through changes in variable costs, for example by hiring or firing workers. In contrast, shedding or constructing new facilities in response to changing expectations about government treatment is costly.

[37] Because our theory largely confirms established findings regarding mobility, we focus on other empirical innovations instead of refining the measure of mobility.

To validate the reasonableness of a firm's fixed assets in its first year as a proxy for startup costs, we probe whether characteristics of the measure are consistent with our expectations of startup costs. First, recall our assumption that startup costs are theoretically distinct from mobility. Thus, our measure of startup costs should be meaningfully different from MOBILE. Consistent with our assumption, MOBILE is insufficient to explain startup costs: in a simple regression of startup costs on MOBILE, the coefficient on MOBILE is significant and positive, but the regression's r-squared is 0.0002.[38] While r-squared values are of course not dispositive, the very low r-squared is consistent with our argument that our startup cost measure at least has the potential to add additional explanatory power. Second, a substantial literature in international business establishes that firms face a "liability of foreignness" when investing abroad, such that foreign firms incur higher operating costs than domestic firms in the host state (Zaheer, 1995). For example, foreign firms must coordinate across geographic distance; incur search costs in acquiring relevant local cultural and political knowledge; and adapt their standard operating procedures to local institutions (e.g. Beazer and Blake 2018; Jia and Mayer 2017; Zhu and Shi 2019; Eden and Miller 2004). Put in our terms, the "liability of foreignness" implies that startup costs are on average higher for foreign firms than domestic firms. In a simple t-test, our data bear this out ($p<0.000$). In short, we see empirical corroboration that fixed assets upon entry can speak to our concept of startup costs. Nonetheless, in Robustness below, we consider alternative startup costs measures based on only a firm's property, plant, and equipment (PPE) in the first year, and a firm's total assets in the first year.

We create the variables STARTUP COSTS: FOREIGN and STARTUP COSTS: DOMESTIC by again employing our averaging strategy that mitigates the effect of outliers. As such, we take the (ln, USD) average of firm fixed assets in their first year by industry-state.[39] We conceptualize startup costs as time-invariant, which we see as appropriate given our short time window (2008-2016).[40] Practically, this means that STARTUP COSTS: FOREIGN creates one startup cost value for foreign firms in South Africa in the NAICS 212 industry (mining, except oil and gas). STARTUP COSTS: DOMESTIC creates a complementary value for domestic firms.

[38] Additionally, r-squared statistics remain low when splitting the sample into observations of mobile or immobile firms. In the mobile subsample, a regression of startup costs on mobile industry dummies has an r-squared of 0.07; in the parallel immobile exercise, the r-squared is 0.14.

[39] Again, data availability leads us to average at the 3-digit NAICS level. Firm-level startup costs equal to 0 are coded as missing and thus do not contribute to averages, given uncertainty over the accuracy or interpretation of such a value.

[40] In the long-run, startup costs may change in states that invest in infrastructure, develop natural resources, or otherwise improve their endowments. Long-run technological improvements can also change relative startup costs across industries and host states that vary in access to technology.

Our use of industry-state averages to measure startup costs is consistent with our theory. We expect that structural characteristics not only of the firm's industry but also of the state in which the investment is located generate exogenous sources of variation in startup costs. For example, while some states have an abundance of natural resources, well-developed transportation networks, and deep financial markets, others do not. We conceptualize these kinds of state characteristics as endowments that are plausibly exogenous at least in the short-run.[41] Which specific state endowments impact startup costs will depend on the needs of a specific industry. Additionally, variation in states' regulatory approaches—fixed and exogenous in the short-run—correlate with variation in the kinds of investments firms tend to locate in different states. For example, formal rules and informal norms in accounting vary by state, given domestic accounting regulations and oversight institutions (Hopwood and Miller, 1994).[42] The data support intuitions born of these sources of variation. For manufacturing in the Cayman Islands, the average startup cost is about one standard deviation below the worldwide mean. The peculiarities of regulations in the Cayman Islands means that it often plays host to financial arms of multinational corporations (in whatever industry). Startup costs that are more about renting mailboxes and office space would understandably be low.

Averaging startup costs is also useful in addressing shortcomings of our overall empirical approach. We aim to measure startup costs that are exogenous to host government behavior, consistent with the conceptualization in our theoretical model. For example, our measure of startup costs should include the price a milling machine has on world markets, exogenous to the government in a particular host state. That said, we recognize that a government can influence startup costs at the margin through, say, a tariff on milling machines. This reality obviously complicates our presumption of exogeneity. Importantly, startup costs (calculated at the NAICS 3-digit level) survive this complication so long as government treatment varies (if at all) across industries at the NAICS 2-digit level (such as mining and manufacturing). It is in fact common in FDI regulations for a host government to differentiate its treatment of FDI by industry (Pandya, 2014). For example, Mongolia implements special regulatory processes for strategic sectors including minerals, media and information, finance, and telecommunications.[43] While we focus on host states' potential to treat foreign firms adversely, states can and do use instruments like investment incentives to

[41] See again footnote 40.

[42] For example, capital expenses that we would conceptualize as startup costs can often—but not always—be amortized over the useful life of the asset. Other startup costs for assets that do not need to be replenished would not typically be amortized. Additionally, accounting norms may vary by industry, given market-driven expectations. We assume that accounting practices are fixed by industry-state in at least the short-run.

[43] "Investment Policy Review: Mongolia." 2013. UNCTAD/DIAE/PCB/2013/3, United Nations Publications.

entice FDI in specific industries (Wellhausen, 2013).[44] Our data structure allows us to leverage exogenous variation in startup costs at the NAICS 3-digit level while controlling for the peculiarities of government treatment at the 2-digit level.

However, it is also possible that a host state "microtargets" a particular firm for differential treatment at entry, a point we addressed in our theoretical discussion. If particular firms expect better government treatment, they could could choose to invest more upon entry—for example, by building a bigger factory.[45] If that were the case, then fixed assets upon entry would not be an exogenous measure but would be exactly determined by expected government treatment. Averaging by industry-state helps to address this concern, as it mitigates the bias introduced by heterogeneity in firms' ability to minimize startup costs caused by within-industry (endogenous) variation in government treatment at entry. Moreover, our other modeling choices explained below help to mitigate such endogeneity concerns, given that our firm- and state-level controls as well as our extensive fixed effects differentiate likely "microtargeting" sending states and receiving firms. Nonetheless, we emphasize the importance of our indirect tests of our political-economic argument at the firm level (Hypothesis 4), which minimize this endogeneity concern.

4.3 Controls and Modeling Choices

Firm-level controls: Since our theoretical model is based on firm-level logic, and our observations are at the firm-state-year level, firm-level controls are crucial to our specifications. First, the size of a firm in a given state is clearly meaningful for the kinds of financial measures that interest us. In particular, heterogeneous trade theory establishes that larger firms are more likely to produce abroad. Further, a firm's potential productivity as measured by ROA is determined in part by its capitalization; the more capital-intensive a firm, the more difficult it is to achieve a high ROA. Therefore, we control for Firm total assets, a standard measure of firm size, which is the (ln, USD) amount of a firm's assets by firm-state-year.

Next, a firm's startup costs, especially as measured by fixed assets upon entry, may be influenced by the firm's mode of entry.[46] For example, if a firm enters via M&A, it may acquire depreciated fixed assets, whereas greenfield investment may be more likely to incur the full cost of new fixed assets. Unforunately, our data do not allow us to measure mode of entry directly. Instead, we first get at mode of entry via the intuition that the firm's percentage of ownership in

[44] We encourage future research to bring together findings about adverse and preferential treatment of foreign firms.
[45] We thank Mike Tomz for highlighting this point.
[46] We thank a Reviewer for raising this issue.

the first year of operation reflects its investment strategy, which is correlated with mode of entry. We expect that greenfield investors are more likely to have 100 percent direct ownership in the first year of operation. Our resulting variable is a dummy, FIRM OWNS 100% AT ENTRY, which is true for 62 percent of foreign firms in our data.[47] Another characteristic that addresses the firm's structure and investment strategy is whether the firm itself is an MNC. Recall that we define foreign firms as those with an ultimate owner in a different state (by merging the Osiris Industrials and Ultimate Owner databases). We leverage the Osiris Subsidiaries database to match whether the firms under analysis themselves have subsidiaries in foreign state(s).[48] We expect that such firms likely share investment strategies that would reflect their mode of entry, therefore making this another an important control. The dummy FIRM IS MNC equals one if a firm has investments in one or more foreign states. In the data, 268 unique foreign firms that form the basis of our analysis are a link in a chain involving investments in at least three different states: the ultimate owner's state, the firm's state, and the subsidiary's (or subsidiaries') state.[49] Moreover, 21,007 domestic firms in our data themselves have foreign subsidiar(ies). These firms, too, likely share characteristics that could be a confounding source of heterogeneity among our sample of domestic firms.

Given findings that a foreign firm's home state can affect its relationships with the government in a host state, we take into account a foreign firm's home state (Beazer and Blake, 2018; Wellhausen, 2015*b*). We expect that foreign firms from OECD home states have meaningfully different FDI strategies than other foreign firms; 62 percent of foreign firms in the sample have an OECD HOME.[50] Finally, recall that while the Osiris databases select on listed firms (on 200 stock exchanges around the world), BVD also includes some unlisted firms in the databases, with justifications that they are also appropriate for inclusion.[51] We mark these firms with the dummy FIRM IS UNLISTED.

State-level controls: We include a set of variables to control for potentially confounding state-level heterogeneity. In general, we know that states differ in domestic institutions that could

[47] Results on variables of interest are consistent across different ownership thresholds; see replication files.

[48] Bureau van Dijk Osiris databases. `bvdinfo.com`. Accessed July 2017. Codebook as of 2007. Unfortunately, the Subsidiaries database is missing financial data that would make it appropriate for use elsewhere in our empirical strategy.

[49] 55 percent of these firms have subsidiaries in multiple states, meaning that the unique firm has investment ties to more than three states.

[50] Minimized OECD measure, for pre-1994 members excluding Turkey. Results of interest are largely robust to using home state fixed effects instead, although with their inclusion we begin to have concerns about degrees of freedom in some analyses.

[51] See again footnote 27.

influence our political-economic outcomes of interest (Dorsch, Mccann and Mcguirk, 2014). We control for DEMOCRACY, which ranges from -10 to 10 (*polity2* from the Polity IV Project). We also control for FDI INFLOWS % GDP, given that overall (net) levels of FDI in the state would influence the state's flexibility with regard to its treatment of foreign investors. A similar logic leads us to control for TRADE % GDP. Finally, we control for (ln) GDP PER CAPITA, which speaks to both domestic market size and development level.

Fixed effects: We account for remaining heterogeneity through fixed effects. INDUSTRY (2-DIGIT NAICS) FE addresses remaining time-invariant heterogeneity within 2-digit industries. STATE FE complement our state-level controls by accounting for time-invariant heterogeneity within states. YEAR FE account for annual shocks that could interfere with our estimations, such as the Great Recession. These multiple fixed effects also relate to endogeneity concerns by our averaging strategy. Our full specification presumes presume that any remaining endogenous components of variables of interest are randomly distributed across 3-digit industries within the observation's overarching 2-digit industry, state, and year.

Estimation: Our use of averages across firms means that we must account for heterogeneity in the set of firms that feed into each average. Indeed, our theory is built on expectations about firms entering and exiting the market, which means that we expect averages to include different firms over time. Our approach is to rely on the long-standing method of weighted least squares. Our empirical target is a population, and weighting moves our data sample closer to measures of that population. Employing weighted least squares allows us to more accurately account for nonrandom variation in the precision of our underlying data (Angrist and Pischke 2009: 92). Weighting also addresses missing data that can cause variation in the precision of our averaged measures. In our regressions, each weight is the count of unique foreign firms present in a given industry-state during the sample time period (or, when appropriate, the count of unique domestic firms in an industry-state). We note that the same ownership structure can extend over different actors' decisions to select in or out of production which would affect the count; we cannot assume that 10 firms in one context is comparable to 10 firms in another. Therefore, we use firm counts as the best strategy for weighting, but we do not pursue inferences based on counts.[52] We use robust standard errors clustered by state.

[52]One could interpret our theoretical model as having implications for the number of firms underlying each observation, which would make the precision of our averaged measures endogenous to the theory. However, because of this ownership complication, we cannot be sure whether the count of firms truly reflects the constellation of investments. We thank Timm Betz for discussion.

4.4 Regression Results

Because of data availability limits, it is difficult to empirically test our key interest: the impact of startup costs on the government treatment of foreign firms. We therefore present a diverse set of direct and indirect empirical tests of our theory. While each individual test may have limitations, these tests in the aggregate provide compelling support for our argument.

We begin with the indirect test of our argument embodied in Hypothesis 1: that government takings from foreign firms will have diverging effects on the observed productivity of domestic and foreign firms that select into the market. Namely, we expect the coefficient for RELATIVE FOREIGN TAKINGS to be positive for foreign firms and negative for domestic firms, as the government takings rate from foreign firms has opposing effects on observed productivity by ownership. Table 3 shows results. The sample in Models 1-4 is foreign firms, and the dependent variable is ROA: FOREIGN, which is averaged by industry-state-year as explained above. In the stripped-down Model 1, we see that RELATIVE FOREIGN TAKINGS has a positive and significant correlation with foreign firms' productivity; however, the introduction of covariates in Models 2-4 erases that significance.[53] Models 5-8 examine domestic firms only, and the dependent variable is ROA: DOMESTIC. In these models, results are fully consistent with our theoretical expectations: RELATIVE FOREIGN TAKINGS are significantly negatively associated with domestic productivity. In other words, consistent with our theory, as the relative takings from foreign firms in a given industry-state-year increases, productivity for domestic firms in that same industry-state-year decreases. The coefficient size is relatively stable across specifications.[54] Although results regarding foreign firms are weak, notice the relatively low N as compared to the domestic firm sample, and recall the reality that taxes are an imperfect proxy for government treatment. Indeed, results on domestic firms give us particular confidence because, irrespective of the large N, our sample of domestic firms is biased given selection on listed firms. Thus, we interpret results in Table 3 as supportive of our theoretical expectations.

[Table 3 goes here.]

While our theoretical model does not generate clear predictions for startup costs or mobility in the context of Table 3, we emphasize that inconsistent results between the two reinforce our argument that that these variables are not measuring the same underlying concept. The coefficients on MOBILE are not consistent in sign or significance, in either the foreign or domestic sample. In

[53] The coefficient turns negative in Model 4, although note the very large standard error.

[54] In general, substantive effects are difficult to report given the complex estimation strategy. Therefore, we focus on sign, significance, and consistency.

Models 1 and 5, we include MOBILE, but we do not include 2-DIGIT NAICS INDUSTRY FE. This means that MOBILE alone is accounting for heterogeneity across aggregated industries. In both Models 1 and 5, we can reject the null hypothesis that the startup and mobility coefficients are equal (at the 95 percent level).[55] In Models 2 and 6, we introduce 2-DIGIT NAICS INDUSTRY FE; the significant result on MOBILE in Models 1 and 5 disappears and the coefficient turns negative in Model 6. These instabilities suggest that mobility alone does not account for aggregated industry-level heterogeneity. Startup costs are significantly associated with higher productivity among foreign firms (Models 1-4), and startup costs are positive but not consistently significant for domestic firms (Models 5-8). Overall, while both startup costs and mobility are positive and statistically significant in Models 1 and 5, signs differ in more robust specifications.

Our results for Hypothesis 2 directly test our key theoretical claim: that startup costs affect government behavior towards foreign firms. The sample is reduced to only foreign firms, and now the dependent variable is TAX RATE: FOREIGN, as our expectation is specifically about the effective cash tax rate of foreign firms. STARTUP COSTS: FOREIGN is the variable of interest; we expect a negative relationship. It is key to our theory that we find a political effect of startup costs that does not operate through firm size. It should not be that firms in industries with high startup costs are simply bigger. With a rate as the dependent variable, a concern is that startup costs could generate change in the ratio via changes in the denominator (income) rather than the numerator (cash tax expense). Therefore, our control for FIRM TOTAL ASSETS is particularly important, to be sure that changes in the dependent variable are not directly related to the size of the firms that go into the averaged measures. As Table 4 shows, the relevant coefficients fail to reach conventional levels of statistical significance. Yet they are consistent across all models in their sign and magnitude. We view that as important (albeit not definitive) evidence given the overall difficulty in measuring government takings.

[Table 4 goes here.]

Finally, we construct two more indirect tests of our theory by focusing on the economic relationships between startup costs, productivity, and revenue. While these relationships are not our main substantive concern, Hypotheses 3 and 4 allow us to indirectly test our theory without relying on imperfect measures of government treatment.

Hypothesis 3 specifies the expected relationship between startup costs and productivity for mobile foreign firms. We find support in Table 5: within the overarching category of mo-

[55] We can also reject the null in Model 6.

bile industries, higher STARTUP COSTS: FOREIGN are significantly associated with higher average industry-level ROA: FOREIGN. Results in Table 5 are particularly important, because the underlying causal mechanism operates through the takings rate but does not require us to measure or control for the takings rate. Our theory establishes that startup costs have a direct economic effect by deterring entry by low-productivity foreign firms. At the same time, deterred entry reduces replaceability and thus leads the government to take less. But when firms are mobile, the government is already taking less because of the obsolescing bargain dynamic, such that any additional political effect of startup costs should be dominated by the economic effect. The positive coefficient on STARTUP COSTS: FOREIGN is consistent with this reasoning. Moreover, this evidence in support of Hypothesis 3 further establishes that startup costs can explain variation of interest beyond mobility alone, by explaining variation among mobile firms.

[Table 5 goes here.]

Finally, regressions reported in Table 6 test Hypothesis 4, the relationship between STARTUP COSTS: FOREIGN and FIRM REVENUE. As hypothesized, higher industry-level STARTUP COSTS: FOREIGN are associated with significantly higher foreign firm-level FIRM REVENUE in all models. This is a crucial result in terms of bolstering our empirical results, given that the firm-level dependent variable avoids concerns about averaging in other dependent variables. Moreover, this evidence supports a key implication of our theoretical model without requiring us to measure government takings. Because higher startup costs deter new foreign firm entry, they reduce competition, increase prices for consumers, and lead to higher revenues for those firms "in" the market. The government, too, is pressured to provide more favorable treatment to foreign firms given the fewer available replacements. Both mechanisms lead to higher revenues, as supported by the positive and significant results here. We also note further evidence that startup costs and mobility are different concepts. Similar to coefficients on startup costs, coefficients on MOBILE are positive and significant (in three of four models); they are however significantly different in three of four models (with 95 percent confidence).

[Table 6 goes here.]

4.5 Robustness

We focus on the robustness of our results to two concerns: first, our measure of startup costs; and second, the potential role of the elasticity of substitution, or how easily a consumer can substitute

different varieties of goods.

First, we have used a firm's fixed assets in the first year as the basis for our measure of startup costs. Fixed assets, which can also be called long-term assets, are categorized as such because they are durable and will last more than one year. Put differently, these are investments that cannot be readily converted to cash in less than one year. We see this measure as a strong match to our theoretical concept of startup costs, as we expect firms to limit their incursion of inflexible commitments in the year of entry when the success of the investment is especially uncertain.

An alternative measure of startup costs would be to focus on property, plant, and equipment (PPE), which is a subcategory of fixed assets (Kerner and Lawrence, 2014). Whether fixed assets are PPE is irrelevant to our theory, but we nonetheless consider PPE-ONLY STARTUP COSTS as a robustness check.[56] In our data, the firm-level correlation between our preferred measure of startup costs and the PPE-only measure is very high (0.93). However, we have PPE for only 53 percent of observations, and only 50 industries (versus 70) and 64 states (versus 89). Sample sizes range from 36 to 53 percent of those in our main results. We cannot reasonably assume that observations are missing at random. For example, startup costs are significantly higher in the PPE-only subsample, and state GDP per capita and democracy are significantly lower. We therefore approach these robustness tests with extreme caution. Our findings in Tables 5 and Table 6 are robust.[57] In contrast, our other results are not robust and even counter to our expectations. We believe that these differences are caused by bias in the missing data; the sample means of 20 of 26 covariates in Table 3, and 8 of 14 covariates in Table 4, are significantly different from those in our main results.[58]

Perhaps instead of measuring startup costs with a smaller component of assets at entry, the larger measure of total assets at entry would be appropriate. Our worry is that the total assets measure is considerably more vulnerable to endogeneity concerns. The accounting definition of total assets includes all items of economic value. Consider cash. A foreign firm in a given state has notable flexibility over the amount of cash it reports on its income statement in the host state, or that it repatriates to and reports on the ultimate owner's income statement, or that it reports elsewhere. This is exactly the kind of strategic cash-shifting that we intend to control for via our complicated empirical specifications. To explicitly include the assets most vulnerable to this in our

[56] We create the firm-level PPE measure by summing the (USD) value of firm-level buildings; plant and machinery; and the "other PPE" category on the income statement.

[57] Four of eight specifications are robust but unreliable, with R-squared values of 0.94-0.99.

[58] All estimates are available in replication files.

measure of interest appears to us inappropriate. Additionally, we include time-varying firm-level total assets as an important control for firm size; relying on those as the basis for startup costs raises collinearity concerns. Nevertheless, we have full coverage of total assets in the data; the correlation between startup costs based on (ln) fixed assets and (ln) total assets in the data is quite high (0.90). Our results are fully robust to the measurement change.[59]

Second, we probe potential unmeasured heterogeneity at the industry-level. As is standard, we assume in our formal model that consumers prefer variety, such that the elasticity of substitution across goods within an industry (σ) is greater than 1. Scholars have developed empirical estimates of the elasticity of substitution by industry-state, which we explore as potential control variables in our regressions. We use the Kim and Zhu (2016) estimate of σ, at the 3-digit NAICS industry-state level appropriate for our data. Unfortunately, while a remarkable data contribution in general, the data are problematic for our purposes. The measure was developed for agriculture, mining, manufacturing, and information industries (2-digit NAICS). This means that estimates of elasticity of substitution are available for only 44 percent of our sample, covering only 29 percent of NAICS 3-digit industries and 42 percent of states. In our context, we cannot presume data is missing as-if at random. Therefore, we approach robustness tests with caution. Nevertheless, our overall takeaway is that results are quite robust to including the estimate of σ, despite limited data coverage.[60] Results in Table 3 are robust in the simplest models (1 and 5); results in Table 5 are fully robust; and results in Table 6 are robust in the simplest model (1).[61] As far as σ itself, its coefficient has an inconsistent sign and rarely achieves statistical significance.

In sum, we are reassured by support (although not fully robust) for the political components of our theory, which are our focus. In testing them, we use a novel measure of government treatment: industry-average tax burdens facing foreign firms in a given state-year. We find evidence that the selection effects generated by higher relative foreign takings raise the average productivity of foreign firms in the market and lower the average productivity of domestic firms in the market (Hypothesis 1). Our findings are consistent with Hypothesis 2, that higher foreign startup costs are associated with more favorable government treatment for foreign firms, although coefficients are not conventionally significant. Importantly, given the compromises necessary in constructing our novel political variables, our theoretical model allows us to further test our political arguments via indirect tests of their observable economic implications. Our strong results on indirect tests

[59] Results on Hypothesis 2 continue to be insignificant; the signs are positive with extremely small magnitude coefficients. See replication files.

[60] See replication files.

[61] Results on Hypothesis 2 are negative in three of four models and, as before, insignificant. See replication files.

therefore enlarge the body of empirical evidence in support of our theoretical model. Finally, our empirical findings reinforce the novelty of our theory focused on startup costs, which explain outcomes of interest beyond that possible with a sole focus on mobility. Endogenizing entry is thus not only theoretically but also empirically important.

5 Conclusion

Our main contribution in this paper is to draw out the political effects of startup costs on host governments' treatment of foreign firms. Our approach highlights that startup costs, which affect market entry decisions, play a crucial role alongside mobility, which affects market exit decisions. When startup costs are high, host governments must take less lest they deter existing and potential foreign firms. Both direct and indirect empirical tests of our argument support our main conclusion: when it is more expensive to enter a market and start up new production, those foreign firms that are capable of doing so enjoy better government treatment.

One implication of our theory pertains to technology. If different technologies advance at different rates, today's ranking of low and high startup costs will likely someday change. Our theory implies that the distribution of government treatment across industries would change as well. Consider the startup costs of small-scale, manual-labor-based farming in the past versus the large-scale, capital-intensive farming of the present. Our theory is consistent with both today's lower risk of agricultural land expropriation in the United States, as well as the fact that highly productive multinational corporations now dominate the agricultural industry. Our approach can thus provide insight into both variation in government treatment across countries and changing patterns of treatment over long time horizons.

Our theory also pushes a new research frontier that emphasizes government tradeoffs between promoting foreign versus domestic firms. This tradeoff is especially salient as domestic firms originating in developing countries increasingly become multinationals. Competition between foreign and domestic firms is also important given normative concerns about the impact of FDI and and its regulation on the advancement of domestic entrepreneurship in developing countries.

In this article, we defend our measure of startup costs: it is simply far more expensive to build a new production facility than it is to rent office space. We aim to identify effects of exogenous industry-average variation in startup costs that outweigh endogenous adjustments at the margin. Future research can build on our approach to examine the impact of host governments' efforts to endogenously manipulate startup costs at entry, for example, via investment incentives (Jensen and

Malesky, 2018). Additionally, one could consider the differences in government treatment generated by variation in treatment at entry and variation generated over the long-run as bargains obsolesce. Our approach suggests that, so long as replaceability is high, adverse treatment expected in the long-run can in fact come quickly.

Appendix

Full derivations and proofs are in an Online Appendix.

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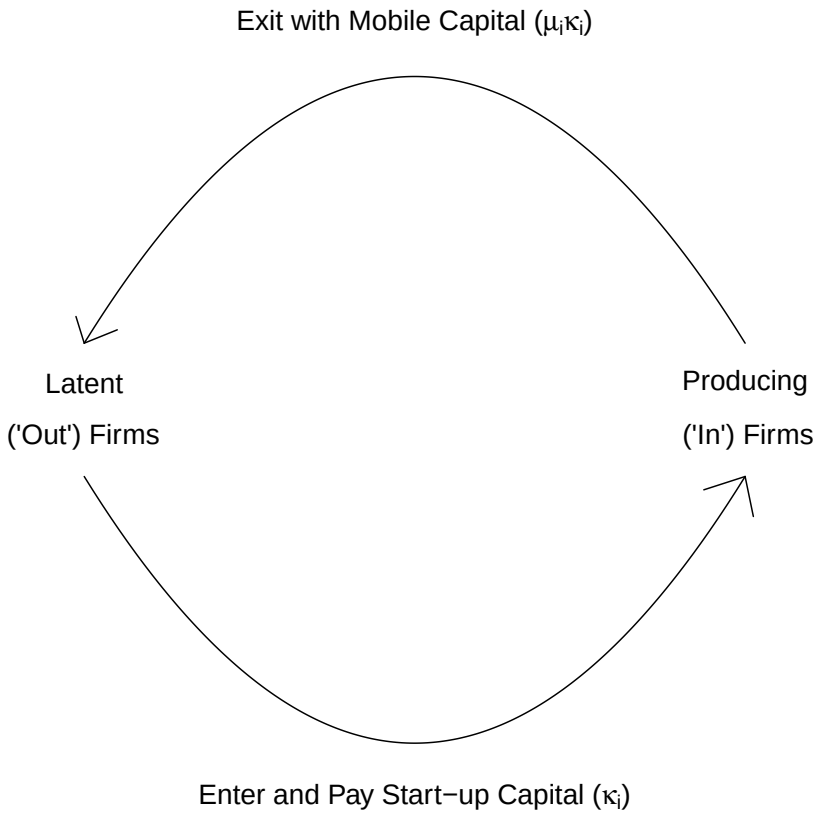
Figure 1: Firm Entry and Exit



Note: This figure shows entry and exit decisions for firms that have already paid an information cost (β) to learn their productivity.

Figure 2: Equilibrium Market Behavior

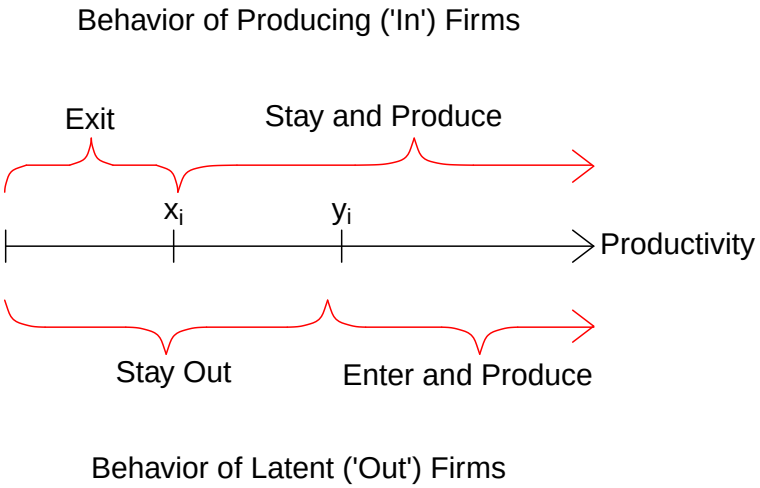


Table 1: Empirical Hypotheses

Hypothesis Number	Theoretical Expectation	Proposition Number	Table Number
1	A higher government takings rate from foreign firms will increase average foreign productivity and decrease average domestic productivity within each industry.	2	3
2	For foreign firms, higher startup costs will be associated with a lower government takings rate within each industry.	4	4
3	For foreign firms that are mobile, higher startup costs will be associated with higher average firm productivity within each industry.	5	5
4	For foreign firms, higher startup costs will be associated with higher revenues at the firm-level.	6	6

Table 2: Summary of Variables

Variable	Measurement	Variation	Hyp.
Dependent Variables			
ROA: Foreign	Avg. foreign firm Return on Assets (%)	Industry-State-Year	H1, H3
ROA: Domestic	Avg. domestic firm Return on Assets (%)	Industry-State-Year	H1
Tax rate: Foreign	Avg. foreign (Cash tax expense)/(Pretax income), ln	Industry-State-Year	H2
Firm Revenue	Firm revenue (USD), ln	Firm-State-Year	H4
Variables of Interest			
Startup costs: Foreign	Avg. foreign firm fixed assets (USD) in first year, ln	Industry-State	H1-H4
Startup costs: Domestic	Avg. domestic firm fixed assets (USD) in first year, ln	Industry-State	H1
Mobile	Dichotomous based on firm 2-digit NAICS industry	Industry	H1-H4
Relative foreign takings	(Tax rate: Foreign – Tax rate: Domestic), ln	Industry-State-Year	H1
Controls: Firm-level			
Firm total assets	Firm-level total assets (USD), ln	Firm-State-Year	
Firm owns 100% at entry	Firm direct ownership is 100% in first year	Firm-State	
Firm is MNC	Firm has foreign subsidiaries	Firm-State	
OECD home	Home is OECD member (pre-1994, excl. Turkey)	Firm	
Firm is unlisted	Firm is private	Firm	
Controls: State-level			
Democracy	*polity2* (-10 to 10) [Source: PolityIV]	State-Year	
FDI inflows % GDP	FDI net inflows % GDP [Source: UNCTAD]	State-Year	
Trade % GDP	(Exports + Imports)/GDP [Source: WDI]	State-Year	
GDP per capita	GDP per capita (USD), ln [Source: WDI]	State-Year	
Fixed Effects			
Industry (2-digit NAICS) FE	Firm 2-digit NAICS industry		
State FE	State location of observation		
Year FE	Year of observation		

Notes: All logged (ln) variables are shifted before logging so as to not log negative values.

Firm- and industry-level data are from BVD Osiris Industrials, with supplements from Osiris Ultimate Owners and Osiris Subsidiaries.

Table 3: With higher relative foreign takings, foreign productivity is higher and domestic productivity is lower.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Relative foreign takings	1.574**	0.719	1.295	-0.163	-0.535***	-0.796***	-0.743***	-0.751***
	(0.610)	(0.634)	(1.976)	(1.698)	(0.128)	(0.0762)	(0.0511)	(0.0548)
Startup costs: Foreign	0.961***	0.803**	1.103***	1.072***				
	(0.354)	(0.376)	(0.118)	(0.0857)				
Startup costs: Domestic					0.502**	0.0766	0.190	0.142
					(0.202)	(0.233)	(0.130)	(0.158)
Mobile	11.51**	3.513	-4.959	9.810	6.351***	-8.341	6.513**	-32.34
	(5.776)	(6.092)	(5.301)	(6.873)	(0.522)	(1021.971)	(2.538)	(24154.8)
Firm total assets			0.926*	2.067***			0.0357	0.0369
			(0.472)	(0.446)			(0.0296)	(0.0292)
Firm owns 100% at entry			0.454	0.0296			-0.00855	-0.0149
			(0.970)	(0.210)			(0.0547)	(0.0592)
Firm is MNC			0.685	0.733			-0.101**	-0.135***
			(1.177)	(0.719)			(0.0442)	(0.0319)
OECD home			4.701***	2.459			0.242***	0.210***
			(1.575)	(1.466)			(0.0642)	(0.0630)
Firm is unlisted			-1.566	1.225			0.147***	0.169***
			(1.695)	(2.153)			(0.0544)	(0.0460)
Democracy				20.75***				0.0103
				(5.404)				(0.0202)
FDI inflows % GDP				-53.32*				-0.0571
				(29.08)				(0.0387)
Trade % GDP				-26.19***				-0.00450
				(6.775)				(0.00377)
GDP per capita				-431.8				0.297
				(1528.6)				(0.839)
Constant	-15.42**	-0.761	-21.52**	4560.1	-5.115	5.149	2.282	-0.520
	(6.861)	(4.985)	(9.758)	(14129.7)	(4.882)	(4.534)	(2.541)	(9.451)
Industry (2-digit NAICS) FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,639	1,639	981	531	249,210	249,210	42,563	31,197
Clusters (State)	83	83	60	44	84	84	69	58
Adj. R-squared	0.412	0.566	0.691	0.851	0.962	0.976	0.975	0.971

WLS, with robust SEs clustered by state. * p < 0.1, ** p < 0.05, *** p < 0.01.

Dependent Variable: Models 1-3: ROA: Foreign; variation by 3-digit NAICS industry-state-year. Models 4-6: ROA: Domestic; variation by 3-digit NAICS industry-state-year. *Variable of Interest*: Relative foreign takings (Foreign effective cash tax rate minus domestic effective cash tax rate, ln); variation by 3-digit NAICS industry-state-year. *Sample:* Models 1-3: Foreign firms, Models 4-6: Domestic firms.

Table 4: For foreign firms, higher startup costs are associated with lower government takings.

	(1)	(2)	(3)	(4)
Startup costs: Foreign	-0.00456	-0.00634	-0.00499	-0.00346
	(0.00551)	(0.00613)	(0.00488)	(0.00306)
Mobile	0.0226	0.0423	0.219***	0.0249
	(0.0267)	(0.0496)	(0.0546)	(0.0787)
Firm total assets			-0.00222	-0.0106
			(0.00199)	(0.0111)
Firm owns 100% at entry			0.0125*	0.00274
			(0.00630)	(0.00252)
Firm is MNC			-0.0269	-0.0372
			(0.0207)	(0.0281)
OECD home			0.0429	0.0391
			(0.0328)	(0.0388)
Firm is unlisted			0.0237	0.0108
			(0.0490)	(0.0404)
Democracy				0.117
				(0.0757)
FDI inflows % GDP				1.463
				(1.416)
Trade % GDP				-0.726
				(0.569)
GDP per capita				-175.5
				(154.0)
Constant	2.136	2.068	3.997***	1630.2
	(1.668)	(1.352)	(0.584)	(1426.5)
Industry (2-digit NAICS) FE	No	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,651	1,651	982	531
Clusters (State)	84	84	60	44
Adj. R-squared	0.109	0.405	0.521	0.784

WLS, with robust SEs clustered by state. * p < 0.1, ** p < 0.05, *** p < 0.01.

Dependent Variable: Tax rate: Foreign (Foreign effective cash tax rate, ln); variation by 3-digit NAICS industry-state-year. *Variable of Interest*: Startup costs: Foreign (fixed assets in first year, ln); variation by 3-digit NAICS industry-state. *Sample:* Foreign firms.

Table 5: For mobile foreign firms, higher startup costs are associated with higher productivity.

	(1)	(2)	(3)	(4)
Startup costs: Foreign	1.204***	1.230***	1.355***	1.123***
	(0.170)	(0.145)	(0.216)	(0.0732)
Firm total assets			2.136*	2.982***
			(1.194)	(0.851)
OECD home			1.591	-2.469
			(4.645)	(3.154)
Firm owns 100% at entry			-0.136	-0.301
			(0.267)	(0.221)
Firm is MNC			2.208*	1.290
			(1.205)	(0.773)
Firm is unlisted			5.060	10.95*
			(7.445)	(5.695)
Democracy				6.188
				(9.534)
FDI inflows % GDP				0.315
				(33.69)
Trade % GDP				-5.706
				(10.60)
GDP per capita				162.2
				(756.5)
Constant	-9.006	-5.459	-57.05***	-1658.9
	(8.938)	(6.398)	(16.54)	(8218.2)
Industry (2-digit NAICS) FE	No	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,196	1,196	733	392
Clusters (State)	77	77	55	38
Adj. R-squared	0.537	0.585	0.675	0.852

WLS, with robust SEs clustered by state. * p < 0.1, ** p < 0.05, *** p < 0.01.
Dependent Variable: ROA: Foreign; variation by 3-digit NAICS industry-state-year.
Variable of Interest: Startup costs: Foreign (fixed assets in first year, ln); variation by 3-digit NAICS industry-state. *Sample:* Mobile foreign firms.

Table 6: For foreign firms, higher startup costs are associated with higher revenues.

	(1)	(2)	(3)	(4)
Startup costs: Foreign	0.0322*	0.0276***	0.0144***	0.0252***
	(0.0182)	(0.00835)	(0.00258)	(0.00417)
Mobile	0.846***	0.474**	0.247	1.083**
	(0.149)	(0.229)	(0.152)	(0.451)
Firm total assets			0.259***	0.321***
			(0.0275)	(0.0279)
OECD home			-0.237**	-0.222**
			(0.114)	(0.109)
Firm owns 100% at entry			-0.0627***	-0.0550***
			(0.0198)	(0.0144)
Firm is MNC			0.0428***	0.0901***
			(0.00969)	(0.0280)
Firm is unlisted			0.141*	0.445***
			(0.0801)	(0.152)
Democracy				-0.247***
				(0.0427)
FDI inflows % GDP				1.379***
				(0.252)
Trade % GDP				0.145**
				(0.0639)
GDP per capita				-23.86
				(19.53)
Constant	17.62***	19.05***	16.93***	231.4
	(0.175)	(0.309)	(0.277)	(180.7)
Industry (2-digit NAICS) FE	No	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,646	1,646	974	520
Clusters (State)	87	87	61	45
Adj. R-squared	0.707	0.777	0.943	0.882

WLS, with robust SEs clustered by state. * p < 0.1, ** p < 0.05, *** p < 0.01.
Dependent Variable: Revenue (revenue, ln); variation by firm-state-year.
Variable of Interest: Startup costs: Foreign (fixed assets in first year of data reporting, ln); variation by 3-digit NAICS industry-state. *Sample:* Foreign firms.