A BIT of Investor Protection: How Bilateral Investment Treaties Impact the Terms of Syndicated Loans

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We study the impact of government expropriation risk on the terms of cross-border syndicated loans. By comparing loans by foreign lenders from countries covered by bilateral investment treaties (BITs) to loans from non-covered countries, we isolate and quantify the impact of strengthening property rights against government expropriation on loans. We find that stronger property rights lead to a lower cost of debt, larger loans, larger syndicates, less collateral, and fewer covenants. Results are stronger in countries with a history of government expropriation and robust to methodologies accounting for the endogenous nature of BITs and for the simultaneous determination of loan terms.

JEL Classification: G15, G32, G38
Keywords: Property rights, political risk, government expropriation, syndicated loans

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I. Introduction

Since the seminal research by La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1997 and 1998), a vast literature has emphasized that lenders need to be concerned not only with the credit worthiness of borrowers, but also with the risks due to weakness of laws and institutions. Building on this insight, Acemoglu and Johnson (2005) distinguish between "property rights institutions" aimed at protecting citizens and firms from expropriation by the government and powerful elites, and "contracting institutions," aimed at regulating contracts between private citizens and firms. In our research, we focus on property rights and investigate the impact of the risk of government expropriation on the terms of cross-border syndicated loans. In doing so, we analyze the impact of bilateral investment treaties (BITs). BITs are agreements between sovereign entities allowing private foreign investors to seek legal protection in third-party arbitration courts against acts by governments that are perceived as expropriating. Delegation to third-party arbitration courts increases the cost of expropriations for governments and, thus, decreases the likelihood of a government-initiated expropriation (Graham, Johnston, Kingsley, 2015). Our research offers two main contributions to the extant literature. First, we isolate the impact of property rights against government expropriation on debt contracting, in contrast with extant literature that focuses on broader institutional quality and strength of contracting institutions.[1] Second, we provide the first evidence of the impact of BITs on debt contracting; as our sample reveals that BITs cover approximately one-quarter of cross-border syndicated lending, we address a meaningful gap in the literature.

The definition of "government expropriation" that we adopt encompasses a broad range of actions by government entities, politicians, and connected parties.[2] As documented by Kobrin (1982, 1984), Minor (1994), and Hajzler (2012), while outright extra-legal takeovers of whole firms and industries are

[1] The impact of contracting institutions on syndicated loans has been studied by Esty and Megginson (2003), Qian and Strahan (2007), and Bae and Goyal (2009).

[2] We mimic extant literature and adopt the definition of "expropriation" in Kobrin (1982 and 1984), Minor (1994), and Hajzler (2012), which are virtually equivalent formulations. Hajzler's explicit formulation distinguishes between four types of actions: "(i) explicit confiscations of property, (ii) breaches of contract (such as forced renegotiation of the contract terms) […], (iii) extra-legal interventions or transfers of ownership effected by private agents and not resolved by government, and (iv) the forced sale of property."

increasingly rare, less direct forms of expropriations are still a real problem for foreign investors. Recent surveys indicate that the risk of expropriations is one of the main concerns of foreign investors.[3] A typical example lies in the actions taken by the government of Hungary in 2011, currently being disputed in both domestic and transnational courts. Over the preceding decade, foreign banks had provided financing to Hungarian firms and private citizens, including over USD 15 billion in mortgage-related loans, mostly denominated in Swiss francs and euros, taking advantage of low interest rates in those currencies. Following a dramatic loss of value of the Hungarian forint, which depreciated against the Swiss franc by over 40% between 2008 and 2011, a large number of domestic borrowers were unable to service foreign-currency debt. This led to a wave of defaults and a rapid depreciation in real estate values. The Hungarian government reacted with laws mandating foreign banks to retroactively redenominate foreign-currency loans into domestic currency and forcing a past-dated exchange rate. Effectively, this constituted expropriation of foreign lenders and a transfer of value to domestic borrowers; while the exact extent of losses to foreign lenders is being debated, most estimates are in the range of USD 1.7 billion.[4] We posit that BITs provide the means for foreign investors to seek compensation in such cases, thus lowering the incentives for governments to expropriate and, in case of expropriations, leading to higher levels of compensation.

Syndicated loans provide an optimal testing ground in this context. First, worldwide, the syndicated loan market, where multiple lenders form a "syndicate" with the purpose of providing financing to a single borrower, is the most important source of external funding for corporations (Lin et al., 2012; Chui et al. 2010).[5] The sheer size of the syndicated lending market ensures that our findings are relevant and

[3] World Bank Group (2013) finds that political risk (broadly defined to include the risk of expropriation) is the second most important concern for foreign investors, trailing closely the risk of macroeconomic instability. It notes that 19 percent of surveyed investors in the Middle-Eastern and North African regions claimed to have cancelled or withdrawn investments in 2012 due to expropriation risk.

[4] The dispute has been widely reported in the media, for example in a Bloomberg news article dated November 10, 2014, titled "Hungary Sets Loan Shift at Market Rate in Relief to Banks."

[5] In 2016, global syndicated lending for the full year totaled USD 4.7 trillion, while global corporate bond issues, the only larger source of financing for corporations, reached approximately USD 5.5 trillion. The totals are, respectively, from the "Thomson Reuters Debt Capital Markets Review (Full Year 2016)" and the "Thomson Reuters Global Syndicated Loans Review (Full Year 2016)," both available at http://dmi.thomsonreuters.com/

generalizable to a large cross-section of firms. Second, syndicated loans allow for the identification of both borrowers and lenders, in contrast with bond markets, where lenders are many and anonymous. Further, syndicated loans offer a wealth of contracting dimensions which can potentially be affected by weak property rights, as lenders use both price and non-price contract terms to mitigate lending risk (Qian and Strahan, 2007). We examine syndicated loans initiated over the period January 1980 to December 2013. We restrict our analysis to cross-border loans, as Qian and Strahan (2007) and Haselmann, Pistor, and Vig (2010) find that foreign lenders are most responsive to differences in legal systems, while Esty (2006) and Lin et al. (2012) find that foreign banks are most exposed to the risk of expropriation.[6] Kobrin (1982) and Minor (1994) similarly find that governments are more likely to expropriate foreign investors than domestic ones. This is due to foreign investors having lower power in influencing domestic political outcomes, to the spread of (often populist and xenophobic) ideology depicting foreign investors as predatory, and to the weaker political connections of foreign firms, thus lacking shelter from predatory acts. Our sample spans 161 countries, and the total number of loans is 45,255, with an aggregate value of USD 20.74 trillion. Approximately one-quarter of the loans in our sample are covered by a BIT.[7]

We further offer a methodological contribution to the broader literature on institutional quality and debt contracting. Empirical research testing the impact of institutional quality and property rights on lending markets faces difficulties in identifying appropriate benchmarks. A number of empirical studies analyzes lending across countries varying in the level of institutional quality. The challenge, for those studies, lies in comparing financial markets across countries which differ among various dimensions, some of which are hard, if not impossible, to quantify.[8] Other studies resort to comparing financial markets from the same

[6] While a portion of government actions affecting foreign lenders stems from the nationalization of domestic borrowers and subsequent defaults (Calfish, 1967; Bederman, 2000), other cases do not affect, and in some cases even benefit, domestic borrowers—as in the previously cited expropriation of foreign lenders by the Hungarian government in 2011.

[7] Due to incomplete data at the borrower and loan level, most of our empirical analysis covers a subset of the data. We have complete data for 4,768 loans, worth approximately USD 4.59 trillion, of which 1,201 are covered by BITs.

[8] Cross-country studies of the impact of institutions on syndicated loans include Esty and Megginson (2003), Qian and Strahan (2007), and Bae and Goyal (2009).

country at different points in time, around some events signifying a change in institutional quality.[9] Yet, such an approach is also likely to face identification problems, as changes in the institutional environment often occur contemporaneously with other economic and social upheaval (privatizations, market deregulations, etc.). Finally, empirical research on property rights suffers from a lack of clear metrics that can be used to identify causation, as extensively discussed by Glaeser et al. (2004). In our empirical analysis, we compare the characteristics of syndicated loans by foreign lenders covered by BITs to near-contemporaneous loans to borrowers in the same country—and, in more restrictive tests, to the same borrowers—issued by foreign lenders who are not covered by BITs. This allows us to isolate the impact of property rights on loan contracts, while holding constant other country and borrower characteristics.

As a first test, we compare loans covered by BITs ("BIT loans") to a set of near-contemporaneous loans sharing the same "loan purpose" and "loan type" classifications and matched by borrower country and industry. Our main proxy for the cost of debt is the "all-in-drawn" spread (for brevity, "spread") defined as the amount the borrower pays over a reference rate (the London Interbank Offered Rate, LIBOR) for each dollar borrowed. The all-in-drawn spread includes the interest over LIBOR paid on the loan and any fee paid to the bank group, in basis points (bp). Our results indicate that loans covered by BITs have lower spreads: the average spread on BIT loans is approximately 159 bp, versus 180 bp for matched loans, indicating a 21 bp discount. Also, BIT loans tend to be larger and loan syndicates comprise a larger number of lenders. We further find that loan contracts covered by BITs rely less frequently on other risk-mitigating mechanisms, such as collateral and financial covenants.

Our initial empirical setup relies on comparing loans from lenders based in different countries. One possible alternative explanation for the observed differences in loan characteristics is that country-level factors have a supply-side impact on lending markets. For example, extant literature finds that foreign investments are affected by country-level factors such as geographic proximity, cultural affinity, differences

[9] For example, Haselmann, Pistor, and Vig (2010), Cerquiero, Ongena, and Roszbach (2016), and Rodano, Serrano-Velarde, and Tarantino (2016) study bank lending around changes in bankruptcy and collateral law.

in legal system development, colonial ties, and others. Accordingly, in a second test, we match BIT loans to non-BIT loans by both borrower and lender countries, using as a benchmark loans initiated prior to the signing of the treaty; we confirm our previous findings. In a third test, we compare BIT loans to non-BIT loans to the same borrower. Even in this most stringent setting, we find that BIT loans have lower spreads, tend to be larger, and involve larger lending syndicates. As a final test, we rely on propensity-score matching to identify loans to "similar" lenders and, once more, we confirm our main findings.

We further note that, if the impact of BITs is due to a reduction in property rights risk, the presence of a BIT should be particularly meaningful when the risk of expropriation is high, *ex-ante*. Using a dataset from Kobrin (1982 and 1984), which provides a count of the number of expropriations for 79 countries over the period 1960-1979, we identify countries whose governments have expropriated investors in the past. We find that, in countries without prior expropriations, loan spreads between BIT loans and matched loans are not statistically different. On the other hand, in countries with prior expropriations, BIT loans are associated with a discount of 40 bp. In additional tests, we similarly find that the impact of BITs is stronger in countries with weak democratic institutions and with weak constraints on the governing executives.

Extant literature has linked loan terms to both firm-level and macroeconomic conditions. Accordingly, to better isolate the impact of BITs, we model loan spreads in a regression framework as in Qian and Strahan (2007), as a function of macroeconomic factors, firm characteristics, and loan characteristics. We find that spreads are negatively related to the presence of a BIT and positively related to past expropriations. Further, the interaction between BITs and past expropriations has a negative impact on loan spreads, confirming that the impact of a BIT on loan terms is stronger in countries with a history of expropriations. As in Qian and Strahan (2007), we further model the non-price terms of loans in a regression framework. We find that expropriations are negatively related to loan maturity, but that the impact is mitigated by BITs. We confirm that, in the presence of BITs, lending syndicates tend to be larger. We also find fewer financial and general covenants in the presence of BITs.

A loan being covered by a BIT is not a random event. Rather, creditors or borrowers might match to seek the protection of BITs for specific loans, possibly those loans that are exposed to a higher risk of

expropriation. Accordingly, unobserved determinants of a loan being covered by a BIT could induce a spurious correlation between BITs and the cost of loans in regression analyses. To mitigate such concerns, we conduct two sets of robustness tests. First, we restrict our analysis to "relationship loans": loans for borrower-lender pairs that are established prior to BIT coverage. Second, we estimate regression coefficients using propensity-score weighting, with weights derived from the estimated probability of BIT coverage. The estimated results confirm our previous findings.

We also note that the signing of a BIT could be non-random. That is, countries might sign BITs during specific economic conditions, as domestic borrowers require access to foreign lenders (or foreign equity investors). To mitigate such concerns, we add country-year fixed effects to our regression models, thus controlling for time-variant country-level characteristics. Our main results are unaffected.[10] We further estimate the impact of BITs on loan spreads using a two-stage Heckman (1979) procedure based on Carr, Markusen, and Maskus (2001) and Jandhyala and Weiner (2014). In the first stage, we model BITs as a function of political constraints, size of the economies, geographical distance between countries, and differences in per capita wealth. Once more, our main results prove robust.

Our analysis indicates that BITs lead to lower spreads. Although matched-sample analysis also identifies more borrower-friendly loan terms for BIT loans, including longer maturities and less frequent use of collateral or financial covenants. In this sense, the actual discount is likely to be underestimated. Given that the price and non-price terms of loans are simultaneously determined we cannot simply include the non-price terms of loans as explanatory variables in the spread equations. We address this issue as in Bharath et al. (2011), by employing an instrumental variable (IV) framework. We use a simultaneous equation model that accounts for the endogenous nature of loan spread, loan maturity, and loan collateral. The results obtained in this framework again suggest that, while expropriation episodes increase the cost of loans, the presence of a BIT largely negates these effects.

[10] We discuss additional tests ruling out endogenous timing driving our finding in Section V.B. We also note that extant literature finds that the signing of BITs is not driven by economic conditions as much as by political shocks (Vandevelde, 2009).

We further recognize that the existence of a BIT between two countries might be correlated with other links. For example, those countries might have stronger past trade links, or might have signed other treaties (such as multilateral trade agreements or double-taxation treaties). Such links are more likely to occur within, rather than across, world regions (Neumayer and Spess, 2005). Accordingly, as a robustness test, we confirm that all our results hold in a sample of loans across world regions.

Our work contributes directly to the literature on lending and legal institutions. Esty and Megginson (2003), Qian and Strahan (2007), and Bae and Goyal (2009) investigate how weak institutions impact the structure of bank loans and how lenders react by adopting contracting structures aimed at mitigating risk. In this sense, our analysis addresses the objection by Acemoglu and Johnson (2005), who lament the lack of distinction between property rights and contracting institutions (creditor rights, in our setting) and emphasize the importance of the former. We find evidence that the risk of government expropriation is priced in loan contracts and that it affects loan terms, distinctly from other forms of institutional weakness, such as weak creditor rights. This distinction is meaningful, as it affects some of our core findings: for example, while Esty and Megginson (2003) and Qian and Strahan (2007) find that weak creditor rights are associated with larger lending syndicates, presumably to allow for diversification and to discourage strategic defaults via costly restructuring, we show that the risk of expropriations is associated with smaller lending syndicates, as concentrated ownership deters government expropriations (Stulz, 2005). Similarly, we find that the risk of expropriation is associated with more use of collateral, while extant literature finds no robust relationship between the use of collateral and creditor rights.

We also contribute to the literature examining links between institutional quality and finance by making use of a novel empirical setting to mitigate the problems described by Glaeser et al. (2004), who describe how traditional metrics of institutional strength suffer from measurement error and endogeneity problems and thus do not allow for a clear interpretation of causal relationships.[11]

[11] Glaeser et al. (2004) further criticize extant metrics for not being true measures of executive constraints, but for reflecting both constraints and policy choices, and for being transitory. BITs, by imposing constraints on the actions of governments and by being, in Glaeser et al. (2004)'s terms, "durable," address both objections.

Finally, we document that governments can successfully mitigate property rights risk by delegating jurisdiction to foreign courts by means of BITs. Our study of BITs, of their risk-mitigating properties, and of their ultimate effect on loan terms is novel in the empirical corporate finance literature. Since our data reveals that about one-quarter of cross-border lending (and approximately the same share of foreign direct investment) is now covered by a BIT, this is a big omission in the literature. Yet, this finding has much broader implications for the debate on how governments can guarantee property rights. While Acemoglu and Johnson (2005) claim that enforceable contracts between the state and individuals are not possible, as governments are the ultimate guarantors of property rights, we find evidence that enforceable contracts are feasible when governments surrender arbitration power to external parties.

This paper is organized as follows. Section II develops testable hypotheses. Section III describes the data sources and the dataset. Section IV focuses on the empirical analysis. Section V presents additional robustness tests. Section VI concludes. Additional detail on BITs and their signing is contained in appendix.

II. Hypotheses Development

In this section, we discuss the relevant extant literature, then develop testable hypotheses.

A. Bilateral Investment Treaties and Expropriations

A BIT is an agreement signed by two sovereign entities enhancing the protection of property rights for foreign investors.[12] The primary purpose of BITs is described by Sachs and Sauvant (2009) as "to protect investors from political risks and instability and, more generally, safeguard the investments made by its nationals in the territory of the other state." In this sense, a BIT is a tool to strengthen property rights of foreign investors against the risk of expropriation by governments.[13] This is accomplished by allowing

[12] Sachs and Sauvant (2009) offer a detailed legal analysis and an extensive historical perspective on BITs. They find that the first BIT was signed in 1959, between Germany and Pakistan, but that, until 1989, only 386 BITs were signed. The number of BITs has since grown dramatically, with more than 2,200 BITs signed over the following fifteen years. While originally BITs mostly involved developing countries, the countries with the highest number of BITs are now Germany, China, and Switzerland.

[13] The main and virtually exclusive purpose of BITs is to strengthen the legal protection of foreign investors. BITs do not contain provisions such as guaranteed market access, tariff reductions, or tax treaties, which are found in other

foreign investors to request binding arbitration in third-party courts (often, the World Bank's International Centre for Settlement of Investment Disputes) in case of dispute with a foreign government. As discussed at length by Caflisch (1967) and Bederman (2000), international law offers stronger protection to foreign equity investors than to foreign creditors. BITs, on the other side, specifically protect both equity investors and creditors from government actions which negatively impact the value of investments. In this sense, BITs strengthen property rights both by lowering the ex-ante probability of government expropriation (by imposing greater costs on the expropriating government) and by offering greater compensation to wronged parties in the case of a government expropriation. Caflisch (1967) and Bederman (2000) discuss their effectiveness and success in providing recourse to both equity investors and foreign lenders. Graham, Johnston, and Kingsley (2015) distinguish between expropriation risk and transfer risk (the latter defined as the risk of constraints on profit repatriation). While they mainly focus on the determinants of transfer risk, they also show that BITs mitigate the risk of expropriation. Finally, Büthe and Milner (2009) cite ample anecdotal, interview-, and survey-based evidence of BIT effectiveness, finding that foreign investors consider the presence of BITs when allocating FDI and that investment promotion agencies (such as the UK Foreign and Commonwealth Office) regularly receive inquiries from foreign investors regarding the existence and content of BITs.

While our study is the first to investigate the impact of BITs on loan contracts, there is extant literature investigating the impact of BITs on the flow of cross-border foreign direct investment (FDI), offering mixed evidence.[14] The challenge in investigating the impact of BITs on aggregate flows lies in the fact that the relation could be endogenously driven by lobbying efforts by borrowers or lenders. Such

types of investment agreements. Also, most BITs are very similar in language and formulation, as a common template is widely employed.

[14] Salacuse and Sullivan (2005) find that BITs signed with the United States increase FDI inflows into developing countries, but that BITs signed with other countries have no impact. Their findings are echoed by Büthe and Milner (2008 and 2009) and Neumayer and Spess (2005), both finding that BITs increase FDI inflows, but that the magnitude of the effect depends on the level of development of the signatories. Hallward-Driemeier (2009) find no statistically significant effect of BITs on FDI inflows, while Tobin and Rose-Ackerman (2009) find a surprising negative impact in countries with high *ex-ante* risk levels and a positive impact in low-risk countries. Sachs and Sauvant (2009) offer a more detailed review of the extant literature, citing multiple single-country or single-region studies.

lobbying is likely to increase when lending grows in volume, leading to a possible feedback effect between lending and BITs and to difficulties in attributing causation (Sachs and Sauvant, 2009), or during periods when foreign investment is most needed (for example, an emerging economy facing rapid growth but low domestic liquidity in financial markets). Aisbett (2009) attributes the conflicting findings on the impact of BITs on FDI inflows to improper controls for endogenous BIT timing. Poulsen (2009) suggests that researchers should focus on the impact of BITs on individual deals (rather than aggregate investment flows), as the study of deal characteristics is less likely to suffer from these empirical challenges—which is what we do in the present manuscript.[15]

The corporate finance literature, while discussing the role of institutional quality since the seminal work by La Porta et al. (1997, 1998), has rarely touched upon the enforcement of property rights against government expropriations (as distinct from enforcement of contracts in general terms). The topic of extraction of corporate assets by politicians and bureaucrats traces back to Rose-Ackerman (1975), although her focus is on politicians requesting bribes to provide access to government contracts.[16] The interaction between politicians and firms has been studied by Shleifer and Vishny (1994) and Hellman et al. (2003), giving rise to a stream of the literature that investigates both asset extractions by politicians, and firms' defensive reactions. Stulz (2005) investigates how both firms' ownership and capital budgeting are affected by the need to shelter assets from expropriation by governments and politicians. Caprio, Faccio, and McConnell (2013) find that, when the risk of political extraction is high (high levels of corruption), firms retain less liquid assets. A stream of the literature has further discussed the relation between politicians and

[15] While higher investment flows might increase the likelihood of investment treaties being signed, it is not obvious why deal terms (for example, lower loan spreads or the inclusion of covenants) should affect the likelihood of two countries signing a BIT, hence reverse causality should not be an issue. Yet, deal level analysis is still vulnerable to omitted variable biases, given the non-random timing of BITs. Accordingly, in the following sections, we present extensive robustness tests mitigating the impact of non-random BIT timing on empirical analysis, including the addition of country-year fixed effects in regression analysis, matching observations by country and year, and propensity-score weighted regressions including time-variant country characteristics in probit estimations.

[16] Corruption is further investigated by Bliss and Di Tella (1997) and Ades and Di Tella (1999), with a particular focus on the impact of corruption on competition and the creation of barriers to entry. A large literature on corruption and its impact on firms has since emerged, including Mauro (1995) and Mo (2001) linking corruption to lower firm and economic growth, and a survey by Bardhan (1997).

lenders—as examples, Sapienza (2004) and Dinç (2005)-provide strong evidence of political distortions in lending. Yet, the literature linking firm-level borrowing data to political extraction has so far been underdeveloped; one exception is Hainz and Kleimeier (2012), who find that loans in high-political-risk countries are more likely to involve development banks and to be structured as "project finance" contracts.[17]

B. Testable Implications in Lending Markets

Qian and Strahan (2007) and Bae and Goyal (2009) find that a weak contracting environment leads to higher interest rates on loans, inces lenders require compensation for the additional risk, but do not address the distinction between property rights (protection versus government expropriation) and contracting environment (protection of creditors versus expropriation by other private entities).[18] We are mindful of the distinction by Acemoglu and Johnson (2005) and of their emphasis on the importance of the former.[19] Accordingly, we expect the cost of the loan to increase in weak property rights, as lenders require compensation for the risk of expropriation by governments. As BITs strengthen property rights, we expect a negative relation between BITs and loan spreads.

Hypothesis H1a: BITs lead to lower loan spreads.

Given that BITs provide recourse to creditors in disputes with government entities, we further hypothesize that the impact of BITs will be greater in the presence of weak property rights, where the ex-

[17] While the empirical setting by Heinz and Kleimeier (2012) is similar to ours, in that they discuss how political risk impacts syndicated loans, the authors only investigate the inclusion of development banks and the project finance structure of loans, not offering any analysis of loan pricing or other contracting features (maturity, covenants, collateral, etc.).

[18] Among the extant studies on institutional quality and lending, only Bae and Goyal (2009) claim to investigate the distinction between property rights and creditor rights. Yet, the main metric of property rights they employ aggregates measures of corruption, contracting quality, and risk of expropriation—and, while calling it a measure of "property rights," the authors more correctly interpret it as a broad measure of quality of legal enforcement. In robustness tests, they attempt to isolate the impact of the risk of expropriations, using a survey-based index by the International Country Risk Guide (ICRG); yet, this index itself measures both "contract viability" and "outright expropriation" and is available for only a subset of years covered by the sample in Bae and Goyal (2009). Finally, the authors find that a higher risk of expropriation is associated with lower spreads, but do not provide a rationale for this puzzling finding. In robustness tests, we show that our findings are not subsumed by the ICRG metric and document a low correlation between this metric and counts of actual expropriation episodes.

[19] Acemoglu and Johnson (2005) argue that "individuals can structure contracts to reduce the adverse effects from contracting institutions […] In contrast, because enforceable contracts between the state and individuals are not possible, property rights institutions constraining arbitrary behavior and expropriation by the state and elites have more important effects on economic outcomes."

ante probability of such disputes is greater.

Hypothesis H1b: BITs reduce loan spreads more in countries with weak property rights.

Diamond (1991 and 1993) finds that banks shorten loan maturity to review lending decisions more frequently when contracting risk is higher, while Demirgüç-Kunt and Maksimovic (1999) show that debt maturities are longer in countries with strong legal institutions. Applying those findings to syndicated lending, Qian and Strahan (2007) and Bae and Goyal (2009) find that borrowers use loan maturity as a risk-mitigating mechanism and that, accordingly, weaker institutions lead to shorter maturities on loans. Similarly, we are expecting loan maturity to increase in property rights. As BITs strengthen property rights, we expect a positive relationship between BITs and maturity:

Hypothesis H2: BITs lead to longer loan maturity.[20]

Extant literature also finds that lenders respond to higher risk by charging higher interest rates, by employing non-price risk mitigating loan terms, and by rationing capital (Stiglitz and Weiss, 1981). Bae and Goyal (2009) consistently find that banks respond to poor enforceability of contracts by reducing the size of loans. We similarly hypothesize that banks mitigate lending risk by reducing loan size in the presence of expropriation risk, and, accordingly, we thus expect BITs to be associated with larger loans.

Hypothesis H3: BITs lead to larger loans.

Esty and Megginson (2003) and Qian and Strahan (2007) show that institutional quality affects the size and concentration of a lending syndicate. They find that, in a strong institutional environment, syndicates tend to be smaller and loans more concentrated, to facilitate monitoring and improve re-contracting flexibility. In contrast, in weak institutional environments, larger syndicates deter strategic defaults. Yet, there are opposite effects at play as well. First, riskier loans could lead to larger lending syndicates, as lenders attempt to spread risk by retaining a smaller fraction of the loan. Second, Stulz (2005) discusses how, when property rights are weak, firm ownership tends to be more concentrated, as stronger

[20] While, for the sake of brevity, we do not explicitly discuss how BITs and property rights interact to affect the non-price loan terms, the implicit hypotheses are that the impact of BITs is stronger when property rights are, *ex-ante*, weaker (as in Hypothesis H1b).

shareholders have greater incentives to oppose expropriations by governments. Extending this argument to syndicated lending markets, smaller lending syndicates could deter government expropriations. Accordingly, the net impact of property rights on the size of the lending syndicate cannot be easily predicted—and, conversely, the link between BITs and the size of lending syndicates is a matter worthy of empirical investigation.

Hypothesis H4: BITs affect the size of the lending syndicate.

Lenders have other non-price means to mitigate loan risk. Among these, the use of collateral in debt contracts has been justified as a way to mitigate information asymmetry between borrowers and lenders. Collateral solves adverse selection problems, as the willingness to provide collateral serves as a credible signal of borrower quality, and mitigates moral hazard, as borrowers can credibly commit to lower asset substitution by providing collateral. Consistently, Berger and Udell (1990) find that collateral is associated with higher levels of risk--at the borrower, lender, and loan levels. Similarly, collateral could mitigate the moral hazard problem faced by a government, as it deters loan defaults following nationalizations (provided lenders can indeed seize such collateral). Accordingly, we expect more frequent use of collateral when the risk of expropriation is higher, and, in turn, less frequent use of collateral in the presence of BITs. On the other hand, the value of collateral as a risk-mitigating tool is possibly affected by institutional quality. When creditor rights are weaker, the value of collateral declines, as it is harder for creditors to re-possess the assets used as collateral in a lending agreement: Qian and Strahan (2007) find that stronger creditor rights are associated with more frequent use of collateral. Similarly, given the risk of expropriation, the value of collateral might decline, due to the risk of it being seized by the government. According to this argument, we would expect lending agreements to rely less on collateral when the risk of expropriation is high and, conversely, loans covered by BITs to employ more collateral. Given these conflicting predictions, the link between BITs and collateral cannot easily be predicted but is worthy of empirical investigation.

Hypothesis H5: BITs affect the likelihood of the use of collateral.

Chava and Roberts (2008) discuss in detail the use of covenants to restrict the actions of borrowers

and to mitigate loan risk. Expropriation of creditors can occur—and often does occur—following a government takeover of the borrower (Caflisch, 1967 and Bederman, 2000). Covenants can, in this sense, restrict the actions of governments following nationalization of a borrower, thus providing protection to lenders. Accordingly, we hypothesize that covenants will be used more frequently and in greater number when the risk of expropriation is greater, and more sparingly in the presence of BITs.

Hypothesis H6: BITs lead to less frequent inclusion of covenants and to a lower number of covenants in lending agreements.

III. Data Sources, Descriptive Statistics and Univariate Analysis

A. Data Sources

The main source of data on syndicated loans used in this study is the Thomson Reuters Loan Pricing Corporation DealScan database ("DealScan"). DealScan includes loans, high-yield bonds, and private placement transactions spanning the globe. The version of the database used in this study covers loans initiated between January 1980 and December 2013. The database includes data on loan pricing, contract details, terms and conditions, and information on loan participants (borrower and lender identities). The loans are organized by "package" and by "facility." Each package represents a loosely-defined "deal" and may contain one or multiple facilities—on an average, there are approximately 1.5 loans in each package. All loans within the same package share the same borrower, but the identity of the lender, or composition of the lending syndicate, type of loan, loan initiation date, and other contract characteristics can all vary between loans from the same package.[21] For each loan, we obtain, as an estimate of cost to the borrower, the all-in-drawn spread (the total annual spread, including fees and interest, paid over LIBOR). We further record the loan maturity (at initiation, in months), the facility amount (in USD), the number of lenders, indicator variables identifying collateralized loans, and loans with financial covenants, and the number of

[21] Chava and Roberts (2008) describe the database extensively. Some recent empirical studies using data from this database include Güner (2006), Qian and Strahan (2007), Sufi (2009), Bae and Goyal (2009), and Haselmann and Wachtel (2010).

financial and general covenants. We also create indicator variables based on the database fields identifying "loan type," "loan purpose," and currency of denomination. [22]

We limit our sample to loans identified as "364-Day Facility," "Bridge Loan," "Term Loan" of all types, "Revolver line" of all maturities and "Other Loan," thus excluding not only bonds and private placements, but also cletters of credit and guarantees. We further exclude loans whose status is "Cancelled" or "Rumor." Further, we exclude all loans for which data on the composition of the lending syndicate is missing and loans with conflicting information (for example, loans marked as single-lender loans for which multiple lenders are listed). Following Qian and Strahan (2007) and Ivashina et al. (2008), we exclude loans to firms operating in the financial sector. In particular, we exclude all loans to depository institutions, non-depository credit institutions, security and commodity brokers, dealers, exchanges, and services, insurance carriers, insurance agents, brokers, and services (SIC codes 6000-6399). Finally, we include in the sample only cross-border loans. We identify loans as cross-border, or foreign, if the majority of syndicate participants are headquartered in a country different from the headquarter country of the borrower. While we include in our main sample all loans for which a majority of lenders are foreign, our results are robust to alternative inclusion criteria, including a more stringent filter in which we consider only loans for which all lenders are foreign. [23]

Accounting data for borrowing firms is obtained from the Thomson Financial Worldscope Global ("Worldscope") database. As DealScan identifies firms only by name and ticker symbol, matching between DealScan and Worldscope is based on company names; due to differences in spelling, much of the matching

[22] When measuring the concentration of a lending syndicate, extant studies often employ concentration indices based on the share of the loan held by each lender. Yet many non-US syndicates report scarce data on exact share allocations. Hence, existing studies examining syndicated lending in non-US markets, including Esty and Megginson (2003) and Qian and Strahan (2007), use the number of lenders in the syndicate as a proxy for how diffused lending is, assuming that a larger lending syndicate implies less concentrated lending. We adopt the same approach.

[23] To the extent that domestic borrowers could play a (minority) role in the lending syndicate in our sample, our results are conservative, as the presence of domestic lenders is likely to mitigate the risk of expropriations. Further, our results are robust to alternative definitions of "foreign loans." In unreported robustness tests, we label as leaders all lenders classified in the Dealscan database as either "administrative agent," "agent," "arranger," "bookrunner," "lead arranger," "lead bank," or "lead manager," as in Ivashina (2009). If the headquarters of the leader are in the same country as those of the borrower, the leader is classified as "domestic"; otherwise, the leader is classified as "foreign." In this robustness test, we classify the loan as "foreign" if all lead lenders are "foreign."

is manual. Out of a total of 66,730 borrowers in the sample, we successfully match 18,347 firms between DealScan and Worldscope.[24] To prevent possible endogeneity issues, we retrieve accounting data for the borrower as of December 31 of the year preceding loan initiation. Data on yearly GDP per capita and GDP growth by country is from the World Bank.

All variables measured in monetary units (such as loan size and firm's total assets) are in USD, adjusted to purchasing power parity in the year 2013, using the CPI Index (CPI-U) by the United States Department of Labor, Bureau of Labor Statistics. All continuous variables are winsorized at the first and ninety-ninth percentile to mitigate the impact of outliers.

B. Bilateral Investment Treaties

We compile data on BITs from information provided by the United National Conference on Trade and Development (UNCTAD).[25] UNCTAD offers data on 2,808 BITs, of which 2,107 are still in operation, spanning 152 countries. The raw data includes, for each treaty, dates of agreement and implementation, starting in 1959 and ending in 2013, and identifies the two countries that signed the treaty.

From the data, we construct a binary variable for each country pair and for each year, set equal to one if a treaty is in force, so for each year between the year of implementation and, when available, the year of treaty withdrawal, or through 2013 otherwise. We find, for each country, an average of 35 treaties in force in any given year. For each loan, we check whether the country of lender headquarters and country of borrower headquarters are signatories of a BIT at the time of loan initiation. If any of the lender countries satisfies the condition, we label the loan as a "BIT loan." Anecdotal evidence is consistent with creditors shopping for favorable legal environments, while *pari passu* legal clauses mandate equal treatments of all creditors participating in a loan syndicate. Accordingly, the protections extended by a BIT to one of the

[24] By comparison, Bae and Goyal (2009) match 4,407 borrowers between the same two databases. Qian and Strahan (2007) engage in a similar exercise but do not reveal the exact number of matches—yet, their data description lists 4,322 loans for which they find borrower-level accounting data. Haselmann and Wachtel (2010) match approximately 7,000 firms between DealScan and Amadeus.

[25] The raw data is available in the "Country specific lists of Bilateral Investment Treaties" provided by UNCTAD at http://unctad.org/en/docs/poiteiiad2.en.pdf

lenders likely extend to all lenders in the same syndicate. Our main results are robust to alternative definitions of "BIT loan"—such as restricting the definition to loans in which the lead lenders are covered by BITs.

C. Expropriation Risk and Institutional Quality

As discussed in the Section I, we expect the risk-mitigating features of BITs to be more valuable, and have a stronger effect on loan terms, the higher the risk of expropriation affecting foreign creditors. Unfortunately, the rights of foreign creditors (as opposed to foreign equity investors) in international law have received little attention in existing research. Accordingly, we are unable to isolate expropriation events affecting creditors specifically. To create a proxy, we obtain data on expropriations of foreign investors from the dataset described in Kobrin (1982, 1984). The dataset spans the years 1960 to 1979 and reports 563 expropriation episodes affecting a total of 1,685 firms in 79 countries. For each expropriation, the dataset includes the name of the country, the year, the sector and industry of the affected firms, and the number of firms affected. From this data, we construct a country-level binary variable, *Expropriation*, equal to one if the country was subject to at least one expropriation episode during the period 1960-1979, and zero otherwise. We further construct a second country-level metric, *Number of expropriations*, equal to the number of expropriation episodes affecting the country during the same time interval. We construct a third country-level metric, *Number of expropriated firms*, equal to the number of firms expropriated within the country during the same time interval. We code the *Number of expropriations* and *Number of expropriated firms* as zero for countries that have never experienced any expropriation episode in the period 1960-1979 and are thus not included in the Kobrin (1982 and 1984) dataset.

As an additional metric of institutional quality, we employ a risk index derived from the property rights index of Djankov, McLiesh, and Shleifer (2007)—we label this variable *Creditor rights risk*.[26] The

[26] Djankov, McLiesh, and Shleifer (2007) develop an index of protection of creditor rights for 129 countries. As their index is available only until 2004, we use year-2004 values for subsequent years. We are reassured in our choice by the fact that, as Djankov, McLiesh, and Shleifer (2007) note in their own analysis, the index displays very little time-series variation, while revealing substantial cross-country differences. Further, as we are interested in a proxy for risk (of expropriation of creditors), rather than a protection index, we use an inverse scale: our country-year index of

index by Djankov, McLiesh, and Shleifer (2007) measures the strength of creditor rights in bankruptcy proceedings and, as such, it is best interpreted as a measure of the quality of the contracting environment, rather than property rights. We note that the coefficient of correlation (unreported) between the *Number of expropriations* and the index of creditor rights is positive, but low (0.0035).

In robustness tests, we employ alternative metrics of institutional strength. First, we use a variable identifying weak democratic institutions and a lack of constraints on the actions of government officials derived from the Polity IV Project database (*Autocracy vs. democracy*). In particular, for each country-year, we compute the average difference between the "Autocracy" and "Democracy" scores over the previous ten years. A similar metric has been widely used in the "law and finance" literature, as in Ayyagari, Demirgüç-Kunt, and Maksimovic (2006) and Rodrik and Wacziarg (2005).[27] We use a ten-year average score following Glaeser et al. (2004) and Acemoglu and Johnson (2005), who lament excessive short-term fluctuation in institutional-quality metrics, particularly those from the Polity IV database, and emphasize the need to measure permanent, or at least durable, traits. Further, when country-level values are missing in the Polity IV database, we use regional averages as a proxy.

As an additional metric of institutional quality, we construct a variable identifying a lack of constraints on the actions of government officials (*Weak executive constraints*). We use data derived from the Polity IV Project database; in particular, we construct our metric by starting from the Polity variable *Executive constraints*. Since this is measured on a scale of 1 to 7, with higher values identifying higher constraints, we invert the scale by subtracting the relevant country-year score from 8. We again use a ten-year average score in our analysis and regional averages to replace missing country-level data points.

creditor rights risk is computed as $1/(1+D_{i,t})$, where $D_{i,t}$ is the Djankov, McLiesh, and Shleifer (2007) index for country *i* in year *t*.

[27] In the extant literature, the variable is usually computed as the difference between the "Democracy" and "Autocracy" scores, rather than the difference between the "Autocracy" and "Democracy" scores. Our approach differs from this literature as we wish to have a measure of risk, with higher scores indicating higher property rights risk, for ease of interpretation.

Finally, we include an *Investment profile risk* score derived from the *Investment profile* index published by the International Country Risk Guide (ICRG).[28] The index is coded on a twelve-point scale (1-12) with higher scores indicating a more business-favorable legal environment. The index is based on three sub-components, measuring the risk of contract non-viability or expropriation, restrictions on profit repatriation, and payment delays. We subtract the index from 13, to obtain a metric with higher values indicating higher levels of risk for investors. All variable definitions are summarized in Table 1.

***** Insert Table 1 about here *****

D. Sample Descriptive Statistics

Descriptive statistics for all variables are reported in Table 2. The table includes, in Panel A, means for two data subsets (loans covered by a BIT and loans not covered by a BIT) and two-sample *t*-tests for differences in means. We find that BIT loans have lower spreads than non-BIT loans (176 vs. 195 bps), longer maturities (61 vs. 59 months), larger lending syndicates (9 vs. 6.6 lenders), less frequent usage of collateral (24% vs. 26% of the number of loans), less frequent usage of financial covenants (5% vs. 11%) and a lower number of both financial (0.09 vs. 0.24) and general covenants (1.07 vs. 1.47).[29] Further, BIT loans are smaller in size compared to non-BIT loans (USD 517 million vs. USD 572 million). In terms of firm characteristics, we note that borrowers receiving BIT loans are larger firms, with lower leverage and higher liquidity, compared to borrowers receiving non-BIT loans. We find no statistically significant differences in valuation (as measured by *Tobin's Q*), profitability (*ROA*), or distress level (*Altman Z*). In terms of country characteristics, BIT loans are to borrowers from countries with lower creditor rights risk (as measured by the index by Djankov, McLiesh, and Shleifer, 2007), but with weaker constraints on the executive, weaker democratic institutions, and higher investment profile risk. Most importantly, BIT loans

[28] ICRG data have been used in numerous extant studies, including Bae and Goyal (2009).

[29] The proportion of firms with financial covenants and the average number of financial and operating covenants are somewhat low, compared to that documented in extant empirical studies of syndicated loans in the United States. They are, however, comparable to previous studies of syndicated lending to non-US borrowers. For example, Kim, Tsui, and Yi (2011) find, in a sample of non-US borrowers, that 12% of loans include financial covenants, which is very close to our estimate of 11.25% for non-BIT loans.

are more frequently extended to borrowers from countries that have experienced expropriations during the period 1960-1979 (31% of BIT loans vs. 8% of non-BIT loans) and countries with a larger number of expropriation episodes over the same time period (3 vs. 0.5 episodes).

We further describe the distribution of loans by country, year, and industry, respectively, in Panels B, C, and D of Table 2. We note that the loans to US-based borrowers account for approximately one-third of the sample and that, overall, countries with strong institutions account for a large portion of the sample. In this sense, our tests are likely conservative, as BITs are potentially more valuable in environments with higher ex-ante risk of government expropriation. Temporally, our sample is tilted towards more recent years, reflecting the recent growth in the syndicated loan market. While approximately half of all loans originate after 2005, our sample contains almost 2,000 loans per year, on average, between 1995 and 2005. In terms of industrial distribution, our sample is dominated by borrowers whose primary industrial classification is manufacturing (approximately 28% of the sample).[30]

While the overall sample contains 45,255 loans, for an aggregate value of USD 20.74 trillion, data on loan terms and borrower characteristics are incomplete. While the univariate two-sample *t*-tests presented above suggest that BITs are associated with cheaper loans and less restrictive loan terms (longer maturities, less collateral, and fewer covenants) in this large sample, in the following sections we will rely on matched tests and regression analyses, both of which require complete data. Accordingly, in Panel E, we present descriptive statistics for a sample subset with complete loan-level data (with no missing data points). We have complete loan-level data for 4,768 loans, worth approximately USD 4.59 trillion, of which 1,201 are covered by BITs. Compared to the overall sample, these loans are larger and have lower spreads, shorter maturities, larger lending syndicates, as well as fewer covenants and less frequent use of collateral. Despite those differences, the core differences between BIT and non-BIT loans persist. BIT loans are

[30] While we exclude loans to the financial sector, 8.72% of the loans are to firms classified as in the "finance, insurance, and real estate" sector: these are loans to real estate firms, which are included in the sample.

cheaper (but the discount is smaller in magnitude, at approximately 8bp), with longer maturities, larger syndicates, less frequent collateral usage, and fewer covenants.

***** Insert Table 2 about here *****

IV. Empirical Analysis

The univariate comparison of BIT and non-BIT loans presented in the previous section gives a first glimpse into the differences in both price and non-price terms, yet it fails to account for country and borrower characteristics that could be related to such differences. The same comparison reveals substantial differences between countries with and without BIT loans, which suggests that confounding effects could be at play. Accordingly, we compare loan characteristics after matching BIT loans to "similar" non-BIT loans in the following sections.

A. Matched Tests: Same Borrower Country and Industry

As a first matching procedure, for each BIT loan, we identify non-BIT loans sharing the same "loan purpose" and "loan type" to borrowers headquartered in the same country and operating in the same industry (as identified by the first two digits of the borrower's SIC code).[31] We select, as a match, the loan with the closest initiation date (within a maximum range of one year). Means of the BIT-loan sample and matched sample, mean differences, and tests for significance are reported in Table 3, Panel A. Our analysis covers the 1,201 BIT loans with complete data, but we do not obtain matches with complete data for every BIT loan. Accordingly, the sample we investigate here contains 1,085 loan pairs. Differences in means are tested by two-sample *t*-tests, with standard errors clustered at the borrowing firm level. We find that the mean spread on BIT (matched) loans is 158.64 bp (179.74 bp) and the estimated discount associated with BITs is 21.11 bp, which is consistent with our hypothesis that BITs lead to a lower cost of debt. BIT loans further display very similar maturity (61.49 vs. 60.52 months), contrary to our expectations, and larger mean loan

[31] When there are no loans fitting this criteria, we relax the matching procedure to one-digit SIC codes. Our results are robust to the exclusion of those loans from the sample.

size (USD 1,173.74 million vs. USD 783.07 million). BIT loans are further associated with larger lending syndicates, with a mean of 13.68 participants, compared to 7.24 participants for non-BIT loans. All differences are statistically significant at the 1% level. BIT loans are also associated with less frequent use of collateral (19.54% of loans vs. 25.62%). Similarly, BIT loans are associated with less frequent inclusion of financial covenants (11.34% of loans vs. 13.82%) and fewer financial covenants (0.23 vs. 0.28) and the results are significant at the 5% level, suggesting that, with stronger property rights, lenders decrease their reliance on non-price risk mitigating loan terms. We find no statistically significant differences in the number of general covenants nor in loan maturity.

B. Matched Tests: Same Borrower Country and Same Lender Country

Our initial setting involves comparing loans originating from different lender countries. In this sense, while we control for borrower-country characteristics through matching, lender-country characteristics are omitted. It is possible that lender-country characteristics influence the propensity and mode of lending to borrowers from a specific foreign country. For example, extant literature finds that foreign investments are affected by country-level factors such as geographic proximity, cultural affinity, differences in legal system development, colonial ties, and other factors. Regulatory differences, such as capitalization requirements, could also incentivize lenders from a particular country to prefer certain loan types or loan characteristics; these country-level factors, if omitted, could confound our findings. Accordingly, in a second set of tests, we compare BIT loans to loans with the same country pair (we match by both lender and borrower countries), initiated prior to the signing of the relevant BIT. As before, we match BIT loans to non-BIT loans sharing the same "loan purpose" and "loan type," with borrowers headquartered in the same country and operating in the same industry. While we inevitably match BIT loans to loans initiated during a different time period (prior to the BIT signing), we mitigate the difference in calendar time by choosing the non-BIT loan satisfying the above requirements and with the closest starting date. We obtain 892 usable loan pairs. Means of the BIT loan sample and matched sample, mean differences, and tests for significance are reported in Table 3, Panel B. This alternative test based on across-time matching leads to findings that are qualitatively very similar to those obtained previously on across-

lender-country matching. For BIT loans, the estimated discount is 34.45 bp, facilities are significantly larger, and loan syndicates are more diffuse (with a larger number of lenders). Further, we find less frequent use of collateral and fewer financial and general covenants. These findings confirm that the observed results are not driven by lender-country factors, as both price and non-price terms of loans originating from the same lender countries are affected by the signing of a BIT.

C. Matched Tests: Same Borrower

In a third test, we identify benchmark loans to the same borrower, matched by loan purpose and with the closest facility start date. While this matching procedure has the advantage of offering the best controls for borrower-level characteristics, the lack of a sufficient number of observations forces us to relax our previous matching criteria in terms of loan type and loan initiation year. Even after relaxing matching criteria, we obtain only 704 usable loan pairs. Means of the BIT loan sample and matched sample, mean differences, and tests for significance are reported in Table 3, Panel C.

While not being able to match on loan type is adding noise to the estimate of differences in loan terms, we still find a statistically significant discount in the cost of BIT loans (about 12.79 bp), larger loans, and larger lending syndicates. We do not, however, find statistically significant differences in loan maturity or in the frequency of using collateral or financial covenants. Nevertheless, this last set of results confirms that BITs are associated with a statistically and economically significant discount in the cost of loans.

C. Matched Tests: Propensity-Score Matching

We recognize that certain borrowers are more likely to seek BIT coverage—presumably, those that are at high risk of expropriation and that might, accordingly, most benefit from BIT protection. Accordingly, borrower and host-country characteristics might drive BIT coverage, leading to possibly spurious findings. To mitigate this possible empirical problem, we identify a propensity-score matched sample of loans sharing similar country and borrower characteristics.[32] For each transaction, we compute

[32] Propensity-score matching is widely used in both the economics and finance literature. Examples of applications in empirical research in corporate finance include Mikkelson and Partch (2003), Lowry, Officer, and Schwert (2010), and Irani and Oesch (2013).

the "propensity score" $\hat{p}$ as an estimate of the conditional probability of finding that the loan is covered by a BIT. Probability estimates are derived from a probit model in which the binary variable *BIT* is regressed on country and borrower characteristics; we include a binary variable identifying "relationship loans," which are loans with country-lead lender pairs that appear in at least one other loan with an earlier initiation date.[33] We construct a propensity-score matched sample (labeled "PSM sample") identifying, for each BIT loan, the non-BIT loan with the closest (least absolute difference) propensity score. To ensure that the PSM sample is truly comparable, we only include matches with a maximum absolute difference in propensity score of 1 percentage point. We match with replacement, as recommended by Roberts and Whited (2012). After identifying 1,179 suitable matched observations, we estimate the mean differences in loan terms between BIT loans and matched loans. Results are reported in Table 3, Panel D.[34]

We find a 12.90 bp discount in the cost of loans associated with BITs, which is significant at the 5% level. In this test, we find evidence of longer loan maturity, by 3 months (but significance is only at the 10% level). On the other side, while loans are smaller, the result is not statistically significant. Finally, we confirm that lending syndicates are larger and that collateral and covenants are used more sparingly in the presence of BITs.

***** Insert Table 3 about here *****

E. *Matched Tests, Difference-in-Difference*

To verify that the channel by which BITs impact the cost of loans is by reducing the risk of government expropriation, we construct a second test. We note that, if our hypothesis is correct, the impact of a BIT on the cost of loans should be stronger if the risk of expropriation is greater *ex-ante*. Accordingly, we subset the sample into two sets of loans, respectively to borrowers in countries with and without prior

[33] Identifying relationship loans in a syndicated-loans setting is complicated by the presence of multiple lenders. We follow extant literature by considering the borrower and lead arranger pair and restrict the definition of "relationship loans" to loans for which we find a match (a previous loan with the same borrower-lead arranger pair) within a five-year window. The same approach is taken by Bharath et al. 2011.

[34] Results for the probit estimation are included in Appendix A. While we do not discuss these in detail, we note that BIT loans tend to be associated with riskier borrowers (smaller and less profitable borrowers, with lower liquidity and higher leverage, but also higher Tobin's Q) based in wealthier countries (higher GDP per capita).

expropriation episodes during the period 1960-1979, as identified by Kobrin (1982 and 1984).[35] Using the propensity-score matched sample, we find that loan spreads between BIT and matched loans are not statistically different in countries without prior expropriations: the means are, respectively, 102.16 and 100.45 bp. On the other hand, in countries with prior expropriations, BIT loans have an average spread of 139.50 bp, while non-BIT loans have an average spread of 179.29 bp. The estimated discount of 39.79 bp is economically and statistically significant (at 10%). The difference-in-difference test, comparing the impact of BITs in high-risk countries to the impact in low-risk countries, is highly statistically significant.

In a second test, we subset our dataset on the basis of a variable measuring the strength of democratic institutions. We posit that, in countries with weak democratic institutions, the risk of government expropriation is higher and, accordingly, we expect to find that BITs have a stronger impact. We consistenly find that BITs are associated with a statistically and economically significant discount of 33.95 bp. in autocratic countries (those with an *Autocracy vs democracy* score greater than the median) In contrast, the discount is of only 4.34 bp in democratic countries, and not statistically significant. The difference-in-difference estimator is highly statistically significant.

In a third test, we subset our dataset on the basis of a variable measuring the strength of executive constraints. We posit that, in countries with weak constraints on executives, the risk of government expropriation is higher and, accordingly, we expect to find that BITs have a stronger impact. Consistently, we find that, in countries with weak executive constraints (those with a *Weak executive constraints* score greater than the median), BITs are associated with a statistically and economically significant spread discount equal to 31.83 bp. In contrast, the discount is of only 6.99 bp in countries with strong executive constraints, and not statistically significant. The difference-in-difference estimator is highly statistically significant.

[35] We use prior expropriations as a proxy for the risk of expropriations as the probability of a contemporaneous expropriation would be affected by the presence of a BIT, as it is plausible that governments would be less likely to expropriate when investors have access to third-party arbitration courts.

In a final test, we subset our dataset on the basis of a variable measuring the "investment profile" of the country, as defined by ICRG. The metric itself is described as measuring how "investor-friendly" the legal system is and one of its subcomponents is a direct measure of the risk of expropriations. Accordingly, we posit that, in countries with a weak investment profile, the risk of government expropriation is higher and, accordingly, we expect to find that BITs have a stronger impact. We actually find that BITs are associated with a discount in the cost of loans in both data subsets but the difference-in-difference estimator is not statistically significant—and the discount is of a larger magnitude in the low investment profile risk data subset. While we do not test further why the separation by investment profile does not produce the expected results, we conjecture it is likely due to the fact that the variable is a particularly noisy metric of the risk of expropriations and suffers from the measurement problems discussed in Glaeser et al. (2004).

For brevity, we report only results related to spreads in these difference-in-difference tests. Analyzing the impact of BITs on the non-price terms of loans reveals that lenders adopt different risk-mitigating tools depending on the *ex-ante* risk of expropriations. In countries with a high risk of expropriations, lenders charge higher spreads, shorten loan maturities, ration capital by restricting the size of loans, and form more concentrated lending syndicates. All of these effects are mitigated by the presence of BITs. The results are particularly dramatic as they pertain to loan maturity: loan maturity increases by almost a full year in the presence of BITs in countries with prior expropriations, but by only three months in countries without prior expropriations.

***** Insert Table 4 about here *****

F. *Regression Analysis of Loan Spreads*

In the following section, we model loan spreads in a regression framework, which allows us to estimate whether the impact of BIT on spreads depends on the risk of creditor expropriations, while controlling for relevant country and firm characteristics affecting loan spreads. The response variable is the "all-in-drawn" spread, as a proxy for the total cost of the loan. The variable of interest is the binary variable indicating that the loan is covered by a BIT (*BIT*). In the selection of control variables, we mirror, as far as data availability permits, Qian and Strahan (2007) and include firm-level characteristics that have been

shown to have an impact on the cost of borrowing: size (log of total assets), leverage (debt-to-asset ratio), profitability (return on assets), valuation (market-to-book ratio), liquidity (cash over total assets), and bankruptcy risk (Altman Z-score). We further include country-level controls: a proxy for the state of the economy, likely related to loan demand (GDP growth), and a proxy for the overall level of economic development (GDP per capita). We also include fixed effects for loan seniority, currency, starting year, borrower industry (first digit of the SIC code), loan type, and loan purpose. Importantly, we control for the strength of the protection of creditor rights with a risk index derived from the property rights index by Djankov, McLiesh, and Shleifer (2007), *Creditor rights risk*. We further include a binary variable (*Expropriation*) equal to one for borrower countries with a past history of expropriations and its interaction with *BIT*. Results are reported in Table 5.

We find that BITs are associated with a spread discount of 16.61 bp. In countries with past expropriations, spreads are higher by 54.91 bp. Most interestingly, the coefficient estimate associated with the interaction between *BIT* and *Expropriation* is negative and equal to 45.82 bp, indicating that the presence of a BIT partially offsets the impact of past expropriations, and that BITs are more valuable when the *ex-ante* risk of expropriations is greater, as hypothesized. All of the results cited are statistically significant at the 1% level or better. While we do not discuss control variables in detail, we note that the signs and magnitudes of coefficients are consistent with extant literature on syndicated loan pricing. Importantly, we verify that spreads are positively related to *Creditor rights risk*, in line with Qian and Strahan (2007) and Bae and Goyal (2009). Our findings persist after including this variable, which indicates that the risk of government expropriation is not captured by the strength of protection of creditor rights and that the two metrics are not both expressions of some over-arching measure of "institutional quality."

In a second specification, we replace the binary variable identifying countries with past expropriations with the natural logarithm of the count of the number of past expropriations, by country. In this model, we find consistent results, as loan spreads are positively related to the number of prior expropriation episodes, but the result is partially offset by the interaction between BITs and the number of prior expropriations. Roughly, a one-percent increase in the number of expropriations leads to a 19.92 bp

increase in the cost of loans, but the presence of a BIT largely offsets this increase (the interaction coefficient is -18.71 bp); results are statistically significant at the 5% level or lower.

In a third specification, we replace the binary variable identifying countries with past expropriations with the natural logarithm of the count of the number of firms affected by past expropriations, by country. Our results are largely consistent with previous findings. A one-percent increase in the number of expropriated firms leads to a 14.24 bp increase in loan spreads, largely negated by the presence of BITs (the interaction coefficient is -13.47 bp).

The models so far presented do not include controls for other non-price terms of loans--such as maturity, loan value, size of the lending syndicate, use of collateral, or number of covenants--for two reasons. First, we recognize that such loan terms are likely to be determined simultaneously with loan spreads, thus creating possible estimation issues in our models. Second, we aim at consistency with extant literature, especially Qian and Strahan (2007) and the large number of subsequent studies that adopted the same loan pricing models. Yet, as a robustness test, we add non-price loan terms as control variables in a fifth model and find that our results are largely unaffected, although the magnitude of the spread discount associated with BITs is slightly smaller (11.08bp). In Section IV.C, we address the simultaneous determination of loan terms using a model proposed by Bharath et al. (2011).

***** Insert Table 5 about here *****

D. Regression Analysis of Non-Price Loan Terms

As in Qian and Strahan (2007), we model the non-price terms of the loan in a regression framework akin to the first model presented in the previous section—with the *BIT* and *Expropriation* binary variables and their interaction as the main variables of interest. Results are presented in Table 6.

We find that loan maturity is negatively related to a past history of expropriations: prior expropriations reduce loan maturity by almost a full year (11.68 months). Yet this impact is mitigated by the presence of a BIT, as the positive interaction coefficient between *BIT* and the *Expropriation* binary variable of 6.94 (significant at the 10% level) indicates that loans covered by BITs suffer a much smaller shortening of maturity (less than five months). This positive association between loan maturity and BITs is

as hypothesized and is consistent with the relation between maturity and institutional quality documented by Qian and Strahan (2007) and Bae and Goyal (2009). Coefficient estimates indicate that BITs are associated with larger loans, but results are not statistically significant. Further, we confirm that BITs are associated with larger lending syndicates. We also confirm that BITs are associated with fewer financial and general covenants.[36]

***** Insert Table 6 about here *****

V. Additional Robustness Tests

A. Lenders and Borrowers Seeking BIT Coverage

As previous mentioned, a loan being covered by a BIT is not a random event. Rather, creditors or borrowers facing a higher risk of expropriation might seek BIT coverage. Accordingly, unobserved determinants of loan coverage by BITs could be inducing a spurious correlation between the *BIT* binary variable and the cost of the loan in the regressions in Table 5. To negate, or at least to mitigate, such concerns, we estimate coefficients by a weighted-least-squares procedure in which the weights are inversely proportional to the probability of a loan being covered by a BIT (or not covered by a BIT, if in the "benchmark" portion of the sample).[37] For each transaction, we compute the "propensity score" $\hat{p}$ as an estimate of the conditional probability of the loan being covered by a BIT. Probability estimates are derived from a probit model in which the binary variable *BIT* is regressed on country and borrower characteristics; we further add a binary variable identifying pre-existing relationships between borrowers and lead arrangers.[38] A BIT loan receives a weight $w_{SWF} = 1/\hat{p}$, while a non-BIT loan receives a weight $w_{BEN} = 1/(1 - \hat{p})$. Using this weighting methodology, we replicate the estimation of the regression models of loan

[36] We note that the number of financial covenants is, somewhat surprisingly, negatively related to past expropriation episodes. Also, the interaction between BITs and past expropriation episodes is positive in both regressions predicting the number of financial covenants and the number of general covenants. We do not include regressions modelling the use of collateral, as the binary response variable would require the estimation of a probit model, which leads to well-known estimation problems for interactions of binary variables (such as *Expropriation* × *BIT*).

[37] Caliendo and Kopeinig (2008) discuss applications of propensity-score weighting techniques and the related literature.

[38] Results for the probit estimation are included in Appendix B.

spreads in Table 5 and present the results in Table 7. Our main findings are largely unaffected. In all models, BITs are associated with a discount in spreads (ranging between 10.03 bp and 12.05 bp). Past expropriations are related to higher spreads, but the impact is mitigated, although not completely offset, by BITs.

***** Insert Table 7 about here *****

As a second robustness test addressing the endogenous matching of borrowers and lenders, we replicate our regression analysis using a subsample of loans in which the borrower-lender pair predates BIT coverage. The main idea is that if a borrower has obtained loans with no BIT coverage from a specific lender, the decision to borrow from the same lender at some point in the future is likely driven by the past relationship (rather than by BIT coverage). In other words, this sample is less likely to suffer from issue related to the potentially endogenous decision by borrowers to rely on a specific bank (a bank with BIT coverage, versus a bank without BIT coverage), as such a decision was made in the past. Accordingly, we identify sets of "relationship loans"—with each set including loans with the same borrower-lead arranger pair.[39] For each set of relationship loans, we identify the loan with the earliest initiation date; if that loan does not include BIT coverage, we keep all subsequent loans (both BIT loans and non-BIT loans) from the same set in our sample. We accordingly construct a data subset including 1,901 relationship loans for which the relationship between lead lender and borrower predates BIT coverage. We replicate our regressions in this subsample, finding robust results: BITs are associated with lower spreads, especially in countries with a past history of expropriations. Results are presented in the first three columns of Table 8.

***** Insert Table 8 about here *****

B. BITs are Signed at Non-Random Times, by Non-Random Countries

We note that the timing of BIT signing by specific countries could be non-random. That is, countries might sign BITs during specific economic conditions, as domestic firms require access to foreign lenders (or equity investors). This non-random timing and country selection might lead to omitted variable

[39] While we employ the same methodology described previously to identify "relationship loans," we do not here constrain our analysis to a five-year window.

bias (if control variables do not fully capture economic conditions) and possibly spurious findings. To mitigate such concerns, we add country-year fixed effects to our regression models, thus controlling for time-variant country-level characteristics. We cannot include in this model variables measuring the ex-ante risk of expropriations, as those would be absorbed by the country-year fixed effects (which motivates our decision not to include country-year fixed effects in our main analysis). We present our findings in Table 8, in the fourth column. BITs are associated with a discount in spreads, of about 11 bp. While the estimated magnitude of the effect is smaller than in previous tests, the result is highly statistically significant.

As an additional robustness test aimed at mitigating the impact of non-random BIT timing, we estimate the impact of BITs on loan spreads using a two-stage Heckman (1979) procedure based on Jandhyala and Weiner (2014), which in turn relies on models of FDI based on Carr, Markusen, and Maskus (2001). The first stage is a probit model, used to estimate the likelihood of two countries being BIT signatories. In this model, explanatory variables are based on country pairs. First, the model includes the difference in wealth between borrower and lender countries (measured by both the absolute and squared difference in per capita GDP, as in the papers referenced above). A second explanatory variable is the aggregate size of the two economies, proxing for the potential for mutual investment. A third variable is a proxy for the worldwide adoption of BITs, the total number of BITs as of the year of interest. A fourth explanatory variable is a metric of the geographical distance between the countries, as a proxy for extant trade. The final explanatory variable is the average political constraint scores of the two countries. All variables are as of the year of loan initiation. The first-stage results are included in Appendix (Table B2). While we do not discuss those results in detail, they are overall consistent with our expectations, indicating that BIT coverage is positively related to political constrains, to the overall number of BITs signed worldwide, to the distance between two countries, and to the aggregate size of the economies. The absolute difference in the size of the economies is positively related to BIT coverage, but the squared difference is negatively related to BIT coverage, indicating a non-linear relationship. The second-stage results are reported in Table 9. The inverse Mills' ratio is highly significant in all spread regressions, indicating that selection bias does play a role in estimating the relation between spreads and BITs. Nevertheless, all results

are consistent, both in terms of statistical significance and economic magnitude, with our previous findings. Loan spreads are lower in countries with BITs, but higher in countries with prior expropriations and increasing in the number of expropriations. The impact of prior expropriations is largely mitigated, but not completely negated, by the signing of BITs.

***** Insert Table 9 about here *****

We note that other empirical results we document rule out the possibility of BIT timing driving our findings. First, previously discussed matched-sample analysis includes a control sample matched on both borrower country and loan initiation year. Second, the significant interaction between BITs and ex-ante risk of expropriations cannot be easily attributed to endogenous timing.[40] Third, we include time-variant country characteristics in computing propensity-score weights, which mitigates possible endogeneity concerns. Finally, we note two theoretical arguments that indicate that endogenous timing is not likely to drive our findings. First, while the timing of BIT signing might be driven by current economic conditions, there is often a long lag (often between two to three years, as discussed by Sachs and Sauvant, 2009) between BIT signing and implementation—and our tests rely on the BIT implementation date. Finally, Vandevelde (2009) finds that the signing of BITs is not driven by economic conditions as much as by political shocks and changes in ruling ideology.

C. Joint Determination of Loan Price and Non-Price Terms

We recognize that price and non-price loan terms are likely to be co-determined, as they are often the result of a complex negotiation between borrowers and loan arrangers. Bharath et al. (2011) address the join determination of loan contract terms—loan spreads, loan maturity, and loan collateral requirements.

[40] The concern is that BITs might be signed during times of high economic growth—and that such growth could lead to loans being perceived as safer and thus result in lower spreads. If such economic growth is not fully captured by control variables, we could observe a spurious correlation between BITs and spreads. While our previous tests including country-year fixed effects rule this possibility out, we further note that such an explanation, while plausible, would fail to account for the finding that BITs are particularly effective in countries with a high *ex-ante* risk of expropriation.

They do so by employing an instrumental variable (IV) framework.[41] Following Dennis, Nandy, and Sharpe (2000), Bharath et al. (2011) assume a unidirectional relationship between the spread and the non-price terms of loans. That is, while maturity and collateral affect each other, loan spreads are only affected by maturity and collateral (but do not, in turn, affect non-price terms). As Bharath et al. (2011) extensively discuss, this is consistent with most syndication processes, as lead banks tend to first negotiate non-price terms with borrowers, then set interest rates on loans in discussion with other potential syndicate members. We employ a similar two stages least squares instrumental variable framework and, as in Bharath et al. (2011), , we use two instruments for loan maturity to achieve identification in this model. First, we compute *Asset maturity* as in Barclay, Marx, and Smith (2003), following the intuition that longer life assets are likely to be financed by longer term debt (Hart and Moore, 1994). Second, we add a variable identifying regulated industries, as in Barclay and Smith (1995), due to the fact that higher regulatory oversight should reduce agency costs of debt and lead to longer maturity. As an instrument for loan collateral, we use *Loan concentration*, the ratio of that loan facility amount to the total firm's debt. As discussed by Bharath et al. (2011), this is consistent with the finding that if a particular loan facility is a large portion of a firm's debt, it is more likely to be secured (as in Berger and Udell, 1990; Boot, Thakor and Udell, 1991; and Dennis, Nandy and Sharpe, 2000). Finally, we use the average all-in-drawn spread on loans over the previous six months as an instrument for loan spreads. As Bharath et al. (2011) discuss, it is not likely that the past average spread would affect the non-pricing terms of the particular loan.[42]

We present our findings in Table 10. We include, for brevity, only results pertaining to the spread equations. In this set of tests, we do not find evidence of a direct link between BITs and loan costs in the absence of prior expropriations. Yet we confirm once more that, while expropriations lead to higher loan spreads, the impact is mitigated by the interaction between expropriations and BITs. These results hold

[41] We provide an overview of the model in this section. For additional detail on the estimation of this simultaneous equation system and tests for instrument validity, we refer interested readers to Bharath et al. (2011).

[42] Since we use an international sample while Bharath et al. (2011) focus only on the U.S., we estimate the average spread using a sample of loans to borrowers headquartered within the same country.

whether we identify expropriations with a binary variable or with a count of individual acts or with a count of the number of firms affected. We find no impact associated with creditor rights, suggesting that BITs act by mitigating property rights risk, rather than by improving the general contracting environment.

***** Insert Table 10 about here *****

D. Other Metrics of Institutional Quality

In additional robustness tests, we control for other metrics of strength of institutions that have been previously employed in extant literature. We use, as a starting point, the base model in which our main explanatory variables of interest are *BIT* and the *Expropriation* binary variables, and their interaction. In the first model presented in Table 11, we add the variable *Investment profile risk*. In the second model, we add *Autocracy vs. democracy*. In the third model, we add *Weak executive constraints*, constructed as discussed in Section IV.E. In all cases, our main findings are confirmed: BITs are associated with a spread discount, between 14.43 bp and 16.19 bp, expropriations are associated with higher spreads, and the impact of prior expropriations is largely but not completely offset by BITs. In unreported results, we find that the results regarding our main variables of interest persist even after adding interaction terms between BITs and the same three additional metrics of institutional quality.

In an additional robustness test, we add a set of four binary variables identifying countries' legal origin.[43] As Qian and Strahan (2007) discuss in detail, extant literature has demonstrated that many aspects of legal protection and institutional efficiency which we do not directly consider are highly correlated with legal origin.[44] Accordingly, we interpret legal origin as a proxy for those omitted variables. Our main findings are confirmed, as BITs are associated with a spread discount (15.30 bp), expropriations are associated with higher spreads, and the impact of prior expropriations is partially offset by BITs.

[43] Our base case is common law legal origin. Our binary variables identify French, German, Scandinavian, and socialist legal origin.

[44] A good example, offered by Qian and Strahan (2007) is the finding by Stulz and Williamson (2003) that religion is an important determinant of credit protection, while Djankov, McLiesh, and Shleifer (2007) show that the relation between religion and creditor rights is subsumed by legal origin.

E. Recent Expropriations

In all of the analyses presented so far, the expropriation data employed is based on the dataset described by Kobrin (1982), which captures expropriation episodes until the year 1979. This design feature is deliberate. First, a measure of *ex-ante* risk of expropriations that captures country traits prior to the beginning of the period we study (out loan sample covers the years from 1980 onwards) suffers from fewer problems of endogeneity and reverse causality. Second, as most BITs are signed after 1980, such a measure of the risk of expropriations is orthogonal to the presence of a BIT. Third, as Glaeser et al. (2004) and Acemoglu and Johnson (2005) emphasize, it is important to identify persistent country traits, rather than transitory characteristics related to recent changes in political regime. Nevertheless, we recognize that legal systems do evolve over time and hence aim to investigate our findings using more recent data in an additional robustness test. Accordingly, we extend the original dataset by identifying more recent expropriation episodes. We replicate, as far as possible, the methodology described in Kobrin (1982) and identify expropriation episodes spanning the years 1980 to 2013. The extended database contains a total of 773 expropriation episodes, affecting 2,073 firms in 112 countries. We construct a new variable, identifying the cumulative number of expropriation episodes, by country, up until the year of loan syndication. We employ this variable in additional robustness tests presented in Table 11. Our main findings are, once more, confirmed. BITs are associated with lower loan spreads, with a discount of 19.63 bp, while expropriation episodes lead to higher spreads; the impact of expropriations on spreads is mitigated by BITs.

***** Insert Table 11 about here *****

F. Loans Across Regions

We further recognize that the existence of a BIT between two countries might correlate with the presence of other links. For example, countries signing BITs might have stronger past trade links, or might have signed other treaties (such as multilateral trade agreements or double-taxation treaties). We note that such links are more likely to occur within, rather than across, world regions. Accordingly, as a robustness test, we identify a sample of foreign loans across world regions, to minimize the chance that other links

between countries are driving our findings.[45] Approximately half of the loans in our sample (5,274 BIT loans and 18,103 non-BIT loans) are across world regions. In unreported tests, we replicate all our tests and find that the core results are unaffected and that the estimated coefficients are of similar magnitude. While the main results retain statistical significance at least at the 10% level, the level of significance is often lower, likely due to the smaller sample size.

G. Loans Including Only Foreign Lenders in the Syndicate

In the analysis presented so far, we have defined as "foreign," and included in the sample, all loans in which the majority of the syndicate members are "foreign" (that is, with headquarters located in a country other than the country in which borrower headquarters are located). Accordingly, the sample analyzed in all tests includes loans with domestic lenders in the syndicate. To the extent that the risk of expropriations is more likely to affect foreign lenders, the presence of domestic lenders in the syndicate could mitigate the impact of weak property rights for all lenders. In this sense, our results are conservative, as we are more likely to observe an impact due to BITs in a sample including only foreign lenders. To verify our intuition, we replicate all the tests using a sample subset including only loans with all lenders being "foreign." For brevity, we do not report these robustness tests. For the most part, the estimated coefficients of interest are of greater magnitude, while significance levels of the results are largely unaffected; we do not formally test the statistical significance of differences in coefficient estimates. Overall, we observe that our results are robust to the use of a smaller sample including loans with only foreign lenders and broadly consistent with the hypothesis that the impact of BITs is even stronger for loans without any domestic participants.

VI. Conclusions

We utilize a novel empirical setting to investigate the impact of property rights protection against government expropriations on the price and non-price terms of foreign loans. By identifying foreign loans

[45] We define a loan to be "across-regions" if the country of headquarters of the borrower and lead arranger are located in different world regions. We follow the classification of countries by the World Bank, which considers the following world regions: East Asia and Pacific, Europe and Central Asia, Latin America and the Caribbean, Middle East and North Africa, South Asia, Northern America, and Sub-Saharan Africa.

covered by BITs, we are able to control for the impact of country and borrower characteristics on loan terms by means of simple matching procedures. Accordingly, we are able to isolate the impact of political risk on loan terms, offering a novel insight into the impact of the risk of expropriations on lending contracts.

Our findings indicate that property rights against government expropriation have an economically significant impact on both price and non-price terms of lending agreements—a novel finding, as extant literature on government expropriations has mostly focused on equity flows (foreign direct investment). In particular, stronger property rights associated with BITs lead to lower loan spreads, larger loans, larger lending syndicates, and fewer covenants. We also find some evidence of BITs being associated with longer loan maturity, but the results are not robust in all tests. We further find that expropriation acts by governments have a long-term impact on the cost of debt of private sector borrowers accessing foreign capital markets, but that the deleterious impact of expropriation events can be mitigated if governments are willing to surrender jurisdiction to foreign courts by means of bilateral investment treaties.

This manuscript, while offering insights into how loan contracts respond to the risk-mitigating function of BITs, ignores the question whether BITs lead to more lending in the first place. In broader terms, extant literature recognizes the empirical challenges of studying the impact of BITs on aggregate investment flows (equity or debt). It is, nevertheless, a question that warrants further investigation. Another limitation of our study lies in treating all BITs as offering equivalent protection against expropriations, without recognizing differences in terms and provisions.

Our research expands the literature on financial markets and legal institutions by investigating two concepts that have so far been overlooked by the empirical corporate finance literature: expropriations by governments and the risk-mitigating impact of bilateral investment treaties. Given their risk-mitigating properties and their large diffusion (BITs cover approximately one quarter of cross-border debt and equity financing), we believe BITs deserve further research. We further add to the political economy literature, which has long held that enforceable contracts between the state and individuals are not possible as governments are the ultimate guarantors of property rights. In contrast, we find evidence that enforceable contracts are feasible when governments surrender arbitration power to external parties.

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Table 1. Variable Definitions

Table 1 includes names, definitions, and sources of the main variables used in the analysis.

Variable Name	Definition	Source
Terms of the loans		
Maturity	Length of time during which the facility will be active from signing date to expiration date, in months.	Dealscan
Loan size	The size of the facility, in US dollars (millions, CPI adjusted to the year 2013). Includes committed funds in revolving loans.	Dealscan
Spread	All-in-drawn spread. Amount the borrower pays in basis points over LIBOR for each dollar drawn down. It includes the interest payment on the loan and any annual (or facility) fee paid to the bank group.	Dealscan
Six-months average spread	Average all-in-drawn spread for loans initiated during the previous six months.	Dealscan
Number of lenders	Number of participants (including lead arranger) in the facility.	Dealscan
Collateral	Binary variable, set equal to one if the facility is secured, zero otherwise.	Dealscan
Financial covenants	Binary variable, set equal to one if the facility includes financial covenants, zero otherwise.	Dealscan
Number of financial covenants	Number of financial covenants included in the loan terms.	Dealscan
Number of general covenants	Number of general covenants included in the loan terms.	Dealscan
Relationship	Binary variable, set equal to one if the borrower has received a loan arranged by the same lead arranger over the previous 5 years, or zero otherwise.	Dealscan
Expropriation		
Expropriation	Binary variable, set equal to one if the country has experienced an expropriation between 1960 and 1979, zero otherwise.	Kobrin (1984)
Number of expropriations	Number of expropriation acts in the country (1960-1979).	Kobrin (1984)
Number of expropriated firms	Number of firms affected by the expropriation acts in the country (1960-1979).	Kobrin (1984)
BIT	Binary variable, set equal to one if country of lead arranger headquarters has an active BIT with the country of the borrower headquarters, or 0 otherwise.	Unctad.org
Country characteristics		
GDP growth	Yearly percentage change in gross domestic product, measured in USD.	World Bank
GDP per capita	Gross domestic product divided by midyear population, in USD (thousands, CPI adjusted to the year 2013).	World Bank
Creditor rights risk	Creditor's right risk: 1/[1+(The creditor's right index from Djankov et al, 2007)].	Djankov et al. (2007)
Autocracy vs. democracy	Ten-year average of *Polity score*. *Polity score = Autocracy – Democracy*. Ranging from -10 (most democratic) to 10 (most autocratic).	Polity IV Project (Center for Systemic Peace)

<Table 1 continued>

Variable Name	Definition	Source
Country characteristics		
Weak executive constraints	Computed as 8 - (ten-year average of *Executive constraints*). *Executive constraints* ranges from 1 (unlimited authority to executives) to 7 (executive parity or subordination).	Polity IV Project (Center for Systemic Peace)
Investment profile risk	Computed as 13 - (*Investment profile*). *Investment profile* ranges from 0 (highest risk) to 12 (lowest risk).	International Country Risk Guide (ICRG)
Average political constraints score	Mean of *Polcon scores* between lender and borrower averaged for all county pairs in the facility. *Polcon score* ranges from 0 to 1; with higher scores indicating higher levels of political constraints.	Henisz (2000)
Abs difference in GDP per capita (log)	Absolute difference in GDP per capita between lender country and borrower country averaged for all country pairs in the facility.	World Bank
Sum of GDP (log)	Sum of GDP of lender country and borrower country averaged for all country pairs in the facility.	World Bank
Squared difference in GDP (log)	Square of difference in GDP between lender country and borrower country averaged for all country pairs in the facility.	World Bank
Total worldwide BITs	Total number of existing BITs worldwide.	Unctad.org
Distance	Geographic distance between lender country and borrower country averaged for all country pairs in the facility.	CEPII databases on bilateral distances
Borrower Characteristics		
Altman Z	1.2 (Net working capital/Total assets) +1.4 (Retained earnings/Total assets) +3.3 (Earnings before interest and taxes/Total Assets) +0.6 (Market value of equity/Book value of liabilities) +1.0 (Sales/Total assets).	Worldscope
DtoA	Total liabilities divided by total assets.	Worldscope
ROA	Net income (proportioned average total assets) at December 31 of the year preceding loan initiation.	Worldscope
TA	Total assets of the company, in USD (thousands, CPI adjusted to the year 2013).	Worldscope
TQ	Tobin's Q = (Market Value of Equity + Book Value of Debt)/(Book Value of Equity + Book Value of Debt).	Worldscope
Cash over TA	Interim cash flow per share during the year preceding loan initiation.	Worldscope

Table 2. Sample Descriptive Statistics

Table 2 presents, in Panel A, the mean and the number of observations for the variables listed in Table 1 for the subsamples of loans covered by BITs (1) and not covered by BITs (2), spanning 161 countries over 1980-2013. All variables are as defined in Table 1. Panel B includes the number of loans, the number of loans as a proportion of the overall sample, and aggregate loan value, by country. Panel C includes the same variables by year. Panel D includes the same variables by industry. Panel E includes the mean and the number of observations for all terms of the loans for the subsamples of loans covered by BITs (1) and not covered by BITs (2) with complete data. Tests for significance of mean differences are implemented as paired *t*-tests with standard errors clustered at the borrower firm and year level; levels of significance are two-sided. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

Panel A: Overall sample

	(1) BIT	Obs	(2) Non-BIT	Obs	Difference: (1)-(2)	*t*	
Terms of the loan							
Spread	176.20	5671	195.50	20173	-19.38	-8.07	***
Maturity (Months)	61.23	10811	59.02	28297	2.21	4.16	***
Loan size (USD M)	517.29	11689	572.33	33481	-55.05	-3.41	***
Number of lenders	9.09	11715	6.57	33540	2.52	29.62	***
Collateral (%)	23.88	11715	25.62	33540	-1.74	-3.75	***
Financial covenants (%)	4.75	11715	11.25	33540	-6.50	-20.68	***
Number of financial covenants	0.09	11715	0.24	33540	-0.14	-19.53	***
Number of general covenants	1.07	11715	1.47	33540	-0.41	-28.24	***
Expropriation							
Expropriation (%)	31.26	11715	7.70	33540	23.56	66.70	***
Number of expropriations	2.99	11715	0.54	33540	2.45	55.71	***
Number of expropriated firms	7.63	11715	1.17	33540	6.46	51.22	***
Country Characteristics							
GDP growth	4.80	9321	2.98	27390	1.82	51.25	***
GDP per capita (USD Th)	17.68	9330	39.56	27390	-21.87	-112.64	***
Creditor rights risk	0.37	11450	0.44	32224	-0.07	-36.89	***
Autocracy vs. democracy	-4.26	11695	-8.98	32695	4.72	102.43	***
Weak executive constraints	2.77	11695	1.34	32695	1.43	107.87	***
Investment profile risk	2.72	11643	2.26	32651	0.45	18.03	***
Borrower Characteristics							
Altman Z	2.70	2917	2.76	7375	-0.06	-0.36	
DtoA	0.59	3679	0.66	8955	-0.06	-2.49	**
ROA	7.14	3532	4.26	8637	2.87	1.54	
TA (USD Th)	20850.00	3680	18770.00	8957	2074.80	1.21	
TQ	1.51	3513	1.96	8630	-0.44	-0.91	
Cash over TA	29.52	3422	25.65	8282	3.86	8.79	***

Panel B: Loans by country

	Country	Number of loans	%	Loan value (USD Bn)		Country	Number of loans	%	Loan value (USD Bn)
1	USA	15288	33.78	6598.14	11	Singapore	796	1.76	178.38
2	United Kingdom	3698	8.17	2821.88	12	Indonesia	780	1.72	97.85
3	Canada	1987	4.39	888.30	13	Russia	757	1.67	354.56
4	France	1328	2.93	1394.48	14	Mexico	699	1.54	212.00
5	Hong Kong	1118	2.47	131.62	15	India	651	1.44	106.18
6	China	1087	2.40	113.17	16	Brazil	622	1.37	195.29
7	Netherlands	1079	2.38	763.86	17	Sweden	575	1.27	277.83
8	Germany	969	2.14	1329.08	18	Korea (South)	570	1.26	50.89
9	Australia	959	2.12	384.24	19	Thailand	546	1.21	53.09
10	Spain	938	2.07	639.58	20	Other	10808	23.88	4151.97

Panel C: Loans by year

Year	Number of loans	%	Loan amount (USD Bn)
year < 1990	902	1.99	343.05
1990 <= year < 1995	4090	9.04	958.36
1995 <= year < 2000	10217	22.58	2915.32
2000 <= year < 2005	8840	19.53	3880.98
2005 <= year < 2010	11149	24.64	6688.37
2010 <= year	10057	22.22	5956.31

Panel D: Loans by industry

Industry classification (NAICS)	Number of loans	%	Loan amount (USD Bn)
Agriculture, Forestry and Fishing	4341	9.59	791.33
Mining and Construction	5222	11.54	2584.17
Manufacturing	12670	28.00	6493.31
Transportation, Communications, Electric, Gas, and Sanitary Services	10703	23.65	5654.17
Wholesale and Retail Trade	3262	7.21	1470.88
Finance, Insurance, and Real Estate	3947	8.72	1272.03
Services	4369	9.65	1936.79

Public Administration	741	1.64	539.71	

Panel E: Sample with complete data

	(1) BIT	Obs	(2) Non-BIT	Obs	Difference: (1)-(2)	*t*	
Terms of the loan							
Spread	113.20	1201	121.40	3567	-8.21	-2.11	**
Maturity (Months)	51.48	1201	48.09	3567	3.39	3.18	***
Loan size (USD M)	1200.30	1201	1196.50	3567	3.80	0.05	
Number of lenders	13.71	1201	12.31	3567	1.40	3.99	***
Collateral (%)	12.16	1201	23.77	3567	-11.62	-8.64	***
Financial covenant (%)	9.66	1201	31.40	3567	-21.74	-15.23	***
Number of financial covenants	0.19	1201	0.65	3567	-0.46	-13.88	***
Number of general covenants	1.22	1201	2.30	3567	-1.08	-16.01	***
Expropriation							
Expropriation (%)	19.98	1201	3.25	3567	16.73	19.85	***
Number of expropriations	2.98	1201	0.24	3567	2.75	19.21	***
Number of expropriated firms	5.68	1201	0.39	3567	5.29	20.77	***
Country Characteristics							
GDP growth	4.80	1201	2.98	3567	1.82	51.25	***
GDP per capita (USD Th)	17.68	1201	39.56	3567	-21.87	-112.64	***
Creditor rights risk	0.39	1201	0.45	3567	-0.06	-9.83	***
Autocracy vs. Democracy	-6.42	1201	-9.60	3567	3.18	31.48	***
Constraint on executive	2.17	1201	1.14	3567	1.02	34.70	***
ICRG risk	1.65	1201	2.20	3567	-0.55	-7.45	***
Borrower Characteristics							
Altman's Z	2.56	1201	2.84	3567	-0.28	-2.46	**
DtoA	0.61	1201	0.65	3567	-0.04	-5.16	***
ROA	7.17	1201	5.55	3567	1.62	1.88	*
TA	21080.00	1201	14110.00	3567	6969.10	6.28	***
TQ	1.52	1201	1.75	3567	-0.24	-4.27	***
Cash over TA	28.05	1201	22.95	3567	5.11	7.44	***

Table 3. Price and Non-Price Loan Terms, Matched Samples

Table 3 compares means of variables related to price and non-price terms of loans covered by BITs to those of matched loans not covered by BITs. All variables are as defined in Table 1; the overall population from which loans are matched is described in Table 2. In Panel A, loans are matched by loan purpose, loan type, borrower country and industry (two-digit SIC code, when matches are available, one-digit otherwise), and initiation year. From this set, the loan with the closest initiation date is selected. In Panel B, loans are matched by loan purpose, loan type, borrower country and industry (two-digit SIC code, when matches are available, one-digit otherwise), and lender country. From this set, the loan with the closest initiation date is selected. In Panel C, loans are matched by loan purpose and by borrower; from this set, the loan with the closest initiation date is selected. In Panel D, loans are matched on the "propensity score" $\hat{p}$, the conditional probability of a loan being covered by a BIT. The probit model used to compute propensity scores is presented in Appendix A. We use a caliper of 1 percentage point to ensure comparable matches. Tests for significance of mean differences are implemented as paired *t*-tests with standard errors clustered at the borrower firm and year level; tests of significance are two-sided. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

Panel A: Matched by borrower country, industry, facility start year, loan purpose, and loan type

Observations: 1085	(1) BIT	(2) Non-BIT	Difference: (1)-(2)	*t*	
Spread	158.64	179.74	-21.11	-4.94	***
Maturity (Months)	61.49	60.52	0.97	0.72	
Loan size (USD M)	1173.74	783.07	390.67	4.54	***
Number of lenders	13.68	7.24	6.44	16.73	***
Collateral (%)	19.54	25.62	-6.08	-3.85	***
Financial covenant (%)	11.34	13.82	-2.49	-1.97	**
Number of financial covenants	0.23	0.28	-0.06	-2.09	**
Number of general covenants	1.23	1.22	< 0.01	0.10	

Panel B: Matched by borrower country and lender country; loans before BIT vs after BIT

Observations: 892	(1) BIT	(2) Non-BIT	Difference: (1)-(2)	*t*	
Spread	152.93	187.38	-34.45	-5.41	***
Maturity (Months)	57.85	59.59	-1.74	-1.26	
Loan size (USD M)	1180.80	935.98	244.83	2.40	**
Number of lenders	15.24	8.01	7.23	18.40	***
Collateral (%)	18.50	26.01	-7.51	-4.15	***
Financial covenant (%)	10.76	12.00	-1.23	-0.93	
Number of financial covenants	0.21	0.27	-0.06	-1.84	*
Number of general covenants	1.23	1.26	-0.03	-0.76	

<Table 3 continued>

Panel C: Matched by borrower and loan purpose

Observations: 704	(1) BIT	(2) Non-BIT	Difference: (1)-(2)	*t*	
Spread	124.41	137.20	-12.79	-2.25	**
Maturity (Months)	55.80	54.71	1.09	0.74	
Loan size (USD M)	1705.47	1460.49	244.98	2.73	***
Number of lenders	16.91	10.56	6.36	10.85	***
Collateral (%)	14.91	16.19	-1.28	-0.71	
Financial covenants (%)	10.09	8.81	1.28	0.88	
Number of financial covenants	0.20	0.16	0.04	1.20	
Number of general covenants	1.26	1.21	0.04	1.02	

Panel D: Propensity score matching

Observations: 1179	(1) BIT	(2) Non-BIT	Difference: (1)-(2)	*t*	
Spread	111.85	124.75	-12.90	-2.20	**
Maturity (Months)	51.52	48.49	3.03	1.80	*
Loan size (USD M)	1209.79	1287.51	-77.73	-0.57	
Number of lenders	13.77	11.33	2.43	5.24	***
Collateral (%)	11.79	15.27	-3.48	-2.22	**
Financial covenant (%)	9.75	16.45	-6.70	-4.06	***
Number of financial covenants	0.19	0.32	-0.13	-3.81	***
Number of general covenants	1.22	1.68	-0.46	-6.04	***

Table 4. The Impact of BITs on Loan Spreads in High and Low Institutional Quality Countries

Table 4 compares all-in-drawn spreads of BIT loans to non-BIT loans. Loans covered by BITs are matched to loans not covered by BITs on the "propensity score" $\hat{p}$, the conditional probability of a loan being covered by a BIT. The probit model used to compute propensity scores is presented in Appendix A. We use a caliper of 1 percentage point to ensure comparable matches. The overall sample is described in Table 2. In the first panel, results are presented for subsamples of loans to borrowers headquartered in countries with (without) prior expropriation episodes. In the second panel, results are presented for subsamples of loans to borrowers headquartered in countries with *Autocracy vs. democracy* above (below) median. In the third panel, results are presented for subsamples of loans to borrowers headquartered in countries with *Weak executive constraints* above (below) median. In the fourth panel, results are presented for subsamples of loans to borrowers headquartered in countries with *Investment profile risk* above (below) median. The difference-in-difference estimators comparing the impact of BIT on spreads across subsamples are included. Tests for significance of mean differences are implemented as paired *t*-tests with standard errors clustered at the borrower firm level; tests of significance are two-sided. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	High risk		Difference		Obs	Low risk		Difference		Obs	Diff in Diff	
	(1) BIT	(2) Non-BIT	(1)-(2)			(3) BIT	(4) Non-BIT	(3)-(4)			[(1)-(2)]-[(3)-(4)]	
Expropriation	139.50	179.29	-39.79	*	206	102.16	100.45	1.70		892	-41.50	***
			-1.95					*0.32*			*-3.51*	
Autocracy vs. democracy	123.60	157.55	-33.95	***	717	89.56	93.90	-4.34		416	-29.61	***
			-4.31					*-0.56*			*-3.09*	
Weak executive constraints	122.72	154.55	-31.83	***	585	94.91	101.90	-6.99		483	-24.84	***
			-3.72					*-0.97*			*-2.65*	
Investment profile risk	138.26	156.42	-18.16	*	523	90.21	115.25	-25.04	***	609	6.88	
			-1.77					*-3.76*			*0.70*	

Table 5. Regression Analysis, Loan Spreads

Table 5 reports parameter estimates from OLS regressions. In each model, the response is the all-in-drawn spread (*Spread*). All variables are as defined in Table 1; the overall sample is described in Table 2. Firm level controls include *Altman Z*, *DtoA*, *ROA*, *TQ*, *TA* (log), *Cash over TA*. Unreported country-level controls include *GDP growth* and *GDP per capita* (log). All models include (unreported) fixed effects for seniority, currency, loan initiation year, industry, loan type and loan purpose. Non-price loan terms included in the fourth column include *Maturity* (log), *Loan size* (log), *Number of lenders* (log), *Collateral*, *Number of financial covenants* (log), and *Number of general covenants* (log). *t*-statistics from two-sided tests of significance are reported under the parameter estimates, in grey italics. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	Spread	*Spread*	*Spread*	*Spread*
BIT	-16.61	-18.27	-18.80	-11.08
	-4.46 ***	*-4.80* ***	*-4.94* ***	*-3.06* ***
Expropriation	54.91			62.72
	3.08 ***			*3.64* ***
Expropriation × *BIT*	-45.82			-46.09
	-2.67 ***			*-2.74* ***
Number of expropriations (log)		19.92		
		2.27 **		
Number of expropriations (log) × *BIT*		-18.71		
		-2.33 **		
Number of expropriated firms (log)			14.24	
			2.05 **	
Number of expropriated firms (log) × *BIT*			-13.47	
			-2.08 **	
Creditor rights risk	29.58	32.09	33.36	20.41
	3.18 ***	*3.45* ***	*3.64* ***	*2.44* **
Altman Z	-4.73	-4.72	-4.70	-4.38
	-5.61 ***	*-5.60* ***	*-5.59* ***	*-5.32* ***
DtoA	35.15	34.87	34.88	25.47
	3.69 ***	*3.65* ***	*3.65* ***	*2.96* ***
ROA	-1.12	-1.11	-1.10	-0.81
	-4.9 ***	*-4.85* ***	*-4.81* ***	*-3.72* ***
TQ	-2.11	-2.20	-2.27	-0.47
	-1.20	*-1.25*	*-1.30*	*-0.28*
TA (log)	-21.02	-20.95	-20.91	-13.28
	-22.20 ***	*-22.22* ***	*-22.20* ***	*-10.88* ***
Cash over TA	0.22	0.22	0.22	0.18
	2.68 ***	*2.65* ***	*2.67* ***	*2.29* **
Intercept	Yes	Yes	Yes	Yes
Country-level controls	Yes	Yes	Yes	Yes
Non-price terms of the loans	No	No	No	Yes
Fixed effects (seniority, currency, year, industry, loan type, loan purpose)	Yes	Yes	Yes	Yes
Observations	4768	4768	4768	4768
Adjusted R^2	0.5044	0.5028	0.5022	0.5528

Table 6. Regression Analysis, Non-Price Loan Terms

Table 6 reports parameter estimates from OLS regressions. The response variables are *Maturity*, *Loan size* (log), *Number of lenders*, *Number of financial covenants*, *Number of general covenants*, respectively. All variables are as defined in Table 1; the sample is described in Table 2. Firm level controls include *Altman Z, DtoA, ROA, TQ, TA* (log), *Cash over TA*. Unreported country-level controls include *GDP growth* and *GDP per capita* (log). All models include (unreported) fixed effects for seniority, currency, loan initiation year, industry, loan type, and loan purpose. *t*-statistics from two-sided tests of significance are reported under the parameter estimates, in grey italics. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	Maturity	*Loan size* (log)	*Number of lenders*	*Number of financial covenants*	*Number of general covenants*
BIT	-1.21	0.05	3.13	-0.30	-0.44
	-1.05	*1.20*	*7.25* ***	*-8.06* ***	*-7.62* ***
Expropriation	-11.68	0.01	-0.57	-0.33	-0.79
	-2.64 ***	*0.07*	*-0.56*	*-3.12* ***	*-4.43* ***
Expropriation × BIT	6.94	0.21	0.82	0.32	0.83
	1.66 *	*1.47*	*0.77*	*2.94* ***	*4.56* ***
Creditor rights risk	0.36	0.03	0.20	0.45	0.93
	0.15	*0.32*	*0.21*	*5.25* ***	*5.43* ***
Altman Z	0.21	-0.01	0.05	0.02	0.01
	0.86	*-0.72*	*0.53*	*1.43*	*0.65*
DtoA	4.40	-0.04	1.88	0.31	0.60
	1.81 *	*-0.41*	*2.22* **	*3.03* ***	*2.95* ***
ROA	0.15	0.01	0.05	-0.01	-0.01
	2.78 ***	*6.49* ***	*3.09* ***	*-2.71* ***	*-2.74* ***
TQ	-0.49	0.17	0.64	-0.03	0.04
	-0.98	*6.54* ***	*3.15* ***	*-1.44*	*0.93*
TA (log)	0.56	0.53	2.29	-0.12	-0.11
	1.94 *	*40.14* ***	*19.68* ***	*-11.08* ***	*-5.19* ***
Cash over TA	0.10	> - 0.01	-0.02	> - 0.01	> - 0.01
	4.35 ***	*-1.87* *	*-3.12* ***	*-0.95*	*-1.35*
Intercept	Yes	Yes	Yes	Yes	Yes
Country-level controls	Yes	Yes	Yes	Yes	Yes
Fixed effects (seniority, currency, year, industry, loan type, loan purpose)	Yes	Yes	Yes	Yes	Yes
Observations	4768	4768	4768	4768	4768
Adjusted R^2	0.3983	0.6890	0.3050	0.2365	0.2804

Table 7. Regression Analysis, Loan Spreads with Propensity-Score Weighting

Table 7 reports parameter estimates from regressions estimated by weighted-least-squares, with propensity score derived weights; the response is the all-in-drawn spread (*Spread*). A BIT loan receives a weight $w_{BIT} = 1/\hat{p}$ and a non-BIT loan receives a weight $w_{NON-BIT} = 1/(1 - \hat{p})$. The "propensity score" $\hat{p}$ is the conditional probability of a loan being covered by a BIT, as in Appendix A. All variables are as defined in Table 1; the sample is described in Table 2. Firm level controls include *Altman Z*, *DtoA*, *ROA*, *TQ*, *TA* (log), *Cash over TA*. Unreported country-level controls include *GDP growth* and *GDP per capita* (log). All models include (unreported) fixed effects for seniority, currency, loan initiation year, industry, loan type, and loan purpose. Non-price loan terms included in the fourth column include *Maturity* (log), *Loan size* (log), *Number of lenders* (log), *Collateral*, *Number of financial covenants* (log), and *Number of general covenants* (log). *t*-statistics from two-sided tests of significance are reported under the parameter estimates, in grey italics. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	Spread	*Spread*	*Spread*	*Spread*
BIT	-10.03	-12.05	-11.84	-11.02
	*-1.93 **	*-2.28 ***	*-2.28 ***	*-2.24 ***
Expropriation	60.49			63.38
	*3.21 ****			*3.43 ****
Expropriation × *BIT*	-45.53			-40.39
	*-2.41 ***			*-2.17 ***
Number of expropriations (log)		24.33		
		*2.56 ***		
Number of expropriations (log) × *BIT*		-18.14		
		*-2.01 ***		
Number of expropriated firms (log)			19.14	
			*2.47 ***	
Number of expropriated firms (log) × *BIT*			-14.18	
			*-1.94 **	
Creditor rights risk	30.14	31.06	31.84	13.91
	*2.41 ***	*2.49 ***	*2.57 ***	*1.29*
Altman Z	-4.51	-4.51	-4.49	-3.96
	*-4.31 ****	*-4.32 ****	*-4.29 ****	*-3.73 ****
DtoA	32.55	32.47	32.71	25.22
	*2.64 ****	*2.64 ****	*2.66 ****	*2.22 ***
ROA	-1.28	-1.28	-1.27	-0.87
	*-4.73 ****	*-4.73 ****	*-4.71 ****	*-3.41 ****
TQ	-3.41	-3.42	-3.49	-1.87
	-1.62	*-1.63*	*-1.66 **	*-0.95*
TA (log USD M)	-22.84	-22.81	-22.84	-12.85
	*-18.40 ****	*-18.40 ****	*-18.39 ****	*-7.90 ****
Cash over TA	0.25	0.25	0.26	0.22
	*2.71 ****	*2.73 ****	*2.77 ****	*2.36 ***
Intercept	Yes	Yes	Yes	Yes
Country level controls	Yes	Yes	Yes	Yes
Non-price terms of the loans	No	No	No	Yes
Fixed effects (seniority, currency, year, industry, loan type, loan purpose)	Yes	Yes	Yes	Yes
Observations	4768	4768	4768	4768
Adjusted R^2	0.5420	0.5418	0.5417	0.5880

Table 8. Regression Analysis, Robustness Tests: Pre-Existing Relationships and Country-Year Fixed Effects

Table 8 reports parameter estimates from OLS regressions. In each model, the response is the all-in-drawn spread (*Spread*). All variables are as defined in Table 1; the overall sample is described in Table 2. Firm level controls include *Altman Z*, *DtoA*, *ROA*, *TQ*, *TA* (log), *Cash over TA*. Unreported country-level controls include *GDP growth* and *GDP per capita* (log). All models include (unreported) fixed effects for seniority, industry, loan type and loan purpose. In column 1 to 3, we provide results for a subsample of loans granted to borrowers that have pre-existing lending relationship with the leader arranger before enforcement of a BIT. In column 4, we include country-year fixed effects. *t*-statistics from two-sided tests of significance are reported under the parameter estimates, in grey italics. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	Spread	*Spread*	*Spread*	*Spread*
BIT	-15.71	-18.17	-18.38	-11.38
	-2.24 **	*-2.50* **	*-2.52* **	*-2.58* ***
Expropriation	88.17			
	2.35 **			
Expropriation × *BIT*	-65.09			
	-2.04 **			
Number of expropriations (log)		24.87		
		1.06		
Number of expropriations (log) × *BIT*		-31.95		
		-2.03 **		
Number of expropriated firms (log)			21.23	
			1.06	
Number of expropriated firms (log) × *BIT*			-26.90	
			-2.03 **	
Creditor rights risk	22.07	27.77	27.52	97.54
	1.36	*1.72* *	*1.70* *	*1.88* *
Altman Z	-6.62	-6.55	-6.51	-5.50
	-4.83 ***	*-4.76* ***	*-4.74* ***	*-5.90* ***
DtoA	36.00	36.01	36.20	35.34
	2.80 ***	*2.80* ***	*2.82* ***	*3.57* ***
ROA	-1.52	-1.49	-1.49	-1.24
	-4.44 ***	*-4.37* ***	*-4.37* ***	*-5.25* ***
TQ	-0.49	-0.64	-0.68	-2.09
	-0.18	*-0.24*	*-0.25*	*-1.08*
TA (log USD M)	-22.22	-22.28	-22.23	-21.42
	-16.11 ***	*-16.22* ***	*-16.18* ***	*-21.60* ***
Cash over TA	0.42	0.43	0.43	0.29
	2.99 ***	*3.03* ***	*3.04* ***	*3.53* ***
Intercept	Yes	Yes	Yes	Yes
Country level controls	Yes	Yes	Yes	Yes
Fixed effects (country × year)	No	No	No	Yes
Fixed effects (currency, year)	Yes	Yes	Yes	No
Fixed effects (seniority, industry, loan type, loan purpose)	Yes	Yes	Yes	Yes
Observations	1901	1901	1901	4768
Adjusted R^2	0.5494	0.5468	0.5466	0.5593

Table 9. Regression Analysis, Loan Spreads with Heckman (1979) two-stage procedure

Table 8 reports parameter estimates from second-stage regressions from a Heckman (1979) two-stage procedure, augmented by the *Inverse Mills Ratio*. The *Inverse Mills Ratio* is obtained from first-stage probit model where dependent variable is the probability of a loan being covered by a BIT, as in Appendix A.2. In each model, the response is the all-in-drawn spread (*Spread*). All variables are as defined in Table 1; the overall sample is described in Table 2. Firm level controls include *Altman Z*, *DtoA*, *ROA*, *TQ*, *TA* (log), *Cash over TA*. Unreported country-level controls include *GDP growth* and *GDP per capita* (log). All models include (unreported) fixed effects for seniority, currency, loan initiation year, industry, loan type and loan purpose. Non-price loan terms included in the fourth column include *Maturity* (log), *Loan size* (log), *Number of lenders* (log), *Collateral*, *Number of financial covenants* (log), and *Number of general covenants* (log). *t*-statistics from two-sided tests of significance are reported under the parameter estimates, in grey italics. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	Spread	*Spread*	*Spread*	*Spread*
BIT	-9.54	-12.52	-13.40	-9.91
	-2.41 **	*-3.07* ***	*-3.28* ***	*-2.60* ***
Expropriation	69.18			67.49
	3.77 ***			*3.84* ***
Expropriation × *BIT*	-56.62			-51.35
	-3.24 ***			*-3.03* ***
Number of expropriations (log)		25.16		
		2.69 ***		
Number of expropriations (log) × *BIT*		-22.69		
		-2.67 ***		
Number of expropriated firms (log)			17.43	
			2.36 **	
Number of expropriated firms (log) × *BIT*			-16.38	
			-2.38 **	
Inverse Mills Ratio	25.39	24.28	23.64	15.31
	7.15 ***	*6.81* ***	*6.65* ***	*4.60* ***
Creditor rights risk	13.40	17.07	19.40	11.90
	1.38	*1.76* *	*2.04* **	*1.36*
Intercept	Yes	Yes	Yes	Yes
Country level controls	Yes	Yes	Yes	Yes
Firm level controls	Yes	Yes	Yes	Yes
Non-price terms of the loans	No	No	No	Yes
Fixed effects (seniority, currency, year, industry, loan type, loan purpose)	Yes	Yes	Yes	Yes
Observations	4597	4597	4597	4597
Adjusted R^2	0.5330	0.5308	0.5298	0.5770

Table 10. Regression Analysis, Instrumental Variable Estimation of Price and Non-Price Loan Terms

Table 9 reports the results of coefficient estimates for all-in-drawn spreads. The spread equations are part of a system of equations for price (*Spread*) and non-price (*Maturity* (log) and *Collateral*) loan terms, as in Bharath et al. (2011). In the system, *Maturity* and *Collateral* affect each other, while *Spread* is only affected by *Maturity* (log) and *Collateral*. The system is estimated by two stages least squares (2SLS) using two instruments for loan maturity: *Asset maturity* as in Barclay, Marx and Smith (2003) and *Regulated*, a binary variable identifying regulated industries as in Barclay and Smith (1995). As an instrument for loan collateral, we use *Loan concentration*, the ratio of that loan facility amount to the total firm's debt. The *Six-months average spread* is the mean all-in-drawn spread for all loans to borrowers in the same country during the six months prior to loan initiation. Unreported firm-level controls include *Altman Z, DtoA, TA* (log), *TQ, and ROA*. Unreported country-level controls include *GDP growth* and *GDP per capita* (log). All models include (unreported) fixed effects for seniority, currency, loan initiation year, industry, loan type, and loan purpose. All other variables are as defined in Table 1; the sample is described in Table 2. Reported results are only for the spread equations. Simple OLS regression results without the instruments are provided for each of the models for comparison. *t*-statistics from two-sided tests of significance are reported under the parameter estimates, in grey italics. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	OLS	IV	OLS	IV	OLS	IV
BIT	-12.19	0.22	-13.97	-1.21	-14.37	-1.28
	-3.34 ***	*0.04*	*-3.75* ***	*-0.25*	*-3.85* ***	*-0.26*
Expropriation	65.68	61.45				
	3.76 ***	*3.05* ***				
Expropriation × *BIT*	-50.85	-38.23				
	-3.02 ***	*-2.00* **				
Number of expropriations (log)			24.90	25.73		
			2.74 ***	*2.67* ***		
Number of expropriations (log) × *BIT*			-21.46	-17.62		
			-2.58 ***	*-2.15* **		
Number of firms affected (log)					18.89	20.37
					2.59 ***	*2.46* **
Number of firms affected (log) × *BIT*					-16.14	-13.93
					-2.37 **	*-2.04* **
Creditor rights risk	22.01	-5.24	24.33	-4.28	25.58	-2.88
	2.61 ***	*-0.57*	*2.86* ***	*-0.47*	*3.05* ***	*-0.32*
Relationship	-1.31	-6.41	-0.98	-6.19	-0.94	-6.17
	-0.55	*-1.97* **	*-0.41*	*-1.92* *	*-0.39*	*-1.91* *
Maturity (log)	-13.47	-84.53	-13.80	-84.94	-13.95	-86.20
	-4.08 ***	*-1.75* *	*-4.17* ***	*-1.77* *	*-4.21* ***	*-1.79* *
Collateral	60.27	168.22	60.08	167.09	60.01	168.10
	14.69 ***	*2.40* **	*14.69* ***	*2.40* **	*14.65* ***	*2.38* **

<Table 10 continued>

	OLS	IV	OLS	IV	OLS	IV
Loan size (log)	-6.68	-0.79	-6.59	-0.72	-6.61	-13.93
	-4.10 ***	*-0.18*	*-4.03* ***	*-0.16*	*-4.05* ***	*-2.04* **
Six-months average spread		0.21		0.22		0.22
		2.65 ***		*2.76* ***		*2.66* ***
Intercept	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4663	4663	4663	4663	4663	4663
Adjusted R^2	0.5463	0.3612	0.5443	0.3620	0.5437	0.3561

Table 11. Regression Analysis, Loan Spreads, Additional Metrics of Institutional Quality

Table 10 reports parameter estimates from OLS regressions. In each model, the response is the all-in-drawn spread (*Spread*). All variables are as defined in Table 1; the overall sample is described in Table 2. Unreported firm-level controls include *Altman Z*, *DtoA*, *ROA*, *TQ*, *TA* (log), *Cash over TA*. Unreported country-level controls include *GDP growth* and *GDP per capita* (log). All models include (unreported) fixed effects for seniority, currency, loan initiation year, industry, loan type and loan purpose. In column 4, fixed effects for legal origin is included. *t*-statistics from two-sided tests of significance are reported under the parameter estimates, in grey italics. Significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	Spread	*Spread*	*Spread*	*Spread*	*Spread*
BIT	-15.98	-14.43	-16.19	-15.30	-19.63
	-4.30 ***	*-3.76* ***	*-4.21* ***	*-4.10* ***	*-5.31* ***
Expropriation	57.76	54.67	55.25	92.33	
	3.34 ***	*3.05* ***	*3.07* ***	*4.85* ***	
Expropriation × *BIT*	-44.68	-51.26	-46.68	-46.95	
	-2.71 ***	*-2.96* ***	*-2.70* ***	*-2.74* ***	
Number of expropriations all years (log)					24.58
					3.14 ***
Number of expropriations all years (log) × *BIT*					-15.56
					-2.22 **
Investment profile risk	5.85				
	2.73 ***				
Autocracy vs. democracy		-2.14			
		-2.94 ***			
Weak executive constraints			-0.89		
			-0.35		
Creditor rights risk	27.84	31.66	30.39	26.27	27.72
	3.00 ***	*3.43* ***	*3.29* ***	*2.58* **	*2.93* ***
Intercept	Yes	Yes	Yes	Yes	Yes
Legal origin	No	No	No	Yes	No
Firm / country-level controls	No	No	No	Yes	Yes
Fixed effects (seniority, currency, year, industry, loan type, loan purpose)	Yes	Yes	Yes	Yes	Yes
Observations	4765	4768	4768	4768	4768
Adjusted R^2	0.5070	0.5058	0.5043	0.5528	0.5050

Appendix A. BITs over Time.

In this appendix, we focus on the spread of bilateral investment treaties (BITs) over time. The first BIT was signed in 1959, between Germany and Pakistan. Until 1989, only 386 BITs were signed. The number of BITs has since grown dramatically, with more than 2,200 BITs signed over the following fifteen years. In Table A1 we report the number of BITs by country, as of December 31, 2012. While originally BITs mostly involved developing countries, the countries with the highest number of BITs are now Germany, China, and Switzerland. In Figures A1 and A2, we depict, respectively, the number of BITs signed and the cumulative number of BITs, by year. We break down the total number of BITs into two groups, identifying BITs signed with OECD and non-OECD countries separately. Our findings indicate that, while most BITs are signed between OECD and non-OECD countries, a substantial number of BITs are signed between non-OECD countries. BITs between OECD countries are relatively less common.

To put the growing popularity of BITs in perspective, Vandevelde (2009) notes that, after the end of the Second World War, three events shaped international investment agreements. First, reacting to the economic depression that had preceded the most recent war and to prevent the re-emergence of the protectionist policies of the 1920s, the victors developed a consensus in favor of free trade. This led to the signing of the General Agreement on Tariffs and Trade (GATT) in 1947, which in turn led to the emergence of multilateral trade agreements, favored by the enforcement mechanisms and institutions championed by GATT. These agreements, by and large, focused on guaranteeing "equitable treatment" and "protection and security" to foreign investors and guaranteed just compensation in case of property right violations. The second major trend was a process of decolonization, which led to the creation of many new, independent countries, most of which were economically underdeveloped. In many of these countries, foreign investment was viewed with suspicion, if not outright hostility, as it was perceived as a legacy of neocolonialism. The third trend was the emergence of the socialist bloc led by the Soviet Union, which provided the ideological basis justifying expropriation of private-sector investors, domestic and foreign. Consequently, a wave of expropriations of foreign investors brought the issue of protection of property rights into the limelight in the 1970s.

These three trends also led, in the early 1970s, to a shift in power in the United Nations General Assembly, with developing and socialist countries gaining the upper hand. Emblematic of this new dominant ideology was the passage, on May 1, 1974, of the Declaration of the New International Economic Order, which recognized sovereign states having full and permanent sovereignty over natural resources and other economic activities, and emphasized the right to nationalize or transfer ownership of the means of production. Lacking support by the United Nations, multilateral trade agreements suddenly became unenforceable. As a result, developed countries started pushing for the adoption of bilateral investment treaties, as a substitute mechanism for gaining protection of property rights for foreign investors. Germany was an unsurprising early adopter, having lost most of its foreign investment as a result of its defeat in the recent war. Other developed countries followed suit. Importantly, we should note that these BITs were fairly uniform in content. Yet, as discussed by Vandevelde (2009), adoption was driven mostly by political ideology, rather than by economic conditions. Over the following years, BITs spread rapidly. Also noticeably, these early BITs were mostly between developed and developing countries. While the agreements were bilateral, the agreements were drafted usually by the developed countries and, in most cases, the investment flows covered were unilateral, flowing from developed to developing countries. As indicated by Figures A1 and A2, a first wave of BITs between developed and developing countries was soon followed by BITs between developing countries.

Their main purpose of BITs was, and is still today, to establish standards for "prompt, adequate, and effective" compensation for expropriation. Most BITs further required non-discrimination of foreign investors (that is, required foreign investors to be treated no worse than domestic investors in all legal proceedings). This initial model is often referred to as the "German" model BIT. A main innovation arose in the 1960s, when BITs signed by the United States adopted the inclusion of arbitration provisions, allowing private citizens recourse to third-party arbitration courts in case of disputes. This came to be informally known as the "American" model BIT agreement. It is also important to note what BITs do not generally cover. Given the existence of other institutions and agreements policing trade, BITs focused specifically on investments, although the exact definition of the term has been debated in court. Importantly,

most legal scholars are today in agreement that "investments" includes both debt and equity agreements. BITs do not generally cover any issue of market access, nor do they impose any restrictions on trade tariffs.

Figure A1. Number of BITs Signed, by Year

Figure A1 reports the number of BITs signed, by year. The number of BITs signed is broken down into BITs between OECD countries, BITs between non-OECD countries, and BITs between OECD and non-OECD countries.

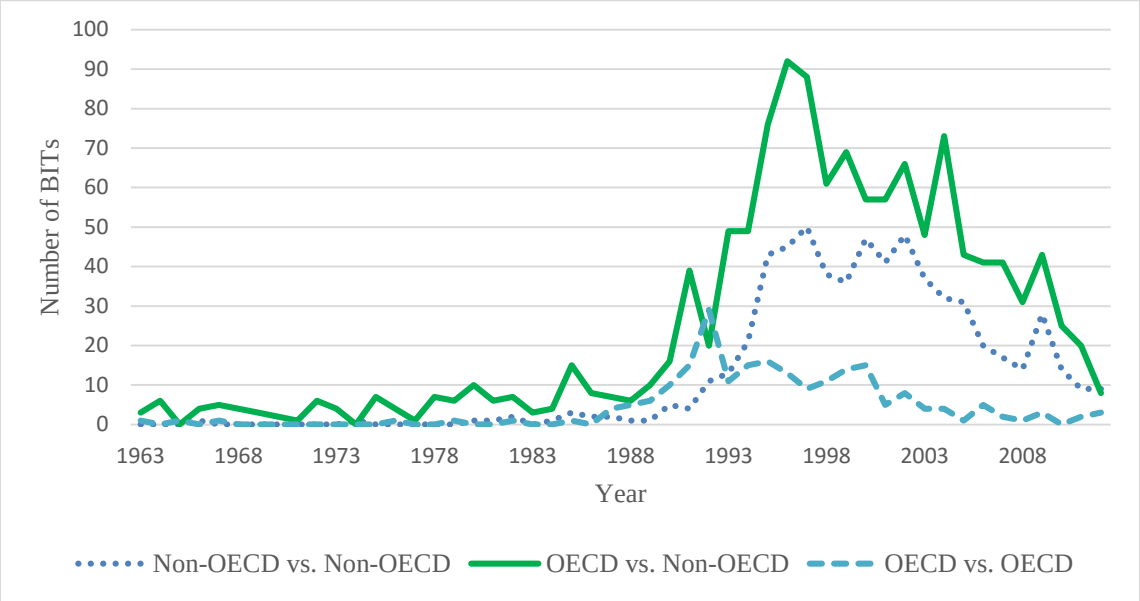

Figure A2. Cumulative Number of BITs, by Year.

Figure A1 reports the cumulative number of BITs, by year. The cumualtive number of BITs is broken down into BITs between OECD countries, BITs between non-OECD countries, and BITs between OECD and non-OECD countries.

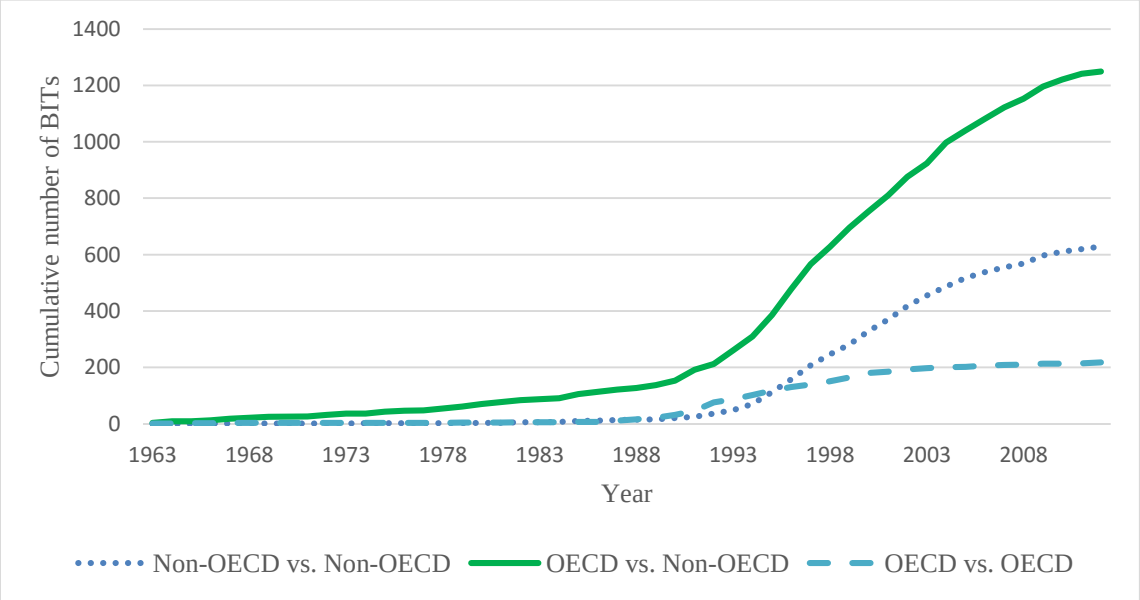

Table A1.

Table B1 lists the number of BITs signed by OECD countries (left panel) and non-OECD countries (right panel).

Rank	OECD countries	Number of BITs	Number of BITs (with OECD countries)	Number of BITs (with non-OECD countries)
1	Germany	111	13	98
2	Switzerland	101	11	90
3	France	87	12	75
4	Netherlands	82	10	72
5	United Kingdom	82	11	71
6	Korea (South)	81	24	57
7	Czech Republic	71	24	47
8	Italy	71	6	65
9	Spain	71	11	60
10	Luxembourg	65	11	54
11	Turkey	65	25	40
12	Finland	64	11	53
13	Sweden	64	11	53
14	Austria	59	11	48
15	Poland	59	28	31
16	Hungary	55	23	32
17	Slovakia	45	23	22
18	Denmark	44	10	34
19	Latvia	44	25	19
20	Chile	38	19	19
21	Greece	38	12	26
22	Portugal	38	11	27
23	USA	37	6	31
24	Slovenia	35	18	17
25	Israel	30	9	21
	Other	137	61	76

Rank	Non-OECD countries	Number of BITs	Number of BITs (with OECD countries)	Number of BITs (with non-OECD countries)
1	China	100	31	69
2	Egypt	70	27	43
3	Romania	70	25	45
4	India	67	25	42
5	Bulgaria	57	23	34
6	Argentina	54	24	30
7	Ukraine	54	27	27
8	Lithuania	50	26	24
9	Russia	49	21	28
10	Malaysia	48	18	30
11	Belarus	47	18	29
12	Croatia	47	24	23
13	Iran	46	12	34
14	Uzbekistan	46	23	23
15	Morocco	45	19	26
16	Indonesia	42	17	25
17	Vietnam	41	20	21
18	Serbia	40	19	21
19	Bosnia and Herzegovina	39	20	19
20	Jordan	39	18	21
21	Kuwait	38	16	22
22	Moldova	38	21	17
23	Lebanon	36	16	20
24	Mongolia	35	19	16
25	Macedonia	34	16	18
	Other	1266	726	540

Appendix B. Models of BIT Coverage

In Table B1, we include estimates from the probit model used to determine the probability of a loan being covered by a BIT, employed in probability-score matching and weighting procedures in the manuscript. The results indicate that smaller borrowers with higher leverage and lower liquidity (overall, riskier borrowers), but with good growth opportunities, seek BIT coverage. BIT coverage is more frequent for borrower from wealthier countries (higher GDP per capita) and for relationship loans (not the first loan between the same borrower-lender pair).

In Table B2, we present the first-stage of the two-stage Heckman procedure based on on Carr, Markusen, and Maskus (2001) and Jandhyala and Weiner (2014). The probit model estimates the probability that the borrower and lender countries have signed a BIT. The results indicate that BIT coverage is positively related to political constrains, to the overall number of BITs signed worldwide, to the distance between two countries, and to the aggregate size of the economies. The absolute difference in the size of the economies is positively related to BIT coverage, but the squared difference is negatively related to BIT coverage, indicating a non-linear relationship.

Table B1. Probability of a Loan Being Covered by a BIT, for Probability-Score Matching and Weighting
The table includes coefficient estimates from a probit model. The response is a binary variable assuming the value of one if the loan is covered by a BIT or zero otherwise. All variables are as defined in Table 1; the sample is described in Table 2. Firm level variables include *Altman Z*, *DtoA*, *ROA*, *TQ*, *TA* (log), *Cash over TA*. Other variables include *GDP growth*, *GDP per capita* (log), *Relationship*. Wald's Chi-square test statistics are reported in gray italics, in the second column. The significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	BIT	*Chi-square*	
Intercept	-4.65	*109.96*	***
Altman Z	0.01	*1.23*	
DtoA	0.23	*3.81*	*
ROA	> - 0.01	*1.23*	
TA (log USD M)	-0.13	*87.56*	***
TQ	0.04	*2.72*	*
Cash over TA	> - 0.01	*19.91*	***
GDP growth	> - 0.01	*0.20*	
GDP per capita (log USD Th)	0.77	*643.28*	***
Relationship	0.15	*11.47*	***
Observations	4768		
Pseudo R^2	0.2077		

Table B2. Probability of a Lender and Borrower Countries Being Covered by a BIT, First-stage of Heckman Methodology

The table includes coefficient estimates from a probit model. The response is a binary variable assuming the value of one if the loan is covered by a BIT or zero otherwise. All variables are as defined in Table 1; the sample is described in Table 2. Wald's Chi-square test statistics are reported in gray italics, in the second column. The significance levels are denoted as follows: "*" indicates significance at the 0.10 level; "**" indicates significance at the 0.05 level; "***" indicates significance at the 0.01 level.

	BIT	*Chi-square*	
Intercept	18.30	*16.23*	***
Average political constraints score	1.65	*8.99*	***
Abs difference in GDP per capita (log)	0.16	*4.29*	***
Sum of GDP (log)	-0.55	*-7.36*	***
Squared difference in GDP (log)	-0.16	*-5.50*	***
Total worldwide BITs	< 0.01	*16.61*	***
Distance	0.35	*9.38*	***
Observations	4597		
Pseudo R^2	0.3760		