



# Bondholders vs. Direct Investors? Competing Responses to Expropriation

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We often presume that international financial actors have the same preferences, but this paper asks whether the property rights of foreign direct investors matter to sovereign bondholders. When governments expropriate direct investors, different investors' preferences could align over property rights issues. However, bondholders likely take positive signals if expropriation generates revenue for the state. Using a novel data set (1995–2011), I find that governments that earn revenue from expropriation can enjoy lower long-term spreads on sovereign bonds. Although governments that expropriate lose out on FDI, they can benefit by generating revenue and enjoying rewards in sovereign debt markets. Unpacking investor preferences thus reveals gaps in market-based informal property rights enforcement. When bondholders' and direct investors' preferences conflict, governments gain space to prioritize other goals over the protection of private property.

In the study of international political economy, we often presume that international financial actors have the same preferences over government behavior. But what if investors take different sides? If a government seizes a multinational corporation's assets, does a hedge fund care? If international financial actors' preferences diverge—or even oppose one another—then governments could maintain capital access in one sphere and act opportunistically in another. In an era in which we expect constraints from economic globalization, and our research agenda usually looks for exceptions to that rule, such a weakness in informal, capital-market pressure for property rights enforcement would prove striking.

In this paper, I question whether a government's treatment of its foreign direct investors shapes its ability to borrow in international markets.<sup>1</sup> When direct investors retreat or divert investment in response to threats of expropriation, they pressure governments to enforce property rights (Li and Resnick 2003; Jensen 2008; Jensen, Biglaiser, Li, Malesky, Pinto, Pinto, and Staats 2012).<sup>2</sup> If governments that expropriate face higher sovereign borrowing costs, then bondholders also value foreign direct investor property rights and contribute to market-based pressure for enforcement. However, if

borrowing costs do not increase—or even decrease—with expropriation, then much the opposite holds: International financial actors' preferences can be at odds.

I argue that both scenarios obtain. When expropriation brings with it no side benefits for sovereign bondholders, bondholders align with direct investors and respond to expropriation in ways that punish an offending government. However, expropriation sometimes raises considerable revenues for government coffers. Because sovereign bondholders ultimately care most about debt serviceability, I argue that they are indifferent to—or even rewarding of—revenue-raising expropriation.

The novel insight of my argument is that bondholders reward governments when revenues increase, despite the fact that revenues increase at the expense of foreign direct investors. This trade-off between direct investor property rights and sovereign borrowing costs challenges the oft-repeated developmental strategy that prioritizes property rights above all, and it contradicts arguments about the “retreat of the state” in the face of economic globalization (Strange 1996). Rather, variation in firm preferences enhances state autonomy (cf. Maxfield and Schneider 1997; Kucik 2012; Pinto 2013; Wellhausen 2015a). When multinational corporations and hedge funds are not on the same side, governments gain space to prioritize other goals over the protection of private property.

To show the mixed effects of expropriation on sovereign bondholder behavior, I use a new data set of public international investment arbitrations (hereafter “arbitrations”) in emerging markets. Public arbitrations are international legal claims that foreign firms file against host governments, usually under the auspices of a Bilateral Investment Treaty (BIT) or similar investment protection instrument. With the rise of BITs and resulting public arbitrations since the 1990s, market actors receive information about a variety of expropriatory actions that foreign investors believe have unlawfully devalued their property. Public arbitrations thus send signals about the security of foreign direct investor property rights in a host country. Indeed, we know that states facing public

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Sincere thanks go to the executives who agreed to be interviewed for this study and to the University of Texas Alumni Association. My thanks to David Singer, Raymond Hicks, Layna Mosley, David Bearce, Terry Chapman, Pat McDonald, Raphael Cuna, Sarah Brooks, Andreas Fuchs, Stephanie Rickard, and participants at the International Studies Association, the American Political Science Association, and the International Political Economy Society. Torben Behmer and Riitta-Ilona Koivumäki provided excellent research assistance.

<sup>1</sup> In doing so, this paper takes up the challenge of Mosley and Singer (2008:405) to “disaggregate the effects of different asset markets to understand the impact of economic globalization on government policies.”

<sup>2</sup> Expropriation includes the transfer of ownership to the state as well as incremental violations of foreign firms' ability to derive full value from their assets.

arbitrations lose foreign direct investment (FDI), as firms divert or drawdown investments in search of friendlier climes (Allee and Peinhardt 2011; Wellhausen 2015b:252–256). I extend this literature by testing the broader relevance of international investment law. Do states facing public arbitrations lose—or gain—the ability to borrow in international markets?

The paper proceeds as follows. After developing the theory of expropriation's competing effects, I introduce the public arbitration data set. I then provide qualitative and quantitative evidence that some expropriations raise considerable revenues and affect sovereign bondholders' perceptions of debt serviceability. Next, I present the main quantitative analyses, using emerging market sovereign bond indices taken from JP Morgan (1995–2011) and error correction models (ECMs).<sup>3</sup> I find a substantial and robust long-term punishment in sovereign bond markets when governments expropriate without generating revenue and face a public arbitration. In stark contrast, revenue-generating expropriations and resulting public arbitrations are associated with significant reductions in the long-term costs of borrowing. In some specifications, they produce null results that suggest bondholder indifference. I conclude by emphasizing a key implication of this paper: Unpacking investors reveals that they sometimes fall on different sides. Public arbitrations, facilitated by the growing legalization of international investment, reveal that economic openness can generate both constraints and opportunities for governments. When investors have conflicting preferences, governments that need to borrow cheaply and want to expropriate can do both.

### The Politics of Bond Spreads

Prior scholarship extensively examines the political determinants of FDI (for example, Jensen, Biglaiser, Li, Malecky, Pinto, Pinto, and Staats 2012), and a growing literature looks at how political developments affect sovereign bondholders.<sup>4</sup> Sovereign rating agencies prioritize a country's default history and macroeconomic indicators (Cantor and Packer 1996; Afonso, Gomes, and Rother 2007; Archer, Biglaiser, and DeRouen 2007), but Fuchs and Gehring (2013) find systematic home bias across rating agencies from six countries. Moreover, Biglaiser and Staats (2012) find that judicial quality and the rule of law also affect ratings. Mosley (2003) argues that investors themselves tend to give developed country governments a pass while scrutinizing more closely the policy choices governments make in emerging economies. Tomz (2007) examines how past behavior influences governments' reputations in international debt markets, while Gray (2013) argues that “the company a state keeps” also shapes their ability to borrow. Tomz and Wright (2012) look at FDI and sovereign debt simultaneously, finding cyclical patterns of expropriation and sovereign default in history (see also Eden, Kraay, and Qian 2012). This article's focus on the trade-off between property rights and the cost of sovereign borrowing helps explain this cyclicity.

By drawing on insights about investor preferences, this article presents a straightforward theory. Foreign direct investors and sovereign bondholders are both interested

in the security of their property rights, whether in the form of assets in the host country or the government's credible claim to service debt. Both seek to avoid or compensate for increased risks on their holdings. For direct investors, expropriation can move together with increased investment risk. At least some direct investors are likely worried that one firm's expropriation indicates that their property is at risk, which leads to investment drawdown or deterrence.

Standard accounts of informal property rights enforcement imply that expropriation has a broader negative effect. Expropriation likely worries at least some other international financial actors—such as holders of sovereign debt—that their property is also at risk. Notwithstanding such worries, sovereign bondholders remain interested in debt serviceability. As such, more money in government coffers suggests fewer risks. I contend that the ultimate signal that sovereign bondholders take from an expropriation depends on the expropriation's effect on government revenues. If an expropriation does not raise revenue for the government, sovereign bondholders' and direct investors' preferences align: All else equal, the expropriation signals increased risks to sovereign bondholders, too. In sovereign bond markets, increased risks raise government borrowing costs.

**H1:** *All else equal, sovereign borrowing costs increase when an expropriation does not raise revenue for the government.*

If an expropriation does raise revenue for the government, however, I expect that the improvement in debt serviceability outweighs the otherwise negative signal about property rights stability. Sovereign bondholders compensate for risks suggested by expropriation, but they also react to improvements in the government's position to service its debt. I argue that the certainty of more funds to service debt trumps concern about the precedents that the expropriation might set for future adverse government behavior.<sup>5</sup> From a government's point of view, this implies that the costs of borrowing decrease with revenue-raising expropriation.

**H2:** *All else equal, sovereign borrowing costs decrease when an expropriation raises revenue for the government.*

A brief vignette demonstrates what H2 looks like in practice. In the late 2000s, bondholders and direct investors were quite aware that Ukrainian firms received regular Value Added Tax (VAT) rebates, while their foreign competitors did not. Ukraine owed US\$1.2 billion in outstanding VAT to multinational exporters by August 2010.<sup>6</sup> ArcelorMittal, in particular, did not get VAT refunded from late 2009 to 2010, and the government asked it to pay its income taxes months in advance, leaving it a creditor to the Ukrainian government for US\$500 million by mid-2010.<sup>7</sup> Understanding the situation, multinational exporters lobbied for an uncharacteristically specific stipulation about VAT repayment in Ukraine's 2009 IMF package, in hopes that expropriation would no longer make them the *de facto* creditors to a nearly insolvent state (Wellhausen 2015a:150–152). Nevertheless,

<sup>3</sup> See Appendix for analysis of emerging market credit default swap (CDS) indices.

<sup>4</sup> Economic models of bond spreads tend to find effects of macroeconomic variables and thus only indirect evidence of a role for politics (Eichengreen and Mody 1998; Hilscher and Nosbusch 2010).

<sup>5</sup> Evidence Part II: Robustness tests address whether the persistence of expropriation mitigates this relationship.

<sup>6</sup> “State Tax Administration: Value-added Tax Bonds.” *Interfax-Ukraine*: August 6, 2010.

<sup>7</sup> Graham Stack. “Value-added tax provides case study in corruption.” *Kyiv Post*: June 3, 2010.

withholding VAT rebates helped the government remain solvent when much of the world saw it as perched on the brink of default. In other words, Ukraine effectively generated an income stream via expropriation. From 2008 to 2010, Ukraine's borrowing costs plateaued, halting what had been a steady climb since 2001.<sup>8</sup> Ukraine's IMF packages in 2009 and 2010 were loans, but not bailouts.

The next sections demonstrate that this vignette reflects systematic variation: Sovereign bondholders are sometimes rewarding of expropriation. I first introduce a new data set measuring instances of expropriation and then leverage it to demonstrate the effects of revenue-raising expropriation on (i) debt serviceability and (ii) sovereign bond spreads.

### Measuring Expropriation: Public International Investment Arbitrations

More than 2700 BITs and hundreds of investment protection clauses in Preferential Trade Agreements (PTAs) and other international treaties outlaw uncompensated expropriation (UNCTAD). At the same time, these agreements publicize expropriation events, because they facilitate public international investment arbitration.<sup>9</sup> From 1990 through 2012, firms sued at least 100 host states at least 535 times in public arbitrations (Figure 1).<sup>10</sup> Firms from a wide range of industries have filed public arbitrations.<sup>11</sup> Of the 208 cases in which it is public knowledge, investors claimed an average of US\$650 million in damages.<sup>12</sup> In 45 of these cases in which the investor won and the amount awarded is known, investors won on average 40 percent of the damages claimed.

Investors' claims in public arbitrations demonstrate that, while nationalization and the forced change of ownership persist, property rights violations come in a variety of forms (Kobrin 1980:67–69). For example, “regulatory taking” occurs when the government changes policy in a way that adversely and discriminatorily affects a foreign investor. This paper employs a wide definition of expropriation, encompassing all investor claims in public arbitrations. This is warranted, as I use public arbitrations as a marker that an investor believes a violation of its property rights has taken place.<sup>13</sup> Unfortunately, because both

public and private international investment arbitrations exist, the true number of arbitrations remains unknown. While a complex selection process behind this censoring indeed exists, it is not an issue for the analysis here. Public arbitrations are useful as an institution that can provide information and signals to otherwise ill-informed actors.<sup>14</sup> We know that public arbitrations provide signals to direct investors, because lower FDI flows follow public-arbitration filings (Allee and Peinhardt 2011; Wellhausen 2015b:252–256). Scholars, though, have yet to examine the information and signals this sort of publicized adverse government behavior might send to other international financial actors.

To further the analysis, I utilize the idea that holders of emerging market sovereign debt may be differentially interested in micro-level policy such as government behavior toward foreign-owned property, making emerging market countries a most likely setting to uncover hypothesized dynamics (Mosley 2003).<sup>15</sup> I narrow the sample of countries to 39, which is the number of emerging market countries for which robust secondary trading markets exist for sovereign debt.<sup>16</sup> Of these, 32 countries faced 237 public arbitrations from 1995 to 2011. Table 1 breaks public arbitrations down by the number of filings per country. We see that Argentina has the most public arbitrations filed against it, the majority of which emerged from its 2001–2002 default and financial crisis. Venezuela has faced so many public arbitrations that it decided in 2008 to withdraw from its BIT with the Netherlands, the facilitator of many cases (although firms retain rights to file under the BIT for a sunset period after withdrawal). Ecuador, too, has faced a number of cases especially in the oil and gas industry and has withdrawn from a number of BITs.<sup>17</sup> However, the distribution of public arbitrations in more than 29 other countries in the sample—and more than 100 countries worldwide—suggests that the phenomenon of expropriation claims and resulting public arbitrations is widespread.

Of the 237 public arbitrations in the sample, 185 are revenue arbitrations (faced by 26 countries) and 52 are non-revenue arbitrations (faced by 19 countries).<sup>18</sup> Expropriations and resulting arbitrations are coded as

<sup>8</sup> Borrowing costs briefly dropped by 12 percent in mid-2009, but this likely had to do with the worldwide financial crisis.

<sup>9</sup> It is usually not a condition that a firm exhaust local courts in the host country before filing an arbitration, nor must home governments know or approve of their nationals' exercise of investment protection clauses.

<sup>10</sup> All cases brought at the most public of arbitration venues, the World Bank's International Center for the Settlement of Investment Disputes (ICSID), are included in this data set. I have also collected public cases, typically brought under arbitration rules provided by UNCITRAL, in venues such as the International Chamber of Commerce, the Stockholm Chamber of Commerce, and the London Court of International Arbitration.

<sup>11</sup> Industries include energy (18 percent); utilities (15 percent); manufacturing (14 percent); services (13 percent); construction (9 percent); and mining (8 percent); as well as finance, telecommunications, agriculture, and trade (23 percent).

<sup>12</sup> Maximum claims come from three investors for US\$11 trillion each in *Hulley Enterprises Ltd v Russian Federation* (2005, PCA Case AA226). Claims per case are calculated based on the lowest damages claimed, excluding claims for interest and fees.

<sup>13</sup> Nonetheless, not every public arbitration invokes the legal principle of expropriation, and the international legal ramifications of the variety of government actions considered in this paper differ (for example, Newcombe 2005; Van Harten 2007; Franck 2008). See Evidence Part II: Robustness for considerations of not just the filing but the legal outcomes of public arbitrations.

<sup>14</sup> Most international investment agreements stipulate at least a 6-month “cooling off” period before filing. I take the public-arbitration filing as a public confirmation that negotiated settlement has failed and the foreign investor perceives that an insufficiently compensated expropriation has taken place. A small number of sovereign bondholders have filed arbitrations, but current thinking among legal practitioners is that this is not a viable means for sovereign bondholders to get settlements (Waibel 2007). As of 2014, none have won a public arbitration.

<sup>15</sup> Poulsen and Aisbett (2013) also focus on developing countries and make an argument that these countries did not anticipate public arbitrations and thus did not alter behavior accordingly. It is prudent to exclude developed countries because, in the time period under consideration, relatively few faced public arbitrations. Most took place between the United States and Canada under the terms of NAFTA, with few in Western Europe and none in Japan. However, developed countries may in the future face more public arbitrations as firms from around the world take advantage of the right of standing investment treaties provide them. UNCTAD, “IIA Issues Note: Investor-State Dispute Settlement,” No. 2, June 2014. If so, future researchers would do well to see if the same dynamics are at play in developed economies.

<sup>16</sup> See Evidence Part II for more information on sample.

<sup>17</sup> See Evidence Part II: Robustness for consideration of the role of Argentina, Venezuela, and Ecuador in the analysis.

<sup>18</sup> In order to categorize arbitrations by type, we augmented the arbitration database with details on the content of the case and characteristics of the investor involved. These details were first culled from court documents. Where unavailable, searches of local and international business press provided details.

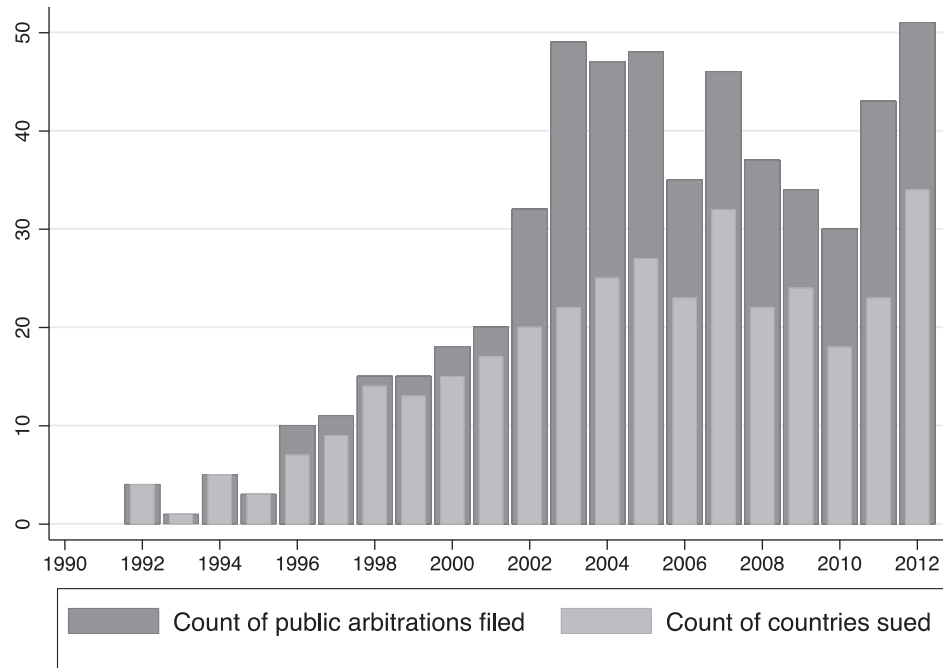


FIG 1. Count of Public International Investment Arbitrations and Countries Sued (1990–2012). Figure includes all public arbitrations worldwide.

Sources: Hajzler 2012; Minor 1994; ICSID, UNCTAD Database of Treaty-based Investor-State Dispute Settlement Cases, Author's records

TABLE 1. Maximum Sample Countries and Count of Public Arbitrations Filed Against (1995–2011)

Country	Count	Country	Count
Argentina	56	Kazakhstan	8
Belize	0	Lebanon	1
Brazil	0	Lithuania	1
Bulgaria	3	Malaysia	2
Chile	3	Mexico	16
China	1	Nigeria	1
Colombia	0	Pakistan	5
Croatia	0	Panama	1
Ecuador	21	Peru	11
Egypt	14	Philippines	3
El Salvador	3	Poland	4
Gabon	2	Russia	8
Georgia	7	Serbia	3
Ghana	1	South Africa	2
Hungary	6	Sri Lanka	2
Indonesia	4	Turkey	8
Iraq	0	Ukraine	11
Ivory Coast	1	Uruguay	1
Jamaica	0	Venezuela	27
		Vietnam	0

(Note. Data are limited by the availability of JP Morgan EMBI+ sovereign bond indices).

“revenue” when the underlying action either generated revenue for the host government or reduced the host government’s liabilities. Revenue expropriations occur when a government action allows it to avoid liabilities, say, by breaking contracted investment incentives or unilaterally canceling a contract. They also occur when the government forces sales of new equity stakes to government actors, demands taxes in excess of contracted amounts, or otherwise acquires property without due compensation. These actions have long-term balance

sheet implications. Offending governments avoid ongoing payments, benefit from additional tax revenue, and gain access to firms’ profits. Expropriation today is not about nationalizing a “pot of gold” but rather about changing the distribution of gains from foreign investment. Table 2A describes four revenue arbitrations from the data set, emerging from excess tax demands, the denial of subsidies, extra-contractual fees, and the withdrawal of tax breaks.<sup>19</sup>

“Non-revenue” arbitrations generally arise when the government changes policies in such way that a foreign firm’s property is discriminatorily devalued. In the course of changing policy, however, the government does not directly gain revenues or avoid liabilities. Table 2B lists several examples from the data set, demonstrating that non-revenue arbitrations can arise from licensing issues, when the government changes its mind on a contract or set of policies, or even perhaps strange scenarios in which foreign property was inadvertently involved with government action.<sup>20</sup>

### Evidence Part I: Revenue Through Expropriation

To establish that sovereign bondholders might sometimes reward expropriation, we first need evidence that governments can raise significant amounts of revenue from expropriation and that those revenues at least

<sup>19</sup> What if one government action triggers more than one public arbitration—as in the case of Ecuador’s “Law 42” (see Table 2A)? The theory is agnostic as to how (or how many) government action(s) caused expropriations and rather focuses on how many investors felt sufficiently expropriated to file public arbitrations. Evidence Part II: Robustness considers alternatives to an additive count of public arbitrations.

<sup>20</sup> Note that the theory in this article, and the coding of public arbitrations, is agnostic as to the government’s motivations for expropriation beyond the implicit revenue motivation. Whether economic nationalist, corruption, or other motivations differ systematically with revenue status is an area for future research.



TABLE 2. (A) Examples of Revenue-raising Expropriations Leading to Public Arbitrations. (B) Examples of Non-revenue-raising Expropriations Leading to Public Arbitrations

<i>Host</i>	<i>Year</i>	<i>Home</i>	<i>Investor's Perspective</i>	<i>Case</i>
(A)				
Ecuador	2008	Spain	Hydrocarbons "Law 42" elevated taxes on windfall profits to 50 percent and then to 99 percent	<i>Repsol YPF Ecuador, S.A. and others vs. Ecuador and Empresa Estatal Petróleos del Ecuador</i> (ICSID Case No. ARB/08/10, among others)
Czech Republic	2004	Netherlands	Beginning in 2000, Czech sugar regulatory regime did not apply European Union subsidy for sugar beet production to the investor's produce	<i>Eastern Sugar B.V. vs. Czech Republic</i> (SCC Case No. 088/2004)
Lebanon	2007	Italy	Highway contract signed in 1997. Investor faced increased customs duties, increased government fees, and increased diesel prices relative to contracted terms	<i>Toto Costruzioni Generali S.p.A. vs. Lebanon</i> (ICSID Case No. ARB/07/12)
Panama	2006	USA	Signed contract to build and operate diesel-powered generation plant. Investor never received promised tax breaks or bonds and, as a result, effectively went bankrupt	<i>Nations Energy, Inc. and others vs. Panama</i> (ICSID Case No. ARB/06/19)
(B)				
Chile	2004	Spain	Fisheries investor claimed discrimination in not allowing a fishing license for certain offshore waters	<i>Sociedad Anónima Eduardo Vieira vs. Chile</i> (ICSID Case No. ARB/04/7)
Lithuania	2005	Norway	Vilnius issued a tender for parking system but eventually canceled the contract, which had been backed by the Lithuanian government	<i>Parkerings-Compagniet AS vs. Lithuania</i> (ICSID Case No. ARB/05/8)
Slovak Republic	2008	Netherlands	Legislative reforms to the insurance market prevented the investor from distributing profits to its shareholders, lowering the value of these businesses	<i>HICEE vs. Slovak Republic</i> (UNCITRAL)
Costa Rica	2007	Canada	Canadian investors, in what turned out to be a Ponzi scheme, filed against the government once the local police took down the scheme	<i>Alasdair Ross Anderson and others vs. Costa Rica</i> (ICSID Case No. ARB(AF)/07/3)

sometimes go to government coffers. Emerging market fund managers acknowledge that expropriations—and the revenues from them—are relevant for their business.<sup>21</sup> One fund manager who invests only in emerging market sovereign debt puts it very clearly: "If expropriation brings revenue to the government, it is positive for the government balance sheet. The negative impact is a loss on the corporate side, but there is a gain on the government side." This manager cares that "property rights are strong in general" but ultimately "pays attention to specific events"—and particularly the revenues generated by specific expropriation events.<sup>22</sup>

An investor into Emerging Asia emphasizes that, because the debt-service ratio factors into his expectations of the movement of sovereign bond spreads, government revenue generation can outweigh the property rights aspects of expropriation.<sup>23</sup> I take this investor's focus on the debt-service ratio (public and publicly guaranteed

debt as a percentage of the export of goods and services) as representative of the industry. Are revenue expropriations indeed associated with improvements in this ratio?<sup>24</sup> If so, we have evidence, consistent with H2, that such expropriations carry a positive signal about a government's ability to service its debt. I hypothesize that revenue expropriations may be associated with decreases in the debt-service ratio numerator, to the extent that governments use at least some of the additional revenue generated to address outstanding debt. For example, Indonesia effectively lowered its debt when it suspended a 30-year power purchase agreement in the wake of the Asian financial crisis, after it could no longer meet US dollar obligations.<sup>25</sup> Revenue expropriations may also be associated with an increase in the denominator, to the extent that increased control over assets might allow the government to redirect those assets toward exports. For example, Venezuela took over a gold mine that had been left inactive due to a fall in commodity prices.<sup>26</sup> In contrast, there is no theoretical expectation that non-revenue expropriations have an effect on the debt-service ratio.

To examine the relationship between revenue expropriation and the debt-service ratio, I use a time-series

<sup>21</sup> The paper draws on five interviews with emerging market fund managers, who were contacted via a "snowball" process beginning with a recommendation from the University of Texas at Austin Alumni Association. Interviews were conducted over the phone with email follow-ups (October–November 2013). Respondents were asked whether and how they account for expropriation in their business decisions. They were also presented with the results of the statistical analysis in Evidence Part II and asked for their reactions.

<sup>22</sup> Interview, emerging market fund manager, 9 years in the industry, October 2013.

<sup>23</sup> Interview, emerging market fund manager, 3 years in the industry, November 2013.

<sup>24</sup> A lower debt-service ratio is preferable, because more funds are available to service debt principal and interest.

<sup>25</sup> *Himpurna California vs. Indonesia* (1998, UNCITRAL).

<sup>26</sup> *Vannessa Ventures Ltd. vs. Venezuela* (2004, ICSID Case No. ARB(AF)/04/6).

cross-sectional estimation strategy with a lagged dependent variable, lagged explanatory variables, country fixed effects, and robust standard errors clustered by country.<sup>27</sup> The dependent variable is the *Debt-service ratio*.<sup>28</sup> Covariates of interest are *Total arbitrations*, *Revenue arbitrations*, and *Non-revenue arbitrations*, which are averages of the number of arbitrations outstanding across each of the 12 months of the year.<sup>29</sup> I report both reduced form models and a model with a battery of covariates that may have an effect on the debt-service ratio: (*logged*) *GDP per capita*, *GDP growth*, *Terms of trade* (value of exports/value of imports), and *Reserves per GDP*, improvements in all of which may reduce the debt-service ratio.<sup>30</sup> Additionally, I include (*logged*) *Lending to emerging markets* as a covariate, in case macro-changes in the availability of capital to emerging markets introduce bias (Oatley 2011).<sup>31</sup>

Table 3 presents results. Results on the lagged dependent variable suggest the strong persistence of the debt-service ratio over time, while control covariates in Model 3 are insignificant. In Model 1, we see that all public arbitrations are associated with improvements in the debt-service ratio, but revenue arbitrations are driving this effect (Models 2 and 3). One additional revenue arbitration is associated with a decrease in the debt-service ratio of about 0.4, which is about 3 percent of one standard deviation of the variable. While substantively small, this significant effect nonetheless provides evidence of the theory's plausibility. Revenue arbitrations are associated with improvements in an indicator that matters to sovereign bondholders' behavior. Even if sovereign bondholders do not follow expropriation per se, they may "see" and react to revenue arbitrations thanks to this association. The next step in the argument is to establish whether this association can outweigh the negative signals that expropriation otherwise sends regarding the security of foreign-owned property.

## Evidence Part II: Expropriation, Revenue, and Bond Spreads

In this section, I link the behavior of sovereign bondholders, as captured by bond spreads, to variation in the revenue status of expropriation, as captured by public arbitrations.

### Research Design

I take the dependent variable of (*logged*) spreads on sovereign bonds (monthly, 1995–2011) from JP Morgan's

<sup>27</sup> All results in the article are robust to using unclustered standard errors. Results in Table 3 are robust to replacing the lagged dependent variable with year fixed effects.

<sup>28</sup> All variables are from the World Bank World Development Indicators unless otherwise noted. Results are robust to excluding the top 10 percent of observations of this variable, which account for a long right tail in its distribution. The dependent variable is unavailable for Croatia, Hungary, Poland, and Iraq.

<sup>29</sup> Recall that, although public arbitrations are filed some time after the underlying expropriation event, they remain useful markers of a revenue expropriation to the extent that the event results in a long-term redistribution of income.

<sup>30</sup> Results are robust to multiple imputation on missing data on these covariates (see replication files).

<sup>31</sup> Commercial banks and other lending (PPG + PNG) (current US\$) to middle-income and developing countries. Lending is negative in the early 2000s, peaks around the beginning of the worldwide financial crisis, drops precipitously, but grows again after 2010. I add a constant to avoid losing data when log transforming the variable.

TABLE 3. Effects of Public Arbitrations on Emerging Market Debt-Service Ratios (Annually, 1995–2011)

	<i>Model 1</i>	<i>Model 2</i>	<i>Model 3</i>
Lagged Debt-service ratio	0.602*** (0.098)	0.598*** (0.099)	0.591*** (0.105)
Total arbitrations	−0.393*** (0.085)		
Revenue arbitrations		−0.468*** (0.094)	−0.402*** (0.104)
Non-revenue arbitrations		0.859 (0.819)	0.970 (0.872)
(Logged) GDP per capita			−0.345 (2.901)
GDP growth			0.085 (0.083)
Terms of trade			−4.673 (3.100)
Reserves per GDP			0.992 (3.492)
(Logged) Lending to EM			−0.495 (0.353)
Constant	8.089*** (2.038)	7.952*** (2.021)	26.762 (21.860)
Observations	531	531	507
Countries	35	35	33
R2 (within)	0.39	0.39	0.39

(Note. Robust standard errors clustered by country. Explanatory variables are lagged. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ ).

EMBI+ indices. To be included in the indices, markets must be sufficiently willing to lend to the state for it to have US dollar-denominated Brady bonds, loans, or Euro-bonds with a minimum outstanding face value of US\$500 million.<sup>32</sup> That governments of these sufficiently credit-worthy states are engaging in expropriation and facing public arbitrations highlights the fact that incentives for expropriation (and the ability to act on those incentives) remain widespread. Figure 2 provides an overview of sovereign bond trends for the 39 countries for which data are available, categorized by emerging world region. On average, the costs of sovereign borrowing have been increasing over this period although there have been recent declines for African countries.

I use monthly measures of outstanding public arbitrations as the covariates of interest. I code public arbitrations by the month of their onset until their resolution, whether via legal processes or out of court settlement.<sup>33</sup> In the main analyses, I use public-arbitration count variables to capture the cumulative total of unresolved expropriation incidents, based on the theoretical expectation that the signals sent by public arbitrations are additive.<sup>34</sup>

Unlike the economics literature on sovereign debt, I include in the analysis a state's *Polity score* (−10 to 10). Regime type provides a control for prior expectations about the likelihood that a government would engage in expropriation (Li 2009). I also include *GDP growth*, as the

<sup>32</sup> Bonds must also meet criteria for secondary market trading liquidity. I follow the literature in using spreads on 5-year instruments.

<sup>33</sup> Of the 88 cases on which data are available, the correlation between the number of months the case lasts and award sought is only 0.03. Thus, it does not appear that "bigger" cases take longer.

<sup>34</sup> Outstanding public arbitrations have a mean of 1.5 and range from 0 to 41 in Argentina (April 2005). See Evidence Part II: Robustness for moving-average and other specifications.

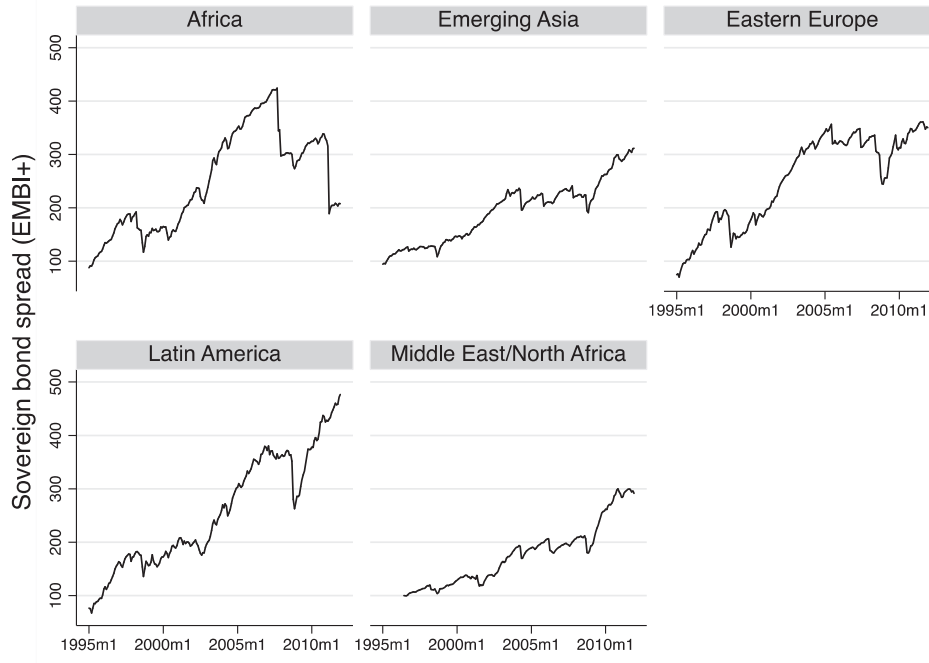


FIG. 2. Sovereign Bond Spreads, by Emerging World Region (1995–2011). Spreads on 5-year instruments.  
Source: JP Morgan EMBI+

general health of the economy may affect incentives to expropriate, though it is not ex-ante clear in which direction: Governments have chosen to expropriate in both bad economic times (as in Ukraine in the late 2000s) and good times (when the fairness of existing contracts may be questioned) (Wellhausen 2015a:18–24). Other controls mimic those in the literature (Hilscher and Nosbusch 2010; Longstaff, Pan, Pedersen, and Singleton 2010; Bai and Wei 2012). As proximity to a sovereign debt crisis certainly influences the cost of sovereign borrowing, I include the *Years since default*, whether via a debt crisis, sovereign debt restructuring, or the initiation of renegotiation with the Paris Club (Fuentes and Saravia 2010; Das, Papaioannou, and Trebesch 2012; Reinhardt and Rogoff 2012). Next, I include the country's *Reserves as a percentage of GDP*, which informs a state's ability to repay its debt. While higher values should tend to be rewarded in bond markets, high values could also have a perverse effect if investors believe a government will soon have trouble repaying and is amassing reserves in advance of that event. I include a state's *Terms of trade*, as negative terms of trade shocks suggest bond market penalties because they can drain hard currency and cause balance of payment problems (Hilscher and Nosbusch 2010:242–245). Following the significant relationship found in Evidence Part I, I include a state's *Debt-service ratio* as well as a state's *External debt per GNI*.<sup>35</sup> Higher values on these would tend to be punished by higher costs to sovereign borrowing. However, an increase in either variable, especially in the short run, might track the idea that a country is a good risk and has been able to borrow accordingly. Finally, to model possible macro-trends at play, I again include (*logged*) *Lending to emerging markets* (Oatley 2011). The research design employs a monthly dependent

variable and monthly public-arbitration variables alongside annual controls.

As explained in De Boef and Keele (2008:189–190), error correction models (ECMs) are appropriate to model time-series cross-sectional data whether or not co-integration is present. The basic ECM model includes both differences and lags of the dependent variable and explanatory variables, as well as the ability to estimate both the short-term and the long-term effects of the variables on the ECM's return to its equilibrium state. Consistent with standard approaches, I lag the annual variables so that they are one year (12 months) behind the values of the dependent variable (and I also difference them over a 12-month period). I include a one-month lag and one-month difference on the dependent variable and the arbitration variables of interest.<sup>36</sup> One-month lags reveal long-term effects of arbitration-generated publicity for different kinds of expropriation, while differences uncover the short-term effects of arbitration on volatile bond markets. The ECM differences each series such that the model sufficiently accounts for any non-stationarity in the data. Because these markets show quarterly seasonality, I include quarter-year fixed effects to account for trends otherwise not captured. Additionally, I include country fixed effects so that identification is off of change within countries and over time.<sup>37</sup> Standard errors are clustered by country. Formally, the overall model specification is as follows.

$$\Delta Y_{i,t} = \alpha_0 + \alpha_1 y_{i,t-1} + \beta_0 \Delta \mathbf{X}_{i,t} + \beta_1 \mathbf{X}_{i,t-1} + \omega_i + \tau_t + \epsilon_{i,t} \quad (1)$$

The term  $\alpha_1 y_{i,t-1}$  is the 1-month lag on the dependent variable, or the error correction term. The next term,

<sup>35</sup> *Debt-service ratio* correlates with *External debt per GNI* at 0.25. Results are robust to excluding *Debt-service ratio* so as to re-include countries for which data are missing. Results are robust to multiple imputation on other covariates that have missing data (see replication files).

<sup>36</sup> Second lags are on the whole not significant and are not significant for variables of interest.

<sup>37</sup> Country fixed effects are especially important as they account for whether a country has a large natural resource endowment, which could affect the propensity to expropriate, expropriation's type, and debt serviceability.

$\beta_0 \Delta X_{i,t^A}$ , contains the short-term effects of explanatory variables, differenced over one or 12 months as appropriate.<sup>38</sup> This term is followed by the lagged terms, again over one or 12 months as appropriate. The next term  $\omega_i$  includes country fixed effects and  $\tau_{t^A}$  includes quarter-year fixed effects.

To estimate models, I use the Stata package *xtpmg* (Blackburne and Frank 2007). This package reports coefficients on differenced covariates,  $\beta_0$ . It also directly reports the long-run multiplier (LRM) for each covariate. The LRM is the total long-term effect of X on Y, equal to the coefficient  $\beta_1$  divided by the absolute value of the error correction rate,  $\alpha_1$ . The ECM readjusts to its long-term equilibrium at the speed determined by the error correction rate. Since the dependent variable is the logged value of bond spreads, effect sizes are best reported as the percent change in bond spreads, calculated by  $e^{(v)} - 1$  where  $v$  is either the value of  $\beta_0$  or the LRM.

### Results

Table 4 summarizes regression results that test H1 and H2. In Model 4, we see that outstanding arbitrations as a unified category have a significant and rewarding effect on the cost of sovereign borrowing in the long term. One additional public arbitration is associated with a long-term 2.9 percent decrease in sovereign bond spreads. Model 5, however, demonstrates that the negative sign on total arbitrations is due to the subset of revenue arbitrations, whereas non-revenue arbitrations have a punishing effect. One more revenue arbitration is associated with a reward for borrowing governments, lowering bond spreads 3.9 percent in the long term. Because the theory predicts that bondholders see revenue generation as a counterweight to the negative signal of a property rights violation, it follows that the effect size on revenue arbitrations should be smaller than that of non-revenue arbitrations. Indeed, one more non-revenue arbitration increases bond spreads by 20.9 percent in the long term. In Model 4 as well as Model 5, changes in the bond spread occur slowly, at a rate of about 3 percent of the change per month for about 33 months. The long-term rather than short-term effects of public arbitrations are consistent with the reasoning that revenue arbitrations are about the long-term redistribution of income.<sup>39</sup> Indeed, when asked about these long-term findings, one emerging market fund manager was unsurprised, saying that other considerations—like the performance of similarly situated emerging economies—shapes short-term variation.<sup>40</sup> In contrast, markets tend to factor in overall “perceptions of risk and reward” in the long term.<sup>41</sup>

A state’s Polity score is a significant long-term predictor of lower spreads, while more lending to emerging markets in general is associated with higher long-term

<sup>38</sup> For concision,  $t^A$  marks terms that include measures of time other than month-year.

<sup>39</sup> Hungary provides some suggestive evidence in line with this finding. Throughout much of 2006 and 2007, Hungary’s rapidly increasing sovereign borrowing costs slowed. This corresponded in time with political actions and lawsuits over changed electricity pricing and the cancellation of promised power plant retrofits. *AES Summit Generation Ltd et al. vs. Hungary* (ICSID Case No. ARB/07/22) and *Electrabel SA vs. Hungary* (ICSID Case No. ARB/07/19).

<sup>40</sup> For evidence consistent with a short-run effect of emerging market “categorization” on bond spreads, see Brooks, Cunha, and Mosley (2014).

<sup>41</sup> Interview, emerging market fund manager, 12 years in the industry, October 2013.

TABLE 4. Arbitration<sup>†</sup> Determinants of (Logged) Sovereign Bond Spreads (Monthly, 1995–2011)

		Model 4	Model 5	
Difference <sup>‡</sup>	Total arbitrations	−0.001 (0.002)		
	Revenue arbitrations		−0.000 (0.003)	
	Non-revenue arbitrations		0.000 (0.004)	
	Polity	−0.001* (0.001)	−0.002* (0.001)	
	GDP growth	0.001** (0.000)	0.001** (0.000)	
	Years since default	0.002 (0.002)	0.002 (0.002)	
	Reserves per GDP	0.096*** (0.036)	0.101*** (0.037)	
	Terms of trade	0.003 (0.005)	0.004 (0.005)	
	Debt–service ratio	−0.040** (0.018)	−0.039** (0.017)	
	External debt per GNI	0.000 (0.000)	0.000 (0.000)	
	(Logged) Lending to EM	−0.005 (0.003)	−0.005 (0.003)	
	Error correction term	−0.028*** (0.009)	−0.030*** (0.009)	
	Constant	0.041 (0.044)	0.050 (0.044)	
	LRM <sup>¶</sup>	Total arbitrations	−0.029*** (0.008)	
		Revenue arbitrations		−0.039*** (0.010)
Non-revenue arbitrations			0.209** (0.099)	
Polity		−0.030* (0.016)	−0.037** (0.017)	
GDP growth		0.017 (0.014)	0.017 (0.013)	
Years since default		−0.014 (0.024)	−0.011 (0.023)	
Reserves per GDP		0.536 (1.049)	0.600 (1.045)	
Terms of trade		0.094 (0.292)	0.228 (0.263)	
Debt–service ratio		0.365 (0.543)	0.433 (0.489)	
External debt per GNI		0.002 (0.003)	0.001 (0.003)	
(Logged) Lending to EM		0.164** (0.071)	0.144** (0.066)	

(Notes. Observations: 4470 (33 countries).

Quarter-year and country fixed effects. Standard errors clustered by country. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

<sup>†</sup>An arbitration observation is the count of outstanding public arbitrations a country faces, by month. Non-arbitration covariates are measured annually.

<sup>‡</sup>Short-term effect.

<sup>¶</sup>LRM (long-run multiplier) =  $\beta_1/|\alpha_1|$ .

spreads. In the short term, the Polity effect remains, while GDP growth is associated with higher spreads. The short-term signs on reserves per GDP and the debt–service ratio are at first unexpected. However, a country amassing reserves may be one that markets worry cannot repay its debts, while a country with more external debt relative to exports may be one that just increased its borrowing. In other words, there are reasons to expect these



signs, especially in the short term; indeed, these covariates are significant only when considering month-on-month changes.

Since Model 4 demonstrates that aggregate arbitrations have a significant and negative effect on bond spreads in the long term, we might worry that the effect of revenue arbitrations is acting through the number of total arbitrations, such that including the mediating variable of total arbitrations would wipe out the effect of revenue arbitrations on spreads. My expectation is that in causal mediation analysis, the treatment variable, status as a revenue arbitration (binary), should retain a direct effect on the outcome even when controlling for the count of public arbitrations a country faces. To test this, I collapse the data and apply causal mediation analysis with the *medeff* package, which follows a two-step process: First, it generates predictions for the effect of the count of public arbitrations in both the treatment and control conditions; second, the outcome model uses that input to predict outcomes of interest (Hicks and Tingley 2011; Imai, Keele, Tingley, and Yamamoto 2011). The analysis controls for the same (lagged) covariates as in Table 4 and clusters by country.

Table 5 shows point estimates and 90 percent confidence intervals after 1000 simulations. The direct effect is the effect that status as a revenue arbitration has on sovereign bond spreads when controlling for the mediating variable, the count of total public arbitrations. That this effect is significantly different from zero demonstrates that the count of public arbitrations does not fully mediate (that is, wipe out) the effect of revenue status. Moreover, the direct effect is greater than the total effect, indicating that mediation is inconsistent: The mediator variable, which includes revenue and non-revenue arbitrations, is suppressing the effect of the treatment.<sup>42</sup> The indirect effect, or the Average Causal Mediation Effect (ACME), is the effect of the treatment on the outcome variable that acts through the mediator. The negative sign on the point estimate occurs because the effect of the treatment on bond spreads is negative. That the ACME is outside standard levels of statistical significance suggests that the causal pathway via the total number of arbitrations is less important than the direct effect of the treatment. The last line captures the amount of the total effect mediated—that is, how much of the effect is attributable to total public arbitrations and how much to revenue status. Only 30 percent of the overall effect comes from total public arbitrations, leaving 70 percent accounted for by revenue status. Moreover, the sign is negative, again indicating that inconsistent mediation is taking place. With this analysis, we can have confidence that the effects of revenue generation are not wiped out by the presence of public arbitrations themselves but rather have an independent effect on sovereign bond spreads.

### Robustness

Several robustness tests show the following: First, results are generally robust to moving-average measures of public arbitrations; second, public-arbitration resolutions are minimally associated with movements in emerging market sovereign bond spreads; third, the distinction between revenue and non-revenue arbitrations remains when

TABLE 5. Causal Mediation Analysis of Revenue (Treatment) via Arbitration Count (Mediator) on (Logged) Sovereign Bond Spreads (Monthly, 1995–2011)

Effect	Mean	[90% Conf. Interval]	
Direct effect	0.311	0.138	0.488
Total effect	0.237	0.040	0.438
ACME	-0.074	-0.177	0.019
Pct of total effect mediated	-0.304	-1.160	-0.160

(Note. Observations: 4298 (33 countries). ACME, Average Causal Mediation Effect).

manipulating the sample and covariates; and fourth, holders of emerging market sovereign debt credit default swaps (CDS) do not appear to take long-term signals from public arbitrations (see Appendix).<sup>43</sup>

First, while the analyses in Table 4 conceptualize the signal sent by public arbitrations as additive, one might hypothesize that there is a decay effect. In Table 6, I use the six-month moving average of the number of public arbitrations outstanding, with a mean of 0.04 public arbitrations in a six-month period (and Argentina facing the maximum of 3.2 in 2 consecutive months). With this measure, the long-term rewarding effect of revenue arbitrations is robust, while non-revenue arbitrations have positive but not significant effects though the coefficients are significantly different (99 percent confidence). That bondholders' sometime discounting of direct investors' property rights holds with this different measure provides confidence that variation in investor preferences has meaningful effects.

Second, I consider whether public arbitrations provide additional signals to bondholders when resolved. I follow firms and legal practitioners in categorizing arbitrations as investor “wins” if the investor wins an award, the case is declared to be settled, or if the case is withdrawn. Ex-ante expectations are unclear: An investor “win” might bring rewards thanks to the mitigation of uncertainty and evidence that the government complied with the process, but it also brings a drag on government revenues. It is important to note, however, that arbitration awards and settlements are lump sum payments that do not have long-term redistributive effects, suggesting that the effects of awards and settlements would be resolved in the short term. Arbitrations are categorized as a government “win” if the government is declared non-liable as a result of the arbitration process. Again, ex-ante expectations are unclear: A state “win” means the state avoids liability—albeit for a lump sum payment—although the “win” carries with it a stamp of approval for an action that a foreign investor saw as expropriatory.

Table 7A provides summary statistics on the 153 public-arbitration resolutions in the data set. Table 7B presents results. In all models, we see that the cumulative outstanding count of revenue and non-revenue arbitrations continue to have the disparate long-term effects found in Table 4. In general, public-arbitration resolutions have few effects. In Model 9, there is a short-term penalty for the resolution of a non-revenue arbitration, and we see in Model 10 that the effect is driven by the penalty for a state winning a non-revenue arbitration.

<sup>42</sup> David A. Kenny, “Mediation.” [Davidakenny.net/cm/mediate.htm](http://Davidakenny.net/cm/mediate.htm) (Last accessed November 1, 2013).

<sup>43</sup> Fifth, results are not robust to using (i) the date of filing or (ii) the date of the underlying expropriation event as the marker of expropriation (see replication files).

TABLE 6. Arbitration Moving Average (MA)<sup>†</sup> Determinants of (Logged) Sovereign Bond Spreads (Monthly, 1995–2011)

		<i>Model 6</i>	<i>Model 7</i>
Difference <sup>‡</sup>	Arbitration MA	0.001 (0.010)	
	Revenue MA		0.003 (0.011)
	Non-revenue MA		0.009 (0.016)
	Error correction term	-0.026*** (0.009)	-0.026*** (0.009)
	Constant	0.133*** (0.044)	0.132*** (0.044)
LRM <sup>¶</sup>	Arbitration MA	-1.012** (0.446)	
	Revenue MA		-1.297*** (0.498)
	Non-revenue MA		0.872 (0.639)

(Notes. Observations: 4470 (33 countries).

Not reported: Polity, GDP growth, Years since default, Reserves per GDP, Terms of trade, External debt, Debt-service ratio, (Logged) Lending to EM. Quarter-year and country fixed effects. Standard errors clustered by country. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

<sup>†</sup>An arbitration observation is moving average of public arbitrations filed against a country in the last 6 months. Non-arbitration covariates are measured annually.

<sup>‡</sup>Short-term effect.

<sup>¶</sup>LRM (long-run multiplier) =  $\beta_1/|\alpha_1|$ .

Thus, we see penalties in bond markets when a property rights violation is signaled twice-over—by its non-revenue status and with a state win.

Third, I take seriously that Ecuador, Venezuela, and Argentina account for many of the observations of public arbitrations in the data set.<sup>44</sup> Table 8 reports various manipulations of the sample. We see that in Models 11 and 12, results are consistent when excluding Ecuador or Venezuela. In Model 13, however, revenue arbitrations lose their long-term rewarding effects. This suggests that the Argentinian public arbitrations play a large role in the sample.<sup>45</sup> As such, Model 13 requires us to qualify support for H2: Revenue arbitrations are not always associated with rewards in sovereign bond markets. However, these null results are not damning to the argument. There remains a significant difference between the coefficients for revenue arbitrations and non-revenue arbitrations in Model 13 (95 percent confidence). Thus, sovereign bondholders continue to have disparate responses to public arbitrations based on the revenue-generating status of the underlying expropriation, even when excluding Argentina. Informal property rights enforcement generated by sovereign bondholder behavior is not consistent across all types of expropriation. Moreover, Model 14 demonstrates that spreads significantly decrease with more outstanding revenue arbitrations for the three countries that account for the most public arbitrations in this sample. Ex ante, we may expect this subsample to be the least likely to show rewards for

<sup>44</sup> As such, these analyses address the concern that places where expropriation is more common—and, in the case of Argentina, directly related to sovereign debt default—are subject to different investor dynamics. As mentioned in Footnote 27, all results are robust to using unclustered standard errors.

<sup>45</sup> Note that there is not a clear theoretical rationale for excluding Argentina. Argentina's bond spreads are relevant in this period, as the country was not priced out of sovereign debt markets: It issued new debt as the result of debt restructuring processes in 2005 and 2010.

revenue arbitrations: The property rights counterweight to the revenue-generation signal is quite strong. That we find evidence supportive of H2 in Model 14 suggests that the signal provided by revenue generation can be compelling even in countries in which property rights appear quite vulnerable.

Several other covariates do not affect the results of interest. One might argue that if expropriation drives out FDI, the government makes the environment worse for bondholders in the long term. However, the saturation of FDI in the economy (FDI flows per GDP) is an insignificant predictor of spreads and does not affect results of interest.<sup>46</sup> Including Fitch long-term sovereign debt ratings does not affect the results.<sup>47</sup> Nor does categorizing public arbitrations by industry mobility have effects on bond spreads.<sup>48</sup>

## Conclusions

Critics of the de facto international legal regime protecting FDI worry about the power it gives foreign investors, since firms gain standing to bring arbitrations directly against host governments (Simmons 2014:41–43). Yet when firms file public arbitrations, other international financial actors do not necessarily act in ways that punish the host government. For a government interested in foreign borrowing, stable or lower borrowing costs following revenue-generating expropriation could be enough to offset the disruption expropriation causes to FDI. International financial actors can and do interpret government actions significantly differently—to the point where they can be at odds with one another, to the benefit of the host government.

Unpacking international financial actors reveals these divergent and sometimes conflicting investor preferences. The fruits of this research strategy suggest caution when considering investors as a single unit (for example, Frieden and Rogowski 1996; Rodrik 2011). More common are studies that examine investors in one capital flow at a time. However, such a focus may overlook important dynamics that cross-investor comparisons allow us to see.<sup>49</sup> Scholars would do well to consider variation across financial actors' sensitivity to time, macroeconomic health, or particular micro-policy choices, in developing or developed countries. Perhaps other conflicting investor preferences create further points of “escape” from the “straightjacket” of financial globalization. A better understanding of when governments maintain sovereign flexibility speaks to scholarship on globalization and governments' domestic policy choices, such as work on globalization and the welfare state (Swank 2002; Hicks and Zorn 2005; Rickard 2012); globalization and labor policy (Locke 2013); and globalization and “race to the bottom” debates more generally (Basinger and Hallerberg 2004; Rudra 2008).

<sup>46</sup> Nevertheless, the combination of expropriation and lower borrowing costs becomes unsustainable when FDI is sufficiently low to make expropriation unattractive or impossible.

<sup>47</sup> See replication files.

<sup>48</sup> Thus, there is no evidence that sovereign bondholders' responses to expropriation vary depending on the mobility of the expropriated assets, a possible implication of Vernon (1971) and Frieden (1994). Results available from the author.

<sup>49</sup> Notable examples of cross-investor analyses include the consideration of portfolio and direct investors in Ahlquist (2006) as well as Singer (2010) on migrant remittances and exchange rate choices.

TABLE 7. (A) Summary statistics, Arbitration Resolutions in Sample (Observations by month of resolution). (B) Arbitration Resolution<sup>†</sup> Determinants of (Logged) Sovereign Bond Spreads (Monthly, 1995–2011)

<i>Variable</i>	<i>Count</i>	<i>Pct of Total</i>		
<i>(A)</i>				
End of revenue arbitration	113	74		
End of non-revenue arbitration	40	26		
Investor settle/win	98	64		
State win	55	36		
Revenue arbitration + Investor settle/win	79	52		
Revenue arbitration + State win	34	22		
Non-revenue arbitration + Investor settle/win	19	12		
Non-revenue arbitration + State win	21	14		
Total resolutions in sample	153			
<i>(B)</i>				
	<i>Model 8</i>	<i>Model 9</i>	<i>Model 10</i>	
Difference <sup>‡</sup>	Revenue arbitrations	−0.001 (0.003)	−0.002 (0.002)	−0.002 (0.002)
	Non-revenue arbitrations	−0.000 (0.005)	0.004 (0.005)	0.004 (0.004)
	Investor settle/win	−0.001 (0.009)		
	State win	−0.002 (0.002)		
	End of revenue arbitration		−0.006 (0.004)	
	End of non-revenue arbitration		0.008* (0.004)	
	Revenue arbitration + Investor settle/win			−0.005 (0.003)
	Revenue arbitration + State win			0.008 (0.007)
	Non-revenue arbitration + Investor settle/win			−0.007 (0.014)
	Non-revenue arbitration + State win			0.010* (0.006)
	Error correction term	−0.030*** (0.009)	−0.030*** (0.009)	−0.030*** (0.009)
	Constant	0.053 (0.043)	0.049 (0.043)	0.051 (0.042)
	LRM <sup>¶</sup>	Revenue arbitrations	−0.038*** (0.009)	−0.038*** (0.010)
Non-revenue arbitrations		0.211** (0.099)	0.203** (0.097)	0.199** (0.096)
Investor settle/win		0.298 (0.309)		
State win		−0.240 (0.278)		
End of revenue arbitration			−0.049 (0.104)	
End of non-revenue arbitration			0.077 (0.434)	
Revenue arbitration + Investor settle/win				−0.117 (0.228)
Revenue arbitration + State win				−0.699 (1.033)
Non-revenue arbitration + Investor settle/win				0.142 (0.455)
Non-revenue arbitration + State win				0.592 (0.373)

(Notes. Not reported: Polity, GDP growth, Years since default, Reserves per GDP, Terms of trade, External debt, Debt–service ratio, (Logged) Lending to EM. Observations: 4470 (33 countries).

Quarter-year and country fixed effects. Standard errors clustered by country. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

<sup>†</sup>An arbitration resolution observation is the count of arbitrations resolved in a particular month-year. Non-arbitration covariates are measured annually.

<sup>‡</sup>Short-term effect.

<sup>¶</sup>LRM (long-run multiplier) =  $\beta_1/|\alpha_1|$ .

TABLE 8. Arbitration<sup>†</sup> Determinants of (Logged) Sovereign Bond Spreads (Monthly, 1995–2011): Manipulating the Sample

		<i>Model 11</i>	<i>Model 12</i>	<i>Model 13</i>	<i>Model 14</i>
		<i>without Ecuador</i>	<i>without Venezuela</i>	<i>without Argentina</i>	<i>only Ecuador, Venezuela, and Argentina</i>
Difference <sup>‡</sup>	Revenue arbitrations	0.000 (0.003)	−0.001 (0.003)	0.004 (0.003)	0.001 (0.002)
	Non-revenue arbitrations	−0.003 (0.003)	0.001 (0.005)	−0.000 (0.005)	0.003 (0.005)
	Error correction term	−0.028*** (0.009)	−0.029*** (0.008)	−0.036*** (0.009)	−0.114** (0.055)
	Constant	0.055 (0.045)	0.038 (0.045)	−0.057 (0.037)	−0.557 (0.388)
LRM <sup>¶</sup>	Revenue arbitrations	−0.038*** (0.011)	−0.045*** (0.014)	0.014 (0.020)	−0.019*** (0.003)
	Non-revenue arbitrations	0.192* (0.102)	0.217** (0.106)	0.139** (0.066)	−0.021 (0.068)

(Notes. Not reported: Polity, GDP growth, Years since default, Reserves per GDP, Terms of trade, External debt, Debt-service ratio, (Logged) Lending to EM. Quarter-year and country fixed effects. Standard errors clustered by country. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ ).

<sup>†</sup>An arbitration observation is the count of outstanding public arbitrations a country faces, by month. Non-arbitration covariates are measured annually.

<sup>‡</sup>Short-term effect.

<sup>¶</sup>LRM (long-run multiplier) =  $\beta_1/|\alpha_1|$ .

This article demonstrates how important conflicting investor preferences and resulting points of sovereign “escape” can be. Heterogeneity across international financial actors’ responses to property rights violations implies that property rights can be differentiable, resulting in the breakdown of mechanisms of informal property rights enforcement. In the context of economic development, these results suggest that strategies prioritizing property rights above all may require governments to trade off potential advantages in sovereign debt markets. Maintaining a fundamental developmental institution can constrict access to a key source of capital for capital-hungry governments. A government may prefer to satisfy domestic incentives to expropriate, generate revenue from that expropriation, and earn rewards or at minimum indifference in international debt markets. Far from constraining

the state’s freedom to act, economic integration can enable the state to enjoy benefits from some financial actors even while undermining the property rights of others.

## Appendix

Here, I look at what effects public arbitrations might have on the market for sovereign debt credit default swaps (CDS). One can think of bond prices as top-censored, as excess upward pressure on prices gets diverted into CDS markets. CDS markets are bottom-censored: If bondholders feel secure, they need not buy insurance. Bottom censoring may thus exclude those investors most sensitive to changes in political risk brought about by expropriation,

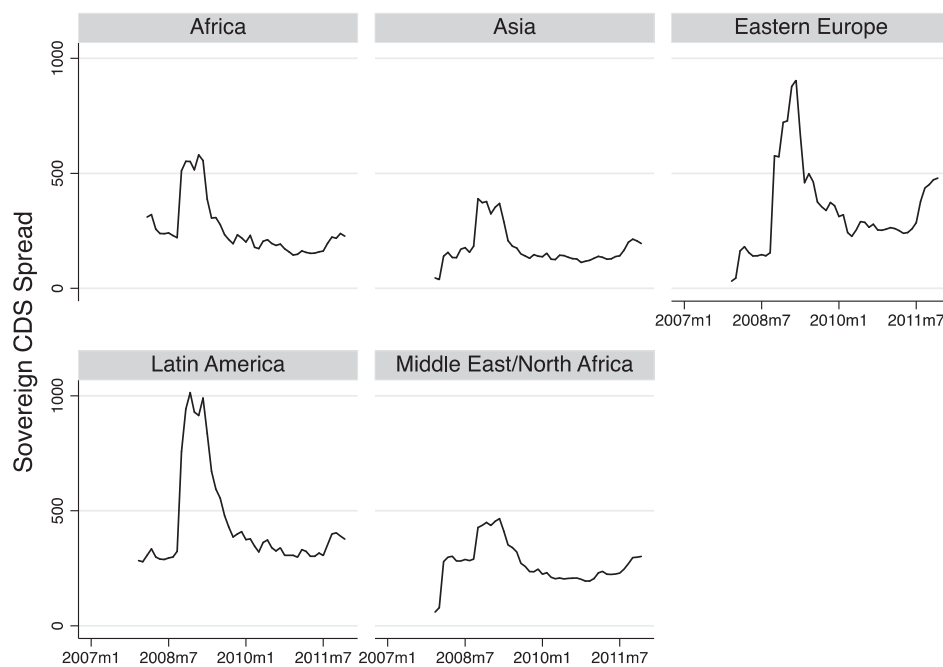


FIGURE A1. Sovereign CDS Spreads, by Emerging World Region (2007–2011). Spreads on 5-year instruments.

Source: Thompson Reuters



TABLE A1. Summary of Variables of Interest in Sample when Dependent Variable is (Logged) Sovereign Debt CDS (Monthly, 2007–2011)

	<i>Count</i>	<i>Countries experiencing action*</i>
Total arbitrations	48	15
Revenue arbitrations	36	9
Non-revenue arbitrations	12	8

(Notes. \*Out of 22 possible countries).

TABLE A2. Arbitration Determinants of (Logged) Sovereign CDS Spreads

	<i>Model A1</i>	<i>Model A2</i>		
Difference <sup>†</sup>	Total arbitrations	0.004 (0.019)		
	Revenue arbitrations		−0.008 (0.023)	
	Non-revenue arbitrations		0.036** (0.017)	
	Polity	−0.006 (0.012)	−0.009 (0.012)	
	GDP growth	−0.010*** (0.003)	−0.010*** (0.003)	
	Years since default	0.018 (0.030)	0.015 (0.029)	
	Reserves per GDP	−0.251 (0.228)	−0.210 (0.231)	
	Terms of trade	0.003 (0.145)	0.026 (0.146)	
	Debt–service ratio	0.378* (0.193)	0.393** (0.186)	
	External debt per GNI	−0.003** (0.001)	−0.003** (0.001)	
	(Logged) Lending to EM	−0.138*** (0.029)	−0.137*** (0.029)	
	Error correction term	−0.348*** (0.036)	−0.348*** (0.036)	
	Constant	1.742** (0.809)	1.704** (0.813)	
	LRM <sup>¶</sup>	Total arbitrations	0.002 (0.017)	
		Revenue arbitrations		−0.011 (0.022)
Non-revenue arbitrations			0.022 (0.039)	
Polity		−0.018 (0.022)	−0.031 (0.025)	
GDP growth		−0.036*** (0.014)	−0.035*** (0.013)	
Years since default		−0.116*** (0.036)	−0.118*** (0.036)	
Reserves per GDP		−0.964 (0.589)	−0.910 (0.606)	
Terms of trade		−0.181 (0.431)	−0.065 (0.479)	
Debt–service ratio		0.991*** (0.365)	0.966*** (0.347)	
External debt per GNI		−0.021*** (0.006)	−0.020*** (0.005)	
(Logged) Lending to EM		0.090 (0.061)	0.093 (0.060)	

(Notes. Observations: 982 (22 countries).

Quarter-year and country fixed effects. Standard errors clustered by country. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

<sup>†</sup>An arbitration observation is the count of outstanding public arbitrations a country faces, by month. Non-arbitration covariates are measured annually.

<sup>‡</sup>Short-term effect.

<sup>¶</sup>LRM (long-run multiplier) =  $\beta_1/|z_1|$ .

suggesting weaker effects of public arbitrations in sovereign debt CDS markets. (Logged) 5-year sovereign debt CDS spreads are taken from Thompson Reuters (2007–2011). The modeling strategy is unchanged. Controlling for macroeconomic factors corrects for their omission in the recent literature (Bai and Wei 2012). Figure A1 and Table A1 describe variation in the dependent variable and in public arbitrations in the sample. As reported in Table A2, non-revenue arbitrations are associated with higher spreads in the short term, but there are no significant long-term effects. Additionally, the Polity score does not reach significance, which calls into question previous results that aspects of democracy have significant effects on sovereign debt CDS (Ibid). These results generate suggestive evidence that the preferences of direct investors and CDS holders are aligned in the short term, but only with regard to non-revenue arbitrations. There is no evidence that their preferences are aligned in the long term. We would expect significant punishments for public arbitrations, of whatever type, if that were the case. At best, holders of sovereign debt CDS appear to be weak participants in market-based, informal FDI property rights enforcement.

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