UNIVERSITY OF CALIFORNIA, SAN DIEGO

Capital and Chaos:
Fragile States, Political Risk and Foreign Direct Investment

A dissertation submitted in partial satisfaction of the requirements for the degree of Doctor of Philosophy

in

Political Science

by

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The Dissertation of Benjamin A.T. Graham is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

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For Lynn.

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LIST OF ABBREVIATIONS

BEA	Bureau of Economic Analysis
CDIS	Coordinated Direct Investment Survey
CEPII	Centre d'Etudes Prospectives et d'Informations Internationales
CIRI	Cingrinelli and Richards
DBI	Doing Business Indicators
DPI	Database of Political Institutions
FDI	Foreign Direct Investment
GDP	Gross Domestic Product
ICRG	International Country Risk Guide
IMF	International Monetary Fund
OECD	Organization for Economic Co-operation and Development
ONDD	Office National Du Ducroire
PITF	Political Instability Task Force
TI	Transparency International
WDI	World Development Indicators
WGI	World Governance Indicators

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ABSTRACT OF THE DISSERTATION

Capital and Chaos:
Fragile States, Political Risk, and Foreign Direct Investment
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Doctor of Philosophy in Political Science
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Professor Miles Kahler, Chair

Fragile states are trapped in cycles of poverty, violence, and instability. War and instability deter investment. Low investment retards growth, and lack of growth engenders further conflict and instability. One path out of this equilibrium is for fragile states to succeed in attracting foreign direct investment (FDI) while political risk remains high. My dissertation explores firm-level variation in how investors experience and respond to political risk, identifying types of investors who are, and are not, willing to invest in post-conflict and other fragile states. I then explore the mechanisms through which these investors manage political risk. I focus specifically on foreign firms that specialize in political risk management, and on diasporans (i.e. migrants and their descendents), a group of potential investors that is theorized to be particularly willing to, and capable of, investing in fragile states. Empirically, I exploit both time-series-cross-sectional data on dyadic FDI between states, as well as firm-level data from an original survey of foreign firms in the post-conflict country of Georgia.

Chapter 1:
Introduction

Fragile states are trapped in a cycle of poverty, violence, and instability. War and instability deter investment, low investment retards growth, and lack of growth engenders further conflict and instability. One path out of this equilibrium is for fragile states to succeed in attracting foreign direct investment (FDI) while political risk remains high.

The pattern of FDI flows into fragile states suggests that wars, coups, and political violence contain both risk and opportunity to foreign firms. While political risk deters investment, it does not deter all investors equally, and does not deter some investors at all. This dissertation asks three related questions: What types of foreign firms are willing and able to invest in fragile states and other states with high levels of political risk? What types of political risk can foreign firms develop capabilities to manage and what types of experience allow them to do so? Among foreign firms that invest in fragile and high-political-risk states, what types of firms contribute more or less to the stabilization and development of these countries?

These three questions all revolve around a core puzzle of political economy: how institutions shape economic behavior. Due both to reasons of data availability and the substantive importance of larger, more advanced, economies, most study of the impact of institutions and economic behavior is conducted in the context of relatively high-performing institutions. However, analyzing economic behavior in the absence of institutions offers a unique window into this relationship as well, establishing a sort of baseline against which institutional effects can be measured. Fragile states often

contain just such an institutional void, and represent a unique opportunity to expand the scope of our understanding.

The study of fragile states also offers a unique opportunity to conduct normatively important and policy relevant research. State fragility produces both suffering for local residents and security problems for foreign states as havens for terrorists, pirates, and black-market traders. As noted above, foreign investment – perhaps particular kinds of foreign investment – offers a potential path out of a normatively bad equilibrium. While most academic work on political risk focuses on the type of institutions that limit political risk effectively, this research agenda focuses on the type of investors that can function in countries in which political risk is not effectively limited, and how those investors can contribute to stabilization and development.

I begin this chapter by describing and defining fragile states and providing some basic description of patterns of foreign direct investment into these states. I then introduce the broad theoretical approach of the dissertation, and outline the four stand-alone papers that comprise it.

Defining and Describing Fragile States

Fragile states can be defined either as states at risk of collapse in the near future or, as I do in this project, as states in which government control is limited and/or violent intrastate conflict is already a reality.[1] Because loss of government control generally occurs gradually and the outbreak of intrastate violence generally

[1] Goldstone et al. (2003, p. 3) write, "States that are 'failing,' 'in failure,' or 'recovering from failure,' may be considered as all—in varying degrees—fragile states." This matches my definition well.

begins prior to full state collapse, measures of state fragility based on these two definitions have substantial overlap: limited government control and the outbreak of state violence are among the best predictors of state collapse. Because intrastate violence, partial loss of government control, and full state collapse are all outcomes of interest, many projects, such as the Political Instability Task Force, move back and forth between these definitions as specific research questions dictate.[2]

Violence and government incapacity are not, of course, the only useful predictors of eventual state failure, and scholars who attempt to measure fragility as the probability of failure often include many other predictors as part of their definitions, including economic, social and political factors, that are correlated with state failure, but which may also be present in states that characterized by peace and high-capacity governance. The most prominent example is Foreign Policy Magazine's Failed States Index, which includes everything from the economic growth rate and income inequality to demographics, elite factionalization, and foreign intervention (Baker 2006).

In the context of this project, which focuses on the impact of state fragility and political risk on foreign investment, I define state fragility narrowly as a set of current conditions, rather than as a predisposition to future collapse. I draw on a variety of data sources, but especially the Political Instability Task Force to measure whether a state is currently experiencing, or has recently experienced, violent conflict or adverse regime change.

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[2] The project has produced a series of reports and articles, all of which are available at http://globalpolicy.gmu.edu/pitf/ (Accessed May 7, 2012).

I define fragile states as states that meet any of the following conditions:

1. A major[3] war, genocide, or politicide on its territory in the past three years.

2. A successful coup or regime-change-by-force in the past three years.[4]

3. Occupation by a foreign power (interruption) or absence of any central governing authority (interregnum) in the past three years.[5]

Who Invests in Fragile States? Cross-National Patterns

Descriptive statistics on investment in fragile states are difficult to compile because state fragility is an effective predictor of data missingness. The International Monetary Fund (IMF) recently began the Coordinated Direct Investment Survey (CDIS), which aims to collect bilateral FDI data for all country pairs in the world, but the coverage remains limited. Figure 1.1 shows the ten home-countries that reported the largest total FDI outflows to fragile states in 2009. The United States tops the chart at $126 billion.[6]

[3] For this component I use the Political Instability Task Force data on wars, genocides, and politicides. Major events are: events in any of these categories in which one or more of the following conditions are met: 1. Over 5,000 fatalities; 2. At least 25% percent of the territory and/or most major cities are affected; 3. At least 10% of the territory (or some major cities) AND at least 1000 fatalities.

[4] I draw this data from both the Powersharing, Agency, and Civil Conflict dataset and the Center for Systemic Peace. If either data source records a successful coup or regime-change-by-force, a country is coded as fragile for the next three years.

[5] These codings are drawn from Polity IV.

[6] Mexico is coded as fragile in 2009 because of cartel-related violence. However, even if Mexico is coded as not-fragile, the U.S. remains the single largest source of FDI to fragile states. With flows to Mexico omitted, the US does fall out of the top 10 in percentage terms.

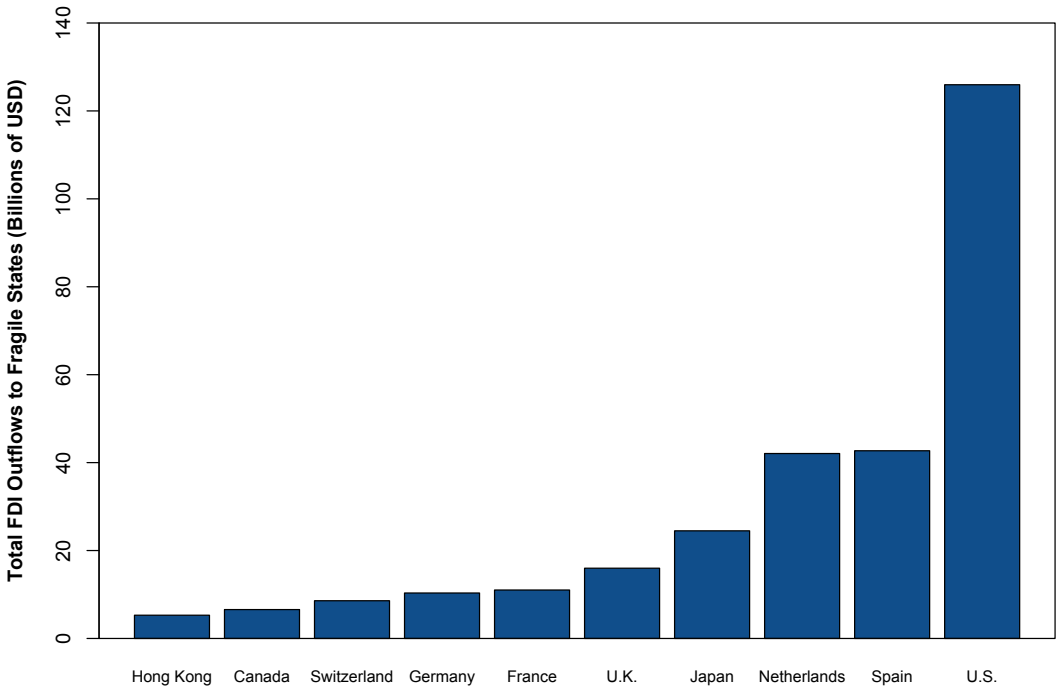

Figure 1.1: Top 10 States by Volume of Outward FDI Into Fragile States (2009)

Of the U.S. outward FDI that went to fragile states in 2009, approximately 78% of that went to Mexico, which is coded as fragile in 2009 because of the violence associated with drug cartels. Figure 1.2 presents the same information as Figure 1.1, while omitting FDI flows to Mexico. Mexico is an outlier among fragile states in its level of economic development, and it attracts much more FDI than most fragile states, but the list of major investment-sending countries remains similar with Mexico removed.

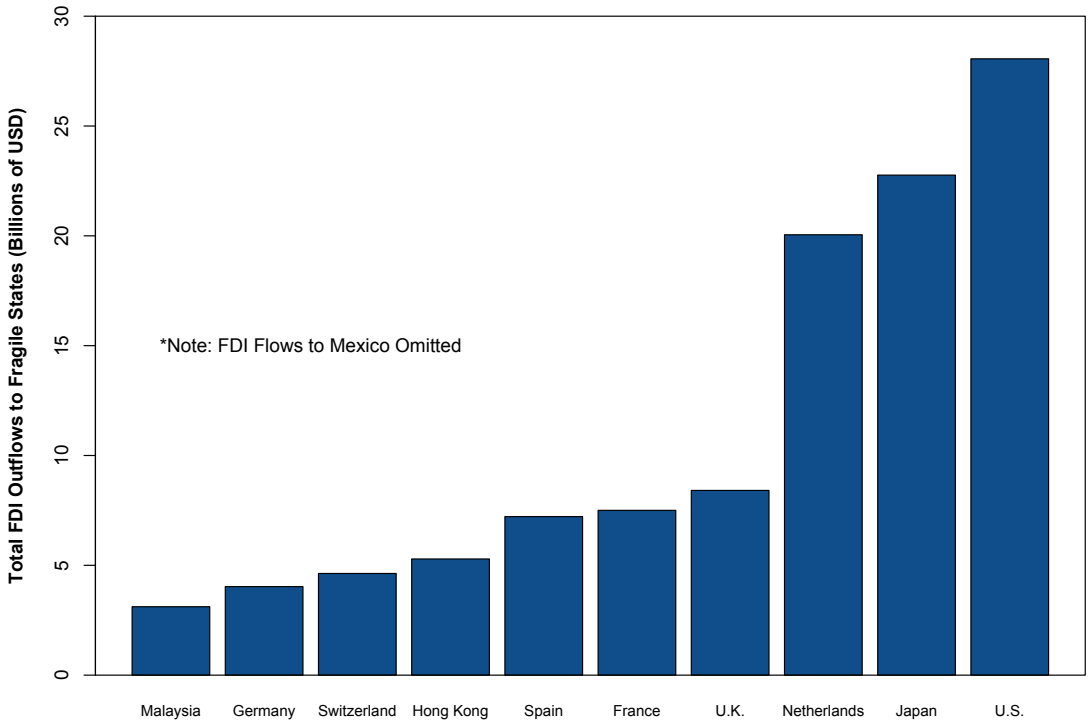

Figure 1.2: Top 10 States by Volume of Outward FDI Into Fragile States (2009)

Figures 1.1 and 1.2 are dominated by large, wealthy states that send large volumes of FDI to both fragile and non-fragile states. Figure 1.3 shows the ten home countries that send the highest percentage of their total FDI outflows to fragile states.

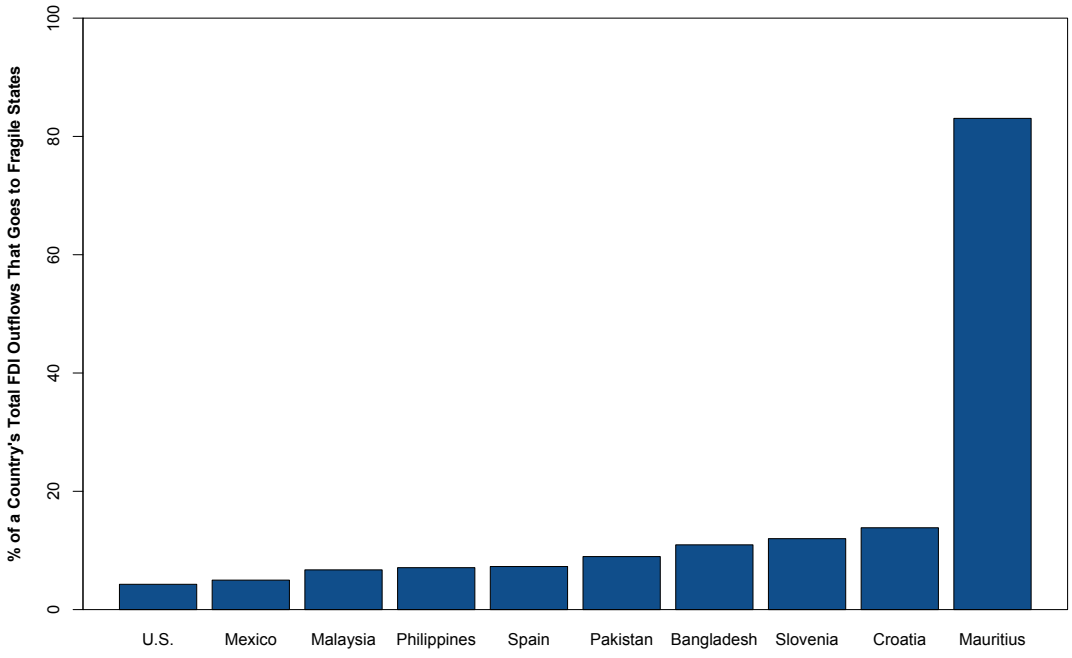

Figure 1.3: Top 10 States by Share of FDI to Fragile States

Seven out of ten of these countries come from outside the Organization for Economic Co-operation and Development (OECD). Mauritius tops the table, sending 83% of its outward FDI to fragile states. This investment flows primarily to countries nearby: Madagascar, the Seychelles, and Mozambique. South Africa is the only non-fragile recipient of Mauritian FDI. Mauritius is incredibly stable, having experienced no violent conflict since decolonization, and consistently scoring the highest possible value on the Polity IV democracy scale since 1982. The only other two African countries that report data on outward flows report either zero FDI to fragile states (Mozambique) or nearly zero (South Africa), despite numerous fragile states nearby. South Africa, in particular, sends a great deal of FDI to OECD countries and to more stable African states such as Botswana, Lesotho, Malawi and Tanzania.

Who Invests in Fragile States? Firm-Level Theory

State fragility, and high levels of political risk more broadly, deter investment, but do not deter all investors equally. Some firms are capable of operating profitably in the face of violence, low institutional function, corruption, and other forms of extreme political risk. But what type of firms? This question is important for understanding why some fragile states succeed in attracting FDI and others fail, but also for understanding what effect FDI is likely to have on the development and stabilization of the fragile states that succeed in attracting it.

Foreign investment not only responds to political conditions, but also affects those conditions. The stock of foreign direct investment in a country affects the costs governments pay for violating property rights, and affects incentives for policy reform (Malesky 2006; Egan 2010). Different types of firms have different preferences over the institutional environment in which they operate. While most firms benefit from lower levels of political risk and higher levels of institutional development, this is not the case for all firms. If a firm is highly capable at managing a particular type of political risk, and if that risk deters competitors from entering the market, the firm may actually benefit from perpetuating a status quo in which political risk remains high and competition remains low.

In understanding the implications of firms' sensitivity to political risk, it is useful to think of three archetypical firms that are capable of investing profitably in fragile states.

Firm A is a firm that has no special risk-management capabilities, but invests in projects for which the returns are high enough to offset substantial expected losses due to political risk. Firm A invests in projects where the direct costs are low relative to the value of the product, allowing the firm to absorb high levels of indirect costs from political instability, war, contract insecurity, etc. If the indirect costs fall, Firm A's profitability increases and it may expand to exploit additional business opportunities. Firm A's presence in a host country increases the likelihood of peace and political stabilization by aiding economic development and by creating a constituency for political stabilization – Firm A prefers higher levels of institutional function.

Firm B has unusual capabilities for managing a particular type of political risk, or managing political risks in a particular country. Because political risk imposes smaller costs on Firm B than it imposes on Firm B's competitors, Firm B can profitably exploit business opportunities that would not be profitable to its competitors. Political risk serves as a barrier to entry for Firm B's competitors, and the presence of political risk increases Firm B's profitability by restricting competition. Firm B represents a constituency opposed to political stabilization – Firm B prefers lower levels of institutional function.

Firm C is a firm willing to absorb high indirect costs from political risk and accept a below-market rate of return on investment in deference to patriotism or related non-pecuniary incentives. Firm C, like Firm A, both contributes to development through direct promotion of economic development and through

development of a constituency for political stabilization. Investors motivated by non-pecuniary incentives may even contribute to development in ways that exceed the contributions of strictly profit-motivated firms.

All three of these archetypical firms are willing and able to invest in fragile states, but they vary in the degree to which their presence is expected to contribute to the development and stabilization of these states.

The current political science literature, which treats political risk as a country-level variable, makes the tacit (and sometimes explicit) assumption that most firms investing in fragile states are like Firm A. Investment projects whose returns are large enough to outweigh large political risks generally derive from the natural endowments of the host country, and are primarily in natural-resource extraction industries, and occasionally in agriculture.

The existence of firms like Firm C has been proposed in the business literature, but empirical evidence for their existence is limited. Chapters 3 and 5 of the dissertation examine evidence at both the cross-national level, using aggregate flows of FDI between countries, and at the sub-national level, looking at the behavior or foreign firms investing in the post-conflict country of Georgia, regarding whether such firms exist.

Firms like Firm B are similarly theoretically plausible, but under-theorized and empirically under-explored. Guidolin and Ferrara (2007) find evidence that some diamond-mining firms in Angola benefitted from that country's civil war because it kept competing firms from entering. However, little is know about how common

similar dynamics are in other countries and other industries, or what allowed some firms to operate successfully in Angola during the war while others could not.

In this dissertation I develop and test theory regarding two types of firms with unusual capabilities for managing political risks: political risk specialists, who develop capabilities for managing a particular type of political risk, and diaspora-owned firms, who have social-networked based abilities specific to a particular country.

Investors with Country-Specific Advantages

Diasporans, i.e. migrants and their descendents, are a group of investors who potentially possess motivations and advantages specific to their homelands. A number of recent programs introduced by the US Agency for International Development and other organizations promote diaspora direct investment as a development tool in fragile states where various forms of political risk are high. I argue that social networks provide diasporans with competitive advantages in their homeland, but that diasporans' access to information actually causes them to be more sensitive to over-time changes in political risk than are other foreign investors. Diaspora investment can serve as a complement to, and incentive for, improvements in governance, but is no more likely than other forms of FDI to precede these investments.

At the cross-national level, I demonstrate that flows of migrant-induced FDI respond more strongly than other FDI flows to over-time changes in the level of political risk, and particularly to decreases in the level of risk. At the firm level, I exploit original survey data to show that diaspora-owned firms rely on social networks more heavily than do other foreign firms and that the owners and managers of

diaspora-owned firms perceive social networks to be more important to firm profitability than do their counterparts at other foreign firms. The informational and social network advantages of diasporans alter both the volume and the nature of diaspora direct investment. Notably, high levels of information allow diasporans to respond quickly and effectively to seize new investment opportunities that emerge in the homeland when political risk increases. This implies that diaspora investment can complement and incentivize improvements in governance and the rule of law, but it cannot substitute for, and generally does not precede, these improvements.

Firms with Risk-Specific Advantages

I also examine investors with unusual capabilities for operating in countries with specific political risk profiles. I argue that firms with experience managing certain types of political risk develop capabilities in managing those specific risks, and can exploit these capabilities across countries. Firms specialize in managing the types of political risks associated with unreliable institutions: e.g. corruption, bureaucratic dysfunction, policy instability, and political violence. Political risks associated with adverse regime change, such as coups and revolutions, are less amenable to specialization. I argue that the risks associated with the (potential) overturn of a political order are not something the firms can learn to manage effectively – indeed it is usually the firms that were most adept at managing the bureaucratic and policy risks associated with the previous regime that are most vulnerable to expropriation and negative treatment by the new regime.

Firms develop political risk management specializations based on the political environments in both their home country and in the host countries in which they invest. Firms with experience in managing a specific type of political risk are more likely to enter new countries with high levels of that specific political risk. Empirically, I show that similarity in the level of bureaucratic risk between the home and host country is associated with greater bilateral FDI flows in that dyad. I also show that, among Japanese manufacturing firms, firms tend to enter new countries that share the political risk profile of countries in which they already do business. I find no evidence that firms benefit from prior experience managing the risk of adverse regime change.

Challenges in the Study of Fragile States

The empirical study of investment in fragile states is plagued most centrally by paucity of data. Fragile states are plagued by low institutional capacity, and many governments of fragile states have reasons to avoid transparency. These two factors combine to insure that vary little data about the economies of fragile states emerges; even the most basic facts, such as gross domestic product, are often unknown.

One of the simplest descriptive questions posed in this dissertation, who invests in the world's most-fragile states, is one that remains largely unanswered at its end. Where the dissertation succeeds, however, is in gaining an understanding of investment in the face of high political risk more broadly. Pointed empirical questions about the most-fragile states remain an important part of the research program of which this dissertation is part, but by expanding the range of inquiry to include states

with high levels of political risk more broadly, the dissertation gains both greater empirical traction and broader policy relevance.

However, despite the empirical difficulties of examining these most extreme cases and the benefits of expanding analysis to encompass a wider range of political risk climates, the study of fragile states enables important theoretical and policy contributions. Therefore, I keep a portion of the dissertation, and of the research agenda in which it is embedded, focused on these most extreme cases.

An Outline of the Dissertation

The body of this dissertation consists of four stand-alone papers on the theme of FDI in weak and fragile states. Two of the papers draw on cross-national evidence, including bilateral flows of foreign direct investment at the national level; two of the papers draw on evidence from an original firm-level survey conducted in the post-conflict country of Georgia in 2009.

Chapter 2: Capital and Chaos

The pattern of FDI flows into weak and fragile states suggests that wars, coups, corruption, inefficient courts, and unconstrained executives represent both risk and opportunity for foreign firms. While political risk is often conceived of as a country-level phenomenon, firms vary widely in their ability to manage these risks. I argue that, based on prior experience managing political risks in the home country and previously-entered host countries, firms develop strategy, structures, and capabilities tailored to the management of particular political risks. I also present a five-part typology of political risks and argue that these types of risk vary in the degree to

which prior home- and host-country experience predisposes firms to seek out future host countries with familiar political risk profiles. Empirical analysis on home-country effects is conducted using data on bilateral flows of FDI from the IMF's Coordinated Direct Investment Survey, while firm-level analysis of host-country effects is conducted using data from the Japanese Overseas Investment database. Results show that prior experience predisposes firms to enter future host-countries with similar levels of bureaucratic risk, policy risk, and risk of war and political violence. However, consistent with the hypotheses presented, no such effects are found with regard to the risk of adverse regime change.

Chapter 3: Political Risk and Diaspora Direct Investment

I argue that diasporans are better informed about the political and economic situation in the homeland than are other potential foreign investors; that higher levels of information allows diaspora investors to anticipate and respond to changes in risk more effectively; and that this makes migrant-induced foreign direct investment (FDI) more sensitive to political risk than is other FDI. I conduct empirical analysis on a dataset of bilateral FDI flows from 30 OECD countries to 105 developing countries from 1994-2008: I find that migrant-induced FDI is more sensitive to political risk than is other FDI, and particularly that migrant-induced FDI is more sensitive to decreases in political risk. This has three central implications. First, diasporans' access to information is a major factor driving the causal relationship between migrant stocks and FDI flows. Second, the effect of political risk on investment flows depends on the mix of investor types (particularly diaspora vs. non-diaspora) in the pool of potential

investors from which a state is drawing. Third, diaspora direct investment has the potential to motivate and sustain governance reform and stabilization in fragile states, but it is no more likely than other FDI to substitute for, or precede, these improvements.

Chapter 4: Diaspora Owned Firms and the Value of Social Networks

Do diaspora-owned foreign firms enjoy a competitive advantage in the homeland, and if so, what accounts for this? This paper uses data from an original survey of 174 foreign-owned firms in the post-conflict country of Georgia to explore mechanisms of diaspora difference, focusing particularly on how firms use social networks in business. I find that diaspora-owned firms use social networks more than other foreign firms in the acquisition of real estate, and that social networks are more important to firm location decisions and to overall profitability of diaspora-owned firms. Firms report using social networks to access information, but not to secure contracts or deter opportunistic behavior. This indicates that social networks provide important competitive advantages to diaspora-owned firms, but that diasporans, and foreign firms in general, do not use social networks to substitute for formal contracting institutions (i.e. courts).

The data for Chapters 4 and 5 was gathered in the *Capital and Conflict: Georgia* survey, the first firm-level survey (to my knowledge) that compares diaspora-owned firms to other foreign firms. The survey was conducted in person and covers a representative sample of all firms in Georgia meeting the following conditions: 1) a for-profit enterprise; 2) at least 10% foreign ownership; 3) registered as active and

paying taxes as of June 1, 2009; 4) obtained its first registration in Georgia after the year 2000. Diaspora-owned firms are those in which at least one of the owners considers him/herself to be Georgian and was living outside of Georgia when the firm first entered the country.

Chapter 5: Diaspora-owned Firms and Social Responsibility

A growing literature in economic sociology and business suggests that diaspora investors are motivated by patriotism and other social and emotional factors, endowing them with unique potential as a force for international development. The existing literature argues that diaspora-owned firms are more socially responsible than other foreign firms, and engage in a range of development-promoting behaviors when investing in the homeland: hiring more local labor, paying higher wages, and taking better care of the local environment. I test this theory at the firm level, using data from the *Capital and Conflict: Georgia* survey. Across a range of self-reported behaviors and priorities, I find no evidence that diaspora-owned firms are more socially responsible than other foreign firms, and some evidence that they are less socially responsible. I argue that diaspora investors are uniquely capable, but not uniquely philanthropic, when doing business in their homelands.

Chapter 6: Conclusion

These four stand-alone papers are followed by a short conclusion that serves both to highlight the joint contributions and implications of the papers, and to outline directions for further research. While this dissertation is not structured as a proto-book, it contains theory and empirical analysis that will be included in two separate

book projects, each of which will require additional development beyond the scope of the four papers presented here.

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Chapter 2:
Capital and Chaos

Abstract

The pattern of FDI flows into weak and fragile states suggests that wars, coups, corruption, inefficient courts, and unconstrained executives represent both risk and opportunity for foreign firms. While political risk is often conceived of as a country-level phenomenon, firms vary widely in their ability to manage these risks. I argue that, based on prior experience managing political risks in the home country and previously-entered host countries, firms develop capabilities tailored to the management of particular political risks. I present a five-part typology of political risks and argue that these types of risk vary in the degree to which prior home- and host-country experience predisposes firms to seek out future host countries with familiar political risk profiles. Empirical analysis on home-country effects is conducted using data on bilateral flows of FDI from the IMF's Coordinated Direct Investment Survey, while firm-level analysis of host-country effects is conducted using data from the Japanese Overseas Investment database. Results show that prior experience predisposes firms to enter future host-countries with familiar levels of bureaucratic risk, policy risk, and risk of war and political violence. However, consistent with the hypotheses presented, no such effects are found with regard to the risk of adverse regime change.

Introduction

Foreign direct investment (FDI) contributes to economic growth, produces technological spillovers, and increases human capital. However, it is often the countries that need FDI the most that are least capable of attracting it: the world's least developed countries (LDC's) received less than 5% of the world's FDI flows in 2010 (UNCTAD 2011). While poverty and under-development themselves are a major deterrent to investment, political risk also plays a major role in limiting developing countries' access to FDI.[1] Poor countries fail to attract investment not only because they are poor, but also because political risk in these countries is high.

While political risk deters most investors, it does not deter all investors equally, and does not deter some investors at all. I argue that this heterogeneity must be taken seriously and that political risk must be analyzed as a firm-level phenomenon as well as a country-level phenomenon. Firms possess varying capabilities for mitigating different types of risk, and firms with high capabilities for mitigating a particular risk may not view the presence of this risk as a major deterrent to investment. In extreme cases, firms that can mitigate a particular type of risk effectively may find that risk to be beneficial, in that it deters competing firms and reduces competition.[2] Identifying firms that are relatively risk insensitive is both intellectually compelling and normatively important, as it informs and assists investment promotion in the world's most capital-starved economies.

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[1] See, for example, Pfefferman, Kisunko, and Sumlinki (1999).
[2] For example, Guidolin and La Ferrera (2007) demonstrate that diamond-mining companies operating in Angola during the civil war benefitted from the presence of the war and lost value when the war ended suddenly.

In the broader research agenda within which this paper is nested, I identify two categories of factors that affect firms' ability to manage political risk: factors that allow a firm to mitigate political risk in a particular country, and factors that allow a firm to mitigate a particular type of political risk across multiple countries. This paper explores the latter category, analyzing the degree to which risk-mitigation expertise is transferable across countries.

I divide political risk into five categories: bureaucratic risk, or risks related to corruption, low bureaucratic capacity, and the governance of the economy by informal institutions; transfer risk, or risks associated with the movement of capital across borders; policy risk, or risks related to policy instability and opportunistic government behavior at the macro level; risk of war or political violence, including government violations of physical integrity rights; and risk of adverse regime changes, including coups and revolutions.

The role of prior experience in the development of risk management capabilities varies by risk type. On one extreme, I expect that firms with experience operating in countries with high levels of corruption, low-quality court systems, and inefficient government bureaucracies develop transferable skills for managing the risks associated with those country characteristics. On the other extreme, I expect that firms with experience operating in countries plagued by frequent adverse regime changes do not develop skills that are useful in mitigating the same type of risk in other countries. The risk of regime overthrow involves the risk that the entire institutional order is overturned, and is not amenable to management through the same

type of strategies that firms can use to navigate the risks associated with the flaws in a particular institutional order.

Firms develop risk mitigation strategies in response to both the risks they face in their home country, as well as the risks they face in host countries they have entered previously. As firms develop a repertoire of effective strategies for mitigating specific types of risk, they become more willing to enter additional countries that are characterized by high levels of this risk. They become, in effect, risk management specialists with competitive advantages based on their capability to manage certain types of political risk.

The empirical portion of this paper is divided into three parts. The first uses factor analysis to assess the degree to which the five categories of political risk I distinguish theoretically can also be distinguished empirically. The second analyzes bilateral flows of FDI to assess whether similarity in political risk profile between home and host country is an important determinant of FDI. The third uses a dataset of overseas investments by Japanese manufacturing firms to assess the degree to which previous experience operating in a host country with a given political risk profile makes a firm more likely to enter additional host countries with similar risk profiles. Results are consistent with the theory, but a variety of revised and additional tests are planned.

Literature Review

In the political science literature, the relationship between governance and foreign direct investment is viewed primarily as a monadic relationship, and political

risk itself is viewed as a country-level variable. The quality of governance in the host country affects the costs, and particularly the risk, facing foreign firms who choose to invest there. Host country characteristics, such as democracy (Li and Resnick 2003; Jensen 2008); constraints on the executive (Henisz 2000); federalism (Jensen and McGillivray 2005), and corruption (Habib and Zurawicki 2003; Javorcik and Wei 2009), have all been found to affect aggregate FDI inflows to a given host country. When the literature addresses governance in a dyadic context, the focus is generally on relationships between governments: bilateral investment treaties (Büthe and Milner 2009; Allee and Peinhardt 2010), bilateral trade agreements (Büthe and Milner 2008), or violent interstate conflict (Li and Vashchilko 2010).

In the last several years, an emerging stream of literature has begun to break from this tradition and explore variations across types of firms in how they experience and respond to political risk. Wellhausen (2011) demonstrates that a firm's nationality (and the number of co-national firms in a given market) affects a firm's vulnerability to breach of contract by the host government. Kerner and Lawrence (2012) argue that vulnerability to political risk is concentrated among firms with investments in fixed capital rather than liquid assets in foreign affiliates. Graham, Kingsley, and Johnson (2012) argue that firms can insulate themselves from transfer risk based on how they finance their direct investments. This paper contributes to this emerging research program.[3]

[3] Other examples of work in this vein include Tan and Meyer (2011) and Kingsley and Noordewier (2011).

Within the business literature, several authors find that firms based in corrupt home countries are less deterred by host-country corruption (Habib and Zurawicki 2002; Cuervo-Cazurra 2006). Holburn and Zelner (2010) argue that firms learn to influence policy and manage policy risk in weakly institutionalized host countries by operating in a weakly institutionalized home country. They find that firms in the electric power generation industry tend to enter countries with high levels of policy risk if they are based in home countries where the level of policy risk is also high. These findings suggest that firms can learn to manage at least some types of political risk, and that these risk-management capabilities are transferable across countries. What the literature lacks, however, is a detailed theory of political risk management capabilities, including the types of political risk that firms can manage effectively and the types of experience (in home vs. host countries) that produce and select for those capabilities.

Theory: Political Risk Management Capabilities as Mobile Assets

Political risk is a firm-level phenomenon as well as a country-level phenomenon. Firms vary in their abilities to mitigate the risks and costs associated with corruption, policy instability, and political violence.[4] The political risk environment(s) in which a firm has previously operated are an important determinant of the firm's ability to mitigate various types of political risks in any new host country it enters.

[4] Political risk management capabilities fit well within the resource based view of the firm, in that they are rare, valuable, inimitable, and non-substitutable.

This argument is based on both learning and survival. Firms invest in developing risk-management capabilities relevant to the types of risk they face in the countries in which they are currently operating (learning), and firms that best succeed in developing these capabilities are most likely to survive in those markets and proceed to enter new markets (survival). For example, if a given home country is characterized by an inefficient judiciary, firms based there face incentives to invest in developing resources for enforcing contracts and resolving civil disputes outside the court system. Firms that succeed in developing alternative means to resolve disputes and enforce contracts will continue to operate in that country, while firms less able to avoid reliance on the court system will not be able to survive. Therefore, firms based in countries with poorly functioning courts are more likely to be skilled in informal dispute resolution than firms based in countries with high quality judiciaries, for reasons of both learning and survival. If firms are able to transfer their informal dispute resolution capabilities from one country to another, we should see the firms based in countries with low-quality courts willing to invest in countries with similar judicial deficiencies. The same logic applies to other host countries in which a firm operates: the firm develops the skills necessary to manage the political risks in one host country, and can then transfer those skills to operations in any future countries in enters that share a similar risk profile.

Expectation 1: Firms develop capabilities (e.g. knowledge, skills, strategy, staffing) appropriate to minimizing the costs and risks associated with the political environment in the firms' home country and the host countries in which they operate.

Expectation 2: Firms who possess or develop capabilities to minimizing the costs and risks associated with the home and host countries in which they operate are more likely to survive than firms that do not possess or develop these capabilities.

These capabilities are not costless to develop, and firms do not develop capabilities for managing risks they do not face. For example, firms operating in corrupt countries will invest resources and structure their operations to deter and appease bribe-seekers in a way that firms in less corrupt countries do not. In a firm operating in a corrupt country, permits must be secured by politically connected senior managers who are selected for their skill in navigating the bureaucracy, while firms in a less corrupt country may treat the securing of permits as a primarily clerical task handled by low-level staff.

The level of political risk in a given country is an important factor affecting firms' decisions of whether or not to invest in that country. However, the cost of mitigating the political risks in a given country are higher for some firms than for others. Firms that are already engaged in mitigating a particular source of risk in one country will face lower costs in developing the capabilities to mitigate that risk in a new host country.

A Typology of Political Risks

I distinguish between five types of political risk: bureaucratic risk, transfer risk, policy risk, risk of war and political violence, and risk of adverse regime change. While I expect that the skills firms develop to manage a specific type of political risk are transferable across countries, I do not expect that skills developed to manage one type of risk are useful in managing a separate type of political risk. I also expect that some types of risk political risk are more amenable to learned management than others.

The business literature distinguishes between exogenous hazards, which cannot be resolved once they materialize, and endogenous hazards, which can be at least partially resolved after the fact. This literature asserts that political risks are exogenous hazards: "Governance hazards … typically take the form of irreversible governmental decisions and political and societal turmoil, and are hence generally impossible for MNCs to resolve once they become reality (Slangen and Beugelsdijk 2009, p. 983)."[5]

The theory of political risk management capabilities as mobile assets challenges this belief directly: I argue that most political risks are endogenous hazards, and that firms with the proper capabilities can mitigate, and not just avoid, these risks. In the following sections I articulate this logic with regard to bureaucratic risk, transfer risk, policy risk, and risk of war and political violence. However, I also argue that the risk of adverse regime change is an exogenous hazard, and it is precisely for this reason that this category of political risk is not amenable to learned management.

[5] See also Cuypers and Martin (2010) and Li and Rugman (2007).

Bureaucratic Risks

Bureaucratic risks are risks related to corruption, low bureaucratic capacity, and the governance of the economy by informal institutions. Among the five categories of political risk I identify, bureaucratic risk is most clearly an endogenous hazard – one that firms can develop capabilities to manage, even after the risk has become realized, i.e. even after the firm has been asked for a bribe, or had a shipment of goods delayed at customs. Firms require specialized skills and strategies to mitigate bureaucratic risks effectively, and there is reason to expect capabilities developed in one country to be useful in another. In the previous example, in which a firm hires senior executives based on their political connections and ability to secure permits and manage relationships with government officials, this strategy should be useful across a range of countries in which arms-length relationships with officials are not conducive to business success, even if the precise nature of the necessary political connections varies from country to country.

I expect both that firms are more likely to enter host countries with similar levels of bureaucratic risk to their home country and with similar levels of bureaucratic risk to host countries in which they are already operating.

Hypothesis 1a: Similarity in bureaucratic risk between home and host country is positively correlated with flows of bilateral FDI.

Hypothesis 1b: The average level of bureaucratic risk in the previous host countries a firm has entered is positively correlated with the level of bureaucratic risk in the next host country selected for entry.

Transfer Risks

Transfer risks are those related to the movement of capital across borders. Transfer risks are less clearly endogenous hazards – they often arise from central government actions, like the imposition of capital controls, that individual firms are unlikely to be able to influence. However, there are strategies that firms can employ, such as structuring project financing to hedge against these risks, that allow some firms to mitigate these risks more effectively than others (Graham, Kingsley, and Johnston 2012). Engaging in hedging and other transfer-risk-mitigation strategies requires firms to acquire costly knowledge and capabilities, and I expect these capabilities to be transferable across countries. Transfer risk is most often conceived of as a threat to foreign firms (e.g. Cosset and Suret 1995; Jensen 2008) rather than as a threat to multinational firms in their home market. However, while purely domestic firms that are not involved in foreign markets are immune from the effects of transfer risk, multinationals move capital in and out of their home country in much the same way they move capital in and out of the host countries in which they operate. Therefore, while a firm based in a high-transfer-risk home country may not experience a prior-learning benefit in its first foreign adventure, it will experience these benefits in future foreign investments, even if its early foreign entries are all in countries with

low levels of transfer risk. The firm develops transferable capabilities for navigating transfer based on its experience moving capital in and out of its home country.

Hypothesis 2a: Similarity in transfer risk between home and host country is positively correlated with flows of bilateral FDI.

Hypothesis 2b: The average level of transfer risk in the previous host countries a firm has entered is positively correlated with the level of transfer risk in the next host country selected for entry.

Policy Risk

Policy risks are risks related to policy instability and opportunistic behavior by government at the macro level. Scholars and risk insurers often group policy risk with bureaucratic risk as "risk of adverse government action," but I distinguish between risks associated with low bureaucratic quality and opportunistic behavior by individual government officials and the risks associated with government policy. These policy risks include changes in laws, regulations and tax rates, nationalizations of private property, and government breach of contract. The two key differences between policy risk and bureaucratic risk are the degree to which individual firms can affect the behavior of the political actors in question, and the degree to which each risk affects domestic firms vs. foreign firms – i.e. the degree to which there is a significant liability of foreignness with regard to each risk type.

Bureaucratic risk is particularly amenable to effective mitigation by capable firms because it more often involves local government actors, such as inspectors, police officers, and clerks, while policy risk involves actors at the national level: presidents, legislatures, and supreme courts. Large firms may have the ability to engage in effective non-market strategies at this level, such as lobbying, campaign contributions, or bribery, but most firms cannot individually affect national policy. As with transfer risk, however, mitigation is still possible. Firms can invest in information-gathering capabilities that allow them to anticipate policy changes before they are announced, or they can partner with domestic firms and other stakeholders to reduce their vulnerability to nationalization or other firm-specific government actions.[6]

Allying with domestic firms can reduce a firm's exposure to policy risk because the cost of government expropriation of assets, creeping or outright, is higher with regard to foreign firms than domestic firms. Costs are higher because domestic firms are constituents of the government, and because they are more likely than foreign firms to source products from other domestic firms, making their failure more economically damaging (Henisz 2000, pp. 337-339). This implies that domestic firms are more protected from policy risks than are foreign firms.

These two factors lead me to predict both that the host-country effects of policy risk are likely smaller than those for bureaucratic risk, and that the home country effects are likely to be non-existent. To the degree that host-country effects do

[6] Markus (2012) has an excellent discussion of how firms can ally with diverse stakeholder groups to reduce their vulnerability to government violations of property rights.

exist, they are likely to exist primarily for industries that are heavily regulated, inducing even domestic firms to develop capabilities in lobbying and campaigning for particular policies, and for very large firms, who are capable of engaging in these sorts of activities effectively.

Hypothesis 3a: Similarity between home and host country with regard to policy risk is NOT positively correlated with higher flows of bilateral FDI.

Hypothesis 3b: The average policy risk in the previous host countries a firm has entered is positively correlated with the policy risk in the next host country selected for entry.

Hypothesis 3a directly contradicts the findings of Holburn and Zelner (2010). There are two reasons for this. First, I argue that the Holburn and Zelner finding is likely driven by the fact that policy risk is highly collinear with bureaucratic risk, which Holburn and Zelner do not control for in their analysis. I expect (and find) large home-country effects regarding bureaucratic risk, effects which are large enough to explain the Holburn and Zelner finding, even if there is no independent home country effect of policy risk.

Second, Holburn and Zelner conduct their analysis in the electric power generation industry, an industry that is extremely heavily regulated, and populated primarily by very large firms. If home country effects for policy risk are, in fact,

present within this industry, it is still likely that such effects do not exist in other industries that are either more lightly regulated or populated by smaller firms incapable of effective lobbying at the national level.

Risk of Adverse Regime Change

All four of the categories of risk discussed above are risks based on the nature of the current political order and can be managed, to some degree, by firms that have developed the necessary skills to navigate that political order. The risks posed by adverse regime change are different; the risk of adverse regime change is the risk that the entire institutional order may be overturned. The nature of the new political order that will replace the current order if it is overthrown is difficult to predict, and the very firms that were successful in mitigating various political risks under the previous regime are likely to be precisely those firms targeted by the new regime. This is particularly true when firms contract directly or engage in joint-ventures with the government or are otherwise perceived to be allies of the current regime.

The risk of adverse regime change is a purely exogenous hazard that firms cannot substantially mitigate once it has been realized, i.e. once a regime is overthrown.

Hypothesis 4a: Similarity between home and host country with regard to the risks of adverse regime change (i.e. coups and revolutions) are NOT positively correlated with higher flows of bilateral FDI.

Hypothesis 4b: The average risk of adverse regime change (i.e. coup or revolution) in the previous host countries a firm has entered is NOT positively correlated with the risk of adverse regime change in the next host country selected for entry.

There is an important caveat when assessing the effect of risk of adverse regime change on investment behavior. When a regime is overthrown, this hurts most MNCs that are already operating in the country at the time of the overthrow, but it also opens up new opportunities for investment. These post-overthrow opportunities include privatization sales, new opportunities for contracts and joint ventures with the government, and the exploitation of economic opportunities opened up by the new regime more generally. Firms may become specialized in seizing these opportunities, even if they have no particular advantage in managing the risk of future regime turnover. Therefore, even though recent experience of a coup is a good predictor of risk of future coups, I do not include this variable in the analysis.

Risk of War and Political Violence

War and political violence pose a range of risks to foreign firms: employees may be killed or injured, property may be destroyed, transport may be disrupted, and the local economy is often crippled in both the short and long-term (e.g. Collier 1999; Ghobarah, Huth, and Russett 2003). The risk of war and political violence are closely related to the risk of adverse regime change – regime overthrow is often accompanied by war and/or political violence. However, the risks posed by violence are distinct from the risks posed by adverse regime change and many cases of civil war and

political violence, particularly secessionist conflicts, occur in situations in which the risk of overthrow is quite low but the risk of business disruption is substantial.[7]

While adverse regime change overturns the entire political order, civil war and political violence are more accurately conceived of as aspects of the current political order. As with bureaucratic risk, transfer risk, and policy risk, firms can invest in capabilities to manage these risks. They can employ private security forces, invest in gathering information, minimize their geographic dispersion and dependence on intra-country transport, and rely more heavily on local, rather than expatriate, employees. Therefore, I expect firms that have experience operating in countries plagued by war or political violence, either in their home country or in previously-entered host countries, to develop skills in managing these risks that give them advantages in managing similar risks in other countries.

One complicating factor is that war and political violence often trigger capital flight. While the level of corruption in a country tends to move slowly and to be stable over time, acute episodes of war and political violence often begin and end abruptly. Firms and private individuals that are living and operating in a peaceful country in year t, may find themselves living in war zone in year t+1, and they may respond to this sudden shift by transferring assets abroad, either into banks, or into direct investments overseas. This type of FDI-as-capital-flight will tend to result in flows from war-torn countries to countries with low levels of war risk.

[7] Goldstone et al. make this distinction well (2010: pp. 191-192).

This FDI-as-capital-flight flows to precisely the opposite type of countries as flows of FDI generated by firms moving to exploit their learned capabilities in managing war risk. Learned capabilities induce firms based in countries that are or have recently experienced war or political violence to invest in other similarly violence-plagued states; FDI-as-capital-flight creates flows of investment from war-torn states to stable, non-violent destinations. The relative expected magnitude of these two competing effects is unclear, but if they are roughly similar, similarity in war risk between home and host country will have no effect on bilateral FDI flows.

Hypothesis 5a: Similarity between home and host country regarding the presence of ongoing war or political violence does NOT have a clear relationship with bilateral flows of FDI.

Hypothesis5b: The proportion of previous host countries a firm has entered that are currently experiencing civil war or severe political violence is positively correlated with the probability that the next host country selected for entry is currently experiencing civil war or severe political violence.

Dependent Variable and Sample for Analysis of Home Country Effects

The dependent variable, *FDI Inflows*, is measured bilaterally as net flows of FDI from the home country to the host country in 2009. These data are taken from the International Monetary Fund's (IMF) 2009 Coordinated Direct Investment Survey

(CDIS); the raw values are measured in millions of USD.[8] I use a logged dependent variable, which is created as *Log_FDI* = log(*FDI Inflows* + 1).[9]

The CDIS was released in June 2011 with data for 2009 only. Prior to this release, the only bilateral FDI data available was from the Organization for Economic Co-operation and Development (OECD), and included only OECD members as home countries. The CDIS data is well suited to this analysis because it includes South-South dyads, and data on high-risk states as both home and host countries. While unique in providing bilateral data on South-South FDI flows, the CDIS data is plagued by the same problem as other bilateral FDI sources, namely nonrandom missing data where data is most likely to be missing in dyads where the reporting countries are poor and/or have limited bureaucratic capacity.[10]

Dependent Variable and Sample for Analysis of Previous Host Country Effects

To assess the effect of previous host-country experience on the selection of new host countries, I use a firm-level dataset of 4,743 entries into 73 foreign countries by 512 Japanese manufacturing firms from 1928-1993, with almost all entries occurring between 1965 and 1991. I restrict the analysis to a firm's first entry into a given host country, which limits the sample to 1,988 entries.[11] The dependent variable is the level of political risk in the host country being entered. For most types of risk

[8] While some of the related literature (e.g. Ahlquist 2006, Buthe and Milner 2008) uses FDI/GDP as a dependent variable, this would be in appropriate here because the concept of interest is the ability of a country to attract FDI, not the dependence of a country's economy on FDI (Li 2009).

[9] The raw data on bilateral FDI inflows includes approximately 100 negative observations out of 6200 total observations. Results are robust to an alternate specification of Log_FDI = ± log|(FDI inflows +1)|

[10] In future versions of the paper I intend to test the robustness of these results to multiple imputation of missing values.

[11] Davidson (1980) and others have shown that previous experience in a given host country is an important determinant of future entries.

this is continuous, for others it takes the form of a dummy variable for whether the host country being entered has experienced a particular type of event, such as a war, in the recent past.

The dataset of firm entries is limited to firms based in a single home country (Japan), and a single broad sector (manufacturing). This holds many potentially confounding variables constant. Unfortunately, it also limit the external validity of the results, but this is somewhat offset by the fact that Japan was an early and prolific investor in developing markets (Belderbos and Sleuwagen 1996; Delios and Henisz 2000), making foreign entries by Japanese firms a compelling universe of cases in its own right. Also appropriate for this study, Japanese firms have been described as taking a capabilities-based approach to investing (Chang 1995).

Independent Variables of Interest

I distinguish theoretically between five types of political risk: bureaucratic risk, transfer risk, policy risk, risk of war and political violence, and risk of adverse regime change. I treat each type of risk is a latent variable, for which I have gathered various observable measures. In the section that follows I analyze the effects of both these individual measures of risk, and indices created to capture each category of risk.

Individual Measures

Bureaucratic risk and policy risk, in particular, are substantially collinear. Countries in which opportunistic behavior by policymakers is effectively constrained are often characterized by similarly effective constraints on bureaucrats. Therefore, in measuring each of these types of risk, I look for narrow and specific measures of

constraints on specific actors, rather than looking at broad measures of government effectiveness or the rule of law.

For bureaucratic risks, I use measures of corruption, judicial efficiency, and customs efficiency. The measures of corruption are the Corruption Perceptions Index from Transparency International (TI) and the corruption component of the International Country Risk Guide (ICRG) assessments. The TI measure is based on expert opinions and survey responses, while the ICRG measure is based solely on expert assessments. The measures of judicial efficiency are the cost of enforcing a contract using the courts (as a % of the claim) and the speed of those courts, measured as the number of days (logged) from the filing of the plaintiffs suit until payment is made. The measures of customs efficiency are the (average) costs per container to import and export goods, as well as the number of days (logged) needed to complete the necessary legal steps to do so.[12] All four of these measures come from the World Bank's Doing Business Indicators.

An expert assessment of transfer risk comes from the *Office National Du Ducroire* (ONDD). ONDD is the worlds largest political risk insurer and the price leader in the industry. Its assessments of risk reflect not only profit-motivated expert attempts to assess risk, but also the actual insurance costs paid by firms who wish to be protected from these risks (Graham 2010; Jensen 2008). The ONDD measure of transfer risk measures the risk that action by foreign authorities, such as the

[12] The World Bank measures the time and cost for importing and exporting separately: I sum the import and export measures to get measures of overall customs efficiency.

introduction of capital controls or other constraints, prevents the transfer of money necessary to repay creditors.

Policy risk is measured both as constraints on the executive, and by the level of political competition. *Political Constraints* is a measure of constraints on the executive based on a spatial model of the preferences of veto players, which comes from Witold Henisz (2000). Political Constraints considers the number of branches of government, the preference alignment between each legislative house and the executive, and the presence of constraints from the judiciary and sub-national governments.[13] *Executive Constraints* and *Political Competition* are component variables of the Polity IV democracy score.

The risk of war and political violence is captured by expert assessments and by binary variables measuring whether war is currently occurring or whether war has ended in the past five years. The measures of current and recent war are taken from the Political Instability Task Force, whose severity codings encompass both the number of fatalities and the geographic spread of a conflict, both of which affect the level of economic impact from a conflict (Goldstone et al. 2010). Expert assessments of war risk are taken from ONDD and ICRG.[14] I also use a measure of the security of physical integrity rights from the CIRI human rights datasets, which measures the government's engagement use of torture, extra-judicial killings, political imprisonment, and disappearances.

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[13] For political constraints, I use data from 2007, the most recent year available.
[14] I create a single measure of ICRG war risk as the sum of the risk of internal conflict and the risk of external conflict.

To assess the risk of adverse regime change, I use a measure of political stability from the World Governance Indicators that captures the likelihood that a regime is "destabilized or overthrown by unconstitutional or violent means."[15]

The independent variables used in the analysis are measures of the similarity between home and host country across each of the dimensions discussed above. These similarity measures are created by taking the absolute value of the difference in a given variable between home and host country and subtracting this value from zero.

Control Variables

Gravity model variables are strong determinants of flows of FDI (e.g. Benassy, Coupet and Mayer 2007), leading FDI to flow primarily to states near the home country. Relevant elements of distance include not only geographic distance, but also cultural distance, economic distance, and political and administrative distance, which includes the elements of political risk at the core of this study (Ghemawat 2004). In order to evaluate the effect of similarity in political risk effectively, I need to control for other aspects of distance that may both affect FDI flows and be correlated with similarity in political risk. For geographic distance I use the logged distance between the capitals of the home and host country and a dummy variable for a shared border. For cultural distance I use the logged number of migrants from the host country living in the home country and dummy variables for a common language, a previous colonial relationship, and a common colonizer. For economic distance I measure similarity in GDP per capita and similarity in educational attainment; both are continuous. I also

[15] This quote is drawn from data documentation available online at http://info.worldbank.org/governance/wgi/pdf/pv.pdf (Accessed May 3, 2012). See also Kaufmann, Kraay, and Mastruzzi (2009).

construct a dummy variable for whether both home and host country are members of the European Union. For an element of political and administrative distance that does not fall directly under the heading of political risk, I also control for common legal origin, a dummy variable.[16]

The measures of distance, common border, common language, common colonizer, common legal system and prior colonial relationship are taken from Head, Mayer, and Ries (2010), courtesy of the *Centre d'Etudes Prospectives et d'Informations Internationales* (CEPII). Data on GDP per capita is taken from the World Development Indicators (WDI). Education data are taken from Barro and Lee (2010) who measure attainment for each country in both 2005 and 2010. Data for 2009 is constructed via linear interpolation between these two values.

In the firm-level analysis I control for the number of prior entries by the subsidiaries of the same firm. Firms with more international experience are expected to be more tolerant of novel environments than less-experienced firms (Davidson 1980). I also include dummy variables for 14 industry categories. All the firms in the sample are manufacturing firms, but firms may vary in their risk tolerance across industries.

Factor Analysis: Creating Indices for Each Type of Political Risk

My theory identifies five distinct types of political risk: bureaucratic risk, policy risk, transfer risk, risk of war and political violence, and risk of adverse regime change. These types of risk are theoretically distinct, but expected to be highly co-

[16] Common legal origin relies on data from La Porta, Lopez-de-Salinas, and Shleifer (2008).

linear, making them difficult to distinguish empirically.[17] I employ factor analysis as a means of both producing a single measure for each type of risk and of verifying that the various measures of political risk cluster as they are expected to. The correlations are expected to be higher between variables measuring the same risk type than between variables measuring distinct risk types.

The section above identifies 15 measures of political risk, each of which I argue is associated with one of these five risk types. Two of these types of risk are captured by only a single measure: transfer risk by the ONDD expert assessment of transfer risk, and risk of adverse regime change, captured only by the political stability measure from the World Governance Indicators. These variables are omitted from the factor analysis, leaving 13 measures that are theoretically associated with three latent variables, bureaucratic risk, policy risk, and risk of war and political violence.[18]

[17] Many of the political risk measures in the literature capture more than one of these risk types, and measures of theoretically distinct risks are often highly correlated with one another.

[18] If these variables are included in the analysis, the measure of transfer risk loads on the bureaucratic risk factor and the measure of political stability loads on the violence factor. Including these measures in the indices of these factors does not substantively change the regression results presented – the alternative indices are correlated with the original indices at >0.96.

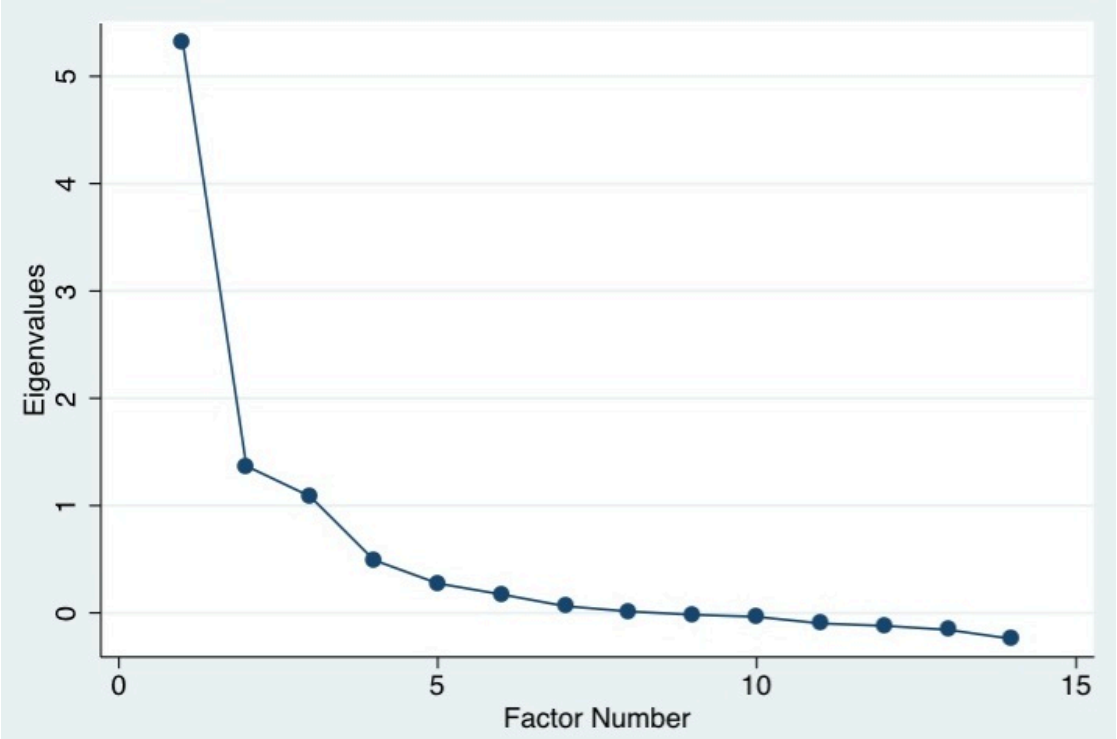

Figure 2.1: Scree Plot From Principal Factor Analysis

Figure 2.1 presents a scree plot drawn from a principal factor analysis of all 13 measures in a sample of 121 countries in 2009. The plot shows that, as expected, most of the variance in these measures can be accounted for by three primary factors; the eigenvalues for each remaining factor are less than 0.5. These three primary factors are then rotated, using a varimax rotation, and these rotated results are shown in Table 2.1.[19] Factor loadings of greater than 0.4 are shown in bold.

[19] The decision to rotate three factors is made based on the scree plot; remaining factors are left unrotated and are omitted from Table 2.1.

Table 2.1: Types of Political Risk: Factor Loadings for Three Primary Factors

	Bureaucratic Risk	Policy Risk	War and Violence	Uniqueness
Corruption (TI)	**0.76**	0.27	**0.41**	0.11
Corruption (ICRG)	**0.70**	0.25	0.28	0.27
Court Costs (DBI)	**0.41**	-0.03	0.06	0.79
Court Delays (DBI)	0.17	-0.12	0.38	0.71
Trade Delays (DBI)	**0.88**	0.22	0.14	0.14
Trade Cost (DBI)	**0.68**	0.03	-0.03	0.39
Political Constraints (Henisz)	0.36	**0.61**	0.18	0.39
Executive Constraints (Polity)	0.23	**0.87**	0.12	0.16
Political Competition (Polity)	0.13	**0.85**	0.11	0.24
War Risk (ONDD)	**0.53**	0.34	**0.54**	0.26
Civil War Risk (ICRG)	0.30	0.17	**0.74**	0.32
Post-Conflict Status (PITF)	-0.03	-0.13	0.21	0.72
Current War (PITF)	0.01	-0.05	**0.65**	0.47
Physical Integrity Violations (CIRI)	0.31	0.31	**0.72**	0.25

The factor loadings on the various measures of political risk match well with theoretical expectations. While several measures load on more than one factor, all measures that load heavily on any factor load most heavily on the measure with which they are theoretically associated, confirming the appropriateness of the measures in the study.

Among the variables that load substantially (>0.4) on more than one measure of risk, the TI measure of corruption loads primarily on bureaucratic risk (0.76), as expected, but also loads on policy risk (0.41). The ICRG measure of war risk loads most heavily on War and Violence (0.54), but almost as heavily on bureaucratic risk (0.53). Court Delays (i.e. the log of the number of days it takes to resolve a contract dispute through the legal system) and Post-Conflict Status, (i.e. a dummy variable for

whether a country is in the first five years following the end of a war that has not re-started), do not load heavily (>0.4) on any factor, though court delays loads almost this heavily on the risk of war and violence. This indicates that court delays and post-conflict status are probably not good measures of bureaucratic risk and risk of war and violence. However, results are robust to the exclusion of these measures from the indices.[20]

Table 2.2: Pairwise Correlations Between Risk Types

Variables	Bureaucratic Risk	Transfer Risk	Policy Risk	Overthrow Risk	War Risk
Bureaucratic Risk	1.000				
Transfer Risk	0.393	1.000			
Policy Risk	0.031	0.171	1.000		
Overthrow Risk	0.132	0.289	0.076	1.000	
War Risk	-0.030	0.106	0.046	0.713	1.000

Table 2.2 shows the pairwise correlations between the five types of risk.[21] Overall, these five types of risk are less collinear than one might expect, with most of the correlations at less than 0.3. Risk of adverse regime change (i.e. overthrow risk) and risk of war and political violence (i.e. war risk) are correlated at 0.7, indicating that there is some difficulty distinguishing these two concepts empirically. Transfer risk and bureaucratic risk are correlated at 0.4, indicating some potential for multi-collinearity in the analysis for these variables as well.

Table 2.3 gives summary statistics for all variables used in the analysis.

[20] If the results in Table 2 are reproduced using indices based on factor analyses from which these two variables are excluded, the results are unchanged, with z-scores >4 on Similarity in Bureaucratic Risk in all models.

[21] This anlaysis is conducted on the same sample as the factor analysis.

Table 2.3: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Bilateral FDI Flows (Billions of USD)	2.785	20.473	-2.961	592.624	6159
Bilateral FDI Flows (Logged)	8.192	9.409	0	27.108	6055
Sim. in Bureaucratic Risk (Index)	-1.085	0.784	-4.1	0	3805
Sim. in Adverse Gov Action (Index)	-0.857	0.856	-3.586	0	3805
Sim. in Violence & Overthrow (Index)	-1.047	0.896	-4.712	0	3805
Sim. in Corruption (TI)	-2.718	1.939	-8.300	0	5651
Sim. in Corruption (ICRG)	-1.402	1.139	-6	0	4438
Sim. in Court Delays (DBI)	-0.547	0.408	-2.263	0	5720
Sim. in Court Costs (DBI)	-17.005	24.097	-154.4	0	5720
Sim. in Import/Export Costs (DBI)	-1380.152	1499.026	-10317	0	5720
Sim. in Import/Export Delays (DBI)	-0.804	0.59	-3.011	0	5720
Sim. in Transfer Risk (ONDD)	-2.567	1.858	-6	0	6040
Sim. in Political Constraints (Henisz)	-0.335	0.285	-0.895	0	5293
Sim. in Executive Constraints (Polity)	-2.144	2.024	-6	0	4969
Sim. in Political Competition (Polity)	-3.171	2.98	-9	0	4969
Sim. in Internal Conflict Risk (ICRG)	-1.672	1.267	-8.5	0	4438
Sim. in War Risk (ONDD)	-1.606	1.469	-6	0	5873
Sim. in Current Conflict (PITF)	-0.203	0.402	-1	0	6159
Sim. in Post-Conflict Status (PITF)	-0.012	0.108	-1	0	6159
Sim. in Political Stability (WGI)	-1.035	0.820	-4.755	0	6126
Sim. in Physical Integrity Violations (CIRI)	-2.518	1.994	-8	0	5790
Sim. in Wealth (GDP Per Capita, Logged)	-13.208	12.842	-103.84	-0.004	5780
Sim. in Educational Attainment (Barro & Lee)	-3.117	2.429	-11.302	-0.003	4511
Bilateral Trade Flows (logged)	3.857	3.124	0	12.974	3070
Common Border	0.029	0.169	0	1	6011
Colonial Relationship	0.03	0.172	0	1	6011
Common Colonizer	0.046	0.209	0	1	6011
Common Legal Origin	0.27	0.444	0	1	6011
Common Language (10% of Pop)	0.129	0.335	0	1	6011
Distance (logged)	8.542	0.913	4.088	9.894	6011
Both Countries in EU (dummy)	0.089	0.285	0	1	6159

Specification of the Model Analyzing Home-Country Effects

The effect of similarity in political risk between home and host country is assessed using an OLS regression with dummy variables for host country ID and home country ID and robust standard errors. The model estimated takes the form of:

$$Y_{ij} = \alpha_i + \alpha_j + \beta_1 Xsim_{ij} + \beta_k controls_{ij} + \varepsilon_{ij} \quad (2.1)$$

where Y_{ij} is the (logged) FDI flow from country *i* to country *j* in 2009; α_i denotes a dummy variable for the home country (from which the FDI originates); α_j denotes a dummy variable for the host country (into which the FDI flows); X_{sim} is the difference in the level of political risk between country *i* and country *j* in year *t;* $controls_{ij}$ is a vector of *k* covariates, and ε_{ij} is the error term.

Specification of the Model Analyzing Host-Country Effects

To analyze the effect of previous host-country experience on a firm's propensity to enter states with a given level of political risk, I use the mean level of political risk in the countries a given firm has previously entered to predict the level of risk in the country currently being entered. The unit of analysis is the firm-entry, and I compare risk in the country being entered with risk in the other host countries in which the firm is operating. For entries in year *t,* I assess the level of risk in previously entered host countries also in year *t.* As noted above, the sample is restricted to first entries by a given firm in a given country. The later addition of new subsidiaries into a previously-entered country are not considered, as the firm already has country-specific knowledge for managing political risk in that country.

The model estimated takes the form of:

$$Y_{it} = \alpha + \beta_1 * (\sum_{1 \to n} \frac{Risk_i}{n}) + \beta_2 X_2 + \beta_k X_k + \varepsilon_{ijt} \qquad (2.2)$$

Y_{it} is the level of political risk in country *i,* entered by firm *j* in year *t.* The independent variable of interest is the mean level of risk across *n* host countries that firm *j* has entered previously. X_2 is the (logged) number of previous entries by firm *j,*

X_k is a vector of industry dummy variables and ε_{ijt} is the error term, which is clustered by firm ID.

An alternative specification involves conditional logit, the same technique used to model an individual's choice been modes of transportation. In a dataset of firm entries, the question is why a firm chooses one country as opposed to any other country, or more tractably, why a firm chooses to enter a country in one risk category instead of a country in any other risk category. This alternative analysis is not yet completed.

Results: Analyzing Home-Country Effects

Table 2.4 shows the effect of similarity in political risk profile between home and host country and bilateral flows of FDI. Hypotheses 1a predicts a positive relationship between similarity in bureaucratic risk between home and host countries and bilateral FDI flows. Hypothesis 2a makes a parallel prediction regarding similarity in transfer risk. Hypotheses 3a, 4a, and 5a predict no significant relationship with regard to similarity in policy risk (3a), risk of adverse regime change (4a) and risk of war and political violence (5a).

Consistent with H1a, similarity in the level of bureaucratic risk across countries is an important predictor of bilateral FDI across all models. Consistent with Hypotheses 3a, 4a, and 5a, similarity in policy risk, risk of war and political violence, and risk of adverse regime change (i.e. overthrow) are not significant predictors of bilateral FDI flows. Contrary to Hypothesis 2a, however, similarity in transfer risk is also not a strong predictor of bilateral FDI flows.

Table 2.4: Political Risk Similarity and FD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sim. in Bureaucratic Risk (Index)	0.824***					0.847***	1.433***
	(0.183)					(0.184)	(0.284)
Sim. in Policy Risk (Index)		0.067				0.152	0.253
		(0.279)				(0.279)	(0.722)
Sim. in War and Pol. Violence (Index)			0.116			0.037	0.317
			(0.198)			(0.270)	(0.457)
Sim. in Transfer Risk (ONDD)				-0.018		-0.141	0.302
				(0.118)		(0.135)	(0.272)
Sim. in Adv. Regime Change (WGI)					0.013	0.201	0.385
					(0.163)	(0.270)	(0.442)
Distance (logged)	-3.073***	-3.122***	-3.117***	-3.121***	-3.182***	-3.044***	-0.978***
	(0.193)	(0.193)	(0.194)	(0.169)	(0.168)	(0.194)	(0.365)
Common Border	-0.502	-0.489	-0.489	-0.319	-0.302	-0.478	0.160
	(0.595)	(0.598)	(0.598)	(0.552)	(0.549)	(0.594)	(0.651)
Colonial Relationship	3.324***	3.254***	3.254***	3.413***	3.245***	3.306***	1.807***
	(0.532)	(0.527)	(0.528)	(0.517)	(0.521)	(0.534)	(0.591)
Common Colonizer	2.361***	2.218***	2.195***	2.054***	1.985***	2.391***	0.979
	(0.708)	(0.709)	(0.711)	(0.508)	(0.506)	(0.711)	(0.956)
Common Language (10% of Pop)	0.664*	0.664*	0.664*	0.569*	0.659**	0.659*	-0.098
	(0.376)	(0.374)	(0.375)	(0.318)	(0.315)	(0.375)	(0.485)
Both Countries in EU (dummy)	0.563	0.366	0.344	0.524	0.349	0.603	0.089
	(0.479)	(0.481)	(0.481)	(0.449)	(0.440)	(0.499)	(0.655)
Sim. in Wealth	-0.239	0.087	0.083	0.267*	0.228*	-0.182	-0.623*
	(0.152)	(0.137)	(0.138)	(0.154)	(0.128)	(0.198)	(0.350)
Sim. in Educational Attainment	0.176**	0.150*	0.141	0.162**	0.168**	0.160*	-0.051
	(0.087)	(0.088)	(0.089)	(0.073)	(0.073)	(0.089)	(0.153)
Common Legal Origin							1.111***
							(0.326)
Bilateral Trade Flows (logged)							1.646***
							(0.165)
Bilateral Migrant Stock (Logged)							0.074
							(0.088)
Home and Host Country Dummies	YES	YES	YES	YES	YES	YES	YES
Constant	29.252***	34.011***	29.630***	30.366***	30.922***	28.767***	12.633***
	(3.663)	(3.695)	(3.504)	(1.893)	(3.305)	(3.756)	(3.302)
Observations	3210	3210	3210	4168	4227	3210	1416
R^2	0.748	0.746	0.746	0.757	0.756	0.748	0.808

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

All models are estimated using OLS regression with dummy variable fixed effects for both home and host countries and Huber/White standard errors. The fixed effects pick up all country-level characteristics, such as size, wealth, and political conditions, that affect the volume of FDI flows into a given host country or out of a given home country. Additional controls include standard gravity model variables (distance, common language, etc) as well as similarity between home and host

countries in terms of both wealth and educational attainment. These last two controls are important because similarity in these factors is likely to be correlated both with greater FDI flows, as firms enter countries with markets similar to their home market, and because these similarities are correlated with similarities in political risk. Results are also robust to the inclusion of common legal origin and bilateral trade flows as controls.[22]

In substantive terms, a one-standard-deviation (0.78 point) increase in similarity in bureaucratic risk is associated with a 108% increase in bilateral FDI flows.[23]

Bureaucratic Risk: Disaggregated Results

Hypothesis 1a states that similarity between home and host country with regard to the level of bureaucratic risk is positively correlated with higher flows of bilateral FDI. Table 2.4 uses an index of bureaucratic risk to show that similarity in bureaucratic risk between two countries is associated with higher bilateral FDI flows. Table 2.5 examines the effect of similarity across individual component measures of bureaucratic risk. The results in Table 2.5 provide evidence consistent with Hypothesis 1a and the findings in Table 2.4. While not all results are statistically significant, many are, and all estimated effects are positive, i.e. similarity between home and host country across each individual measure of bureaucratic risk is associated with higher bilateral FDI flows.

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[22] I do not consider these variables as appropriate for inclusion in the main model because common legal origin overlaps conceptually with similarity in bureaucratic risk (deriving from efficient judicial resolution of civil disputes) and because political risk affects importers and exporters in ways similar to how it affects foreign investors (Gillespie 1990).

[23] Based on the coefficient in Model 6.

Table 2.5: Similarity in Bureaucratic Risk (Disaggregated)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sim. in Corruption (TI)	0.113	0.418***										
	(0.101)	(0.144)										
Sim. in Corruption (ICRG)			0.094	0.255								
			(0.136)	(0.205)								
Sim. in Court Delays (DBI)					0.494	0.682						
					(0.322)	(0.437)						
Sim. in Court Costs (DBI)							0.043***	0.061***				
							(0.015)	(0.019)				
Sim. in Import/Export Costs (DBI)									0.330**	0.751***		
									(0.149)	(0.260)		
Sim. in Import/Export Delays (DBI)											0.618**	1.326***
											(0.274)	(0.417)
Sim. in Policy Risk (Index)	0.056	0.129	0.075	0.168	0.074	0.144	0.120	0.243	0.065	0.100	0.078	0.226
	(0.282)	(0.739)	(0.280)	(0.738)	(0.280)	(0.735)	(0.280)	(0.740)	(0.280)	(0.738)	(0.279)	(0.732)
Sim. in War and Pol. Violence (Index)	0.059	0.298	0.039	0.162	0.012	0.126	0.028	0.142	0.001	0.136	0.040	0.186
	(0.273)	(0.473)	(0.272)	(0.472)	(0.273)	(0.471)	(0.272)	(0.468)	(0.273)	(0.466)	(0.272)	(0.464)
Sim. in Transfer Risk (ONDD)	-0.150	0.187	-0.155	0.128	-0.143	0.151	-0.136	0.154	-0.171	0.163	-0.155	0.207
	(0.135)	(0.270)	(0.136)	(0.271)	(0.135)	(0.271)	(0.136)	(0.269)	(0.137)	(0.273)	(0.135)	(0.272)
Sim. in Political Stability (WGI)	0.063	0.122	0.120	0.447	0.139	0.510	0.153	0.546	0.197	0.514	0.147	0.447
	(0.275)	(0.459)	(0.269)	(0.453)	(0.269)	(0.453)	(0.268)	(0.449)	(0.270)	(0.453)	(0.269)	(0.447)
Sim. in Wealth	0.031	-0.453	0.134	0.032	0.177	0.155	0.140	0.075	0.179	0.155	0.024	-0.280
	(0.229)	(0.380)	(0.200)	(0.336)	(0.185)	(0.316)	(0.185)	(0.313)	(0.185)	(0.319)	(0.198)	(0.348)
Sim. in Educational Attainment	0.145	-0.088	0.148	-0.069	0.134	-0.092	0.140	-0.120	0.150*	-0.070	0.139	-0.091
	(0.090)	(0.151)	(0.090)	(0.153)	(0.090)	(0.153)	(0.090)	(0.153)	(0.090)	(0.154)	(0.090)	(0.152)
Common Legal Origin		1.216***		1.223***		1.209***		1.240***		1.262***		1.178***
		(0.327)		(0.328)		(0.327)		(0.326)		(0.327)		(0.328)
Bilateral Trade Flows (logged)		1.599***		1.618***		1.620***		1.640***		1.643***		1.644***
		(0.166)		(0.168)		(0.167)		(0.167)		(0.167)		(0.166)
Bilateral Migrant Stock (Logged)		0.102		0.106		0.111		0.111		0.099		0.093
		(0.089)		(0.089)		(0.089)		(0.088)		(0.088)		(0.089)
Gravity Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Home and Host Country Dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	28.924***	11.329***	29.243***	12.549***	29.384***	12.394***	36.621***	13.024***	29.644***	12.809***	28.935***	12.378***
	(3.658)	(3.346)	(3.581)	(3.340)	(3.702)	(3.330)	(4.057)	(3.320)	(3.617)	(3.352)	(3.664)	(3.334)
Observations	3210	1416	3210	1416	3210	1416	3210	1416	3210	1416	3210	1416
R^2	0.746	0.805	0.746	0.804	0.747	0.804	0.747	0.806	0.747	0.805	0.747	0.806

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

The models estimated in Table 2.5 are similar to those estimated in Table 2.4 The most robust results in this table come from the measures of similarity in import/export delays and similarity in the costs of resolving civil disputes through the court system; however, significant results are also obtained for similarity in corruption (TI) and similarity in import/export delays.

Policy Risk: Disaggregated Results

In contrast to Hypotheses 1a and 2a, Hypothesis 3a predicts no positive correlation between similarities in policy risk and bilateral FDI flows. The results in Table 2.6 show positive raw correlations between similarity and policy risk and bilateral FDI flows, but no robust results.

Table 2.6: Similarity in Policy Risk: Disaggregated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sim. in Political Constraints (Henisz)	1.201***	-1.091*	-1.747						
	(0.357)	(0.637)	(1.383)						
Sim. in Executive Constraints (Polity)				0.102*	-0.023	-0.043			
				(0.052)	(0.082)	(0.144)			
Sim. in Political Competition (Polity)							0.132***	0.104*	0.030
							(0.035)	(0.054)	(0.085)
Colonial Relationship	3.717***	3.302***	1.717***	3.622***	3.262***	1.818***	3.610***	3.257***	1.840***
	(0.490)	(0.533)	(0.587)	(0.509)	(0.536)	(0.589)	(0.508)	(0.536)	(0.591)
Common Colonizer	2.195***	2.302***	0.920	2.229***	2.426***	0.887	2.211***	2.478***	0.891
	(0.430)	(0.711)	(0.956)	(0.458)	(0.723)	(0.954)	(0.457)	(0.725)	(0.955)
Common Language (10% of Pop)	0.837***	0.635*	-0.074	0.771**	0.699*	-0.112	0.783***	0.745**	-0.113
	(0.294)	(0.376)	(0.483)	(0.301)	(0.378)	(0.486)	(0.300)	(0.378)	(0.487)
Distance (logged)	-3.036***	-3.069***	-1.005***	-3.195***	-3.057***	-0.975***	-3.191***	-3.035***	-0.975***
	(0.166)	(0.194)	(0.366)	(0.170)	(0.194)	(0.365)	(0.170)	(0.194)	(0.365)
Common Border	0.369	-0.499	0.059	0.394	-0.485	0.187	0.338	-0.514	0.178
	(0.547)	(0.593)	(0.635)	(0.553)	(0.594)	(0.649)	(0.552)	(0.596)	(0.650)
Both Countries in EU (dummy)	1.106***	0.682	0.161	0.747*	0.593	0.005	0.633	0.527	0.003
	(0.427)	(0.497)	(0.657)	(0.437)	(0.498)	(0.653)	(0.433)	(0.498)	(0.656)
Sim. in Bureaucratic Risk (Index)		0.873***	1.480***		0.815***	1.379***		0.843***	1.389***
		(0.183)	(0.286)		(0.186)	(0.286)		(0.186)	(0.286)
Sim. in War and Pol. Violence (Index)		0.015	0.267		0.006	0.192		-0.008	0.197
		(0.270)	(0.451)		(0.274)	(0.454)		(0.275)	(0.457)
Sim. in Transfer Risk (ONDD)		-0.125	0.293		-0.138	0.299		-0.159	0.299
		(0.136)	(0.274)		(0.136)	(0.276)		(0.136)	(0.275)
Sim. in Political Stability (WGI)		0.258	0.527		0.240	0.458		0.251	0.453
		(0.272)	(0.456)		(0.273)	(0.446)		(0.273)	(0.446)
Sim. in Wealth		-0.144	-0.594*		-0.170	-0.609*		-0.210	-0.624*
		(0.198)	(0.354)		(0.200)	(0.353)		(0.199)	(0.352)
Sim. in Educational Attainment		0.172*	-0.027		0.170*	-0.042		0.151*	-0.049
		(0.089)	(0.154)		(0.090)	(0.155)		(0.090)	(0.154)
Common Legal Origin			1.109***			1.065***			1.074***
			(0.325)			(0.332)			(0.330)
Bilateral Trade Flows (logged)			1.634***			1.671***			1.670***
			(0.166)			(0.165)			(0.165)
Bilateral Migrant Stock (Logged)			0.086			0.077			0.076
			(0.088)			(0.088)			(0.088)
Home and Host Country Dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	29.582***	28.996***	12.678***	27.289***	29.053***	5.489	27.656***	33.377***	5.485
	(1.927)	(3.626)	(3.289)	(2.310)	(3.633)	(3.476)	(2.300)	(3.822)	(3.477)
Observations	5140	3210	1416	4806	3172	1403	4806	3172	1403
R^2	0.749	0.748	0.809	0.748	0.747	0.807	0.748	0.747	0.807

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

While all three of the measures of similarity in policy risk are strongly correlated with bilateral FDI flows, these correlations are not robust to the inclusion of additional measures of risk similarity. This suggests that the correlations are likely spurious, arising only because of the correlation between policy risk and other risk types, such as bureaucratic risk, and not because of any independent effect of similarity in policy risk on FDI. These results are not, in and of themselves, grounds on which I can firmly reject the null hypothesis that similarity in policy risk is positively correlated with bilateral FDI flows. However, these results are consistent with Hypothesis 2a, and lend credibility to the theory.

Risk of War and Political Violence: Disaggregated Results

Hypothesis 5a predicts that similarity between home and host country in the risks of war and political violence have no clear relationship with bilateral FDI flows. In Table 2.3, I show evidence consistent with this predication. In Table 2.7, I show disaggregated results for the component measures and again find no evidence of a positive relationship.

Table 2.7: Similarity in the Risk of War and Political Violence (Disaggregated)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sim. in Internal Conflict Risk (ICRG)	0.115	0.143								
	(0.085)	(0.196)								
Sim. in War Risk (ONDD)			-0.033	0.323						
			(0.089)	(0.333)						
Sim. in Current Conflict					-0.283	0.595				
					(0.377)	(1.266)				
Sim. in Recent War							-0.262	0.885		
							(0.355)	(1.070)		
Sim. in Physical Integrity Violations									-0.022	-0.065
									(0.042)	(0.115)
Common Legal Origin		1.082***		1.133***		1.119***		1.128***		1.168***
		(0.327)		(0.325)		(0.326)		(0.327)		(0.332)
Sim. in Bureaucratic Risk (Index)		1.412***		1.441***		1.413***		1.416***		1.450***
		(0.285)		(0.286)		(0.284)		(0.284)		(0.289)
Sim. in Policy Risk (Index)		0.219		0.123		0.289		0.359		0.204
		(0.729)		(0.731)		(0.724)		(0.730)		(0.738)
Sim. in Transfer Risk (ONDD)		0.321		0.315		0.288		0.277		0.377
		(0.272)		(0.273)		(0.276)		(0.275)		(0.279)
Sim. in Political Stability (WGI)		0.446		0.436		0.577*		0.525		0.617*
		(0.412)		(0.398)		(0.327)		(0.332)		(0.344)
Sim. in Wealth		-0.625*		-0.706**		-0.621*		-0.607*		-0.730**
		(0.348)		(0.355)		(0.352)		(0.351)		(0.350)
Sim. in Educational Attainment		-0.055		-0.052		-0.042		-0.039		-0.007
		(0.153)		(0.154)		(0.155)		(0.153)		(0.154)
Bilateral Trade Flows (logged)		1.652***		1.672***		1.652***		1.650***		1.611***
		(0.165)		(0.168)		(0.165)		(0.165)		(0.167)
Bilateral Migrant Stock (Logged)		0.074		0.069		0.072		0.072		0.072
		(0.089)		(0.088)		(0.088)		(0.088)		(0.089)
Gravity Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Home and Host Country Dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	28.646***	12.891***	30.253***	5.083	31.831***	12.759***	31.840***	12.732***	26.392***	26.688***
	(3.402)	(3.290)	(3.456)	(3.632)	(3.534)	(3.298)	(3.533)	(3.298)	(2.712)	(2.720)
Observations	4348	1416	5711	1416	5926	1416	5926	1416	5537	1390
R^2	0.742	0.808	0.746	0.808	0.745	0.808	0.745	0.808	0.747	0.809

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Across a range of measures of risk of war and political violence, similarity between home and host country is not a significant predictor of bilateral FDI flows, and we see a mixture of positive and negative signs. This provides evidence consistent with the prediction of no correlation.

Results: Analyzing Host-Country Effects

Three of the predictions regarding host-country effects mirror those of home-country effects while two conflict. Bureaucratic risk and transfer risk are expected to have both home and host country effects, while the risk of adverse regime change is expected to have neither home nor host country effects. Policy risks are expected to have host country effects, but not home country effects. Policy risks are expected to affect foreign firms, who are not constituents of the regime in the host country, to a much greater degree than domestic firms, who wield greater influence over the government and are less often the subject of government expropriation. Risk of war and political violence is expected to have both home and host country effects, but because of the offsetting effects of capital flight discussed above, I predict that home-country effects are offset and not observed. Therefore, the expectation is that only host-country effects are observed in the data.

I will analyze the host-country effects of each category of risk separately.

Bureaucratic Risk

Hypothesis 1b predicts that the mean value of bureaucratic risk in the host countries in which a firm is already operating is a good predictor of the bureaucratic risk in the host countries that firm will enter next. Therefore, I expect to find that the

mean value of corruption in previously-entered host countryis predicts the level of corruption in the host country currently being entered. I do not find a strong effect; the analysis presented in Table 2.8 fails to find evidence in support of Hypothesis 1b.

Table 2.8: Bureaucratic Risk (ICRG Corruption): Firm Level Data

	(1)	(2)	(3)
Corruption (ICRG) in Prior HCs	-0.000	0.043	0.113
	(0.049)	(0.076)	(0.076)
Number of Prior Foreign Entries (logged)	-0.012	-0.020	-0.036
	(0.053)	(0.058)	(0.062)
Wealth of Prior HCs		-0.118	-0.205
		(0.125)	(0.145)
Education in Prior HCs		0.035	0.063
		(0.043)	(0.043)
Political Constraints in Prior HCs			-0.950*
			(0.491)
Current War in Prior HCs			-1.526***
			(0.504)
Industry Dummies YES	YES	YES	
Constant	3.473***	3.856***	4.879***
	(0.397)	(0.817)	(0.937)
Observations	732	698	698
R^2	0.027	0.034	0.055

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Only one of the component measures of bureaucratic risk included in the index in Table 2.3 is analyzed here because of limitations on data availability. Even the measure that is used, the ICRG measure of corruption, covers only half of the country-years in which firm entries occur. Most of the firm entries in the dataset occurred in

the 1970s and 1980s, years for which the better measures of bureaucratic risk are not available and in which cross-sectional coverage of the ICRG measure is limited. The next step in this research is to test this hypothesis on more recent firm entry data, allowing for a more definitive test of the theory. The results in Table 2.8 do not allow us to reject the null hypothesis of no host-country effects for bureaucratic risk.

Transfer Risk

Due to a lack of data on transfer risk prior to 1992, Hypothesis 2b cannot be tested at this time.

Policy Risk

In contrast with Hypothesis 3a, which predicts no home-country effects for policy risk, Hypothesis 3b predicts that previous experience managing policy risk in other host countries should increase firms' willingness to enter future host countries with high levels of policy risk (though these effects are not expected to be as strong as those for bureaucratic risk).

The results presented in Table 2.9 show no evidence of robust host country effects for policy risk. Models 1, 4 and 7 show positive and significant host country effects when the level of experience with bureaucratic risk (i.e. corruption) is not controlled for. However, these results disappear, and in fact reverse sign, when corruption is controlled for. This suggests that previous findings of learning effects with regard to policy risk are likely driven by the confounding correlation between policy risk and bureaucratic risk, and not by the independent effect of policy risk.

Table 2.9: Policy Risk: Firm Level Data

	(1) DV=PolCon	(2) DV=PolCon	(3) DV=PolCon	(4) DV=XConst	(5) DV=XConst	(6) DV=XConst	(7) DV=PolComp	(8) DV=PolComp	(9) DV=PolComp
Political Constraints in Prior HCs	0.228***	0.137**	-0.203**						
	(0.033)	(0.058)	(0.095)						
Executive Constraints in Prior HCs				0.132***	0.040	-0.030			
				(0.031)	(0.066)	(0.087)			
Political Competition in Prior HCs							0.181***	0.075	-0.001
							(0.031)	(0.058)	(0.071)
# of Prior Foreign Entries (logged)	0.009	0.011	-0.035***	0.149***	0.187***	0.095	0.128	0.183**	-0.149
	(0.008)	(0.008)	(0.011)	(0.056)	(0.056)	(0.070)	(0.084)	(0.086)	(0.108)
Wealth of Prior HCs		0.020	-0.040		0.185*	-0.488***		0.305*	-0.454
		(0.015)	(0.027)		(0.103)	(0.171)		(0.157)	(0.278)
Education in Prior HCs		0.003	0.008		0.016	0.039		0.039	0.024
		(0.007)	(0.008)		(0.052)	(0.060)		(0.081)	(0.096)
Corruption (ICRG) in Prior HCs			0.033*			0.130			0.064
			(0.017)			(0.113)			(0.167)
Current War in Prior HCs			-0.172*			-2.998***			-2.881***
			(0.098)			(0.722)			(1.079)
Industry Dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	0.415***	0.166	0.818***	3.698***	2.187***	8.058***	5.089***	2.518**	10.341***
	(0.077)	(0.127)	(0.159)	(0.641)	(0.817)	(1.129)	(0.851)	(1.143)	(1.836)
Observations	1338	1247	689	1334	1239	682	1334	1239	682
R^2	0.043	0.040	0.058	0.024	0.030	0.071	0.031	0.033	0.039

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Risk of Adverse Regime Change

Due to a lack of data on political stability (WGI) prior to 1996, Hypothesis 5b, which predicts that experience in managing the risks associated with regime overthrow does not predispose firms to enter other similarly risky countries, is not tested at this time.

Risks of War and Political Violence

The results regarding war and political violence are mixed. Firms with investments in countries currently at war are more likely to invest in future host countries that are also at war (Models 1 and 2); however, this does not seem to apply to post-conflict countries (Models 3 and 4), and the average war risk score of prior host countries is not a good predictor of the war risk of future entries (Models 5 and 6).

Table 2.10: Risk of War and Political Violence: Firm Level Data

	(1) DV=War	(2) DV=War	(3) DV=PostWar	(4) DV=PostWar	(5) DV=WarRisk	(6) DV=WarRisk
Current War in Prior HCs	2.970***	2.263*				
	(0.640)	(1.303)				
Postconflict Status in Prior HCs			-2.626	-3.206		
			(2.534)	(3.724)		
War Risk in Prior HCs					-0.082	-0.065
					(0.064)	(0.074)
# of Prior Foreign Entries (logged)	0.012	0.379**	0.024	0.007	-0.043	-0.060
	(0.094)	(0.155)	(0.101)	(0.173)	(0.128)	(0.138)
Wealth of Prior HCs	0.047	0.107	0.392**	0.687*	-0.047	-0.095
	(0.177)	(0.340)	(0.183)	(0.372)	(0.251)	(0.308)
Education in Prior HCs	-0.069	0.024	-0.149**	-0.203**	-0.108	-0.104
	(0.066)	(0.110)	(0.066)	(0.099)	(0.094)	(0.093)
Corruption (ICRG) in Prior HCs		-0.688**		-0.074		0.118
		(0.290)		(0.235)		(0.234)
Political Constraints in Prior HCs		3.415*		-0.665		-0.143
		(1.887)		(1.020)		(1.073)
Industry Dummies	YES	YES	YES	YES	YES	YES
Constant	-1.729	-4.391*	-4.364***	-5.687**	-18.607***	-18.208***
	(1.439)	(2.375)	(1.488)	(2.835)	(1.621)	(1.933)
Observations	1301	714	1301	692	698	698
R^2					0.036	0.037

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

The correlation between the proportion of previously entered host countries in which there is ongoing war and the probability that the host country currently being entered is experiencing war is strong (Models 1 and 2). This suggests that firms who are currently operating in war-torn host countries are more likely to enter additional war-torn host-countries. There is no evidence of similar host-country effects for expert assessments of war risk (Models 5 and 6) or post-conflict status (Models 3 and 4). One interpretation consistent with this evidence is that operating in countries at risk of future war or in economies recovering from war does not lead firms to invest in capabilities that allow them to manage war risk (and does not eliminate war-

vulnerable firms). Only operating in countries actually experiencing war induces these learning and selection effects.

These results are consistent with Hypothesis 5b, which predicts host-country effects for risk of war and political violence, but are not conclusive.

Interpreting Results

Taken together, these results are supportive of the theory. First, the factor analysis provides empirical evidence supportive of the theoretical distinction between the five categories of political risk.[24] Second, consistent with the theory, I find strong evidence of home-country effects for bureaucratic risk, and no evidence of home-country effects for policy risk, risk of adverse regime change, and risk of war and political violence. The host-country results are sharply limited by lack of data, but do provide evidence consistent with host-country effects for risk of war and political violence (regarding current war specifically).

However, three hypotheses are unsupported by the data: I fail to find evidence of expected home-country effects for transfer risk or of expected host-country effects for policy risk (which are expected to be weak) and bureaucratic risk (which are expected to be strong). The failure to find host-country effects for bureaucratic risk may be, in part, due to a lack of data on key measures of bureaucratic risk, and further testing is necessary before this hypothesis is rejected. Similarly, the number of observations is limited on which I could run a fully specified model of host-country effects for policy risk.

[24] This analysis could not speak to the uniqueness of transfer risk and risk over adverse regime change specifically, given that each is captured by only a single measure.

The failure to find evidence of home-country effects for transfer risk is somewhat more damning. It seems likely that transfer risk may, in fact, affect firms very differently in home and host countries, and that the skills developed to manage transfer risk in the home country are not transferable to host-countries. However, prior to embarking on a revision of theory, it is critical to test for the presence of expected host-country effects for transfer risk, one of two hypotheses that is not tested at this time due to a lack of data.

Conclusion and Next Steps

The results presented here are broadly consistent with the theory. I present a five-part typology of political risks and show that, for the three types of risk for which I have multiple measures, these measures cluster appropriately around the risk types with which they are theoretically associated. I am also find empirical support for several key hypotheses regarding the ability of firms to employ risk management capabilities developed in one country to manage similar risks in other countries. These findings constitute important progress in understanding both variations across firms in their ability to manage political risk, and variation across types of political risk in their amenability to learned management.

However, the firm level tests are limited by the age of the firm-level data; most of the firm entries in the dataset occur during years for which detailed political risk data is not available. More thorough testing of the hypotheses requires more recent firm level data. I have recently passed the penultimate hurdle in accessing data from the Bureau of Economic Analysis on foreign direct investment by US Multinationals

abroad. The US is the world's top supplier of FDI to fragile states (by volume), and this firm-level data should provide an excellent means for testing the hypotheses regarding the effect of prior host-country experience on firms' willingness to enter fragile states. I am also exploring other firm-level data options, including the OSIRIS database from Bureau Van Dijk. This additional testing is necessary before the findings presented here are sufficiently strong to justify more definitive statements regarding their theoretical and policy implications.

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Chapter 3:
Political Risk and Diaspora Direct Investment

Abstract

I argue that diasporans are better informed about the political and economic situation in the homeland than are other potential foreign investors; that higher levels of information allows diaspora investors to anticipate and respond to changes in risk more effectively; and that this makes migrant-induced foreign direct investment (FDI) more sensitive to political risk than is other FDI. I conduct empirical analysis on a dataset of bilateral FDI flows from 30 OECD countries to 105 developing countries from 1994-2008: I find that migrant-induced FDI is more sensitive to political risk than is other FDI, and particularly that migrant-induced FDI is more sensitive to decreases in political risk. This has three central implications. First, diasporans' access to information is a major factor driving the causal relationship between migrant stocks and FDI flows. Second, the effect of political risk on investment flows depends on the mix of investor types (particularly diaspora vs. non-diaspora) in the pool of potential investors from which a state is drawing. Third, diaspora direct investment has the potential to motivate and sustain governance reform and stabilization in fragile states, but it is no more likely than other FDI to substitute for, or precede, these improvements.

Introduction

A causal link between bilateral migrant stocks and bilateral flows of foreign direct investment (FDI) has been established empirically (Docquier and Lodigiani 2010; Javorcik et al. 2011; Kugler and Rapoport 2007; Leblang 2010), and the amounts in question are not small. By some estimates, diaspora direct investment (i.e. investment by migrants and their descendents) accounted for over 50% of FDI inflows to China during the 1990s (Huang 2003) and 20-30% of FDI flows into India during the same time period (Ye 2010).[1] A related literature has also shown that at least some subset of diaspora investors are socially and emotionally motivated (Gillespie, Sayre, and Riddle 2001; Nielsen and Riddle 2010). This raises the possibility that diasporans are capable of providing capital to their troubled homelands at times of high political risk when other foreign investors are scarce.

I argue that diaspora investors have access to privileged channels of information via social networks and face lower costs of monitoring publicly available information sources. Higher levels of information cause diaspora investors to respond both more strongly and more quickly to over-time changes in the level of political risk than do other foreign investors. Consistent with this theory, I present evidence that FDI flows in high-migrant dyads are more sensitive to over-time fluctuations in political risk than are flows in low migrant dyads, particularly with regard to decreases in political risk, i.e. stabilizations, conflict terminations and institutional improvements.

[1] In the Chinese case, these estimates include a substantial amount of "round-tripping" to Taiwan and Hong Kong, but the impact of diaspora investment remains substantial.

In the current economics and political science literature, political risk is generally treated as a country-level variable, ignoring variation across types of firms (e.g. Busse and Hefeker 2007; Jensen 2008; Kolstad and Villanger 2007). I argue that diasporans differ from other foreign investors in terms of both their access to private information and their attention to publicly available information, both of which affect their response to political risk. If diaspora direct investment responds differently to political risk than other types of FDI, then the effect of political risk on investment flows is moderated by the mix of investors types (i.e. diaspora vs. non-diaspora) in the pool of potential investors from which a country is drawing.

In the empirical sections of this paper, I present evidence consistent with my argument that diaspora direct investment is more sensitive to variations in the level of political risk than is other FDI. These findings imply that, while diaspora direct investment is likely not a reliable source of capital for the world's most fragile states, it can serve both as a complement to, and as an incentive for, improvements in governance: diaspora investors will be among the investors with the strongest response to governance improvements, giving diaspora investment the potential to motivate and sustain important reforms, and contribute to the overall stabilization and growth of the economy.

The paper is organized as follows: First, a brief review of the relevant literature including the theory and evidence in economic sociology that supports the hypothesis that diaspora-induced FDI is less sensitive to political risk than is other FDI. I then present a theoretical argument for why I expect diaspora direct investment to be more

sensitive to political risk than other FDI, and proceed to test these competing argument empirically using data on dyadic (pairwise) FDI flows from 30 OECD countries to 105 developing countries from 1994-2008. This is followed by a discussion of theoretical implications, policy implications and conclusions.

The Current Literature

The empirical literature on political risk and foreign investment generally treats political risk as a country-level variable, and ignores important variation between types of foreign firms. This literature finds that political risk in a developing country negatively affects its ability to attract FDI (Schneider and Frey 1985; Globerman and Shapiro 2003; Busse and Hefeker 2007; Desbordes 2009), and identifies a variety of risk-reducing institutional characteristics and arrangements that help attract FDI to developing countries.[2] These include democracy (Li and Resnick 2003; Jensen 2008);[3] constraints on the executive (Henisz 2000); federalism (Jensen and McGillivray 2005); bilateral investment treaties (Elkins, Guzman, and Simmons 2006; Kerner 2009; Allee and Peinhardt 2010); and multilateral and bilateral trade agreements (Büthe and Milner 2008).

I argue, however, that the tacit assumption that political risk affects all types of foreign firms equally is a faulty and costly one. If different types of firms vary in their ability to manage political risk, then these types firms can be expected to vary in their

[2] Bevan and Estrin (2004) and Sethi et al. (2003) do not find a relationship between political risk and FDI.
[3] ONeal (1994) fails to find a relationship between regime type and FDI inflows, and Choi and Samy (2008) suggest that the relationship is weak if it exists at all.

sensitivity to risk. Diaspora-owned firms are one type of firm that I expect to experience and respond to political risk differently than other foreign firms.

The economic sociology literature was the first to argue that diasporans may respond differently to political risk than other foreign firms. This literature argues that investment in the homeland by a member of the diaspora is not just an economic act, but an emotional, social, and political act (Bandelj 2008). Diasporans may invest for social reasons, such as raising their social standing in the diaspora community in the country of residence, or for emotional reasons, such as patriotism (Aharoni 1960; Schulte 2008; Nielsen and Riddle 2010). These motivations may be sufficient to induce diasporans to accept a lower risk-adjusted rate of return in their homeland than they would elsewhere, in turn making their investment decisions less dependent on factors like political risk that moderate the expected rate of return. This theory of non-pecuniary motivation and risk insensitivity has catalyzed substantial interest in the policy community, inducing a 2009 report written for USAID to refer to the "bravery and resilience" of diaspora direct investment (Debass and Ardovino 2009: p. 17).

Survey work with diaspora populations in the US confirms that some diasporans are motivated to invest in their homeland by non-pecuniary incentives, such as emotional ties to the homeland and social ties to the diaspora community in the country of residence (Gillespie et al. 1999; Nielsen and Riddle 2010). A longitudinal survey of Palestinians living in the United States even found evidence that diaspora interest in homeland direct investment does not decrease when diasporans' perceptions of political and economic risk increases (Gillespie, Sayre and

Riddle 2001). These surveys use diasporans' interest in investing as the dependent variable, rather than actual investment behavior. Therefore, one of the important empirical contributions of this paper is to test theories of diaspora difference using data on actual investment flows.

While this literature deserves credit for theorizing about variations in risk sensitivity across firm types, it overlooks the implications of key elements of diaspora difference affecting investor capabilities. In particular, while diasporans' levels of information about the homeland are theorized to be an important cause of diaspora direct investment (Leblang 2010; Javorcik et al. 2011; Doquier and Lodigiani 2010), the implications of high levels of information for political risk sensitivity are never considered. In the following section, I demonstrate that, while motivation-based theories of diaspora difference predict that diaspora direct investment is less sensitive to political risk than is other FDI, informational advantages imply the opposite. In later sections, I conduct an empirical test that allows us to pit the predictions of this theory against those derived from a theory of diaspora difference based on motivations.

Theory

Incomplete and asymmetric information is a major cause of the international immobility of capital and an impediment to international trade (Gordon and Bovenberg 1996, Portes and Rey 2005).[4] The existing literature on diaspora investment argues the diaspora investors enjoy a competitive advantage in their homeland that derives from their social networks, their linguistic abilities, and their

[4] For a review of the literature on "home bias" in investing, see Lewis 1999.

cultural familiarity (Javorcik et al. 2011, Leblang 2009). I accept this general premise and argue more specifically that diasporans have two informational advantages over other foreign investors: 1) diasporans' have access to social networks that provide privileged channels of information; 2) diasporans have language skills, cultural literacy, and non-investment related reasons for monitoring political, social, and economic conditions in the country-of-origin, and therefore face lower costs to maintaining a high level of knowledge about these conditions.

Social Networks and Access to Information

Social networks alleviate cross-border information asymmetry by increasing both the quantity and the quality of information that firms have access to (Uzzi 1996, 1999; Yli-Renko, Autio and Sapienza 2001). Transnational ethnic and social networks can provide privileged channels of information that allow efficient international matching between potential business collaborators and between capital and opportunity (Rauch and Casella 2003). Diasporans, and particularly first-generation migrants, retain access to social networks in their homelands that other potential foreign investors generally do not have. Especially when members of diasporans' social networks are directly involved in business or politics, they may have private information about conditions in the homeland and provide this information to members of the network.

Attention to Publicly Available Information

Some of the informational advantages enjoyed by diasporans have nothing to do with private or privileged access to information, but rather with greater attention to

publicly available information. Most foreign firms actively monitor publicly available information about political risk in the countries in which they already have investments. However, firms may not always choose to bear the cost of closely monitoring conditions in countries where they could potentially make profitable new investments. The list of countries in which profitable opportunities could arise is long, and the cost of monitoring political and economic conditions in each country, even when relying exclusively on publicly available information, is not trivial. Diasporans, however, may have an interest in monitoring information about the homeland because of the friends and family they have living in the homeland, or because of patriotism or emotional connection to the homeland.

Language skills and cultural familiarity are also expected to lower diasporans' costs of accessing and interpreting publicly available information, particularly information in the local language of the homeland. With both lower costs of acquiring publicly available information and greater non-investment related motivations for doing so, diasporans are likely to acquire more publicly available information about their homeland than other potential foreign investors.

From Information to Investment: Hypotheses

FDI flows respond to the level of political risk in the receiving country, increasing when political risk drops and decreasing when political risk rises. However, not all investors respond to fluctuations in political risk to the same degree or with the same speed. I expect that investors with more information respond to changes in political risk both more strongly and more quickly than do investors with less

information. If diasporans are better informed, they should have greater ability to identify and exploit profitable business opportunities in the homeland and to anticipate risks that may arise there.

Changes in the level of political risk in a country either close off or open up profitable investment opportunities. The more information an investor has, the more likely she is aware that a particular investment opportunity has just become more or less attractive, and the more likely she will increase or decrease investment in response to the change in risk. Therefore, I expect better-informed investors to respond more strongly to changes in the level of political risk than less-informed investors. Because diaspora investors have accessed to privileged channels of information via social networks, and because they face lower costs of acquiring publicly available information about the homeland, I expect migrant-induced FDI to be more sensitive than other FDI to changes in political risk.

Hypothesis 1: Migrant-induced FDI flows respond more strongly to changes in the level of political risk than do other FDI flows.

If we find evidence that diaspora direct investment is more political-risk sensitive than other FDI, it becomes important to understand whether this greater sensitivity applies to both increases and decreases in political risk. Investment flows that drop dramatically in response to a rise in political risk increase economic volatility and can turn political crises quickly into economic crises, begetting a vicious

cycle in which instability begets capital flight, which begets economic crisis, which begets instability. Conversely, economic flows that rise quickly in response to decreases in political risk can generate a virtuous cycle in which political reforms begets an increase in investment, which begets economic growth and stabilization of the post-reform political order. I expect that better-informed investors will both be more likely to reduce their investments when risk increases as well as to increase investment when risk falls.

Hypothesis 2A: Migrant-induced FDI flows respond more strongly to *increases* in the level of political risk than do other FDI flows.

Hypothesis 2B: Migrant-induced FDI flows respond more strongly to *decreases* in the level of political risk than do other FDI flows.

Social and Emotional Motivations for Investment

Hypotheses 1, which is derived from a theory of diaspora difference based on access to information, makes a prediction diametrically opposed to the predictions that emerge from the theory of diaspora difference based on motivation in the economic sociology literature. This allows us to test a central implication of each of these theories simultaneously simply by evaluating the risk sensitivity of migrant-induced FDI to political risk. If Hypothesis 1 is correct and migrant-induced investment is more sensitive to political risk than is other FDI, it provides evidence consistent with a

theory of diaspora difference based on access to information and in opposition to a central prediction derived from theories of diaspora difference based on investor motivations.

Empirical Strategy

The central empirical task of this paper is to establish whether diaspora investors (and therefore migrant-induced FDI) are more or less sensitive to political risk than are other foreign investors. This is made challenging by the fact that data on FDI flows do not distinguish investments made by diasporans from investments made by non-diasporans. Because I cannot compare flows of diaspora and non-diaspora FDI directly, I compare FDI flows in country pairs in which migrants from a given homeland (investment-receiving country) make up varying size shares of the population in the country-of-residence (investment-sending country). If diaspora investors are more sensitive to political risk than other investors, and if diaspora direct investment is a substantial portion of overall FDI flows in dyads with large bilateral migrant stocks, I should observe that FDI flows are more sensitive to political risk in country pairs in which migrants from the investment-receiving country make up larger proportions of the pool of potential investors. I use cross-sectional data (1990) on bilateral migrant stocks as a share of the country-of-residence population as a proxy for diaspora investors as a share of potential investors in the period 1994-2008.

Relying on a comparison of risk sensitivity across country pairs with varying-size migrant stocks makes establishing a statistically significant result challenging. Diaspora direct investment (i.e. investment by diaspora-owned firms) is a subset of

migrant-induced investment, which in turn accounts for only a portion of dyadic FDI flows even in country pairs in which migrants are numerous. Any difference between diaspora-owned and non-diaspora-owned FDI is diluted, increasing the likelihood of a finding of no difference. The smaller the proportion of total FDI flows that is made up of diaspora capital, the greater the dilution. Therefore, a significant finding indicates not only that the difference between the two types of investors is large, but also that diaspora direct investment represents a non-trivial portion of total FDI flows in high-diaspora country pairs.

The sensitivity of diaspora direct investment to political risk is evaluated by an interaction term between the level of migrant stocks in a dyad (as a share of the country-of-residence population) and the level of political risk in the homeland. In terms of main effects, I expect the effect of migrant stocks on FDI flows to be positive – more FDI should flow in country pairs with higher diaspora investment potential. I expect a negative effect of political risk on FDI flows: less FDI is expected to flow when risk in the homeland is high. The term of interest, however, is the interaction between these two. If diaspora direct investment is more sensitive to political risk than are other types of FDI, the interaction term between migrant stocks and homeland political risk should be negative: when migrants make up a large proportion of potential investors, the (negative) impact of political risk should be amplified. This would be consistent with Hypothesis 1.

Conversely, if non-pecuniary motivations reduce diasporans risk sensitivity and migrant-induced FDI is less sensitive to political risk than are other types of

foreign direct investment, the sign on the interaction term between risk and migrant stocks should be positive. The main effect of political risk should still be negative and the main effect of migrant stocks should still be positive.

Evaluating Hypothesese 2A and 2B requires exploring increases and decreases in political risk separately, which I do simply by substituting dummy variables for risk increases and risk decreases for a continuous measure of political risk.

Dependent Variable and Sample

The data used in this analysis consist of dyads pairing countries of residence in the OECD with developing country homelands, with annual observations from 1994-2008. Due primarily to increases in the availability of political risk data over time, the panel is unbalanced, with more observations in later years.

The dependent variable, *FDI Inflows*, is measured bilaterally as net annual FDI inflows into 105 developing countries[5] from 30 OECD countries. These data are taken from stats.oecd.org; the raw values are measured in millions of USD.[6] I use a logged dependent variable in all specifications. For specifications analyzing levels of FDI, this logged DV is created as $ln\_FDI = \pm \ln|(\text{FDI inflows} + 1)|$.[7]

[5] While data are available on flows between OECD countries, only flows from the OECD to countries on the IMF's list of developing countries (some of which are OECD members) are considered. Because the data is collected by the OECD, data on bilateral flows between pairs of non-OECD countries is not available.

[6] While some of the related literature (e.g. Ahlquist 2006, Buthe and Milner 2008) uses FDI/GDP as a dependent variable, this would be in appropriate here because the concept of interest is the ability of a country to attract FDI, not the dependence of a country's economy on FDI (Li 2009).

[7] The raw data on bilateral FDI inflows includes a substantial number of negative values, including some very large negative values. Taking the log of the absolute value of FDI plus one and then restoring the negative sign to negative FDI flows retains the appropriate location of zero while reducing the impact of both extremely large negative and positive values.

For specifications in changes, the dependent variable is $ln\_\Delta FDI_{ijt}$ where $\ln\_\Delta FDI_{ijt} = \pm \ln|(FDI_{ijt} - FDI_{ij(t-1)})+1|$ and FDI_{ijt} is the net inflow of FDI to Country *j* from Country *i* in period *t*.

The full sample contains a number of country pairs in which the annual flows of FDI are zero in all years – some of the smaller OECD economies have FDI in only a handful of developing countries. To analyze the responsiveness of FDI flows to political risk in these perpetually zero-FDI country pairs is illogical: FDI will be zero regardless of the level of risk, making FDI in these dyads appear completely risk-insensitive. Therefore, I remove from the samples all country pairs in which FDI is zero for all years. [8]

Independent Variables

The primary measure of political risk, *political risk,* is drawn from the Belgian export credit agency *Office National Du Ducroire* (ONDD). ONDD is the world's largest political risk insurer and the price leader in the industry – its assessments of risk reflect not only profit-motivated expert attempts to assess risk, but also the actual insurance costs paid by firms who wish to invest without shouldering the burden of political risk themselves. ONDD data are used in a similar context by Jensen (2008) and Jensen and Young (2008).

[8] While the average number of migrants is lower among the zero-FDI dyads than other dyads, more than half of zero-FDI dyads have recorded migrant stocks greater than zero. Results are robust to the inclusion of these dyads in the main specification of the analysis on levels; the results fall from significance, but retain their sign, when these dyads are added to the analysis on changes.

ONDD scores each country in three categories: *war risk*; *government risk,* which is the risk of expropriation or adverse government action;[9] and *transfer risk*, which refers to risk that action by foreign authorities, such as the introduction of capital controls or other constraints, prevents the transfer of money necessary to repay creditors.[10] From 2002-2010 these three classifications of risk are all coded annually on a 7 point scale. From 1992-2001, war and government action risks were combined and measured annually on a single 5 point scale *(wargovrisk)*. From 1994-2010 transfer risk is measured on a 7 point scale. The ONDD data is available for 126 countries in 1994, but this expands to 186 countries by 2010, producing an unbalanced panel.[11]

The primary political risk variable used in this analysis is political risk. From 2002-2008 this variable is a simple sum of war risk + government risk + transfer risk. From 1994-2001, however, some rescaling is necessary. For this period: political risk = transfer risk + (wargovrisk * 14/5). To ease the interpretation of coefficients on the interaction term, this variable is demeaned before it is interacted.[12]

While risk data are issued annually, a team at ONDD meets four times per year to update risk evaluations, addressing ¼ of countries (by region) in each meeting. However, if events justify it, a country's risk rating may be revised at a meeting in which it is not otherwise scheduled to be discussed, allowing the potential for multiple

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[9] ONDD defines government action as "local authorities' decisions, deficiencies, and impairments that are of an arbitrary and discriminatory nature."
[10] The ONDD's lexicon can be accessed online at http://www.ondd.be/webondd/Website.nsf/weben/Documentation_Lexicon?OpenDocument#NT0000408A (accessed April 20, 2010).
[11] Only developing host countries are analyzed, limiting the sample to 147 countries in 2010.
[12] This is done in both the specification in levels and the specification in changes.

revisions during a year (Jensen 2008). Therefore, the score given to a country for any given year will reflect any major changes that occurred during that year prior to ONDD's last meeting of the year.

Bilateral Migrant Stocks is measured as the number of migrants from Country *j* living in Country *i* in 1990 divided by the total population of Country *i* in that year. Migrant stocks are divided by the population of Country *i* because this variable is being used to proxy for diaspora investors as a *share* of potential investors.[13] Stocks are measured as of 1990 (prior to the first year of the analysis) to reduce endogeneity.

The raw data on bilateral migrant stocks is taken from Docquier, Lowell, and Marfouk (2007). Bilateral migrant stocks is measured cross-sectionally and does not vary over time.[14] Like political risk and Δ political risk, bilateral migrant stocks is demeaned before it is interacted.

The variable *other FDI outflows from Country i* is the total FDI outflows from Country *i* in year *t*, minus the value of FDI flows from *i* to *j* in that year. Data on bilateral investment treaties (BITs) are taken from Allee and Peinhardt (2010). The variable *BIT to date* takes a value of 1 if a BIT has been signed by the two countries in the dyad and zero otherwise. Average years of schooling in the homeland, *education,*

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[13] Leblang (2010) uses logged bilateral migrant stocks, without dividing by country *i* population. In the context of this study, this variable is inappropriate because its use implies that flows of FDI from large home countries (with larger migrant populations in absolute terms) contain a larger proportion of diaspora direct investment than flows from smaller countries (whose migrant stocks are small in absolute terms). Not surprisingly, using this alternate variable of interest causes results fall from statistical significance (though the direction of estimated effects remains consistent).

[14] I have discovered what I believe to be an error in this data systematically overstating migrant stocks in Finland, perhaps by a factor of 10. The main results in both levels and changes are robust to dropping the dyads involving Finland, though in models with the most additional controls, the main effect sometimes fall from significance when these dyads are omitted.

is drawn from Barro and Lee (2010). They estimate the average number of years of schooling attained by members of the population aged 25 and older. Their estimates are provided at 5-year intervals – for use in this panel I have annualized their data via linear interpolation.

Oil Reserves is time-invariant. It is measured as the maximum value of proven oil reserves 1960-1993 and is constructed from data collected by Macartan Humphreys (2005).[15] *GDP per capita* is the per capita GDP of the homeland. *Population* is the log of the homeland population. *Distance* is the logged distance between the capitals of the homeland and country of residence; *border* is a dummy variable for a shared border; *common language* is a dummy variable for a common official language and *colony* is a dummy variable for a previous colonial relationship.[16] The data on *population* is taken from the World Development Indicators, and the *distance, border*, *common language,* and *colony* measures are taken from CEPII.

For specifications using changes, the independent variables are transformed in the same manner as the dependent variable.[17] Those independent variables that do not vary over time are retained in levels.

[15] The level of oil reserves is preferable to the level of oil exports because it is less dependent on the level of investment. However, discovering and proving reserves also requires investment, making it endogenous to FDI. Therefore, I take the maximum value of reserves prior to the start of the study period, rather than using panel data on the variable.

[16] While some gravity models include a dummy for strict customs unions, I exclude it here. The only two dyads in the sample in which a customs union was present as of 1997 are Australia/Kiribati and US/Panama. No OECD/developing country dyads share a common colonizer, so this is also excluded.

[17] In the case of the *Other FDI Outflows from Country i,* I log this variable in the same manner as the dependent variable.

A Specification in Levels and a Specification in Changes

I test Hypothesis 1 using two different specifications: one in levels and one in changes. The model specified in levels is a least squares dummy variable regression (LSDV) with dyad fixed effects.[18] I run this model on annual data. The model estimated takes the form of:

$$y_{ijt} = \alpha_{ij} + \mathrm{B}_1 y_{ij(t-1)} + \mathrm{B}_2 politicalrisk_{jt} + \mathrm{B}_3 (politicalrisk_{jt} * migrants_{ij}) + \mathrm{B}_4 X_{ijt} + \varepsilon_{ijt} \qquad (3.1)$$

where y_{ijt} indicates the log of FDI flows from Country *i* (the country of residence in the OECD) to Country *j* (a developing-country homeland) in year *t*; political risk$_{jt}$ is the level of political risk in Country *j* in year *t;* migrants$_{ij}$ is the stock of migrants from Country *j* living in Country *i* in 1990 (as a share of the Country *i* population, which proxies for the share of FDI from Country *j* to Country *i* that is made up by diaspora investment; X_{ijt} is a vector of covariates, α_{ij} denotes dyad fixed effects, which may correlate with the covariates, and ε_{ijt} is the error term.

For the specification in changes, I collapse the fifteen years of annual observations for which political risk data is available into five three year periods, taking the mean value of FDI Inflows in each period. FDI is slow-moving relative to other forms of investment (e.g., Ahlquist 2006), and the effects of a change in the level

[18] While the panel dataset covers 15 years, the average number of observations per dyad is 5.5. In short panels with a lagged dependent variable and fixed effects, the estimated coefficient on the lagged dependent variable (as well as the coefficients on the fixed effects dummies) is biased (Nickell 1981; Beck and Katz 2004). However, because I do not have a substantive interest in interpreting the estimated dynamics, I do not view this as a large drawback to this model – however, I would caution others against drawing substantive conclusions for the estimated coefficients reported on the lagged dependent variable. Results are robust to an alternative specification using differenced data, which does not suffer from Nickell bias.

of political risk may be spread out over several years.[19] Descriptive statistics for both the annual data in levels the 3-year blocks of data in differences are in the appendix. The specification in changes is given as:

$$\Delta y_{ijt} = \alpha + \mathrm{B}_1 \Delta politicalrisk_{jt} + \mathrm{B}_2(\Delta politicalrisk_{jt} * migrants_{ij}) + \mathrm{B}_3 X_{ijt} + \varepsilon_{ijt} \qquad (3.2)$$

Δy_{ijt} is the (logged) change in FDI flows from Country *i* to Country *j* between years *t-1* and *t*. Δ political risk$_{jt}$ is the change in political risk in Country *j* from period *t*-1 to period *t*. X_{ijt} is a vector of covariates, some specified in changes, some in levels.

Results for Specifications Analyzing Levels of FDI

In testing Hypothesis 1, I find that diaspora direct investment is much more sensitive to political risk than are other types of FDI. Figure 3.11 is a plot of the marginal effect of political risk on bilateral FDI flows in dyads with migrant stocks of varying sizes. The full results of that regression are presented in Table 3.1, Model 2 on the following page.

Consistent with Hypothesis 1, the negative relationship between bilateral FDI flows and Political Risk is much stronger in dyads with large migrant stocks than dyads with small migrant stocks.

[19] Panel regression would be inappropriate on 3-year blocks of data because the lower number of time periods would increase the salience of Nickell bias.

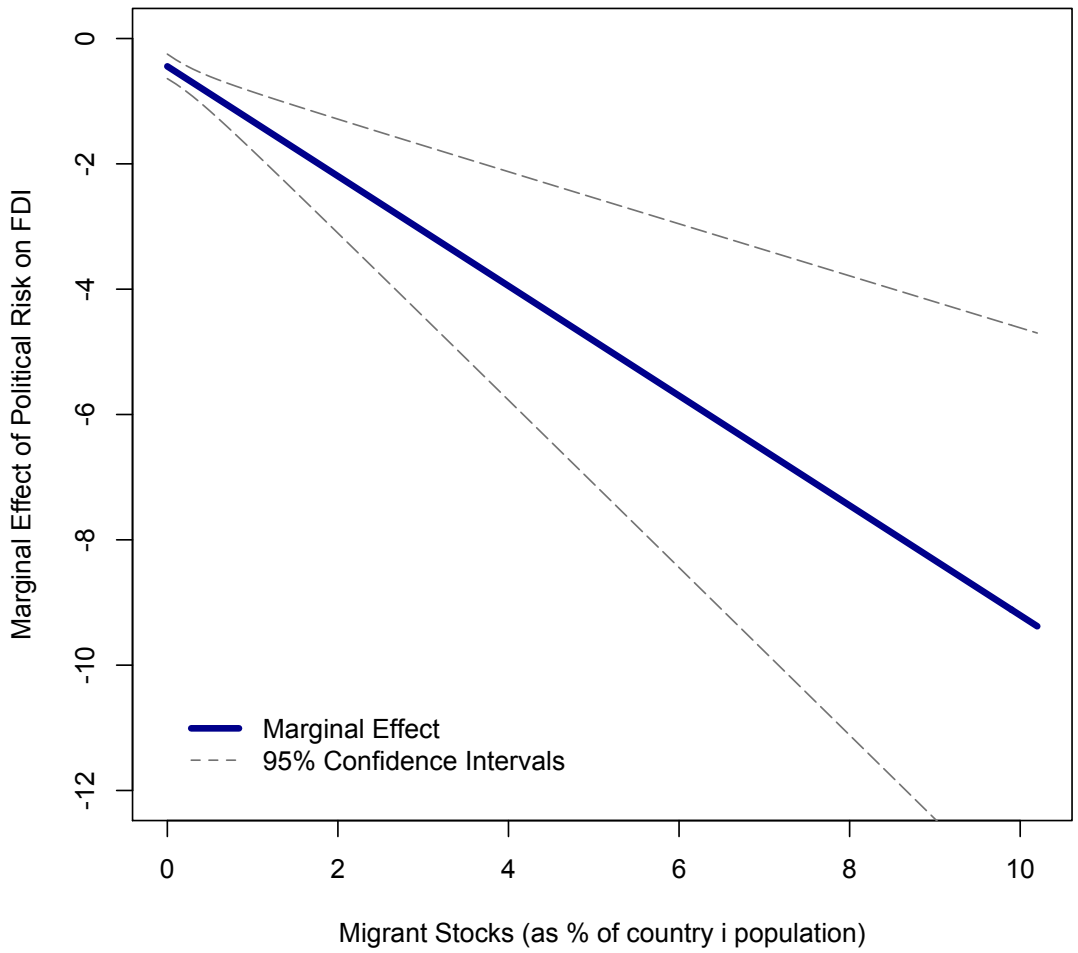

Figure 3.1: Marginal Effect of Political Risk on Bilateral FDI as Migrant Stocks Varies

Figure 3.1 shows the marginal effect of political risk on bilateral flows of FDI (logged) as bilateral migrant stocks varies along the x-axis. The y-axis values are marginal effects. In dyads where migrant stocks are zero, a one-unit increase in political risk is associated with a small decrease in FDI flows (about -0.5 on a log scale), while in dyads with large migrant stocks, the negative effect of political risk increases on FDI flows is more severe.

Table 3.1: Political Risk and Migrant FDI: Dyad Fixed Effects

	(1) Main Effect	(2) Primary Model	(3) All Dyads
Political Risk	-0.420***	-0.474***	-0.486***
	(0.086)	(0.108)	(0.177)
1 year lag of DV	-0.126	-0.463***	-0.477***
	(0.130)	(0.159)	(0.168)
Other FDI outflows from country i	0.037*	0.037	0.042
	(0.020)	(0.023)	(0.025)
Political Risk*Migrant Stock		**-875.988*** **	**-766.348*** **
		(257.555)	**(242.470)**
Education		-60.913	-59.057
		(49.831)	(54.234)
BIT to Date		-164.098	-156.962
		(108.339)	(120.120)
Political Risk*Distance			-359.280***
			(134.678)
Political Risk*Common Language			742.802
			(470.419)
Political Risk*Common Border			4.357
			(404.827)
Political Risk*Colony			-1186.210**
			(531.581)
Political Risk*BIT to Date			34.727
			(202.776)
Political Risk*Oil Reserves			1.957
			(1.341)
Constant	6.466***	11.703***	12.045***
	(0.472)	(3.542)	(3.810)
Observations	13193	9196	8077
R^2	0.004	0.008	0.010

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

The following independent variables are dropped because they do not vary over time: Migrants, Natural Resources, Distance, Common Language, Common Border, Colony.

In Table 3.1 the sign on the interaction term between political risk and migrant stocks, *Political Risk*Migrant Stocks*, is strongly negative and robust to controls. This indicates that migrant-induced FDI is indeed more sensitive to political risk than is other FDI. All the models in Table 3.1 include dyad fixed effects. The independent effect of political risk is negative, as expected. Because both political risk and bilateral migrant stocks have been demeaned before they are interacted, the coefficient on political risk in Model 2 can be interpreted as the independent effect of political risk on FDI inflows when bilateral migrant stocks is at its mean.

The dummy gravity variables are not demeaned before interaction, while the continuous measures, distance and oil reserves, are demeaned. Therefore, the coefficient of political risk in Model 3 can be interpreted as the marginal effect of political risk on FDI inflows when bilateral migrant stocks, oil reserves, and distance are at their means and common border, colonial relationship, common language and BIT to date are equal to zero. Higher political risk in the homeland is associated with lower FDI inflows.

Because bilateral migrant stocks, oil reserves, colonial relationship, common border, common language, and distance are all time-invariant, no estimate of their main effect is produced by these fixed effects regressions. These main effects are examined in a cross-sectional regression in Table 3.3.

The most important control variable is other FDI outflows from Country i, which measures FDI outflows from the country of residence to all countries except the homeland in a particular year. The coefficient is positive and significant in all

specifications. This effectively controls for all supply-side factors affecting the overall availability of FDI outflows from each Country *i*, allowing us to focus narrowly on the decision of investors from Country *i* to invest in Country *j* (the homeland) as opposed to other possible destinations.

It is possible that some form of confounding variable causes both large migrant stocks and high FDI flows. In particular, it is plausible that a variable that causes migration, such as distance between the two countries, a common border, common language, or colonial tie might be driving the reduced sensitivity to political risk, rather than the migrant population itself. Model 3 includes interaction terms between all four of these gravity variables and political risk, as well as an interaction between BIT to date and political risk. Results are robust to the inclusion of these interactions.

Like migrant stocks, a colonial relationship and geographic proximity are associated with greater risk sensitivity in FDI flows. This is consistent with the information-based argument put forward in this paper. Multinational firms based in countries that are near a particular developing country or that were former colonizers of that developing country are likely to have greater access to information about that developing country than are multinationals based in more distant or unrelated countries. Higher levels of information allow firms to respond quickly and effectively to changes in the level of political risk, making FDI flows in these dyads more sensitive to changes in political risk in County *j*.

Results for Specifications Analyzing Changes in FDI

The expectations regarding the specification in changes are the same as with regard to the specification in levels. If Hypothesis 1 is correct, the interaction between Δ political risk and bilateral migrant stocks will be negative. I expect the main effect of Δ political risk on changes in FDI flows also to be negative.

Because this model does not contain dyad-fixed effects, the main effect of bilateral migrant stocks does not drop out of the regression, and a coefficient is estimated. However, it is important to note that bilateral migrant stocks is measured in levels as of 1990 – it does not vary over time. There is no theoretical reason to expect changes in FDI flows to be different in high-migrant dyads as opposed to low migrant dyads, so the expectation is that the coefficient on bilateral migrant stocks is close to zero. This expectation is borne out. Similar expectations apply to the main effects of control variables added in Model 3: these variables are measured in levels, not changes, and there is no theoretical reason to expect their coefficients to be significantly different from zero. The inclusion of these terms serves only to allow interpretation of the interaction effects. Their main effects, along with the main effect of bilateral migrant stocks, are estimated in a cross-sectional regression in Table 3.3.

Table 3.2: Political Risk and Migrant FDI: Specifications in Changes

	(1) Main Effects	(2) Primary Model	(3) Added Controls	(4) Country FE
Δ Political Risk	-0.739***	-0.701***	-0.558***	-0.507**
	(0.156)	(0.156)	(0.180)	(0.210)
Bilateral Migrant Stock (1990)	0.046	0.032	0.003	0.025
	(0.075)	(0.061)	(0.059)	(0.067)
Δ Other FDI Outflows from Country i	0.097***	0.098***	0.094***	0.083***
	(0.013)	(0.013)	(0.014)	(0.016)
Δ BIT To Date	0.952	0.958	1.231	1.750
	(1.924)	(1.925)	(2.016)	(2.171)
Δ Education	0.548	0.582	0.668	1.399
	(1.361)	(1.358)	(1.512)	(2.105)
Δ Political Risk * Migrant Stock		**-0.171*** **	**-0.174*** **	**-0.167*** **
		(0.064)	**(0.060)**	**(0.057)**
Oil Reserves Per Capita			2.493	
			(2.720)	
Δ Political Risk*Oil Reserves			0.540	1.592
			(2.057)	(3.023)
Distance (logged)			-1.428***	-1.056
			(0.364)	(0.711)
Δ Political Risk * Distance			-0.388*	-0.466*
			(0.226)	(0.244)
Common Language Dummy			1.559	1.863
			(1.052)	(1.281)
Δ Political Risk * Common Language			1.320*	1.018
			(0.737)	(0.770)
Shared Border Dummy			-0.779	-0.110
			(2.339)	(2.487)
Δ Political Risk * Shared Border			1.112	0.862
			(1.558)	(1.655)
Colonial Relationship Dummy			0.698	0.367
			(1.516)	(1.685)
Δ Political Risk * Colonial Relationship			-1.160	-1.203
			(0.960)	(0.962)
Constant	2.626***	2.600***	2.445***	-2.078
	(0.479)	(0.478)	(0.532)	(6.646)
Observations	3808	3808	3281	3281

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

The following independent variables are dropped because they do not vary over time:
Migrants, Natural Resources, Distance, Common Language, Common Border, Colony.

These results match well with those from the specification in levels. The interaction term of interest, Δ *Political Risk * Migrant Stocks*, is negative, significant, and robust to a range of controls. This indicates that FDI flows are more risk sensitive in dyads with larger migrant stocks, and this is consistent with diaspora investors responding more strongly to political risk than non-diaspora foreign investors.

As with the regressions in levels, both Δ political risk and bilateral migrant stocks are demeaned before they are interacted, so the coefficient on Δ political risk in Model 2 can be interpreted as the independent effect of Δ political risk on FDI inflows when bilateral migrant stocks is at its mean. The dummy gravity variables are not demeaned before interaction, while the continuous measures, distance and oil reserves, are demeaned. Therefore, interpretation of the coefficient of Δ political risk in Models 3 and 4 can be interpreted as the marginal effect of Δ political risk on FDI inflows when bilateral migrant stocks, oil reserves, and distance are at their means and common border, colonial relationship and common language are equal to zero. The main effect of Δ political risk is negative and significant, as expected: increases in political risk are associated with decreases in net FDI inflows. Among the control variables, the level of FDI outflows from Country *i* remains positive and significant, as expected. The interaction terms between colonial relationship and political risk and between distance and political risk are not significant in this specification, though their signs are the same as in the specification in levels.

Model 4 of Table 3.2 includes fixed effects for both home and host country. This model does not allow the main effect of Oil Reserves Per Capita (in Country j) to

be estimated because this variable does not vary over time, and hence its main effect is picked up by the country fixed effects for Country *j*. The results regarding the interaction of interest are robust to the inclusion of these fixed effects.

The results from Table 3.2 demonstrate the robustness of my findings to starkly differently specifications. The results are the same when analyzing annual-data specified in levels and 3-year increments of data specified in changes. These results provide strong support for Hypothesis 1. They indicate that migrant-induced FDI is actually more sensitive to political risk than is other FDI. This is consistent with the theory that diasporans are better informed than other foreign investors and contradicts central predictions derived from theories of diaspora difference based on non-pecuniary motivations.

Cross-Sectional Results

While dyad-fixed-effects modeling offers the clearest insight into the causal relationship between political risk and diaspora direct investment, it is useful to present a specification in which the main effect of bilateral migrant stocks and time invariant controls can be estimated.[20] Hausman test results indicate that a random-effects specification would be biased; therefore, I opt for a simple cross-sectional regression on a single period.

[20] Plümper and Troeger (2007) propose a three-stage estimator, which they refer to as fixed effects vector decomposition and claim allows for efficient estimation of the effects of time invariant variables in fixed effects panel regressions. Greene (2011) points out several critical flaws in the estimator, leading me not to use it here. Using this estimator produces results that are consistent with the key interaction effects in Tables 3.2 and 3.4, i.e. both the main effect of political risk and the coefficient on the interaction between political risk and migrant stocks are negative and significant. However, the main effect of migrant stocks on FDI is negative (and not significant), which I attribute to the nature of the estimator, which relies on a misspecified random effects panel regression to assess the main effects of time-invariant variables.

I omit the interaction between political risk and migrant stocks from these regressions. Any diasporan has only one homeland with regard to which her knowledge and connections are relevant. Therefore, it is the over-time variation in diasporans' willingness to invest in their homeland, rather than their cross-sectional decision about which country to invest in, on which the theories in this paper bear most directly.

In the cross-sectional analysis I utilize bilateral FDI data from the International Monetary Funds's 2009 Coordinated Direct Investment Survey. This survey provides data on FDI outflows for a wider range of FDI-sending countries – the OECD data contains data of FDI outflows only from OECD members. However, the data is only available for a single year, making it useful only in the cross-sectional context. Because dyad fixed effects are not included in the following models, I include standard gravity-model determinants of FDI (e.g., Benassy-Quere, Coupet, and Mayer 2007). I also use the most up-to-date measure of migrant stocks available, which comes from the World Development Indicators and is measured as of 2000.[21]

Controls include dummy variable fixed effects for the FDI-sending country (Country *i)* and economic characteristics of the FDI-receiving country (Country *j),* including political risk. Due to data limitations the control variable BIT to date only captures BITs signed in 2006 and earlier.

[21] In Tables 3.1 and 3.2, I use a measure of bilateral migrant stocks from 1990 to reduce endogeneity.

Table 3.3: Political Risk and Migrant FDI: Cross Sectional Regression

	(1) Main Effects	(2) Controls	(3) More Controls	(4) Country *i* & *j* FE
Bilateral Migrant Stocks (2000)	155.6***	133.6***	43.09***	47.11***
	(36.344)	(31.206)	(14.630)	(13.700)
Political Risk (2009)	-0.615***	-0.351***	-0.267***	
	(0.020)	(0.034)	(0.032)	
Population of Country *j* (logged)	1.084***	1.276***	1.306***	
	(0.064)	(0.079)	(0.076)	
GDP Per Capita in Country *j*		4693.0***	7052.3***	
		(1637.737)	(1646.033)	
Education in Country *j*		0.593***	0.474***	
		(0.053)	(0.049)	
Oil Reserves in Country *j*		2.121***	1.283*	
		(0.727)	(0.728)	
Political Risk * Oil Reserves		0.312	0.0436	
		(0.217)	(0.216)	
Common Language (10% of Pop)			1.381***	0.805**
			(0.347)	(0.313)
Common Border			1.482**	1.244**
			(0.575)	(0.571)
Colonial Relationship			2.436***	2.513***
			(0.575)	(0.574)
Distance (logged)			-2.455***	-3.020***
			(0.139)	(0.168)
BIT (2006)			2.396***	2.571***
			(0.325)	(0.317)
Dummies for Country *i*	YES	YES	YES	YES
Dummies for Country *j*	NO	NO	NO	YES
Constant	-4.512***	-17.59***	1.880	32.67***
	(1.650)	(1.509)	(1.949)	(3.242)
Observations	5103	4085	4085	5983
R^2	0.541	0.581	0.638	0.659

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

The purpose of this table is to show the main effects of bilateral migrant stocks and time-invariant control variables. As expected, the main effect of bilateral migrant stocks is positive and the main effect of political risk is negative. The effect of bilateral migrant stocks is weakened when a sufficient range of controls is added, but its coefficient remains significant at the 0.05 level in a one-tailed test.[22] Substantively, this is consistent with what is established in the literature, that migrants promote the flow of FDI from their countries of residence to their homelands.

Models 4 and 5 in Table 3.3 include a variety of elements of the relationship between Country *i* and Country *j* whose main effects could not be estimated in Tables 3.1 and 3.2. Not surprisingly, these characteristics have a strong influence on bilateral FDI flows: FDI flows are largest between countries that are geographically proximate and that share linguistic and cultural ties. Bilateral investment treaties are also associated with larger FDI flows.

Increases vs. Decreases in Risk: Dummy Independent Variables

Hypotheses 2a and 2b state that the greater risk sensitivity of diaspora direct investment observed above is expected to manifest itself with regard to both increases and decreases in risk. To test this, I substitute dummy variables for political risk increase and political risk decrease in place of the continuous measure of Δ political risk. *Risk up* takes a value of 1 if Δ political risk > .5, *risk down* takes a value of 1 if Δ political risk < -.5, while both variables are zero for observations with very small or no

[22] A one-tailed test is appropriate here because the expected direction of effect (positive) is clear. Larger (and more robust) main effects can be achieved using an alternative measure of migrant stocks, such as the raw or logged number of migrants in a dyad. However, a ratio measure of migrants/population is more theoretically appropriate for estimating the share of outward FDI attributable to migrants.

changes in political risk. Because risk up and risk down are binary, they are not demeaned before they are interacted with bilateral migrant stocks (which is demeaned).

Equation 3.3 gives the equation estimated in model 4 of Table 3.4 below.

$$\Delta y_{ijt} = \alpha + \mathrm{B}_1 \Delta risk\_up_{jt} + \mathrm{B}_2 (risk\_up_{jt} * migrants_{ij}) + \mathrm{B}_3 \Delta risk\_down_{jt} + \mathrm{B}_4 (risk\_down_{jt} * migrants_{ij}) + \mathrm{B}_5 X_{ijt} + \varepsilon_{ijt} \quad (3.3)$$

Table 3.4: Political Risk and Migrant FDI: Decreases vs. Increases in Risk

	(1) Main Effects	(2) Risk Up	(3) Risk Down	(4) Up & Down	(5) Controls	(6) Controls & FE
Δ Other FDI Outflows from Country i	0.094***	0.095***	0.095***	0.095***	0.088***	0.079***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.016)
Δ BIT To Date	1.153	0.768	1.193	1.119	1.145	1.627
	(1.953)	(1.922)	(1.938)	(1.948)	(1.981)	(2.166)
Δ Education	0.575	0.414	0.788	0.690	0.797	1.568
	(1.207)	(1.389)	(1.194)	(1.204)	(1.318)	(2.110)
Bilateral Migrant Stock (1990)	0.047	0.049	-0.104***	-0.104***	-0.103***	-0.091
	(0.058)	(0.066)	(0.030)	(0.022)	(0.022)	(0.089)
Risk Up	-0.640	-2.162**		-0.688	0.380	0.441
	(1.039)	(0.976)		(1.029)	(0.986)	(0.984)
Risk Down	2.298***		2.469***	2.165***	2.172**	2.642***
	(0.714)		(0.694)	(0.709)	(0.854)	(0.869)
Risk Up * Migrants		**-0.210**		**-0.056**	**-0.138**	**-0.023**
		(0.393)		**(0.398)**	**(0.288)**	**(0.384)**
Risk Down * Migrants			**0.412*** **	**0.411*** **	**0.366*** **	**0.364*** **
			(0.118)	**(0.120)**	**(0.105)**	**(0.117)**
Distance (logged)					-0.250	-0.082
					(0.734)	(1.100)
Risk Up * Distance					-3.462***	-3.391**
					(1.089)	(1.322)
Risk Down * Distance					-0.598	-0.383
					(0.906)	(1.065)
Common Language Dummy					2.687	3.481*
					(1.928)	(2.054)
Risk Up * Common Language					0.907	-0.111
					(2.637)	(2.912)
Risk Down * Common Language					-2.513	-3.171
					(2.681)	(2.503)
Shared Border Dummy					-0.263	-0.645
					(2.961)	(5.780)
Risk Up * Common Border					1.489	0.857
					(6.173)	(8.464)
Risk Down * Common Border					-1.758	-0.281
					(3.534)	(6.338)
Colonial Relationship Dummy					-1.102	-2.123
					(3.372)	(3.005)
Risk Up * Colonial Relationship					-2.702	-1.894
					(6.297)	(4.865)
Risk Down * Colonial Relationship					4.389	4.944
					(3.838)	(3.459)
Dummy Variables for Country *i* and Country *j*	NO	NO	NO	NO	NO	YES
Constant	1.610**	3.179***	1.292**	1.624**	1.277*	2.161
	(0.702)	(0.692)	(0.585)	(0.702)	(0.746)	(6.105)
Observations	3808	3808	3808	3808	3353	3353

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$
In Tables 1-5, errors are clustered on country *i*'s country ID.

Hypothesis 2A states that migrant-induced FDI responds more strongly than other FDI to increases in political risk, while Hypothesis 2B makes a parallel assertion

with regard to decreases in political risk. The results in Table 3.4 allow us to reject the null hypothesis with regard to Hypothesis 2B, but not with regard to 2A. The coefficient on the interaction between risk down and bilateral migrant stocks is positive and significant in all specifications, indicating that the positive effect of risk decreases on FDI is amplified in dyads with large migrant stocks, consistent with Hypothesis 2B.

The coefficient on the interaction between risk up and bilateral migrant stocks is negative in all models, but not statistically significant. This indicates that the negative effect of risk increases on FDI flows is perhaps somewhat larger in dyads with large migrant stocks, but this interaction effect is much weaker and in some models very near zero. Therefore, there is little evidence in support of Hypothesis 2A.

Across all levels of migrant stocks, decreases in risk have a substantially larger effect on changes in FDI than do risk increases, despite the fact that each direction of change has the same average magnitude.[23] In other words, relative to a static level of risk, institutional improvements prompt greater changes in investment behavior than institutional deteriorations. This is a finding that justifies future research. It may be that, while investment in established sectors is generally continued after risk rises, FDI into new sectors generally takes off in response to institutional stabilizations and improvements.

As in Table 3.2, the coefficient on bilateral migrant stocks is not expected to be significantly different from zero. However, in Models 2 and 3 the coefficient is

[23] The mean size of risk increases in the sample is 1.65, while the mean risk decrease is 1.68.

negative and significant. While not significant once controls are added, analyzing these coefficients in conjunction with the coefficients on the interaction terms suggests that when political risk in Country *j* remains neutral or increases (i.e. risk down = 0), dyads with larger migrant stock experience slightly negative (or less positive) changes in FDI flows relative to similar dyads. This effect is reversed in dyad-periods with risk decreases in Country *j*, when dyads with large migrant stocks see more positive changes in FDI relative to similar dyads.

The control variables included in Table 3.4 are very similar to those in Table 3.2, however, gravity model variables like common language and shared border are now interacted with dummy variables for both risk increase and risk decrease, instead of with a continuous measure of change in political risk. Most of the coefficients on these interactions are not significantly different than zero. However, FDI flows between geographically proximate countries (i.e. dyads with smaller values for distance) are more sensitive to risk increases, which is consistent with the information-based theory I present. Potential investors are likely better informed about states in their region than those far away, and hence are better able to seize on investment opportunities that arise when political risk decreases.

Substantively, these results show that the difference in the risk sensitivity of FDI inflows between high-migrant and low-migrant dyads is primarily a difference in responsiveness to decreases in political risk in Country *j*, i.e. stabilizations and institutional improvements. While Table 3.4 provides no evidence that FDI flows in high-migrant dyads are *more* stable in the face of crises than flows in low-migrant

dyads, there is also no evidence that flows in these dyads are significantly more sensitive to crises. These finding are consistent with the information-based theory of diaspora difference I propose, and suggest that diaspora investors are more likely than other foreign investors to make or expand investments in response to improving risk conditions. Diasporans constantly monitor conditions in the country of origin for non-business reasons, and when the political risk climate improves, they have the information they need to seize new opportunities associated with the improving investment climate.

Theoretical Implications

The results presented in this paper provide evidence that migrant induced FDI is more sensitive to changes in political risk, and particularly to decreases in political risk, than is other FDI. This is consistent with the theory that diaspora-owned firms experience and respond to political risk differently than other foreign firms, and demonstrates that the way in which aggregate flows of investment respond to changes in political risk depends on the mix of diaspora and non-diaspora investors on which that country is drawing. Theses findings also contradict some of the more optimistic theories in economic sociology, which claim that diaspora investors are less risk sensitive than other investors and are more willing to make and sustain investments during times of acute political crisis. While it is with regard to risk decreases that migrant-induced FDI is much more sensitive, it is no less sensitive than other FDI with regard to risk increases. These findings are consistent with the theory of diaspora difference based on information I propose: that diasporans gain access to private

information about the homeland via social networks, and that diasporans are more attentive to publicly available information about the homeland than other potential foreign investors.

My informational theory assumes that diasporans' non-pecuniary motivations do shape some behaviors. Diasporans bear the costs of monitoring the political climate in the homeland because of the social and emotional ties they keep with the homeland and its inhabitants. This attentiveness causes diasporans to be better informed about the homeland than other potential foreign investors, and in turn positions them to seize new investment opportunities that arise when political risk decreases. Therefore, while I argue that some of the more optimistic expectations in the economic sociology literature must be amended, I do not argue that diasporans' social and emotional ties to the homeland are irrelevant to their investment behavior. These ties, and the behaviors they motivate, are important components of diaspora difference and underscore the importance of taking diaspora identity seriously when studying the relationship between political institutions and economic behavior.

Policy Implications

Fragile states often persist in a cycle where instability reduces investment, investment reduces growth, and lack of growth begets further instability. The current policy literature is optimistic that diasporans may provide a risk-insensitive source of capital that continues to flow as risk increases and may help prevent or end this cycle (e.g. Debass and Ardovino 2009). The theoretical argument and empirical results in this paper belie these optimistic expectations.

This has important policy implications. If the risk sensitivity of migrant-induced investment demonstrated here is, as I argue, driven by the risk sensitivity of diaspora investors, efforts dedicated to recruiting diaspora investment into fragile states during times of acute crisis and high political risk are likely to fare much less well than efforts to recruit this investment made just after settlements have been reached or reforms have been made. While I find no evidence to support previous theories that diaspora investors are less sensitive than their peer firms to risk increases (i.e. crises) in the homeland, my results do suggest that diaspora investors are much more responsive than other investors to reductions in political risk in the homeland, i.e. stabilizations and improvements. While diaspora investment cannot serve as a substitute for political reform in fragile states, it may be capable of serving as both a complement to, and incentive for, institutional reforms that limit political risk.

Political reforms that limit risk are often costly to actors who undertake them. Take for example, executive constraints. Constraints on executive authority, such as those provided by an opposition-controlled legislature or an independent supreme court with the right of judicial review, are associated with a higher degree of policy stability and lower levels of political risk (e.g. Keefer and Knack 1997; Henisz 2000). However, when an executive accepts additional constraints on her authority, she necessarily loses the ability to pursue its interests and preferences as freely as before – this is a substantial cost. If the executive, or any other political actor, understands that costly reforms that reduce political risk will likely be followed by a rapid influx of diaspora capital, this provides some additional incentive to undertake such reforms.

Similarly, if political actors know that actions that increase political risk will lead to a reduction in FDI flows (even from diaspora investors), it provides incentives to avoid such actions.

Political-risk insensitivity can be a desirable characteristic of capital flows in countries plagued by a cycle of instability, low investment, and low growth – breaking the cycle by providing investment in the absence of stability. However, risk sensitive capital also provides a means out of the same cycle – incentivizing the creation of stability in the absence of growth. A high volume, risk sensitive source of potential capital can provide valuable incentives for political actors to initiate difficult reforms before any investment is in place; the entrance of these investors can provide the capital (and growth) to sustain these reforms.

Alternative Explanations, Causal Identification, and Future Research

The most substantial impediment to causal identification in this analysis is the risk is that the observed interaction effects in Tables 1, 2, and 4 are driven by some omitted factor that affects both migration and investment, and which affects investment more in the countries in which its effect on migration is also largest. I have mitigated this risk by controlling for both the main and interaction effects of a number of variables expected to affect both migration and FDI: geographic proximity and cultural, linguistic and historical connections, and oil reserves in the homeland. Results are robust to these controls.

While the results presented here are consistent with the information-based theory of diaspora difference that I have proposed, evidence at this level of

aggregation cannot decisively rule out all alternative theories. For example, Kugler and Rapoport (2007), argue that the presence of migrants provides information to all the firms in the migrants' country of residence about the labor market (and possibly other conditions) in the homeland. This would imply that the informational advantages provided by migrants are not limited to firms that are migrant-owned, or even to firms that hire migrants.

It is also possible that the high political risk sensitivity of diaspora investors is driven, at least in part, by political vulnerabilities unique to diaspora-owned firms. Diaspora direct investors function inside the domestic political milieu of the homeland in a way that other foreign direct investors do not; many still retain their citizenship in the homeland and hence occupy an uncertain space between domestic and international business. Some lack citizenship in their country of residence, depriving them of the protections afforded to investors by the country-of-residence government and its embassy. This may cause them to be uniquely vulnerable to certain types of political risk including adverse government actions such as expropriation, extortion, or limitations on the repatriation of profits.

The most promising path to solidify (or challenge) the causal inference made in this paper and to parse between the information-based theory proposed here and plausible alternatives involves data that differentiates directly between diaspora and non-diaspora FDI. The next step in this research agenda is a firm-level comparison between diaspora-owned firms and other foreign firms.

Conclusion

I argue that diaspora investors differ from other foreign investors in both their access to information and their costs of accessing publicly available information, and that these information-related differences make diaspora direct investors more sensitive to changes in political risk in the homeland. Empirically, I demonstrate that FDI flows in high-migrant dyads are more sensitive to over-time changes in political risk in the homeland than are FDI flows in low-migrant dyads, and that this differential sensitivity manifests primarily with regard to decreases in political risk. This suggests a need to revise downward some of the most optimistic expectations in the policy literature regarding diaspora direct investment as a source of capital for fragile states. It also challenges current practice in the international political economy literature, where it is common to treat political risk as a country-level variable and ignore variations across types of firms. These results indicate that disaggregating investment flows by investor type would substantially improve our understanding of how political risk affects investment behavior.

The political science and economics literature demonstrates a causal relationship between diaspora populations and flows of FDI, I develop, and test key implications of, an information-based theory of diaspora difference. The evidence that migrant-induced investment is more sensitive than other FDI to risk-decreases is consistent with the theory that diasporans are more attentive to information about the homeland than are other potential investors. I argue that diasporans monitor political and social conditions in the homeland for non-business reasons, and when the risk

climate improves, they have the information necessary to exploit the new opportunities that these improvements produce.

My findings suggest that diasporans' non-pecuniary motivations do not reduce their sensitivity to political risk, and overturn expectations of risk insensitivity that are currently influential in US development policy. However, while diaspora direct investment diaspora direct investment does not substitute for, and is no more likely than other FDI to precede, basic institutional stabilization in fragile states, these results suggest it can serve as a robust incentive for, and complement to, these improvements.

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Appendix

Table 3.5: Descriptive Statistics From Annual Data

Variable Name	Category	Mean	Standard Deviation	Minimum	Maximum	Number of Observations
FDI Inflows (Millions of USD)	Overall	65.8	468	-23700	15700	N=31100
	Between		168			N=3920
	Within		393			T-bar=7.94
FDI Inflows (logged)	Overall	4.66	10.6	-23.9	23.5	N=31100
	Between		6.09			n=3920
	Within		8.62			T-bar=7.94
Other FDI Outflows from Country *i* (logged)	Overall	21.2	9.38	-24.4	26.7	N=30900
	Between		6			N=3910
	Within		7.72			T-bar=7.91
Political Risk	Overall	11.5	4.45	3	21	N=34100
	Between		4.17			N=4040
	Within		1.54			T-bar=8.46
Migrant Stocks	Overall	34.3	312	0	10200	N=48500
	Between		265			n=4590
	Within		0			T-bar=10.6
Education in Country *j*	Overall	-0.116	2.01	-86.7	22.9	N=32600
	Between		1.55			n=3690
	Within		0.453			T-bar=8.84
BIT to Date	Overall	0.195	0.396	0	1	N=54083
	Between		0.336			n=6066
	Within		0.159			T-bar=8.92
Distance (logged)	Overall	0.00245	0.0043	0	0.01	N=41400
	Between		0.00358			n=5010
	Within		0.00177			T-bar=8.27
Common Language	Overall	0.00057	0.00366	-0.0159	0.0286	N=28300
	Between		0.00331			n=3110
	Within		0.00121			T-bar=9.11
Common Border	Overall	0.000055	0.00143	-0.00851	0.00949	N=28300
	Between		0.00135			n=3110
	Within		0.00049			T-bar=9.11
Colonial Relationship	Overall	-0.000037	0.000563	-0.00851	0.00949	N=28300
	Between		0.000485			n=3110
	Within		0.000168			T-bar=9.11
Oil Reserves Per Capita	Overall	-0.000336	0.00218	-0.00851	0.00949	N=26700
	Between		0.00173			n=3930
	Within		0.000958			T-bar=6.8

Table 3.6: Descriptive Statistics From 3-Year Blocks of Data

Variable Name	Category	Mean	Standard Deviation	Minimum	Maximum	Number of Observations
Δ FDI Inflows	Overall	31.2	356	-7810	6320	6630
	Between		136	-1910	2300	3340
	Within		314	-5860	4900	1.99
Δ FDI Inflows (logged)	Overall	2.73	12.8	-22.8	22.6	6630
	Between		7.42	-20.4	21	3340
	Within		10.7	-29.5	34.8	1.99
Δ Other FDI Outflows from Country *i* (logged)	Overall	11.6	19.6	-25.8	26.4	6620
	Between		13.3	-25.4	26.4	3330
	Within		15.3	-25.7	45.4	1.99
Political Risk	Overall	0.0186	0.114	0	1	11400
	Between		0.0591	0	0.5	4410
	Within		0.0927	-0.481	0.769	2.59
Migrant Stocks	Overall	0.258	0.174	-0.537	1.46	8430
	Between		0.135	-0.0741	1.01	3170
	Within		0.112	-0.666	1.33	2.66
Education in Country *j*	Overall	-0.459	1.59	-8.9	10.4	8680
	Between		1.11	-4.3	5.67	3430
	Within		1.24	-7.53	6.81	2.53
BIT to Date	Overall					
	Between					
	Within					
Distance (logged)	Overall	0.0224	0.0923	0	0.77	13300
	Between		0.093	0	0.77	3940
	Within		0	0.0224	0.0224	3.36
Common Language	Overall	8.19	0.742	4.76	9.38	13000
	Between		0.718	4.76	9.38	3660
	Within		0	8.19	8.19	3.56
Common Border	Overall	0.121	0.326	0	1	13000
	Between		0.338	0	1	3660
	Within		0	0.121	0.121	3.56
Colonial Relationship	Overall	0.00975	0.0983	0	1	13000
	Between		0.0916	0	1	3660
	Within		0	0.00975	0.00975	3.56

Chapter 4:
Diaspora-Owned Firms and the Value of Social Networks

Abstract

A causal relationship between diaspora populations and bilateral foreign direct investment has been established empirically, but the causal mechanism driving this relationship remains unclear. Do diaspora-owned foreign firms enjoy a competitive advantage in the homeland, and if so, what accounts for this? I argue that diasporans have important social network based advantages over non-diaspora-owned firms, and that these competitive advantages explain the relationship between diaspora populations and investment flows. This paper uses data from an original survey of 174 foreign-owned firms in Georgia to explore mechanisms of diaspora difference, focusing particularly on how firms use social networks in business. I find that diaspora-owned firms use social networks more than other foreign firms in the acquisition of real estate, and that owners and managers at diaspora-owned firms perceive social networks to be more important to firm location decisions and to overall profitability than do their counterparts at non-diaspora-owned firms. This indicates that social networks provide important competitive advantages to diaspora-owned firms, advantages that may explain the causal relationship between diaspora populations and global flows of foreign direct investment.

Introduction

A recent literature in economics and political science has established a causal relationship between the size of migrant populations and bilateral flows of foreign direct investment (FDI) to the migrants' homeland (Javorcik et al. 2011; Leblang 2010; Kugler and Rapoport 2007; Docquier and Lodigiani 2010). The amounts in question are not small. While direct measures of the volume of diaspora investment (investment by migrants and their descendents) are not available for most developing countries, some estimates suggest that diaspora direct investment accounted for over 50% of FDI inflows to China during the 1990s (Huang 2003; Ye 2010) and 20-30% of FDI flows into India during the same time period (Ye 2010). The survey discussed below finds that 17% of foreign-owned firms in Georgia, including the largest firm in the sample, have at least one diaspora owner.

Despite the substantive importance of migrant-induced investment, the causal mechanisms driving the relationship have not been established empirically, and indeed there is ambiguity as to whether these flows are attributable to diaspora-owned firms, firms employing members of the diaspora, or other factors entirely (Kugler and Rapoport 2007, Leblang 2010). This uncertainty remains because the existing empirical literature examines aggregate cross-national flows of FDI, which do not allow researchers to distinguish directly between flows of diaspora and non-diaspora investment. To identify the causal mechanism linking migrant populations to flows of investment, it is necessary to differentiate the behavior of diaspora-owned firms from that of other foreign firms. Only at the firm level is it possible to determine whether

diaspora-owned firms enjoy a competitive advantage in the country of origin and, if so, to identify the source of that competitive advantage. This paper introduces the results of a new firm-level survey designed that offers the first empirical comparison between diaspora-owned firms and a comparable set of non-diaspora-owned foreign firms.

Of primary interest is the use of social networks by diaspora-owned firms. I argue that diasporans' social networks in the homeland provide diasporans with two important competitive advantages: access to privileged channels of information, and a means of establishing trust with business counterparts. In concurrent work I use cross-national evidence on bilateral flows of FDI to show that migrant-induced FDI responds more strongly and more quickly to changes in the level of political risk than does FDI, a finding I attribute to diasporans' access and attention to information (Graham 2010). The most direct implications of this theory, however, are only testable at the firm level. I test those implications here.

The core hypotheses of this paper are that diaspora-owned firms use social networks more than their non-diaspora-owned peers and that firm owners and managers at diaspora-owned firms perceive these networks to be more important to firm profitability and firm location decisions that do their peers at non-diaspora-owned firms. These hypotheses are tested using new survey data on 174 foreign-owned firms operating in Georgia. This data allows direct comparison between diaspora-owned

firms and non-diaspora-owned firms operating in the same risk environment.[1] While the analysis is based on firms operating in a single developing country, the theorized differences between diaspora-owned and other foreign firms are expected to hold across a wide range of diaspora communities. In terms of its investment climate in particular, Georgia is a typical developing country, and hence a "typical case" in the language of small-N case selection.

The paper is organized as follows. First, I review the relevant literature and establish a set of hypotheses regarding the use of social networks by diaspora-owned firms. Then I describe the details of the survey and the data it produced, and conduct hypothesis tests. I finish by discussing open-ended responses in the survey, drawing conclusions and outlining plans for future research.

A Review of the Current Literature

There is recent evidence that migrant populations cause flows of FDI from their country of residence to their homeland (Javorcik et al. 2011, Leblang 2010, Kugler and Rapoport 2007; Docquier and Lodigiani 2010). One possible explanation is that diasporans have access to privileged channels of information about the homeland via their social networks. Survey evidence from domestic firms in the United States shows that social networks increase not only the quantity of information firms have access to, but also the quality of information (Uzzi 1996, 1999; Yli-Renko, Autio and Sapienza 2001). Similarly, transnational ethnic and social networks provide privileged channels of information that allow efficient matching between potential

[1] One hundred seventy-four firms responded to the survey. Due to item non-response, only 161 are used in the analyses presented here.

business collaborators and between capital and opportunity (Rauch and Casella 2003). Migrant-induced FDI is more sensitive than other FDI to over-time changes in the level of political risk in the homeland, which is consistent with the argument that migrants' levels of information about the homeland serves as an important mechanism in this causal relationship (Graham 2010).

There is an extensive management literature that links the social networks of firm owners and managers to a variety of benefits beyond mere access to information: these include access to capital[2] (Uzzi 1999; McMillan and Woodruff 1999), adaptation of innovation (Abrahamson and Rosenkopf 1997; Moran 2005), and firm profitability and survival (Hochberg, Ljungqvist and Lu 2007; Musteen and Francis 2010).[3] Additionally, there is evidence that social networks are more important in environments, like emerging markets, where market uncertainty is high (Podolny 1994). While networks are important to all these outcomes, simply adding more connections to other firms may not increase positive outcomes in a linear fashion (e.g., Uzzi 1996; Gargiulo and Benassi 2000). However, the simple expectation presented in this paper – that diaspora firm owners are able to use social networks to greater advantage than non-diaspora foreign owners – is consistent with even these more nuanced understandings of the effect of network structure.

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[2] Malesky and Taussig (2009) find that, while socially connected firms in Vietnam have greater access to bank capital, relationship-based lending is not an efficient means of allocating capital. They find that firms with access to bank capital are not more profitable than firms without access, and the most successful investors forego bank capital entirely.

[3] This literature overlaps with a literature in which the network position of firms in considered as an independent variable explaining similar outcomes: because I am focused on differentiating on the network access of firm owners in particular, I restrict this discussion to the literature references the social networks of individuals rather than entire firms.

Diasporans often have extended family members still living in the homeland, and first-generation migrants in particular often still retain ties to friends in the homeland as well (Gillespie et al. 1999; Sheffer 2006; Portes, Guarnizo and Haller 2002). While a literature on diaspora investment motivations has shown that diasporans believe that their diaspora identity would give them a competitive advantage in investing, it has yet to be demonstrated that this is the case (Gillespie, Sayre and Riddle 2001). Anecdotal evidence suggests that many diasporans over-estimate the degree to which their social ties will facilitate business success (Riddle, Hrivnak and Nielsen 2008).

The questions that the current literature leaves unanswered are: do diasporans actually retain the type of social networks that can enhance the profitability and survival of diaspora-owned firms? Do diaspora-owned firms use social networks more than their non-diaspora-owned counterparts, and do managers perceive these networks to be important to the profitability of their firms? I answer those questions here.

Theory: Diaspora-Owned Firms and Social Networks

Both the causal link between migrant stocks and bilateral FDI flows found in the literature, and the sensitivity of diaspora direct investment to political risk are consistent with diasporans using social networks to gain access to information. As noted above, at least some diasporans believe that the social networks they retain in the homeland are useful in business. However, it is possible that the types of social networks diasporans retain are, in fact, of no business use at all. It may be that

diasporans' friendship and family ties, and the obligations that come with them, are an impediment to running a profitable business rather than an advantage. Hence, the core empirical tasks of this paper are to establish whether diaspora-owned firms use social networks more or differently than their non-diaspora-owned peers, and whether firm owners and managers at diaspora-owned firms report that social networks are more important to firm profitability and firm location decisions than do their peers at non-diaspora-owned firms.

There are three primary ways in which I expect diaspora-owned firms to use social networks to increase the profitability of their firms:

1) Social networks provide privileged channels of information about potential risks and opportunities for investments in the homeland;

2) Social networks serve as an informal means of enforcing contracts with other firms and individuals in the homeland;

3) Relational norms, such as trust, can operate within social networks as substitutes for formal contracting (e.g., Granovetter 1985; Adler 2001).

These three uses for social networks are not mutually exclusive. These uses may complement each other and overlap, and a single firm may use social networks in all of these ways simultaneously. Indeed, it is plausible that social networks allow the formation of trusting relationships between foreign firms and the firms and individuals with which they interact, and that this trust facilitates the sharing of information and collaboration to enforce contracts.

Diasporans' Use of Social Networks: Access to Information

Incomplete and asymmetric information is a major cause of the international immobility of capital and an impediment to international trade (Frankel 1992; Gordon and Bovenberg 1996).[4] Social networks increase not only the quantity of information that firms have access to, but also the quality of information (Uzzi 1996, 1999; Premaratne 2001). Diasporans, and particularly first-generation migrants, retain access to social networks in their homelands that other potential foreign investors generally do not have access to. If diasporans are better informed, they should have greater ability to identify and exploit profitable business opportunities in the homeland and to anticipate risks that may arise there. While previous literature has asserted that diasporans use social networks to access information (e.g. Freinkman 2002; Bandelj 2002), this has not been tested directly.[5]

One of the implications of diasporans' use of social networks to access information is that they should be better able to anticipate and avoid certain types of political risk. Let us take as an example the type of transfer risk that can arise when a country passes new capital control laws. Some diaspora-owned firms may become aware that such laws are under consideration before deliberations are made public – they may have friends or family members that work in government or otherwise have access to classified or private information. Probably much more common, however, is that early information about laws under consideration is reported only in the local

[4] For a review of the literature on "home bias" in investing, see Lewis (1999).
[5] It is interesting that one of the few existing studies that explore diaspora business advantages at the firm level, Schulte (2008), mentions a linguistic advantage but makes no mention of social networks at all.

language press. Furthermore, only a relatively small number of well informed homeland residents may be able to accurately forecast the likelihood that the laws being considered will ever be passed, much less take effect, and their opinions may not be widely circulated. Therefore, regarding any change in the political or legal system, local social networks provide access to the type of knowledge needed to anticipate adverse (or favorable) changes in the political and legal situation before they occur. Diasporans' social networks may be useful in this role even if those networks do not include highly placed government officials or others with access to secret information.

Access to information, however, is useful in more contexts than simply anticipating government behavior. We might expect social networks to be most effective at providing information involving actors that enjoy less media coverage than the central government. Particularly in less-developed markets, lack of information often prevents firms from identifying trading opportunities and leads to inefficient matching between buyers and sellers (Solinger 1989; Ronnas 1992; McMillan 1997). If diasporans' social networks are effective in delivering market information as well as risk information, it gives them a competitive advantage even in countries where political risk is low.

Access to market and risk information is an advantage that local firms generally have over foreign firms, and that diaspora-owned firms may have over other foreign firms.

Diasporans' Use of Social Networks: Trust and Contract Enforcement

The law and investment literature suggests that formal contracting institutions and social networks are substitutes. Either firms can rely on formal institutions to enforce contracts and protect them from opportunistic behavior, or they can rely on social networks and personal relationships to play these same roles. The transformation from kinship and social ties to formal institutions as an organizing social principle is often viewed as a pivotal step in the process of economic development (Berry 1993). When formal institutions are "weak, hostile, or indifferent," social structures can substitute for them (Woolcock and Narayan 2000). Wang (2000) argues that informal *Guanxi* networks in China substitute for weak formal institutions, and that members of the Chinese diaspora serve as "agents of transmission" who connect foreign firms with these networks and thereby facilitate FDI.[6]

There are two related mechanisms through which social networks may be used to substitute for formal contracting institutions. The first is by providing a direct means of enforcing contracts and sanctioning opportunistic behavior. The second is by establishing trust between diaspora-owned firms and potential business counterparts, reducing the likelihood that diaspora-owned firms are victimized by opportunistic behavior.

First, social networks might be expected to facilitate direct enforcement of a formal contract. Consider a hypothetical dispute between a diaspora-owned firm and a

[6] Weidenbaum and Hughes (1996) make similar observations.

domestic customer that has received a shipment of goods from the diaspora-owned firm but refuses to pay. If the court system in the homeland is efficient and reliable, the diaspora-owned firm can bring suit against the domestic customer and eventually win payment of their claim. If the domestic courts are slow, expensive, or unreliable, it might be more profitable for the diaspora-owned firm to rely on informal means of enforcing the contract. If the owner of a diaspora firm has friendship or family ties with a large number of other firms in the same industry, she can encourage these firms to refuse to do business with the customer in question until the debt is paid. If the diaspora-owned firm and the customer have friendship or family ties with each other, they may opt for informal mediation of the dispute by another friend or family member to avoid rupturing the social bond between them in an all-out dispute. In either scenario, the social ties of the diaspora owner facilitate the informal, rather than the judicial, resolution of the dispute. While potentially useful in any institutional environment, these informal contract enforcement and dispute resolution mechanisms are most valuable when formal contracting institutions are expensive, slow, or unreliable.

It is also possible for diasporans to use social networks as a more indirect substitute for, or complement to, formal contracting institutions. The role of relational trust in reducing transaction costs and increasing economic efficiency is well established at the cross-national level (e.g. Knack and Keefer 1996; Knack and Zak 1999) and at the firm level (Sako 1992; Barney and Hansen 1994; Chow and Holden

1997).[7] Reputation, trust, and norms of reciprocity are important factors determining the duration and stability of exchange structures between firms and individuals (e.g. Granovetter 1985; Larson 1992; Adler 2001).

If diasporans retain social networks in the homeland, they do not start from scratch in forming important business relationships when they start a new business or expand the operations of an existing business into the homeland. Consistent with the theory outlined above, social ties are expected to provide a level of trust not only between a diasporan and his/her family and friends: individuals with whom the diasporan shares a social tie can serve as the diasporan's advocate in forming trusting relationships with people the diasporan has never met.[8] Diasporans' social networks in the homeland increase the degree to which diaspora-owned firms can use social trust to lower the likelihood that they are victimized by opportunistic behavior.

Testable Implications

If diaspora-owned firms have access to business-relevant social networks in the homeland, and if they use these social networks to gain competitive advantage, this should manifest itself in differences between diaspora- and non-diaspora-owned foreign firms across a range of behaviors. The first implication addresses firms' initial decision to invest in Georgia. The initial decision to enter the country is crucial

[7] Jeffries and Reed (2000) find that too much trust in a relationship, as well as too little trust, can damage firms' abilities to solve problems of adaptation in relational contracting. This is one of several findings that complicate the relationship between trust and contracting efficiency, but the broad consensus in the literature is that higher levels of trust in business relationships is generally beneficial to both parties.

[8] Wong and Boh (2010) examine managers' intrafirm networks to demonstrate the ability of third parties to serve in this capacity. I extend this logic to diasporans' interfirm networks.

because part of what we are seeking to explain is the impact of diaspora populations on flows of FDI. Do social networks drive the decision of diaspora-owned firms to invest in the homeland?

Hypothesis 1: Diaspora-owned firms are more likely than other foreign firms to cite "your firm's friendship and family ties to Georgia" as an important positive factor influencing their decision to invest in Georgia.

Social networks will affect firm entry decisions if owners and managers expect these networks to augment firm profitability in the new location. However, it is also important to look at whether those expectations prove realistic. Are social networks important to the profitability of diaspora-owned firms? Perhaps the most straightforward means of assessing the importance of social network relationships to firm profitability is to ask respondents to assess this importance subjectively. This has the advantage of capturing a broad range of mechanisms through which social networks might convey advantage in a single metric.

Hypothesis 2A: Diaspora-owned firms report that their owners' and managers' **family relationships** are more important to the firm's profitability than do their counterparts at non-diaspora owned firms.

Hypothesis 2B: Diaspora-owned firms report that their owners' and managers' **friendships** are more important to the firm's profitability than do their counterparts at non-diaspora owned firms.

A more objective metric is provided by exploring firms' use of social networks in the purchase/rental of real estate. Almost all firms, regardless of sector, require at least some form of real estate – be it for offices, storefront, warehouses, or other use.[9] Therefore, we might expect that the purchase/rental of real estate is a task facing most firms and one in which they will deploy social networks if it is advantageous to do so. If diaspora-owned firms are better able than other foreign firms to use social networks to augment their profitability, this is one area where we would expect this difference to manifest itself.

Hypothesis 3A: Diaspora-owned firms are more likely than non-diaspora owned firms to report having rented or purchased real estate with the help of a **family member** of one of the firm's owners or managers.

Hypothesis 3B: Diaspora-owned firms are more likely than non_diaspora owned firms to report having rented or purchased real estate with the help of a **friend** of one of the firm's owners or managers.

[9] When testing hypotheses 3A and 3B, I omit firms in the real estate sector on the grounds that this question has a distinctly different meaning to firms whose business is the buying and selling of real estate.

These hypotheses allow us to test for differences in social network use between diaspora- and non-diaspora-owned foreign firms with regard to firm entry decisions and later firm behavior, in terms of concrete firms behavior as well as subjective impact on profitability.

Why Georgia?

In the language of small-N case selection, Georgia is a "typical case" for a developing country with regard to its wealth and political institutions, and an "extreme case" regarding the size of its diaspora population.[10] Because cross-national data do not differentiate between diaspora and non-diaspora FDI, it is difficult to accurately assess the volume of diaspora direct investment in a given country before a survey is conducted. Georgia has a large diaspora population, receives substantial remittances, and receives a large amount of FDI relative to the size of its GDP, making it likely ex ante Georgia was a recipient of substantial diaspora investment. However, as the figure below demonstrates, Georgia is a relatively typical developing country in terms of its wealth and the quality of its institutions.

Only in terms of war risk does Georgia score substantially higher than the median developing state, and its level of war risk is typical of fragile states. Georgia's score of 5 (out of 7) for war risk in 2009, the year after it experienced a small, weeklong war with Russia, was the same as its level of war risk in 2003, the first year for which data is available. I argue that, despite the 2008 conflict, Georgia continued

[10] See Seawright and Gerring (2008) for a discussion of the use of "extreme cases" and "typical cases" in hypothesis testing. As discussed in related work, the political relationship between the Georgian government and the Georgian diaspora at the time of this writing was generally positive and uncomplicated, with efforts made by the Georgian government to promote diaspora investment (Graham 2011).

throughout the 2000s, to be a business climate typical of developing states – one in which political turmoil and the risk of political violence were ever-present, but only occasionally acute, sources of risk for foreign investors.

Table 4.1:
Georgia in Comparison to Other Fragile and Developing Countries

	Georgia	Fragile states median (25^{th} -75^{th} percentile)	Developing States Median (25^{th} - 75^{th} percentile)
GDP (millions of USD)	5,500	12,300 (5,500 – 55,000)	6,500 (1,700 – 24,000)
GDP per capita (USD)	1,300	1,200 (280 – 2,200)	1,100 (380 – 2,000)
Population (millions)	4.3	20 (7.3 – 41)	9.7 (3.1 – 27)
FDI Inflows (Millions of USD)	1,600	790 (120 – 2,600)	748 (120 – 2,200)
FDI Inflows (% of GDP)	12	3.6 (1.9 – 6.1)	4.3 (1.9 – 7.9)
Remittances (Millions of USD)	73	73 (15 – 270)	73 (8.1 – 290)
Remittances (% of GDP)	5.7	2.0 (0.7 – 5.7)	4.3 (0.4 – 5.6)
Emigrant Stock (1990)	48,000	110,000 (24,000 – 300,000)	88,000 (23,000 – 330,000)
Emigrant Stock (2005)	190,000	216,863 (82,000 – 640,000)	168,220 (36,000 – 490,000)
Emigrant Stock (% of population) (2005)	4.3	1.9 (0.7 – 3.0)	2.0 (0.9 – 4.4)
Cost of Dispute Resolution (% of claim)	35	30 (23 – 44)	33 (23 – 45)
War Risk (1-7) (Higher = Riskier)	5	5 (3 – 6)	3 (2 – 4)
Government Risk (1-7) (Higher = Riskier)	4	4 (4 – 5)	4 (3 – 5)
Transfer risk (1-7) Higher = Riskier	6	6 (4 – 7)	5 (4 – 6)

Fragile countries n = 31; Developing countries n = 107

The range from 25^{th} percentile to 75^{th} percentile is given in parentheses.
All items are measured as of 2008, except for the measures of migrant stocks, whose years are given.

I define developing countries as those with a 2008 GDP per capita of less than $3,855 – those countries defined by the World Bank as low income and lower middle income.

Fragile states are those that meet any of the following criteria: 1. A violent intra-state conflict in the last 3 years; 2). A change of executive leadership via coup or military overthrow in the last three years; 3) failure to control all their territory 4) occupation by a foreign power or not controlled by a unified entity (i.e. coded as "Interregnum" or "Interruption" in the Polity IV data).

The Survey

The *Capital and Conflict: Georgia* survey is the first firm-level survey (to my knowledge) that compares diaspora-owned firms to other foreign firms. Surveys were conducted, in person, with the owner or manager of 174 foreign owned firms between February and June 2010. Each respondent was given a choice of taking the survey in English or Georgian.[11]

The sampling frame was derived from a list of foreign-owned firms provided by the Georgian Ministry of Finance. The list included all foreign firms that met the following criteria: 1) a for-profit enterprise; 2) at least 10% foreign ownership; 3) registered as active and paying taxes as of June 1, 2009; 4) obtained its first registration in Georgia after the year 2000. This sample was supplemented with a randomly drawn sample of 300 of the 450 firms that responded to the Ministry of Finance's Balance of Payments survey in 2009. These firms also met criteria 1-3, but some were initially registered prior to 2000.[12]

Diasporans are defined in the literature as a subset of migrants and their descendents: individuals that, in some way retain a relationship with their homeland and identify themselves as part of a community associated with that homeland (e.g. Safran 1991; Gillespie et al. 2001; Sheffer 2006). To identify diaspora-owned firms, enumerators read the following text:

> I'm going to refer to the "Georgian diaspora" in some of the following questions. The Georgian diaspora includes all individuals

[11] The survey was taken through three rounds of reverse translation and refinement to ensure equivalence between the English and Georgian versions.
[12] Some firms on the Ministry of Finance list also turned out to be registered before the year 2000, but had been re-registered after that date.

> who live outside of Georgia but who consider themselves to be Georgian. It includes people who were born in Georgia and emigrated to other countries. It also includes people who were born in other countries, but whose ancestors are from Georgia. If a Georgian owner of the firm was living abroad when he decided to start this firm in Georgia, we count this as diaspora investment even if the owner moved back to Georgia.
>
> Are any of the owners of your firm members of the Georgian diaspora?

The answer to this question is used to categorize firms as either diaspora-owned or non-diaspora-owned.[13]

Survey Non-Response

Attempts were made to contact a total of 1,024 firms between February and June 2010, representing over 80% of the foreign-owned firms that officially entered Georgia during the target period. Only 484 could be contacted and, of those, only 362 met the criteria listed above (foreign ownership, for-profit, and currently operating).[14] Many of the firms that could not be contacted were closed – some had closed recently and others had closed before 2009 but had not been purged from the tax rolls. Other firms could not be contacted because they had changed addresses and phone numbers since the lists were updated. Georgia has no up-to-date telephone or address directory, and so only a minority of the firms that have relocated within the last several years could be contacted. Few of the foreign firms operating in Georgia maintain websites.

[13] Respondents at 12 firms either did not know or chose not to answer this question, and these firms are not used in analyses.

[14] We only attempted to contact firms that the Ministry of Finance or State Department of Statistics indicated met the three criteria for inclusion, but nonetheless many firms on the lists provided were either non-profit, fully Georgian-owned, or were closed.

Of the 362 firms that were successfully contacted and that met the basic criteria for inclusion in the sample, surveys were successfully conducted with 167; 195 firms refused to participate. In most firms, enumerators were successful in scheduling interviews with either the firm owner or a firm manager. In some cases, however, this was not possible and a shorter version of the survey (10 of 53 questions) was asked of front-desk or other available personnel. These "front desk" surveys asked questions about firm demographics, such as sector, headquarters country, firm size, and diaspora ownership. The front desk surveys do not include sufficient information to include these firms in most hypothesis tests, but do provide valuable additional information about firm demographics in the sampling frame.

The Sample

The sampling lists discussed above produced 167 respondents: 26 diaspora-owned firms and 129 non-diaspora-owned firms (12 respondents did not answer the question on diaspora ownership).[15] Because of the low number of diaspora-owned firms, a non-random supplement of seven additional diaspora-owned firms was added to the sample, bringing the total number to 174.[16] Table 4.12 in the appendix provides a comparison between the diaspora-owned firms in the random sample and those in the supplemental sample.

[15] All firms in the sample are privately owned, i.e. none are government-owned or publicly traded.
[16] Two of these supplemental firms were identified by the State Office for the Diaspora; five more were located via snowball sampling. To create the snowball supplement, enumerators asked respondents at the end of the interviews if they knew of any diaspora-owned firms that we might be able to contact.

Table 4.2: Number of Respondents by Diaspora Ownership					
Survey Type	Diaspora-owned firms (random only)	Diaspora-owned firms (supplement included)	Non-diaspora-owned firms	Diaspora ownership unknown	Total
Owners/Manager's Survey	23	30	121	10	161
Front Desk Survey	3	3	8	2	13
Total	26	33	129	12	174

The only demographic information available from firms that refused to participate in the survey is the firm's home country, which in most cases could be drawn from the firm lists provided by the Georgian government.[17] Therefore, I examine the number of respondents and non-respondents from each region. The following table shows that refusals are distributed evenly across regions. The percentage of diaspora-owned firms is smaller in the West than in other regions, but because this is the largest region in terms of number of firms, the number of diaspora firms from the West is still substantial.

[17] Not all firms have home countries listed on the original sampling lists. For those firms that responded to the survey but did not have a home country listed, I treat the location of the firm headquarters as the home country. Some respondents did not report a headquarters location outside of Georgia – the home country of those firms remains as missing data.

Table 4.3:
Distribution of Diaspora and Non-Diaspora Ownership by Home Region

Region	Number of Qualifying Firms Contacted	Number of Responses	Percentage Non-Response	Percentage of respondents who are diaspora-owned
Former Soviet Union	69	31	55%	23%
W. Europe, U.S. & Australia	162	73	55%	13%
Middle East	68	31	54%	27%
Other	14	7	50%	14%
Region Unknown	56	24	57%	33%
Total	**369**	**174**	**56%**	**20%**

Within the set of firms that were successfully interviewed, I compare diaspora and non-diaspora-owned firms across a range of covariates. By establishing that firm demographics between the two groups are similar, I am then better able to attribute any observed differences in firm behavior to diaspora ownership itself, rather than alternative factors like firm size or sector. Firm size is captured by the number of employees; I use dummy variables for minority, majority, and 100% foreign ownership, and Greenfield investment, as whether the firm is located in the capital city of Tbilisi. I also include dummy variables for primary, secondary, tertiary, and quaternary sectors. Because of the 2008 war between Russia and Georgia, I also include a dummy variable for whether the firm is headquartered in Russia.

With regard to the demographic characteristics of individual respondents, I examine whether the respondent was an owner of the firm (as opposed to a manager)

and whether the respondent self-identified as ethnically Georgian. I also examine the language in which the survey was conducted.

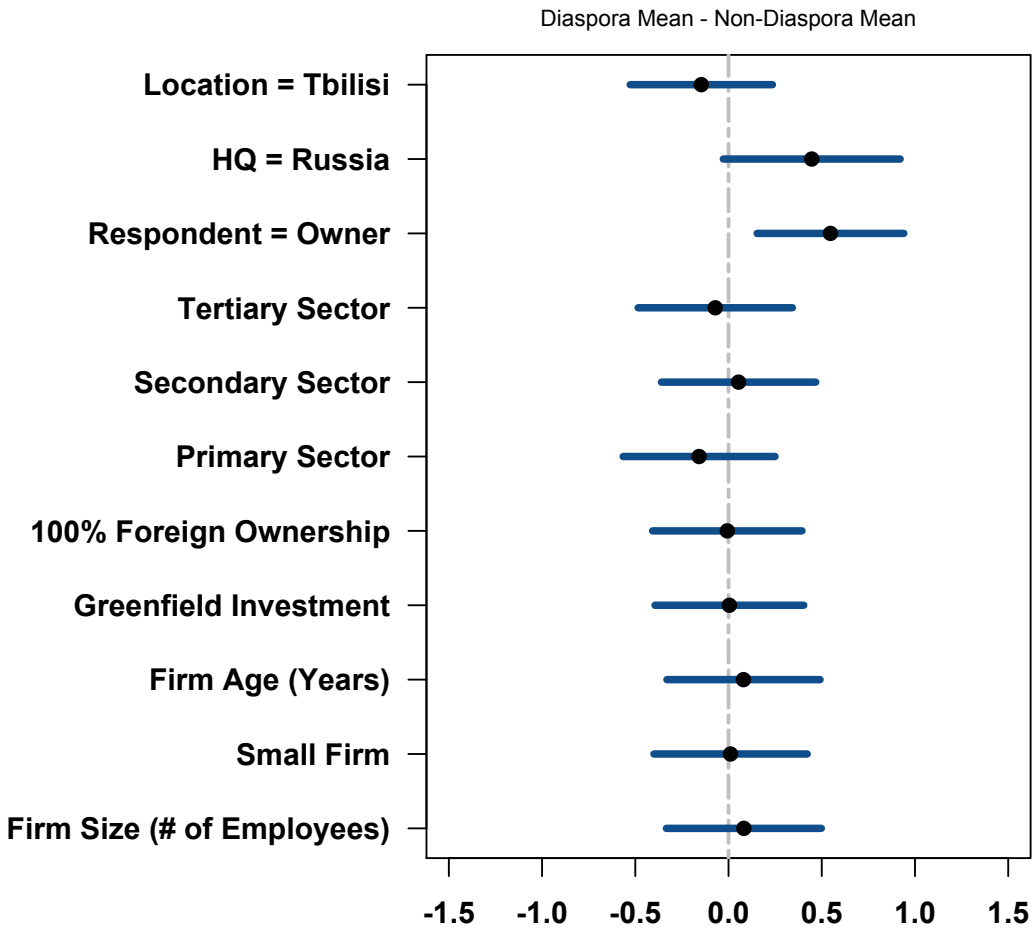

Figure 4.1: Demographic Comparison

*Note: Units = standard deviations, horizontal lines give 95% confidence intervals.

The demographic characteristics for which I observe the large difference between diaspora and non-diaspora-owned firms are the whether the respondent is the firm owner and whether the firm is headquartered in Russia. At each firm, enumerators asked for a meeting with the firm owner or manager. At diaspora-owned firms, the firm owner was more likely to be available for interview. This makes sense if diaspora owners are more likely than other foreign owners to live in Georgia at least part time while running their business. All of the respondents at diaspora-owned firms self-identified as ethnically Georgian, as did 89% of respondents at non-diaspora-owned firms. A higher proportion of diaspora-owned firms are headquartered in Russia, which can be attributed to the large size of the Georgian population living in Russia.

Firm owners may have somewhat different perspectives on the behavior of their firm than do managers. Similarly, firms based in Russia, which has a hostile relationship with Georgia, may face unique political risks and have unique uses for social networks. This necessitates a multiple-regression framework controlling for the identity (owner vs. manager) of the respondent and whether or not the firm is headquartered in Russia. Other firm demographic characteristics are included as supplemental controls, but do not substantively affect results.

The range in firm size in the sample is substantial, with firms varying from a single employee to as many as 1400. However, as Figure 1 demonstrates, the difference in firm size between diaspora-owned and non-diaspora-owned firms is not large. Three-quarters of firms in both groups qualify as “small” enterprises: 75% of

diaspora-owned firms and 77% of non-diaspora-owned firms have 50 employees or less. Similar consistency is observed in the distribution of firms between sectors,[18] the distribution of firms between regions (not pictured), the share of foreign ownership, and firm age.[19]

Item Non-Response, Multiple Imputation, and Summary Statistics,

In dealing with them missing values that arise from item non-response, I employ the multiple imputation by chained equations technique developed by Patrick Royston (2004, 2009).[20] Multiple imputation reduces the bias and eliminates the sample-size reduction associated with list-wise deletion, allowing full use of the information collected in the survey (King et al. 2001).[21] In the raw data, the independent variable of interest, diaspora ownership, is 6% missing, firm demographic characteristics used as controls are between 2% and 18% missing, and the dependent variables range from 9% to 11% missing.[22]

Item-non-response is relatively low in the data, and multiple imputation does not substantively alter the results of the analysis. Similar coefficients of interest are

[18] I also checked the balance in a composite of real estate and construction – the balance here is quite even. I checked because these sectors were particularly hard-hit during the downturn.
[19] It is worth noting that very few firms in the sample entered after the 2008 conflict with Russia: of the 18 that entered during this period, 5 were diaspora-owned and 12 non-diaspora-owned (1 unknown). Most of the firms in the sample entered Georgia as Greenfield investments, rather than mergers, partnerships, or acquisitions of Georgian firms: 76% of diaspora-owned and 78% of non-diaspora-owned firms.
[20] This is implemented using the ice and mim commands in Stata 10. I create 10 imputed datasets for analysis.
[21] The coefficients of interest are similar in analyses using list-wise deletion.
[22] These percentages refer only to the 161 firms that completed an owners/managers survey. Those firms where only a front-desk survey was completed are omitted from analysis (and imputation) because none of the social network questions are included on the front desk survey.

estimated using list-wise deletion. None of the substantive results reported are altered by the decision to impute missing values.

Table 4.4: Summary Statistics

Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
Diaspora Ownership	151	0.199	0.4	0	1
Respondent = Owner	157	0.242	0.43	0	1
Primary Sector	156	0.0705	0.257	0	1
Secondary Sector	156	0.301	0.46	0	1
Tertiary Sector	156	0.487	0.501	0	1
Quaternary Sector	156	0.141	0.349	0	1
100% Foreign Ownership	157	0.599	0.492	0	1
HQ in West	132	0.53	0.501	0	1
Number of Employees	144	89.4	218	1	1400
Tbilisi (location)	161	0.789	0.401	0	1
HQ in Russia	132	0.083	0.28	0	1
Real Estate Sector	158	0.057	0.232	0	1
Friendships (Importance)	148	4.149	2.183	1	7
Family (Importance)	147	2.585	1.944	1	7
Friendship (Real Estate)	143	0.252	0.436	0	1
Family (Real Estate)	144	0.118	0.324	0	1
Social Networks (Location)	148	0.278	0.449	0	1

A Theoretical Note About Diaspora Ownership

One important issue when moving from findings regarding migrant-induced FDI in the economics and political science literature to hypotheses regarding the behavior of diaspora-owned firms is that, in addition to investments by firms they own, migrants may induce investment in the homeland by firms from the country of residence they do not own. In particular, foreign firms can hire diasporans, perhaps gaining many of the advantages of diaspora ownership. The *Capital and Conflict: Georgia* survey does not distinguish between firms that hire diasporans and those that do not. By pooling across non-diaspora-owned firms that do and do not hire diasporans, I potentially bias results toward a finding of no difference between diaspora-owned and other foreign firms.

Results: Testing Hypothesis 1

Hypothesis 1 asserts that diaspora-owned firms are more likely than foreign firms to cite friendship and family ties to Georgia as an important reason for locating their investment in Georgia. Each respondent was given a set of 12 factors on show cards and asked to first sort the cards between factors that positively affected their firm's initial desire to invest in Georgia and those that negatively affected it. Respondents were then asked to rank order the most important positive and negative factors.

The dependent variable in Table 4.5 is whether or not a respondent ranked "your firm's family and/or friendship ties to Georgia" among the top three positive factors influencing their firm's initial decision to invest in Georgia.[23]

[23] The mean number of positive factors identified by diaspora owned firms is 3.03, for non-diaspora-owned firms the mean is 3.14. This similarity indicates that the difference in the probability of listing social networks among the top 3 positive factors is not driven by the total number of positive factors firms in each group identify.

Table 4.5: Friendship and Family Ties in Firm Entry Decisions

	(1)	(2)	(3)
Diaspora-Owned Firm	0.808***	0.671**	0.647**
	(0.265)	(0.282)	(0.300)
Respondent = Owner		0.718***	0.667**
		(0.262)	(0.275)
Primary Sector			0.207
			(0.571)
Secondary Sector			0.298
			(0.443)
Tertiary Sector			-0.189
			(0.449)
100% Foreign Ownership			-0.238
			(0.244)
Employees (logged)			-0.115
			(0.0845)
Greenfield			0.0520
			(0.311)
Location = Tbilisi			-0.367
			(0.298)
HQ = Russia			0.454
			(0.424)
Constant	-0.785***	-0.960***	-0.319
	(0.134)	(0.146)	(0.559)
Observations	161	161	161

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Consistent with Hypothesis 1, the results in Table 5 show that diaspora-owned firms are more likely than other foreign firms to cite friendship and family ties to Georgia as an important reason for their initial investment. This difference is significant at the

0.05 level in all specifications. In models 2 and 3, I control for whether the respondent is an owner (as opposed to a manager) because owners comprise a higher proportion of respondents among diaspora-owned than among non-diaspora-owned firms. In Model 3, I also control for sector,[24] a dummy variable for 100% foreign ownership, firm size (the logged number of employees), and a dummy variable for Greenfield investment. I continue to use these control variables in subsequent hypothesis tests of this form.

In substantive terms, 50% of diaspora-owned firms rank "your firm's friendship and family ties with Georgia" among the top three positive reasons influencing their firm's decision to enter Georgia, while only 21% of non-diaspora-owned rank social networks this highly.

Results: Testing Hypothesese 2A and 2B

Hypothesis 2A states that diaspora-owned firms report that their owners' and managers' friendships are more important to the profitability of the firm than do non-diaspora-owned firms. Hypothesis 2B makes the same prediction regarding family relationships.

These questions ask respondents to answer the following questions on a seven-point scale from "not important at all" to "extremely important." "How important are your owners' and managers' family relationships to increasing the profitability of your firm?**"** While there are some drawbacks to this approach – primarily that the responses to these questions are not anchored – there is no particular reason to believe

[24] The quaternary sector is the omitted category.

that respondents at diaspora-owned firms use a Likert scale in a systematically different way than their counterparts at non-diaspora-owned firms. As was established earlier, the demographics of respondents in the two groups are quite similar – nonetheless, the ordered logit models presented in Tables 4.6 and 4.7 include a range of control variables.

The dependent variable in Tables 4.6 and 4.7 is respondents' answers on a seven-point scale.

Table 4.6: The Subjective Importance of Family Relationships

	(1)	(2)	(3)
Diaspora-Owned Firm	0.973**	0.918**	1.014**
	(0.413)	(0.420)	(0.447)
Respondent = Owner		0.280	0.224
		(0.361)	(0.381)
Primary Sector			0.734
			(0.834)
Secondary Sector			1.224*
			(0.650)
Tertiary Sector			1.109*
			(0.656)
100% Foreign Ownership			-0.122
			(0.322)
Employees (logged)			-0.0332
			(0.118)
Greenfield			-0.777*
			(0.402)
Location = Tbilisi			0.592
			(0.446)
HQ = Russia			-0.167
			(0.578)
Cut 1	0.154	0.211	0.878
	(0.180)	(0.197)	(0.939)
Cut 2	0.699***	0.758***	1.459
	(0.188)	(0.203)	(0.942)
Cut 3	0.878***	0.937***	1.648*
	(0.194)	(0.211)	(0.934)
Cut 4	1.680***	1.738***	2.495**
	(0.224)	(0.242)	(0.943)
Cut 5	2.365***	2.424***	3.206***
	(0.278)	(0.283)	(0.957)
Cut 6	3.143***	3.204***	3.994***
	(0.382)	(0.389)	(0.975)
Observations	161	161	161

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

The results in Table 4.6 support Hypothesis 2A: there is evidence that respondents at diaspora-owned firms believe their owners' and managers' family relationships to be more important to firm profitability than do their counterparts at non-diaspora-owned firms. This result is significant at the 0.05 level. Among diaspora-owned firms, 37% report that family relationships are more than "somewhat important" to firm profitability (five or higher on a seven-point scale), while only 15% of non-diaspora-owned firms report this.

It is worth noting that firms engaged in Greenfield investments report that family relationships are less important to their profitability. This is consistent with the expectation that firms with large and relevant social networks are more likely to engage in joint ventures, but counter to the expectation that foreign firms' social networks are substitutes for partnerships with domestic firms.

Table 4.7: The Subjective Importance of Friendships

	(1)	(2)	(3)
Diaspora-Owned Firm	0.790**	0.661*	0.756*
	(0.367)	(0.386)	(0.418)
Respondent = Owner		0.801**	0.902**
		(0.349)	(0.347)
Primary Sector			0.964
			(0.761)
Secondary Sector			0.537
			(0.543)
Tertiary Sector			0.421
			(0.502)
100% Foreign Ownership			0.421
			(0.333)
Employees (logged)			0.00195
			(0.104)
Greenfield			-0.544
			(0.367)
Location = Tbilisi			1.291***
			(0.446)
HQ = Russia			0.274
			(0.609)
Cut 1	-1.127***	-0.993***	0.309
	(0.210)	(0.221)	(0.918)
Cut 2	-0.701***	-0.558***	0.788
	(0.192)	(0.204)	(0.916)
Cut 3	-0.515***	-0.369*	0.993
	(0.187)	(0.200)	(0.916)
Cut 4	0.342*	0.505**	1.925**
	(0.179)	(0.198)	(0.916)
Cut 5	0.785***	0.957***	2.406**
	(0.193)	(0.212)	(0.938)
Cut 6	1.729***	1.928***	3.417***
	(0.245)	(0.268)	(0.967)
Observations	161	161	161

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

The finding in support of Hypothesis 2B is also significant at the 0.05 level, and the observed difference, though slightly smaller, is more remarkable with regard to friendships than with regard to family-relationships. Among diaspora-owned firms, 49% report that friendships are more than "somewhat important" to profitability, while 40% of non-diaspora-owned firms report this.

While non-diaspora firm owners cannot create family relationships in the homeland, they can form friendships, and one might expect that the type of non-diaspora owners who choose to invest in a given developing country are those with the densest social networks in that country. The findings in Table 4.7 suggest that, even if this type of selection occurs, diaspora-owned firms still rely more heavily on their owners' and managers' friendships than do non-diaspora-owned firms.

Results: Testing Hypotheses 3A and 3B

In testing Hypotheses 3A and 3B, I turn to survey questions about whether firms use their owners' or managers' friends and family members to assist them in renting or purchasing real estate, and the type of assistance they report receiving. Respondents are asked if they have ever rented or purchased real estate with the help of a friend of one of the firm's owners or managers, and are then asked the same question about family members. Firms in the real estate sector are excluded from these regressions.

Again, results support both hypotheses. We observe significant differences between diaspora-owned and non-diaspora-owned firms.

Table 4.8: The Use of Family Relationships to Buy or Purchase Real Estate

	(1)	(2)	(3)
Diaspora-Owned Firm	1.060***	0.957***	0.934**
	(0.344)	(0.339)	(0.373)
Respondent = Owner		0.397	0.652*
		(0.305)	(0.358)
Primary Sector			-0.389
			(0.821)
Secondary Sector			-0.121
			(0.562)
Tertiary Sector			0.0783
			(0.528)
100% Foreign Ownership			0.0617
			(0.355)
Employees (logged)			0.102
			(0.0973)
Greenfield			-0.255
			(0.354)
Location = Tbilisi			-0.719**
			(0.356)
HQ = Russia			0.546
			(0.540)
Constant	-1.394***	-1.486***	-1.270*
	(0.208)	(0.240)	(0.694)
Observations	151	151	151

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 4.8 shows that diaspora-owned firms are more likely to report having rented or purchased real estate with the help of a family member of one of the firm's owners or managers. Table 4.9 shows the results with regard to the help of a friend of one of the firms' owners or managers; the difference is stronger with regard to family

members, and only of borderline with regard to friendships. Both tables are produced via probit regressions.

Just 6% of non-diaspora-owned firms reported using owners' or managers' family relationships to purchase real estate, compared to 34% of diaspora-owned firms. With regard to friendships, the divide was 21% for non-diaspora-owned to 39% for diaspora-owned.

Table 4.9: The Use of Friendships to Buy or Purchase Real Estate

	(1)	(2)	(3)
Diaspora-Owned Firm	0.519*	0.443	0.471
	(0.287)	(0.294)	(0.309)
Respondent = Owner		0.306	0.418
		(0.265)	(0.289)
Primary Sector			0.723
			(0.538)
Secondary Sector			-0.163
			(0.406)
Tertiary Sector			0.0486
			(0.388)
100% Foreign Ownership			0.334
			(0.296)
Employees (logged)			-0.0905
			(0.0849)
Greenfield			0.271
			(0.308)
Location = Tbilisi			0.375
			(0.352)
HQ = Russia			0.424
			(0.500)
Constant	-0.773***	-0.841***	-1.449**
	(0.138)	(0.152)	(0.538)
Observations	151	151	151

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Discussion of Results

Taken together, the hypotheses above provide strong evidence for core theoretical expectations of the paper. They show that diaspora-owned firms make greater use of social networks than do their non-diaspora-owned counterparts and that respondents at diaspora-owned firms perceive these networks to be more important to

firm location decisions and profitability than respondents at non-diaspora-owned firms, despite the fact that most respondents in both categories are local, ethnic-Georgian firm managers.

The magnitude and consistency of these differences is particularly dramatic in light of the fact that all questions are asked about the friends and family members of firm managers as well as of firm owners. If friends and family members are important to foreign firms, it is striking that non-diaspora-owned firms do not make more aggressive use of the social networks of their managers to compensate for their owners' lack of family in the homeland. This suggests one of several things: that the social relationships of diaspora owners are particularly valuable; that the value of social ties is not easily transferred to non-diaspora-owned firms when they hire local managers; or that firm culture or strategy in diaspora-owned-firms is conducive to the use of social networks in a way that it is not in non-diaspora-owned firms. Developing tests that can distinguish between these possibilities represents a compelling avenue for future research.

How Firms Use Social Networks

The empirical analysis above establishes that owners and managers at diaspora-owned firms perceive social networks to be more important to firm profitability and entry decisions than do their counterparts at other foreign firms, and that diaspora-owned firms use social networks more often, at least in buying and renting real estate. It is also useful, however, to understand in more detail exactly how diaspora-owned firms are using social networks.

To explore this we evaluate the open-ended responses of firm managers to the following question: "Can you briefly describe the effects that your owners' and managers' friendships and/or family relationships have on the way your firm does business?" These open-ended responses were recorded by the enumerator in the language in which the response was given, and then translated later if necessary.

Mentions of trust are coded in two ways. First, as a binary measure of whether the respondent indicates that the trust generated by social relationships is useful to his firm in some way. Second, among those respondents who indicate that social networks generate trust that is useful to the firm, we code whether the use of trust mentioned is inter-firm, intra-firm, or non-specific. A similar approach is taken to coding *enforcement* and *information.* Enforcement is coded one if the respondent mentions using social networks for any form of dispute resolution, contract enforcement, or sanctioning of opportunistic behavior. Information is coded one if respondents mention using social networks for gathering information, soliciting advice, evaluating risk, or identifying customers, suppliers, or clients, or to make "contacts" more generally.

Non-responses and responses such as, "No," or, "These relationships are not important to our business," are coded as missing for these purposes. Answers where some use of social relationships was mentioned, even vague generalities such as "Friendship plays a positive role in the conduct of business," but in which the concept of trust is not explicitly mentioned, are coded zero.

Consistent with the results from the closed-ended questions, 70% of respondents at diaspora-owned firms described at least some positive use of social relationships by the firm, while only 42% of non-diaspora-owned firms offered such a description. It is also noteworthy that, despite a phrasing of the question that allowed respondents to discuss any negative effects that social networks have on the function of their business, no such negative effects were mentioned.

Table 4.10: Ways Respondents Report That Their Firms Use Social Networks

	Diaspora-owned firms	Non-diaspora-owned firms	All Respondents
Trust (Any claim by respondent that social networks generate trust that is useful to their firm)	8 (38%)	19 (37%)	28 (39%)
Specifically inter-firm trust	1 (5%)	5 (10%)	6 (8%)
Specifically intra-firm trust	2 (10%)	7 (14%)	9 (13%)
Information (i.e. Identifying potential business counterparts, and receiving advice or risk information)	7 (33%)	11 (22%)	20 (28%)
Enforcement (i.e. Dispute resolution, contract enforcement, or deterrence of opportunistic behavior)	**0 (0%)**	**0 (0%)**	**0 (0%)**
Number of respondents who provided at least some description of their firm's use of social networks.	21	51	73
Note: The numbers in "all respondents" exceeds the sum of diaspora-owned and non-diaspora-owned firms because the diaspora/non-diaspora identity of one responding firm is not known.			

Table 4.10 depicts broadly similar patterns of social network use by diaspora-owned and non-diaspora-owned firms. It seems that differences between these two

groups of firms are more in the percentage of firms that use social networks actively in their business strategy, rather than in the types of uses these networks are put to.

Strikingly, we see that there are zero cases where anything related to contract enforcement or dispute resolution is mentioned, despite the variety of other uses respondents choose to list. Of 87 responses indicating at least some use of social networks in business, none mention anything related to these areas. If firms are using social networks to substitute for formal contracting institutions, this use should be important enough and salient enough to be mentioned by at least some firms; it is mentioned by none. There is a possibility that social desirability bias or related factors lead firms that use networks in this way not to mention doing so, and further empirical examination with new data is necessary.[25]

Thirteen firms mentioned using social networks to identify clients, customers, suppliers, or other contacts; seven made reference to more generic uses of social networks to gather information. This establishes that at least some firms use social networks to gather information, a finding consistent with Hypothesis 3B, but most remarkable in contrast to absence of any references to *enforcement*.

At the high end of the spectrum, twenty-eight respondents explicitly mentioned the usefulness of social networks in establishing trust. Respondents mentioned primarily that they prefer to do business with people or firms they know and trust, but in a few cases they were more detailed about the benefits trust conveys, such as stating

[25] More direct questions about firms' use of courts and relationship with formal political institutions were stripped from the survey during the pilot stage because they made respondents uncomfortable. In the Post-Soviet context, questions about business-government relations are generally interpreted as questions about corruption. I expect these questions to be less sensitive in the Philippines, where the next iteration of this survey will be conducted.

that the trust generated by social ties allows them to extend credit to customers they would not otherwise extend credit to.

Conclusions

Diaspora populations residing in rich countries have been identified as an important cause of FDI flows into developing countries. However, the existing literature lacks the firm-level data necessary to identify the causal mechanisms behind these flows. This paper introduces a theory of social-network based diaspora advantage, and uses a new firm-level survey from the developing country of Georgia to directly compare the behavior of diaspora-owned firms and other foreign firms operating in the same environment. I show that diaspora-owned firms use social networks more than non-diaspora-owned firms and that the managers and owners of diaspora-owned firms view social networks as more important to both their firm's profitability and investment-location decisions than do their counterparts at non-diaspora-owned firms.

These findings offer the first direct empirical comparison between the behavior of diaspora-owned and non-diaspora-owned firms. The differences in behavior I demonstrate are consistent with the social-network-based theory of diaspora difference that I present: that diasporans have access to social networks in the homeland and that these networks provide important competitive advantages to diaspora-owned firms.

Future iterations of this survey are planned in India, the Philippines, and Vietnam that will provide the opportunity to both establish that the findings presented here are not unique to the Georgian case, and to develop stronger tests to parse

between the different mechanisms through which diaspora-owned firms use social networks to increase profitability. Of particular interest is the degree to which diasporans' social networks can substitute for courts and other poorly functioning formal political institutions.

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Appendix

Diaspora-Owned and Non-Diaspora-Owned Firms by Home Country

Table 4.3 in the text shows the percentage of non-responses and diaspora ownership by region. Table 4.11 shows the number of responses, number of refusals, and number of diaspora-owned and non-diaspora-owned respondents for each investing country. Firms from Azerbaijan, France, Israel, the Netherlands, and the United States were particularly likely to refuse to participate in the survey, but there is no obvious common characteristic across these countries that appears to drive non-response.

Table 4.11: Home Country by Response Type				
	Refused	Responded		Total
Country		Diaspora	Non-Diaspora	
Armenia	2	0	2	4
Australia	0	0	1	1
Austria	6	1	3	10
Azerbaijan	10	0	2	12
Belgium	3	0	1	4
Bulgaria	1	0	1	2
Canada	0	0	1	1
Cayman Islands	1	1	0	2
China	0	0	3	3
Cyprus	1	0	4	5
Czech Republic	3	0	2	5
Estonia	0	1	1	2
France	5	0	2	7
Germany	10	1	9	20
Greece	5	0	0	5
Iran	2	1	3	6
Israel	9	2	3	14
Italy	4	1	1	6
Japan	1	0	0	1
Jordan	1	0	0	1
Kazakhstan	2	0	2	4
Latvia	3	0	3	6
Lithuania	2	0	1	3
Luxemburg	0	0	2	2
Marshall Islands	2	0	1	3
Netherlands	9	2	4	15
Panama	2	0	1	3
Philippines	0	0	1	1
Poland	0	0	1	1
Russia	12	5	7	24
Sweden	0	0	1	1
Switzerland	6	0	3	9
Syria	1	0	0	1
Turkey	23	5	14	42
UAE	1	0	2	3
UK	19	4	19	42
USA	20	0	10	30
Ukraine	3	1	1	5
Virgin Islands	2	0	0	2
Unknown	26	6	10	42
Total	197	31	122	350

Evaluating the Supplemental Sample

Table 4.12 provides the results of difference-in-means tests comparing diaspora-owned firms in the random sample from those in the supplemental sample. The demographic differences between these groups of firms is minimal, as are the differences in the firms' assessments of the importance of social networks to firm profitability. With regard to the importance of social networks to firm entry decisions, and firms' use of social networks to procure real estate. The largest difference is with regard to the use of social networks to acquire real estate. However, even if the supplemental firms are dropped from the analysis, the substantive results presented in Tables 8 and 9 remain the same. Diaspora-owned firms are more likely to report having used family relationships to assist in renting or purchasing real estate (significant at the 0.05 and robust to the exclusion of supplemental firms), and slightly more likely to report having used friendships in the same way (not statistically significant).

Table 4.12: Comparing Diaspora-Owned Firms from the Random Sample and the Supplement

Variable	Random Sample Mean	Supplemental Sample Mean	Difference in means	Standard Error (of difference)	t-statistic
Respondent = Owner	0.43	0.43	0.006	0.22	0.02
Primary Sector	0.04	0.17	0.13	0.11	0.1
Secondary Sector	0.28	0.5	0.22	0.22	1.0
Tertiary Sector	0.52	0.17	0.35	0.22	1.6
Quaternary Sector	0.16	0.17	0.007	0.17	0.04
100% Foreign Ownership	0.58	0.71	0.14	0.21	0.64
HQ in West	0.33	0.67	0.33	0.30	1.0
Number of Employees	145	124	21	148	0.14
Tbilisi (location)	0.81	0.71	0.09	0.18	0.52
HQ in Russia	0.19	0.33	0.14	0.36	0.55
Real Estate Sector	0.08	0	0.08	0.11	0.70
Friendships (Importance)	5.1	4.6	0.5	0.9	0.6
Family (Importance)	3.4	3.6	0.2	1.0	0.2
Friendship (Real Estate)	0.29	0.71	0.42	.2	2.1**
Family (Real Estate)	0.27	0.57	0.29	0.2	1.5
Social Networks (Location)	0.43	0.71	0.28	0.22	1.3

Chapter 5:
Diaspora-Owned Firms and Social Responsibility

Abstract

A causal relationship between diaspora populations and bilateral foreign direct investment has been established empirically, but the question of which elements of diaspora difference are responsible for this relationship, and what this implies for development, remains unanswered. A growing literature in economic sociology and business suggests that diaspora investors are motivated by patriotism and other social and emotional factors, endowing them with unique potential as a force for international development. This literature argues that diaspora-owned firms are more socially responsible than other foreign firms, and engage in a range of economic-development-promoting behaviors when investing in the homeland: hiring more local labor, paying higher wages, and making more contributions to charity. I argue instead that diaspora-owned firms enjoy competitive advantages in the homeland based on access and attention to information. I test this theory at the firm level, using data from an original survey of 174 foreign-owned firms in the post-conflict country of Georgia. Across a range of self-reported behaviors and priorities, I find no evidence that diaspora-owned firms are more likely to engage in a specific set of socially responsible behaviors than are other foreign firms, and some evidence that they are less likely to do so. I argue that diaspora investors are uniquely capable, but not uniquely philanthropic, when doing business in their homelands.

Introduction

Diaspora direct investment, i.e. foreign direct investment (FDI) in the homeland by migrants and their descendents, is a major source of capital in many developing countries. While direct measures are not available, scholars estimate that diaspora direct investment accounted for over 50% of FDI inflows to China during the 1990s (Huang 2003; Ye 2010) and 20-30% of FDI flows into India during the same period (Ye 2010). The survey discussed below finds that 17% of foreign-owned firms in Georgia, including the largest firm in the sample, have at least one diaspora owner.

The policy community in the United States has shown optimism regarding diaspora direct investment as a tool of international economic development, recently launching a number of programs targeting diaspora direct investors.[1] The optimism regarding the economic development potential of diaspora direct investment is based not just on its volume, but also on its alleged unique pro-development characteristics. Policymakers believe that diaspora-owned firms act with greater social responsibility than other foreign firms, and that their presence produces unusually large pro-development spillovers (Foreign Service Institute 2010; Debass and Ardovino 2009.) Unfortunately, enthusiasm has run ahead of the empirical evidence. To date there is no firm-level research demonstrating that diaspora-owned firms actually behave differently than other foreign firms, and therefore no direct evidence that warrants the optimism on which policy is now based.

[1] The African Diaspora Marketplace project launched by USAID in 2009 is one example. See also Foreign Service Institute, 2010.

In political science, scholars generally analyze the effect of variations in host country institutions on investment flows, while making the tacit assumption that the foreign firms doing the investing are interchangeable. Diaspora-owned firms are compelling subjects of study because they very clearly do not interact with and respond to institutions in the host country (their homeland) in the same way as other foreign firms. Economic sociologists argue that diasporans are motivated to invest in their homeland by social and emotional factors, and that these motivational differences manifest themselves in firm behavior (Gillespie et al. 1999). Specifically, this literature contends that diaspora direct investors are less sensitive to the risks imposed by political violence (Gillespie, Sayre, and Riddle 2001) and that diaspora investors are more likely to hire local labor, pay higher wages, employ more environmentally friendly business practices, and generally strive harder to contribute to the economic development of the homeland (Nielsen and Riddle 2010). While these alleged behavioral differences are sometimes used to justify a policy focus on promoting diaspora investment (Foreign Service Institute 2010; Debass and Ardovino 2009), there is recognition in the academic literature that rigorous empirical evaluation of this potential is urgently needed (Nielsen and Riddle 2010, p. 442). I provide that here.

This work also contributes to a literature in political science and economics that has identified bilateral migrant stocks as a cause of FDI, but has not clearly identified a causal mechanism (Leblang 2010; Javorcik et al. 2011; Kugler and Rapoport 2007). By conducting firm-level analysis directly comparing the behavior of diaspora-owned firms and other foreign firms, this paper advances our understanding

of the elements of diaspora difference that account for diasporans' tendency to invest in their homelands.

This paper draws on an original survey of 174 foreign firms operating in the post-conflict country of Georgia. This survey provides the first data that allows for direct comparison between diaspora-owned and non-diaspora owned foreign firms and includes information regarding firms' propensity to engage in a set of socially responsible and pro-economic development behaviors. I find no evidence that diaspora owned firms make greater effort to promote homeland economic development than their non-diaspora-owned peers, and some evidence that they take less effort. This failure to find evidence of diasporans engaging in more pro-development behavior than their peers is all the more striking in light of other results from the same survey reported in concurrent work, which show large and statistically significant differences between diaspora and non-diaspora owned firms regarding their exploitation of social networks for competitive advantage (CITATION REDACTED). Diaspora-owned firms behave much differently than other foreign-owned firms but, contrary to current expectations in the literature, these differences are born of pecuniary interest, not benevolence.

This paper begins by surveying the existing literature on the development potential of diaspora investment, including a summary of existing theories of diaspora difference based on motivation. I then derive a series of testable hypotheses regarding the social responsibility and pro-development behavior of diaspora-owned firms and

test these hypotheses empirically. The paper concludes with a discussion of the theoretical and policy implications of these results.

Diasporas and Development

Well into the 1990s, the dominant view in the literature on migration and development was that emigration was a symptom of under-development and economic insecurity[2] and the literature emphasized the negative effects of brain drain on the level of human capital in migrant-sending countries (e.g. Bhagwati and Hamada 1974; Miyagiwa 1991). While several region- or country-specific studies during this period pointed to specific cases in which labor emigration contributed to development, these findings received little attention.[3]

The positive effects of emigration (and emigrants) on the homeland economy gained more notice with later work that demonstrated the effect of migrant networks in promoting trade between migrants' homelands and their countries of residence (e.g. Rauch and Trindade 2003). Beginning in the early 2000s, scholars also awoke to the large scale of remittances, or money sent by migrants back to their families in the homeland (e.g. Orozco 2002; Maimbo and Ratha 2005; Giuliano and Ruiz-Arranz 2009).[4] Since then, the literature on migrants' contributions to homeland development has broadened rapidly to include diasporans' role in promoting the transfer of knowledge from countries of residence back to the homeland (e.g. Saxenian 2006;

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[2] See de Haan (1999) and Faist (2008) for reviews of this literature. Bakewell (2009) also documents a strong anti-migration bias among Western governments and development NGOs.
[3] For example, Chirwa (1997) had noted the positive effects of labor emigration on rural sending communities in Malawi, and the negative effects of the forced repatriation of those migrants.
[4] Some excellent scholarly work on the development impacts of remittances had preceded this, such as Oberai and Singh (1980), but it was not until after the World Bank began to collect data on the volume of these flows globally that academic interest in the topic took off.

Kapur 2010; Mullings 2011), the role of emigration opportunities in inducing investment in education,[5] diaspora philanthropy (Brinkerhoff 2008); diaspora direct investment (Weidenbaum and Hughes 1996; Riddle, Hrivnak, and Nielsen 2010); and diaspora venture capital (Vaaler 2011; Pandya and Leblang 2012).

This paper focuses on the channel of direct investment in the homeland as a means through which diasporans contribute to economic development in the homeland.

Diaspora Investment as a Tool for Economic Development

Developing countries generally, and states with high levels of political risk in particular, often struggle to attract foreign direct investment. Diaspora investors are heralded in the recent literature as unique among foreign investors in both their willingness to tolerate political risk and their propensity to engage in socially responsible and pro-development behavior when in investing in the homeland (e.g. Brinkerhoff 2009). Diasporans are alleged to possess non-pecuniary motivations for investment in the homeland (e.g. Gillespie, Sayre, and Riddle 2001; Riddle and Nielsen 2010), and these motivations are expected to drive attention to social objectives and homeland economic development by diaspora-owned firms.

Cross-national research has established a causal link between diaspora populations and flows of foreign direct investment from diasporans' countries of residence to their homelands (Javorcik et al. 2011; Leblang 2010; Kugler and Rapoport 2007; Docquier and Lodigiani 2010; Graham 2010). This literature theorizes

[5] The work of Michael Clemens (2011) and others has also highlighted the positive effects of emigration on migrants themselves, pointing out that a singular focus on the effects of emigration on the sending economy ignores the benefits to the very agents creating the phenomenon.

that the propensity of diasporans to invest in their homelands (or otherwise channel FDI from their country of residence to their homeland) is based on competitive advantage. Migrants have familiarity with the culture and easier access to information in the homeland (e.g. Javorcik et al. 2011; Leblang 2010), they may enjoy higher levels of trust with co-ethnics, (Docquier and Lodigiani 2010; Rauch 2003), and firms in the countries where migrants reside may glean information from them about their homeland (Kugler and Rapoport 2007). In concurrent work that draws on the same survey used in this paper, I demonstrate that diaspora-owned firms are more likely than other foreign firms to exploit social networks in the homeland to gain competitive advantage (Graham 2012).

A parallel, and not necessarily incompatible, literature on the motivations of diaspora investors has also developed in economic sociology and business. This literature perceives investment in the homeland by a member of the diaspora as not just an economic act, but as an emotional, social, and political act (Bandelj 2008). It argues that, in addition to the desire to earn a profit, diasporans may invest for social reasons, such as raising their social standing in the diaspora community in their country of residence, or for emotional reasons, such as patriotism (Aharoni 1960; Schulte 2008; Nielsen and Riddle 2010). Empirically, surveys of US-based diasporas have shown that diasporans' self-reported interest in homeland investment is greatest among those diasporans with the strongest emotional ties to the homeland, as well as the strongest social ties to their diaspora community in the United States (Gillespie et al. 1999; Gillespie, Sayre and Riddle 2001; Raveloharimisy et al. 2010).

These findings of non-pecuniary investment motivations have motivated further theorizing about the potential of diaspora investors as sources of social and economic development in the homeland. If diaspora investment in the homeland is motivated by the desire to increase social standing in the diaspora community and to engage positively with a homeland to which the investor has strong emotional ties, it is reasonable to expect that they will engage in behaviors consistent with these goals. Specifically, the economic sociology literature theorizes that diaspora-owned firms may rely more heavily on local labor and local inputs, pay above-market wages, and strive to provide a high quality of life for their employees, protect the local environment, and generally seek to promote development of the homeland (Nielsen and Riddle 2010). However, these assertions remain almost entirely theoretical. The evidence supporting these claims is anecdotal, including examples from places like Afghanistan where diasporans have undertaken large, socially responsible investment projects (Brinkerhoff 2004; Nyberg-Sørensen 2007; Riddle, Hrivnak, and Nielsen 2010). However, while the theoretical arguments in this literature are well developed, the need for rigorous empirical testing is acute and acknowledged in the theoretical literature itself (Nielsen and Riddle 2010, p. 442).

The rush to roll out diaspora investment promotion programs is not necessarily driven by a lack of understanding among policymakers that these theories have not yet been tested. The theories are difficult to test well, and if the theories are correct, the potential upsides of effective diaspora direct investment promotion are very large,

justifying the decision to proceed optimistically, at least until empirical evidence becomes available.

Socially responsible investment is particularly desirable in developing countries where the ability of the government to restrain rapacious firms and provide social insurance is low. While exposure to the global economy is associated with greater welfare spending in rich countries, the reverse is true in the developing world (Wibbles 2006; Rudra 2002). Developing countries often open their economies to foreign direct investment to spur development, but their very openness prevents large social programs that might expand the benefits of growth to larger segments of the population (Kaufman and Segura Ubiergo 2001; Reuveny and Li 2003). Against this backdrop of limited government welfare capabilities, social responsibility among foreign investors is all the more salient.

Defining Socially Responsible Behavior

Caroll (1979) divides firms' social responsibilities into four categories: economic, legal, ethical, and discretionary. In this paper, I focus empirically on behaviors that foreign firms take to maximize their contributions to economic development and social welfare in the host country. More specifically, I focus on the treatment of employees, contributions to charity, and self-perceived contributions to economic development in Georgia. These behaviors fall into Carroll's final category of discretionary behaviors, i.e. behaviors that contribute to the wellbeing of society, but which are not legally required and the omission of which is not considered inherently unethical. I do not suggest that these behaviors capture the full range of

means through which a firm can be "socially responsible."[6] However, expectations regarding these behaviors are critical in shaping investment promotion and development policy, and theory predicts they should be observed at elevated levels in firms with socially and emotionally motivated owners.

The argument that the motivations of diaspora owners might serve as causes of socially responsible firm behavior, such as paying above-market wages and contributing to charity, in their developing homelands is consistent with the general literature on the reasons firms adopt corporate social responsibility (CSR). This literature suggests that the personal motivations of top management are important causes of CSR (Hemingway and Maclagan 2004; Juholin 2004), that small, privately held firms are most likely to engage in CSR (Jones 1999), and that philanthropic, rather than strategic, CSR is particularly prevalent in the developing world (Jamali and Mirshak 2008).

Deriving Testable Hypotheses

Drawing on the theoretical work of Nielsen and Riddle (2010), I focus empirically on diaspora-owned firms' treatment of employees, contributions to charity, and self-perceived contributions to economic development in the homeland. These activities do not encompass the full range of ways in which non-pecuniary motivations for investment may manifest themselves in socially responsible behavior, but they represent some of the most likely manifestations. They also represent a core

[6] For broad reviews of the corporate social responsibility literature, see Garriga and Melé (2004) and Elms and Westermann-Behaylo (*forthcoming*).

set of behavioral expectations on which optimism about the pro-development potential of diaspora direct investment is based.

Fair or generous treatment of employees is both a core tenet of many definitions of social responsibility (e.g. Hemphill 1997) and one that may be particularly relevant for diaspora-owned firms in developing countries. Developing countries, particularly those with weak formal institutions of economic governance, are often characterized by a high proportion of family firms (Bertrand and Schoar 2006). In this context, one of the normal ways through which firm owners contribute to the wellbeing of their family and community is through the provision of quality employment. Providing good jobs to relatives, friends, and members of the community whose respect they seek is therefore, among the most theoretically likely means through which emotionally, and especially socially, motivated diasporans may act out those motivations.[7]

Treatment of employees, however, captures but a narrow subset of the ways in which diaspora-owned firms may engage in socially responsible behavior. The two other empirical areas I examine are much broader in the types of behavior and objectives they encompass. I also examine whether diaspora-owned firms are more likely than other foreign firms to contribute to charity or to self-report that they make above-average contributions to economic development in the homeland. The self-report of contributions to economic development avoids limiting the means through which diaspora-owned firms might seek to contribute to economic development in the

[7] This would be consistent with the literature on family firms and CSR (e.g. Uhlaner, van Goor-balk, and Masurel (2004).

homeland, capturing any perceived success toward that end. Broader still, I examine whether diaspora-owned firms contribute to any charitable organizations. The goals of such organizations are left entirely open, and may focus on economic development or other non-economic objectives that diasporans may have. For almost any goals a diaspora investor might have outside of a high return on investment, firm donations to charitable organizations represent a logical means (though of course not the only means) through which those ends might be achieved.

I begin by specifying two general hypotheses based on existing theory, and then I proceed to derive a subset of specific and directly testable hypotheses.

Hypothesis 1: Diasporans' social and emotional motivations for investment lead diaspora-owned firms to engage in more socially responsible labor practices.

Hypothesis 2: Diasporans' social and emotional motivations for investment lead diaspora-owned firms to make greater effort to contribute to economic development in the homeland and to be more likely to make charitable contributions.

Socially Responsible Labor Practices

One of the primary means by which foreign investment benefits the host country is the employment of locals, which involves labor training and the transfer of

knowledge (e.g., de Mello 1999). Hiring local labor also reduces unemployment and injects wages directly into the local economy. If diaspora investors are motivated to promote development in the homeland, this is perhaps the most obvious channel through which they might do so. Particularly important in this mechanism is human capital development, so one would expect diaspora-owned firms to both prioritize the hiring of local labor and to provide their employees with opportunities for professional development. One would also expect that socially responsible firms would pay above-market wages and provide the means for their employees to provide a high quality of life for their families, and that they would pursue these ends even if it increased their total labor costs.

Hypothesis 1A: Diaspora-owned firms are more likely than non-diaspora-owned firms to report that their firm prioritizes the hiring of local labor over foreign labor.

Hypothesis 1B: Diaspora-owned firms are more likely than non-diaspora-owned foreign firms to report that their firm offers higher salaries than other firms in their sector.

Hypothesis 1C: Diaspora-owned firms are more likely than non-diaspora-owned foreign firms to report that employees at their firm are able to provide a higher quality of life for their families than are employees at other firms in their sector.

Hypothesis 1D: Diaspora-owned firms are more likely than non-diaspora-owned foreign firms to report that, compared to other firms in their sector, employees at their firm have many opportunities for professional development.

Hypothesis 1E: Diaspora-owned firms are **less** likely than other foreign firms to report that their firm keeps their total labor costs as low as possible.

Contributions to Development and Charitable Contributions

Firms have a variety of mechanisms for contributing to development in their homeland, many of which fall outside the area of employment and may not be captured by questions about specific firm behaviors. Therefore, I also assess some more general questions about how firms balance the sometimes competing priorities of profit maximization and development contribution.

While a firm's contribution to development is distinct from its effort to make such a contribution, both are interesting. From a policy perspective, it is the actual impact that is of greatest interest. If diaspora-owned firms do not contribute more to the development of the homeland than other foreign firms, it undermines key arguments currently made for the targeted recruitment of diaspora investors into developing countries.[8]

[8] For examples of these arguments, see Debass and Ardovino (2009) and Terrazas (2010).

Hypothesis 2A: Diaspora-owned firms are **more** likely than non-diaspora-owned foreign firms to report that their firm contributes to economic development in their homeland more than other firms in their sector.

I also assess whether firms report having contributed to charity in the past year. I acknowledge that firms may be able to reap marketing benefits from some types of charitable contribution, and large firms might be able to reap sufficient benefits from social improvements over the long run. For example, a charitable contribution to education may produce a better educated workforce that, over the extremely long run, may benefit the firm (Porter and Kramer 2002). However, charitable contributions mark a firm's most direct contributions to non-pecuniary ends, at the most obvious cost to short-term profitability. As noted previously, charitable contributions also represent a likely means toward a wide variety of objectives that diaspora investors might have, including those far removed from economic development.

Hypothesis 2B: Diaspora-owned firms are **more** likely than non-diaspora-owned foreign firms to report having made contributions to charity within the last year.

The testable implications of Hypotheses 1 and 2 encompass a range of ways in which diaspora-owned firms may manifest a commitment to social responsibility and development of the homeland. Some of these may serve as substitutes for one another, but if the underlying theory is correct, and diasporans' social and emotional

motivations lead diaspora-owned firms to engage in more socially responsible and pro-development behavior, I should find evidence in support of at least some of these hypotheses. If I cannot find evidence of the expected differences between diaspora-owned and non-diaspora-owned firms across this wide array of behaviors and priorities, it is reasonable to conclude that diaspora-owned firms do not actually behave in a more socially responsible manner than do other foreign firms.

In the following sections I discuss the structure of the survey used to test the hypotheses outlined above.

Why Georgia?

In the language of small-N case selection, Georgia is a "typical case" with regard to its economic characteristics, and an "extreme case" regarding the likelihood of observing socially responsible behavior by diaspora investors.[9] For reasons outlined below, Georgia represents precisely the type of country in which I would expect the social and emotional motivations of diaspora investors to be salient, and in which I would expect to see diasporans using investment as a means to foster development in the homeland. If the predictions made by theories of diaspora difference based on motivations are not borne out in the Georgia case, this would contradict the general theory from which these predictions are drawn.

Georgia is a small country surrounded by volatile and sometimes hostile neighbors. It has only modest endowments of natural resources but is located on a major transport corridor for both energy (oil and gas) and goods between the

[9] See Seawright and Gerring (2008) for a discussion of the use of "extreme cases" and "typical cases" in hypothesis testing.

Black Sea and Central Asia. The amount of arable land is not vast, but the climate allows cultivation of some high value crops, including citrus and nuts. While Georgia cannot become wealthy on the strength of its natural endowments alone, with political stability and competent institutions it could certainly become wealthy.

Following Mikheil Saakashvili's ascent to power in the "Rose Revolution" of 2003, Georgia embarked on a series of free-market reforms aimed at increasing foreign investment and integrating Georgia with the West both economically and politically. These reforms have born substantial fruit: Georgia rocketed up through the World Bank's Ease of Doing Business rankings and FDI surged from $335 million in 2003 to $1.7 billion in 2007. Georgia, however, has remained a high-risk destination for foreign investment. Large protests in November 2007 underscored the fragility and unpredictability of the domestic political situation; corruption, though improved, remains substantial; Transparency International has accused the government of practicing "tax terrorism" (Transparency International 2010); and a brief war between Russia and Georgia in August 2008 highlighted the international instability generated by Georgia's secessionist regions, which remain outside the government's control.

In addition to attracting substantial FDI, Georgia has a large diaspora population relative to its size, making it an excellent venue for studying diaspora

direct investment.[10] The other relevant idiosyncrasy is the aforementioned 2008 war between Georgia and Russia over the territory of South Ossetia. Two regions of Georgia, Abkhazia and South Ossetia, fought wars of secession against the Georgian government in the early 1990s and secured *de facto* territorial control. Fighting broke out between South Ossetia and the Georgian government in 2008, with Russian troops doing much of the fighting on behalf of the Ossetians. The war was brief, limited of scope, and minor in terms of its economic impacts, but it may have affected the relationship between segments of the Georgian diaspora and the Georgian government.

Nielsen and Riddle (2010) theorize that non-pecuniary motivations may be more salient to diasporans whose homeland is a post-conflict state. However, in cases of civil conflict, the role of the diaspora is not always to promote peace and economic development.[11] The Abkhaz and Ossetian diasporas (primarily residing in Russia) are very likely to oppose the Georgian government. Most of the Georgian diaspora, on the other hand, is comprised of ethnic Georgians, and enjoys positive relationships with the government.[12] Reflecting these good relations, the Georgian government established the Office of the State Minister of Diaspora in 2008 and has made substantial efforts to attract diaspora investment. For logistical reasons, this survey excluded firms in the contested regions of Abkhazia and South Ossetia, and all of the

[10] Because of a paucity of cross-national data on diaspora investment, it is difficult to assess the volume of diaspora investment in a given country *ex ante.*

[11] Collier and Hoeffler (2004) find that diaspora populations in rich countries increase the risk of civil conflict, presumably because they represent a potential source of rebel financing.

[12] This relationship is most likely to be strained in the case of firms based in Russia. Neither dropping Russian firms from the analysis nor including a dummy variable for Russian origin changes the substantive results.

diaspora investors covered in the survey self-reported their ethnic identity as Georgian. Given these facts, existing theory suggests the 2008 war should have increased the relevance of non-pecuniary motivations for the diaspora investors captured in the survey.

This makes Georgia an apparently easy case in which to demonstrate diasporans' social responsibility: if diaspora-owned firms act with greater social responsibility than other foreign firms, Georgia is exactly the type of case in which such a difference should manifest itself. A failure to find such evidence in Georgia would contradict the general theory.

Table 5.1:
Georgia in Comparison to Other Fragile and Developing Countries

	Georgia	Fragile states median (25^{th} -75^{th} percentile)	Developing States Median (25^{th} - 75^{th} percentile)
GDP (millions of USD)	5,500	12,300 (5,500 – 55,000)	6,500 (1,700 – 24,000)
GDP per capita (USD)	1,300	1,200 (280 – 2,200)	1,100 (380 – 2,000)
Population (millions)	4.3	20 (7.3 – 41)	9.7 (3.1 – 27)
FDI Inflows (Millions of USD)	1,600	790 (120 – 2,600)	748 (120 – 2,200)
FDI Inflows (% of GDP)	12	3.6 (1.9 – 6.1)	4.3 (1.9 – 7.9)
Remittances (Millions of USD)	73	73 (15 – 270)	73 (8.1 – 290)
Remittances (% of GDP)	5.7	2.0 (0.7 – 5.7)	4.3 (0.4 – 5.6)
Emigrant Stock (1990)	48,000	110,000 (24,000 – 300,000)	88,000 (23,000 – 330,000)
Emigrant Stock (2005)	190,000	216,863 (82,000 – 640,000)	168,220 (36,000 – 490,000)
Emigrant Stock (% of population) (2005)	4.3	1.9 (0.7 – 3.0)	2.0 (0.9 – 4.4)
Cost of Dispute Resolution (% of claim)	35	30 (23 – 44)	33 (23 – 45)
War Risk (1-7) (Higher = Riskier)	5	5 (3 – 6)	3 (2 – 4)
Government Risk (1-7) (Higher = Riskier)	4	4 (4 – 5)	4 (3 – 5)
Transfer risk (1-7) Higher = Riskier	6	6 (4 – 7)	5 (4 – 6)

Fragile countries n = 31; Developing countries n = 107

The range from 25^{th} percentile to 75^{th} percentile is given in parentheses.
All items are measured as of 2008, except for the measures of migrant stocks, whose years are given.

I define developing countries as those with a 2008 GDP per capita of less than $3,855 – those countries defined by the World Bank as low income and lower middle income.

Fragile states are those that meet any of the following criteria: 1. A violent intra-state conflict in the last 3 years; 2). A change of executive leadership via coup or military overthrow in the last three years; 3) failure to control all their territory 4) occupation by a foreign power or not controlled by a unified entity (i.e. coded as "Interregnum" or "Interruption" in the Polity IV data).

Data on GDP, GDP per capita, Population FDI inflows, migrant stocks, and remittances are taken from the World Development Indicators. Data on war risk, government, and transfer risk are taken from *Office National Du Ducroire*

As noted above, Georgia is a "typical case" with regard to its economic characteristics. For 2008-2010, Georgia's risk profile matched the risk profile of the median fragile state in all three categories of political risk: government risk, which is the risk of expropriation and adverse government action; transfer risk, which is the risk of capital controls or other regulations that might prevent the repatriation of capital by an investing firm; and war risk. This profile also places it near the developing states median in these categories, except for war risk, which is particularly high in Georgia.

Across a range of other variables, such as wealth, total FDI inflows, and judicial efficiency, Georgia is within one standard deviation of the median value for both fragile states and developing states. The exceptions to this derive from the fact that Georgia is quite small relative to the mean in either category of state, but has a large diaspora and attracts a large amount of FDI relative to its size.

The Survey

The *Capital and Conflict: Georgia* survey is the first firm-level survey that compares diaspora-owned firms to other foreign firms. Surveys were conducted, in person, with the owner or manager of 174 foreign owned firms between February and June 2010. Each respondent was given a choice of taking the survey in English or Georgian.[13]

[13] The survey was taken through three rounds of reverse translation and refinement to ensure equivalence between the English and Georgian versions.

The sampling frame was derived from a list of foreign-owned firms provided by the Georgian Ministry of Finance. The list included all foreign firms that met the following criteria: 1) a for-profit enterprise; 2) at least 10% foreign ownership; 3) registered as active and paying taxes as of June 1, 2009; 4) obtained its first registration in Georgia after the year 2000. This sample was supplemented with a randomly drawn sample of 300 of the 450 firms that responded to the Ministry of Finance's Balance of Payments survey in 2009. These firms also met criteria 1-3, but some were initially registered prior to 2000.[14]

Each of the testable hypotheses listed above are tested using a specific question on the survey. All questions, except that used to test Hypothesis 2B, elicit answers on a seven point Likert scale from "strongly agree" (7) to "strongly disagree" (1).

Table 5.2: Survey Questions

Hypothesis	Question
	Please indicate how well each statement describes your company's Georgian operations:
H1A	This firm gives priority to hiring local staff over foreign staff.
H1B	This firm offers higher salaries than other firms in this sector.
H1C	Compared to employees at other firms in this sector, employees at this firm are able to provide a higher quality of life for their families.
H1D	Compared to employees at other firms in this sector, employees at this firm have many opportunities for professional development.
H1E	This firm keeps its total labor costs as low as possible.
H2A	This firm contributes to economic development in Georgia more than other firms in this sector.
	As a yes or no question:
H2B	Does your firm make any donations to charitable causes?

[14] Some firms on the Ministry of Finance list also turned out to be registered before the year 2000, but had been re-registered after that date.

Non-Response

Attempts were made to contact a total of 1,024 firms between February and June 2010, representing over 80% of the foreign-owned firms that officially entered Georgia during the target period. Only 484 could be contacted and, of those, only 362 met the criteria listed above (foreign ownership, for- profit, and currently operating).[15] Many of the firms that could not be contacted were closed – some had closed recently and others had closed before 2009 but had not been purged from the tax rolls. Other firms could not be contacted because they had changed addresses and phone numbers since the lists were updated. Georgia has no up-to-date telephone or address directory, and so only a minority of the firms that have relocated within the last several years could be contacted. Few of the foreign firms operating in Georgia maintain websites.

Of the 362 firms that were successfully contacted and that met the basic criteria for inclusion in the sample, surveys were successfully conducted with 167; 195 firms refused to participate. In most firms, enumerators were successful in scheduling interviews with either the firm owner or a firm manager. In some cases, however, this was not possible and a shorter version of the survey (10 of 53 questions) was asked of front-desk or other available personnel. These "front desk" surveys asked questions about firm demographics, such as sector, headquarters country, firm size, and diaspora ownership. The front desk surveys do not include sufficient information to include these firms in most hypothesis tests, but do provide valuable additional information about firm demographics in the sampling frame.

[15] We only attempted to contact firms that the Ministry of Finance or State Department of Statistics indicated met the three criteria for inclusion, but nonetheless many firms on the lists provided were either non-profit, fully Georgian-owned, or were closed.

The sampling lists discussed above produced 167 respondents: 26 diaspora-owned firms and 129 non-diaspora-owned firms[16]; twelve respondents did not answer the question on diaspora ownership.[17] Because of the low number of diaspora-owned firms, a non-random supplement of seven additional diaspora-owned firms was added to the sample, bringing the total number to 174.[18]

Table 5.3: Number of Respondents by Diaspora Ownership

Survey Type	Diaspora-owned firms (random only)	Diaspora-owned firms (supplement included)	Non-diaspora-owned firms	Diaspora ownership unknown	Total
Owners/Manager's Survey	23	30	121	10	161
Front Desk Survey	3	3	8	2	13
Total	26	33	129	12	174

The only demographic information available from firms that refused to participate in the survey is the firm's home country, which in most cases could be drawn from the firm lists provided by the Georgian government.[19] Therefore, I examine the number of respondents and non-respondents from each region. The following table shows that refusals are distributed evenly across regions. The percentage of diaspora-owned firms is smaller in the West than in other regions, but

[16] I treat any firm with at least one diaspora owner as "diaspora-owned." However, all the empirical results that follow are robust to treating only majority-diaspora-owned firms as diaspora-owned.
[17] All firms in the sample are privately owned, i.e. none are government-owned or publicly traded.
[18] Two of these supplemental firms were identified by the State Office for the Diaspora; five more were located via snowball sampling. To create the snowball supplement, enumerators asked respondents at the end of the interviews if they knew of any diaspora-owned firms that we might be able to contact.
[19] Not all firms have home countries listed on the original sampling lists. For those firms that responded to the survey but did not have a home country listed, I treat the location of the firm headquarters as the home country. Some respondents did not report a headquarters location outside of Georgia – the home country of those firms remains as missing data.

because this is the largest region in terms of number of firms, the number of diaspora firms from the West is still substantial.

Table 5.4: Distribution of Diaspora Ownership by Home Region

Region	Number of Qualifying Firms Contacted	Number of Responses	Percentage Non-Response	Percentage of respondents who are diaspora-owned
Former Soviet Union	69	31	55%	23%
Western Europe, US & Australia	162	73	55%	13%
Middle East	68	31	54%	27%
Other	14	7	50%	14%
Region Unknown	56	24	57%	33%
Total	**369**	**174**	**56%**	**20%**

Within the set of firms that were successfully interviewed, I compare diaspora and non-diaspora-owned firms across a range of covariates. By establishing that firm demographics between the two groups are similar, I am then better able to attribute any observed differences in firm behavior to diaspora ownership itself, rather than alternative factors like firm size or sector. Firm size is captured by the number of employees: I compare diaspora and non-diaspora firms both in terms of average number of employees and the likelihood of being a small firm, i.e. a firm with 50 employees or less.[20] I use dummy variables for minority, majority, and 100% foreign ownership, and a dummy variable for Greenfield investment. I also include dummy variables for primary, secondary, tertiary, and quaternary sectors.

[20] Seventy percent of firms in the sample have 50 employees or less. The median number of employees is 12.

With regard to the demographic characteristics of individual respondents, I examine whether the respondent was an owner of the firm (as opposed to a manager) and whether the respondent self-identified as ethnically Georgian. I also examine the language in which the survey was conducted.

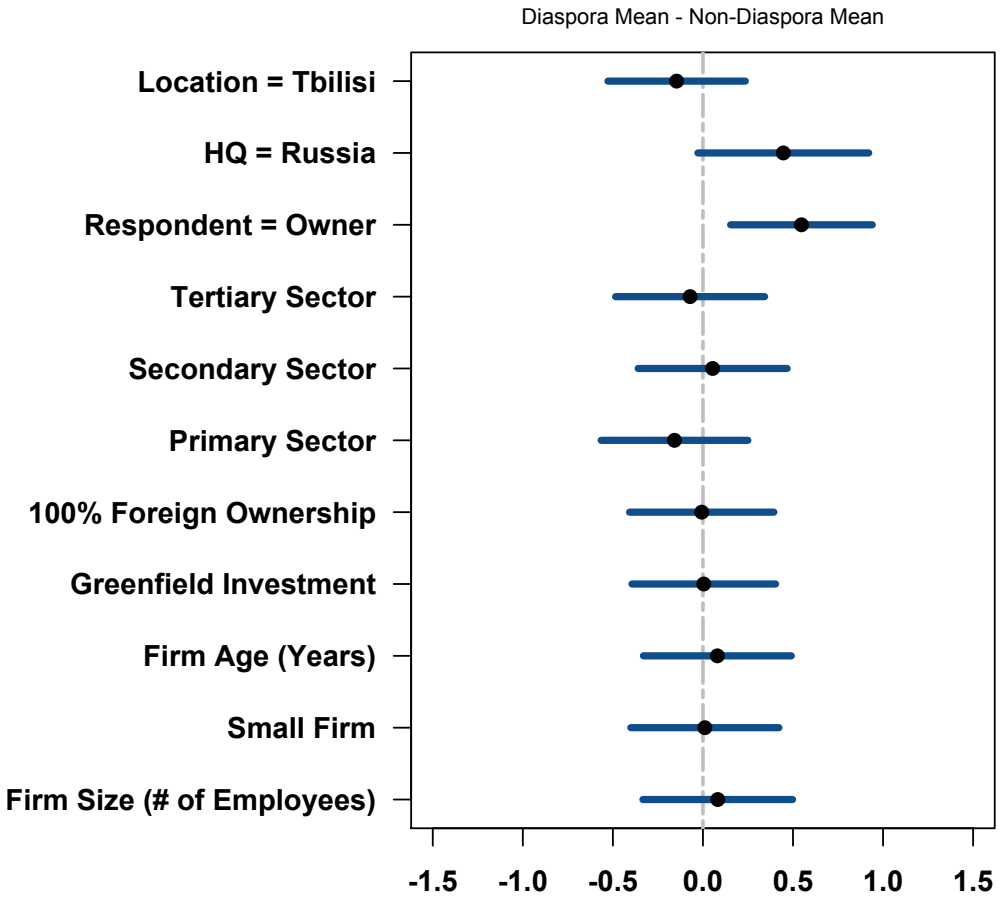

Figure 5.1: Demographic Comparison

*Note: Units = standard deviations, horizontal lines give 95% confidence intervals.

The only demographic characteristic for which I observe a large difference between diaspora and non-diaspora-owned firms is in the probability that the respondent is the firm owner. At each firm, enumerators asked for a meeting with the firm owner or manager. At diaspora-owned firms, the firm owner was more likely to be available for interview. This makes sense if diaspora owners are more likely than other foreign owners to live in Georgia at least part time while running their business. All of the respondents at diaspora-owned firms self-identified as ethnically Georgian (i.e. both owners and managers), as did 89% of respondents at non-diaspora-owned firms (i.e. almost all managers but no owners).

Firm owners may have somewhat different perspectives on the behavior of their firm than do managers; this necessitates a multiple-regression framework controlling for the identity (owner vs. manager) of the respondent. Other firm demographic characteristics are included as supplemental controls, but do not substantively affect results.

The range in firm size in the sample is substantial, with firms varying from a single employee to as many as 1400. However, as Figure 1 demonstrates, the difference in firm size between diaspora-owned and non-diaspora-owned firms is not large. Three-quarters of firms in both groups qualify as “small” enterprises: 75% of diaspora-owned firms and 77% of non-diaspora-owned firms have 50 employees or

less. Similar consistency is observed in the distribution of firms between sectors,[21] the distribution of firms between regions (not pictured), the share of foreign ownership, and firm age.[22]

Summary Statistics and Multiple Imputation

In dealing with missing values that arise from non-response, I employ the multiple imputation by chained equations technique developed by Patrick Royston (2004, 2009).[23] Multiple imputation reduces the bias and eliminates the sample-size reduction associated with list-wise deletion, allowing full use of the information collected in the survey (King et al. 2001).[24] In the raw data, the independent variable of interest, diaspora-ownership, is 6% missing, firm demographic characteristics used as controls are between 2% and 11% missing, and the dependent variables range from 9% to 30% missing.[25]

[21] I also checked the balance in a composite of real estate and construction – the balance here is quite even. I checked because these sectors were particularly hard-hit during the downturn.
[22] It is worth noting that very few firms in the sample entered after the 2008 conflict with Russia: of the 18 that entered during this period, 5 were diaspora-owned and 12 non-diaspora-owned (1 unknown). Most of the firms in the sample entered Georgia as Greenfield investments, rather than mergers, partnerships, or acquisitions of Georgian firms: 76% of diaspora-owned and 78% of non-diaspora-owned firms.
[23] This is implemented using the ice and mim commands in Stata 10. I create 10 imputed datasets for analysis.
[24] The coefficients of interest are similar in analyses using list-wise deletion.
[25] These percentages refer only to the 161 firms that completed an owners/managers survey. Those firms where only a front-desk survey was completed are omitted from analysis (and imputation) because none of the social responsibility questions are included on the front desk survey.

Table 5.5: Summary Statistics

Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
Diaspora Ownership	151	0.199	0.4	0	1
Respondent = Owner	157	0.242	0.43	0	1
Primary Sector	156	0.0705	0.257	0	1
Secondary Sector	156	0.301	0.46	0	1
Tertiary Sector	156	0.487	0.501	0	1
Quaternary Sector	156	0.141	0.349	0	1
100% Foreign Ownership	157	0.599	0.492	0	1
HQ in West	132	0.53	0.501	0	1
Number of Employees	144	89.4	218	1	1400
Greenfield Investment	154	0.76	0.429	0	1
Prioritize Local Labor	139	6.3	1.08	3	7
Pay Higher Salaries	126	4.77	0	1	7
Higher Quality of Life	124	0	1.2	1	7
Professional Development	129	5.44	1.38	1	7
Contribute to Charity	145	0.586	0.494	0	1
Minimize Total Labor Costs	138	4.01	1.66	1	7
Contribute to Development	112	4.96	1.3	2	7
Focus on Profits	126	5.42	0	1	7
Focus on Efficiency	135	5.77	0	3	7

Differential Item Response

Many of the questions in this survey rely on respondents' subjective assessments. One of the central concerns in questions of this nature is differential item functioning, the problem that individuals may understand questions differently or that the scales of their answers may not match. Many of the responses reported here are subjective assessments comparing the behavior of a respondent's firm to that of "other firms in your sector." This peer comparison creates a self-anchoring scale and reduces

the problem of differential item functioning (King et al. 2004).[26] Self-anchoring scales are effective when respondents are referencing the same or similar anchor points – in this case, when they are comparing their firm to the same or a similar group of peer firms. Georgia is a small country, making it reasonable to assume that, within a given sector, firms are referencing the same or similar firms in anchoring the scale of their answers. A less restrictive, but still sufficient, assumption is that the referenced peer firms do not vary systematically between diaspora-owned and non-diaspora-owned firms.

The within-question comparison to peer firms does not address the possibility that some firms may use the Likert scale differently than others. This potential problem is explored by the inclusion of questions that use the Likert in opposing directions. For example, respondents are asked the degree to which they agree or disagree with the following statements: "This firm offers higher salaries than other firms in this sector," and six questions later, "This firm keeps its total labor costs as low as possible." There is evidence that the direction of the Likert scale does affect answers. Responses to the statement that the firm in question pays higher salaries than its peers and that the firm minimizes total labor costs, which use the Likert scale in opposite directions, are correlated at -0.2. Responses to the salaries statement and the statement "Compared to employees at other firms in this sector, employees at this firm are able to provide a higher quality of life for their families," which use the Likert scale in the same direction, are correlated at 0.75. These three questions are not exact

substitutes, but the correlations in responses to all three questions were expected to be high, and the weak negative correlation is surprising. However, across all three questions, diaspora owned firms are less likely than non-diaspora-owned firms to report pro-labor behavior. Observing a theoretically consistent direction of difference between diaspora-owned and non-diaspora-owned firms across similar statements in opposing directions indicates that results are not driven by systematic differences in use of the Likert scale between those two groups of firms.

Results for H1A-H1E: Local Labor, Wages, and Professional Development

Hypothesis 1 states that diasporans' social and emotional motivations for investment lead diaspora-owned firms to engage in more socially responsible and pro-development behavior than non-diaspora-owned foreign firms. In the questions used to test this hypothesis, firm managers are asked, using a seven point Likert scale, to what degree a set of statements reflects their firm's Georgian operations.[27] Table 5.6 presents results from tests of Hypotheses 1A-1E, each of which specifies a labor-related means through which diaspora-owned firms might show themselves to be more socially responsible than other foreign firms.

I also test the impact of diaspora ownership on an index of the question used to test Hypotheses 1B and 1C. In factor analysis, it was these two questions that hang together as measure of firms' willingness to bear costs to provide additional employee benefits.[28]

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[27] The questions referred to in this section were developed collaboratively with Professor Maia Mestvirishvili of Tbilisi State University.

[28] The Cronbach's alpha for these two variables is 0.8.

Table 5.6: Diaspora Ownership and Social Responsibility: Labor

	(1) Local Labor	(2) Salaries	(3) Qaul. of Life	(4) Prof. Dev.	(5) Labor Costs	(6) Labor Index
Diaspora-Owned Firm	0.405	-0.559	-0.342	0.043	1.014**	-0.882
	(0.533)	(0.504)	(0.487)	(0.474)	(0.438)	(0.685)
Respondent = Owner	0.609	0.632	0.600	0.559	-0.269	0.977*
	(0.491)	(0.417)	(0.430)	(0.427)	(0.444)	(0.559)
Primary Sector	0.178	0.762	0.579	0.288	-0.479	1.051
	(0.740)	(0.832)	(0.782)	(0.677)	(0.626)	(1.017)
Secondary Sector	-0.360	0.051	0.071	0.270	0.037	0.259
	(0.566)	(0.614)	(0.568)	(0.603)	(0.520)	(0.833)
Tertiary Sector	0.056	-0.289	-0.188	0.348	0.285	-0.171
	(0.588)	(0.590)	(0.540)	(0.580)	(0.478)	(0.777)
100% Foreign Ownership	0.248	0.423	0.870**	-0.096	-0.407	0.940*
	(0.375)	(0.366)	(0.360)	(0.352)	(0.354)	(0.461)
Western HQ	-0.320	0.106	-0.060	0.045	0.075	0.037
	(0.446)	(0.414)	(0.437)	(0.370)	(0.380)	(0.558)
Employees (logged)	0.186	0.179	0.157	0.168	-0.135	0.257
	(0.135)	(0.131)	(0.132)	(0.141)	(0.109)	(0.166)
Greenfield	0.394	-0.104	0.019	-0.559	-0.155	-0.082
	(0.447)	(0.383)	(0.421)	(0.369)	(0.428)	(0.545)
Constant						8.061***
						(1.038)
Cut 1: Constant	-2.152**	-2.271**	-2.587**	-2.650**	-2.645***	
	(1.046)	(0.861)	(1.244)	(0.986)	(0.774)	
Cut 2: Constant	-1.037	-1.924**	-1.842*	-2.309**	-1.728**	
	(0.903)	(0.784)	(0.922)	(0.918)	(0.765)	
Cut 3: Constant	-0.403	-1.162	-1.264	-1.954**	-1.262	
	(0.884)	(0.743)	(0.834)	(0.878)	(0.754)	
Cut 4: Constant	0.537	0.385	0.690	-0.587	0.187	
	(0.904)	(0.723)	(0.752)	(0.845)	(0.730)	
Cut 5: Constant		1.507*	2.041**	0.222	0.917	
		(0.752)	(0.766)	(0.841)	(0.741)	
Cut 6: Constant		3.103***	3.247***	1.655*	2.039**	
		(0.832)	(0.828)	(0.845)	(0.821)	
Observations	161	161	161	161	161	161

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Models 1-5 use ordered logit regression, model 6 uses OLS. All analysis is conducted on imputed data.

In the only statistically significant result (Model 5), diaspora-owned firms are more likely than other foreign firms to report keeping their total labor costs as low as

possible.[29] This is consistent with results in Models 2 and 3, which show that diaspora-owned firms are slightly less likely than other foreign firms to report devoting resources to socially responsible employment practices, namely paying above-market wages or providing a higher quality of life to their employees than peer firms. Model 6 shows a negative, but not significant effect of diaspora ownership on an additive index of responses to the questions used as dependent variables in Models 2 and 3.

The coefficient in Model 4 is approximately zero, indicating no difference between diaspora-owned and non-diaspora owned firms in the opportunities they provide employees for professional development.

The only result in Table 5.6 suggesting a greater social responsibility from diaspora-owned firms is Model 1, which indicates that diaspora-owned firms may be somewhat more likely than other foreign firms to prioritize the hiring of local labor. However, in light of the results in Models 2, 3, 5, and 6, this is more reasonably interpreted as a strategy for limiting labor costs than as a means to maximize pro-development impact.

Taken together, these results run directly counter to Hypothesis 1. There is no evidence that the labor practices of diaspora-owned firms are more socially responsible than those of other foreign firms, and some evidence that their practices are less socially responsible. While a larger sample size might provide us with evidence that diaspora-owned firms are marginally more committed to hiring local labor, there is no evidence that they treat the labor they do hire any better than other

[29] Substantively, a respondent from a diaspora-owned firm is 2.8 times as likely as an otherwise identical non-diaspora-owned firm to respond with a higher category response across any given cut-point on a seven point scale from "strongly disagree" to "strongly agree."

foreign firms. In fact, my results show diaspora-owned firms are more committed to minimizing total labor costs than are other foreign firms, and there is at least some evidence that this manifests itself in lower wages and a correspondingly lower ability of employees to provide a high quality of life for their families.

H2A and H2B: Development Impact and Efficiency vs. Social Responsibility

Hypotheses 2A and 2B posit more general ways in which diaspora-owned firms might demonstrate their commitment to social responsibility and the development of the homeland: by reporting that they contribute to the development of the homeland more than their peer firms and by being more likely to report making contributions to charity.

Table 5.7: Contributions to Development and Donations to Charity

	(1) Development	(2) Charity
Diaspora-Owned Firm	0.155	-0.314
	(0.556)	(0.536)
Respondent = Owner	0.192	0.043
	(0.512)	(0.440)
Primary Sector	-0.288	1.181
	(0.648)	(0.898)
Secondary Sector	-0.514	0.125
	(0.608)	(0.618)
Tertiary Sector	-0.129	0.045
	(0.546)	(0.621)
100% Foreign Ownership	0.729*	-0.335
	(0.377)	(0.406)
Employees (logged)	0.331**	0.527***
	(0.134)	(0.169)
Greenfield	0.440	-0.389
	(0.482)	(0.505)
Western HQ	0.061	-0.528
	(0.411)	(0.426)
Constant		-0.427
		(0.901)
Cut1: Constant	-0.952	
	(0.931)	
Cut 2: Constant	-0.256	
	(0.845)	
Cut 3: Constant	1.423*	
	(0.810)	
Cut 4: Constant	2.222**	
	(0.805)	
Cut 5: Constant	3.108***	
	(0.862)	
Observations	161	161

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$
Model 1 uses ordered logit regression, model 2 uses OLS.
All analysis is conducted on imputed data.

Neither of the differences between diaspora and non-diaspora owned firms are statistically significant, with the coefficient on diaspora ownership in Model 1 almost

exactly equal to zero, indicating that diaspora-owned firms are no more likely than their peers to report high contributions to the development of the homeland.

The positive sign on the coefficient in Model 2 indicates that diaspora-owned firms might be slightly more likely to contribute to charity, but the magnitude of the coefficient is less than half that of the standard error. The substantive size of this estimated effect is moderate; the expected probability a firm with mean characteristics will report contributing to charity increases from 42% if the firm is non-diaspora-owned to 52% if it is diaspora-owned.[30] Larger firms are more likely to contribute to charity and more likely to believe that the contribute more to economic development than their peers, which is unsurprising: the scale of their economic activity is larger as is the pool of resources they can draw on to make charitable contributions.[31]

As with the results in Table 5.6, the results presented here fail to give any evidence that diaspora-owned firms are more socially responsible than other foreign firms.

Graphical Presentation of Results

Figure 5.2 provides a graphical supplement to Tables 5.6 and 5.7. I omit the control variables and present a simple comparison of means between diaspora-owned and non-diaspora owned firms. Because of the similarity in firm demographics between diaspora-owned and non-diaspora-owned firms, these simple mean comparisons depict the same substantive results as the more complex analysis above.

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[30] The odds ratio for the diaspora dummy in Model 2 is 1.23.

[31] Analysis was also run including an interaction term between diaspora identify and firm size, but no statistically significant results were found.

Before the mean of each group is taken, responses to each question are rescaled to a mean of zero and a standard deviation of one.

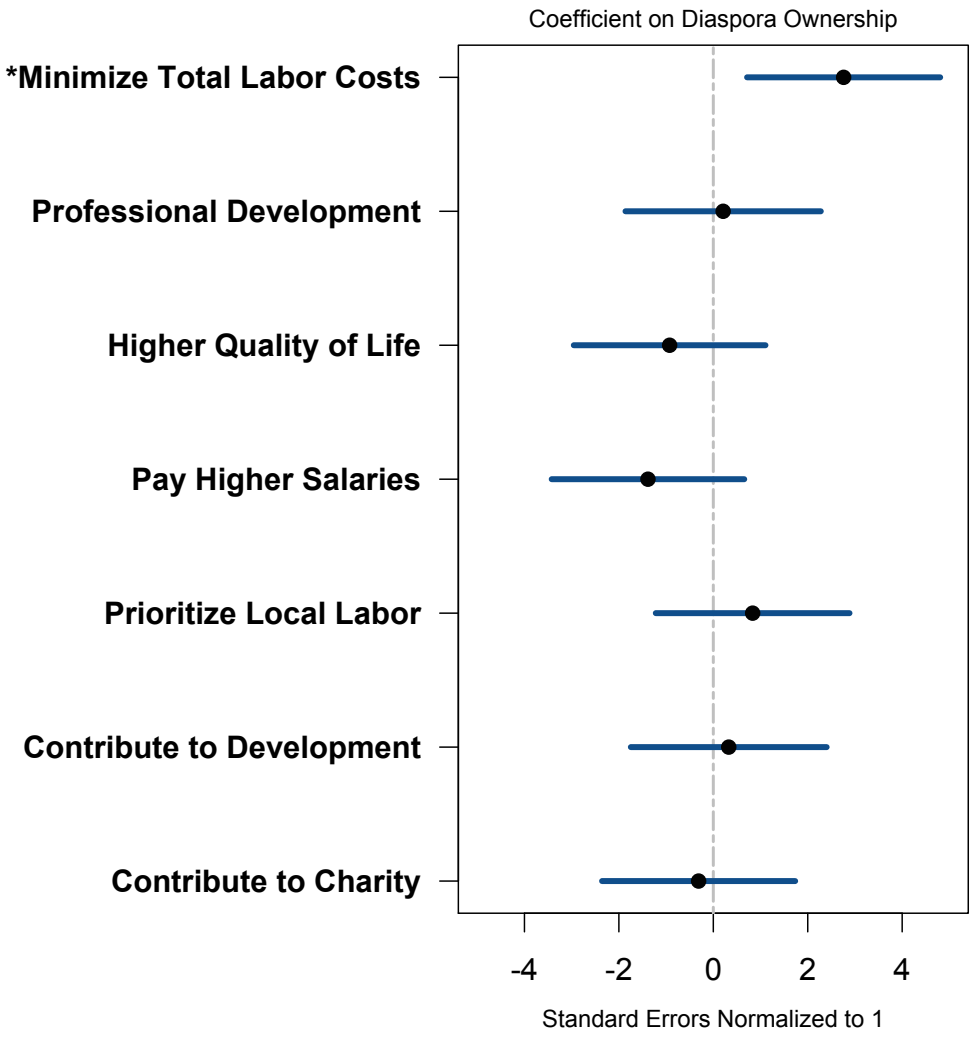

Figure 5.2: Social Responsibility

*Note: Horizontal lines give 95% confidence intervals.

It is in the final category, in which a positive difference indicates less socially responsible behavior by diaspora owned firms, that we observe the only statistically

significant difference between the two groups. Diaspora owned firms are more likely than other foreign firms to report that they minimize total labor costs. Across all other categories, a positive difference would indicate more socially responsible behavior by diaspora owned firms, and among these, it is only in the area of prioritizing local labor that we see any evidence that diasporans might be more socially responsible than their peers. As discussed above, other results suggest that this is more reasonably interpreted as a strategy for limiting labor costs than as a means to maximize pro-development impact.

Interpretation and Implications of Results

These results indicate that, even if non-pecuniary motivations are an important cause of diaspora investment, these motivations do not translate into socially responsible behavior by diaspora-owned firms. The hypotheses tested here did not capture all of the ways in which diasporans' non-pecuniary motivations might manifest themselves in socially responsible or pro-development actions, but if diaspora-owned firms are more socially responsible or engage in more pro-development behaviors than their non-diaspora-owned peers, this should have manifested itself with regard to at least some of the hypotheses tested here. It did not. This allows us to reject Hypotheses 1 and 2, and provides evidence that diaspora-owned firms are not more socially responsible than non-diaspora-owned firms. Because the results presented here are from only a single country, it is possible that diaspora-owned firms in other contexts are more socially responsible than their peers. However, based on current theory, Georgia was an unusually likely case in which

these behaviors should have been observed if current theory was correct. While levels of social responsibility among diaspora-owned firms in other contexts has not been empirically evaluated, the only extent theory that would lead us to expect these levels to be higher than among foreign firms generally is inconsistent with the results presented here.

The failure of diaspora-owned firms to report more socially responsible behavior than their non-diaspora-owned peers is particularly striking in contrast with the large and statistically significant behavioral differences between diaspora-owned and non-diaspora-owned firms found regarding their use of social networks. In concurrent work based on the same survey, I show that respondents at diaspora-owned firms are more likely to report that social networks are important to the profitability of their firm and to report that their firm has used friends and family members to help rent or purchase real estate (Graham 2012). Diasporans' social connections to the homeland affect firm strategy and augment firm profitability, but they do not motivate socially responsible firm behavior.

While these survey results come from a single, post-conflict developing country, Georgia is a relatively typical developing country, and one whose idiosyncrasies are expected to predispose diaspora-owned firms towards more socially responsible behavior, not less. Georgia recently fought an interstate war with Russia, which should have made the patriotic loyalties of diasporans more salient (Nielsen and Riddle 2010), and its diaspora enjoys a generally positive relationship with the current government. If diasporans' social and emotional motivations cause diaspora-owned

firms to engage in socially responsible and pro-development behaviors, Georgia is among the countries in which those differences were most likely to be found.

While my empirical findings contradict the theory and expectations that drive some of the current optimism regarding diaspora investment as a tool for development, it is important to note that the findings presented here are not necessarily incompatible with the argument that social and emotional motivations induce diaspora direct investment. Even if diaspora-owned firms do not demonstrate greater social responsibility than other foreign firms, their presence still contributes to the development in the homeland. The implication of these findings is not that developing country governments should not seek to attract diaspora direct investment, but rather that they should not view these firms as inherently more socially responsible than other foreign firms.

Directions for Future Study

Thus far, the literature on remittance has focused heavily on diasporans from Latin America and, to a lesser extent, Sub-Saharan Africa, while much of the research on diaspora direct investment has focused on diasporans from Asia, North Africa, and the Former Soviet Union. Both literatures have focused almost exclusively on diaspora populations living in the US and Europe. Comparatively little research has been done on diaspora direct investment in Latin America and Sub-Saharan Africa, and still less work on diaspora populations residing outside of the US and Western Europe. While the Georgian case is ideal for the purposes of this paper (i.e. it is a "most likely" case in which to observe socially responsible investment behavior by diaspora-owned

firms), there is an urgent need for both theory and empirical evidence regarding the behavior of diaspora populations outside of the regional contexts in which they are currently being studied. Diaspora populations vary in their geographic concentration, their social integration in the country-of-residence, and their intention/desire to eventually return to their homeland.[32] For example, South-East Asian migrants working on oil rigs in the Middle East have very different social structures, and likely very different remittance and investment behavior, than the diaspora populations that have so far been the primary objects of study. Building and testing a complete theory of diaspora investment motivation and reaching a full understanding of the development potential of diaspora direct investment requires careful attention to these sources of variation.

Building datasets that allow the direct comparison of diaspora-owned and non-diaspora-owned firms is a painstaking process, but I hope this paper demonstrates the benefits of doing so. As theory in this area becomes more nuanced, more and better data will be necessary to test it. As Newland notes, "The quality of information, much less hard data, about Diaspora influences [on development] is in general very poor, posing a serious challenge to policy development (2004, p. iv)."

Conclusion

Migrant populations are significant drivers of global flows of foreign direct investment, and this article takes another step forward in identifying the elements of diaspora difference that generate this causal relationship. Related research has

[32] An exciting first step in this research agenda was taken by Vaaler (2012) who finds that remittances from geographically concentrated diasporas have a larger impact on the availability of venture capital in the homeland than do remittances from geographically dispersed diasporas.

provided evidence for one mechanism, that diaspora investors use greater access and attention to information to gain competitive advantage in the homeland (CITATION REDACTED). This paper tests, and fails to find evidence for, an alternative theory of diaspora difference based on diasporans' non-pecuniary motivations for investment.

The economic sociology literature has established the importance of social and emotional motivations to diaspora investors at the ideational stage. The theoretical connection between these motivations and socially responsible behavior by diaspora-owned firms is clear and direct. Policymakers have launched programs to promote diaspora investment based, in part, on expectations that these firms will engage in more socially responsible and pro-development behavior than other foreign firms. This paper provides the first empirical comparison of social responsibility between diaspora-owned and non-diaspora-owned foreign firms. I find no evidence that diaspora-owned firms are more socially responsible than other foreign firms, and I find some evidence that they are less socially responsible (i.e. with regard to wages, Table 6 and Fig. 2).

These results underscore the point that diaspora-owned firms are profit seeking enterprises, not development NGOs. Diaspora-owned firms behave differently than other foreign firms – in particular, they exploit social networks in the homeland to gain information and secure trusting relationships with business counterparts – but they still behave as firms. This does not imply that diaspora investment is bad for development. The diaspora-owned firms in this survey have hired and trained workers,

given to charity, and contributed to the development of the Georgian economy. But they did not do more of these things than other foreign firms.

These findings should cause policymakers to revise expectations downward regarding the development-promoting effect of diaspora direct investment. As I have shown in concurrent work, diaspora-owned firms have important competitive advantages in the homeland, which may lead them to be more willing than other foreign firms to invest in fragile and developing countries, particularly as they stabilize. Diaspora investors should be recruited as uniquely capable, rather than uniquely socially responsible, investors in fragile and developing countries.

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Appendix

Diaspora-Owned and Non-Diaspora-Owned Firms by Home Country

A table showing the percentage of non-responses and diaspora ownership by region is included in the text. Table 5.8 shows the number of responses, number of refusals, and number of diaspora-owned and non-diaspora-owned respondents for each investing country. Firms from Azerbaijan, France, Israel, the Netherlands, and the United States were particularly likely to refuse to participate in the survey, but there is no obvious common characteristic across these countries that appears to drive non-response.

Table 5.8: Response Types by Home Country				
	Refused	Responded		Total
Country		Diaspora	Non-Diaspora	
Armenia	2	0	2	4
Australia	0	0	1	1
Austria	6	1	3	10
Azerbaijan	10	0	2	12
Belgium	3	0	1	4
Bulgaria	1	0	1	2
Canada	0	0	1	1
Cayman Islands	1	1	0	2
China	0	0	3	3
Cyprus	1	0	4	5
Czech Republic	3	0	2	5
Estonia	0	1	1	2
France	5	0	2	7
Germany	10	1	9	20
Greece	5	0	0	5
Iran	2	1	3	6
Israel	9	2	3	14
Italy	4	1	1	6
Japan	1	0	0	1
Jordan	1	0	0	1
Kazakhstan	2	0	2	4
Latvia	3	0	3	6
Lithuania	2	0	1	3
Luxemburg	0	0	2	2
Marshall Islands	2	0	1	3
Netherlands	9	2	4	15
Panama	2	0	1	3
Philippines	0	0	1	1
Poland	0	0	1	1
Russia	12	5	7	24
Sweden	0	0	1	1
Switzerland	6	0	3	9
Syria	1	0	0	1
Turkey	23	5	14	42
UAE	1	0	2	3
UK	19	4	19	42
USA	20	0	10	30
Ukraine	3	1	1	5
Virgin Islands	2	0	0	2
Unknown	26	6	10	42
Total	197	31	122	350

Evaluating the Supplemental Sample

Table 5.9 provides the results of difference-in-means tests comparing diaspora-owned firms in the random sample from those in the supplemental sample. The demographic differences between these groups of firms is minimal, as are the differences in the firms' responses to statements about social responsibility and development contribution. The core results of the paper are robust to the exclusion of the supplemental firms.

Table 5.9: Comparing Diaspora-Owned Firms from the Random Sample and the Supplement

Variable	Random Sample Mean	Supplemental Sample Mean	Difference in means	Standard Error (of difference)	t-statistic
Respondent = Owner	0.43	0.43	0.006	0.22	0.02
Primary Sector	0.04	0.17	0.13	0.11	0.1
Secondary Sector	0.28	0.5	-0.22	0.22	-1.0
Tertiary Sector	0.52	0.17	0.35	0.22	1.6
Quaternary Sector	0.16	0.17	-0.01	0.17	-0.04
100% Foreign Ownership	0.58	0.71	-0.14	0.21	-0.64
HQ in West	0.33	0.67	-0.33	0.30	-1.0
Number of Employees	145	124	21	148	0.14
Tbilisi (location)	0.81	0.71	0.09	0.18	0.52
HQ in Russia	0.19	0.33	-0.14	0.36	-0.55
Pays Higher Salaries	4.59	4	0.59	0.84	0.70
Quality of Life	4.53	4.83	-0.30	0.74	-0.41
Labor Costs	4.81	4	0.81	0.79	1.02
Contribution to Development	5.06	4.75	0.31	1.02	0.31
Prioritizes Local Labor	6.57	5.8	0.74	0.54	1.37
Contributes to Charity	0.57	0.67	-0.10	0.24	-0.40

Chapter 6:
Conclusion and Directions for Future Research

To explain why some fragile states succeed in attracting FDI and others fail, and to craft policy approaches to help these states stabilize and achieve economic growth, we must understand what type of investors are capable of investing in the face of political risk. In Chapter 2, I argue that political risk specialists provide a reliable source of capital for weak states plagued by problems of violence and regime incompetence, but not for fragile states plagued by risk of adverse regime change. In Chapters 3, 4, and 5, I argue that, while diasporans enjoy substantial competitive advantages in their (sometimes fragile) homelands, there is no evidence that diasporans' social and emotional motivations for investment make them either risk insensitive or more socially responsible than their peers. However, the high sensitivity of migrant-induced FDI to improvements in political risk suggests that diaspora direct investment can serve as a critical complement to, and incentive for, improvements in governance in fragile and developing states.

These findings highlight the differences between FDI flows into fragile states, where regimes are unstable and risk of adverse regime change tends to be high, and weak states, where policy is unstable and the bureaucracy is corrupt or incompetent. They also identify types of investors who can succeed in investing in states with high levels of political risk, and the means by which they do so.

With regard to diaspora investors in particular, the expectations currently guiding much policy are inaccurate. The expectations that diasporans are "brave and resilient" investors who can be counted on to invest in the face of high levels of

political risk, and that diaspora-owned firms are more socially responsible than their peers, are not supported by the evidence. The evidence presented here suggests instead that diaspora investors represent a pool of attentive and informed investors who are likely to respond quickly to improvements in governance. This implies that focusing on governance improvements during the early stages of fragile-state-stabilization has the potential to trigger inflows of diaspora direct investment. A wealthy and engaged diaspora is a valuable asset for a developing country, but the investment potential of this population cannot be effectively exploited without first limiting political risk.

One of the major strengths of this project is that I apply a mixture of methods and test the implications of my theory at multiple levels of analysis. The results from the firm-level survey in Georgia reinforce and unpack findings regarding bilateral flows of FDI between countries and the location decisions of Japanese manufacturing firms. However, the firm-level analyses are currently limited by a lack of data availability, as is the analysis of the patterns of FDI flowing into fragile states. This makes the acquisition and analysis of additional data the most important next steps in pushing this research agenda forward.

Next Steps

There are three channels through which I am currently pursuing new data: I have recently been granted access to classified firm-level data from the Bureau of Economic Analysis (BEA); I will be rolling out a new firm-level survey of domestic and foreign-owned firms (including diaspora-owned firms) in the Philippines in early 2013; and I am pursuing both an updated version of the Japanese Overseas Investment

data and several possible substitute datasets through the University of Southern California library system.

The U.S. is the single largest provider of FDI to fragile states and the BEA data will allow me to look in detail at the composition of U.S. FDI into fragile and poor countries, including the sector, size, and previous international experience of the entrants. The data also includes information on firm growth and profitability, allowing me to examine not only who invests in fragile states, but who succeeds there.

The survey on the Philippines will repeat many of the questions from the Capital and Conflict: Georgia survey, but will ask additional questions about firms' interactions with the government – especially their use of civil courts and interactions with regulators, customs officials, and tax authorities. Two of the primary goals of the survey are to confirm the core findings in Chapters 4 and 5, and to test whether diaspora-owned firms can use their social networks to substitute for poorly functioning formal political institutions, particularly civil courts. The survey will include both foreign and domestic firms, allowing me to assess the role of diaspora capital and the use of social networks in both categories of firm

An updated version of the Japanese Overseas Investment in necessary to re-run the firm-level analyses from Chapter 5 with a wider range of risk variables. This will allow me to test the two hypotheses left untested in the current version, and do a more thorough job of testing those I was able to examine. I also am exploring the OSIRIS database from Bureau Van Dijk and several other options. These databases contain information on foreign entries by multinationals based in a range of home countries,

which may allow me to test for effects of both home- and host- country political risk experience at the firm level. However, it is not clear how easy it will be to convert information from these data sources into a useable format for this purpose.

Two Book Projects

This dissertation contains the seeds of two separate book projects. One book project will share its title with the dissertation and focus on foreign investment in fragile states. This book will depend heavily on data from the BEA to provide descriptive statistics regarding the type of U.S. investment taking place in fragile states. It will also rely on firm-level data from the Japanese Overseas Investment data and potentially other firm-level databases. The book will first introduce descriptive statistics, then present theory about who invests in fragile states and who succeeds there (hypotheses about home country, prior international experience, sector, size, and ownership structure), and then test these hypotheses using several distinct firm-level datasets.

The other book project is on the political economy of diaspora direct investment. This book will primarily present a general theory of diaspora direct investment, discuss the findings from the Georgia and Philippines surveys as well as from Chapter 3. I am also exploring the possibility of including data from a third survey – most likely in India.

As noted above, expanding the survey to additional countries will allow me both to improve the external validity of my findings, as well as drill deeper into how diasporans interact with the homeland government, including their use of courts, their

interface with regulatory bodies, and their involvement in local politics. In India I expect to be able to pre-stratify the sample by diaspora ownership, which I was not able to do in Georgia. In the Philippines I will be conducting the survey in collaboration with Cesi Cruz, who has already run a number of successful surveys looking at vote buying in the Philippines and has the necessary experience asking politically sensitive questions in that country.

These book projects provide the avenue for pursuing the empirical testing begun in this dissertation, as well as an opportunity to further sharpen the theory to derive additional testable implications. The theoretical and normative implications of the project are substantial, and the current state of knowledge quite limited. I am optimistic that a broad sustained research agenda in this area can be extremely productive.