Does conflict deter capital?
The subnational political economy of political violence and foreign direct investment

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Abstract

What are the consequences of political violence for foreign direct investment (FDI)? The conventional wisdom in political science suggests that political violence inhibits foreign investment in the developing world. However, the existing empirical evidence is mixed. I argue that firms do invest in conflict-prone countries, but only in areas not affected by the conflict and in sectors where they have the opportunity to secure prospective monopolies. Analyzing FDI to 130 countries from 2003–2017, I use spatial autoregression to confirm that political violence deters FDI at the subnational level. I then relax the sectoral homogeneity assumption and show that investors in industries that are prospective monopolies (such as such as utilities, telecommunications, and logistics) are not deterred by violence. Building on work in business and economics, these findings shed new light on the relationship between conflict and capital, signaling a need to update our theories to account for both subnational variation and sectoral variation.

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1 Introduction

The conventional wisdom in political science and economics tells us that violence is bad for business. There are strong theoretical reasons to believe that this would be the case: 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. However, existing theory does not seem to correctly predict the empirical reality: Pakistan, Sudan, Colombia, and other countries we tend to associate with political violence receive plenty of investment. 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 excluded[1] from the graph, there is a lot of interesting variation in the middle.

It is unsurprising, then, that existing empirical studies on this topic struggle to find a statistically significant relationship between political violence and foreign direct investment (FDI).[2] I argue that this is, in part, because existing explanations are focusing on the wrong geographic unit. To understand the relationship between political violence and FDI, we need to look beyond the country level and focus on subnational variation.

There are many variables that the existing literature has already identified that do affect FDI location choice and also vary at the country level. These include, but are not limited to, regime type (Li & Resnick, 2003; Jensen, 2003), respect for property

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

[2]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); Li (2006)
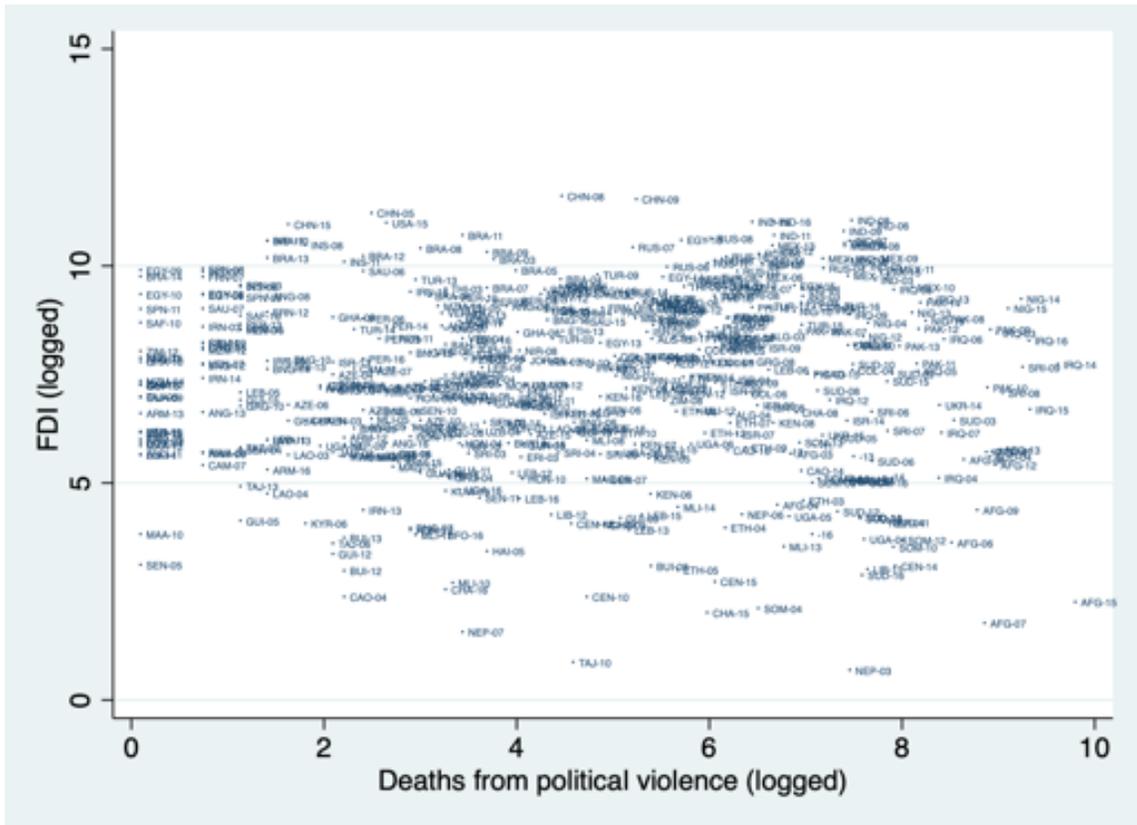
Figure 1: Cases in which there is both violence and investment

rights (Wellhausen, 2014), regulatory standards (Pandya, 2013), bilateral investment treaties (Sachs & Sauvant, 2009; Swenson, 2005; Egger & Merlo, 2007; Busse *et al.* , 2010; Yackee, 2008; Berger *et al.* , 2011; Haftel, 2010), preferential trade agreements (Büthe & Milner, 2014, 2008), corporate taxation, size of the market, and cost of labor. In previous studies, political violence, too, has been studied as an independent variable at the country level (Jensen & Young, 2008; Li, 2006).

However, I argue that firms do not consider political violence as an FDI location determinant at the country level. They look to see specifically where the violence is and then invest elsewhere within the country. There are many empirical illustrations of this: there was plenty of investment in Algeria, Colombia and Sri Lanka during each of their respective civil wars. The investment simply was not where the war was. A quick glance at Figure 2, a heat map of both FDI and political violence, demonstrates the point that violence and investment are not in the same place within the country. This variation is obscured in country-year studies.

This has important implications for firms because if many of the desirable attributes of developing countries, for investment purposes, vary at the country level, but political violence varies at the subnational level, then firms can still reap the benefits of investing in these countries (low corporate taxation, cheap and ample labor, etc.) but they can avoid violence, if that is in fact what they are looking to do.

Subnational geography is not the only the only predictor of the relationship between conflict and investment. The relationship between the two also varies with market structure. 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
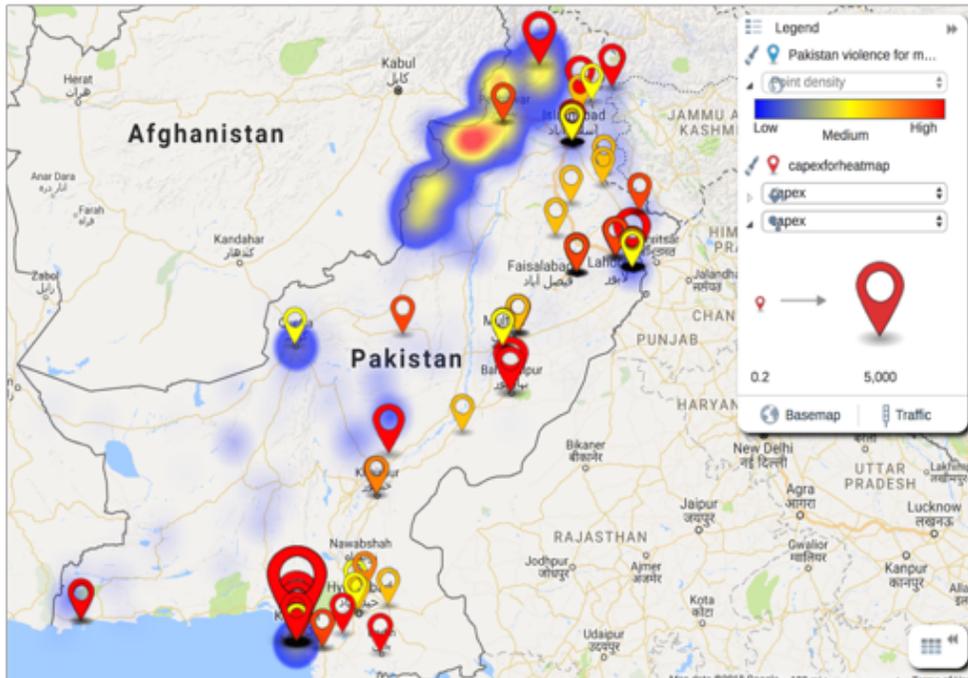
Figure 2: Heat map of FDI and violence in Pakistan

these markets, conflict could even increase opportunities for profit.

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 reconstruction. 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

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of conflict on capital flows.

I proceed as follows: After outlining existing work, I present a theory of why subnational geography matters, arguing that there are certain geographies that will allow firms to reap country-level benefits regardless of conflict. I then present my research design, test my theory empirically, 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 where firms choose to place their non-site specific investments.

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). 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]

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[3]See, among others, Li & Resnick (2003); Jensen (2003); Pandya (2016)
[4]See, among other, Büthe & Milner (2014, 2008); Elkins *et al.* (2006)
[5]Pandya (2013)
[6]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.[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 civil wars, but we do. We would not expect to observe new electricity infrastructure appearing in Afghanistan

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[7]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); Li (2006)

or South Sudan, but again, we do. 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 where they are is profit maximization, which drives any competitive firm's behavior.

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 Nigeria[8]) 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

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[8]Howden (2006); Reuters (2016)

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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 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.

However, the risks are high: investments may be destroyed, supply chains inter-

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

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

rupted, personnel harmed, laws changed or assets sized 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.

3.3   Location determinants that vary at the country level

I argue that firms do not consider political violence as an FDI location determinant at the country level. They look to see specifically where the violence is and then invest elsewhere within the country. This partially explains why we continue to see so much investment during civil wars. The investment is simply not where the war is taking place. This also explains why we continue to find a null result at the country-year level: this variation is obscured in aggregate studies.

The logic behind this is fairly intuitive: firms are interested in taking advantage of many of the desirable investment properties of developing countries. Developing countries often offer things like abundant and inexpensive labor, loose regulation, low corporate taxation, etc. Importantly, these variables vary at the country level. This means that as long as the firm invests somewhere in the country, they can take advantage of these benefits. As such, firms go to the country, but go the safest place in the country.

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[11]Hersher (2016)
[12]Howden (2006); Reuters (2016)
[13]Alderman *et al.* (2018)

3.4 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 is 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, 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. 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

Like the theory, the research design has two parts. I first use a large-N empirical analysis to test whether or not political violence impacts FDI on the town level. I

then take the analysis one step further and test to see whether political violence impacts FDI on the town and sector level. In both cases I use spatial autoregression to account for spatial autocorrelation as well as country and year fixed effects. The country fixed effects hold everything else about the country constant and the year fixed effects control for global macroeconomic trends. My spatial models account for 5069 towns in the developing world.

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 the second part of the theory argues that the relationship between conflict and capital is not dependent only on subnational geography but also on market structure, I also 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 countries[14] between 2003 and 2017.[15] This yields 76,035 observations per model. 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.

In all models I use a lagged endogenous variable. In the model of prospective monopolies, I also control for FDI to other industries. In order to avoid selecting on the dependent variable, I include all country-years, even those in which there is no

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[14]I operationalize this as countries that are not classified as "high income" by the World Bank.
[15]These are the only years available from FDImarkets.

investment.

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 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 five years prior to the investment, and then include one year of lag time to account for the time between the final investment decision and the actual implementation of the investment. [17] My theory first predicts that `political violence` will have a *negative* and significant impact on FDI at the subnational level. However, it also predicts that `political violence` will have a *positive* and significant impact on FDI at the subnational level

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

[17]For robustness, I also

when we look only at FDI to prospective monopolies.

4.1   The need for spatial autoregression

Many theories in international political economy (IPE) predict spatial clustering of outcomes around neighboring units of observation. For instance, financial policies diffuse to neighboring countries (Brooks & Kurtz, 2012; Bodea & Hicks, 2015), investors evaluate sovereign borrowers based not only on the country in question but also based on its peer group (Brooks *et al.* , 2015), and countries may be more likely to trade with or invest in neighboring countries. In short, many of our theories suggest that spatially proximate countries (or units) are more likely to behave similarly than spatially distant units (Darmofal, 2006; Huckfeldt, 1986; Vasquez, 1995; Cardoso & Enzo, 1979; Berry & Berry, 1990). However, whether scholars of IPE explicitly or implicitly recognize the importance of space in their theories, we do not always take space seriously in our empirics. 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) 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).

Much of the data we use in IPE are spatial data—the phenomena we study occur at specific geographic locations (Darmofal, 2006). Some of our most-studied variables, such as financial and monetary policies, bilateral investment treaties (BITs), preferential trade agreements (PTAs), regime type, international organization memberships, capital openness, central bank independence (CBI), exchange rates, inflation

targets, arbitration laws, immigration policies, alliances and risk ratings, are specific to countries, which have a unique, and not insignificant, geographic location. Still others, such as foreign direct investment, firm location, trade preferences, resource location, GDP, population, aid projects, migrant populations and political violence, vary sub-nationally in terms of location.

There are two substantive ways to think about the impact of space. The first is *spatial diffusion*, or a diffusion of behavior between neighboring units. In this case, there are interactions between independent units that produce this behavior, and it is critical to understand these interactions. The second is *spatial clustering*, which is when neighboring units have similar behavior due to independent adoptions of the behavior. This is sometimes called *attributional dependence* because neighboring units have shared attributes that produce the clustering of behaviors (Darmofal, 2006). It is important to understand which process is creating spatial dependence, as there are different methodological approaches to dealing with them. In short, spatial diffusion can be modeled with a spatially lagged dependent variable on the right-hand side, and spatial clustering can be modeled with a spatially lagged error term.

Not dealing with space properly in our statistical models poses problems for inference. 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). Spatial autoregressive (SAR) models are fit using data that contain geo-located observations or on any units with a spatial representation.[18] SAR works by fitting linear models with autoregressive errors and spatial lags of the dependent and independent variables. It

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[18]Importantly, spatial data does not have to be geographic data. For instance, social network nodes could be analyzed using this method. For a more complete treatment of the topic of using spatial methods for non-geographic data, please see Beck *et al.* (2006).

allows the user to specify spatial lags using spatial weighting matrices. One might create standard weighting matrices, such as inverse distance or nearest neighbor, or create custom matrices.

Before using an SAR model, one should first check if ordinary linear regression is adequate. Once the data have become spatial data, they can be used for both spatial and nonspatial analysis. As such, the best way to test if a model requires SAR is to first turn the data into spatial data and then estimate a simple linear regression. If the residuals of the model are spatially correlated, you need to use SAR.

In order to know if the residuals are spatially correlated, we can estimate the regression using spatial data but without using a W spatial weighting matrix and look at the spatial autocorrelation parameter, ρ.[19] Rho (ρ) is bounded between -1 and 1, where 1 is perfect positive spatial autocorrelation and 0 is no spatial autocorrelation. If ρ is statistically significant, then you must account for the spatial dependence in your model.

One might wonder if it would make sense to always use SAR, just in case, or if there are drawbacks to this approach. If you use SAR for models without spatial lags or autocorrelated errors, you will obtain the same coefficient estimates, but they will have slightly different standard errors. Additionally, SAR models report Z and X^2 statistics instead of t and F statistics. Finally, SAR models do not include finite-sample adjustments like OLS models.

5 Results

I first test to see whether political violence affects investment at the town level. Recall that my theory predicts that `political violence` will have a *negative* and

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[19]This is for the case of panel data, which this paper uses. For cross-sectional data, you can use Moran's I (the most common), the Legrange multiplier or the Kelejian and Robinson diagnostic
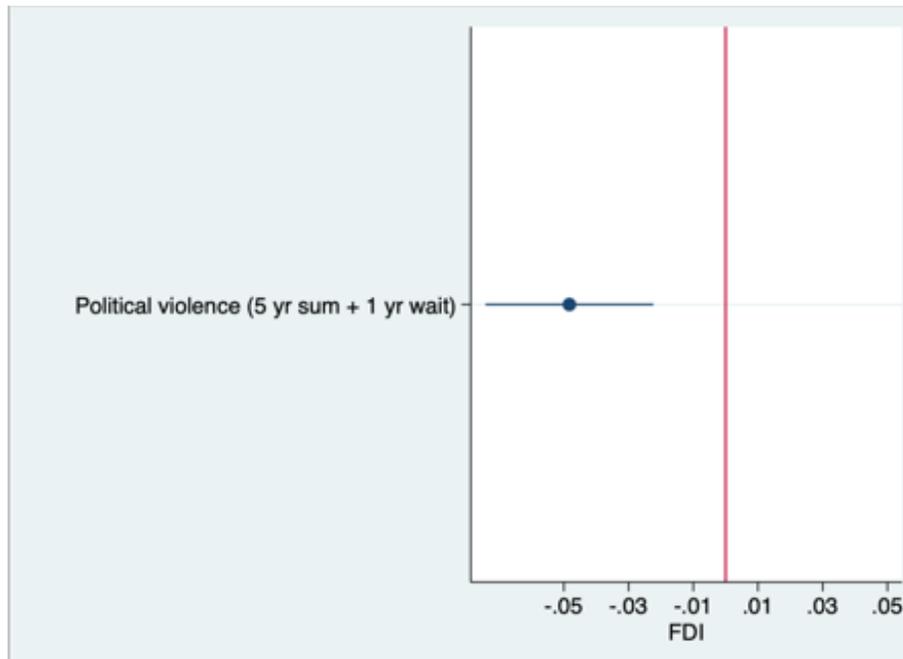
Figure 3: Violence deters FDI at a subnational level

significant impact on FDI at the subnational level. As I show in Figure 3 below, `political violence` does have a negative impact on FDI at the town level.

My theory also predicted that `political violence` would have a *positive* and significant impact on FDI at the subnational level when we look only at FDI to prospective monopolies. As I show in Figure 4 below, political violence actually has a positive effect for firms looking to invest in prospective monopolies.

These results help us better understand the null finding at the aggregate level. First, if firms are investing in some places in a country but not others, we will have a null finding at the country-year level. Second, if conflict is deterring firms in some industries but incentivizing firms in others, it will generate a null finding at the aggregate level.

I briefly address a few potential empirical concerns. First, I confirmed that neither the dependent variable (FDI) nor the main independent variable of interest (polit-
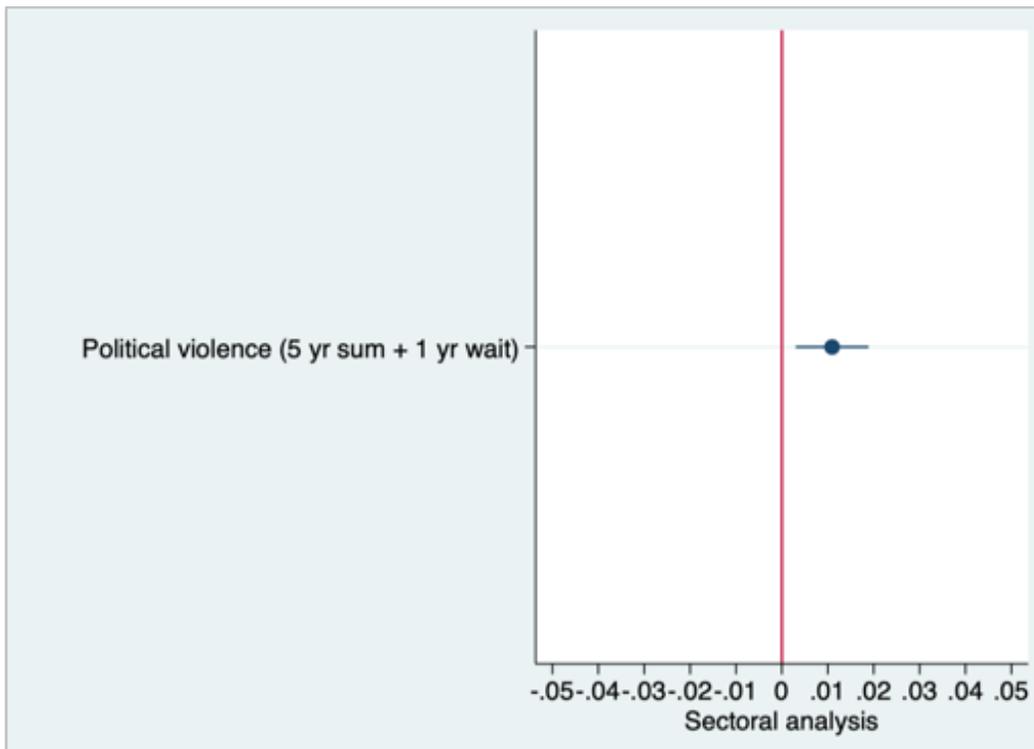
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Figure 4: The effect of violence on FDI varies with market structure

ical 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.[20]

6 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 first argued that firms are willing to invest in countries with political violence, they simply set up shop far away from the violence. This means that they are able to reap the benefits of many of the desirable attributes of developing countries, such as low corporate taxation and cheap labor, while avoiding violence. I then 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 spatial and sector-level FDI data. I found that though we do see much investment in countries with violence,

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[20]Please refer to Table 1 in the appendix.

conflict does deter FDI at the subnational level. I also found 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 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?[21]

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

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[21]Büthe & Milner (2014, 2008)

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.

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7   Appendix

7.1   Figure 1

Figure 5 replicates Figure 1 but includes cases where either investment or political violence is zero.

Figure 5: Political violence (lagged) vs. FDI (lagged)

7.2   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. In Table 1 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 and use the same model specifications.

Table 1: FDI does not predict conflict

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

Standard errors in parentheses
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$