Does regulatory cooperation help integrate equity markets?

By Roger Silvers*

Abstract: In this study, I test for the effect of cooperation between securities regulators on market integration. Cooperative arrangements between securities regulators enable (i) enhanced cross-border enforcement, (ii) better regulatory decisions through learning and shared experiences, and (iii) reduced regulatory red tape in cross-border activities. The formation of these arrangements signifies changes in cooperative capacity within the time series of country-pairs and occurs at different times for different country-pairs. I find that these staggered events are associated with an 11% increase in cross-border equity investment. Asset pricing tests reveal a shift in risk exposures from local to global market indices, consistent with enhanced market integration. Cross-sectional tests show patterns consistent with stronger effects in contexts where (1) cooperation is likely to be effective and (2) risks of exploitation and information asymmetry are likely to be greatest. Cross-border investment and market integration thus depend, in part, on regulatory counterparts working together to extend legal and institutional capacities across borders.

Keywords: cross-border cooperation, regulatory networks, integration, capital mobility

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I. Introduction

Theory suggests that global integration of capital markets provides important benefits. Cross-border investment helps firms raise more capital at lower costs while allowing investors to diversify their portfolios and access higher yields than in domestic markets (Grauer et al. 1976; Errunza and Losq 1985; Alexander et al. 1987). Yet investors still tend to significantly overweight local assets, leaving the benefits of international diversification partially unrealized, both for them and for firms (Karolyi and Stulz 2003). Multiple overlapping literature streams explore why investors forgo the benefits of diversification.[1] Frictions such as capital controls, political risk, taxes, transaction costs, information asymmetry, and fear of expropriation all appear to play roles.

In domestic settings, securities regulators moderate many of these frictions as part of their mandate to facilitate capital formation, promote fair and liquid capital markets, and protect investors. They select policies that influence disclosure requirements, political risks, investment restrictions, information asymmetry, and transaction costs. They balance effective and onerous requirements for local market participants. And they enforce laws that target various exploitative behaviors (insider trading, front running, cyberattacks, etc.).

In cross-border settings, however, regulators' ability to unilaterally moderate these frictions is often limited. Regulatory requirements that are effective in one jurisdiction may, in conjunction with another country's requirements, prove burdensome, duplicative, and costly. In foreign jurisdictions, regulators have no legal right to acquire information or execute the tactics required for investigation and prosecution, and must turn to local authorities for assistance. In the past, regulators could expect little, if any, support from foreign counterparts, so the prospects for effective policy coordination or investigations were bleak. Wrongdoers who recognized cross-border regulatory gaps could, with virtual certainty, use them to evade repercussions. Thus, even between two countries with effective local regulation, market integration may depend (in part) on resolving the regulatory frictions between them.

In this paper, I study the potential for regulatory cooperation to resolve cross-border investment frictions and thereby enhance market integration. I evaluate changes in (i)

[1] Prior work frames global market integration in the context of asset pricing (Black 1974; Solnik 1974; Brennan et al. 1977; Stulz 1981; Dumas and Solnik 1995; Bekaert and Harvey 1995; Bekaert et al. 2002; Bekaert et al. 2011), cross-listing (Karolyi 2006; Lewis 2017), capital mobility (Feldstein and Horioka 1980; Gordon and Bovenberg 1996; Obstfeld and Taylor 2005; Bayoumi et al. 2015), foreign portfolio allocation (Adler and Dumas 1983; Stulz 1995; Brennan and Cao 1997; Portes and Rey 2005; Daude and Fratzscher 2008; Lane and Milesi-Ferretti 2008a, 2008b, 2017), home bias (French and Poterba 1991; Bekaert and Wang 2012; Coeurdacier and Rey 2013), and international capital flows (Chuhan et al. 1998; Alfaro et al. 2007; Edison and Warnock 2008; Coppola et al. 2019).

investors' cross-border ownership, and (ii) firms' market-risk exposures to local and global indices in asset-pricing tests. To measure changes in cooperation policy, I exploit the cooperative arrangements called "memoranda of understanding" (MoUs), which securities regulators use to address cross-border frictions. Regulators claim that such arrangements enhance enforcement capacity, improve regulatory decisions (by leveraging shared experience), and reduce red tape, which in turn builds "investor confidence" in foreign investment (SEC 2010). Consistent with this view, I find support for the idea that regulatory cooperation resolves frictions and promotes market integration.

An MoU is a reciprocal statement of intent to cooperate, collaborate, and share information in connection with regulatory and enforcement issues. Though not legally binding, MoUs address cross-jurisdictional legal incompatibilities and enhance various regulatory tactics between the involved nations (without requiring harmonization or convergence). MoUs improve cross-border enforcement across a wide range of cases and countries (Silvers 2020). The formation of MoUs also marks changes in cross-border capacities for pairs of countries at precise points in time, creating a complex treatment pattern that is staggered across time and country pairs. This unusual pattern helps me to identify the effect of cooperation policy from time-series variation in investment between pairs of countries.

An obvious concern with using cooperative arrangements to measure cooperation is that, like any institutional attribute, they could arise out of an endogenous process. To help mitigate this issue, I draw inferences only from the International Organization of Securities Commissions' (IOSCO's) *Multilateral* Memorandum of Understanding (*M*MoU), because prior research argues that the country-pair links formed by the *M*MoU are largely exogenous to investors, firms, and even regulators (Silvers 2020). Typically, market forces dictate a regulator's policy agenda. But the *M*MoU was created after the events of 9/11 intensified geopolitical attention on terrorism and money laundering. Instead of being market-driven, the push for the *M*MoU came top-down from heads of state. The *M*MoU also has wide participation, with 116 different countries forming over six thousand country-pair linkages as of January 2020. Thus, each signatory has 115 connections with other signatories, formed at different times from October 2002 to the present.

Several factors affect the timing of a country's *M*MoU admission. First, there is the decision to join; this is generally dictated by top-down political agendas over which market participants have minimal sway. Once the decision has been made, the timing is subject to

idiosyncratic factors such as the country's capacity to comply with *M*MoU standards and adeptness in remedying arcane laws that prevent information sharing with foreign authorities. The workloads of the applicant country's staff and of the *M*MoU verification team members (who have full-time duties as regulatory staff members) may also have an effect. In a given country pair, a link is formed only when regulators from both countries have been independently admitted to the *M*MoU. All of these factors indicate that the *M*MoU linkages are plausibly exogenous, at least with respect to cross-border investment. In sum, the *M*MoU setting allows for credible insights about how efforts that allow institutional features to transcend jurisdictional boundaries impact market integration.

My first analyses explore the effect of cooperation on market integration from the perspective of the *investor*. Exploiting the staggered country-pair shocks created by the *M*MoU, I examine foreign portfolio investment (FPI). The design compares time series changes in FPI for a cooperating pair with time series changes in FPI for a counterfactual benchmark (country pairs that share either the same investee or investor country as the treated pair). This is achieved using three-way fixed effects for (i) country pairs, to control for time-invariant country-pair characteristics; (ii) investee×time, to control for "pull" factors (unobserved changes in an invest*ee* country's economic conditions); and (iii) investor×time, to control for "push" factors (changes in outbound FPI that are common to all invest*ee* countries). This generalized difference-in-difference design also helps to rule out country-level omitted variables (e.g., laws, domestic yields, economic conditions, or foreign investment policies), since these factors should similarly affect investment to (and from) counterparts. Bilateral MoUs capture the same theoretical construct as the *M*MoU but are more likely to be subject to endogeneity concerns. Thus, they are included only as controls.

Although concerns about omitted variables and reverse causality cannot be ruled out, they are mitigated by the elaborate network-formed linkage pattern and features of the research design. To bias the estimates, an omitted variable would have to affect specific pairs of countries at the time they experience the shock, but not the counterfactual country-pairs (country-pairs that include either the same investee or investor country).[2]

Using Poisson pseudo-maximum likelihood (PPML) estimation (Gourieroux et al.

[2] Due to the multilateral nature of the *M*MoU, if a given country was enticed by a single counterpart to enter the *M*MoU, the effect would be counteracted by 114 other linkages that are not subject to this bias. Thus, a single endogenous linkage would need to have an extraordinary magnitude to impart a substantial bias on the estimate. Multiple endogenous linkages would need to map onto a very unique sequence and timing across country pairs. This seems unlikely. These arguments also apply to reverse causality (e.g., joining the *M*MoU in response to investment), as regulators would need to reverse engineer the alignment of multiple events (many of which occur in the future, and are thus beyond the applicant's control).

1984; Silva and Tenreyro 2006), I find that *M*MoU linkages (bilateral MoUs) are associated with an 11% (9%) increase in FPI, relative to the changes that occur in the time series of benchmark country pairs. This is consistent with regulatory cooperation resolving investment frictions that might otherwise prevent investors from diversifying their portfolios across borders. This supports the diversification cost-benefit tradeoff presented in Brennan and Cao (1997). Over the sample period from 2001 to 2017, the average FPI across all countries is $16.8 trillion. Thus, the 11% increase attributable to cooperation policy equates to about $1.8 trillion. I observe no effect when capital controls are in place. Larger effects occur where cooperation is expected to be most effective (e.g., between developed market regulators) and where information and expropriation risks are likely to be pronounced (e.g., between country pairs that are geographically more distant and for weak rule of law investee countries). The bulk (78%) of the effect occurs during the year of the treatment, and placebo tests show that it is unique to the specific sequence and timing of the *M*MoU.

An emerging literature on regulatory cooperation starts with Silvers (2020), which shows that cross-border cooperation increases enforcement and enhances liquidity for firms cross-listed between participating countries. Because the enhanced enforcement resulting from the *M*MoU is a non-excludable public good, Lang et al. (2020) use the setting to explore the possibility of a regulatory spillover that affects investors from third-party countries (those unaffiliated with the US or the cross-listed firms' country). They find that mutual funds from third-party countries shift their existing holdings of a country out of non-US-cross-listed and into US-cross-listed firms in the same country. From a country-level perspective, this exclusively within-country reallocation implies that cooperation has *no net effect* on diversification, cross-country risk exposures, or market integration.[3]

Building on the idea that cooperation affects investors' preferences, I use a different research design and larger sample to answer a different research question: "Does market integration depend on regulatory cooperation?" My results imply country-level welfare effects, whereas the within-country reallocations of existing investment found in Lang et al. (2020) do not. The increased cross-country diversification that I observe, unlike the within-

[3] It might seem predictable that Lang et al.'s result would extend to cross-country changes, but a supplemental analysis in their paper explores this question and finds no cross-country portfolio reallocation. Lang et al. note that their test, constructed differently than mine, is prone to endogeneity from investee-country-level factors, and that their research design, outcome variable, and specification are ill-suited to identify cross-country changes. My analyses do not focus exclusively on cross-listings and are therefore not subject to the firm-level data requirements that limit the Lang et al. sample to less than a tenth of total FPI. Aside from the obvious benefit of a larger sample, using the full sample of FPI also provides a more fully populated matrix of country-pair ownership. This enables three-way fixed effects that control for a variety of issues, including the investee-country-level factors that impair the Lang et al. (2020) inferences.

country reallocations shown in Lang et al. (2020), enhances global risk-bearing capacities and consumption smoothing by decoupling saving from investment (Feldstein and Horioka 1980; Sørensen et al. 2007).

The second set of tests explore market integration from *firms'* perspective, using asset pricing tests. Changes in market integration hinge on the extent to which the increased investment (described above) alters the marginal investor's pricing of an asset. Alexander et al. (1987) and Errunza and Losq (1985) suggest that cross-border ownership diversification enables firms to achieve a higher equilibrium price and a lower expected return than they would in a single segmented market. Their models imply that a shift from local pricing of a firm (a segmented market) to global pricing of a firm (an integrated market) should lead to changes in observed local and world market risk exposures (which also implies a reduction in the firm's cost of capital).

I examine cross-listed firms when their home and host country regulators begin cooperating via the *M*MoU, and find that global (local) risk exposures increase (decrease) in the year after the *M*MoU (as compared to the year before). This is consistent with an increase in market integration, an impact on the marginal investor, and a reduction in cost of capital. The effects are stronger where the local legal infrastructures are weak. I also examine purely domestic firms, using the date their home country regulator enters the *M*MoU, and find that integration increases nearly monotonically with firm size: small firms exhibit no changes in integration, but large firms do. On average, the effects are larger for cross-listed firms than for domestic firms, even though cross-listed firms begin with a higher degree of integration with global markets.

This is the first study to consider the effect of regulatory cooperation on market integration, and it contributes to the literature in three ways. First, my findings support the notion that cooperation policy resolves investment frictions and integrates capital markets. My key finding—that cooperation impacts both firms and investors and may have country-level welfare effects—advances several interrelated literatures on the frictions that lead investors to forgo the benefits of international diversification. By demonstrating that the ramifications of cooperation are larger and farther-reaching than was previously known, this paper also adds to a nascent literature on cooperation between securities regulators.

Second, this paper is related to two additional strands of the economics and finance literatures: one that stresses that a country's domestic institutional features define its suitability for foreign investment (Knack and Keefer 1995), and one that portrays

institutional features as a country-level phenomenon (LaPorta et al. 1998; Acemoglu et al. 2001; Glaeser et al. 2004; LaPorta et al. 2008). Although legal systems—and therefore property rights, contract enforcement, judicial quality, and securities regulation—are organized at the country level, this study reveals that institutional aspects defined at the *country-pair* level significantly influence cross-border investment.

Third, this paper relates to the bonding hypothesis, which views foreign legal systems as a potential source of investor protection and firm value (Coffee 1999; Stulz 1999). In a general sense, I find support for this idea, yet my results challenge the maintained assumption, in prior work, that regulatory standards revert to the stronger of the two involved legal systems. I argue that, in practice, *interactive* coordination between securities regulators determines how well a firm can bond to a foreign legal system. Thus, investors' level of protection and firms' access to and cost of cross-border financing depend not only on a firm's decision to cross-list, but also on regulatory pairs' capacity and willingness to cooperate.

II. Background

A. Motivation and related literature

Cooperation helps institutional features transcend territorial boundaries, which can (i) enhance enforcement, (ii) improve regulatory decisions through learning and shared experiences, and (iii) reduce red tape. This increases "investor confidence" and makes investment more attractive to foreign investors (SEC 2010, p 4).

Absent appropriate enforcement, investors face significant risks when investing abroad. Kang and Stulz (1997) suggest that investors consider risks arising from information asymmetry and political uncertainty (e.g., confiscation of, or troubles repatriating, their foreign holdings). Local investors possess information advantages, either through direct relations with firm insiders, such as managers, officers, directors, or controlling blockholders, or through proximity to outsiders who possess firm-specific information, such as lenders, suppliers, banks, customers, and politicians. Exacerbated by regulatory inadequacies, these advantages can lead to fraud and expropriation, which discourage foreign investment.

By promoting robust enforcement, cooperation can deter behaviors that unfairly take advantage of information asymmetry. Cooperation allows regulators to swiftly investigate insider trading, related-party transactions, cyberattacks, market manipulation, front

running, and clearing and settlement failures.[4] If a foreign firm is cross-listed or multinational, cooperation between regulators can ensure the firm's compliance with applicable listing, auditing, and disclosure obligations. By resolving issues that prevented enforcement, cooperation deters abusive behaviors. This makes investment more attractive to foreign investors.

Cooperation also allows regulators to benefit from a wider set of shared experiences in connection with common regulatory concerns. Regulators who are linked by the *M*MoU meet to deliberate both day-to-day issues and crises, and consult one another in IOSCO meetings, technical assistance programs, and ad hoc interactions. Their consensus building gives them leverage over lawmakers when the regulators seek the laws or authority to carry out their mandate in accordance with international standards. And their shared experiences may help them understand and encourage the appropriate policy response to FPI. For example, foreign capital inflows must be managed in ways that avoid currency appreciations that destabilize the broader economy (and ultimately threaten the viability of foreign investment) (Prasad et al. 2007). Closer interactions between regulators seem likely to lead to a better environment for foreign investment.[5]

Cooperation can also help reduce regulatory red tape and complicated or duplicative requirements, which can be particularly burdensome for international market participants. For example, regulators can simplify compliance burdens on trade infrastructures by allowing ad hoc exemptions, modified requirements, waivers, or "substituted compliance" (the concept that the rules in a foreign jurisdiction, though technically different, are of sufficient quality to substitute for domestic requirements). This lowers the costs of foreign transactions for broker-dealers, central counterparties, transfer agents, and other back-end functions. These cost reductions are also enjoyed, in part, by investors transacting in foreign shares.

Each of these factors reduce frictions related to foreign investment. Enhanced enforcement should deter malfeasant behaviors, compensate harmed investors, and promote

[4] As examples of tactics requiring assistance, consider acquiring records (banking, beneficial ownership, brokerage, telephone, purchase, travel); serving a defendant; contacting witnesses and deposing them or compelling their testimony; pursuing restraining orders that prohibit destruction of documents or halt flight risks; and identifying, freezing, and repatriating ill-gotten assets.

[5] This relates to a literature on regulatory harmonization. Prior work evaluates efforts to harmonize aspects of markets, including common currencies (like the European Monetary Union) (Bekaert et al. 2013; Larch et al. 2019; Glick and Rose 2016), accounting standards (Yu and Wahid 2014), and laws regarding disclosure obligations and market abuse (Christensen et al. 2016)). Bekaert et al. (2013) is the only one focused on market integration. However, note that harmonization is neither the stated goal, nor the outcome, of the *M*MoU. Instead, the *M*MoU seeks to establish a mechanism to support cross-border cooperation across regimes—even ones with very different legal procedures and regulatory regimes. Thus, cooperation should not be confused with harmonization.

more symmetric information. Consultation between regulators should provide a richer set of experiences that help regulators arrive at better decisions. And reduced compliance costs should make ownership of foreign shares easier and less costly.[6]

Although prior work focuses on the importance of institutional features at the country level (Hall and Jones 1999; Acemoglu et al. 2001; Alfaro et al. 2004), the preceding discussion implies that, with respect to capital markets, cooperation represents an important institutional feature at the *country-pair* level.

Only recently has the literature begun exploring cross-border cooperation between securities regulators, but the findings to date are broadly consistent with the discussion above. Silvers (2020), the first empirical study of international cooperation between securities regulators, provides comprehensive institutional detail about the history of cooperation, including the progression of information sharing and the use of cooperative arrangements. Although cooperation can take place through several mechanisms, including ad hoc requests, letters rogatory, and Mutual Legal Assistance Treaties, Silvers (2020) describes a host of problems with these methods. Instead, MoUs—and IOSCO's *M*MoU, in particular—provide the main avenue for cooperation.

Silvers (2020) finds that, after controlling for other factors, cross-border enforcement is about three times as likely after the *M*MoU connects two regulators. This is consistent with the anecdotal evidence of regulators, who indicate that the *M*MoU has revolutionized their cross-border capacities (IOSCO 2012). Moreover, using share-level data, Silvers (2020) shows that transaction costs decline for cross-listed shares (even relative to non-cross-listed firms from the same country). This implies a reduction in the risks perceived, and/or costs borne, by liquidity providers.[7] A related study by Silvers (2017) demonstrates that US cross-listed firms' financial reporting becomes less opaque after the *M*MoU; this, too, is consistent with a decline in expropriation risks.

Focusing on US oversight of US cross-listings in 27 countries, Lang et al. (2020) show that when the *M*MoU links the SEC to a foreign counterpart, funds in (unaffiliated) third-party countries free-ride on US oversight by shifting existing investment out of non-US-cross-listed firms and into US-cross-listed firms from the same country. Their study is similar

[6] Certain political risks may also decline with the *M*MoU, since it is part of the Financial Sector Assessment Program (FSAP), which can influence IMF/World Bank lending. Thus, risks that arise from the threat of sovereign defaults may contemporaneously decline, contributing to more suitable conditions for foreign investment.

[7] This comports with literature that documents a decline in opportunistic trading and price informativeness following insider trading enforcement actions publicized by the US SEC, which increase the expected cost of insider trading (Del Guercio et al. 2017).

to mine in that it documents investors' preference for more robust regulatory oversight, all else equal. However, its focus on spillover investor clientele effects and within-country reallocations neglects a potentially larger phenomenon: *cross-border* reallocations that increase cross-border capital mobility and market integration. Lang et al. conclude that the *M*MoU is not associated with net changes in cross-country investment; this implies that cooperation is a zero-sum game at the country level. They note, however, that their study is ill-suited to identify net cross-country investment behavior (the subject of my tests), because it cannot control for unobserved economic circumstances that could change the attractiveness of a given investee country (Lang et al. 2020, p. 28). Thus, Lang et al. (2020) provide no insights about market integration, aggregate changes in cross-border investment, or market risk exposures—none, at least, that conform to the inferences from my study.

Although some might assume that the United States is the only country actively using the *M*MoU to pursue cross-border cases, this is not accurate. According to IOSCO (2017), out of the 3,330 requests in 2016, the top three requesting countries were France (374), the US (360), and the UK (329). Silvers (2020) reports that in 2017, only about 12.5% of the 4,803 total requests under the *M*MoU were made by the US securities regulator. Thus, the overwhelming majority of requests are not made by the US. Indeed, "The IOSCO MMoU is a widely used arrangement under which 121 securities regulators have agreed the basis on which they exchange information for the purposes of their enforcement mandates," says Ashley Alder, chair of IOSCO and former head of the Securities and Futures Commission in Hong Kong (ESMA 2019).

Finally, an important departure from prior research, in this paper, is the recognition that the benefits of cooperation likely extend beyond cross-listed firms. Lang et al. (2020) and Silvers (2017) focus exclusively on US oversight of US-cross-listed firms. Although a regulator's right of action is clearest for these firms, the factors described above (enforcement, regulatory decision quality, and red tape) extend to all firms. Indeed, numerous cross-border enforcement cases occur outside the realm of cross-listed firms. Thus, cooperation potentially has broader implications that translate to enhanced demand for cross-border ownership of cross-listed and non-cross-listed firms alike.

III. Cross-border investment

A. Research design

In the empirical analyses, I am agnostic with respect to an "optimal" portfolio

allocation (unlike in the home bias literature, which specifies the world market portfolio as the normative benchmark). Instead, I evaluate the association between cooperation and FPI. Increases in FPI support the hypothesis that cooperation remediates investment frictions that would otherwise deter foreign investment.

On the surface, the perspective that cross-border cooperation is relevant for non-cross-listed firms may seem like a subtle departure from prior work. In exploring this potentially broader effect, however, I am able to use the full sample of FPI, which provides a fuller matrix of country-to-country investment between country pair combinations (as opposed to the sparsely populated matrix of cross-listings). Aside from the obvious benefit of a larger sample, this more fully populated matrix enables a design that tackles a variety of issues using numerous fixed effects. This includes (a) country-pair fixed effects to control for time-invariant country-pair factors that lead to different levels of investment between different country pairs. It also includes (b) investee×time fixed effects to control for common increases in investment to a particular investee country (as might happen when a country becomes a more attractive target for investors for economic reasons). These fixed effects largely remove changes due to investee-country-level economic circumstances, such as increases in FPI for a given investee country that are common to all investor countries (which resolves a key limitation in the study of cross-border investment in Lang et al. (2020)). Also included are (c) investor×time fixed effects, to control for an expansion in investment from a particular investor country that is common to all countries worldwide (as might happen when the investor country has excess capital and few or low-return domestic investment opportunities).[8] In fact, the fixed effects described as (b) and (c) control for both "push" (outbound investment) and "pull" (inbound investment) factors (Griffin et al. 2004; Fratzscher 2012; Alderighi et al. 2019). Finally, I include (d) linear time trends for each pair to capture any temporal trends in FPI that are unique to the country-pair, because Bergstrand et al. (2015) argue that within country-pair trends bias the estimated effects of economic agreements upward.

Ideally, the estimates would indicate the effect of cooperation on a country pair's FPI, compared to a hypothetical state of no cooperation. In my tests, the counterfactual takes the form of other country pairs that possess the treatment pair's investee or investor country at the same point in time. Note that this is a generalized difference-in-difference design (Bertrand et al. 2004). The model does not use the traditional treatment, post, and

[8] This design comports with the intuition in Anderson and van Wincoop (2003) that *relative* barriers determine bilateral interactions.

treatment×post indicators, because they are linear combinations of the more comprehensive fixed effects described above.

The pair fixed effects also subsume all time-invariant cross-sectional characteristics, making it both unnecessary and impossible to include the variables that often appear in gravity models of trade, such as distance, common language, and colonial relationships (Tinbergen 1962). Similarly, the investor(ee)×time fixed effects make it unnecessary and impossible to include country-time variables such as GDP, market-wide returns, inflation, and other macroeconomic conditions. This allows me to isolate the *cross-border cooperation* aspect, as opposed to other country-level factors that could accompany the *M*MoU (such as unobserved changes in economic circumstances that occur near the *M*MoU).

Despite the comprehensive fixed effects, it is worth ruling out known country-pair specific, time-variant changes from other events that may alter the underlying economic relationship between two countries. Thus, I control for pair-level events that could affect FPI, including trade agreements (from Hofmann et al. (2017)), tax treaties (from the International Bureau of Fiscal Documentation), and investment treaties (from the UNCTAD Investment Policy Hub).

Model 1 below presents the key coefficient(s) of interest—λ_1 (and λ_2)—the indicator for the *M*MoU (and the indicator for bilateral arrangements, some of which are potentially endogenous). This coefficient captures time series variation in FPI that cannot be explained by fixed effects and other controls and is associated with the *M*MoU (or bilateral arrangements).

(1) $$Investment_{ijt} = \lambda_0 + \lambda_1 MMoU\ Link_{ijt} + \lambda_2 MoU\ Link_{ijt} + \sum_{l=1}^{L} \lambda_l\ Pair\ time\ trends_{ij} + \sum_{i=1}^{I} \lambda_{it}\ Investor \times time\ FEs + \sum_{j=1}^{J} \lambda_{jt}\ Investee \times time\ FEs + Investor \times \sum_{m=1}^{M} \lambda_m\ Investee\ (country\ pair) FEs + \upsilon_{ijt}$$

I estimate model 1 using cross-border investment for country '*i*' in country '*j*' at the end of period '*t*' (denominated in US dollars). Recent research indicates that log-linear ordinary least square (OLS) estimates can impart substantial bias in the presence of heteroscedasticity and inconsistent estimates in the presence of many zero observations for the dependent variable (as is the case in my setting) (Silva and Tenreyro 2006). To deal with many zero observations and the heteroscedasticity they create, Silva and Tenreyro (2006) present a computationally feasible solution that uses Poisson pseudo-maximum likelihood (PPML) estimation. PPML is a consistent estimator and naturally bounded at zero. It also takes advantage of using dollars of FPI as a natural way to characterize

investment (as opposed to a transformed or scaled dependent variable). Finally, standard errors are corrected for clustering at the country-pair level (the same level as the treatment (Abadie et al. 2017)).

Figure 1 shows the adjacency matrix for the country-pair observations included in the sample. Investor countries are reported across the top and investee countries are reported on the left-hand side; each cell corresponds to a country pair. The figure is based on the actual sample coverage of FPI (described in detail later), which covers investor countries more extensively than investee countries. For example, although Sri Lanka joins the *M*MoU in 2004 and is observed as an investee country, it does not appear as an investor country because it does not report to the IMF's survey.

Note that Figure 1 also indicates the *timing* of the *M*MoU treatment across country pairs. Countries adopt the *M*MoU at different times, leading to the formation of multiple linkages for each new entrant: *n-1* new linkages occur as the n^{th} member joins the arrangement. To illustrate this variation in the timing of the linkage across country pairs, I organize the countries by the year in which they signed the *M*MoU on both the investor and investee dimensions (instead of alphabetical sorting). Blocks with the same color represent country pairs that experience the shock at the same time; blocks with different colors represent country pairs that experience the treatment at different times.

An association between the *M*MoU and inbound/outbound FPI would indicate that FPI conforms to a specific and fairly elaborate pattern of connections between country pairs. For example, Singapore's outbound investment into other countries, shown in Figure 1 in the vertical column "2005/SGP," indicates that the country formed 24 connections simultaneously upon joining the *M*MoU in 2005, and an additional 68 connections as future investee countries entered the network. Inbound investment *from* other countries *into* Singapore is represented by the horizontal "SGP" row. Singapore formed 23 connections upon joining, and 42 more as future investor countries entered the network. Once again, the staggered nature of the treatment is illustrated. *M*MoU-prompted changes in inbound and outbound FPI for pairs involving Singapore should occur in 2005 for Germany (DEU) and Belgium (BEL), in 2006 for Denmark (DNK), in 2007 for the Netherlands (NLD), in 2009 for Austria (AUT), in 2010 for Switzerland (CHE), and so forth. Thus, these arguably similar counterpart countries experience an offset timing of the treatment, making them

ideal counterfactual benchmarks.[9] Furthermore, this example illustrates how the design reduces concerns about omitted variables or reverse causality. For example, an omitted variable can substantially affect the estimates only if it is of an extraordinary magnitude or aligns with the multiple events generated by the treatment (described in the introduction and footnote 2).

B. Sample

The FPI (cross-border equity ownership) sample comes from the IMF's Coordinated Portfolio Investment Survey (CPIS), which covers a maximum of 88 investor and 203 investee countries annually for the years 2001–2017. Annual equity investment reached a high of $30.5 trillion in 2017 and is $16.8 trillion on average.[10] This represents 261,035 country-pair years (see Figure 1)—15,355 pairs over a 17-year period. This time period excludes the many market liberalizations prior to the turn of the century and helps ensure that they do not influence my tests. Figure 2 shows that aggregate levels of equity throughout the sample period increase almost monotonically. The average level of FPI during the 17-year period is $16.8 trillion (roughly $1 billion per country pair).

C. Empirical results

In Table 1, column 1, the results using PPML show that both the *M*MoU and bilateral arrangements have strong associations with cross-border equity ownership, even after including the comprehensive three-way fixed effects (for pair, investor×year, and investee×year) and the other controls. The magnitude—about 11% for the *M*MoU and 9% for bilateral arrangements—is statistically significant in both cases.[11,12] The average FPI across all countries during the sample period is $16.8 trillion, so the 11% increase attributable to cooperation policy equates to about $1.8 trillion. The estimates on the

[9] Alternatively, consider the connections Singapore forms with Hong Kong in 2005, China in 2007, Japan in 2008, and Taiwan in 2011, or with Isle of Man in 2005, Malta in 2006, the British Virgin Islands in 2007, Cyprus and Guernsey in 2009, and Gibraltar in 2013.

[10] Note that, for my purposes, cross-border positions by organizations explicitly unaffiliated with a particular jurisdiction (e.g., the IMF) are excluded from the analyses. Also, observations are redacted in some instances. The redacted observations appear to represent a trivial portion of total cross-border investment. Finally, the CPIS is now conducted semi-annually, but only since 2012. I exclude 1997 because it has lower coverage and quality and is separated from the nearest year by four years.

[11] Because Poisson uses a log-link function, the coefficient interpretation is precisely the same as in log-linear models. An economic interpretation requires transformation using the expression $\hat{g}=\exp(\theta)-1$, where θ is the coefficient estimate from the tables. The interpretation is that a one-unit change in the independent variable is associated with a $\hat{g}$ percent change in the dependent variable (Halvorsen and Palmquist 1980; van Garderen and Shah 2002; Kennedy 1981). When the independent variable is also in log form, the interpretation is that a 1% change in the independent variable is associated with a θ% change in the dependent variable.

[12] Technically, this is investment in excess of non-cooperating country-pairs that include the same investee or investor country—so, in some circumstances, this could represent less retrenchment rather than an absolute expansion in investment.

control variables related to tax, trade, and investment treaties are insignificant.

A more traditional approach using a log-linear model yields the same overall inference, but with a substantially larger magnitude. Column 2 in Table 1 shows that relative to control pairs (i.e., pairs that do not cooperate), cross-border investment is 46% greater for pairs linked via the *M*MoU and 75% greater for pairs linked by bilateral arrangements, which reinforces the idea that regulatory cooperation influences cross-border investment. The sizable difference in magnitudes across the PPML and log-linear regressions is consistent with recent studies in which these same two alternative estimation techniques were used (Glick and Rose 2016; Larch et al. 2019). The design and specifications in those studies also measure an effect that is similar to the *M*MoU's effect on FPI: the effect of currency unions on bilateral trade. However, log-linear estimates may be misleading in terms of magnitude. PPML's theoretical superiority with respect to bias and consistency has made it the prevailing "workhorse" estimator for evaluating policies in settings with similar pairwise structures (e.g., international trade) (Weidner and Zylkin 2020). Therefore, I use PPML as the preferred methodology hereafter.

The results are consistent with cooperative arrangements having larger economic effects than was previously known. The primary effect of cooperation on FPI does not occur through a spillover involving unaffiliated countries, but rather it occurs directly via investment between cooperating country pairs. The evidence supports the idea that regulatory cooperation enhances international capital mobility and market integration. Thus, cooperation may play a role in enhancing "investor confidence" between country-pairs by remediating investors' distrust of foreign markets (which has been shown to curb cross-border portfolio investment (Guiso et al. 2008, 2009)).

D. Cross-border investment: cross-sectional tests

This study proposes that there are impediments to foreign investment and that regulatory cooperation can partially resolve these impediments through better enforcement, regulatory decisions, and a reduction of red tape. Two opposing effects could condition the outcome from the previous section: (i) the extent of these impediments (e.g., expropriation risks and red tape), and (ii) the capacity to resolve them (via enforcement cooperation and streamlined procedures). For example, a change in enforcement of a given magnitude would likely have a larger effect on an investee country with low regulatory quality, where concerns about expropriation risks are higher. However, investee countries with low regulatory quality

are less likely to provide strong assistance because their capacities for cooperation are limited, relative to countries with high regulatory quality. The impediments (e.g., expropriation risks, red tape, etc.) *and* the capacity to remediate them with cooperation could simultaneously influence the effect (in different directions), either offsetting each other or inducing U-shaped non-linearities in the cross-section.

Below, I test for cross-sectional patterns, using tests that are somewhat exploratory. Given the complexity of the treatment pattern, these tests are not vital to the identification strategy (as is sometimes the case for studies examining a common shock).

Empirically, I study the cross-sectional effect of the *M*MoU by exploring the interactions of the linkage indicator with partitioning variables intended to capture the following attributes: (1) geographic distance between country-pairs, (2) capital controls, (3) attributes of a country's institutions (e.g., legal strength and origin), and (4) market development. In addition to being proxies for impediments to FPI (e.g., expropriation risk), these attributes may also serve as inverse proxies for cooperative capacity. The *un*interacted partitioning variable need not (indeed, cannot) be included because of the investor×time and investee×time fixed effects.

Prior research uses geographic distance as a proxy for information asymmetry between country-pairs (Portes and Rey 2005). I interact indicators for the geographic distance tercile with the *M*MoU. In panel A of Table 2, I report the percentages implied by the coefficient estimates. The effect of the *M*MoU increases monotonically with geographic distance, consistent with larger effects occurring in country-pairs that are farther apart and have greater natural information asymmetries. FPI increases by 5%, 9%, and 15% for the small, medium, and large distances (the first and third terciles are statistically different, p=0.03). This pattern is consistent with reductions in foreign-versus-local information asymmetry.

Next, I explore the effect of explicit prohibitions on foreign investment that can occur through capital controls. Note that capital controls are uniquely binding frictions that are perhaps unlikely to be resolved by cooperation between securities regulators. Thus, cooperation may have no effect on countries that use capital controls. Panel B in Table 2 supports this prediction. It shows that cooperation has no effect on FPI in the presence of capital controls but is associated with a 13.5% increase in FPI in countries without capital controls. The effect in countries without capital controls is larger than the 11% shown in the main test; this suggests that pooling these two groups brings down the average effect.

The remaining cross-sectional tests explore various attributes of a country's institutional, economic, and market-related features using the following measures: (i) indicators for common law legal origin (LaPorta et al. 2008); (ii) the World Bank's index for rule of law (Kaufmann et al. 2010); (iii) equity market size; and (iv) market development classifications (from MSCI). Because these dimensions vary for both the investee and investor countries, I use tercile indicators for continuous measures and interact them to break down the effects of the *M*MoU across various combinations of country attributes. I report the effect in a 3×3 table of investor/investee pairings (2×2 in the case of common law legal origin).[13]

Legal systems with a common law lineage may better protect property rights, resolve disputes, and protect shareholders (LaPorta et al. 2008). Table 2, panel C shows that common law appears to be relatively unimportant in conditioning the effect of cooperation. Cooperation between two common law countries yields the greatest increases in FPI (15.1%). However, no combination of legal systems produces effects that are statistically different from the rest.

Panel D shows the effect of cooperation across combinations of the rule-of-law dimension, which measures agents' confidence in the rules of society—particularly the quality of contract enforcement, property rights, and the courts, and the likelihood of preventing crime (Kaufmann et al. 2010). Moving from the upper left to the bottom right corner—from two weak rule of law countries to two strong rule of law countries—there is a non-linear U-shaped pattern, with the largest effects occurring in pairs of weak countries, insignificantly negative effects in middle-to-middle strength countries, and a moderate effect (of 10.2%) in pairs of strong countries. This is consistent with the two countervailing effects described above: the risk level and the countries' capacity to remediate risk appear to simultaneously influence the effect, inducing non-linearities in the cross-section. The off-diagonals display considerable symmetry, consistent with the concept of reciprocity between regulators, which prior research argues is critical in cross-border cooperation (Silvers 2020).

Next, I partition by equity market size and market development. Panel E uses market size to partition the effect. Significant increases in FPI occur exclusively in pairings that include investee countries with medium or large market sizes. This is consistent with the

[13] These partitions do not weight the effect of the *M*MoU in a way that reconciles to the overall effect of 10.5 (11%) from Table 4. Lack of reconciliation occurs both because some variables (e.g., common law, or market development) do not partition the sample with equal numbers of observations and because the *M*MoU indicator occurs disproportionately in different cells.

notion that higher market size is associated with greater regulatory sophistication, which in turn increases the capacity for, and effects of, cooperation. Panel F uses market development to partition the effect of cooperation.[14] Market development increases incrementally as one moves from frontier, to emerging, to developed markets. The strongest statistical relation for increases in FPI is between countries with developed markets (13.5%, significant at $p<0.01$); this echoes previous results in which the most stable effects occur between countries that share common law backgrounds or a strong rule of law. Note, however, that cooperation also influences frontier markets: they increase their holdings in emerging and developed markets by 26.5% and 8.1%, respectively.

A recurring theme throughout the cross-sectional tests is that, when paired countries share common law backgrounds, a strong rule of law, or developed market status, the regulatory effects of cooperation are statistically strong, if not the strongest observed, despite being more moderate in economic magnitude. In other words, the effects of cooperation on foreign investment are most consistent between two "strong" countries.

When examined in isolation, however, certain other country pairings exhibit unpredicted increases in FPI (e.g., weak investor rule of law/strong investee rule of law (Panel D) and frontier market investor/developed market investee (Panel F)). These increases, which tend to be statistically weak, could conceivably be attributed to reciprocity or even measurement error. An alternative explanation is that they are consistent with the Lucas paradox—the well-documented observation that capital does not flow from developed countries to developing countries even though developing countries have lower levels of capital per worker (Lucas 1990). One rationale for the Lucas paradox is that low institutional quality impedes investment from rich to poor countries (Alfaro et al. 2008). My results appear consistent with this. Regulatory cooperation may provide a resolution to the distrust and fear of expropriation that investors from weak, frontier, or code law countries have when investing in more sophisticated markets.[15]

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[14] I use MSCI's classification and consider any country not included to be a frontier market.

[15] The notion that the effects are larger for developed markets than for emerging markets, though plausible, is unsettling for two reasons. First, it implies that gains from cooperation are asymmetric, and raises practical questions about fairness and reciprocity, which are core principals of effective cooperation (see Licht (1999) for game-theoretic models of cooperation between securities regulators. If the effects of cooperation are indeed uneven, then opportunities may exist to improve cross-border cooperation and enhance investment in emerging and/or frontier markets, or to contemplate side payments if benefits cannot be made reciprocal. Second, the idea that larger effects are associated with developed markets triggers the concern that a more complex dynamic underlies the empirical results: developed markets could follow a different course for FPI, in which case the benchmark pairs could be unsuitable counterfactuals. Although the pair time trends partly control for this, I investigate whether this explanation could account for the correlation between FPI and cooperation. To do so, I re-estimate the effect of cooperation in a sample that is confined to the 22 *developed* investee and investor markets. By removing some of the heterogeneity in countries, I seek to ensure that the benchmark country pairs are similar with respect to market

Overall, it appears that cooperation plays a critical role in cross-border investment decisions, which is subject to complex dynamics. The cross-sectional results are consistent with both of the arguments presented above: weaker investee countries have the most to gain, but stronger countries are the best cooperative partners.

E. Cross-border investment: identification and robustness tests

In this section, I provide additional tests to explore the identification and robustness of the results across various subsamples and estimation methods. A difference-in-difference design requires that the untreated group follow the same trend in the absence of the treatment (Bertrand et al. 2004). To explore the assumption that the benchmark country pairs meet this criterion, I perform two tests.

First, I eliminate, from the sample, any countries (country pairs) that never experience the *M*MoU shock, then re-estimate the effect. The identification comes from pairs that are eventually treated but have not *yet* experienced the shock. The results in column 1 of Table 3 indicate that *M*MoU's association with FPI persists at a similar magnitude in this subsample. Thus, there is no evidence that an unobserved heterogeneity across countries (country pairs) which is associated with accession to the *M*MoU drives the result. This result also ensures that the results are not concentrated in economically trivial observations (pairs of small countries with inconsequential levels of FPI).

Second, I map out the effect of the cooperation linkages in event time to probe for violations of the parallel trend assumption and to assess the timing of the effect. It is possible that investors anticipate a country's *M*MoU admission by a year (or possibly two), since *M*MoU applicants must often pass new legislation and must always wait for an official verification prior to the formal signing. Moreover, qualifying countries frequently defer the formal signing until a ceremony is held at the IOSCO annual meeting.[16] (Internet Appendix I provides a detailed hypothetical timeline of the various steps in the application process.) The investors' anticipation of the event could introduce some predictable measurement error. As an example, consider a country that proposes draft legislation in January of one year and signs the *M*MoU in February of the following year. FPI is measured on an annual

development. Despite this rather demanding restriction, I find a similar result (see Internet Appendix Table I, column 1 for details). Thus, the association between FPI and cooperation does not appear to be attributable to an unknown endogenous factor related to market development (unless, of course, this factor corresponds to the elaborate sequence and timing of the shocks—which seems improbable). The next section explores this theme in greater detail.

[16] See http://www.csrc.gov.cn/pub/csrc_en/affairs/AffairsIOSCO/201205/P020120524357975007952.pdf for examples.

basis at the end of each year. Thus, if investors anticipate the *M*MoU during the year the draft legislation is passed (and perhaps even goes into force), their investment could drive changes in FPI well before the signing occurs. Moreover, the annual unit of observation makes it possible for a 13-month anticipation to span two year-ends, thus resembling a two-year anticipation.

Figure 3 shows the six years before and after the *M*MoU linkage and is consistent with investors narrowly anticipating the signing. The effect is largely concentrated in the year of the *M*MoU linkage (78% of the total effect of 0.105 from Table 1, column 1 occurs during the first year of the *M*MoU linkage). The trend before and after the link appears fairly level, which supports the parallel trend assumption.

Some moderate preemption appears to narrowly anticipate the formal signing of the *M*MoU. It is not possible to know with certainty whether this is due to measurement error from the lengthy process of becoming a signatory or to reverse causality (or elements of both). Silvers (2020) reports that, on average, it takes 14 months from application to approval. Thus, for reverse causality to explain Figure 3, the average regulator would need to anticipate increased investment over a year into the future. I cannot rule out this explanation, but the premise seems doubtful. Furthermore, each *M*MoU admission generates multiple connections that are outside the regulator's control, making reverse causality even less likely. Although no test (including this one) can conclusively affirm the appropriateness of the benchmark country pairs, there is no indication that the parallel trend assumption has been violated.

Next, I attempt to rule out the concern that an unknown tautological design feature or misspecification drives the results. Bertrand et al. (2004) show that generalized difference-in-difference designs can be untrustworthy, as random assignment of state-level treatments rejects the null hypothesis (of no effect) too often. If selecting any random year to partition the time series of a country-pair produces a result similar to the one in Table 1, the model could be poorly specified, or a different mechanism could underlie the result. I thus assign the real *M*MoU years to countries at random and recalculate the linkage date for country pairs as a pseudo-treatment. The distribution of the pseudo-treatment estimates is centered at -0.00017 and exceeds the estimates from the real treatment dates just 32 times out of 1,000 (p=0.032).[17] (Internet Appendix Figure 1 provides a histogram of the placebo

[17] Of these, only nine of the placebo tests exceed the *statistical* significance of the real estimate (z>2.95), which suggests that they are mostly driven by a few extreme observations.

coefficient estimates.) This finding is inconsistent with tautological design features or misspecification influencing the results.[18]

The preceding simulation indicates that, in expectation, the effect of a linkage created at random is zero. Thus, for endogeneity to account for an 11% increase in FPI, an omitted variable (or reverse causality) would need to align with a very intricate linkage pattern, and each linkage would need to have an endogenously determined 11% magnitude. Alternatively, if each individual country joined the *M*MoU because of an inducement by another counterpart that was endogenously related to FPI, the magnitude of the endogenous linkages would need to be very large. (Of the 114 country-pair connections formed by the *M*MoU, 92 have sample data. Thus, assuming the other roughly 92 country-pair connections were exogenous, the magnitude of the endogenous linkage would need to be roughly 1,012%.)

The time-variant country-pair controls (e.g., treaties) may be coarse with respect to other evolving pair-specific economic conditions. Prior studies often try to predict trade itself, making trade impossible to use as a control variable; in my study, however, trade can be used. Specifically, I include (log-transformed) bilateral trade in both directions (investor to investee, and vice-versa). The data, which comes from the UN Comtrade dataset, represents commodities traded between country pairs in US dollars. Column 2 of Table 3 demonstrates that the estimated effect of the *M*MoU is virtually unchanged, supporting the idea that other economic factors do not drive the results. However, my use of trade as a control does weaken the estimated effect for bilateral arrangements, perhaps reflecting endogenous relations between bilateral arrangements and economic forces.

Finally, I add controls for various potentially influential subsets of country pairs and find results similar to the main result in column 1 of Table 1. The design makes it unlikely that any given country pair could substantially impact the overall estimates, because each country makes a fairly small contribution to the sample. Nevertheless, columns 3 to 6 of Table 3 show that the main inference is unaffected even after I separately control for the *M*MoU's effect on pairs of EU countries (which represent a significant fraction of the sample), country pairs involving the US (as investee or investor), pairs involving any of the 27 investee countries that have a significant number of US cross-listings (the subsample examined by Lang et al. (2020)), and country-pairs possessing at least one cross-listed firm.

[18] The simulation exercise in Bertrand et al. (2004) rejected the null 45% of the time, indicating a serious design flaw. By contrast, over-rejection of the null hypothesis in the simulations is less prevalent in my setting: 133 of the 1,000 replications have a p-value less than 0.10. (In theory, this should only happen 100 times out of the 1,000 simulations.)

The *M*MoU estimates remain fairly stable (between 0.087 (9%) and 0.110 (12%)), demonstrating the robustness of the inference to various factors. The primary effect of cooperation is thus extensive, not driven by US or EU observations, and more far-reaching than was previously known. The effect is not exclusive to pairs that involve countries with US cross-listings; nor is it confined to country-pairs that have cross-listings. This implies that investors also perceive a reduction in investment risks for non-cross-listed firms.

The idea that cross-border issues are relevant even for purely domestic firms underscores the global nature of capital markets. Malfeasant conduct by banks, broker-dealers, investment advisors, credit rating agencies, transfer and clearing agents, consultants, analysts, short sellers, and individuals routinely extends between jurisdictions, and there is no reason to believe that its effects are confined to cross-listed firms. As Beyea (2011) observes, it is "very rare to find a modern securities fraud case that does not have an international facet of some kind." Anecdotes from staff at the US SEC suggest that more than 30% of the cases they pursue have at least some cross-border element, and most of the SEC's cases do not involve cross-listed firms. Thus, the empirical support for cooperation having a broader impact than was previously characterized also comports with a practical understanding of modern regulatory environments.

Overall, I find evidence that cooperation relaxes an impediment to investment, raising the equilibrium cross-border investment for foreign investors who must balance the benefits of foreign diversification against the expected risks (Brennan and Cao 1997). This supports the idea that cooperation increases capital mobility.

IV. Market integration

A. Tests of market integration

The previous tests examine integration from the perspective of investment holdings. Whether the changes in ownership I observe translate into meaningful differences in the way firms are priced remains an open question. To address this question, the next tests follow previous studies of cross-listing events that examine integration at the firm level. Alexander et al. (1987) and Errunza and Losq (1985) suggest that when formal investment barriers are removed, firms can achieve a higher equilibrium price and a lower expected return than they would in a single segmented market. Their models imply that a shift from local pricing of a firm (a segmented market) to global pricing of a firm (an integrated market) should lead to changes in local and world market risk exposures and the firm's cost of capital. Bekaert and

Harvey (1995) formalize this intuition by combining local and international capital asset pricing models (CAPM). The local CAPM describes expected returns in a perfectly segmented market, where assets are priced locally and the price of risk is determined locally (by risk aversion and the local risk-free rate). The international CAPM describes expected returns in a perfectly integrated market, where assets are priced globally. In the international CAPM, the implication is that assets with a given risk level are priced the same regardless of the market in which they trade.

Building on this prior work, the intuition in the cooperation setting is that a decline in local beta, an increase in world beta, or both are evidence of an increase in integration, representing a potential shift toward a global market pricing of an asset.[19] More formally, model (2) shows that the expected return of security i is a function of its local and world price of covariance risk (ψ) and covariance with local and world returns, where Ret, R^L, and R^W represent the firm, local market, and world market returns, respectively. Integration can be inferred from the relative exposures to the local and world indices. This model, shown in (4) below, includes Φ, a continuous integration parameter ranging from 0 (a fully segmented market) to 1 (a fully integrated market). It essentially captures the fraction of the total *quantity* of risk (composite beta) that is attributable to global market beta. Note that, ceteris paribus, Φ increases when local (global) market risk exposures decrease (increase).

$$E_{t-1}[Ret_{it}] = (1 - \Phi_{i,t-1})\psi^L_{t-1}\, Cov_{t-1}[Ret_{it}, R^L] + \Phi_{i,t-1}\psi^W_{t-1}\, Cov_{t-1}[Ret_{it}, R^W] \quad (2)$$

Empirically, I estimate a model based on the intuition described by equation (2), allowing risk exposures to change based on the *M*MoU as an indicator of a structural break in the risks perceived by investors (as a consequence of regulatory cooperation). Model (3) below illustrates the basic structure, with firm and time subscripts omitted. *Post* is an indicator equal to 1 when cooperation occurs. A decrease in the local beta (β_4) and/or an increase in the world beta (β_5) implies that cooperation promotes market

[19] I am *not* testing for "abnormal" returns or endorsing the ability of this two-factor model to correctly price an asset. As Bekaert et al. (2011) point out, there is no consensus about the best asset-pricing model, since world and local betas do not fully explain the cross-section of returns. I am more interested in market integration and whether market risk exposures change with cooperation, which would support my hypothesis (even if priced risk factors were omitted from my model). Bekaert et al. (2014) use a similar asset pricing model, which tests for changing risk exposures (described below). The staggered-shock design makes it unlikely that even a misspecified asset-pricing model would confound my inferences regarding changing market risk exposures, because any omitted risk factors would need to change at the same points in time (which seems unlikely).

integration.

$$(3) \qquad Ret = \beta_0 + \beta_1 R^L + \beta_2 R^W + \beta_3 Post + \beta_4 R^L * Post + \beta_5 R^W * Post + \varepsilon_t$$

I expect that cooperation between securities regulators, by reducing a variety of investor risks, resolves informal barriers to foreign investment. In the context of asset pricing, increases in cross-border investment imply that assets may be priced globally rather than locally, as shown in changing risk exposures from the local market to the world market. Of course, this depends on whether the changes in FPI are large enough to influence the *marginal* investor.

The FPI analyses imply that the effects of cooperation are not confined to countries with cross-listings. Thus, enhanced integration may occur both for cross-listed and purely domestic firms. On one hand, cooperation and FPI may be most relevant for cross-listed firms, given that they are co-supervised by a home and host regulator. On the other hand, cross-listed firms are likely to already be more integrated. This leaves open the possibility that cooperation affects the integration of one or both groups. In subsections C and D below, I explore changes in integration for cross-listed firms and purely domestic firms, respectively.

B. Sample

I estimate model (3) on a sample of cross-listed firms, then repeat the analyses on purely domestic firms.[20] As in prior, I use weekly (Wednesday to Wednesday), US dollar-denominated returns that are adjusted for the T-bill rate. Datastream is the source of the market return data for the asset-pricing tests in both the cross-listed and domestic subsamples. To designate cross-listed firms, I primarily use the Datastream data, supplemented with various lists from the major ADR banks.[21] Firms without data during the 52 weeks before and the 52 weeks after the *M*MoU are necessarily excluded from the analyses. This helps reduce the possibility that the sample includes firms that (endogenously) pursue a cross-listing *because of* the *M*MoU and prevents comingling the effects of cross-listing with the effects of cooperation.

The cross-listed sample consists of 1,164 individual firms from 206 country-pairs that

[20] See Jorion and Schwartz (1986), Chan et al. (1992), Dumas and Solnik (1995), Foerster and Karolyi (1999), and Lewis (2017).

[21] To allow for estimates of reasonable quality, I require that a firm have weekly return data for at least 75% of the observations in both the pre- and post-treatment time periods.

experience the shock during the period from 2001 to 2014. The domestic sample consists of 6,535 individual firms from 52 countries.

C. Cross-listed firms

Relying on the logic described above, several prior studies examine changes in beta(s) at the time of a US cross-listing. Their findings support the notion that cross-listing promotes market integration. Using a single local market index, Foerster and Karolyi (1993) show that Canadian firms' exposure to local market risk declines following US cross-listings. Using a global sample and an analogous two-factor model that includes the local and world indices, Foerster and Karolyi (1999) observe a decline in local market betas and no change in world betas following US cross-listings. And Jayaraman et al. (1993) show a decrease in local beta and no change in US beta in 95 firms from Japan and the UK that cross-list in the United States.

Though structurally similar to previous studies, my tests focus on cooperation. Instead of the cross-listing event, I identify the linkage between the home and host regulator as the treatment (analogous to the FPI treatment).[22] Recall that the bonding hypothesis posits that the benefits of cross-listing result from bonding to a stringent legal system in which firms must uphold higher investor protection and disclosure standards (Coffee 1999; Stulz 1999). A cross-listing event likely comingles changes in market segmentation with changes in legal infrastructures. A key innovation of my test is that it dampens potentially confounding factors such as the inevitability of the asset.

C.1. Cross-listed firms: main empirical results

As shown in column 1 of Table 4, the equilibrium betas prior to the *M*MoU are dominated by the local market betas: 0.598 for the local beta and 0.380 for the world beta. These numbers indicate that the risk of the assets is priced in the local market more than the global market. The composite beta is very close to 1. After the *M*MoU, the local and world betas change in opposite directions. Exposure to the local market decreases by 0.081, while exposure to the world market increases by 0.065. The post-cooperation betas are consistent with an increase in the integration parameter (Φ) from 0.39 [0.380/(0.598+0.380)] to 0.46 [0.445/(0.517+0.445], representing a 19% increase in market integration. The

[22] To avoid using commercially created indices, which, when weighted by market capitalization, might consist mainly of treatment firms, I construct the market indices manually after excluding the cross-listed firms.

composite betas are near 1 both before and after regulatory cooperation, indicating a stable overall quantity of market risk. Nevertheless, this shift from local to world pricing implies a reduction in the cost of capital because it replaces local market equity risk premiums with global equity risk premiums, which are generally much lower.

Unlike the FPI analysis, the sample is strongly influenced by US cross-listings. US cross-listings represent about 38% of the sample firms and could drive the result. To test for this, in columns 2 and 3 of Table 4, I re-estimate the effects on the non-US-cross-listed and US-cross-listed samples, respectively. The results are substantially smaller and weaker without the US cross-listings (for example, the post-*M*MoU local beta declines by 0.065, *p=0.083*). This group likely pools a heterogeneous set of effects (which I explore in the next subsection). The US cross-listings exhibit a stronger response to cooperation (for example, the local beta drops by 0.135, *p=0.032*). The larger effect is expected, given that US regulators are among the most proactive regarding cross-border issues.

In terms of magnitude, the estimated effect of the *M*MoU is smaller than the effects that Foerster and Karolyi (1999) observed around US cross-listing events. This result is also expected, given that cross-listing events appear to have more profound implications for investability, co-bundle several factors, and are potentially endogenous. In sum, Table 4 supports the premise that regulatory integration facilitates market integration.

C.2. Cross-listed firms: cross-sectional tests

The degree to which cooperation resolves hindrances to market integration likely varies in the cross section of country-pairs. The country-pair matrix of cross-listed firms is sparsely populated and has less variation than the FPI sample (particularly for host countries). Moreover, the integration signals—beta loadings on local and global indices—are potentially noisier than previous tests of FPI. To mitigate these issues, I reduce noise through aggregation by using coarser partitions for continuous variables in the cross-sectional tests. The cross-sectional partitioning variables are the same as those used in the FPI analyses: geographic distance, common law indicators, rule of law, market size, and development classifications.

Panel A of Table 5 reports the effect of geographic distance on the effect of the *M*MoU. It shows that the changes in beta that accompany the *M*MoU increase with distance (as monitoring becomes more difficult). The differences in the changes in local and world betas for close and far country pairs are both significant at the 0.01 level.

Panel B partitions the sample based on capital controls. Note that, by virtue of being cross-listed, firms may to a large extent circumvent capital controls (Auguste et al. 2006). Thus, there is no clear prediction regarding the cross-sectional magnitudes. I find that the changes in local and world betas are of similar magnitude for firms in countries with, and for firms in countries without, capital controls. In both cases, the changes were statistically insignificant.

In a 2x2 table for code/common law and home/host market, Panel C reports the change in the betas on local and world market indices. When both the home and host markets possess common law legal origin, the *M*MoU is associated with a 0.11 reduction in the local market beta and a 0.13 increase in the world market beta. This is the largest, and most significant, result. The effect is fairly homogenous across the four cells, as it was in the FPI analyses.

Panel D describes the results partitioned by rule of law. Recall that there is fairly little variation in host country rule of law. The strongest results, in this panel, are for strong/strong country-pairs. Firms from strong home countries listed in weak host countries experience little change with cooperation, a result that is intuitive under the bonding hypothesis.

Panel E shows the results partitioned by market size. The effects are concentrated in country-pairs in which the home and host country are both large or both small.

Panel F provides the results partitioned by development classification. The benefits of cooperation are largely concentrated in cross-listings between developed markets, which is similar to the pattern in the FPI analyses. The point estimates are also high for firms from countries with undeveloped markets that cross-list in countries with developed markets. Again, this is consistent with the bonding hypothesis.

Overall, the cross-sectional patterns closely resemble those from the FPI analyses, in the that the strongest results typically occur between countries that both have common law origins, strong rule of law, or developed markets.

C.3. Cross-listed firms: robustness tests

To demonstrate the robustness of my methods, I perform additional tests that assess the timing of the effect and the sensitivity of my inferences. First, I construct tests to explore the sensitivity of my results to alternative estimation windows. I expand the estimation window in both directions in half-year increments up to an eight-year time series (four years

pre/post). In wider estimation windows, integration parameters are smaller in both the pre and post periods. Moreover, there is some evidence that expanding the estimation window by six months increases the integration parameter ($\Delta\Phi$) and thus influences the estimates in integration. But the change in the integration parameter is fairly insensitive to the choice of estimation horizon. Ultimately, there is no evidence that my results capture a gradual trend or reflect a temporary period of high (low) world (local) beta that reverts in future periods.

Unreported tests show that the changes in integration derive almost entirely from a reduction in the standard deviation of asset returns (consistent with a reduction in risk) and decreases/increases in the correlation between asset returns and local/global market indices (consistent with market integration).

D. Integration: domestic firms

In this section, I perform analogous tests using purely domestic firms (weekly, US dollar–denominated, T bill–adjusted returns during the 52 weeks before and the 52 weeks after the *M*MoU, regressed on local and world indices). One critical difference in this test is that the purely domestic firms necessitate a different definition of the treatment, because, unlike the subjects of the previous tests, they do not have a home and host regulator. As the treatment, I use the date that the home market joins the *M*MoU. Admittedly, this country-wide treatment partially negates some of the design features relative to the within-country staggered treatment of the previous tests.

The discussion and empirical results for the FPI analyses suggest that the effects of cooperation on foreign investment extend to purely domestic firms. Once again, however, it is not clear whether this translates into different pricing of the firm or which firms would be affected. If investors respond to cooperation by holding a fully diversified set of firms from the cooperating country, then the impact on individual assets could be weak, because the investment is diffused across many assets. However, prior research shows that foreign investors prefer large, liquid firms (Ferreira and Matos 2008). This suggests that the effects may be concentrated in the largest firms in a given country. Since they are less likely to be affected (and subject to measurement issues that arise from illiquidity), I exclude nano- and micro-cap stocks (those under $500 million in market capitalization).

D.1. Domestic firms: main empirical results

The main empirical test, in Table 7, provides several insights. First, the average level of integration (implied by the local and world betas) is lower for purely domestic firms than for cross-listed firms, both before and after the *M*MoU. This result is predictable based on the intuition that cross-listed firms are more globally integrated than domestic firms. For example, before the treatment, purely domestic firms have local and world betas of 0.672 and 0.377, whereas cross-listed firms show less exposure to their local market and more exposure to the world market with local and world betas of 0.598 and 0.380. Second, although the purely domestic firms' post-*M*MoU decline in the local market beta (-0.154) supports the idea of enhanced integration and is of comparable economic magnitude to that of the cross-listed firms, it is not significant. Of course, statistical insignificance may arise from measurement error in selecting the appropriate date for changes in cooperation, or from pooling a heterogeneous mix of firms—some that experience increased integration with others that do not. The next section is devoted to cross-sectional tests, some of which examine the latter explanation.

D.2. Domestic firms: cross-sectional tests

This section investigates subsamples split by firm size and along the same country-level dimensions as previous tests: legal origin, rule of law, market size, and market development. (I dispense with the distance measure, given that there is no secondary regulator from which to calculate a distance.)

Panel A splits the sample by median firm size within each country. Small firms experience no significant changes in risk exposures (and imply virtually no change in integration). For large firms, however, the local beta declines significantly (by -0.19), and the world beta increases (by 0.05). This indicates that changes in integration are concentrated within the largest firms, and supports the notion that changes in integration extend beyond cross-listed firms to include at least some domestic firms.

The cross-sectional partitions in Panels B-F reveal almost no significant changes in local or world betas. Directionally, they imply more integration for countries with code law, weak rule of law, small market size, and undeveloped markets. This lends stronger support to the idea that cooperation fosters more integration in markets characterized by subpar institutional features. Firms from countries with common law, strong rule of law, and large market size show a slight decrease in integration, which is not expected. Again, measurement error in selecting the date for changes in cooperation may play a role in

these results. Furthermore, these changes in integration could reflect a more sophisticated dynamic that is beyond the scope of this study. For example, domestic investors may be likely move some of their holdings out of cross-listed shares in their own country and into domestic shares in their own country (as is implied by Lang et al. (2020)). How this plays out for the average firm may be complex.

D.3. Domestic firms: robustness tests

Again, I expand the estimation window to gauge the sensitivity and stability of the results. Table 9 demonstrates that the integration effect stays fairly constant over different time horizons. It does not appear to reflect a gradual trend or a temporary period of high (low) world (local) beta.

Overall, the evidence provides modest support for the idea that the effects of cooperation on market integration extend to purely domestic firms. However, the integration effect of cooperation appears to be concentrated in large firms. Relative to cross-listed firms, which start off with a higher degree of integration with global markets, purely domestic firms exhibit a spectrum of integration. Small firms exhibit no effect, but large firms exhibit an increase in integration that is larger than the increase for cross-listed firms.

V. Conclusion

The analyses in this paper shed light on an opaque and hitherto unexplored aspect of capital market integration—cooperation between securities regulators. I study the role of cross-border cooperation between securities regulators in integrating equity markets. Using a research design with properties that rule out many alternative explanations, I find that cooperation via the *M*MoU is associated with an 11% increase in FPI, relative to the time-series of other pairs that include the same investor or investee country. I find similar support for market integration using asset pricing tests. Thus, global risk sharing via investment diversification and integration with world markets appears to depend, at least in part, on regulators' capacity and willingness to cooperate. Such cooperation should benefit both (a) investors, who must balance diversification benefits against adverse selection and other risks, and (b) firms, which often seek to achieve higher valuations and lower cost of capital by integrating with global capital markets.

In addition to being relevant for investors, firms, and regulators, these findings may also be applicable in the context of contemporary policy coordination issues, such as the

Brexit, the EU's Capital Markets Union, and the regulatory responses to recent pandemics.[23] However, policymakers should consider the costs of cooperation under the current system alongside alternative mechanisms or configurations that fall outside the scope of this study. Although the current system has demonstrated its effectiveness, it remains a delicate system built on reciprocal and prudent behavior by signatories. Extending the privilege of cross-border assistance to regulatory counterparts relies on a recipient's judicious use of shared information.

[23] For example, securities regulators have actively pursued a coordinated response to COVID-19 through IOSCO (IOSCO 2020).

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TABLE 1—CROSS-BORDER INVESTMENT

	(1) *Main Result (PPML)*	(2) *Main Result (log-linear)*
MMoU	0.105***	0.377***
	(2.95)	(4.60)
Bilateral MoU	0.084*	0.562***
	(1.91)	(2.79)
Investment treaty	-0.034	0.567***
	(-0.51)	(5.12)
Trade treaty (PTA)	0.026	0.218**
	(0.51)	(2.30)
Tax Treaty	-0.053	-0.039
	(-1.04)	(-0.26)
N	63,957	260,856
R^2	0.99	0.83
Country-pair FEs	Y	Y
Investor-year FEs	Y	Y
Investee-year FEs	Y	Y
Pair time trends	Y	N

Source: Author calculations. This table presents the results of PPML regressions of cross-border investment. Column 2 uses a log-linear specification. Standard errors are clustered by the country-pair level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 2—CROSS-BORDER INVESTMENT ACROSS LEVELS OF PARTITIONING VARIABLES

Panel A: Distance

Close	5.2%*
Medium	9.10%***
Far	14.6****

Panel B: Capital controls

Investee country	*Capital controls*	-2.0%
	(No controls)	13.6%***

Panel C: Common Law Origin

		Investor country	
		Other	*Common*
Investee country	*Other*	8.8%**	9.4%**
	Common	8.4%	15.1%***

Panel D: Rule of Law

		Investor country		
		Weak	*Middle*	*Strong*
Investee country	*Weak*	74.8%	68.4%*	32.1%***
	Middle	86.8%	-19.2%	-3.9%
	Strong	45.8%**	-2.1%	10.2%***

Panel E: Market size

		Investor country		
		Small	*Medium*	*Large*
Investee country	*Small*	-6.2%	-4.0%	-2.7%
	Medium	10.9%	0.7%	7.5%
	Large	11.8%	9.3%**	11.9%***

Panel F: Market Development

		Investor country		
		Frontier	*Emerging*	*Developed*
Investee country	*Frontier*	-0.2%	2.0%	8.6%
	Emerging	26.5%*	-6.1%	0.6%
	Developed	8.1%*	7.7%	13.5%***

Source: Author calculations. This table constructs the estimates of the effect of the *M*MoU across nine conditions of market development for country pairs, using PPML regressions of cross-border investment. Standard errors are clustered by the country-pair level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level. No adjustments are made to account for performing multiple tests.

TABLE 3—CROSS-BORDER INVESTMENT (ADDITIONAL TESTS)

Test description	(1) *M*MoU countries only	(2) Controlling for bilateral trade	(3) EU pairs	(4) US	(5) Countries with US cross-listings (from Lang et al. 2019)	(6) Controlling for cross-listings
MMoU	0.091***	0.098***	0.087***	0.098***	0.093**	0.110**
	(2.98)	(3.80)	(2.71)	(3.04)	(2.29)	(2.52)
Bilateral MoU	0.122***	0.065	0.104***	0.075*	0.075*	0.080
	(3.11)	(1.33)	(2.65)	(1.89)	(1.90)	(1.61)
*MMoU*EU pair*			0.000			
			(0.00)			
$MMoU*US_i$				0.038		
				(1.03)		
$MMoU*US_j$				0.014		
				(0.44)		
$MMoU*Countries\ with\ US\ x\text{-}list_i$					0.013	
					(0.36)	
$MMoU*Countries\ with\ US\ x\text{-}list_j$					0.016	
					(0.54)	
$Trade_{i\ to\ j}$		-0.004				
		(-0.69)				
$Trade_{j\ to\ i}$		0.003				
		(0.57)				
*MMoU*X-list_indicator*						-0.008
						(-0.03)
Investment treaty	-0.018	-0.017	-0.010	-0.018	-0.017	0.030
	(-0.43)	(-0.46)	(-0.24)	(-0.46)	(-0.43)	(0.67)
Trade treaty (PTA)	0.045*	0.033	0.051**	0.032	0.030	-0.059
	(1.78)	(0.92)	(2.05)	(1.12)	(1.04)	(-1.03)
Tax Treaty	-0.051	-0.066*	-0.062*	-0.046	-0.044	-0.044
	(-1.45)	(-1.73)	(-1.90)	(-1.29)	(-1.25)	(-0.77)
N	44,288	61,957	61,957	61,957	61,957	61,957
R^2	0.99	0.99	0.99	0.99	0.99	0.99
Country-pair FEs	Y	Y	Y	Y	Y	Y
Investor-year FEs	Y	Y	Y	Y	Y	Y
Investee-year FEs	Y	Y	Y	Y	Y	Y

Source: Author calculations. This table presents the results of PPML regressions of cross-border investment. Standard errors are clustered at the country-pair level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 4—MAIN TESTS OF CHANGES IN BETA-*CROSS-LISTED FIRMS*

	(1) *Main result*	(2) *Non-US cross-listed*	(3) *US-Cross-listed*
R^L	0.598***	0.663***	0.481***
	(6.54)	(5.73)	(3.17)
R^W	0.380***	0.319***	0.477
	(6.43)	(4.08)	(4.61)
Post	-0.001	-0.001	-0.000
	(-0.75)	(-0.86)	(-0.07)
R^L*post*	-0.081**	-0.065*	-0.135**
	(-1.98)	(-1.74)	(-2.16)
R^W*post*	0.065	0.052	0.128**
	(1.24)	(1.03)	(3.33)
N observations	120,639	71,962	48,677
N firms	1,166	718	448
N clusters	206	172	34
R^2	0.167	0.193	0.135

Source: Author calculations. This table reports the results of estimating asset pricing models based on equation (3): $Ret=\beta_0+\beta_1 R^L+\beta_2 R^W+\beta_3 Post+\beta_4 R^L * Post+\beta_5 R^W * Post+\varepsilon_t$. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level. Standard errors are clustered at the country-pair level.

TABLE 5—CROSS-SECTIONAL TESTS OF CHANGES IN BETA-*CROSS-LISTED FIRMS*

Panel A: Distance

		ΔR^L	ΔR^W
	Close	0.02	-0.05
	Medium	-0.04	0.00
	Far	-0.21***	0.18***

Panel B: Capital controls

		ΔR^L	ΔR^W
Home country	*Capital controls*	-0.10	0.07
	(No controls)	-0.08**	0.08**

Panel C: Common law

		Host country			
		Code		*Common*	
		ΔR^L	ΔR^W	ΔR^L	ΔR^W
Home country	*Code*	-0.08	0.03	-0.09**	-0.01
	Common	-0.07	-0.03	-0.11***	0.13***

Panel D: Rule of law

		Host country			
		Weak		*Strong*	
		ΔR^L	ΔR^W	ΔR^L	ΔR^W
Home country	*Weak*	-0.04	0.08*	-0.06	0.15
	Strong	-0.13**	0.02	-0.30***	0.12

Panel E: Market size

		Host country			
		Small		*Large*	
		ΔR^L	ΔR^W	ΔR^L	ΔR^W
Home country	*Small*	-0.07	0.07	-0.17***	0.15**
	Large	-0.17*	0.08	-0.27**	0.22**

Panel F: Market development

		Host country			
		Undeveloped		*Developed*	
		ΔR^L	ΔR^W	ΔR^L	ΔR^W
Home country	*Undeveloped*	-0.05	0.04	-0.08	0.02
	Developed	0.01	0.10	-0.08**	0.08**

Source: Author calculations. This table reports the results of estimating asset pricing models based on equation (3): $Ret=\beta_0+\beta_1 R^L+\beta_2 R^W+\beta_3 Post+\beta_4 R^L * Post+\beta_5 R^W * Post+\varepsilon_t$.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level. Standard errors are clustered at the country-pair level.

TABLE 6—ALTERNATIVE TIME HORIZONS-*CROSS-LISTED FIRMS*

Years pre/post	Φ_{Pre}	Φ_{Post}	$\Delta\Phi$
1	0.389	0.463	0.074
1.5	0.301	0.461	0.160
2	0.286	0.423	0.137
2.5	0.231	0.417	0.187
3	0.225	0.404	0.178
3.5	0.224	0.399	0.175
4	0.203	0.396	0.193

Source: Author calculations. This table reports the results of estimating asset pricing models based on equation (3): $Ret=\beta_0+\beta_1 R^L+\beta_2 R^W+\beta_3 Post+\beta_4 R^L * Post+\beta_5 R^W * Post+\varepsilon_t$. Each estimation is described in the text. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.
Standard errors are clustered at the country-pair level.

TABLE 7—MAIN TESTS OF CHANGES IN BETA-*DOMESTIC FIRMS*

	(1)
R^L	0.672***
	(4.76)
R^W	0.377*
	(1.78)
Post	0.000
	(0.21)
R^L*post*	-0.154
	(-1.51)
R^W*post*	0.012
	(0.31)
N observations	495,880
N firms	6,535
N clusters	51
R^2	0.181

Source: Author calculations. This table reports the results of estimating asset pricing models based on equation (3): $Ret=\beta_0+\beta_1 R^L+\beta_2 R^W+\beta_3 Post+\beta_4 R^L*Post+\beta_5 R^W*Post+\varepsilon_t$.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level. Standard errors are clustered at the country level.

TABLE 8—CROSS-SECTIONAL TESTS OF CHANGES IN BETA-
DOMESTIC FIRMS

Panel A: Firm size

		ΔR^L	ΔR^W
	Small	-0.13	-0.02
	Large	-0.19*	0.05

Panel B: Capital controls

		ΔR^L	ΔR^W
Home country	*Capital controls*	0.04	-0.10
	No capital controls	-0.19	-0.03

Panel C: Common law

		ΔR^L	ΔR^W
Home country	*Code*	-0.15	0.01
	Common	0.06	0.02

Panel D: Rule of law

		ΔR^L	ΔR^W
Home country	*Weak*	-0.17	0.04
	Strong	0.07	-0.07

Panel E: Market size

		ΔR^L	ΔR^W
Home country	*Small*	-0.13	-0.03
	Large	0.01	-0.10

Panel F: Market development

		ΔR^L	ΔR^W
Home country	*Undeveloped*	-0.15	-0.04
	Developed	0.06	-0.14

Source: Author calculations. This table reports the results of estimating asset pricing models based on equation (3):
$Ret=\beta_0+\beta_1 R^L+\beta_2 R^W+\beta_3 Post+\beta_4 R^L * Post+\beta_5 R^W * Post+\varepsilon_t$.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level. Standard errors are clustered at the country-level for panel A. Panels B-F contain cells with as few as 17 country clusters, so standard errors are clustered at the country-year level.

TABLE 9—ALTERNATIVE TIME HORIZONS-*DOMESTIC FIRMS*

Years pre/post	Φ_{Pre}	Φ_{Post}	$\Delta\Phi$
1	0.36	0.43	0.07
1.5	0.33	0.43	0.10
2	0.28	0.36	0.07
2.5	0.27	0.34	0.07
3	0.25	0.33	0.08
3.5	0.26	0.33	0.07
4	0.28	0.31	0.04

Source: Author calculations. This table reports the results of estimating asset pricing models based on equation (3): $Ret=\beta_0+\beta_1 R^L+\beta_2 R^W+\beta_3 Post+\beta_4 R^L * Post+\beta_5 R^W * Post+\varepsilon_t$. Each estimation is described in the text. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.
Standard errors are clustered at the country level.

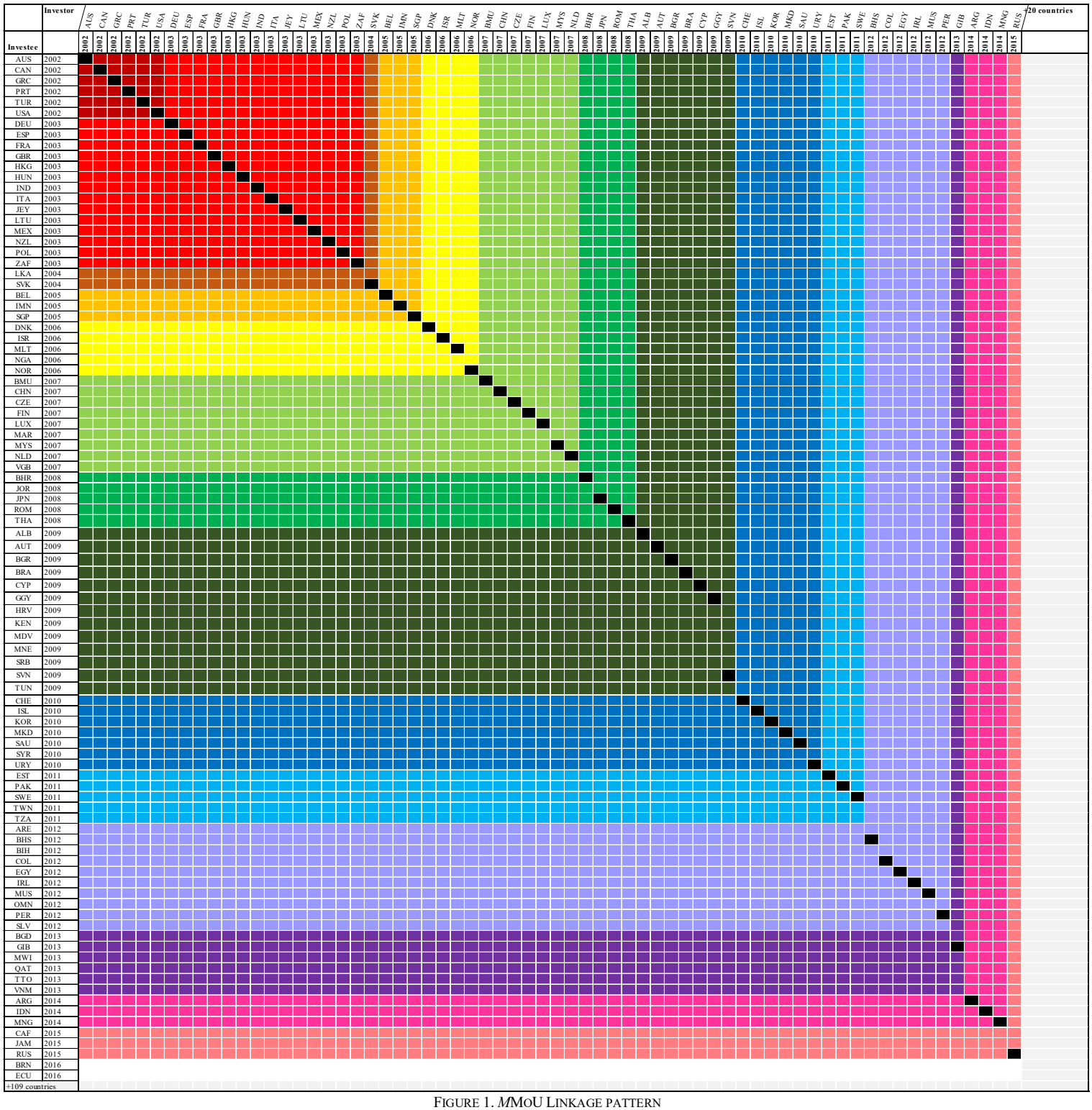


FIGURE 1. *M*MOU LINKAGE PATTERN

Notes: This adjacency matrix represents each of the country pairs in the FPI analysis and indicates when the treatment occurs for each pair. For brevity, investor and investee countries are reported using their International Organization for Standardization (ISO) three-digit codes. Shocks that occur in the same year have the same color, so blocks of the country pairs with the same color experience the shock at the same time (and different colored pairs experience the treatment at different times). The year of the treatment is the larger of the *M*MoU adoption years for the associated row and column. The matrix demonstrates that each individual country (row or column) typically has substantial variation in the treatment date, depending on its joining date and the counterpart country's joining date. Each cell has a 17-year time series. An additional 109 investee countries and 20 investor countries are suppressed because of space considerations, and because they do not receive the treatment.

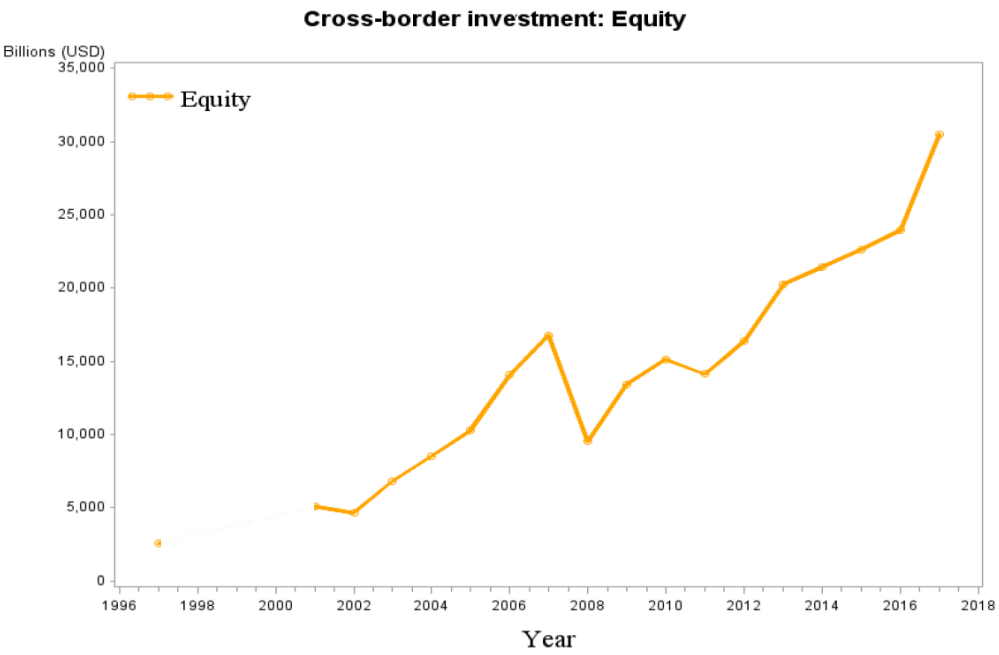


FIGURE 2. TOTAL CROSS-BORDER INVESTMENT OVER TIME

NOTES: THIS FIGURE SHOWS CROSS-BORDER INVESTMENT IN THE FORM OF EQUITY FROM THE CPIS SURVEY.

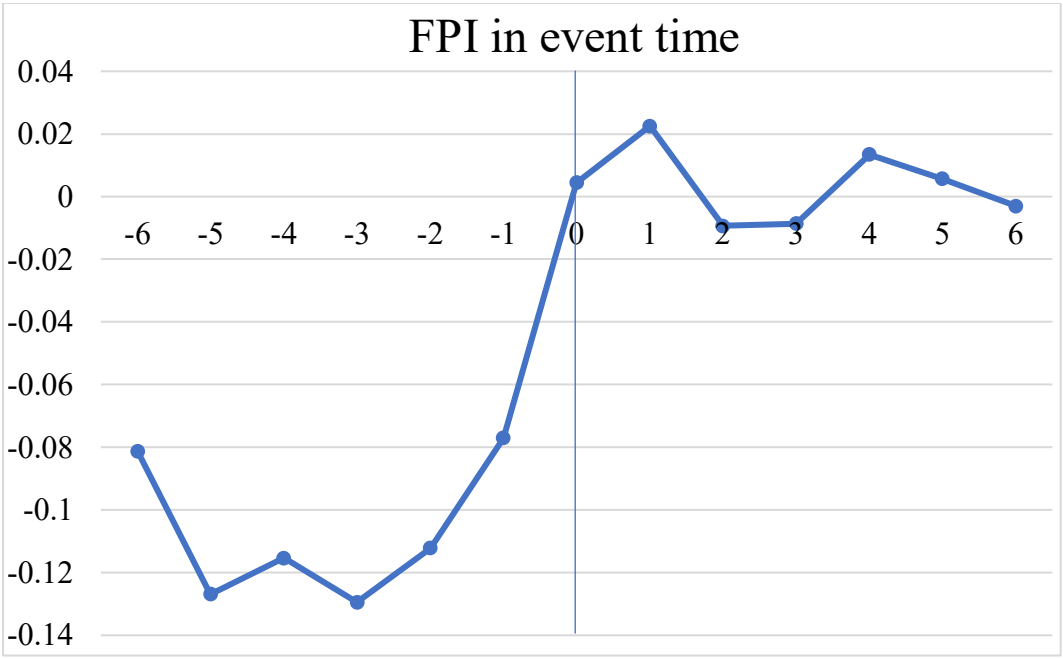
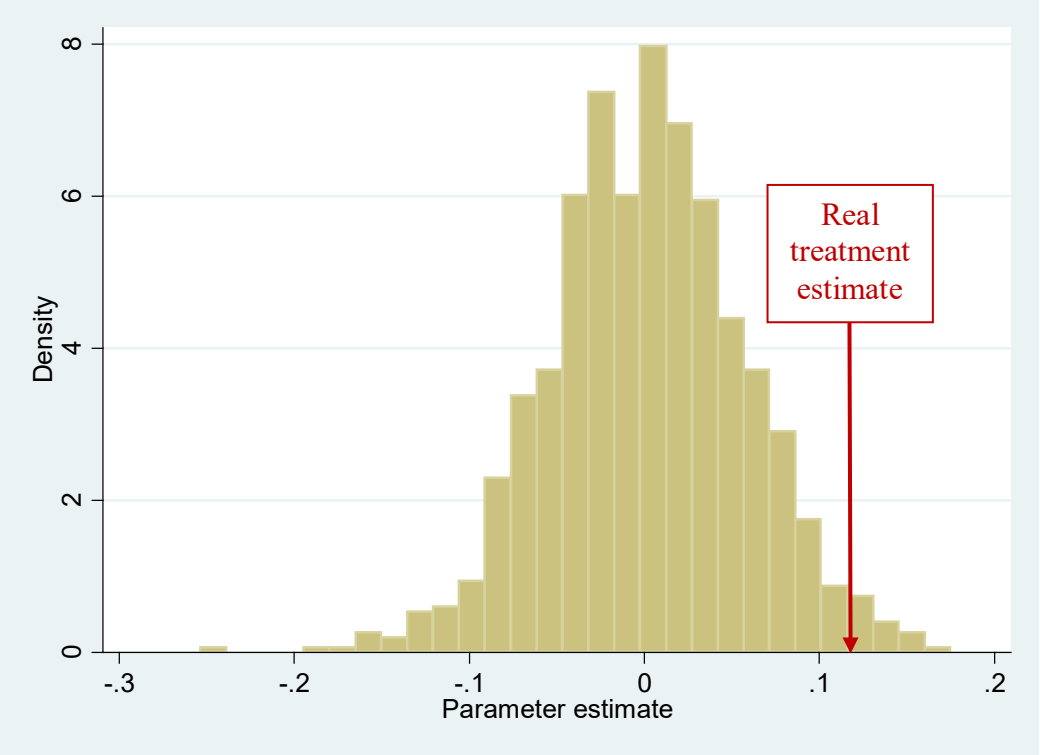


FIGURE 3. CROSS-BORDER INVESTMENT IN EVENT TIME

NOTES: THIS FIGURE SHOWS THE EFFECT OF THE *M*MOU ON FPI IN EVENT TIME. THE X-AXIS REPRESENTS YEARS RELATIVE TO THE *M*MOU LINKAGE DATE, AND THE Y-AXIS REPRESENTS THE COEFFICIENT (PRIOR TO EXPONENTIATING).



INTERNET APPENDIX FIGURE 1. DISTRIBUTION OF ESTIMATES FROM PSEUDO-TREATMENT

NOTES: THIS HISTOGRAM SHOWS THE DISTRIBUTION OF THE PSEUDO-TREATMENT COEFFICIENTS (AS DESCRIBED IN SECTION *III CROSS-BORDER INVESTMENT: E. IDENTIFICATION AND ROBUSTNESS TESTS*).

IOSCO's Committee 4 is a subcommittee that comprises a group of global representatives from IOSCO members. Committee 4's full name is the "Committee on Enforcement and the Exchange of Information and the Multilateral Memorandum of Understanding Screening Group," but it is known as "SG/C4." When a regulator submits its application to the *M*MoU, the application is closely examined by a specially constituted Verification Team (the "VT") (usually a geographically diverse, nine-member group that, for efficiency, often includes members who are familiar with the applicant). The process either starts a formal application, or a consultation with the SG regarding any special circumstances in terms of a regulator's eligibility. A hypothetical timeline based on actual applicants is provided below.

January 1, 2010: Mounting global political pressure to combat terrorist financing and money laundering stirs a regulator (which I will call the "FMA") to pursue the *M*MoU. This pressure can come from parliaments or presidential cabinet members (or other legislative authorities), IOSCO itself (in 2005, IOSCO gave its members 5 years to sign the *M*MoU or risk losing their voting rights), the IMF, World Bank, FSB, or other regulatory peers.

March 1, 2010: After evaluating their own qualifications pursuant to the *M*MoU, the FMA, in conjunction with their local government, proposes new legislation to revise the legal framework for cooperation in the field of securities supervision. This draft legislation is intended to address shortcomings that arise from bank secrecy laws, blocking statutes, or procedural issues. For market participants, this is an early signal that the FMA is preparing to sign the *M*MoU.

April 1, 2010: The FMA files its application to become a signatory of the *M*MoU. As part of the application, they submit a completed questionnaire, including descriptions of the pending legislative proposals.

April 15, 2010: The application is forwarded to the VT members for review.

July 1, 2010: Based on the FMA's answers to the *M*MoU questionnaire, an initial evaluation is made and a report is drafted with a recommendation to accept (or require revisions or further information). The report is delivered to the SG for consideration at the next semi-annual SG meeting.

August 1, 2010: The draft law proposed on March 1, 2010, is passed and enters into force on January 1, 2011.

November 1, 2010: At the SG meeting, representatives of the FMA are invited to attend a discussion of the FMA's eligibility. The SG could reach a consensus that the applicant meets all of the criteria and thus proceed to the next step, or it could require additional legislative changes or solicit more information, in which case the application could be reconsidered at the next semi-annual meeting (six months later).

October 15, 2010: If additional changes are required, additional iterations of the step described on March 1 can take place.

January 1, 2011: The new laws enter into force on January 1, 2011.

February 15, 2011: *M*MoU is signed (unless signing is delayed until IOSCO Annual Meeting in April).

INTERNET APPENDIX TABLE I—CROSS-BORDER INVESTMENT

	(1)
	FPI: Developed country-pair sample
MMoU	0.088**
	(2.13)
Bilateral MoU	-0.053
	(-0.99)
Investment treaty	0.045
	(1.37)
Trade treaty (PTA)	-0.114**
	(-2.19)
Tax Treaty	-0.056
	(-0.94)
N	6,720
R^2	0.99
Country-pair FEs	Y
Investor-year FEs	Y
Investee-year FEs	Y

Source: Author calculations. This table presents the results of estimations described in the text. Standard errors are clustered by the country-pair level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.