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RESEARCH ARTICLE

Violence, Predation, and FDI Entry

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Abstract

I present a theoretical framework that links different configurations of organized violence to global patterns in foreign direct investment (FDI). Insurgents, states, and rogue government agents all use violence for political purposes (i.e., incapacitating rivals), but they vary in how they use violence for economic purposes (i.e., generating income). Applying Olson's (1993) concepts of "roving" and "stationary" banditry, I hypothesize that violence perpetrated by rebels and rogue agents indeed depresses a host country's commercial appeal, but that violence perpetrated willfully by the state doesn't. This claim is tested against data on FDI "entry" by several thousand multinational corporations between 1994 and 2018.

Keywords: globalization; foreign direct investment; political violence; economic predation

Introduction

How does political violence shape patterns in foreign direct investment (FDI)? Violence is inherently destructive. It is antithetical to production, which both bolsters and is bolstered by the physical, social, and human capital that violence destroys. Organizations specializing in production would be better off in a world devoid of organizations that specialize in violence. The fact of the matter, however, is that the international system of states has been constructed by means of violence, and both the threat and use of violence continue to play a central role in our sociopolitical relations. Indeed, political order, as we know and understand it, is predicated on the organization of violence (Wagner 2007).

Although an inescapable feature of our system, violence varies in its form and function across space and time. How violence is organized and used is thought to have profound implications for societies' economic development (Bates 2010). This relationship is perhaps especially pronounced today, as capital becomes increasingly mobile. Multinational corporations (MNCs)—the large and rapidly growing share of firms with the means to internationalize production and distribution—enjoy the freedom to *choose* the environments in which they operate. They can locate capital in places where violence is organized in (relatively) less detrimental and disruptive ways.

The aim of this study is to evaluate and to understand how variations in the organization of violence influence MNCs' investment activity. In doing so, I draw together two strands of the FDI literature, presenting a unifying theoretical framework that addresses gaps in each.

The first concerns the relationship between insurgency and FDI. There is an extensive body of research on the topic.[1] The common hypothesis is that MNCs are wary of investing in unstable and war-torn environments. The reasons are myriad and often undifferentiated in empirical models. Fighting can directly affect firms when their property or personnel are targeted (Powers and Choi 2012; Klapper et al. 2013; Dai et al. 2013; Camacho and Rodriguez 2013). But even those not immediately exposed to fighting are still affected by the general destruction of infrastructure and capital, the disruptions in local supply and distribution networks, and the political uncertainty that accompanies violent contests for power (Nigh 1985; Braithwaite et al. 2014; Barry 2018; Doctor and Bagwell 2020; Joshi and Quinn 2020).

[1]See Li 2006 for a review.

The second strand concerns the relationship between repression and FDI. While there is a large literature on how MNCs respond to civil and political rights broadly construed, there has been relatively little empirical study of how they respond to governments' abuse of physical integrity rights specifically.[2] Early scholarship posited that MNCs prefer partnering with states willing to physically suppress agitators (e.g., O'Donnell 1978; London and Ross 1995; Tuman and Emmert 2004). The mainstream hypothesis today is rather the opposite. The mechanisms motivating this hypothesis are imprecise and leave something to be desired. One line of reasoning, for instance, is that repression encourages political unrest, depresses human capital and productivity, and weakens foreign firms' ability to integrate into the local market (Blanton and Blanton 2007, 2009). If we assume that these are unfavorable conditions for MNCs, it follows that human rights abuses discourage direct investment. Another line of reasoning is that repression deters foreign investors who wish to avoid "guilt by association" in a world increasingly committed to basic human rights and other universal values (Spar and La Mure 2003). While there is certainly nothing wrong with these arguments, the first only establishes a very indirect link between repression and FDI, while the second speaks to a tangential mechanism (i.e., perceptions of human rights disrepute) that can be independently measured and tested (Barry et al. 2013; Vadlamannati et al. 2018).

Correlational studies are decidedly not uniform in their findings for either hypothesis (e.g., Fatehi-Sadeh & Safizadeh 1989; Crenshaw 1991; Loree and Guisinger 1995; Barry et al. 2013; Garriga 2016; Witte et al. 2017). But on balance, the evidence suggests that FDI activity decreases with both conflict and repression (Blanton and Blanton 2012; Braithwaite et al. 2014; Barry 2018). The empirical focus, however, has been squarely on the general *presence* and *intensity* of these conditions. What's missing is an appreciation for how the *form* and *function* of organized violence might also affect commercial interests.

I rectify this deficiency here by considering how the means of violence are distributed in the host country and by focusing on the incentives motivating different types of violent organizations. Specifically, I apply Olson's (1993) concepts of "roving" and "stationary" banditry to three different types of violent actors—rebels, the state, and government agents. Combining this logic with the "political risk" theory of FDI yields some distinct hypotheses about how MNCs respond to different configurations of organized violence. I contend that MNCs' interest in a host site decreases with the number of violent rebel groups and the frequency with which violence is perpetrated by rogue government agents; but that MNCs are more accepting of violence when it is administered willfully by the state. I test these hypotheses by analyzing patterns in FDI *entry* at the firm level. The data are consistent with theoretical expectations.[3]

This study contributes to the literature in several ways. Narrowly, it adds another pathway by which rebellion affects FDI and also clarifies the theoretical and empirical links between repression and FDI. Broadly, it provides a framework for thinking about the relationship between violence and investment in the contemporary world—a world that is still politically structured by organizations specializing in violence, even while it is being economically "globalized" by those that specialize in production.[4]

Theory

A basic framework

Organized violence serves two purposes. The first is political. An organization can strengthen its own position and influence by eliminating or otherwise incapacitating rivals. The terms of the arrangement

[2]See Blanton and Blanton 2012 for a review of the literature on rights, institutions, and FDI.

[3]I isolate FDI *exit* in supplementary analyses presented in the Appendix. The results indicate that *entry* and *exit* are governed by distinct processes, and that existing theories and models are tuned to the former. The latter demands far more direct attention and tailored theorizing than it has so far received (Boddewyn 1983; Tan and Sousa 2019).

[4]This framework is presented in its most generalizable form here. Additional nuances can—and should—be introduced in future research. Such nuances might include, but are not limited to, the *industry* of investment (Witte et al. 2017; Wright and Zhu 2018), *home country/culture* (Beazer and Blake 2018), the MNC's *size and experience* (Jiménez et al. 2014; Oh and Oetzel 2017; Oh et al. 2021), and *mode of entry* (Dikova and Witteloostuijn 2007). As possible moderating factors, their exclusion should only bias the results *against* the hypotheses considered in this study.

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by which political communities at any level operate are set by those with the most bargaining leverage; and bargaining leverage is ultimately a function of groups' relative capacities to enforce or disrupt the status quo by means of violence (Wagner 2007). The second purpose is economic. In lieu of productive activity, violent organizations (or their membership) can threaten or use violence as a means of generating income. That is, they engage in economic "predation"—the forceful redistribution of wealth and value from those who create it to those who have the capacity to destroy it (Tilly 1985).

Because both activities *motivate* the organization of violence, groups that achieve the feat are necessarily inclined to *perform* them. What varies across groups, however, is the form these activities take in practice. In what is fundamentally a theory of the state, Olson provided a useful, intuitive framework for thinking about this variation (1993).

He posited two types of violent organizations. One is the "roving bandit." This type of organization roams the countryside in search of settlements to plunder. Upon discovering one, its incentive is to engage in unrestrained theft—there is no guarantee, after all, about when or where it'll have another opportunity to provide for itself. The second type is what he terms the "stationary bandit." A clever predatory band will realize that, by settling down and laying exclusive claim to a settlement, it might generate a steady and reliable stream of income for itself. There is a consequent change in behavior that accompanies this transition. The "stationary bandit" is best served by engaging in a more restrained form of theft. An optimal extortion rate is one that is limited and predictable, instilling confidence in the residents that any value they produce *above* the extortion rate will remain theirs. Furthermore, the "stationary bandit" will protect the residents from unrestrained theft by other violent organizations. In this context, people will work harder and create more overall wealth, leaving everyone better off. For Olson, these are the conditions that ultimately give rise to political order, investment, innovation, and development.

Olson's framework has been applied before to theories of FDI. It has featured most prominently in the literature on "political risk," which is rooted in Vernon's observation that foreign investors are particularly vulnerable to *ex post* adjustments in the extortion rate charged by host governments (1971). MNCs cannot know for sure which governments are sincere or insincere in their *ex ante* commitments.[5] This has implications for how FDI activity is distributed, especially among less developed countries. A popular argument is that political and legal constraints on democratically elected governments make their *ex ante* commitments more credible, resulting in a "democratic advantage" in FDI (Li and Resnick 2003; Jensen 2003, 2008). In other words, the degree of certainty about the state's *de facto* extortion rate varies in accordance with governing institutions (Olson 1993); and this, in turn, shapes patterns in FDI.

While this literature makes good use of Olson's framework, it tends to focus exclusively on the state as a source of predation. Not only does this neglect other sources (Jakobsen 2010), but it (implicitly) assumes that the market for organized violence is monopolized. We are thus left with a special case of Olson's model—one in which the world is comprised of only "stationary bandits." In reality, of course, there is considerable variation in how the means of organized violence are distributed. In some countries, the ability to threaten or use violence for political and economic purposes *is* highly centralized and belongs almost exclusively to the state; but in others, the ability to threaten or use violence is dispersed. Different configurations of organized violence lend themselves to different forms of predation and thus pose differing levels of risk to investors. I will apply Olson's framework here to evaluate configurations involving not only the state but also rebel groups and rogue government agents.

Rebel groups and FDI

It has been commonly hypothesized that FDI activity decreases with political unrest and war (Nigh 1985; Li 2006; Braithwaite et al. 2014; Doctor and Bagwell 2020). This literature tends to emphasize the general destruction and disruption that accompanies fighting, particularly when it is intense and prolonged (Barry 2018). While the intensity of fighting is certainly an important factor in

[5]Vernon refers to this dynamic in the MNC–host relationship as the "obsolescing bargain."

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the relationship between conflict and FDI, another factor that is often overlooked (neglected, even) is the incentive for economic predation that accompanies rebel mobilization. By definition, civil war involves the participation of rebel groups. Their presence in a country is thus necessarily subsumed in existing empirical studies. An application of Olson's (1993) logic, however, implies that the formation of rebel organizations has an effect on investment that is *independent from the intensity of fighting*, and one that should therefore be isolated both theoretically and empirically.

A rebel group is a violent organization that operates outside of the existing political order. That such a group exists means that it controls a nontrivial share of the market for organized violence.[6] We can assume that it endeavors to expand its market share by violently eliminating rivals and opponents. But rebel groups also use violence for the purpose of securing wealth and resources. "Greed"-based explanations for civil war posit that revenue generation is necessary to sustain an insurgency (Collier and Hoeffler 2004). It may even be a primary motivation, if the wages of rebellion are greater than what can be earned from normal economic activity (Grossman 1999).

There are a variety of ways for rebels to generate revenue. Among these are plunder and extortion (Mehlum et al. 2002). Rebel organizations (or their membership) might occupy and appropriate a firm's property, demand cash payments in exchange for "protecting" the firm's facilities and personnel, or intercept the firm's cargo while in transit. Whatever the method, these activities impose significant costs on the targeted firms (Klapper et al. 2013; Camacho and Rodriguez 2013), which is reason alone for MNCs to avoid countries where rebel groups operate. Indeed, foreign firms might be especially attractive targets, given that they are typically much wealthier than their local counterparts, and because they are "outsiders."

Rebel groups are akin to Olson's "roving bandits." By definition, these organizations have monopolized neither violence nor theft; and the lives of their membership are constantly under threat. Securing resources is necessary for the continued survival of the organization, and members have little reason to *not* acquire as much wealth for themselves as possible when the opportunity presents itself. The incentive is to engage in *unrestrained* predation.[7] Where and when firms are targeted will also be *unpredictable*, making it difficult to implement effective mitigation strategies.

It is important to note that this is not an affliction of *rebellion*, per se, but rather one of the *rebel group* as a particular type of violent organization. This implies that the corresponding risk of predation increases with the number of rebel groups. When there are more rebel groups, there are more opportunities for firms to be indiscriminately and ruthlessly targeted. It also means there is more competition for resources and market share among rebel groups, which should only further weaken any incentive to practice restraint.

A simple hypothesis follows: **Hosts' FDI appeal declines with the number of rebel groups operating within their borders**. Importantly, because the threat of unrestrained and unpredictable predation is a distinct mechanism, this relationship should be observed independently from the overall intensity of fighting.

The state, its agents, and FDI

While much of the literature on violence and FDI has focused on bottom-up political violence (i.e., dissent and civil war), a handful of studies have evaluated political violence from the top-down (i.e., repression). In a sharp break from traditional thinking, the mainstream hypothesis today is that FDI increases with government restraint (Blanton and Blanton 2007). The strength of this link, however, remains suspect. It is not clear, either theoretically or empirically, if MNCs actually care about

[6]The size of that share can vary considerably, of course. But if the group exists at all, it means that the government has failed to incapacitate it. Even if the group is too weak to pose an existential threat to the state, it is still capable of threatening or using violence for its own purposes. I assume that this capability is removed only with the incapacitation of the organization.

[7]Not all rebel groups are equally unrestrained, of course (Humphreys and Weinstein 2006; Sabates-Wheeler and Verwimp 2014; Stewart and Liou 2017). I am simply assuming that, on average, rebel groups are closer to "roving bandits" than they are to "stationary bandits" on Olson's (1993) spectrum of predation. I relax this assumption in sensitivity tests (see section B in the Appendix).

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hosts' repressive practices specifically, or if they are responding instead to a variety of generic sociopolitical concerns that are exacerbated by state violence (Blanton and Blanton 2012; Barry et al. 2013; Garriga 2016). Combining Olson's framework with insights from contemporary theoretical work on repression presents more precise and direct pathways connecting repression and FDI.

The modern state is complex and serves myriad functions. That said, it is still fundamentally a violent organization, and one that is unmatched in its ability to threaten or use violence as a means for realizing its interests. The degree to which violent repression is practiced varies considerably across space and time.[8] Theory has settled on two primary explanations for this variation. The first treats repression as a strategic choice. Repression has costs, but the state is willing to accept these costs when political challengers pose credible threats to the status quo (Poe 2004; Ritter 2014). The second treats repression as an agency problem. Repression in this context isn't central policy but rather reflects a government's inability to rein in wayward offices and employees who use the tools of the state (i.e., the means of violence) to serve their own interests (Englehart 2009; Cingranelli et al. 2013).

The natural conclusion from this work is that not all instances of repression are the same. While an equally high level of repression might be observed among a pair of states, the contexts in which repression is being practiced could be very different. If we plug these different contexts into Olson's framework, we see that violent repression has divergent implications for economic predation and thus for MNCs' investment activity.

In the latter context, wherein repression reflects agency loss, the means of organized violence are dispersed. When police and military and governmental factions are able to act on their own interests with little fear of discipline or reprisal from above, they operate, in effect, as autonomous violent organizations. Like rebel groups, the incentive structure for such organizations is akin to that of the "roving bandit." Not only will these groups use violence as a means of removing political opponents and challengers but also to enrich themselves. When there are opportunities to engage in economic predation—for instance, during periods when there is little direct monitoring or oversight by the central government—they will threaten or use violence (or, alternatively, threaten to withhold protection from theft and violence by other groups) to extort as much wealth as is feasible.[9] The targets and the timing will be relatively unpredictable, as will be the price of compliance. When the use of violence by state agents is unrestrained, so too will be their theft. In this context, the risk of predation increases with violent repression.

This does not mean, however, that the risk of unrestrained predation *always* increases with repression. In the other context, wherein repression is employed strategically and deliberately by the state, the means of organized violence remain centralized. Like rebels and rogue agents, the state is a violent organization that uses violent repression to incapacitate rivals and opponents. Unlike rebels and rogue agents, however, the state has much less incentive to engage in unrestrained predation. Per Olson's framework, the state is a "stationary bandit." It relies on a relatively limited and formalized system of extortion that is (approximately) optimized.

The optimized extortion rate varies across states and is determined by governing institutions and other factors. Indeed, as noted earlier, this has been studied extensively in the FDI literature concerning regime type and "political risk" (Li and Resnick 2003; Jensen 2006). But even in countries where the *de facto* extortion rate is higher or less certain, the risk that the state threatens or uses violent repression for the purpose of unrestrained, unregulated, indiscriminate theft is relatively low. Despite its prominence in the "political risk" literature, outright nationalization/expropriation of multinationals' property is rare (Kobrin 1984).[10] Instead, economic predation by the state is typically restrained and systematized.

[8]See Fariss (2014) and Hill and Jones (2014) for extensive reviews of the literature on the empirical study of human rights practices around the world.

[9]This does not mean that they will be entirely unrestrained. Too much predation might force the government to redeploy its monitoring and oversight assets, for instance, which would be an undesirable outcome for the group. The argument is simply that these groups will typically be closer to the "roving bandit" end of Olson's spectrum.

[10]Even the risk of this most extreme form of theft by the host state varies in systematic ways and is thus, to some degree, predictable (Li 2009).

China exemplifies this distinction. A combination of autocratic governing institutions and the sheer force of its economic "gravity" results in an optimal extortion rate that is high relative to other states. This is readily observed in how China treats its foreign guests. It routinely demands technology sharing and local sourcing. The fact that these practices are routine, however, speaks to their systematic nature. MNCs know what the costs of doing business in China are beforehand. A few firms are perhaps deterred by this; but many others accept the costs and simply factor them into their long-term business projections.[11]

The systematic nature of economic predation by the state doesn't change when it deploys violence against its subjects. On the contrary, when repression is a strategic choice, its purpose is to preserve the existing political order—the very system in which the state's economic practices are regulated and routinized. In this context, the risk of unrestrained predation *does not* increase with repression.

There are other reasons why state repression might coincide with a generally less-appealing business environment. These indirect or tangential mechanisms are already explored in the existing literature (Blanton and Blanton 2007, 2012; Barry et al. 2013). But the logic proffered here fails to identify any increased risks to firms' property and welfare resulting from willful repression by the state. It therefore offers no theoretical justification for the hypothesis that MNCs are deterred by such violence. What it does suggest, however, is that *unwilful* repression—the type practiced by government agents pursuing their own interests—does pose heightened risks to firms' economic interests. The implied hypothesis is that **hosts' FDI appeal declines with the use of repression by rogue agents**, **but not with the use of repression by the state.**

Data & method

To test these hypotheses, I analyze patterns in MNCs' annual FDI activity between 1994 and 2018. The sample is comprised of nearly ten thousand major multinationals, paired with all host countries from the developing world that are recognized in the COW State System Membership list (2017) and for which data are available.[12] The unit of analysis is thus the company-country-year. Enterprise-level data were acquired from the LexisNexis *Corporate Affiliations* historical database and then aggregated at the level of "ultimate-parent" to create an MNC-level dataset recording the total number of enterprises firm *i* operates in country *j* at time *t*.[13] More than one hundred different "home" countries are represented in the sample.[14] Most industries are also represented, as measured at the 4-digit level of precision in the SIC schema. The median number of industries attached to each MNC during a given year is 7 (mean of 14); the median number of enterprises owned is 18 (mean of 40); and the median number of countries partnered with is eight (mean of 13).

The outcome variable employed in the analysis is binary and records *entry* into the host market.[15] The sample is thus limited to company–country pairs for which there is *no* existing FDI relationship at time *t-1*. *Entry* is observed when an FDI relationship is actualized—that is, once the number of subsidiaries operated by firm *i* in country *j* changes from zero to one or more. I also limited the estimation sample to years in which firm *i* is observed as entering into at least one new market in the relevant population of countries. This is to weed out superfluous observations for which *entry* is not even possible because the MNC in question is not actively seeking new FDI partnerships in the "developing" world. I also estimate the models of FDI *exit*, but these are relegated to the Appendix.

[11]See Graham et al. (2016) on political risk management by MNCs.

[12]It is now common practice to isolate developing countries in the analyses of FDI (Blonigen and Wang 2005). I adopt a broad definition of "developing" here, which includes all countries except those comprising the advanced capitalist bloc (i.e., Western Europe, the United States, Canada, Japan, and South Korea).

[13]*Corporate Affiliations* is a rich and reliable source that has been used regularly in scholarly research (e.g., Berry et al. 2010; Oladottir et al. 2012; Barry 2016).

[14]These are, of course, not evenly distributed. As should be expected, the United States is the most represented home country (~30%), followed by Japan (~12%), Germany (~7%), and the UK (~7%).

[15]I do not account for the *size* of the original investment, as such records are missing for most enterprises in the *Corporate Affiliations* database. This biases the results against my hypotheses, if firms manage "risk" not by avoiding *entry* but instead by committing fewer resources up front. It's another nuance that can and should be explored further in future research.

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The theory is attuned to the host-selection stage, when the parent MNC enjoys freedom of choice and selects from among several plausible partners based on projected risks and rewards. Theoretical expectations are less clear post-entry, when the decision-making context shifts from one of generalized cross-host comparison to site-specific management and enterprise performance (Boddewyn 1983; Tan and Sousa 2019).[16] I thus treat *exit* as an empirical curiosity—one that is secondary to the primary focus of this study, but that is still interesting and relevant insofar as the statistical results have implications for future theoretical development and research.

The argument is that hosts' appeal to foreign investors is influenced not merely by violence but by the configuration of organized violence. I identified three types of violent actors that factor into this configuration. Rebel groups comprise one type, rogue government agents a second type, and the state itself a third type. Testing the hypotheses thus requires measuring the presence and prevalence of violence by each type in host country j during the given year. The first is straightforward. Using data from the UCDP Armed Conflict Database (Harbom et al. 2008), I generated a variable counting the number of distinct *rebel groups* mobilized and engaged in armed insurgency against the host state. Directly measuring the other two is more difficult. While there are a couple of widely used datasets that record levels of government repression, these measures do not identify when the use of repression is central policy versus when it is the result of agency loss. This poses a problem because the hypothesis is that MNCs are responsive to the latter type but not the former. In lieu of direct measures, I approximate the two contexts by means of an interaction term in the statistical models.

One variable included in the interaction is an ordinal measure of *repression*. This is the "physical integrity rights index" from the CIRIGHTS project (Cingranelli et al. 2014; Mark et al. 2022), transposed so that it increases with government abuses.[17] The second variable included in the interaction term is an indicator of *state capacity*. Specifically, I use the new "absolute political capacity" metric developed by Fisunoglu et al. (2023). It builds on the classic "relative political capacity" metric (Arbetman-Rabinowitz et al. 2012) by combining data on both the government's ability to generate *inputs* (i.e., absolute extraction capacity, excluding natural resource rents) and the value of its *outputs* (i.e., life expectancy).[18] The resulting variable (theoretically) ranges from 0 to 1, rescaled here to range from 0 to 100. Although it is a basic function of the state, collecting revenues is actually quite difficult and requires significant investment in administrative and bureaucratic infrastructure. States that demonstrate an ability to do so effectively—and to then convert those resources into real societal improvements—are typically more competent and capable.[19]

I am assuming that agency loss declines with state capacity, as administratively adept governments are better able to discipline, and therefore deter, self-serving behavior by wayward employees. By interacting this variable with repression, I am assuming that physical integrity rights abuse in higher-capacity states reflects repression as strategic choice, whereas such abuse in lower-capacity states reflects at least some degree of repression by rogue agents.[20] Given the measurement scheme, the empirical hypotheses can be restated as follows:

[16]For instance, sensitivities to general risk factors seem to dampen once a firm has settled into a host market (Benito 1997; Dai et al. 2013; Barry 2018).

[17]In sensitivity tests, I isolate each of the four "physical integrity rights" components. I also substitute the Political Terror Scale (Gibney et al. 2020) and the "physical violence index" from the V-Dem project (Coppedege et al. 2020) for the CIRIGHTS measure. The findings are unchanged. See Figures B3 and B5 in the Appendix.

[18]There is some correlation between *state capacity* and *repression*, but it's relatively weak. A look at the descriptive statistics for *state capacity* within three levels of *repression* (i.e., "low," "moderate," and "high") shows that the distributions of the values are all closely aligned. In other words, all levels of *repression* are observed with some regularity among low-capacity states, high-capacity states, and everything in between. See Figure A2 in the Appendix.

[19]See *The Performance of Nations*, edited by Kugler and Tammen (2012), for a description and defense of this concept, along with several studies demonstrating its applicability.

[20]Of course, even administratively inept states can and do use repression as a matter of central policy. However, the real issue at stake here is the feasibility of repression by rogue agents, and such behavior is far more likely to be found in low-capacity states than in high-capacity states. In other words: The amount of observed repression owing to agency loss decreases with state capacity (Englehart 2009). While existing theory and research lead me to believe this is a sound approach, it is ultimately an empirical proxy and some caution is warranted.

H1: Pr*(Entry) by firm* i *in country* j *decreases with the number of rebel groups.*
H2: Pr(*Entry) by firm* i *in country* j *decreases with repression at low levels of state capacity, but is less responsive to repression at high levels of state capacity.*

The baseline probability that *entry* occurs is primarily a function of firm *i*'s overall FDI expansionism during the year—a dynamic that is particular to the MNC, and that is largely independent of the conditions characterizing country *j*. I thus include a variable measuring firm *i*'s *entry rate* during year *t*. This is calculated by taking the total number of observed entries in the "developing world" as a proportion of the total number of candidate sites (i.e., countries with which firm *i* does *not* yet have an FDI relationship).[21] This is conceptually akin to estimating MNC-year fixed effects but collapses 15,081 constants (i.e., the number of distinct MNC-year groups included in this analysis) into a single variable.[22] If new FDI ventures were randomly allocated, then this is the only variable that would associate reliably with the probability of *entry*. Of course, new FDI ventures are *not* randomly allocated; country-level factors are thus expected to inform whether country *j* performs above or below the baseline probability of *entry* by firm *i*, as recorded by this variable.[23]

I control for a variety of other factors thought to be important determinants of FDI. These include geographic, economic, and sociopolitical characteristics of the host country that either add to or detract from its viability as an investment site.[24] The argument implies that the configuration of violent organizations operating in a country should affect MNC decision-making *independently from* the actual violence. It is thus important to account for other dimensions of rebellion and conflict. I use the number of battle-related deaths (recorded in hundreds) resulting from intrastate conflict during the year to measure the *intensity* of fighting (Davies et al. 2022). I also record the *duration* of conflict with a variable that counts the number of consecutive years fighting has been observed.[25] Both variables correspond with episodes of intrastate conflict recorded in the UCDP/PRIO Armed Conflict Database (Gleditsch et al. 2002).[26]

Another potential confounding factor is the quality of the host's governing institutions. Those that provide for transparency in policy-making and reliability of enforcement are particularly important for fostering a positive business environment (Jensen 2003, 2008; Biglaiser and Staats 2010). I use the *rule of law* index from the V-Dem Project (Coppedge et al. 2018), which is informed by judicial independence, respect for the constitution, access to justice, public sector accountability, impartiality in public administration, and legal transparency. It ranges (theoretically) from 0 to 1, rescaled here to range from 0 to 100.[27] A host's *financial openness* and *trade integration* determine the ease with which prospective investors can integrate local enterprises into their existing global networks. These are measured using KOF's "de jure financial globalization" and "de facto trade globalization" indices, respectively (Gygli et al. 2019), and are expected to correlated positively with *entry*.[28]

[21]The resulting variable ranges (theoretically) from 0 to 1 and is rescaled to range (theoretically) from 0 to 100.

[22]In effect, I am assuming that firm *i* first decides how many markets, *x*, it will *enter* during the year and then proceeds to select from the viable candidates (i.e., countries *j . . . z*). The conditions characterizing country *j* inform the relative likelihood that it is among the *x* selected by firm *i*.

[23]Because total FDI activity is known to vary from 1 year to the next with changes in global economic conditions, it is common to estimate yearly fixed effects in models of FDI flows. However, such global fluctuations are already subsumed by this MNC-year-level measure—if global conditions encourage more (or fewer) *entries* during a given year, that will be reflected across firms in higher (or lower) values on the *entry rate* variable.

[24]Because the baseline probability of *entry* is assumed to be set at the level of MNC year (i.e., via the *entry rate* variable, noted above), country-level factors are evaluated cross-sectionally. I thus opt to include a carefully constructed, but comprehensive set of control variables.

[25]This variable takes on a value of 1 with the onset of intrastate conflict and increases with each year that fighting is sustained. It resets to 0 with the cessation of fighting.

[26]Across conflict episodes, the *rebel groups* variable only correlates weakly with fighting *intensity* and *duration*, suggesting that it indeed captures a distinct dimension of intrastate conflict. See Figure A1 in the Appendix.

[27]In sensitivity tests, I also include a separate measure of *political constraints* (Henisz 2000). See Table B1 in the Appendix.

[28]The "de jure financial globalization" index reflects the country's legal openness to cross-border capital flows, both inward and outward. The "de facto trade globalization" index reflects both the volume of trade and the country's position in international trade networks.

MNCs are thought to show preference to host countries that are "closer" to home. This includes geographic proximity but also institutional similarity (Beazer and Blake 2018). I control for both the *geographic distance* and the *administrative distance* between home and host (Berry et al. 2010). The former is measured as the number of kilometers separating each country's geo-coordinated "centers." The latter is a spatial metric informed by colonial history, commonality in legal systems, and religious traditions. Both increase with distance, meaning that they are expected to correlate negatively with *entry*.

Market size and sophistication are the most reliable correlates of FDI. I use *population* to measure the former and per capita GDP (i.e., *development*) to measure the latter. Both variables are logged. Physical size is measured by total *land area* in square kilometers, also logged. This variable captures the geographic scope and breadth of the host country; it also correlates strongly (~0.8) with primary sector production. The data on population and land area are taken from the World Bank's World Development Indicators database (2019), while data on GDP come from the UN National Accounts database United Nations (2019). Because the options for transport are necessarily more limited in *island* and *landlocked* countries alike, these conditions likely depress FDI. I include a binary indicator for each in the analyses.

A final control variable, *MNC stock*, records the number of foreign multinationals with FDI projects in country *j*. The measure is logged and was generated using the MNCs that populate the *Corporate Affiliations* historical database. Although this source doesn't include the full universe of MNCs, it is large sample of many thousands and is likely a fair representation of overall FDI distribution and concentration. I anticipate it will correlate strongly with *entry*, as foreign investments tend to agglomerate.

All variables except *entry rate* are lagged 1 year.[29] This is to account for the time it takes between when the plan to *enter* a given market is made and when it is actually implemented. And, though it is unlikely that *entry* by a single firm *i* has an immediate causal effect on any of the host country conditions represented on the right-hand side of the equations estimated here, it is unlikelier still that it would exert influence on them in the year prior. While there are certainly valid concerns about reverse causation when studying FDI,[30] I've adopted a modeling strategy aimed at minimizing that particular risk.

Results

The results from a series of logit models are presented in Table 1.[31] For the sake of clarity, the estimated average marginal effects (AMEs) are reported, and as percentages (i.e., on a 0–100 scale).[32] It is important to note that, because of the structure of the dataset (i.e., company-country-year), and because the analysis isolates significant changes in the firm's FDI position (i.e., its *entry* into a new market), nonzero outcomes are (relatively) infrequent. Simply put, the likelihood that firm *i* initiates a new FDI relationship with country *j* at time *t* is necessarily quite low.[33] Indeed, *entry* is recorded in 2.22% of cases. Meaningful interpretation of the results requires some extrapolation to the "bigger picture." The AMEs should be viewed not only in terms of what they mean for the probability of investment by firm *i* but what they mean for the overall distribution of investment activity by the universe of firms $i \ldots n$. In this light, even fractions of a percent might ultimately translate into several more (or fewer) new FDI projects in the host country. And seeing as FDI often entails a long-term commitment, this difference can have notable consequences for the host's economy into the future.

[29]See Table A1 in the Appendix for summary statistics.

[30]See, for instance, the work on how foreign investment (and "globalization" more generally) can trigger unrest and even war (Bussmann and Schneider 2007; Pinto and Zhu 2022).

[31]Errors are clustered by parent MNC in all models. This is to guard against the risk of *de facto* clustering in the production of the *Corporate Affiliations* enterprise-level database from which the present sample of MNCs is constructed (Abadie et al. 2017). See Table A2.2 and a corresponding discussion in the Appendix.

[32]See Table A2.1 in the Appendix for the original coefficient estimates.

[33]This does *not* mean that FDI *entry* is a "rare event." It is, in fact, common. Rather, this simply means that, at this very high level of resolution, the number of 0s far exceed the number of 1s recorded in the dataset.

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Table 1. FDI entry

	M1	M2	M3	M4
Fight intensity	−0.0051*	−0.0033*	−0.0015	−0.00072
Fight duration		−0.011***	−0.0067***	−0.0056***
Rebel groups			−0.085***	−0.046**
Repression				−0.062***
State capacity				0.0084***
Rule of law	0.019***	0.020***	0.020***	0.017***
Trade integration	0.018***	0.018***	0.019***	0.019***
Financial openness	0.010***	0.0096***	0.0092***	0.013***
Admin. distance	−0.0064***	−0.0056***	−0.0055***	−0.0061***
Geo. distance	−0.00017***	−0.00017***	−0.00017***	−0.00017***
Population	1.12***	1.15***	1.17***	1.27***
Development	0.69***	0.69***	0.67***	0.71***
Island	−0.41***	−0.18**	−0.17**	−0.15*
Landlocked	−0.21***	−0.23***	−0.22***	−0.31***
Land area	−0.12***	−0.12***	−0.13***	−0.15***
MNC stock	0.91***	0.90***	0.90***	0.83***
Entry rate	0.44***	0.44***	0.44***	0.44***

Notes: Logit estimation. Average marginal effects (AMEs), reported as percentages (0–100). 1,878,689 observations. 9,709 MNCs and 131 countries, 1994–2018. Entry recorded in 2.22% of cases. Entry rate recorded in period *t*. All other variables lagged 1 year. Errors clustered by MNC. Two-tailed significance tests: $^{*}p < 0.05$ $^{**}p < 0.01$ $^{***}p < 0.001$.

Four models are presented here. A 1% increase in the *entry rate* corresponds to a nearly half-point increase in the probability of *entry*, and this is consistent across all models. It is, as anticipated, a strong effect, but it is not deterministic. There is still considerable variation in hosts' respective prospects for selection, attributable to country-level factors.

The probability of *entry* by firm *i* increases reliably with *rule of law*, *trade integration*, *financial openness*, *population*, *development*, and the existing *MNC stock*. It decreases reliably with *distance* from the home country (both in terms of *geography* and *administrative* similarity) and *land area*. *Islands* and *landlocked* countries are also disadvantaged, all else being equal. The evidence from existing research suggests that market size and sophistication are the strongest correlates of total FDI.[34] And that is the case here, too: doubling the *population* size corresponds to a nearly 1% difference in the probability of *entry*, and doubling per capita GDP increases the predicted probability by just over one-half percentage point. *MNC stock* is also an especially strong correlate, as doubling the number of MNCs present corresponds to a 0.75% improvement in luring any given firm *i*. When one considers that, on average, *entry* is observed in 2.2% of cases, these estimated effects appear to be fairly powerful.

I gradually introduce the variables of theoretical interest across the four models. Model 1 includes only the fighting *intensity* variable. Consistent with earlier research, the probability that firm *i* strikes up a new FDI relationship with country *j* at time *t* decreases with the body count (e.g., Witte et al. 2017). That said,

[34]In most studies, FDI is aggregated across many or all sectors. Though fewer in number, higher-resolution studies do indicate that primary sector FDI, in particular, behaves differently than secondary and tertiary sector FDI (e.g., Wright and Zhu 2018). Although theorizing such differences goes beyond the scope of this paper, in sensitivity tests I isolate entries in extractive industries from non-extractive industries. The results are indeed different and interesting: *population* and *development* are found to have a much weaker and less reliable correlation with *extractive entry*, while *land area* (which proxies for natural resource abundance) is found to have a strong and positive correlation. See Table B2 in the Appendix.

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the magnitude of the estimated effect is relatively weak. A one-unit increase (i.e., about 100 battle deaths) reduces the viability of a potential host site by only 0.005%. Further calculations indicate that the AME attributed to 1000 additional battle deaths—a notable escalation in intensity—is 0.05%. This is roughly equivalent to the estimated improvement in host site viability corresponding to a 5% increase in *population* size, a 5.5% increase in *MNC stock*, a 7.5% increase in *development* level, or a three-point change on the 100-point *rule of law* index—all relatively minor differences in host country conditions.

The estimated effect attributed to *fight intensity* decreases further with the addition of *fight duration* in Model 2. Each additional year of fighting is estimated to reduce the probability of *entry* by 0.01%; 5 years of fighting reduces it by just over 0.05%, which is approximately equal to the magnitude of the effect attributed to 1000 additional battle deaths in Model 1. Though not as strong as the influences exerted by the best-performing variables, it suggests that the cumulative longevity of intrastate conflicts perhaps matters as much as, if not more than, the level of violence at any given time.

The *rebel groups* variable is introduced in Model 3. It is found to correlate reliably and negatively with *entry*—even in the presence of the fighting *intensity* and *duration* variables—as anticipated by the theory. The predicted probability is estimated to drop by nearly 0.1% with the addition of one rebel group, roughly equivalent in magnitude to the expected gain corresponding to an 8% increase in *population*. It drops by about 0.25% with the addition of three rebel groups, and just under 0.4% with the addition of five rebel groups—akin to the gains corresponding to a 22% and 35% increase in *population* size, respectively. Though not the most dominant determinant of *entry* found in the analysis, MNCs do seem more sensitive to the number of rebel organizations than they are to other dimensions of rebellion and conflict.

Indeed, when the *rebel groups* variable is included, the AMEs attributed to fighting *intensity* and *duration* both diminish substantially (and effectively drop to 0 for the former). While this wasn't predicted by the theory, it doesn't contradict the theory either. Indeed, what it suggests is that a nontrivial proportion of the effect that conflict exerts on FDI owes to the corresponding risk of economic predation by rebels. Only some of the effect is attributable to the general destructiveness and disruptiveness of fighting, which are the more commonly theorized mechanisms linking war to decreases in new investment.

Finally, *repression* and *state capacity* are introduced in Model 4. The results indicate that MNCs are deterred by violence perpetrated by the government. The probability of *entry* decreases reliably with the scale and scope of *repression*; and the AME attributed to each one-point change is similar in magnitude to what an approximately 3.5-point change in *rule of law* is expected to yield. What these results do not make clear, however, is whether these effects are generalizable across context. I theorized that MNCs care about repression when it is practiced by rogue agents acting in pursuit of their own political and economic interests, but less so when it is practiced by the central government as a means of deterring agitators from challenging the political status quo. To differentiate between these two contexts, I estimate a fifth model of *entry* wherein *repression* and *state capacity* are interacted. The results are displayed in Figure 1.[35] The empirical findings are consistent with the hypothesis.

The panel on the left shows the estimated AME of *repression* across the range of *state capacity* found in the sample. While the probability of *entry* always decreases with an increase in government abuse, the correlation is clearly most pronounced among low-capacity states. Indeed, among the weakest states (i.e., those in the bottom 5$^{\text{th}}$ percentile), each one-point increase in *repression* is corresponds to a 0.1% decline in the country's viability as a host site. Among those with moderate capacity, the decline drops to about 0.075%—a 25% difference in effect magnitude. It drops further still to 0.05% (a 50% difference in relative magnitude) for the highest capacity states (i.e., those in the top 5$^{\text{th}}$ percentile), with the upper-bound of the confidence interval approaching zero.

The panel on the right shows these same results from a different angle, plotting the predicted probability of *entry* across *repression* at three fixed levels of *state capacity*.[36] In the absence of repressive

[35]The model assigns a negative coefficient to *repression* ($p < 0.001$) and a positive coefficient to the interaction term ($p < 0.001$), indicating that the negative correlation with government abuse attenuates reliably with increases in *state capacity*. See Table A2 in the Appendix for the coefficient estimates from all five models.

[36]As these are contrived scenarios, the standard caution is warranted. That said, they are all found in the real-world data (see Figure A2 in the appendix). Indeed, positive outcomes can be found across all combinations of repression and state capacity,

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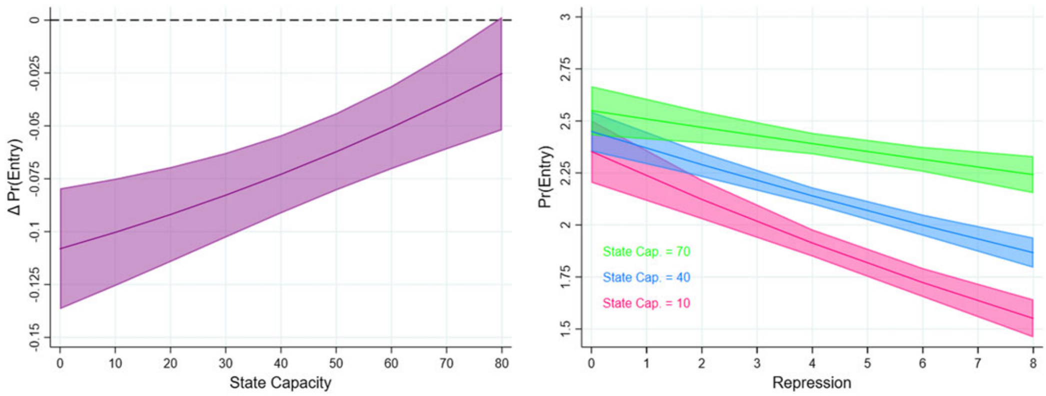

Figure 1. Conditional marginal effect of repression on FDI entry. Note: 95% confidence intervals in shaded areas. Values on y-axes reported as percentages (0–100). State capacity set to approximately 5th, 50th, and 95th percentiles, respectively, in panel on right.

practices, there is no statistical difference in FDI appeal between lower- and higher-capacity states. But notable separation is found among those where *repression* is observed more frequently. Among administratively inept governments, the probability of *entry* is expected to decrease from greater than 2.25% at low levels of *repression* to lower than 2% at moderate levels of *repression*, bottoming out at nearly 1.5% at the highest levels of *repression*. In other words, weak states with high levels of repression are anticipated to attract 33% fewer MNCs than their less repressive counterparts. The contrast with adept states is considerable: the predicted probability of *entry* drops from a high of about 2.5% at low levels of *repression* to a low of 2.25% at high levels of *repression*—only a 10% difference in overall performance (and a 33% better performance than their low-capacity counterparts with equally high levels of *repression*).[37]

These results are striking for three reasons. First, they imply that, as theorized, the perceived risks of unrestrained predation increase with repression, and especially so in environments marked by agency loss. MNCs are wary of the unpredictable, unregulated brand of theft that violent rogue agents might engage in. In contrast, they are more willing to accept repression as central policy. In this context, violence is used by the state to serve a largely political purpose, not an economic one. It is thus less threatening to foreign investors. Second, the magnitude of the effect among low-capacity states is fairly large when compared to the other variables measuring political violence employed in this study. It clearly represents a stronger deterrent than is attributed to *fight intensity* or *fight duration* and is moderately stronger than the effect attributed to *rebel groups*. Third, these results support the finding from other studies that accounting for *state capacity* is important and not only for whatever direct effects it might exert on outcomes of interest. Crucially, government strength likely moderates the effects exerted by *others factors* (Coan and Kugler 2008). In this case, that other factor is repression (and, notably, who is practicing it).

While the basic empirical model fits the data on firm *entry* fairly well, it struggles to fit the data on firm *exit*. These results are presented in the Appendix.[38] Distinctly theorizing FDI termination goes beyond the scope of this paper; and, indeed, the findings suggest that it is a question that warrants further scholarly attention and theoretical refinement (Boddewyn 1983; Tan and Sousa 2019). But there is one pattern that has emerged in the (limited) empirical work on the matter: MNCs' sensitivity to

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though in varying rates as predicted by the theory. There is a particularly interesting spike in the rate of entry among countries scoring high on both the repression and state capacity measures. China, Russia, Colombia, and Israel occupy this bin for extended periods, along with several other countries that meet the criteria for shorter durations.

[37]A variety of sensitivity tests on this hypothesis are presented in the Appendix. The basic findings are robust to alternative measures of *repression* (Figure B3), more flexible functional forms (Figure B4), and isolated types of repressive practices (Figure B5). They are also robust to using *rule of law* instead of *state capacity* (Figure B1).

[38]See Table A3 and Figure A3.

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general political risk factors seems to wane post-entry (Dai et al. 2013; Barry 2018). This apparent, sudden shift in sensitivity reveals an intriguing possibility—that MNCs' behavioral tendencies align more with "loss aversion" than "risk aversion," per prospect theory (Kahneman and Tversky 1979). Of course, that's just one plausible explanation for the disparities in the data. I'll leave it to future research to piece together this particular puzzle.

Conclusion

The scholarship on organized violence and FDI has two problems. First, it often fails to isolate the distinct (or even direct) means by which violence threatens MNCs. Precise and targeted mechanisms are subsumed by the general conditions that characterize a violent environment. Second, it lacks a unified and coherent theoretical framework. Studies on the relationship between conflict and FDI are treated as tangentially related to, but ultimately distinct from, studies on the relationship between repression and FDI. In reality, both are merely different manifestations of organized violence; and how they influence FDI activity should therefore follow a common causal pathway.

In this paper, I set out to solve these problems. I adopted Olson's (1993) model of "stationary" and "roving" banditry to serve as the overarching theoretical framework and applied it to three different types of violent organizations: the state, its agents, and rebels. The exercise yielded a few straightforward hypotheses. In short, MNCs are repulsed by configurations of organized violence that include rebel groups and rogue government agents but are indifferent to violent repression when it is willfully and deliberately employed by the state against its subjects. Testing these hypotheses against real-world data on FDI *entry* produced promising results. The findings are largely consistent with the theory; they also add nuance to the received understanding about the relationship between insurgency and FDI and challenge the now mainstream hypothesis that states who abuse the physical integrity rights of their subjects are punished by multinationals.[39]

Interestingly, supplemental analyses revealed that the general risks associated with operating in a predatory environment have little bearing on MNC *exit*. Indeed, few of the variables characterizing the host country environment correlate reliably with FDI termination, which is consistent with the limited body of work on the matter (Boddewyn 1983; Benito 1997; Barry 2018). There is clearly a need for more research on *exit* moving forward (particularly in the political economy literature).

Aside from explicitly theorizing *exit*, there are several other ways in which the present study can (and should) be improved upon in the future. I have presented a very generalized theoretical framework and demonstrated its plausibility. But the model can almost certainly be fine-tuned by introducing contextual factors that condition the hypothesized relationships. Firms' sensitivities likely vary with the industry of investment, for example (Blanton and Blanton 2009; Barry 2016; Witte et al. 2017);[40] or with their country and culture of origin (Beazer and Blake 2018), or with their size and experience (Jiménez et al. 2014). Likewise, not all rebel (or nonstate) entities are equal in their propensity to practice unrestrained economic predation (Humphreys and Weinstein 2006; Stewart and Liou 2017). Sensitivity tests revealed that MNCs do seem to discriminate between conflict environments where rebels generate income via commercial theft versus those where they don't (see Figure B2 in the Appendix). This could be probed further by, for instance, zooming in to the subnational level and evaluating where firms' subsidiaries are located in relation to contested versus uncontested territories (Dai et al. 2013) and, when uncontested, by measuring the propensities for unrestrained predation exhibited by the group holding the territory (whether rebel, state, or state-sponsored militia).

Such efforts will enhance the richness of the theoretical framework and the predictive power of the empirical model. But there are broader conclusions from the present study that are worth noting. Namely, that, in the age of international capital mobility and globalized production, there are perhaps

[39]This does not necessarily refute the idea that firms are sensitive to the "reputational costs" of doing business with notoriously abusive governments. However, previous research demonstrates that, if this is the case, firms respond to public "shaming" by media and activist organizations, rather than to states' actual practices (Barry et al. 2013; Vadlamannati et al. 2018).

[40]Sensitivity tests reported in the Appendix suggest that primary sector *entry* behaves differently than *entry* in non-extractive sectors. See Table B2 and Figure B6.

significant long-term costs for governments that are unable to centralize and control the means of violence. Such countries will struggle to forge new alliances with the MNCs who lay claim to much of the world's financial, technological, and intellectual capital. Curiously—and contrary to some theories of globalization (e.g., Strange 1998)—one implication is that, by rewarding and reinforcing states that are capable of monopolizing the means of organized violence and punishing those that aren't, global capitalism will in fact buttress the international system of states, rather than threaten its demise.

But this does not mean that global capitalism will tame violence altogether. The results of this study suggest that, though MNCs are wary of violence when it is perpetrated by rebels and rogue government agents, they are notably *less* deterred by violence when it is wielded as a tool by a competent state against political opponents and agitators. Capital mobility may incentivize governments to rein in rival organizations and wayward employees, but it does not disincentivize the deliberate and targeted use of violence by governments themselves. At least, not without "spotlighting" by human rights activists (Spar and La Mure 2003).[41] Indeed, the results of this study should serve to highlight the pivotal role that civil society might need to play in shaping the global capitalist system—in transforming it from one that merely reflects the profit- and efficiency-minded interests of the world's largest corporations to one that safeguards the basic rights and dignity of its people, too.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/bap.2023.30.

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