

Is violence bad for business?

The consequences of political violence for foreign direct investment

By

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Introduction & theory

Please note that this is the introduction/theory for the book version of this project, which is different from the three-paper dissertation that follows.

What are the consequences of political violence for foreign direct investment (FDI)? The conventional wisdom suggests that political violence, a type of political risk, inhibits foreign investment in the developing world (Jensen & Young, 2008). There are strong theoretical reasons to expect that the conventional wisdom is right: violence imposes costs. Violence can lead to direct destruction of assets, indirect destruction of assets via disrupted supply chains, labor forces, or domestic markets, increased transportation costs, heightened security measures, and/or regime turnover that is hostile to investors.

Despite these logical theoretical expectations, the empirical record paints a more mixed picture: Iranian firms recently began investing in telecommunications and electricity in Syria and Dutch firms have been profitable selling mobile phones in Sudan. Investors rebuild destroyed assets in places like Rwanda, Haiti, Liberia, and Sierra Leone. In the last two decades, more than 500 MNCs have invested in countries experiencing political violence, accounting for total stock of over \$200 US billion (Driffield *et al.*, 2013; UNCTAD, 2017). Similarly, the existing empirical work on this topic is inconclusive, suggesting that existing explanations are incomplete.¹ This null finding begs explanation—why is political violence not deterring investors as we would expect?

I theorize that the short answer must be about profit maximization, as that is what drives any competitive firm's behavior. But what makes these firms think they will be able to maximize profit in a conflict zone, especially when they have other options as to where to invest? I argue that the world is actually much more complicated than the conventional wisdom would have us believe. For the most part, the literature has only looked at the political determinants of FDI at a very high level. While these monadic, country-level

¹The null finding between political violence and FDI holds even after replicating previous studies with more recent data, which better captures FDI to the developing world, and a measurement of FDI (capital expenditure) that is more relevant to the theory.

studies are an important place to start, when we begin to examine subnational variation, sectoral variation, and source-country variation, we have a far more complex picture. We begin to see that different firms in different sectors from different countries have different incentives. I shed new light on our understanding of conflict and capital by showing how firms make decisions about investing in conflict zones varies on multiple levels.

Despite being addressed by multiple disciplines, scholars have yet to fully tackle the relationship between political violence and FDI. My theory suggests that existing accounts that posit that violence is exclusively bad for firms are incomplete. This project sheds light on the larger question in the literature of how firms make decisions about where to invest in the developing world. The existing international political economy literature provides explanations about FDI location choice that focus on regime type, respect for property rights, and regulatory standards. These explanations do well with the primarily North-North FDI that we observed up until 2000. However, they overlook much of the interesting variation in where firms choose to invest, particularly in the developing world.²

Understanding the effects of political violence on FDI is relevant beyond academic interest. FDI is critical to the global economy, exceeding the value of all other forms of cross-border capital flows combined. FDI is also critical to post-conflict reconstruction. In this sense, this research is critical for policymakers and economic development professionals. The results are directly relevant to firms, specifically risk managers and investors, who need to understand the relationship between political violence and FDI to efficiently allocate capital, make informed decisions and maintain investor confidence. In addition to speaking to the determinants of FDI literature, this research is also in conversation with the literature

²The degree to which firms can “choose” their investment location depends on the type of industry. For instance, we know that extractive FDI, due to the location specificity of the resources, is going to be where the resources are. Firms often have some choice as to where they would like to extract resources such as oil or gold from, but they do not have the same catalog of choices that, say, manufacturing firms enjoy. For instance, ArcelorMittal has little choice in locations with iron ore, so they invest in Liberia. These companies recognize the risk but invest anyway because there are not alternatives. Often, such as in the case of Sierra Leone, these firms hire private “militaries” for protection. That said, political violence has forced extractive FDI out of countries before (consider the example of Shell in Nigeria) and, theoretically, all other things being equal, extractive companies would prefer to invest in locations with less political violence. I account for this FDI in my analyses, but this is not the interesting variation that I seek to explain.

on the economic consequences of civil war, which, to this point, has largely evaded studying firms as the primary actors. Overall, this project builds on two sets of central questions in the discipline: questions about international finance and information and questions about the impact of conflict on capital flows.

The rest of this chapter proceeds as follows: after reviewing the existing the literature, I present a general theoretical framework for how firms think about political risk and the risk-reward trade-off inherent in investing in a conflict zone. I then return to the idea that different firms in different sectors from different countries have different incentives, that outcomes will vary with subnational geography, and that I consider each of these levels of variation and the associated observable implications in turn.

1 Empirical evidence on FDI and political violence is mixed

There is a robust literature in political science about the determinants of FDI. Scholars have proposed a series of independent variables, chief among them regime type,³ particularly as it pertains to respect for property rights, international agreements (such as BITs and PTAs),⁴ and domestic regulation.⁵ These explanations do well with the primarily North-North FDI that we observed up until 2000. However, they do not completely capture the interesting variation in which developing countries firms choose to place their non-site specific investments in.

The literature on FDI in political science has also largely overlooked political violence as a key determinant of FDI, with two important exceptions (Li, 2006; Jensen & Young, 2008). (Li, 2006) disaggregates by type of violence and argues that unanticipated civil and international conflict has a negative impact on FDI but that anticipated civil and international

³See, among others, Li & Resnick (2003); Jensen (2003); Pandya (2016)

⁴See, among others, Bütthe & Milner (2008, 2014); Elkins *et al.* (2006)

⁵Pandya (2013)

conflict does not. (Jensen & Young, 2008) model how the risk of political violence plays into investor perceptions (measured as the price investors paid for political risk insurance). Controlling for recent past experiences with violence, they find that the wealthier and more democratic a country is, the less likely investors are to perceive them as risky. Within the existing literature on the topic, the default assumption has been that political violence is bad for business (Li, 2006; Jensen & Young, 2008).

Other disciplines, notably economics and business, have also addressed the topic, though with decidedly inconclusive results. The econometric evidence is mixed, though generally finds that there is no statistical relationship between political violence and FDI at the country-year level (Kobrin, 1979; Schneider & Frey, 1985; Nigh, 1985; Fatehi-Sedeh & Safizadeh, 1989; Woodward & Rolfe, 1993; Loree & Guisinger, 1995; Olibe & Crumbley, 1997; Li & Resnick, 2003; Sethi *et al.*, 2003; Globerman & Shapiro, 2003). The qualitative evidence (primarily survey evidence) firmly supports the idea that firms take political violence into account when making investment decisions (Bass *et al.*, 1977; Porcano, 1993; Li, 2006). FDI scholars have noted the inconsistency between the qualitative and econometric evidence on this topic (Pearce *et al.*, 1992; Li, 2006). All of the studies make the theoretical assumption that violence is bad for business, but their empirical findings are largely mixed. Why is the evidence mixed when scholars seem to have this intuitive idea that violence is bad for business?

2 General theoretical framework

Conventional wisdom suggests that violence is bad for business, but the world is much more complicated. For the most part, the literature has only looked at the political determinants of FDI at a very high level. While these monadic, country-level studies are an important place to start, when we begin to examine subnational variation, sectoral variation, source-country variation and firm-level variation, we have a far more complex picture. We

begin to see that different firms in different sectors from different countries have different incentives.

I use these layers of variation as the organization for this project. I will first address variation in market structure, showing that a firm's relationship to violence varies by sector. Next, I will use spatial autoregressive models to show that while there is not a robust relationship between violence and FDI at the country level, when we look at the subnational level, violence strongly deters FDI. Finally, I consider source-country variation and show that the effect of conflict on FDI varies by source-country strategy, specifically with regard to asserting regional or global influence.

2.1 Firm-level variation

Firms vary on many dimensions, some of which are more relevant to this project than others. Perhaps the most obvious way firms differ is by sector. I will discuss this in depth later in this chapter. Firms also vary by size, sensitivity to reputational costs, how multinational they are, how diversified they are, and how they assess risk. For instance, there are niche operators with a particularly high tolerance for risk. One example is Standard Bank, a major international bank with a particular specialty in emerging markets. It was one of the first to open a branch in Afghanistan, and has operated in Sierra Leone since colonial times. Another is Luxembourg-based Millicom International, whose business model is to sell phones in Sub-Saharan Africa. Diaspora investors—firms run by people who originated in the host countries but have established themselves abroad—also often have a higher appetite for risk in their countries or regions of origin. Such is the case with Celtel, whose Sudanese founder, Dr. Mohammed Ibrahim, set up a network of companies across 13 countries in Sub-Saharan Africa. We also see this with Afghan-American investors in Afghanistan International Bank (managed by ING) and with expatriate investors in Somalia's mobile phone networks. Firms also assess risk of reputational costs differently: state-owned companies are less sensitive to reputational risks than companies that are directly accountable to their shareholders. This

may help explain investment by state-owned companies from China, India and Malaysia in war-torn Sudan, for example.

Firms also vary in their sources of finance, which in turn affects their appetite for risk. This is highly correlated with the type of foreign investor they are: they might be a contractor for an aid organization, a service provider, or an independent investor. If they are a contractor for the World Bank, their risk of not getting paid (credit risk) is quite low. For an independent investor, this is not the case. The sources of finance also vary by host country: in certain countries, it is far easier to get a risky loan. For instance, Chinese companies are all the more competitive because they are often backed by soft loans. Related, firms vary based on their home country, a topic I return to later in the chapter. The source country's regional or global strategy is of critical importance. Consider, for example, Austrian banks in Bosnia, ANZ (Australia-New Zealand) Bank in Timor Leste, and South African Stanbic bank in 13 countries in Sub-Saharan Africa, including the Democratic Republic of the Congo. Alternatively, consider the bigger case of China asserting influence in Africa via FDI. At the source-country level, firms also vary in terms of which markets they have which degree of access to via preferential trade agreements (PTAs) between their home country and the destination country. The same is also true with bilateral investment treaties (BITs). Similarly, the alliances that a home country has are important, particularly in sectors that protected in some way. For instance, a firm from a country allied with Pakistan probably has a better chance of winning its telecom auction than an Indian firm would. I control for these existing economic and political relationships in my empirical analyses.

As with firms, there is interesting variation at the level of the investment. A foreign direct investment could be a greenfield investment or a merger acquisition (M&A). There is variation not just in the country of the investment but also in the subnational location of the investment, a topic I return to in greater detail later in the chapter. The anticipated duration of the investment varies based off of either the length of the contract or the amount of time it will take the firm to become profitable. For instance, mobile phone companies are typically

able to become profitable within a few years, whereas it will take an oil producer up to a decade. As with firms, the sector and the source of the financing (donor, investment bank, host country) vary. I explain how I conceptualize and measure FDI in the first empirical chapter.

2.2 Firms and political risk

Before delving into the theoretical motivations for the specific empirical analyses, it is useful to consider how firms think about political violence and price it in. Firms are the critical agents here because they are the decision-makers in the realm of FDI. I first consider the process by which political violence might affect FDI at the firm level. At a most basic micro-economic level, the firm's objective is to maximize profit and minimize cost. In the service of this objective, firms, who are considering foreign investment, pay attention to whether there is political violence, a type of risk that imposes costs, in a potential investment location. They take these observations into the FDI decision-making process. Once they have made a decision about where to invest, they must engage in the process of approvals and permitting in the host country. Assuming that goes well, the firm then begins its actual investment in a given country. This stage is known as the Final Investment Decision. Once the investment has begun, political violence does or does not occur in the investment location. The firm then has the option to continue putting money into the investment, to stop putting new money in, or to divest. I make the distinction between the latter two options because it is possible that losses are so high that a firm will not bother divesting to recover only a small percentage of their money. My theory focuses on FDI decision-making. Where do firms choose to invest, and how does political violence factor into that decision?

As firms optimize mathematically for profit, they are able to price political violence like any other cost. By political violence, I mean any form of organized violence carried out by political actors, including governments, rebel groups, insurgents, or terrorist organizations (Valentino, 2014). By conflict zone, I mean a place that is experiencing sustained political

violence.⁶ Political violence imposes different costs that can be priced in. The type and magnitude of the costs imposed by the violence vary with the type of violence, but can nonetheless be accounted for in profit maximization equations. Just as the type and magnitude of the costs vary with the type of violence, so does the likelihood of the cost. The cost is then multiplied by its likelihood before being added to the equation. Some types of political violence, such as international terrorism, are considered episodic,⁷ while others, such as civil war, are considered sustained. Another way to think about this is in terms of how predictable the costs are. In contrast to previous scholars who have argued that it is sustained conflict that deters investment,⁸ I argue that firms price in the costs imposed by political violence multiplied by their likelihood and proceed to optimize for profit.

Political violence is not the only form of political risk that firms must account for when investing abroad. Though not the direct topic of this project, these risks are not independent from political violence and thus merit a brief discussion. One major risk is credit risk, or the risk that a firm will not get paid. Construction companies in South Africa experienced difficulty with this ahead of the 2010 World Cup, and it has led to an unwillingness to do business with the African public sector unless there are Western donors or significant offshore financing. Another risk is expropriation,⁹ and with it are the associated costs of enforcing a contract, which may be expensive and difficult, especially without a robust legal or judicial system. This is often correlated with weak government institutions. Relatedly,

⁶One might ask if there is a meaningful distinction between political violence and “regular” violence, or perhaps organized crime. The distinction between is becoming increasingly blurry. For instance, [Barnes \(2017\)](#) argues that although criminal organizations do not seek to break away from the state, they have increasingly engaged in politics, developing collaborative and competitive relationships that have produced heightened levels of violence and have gained significant authority.

To answer whether or not there is a meaningful distinction for the purposes of this study, we must consider the pathways through which violence affects investors. Criminal violence, like political violence, affects investors by imposing costs, which I unpack below. The difference between the two, for the purposes of investors, lies in how predictable the violence is, which has implications for budgeting. Places tend to have relatively constant rates of criminal violence, but political violence can be episodic (terrorism) or continuous (civil war). Moreover, political violence, unlike criminal violence, has the potential to lead to regime change, which may or may not be favorable to investors. Can firms nonetheless plan around political violence?

⁷In the international business literature, this distinction is often termed “continuous” versus “discontinuous” risk.

⁸[Witte et al. \(2016\)](#); [D Miller \(1992\)](#); [Ramanujam & Goodman \(2003\)](#); [Li \(2006\)](#)

⁹For a thorough treatment of this topic, see [Wellhausen \(2014\)](#).

firms may face high costs of tendering for contracts or deal with onerous requirements for operating licenses. A limited choice of local partners and/or a weak local private sector can make business difficult for the firm, as can different business cultures or language barriers. A lack of market information can also prove costly. This is exacerbated by the informality characteristic of many developing markets. Finally, firms risk international competition. For instance, South African investors have been struggling vis-à-vis Chinese investors in the telecommunications markets in Africa as the Chinese are able to beat them in terms of price.

2.3 The relationship between conflict and investment: risk versus reward

To consider the risks posed to investors without considering the rewards is incomplete. The rewards of investing in a conflict zone are potentially great: given that these markets are generally untapped, opportunities for growth are the biggest (you can sell more cell phones if only ten percent of the population has them). This is particularly important if the industry is considered mature elsewhere (i.e. there are limited opportunities for growth). The competition from other firms is the lowest: sustained political violence drives down firm competition because it both disincentives entry and causes some firms to either flee or go out of business due to direct or indirect destruction of assets. This reduces competition for either remaining firms or new firms entering the market, allowing market control. Market control is particularly important when countries are unable to import certain goods due to the war, because prices can be justifiably high. These remaining or new firms calculate that they can maximize profit by controlling the market and taking advantage of other lower costs, even while paying to mitigate security risks. Firms may mitigate security risks by hiring private securities companies, building protective fortresses, creating evacuation plans, or directly paying rebel groups for protection. Conflict zones may offer site-specific resources that could provide great financial rewards. They also provide guaranteed business in the form of reconstruction.

Certain conditions can make firms more likely to profit in these markets: firms may benefit from government protection, particularly if they are a first mover in the industry or if their home government is an important ally. Firms may also be able to gain a first-mover advantage that allows them to influence government policy. For instance, if a government does not have an existing petroleum code and a firm is able to help them draft it, they can partly control the outcome. A guaranteed source of financing, such as the home country government or an aid donor, is also a distinct advantage. Many firms have also found that providing a public good, such as telecommunications, can act as form of protection. Consider the case of Anglo-Dutch Celtel, who profitably provided mobile phones during the conflict in Sierra Leone. As the CEO explained, “We try not to take risks. Fortunately our service is seen as universal and various parties to any conflict benefit from what we do. They see it as just like water or like air, so we have never been harmed or targeted.” (Cronin 2004) This is certainly not universally true, as telecommunications equipment has been targeted by terrorist groups, but it may provide an extra shield for firms.

Many of these countries also provide benefits such as abundant cheap labor and regulatory and tax incentives. This begs the interesting question of which country characteristics are specific to conflict zones as opposed to developing countries in general. In general, I argue that while some country characteristics are exogenous, conflict exacerbates many of the favorable and unfavorable conditions in developing countries. In terms of favorable characteristics, to return to the example I just used, it is easier for a firm to receive regulatory and tax incentives in countries where no one wants to invest. Regulations are also less likely to be enforced during a conflict.

In terms of unfavorable characteristics, in addition to direct risks from the conflict itself, such as investments being destroyed, personnel harmed or the conflict proceeding longer than anticipated or worsening, firms will have heightened risk of their supply chain being disrupted. They will also have the risk of threatened access to natural resources, such as water, as well as power generation. Conflict also increases reputational risks to firms, given

possible interaction with problematic regimes or rebel groups or human rights violations (real or perceived). Conflict increases the likelihood of regime turnover, which could, in theory, be positive or negative for an investor, but is generally considered undesirable from a predictability standpoint. Additionally, conflict interferes with traditional payment systems, limiting access to credit markets and forcing deals to occur in cash. Finally, conflict diminishes social capital and breaks down trust between individuals, groups and organizations.

These costs are real, and would deter investors for good reason: the Hyatt hotel built in Kabul in 2003 sits deserted, Shell was temporarily driven out of Nigeria, and one of LeFarge's cement plants now belongs to ISIS. The biggest evidence of this trade-off is that some firms directly experience the costs of conflict but continue to invest in conflict zones anyway. Consider the case of Millicom: it was forced to pull out of the Democratic Republic of the Congo because of the renewed civil war and was accused of bribery in Cambodia because a government minister sat on the Board of Directors of its subsidiary. Millicom continues to operate in 16 developing markets, including Sierra Leone. Clearly, looking only at the risks is incomplete.

Now that I have outlined a general framework for how firms think about political risk and the risk-reward trade-off inherent in investing in a conflict zone, I return to the proposition that different firms in different sectors from different countries have different incentives. I consider each of these levels of variation in turn and put forward the observable implications, which I test in later chapters.

3 Study 1: Market structure

In my first study, I hypothesize that non-competitive sectors have a unique relationship to conflict. Non-competitive markets occur when the agents acting in such a market have the power to influence the price, directly or indirectly, which does not occur under perfect competition. These are markets where there are many buyers yet few sellers, and as such

the firm is a price setter, rather than a price taker. Key examples include utilities, such as electricity and ICT, and logistics, such as trucking. For example, one does not often get to pick their electricity provider, one gets very little choice in telecommunications providers, and logistics and transport are generally based on location, not on consumer choice. Though less relevant on a country level, the fact that the global water industry is dominated by three large corporate groups confirms the tendency of utilities to become natural monopolies. The idea that certain sectors lend themselves to becoming monopolies is not new: in a recent report on service FDI, UNCTAD (2014) noted that “some services (especially basic utilities and infrastructure) may be natural monopolies and hence susceptible to abuses of market power.”

Non-competitive markets have a unique relationship to conflict. First, agents in non-competitive markets tend to provide goods for which there is local, inelastic demand (such as electricity), even during conflict. Inelastic demand leads to high prices, which favors a few large, consolidated players. Second, non-competitive markets are guaranteed, lucrative markets for the few operators who get a privileged spot in them. These operators would be reluctant to give up the position, even during conflict. Third, conflict disadvantages domestic firms and favors large, well-diversified, multinational corporations, whose headquarters and the bulk of their resources are safe in non-conflict zones.

Firms can profit in conflict zones when they provide goods for which there is local, inelastic demand (such as electricity), even during conflict. By local demand, I refer to the distinction in the literature between market and export-oriented FDI. Market-oriented investment stands in contrast to export-oriented investment and means that the good or service being produced is intended for the foreign market in which it is being produced. Physical market presence allows firms to directly access the market without import taxes or transportation costs. Inelastic demand simply means that the buyer’s demand does not change as the price changes. The demand will be high regardless of circumstances that might drive the price up (like conflict). Inelastic demand leads to high prices, which favors a few

large, consolidated players, i.e. a non-competitive or monopolistic market.

A related perk of non-competitive sectors is that because these are protected industries with high barriers to entry, they often come with particularly high levels of government protection. In this sense, not only are firms guaranteed a spot in a lucrative market, their concerns about risk can be partially alleviated by knowing that they will likely receive preferential governmental treatment. Not all firms exploit the opportunities these opportunities for profit: If they did, these would be saturated markets. Conflict disadvantages domestic firms and favors large, well-diversified, multinational corporations, whose headquarters and the bulk of their resources are safe in non-conflict zones. Political violence drives down firm competition because it both disincentivizes entry and causes some firms to either flee or go out of business due to direct or indirect destruction of assets. This reduces competition for either remaining firms or new firms entering the market, allowing prices to be justifiably high. In other words, conflict will incentivize FDI in non-competitive sectors (electricity, ICT, logistics, trucking).

We have observed this empirically: Chinese and Iranian firms are currently investing in the electricity sector in Syria ([Reuters, 2017](#)). Iran is also entering Syria's telecommunication industry. Similarly, Swedish giant Ericsson has had great success in telecommunications in Sudan, among other conflict zones. South African MTN and Vodacom have successfully invested in mobile phones across Africa. Anglo-Dutch Celtel was profitable in Sierra Leone during the conflict. Consumers tend to have inelastic and often universal demand for the products supplied in non-competitive industries. Further, firms are able to profit in this sector by selling pre-paid cards to prevent collection problems. Additionally, as discussed above, given lower fixed costs, telecommunications firms are also to move into profit within two to three years, which is much faster than the ten or so years typically required in, for example, extractive industries. This leaves open the question of why some firms would exploit this opportunity while others would not. Importantly, all source countries (and in turn, firms) may not have equal access to these coveted markets, de facto or de jure. There

are theoretical reasons to expect that governments would favor allies, donors, or countries with whom they have investment treaties. For instance, the telecommunications market is not easy to access: many governments hold spectrum auctions to determine which firms get licenses for things like 3G and broadband. This is another reason why it is critical to incorporate political science into explanations of this phenomenon.

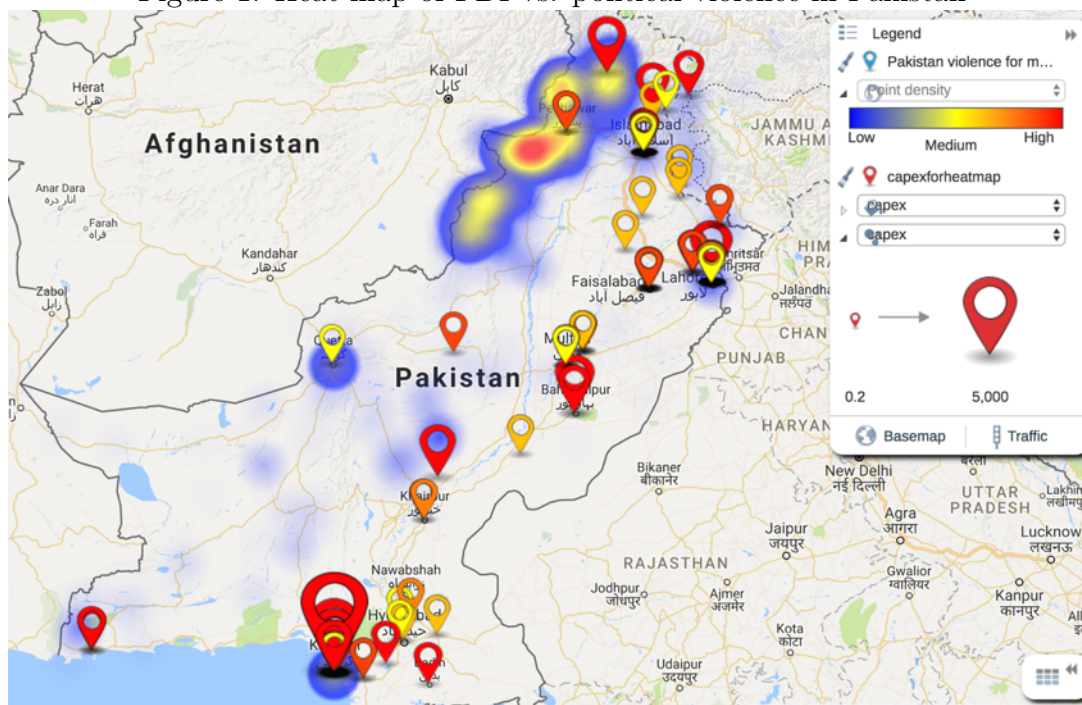
3.1 Extractive sectors and conflict

As discussed previously, for site-specific sectors, such as the extractive sectors, the primary determinant of investment location is geography. Firms often have some choice as to where they would like to extract resources such as oil or gold from, but they do not have the same catalog of choices that, say, manufacturing firms enjoy. Theoretically, all else being equal, extractive firms would still prefer to avoid political violence than not, but it is not a primary concern. This is not to say that political violence has not caused problems in the extractive sectors in the past. In fact, the contrary is true. Shell was temporarily forced out of Nigeria. Things also did not go as hoped with Talisman (Canada) in Afghanistan or Unocal (US) in Afghanistan. As such, I argue that political violence will not have a significant effect on FDI in the extractive sectors. I control for natural resource endowment in the empirical analyses.

4 Study 2: Subnational variation

To date, no study of FDI has accounted for cross-national and subnational variation simultaneously. Studies of specific countries, such as Japan and China, consider the subnational location of investments, but these do not give us leverage on macro trends. This is a critical undertaking, because both FDI and political violence vary sub-nationally and we cannot fully understand the effect of one on the other without taking this into account. I argue that firms will invest in countries with political violence but that they will avoid the

Figure 1: Heat map of FDI vs. political violence in Pakistan



areas sub-nationally with political violence, unless they are forced to that location because of a site-specific resource. For instance, investors in Pakistan might be far more interested in Karachi, a city with a booming economy and 20 million people, than the Taliban-controlled Federally Administered Tribal Areas (FATA). To get a sense of what this variation might look like, I created a heat map of political violence and capex in Pakistan using geocoded data (Figure 1). As is clear in the map, the violence is concentrated in FATA and investment is largely outside of the most violent areas, though the correlation is not perfect.

As such, while there is not a robust relationship between political violence and FDI at the country level, there are theoretical reasons to expect one at the subnational level. Recall that some of the risks and rewards outlined previously vary sub-nationally: risk of kidnapping may be localized, whereas regulatory and tax incentives are likely nationwide. This means that the risk-reward trade-off will be different in different areas within the country. In other words, by carefully selecting their within-country location, investors may be able to reap the benefits of investing in conflict zones without directly exposing themselves to some of the

risks. After testing this initial hypothesis, I test to see whether the market value is robust to subnational variation.

I also test this using a monadic panel data set of all developing countries from 2003 to 2017, but this time the data is subnational. Again, I use a spatial autoregressive model. It is important to use this model because there are theoretical reasons to expect that both my dependent variable, FDI, and my main independent variable, political violence, are spatially clustered. Much like temporal autocorrelation, spatial autocorrelation, or the spatial clustering of similar behaviors among neighboring observations, presents a threat to inference and unique challenges for statistical modeling. As [Galton \(1889\)](#) initially pointed out, trying to draw inference from comparisons across units while assuming that observations are independent can yield misleading conclusions, especially if the variation in the outcome of interest stems from diffusion among units ([Beck *et al.*, 2006](#)). Estimating an ordinary least squares (OLS) regression that ignores a diffusion process produces biased and inconsistent estimates. Estimating an OLS model that fails to account for spatial clustering will produce inefficient estimates, standard errors that are biased downwards, and Type 1 errors ([Darmofal, 2006](#)). SAR works by fitting linear models with autoregressive errors and spatial lags of the dependent and independent variables.

5 Study 3: Source-country variation

The previous studies demonstrate that the effect of political violence on FDI depends on the magnitude of the political violence, the subnational location of the violence and investment, and the sector of the investment. However, these studies still do not paint a complete picture. We must also consider the home country of the firm. As mentioned previously in the section on firm variation, the regional and global strategy of the firm's home country is important. The most obvious global example of this is China's investment in Africa. South Africa and Egypt have made similar attempts to assert their influence over

the region. Countries often invest in conflict zones if they fit into their regional portfolio.

Consider, for instance, the idea of regional influence as it applies to the banking sector: in 2001, ANZ became the first foreign bank in East Timor (Callick, 2001). Stanbic bank from South Africa serves 17 countries in Sub-Saharan Africa, including the DRC. Pakistani and Iranian banks set up shop in Afghanistan despite the conflict. Austrian banks were the first to begin operations in Bosnia. They argued that they had a greater comparative advantage because of their regional knowledge, as well as a greater commitment. The banking sector is an interesting example here because bankers are notoriously risk averse. Typically, when banks begin operations in conflict zones they expect their clients to be international (diplomats or NGOs). The general expectation is that domestic retail banking will come after the conflict ends. These markets are attractive to banks because they can charge higher premiums. However, banks will not begin operations unless a country has well-drafted banking laws and foreign exchange operations.

In addition to their respective strategies for exerting regional and global influence, source countries differ in the terms of the loans that they give investors. Put differently, there is variation in how much for the risk host countries are willing to absorb for investors. The explosion of Chinese FDI to Africa is likely highly related to the availability of soft loans and other capital for investors. For instance, China's first African Policy Paper, issued in January 2006, outlines the new Chinese policy toward Africa, which emphasizes "win-win" results in economic relations. During the Beijing Summit of the Forum on China-African Cooperation in November 2006, President Hu Jintao committed a US \$5 billion China-Africa Development Fund to support Chinese FDI in Africa, among a host of other commitments. In this way, the strategy variable and the risk absorption variable may be highly correlated.

China is a notable example of state-owned enterprise, but countries like the U.S. implicitly have similar policies, albeit to a lesser degree, as evidenced by policies such as "too big to fail." It is possible that when investors are able to borrow from Systemically Important Financial Institutions (SIFIs), which are financial institutions whose failure might trigger a

financial crisis, they are able to more easily receive loans for risky investments in countries prone to political violence. It may also be the case that the risk-acceptant external financing is more available in authoritarian regimes. I expect that if a firm's host government is absorbing the risk on its behalf, then it is less likely to be deterred by political violence. At minimum, we should see this empirical pattern hold in China's investments.

Source countries also vary in amount of political violence they experience themselves. Just as a Latin American investor who is no stranger to corruption might be less concerned about dealing with it elsewhere, investors from countries with political violence are less likely to be deterred by familiar circumstances. Finally, as discussed previously, a source country's existing economic (BITs, PTAs) and political (alliances, colonial) ties matter. I control for these in my empirical analyses.

In short, source countries vary in a few important ways: their global and regional strategies, their willingness to absorb risk on behalf of their investors, their level of political instability, and their existing economic and political ties. In this way, Study 3 acts as a scope condition analysis. If we look only at Chinese, American or EU FDI, does my theory hold? Is there a scope condition by country? I perform individual analyses for firms from the US, China, the EU, India, Brazil, and South Africa. In the corresponding chapter, I present my theoretical expectations for each case. For example, in both the Chinese and South African cases I expect high tolerance for conflict in Africa, because it is in line with their respective strategies, but not otherwise. I expect to see South African investment in Sudan and DRC, but not in Afghanistan or Myanmar. I test this for the entire sample, all dyads where the destination country is a developing country, using dyadic fixed effects models with spatially clustered errors. I then repeat this process for the US, China, the EU, India, Brazil, and South Africa.

6 Road map

The rest of the book proceeds as follows. I devote one chapter to each of the studies described in this chapter, expanding on the theoretical expectations and then testing each observable implication. In each study, I move beyond the statistical analyses with mini case studies. These help to demonstrate the causal processes at play. Finally, I conclude and present implications both for firms and policymakers.

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Paper 1:
Is violence bad for business? The consequences of political violence
for foreign direct investment

Abstract

What are the consequences of political violence for foreign direct investment (FDI)? The conventional wisdom suggests that political violence, a type of political risk, inhibits foreign investment in the developing world. I argue that this explanation is incomplete: the effect of political violence on investment varies with market structure. While current knowledge holds for investment in competitive markets—these firms are indeed deterred by violence—conflict has the opposite effect on investment in prospective monopolies (such as utilities, telecommunications, and logistics). Specifically, violence creates opportunities for firms to profit as these are markets with inelastic demand regardless of conflict. I test the theory using novel sector-level FDI data and a conservative estimation strategy. Building on work in business and economics, this project forces us to rethink the relationship between conflict and capital. This has important consequences for the determinants of FDI and post-conflict reconstruction literatures.

1 Introduction

Somalia, a country typically associated with civil war and decades sans an internationally-recognized government, has one of the most developed telecommunications networks in Africa. It boasts some of the lowest international calling rates in the entire world. During the first decade of its civil war, fixed phone lines increased by more than 550 percent (for comparison, they increased by 109 percent in the rest of the world during the same period).¹ This would not be the case but for a handful of international investors who selected Somalia, in the midst of its civil war, as the most desirable location for their mobile phone businesses. Why would these investors, who had the option of investing in any number of developing countries, pick one in the middle of a war?

¹World Bank 2019.

The conventional wisdom in political science suggests that political violence, a type of political risk, inhibits foreign investment in the developing world (Jensen & Young, 2008). There are strong theoretical reasons to expect that the conventional wisdom is right: violence imposes costs. Violence can lead to direct destruction of assets, indirect destruction of assets via disrupted supply chains, labor forces, or domestic markets, increased transportation costs, heightened security measures, and/or regime turnover that is hostile to investors.

Despite these logical theoretical expectations, the empirical record paints a more mixed picture: Iranian firms recently began investing in telecommunications and electricity in Syria and Dutch firms have been profitable selling mobile phones in Sudan. Investors rebuild destroyed assets in places like Rwanda, Haiti, Liberia, and Sierra Leone. In the last two decades, more than 500 multinational corporations (MNCs) have invested in countries experiencing political violence, accounting for total stock of over \$200 US billion (Driffield *et al.*, 2013; UNCTAD, 2017). It follows that empirical work on this topic is inconclusive. This null finding begs explanation—why is political violence not deterring investors as we would expect? Might conflict be deterring some firms but incentivizing others, generating a null finding at the aggregate level? Figure 1 indicates that the relationship between political violence and FDI is not as clear cut as the conventional wisdom would hold. Though there are many cases in which there is either no investment or no political violence, which I have excluded² from the graph, there is a lot of interesting variation in the middle.

I theorize that the short answer must be about profit maximization, as that is what drives any competitive firm's behavior. But what makes these firms think they will be able to maximize profit in a conflict zone, especially given that they have other options as to where to invest and that conflict is difficult to predict (Cederman & Weidmann, 2017)? I argue that conflict does not deter investment in the aggregate because there are certain market structures that allow firms to profit regardless of, and even because of, conflict. Specifically, firms in prospective monopolies, such as utilities (electricity, information and communica-

²I have included the same graph, but with the cases where there is no violence or no investment, in the appendix.

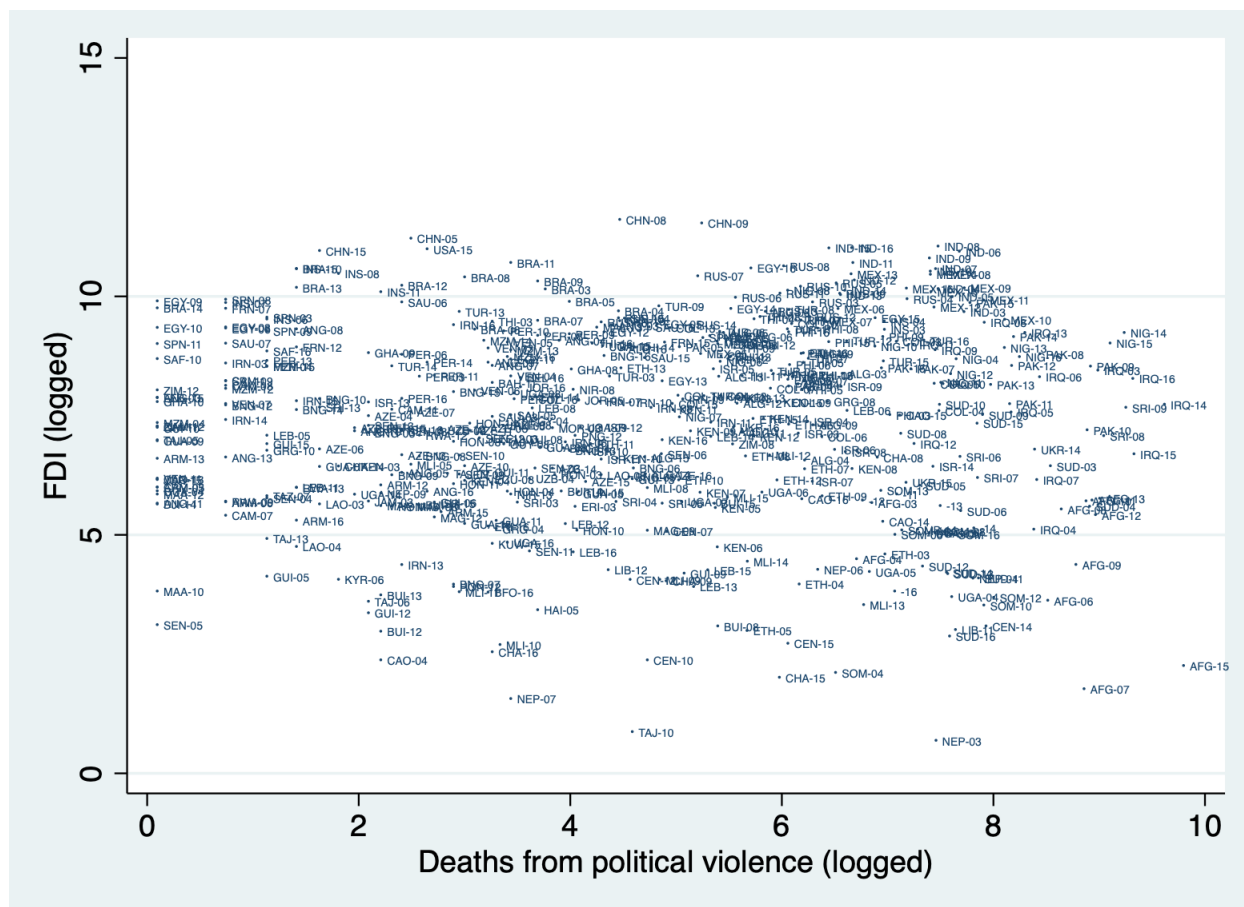


Figure 1: Cases in which there is both violence and investment

tions technology (ICT), water) and logistics (trucking, transportation) are able to profit regardless of conflict. In fact, in these markets, conflict could even increase opportunities for profit.

I shed new light on our understanding of conflict and capital by showing that firm decision-making about investing in conflict zones varies by market structure. Prospective monopolies have a unique relationship to conflict. First, agents in prospective monopolies tend to provide goods for which there is inelastic demand, even during conflict. Inelastic demand leads to high prices, which favors a few large, consolidated players. Second, prospective monopolies are guaranteed, lucrative markets for the few operators who get a privileged spot in them. These operators would be disinclined to give up that position, even during conflict. Third, conflict disadvantages domestic firms and favors large, well-diversified MNCs, whose headquarters and the bulk of their resources are safe in non-conflict zones. I test this argument empirically and find that political violence does, indeed, incentivize firms operating in prospective monopolies but deters those in competitive markets. Together, the very different dynamics in different types of markets help explain the null finding at the aggregate level.

Despite being addressed by multiple disciplines, scholars have yet to fully tackle the relationship between political violence and FDI. My theory suggests that existing accounts that posit that violence is exclusively bad for firms are incomplete. This article sheds light on the larger question in the literature of how firms make decisions about where to invest in the developing world. The existing international political economy literature provides explanations about FDI location choice that focus on regime type, respect for property rights, and regulatory standards (see [Pandya \(2016\)](#); [Jensen \(2003\)](#); [Li & Resnick \(2003\)](#), among many others). These explanations do well with the primarily North-North FDI that we observed up until 2000. However, they overlook much of the interesting variation in where firms choose to invest, particularly in the developing world.

Understanding the effects of political violence on FDI is relevant beyond academic interest. FDI is critical to the global economy, exceeding the value of all other forms of

cross-border capital flows combined. FDI is also important for post-conflict re-construction. In this sense, this research is relevant for policymakers and economic development professionals. The results are directly pertinent to firms, specifically risk managers and investors, who need to understand the relationship between political violence and FDI to efficiently allocate capital, make informed decisions and maintain investor confidence. In addition to speaking to the determinants of FDI literature, this research is also in conversation with the literature on the economic consequences of civil war, which, to this point, has largely evaded studying firms as the primary actors. Overall, this project builds on two sets of central questions in the discipline: questions about international finance and information and questions about the impact of conflict on capital flows.

I proceed as follows: After outlining existing work, I present a theory of why conflict does not deter investment as we would expect, arguing that there are certain market structures that will allow firms to profit regardless of conflict. I then present my research design, test my theory empirically, illustrate it with real-world examples, and conclude.

2 Existing evidence on FDI and conflict is mixed

There is a robust literature in political science about the determinants of FDI. Scholars have proposed a series of independent variables, chief among them regime type,³ particularly as it pertains to respect for property rights, international agreements (such as BITs and PTAs),⁴ and domestic regulation.⁵ These explanations do well with the primarily North-North FDI that we observed up until 2000. However, they do not completely capture the interesting variation in which developing countries firms choose to place their non-site specific investments in.

The literature on FDI in political science has also largely overlooked political violence as key determinant of FDI, with the important exceptions of [Li \(2006\)](#) and [Jensen & Young](#)

³See, among others, [Li & Resnick \(2003\)](#); [Jensen \(2003\)](#); [Pandya \(2016\)](#)

⁴See, among other, [Büthe & Milner \(2014, 2008\)](#); [Elkins *et al.* \(2006\)](#)

⁵[Pandya \(2013\)](#)

(2008). Li (2006) disaggregates by type of violence and argues that unanticipated civil and international conflict has a negative impact on FDI but that anticipated civil and international conflict does not. Jensen & Young (2008) model how the risk of political violence plays into investor perceptions (measured as the price investors paid for political risk insurance). Controlling for recent past experiences with violence, they find that the wealthier and more democratic a country is, the less likely investors are to perceive them as risky. Within the existing literature on the topic, the default assumption has been that political violence is bad for business.⁶

Other disciplines, notably economics and business, have also addressed the topic, though with decidedly inconclusive results. The econometric evidence is mixed, though generally finds that there is no statistical relationship between political violence and FDI at the country-year level.⁷ The qualitative evidence (primarily survey evidence) firmly supports the idea that firms take political violence into account when making investment decisions (Bass *et al.*, 1977; Porcano, 1993; Li, 2006). FDI scholars have noted the inconsistency between the qualitative and econometric evidence on this topic (Pearce *et al.*, 1992; Li, 2006). All of the studies make the theoretical assumption that conflict is likely to deter investment, but their empirical findings are largely mixed. Why is the evidence inconclusive when scholars seem to have this intuitive idea that violence is bad for business?

3 Theory

The conventional wisdom suggests that political violence inhibits foreign investment in the developing world. However, existing evidence suggests that this is not empirically accurate. This requires an explanation, particularly as there are strong theoretical reasons to expect that the conventional wisdom is right: violence imposes costs. Violence can lead to

⁶Li (2006); Jensen & Young (2008)

⁷Kobrin (1979); Schneider & Frey (1985); Nigh (1985); Fatehi-Sedeh & Safizadeh (1989); Woodward & Rolfe (1993); Loree & Guisinger (1995); Olibe & Crumbley (1997); ?; Sethi *et al.* (2003); Globerman & Shapiro (2003); Li (2006)

direct destruction of assets, indirect destruction of assets via disrupted supply chains, labor forces, or domestic markets, increased transportation costs, heightened security measures, and/or regime turnover that is hostile to investors. Given this, we would not expect to observe non-extractive FDI to Colombia, Northern Ireland, or Sri Lanka during their respective civil wars, but we do. We would not expect to observe new electricity infrastructure appearing in Afghanistan or South Sudan, but again, we do. To be sure, the amount of inbound FDI during conflicts does not rival the post-conflict investment boom or the amount of FDI going to non-conflict zones. But why are firms with other location options choosing to invest in war zones? Might conflict be deterring some firms but incentivizing others, generating a null finding at the aggregate level?

I theorize about this from the perspective of firms, who are the decision-makers in the realm of FDI. Basic microeconomics can tell us that the reason firms are choosing to invest in conflict zones is profit maximization, which drives any competitive firm's behavior. But what leads firms to believe that they will be able to maximize profit in a conflict zone, especially in the face of other investment options? I argue that conflict does not deter aggregate investment because certain market structures allow firms to profit regardless of conflict. Specifically, firms in prospective monopolies, such as utilities (electricity, telecommunications, water) and logistics (trucking, transportation) can profit even in the midst of violence conflict. In fact, for some firms, conflict may provide unique opportunities for long-term financial growth.

It is worth briefly noting that the degree to which firms can "choose" their investment location depends on the type of industry. For instance, we know that extractive FDI, due to the location specificity of the resources, is going to be where the resources are. Firms often have some choice as to where they would like to extract resources such as oil or gold from, but they do not have the same catalog of choices that, say, manufacturing firms enjoy. For instance, ArcelorMittal has little choice in locations with iron ore, so they invest in Liberia. These companies recognize the risk but invest anyway because there are not

alternatives. Often, such as in the case of Sierra Leone, these firms hire private “militaries” for protection. That said, political violence has forced extractive FDI out of countries before (consider the example of Shell in Nigeria⁸) and, theoretically, all other things being equal, extractive companies would prefer to invest in locations with less political violence. I account for this FDI in my analyses, but this is not the interesting variation that I seek to explain. Interestingly, the coefficient on political violence is close to zero and insignificant when the dependent variable is only FDI in natural resources, confirming my prior that violence is not a major predictor for this sector.

3.1 Political violence, criminal violence & conflict zones

Before proceeding, it is important to clarify key concepts. By *political violence*, I mean any form of organized violence carried out by political actors, including governments, rebel groups, insurgents, or terrorist organizations (Valentino, 2014). By *conflict zone*, I mean a place that is experiencing sustained political violence. One might ask if there is a meaningful distinction between *political* violence and “regular” violence, or perhaps organized crime. The distinction between them is becoming increasingly blurry. For instance, Barnes (2017) argues that although criminal organizations do not seek to break away from the state, they have increasingly engaged in politics, developing collaborative and competitive relationships that have produced heightened levels of violence and have gained significant authority.

To answer whether or not there is a meaningful distinction for the purposes of this study, we must consider the pathways through which violence affects investors. Criminal violence, like political violence, affects investors by imposing costs, which I unpack below. The difference between the two, for the purposes on investors, lies in how predictable the violence is, which has implications for budgeting. Places tend to have relatively constant rates of criminal violence, but political violence can be episodic (terrorism) or sustained for a number of years (civil war). Moreover, political violence, unlike criminal violence, has the

⁸Howden (2006); Reuters (2016)

potential to lead to regime change, which may or may not be favorable to investors. Can firms nonetheless plan around political violence?

3.2 FDI decision-making

I first consider the process by which political violence might affect FDI at the firm level. At a basic microeconomic level, the firm's objective is to maximize profit and minimize cost. In the service of this objective, firms consider whether there is political violence, a type of risk that imposes costs, in potential investment locations. Once they have made a decision about where to invest, they engage in the process of approvals and permitting in the host country. Assuming that goes well, the firm then begins its actual investment in a given country. Political violence then does or does not occur in the investment location. The firm then has the option to continue putting money into the investment, to stop putting new money in, or to divest. I make the distinction between the latter two options because it is possible that losses are so high that a firm will not bother divesting to recover only a small percentage of their money. My theory focuses on initial FDI decision-making. Where do firms choose to invest, and how does political violence factor into that decision?

As in any other situation, firms mathematically optimize for profit, and the costs of political violence can be priced in. The frequency and severity of the costs imposed by the violence vary with the type of violence, but can nonetheless be accounted for in profit maximization equations. Some types of political violence, such as international terrorism, are considered episodic,⁹ while others, such as civil war, are considered sustained. Another way to think about this is in terms of how predictable the costs are. In contrast to previous scholars who have argued that it is sustained conflict that deters investment,¹⁰ I argue that firms price in the costs imposed by political violence multiplied by their likelihood and proceed to optimize for profit.

⁹In the international business literature, this distinction is often termed "continuous" versus "discontinuous" risk.

¹⁰Witte *et al.* (2016); D Miller (1992); Human Security Report (2014); Ramanujam & Goodman (2003); Li (2006)

Conflict zones present both risks and rewards to investors. The rewards are potentially great: given that these markets are generally untapped, opportunities for growth are the biggest (you can sell more cell phones if only ten percent of the population has them), the competition from other firms is the lowest, and many of these markets offer other advantages, such as low labor and land costs, tax incentives, and loose regulation.

Sustained political violence drives down firm competition because it both disincentivizes entry and causes some firms to either flee or go out of business due to direct or indirect destruction of assets. This reduces competition for either remaining firms or new firms entering the market, allowing market control. Market control is particularly important when countries are unable to import certain goods due to the war, because prices can be justifiably high. These remaining or new firms calculate that they can maximize profit by controlling the market, even while paying to mitigate security risks. Firms may mitigate security risks by hiring private security companies, building protective fortresses, creating evacuation plans, or directly paying rebel groups for protection.

However, the risks are high: investments may be destroyed, supply chains interrupted, personnel harmed, laws changed or assets seized as a result of regime turnover hostile to investors, etc. The conflict could proceed longer than anticipated or worsen. These costs are real, and would deter investors for good reason: The Marriott Hotel Kabul, built in Afghanistan in 2003, sits deserted,¹¹ Shell has had more than a few issues with militants in Nigeria,¹² and one of LeFarge's cement plants was seized by ISIS.¹³ Not all firms are equally able to navigate the trade-offs between risk and reward in conflict zones. I argue that a key determinant of profitability in conflict zones is the market structure of the firm's industry.

Specifically, I argue that a non-competitive market structure is what allows firms to be confident that they will profit in spite of, or even because of, conflict. However, this leaves open the question of whether political violence will deter investment in competitive markets.

¹¹Hersher (2016)

¹²Howden (2006); Reuters (2016)

¹³Alderman *et al.* (2018)

I argue that if violence imposes costs and certain market structures protect firms, then firms that are not protected by the market structure should expect violence to be costly enough to threaten profit. In other words, my theory suggests that the conventional wisdom, that political violence is bad for business, should hold for competitive, price-taking firms.

3.3 Prospective monopolies and conflict

Prospective monopolies occur when the agents acting in such a market have the power to influence the price, directly or indirectly, which does not occur under perfect competition. How do we know if firms are investing in a competitive market or not? Economists have provided us with a clear definition of prospective monopolies (also referred to as non-competitive markets). Key examples include utilities, such as electricity, telecommunications, and water management, and logistics, such as trucking and transportation ([Mankiw, 2015](#); [Slavin, 2009](#); [Sharkey, 1982](#)). The idea that certain sectors lend themselves to becoming monopolies is not new: in a recent report on FDI, UNCTAD noted that “some services (especially basic utilities and infrastructure) may be natural monopolies and hence susceptible to abuses of market power.”

We have observed this empirically: Chinese and Iranian firms are currently investing in the electricity sector in Syria ([Reuters, 2017b](#)). Iran is also entering Syria’s telecommunication industry. Similarly, Swedish giant Ericsson has had great success in telecommunications in Sudan, among other conflict zones. South African MTN and Vodacom have successfully invested in mobile phones across Africa. Anglo-Dutch Celtel was profitable in Sierra Leone during the conflict. Celtel’s CEO explained: “We try not to take risks. Fortunately our service is seen as universal and various parties to any conflict benefit from what we do. They see it as just like water or like air, so we have never been harmed or targeted” ([Cronin, 2004](#)). This highlights the inelastic and often universal demand consumers have for the products supplied in non-competitive industries. Further, firms are able to profit in this sector by selling pre-paid SIM or electricity cards to prevent collection problems. Given lower fixed

costs, telecommunications firms are also able to move into profit within two to three years, which is much faster than the ten or so years typically required in, for example, extractive industries.

Prospective monopolies have a unique relationship to conflict. First, agents in prospective monopolies tend to provide goods for which there is local, inelastic demand (such as electricity), even during conflict. Inelastic demand leads to high prices, which favors a few large, consolidated players. Second, prospective monopolies are guaranteed, lucrative markets for the few operators who get a privileged spot in them. These are often protected industries with high barriers to entry, and as such frequently come with particularly high levels of government protection. Firms would be reluctant to give up the position, even during conflict. Third, conflict disadvantages domestic firms and favors large, well-diversified, multinational corporations, whose headquarters and the bulk of their resources are safe in non-conflict zones. If all firms had equal ability to exploit these opportunities, these would be saturated markets.

In sum, because violence imposes costs, firms will only invest if they are confident that that they will be able to maximize profit despite these costs. The non-competitive market structure allows firms to be confident that they will profit in spite of, or even because of, conflict. Firms that will not be protected by the non-competitive market structure should expect violence to be costly enough to threaten profit. As such, the conventional wisdom, that political violence is bad for business, should hold for competitive, price-taking firms.

4 Research design

As I seek to understand how violence affects foreign investment to a country, I test whether political violence predicts FDI in a given country in a given year. However, because I theorize that the relationship will vary based on the market structure of the FDI, I divide FDI into two dependent variables—FDI in prospective monopolies and FDI in competitive

markets. I aggregate project-level data to the country-year level for the purposes of my empirical analysis, as this is a good measure of aggregate firm behavior.

My sample is all developing countries¹⁴ between 2003 and 2017.¹⁵ I focus on developing countries for three reasons. First, we know far more about North-North FDI than we do about North-South or South-South FDI. Second, existing research shows that foreign investment into OECD and non-OECD countries are fundamentally different phenomena and should be studied separately. Third, my theory is not applicable to most developed countries, as they are not the countries experiencing political violence.

My dependent variable is FDI. Following [Kerner \(2014\)](#), I use capital expenditures (capex) to measure FDI, as opposed to flows or stock: because the theory concerns a firm's fixed capital (expenditure on land, physical structures, equipment, etc.), not the net value of border-crossing capital movements between MNCs and their foreign affiliates (flows) or the value of foreign direct investors' stake in the foreign affiliates operating in a host country at any given time (stock). This is in contrast to existing studies of political violence and FDI, which primarily measure FDI using flows. My capex data is from FDImarkets. I log the values as the data is highly skewed. I disaggregate FDI by sector and then re-aggregate to create two categories for each country: FDI to prospective monopolies and FDI to competitive markets. Specifically, following the theoretical discussion, I classify the utilities (ICT, electricity, water), transportation and logistics sectors as non-competitive, and all other sectors as competitive.¹⁶ I treat the competitiveness of a market as time and country invariant, which is reasonable given the short time sample.

The primary independent variable is political violence. I use PRIO's GED event data ([Gleditsch, 2002](#)) and aggregate it using their best estimate of deaths. Following the theoretical discussion about the time it takes to make an investment decision, I aggregate deaths from political violence in a country for the three years prior to the investment. I log the

¹⁴I operationalize this as countries that are not classified as "high income" by the World Bank.

¹⁵These are the only years available from FDImarkets.

¹⁶I test a slightly more conservative specification of the dependent variable in the appendix and find similar results.

data as it is also highly skewed.

My theory predicts that `political violence` will have a *positive* and significant impact on FDI to non-competitive industries and a *negative* and significant impact on competitive industries. I use panel ordinary least squares (OLS) models with a lagged endogenous variable as well as country and year fixed effects to test this. The country fixed effects hold everything else about the country constant and the year fixed effects control for global macroeconomic trends. In both models I control for the other type of FDI in addition to using a lagged endogenous variable. As mentioned above, my sample is all developing countries¹⁷ between 2003 and 2017.¹⁸ In order to avoid selecting on the dependent variable, I include all country-years, even those in which there is no investment. I have 1560 observations in each model.

5 Results

I present the findings of my models in Table 1 and Figure 2. These models demonstrate that, as predicted, `political violence` has a positive and significant impact on FDI to prospective monopolies and a negative and statistically significant impact on FDI to competitive industries. The fact that the sign of `political violence` switches from positive to negative in an otherwise identical model should help to alleviate any concerns about model specification.

These results help us better understand the null finding at the aggregate level: if the effect of conflict on FDI varies significantly with market structure, then the aggregate finding will be null. These results suggest that the conventional wisdom in political science—that violence is bad for business—is correct insofar as it applies to competitive, price-taking firms. However, it confirms that our existing explanations are incomplete: there is a subset of firms that can actually profit from political violence.

¹⁷I operationalize this as countries that are not classified as “high income” by the World Bank.

¹⁸These are the only years available from FDImarkets.

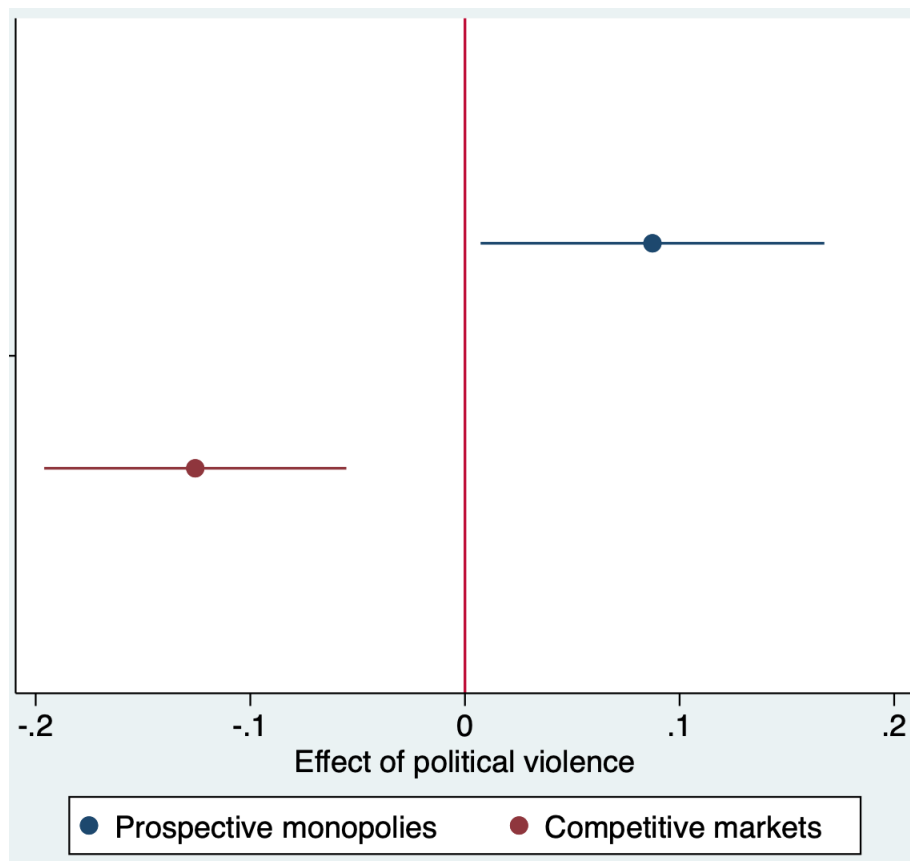


Figure 2: Market structure explains null aggregate findings

Table 1: Effect of political violence on FDI varies with market structure

	(1)	(2)
	Prospective monopolies	Competitive markets
Political violence	0.087** (0.041)	-0.126*** (0.036)
Constant	-6.106*** (0.792)	-2.110** (0.696)
Lagged endogenous variable	0.007 (0.027)	0.022 (0.027)
Country fixed effects	✓	✓
Year fixed effects	✓	✓
Control for other type FDI	✓	✓
Developing countries only	✓	✓
<i>N</i>	1560	1560

Standard errors in parentheses

** $p < 0.05$, *** $p < 0.01$

I assuage a few potential empirical concerns. First, I confirmed that neither the dependent variable (FDI) nor the main independent variable of interest (political violence) have a unit root. In other words, they are trend-stationary. As the statistical tests for unit roots are known for having low statistical power, I employed a battery of them, including Dickey Fuller, Levin-Lin-Chu, and Harris-Tzavalis. All tests present overwhelming evidence against the null hypothesis of a unit root and as such I conclude that both FDI and political violence are stationary.

Next, I address endogeneity concerns. In addition to using a lagged endogenous variable, I alleviate endogeneity concerns by confirming that FDI is not a statistically significant predictor of political violence. In the appendix, I demonstrate that total FDI, non-competitive FDI and competitive FDI are not statistically significant predictors of political violence.¹⁹

6 Telecommunications in Somalia

Consider the case of mobile phones in Somalia. Somalia, designated as either a failed state or a fragile state for the past few decades, hardly seems like a likely candidate for foreign investment. However, MNCs have invested profitably in Somalia since the collapse of the central government in the early 1990s. A variety of interim federal administrations were established in the 2000s, but Somalia did not have an internationally-recognized government until 2012. Somalia is cited as an example of a real-world anarchist stateless society (Gettleman, 2007).

It may seem puzzling, then, to talk about Somalia in a paper on foreign investment. Recall, however, the starting point of this paper: investors are interested in profit, and profit is not necessarily correlated with peace. In fact, as I have just demonstrated empirically, often it is the absence of peace that creates opportunities for investors. This is not universally true, but it is in the case of prospective monopolies, such as telecommunications. As discussed earlier, the telecommunications industry is a natural monopoly because duplication

¹⁹Please refer to Table 2 in the appendix.

of infrastructure is economically inefficient. As with other public utilities, there are huge economies of scale: the average total cost decreases with each additional line ([Whiteman, 1990](#)). Typically, governments manage this by allowing firms to bid for a certain number of licenses, as more competition is not thought to be advantageous.

In 1991, Somalia had 15,000 fixed phone lines. By 2003, it was up to 100,000. That is a 567 percent increase, during anarchy and civil war. For comparisons sake, the rest of the world saw a 109 percent increase during the same time. Similarly, in 2000, 1.1 Somali out of every 100 owned a mobile phone. Now, less than two decades later, nearly 50 out of 100 people do. Additionally, Somalia has the lowest international call rates in Africa and one of the cheapest in the world ([The World Bank, 2019](#)).

How did this happen? When the dictatorial regime of Siad Barre collapsed in 1991, so did the state's complete monopoly on telecommunications, prompting private actors to enter ([Winter, 2004](#)). A manager of Telcom Somalia, the first private telecommunications firm to enter Somalia's market, spelled out the firm's logic: "The government post and telecoms company used to have a monopoly but after the regime was toppled, we were free to set up our own business. We saw a huge gap in the market, as all previous services had been destroyed. There was a massive demand." He explained that though the major airport and port were destroyed in the violence, firms were able to set up a small air strip and use natural harbors to import their equipment.²⁰

Demand for telecommunications is particularly high and inelastic in conflict zones. As [Deibert \(2013\)](#) points out, militants from warring clans can all agree on the need for telecoms. He outlines that militants need a functioning cellphone system to operate their ventures, issue threats via text messaging, make mobile payments for drugs and weapons, and organize their tribal factions. He also notes that mobile phones are less easy to intercept than land lines and provide a degree of anonymity because SIM cards and phones can be recycled and exchanged.

²⁰[Winter \(2004\)](#)

Interestingly, Hormuud, one of the major telecommunications providers, actually profits when there is more violence in al-Shabaab areas of Somalia because people are too afraid to leave their homes. Instead, they stay in and use their phones to communicate with friends and relatives. Violence also encourages the use of mobile phones because mobile phones facilitate use of mobile banking, which is much safer than carrying around hard currency due to the likelihood of theft. Finally, because of the violence, many Somalis have fled to other parts of the world and send remittances back home via an informal banking network called hawala. Telecommunication is critical for the functioning of hawala.²¹

In addition to inelastic demand, there were certainly other benefits of the chaotic situation: there were no taxes whatsoever, no need to bid for the telecommunications licenses that are typically provided by the government, no corrupt officials to bribe, and no laws or regulations governing firm behavior.²² In the case of Somalia, mobile phone companies actually agreed to a certain degree of self-regulation via the Somali Telecom Association (Bray, 2005). Foreign investors seem to favor loose regulation over no regulation at all. As the secretary general of the Somali Telecom Association stated, “we need a government to regulate and provide necessary legislation as well as ensure security and peace which would attract much needed foreign investment to the sector” (Bhalla, 2004).

There is a fair amount of variation in what loose regulation in a conflict zone may look like, ranging from a government that is otherwise too preoccupied to enforce regulation, to governments dramatically reducing regulation to attract foreign investors, to the complete absence of a government to impose regulations to begin with.

Investing in violence, however, is not without challenges: the security situation is difficult to navigate, staff must go abroad for any training, and it is difficult to transfer money to foreign partners without a central bank.²³ Somalia is considered one of the most ethnically

²¹Deibert (2013)

²²Lack of regulation is a large problem from an economic development standpoint for a variety of reasons, one of which being that almost every dollar made by foreign telecoms companies is a dollar that leaves Somalia (Hassan, 2003).

²³Winter (2004)

homogeneous countries, with 85 percent of the inhabitants being ethnic Somalis. However, there are many warring clans. In order to navigate this and protect business, Hormuud, one of the largest operators, intentionally sells shares to each clan, such that everyone has a stake in the business (Mohamed & Childress, 2010). This gives the company a remarkable amount of soft power in protecting its interests (Clingendael, 2019). The dispersed ownership structure also builds trust with different consumer bases. A representative from the company was explicit about their objectives, saying, “we cannot sit and watch when the state is unable to react: we have different shareholder structure. We have shareholders in every clan, which is how we gain access to every client.”²⁴ This has not been an entirely foolproof plan, as Hormuud has lost both employees and fixed lines to mortar explosions, but it does serve to mitigate risk. Telecom companies in Somalia spend five percent or more on safety.

One might reasonably wonder who these investors are that felt comfortable setting up shop in Somalia during the civil war. Some, but not all, are diaspora investors, or Somali expatriates who decide to return. This phenomenon is not exclusive to Somalia: Mo Ibrahim, the founder of Celtel, which set up mobile phone networks in 13 African countries, was born in Sudan. To give an example from another industry, Afghan-Americans were largely responsible for funding the Afghanistan International Bank (Bray, 2005).

In the case of Somalia, a variety of Somali expatriates began to invest in the 1990s. Each had international partners, including Norway-based Telenor as well as US-based AT&T and Starlight Communications. There are now four main telecommunication companies in Somalia, three of which signed an interconnectivity deal in 2005 to set prices and control competition (Mohamed & Childress, 2010).

Somalia between 1991-2012 is clearly the most extreme case given the rare existence of anarchy, but the large-N statistical analysis should ease any concerns that anarchy is required for the theory to hold. For example, we have observed a comparable situation in the Democratic Republic of the Congo. Similarly, South African MTN, a mobile-telecoms

²⁴Clingendael (2019)

firm, is currently one of two mobile carriers in Syria and is operating profitably ([Economist, 2014](#)).

7 Other examples

There also plenty of examples outside of the telecommunications industry. Water, as a public utility, is another example of a natural monopoly. As the OECD explains, this is because providing water entails high fixed costs, long-term irreversible investments, and inelastic demand ([OECD, 2009](#)). The subject of water privatization insofar as it pertains to economic development and ethics is far beyond the scope of this paper, but the sector still provides an illustrative example of firms being willing to invest in conflict zones.

Many countries that have experienced conflict also have at least partially privatized water management. These include, but are not limited to, Ivory Coast, Algeria, Colombia, Mexico, the Philippines, Nigeria, Egypt, Djibouti, and Pakistan. For example, Hyflux, a Singaporean water treatment firm, has ventures in Nigeria and Egypt. They also won a \$100 million contract in 2009 Libya to desalinate seawater. Italian WTD desalinates water in Iran, and French Stereau has done something similar in Djibouti. US-based firm Global EnviroScience Technologies signed an agreement worth \$2.8 billion for a power plant and desalination system in Pakistan in 2008.

Of course, the risk versus reward tradeoff does not always work out exactly as the firm expects. Hyflux's contract with Libya has been on hold since 2011 due to increasing political violence in the country.²⁵ French Saur pulled out of Mozambique and Zimbabwe in favor of less risky markets. Empirically, however, the potential rewards in water management are high given the prospective monopoly aspect.

Electricity is another example of a natural monopoly. As with water, there are many examples of countries that both experience conflict and have privately-provided electricity (typically referred to as an Independent Power Producer or IPP in this industry). These

²⁵[Reuters \(2011\)](#)

include Algeria, Ivory Coast, Egypt, Kenya, Zambia, Pakistan and Syria. For example, British Azuri Technologies provides pay-as-you-go electricity across Sub-Saharan Africa²⁶ and M-Kopa, a Kenyan solar energy firm, does the same across East Africa.²⁷ Australian Pierlite operates in Pakistan. Finnish Wärtsilä opened a new power plant in Syria in 2010.²⁸ Even in the most dire conflicts, we still see foreign investment in electricity.

8 Conclusion

This paper addressed the consequences of political violence for FDI. It challenged the conventional wisdom in political science that political violence inhibits foreign investment in the developing world. I argued that conflict does not deter aggregate investment because there are certain market structures that will allow firms to profit not only in spite of, but even because of, conflict. Specifically, violence creates opportunities in prospective monopolies (such as utilities and logistics) as these are markets with inelastic demand regardless of conflict. I tested the theory using novel sector-level FDI data, which I aggregated to competitive industries and prospective monopolies. I found that the conventional wisdom is correct as it applies to firms in competitive markets—these firms do not like violence—but that firms in prospective monopolies behave systematically differently in the face of conflict.

This paper resolves mixed empirical evidence by introducing the idea that we must move beyond thinking about FDI as homogenous. By introducing sectoral heterogeneity to my model, I show that the relationship between conflict and investment is not nearly as simple as previous work suggests. This point holds more broadly: the current IPE literature tends to disregard the interesting variation within FDI. We need to move beyond thinking about FDI as simply the sum of capital expenditure/stocks/flows and instead test our theories with attention to sectoral, subnational, source-country, and investing firm heterogeneity. There are strong theoretical reasons to expect that firms from different countries behave differently,

²⁶Reuters (2017a)

²⁷Faris (2015)

²⁸Corporation (2010)

that bigger and more diversified firms behave differently than smaller ones, that certain geographic areas of countries might be more stable and thus more desirable for investment than others, etc.

In that vein, this paper leaves open important avenues for future research. First, this analysis highlights that there are dynamics between domestic and foreign firms that remain under-explored in the literature. Second, existing literature indicates that bilateral ties, such as trade and bilateral investment treaties, are important predictors of FDI. How do those explanations weigh against the structural explanation posed here? Third, how do other IPE theories that use FDI as a dependent variable hold up if we introduce heterogeneity in the outcome? For instance, do trade agreements affect all types of FDI the same way?²⁹

This analysis helps to complete our understanding of the relationship between political violence and FDI. It also moves forward the determinants of FDI literature by re-introducing a critical independent variable. In the broader context, this article sheds light on two sets of central questions in the discipline: questions about international finance and information and questions about the impact of conflict on capital flows.

²⁹[Büthe & Milner \(2014, 2008\)](#)

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9 Appendix

9.1 Figure 1

Figure 3 replicates Figure 1 but includes cases where either investment or political violence is zero.

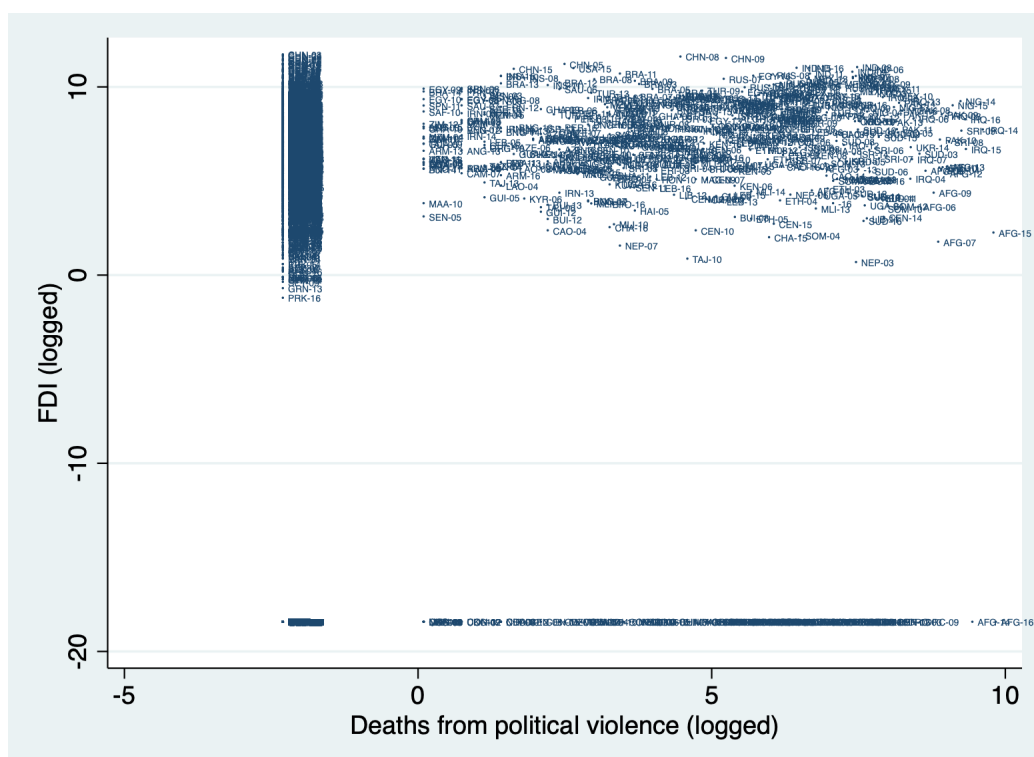


Figure 3: Political violence (lagged) vs. FDI (lagged)

9.2 Endogeneity concerns

In addition to using a lagged endogenous variable, I alleviate endogeneity concerns by confirming that FDI is not a statistically significant predictor of political violence. Here, I demonstrate that total FDI, FDI to prospective monopolies and FDI to competitive markets are not statistically significant predictors of political violence. I operationalize all variables the same way as in the main model (Table 1) and use the same model specifications.

Table 2: FDI does not predict conflict

	(1)	(2)	(3)
	Political violence	Political violence	Political violence
Total FDI	0.000 (0.021)		
Prospective monopolies		0.002 (0.015)	
Competitive markets			0.014 (0.017)
Constant	-4.569*** (0.435)	-4.556*** (0.444)	-4.529*** (0.437)
Year fixed effects	✓	✓	✓
Country fixed effects	✓	✓	✓
Lagged endogenous variable	✓	✓	✓
<i>N</i>	1560	1560	1560

Standard errors in parentheses

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

9.3 Alternative specification of the dependent variable

For robustness, I also estimate the models using a slightly different specification of the dependent variable. Based on microeconomic theory it is most correct to use the specification of prospective monopolies and competitive markets used in the main paper, but if we use a more conservative definition that only counts logistics, telecommunications and electricity as prospective monopolies, the results are similar. In the case of prospective monopolies, political violence is positive and $p = 0.15$. In the case of competitive markets, political violence is negative and significant. I present the results in Table 3.

Table 3: Effect of political violence on FDI varies with market structure

	(1)	(2)
	Prospective monopolies	Competitive markets
Political violence	0.084 (0.059)	-0.171** (0.057)
Constant	-14.412***	-10.227***
Lagged endogenous variable	0.013 (0.026)	-0.014 (0.027)
Country fixed effects	✓	✓
Year fixed effects	✓	✓
Control for other type FDI	✓	✓
Developing countries only	✓	✓
<i>N</i>	1560	1560

Standard errors in parentheses

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Paper 2:
Is violence bad for business? The consequences of political violence
for foreign direct investment

Abstract

What are the consequences of political violence for foreign direct investment (FDI)? The conventional wisdom suggests that political violence, a type of political risk, inhibits foreign investment in the developing world. I argue that this explanation is incomplete: the effect of political violence on investment varies with market structure. While current knowledge holds for investment in competitive markets—these firms are indeed deterred by violence—conflict has the opposite effect on investment in prospective monopolies (such as utilities, telecommunications, and logistics). Specifically, violence creates opportunities for firms to profit as these are markets with inelastic demand regardless of conflict. I test the theory using novel sector-level FDI data and a conservative estimation strategy. Building on work in business and economics, this project forces us to rethink the relationship between conflict and capital. This has important consequences for the determinants of FDI and post-conflict reconstruction literatures.

1 Introduction

Somalia, a country typically associated with civil war and decades sans an internationally-recognized government, has one of the most developed telecommunications networks in Africa. It boasts some of the lowest international calling rates in the entire world. During the first decade of its civil war, fixed phone lines increased by more than 550 percent (for comparison, they increased by 109 percent in the rest of the world during the same period).¹ This would not be the case but for a handful of international investors who selected Somalia, in the midst of its civil war, as the most desirable location for their mobile phone businesses. Why would these investors, who had the option of investing in any number of developing countries, pick one in the middle of a war?

The conventional wisdom in political science suggests that political violence, a type of political risk, inhibits foreign investment in the developing world (Jensen & Young, 2008). There are strong theoretical reasons to expect that the conventional wisdom is right: violence

¹World Bank 2019.

imposes costs. Violence can lead to direct destruction of assets, indirect destruction of assets via disrupted supply chains, labor forces, or domestic markets, increased transportation costs, heightened security measures, and/or regime turnover that is hostile to investors.

Despite these logical theoretical expectations, the empirical record paints a more mixed picture: Iranian firms recently began investing in telecommunications and electricity in Syria and Dutch firms have been profitable selling mobile phones in Sudan. Investors rebuild destroyed assets in places like Rwanda, Haiti, Liberia, and Sierra Leone. In the last two decades, more than 500 multinational corporations (MNCs) have invested in countries experiencing political violence, accounting for total stock of over \$200 US billion ([Driffield *et al.*, 2013](#); [UNCTAD, 2017](#)). It follows that empirical work on this topic is inconclusive. This null finding begs explanation—why is political violence not deterring investors as we would expect? Might conflict be deterring some firms but incentivizing others, generating a null finding at the aggregate level? [Figure 1](#) indicates that the relationship between political violence and FDI is not as clear cut as the conventional wisdom would hold. Though there are many cases in which there is either no investment or no political violence, which I have excluded² from the graph, there is a lot of interesting variation in the middle.

I theorize that the short answer must be about profit maximization, as that is what drives any competitive firm's behavior. But what makes these firms think they will be able to maximize profit in a conflict zone, especially given that they have other options as to where to invest and that conflict is difficult to predict ([Cederman & Weidmann, 2017](#))? I argue that conflict does not deter investment in the aggregate because there are certain market structures that allow firms to profit regardless of, and even because of, conflict. Specifically, firms in prospective monopolies, such as utilities (electricity, information and communications technology (ICT), water) and logistics (trucking, transportation) are able to profit regardless of conflict. In fact, in these markets, conflict could even increase opportunities for profit.

²I have included the same graph, but with the cases where there is no violence or no investment, in the appendix.

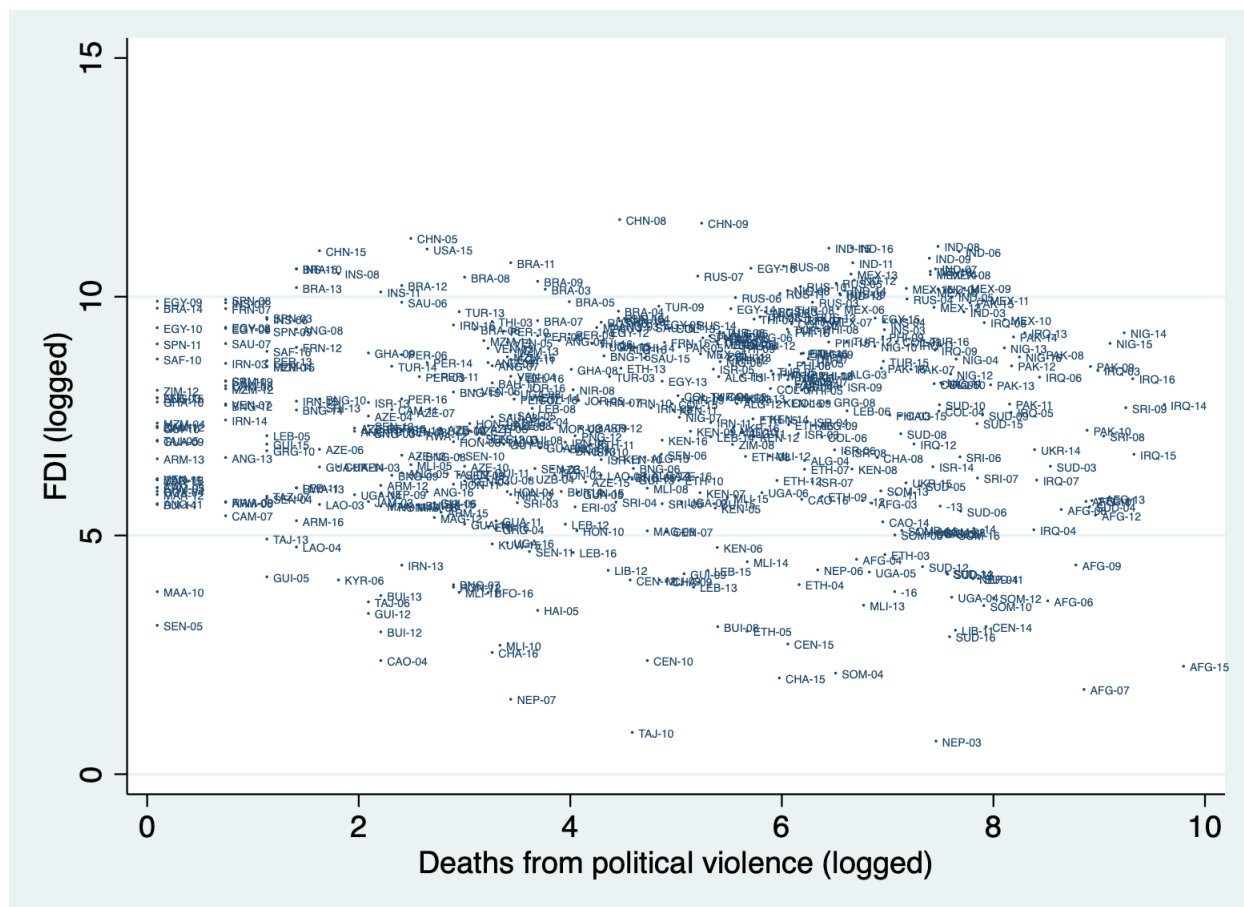


Figure 1: Cases in which there is both violence and investment

I shed new light on our understanding of conflict and capital by showing that firm decision-making about investing in conflict zones varies by market structure. Prospective monopolies have a unique relationship to conflict. First, agents in prospective monopolies tend to provide goods for which there is inelastic demand, even during conflict. Inelastic demand leads to high prices, which favors a few large, consolidated players. Second, prospective monopolies are guaranteed, lucrative markets for the few operators who get a privileged spot in them. These operators would be disinclined to give up that position, even during conflict. Third, conflict disadvantages domestic firms and favors large, well-diversified MNCs, whose headquarters and the bulk of their resources are safe in non-conflict zones. I test this argument empirically and find that political violence does, indeed, incentivize firms operating in prospective monopolies but deters those in competitive markets. Together, the very different dynamics in different types of markets help explain the null finding at the aggregate level.

Despite being addressed by multiple disciplines, scholars have yet to fully tackle the relationship between political violence and FDI. My theory suggests that existing accounts that posit that violence is exclusively bad for firms are incomplete. This article sheds light on the larger question in the literature of how firms make decisions about where to invest in the developing world. The existing international political economy literature provides explanations about FDI location choice that focus on regime type, respect for property rights, and regulatory standards (see [Pandya \(2016\)](#); [Jensen \(2003\)](#); [Li & Resnick \(2003\)](#), among many others). These explanations do well with the primarily North-North FDI that we observed up until 2000. However, they overlook much of the interesting variation in where firms choose to invest, particularly in the developing world.

Understanding the effects of political violence on FDI is relevant beyond academic interest. FDI is critical to the global economy, exceeding the value of all other forms of cross-border capital flows combined. FDI is also important for post-conflict re-construction. In this sense, this research is relevant for policymakers and economic development professionals. The results are directly pertinent to firms, specifically risk managers and investors, who

need to understand the relationship between political violence and FDI to efficiently allocate capital, make informed decisions and maintain investor confidence. In addition to speaking to the determinants of FDI literature, this research is also in conversation with the literature on the economic consequences of civil war, which, to this point, has largely evaded studying firms as the primary actors. Overall, this project builds on two sets of central questions in the discipline: questions about international finance and information and questions about the impact of conflict on capital flows.

I proceed as follows: After outlining existing work, I present a theory of why conflict does not deter investment as we would expect, arguing that there are certain market structures that will allow firms to profit regardless of conflict. I then present my research design, test my theory empirically, illustrate it with real-world examples, and conclude.

2 Existing evidence on FDI and conflict is mixed

There is a robust literature in political science about the determinants of FDI. Scholars have proposed a series of independent variables, chief among them regime type,³ international agreements (such as BITs and PTAs),⁴ and domestic regulation.⁵ These explanations do well with the primarily North-North FDI that we observed up until 2000. However, they do not completely capture the interesting variation in which developing countries firms choose to place their non-site specific investments in.

The literature on FDI in political science has also largely overlooked political violence as key determinant of FDI, with the important exceptions of [Li \(2006\)](#) and [Jensen & Young \(2008\)](#). [Li \(2006\)](#) disaggregates by type of violence and argues that unanticipated civil and international conflict has a negative impact on FDI but that anticipated civil and international conflict does not. [Jensen & Young \(2008\)](#) model how the risk of political violence plays into investor perceptions (measured as the price investors paid for political risk insurance).

³See, among others, [Li & Resnick \(2003\)](#); [Jensen \(2003\)](#); [Pandya \(2016\)](#)

⁴See, among other, [Büthe & Milner \(2014, 2008\)](#); [Elkins *et al.* \(2006\)](#)

⁵[Pandya \(2013\)](#)

Controlling for recent past experiences with violence, they find that the wealthier and more democratic a country is, the less likely investors are to perceive them as risky. Within the existing literature on the topic, the default assumption has been that political violence is bad for business.⁶

Other disciplines, notably economics and business, have also addressed the topic, though with decidedly inconclusive results. The econometric evidence is mixed, though generally finds that there is no statistical relationship between political violence and FDI at the country-year level.⁷ The qualitative evidence (primarily survey evidence) firmly supports the idea that firms take political violence into account when making investment decisions (Bass *et al.* , 1977; Porcano, 1993; Li, 2006). FDI scholars have noted the inconsistency between the qualitative and econometric evidence on this topic (Pearce *et al.* , 1992; Li, 2006). All of the studies make the theoretical assumption that conflict is likely to deter investment, but their empirical findings are largely mixed. Why is the evidence inconclusive when scholars seem to have this intuitive idea that violence is bad for for business?

3 Theory

The conventional wisdom suggests that political violence inhibits foreign investment in the developing world. However, existing evidence suggests that this is not empirically accurate. This requires an explanation, particularly as there are strong theoretical reasons to expect that the conventional wisdom is right: violence imposes costs. Violence can lead to direct destruction of assets, indirect destruction of assets via disrupted supply chains, labor forces, or domestic markets, increased transportation costs, heightened security measures, and/or regime turnover that is hostile to investors. Given this, we would not expect to observe non-extractive FDI to Colombia, Northern Ireland, or Sri Lanka during their respective

⁶Li (2006); Jensen & Young (2008)

⁷Kobrin (1979); Schneider & Frey (1985); Nigh (1985); Fatehi-Sedeh & Safizadeh (1989); Woodward & Rolfe (1993); Loree & Guisinger (1995); Olibe & Crumbley (1997); Li & Resnick (2003); Sethi *et al.* (2003); Globerman & Shapiro (2003); Li (2006)

civil wars, but we do. We would not expect to observe new electricity infrastructure appearing in Afghanistan or South Sudan, but again, we do. To be sure, the amount of inbound FDI during conflicts does not rival the post-conflict investment boom or the amount of FDI going to non-conflict zones. But why are firms with other location options choosing to invest in war zones? Might conflict be deterring some firms but incentivizing others, generating a null finding at the aggregate level?

I theorize about this from the perspective of firms, who are the decision-makers in the realm of FDI. Basic microeconomics can tell us that the reason firms are choosing to invest in conflict zones is profit maximization, which drives any competitive firm's behavior. But what leads firms to believe that they will be able to maximize profit in a conflict zone, especially in the face of other investment options? I argue that conflict does not deter aggregate investment because certain market structures allow firms to profit regardless of conflict. Specifically, firms in prospective monopolies, such as utilities (electricity, telecommunications, water) and logistics (trucking, transportation) can profit even in the midst of violence conflict. In fact, for some firms, conflict may provide unique opportunities for long-term financial growth.

It is worth briefly noting that the degree to which firms can "choose" their investment location depends on the type of industry. For instance, we know that extractive FDI, due to the location specificity of the resources, is going to be where the resources are. Firms often have some choice as to where they would like to extract resources such as oil or gold from, but they do not have the same catalog of choices that, say, manufacturing firms enjoy. For instance, ArcelorMittal has little choice in locations with iron ore, so they invest in Liberia. These companies recognize the risk but invest anyway because there are not alternatives. Often, such as in the case of Sierra Leone, these firms hire private "militaries" for protection. That said, political violence has forced extractive FDI out of countries before (consider the example of Shell in Nigeria⁸) and, theoretically, all other things being equal,

⁸Howden (2006); Reuters (2016)

extractive companies would prefer to invest in locations with less political violence. I account for this FDI in my analyses, but this is not the interesting variation that I seek to explain. Interestingly, the coefficient on political violence is close to zero and insignificant when the dependent variable is only FDI in natural resources, confirming my prior that violence is not a major predictor for this sector.

3.1 Political violence, criminal violence & conflict zones

Before proceeding, it is important to clarify key concepts. By *political violence*, I mean any form of organized violence carried out by political actors, including governments, rebel groups, insurgents, or terrorist organizations (Valentino, 2014). By *conflict zone*, I mean a place that is experiencing sustained political violence. One might ask if there is a meaningful distinction between *political* violence and “regular” violence, or perhaps organized crime. The distinction between them is becoming increasingly blurry. For instance, Barnes (2017) argues that although criminal organizations do not seek to break away from the state, they have increasingly engaged in politics, developing collaborative and competitive relationships that have produced heightened levels of violence and have gained significant authority.

To answer whether or not there is a meaningful distinction for the purposes of this study, we must consider the pathways through which violence affects investors. Criminal violence, like political violence, affects investors by imposing costs, which I unpack below. The difference between the two, for the purposes on investors, lies in how predictable the violence is, which has implications for budgeting. Places tend to have relatively constant rates of criminal violence, but political violence can be episodic (terrorism) or sustained for a number of years (civil war). Moreover, political violence, unlike criminal violence, has the potential to lead to regime change, which may or may not be favorable to investors. Can firms nonetheless plan around political violence?

3.2 FDI decision-making

I first consider the process by which political violence might affect FDI at the firm level. At a basic microeconomic level, the firm's objective is to maximize profit and minimize cost. In the service of this objective, firms consider whether there is political violence, a type of risk that imposes costs, in potential investment locations. Once they have made a decision about where to invest, they engage in the process of approvals and permitting in the host country. Assuming that goes well, the firm then begins its actual investment in a given country. Political violence then does or does not occur in the investment location. The firm then has the option to continue putting money into the investment, to stop putting new money in, or to divest. I make the distinction between the latter two options because it is possible that losses are so high that a firm will not bother divesting to recover only a small percentage of their money. My theory focuses on initial FDI decision-making. Where do firms choose to invest, and how does political violence factor into that decision?

As in any other situation, firms mathematically optimize for profit, and the costs of political violence can be priced in. The frequency and severity of the costs imposed by the violence vary with the type of violence, but can nonetheless be accounted for in profit maximization equations. Some types of political violence, such as international terrorism, are considered episodic,⁹ while others, such as civil war, are considered sustained. Another way to think about this is in terms of how predictable the costs are. In contrast to previous scholars who have argued that it is sustained conflict that deters investment,¹⁰ I argue that firms price in the costs imposed by political violence multiplied by their likelihood and proceed to optimize for profit.

Conflict zones present both risks and rewards to investors. The rewards are potentially great: given that these markets are generally untapped, opportunities for growth are the

⁹In the international business literature, this distinction is often termed “continuous” versus “discontinuous” risk.

¹⁰Witte *et al.* (2016); D Miller (1992); Human Security Report (2014); Ramanujam & Goodman (2003); Li (2006)

biggest (you can sell more cell phones if only ten percent of the population has them), the competition from other firms in the lowest, and many of these markets offer other advantages, such as low labor and land costs, tax incentives, and loose regulation.

Sustained political violence drives down firm competition because it both disincentives entry and causes some firms to either flee or go out of business due to direct or indirect destruction of assets. This reduces competition for either remaining firms or new firms entering the market, allowing market control. Market control is particularly important when countries are unable to import certain goods due to the war, because prices can be justifiably high. These remaining or new firms calculate that they can maximize profit by controlling the market, even while paying to mitigate security risks. Firms may mitigate security risks by hiring private securities companies, building protective fortresses, creating evacuation plans, or directly paying rebel groups for protection.

However, the risks are high: investments may be destroyed, supply chains interrupted, personnel harmed, laws changed or assets seized as a result of regime turnover hostile to investors, etc. The conflict could proceed longer than anticipated or worsen. These costs are real, and would deter investors for good reason: The Marriott Hotel Kabul, built in Afghanistan in 2003, sits deserted,¹¹ Shell has had more than a few issues with militants in Nigeria,¹² and one of LeFarge's cement plants was seized by ISIS.¹³ Not all firms are equally able to navigate the trade-offs between risk and reward in conflict zones. I argue that a key determinant of profitability in conflict zones is the market structure of the firm's industry.

Specifically, I argue that a non-competitive market structure is what allows firms to be confident that they will profit in spite of, or even because of, conflict. However, this leaves open the question of whether political violence will deter investment in competitive markets. I argue that if violence imposes costs and certain market structures protect firms, then firms that are not protected by the market structure should expect violence to be costly enough

¹¹Hersher (2016)

¹²Howden (2006); Reuters (2016)

¹³Alderman *et al.* (2018)

to threaten profit. In other words, my theory suggests that the conventional wisdom, that political violence is bad for business, should hold for competitive, price-taking firms.

3.3 Prospective monopolies and conflict

Prospective monopolies occur when the agents acting in such a market have the power to influence the price, directly or indirectly, which does not occur under perfect competition. How do we know if firms are investing in a competitive market or not? Economists have provided us with a clear definition of prospective monopolies (also referred to as non-competitive markets). Key examples include utilities, such as electricity, telecommunications, and water management, and logistics, such as trucking and transportation ([Mankiw, 2015](#); [Slavin, 2009](#); [Sharkey, 1982](#)). The idea that certain sectors lend themselves to becoming monopolies is not new: in a recent report on FDI, UNCTAD noted that “some services (especially basic utilities and infrastructure) may be natural monopolies and hence susceptible to abuses of market power.”

We have observed this empirically: Chinese and Iranian firms are currently investing in the electricity sector in Syria ([Reuters, 2017b](#)). Iran is also entering Syria’s telecommunications industry. Similarly, Swedish giant Ericsson has had great success in telecommunications in Sudan, among other conflict zones. South African MTN and Vodacom have successfully invested in mobile phones across Africa. Anglo-Dutch Celtel was profitable in Sierra Leone during the conflict. Celtel’s CEO explained: “We try not to take risks. Fortunately our service is seen as universal and various parties to any conflict benefit from what we do. They see it as just like water or like air, so we have never been harmed or targeted” ([Cronin, 2004](#)). This highlights the inelastic and often universal demand consumers have for the products supplied in non-competitive industries. Further, firms are able to profit in this sector by selling pre-paid SIM or electricity cards to prevent collection problems. Given lower fixed costs, telecommunications firms are also able to move into profit within two to three years, which is much faster than the ten or so years typically required in, for example, extractive

industries.

Prospective monopolies have a unique relationship to conflict. First, agents in prospective monopolies tend to provide goods for which there is local, inelastic demand (such as electricity), even during conflict. Inelastic demand leads to high prices, which favors a few large, consolidated players. Second, prospective monopolies are guaranteed, lucrative markets for the few operators who get a privileged spot in them. These are often protected industries with high barriers to entry, and as such frequently come with particularly high levels of government protection. Firms would be reluctant to give up the position, even during conflict. Third, conflict disadvantages domestic firms and favors large, well-diversified, multinational corporations, whose headquarters and the bulk of their resources are safe in non-conflict zones. If all firms had equal ability to exploit these opportunities, these would be saturated markets.

In sum, because violence imposes costs, firms will only invest if they are confident that that they will be able to maximize profit despite these costs. The non-competitive market structure allows firms to be confident that they will profit in spite of, or even because of, conflict. Firms that will not be protected by the non-competitive market structure should expect violence to be costly enough to threaten profit. As such, the conventional wisdom, that political violence is bad for business, should hold for competitive, price-taking firms.

4 Research design

As I seek to understand how violence affects foreign investment to a country, I test whether political violence predicts FDI in a given country in a given year. However, because I theorize that the relationship will vary based on the market structure of the FDI, I divide FDI into two dependent variables—FDI in prospective monopolies and FDI in competitive markets. I aggregate project-level data to the country-year level for the purposes of my empirical analysis, as this is a good measure of aggregate firm behavior.

My sample is all developing countries¹⁴ between 2003 and 2017.¹⁵ I focus on developing countries for three reasons. First, we know far more about North-North FDI than we do about North-South or South-South FDI. Second, existing research shows that foreign investment into OECD and non-OECD countries are fundamentally different phenomena and should be studied separately. Third, my theory is not applicable to most developed countries, as they are not the countries experiencing political violence.

My dependent variable is FDI. Following [Kerner \(2014\)](#), I use capital expenditures (capex) to measure FDI, as opposed to flows or stock: because the theory concerns a firm's fixed capital (expenditure on land, physical structures, equipment, etc.), not the net value of border-crossing capital movements between MNCs and their foreign affiliates (flows) or the value of foreign direct investors' stake in the foreign affiliates operating in a host country at any given time (stock). This is in contrast to existing studies of political violence and FDI, which primarily measure FDI using flows. My capex data is from FDImarkets. I log the values as the data is highly skewed. I disaggregate FDI by sector and then re-aggregate to create two categories for each country: FDI to prospective monopolies and FDI to competitive markets. Specifically, following the theoretical discussion, I classify the utilities (ICT, electricity, water), transportation and logistics sectors as non-competitive, and all other sectors as competitive.¹⁶ I treat the competitiveness of a market as time and country invariant, which is reasonable given the short time sample.

The primary independent variable is political violence. I use PRIO's GED event data ([Gleditsch, 2002](#)) and aggregate it using their best estimate of deaths. Following the theoretical discussion about the time it takes to make an investment decision, I aggregate deaths from political violence in a country for the three years prior to the investment. I log the data as it is also highly skewed.

My theory predicts that **political violence** will have a *positive* and significant impact

¹⁴I operationalize this as countries that are not classified as "high income" by the World Bank.

¹⁵These are the only years available from FDImarkets.

¹⁶I test a slightly narrower specification of the dependent variable in the appendix and find similar results.

on FDI to non-competitive industries and a *negative* and significant impact on competitive industries. I use panel ordinary least squares (OLS) models with a lagged endogenous variable as well as country and year fixed effects to test this. The country fixed effects hold everything else about the country constant and the year fixed effects control for global macroeconomic trends. In both models I control for the other type of FDI in addition to using a lagged endogenous variable. As mentioned above, my sample is all developing countries¹⁷ between 2003 and 2017.¹⁸ In order to avoid selecting on the dependent variable, I include all country-years, even those in which there is no investment. I have 1560 observations in each model.

5 Results

I present the findings of my models in Table 1 and Figure 2. These models demonstrate that, as predicted, `political violence` has a positive and statistically significant impact on FDI to prospective monopolies and a negative and statistically significant impact on FDI to competitive industries. The fact that the sign of `political violence` switches from positive and significant to negative and significant in an otherwise identical model should help to alleviate any concerns about model specification.

These results help us better understand the null finding at the aggregate level: if the effect of conflict on FDI varies significantly with market structure, then the aggregate finding will be null. These results suggest that the conventional wisdom in political science—that violence is bad for business—is correct insofar as it applies to competitive, price-taking firms. However, it confirms that our existing explanations are incomplete: there is a subset of firms that can actually profit from political violence.

I assuage a few potential empirical concerns. First, I confirmed that neither the dependent variable (FDI) nor the main independent variable of interest (political violence) have a unit

¹⁷I operationalize this as countries that are not classified as “high income” by the World Bank.

¹⁸These are the only years available from FDImarkets.

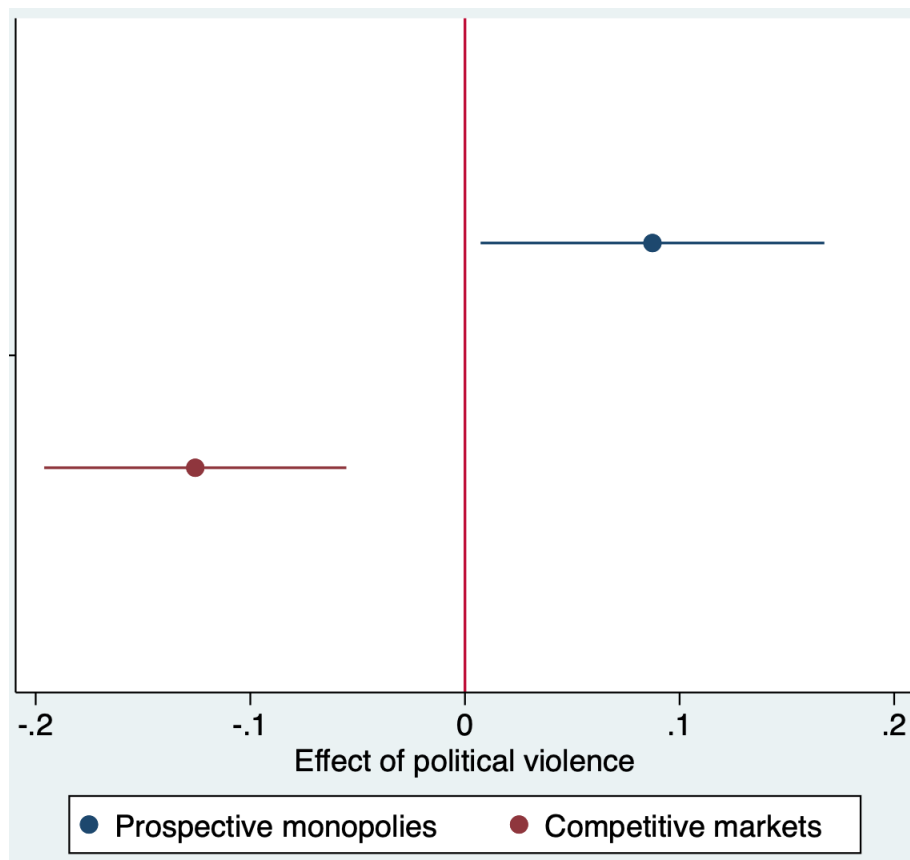


Figure 2: Market structure explains null aggregate findings

Table 1: Effect of political violence on FDI varies with market structure

	(1)	(2)
	Prospective monopolies	Competitive markets
Political violence	0.087** (0.041)	-0.126*** (0.036)
Constant	-6.106*** (0.792)	-2.110** (0.696)
Lagged endogenous variable	0.007 (0.027)	0.022 (0.027)
Country fixed effects	✓	✓
Year fixed effects	✓	✓
Control for other type FDI	✓	✓
Developing countries only	✓	✓
<i>N</i>	1560	1560

Standard errors in parentheses

** $p < 0.05$, *** $p < 0.01$

root. In other words, they are trend-stationary. As the statistical tests for unit roots are known for having low statistical power, I employed a battery of them, including Dickey Fuller, Levin-Lin-Chu, and Harris-Tzavalis. All tests present overwhelming evidence against the null hypothesis of a unit root and as such I conclude that both FDI and political violence are stationary.

Next, I address endogeneity concerns. In addition to using a lagged endogenous variable, I alleviate endogeneity concerns by confirming that FDI is not a statistically significant predictor of political violence. In the appendix, I demonstrate that total FDI, non-competitive FDI and competitive FDI are not statistically significant predictors of political violence.¹⁹

6 Telecommunications in Somalia

Consider the case of mobile phones in Somalia. Somalia, designated as either a failed state or a fragile state for the past few decades, hardly seems like a likely candidate for foreign investment. However, MNCs have invested profitably in Somalia since the collapse of the central government in the early 1990s. A variety of interim federal administrations were established in the 2000s, but Somalia did not have an internationally-recognized government until 2012. Somalia is cited as an example of a real-world anarchist stateless society ([Gettleman, 2007](#)).

It may seem puzzling, then, to talk about Somalia in a paper on foreign investment. Recall, however, the starting point of this paper: investors are interested in profit, and profit is not necessarily correlated with peace. In fact, as I have just demonstrated empirically, often it is the absence of peace that creates opportunities for investors. This is not universally true, but it is in the case of prospective monopolies, such as telecommunications. As discussed earlier, the telecommunications industry is a natural monopoly because duplication of infrastructure is economically inefficient. As with other public utilities, there are huge economies of scale: the average total cost decreases with each additional line ([Whiteman,](#)

¹⁹Please refer to Table 2 in the appendix.

1990). Typically, governments manage this by allowing firms to bid for a certain number of licenses, as more competition is not thought to be advantageous.

In 1991, Somalia had 15,000 fixed phone lines. By 2003, it was up to 100,000. That is a 567 percent increase, during anarchy and civil war. For comparison's sake, the rest of the world saw a 109 percent increase during the same time. Similarly, in 2000, 1.1 Somali out of every 100 owned a mobile phone. Now, less than two decades later, nearly 50 out of 100 people do. Additionally, Somalia has the lowest international call rates in Africa and one of the cheapest in the world ([The World Bank, 2019](#)).

How did this happen? When the dictatorial regime of Siad Barre collapsed in 1991, so did the state's complete monopoly on telecommunications, prompting private actors to enter ([Winter, 2004](#)). A manager of Telcom Somalia, the first private telecommunications firm to enter Somalia's market, spelled out the firm's logic: "The government post and telecoms company used to have a monopoly but after the regime was toppled, we were free to set up our own business. We saw a huge gap in the market, as all previous services had been destroyed. There was a massive demand." He explained that though the major airport and port were destroyed in the violence, firms were able to set up a small air strip and use natural harbors to import their equipment.²⁰

Demand for telecommunications is particularly high and inelastic in conflict zones. As [Deibert \(2013\)](#) points out, militants from warring clans can all agree on the need for telecoms. He outlines that militants need a functioning cellphone system to operate their ventures, issue threats via text messaging, make mobile payments for drugs and weapons, and organize their tribal factions. He also notes that mobile phones are less easy to intercept than land lines and provide a degree of anonymity because SIM cards and phones can be recycled and exchanged.

Interestingly, Hormuud, one of the major telecommunications providers, actually profits when there is more violence in al-Shabaab areas of Somalia because people are too afraid to

²⁰[Winter \(2004\)](#)

leave their homes. Instead, they stay in and use their phones to communicate with friends and relatives. Violence also encourages the use of mobile phones because mobile phones facilitate use of mobile banking, which is much safer than carrying around hard currency due to the likelihood of theft. Finally, because of the violence, many Somalis have fled to other parts of the world and send remittances back home via an informal banking network called hawala. Telecommunication is critical for the functioning of hawala.²¹

In addition to inelastic demand, there were certainly other benefits of the chaotic situation: there were no taxes whatsoever, no need to bid for the telecommunications licenses that are typically provided by the government, no corrupt officials to bribe, and no laws or regulations governing firm behavior.²² In the case of Somalia, mobile phone companies actually agreed to a certain degree of self-regulation via the Somali Telecom Association (Bray, 2005). Foreign investors seem to favor loose regulation over no regulation at all. As the secretary general of the Somali Telecom Association stated, “we need a government to regulate and provide necessary legislation as well as ensure security and peace which would attract much needed foreign investment to the sector” (Bhalla, 2004).

There is a fair amount of variation in what loose regulation in a conflict zone may look like, ranging from a government that is otherwise too preoccupied to enforce regulation, to governments dramatically reducing regulation to attract foreign investors, to the complete absence of a government to impose regulations to begin with.

Investing in violence, however, is not without challenges: the security situation is difficult to navigate, staff must go abroad for any training, and it is difficult to transfer money to foreign partners without a central bank.²³ Somalia is considered one of the most ethnically homogeneous countries, with 85 percent of the inhabitants being ethnic Somalis. However, there are many warring clans. In order to navigate this and protect business, Hormuud,

²¹Deibert (2013)

²²Lack of regulation is a large problem from an economic development standpoint for a variety of reasons, one of which being that “almost every dollar made by foreign telecoms companies is a dollar that leaves Somalia” (Hassan, 2003).

²³Winter (2004)

one of the largest operators, intentionally sells shares to each clan, such that everyone has a stake in the business (Mohamed & Childress, 2010). This gives the company a remarkable amount of soft power in protecting its interests (Clingendael, 2019). The dispersed ownership structure also builds trust with different consumer bases. A representative from the company was explicit about their objectives, saying, “we cannot sit and watch when the state is unable to react: we have different shareholder structure. We have shareholders in every clan, which is how we gain access to every client.”²⁴ This has not been an entirely foolproof plan, as Hormuud has lost both employees and fixed lines to mortar explosions, but it does serve to mitigate risk. Telecom companies in Somalia spend five percent or more on safety.

One might reasonably wonder who these investors are that felt comfortable setting up shop in Somalia during the civil war. Some, but not all, are diaspora investors, or Somali expatriates who decide to return. This phenomenon is not exclusive to Somalia: Mo Ibrahim, the founder of Celtel, which set up mobile phone networks in 13 African countries, was born in Sudan. To give an example from another industry, Afghan-Americans were largely responsible for funding the Afghanistan International Bank (Bray, 2005).

In the case of Somalia, a variety of Somali expatriates began to invest in the 1990s. Each had international partners, including Norway-based Telenor as well as US-based AT&T and Starlight Communications. There are now four main telecommunication companies in Somalia, three of which signed an interconnectivity deal in 2005 to set prices and control competition (Mohamed & Childress, 2010).

Somalia between 1991-2012 is clearly the most extreme case given the rare existence of anarchy, but the large-N statistical analysis should ease any concerns that anarchy is required for the theory to hold. For example, we have observed a comparable situation in the Democratic Republic of the Congo. Similarly, South African MTN, a mobile-telecoms firm, is currently one of two mobile carriers in Syria and is operating profitably (Economist, 2014).

²⁴Clingendael (2019)

7 Other examples

There also plenty of examples outside of the telecommunications industry. Water, as a public utility, is another example of a natural monopoly. As the OECD explains, this is because providing water entails high fixed costs, long-term irreversible investments, and inelastic demand (OECD, 2009). The subject of water privatization insofar as it pertains to economic development and ethics is far beyond the scope of this paper, but the sector still provides an illustrative example of firms being willing to invest in conflict zones.

Many countries that have experienced conflict also have at least partially privatized water management. These include, but are not limited to, Ivory Coast, Algeria, Colombia, Mexico, the Philippines, Nigeria, Egypt, Djibouti, and Pakistan. For example, Hyflux, a Singaporean water treatment firm, has ventures in Nigeria and Egypt. They also won a \$100 million contract in 2009 Libya to desalinate seawater. Italian WTD desalinates water in Iran, and French Stereau has done something similar in Djibouti. US-based firm Global EnviroScience Technologies signed an agreement worth \$2.8 billion for a power plant and desalination system in Pakistan in 2008.

Of course, the risk versus reward tradeoff does not always work out exactly as the firm expects. Hyflux's contract with Libya has been on hold since 2011 due to increasing political violence in the country.²⁵ French Saur pulled out of Mozambique and Zimbabwe in favor of less risky markets. Empirically, however, the potential rewards in water management are high given the prospective monopoly aspect.

Electricity is another example of a natural monopoly. As with water, there are many examples of countries that both experience conflict and have privately-provided electricity (typically referred to as an Independent Power Producer or IPP in this industry). These include Algeria, Ivory Coast, Egypt, Kenya, Zambia, Pakistan and Syria. For example, British Azuri Technologies provides pay-as-you-go electricity across Sub-Saharan Africa²⁶

²⁵Reuters (2011)

²⁶Reuters (2017a)

and M-Kopa, a Kenyan solar energy firm, does the same across East Africa.²⁷ Australian Pierlite operates in Pakistan. Finnish Wärtsilä opened a new power plant in Syria in 2010.²⁸ Even in the most dire conflicts, we still see foreign investment in electricity.

8 Conclusion

This paper addressed the consequences of political violence for FDI. It challenged the conventional wisdom in political science that political violence inhibits foreign investment in the developing world. I argued that conflict does not deter aggregate investment because there are certain market structures that will allow firms to profit not only in spite of, but even because of, conflict. Specifically, violence creates opportunities in prospective monopolies (such as utilities and logistics) as these are markets with inelastic demand regardless of conflict. I tested the theory using novel sector-level FDI data, which I aggregated to competitive industries and prospective monopolies. I found that the conventional wisdom is correct as it applies to firms in competitive markets—these firms do not like violence—but that firms in prospective monopolies behave systematically differently in the face of conflict.

This paper resolves mixed empirical evidence by introducing the idea that we must move beyond thinking about FDI as homogenous. By introducing sectoral heterogeneity to my model, I show that the relationship between conflict and investment is not nearly as simple as previous work suggests. This point holds more broadly: the current IPE literature tends to disregard the interesting variation within FDI. We need to move beyond thinking about FDI as simply the sum of capital expenditure/stocks/flows and instead test our theories with attention to sectoral, subnational, source-country, and investing firm heterogeneity. There are strong theoretical reasons to expect that firms from different countries behave differently, that bigger and more diversified firms behave differently than smaller ones, that certain geographic areas of countries might be more stable and thus more desirable for investment

²⁷Faris (2015)

²⁸Corporation (2010)

than others, etc.

In that vein, this paper leaves open important avenues for future research. First, this analysis highlights that there are dynamics between domestic and foreign firms that remain under-explored in the literature. Second, existing literature indicates that bilateral ties, such as trade and bilateral investment treaties, are important predictors of FDI. How do those explanations weigh against the structural explanation posed here? Third, how do other IPE theories that use FDI as a dependent variable hold up if we introduce heterogeneity in the outcome? For instance, do trade agreements affect all types of FDI the same way?²⁹

This analysis helps to complete our understanding of the relationship between political violence and FDI. It also moves forward the determinants of FDI literature by re-introducing a critical independent variable. In the broader context, this article sheds light on two sets of central questions in the discipline: questions about international finance and information and questions about the impact of conflict on capital flows.

²⁹Büthe & Milner (2014, 2008)

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9 Appendix

9.1 Figure 1

Figure 3 replicates Figure 1 but includes cases where either investment or political violence is zero.

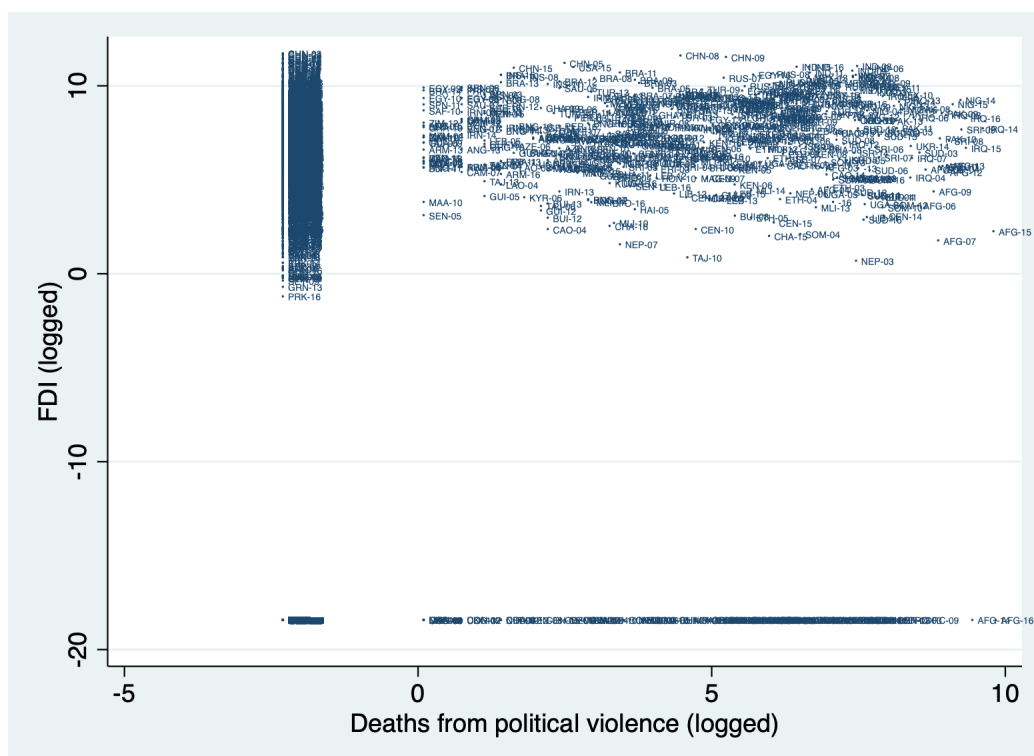


Figure 3: Political violence (lagged) vs. FDI (lagged)

9.2 Endogeneity concerns

In addition to using a lagged endogenous variable, I alleviate endogeneity concerns by confirming that FDI is not a statistically significant predictor of political violence. Here, I demonstrate that total FDI, FDI to prospective monopolies and FDI to competitive markets are not statistically significant predictors of political violence. I operationalize all variables the same way as in the main model (Table 1) and use the same model specifications.

Table 2: FDI does not predict conflict

	(1)	(2)	(3)
	Political violence	Political violence	Political violence
Total FDI	0.000 (0.021)		
Prospective monopolies		0.002 (0.015)	
Competitive markets			0.014 (0.017)
Constant	-4.569*** (0.435)	-4.556*** (0.444)	-4.529*** (0.437)
Year fixed effects	✓	✓	✓
Country fixed effects	✓	✓	✓
Lagged endogenous variable	✓	✓	✓
<i>N</i>	1560	1560	1560

Standard errors in parentheses

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

9.3 Alternative specification of the dependent variable

For robustness, I also estimate the models using a slightly different specification of the dependent variable. Based on microeconomic theory it is most correct to use the specification of prospective monopolies and competitive markets used in the main paper, but if we use a more conservative definition that only counts logistics, telecommunications and electricity as prospective monopolies, the results are similar. In the case of prospective monopolies, `political violence` is positive and $p = 0.15$. In the case of competitive markets, `political violence` is negative and significant. I present the results in Table 3.

Table 3: Effect of political violence on FDI varies with market structure

	(1)	(2)
	Prospective monopolies	Competitive markets
Political violence	0.084 (0.059)	-0.171** (0.057)
Constant	-14.412***	-10.227***
Lagged endogenous variable	0.013 (0.026)	-0.014 (0.027)
Country fixed effects	✓	✓
Year fixed effects	✓	✓
Control for other type FDI	✓	✓
Developing countries only	✓	✓
<i>N</i>	1560	1560

Standard errors in parentheses

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Paper 3:

Global diversification and political risk: discount or premium?

Abstract

Does global diversification lead to a “diversification discount”? Firms are increasingly globally diversifying, or expanding their operations to other countries. Of the nearly 5,000 publicly-traded US firms with subsidiaries, more than 1,000 have operations in at least one foreign country. This is puzzling, given that the conventional wisdom in finance indicates that there is a “diversification discount,” or that firms that diversify are valued at a discount relative to comparable, un-diversified firms. I argue that this conventional wisdom fails to account for politics. Specifically, firms both reap the benefits of investing in developing countries (lower corporate taxes, looser regulation, cheaper labor and inputs, direct access to a new and possibly untapped market) and hedge against the heightened political risk in those countries by globally diversifying. This is because political risk across different countries is largely uncorrelated, which is the basic principle of risk management. Further, I argue that the diversification discount (premium) varies by industry. I test my theory using a conservative estimation strategy on data on all publicly-traded US firms. I find that the extent of their global diversification positively affects the degree to which they are valued relative to the market. This research takes the conversation as to how politics influences foreign direct investment a step further by exploring how firms balance their portfolio of investments in response to the politics of each country they are investing in.

1 Introduction

Does global diversification¹ lead to a “diversification discount”? Firms are increasingly globally diversifying, or expanding their operations to other countries.² Of the nearly 5,000 publicly-traded US firms with subsidiaries, more than 1,000 have operations in at least one foreign country.³ The *Wall Street Journal* recently announced that the “Global Market is Good to US Firms.” This is puzzling, given that the existing literature tells us that there is a “diversification discount,” or that firms that diversify are valued at a discount relative to

¹Also referred to as international diversification, internationalization, geographic diversification, international expansion, globalization, and multinationality.

²This is distinct from international portfolio diversification. For more on this topic, see, among others, Berger *et al.* (2011).

³Author’s calculation based on Dun & Bradstreet data.

comparable, un-diversified firms. The literature contends that this discount holds both for industrial and global diversification.

I argue that the literature fails to account for the fact that firms both reap the benefits of investing in developing countries (lower corporate taxes, looser regulation, cheaper labor and inputs, direct access to a new and possibly untapped market) and hedge against the heightened political risk in those countries by globally diversifying. This is because political risk across different countries is largely uncorrelated. Adverse regime change in Indonesia is highly unlikely to be correlated with political violence in Pakistan. Further, I argue that the diversification discount (premium) varies by industry. For example, firms in labor-intensive industries, such as manufacturing and construction, have much to gain by diversifying to countries with lower labor costs.

I test my theory using data on all publicly-traded US firms. I find that the extent of their global diversification, or the number of countries they have expanded to, affects the extent to which they are valued relative to the market. I then show that this effect varies across industries. This paper makes a series of important contributions. First, it overturns a key finding in the literature, which has implications both for academic debate but also for the firms themselves. Second, it incorporates twenty new years of data, which is important given that the world has continued to globalize in dramatic fashion since the seminal studies on this topic were published in the early 2000s. Third, it overcomes methodological challenges related to endogeneity, the imputed-value method, and the excess value measure. Fourth, it introduces a new measure of firm exposure to political risk.

The rest of the paper proceeds as follows: I first situate the puzzle in the existing literature. I then present a theory of why global diversification should enhance firm value. Next, I lay out the research design and test my theory empirically in three stages. Finally, I illustrate the theory with the case of Coca-Cola and conclude.

2 Existing literature

This project is situated between two robust literatures: that of global diversification and that of foreign direct investment (FDI) and political risk. I discuss each in turn and then carve out my contribution.

2.1 Global diversification

There are multiple ways for firms to diversify. There is industrial diversification, which is when a firm diversifies across multiple lines of business. There is also global diversification, which is when a firm diversifies across different national markets. There is a robust literature on industrial diversification, which largely finds that diversified firms are valued at a discount relative to comparable, un-diversified firms. In short, there is a “diversification discount” (Berger & Ofek, 1995; Lang & Stulz, 1994; Lloyd & Jahera, 1994; Rumelt, 1974; Servaes, 1996).⁴ However, industrial diversification has been declining since the mid 1980s (Comment & Jarrell, 1995) but global diversification has been increasing (Denis *et al.*, 2002).

The question then remains whether the same diversification discount applies to global diversification. There is a growing literature that examines the relationship between global diversification and firm performance. The results are mixed, to say the least: some say there is a negative relationship between global diversification and firm performance (Denis *et al.*, 2002; Riahi-Belkaoui, 2002; Riahi-Belkaoui & Alnajjar, 2002), others an S-shaped relationship (Contractor *et al.*, 2003; Lu & Beamish, 2004; Riahi-Belkaoui, 1998; Thomas, 2004), others a U or inverted U shaped relationship (Capar & Kotabe, 2003; Gomes & Ramaswamy, 1999; Hitt *et al.*, 1997; Lu & Beamish, 2001). One study finds a positive relationship (Ramírez-Alesán & Antonio Espitia-Escuer, 2001). Adding, perhaps, to the confusion is the fact that this topic is discussed in many disciplines: management, finance, accounting, international business, and marketing.

⁴Scholars argue that this is due to inefficient investment policies (Rajan *et al.*, 2000; Scharfstein & Stein, 2000; Shin & Stulz, 1998).

A second group of scholars have proposed many variables that moderate the relationship between global diversification and firm performance. They include regional diversification (Li & Qian, 2005), firm size (Qian & Li, 2002; Qian *et al.*, 2003), research and development (R&D) intensity (Kotabe *et al.*, 2002; Qian *et al.*, 2003), marketing intensity (Kotabe *et al.*, 2002), diversity of investment opportunity set (Riahi-Belkaoui, 1998), size at time of IPO (Bloodgood *et al.*, 1996), and unrelated diversification (Riahi-Belkaoui, 1996). Scholars have also examined the many antecedents, or causes, of global diversification, such as agency costs and outside directors, as well as outcomes other than firm performance associated with global diversification, such as technological learning, innovation, and asset turnover. However, I consider these studies to be outside the realm of this paper.

Existing literature also outlines potential costs and benefits of global diversification. In terms of benefits, Morck & Yeung (1991) propose that there are synergistic benefits from information-based assets within the firm.⁵ In this way, global diversification increases intangible assets. Similar to the “winner-picking” argument that Stein (1997) proposes for industrial diversification, Denis *et al.* (2002) propose that global diversification provide firms with flexibility. For instance, if a firm has multiple foreign subsidiaries, they can shift production to the country where the production costs are the lowest or choose to distribute more in countries where the demand is highest. They also have the flexibility to exploit different tax laws, choosing to raise capital in countries where the taxes are the lowest. I find this argument convincing, but they do not fully explore it, theoretically or empirically, beyond a few sentences.

As for costs, Harris *et al.* (1982) and Myerson (1982) argue there are complexity costs associated with industrial diversification. They argue that it is difficult to coordinate corporate policies and there will be information asymmetries between different segments and divisional managers. Denis *et al.* (2002) point out these complexity costs might also apply to global diversification. The industrial diversification literature also points to costs associated with

⁵The internalization theory of synergy was originally proposed by Caves (1971)

cross-subsidizing less profitable divisions (Meyer *et al.*, 1992; Rajan *et al.*, 2000; Scharfstein & Stein, 2000), which may apply to global diversification.

2.2 Political risk and foreign direct investment

There is also a robust literature on political risk and foreign direct investment that spans political science, international business, and applied economics. Scholars have examined different political variables that might affect FDI location choice. These include regime type (Jensen, 2003; Li & Resnick, 2003), the risk of political violence (Fatehi-Sedeh & Safizadeh, 1989; Globerman & Shapiro, 2003; Jensen & Young, 2008; Kobrin, 1979; Li, 2006; Loree & Guisinger, 1995; Nigh, 1985; Olibe & Crumbley, 1997; Schneider & Frey, 1985; Sethi *et al.*, 2003; Woodward & Rolfe, 1993) and the risk of expropriation (Wellhausen, 2014; Pandya, 2016; Vernon, 1971). In general, the authors find that firms do account for political risks when making their long-term investments in the developing world.

2.3 Contribution

In this paper I bring these two literatures together and show that global diversification is value-adding for firms because it is an effective form of political risk management. There is very little empirical work that explicitly brings politics into the study of global diversification and the work that exists is quite narrow in scope.⁶ I show that the greater the geographic dispersion of the risks, the higher the diversification premium. I do this by proposing a causal mechanism that has previously been ignored – political risk management. Globalization has also changed the world considerably in the 20 years since the bulk of the papers on this topic were written. Additionally, previous work either treats all firms as homogeneous with respect to sector or only looks at one sector. I test to see if the diversification discount (premium) varies by industry. Finally, I contribute a new measure of firm exposure to political risk.

⁶For example, Jimenez (2010) looks only at the case of Spain, Cohen *et al.* (2011) look only at oil and gas diversification, Holburn & Zelner (2010) look at electric power generation, and Ellstrand *et al.* (2002) look at manufacturing.

3 Theory

3.1 Risks and rewards of foreign markets

There is much for firms to gain by expanding operations to other countries. Land/labor costs and regulatory standards affect investment location choice. Firms often choose to engage in FDI to take advantage of lower labor costs. [Korten & Burns \(2016\)](#) argue that these favorable labor costs are widely available throughout the developing world. Similarly, firms choose to avoid countries with higher regulatory standards in regard to labor laws and environmental protection, but, conveniently, studies have shown that developing countries lower regulatory standards in order to attract foreign capital ([Castles *et al.*, 1995](#); [Encarnation & Mason, 1990](#); [Goodman & others, 1993](#); [Rodrik, 1997](#); [Simmons & Elkins, 2003, 2004](#)). The combination of widely available cheap labor and low regulatory standards means that today there are plenty of attractive locations for FDI in developing countries. Unsurprisingly, firms also choose to avoid countries with high corporate taxation. Firms also benefit from cheaper inputs and direct access to a new and possibly untapped market. The literature has also proposed that firms diversify abroad to achieve operating synergies ([Feinberg & Phillips, 2002](#)), to realize economies of scale, to meet portfolio diversification objectives of investors, access to new resources, and knowledge acquisition.

3.2 Risks of foreign markets

There are many risks associated with diversifying internationally. Some are political, some are not. The non-political risks that have already been proposed by the literature include agency costs, coordination costs, and diminished shareholder value. Political risk includes the risk of expropriation (that the government will seize your assets), credit risk (that you won't get paid or the exchange rate will plummet), and the risk of political violence (terrorism, civil war, etc). The countries with the desirable investment properties discussed above, such as cheaper land and labor, also typically also have higher political risk.

Depending on the location substitutability of their investments, firms have varying degrees of control over how much risk they must take on. Extractive FDI in general has to have a much higher risk tolerance because when firms require particular natural resources, the location is less substitutable. For instance, ArcelorMittal has little choice in locations with iron ore, so they invest in Liberia. These companies recognize the risk but invest anyway because there are not alternatives. In the case of political violence, sometimes the natural resources are far from the conflicts within the country, rendering the conflict irrelevant. In the case of Algeria, where the majority of FDI is in coal, oil, gas and minerals, the majority of investment is in the southern part of the country and the majority of the violence which occurred during the Algerian civil war (1991-2002) occurred in the north part of the country, which is separated from the south by a desert. One executive of a firm that invests in Algeria noted, “our confidence comes from the desert.” Similarly, there are numerous infrastructure companies, such as Petrofac, Bechtel and Halliburton, that provide services to the extractive sector. As such, they also do not have much choice in location. They do have a choice of who to work with and where. While it used to be that these firms relied primarily on the extractive company that they contracted with, such as BP or Shell, to convey the level of associated risk, they increasingly have their own risk management teams. Agro-business firms, such as banana and palm oil farmers, are also restricted to certain locations globally as bananas only grow within a certain degree of the equator and require much land to be harvested.

3.3 Mitigating political risk

One of the defining characteristics of FDI that differentiates it from other types of international capital flows is its illiquid nature (Feinberg & Phillips, 2002). Time horizons do vary based on obstacles to divestment (Jensen & Young, 2008) but generally speaking FDI decisions are long-term decisions that require transportation networks, production facilities and training workers. Importantly, these sunk costs, in addition to the potential of the host

countries changing the initial contract terms (Vernon, 1971), make barriers to exit high. This means that firms must take risks quite seriously, because even if they sell their assets, their assets' values are likely to also decrease with the violence (Jensen & Biglaiser, 2012).

Theoretically, firms with less expensive investments that do not have to be managed from in-country (such as cell towers) would need to be less concerned with mitigating risk. Firms have many ways to hedge against political risk. For example, they have the opportunity to purchase risk insurance, which is priced based on the actuarial perception of the likelihood of a political risk in a particular country. Political risk insurance goes back to the Marshall Plan, which intended, in part, to insure U.S. investments against violence and activities of other governments (Hansen, 2005; Jensen & Young, 2008). Today, one of the biggest providers is the World Bank's Multilateral Investment Guarantee Agency (MIGA), whose mandate is to provide investment insurance and investment promotion to developing countries. Between 1990 and 2000 alone, MIGA issued 473 political risk insurance contracts (totaling \$7.1 billion), which paved the way for \$36 billion in FDI to some of the most risky countries (West & Tarazona, 2001). Another major insurer is the U.S. government's political risk insurance agency, the Overseas Private Investment Corporation (OPIC), who, in 2016, had active projects in more than 100 countries. Importantly, one third of OPIC's portfolio is in conflict-affected countries (OPIC 2016). Between 1971 and 2008, OPIC issued \$969.6 million for 286 claims (Jensen & Biglaiser, 2012). Claims spanned multiple industries: mining, gas, oil, infrastructure, and mining, but the vast majority of claims are related to expropriation (Jensen & Biglaiser, 2012; O'Sullivan, Robert C., 2005).

Similarly, most OECD countries, and increasingly non-OECD countries, have government agencies that provide political risk insurance (N. M. Jensen 2008). Nippon Export and Investment Insurance of Japan, Compagnie Francaise d'Assurance pour le Commerce Etranger (COFACE) of France, and Export Development Corporation (EDC) of Canada all fall into this category. In addition to MIGA, other multilateral development agencies provide risk insurance, notably the Inter-American Development Bank (IDB), and the Asian Devel-

opment Bank (ADB). The main private political risk insurers are American International Group (AIG), Lloyd's of London, Zurich Emerging Markets Solutions, and Sovereign Risk Insurance.

However, political risk insurance does not cover all types of political risk. Additionally, coverage can be extremely expensive and only covers 90 percent of the investment, presumably for reasons related to moral hazard. OPIC, for instance, charges oil and gas companies 3.4 percent the total price of their assets per year for full coverage and requires that if a firm files a claim they have to hand their assets over to their provider. In essence, a firm gives their asset to their insurer in exchange for the claim.

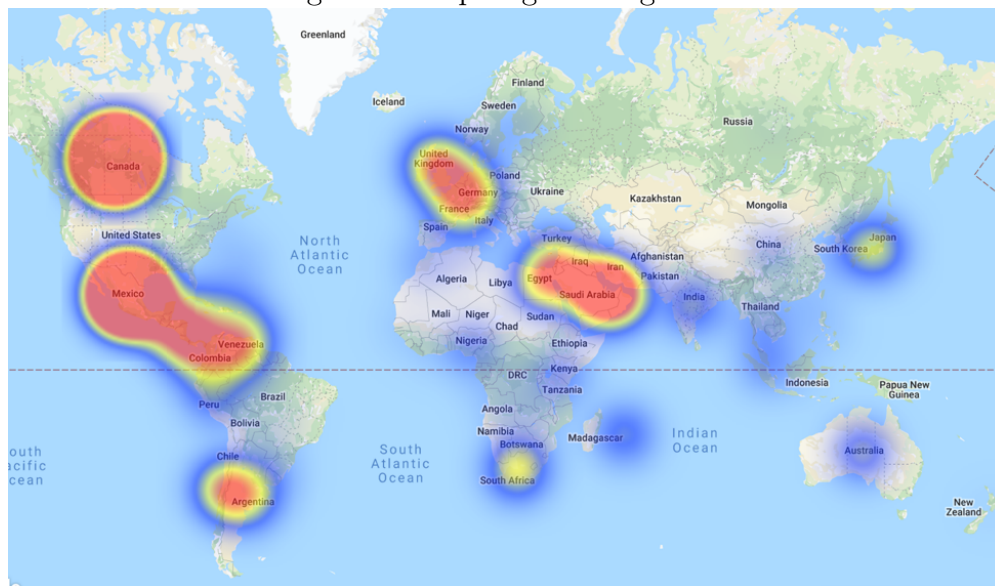
In addition to or in lieu of political risk insurance, firms can also wait to see if there is a dispute with the host state and utilize international law vis-à-vis Investor-State Dispute Settlement (ISDS) or the World Bank's International Centre for Settlement of Investment Disputes (ICSID). They can rely on informal diplomacy and bilateral ties for protection (Wellhausen, 2014). Or, they take advantage of the fact that political risks across different countries are highly uncorrelated⁷ and globally diversify. They may also employ these risk management strategies in conjunction.

3.4 Global diversification and uncorrelated political risks

Does global diversification lead to a “diversification discount”? Firms are increasingly globally diversifying, or expanding their operations to other countries. Of the nearly 5,000 publicly-traded US firms with subsidiaries, more than 1,000 have operations in at least one foreign country. This is puzzling, given that the existing finance literature tells us that there is a “diversification discount,” or that firms that diversify are valued at a discount relative to comparable, un-diversified firms. The literature contends that this discount holds both for industrial and global diversification. I argue that the literature fails to account

⁷There are exceptions. The Arab Spring spread like wildfire regionally and lead to regime change in multiple countries. Regime change can create problems for investors (consider the case of Indonesia). Similarly, political violence can have spillover effects into neighboring countries. Most of the time, however, it does not, and certainly risks are regionally uncorrelated.

Figure 1: Map of global segments



for the fact that firms both reap the benefits of investing in developing countries (lower corporate taxes, looser regulation, cheaper labor and inputs, direct access to a new and possibly untapped market) and hedge against the heightened political risk in those countries by globally diversifying. This is because political risk across different countries is largely uncorrelated. Adverse regime change in Indonesia is highly unlikely to be correlated with political violence in Pakistan. Figure 1 maps the locations of the geographic segments of U.S. public firms.

Further, I argue that the diversification discount (premium) varies by industry. For example, firms in labor-intensive industries, such as manufacturing and construction, have much to gain by diversifying to countries with lower labor costs. In short, contrary to what the literature predicts, we should observe a diversification premium when firms diversify globally. It should be more prominent in some industries than others. Additionally, globalization has decreased the cost to firms of expanding operations to other countries. Importantly, the level of globalization has changed dramatically since the bulk of the papers on this topic were written (the time sample for Denis *et al.* (2002) is 1984-1997). In short, contrary to what the literature predicts, we should observe a diversification premium when firms diversify

globally. It should be more prominent in some industries than others.

4 Research design

I test my theory statistically in three stages. The first stage looks at whether there is a global diversification premium overall, the second looks at whether it varies by industry, and the third incorporates political risk as a mechanism. The unit of analysis is the firm-year. In all models, I use panel OLS with firm and year fixed effects, lagged independent variables (to facilitate causal inference), and a lagged endogenous variable. The sample is all public firms incorporated in the US between 1976–2019, resulting in 350,336 firm-year observations. I restricted my sample to public firms as they are required to file their fundamental information with the SEC, which, in turn, makes this information publicly available.

4.1 Dependent variable

There is much debate in this literature as to the correct dependent variable to measure a firm's market value. Rather than making a strong argument for which is the best, in this paper I demonstrate robustness to multiple measures. Specifically, I utilize market value, or the total dollar value of a firm's outstanding shares of stock, dividend yield, or the ratio of the amount of dividends (distribution of a portion of the company's earnings to shareholders) relative to the market value per share, earnings per share, or the portion of a firm's profit allocated to each outstanding share, and book value on equity (also known as shareholder's equity) , or the money that would be left if all assets of a company were sold and all the liabilities paid off.

4.2 Independent variables & creating a measure a firm's political risk

There is not one consistently accepted way to measure geographic diversification. In fact, a recent review paper found that there are no fewer than 25 different specifications of global diversification in this literature (Hitt *et al.*, 2006). Some consider scale of global diversification through measures such as the ratio of foreign sales to total sales, foreign assets to total assets, or foreign employees to total employees. Others consider how many countries a firm is in or the number of foreign offices. Given that I am interested in measuring the extent to which a firm has uncorrelated political risks, I need to know both the number of countries that the firm has subsidiaries in as well as the relative risks of those countries.

As such, I created a novel measure of a firm's composite political risk (or the collective political risk of all of its geographic segments). To do this, I used detailed geographic segment data from the SEC, which tells me which geographic segments a firm has in a given year, including which countries⁸ the firm has segments in. I then matched each country-year with its respective political risk score from the International Country Risk Guide (ICRG). The ICRG political risk score, which is updated annually, is an average of 12 country-level variables: government stability, socioeconomic conditions, investment profile, internal conflict, external conflict, corruption, military in politics, religion in politics, law and order, ethnic tensions, democratic accountability, and bureaucracy quality. This information is available annually from 1984 on, and as such the third statistical analysis has a restricted sample that does not begin until 1984.

I then created an index of the likelihood that all of a firm's geographic segments would be negatively impacted by political risk simultaneously (within one year). This required two assumptions: (1) that the political risk in every country is independent⁹ and (2) that a

⁸This data also includes geographic diversification across U.S. states, but that is outside the scope of this paper.

⁹As mentioned above, there are exceptions. The Arab Spring spread like wildfire regionally and lead to regime change in multiple countries. Regime change can create problems for investors (consider the case of Indonesia). Similarly, political violence can have spillover effects into neighboring countries. Most of the

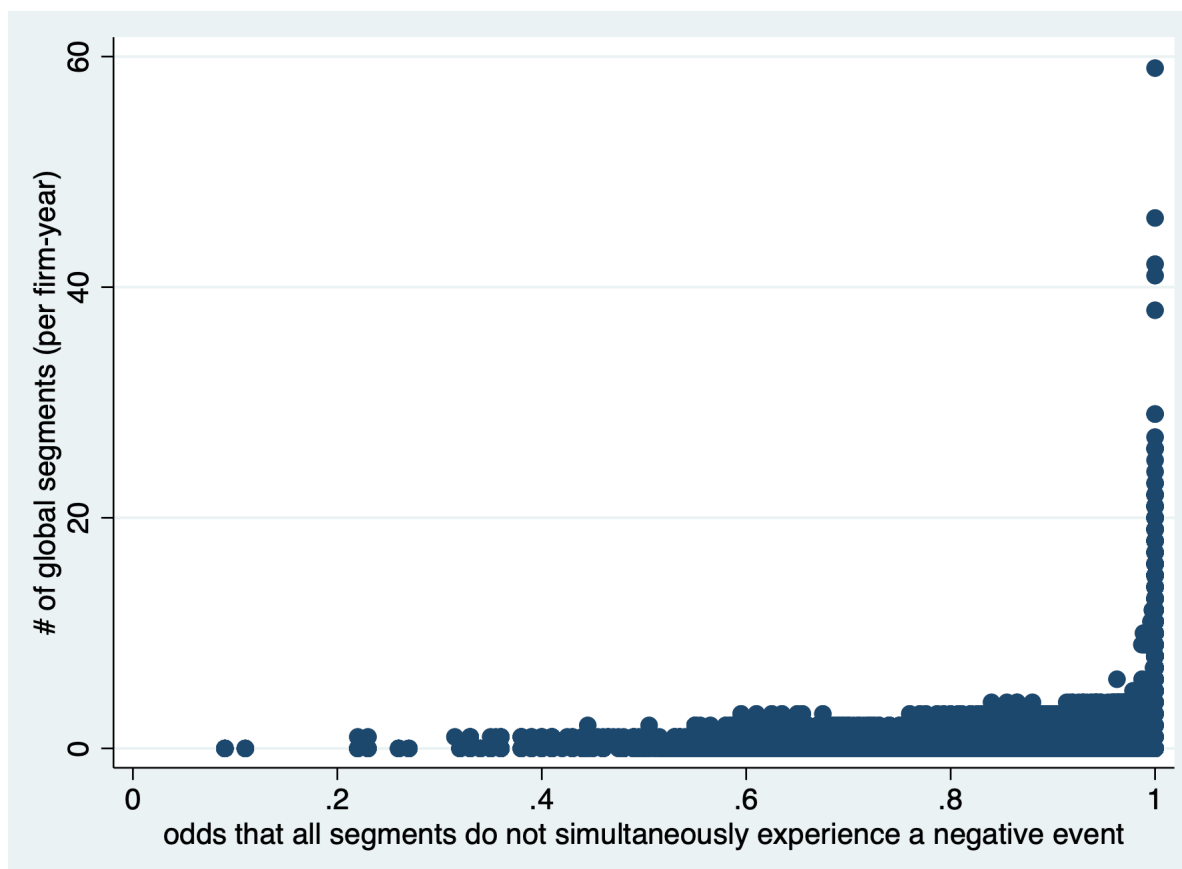


Figure 2: Number of foreign segments vs. probability of simultaneous negative events

country's risk score (1-100, where higher values indicates lower risk) is a proxy for the chance that there will *not* be a negative event. For example, if Venezuela has a risk score of 40 in a given year, that means that there is a 60 percent chance that a negative event will occur, and a 40 percent chance that a negative event will not occur. Using this information and the formula for the compound probability of independent events, I created an index of the likelihood that a firm's geographic segments are not all negatively affected simultaneously. I show this relationship in Figure 2.

However, because the existing literature does not measure global diversification in this way, as their theories are not about political risk diversification, I do not utilize this measure until the third round of statistical analysis. In the first two rounds, I operationalize global diversification to mean the number of foreign segments that a firm has in a given year.

time, however, it does not, and certainly risks are regionally uncorrelated.

4.3 Control variables

In keeping with the current literature, I control for a host of financial variables. First, I control for industrial diversification, because even with firm fixed effects the same firm might diversify industrially diversify throughout the sample and I want to be sure I am picking up the effect of the global diversification. `Industrial diversification` is an indicator variable that takes a 1 if a firm is industrially diversified in a given year. I also control for `current liabilities`, or obligations that a company must pay off within the year, `earnings before interest and taxes (EBIT)`, or operating income, `total assets`, or the sum of all of the book values of the firm's assets, and `Research and development (R&D) expense`, or the money a firm spent to gather new knowledge and use it to improve existing products or introduce new ones. Additionally, I control for `sales/turnover (net)`, or the amount of revenue generated by the firm, `capital expenditures`, or money used for the purchase, improvement, or maintenance of long-term assets, `long-term debt`, or outstanding debt that has a maturity of more than twelve months, and `advertising expense`, or money spent on advertising and promotion. Summary statistics for all variables are available in Table 1.

5 Results

As mentioned above, I test my theory statistically in three stages. The first stage looks at whether there is a global diversification premium overall, the second looks at whether it varies by industry, and the third incorporates political risk as a mechanism. To reiterate, the unit of analysis is the firm-year. In all models, I use panel OLS with firm and year fixed effects, lagged independent variables (to facilitate causal inference), and a lagged endogenous variable.

Table 1: Summary statistics

	count	mean	sd	min	max
Market value	254802	1501.716	11780.79	0	1819782
Dividend yield	256843	.0787288	19.84903	0	9210
Earnings per share	254851	-18.43783	34996.55	-1.09e+07	1.26e+07
Book value of equity	254802	1501.716	11780.79	0	1819782
# Global segments	350336	.329949	1.079757	0	59
Global diversification	322739	.1668624	.3728535	0	1
Industrial diversification	322739	.0169673	.1291489	0	1
Political risk	73699	.8743166	.1166118	.09	1
Current liabilities	254848	309.1995	1970.525	-.002	155729
EBIT	266892	132.3509	958.2384	-25913	71230
Total assets	271244	1749.15	11977.79	0	797769
R&D expense	138386	47.13112	382.7108	-.648	28837
Sales/Turnover (net)	269930	1210.55	7426.557	-12519.5	511729
Capital expenditure	265633	96.17329	642.8119	-401.609	37985
Long-term debt	270661	458.1813	3618.526	-.023	377138
Advertising expense	91210	42.8556	250.333	-1.167	9729
Year	350336	—	—	1976	2019

5.1 Stage 1: Overall relationship

The first stage of statistical analysis seeks to answer two questions: (1) among all firms, including those that are not diversified in any way (i.e. that are not industrially, domestically or globally diversified), is there a global diversification discount? (2) Among only firms that are diversified in some way (industrially, domestically, and/or globally), is there a diversification discount? To answer these questions, I present eight models on four dependent variables: market value, dividend yield, earnings per share, and book value on equity (as described above, there is debate among which measure of firm valuation is the best, so I check for robustness to multiple measures). For each dependent variable, there are two models: the first tests the impact of global diversification on all firms, and the second tests the impact of global diversification only on firms that are diversified in some way. As mentioned above, the sample is public US firms. I operationalize global diversification to mean the number of

foreign segments that a firm has in a given year.¹⁰ I present the results of these models in Table 2. As you can see, `global diversification` is positive and significant in all models, albeit to varying degrees. This confirms my theory that there is something about the extent of global diversification that matters for a firm's market value.

¹⁰This is different from the number of unique countries a firm has segments in in a given year, but the results are robust to that specification.

Table 2: Overall number of foreign segments

	(1) Market value All	(2) Market value Diversified	(3) Dividend yield All	(4) Dividend yield Diversified	(5) Earnings per share All	(6) Earnings per share Diversified	(7) Book value (equity) All	(8) Book value (equity) Diversified
# Global segments	161.684*** (47.017)	163.729** (50.128)	0.001* (0.001)	0.001* (0.000)	3.686* (1.819)	3.788† (1.944)	161.684*** (47.017)	163.729** (50.128)
Industrial div.	1379.468*** (400.830)	1398.151** (432.471)	0.002 (0.005)	0.001 (0.004)	-0.852 (15.607)	-0.959 (16.868)	1379.468*** (400.830)	1398.151** (432.471)
Current liabilities	0.407*** (0.047)	0.409*** (0.050)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.002)	0.000 (0.002)	0.407*** (0.047)	0.409*** (0.050)
EBIT	1.070*** (0.099)	1.070*** (0.105)	0.000 (0.000)	0.000 (0.000)	0.001 (0.003)	0.001 (0.004)	1.070*** (0.099)	1.070*** (0.105)
Total assets	-0.178*** (0.024)	-0.173*** (0.025)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.001)	-0.000 (0.001)	-0.178*** (0.024)	-0.173*** (0.025)
R&D expense	5.856*** (0.197)	5.856*** (0.209)	0.000 (0.000)	0.000 (0.000)	0.000 (0.007)	0.000 (0.008)	5.856*** (0.197)	5.856*** (0.209)
Sales/Turnover (net)	0.072*** (0.012)	0.067*** (0.014)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.001)	0.072*** (0.012)	0.067*** (0.014)
Capital expenditures	-0.831*** (0.188)	-0.821*** (0.201)	-0.000 (0.000)	-0.000 (0.000)	-0.001 (0.007)	-0.001 (0.008)	-0.831*** (0.188)	-0.821*** (0.201)
Long-term debt	-0.081 (0.051)	-0.092† (0.054)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.002)	-0.000 (0.002)	-0.081 (0.051)	-0.092† (0.054)
Advertising expense	2.357*** (0.389)	2.378*** (0.413)	-0.000 (0.000)	-0.000 (0.000)	0.002 (0.015)	0.002 (0.016)	2.357*** (0.389)	2.378*** (0.413)
Constant	219.708 (337.821)	262.608 (415.113)	0.018*** (0.004)	0.020*** (0.004)	10.368 (11.250)	13.883 (14.071)	219.708 (337.821)	262.608 (415.113)
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Lagged endog. var.	✓	✓	✓	✓	✓	✓	✓	✓
N	43018	38390	43076	38397	49209	43880	43018	38390

Standard errors in parentheses

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: SIC divisions

0100-0999	Agriculture, Forestry and Fishing
1000-1499	Mining
1500-1799	Construction
2000-3999	Manufacturing
4000-4999	Transportation, Communications, Electric, Gas and Sanitary service
5000-5199	Wholesale Trade
5200-5999	Retail Trade
6000-6799	Finance, Insurance and Real Estate
7000-8999	Services
9100-9729	Public Administration
9900-9999	Nonclassifiable

5.2 Stage 2: Sectoral variation

In the second stage, I look to see how the extent of global diversification varies by sector. My theory suggests that industries that have more to gain from diversifying to developing countries, such as access to cheap land, labor and inputs, should benefit more global diversification than others. As such, I expect global diversification to be particularly valuable for both the manufacturing and construction industries. I use the Standard Industrial Classification (SIC) system for classifying industries simply because it is what the SEC uses, eliminating any missing data issues. The SIC codes are four digits, and can be broken into eleven divisions, as seen in Table 3. Given space constraints, I use only the first word of each division in the models. Additionally, given a small sample size, I eliminated Public Administration. I also eliminated Nonclassifiable as it not clear what we could learn theoretically from that sample.

Table 4 shows the full results across sectors and Figure 3 shows a coefficient plot of the independent variable of interest, which again is the extent of global diversification, measured as the number of global segments a firm has in a given year. Given space constraints I use `market value` as the dependent variable, but I demonstrate robustness to other dependent variables in the appendix (see, for example, Table 6 and Figure 5). To maintain a reasonable sample size, the models look at all firms together, both diversified and undiversified. As is

clear from both the table and the plot, the effect of global diversification varies by sector. Specifically, as my theory predicted, it has a positive and significant impact on manufacturing and construction. This makes sense given the inputs, land and labor required in these industries.

Table 4: Effect of global diversification varies by sector

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Agriculture	Mining	Construction	Manufacturing	Transport	Wholesale trade	Resale trade	Finance	Services
# Global segments	1140.183 (1238.037)	-256.739 ⁺ (143.882)	159.580*** (41.655)	84.412** (26.416)	-1059.183** (330.847)	-90.486* (39.822)	329.820 (560.867)	26.663 (48.250)	68.029 (102.408)
Industrial div.	0.000 (.)	532.003 (570.794)	0.000 (.)	795.410** (247.995)	29549.509*** (3887.556)	292.676* (142.377)	-1155.024 (1950.002)	1.858 (785.740)	5034.168*** (964.128)
Current liabilities	-7.614 (4.834)	15.191*** (0.754)	-0.134 (0.306)	-0.460*** (0.059)	0.263 (0.198)	-0.163 (0.157)	4.377*** (0.752)	0.883* (0.395)	1.977*** (0.158)
EBIT	-36.515* (15.183)	-0.813* (0.346)	0.752 (0.906)	1.263*** (0.085)	-1.009** (0.387)	2.285*** (0.548)	2.536* (1.016)	1.494*** (0.406)	-0.684* (0.307)
Total assets	8.257 (4.995)	-1.663*** (0.227)	0.246 (0.465)	0.163*** (0.019)	-0.188** (0.070)	0.208** (0.070)	-0.087 (0.280)	-0.427** (0.135)	0.803*** (0.067)
R&D expense	-54.250 (32.671)	-117.863*** (6.661)	35.588 (26.094)	-0.101 (0.140)	7.610** (2.525)	-3.001 (4.935)	11.363*** (1.629)	-0.251 (2.212)	8.216*** (1.004)
Sales/Turnover (net)	11.275** (4.094)	-0.997*** (0.036)	0.148 (0.266)	0.114*** (0.023)	-1.208*** (0.213)	-0.087** (0.030)	-0.495*** (0.126)	0.387*** (0.091)	-0.768*** (0.163)
Capital expenditures	10.052 (21.929)	11.470*** (0.547)	-1.276 (0.817)	-0.431** (0.137)	-0.135 (0.798)	-4.680*** (0.649)	-2.366* (1.097)	-0.098 (1.425)	9.638*** (0.559)
Long-term debt	0.163 (1.139)	0.120 (0.387)	-0.177 (0.716)	-0.346*** (0.039)	0.418* (0.185)	0.030 (0.103)	-0.027 (0.411)	1.266*** (0.300)	-0.637*** (0.109)
Advertising expense	-587.898* (269.687)	38.976 (28.490)	-18.895 ⁺ (10.412)	0.609** (0.234)	32.696*** (2.948)	2.297 ⁺ (1.191)	-8.691** (3.333)	-6.770** (2.224)	5.375** (1.771)
Constant	-4914.454 (4645.825)	-439.951 (271.017)	117.413** (32.181)	-333.929 (203.205)	3754.940 (2471.774)	23.480 (103.693)	-452.182 (1814.074)	38.210 (122.973)	556.668 (1076.204)
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Lagged endog. var.	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	119	476	89	24197	756	1175	6900	1025	7871

Standard errors in parentheses

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.3 Stage 3: Parsing the mechanism

In the third stage I look for evidence of the political risk mechanism I proposed earlier in the paper. As a reminder, the basic principle is that political risks across countries are uncorrelated, and, as such, it benefits investors to be diversified across multiple national markets. I measure this using the political risk diversification index described above interacted with the number of the global segments a firm has in a given year (the independent variable used in the previous models). As in the second stage, I test this looking at all firms (both diversified and undiversified). Again, I use market value and assess variation by sector. The results of this model are presented in Table 5. As my theory suggested, the interaction is positive and significant for manufacturing. Though future research should pursue this further, this provides initial evidence for the political risk mechanism.

Figure 3: Effect of global diversification varies by sector

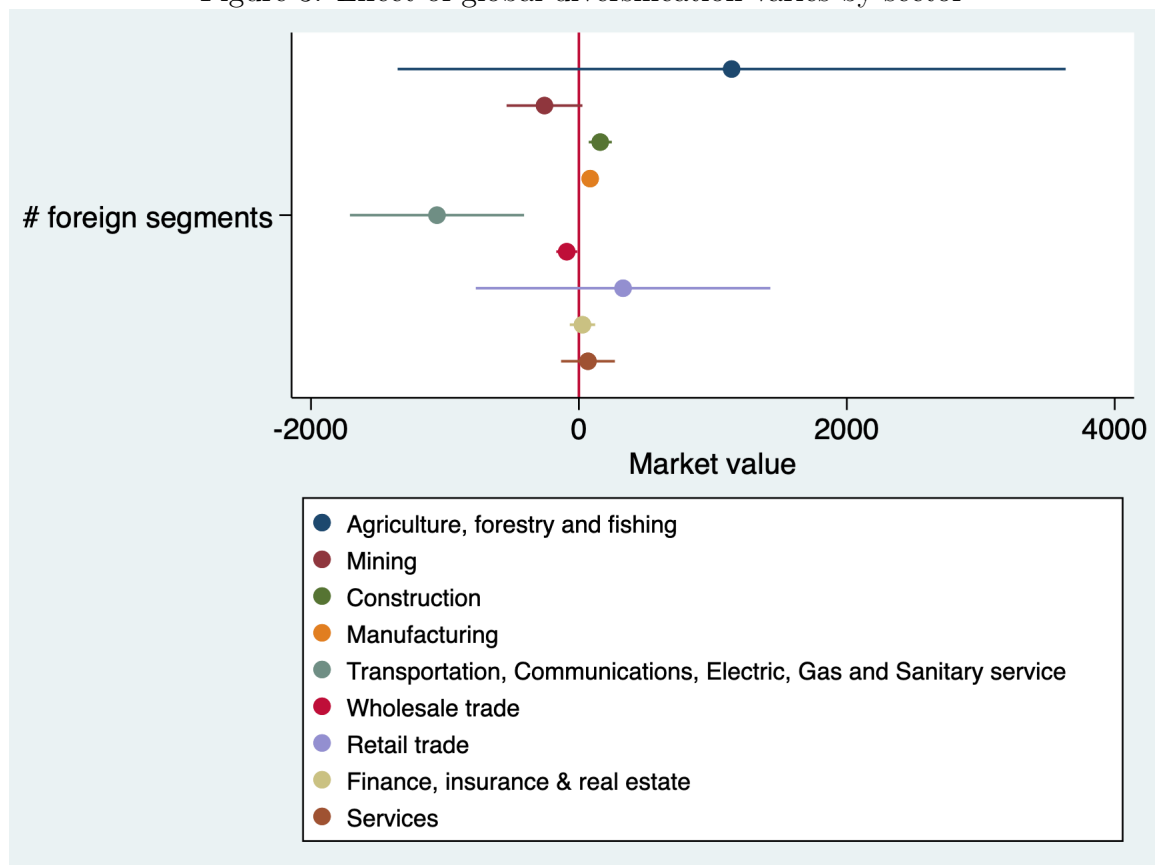


Table 5: Interaction table

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Agriculture	Mining	Manufacturing	Transport	Wholesale trade	Retail trade	Finance	Service
Political risk	6073.023 (25246.795)	-483916.423 (171181.854)	-117418.608*** (14166.203)	-177.393 (5926.137)	-43689.391 (64258.425)	-68972.938* (29759.257)	543.329 (659.422)	18607.706 (12763.469)
Global div.	0.000 (.)	0.000 (.)	-109390.745*** (13164.134)	0.000 (.)	0.000 (.)	-65395.220* (27816.701)	0.000 (.)	13210.429 (11388.432)
Political risk * Global div.	0.000 (.)	483254.305 (170526.759)	118980.732*** (14230.506)	0.000 (.)	42049.084 (64275.258)	77474.327* (30164.122)	0.000 (.)	-16820.263 (13195.495)
Industrial div.	0.000 (.)	62009.813 (27053.658)	2673.376*** (788.439)	-1025.731 (1703.177)	1206.772* (474.290)	11902.668*** (2364.385)	0.000 (.)	-191.166 (2004.194)
Current liabilities	2.834 (6.536)	-1.521 (4.003)	-0.187 (0.165)	-1.610 (1.079)	-0.500 (0.853)	-3.470*** (0.937)	-5.353*** (1.225)	7.268*** (0.372)
EBIT	-17.141 (23.495)	5.464 (1.560)	1.750*** (0.193)	10.599** (3.362)	5.773* (2.649)	8.849*** (1.019)	-10.303*** (2.284)	1.684* (0.705)
Total assets	13.687 (11.030)	-0.281 (1.347)	0.231*** (0.055)	0.231 (0.948)	0.684 (0.543)	0.536 (0.526)	2.860** (0.916)	-1.414*** (0.178)
R &D expense	-44.212 (48.100)	179.287 (162.863)	-1.341*** (0.395)	-112.531+ (56.369)	-26.797 (55.290)	44.192*** (1.537)	-47.743*** (11.613)	18.126*** (2.407)
Sales/Turnover (net)	-5.235 (7.599)	-1.605 (1.132)	0.171** (0.055)	-0.982* (0.470)	-0.058 (0.328)	-0.255 (0.166)	0.294 (0.858)	-0.633+ (0.355)
Capital expenditures	11.534 (43.656)	6.489 (1.727)	-0.498 (0.419)	-0.330 (2.931)	-17.410*** (3.851)	-2.875* (1.377)	8.207 (5.899)	15.770*** (1.413)
Long-term debt	-1.149 (1.874)	-3.977 (4.691)	-0.648*** (0.090)	-1.097 (1.264)	0.020 (0.479)	-2.137*** (0.517)	-0.228 (2.176)	0.846*** (0.232)
Advertising expense	-359.916 (425.928)	313.781 (181.299)	1.950+ (1.022)	-22.443 (21.046)	21.660* (9.462)	-0.074 (3.244)	-5.177 (6.095)	-29.817*** (3.792)
Constant	-39103.660 (39734.349)	83167.067 (46227.560)	107481.500*** (13103.812)	35377.188** (10223.453)	2173.844 (1958.326)	65057.591* (27676.769)	127.655 (636.575)	-14710.401 (10993.280)
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Lagged endog. var.	✓	✓	✓	✓	✓	✓	✓	✓
<i>N</i>	54	55	6568	89	153	477	90	1756

Standard errors in parentheses

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6 Robustness checks

As [Denis *et al.* \(2002\)](#) point out, firm value and the decision to diversify could be endogenously related in some way. Specifically, firm value and the decision to diversify could be inherent to firm-specific factors. The firm fixed effects and the lagged endogenous model used in the models should alleviate this concern. Another concern is the imputed value method used by [Denis *et al.* \(2002\)](#), as well as others in this literature. [Graham *et al.* \(1999\)](#) show that the imputed value method leads to selection bias. This is not a problem in my study as I do not use this method and I confirm robustness to multiple dependent variables. Finally, many problems have been pointed out with regard to the excess value dependent variable originally posed by [Berger & Ofek \(1995\)](#). Studies have found it to be biased downwards because it only captures shareholder value, not firm value. Again, this is not a problem in my study because I do not use this as the dependent variable.

7 Case: Coca-Cola

Coca-Cola, one of the most recognized brands in the world, is in many ways a poster child for the strategy of global diversification ([Frynas & Mellahi, 2015](#)). It has had tremendous success in emerging markets ([Damodaran, 2005](#)) and has segments in dozens of countries, including Angola, Kenya, Pakistan, the Philippines, Vietnam, India, China, Turkey, Zimbabwe, Ghana, Bangladesh, Saudi Arabia, Egypt, Morocco, Tajikistan, Mozambique, Laos, Myanmar, Papua New Guinea, Moldova, Azerbaijan, Mongolia, Kosovo, Afghanistan, and Serbia. Many of the countries it has segments in have high levels of political risk, yet Coca-Cola continues to perform well. In fact, more than 75 percent of its revenue comes from abroad ([DuBrin, 2015](#)). It began expanding more aggressively in the 1990s after facing a relatively flat domestic soft drink market ([Brigham & Houston, 2019](#)). Most recently, Coca-Cola has outperformed other beverage companies during the COVID-19 pandemic due, in part, to its aggressive global diversification strategy ([Hyett, 2020](#)). While, by definition, a

pandemic is widespread, COVID-19 has affected some countries more intensely than others, and government responses have varied considerably. For example, in countries where all restaurants and bars are shut down, Coca-Cola has suffered, but because responses have varied, Coca-Cola has not suffered everywhere.

It is important to note that I am not arguing that Coca-Cola is succeeding simply because of this global diversification strategy. Clearly, their success in each individual country is due to a range of factors including tax incentives, price and availability of labor and land, infrastructure, market penetration, domestic economic growth, and clever marketing strategies, such as using soccer to advertise in South Africa ([Deloitte, 2013](#)). However, global diversification helps to minimize the impact of each individual political risk that Coca-Cola faces as a result of the emerging economies it operates in. Coca-Cola does explicitly reference politics as a factor in many of its decisions to expand. For example, when talking about a new segment in Kenya, Ahmet Bozer, the company's Eurasia-Africa group president, said "[Kenya's] steady rate of economic growth, well-educated population and efforts by the Government in reform and infrastructure upgrade over the last six years have given us confidence and optimism about doing business here."¹¹ Then-CEO Muhtar Kent made a similar comment about governance when discussing new operations in the Philippines ([Remo, 2015](#)).

What is perhaps most interesting about Coca-Cola's overall success is that, in reality, many of the political risks it chose to incur did materialize into negative events. Importantly, and in line with the premise of this paper, is that these negative events were uncorrelated. For example, Coca-Cola has dealt with supply chain disruptions (or near disruptions), such as when its sugar supply from Venezuela was threatened in 2016 ([Flannery, 2016](#)) or when U.S. sanctions on Sudan did not originally include an exemption for gum arabic, the emulsifying ingredient in soft drinks that keeps sugar from sinking to the bottom and is produced almost entirely in Sudan ([Allison, 2013](#)). It has also faced regulatory challenges. One example that become famous in international business textbooks is Coca-Cola's struggles in India in the

¹¹[Bruce \(2011\)](#)

1970s, when a statute change would have required Coca-Cola to transfer 60 percent of its equity to an Indian company (Gopinath & Prasad, 2013). It refused to do this and left the country in 1977 (Chakravarti, 1991), returning in 1993 . Ironically, Coca-Cola again faced regulatory challenges in India recently with a Nepalese-Indian trade blockade that cost Coca-Cola over \$1 million dollars (which has since turned into a legal battle with its political risk insurers, who refused to cover the event) (Frost, 2017). Coca-Cola also faced revolutionary expropriation in China in 1949 (Chong, 2013).

Coca-Cola has also dealt with political risks relating to regime change. For example, in Uzbekistan, Coca-Cola partnered with the son-in-law of president Islam Karimov and made him the president of its bottling company. However, when the son-in-law split from Karimov's daughter, the government forced the bottling company to shut down operations for 18 months and launched an investigation into it (Shenkar & Luo, 2008). The company is also no stranger to political violence. It has entered markets with ongoing political violence, entered markets that later experienced political violence, been the direct target of violence, and been accused of perpetrating violence. In terms of entering markets with ongoing violence, Coca-Cola entered Angola in 2000 in the midst of civil unrest.¹² Typically when companies do this they choose a location that is far from the violence, but in this case there was fighting quite near the plant (Rice & Zegart, 2018). Similarly, it entered Myanmar in 2011, partnering with a local firm that was sanctioned in the US and located directly in the midst of the unrest (Oetzel & Miklian, 2017; Peel, 2015). Ukraine in 2014 is one example of Coca-Cola's profits suffering due to political violence in a country it was already operating in (Bias, 2013). As for being the direct subject of political violence, in 2001, Muslim rebels bombed an Indian Coca-Cola plant and Maoist guerrillas in Nepal bombed Coca-Cola facilities in Kathmandu (Shenkar & Luo, 2008; Dugger, 2001; Press, 2001). Protesters have also attacked Coca-Cola facilities multiple times in Mexico (Press, 2015). The company has also dealt with lawsuits and protests in India over concerns of water shortage (Raman, 2005).

¹²They may have done this because it was a low-value/high vulnerability asset (Rice & Zegart, 2018).

It has also faced claims of perpetrating violence, particularly in Guatemala ([McWilliams, 2010](#)) and Colombia ([Gill, 2016](#)). Similarly, Coca-Cola has faced an ongoing boycott due to alleged support of Israel. The website Innovative Minds, a pro-Palestine website, claims that Israel has been staunch supporter of Israel since 1966, for reasons including “Coca-Cola’s co-hosting of a reception in Atlanta for Israeli Brigadier-General Binyamin Ben-Eliezer in 2009, ties with the American-Israel Chamber of Commerce in Atlanta, and because a subsidiary reportedly owns a dairy farm in occupied territory” ([Timmons, 2014](#)). Additionally, political risk can entail social and cultural issues, which Coca-Cola has also run up against. For example, at one point a rumor spread in Egypt that if you invert Coca-Cola’s label it spells “No Mohammed, Mo Mecca” in Arabic (see [Figure 4](#)). This became a big enough problem for profits that Coca-Cola had to have Egypt’s Mufti (top religious expert) issue a decree saying that the logo did not, in fact, defame Islam ([Shenkar & Luo, 2008](#); [Minato, 2012](#)).

Figure 4: Coca-Cola’s logo controversy ([Minato, 2012](#))



Despite all of these negative events, Coca-Cola continues to succeed. This suggests that it is possible to profitably engage in risky countries, so long as you diversify your risks. Conveniently, political risks across countries are largely uncorrelated. Global diversification also protects against more than political risk. It has also helped to protect Coca-Cola against

the cultural shift away from sugary beverages in Western countries (Salmon, n.d.). This has also been a problem on an international scale—for example, in 2009, Venezuela’s health minister banned Coke Zero in order to “preserve the health of Venezuelans” (Rice & Zegart, 2018). Global diversification also protects against domestic economic downturns. When Coca-Cola’s performance in Greece suffered due to austerity measures, the company overall was resilient due to its diversification (Bias, 2013). In short, global diversification has only enhanced Coca-Cola’s market value, as the theory suggests.

8 Conclusion

This paper addressed the global diversification discount. I argued that the literature fails to account for the fact that firms both reap the benefits of investing in developing countries (lower corporate taxes, looser regulation, cheaper labor and inputs, direct access to a new and possibly untapped market) and hedge against the heightened political risk in those countries by globally diversifying. This is because political risk across different countries is largely uncorrelated. Further, I argued that the diversification discount (premium) varies by industry. I found that firms in labor-intensive industries, such as manufacturing and construction, have much to gain by diversifying to countries with lower labor costs.

I tested my theory using data on all publicly-traded US firms. I found that the extent of their global diversification, or the number of countries they have expanded to, affects the extent to which they are valued relative to the market. I then showed that this effect varies across industries and offered evidence of the mechanism, both through creating a political risk index and walking through the example of Coca-Cola. This paper makes a series of important contributions. First, it overturns a key finding in the literature, which has implications both for academic debate but also for the firms themselves. Second, it incorporates twenty new years of data, which is important given that the world has continued to globalize in dramatic fashion since the seminal studies on this topic were published in the early 2000s. Third, it

overcomes methodological challenges related to endogeneity, the imputed-value method, and the excess value measure. Fourth, it introduces a new measure of firm exposure to political risk.

Opportunities for future research include seeing if this finding also holds for firms from other countries (my theory would suggest that it should), testing whether existing bilateral financial ties between the US and other countries condition the relationship, and testing whether there are threshold effects on the type of political risk companies will tolerate.

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9 Appendix

Table 6: Robustness check: sectors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Agriculture	Mining	Construction	Manufacturing	Transport	Wholesale trade	Resale trade	Finance	Services
# Global segments	1140.183 (1238.037)	-256.739 ⁺ (143.882)	159.580*** (41.655)	84.412** (26.416)	-1059.183** (330.847)	-90.486* (39.822)	329.820 (560.867)	26.663 (48.250)	68.029 (102.408)
Industrial div.	0.000 (.)	532.003 (570.794)	0.000 (.)	795.410** (247.995)	29549.509*** (3887.556)	292.676* (142.377)	-1155.024 (1950.002)	1.858 (785.740)	5034.168*** (964.128)
Current liabilities	-7.614 (4.834)	15.191*** (0.754)	-0.134 (0.306)	-0.460*** (0.059)	0.263 (0.198)	-0.163 (0.157)	4.377*** (0.752)	0.883* (0.395)	1.977*** (0.158)
EBIT	-36.515* (15.183)	-0.813* (0.346)	0.752 (0.906)	1.263*** (0.085)	-1.009** (0.387)	2.285*** (0.548)	2.536* (1.016)	1.494*** (0.406)	-0.684* (0.307)
Total assets	8.257 (4.995)	-1.663*** (0.227)	0.246 (0.465)	0.163*** (0.019)	-0.188** (0.070)	0.208** (0.070)	-0.087 (0.280)	-0.427** (0.135)	0.803*** (0.067)
R&D expense	-54.250 (32.671)	-117.863*** (6.661)	35.588 (26.094)	-0.101 (0.140)	7.610** (2.525)	-3.001 (4.935)	11.363*** (1.629)	-0.251 (2.212)	8.216*** (1.004)
Sales/Turnover (net)	11.275** (4.094)	-0.997*** (0.036)	0.148 (0.266)	0.114*** (0.023)	-1.208*** (0.213)	-0.087** (0.030)	-0.495*** (0.126)	0.387*** (0.091)	-0.768*** (0.163)
Capital expenditure	10.052 (21.929)	11.470*** (0.547)	-1.276 (0.817)	-0.431** (0.137)	-0.135 (0.798)	-4.680*** (0.649)	-2.366* (1.097)	-0.098 (1.425)	9.638*** (0.559)
Long-term debt	0.163 (1.139)	0.120 (0.387)	-0.177 (0.716)	-0.346*** (0.039)	0.418* (0.185)	0.030 (0.103)	-0.027 (0.411)	1.266*** (0.300)	-0.637*** (0.109)
Advertising expense	-587.898* (269.687)	38.976 (28.490)	-18.895 ⁺ (10.412)	0.609** (0.234)	32.696*** (2.948)	2.297 ⁺ (1.191)	-8.691** (3.333)	-6.770* (2.224)	5.375** (1.771)
Constant	-4914.454 (4645.825)	-439.951 (271.017)	117.413** (32.181)	-333.929 (203.205)	3754.940 (2471.774)	23.480 (103.693)	-452.182 (1814.074)	38.210 (122.973)	556.668 (1076.204)
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Lagged endog. var.	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	119	476	89	24197	756	1175	6900	1025	7871

Standard errors in parentheses

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

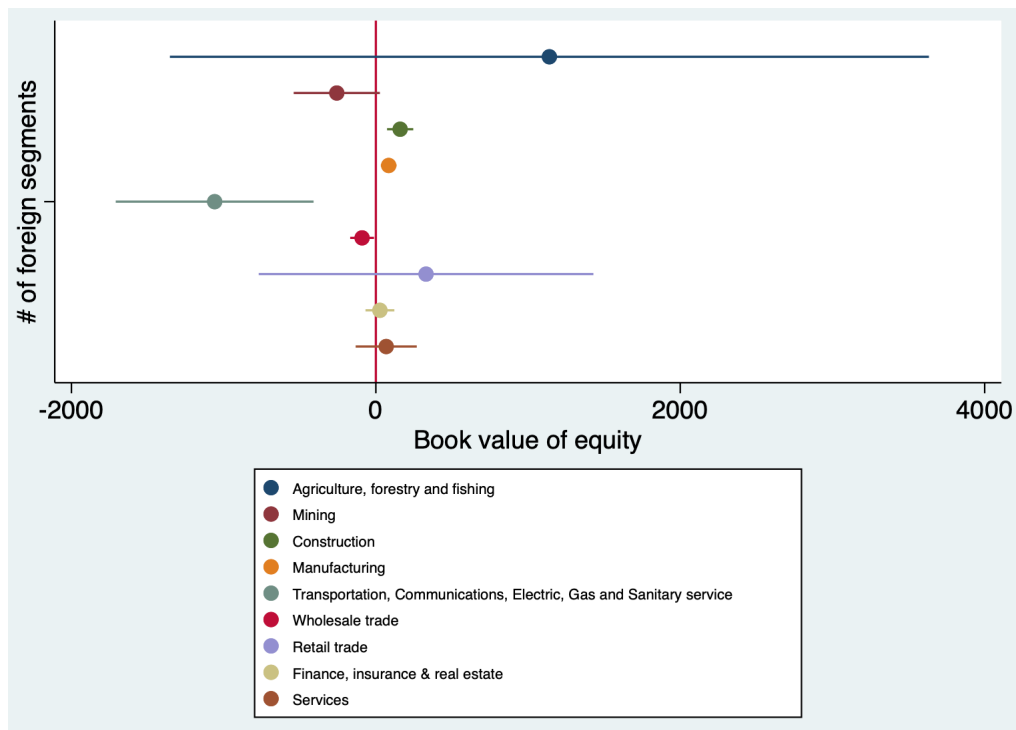


Figure 5: Robustness: Sectors (book value of equity)