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Money-Laundering, Grey-Listing by the FATF, and Cross-Border Financial Flows

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Abstract

Does Financial Action Task Force (FATF) grey-listing disrupt cross-border financial flows? The answer is not obvious. Grey-listing imposes no formal transaction restrictions, yet it can trigger compliance responses by financial institutions facing heightened regulatory risk and the prospect of future blacklisting. Identification is further complicated by anticipation, as firms may adjust during the evaluation process, attenuating observed changes at the time of designation. We combine banking transactions, balance-of-payments capital flows, and foreign-exchange data with complementary identification strategies to isolate the incremental impact of formal grey-listing. To address high-dimensional confounding and separate medium-run adjustments from announcement-driven dynamics, we pair Double Debiased Machine Learning estimates in a fixed-effects panel with a Regression Discontinuity in Time design that exploits the sharp publication timing of FATF decisions. Across approaches, grey-listing produces economically meaningful contractions in inflows: banking and capital inflows fall by 1.3–2.6 percent of quarterly GDP, while outflow responses are smaller and less robust. Regression discontinuity estimates corroborate these results, showing a 1.2–1.4 percentage point drop in inflows at the listing cutoff. Delisting triggers only partial recovery (40–70 percent), consistent with persistent frictions in correspondent banking relationships.

Keywords: FATF grey-listing, capital flows, International financial regulation, Compliance, Correspondent banking, Machine learning. **JEL Codes:** C14, F21, F36, F38, G15.

1 Introduction

In the current global context, cross-border trade, investment, and migration are increasingly mediated by international payment systems and correspondent banking networks. Because much of this infrastructure is governed by risk management and compliance protocols at global intermediaries, regulatory actions and public designations can generate externalities that alter the effective cost of moving capital across borders. This paper studies one such designation: FATF grey-listing, a prominent tool of international AML/CFT enforcement, and asks whether the formal designation itself activates discrete compliance responses that measurably disrupt cross-border financial flows.

To safeguard the integrity of the international financial system, G7 countries created the Financial Action Task Force (FATF) as a global standard-setting body.[1] FATF's mandate is to promote the adoption and effective implementation of minimum anti–money laundering and counter–terrorist financing (AML/CFT) standards worldwide. To incentivize compliance, FATF relies on a system of evaluations and public listings that identify jurisdictions with strategic deficiencies in their AML/CFT frameworks.[2]

FATF maintains two public lists. Jurisdictions under increased monitoring, commonly referred to as the *grey list*, are those that have committed to working with FATF to address identified deficiencies. High-risk jurisdictions subject to a call for action, often referred to as the *black list*, are those that have failed to take sufficient remedial steps and are not cooperating with FATF. These listings represent FATF's most prominent enforcement mechanism. While FATF continuously updates its standards to address emerging risks, many policymakers and observers question whether listings, particularly grey-listing, continue to exert meaningful influence on countries' economic and financial outcomes.

Despite the central role of FATF listings in the global AML/CFT architecture, a fundamental empirical question remains unresolved: does grey-listing disrupt countries' access to cross-border finance in an economically meaningful way? While grey-listing does not itself impose formal restrictions on financial transactions, the public designation can prompt risk-based adjustments by foreign intermediaries—including changes in access to payment services, correspondent banking lines, and trade-finance relationships—that raise the effective cost of moving capital across borders. Identifying the incremental effect of the formal designation is challenging because financial institutions may respond during the evaluation

[1] The FATF was created by the G7 countries and the European Commission—Canada, France, Germany, Italy, Japan, the United Kingdom, the United States, and the European Commission—together with eight additional jurisdictions: Australia, Austria, Belgium, Luxembourg, the Netherlands, Spain, Sweden, and Switzerland.

[2] Evaluation outcomes can broadly result in satisfactory ratings, placement on the grey list, or placement on the black list.

process, muting observable changes at the publication date.

Existing studies provide mixed and sometimes weak evidence, leaving open whether grey-listing operates as a binding regulatory shock or primarily reflects information already priced into global financial relationships. These ambiguities largely reflect limitations in both data and empirical design. Much of the existing literature relies on annual or low-frequency data that cannot isolate responses at the time of listing, and on reduced-form panel specifications with manually selected controls in a high-dimensional global macro-financial environment. In such settings, anticipatory adjustments during the FATF evaluation process, combined with omitted global push factors, can attenuate estimates and mask the incremental effects of formal grey-listing.

Importantly, the object of interest in this paper is deliberately narrow. We do not seek to estimate the total economic cost of AML/CFT compliance, nor the full effect of information revelation about country risk associated with FATF monitoring. Instead, we focus on the residual publication-date component (and short-window deviations around publication) that remains after accounting for pre-publication adjustment, and interpret it as the component most closely linked to compliance activation in global payment and correspondent-banking infrastructure.

This question has gained renewed relevance as FATF enters its Fifth Round of mutual evaluations, which began in late 2025 and will continue for nearly six years. Unlike earlier rounds, the Fifth Round places greater emphasis not only on technical compliance, but also on the effectiveness of AML/CFT systems in practice, which may be harder to attain. As more countries undergo an evaluation under this framework, understanding whether grey-listing continues to impose real and measurable financial consequences—and through which channels—has become increasingly important.

To mitigate omitted-variable bias in a high-dimensional environment and to isolate timing-driven effects, the paper combines two complementary designs. First, Double Selection Lasso and Double Debiased Machine Learning (DDML) estimate medium-run changes in cross-border flows while selecting controls from a rich macro-financial set. Second, a Regression Discontinuity in Time (RDiT) design exploits the sharp and public timing of FATF listing decisions to identify localized discontinuities at the official cutoff using narrow symmetric windows. We implement these approaches across multiple datasets that capture cross-border financial activity, including balance-of-payments aggregates, banking transactions, and foreign-exchange market outcomes.

Across specifications, we find economically meaningful contractions in cross-border inflows around grey-listing, while outflow responses are smaller and less robust. These patterns are consistent with compliance-driven de-risking that disproportionately affects inbound fi-

nancial activity: formal grey-listing can trigger automated risk protocols at global correspondent banks and payment intermediaries, tightening inbound credit lines and increasing screening frictions in cross-border payments. Announcement-timed estimates are somewhat larger than listing-timed ones, indicating partial anticipation during the evaluation process; however, the sharp discontinuities at the listing cutoff suggest that formal listing activates additional compliance mechanisms not fully priced in beforehand. Outflow responses are more heterogeneous—consistent with slower portfolio reallocation and substitution across channels—and therefore less stable across designs. At delisting, responses partially unwind, consistent with persistent cross-border financial frictions—likely reflecting the fixed costs of rebuilding correspondent relationships, which unwind more slowly than they arise.

These findings are directly relevant to the Fifth Round of FATF evaluations. They provide empirical evidence that grey-listing continues to operate as an effective market-enforcement and compliance-pressure mechanism—at a measurable financial cost—by tightening access to external finance on the inflow margin and, in some cases, accelerating outflows. More broadly, the results inform ongoing debates about the trade-offs inherent in AML/CFT enforcement by quantifying immediate disruptions and medium-run reallocations in cross-border finance. Importantly, these findings speak to compliance activation and market discipline in formal financial channels, not to AML effectiveness in the narrower sense of reducing money laundering, predicate offences, or other illicit activity.

A final feature of the results is that the average effects are not uniform across country types. We show that the contraction in inflows is concentrated in EMDEs, in more trade-open economies, and in countries with higher pre-treatment capital account openness. These heterogeneity patterns are consistent with the interpretation that grey-listing matters most where cross-border intermediation, external funding, and correspondent-banking links are more central to the functioning of the formal financial system.

Throughout the paper we focus on two related but distinct parameters of interest. First, using Double Selection Lasso and DDML, we estimate a medium-run, window-average effect of grey-listing on cross-border financial flows: the average deviation of flows from their counterfactual path in a symmetric window around the listing date, conditional on country and time fixed effects and a rich set of macro-financial controls. Second, using the RDiT design, we estimate the local discontinuity in flows at the formal publication date of the grey list, interpreted as the instantaneous activation effect of binding compliance frictions in global payment and correspondent-banking systems. These two parameters are complementary: the medium-run panel estimates summarize the overall adjustment in the quarters surrounding grey-listing, while the RDiT estimates isolate the sharp policy-induced jump at the listing cutoff. In the empirical sections we show that both effects are quantitatively important,

and we use their joint behavior to distinguish anticipatory information effects from discrete compliance activation and from the slow unwinding of frictions at delisting.

This paper contributes to the literature in three main ways. First, it introduces a new empirical framework to study the financial consequences of FATF grey-listing by combining multiple high-frequency datasets that capture distinct channels of cross-border financial activity. The analysis integrates balance-of-payments statistics, banking transaction proxies, and foreign-exchange turnover measures, allowing us to observe how different components of international financial flows respond to AML/CFT enforcement signals. This multi-source approach provides a more granular view of the financial transmission channels through which compliance shocks affect cross-border finance.

Second, the paper separates two mechanisms that the existing literature often conflates: anticipatory adjustments during the FATF evaluation process and the compliance activation that follows the formal publication of grey-listing decisions. To distinguish these effects, the empirical strategy combines panel-based causal inference methods with a regression discontinuity in time (RDiT) design around official listing and delisting dates. This approach isolates the incremental impact of the public designation itself, over and above the adjustments that may occur during the evaluation phase.

Third, the paper provides new evidence on the mechanisms linking FATF grey-listing to disruptions in cross-border financial flows. The results indicate that grey-listing produces large and immediate contractions in financial inflows, particularly in banking-related transactions, while the response of outflows remains smaller and less systematic. This asymmetric pattern aligns with compliance-driven de-risking by global financial intermediaries, consistent with the growing literature on correspondent banking retrenchment and regulatory spillovers in international finance.

The remainder of the paper proceeds as follows. Section 2 reviews the relevant literature. Section 3 describes the empirical strategy, data sources, and variable construction. Section 4 presents the associated estimation results and robustness checks. Section 5 concludes with a discussion of policy implications.

2 Relevant Literature

From an international-economics perspective, FATF listing decisions can be interpreted as a globally coordinated regulatory shock that propagates through cross-border banking and payment intermediation, with potential spillovers to external finance and trade-related payments. The empirical literature examining the effects of FATF grey-listing on international financial flows has expanded in recent years, yet it remains characterized by mixed findings

and substantial ambiguity regarding causal interpretation. While grey-listing is often discussed as a mechanism capable of disrupting cross-border financial activity, existing evidence varies widely in magnitude, sign, and robustness across studies. A central reason for this heterogeneity lies not only in differences across countries or flow categories, but also in the empirical strategies used to identify grey-listing effects.

Descriptive and Panel-Based Evidence. A first strand of the literature relies on descriptive analyses and panel regressions using low-frequency balance-of-payments, banking, or financial market data. These studies typically estimate associations between grey-listing status and aggregate financial outcomes over long horizons. For example, Kudrle (2009), analyzing 38 tax havens between 2000 and 2007, finds little systematic evidence that grey-listing affects jurisdictions' shares of global financial activity as measured by BIS offshore banking statistics. Similarly, Balakina et al. (2017), using annual data for 126 countries, report no consistent effect of grey-listing on the growth of total foreign claims. When disaggregating flows, they document increases in outflows (foreign assets) but limited effects on inflows (foreign liabilities), pointing to asymmetric but noisy responses.

Related work using macro-financial indicators reaches similar conclusions. Farias and de Almeida (2014) examine the reaction of stock markets and foreign exchange rates to FATF public statements and find negligible or statistically insignificant short-run effects. While suggestive, these results rely on annual or low-frequency data and reduced-form specifications that make it difficult to disentangle the impact of grey-listing from contemporaneous macroeconomic shocks, regional trends, or pre-existing institutional weaknesses.

While informative, this strand faces important identification challenges. Grey-listing is inherently endogenous: jurisdictions are placed on the list precisely because of weaknesses in their regulatory frameworks, which are themselves correlated with financial flows. Moreover, the reliance on annual data masks the immediate impact of listing events, conflating the treatment with broader macroeconomic cycles and global financial conditions. As a result, estimates from these specifications are often attenuated or otherwise difficult to interpret causally.

Event Studies and Quasi-Experimental Approaches. A second strand seeks to improve identification by exploiting the timing of FATF decisions through event studies or difference-in-differences designs. These approaches examine changes in financial flows around listing or delisting episodes, aiming to capture short- to medium-run dynamics. For example, Kida and Paetzold (2021) document declines in foreign direct investment following grey-listing, though with substantial heterogeneity across countries. More recently, Azinge-Egbiri

et al. (2025) argue that listing can trap developing economies in cycles of superficial compliance while generating real economic costs. Complementary evidence by De Koker et al. (2023) shows that grey-listing is associated with reductions in official development assistance and multilateral lending, suggesting that risk aversion extends beyond private markets to public-sector donors.

Despite these advances, event-study and quasi-experimental designs remain sensitive to model specification and control selection. Traditional implementations often rely on a limited set of manually chosen covariates and relatively coarse data frequency. In a high-dimensional global financial environment, financial flows respond to a wide array of push factors—such as global risk aversion, U.S. monetary policy, and commodity price cycles—that may correlate with the timing of FATF decisions. Manual control selection risks omitting relevant confounders, leading to unstable or sample-sensitive estimates.

Furthermore, simple pre–post comparisons frequently fail to account for anticipatory behavior. Because the FATF evaluation process unfolds over extended periods and generates information prior to formal listing decisions, financial institutions may adjust their behavior in advance. In such settings, before–after designs risk understating or misattributing the true effect of grey-listing if anticipation is not explicitly addressed.

Recent related work also suggests that the relationship between AML/CFT enforcement and foreign direct investment is unlikely to be monotonic. Ofoeda et al. (2022b) show that stronger AML systems can support FDI at lower levels of compliance intensity but may discourage inflows once compliance burdens become sufficiently high or in offshore jurisdictions. In related work, Ofoeda et al. (2022a) show that AML regulation conditions the FDI–growth nexus, while De Koker et al. (2023) synthesize evidence that FATF grey-listing is associated with broader market costs, including weaker capital inflows. More broadly, recent work on FATF blacklisting also shows that estimated financial-flow effects are sensitive to sample construction and treatment definition, reinforcing the need for careful identification in this literature (Case-Ruchala and Nance, 2024).

Granular Data, Mechanisms, and Market Enforcement. A third strand of the literature leverages granular data and institutional frameworks to shed light on the mechanisms underlying grey-listing effects. Morse (2019) provides a key theoretical foundation, arguing that the primary influence of the grey list operates through "market enforcement." In this view, global banks and financial intermediaries act as de facto enforcers of FATF standards by adjusting correspondent relationships and exposure to jurisdictions perceived as high-risk, in order to manage compliance costs and regulatory risk.

Building on this market-enforcement perspective, Masciandaro (2013) frames FATF list-

ing as a public "name-and-shame" signal that can generate non-linear compliance and reputational costs for cross-border counterparties, thereby discouraging transactions with the listed jurisdiction. Importantly, this stigma-based discipline is consistent with asymmetric responses across flow directions: external intermediaries may more readily limit new exposures and inbound payment relationships than unwind pre-existing outward positions, while the strength of the response depends on market efficiency and country-specific characteristics. This observation motivates identification strategies that exploit higher-frequency data around discrete listing events.

Beyond the FATF-specific literature, our proposed mechanism is also closely related to the broader correspondent-banking de-risking literature. Since 2015, the Financial Stability Board (FSB) and the Committee on Payments and Market Infrastructures (CPMI) have documented a persistent decline and increasing concentration in correspondent banking relationships, emphasizing the implications for cross-border payments, trade, remittances, and financial inclusion (Financial Stability Board, 2015, 2016, 2018, 2019; Committee on Payments and Market Infrastructures, 2016, 2023). In our setting, FATF grey-listing can be interpreted as a jurisdiction-specific trigger that raises compliance costs within precisely those correspondent-banking and payment-message networks, making de-risking a natural transmission channel rather than a separate phenomenon.

Empirical work using granular payment and network data lends support to this channel, albeit with important nuances. Using monthly SWIFT data, Collin et al. (2016) show that grey-listing is associated with a 7–10 percent decline in the number of payments received by affected countries, while finding no consistent effect on payments sent or on bilateral volumes between jointly listed countries. This pattern highlights pronounced asymmetries, suggesting that disruptions operate primarily through inbound financial channels. Similarly, Maslen (2023) documents reductions in correspondent banking relationships following grey-listing, identifying de-risking as a central transmission mechanism.

Although several of the studies cited above aim to estimate causal impacts, limitations in data frequency, control selection, and identification design often complicate causal interpretation. Even with granular data, disentangling grey-listing effects from global financial conditions, sovereign risk dynamics, or concurrent regulatory changes remains challenging.

Methodological Contribution. This paper advances the literature by directly addressing these identification limits. First, we build on the methodological opening provided by Kida and Paetzold (2021), who introduced Lasso-based variable selection into the study of grey-listing and capital flows. We extend this approach by employing Double Selection Lasso (DSL) and Double Debiased Machine Learning (DDML) on a broader, higher-frequency

dataset that includes banking messaging and foreign-exchange turnover. This strategy allows us to mitigate omitted-variable bias in a high-dimensional environment while accommodating the complex correlation structure of global macro-financial variables.

Second, we complement these machine-learning-based panel estimates with a Regression Discontinuity in Time (RDiT) design. While ML methods improve control selection, the RDiT framework exploits the sharp and public timing of official listing and delisting decisions to isolate localized causal effects. This design overcomes the granularity limits of annual data and further mitigates concerns about endogeneity in country fundamentals.

By organizing identification around both high-dimensional control selection and sharp temporal discontinuities, we provide a unified framework that helps reconcile previously conflicting findings. Once identification is tightened, the ambiguous effects reported in earlier work resolve into a clear and asymmetric pattern: a sharp contraction in inflows consistent with the market-enforcement mechanism emphasized by Morse (2019), alongside a more muted and heterogeneous response in outflows. This asymmetry is consistent with structural frictions in cross-border payment systems and correspondent banking relationships rather than symmetric shifts in investor demand.

3 Bringing the pieces together: financial flows data, grey list events, and control variables

Data, Events, and Control Variables

In light of previous studies that have examined the relationship between sanctions, listings, and financial flow behavior, this paper focuses on contributions that help close existing knowledge gaps. In particular, we aim to implement econometric methodologies that reduce variable selection bias and allow us to statistically revisit the effects of the grey-listing process as a discrete event that may—or may not—have causal impacts on cross-border financial flows. We are especially interested in whether observable measures of external exposure—such as trade openness and economy size—magnify or mitigate the effects of grey-listing. Given data constraints, we interpret these covariates as reduced-form proxies for global integration rather than as measures of bilateral network position. With this in mind, the paper builds on earlier work (Kudrle (2009), Collin et al. (2016), Kida and Paetzold (2021), Azinge-Egbiri et al. (2025)), all of which use various types of balance of payments flow data as dependent variables, treating FATF grey-listing as the key event variable.

To fix notation, we write the empirical relationship of interest in the following reduced-

form specification:[3]

$$y_{i,t} = \beta_0 + \delta_0 z_{i,t} + X_{i,t}\beta_1 + W_{i,t}\beta_2 + \epsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ corresponds to a financial flow, $z_{i,t}$ represents a listing event variable, $X_{i,t}$ and $W_{i,t}$, the domestic and external control variables, respectively.

As a first step, we describe the data and the variables that we include in the model. We then explain the methodology we follow to select the control variables. Finally, we define the variable for the event, $z_{i,t}$, based on a step-by-step breakdown of the mutual evaluation process that can result in the addition of a country to the grey list.

Variables to proxy Financial Flows

The objective of this paper is to identify evidence—ideally causal—on the impact of FATF grey-listing events on financial flows. The first step involves selecting metrics that effectively approximate cross-border financial movements. These may include trade flows, capital flows, foreign-exchange (FX) transactions, or interbank payment and settlement activities between countries. A second criterion for data selection focuses on comparability across countries. This requires identifying datasets that consistently measure the same variables across jurisdictions, ideally compiled using stable methodologies over time. A prominent example is the International Financial Statistics (IFS) database maintained by the International Monetary Fund (IMF). Third, the analysis prioritizes data density—that is, maximizing the number of observations across countries and over time, both cross-sectionally and longitudinally.[4]

Based on a review of the literature and available datasets, this analysis focuses on financial flows recorded in the financial account of the balance of payments, using proprietary bank load data[5], the IFS IMF (2025a) and the Bank for International Settlements BIS (2025). The set of variables includes capital flows, FX flows, and banking flows, each separated into inflows and outflows.

Capital flow data originate from the IFS database, which reports quarterly figures on foreign direct investment (FDI), portfolio flows (debt and equity), and other investments (private, official, bank, and non-bank). The dataset provides both gross and net figures, expressed in nominal terms (millions of USD) and relative to quarterly GDP. The analysis prioritizes data expressed as a share of GDP to ensure cross-country comparability.

FX flow data originate from the BIS Data Portal (BIS (2025)), which publishes quarterly

[3]Our main objective is to identify the (ideally causal) relationship between a grey-listing event and the behavior of one or more cross-border financial flow indicators.

[4]Appendix A.1 provides a summary of the selected variables.

[5]Upon request, further details on the source of these data can be shared.

figures on currency exchange volumes and flows. The analysis uses both gross and net flows, normalized by GDP. These FX flows capture clearing transactions for currency exchanges reported by banks and other financial institutions. This dimension is particularly relevant, as correspondent banking channels—especially those involving U.S. dollars—often respond first to grey-listing events. However, the FX flow variable does not capture dollar transactions conducted through Money Services Businesses (MSBs) or cryptocurrency platforms (e.g., USDC, USDT, and others), which may also play a significant role in cross-border financial activity but remain outside the BIS reporting framework.

An important limitation of the FX measure is that it captures reported clearing transactions in formal banking channels, but not potential substitution toward money service businesses, informal transfer systems, or virtual-asset channels such as stablecoins. To the extent that grey-listing increases the cost of formal cross-border intermediation, affected agents may partially re-route transactions through these alternative circuits. Our estimates should therefore be interpreted as effects on observed formal financial flows, not as a complete measure of total cross-border financial activity. This displacement channel may attenuate the aggregate economic cost of grey-listing, while simultaneously shifting activity toward less observable and potentially less regulated channels.

In addition to the IFS and BIS sources, this analysis revisits a complementary approach based on bilateral transaction records from an interbank messaging system. Previous studies—such as Collin et al. (2016) and Cook and Soramaki (2014)—have used these type of data to examine global financial flows. Following a similar approach, this study analyzes quarterly bilateral cross-border financial transactions among 220 countries and jurisdictions for the period 2010–2017.[6]

A well-documented challenge in using banking data lies in choosing between two possible metrics: the number of transaction-initiating messages and the transaction value embedded in each message.[7] Although embedded values may not map one-for-one to actual bilateral financial flows, we validate their relevance by documenting how banking activity co-moves with broader macro indicators across 204 countries. In Figure 1, applying logarithmic transformations reveals a clear positive relationship between banking message activity and balance-of-payments flows; Panel 1a shows the association with exports and Panel 1b shows the association with FDI. These patterns confirm that these messages serve as a relevant

[6]To assess comparability across datasets, we re-estimate all specifications on the intersection of these panels: the 2010–2017 window used for the banking series and the 86-country set (45 ever grey-listed). The main qualitative findings are unchanged, although estimates are somewhat noisier due to the shorter sample. The corresponding results can be found in Appendix C.11.

[7]Appendix A.2 provides further details on the distinction between banking messages and their associated values, as well as background information on how the system operates.

proxy for cross-border banking activity in our analysis. There is meaningful regional and country heterogeneity—correlations are weaker in economies that rely heavily on non-bank remittance channels—so a perfect one-to-one mapping is not expected.[8] Beyond these correlations, the main value of these data is its dense coverage, which allows us to apply the same identification strategy used with our other datasets; this supports credible estimation of causal effects and improves precision around listing and delisting dates.

Finally, the assembled dataset covers 86 countries, including 45 jurisdictions that were formally grey-listed during the sample period.[9] Although the underlying time series span 2000–2024, data density is highest between 2005 and 2022, reflecting missing observations at the beginning and end of the sample window.

Because the IFS, BIS, and banking-based measures differ in coverage and reporting density, the effective estimation sample can vary across outcomes and specifications. To ensure that comparisons across flow measures are not mechanically driven by changes in sample composition, we report baseline results using each outcome's maximal available sample and complement them with a harmonized-sample robustness exercise that restricts estimation to a common set of country–quarter observations across data sources.

Control variables

As in all econometric analyses involving macroeconomic time series, selecting and structuring control variables plays a critical role. Previous sections outlined the criteria for choosing variables: economic relevance, cross-country comparability, and data density. In selecting control variables, it is also essential to account for their economic significance and their correlation with both dependent and independent variables.

This analysis combines insights from the relevant literature with statistical tools commonly associated with machine learning to guide the selection of controls. The remainder of this subsection first reviews the economic justification for each variable and its data source, followed by a description of the machine learning methodology used to refine the selection process.

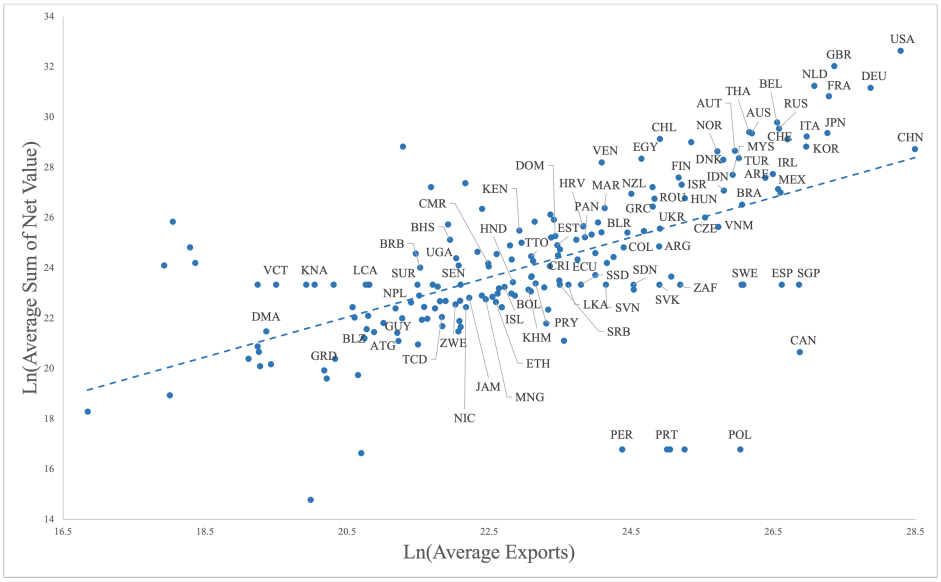
Empirical studies on foreign direct investment (FDI) and other cross-border financial flows routinely include a broad set of control variables to isolate the effects of specific shocks or policy interventions. These controls typically reflect macroeconomic fundamentals, in-

[8] Transfers are classified by nature and use: some fall under current transfers in the current account, others under capital transfers in the capital and financial account. Workers' remittances reflect current transfers between residents of different countries; migrants' transfers are capital-account changes during relocation. Consequently, bank transactions need not mirror remittance values in the balance of payments.

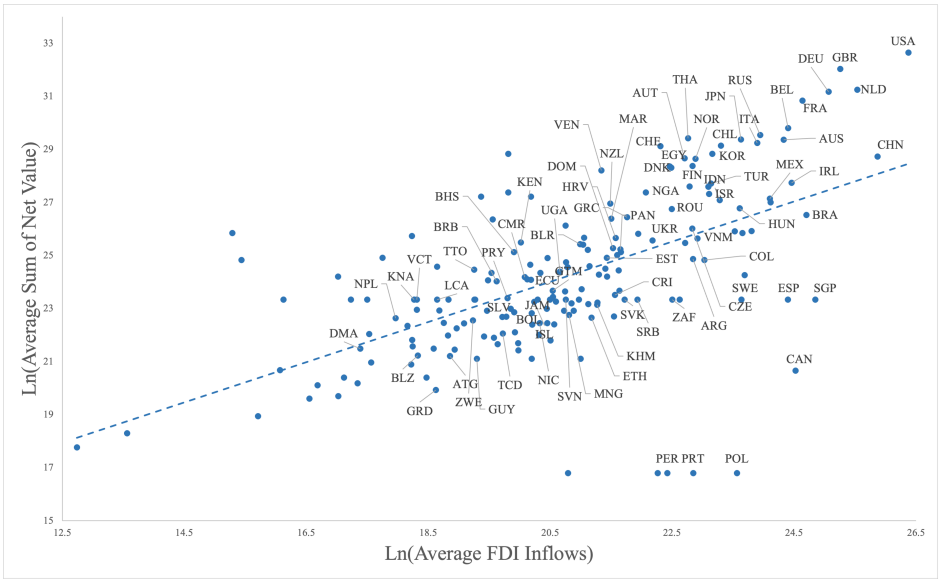
[9] The complete list of grey-listed countries is provided in Appendix B. Due to data availability constraints, only 45 of these jurisdictions can be included in the final estimations.

Figure 1: Correlation between BoP and Message Flows, 2010-2017

(a) Panel A. Exports and Message Flows



(b) Panel B. FDI and Message Flows



Source: Own calculations using banking messages and IFS data (IMF (2025a)).

stitutional quality, and exposure to global financial conditions. Macroeconomic indicators such as GDP levels or growth rates, inflation, real effective exchange rates, interest rates, and current account balances capture market size, macroeconomic stability, and competi-

tiveness—all of which influence investor behavior.[10] We leverage this literature to guide our selection of candidate control variables.

Variables capturing trade openness and integration into global markets—such as trade-to-GDP ratios or participation in trade agreements—serve as proxies for market accessibility and supply-chain dynamics (Helpman et al. (2004)). In particular, we include a measure of capital account openness and an indicator of exchange rate flexibility constructed by Ilzetzki et al. (2019).[11]

To capture the depth of domestic financial systems, we consider indicators such as domestic credit to the private sector and the size of capital markets (Alfaro et al. (2004)). In some cases, sovereign credit ratings are also used as proxies for financial development. Additionally, global push factors—including U.S. interest rates, commodity prices, and global risk sentiment (proxied by the VIX)—are included to reflect capital movements driven by external conditions (Forbes and Warnock (2012)).

Finally, we also take into consideration institutional variables identified in the literature as relevant for FDI decisions. Measures of regulatory quality, rule of law, and corruption matter because FDI depends not only on expected returns but also on political and legal risks (Globerman and Shapiro (2003)).

Defining the Event: What Does Grey List Actually Mean?

The FATF evaluation process is a multi-stage procedure involving a series of coordinated tasks carried out by both the review body and the country under evaluation. The objective of this discussion is to provide a comprehensive overview of the evaluation process that each country undergoes prior to the defining plenary session. It is during this plenary that a country may be placed on the grey list. However, in order to accurately identify and define the "event" for the purposes of econometric estimation, it is essential to analyze the full evaluation timeline in greater detail. This deeper understanding allows us to construct a more precise treatment variable and ensures that the estimation strategy captures the timing and potential impact of grey-listing more effectively.

The FATF Mutual Evaluation Process. Any review process is inherently lengthy, primarily due to the multiple milestones involved. Broadly speaking, the FATF mutual evaluation procedure unfolds in four main phases: (i) preparation for the on-site (in-situ)

[10] For example, Alfaro et al. (2004); Blonigen (2005) use these variables in studies of cross-border investment flows.

[11] Ilzetzki, Reinhart, and Rogoff classify exchange rate regimes into 15 categories according to their de facto flexibility. For a detailed explanation of the construction of this variable, see Ilzetzki et al. (2019). We present the categories in Appendix A.3.

visit, (ii) the in-situ visit itself, (iii) preparation of the Mutual Evaluation Report (MER) by the evaluation team, and (iv) the plenary discussion. At least six months before the in-situ visit—ideally earlier—the FATF Secretariat sets the timeline for the entire process in coordination with the country under review, including the date of the visit and key deadlines. During this period, the country must demonstrate that it meets FATF standards and that its Anti-Money Laundering and Combating the Financing of Terrorism (AML/CFT) regime is effective. To this end, the country is expected to provide all relevant information to the evaluation team. Evaluators must be able to request and access documents, data, or other materials they deem necessary for their assessment.

To better understand the timing and scope of each stage, it is necessary to begin with the designation of the evaluation team. The FATF Secretariat confirms the evaluation team, typically at least four months prior to the in-situ visit, coordinating with member countries that have volunteered to serve as evaluators. The team generally consists of five experts, including at least one legal, financial, and law enforcement specialist. Team selection is based on several criteria: (i) operational relevance and prior evaluation experience; (ii) the language of the jurisdiction under review; (iii) the nature of the legal and institutional system (civil law or common law); and (iv) country-specific characteristics such as the size and structure of the economy and financial sector, geographic context, and cultural or commercial ties. These factors help ensure that the team has the appropriate balance of expertise.

The in-situ visit represents the most critical opportunity to clarify questions concerning the country's AML/CFT system. Evaluators are expected to come fully prepared to assess immediate outcomes related to system effectiveness and to address any outstanding technical compliance issues. A standard in-situ visit typically spans about ten business days, although this may be extended in the case of large or complex jurisdictions. Following the visit, there is a mandatory minimum of 27 weeks before the Mutual Evaluation Report (MER) is discussed in plenary. During this period, the evaluation team works closely with the country to finalize the report. Both parties are granted three weeks to review the comments provided by external reviewers, address outstanding issues, and identify key matters for plenary discussion. Prior to the plenary, a dedicated meeting between the evaluation team and the country is held to resolve disagreements regarding technical compliance or effectiveness ratings and to agree on which issues should be brought forward.

It is important to note that the MER is not simply a product of the evaluation team; rather, it is an FATF report. Accordingly, the plenary holds final authority over the content and wording of the report. While the plenary takes into account the views of both the evaluation team and the country, it is ultimately responsible for ensuring the report adheres to FATF standards and maintains consistency across all evaluations.

The FATF maintains and publishes a list of jurisdictions with significant strategic deficiencies in their AML/CFT frameworks. These public listings appear three times per year—in February, June, and October. The FATF defines: the "grey list" refers to jurisdictions subject to increased monitoring, meaning they are actively working with FATF or a regional body to improve their systems and implement recommended reforms. The "black list," in contrast, includes high-risk jurisdictions that have made insufficient progress or shown a lack of political will to address their deficiencies.

For the purposes of this study, we distinguish between the onset of information/anticipation and the activation of a discrete compliance regime. The end of the in-situ visit is a natural lower bound for information leakage because it is the first point at which domestic authorities have a relatively complete signal about likely weaknesses and remediation demands; in some jurisdictions, expectations may form even earlier when deficiencies are widely recognized from prior monitoring or public enforcement episodes. As a result, some adjustment in cross-border flows can begin during the post-visit phase—as the MER is drafted, reviewed, and negotiated ahead of the plenary—well before the official statement is published.

In the econometric implementation, we therefore allow pre-publication quarters to be part of the treatment window in the ML panel estimators: the event indicators are defined over short symmetric windows around the official listing and delisting statement quarters, so the resulting window-average effects are intentionally permitted to reflect anticipation as well as early post-publication adjustment. By contrast, when the goal is to isolate the incremental activation component, the RDiT design uses the official publication quarter as the cutoff and interprets any earlier movements as part of the smooth pre-cutoff evolution; we then benchmark the publication-timed discontinuity against analogous discontinuities aligned to earlier public announcement dates (reported later in the RDiT diagnostics) to gauge how much of the response is already underway before formal listing.

To better understand the extent of anticipation, we investigate whether there is statistical evidence of smoothing effects around the listing date. While it is crucial to analyze the negative effects on financial flows, policymakers must also account for market expectations during the period leading up to the country's exit from the list and the subsequent recovery. In general, countries that are grey- or black-listed, or receive a poor Mutual Evaluation Report (MER), are required to undertake substantial reforms to improve their AML/CFT frameworks. As these countries communicate their progress, markets tend to internalize this information, implying that the negative impacts may be temporary. Some countries may experience a slow or incomplete recovery—an effect commonly referred to as the "stigma effect"—while others may rebound more quickly and even surpass pre-listing flow levels.

Why the Listing Date Still Matters. Importantly, grey-listing is not a purely informational event. The official publication of the FATF grey list constitutes a discrete regulatory threshold that activates binding compliance and risk-management protocols within global financial institutions. In practice, many banks, correspondent institutions, and payment service providers rely on formal country-risk taxonomies embedded in automated compliance systems.[12] These taxonomies are typically updated only upon the release of the official FATF statement, triggering changes in enhanced due diligence requirements, transaction monitoring thresholds, internal risk limits, or the suspension of correspondent banking relationships.

As a result, announcement effects primarily capture expectation formation by forward-looking agents, while the listing date marks the activation of operational and regulatory frictions that directly affect the execution of cross-border payments and financing. The presence of a residual discontinuity at the listing date is therefore consistent with rational anticipation under discrete compliance regimes rather than frictionless adjustment. In this sense, the RDiT estimates at the listing cutoff should be interpreted as lower-bound, policy-relevant effects associated with the formal transition into FATF increased monitoring.

Defining the Event for this Paper. The empirical objective of this paper is to isolate the effect of formal FATF grey-list publication on cross-border financial flows, over and above information that may already be priced in during the mutual evaluation process. Because confidentiality rules and market learning imply that some adjustment can occur before the official statement, we do not treat grey-listing as an unexpected shock. Instead, we design our main panel estimands to capture average flow changes in a short window around the formal decision date, and we complement these with a local discontinuity design that isolates any remaining jump exactly at publication.

Intermediate mutual-evaluation milestones (e.g., draft MER circulation, reviewer comment windows, or the precise timing of plenary deliberations) are not consistently observed at quarterly frequency across jurisdictions in our cross-country sample, so the systematic timing comparisons we implement focus on standardized public statement dates (publication and announcement) while allowing the panel event window to capture any leakage that begins in the post–in-situ phase.

Let T_i^L denote the quarter in which country i is formally added to the FATF grey list (official publication), and let T_i^D denote the quarter of formal delisting. For the panel

[12] Straight-Through Processing (STP) refers to payment transactions that are processed entirely electronically without manual intervention. An "STP failure" or "repair" occurs when a bank's automated screening software flags a transaction—typically due to a match against updated sanctions or high-risk country lists—and diverts it to a manual queue for Enhanced Due Diligence (EDD). This shift from automated to manual processing dramatically increases transaction costs and delays, effectively acting as a non-tariff barrier to capital flows.

estimators, we define the event indicator as a symmetric window around the relevant formal decision date:

$$z_{i,t}^{E,k} = \begin{cases} 1 & \text{if } t \in [T_i^E - k,\ T_i^E + k], \\ 0 & \text{otherwise,} \end{cases} \qquad \text{for } E \in \{L, D\} \text{ and } k \in \{1, 2, 3\}. \tag{2}$$

That is, $z_{i,t}^{L,k}$ flags quarters within k quarters of the listing publication date, and $z_{i,t}^{D,k}$ flags quarters within k quarters of the delisting publication date. We vary k between 1 and 3 quarters to assess how quickly flows adjust around each formal decision. Throughout the paper, outcomes are expressed in quarterly percent of GDP, so the coefficients can be read as economically interpretable changes in flow intensity.

For countries that appear on the grey list more than once, we treat each listing–delisting cycle as a distinct event. If country i experiences event $s \in \{1, \ldots, S_i\}$, the relevant cutoffs are the event-specific publication dates (T_{is}^L, T_{is}^D), and the event indicator in equation 2 is evaluated relative to the corresponding event. Thus, re-listed countries contribute multiple short event windows over the full sample rather than a single permanent treatment episode. This coding choice matches the institutional setting: re-listing reflects a renewed entry into FATF increased monitoring, not a continuation of the earlier event.

We therefore do not attempt to code the full mutual-evaluation process as treatment. A multi-year pre-listing treatment would combine heterogeneous and only partially observed milestones across jurisdictions, blurring the distinction between information accumulation and the formal activation of compliance frictions. Our identifying strategy is narrower: the ML estimators are intentionally allowed to absorb short-horizon anticipation through symmetric windows around publication, while the RDiT design isolates the residual publication-date jump that remains once earlier information has already been incorporated into flows.

Causal object and mapping across empirical strategies. The coefficients on $z_{i,t}^{E,k}$ identify average deviations in flows within a short event window around the formal decision, relative to the counterfactual path implied by country fixed effects, time fixed effects, and a rich set of macro-financial controls. Because the window includes quarters just before publication, these panel estimands are intentionally allowed to reflect anticipation as well as immediate post-publication adjustment. They answer the question: how much do flows shift in the quarters surrounding the formal listing, conditional on observed confounders?

Our Regression Discontinuity in Time (RDiT) design targets a different, narrower object: the local activation effect at publication. Under the assumption that, absent listing, expected flows evolve smoothly through T_i^L (and similarly for T_i^D), the RDiT estimand is

the discrete jump in flows at the cutoff. We estimate separately the listing and delisting cutoffs. Comparing the panel window-average ML estimates to the RDiT discontinuity is informative about timing. If formal publication mainly activates compliance constraints, then the RDiT estimates should be sizable and close to the window-average ML effects. If anticipation plays an important role, then the window-average effects can be larger in magnitude than the RDiT jump, because part of the adjustment occurs before the formal publication, muting the observed discontinuity. Finally, if persistence is important, then delisting effects should only partially offset listing effects, consistent with slow rebuilding of correspondent relationships and other fixed costs. In this sense, "incremental effect" refers to a residual discontinuity at publication after allowing for anticipation in the pre-publication portion of the event window and in announcement-aligned cutoff checks, rather than an ex post attribution to a single milestone.

In the next section, we operationalize these estimands using (i) ML panel estimators that differ in how they address high-dimensional confounding but share the same event-window target, and (ii) an RDiT design that isolates local discontinuities at the formal listing and delisting thresholds.

4 Empirical Strategy and Estimation Results

We organize the empirical evidence in three steps. We first present baseline panel estimates using Double Selection Lasso (DSL), then show how Double Debiased Machine Learning (DDML) strengthens the results under weaker assumptions, and finally use a Regression Discontinuity in Time (RDiT) design to isolate the publication-date discontinuity at listing and delisting.

We start from the reduced-form specification

$$y_{i,t} = \beta_0 + \delta_0 z_{i,t} + X'_{i,t}\beta_1 + W'_{i,t}\beta_2 + \epsilon_{i,t}, \tag{2}$$

where $y_{i,t}$ denotes a financial flow, $z_{i,t}$ is the event-window indicator defined in Section 3, and $X_{i,t}$ and $W_{i,t}$ collect domestic and external controls, respectively.

Given the event-window indicator $z_{i,t}$ defined in Section 3, we interpret δ_0 in (2) as a medium-run, window-average effect of grey-listing on financial flows. Let $t_0(i)$ denote the first grey-listing quarter for country i and let the symmetric event window be $\{t : t_0(i) - k \leq t \leq t_0(i) + k\}$. Abstracting from fixed effects and controls, the target object can be written as

$$\delta_0 \;\approx\; \mathbb{E}[\, y_{i,t}(1) - y_{i,t}(0) \,|\, i \text{ grey-listed},\; t_0(i) - k \le t \le t_0(i) + k \,], \tag{3}$$

where $y_{i,t}(1)$ and $y_{i,t}(0)$ denote potential outcomes for financial flows with and without grey-listing, respectively. In words, δ_0 summarizes the average deviation of flows from their counterfactual path in a symmetric window around the listing date, conditional on country and time fixed effects and a high-dimensional set of macro-financial controls selected by the machine-learning procedures described below.

Our objective is to estimate whether exposure to grey-listing, captured by $z_{i,t}$, has a causal effect—positive or negative—on cross-border financial flows. Event-study approaches, including Kida and Paetzold (2021), provide useful descriptive evidence on how flows evolve around listing episodes. However, event-time patterns alone do not establish causality: grey-listing typically coincides with changes in global financial conditions and domestic fundamentals that also affect capital movements. Without adequately controlling for these confounders, estimated pre/post differences may conflate the impact of listing with contemporaneous macro-financial shocks, leading to biased attribution.

To strengthen causal inference, we explicitly control for macro-financial confounders that jointly influence grey-listing exposure and cross-border flows. This is challenging in practice because the set of plausible controls is large, many of them are highly correlated, and their predictive content may be nonlinear. We therefore draw on recent econometric work at the intersection of machine learning and causal inference that delivers valid treatment-effect estimation in high-dimensional settings.

Following Belloni et al. (2012), Belloni et al. (2014), and Belloni et al. (2016), we implement the Double Selection Lasso (DSL) estimator. DSL applies Lasso-based variable selection to both the outcome and treatment equations and then estimates the treatment effect using the union of selected controls. This procedure provides valid inference on δ_0 while limiting sensitivity to ad hoc control choice, allowing the data to identify which domestic and external variables are most relevant confounders for grey-listing and financial flows.

A key advantage of DSL, emphasized by Belloni et al. (2014), is that it extends naturally to panel settings with fixed effects. Unlike most supervised machine learning methods that prioritize prediction over inference (Tiffin (2019)), DSL is designed to deliver unbiased coefficients and standard errors for causal interpretation. The method remains robust to moderate model selection mistakes and does not rely on restrictive "beta-min" conditions. Its validity requires that approximate sparsity holds—that is, the confounding structure can be well approximated by a sparse linear combination of covariates. We treat the DSL estimates as our baseline.

We then relax these assumptions by implementing Double Debiased Machine Learning

(DDML), a generalization of the partially linear framework developed by Chernozhukov et al. (2018). DDML allows for a broader class of machine learners beyond Lasso (e.g., random forests, boosting, and neural networks), combining cross-fitting and orthogonalized moment conditions to preserve valid inference under weaker functional-form restrictions. In our application, DDML serves as a robustness check that the estimated grey-listing effects do not hinge on the specific choice of Lasso as the nuisance learner or on a strictly sparse representation of confounding.

Double Selection Lasso as the Machine Learner

Selecting an appropriate control set is nontrivial in this context. Macro-financial variables exhibit strong contemporaneous and lagged correlations, and standard transformations—lags, differences, and nonlinear terms—quickly generate a high-dimensional set of candidate predictors. Conventional approaches typically impose a parsimonious linear specification and rely on a combination of information criteria and expert judgment to choose among competing control sets, which can leave results sensitive to subjective specification choices.

This challenge is particularly salient in international macroeconomics, where cross-border flows respond to a wide range of domestic and global factors—including global risk sentiment (VIX), commodity prices, sovereign risk indicators, and domestic cyclical conditions—that are often jointly determined and highly collinear. In such settings, ad hoc control selection can generate omitted-variable bias and, more importantly, introduces scope for specification search. To reduce this discretion, we implement Double Selection Lasso (DSL) as a formal, data-driven procedure that selects controls based on their predictive content for both the treatment (grey-listing exposure) and the outcome (financial flows). The resulting estimation set is therefore anchored in observed relationships in the data rather than in a single researcher-chosen specification.

Formally, we follow Belloni et al. (2012) and Belloni et al. (2014). Lasso augments linear regression with an ℓ_1 penalty that shrinks many coefficients toward zero, yielding a sparse set of selected predictors. DSL applies this selection step to both the outcome and treatment equations and then estimates the treatment effect using the union of selected controls, delivering valid inference under approximate sparsity.[13] The procedure is computationally efficient and extends naturally to panel settings with fixed effects Belloni et al. (2016).

Estimation Procedure. We implement Double Selection Lasso (DSL) to obtain valid inference on the grey-listing coefficient δ_0 in the presence of a high-dimensional control set.

[13] Approximate sparsity means that, although the number of candidate controls may be large, the confounding structure can be well approximated by a relatively small subset of predictors.

Starting from equation (2), DSL proceeds in three steps:

1. **Outcome selection.** Regress the outcome on the full set of candidate controls, excluding the treatment indicator:

$$y_{i,t} = \beta_0 + X'_{i,t}\beta_1 + W'_{i,t}\beta_2 + \nu_{i,t}, \tag{4}$$

   and use Lasso to select the subset of controls with nonzero coefficients. Denote the selected set by $\widehat{S}_y$.

2. **Treatment selection.** Regress the treatment indicator on the same candidate control set:

$$z_{i,t} = X'_{i,t}\pi_1 + W'_{i,t}\pi_2 + \varepsilon_{i,t}, \tag{5}$$

   and use Lasso to select the subset of controls with nonzero coefficients. Denote the selected set by $\widehat{S}_z$.

3. **Post-selection estimation.** Estimate the treatment effect by OLS including the treatment indicator and the *union* of controls selected in the first two steps:

$$y_{i,t} = \alpha_i + \theta_t + \delta_0 z_{i,t} + \Gamma'_{i,t}\eta + u_{i,t}, \tag{6}$$

   where $\Gamma_{i,t}$ collects all controls in $\widehat{S} = \widehat{S}_y \cup \widehat{S}_z$.[14]

Two features of this design are central. First, the Lasso steps in (4) and (5) are predictive by construction: they select controls that best explain variation in the outcome and in treatment assignment, respectively, and are not themselves meant to carry a causal interpretation. Second, the causal content arises in the post-selection regression (6). By including the union $\widehat{S}_y \cup \widehat{S}_z$, DSL guards against selection mistakes that would occur if controls were chosen using only one of the two predictive relationships. Intuitively, variables excluded from $\widehat{S}$ are those that are weakly related to both $y_{i,t}$ and $z_{i,t}$, limiting the scope for omitted-variable bias. Under approximate sparsity, this procedure yields valid inference on δ_0 and can be viewed as a high-dimensional analog of partialling-out in the partially linear model Robinson (1988).

Estimations After DSL. After selecting the relevant set of controls and defining the grey-listing event indicator, we estimate the following two-way fixed-effects specification:

$$y_{i,t} = \alpha_i + \theta_t + \delta_0 z_{i,t} + X'_{i,t}\beta_1 + W'_{i,t}\beta_2 + \epsilon_{i,t}, \tag{7}$$

[14] We also report robustness checks using standard transformations of the candidate controls (e.g., levels, differences, and lags).

where $y_{i,t}$ denotes a financial flow for country i at time t (expressed as a share of GDP), α_i and θ_t denote country and quarter fixed effects, and $z_{i,t}$ is the event-window indicator around the formal grey-listing quarter defined in Section 3. The vectors $(X_{i,t}, W_{i,t})$ collect domestic and external controls, respectively, but the estimation restricts attention to the subset selected by the DSL procedure.[15]

By construction, the country and time fixed effects absorb time-invariant country characteristics and global (or region-wide) shocks common to all jurisdictions. The remaining identifying variation therefore comes from within-country, within-quarter deviations relative to the global environment. Under the standard DSL conditions, the resulting estimate of δ_0 admits a causal interpretation as long as time-varying, country-specific confounders correlated with both grey-listing and financial flows can be summarized by a relatively small number of observed characteristics selected from the high-dimensional candidate set. In this sense, DSL provides a principled way to structure control selection when the set of potential confounders is large and highly correlated.

All DSL estimation outputs are reported in Appendix C. We present results sequentially by flow category, starting with banking flows, then balance-of-payments capital flows, and finally foreign-exchange (FX) flows. Across outcomes, specifications follow an incremental structure in which additional macro-financial and institutional controls enter progressively, allowing us to assess robustness and sensitivity.

Banking flows. As a first case, we estimate equation (7) using banking inflows and outflows as dependent variables as a share of GDP.[16] For the Lasso selection stage, we begin from the full candidate set and retain the controls assigned non-negligible weights, reported in Table 9 in Appendix C.1.[17] Using the data sources described in Table 6 in Appendix A.1, we apply standard transformations common in macro-finance applications, including logs, lags, and squares. The reported Lasso weights therefore summarize a multistep selection procedure that incorporates these feature expansions.[18]

Tables 15 and 16 in Appendix C.3 report the baseline DSL estimates for banking inflows and outflows. The specifications follow an incremental structure, where each column introduces additional macro-financial and institutional controls to assess the sensitivity of the grey-listing coefficient.

For inflows, the coefficient on the grey-list dummy is negative across specifications and

[15] Equivalently, the controls included in equation (7) correspond to the union of variables selected from the outcome and treatment selection steps, $\widehat{S} = \widehat{S}_y \cup \widehat{S}_z$, as described in the DSL estimation procedure.

[16] We proxied by interbank transaction values.

[17] Table 9 in Appendix C.1 reports variables with nonzero weights up to the second decimal place. A broader set of variables receive weights that are close to zero and are therefore effectively excluded from the post-selection regression.

[18] A further explanation of the penalty choice and the selection procedure is provided in Appendix D.

becomes statistically significant once core macro-financial controls enter. In the baseline model (column 1), which includes only country and time fixed effects, the estimate is negative but statistically insignificant—a pattern consistent with earlier studies (e.g., Balakina et al., 2017; Collin et al., 2016) that find weak baseline effects absent controls. Once global push factors are introduced, the coefficient gains precision and increases in magnitude. In particular, U.S. financial conditions—including the short-term rate and the ten-year rate (columns 3–5)—enter significantly and reinforce the robustness of the main result. Across these specifications, the estimated effect of grey-listing ranges between -0.6% and -1.9% of quarterly GDP, with significance typically at the 5 percent level (e.g., columns 4, 5, and 9–15).

From column 7 onward, adding further controls—including credit-rating outlooks, reserves, GDP growth, and capital account openness—does not overturn the core result, though many of these variables contribute limited incremental explanatory power once fixed effects and global push factors are already accounted for. In the most comprehensive specifications (columns 13–15), the estimated effect stabilizes between -2.1% and -2.3% of quarterly GDP and remains significant at conventional levels. Overall, the inflow results imply that grey-listing is associated with a sizable contraction in cross-border banking inflows.

The behavior of the selected controls aligns with standard macro-finance priors. Higher U.S. short-term rates exert a strong negative influence on banking inflows (columns 3–9), consistent with tighter global financial conditions dampening cross-border intermediation, and higher U.S. long-term rates also reduce inflows (columns 5–10).[19] Credit-rating outlooks are strongly significant in several specifications, suggesting that sovereign-risk perceptions amplify the response to grey-listing. Other controls (e.g., exchange-rate flexibility, reserves, and GDP growth) often display expected signs but are rarely statistically significant once fixed effects are included.

Overall, the inflow regressions yield key insights. First, grey-listing consistently exerts a negative effect on banking inflows. Second, the magnitude of this effect increases when institutional and financial conditions are taken into account, suggesting that compliance-driven de-risking and regulatory risk-management responses interact with country risk factors, reinforcing the idea that global and domestic conditions jointly shape the extent of grey-listing impacts.

Capital flows (balance of payments). Tables 17 and 18 in Appendix C.4 replicate the DSL

[19]The prominence of U.S. interest rates among selected controls reflects their role as global push factors. Importantly, the grey-listing coefficient is identified from within-period variation conditional on time fixed effects and the selected controls; it does not capture global monetary tightening per se, but rather differential breaks in financial flows around the listing threshold within the same global environment. This interpretation applies to both DSL and DDML.

framework using balance-of-payments capital inflows and outflows as dependent variables. For inflows, the coefficient on the grey-list dummy is negative throughout and becomes increasingly significant as controls enter. In the baseline model (column 1), the estimate is negative but imprecise; once U.S. financial conditions are included (columns 2–3), the magnitude rises to approximately -0.5% to -0.6% of quarterly GDP. Adding the real exchange rate (column 4) sharpens the estimate to roughly -1.5%, and the inclusion of credit ratings, capital account openness, and outlook variables strengthens the results further. In the fully specified models (columns 7–9), capital inflows fall by -2.3% to -2.6% of quarterly GDP, with significance at conventional levels.

For outflows (Table 18), the evidence points in the opposite direction. While the baseline specification yields a small and insignificant coefficient, the effect grows and becomes statistically significant once macro-financial controls are included. From columns 3–7, the coefficient reaches roughly $+1.8\%$ of quarterly GDP, consistent with capital flight or deleveraging dynamics operating alongside reduced inflows.

The selected controls behave in a manner consistent with standard push–pull mechanisms, providing an internal validity check on the DSL estimates. For capital inflows, nonlinear terms in U.S. short-term rates and the federal funds rate enter significantly in multiple specifications, reflecting the sensitivity of inflows to global monetary conditions, while the real exchange rate is positive and significant once included. Sovereign-risk measures—captured by credit ratings and outlook indicators—also emerge as economically relevant, underscoring that country-risk perceptions interact with the grey-listing shock. For capital outflows, U.S. short-term rates enter positively and with high significance across specifications, consistent with flight-to-safety and balance-sheet adjustment dynamics; weaker credit profiles also predict larger outflows. Measures of capital account openness become more salient in later specifications, consistent with more open systems exhibiting greater scope for outward reallocation during periods of heightened regulatory risk.

Together, the balance-of-payments results highlight a second asymmetric pattern: grey-listing reduces capital inflows while simultaneously increasing outflows. This dual channel magnifies the net financial cost of grey-listing, consistent with precautionary portfolio reallocation in response to heightened regulatory risk operating alongside compliance-driven de-risking that restricts new inflows.

FX flows. Finally, Tables 19 and 20 in Appendix C.5 report the corresponding estimates for quarterly FX inflows and outflows. For FX inflows (Table 19), the results show considerable variation across specifications. In the baseline models (columns 1–3), the estimated coefficients are large in magnitude but display low statistical significance, with signs that alternate depending on the set of controls. Once the real exchange rate and capital account

openness are included (columns 4–6), the coefficient on grey-listing becomes negative and highly significant, with estimates between −2.4% and −2.9% of GDP. This effect weakens in magnitude but remains negative and statistically significant in the more restrictive samples (columns 7–9), converging to a smaller yet robust contraction of about −0.5% of GDP in the final specification. Importantly, significance tends to appear in the smaller-sample estimations, where the data cover a larger number of countries with available FX information but include fewer overlapping macro-financial controls. This pattern suggests that the robustness of the FX inflow effect partly reflects data availability and variable coverage, rather than a uniformly stable effect across all samples.

The behavior of the control variables supports this interpretation. Greater exchange rate flexibility consistently correlates with higher inflows and is highly significant across specifications. Nonlinear effects of capital account openness also enter positively, particularly in earlier columns, confirming the role of financial liberalization in attracting FX inflows. Credit ratings show mixed results: short-term ratings become significant only in the final specification, while outlook variables remain mostly insignificant. The nominal exchange rate appears in the final model with a positive and highly significant coefficient, highlighting the importance of exchange rate conditions in shaping FX transactions.

For FX outflows (Table 20), the results differ. In the baseline specification (column 1), the grey list dummy is negative and statistically significant, suggesting a modest contraction in outflows. However, once controls are added (columns 2–16), the coefficient becomes small, statistically insignificant, and remains close to zero across specifications. This pattern indicates that grey-listing does not systematically increase FX outflows, in contrast with the behavior observed for balance of payments capital outflows.

Overall, the FX estimations suggest that grey-listing mainly reduces inflows rather than increasing outflows. The inflow contraction is robust but varies in magnitude depending on the specification and sample coverage, while outflows remain largely unaffected. This asymmetric pattern reinforces the interpretation that grey-listing operates primarily through compliance-driven de-risking and regulatory frictions, rather than by directly triggering immediate capital flight in the FX market.

To align with common practice in the literature, Table 58 in Appendinx E reports the main quarterly estimates alongside their GDP-weighted annualized counterparts for ease of comparison. Appendix E details the conversion procedure. In annual terms, banking and capital inflows contract by about −0.6% to −0.8% of annual GDP, while FX inflows decline modestly—around −0.1%. Outflows are statistically insignificant for banking and FX, but capital outflows rise by roughly +0.4%. Although these annual figures may appear small in absolute terms, they represent economically meaningful reallocations when scaled by annual

GDP. The total annual impact of being grey-listed sits at around -1.1% of GDP.

Additionally, to assess comparability across datasets, we re-estimate all specifications on the intersection of all panels: the 2010–2017 window used for the banking series and the 86-country set (of which 45 have been grey-listed). The main qualitative findings are unchanged, although estimates are somewhat noisier due to the shorter sample. The corresponding results can be found in Table 55 in Appendix C.11.

Double Debiased Machine Learner

The two-step Lasso strategy provides a disciplined approach to variable selection and delivers robust results under approximate sparsity. Yet it also has limitations. When the sparsity assumption weakens, or when researchers wish to exploit richer machine learning tools, Lasso may not be the best option. In addition, Lasso requires deliberate feature engineering to capture nonlinearities and interaction effects.

The impact of grey-listing on financial flows illustrates these challenges. Multiple interconnected channels may transmit the effect of grey-listing, including reputation, investor sentiment, correspondent banking, macroeconomic fundamentals, and regulatory perceptions. Given this complexity, a strictly sparse set of predictors cannot fully explain variation in financial flows. Macro-financial variables such as exchange rates, capital account measures, and interest rates often move together, creating correlations that undermine the sparsity assumption.

Lasso's regularization can still highlight a core set of relevant predictors when sparsity holds only approximately. Because grey-listing is a discrete event, short-run effects may concentrate along a small number of channels, supporting a sparse representation. To move beyond correlations and approximate causal effects, we complement the Lasso-based strategy with the Double Debiased Machine Learning (DDML) framework of Chernozhukov et al. (2018), designed to reduce selection bias and deliver valid inference in high-dimensional settings.

This approach introduces two innovations. First, it applies *cross-fitting*, which partitions the sample into folds and rotates them across training and estimation, reducing overfitting and ensuring independence between nuisance estimates and the target parameter. Second, it uses *Neyman-orthogonal scores*, which construct moment conditions robust to small misspecifications of the nuisance functions, making the final estimator less sensitive to errors in the first stage.

Estimation Procedure. Revisiting equation (7), we adapt it into a partially linear model:

$$y_{i,t} = \beta_0 + \delta_0 z_{i,t} + g_0(X_{i,t}, W_{i,t}) + \epsilon_{i,t}, \tag{8}$$

where $z_{i,t} = z_{i,t}^{E,k}$ is the event-window indicator around the formal listing date defined in Section 3, and $g_0(X_{i,t}, W_{i,t})$ is an unknown function of controls estimated non-parametrically. The parameter of interest, δ_0, is the same window-average effect defined in equation (1): it captures the average deviation of $y_{i,t}$ from its counterfactual path in the event window, after flexibly controlling for $(X_{i,t}, W_{i,t})$.

The corresponding Neyman-orthogonal score is:

$$\psi(W_i; \theta, g, m) = \big(y_{i,t} - g(X_{i,t}, W_{i,t}) - \delta\big(z_{i,t} - m(X_{i,t}, W_{i,t})\big)\big)\,\big(z_{i,t} - m(X_{i,t}, W_{i,t})\big), \tag{9}$$

where $W_i = (y_{i,t}, z_{i,t}, X_{i,t}, W_{i,t})$, and $g(\cdot)$ and $m(\cdot)$ denote nuisance functions for the outcome and treatment equations. The estimator $\hat{\delta}_0$ solves the moment condition:

$$\frac{1}{n}\sum_{i=1}^{n} \psi(W_i; \hat{\theta}, \hat{g}, \hat{m}) = 0. \tag{10}$$

We estimate the nuisance functions

$$g(X_{i,t}, W_{i,t}) \approx \mathbb{E}[y_{i,t}|X_{i,t}, W_{i,t}], \quad m(X_{i,t}, W_{i,t}) \approx \mathbb{E}[z_{i,t}|X_{i,t}, W_{i,t}], \tag{11}$$

using random forests, which flexibly capture nonlinearities and interactions with minimal tuning. To maintain orthogonality, we apply cross-fitting: the data are clustered by country and partitioned into K folds ($K = 5$, with consistent results for $K = 10$). For each fold k, we estimate $\hat{g}_{-k}$ and $\hat{m}_{-k}$ on the remaining folds and use these models to construct residualized outcomes and treatments:

$$\tilde{y}_i = y_{i,t} - \hat{g}_{-k}(X_{i,t}, W_{i,t}), \quad \tilde{z}_i = z_{i,t} - \hat{m}_{-k}(X_{i,t}, W_{i,t}). \tag{12}$$

Stacking residuals across all folds, we compute the final estimator with ordinary least squares (OLS):

$$\hat{\delta}_0 = \frac{\sum_{i=1}^{n} \tilde{z}_i \tilde{y}_i}{\sum_{i=1}^{n} \tilde{z}_i^2}, \tag{13}$$

and cluster standard errors at the country level.

Estimation Results. We estimate equation 8 for each type of financial flow. Similar to DSL, we report in Tables 12-14 in Appendix C.2 the most important variables for each regression, according to the model, with their respective importance scores.[20] Table 1 presents the estimation results.

Table 1: DDML OLS Estimates of the Effect of Grey-listing by Type of Financial Flow (percent of GDP)

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Grey list dummy	-1.753**	1.145*	-2.271**	1.779**	-0.942	1.123
Std. Err.	(0.314)	(0.425)	(0.188)	(0.247)	(3.379)	(3.324)
R-squared	0.1134	0.0921	0.1302	0.1278	0.0875	0.0878
N	5598	5540	12846	12767	4712	5484

Note: The table presents estimates of the effect of grey-listing on quarterly financial flows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 1 confirms the asymmetric impact of grey-listing. For inflows, the effect is negative and significant for both banking inflows (-1.8% of GDP) and capital inflows (-2.3% of GDP). FX inflows also decline modestly (-0.9%), though without significance, likely due to smaller sample coverage and fewer available controls. For outflows, the pattern reverses: banking and capital outflows increase significantly ($+1.1\%$ and $+1.8\%$ of GDP), while FX outflows rise but without significance. These estimates suggest that grey-listing reduces inflows and amplifies outflows, reinforcing the interpretation of a stigma effect where international investors react by pulling funds.

The relatively low R^2 values in Table 1 are not unusual in quarterly macro-financial panels with country and time fixed effects, where a large share of high-frequency flow variation is idiosyncratic and difficult to predict. In the DDML framework, overall fit is not the object of interest: identification comes from orthogonalizing the grey-listing indicator with respect to a rich set of controls, not from maximizing in-sample prediction of the raw outcome. What matters substantively is that the inflow effects are stable across DSL, DDML, and RDiT despite very different nuisance-function approximations. Moreover, because the decline in inflows is larger in absolute value than the rise in outflows, the gross-flow asymmetry still implies a deterioration in the net private capital balance rather than a near-zero capital-account response.

Once again, Table 58 in Appendix E reports the main quarterly estimates alongside their GDP-weighted annualized counterparts. In annual terms, banking and capital inflows

[20] Tables 12-14 in Appendix C.2 display only those variables with nonzero weights up to the second decimal place. However, unlike Lasso, this model considers the entire set of variables for the final estimation, even those with importance scores close to zero.

contract around −0.6% of annual GDP, while their respective outflows rise by roughly +0.4%, giving a total annual impact of around −1.0% of GDP.

We further probe external validity by examining which country types drive the aggregate results. Appendix C.12 reports the corresponding DDML interaction estimates and split-sample comparisons by development status, trade openness, and capital account openness. The pooled interaction specifications retain the full estimation sample, while split-sample estimates are reported for ease of interpretation. Three patterns stand out. First, the contraction in inflows is materially larger in EMDEs than in advanced economies: for example, banking inflows fall by −2.087 percent of quarterly GDP in the EMDE subsample, compared with −0.903 in advanced economies, while capital inflows fall by −2.464 versus −1.160. Second, the same qualitative pattern appears when countries are split by external exposure. In the high-trade-openness subsample, banking and capital inflows decline by −2.052 and −2.557, compared with −1.454 and −2.059 in the low-trade-openness subsample. Similarly, in the high-capital-account-openness subsample, banking and capital inflows decline by −1.962 and −2.493, compared with −1.431 and −1.931 in the low-openness subsample. Third, outflow responses remain smaller and less stable across groups, while the FX results are comparatively noisy. Taken together, these heterogeneity patterns suggest that the aggregate estimates are not being driven solely by a handful of very small financial centers; rather, they are strongest in countries for which correspondent-banking frictions, external funding reliance, and compliance-related intermediation costs are likely to be economically most relevant.

In summary, the DDML results strengthen three conclusions. First, the negative impact on inflows remains robust across methods. Second, the positive coefficients on banking and capital outflows are consistent with medium-run deleveraging and portfolio reallocation during grey-listing episodes, and should not be read as evidence that outbound cross-border payments become frictionless. In balance-of-payments accounting, an outflow can reflect either an increase in residents' foreign asset acquisition or a reduction in nonresidents' claims (e.g., repayments and non-rollover of existing liabilities), both of which can rise when new external funding is curtailed. Third, the weaker evidence for FX flows likely reflects measurement limitations and substitution across channels rather than the absence of underlying dynamics. Importantly, the DDML estimates closely match the order of magnitude of the DSL results, although more modest, underscoring the robustness of the findings across two distinct methodologies.

Regression Discontinuity in Time Series

The previous set of methodologies employed machine learning to guide variable selection and treatment effect estimation. These approaches mitigate omitted variable bias and reduce overfitting, providing effective strategies to estimate the impact of grey-listing on financial flows. DSL remains a linear estimator and therefore relies on functional-form assumptions when modeling the relationship between covariates and outcomes. DDML relaxes this restriction by allowing for flexible, nonparametric learners in the estimation of nuisance components. Nevertheless, both approaches primarily exploit cross-sectional or panel variation, and therefore may not fully leverage the quasi-experimental timing of grey-listing events.

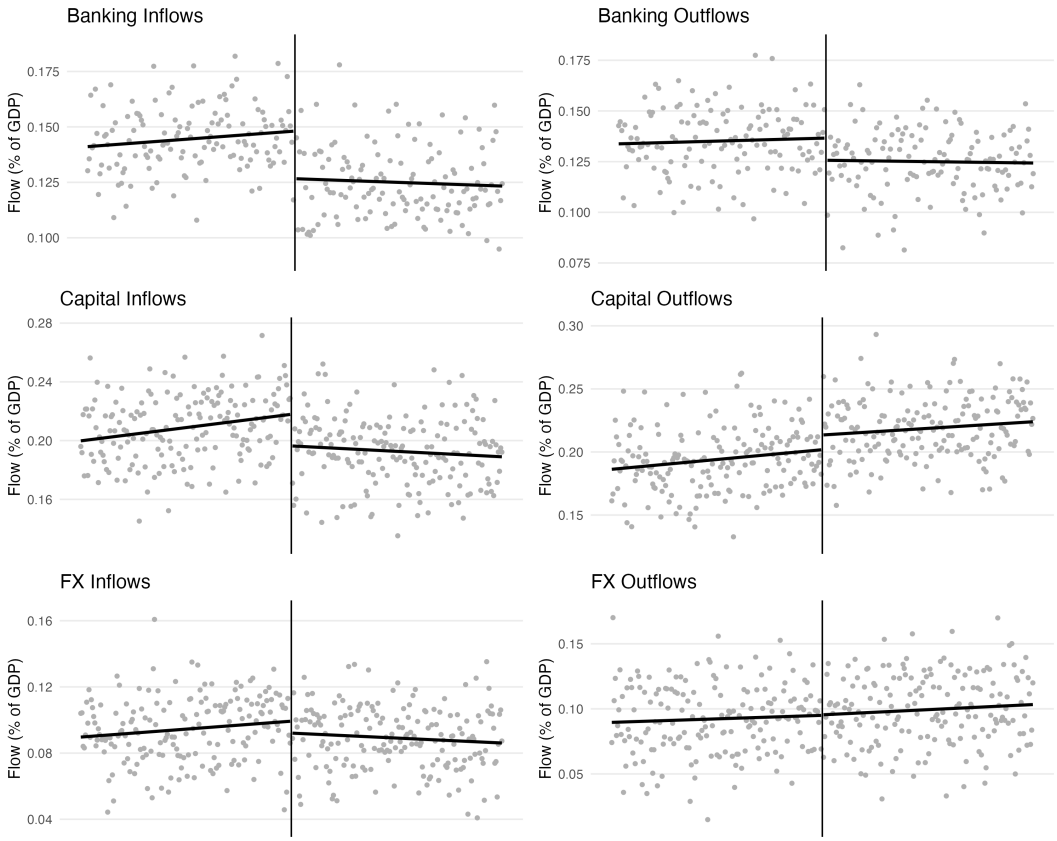
This section complements the ML estimators by focusing on local, within-country variation around the timing of FATF statements. Specifically, whereas DSL and DDML recover short-window average effects around the formal listing event—thereby combining limited anticipation with early post-publication adjustment—the Regression Discontinuity in Time (RDiT) design exploits the sharp timing of the official statement to isolate the immediate regulatory shock—such as the activation of compliance screening, STP failures, and the repricing of correspondent relationships—that occurs at publication. In this sense, the design functions as an event study embedded in a time-series framework. However, given this reliance on time-series variation, careful attention to persistence, seasonality, and global co-movements is crucial to avoid spurious discontinuities (Hausman and Rapson (2018)).

As a first step, we present graphical evidence in Figure 2. For each type of financial flow, we plot the outcome variable against event time centered on the grey-listing quarter and fit local linear trends on either side of the cutoff. The figures suggest a marked decline in several categories of inflows, most clearly for banking and capital inflows. Outflows are more heterogeneous across flow types. While these patterns are descriptive, they provide an intuitive first look at the discontinuities that the subsequent estimations formally test.

Why Regression Discontinuity in Time Is Informative in This Context

The use of RDiT in this paper is motivated by the institutional features of the FATF grey-listing process and by the need to isolate the causal impact of formal listing from both anticipation effects and confounding macro-financial dynamics. While grey-listing is the outcome of a lengthy and endogenous evaluation process, the official publication of the FATF list constitutes a discrete regulatory threshold that can generate localized changes in cross-border financial intermediation. This subsection clarifies why RDiT is informative in this setting and states explicitly the assumptions under which the design yields credible causal evidence.

Figure 2: Local Linear RDiT Graphs



Note: These figures plot regression discontinuity in time estimates using a local linear approach with 4 quarters of observations on either side of the threshold. Source: Own calculations.

Smoothness and global shocks. A core identifying assumption of the RDiT design is that, absent the listing decision, financial flows would evolve smoothly around the cutoff. A natural concern is whether FATF plenary meetings systematically coincide with global financial, regulatory, or macroeconomic shocks that could independently affect cross-border flows. Two features mitigate this concern. First, FATF statements are released on a predetermined calendar and only at a small number of discrete meeting dates each year, limiting scope for strategic alignment with contemporaneous shocks. Second, we present direct evidence on this smoothness assumption using placebo outcomes, predetermined outcomes (Figure 3), and calendar-placebo randomization inference tests (Figure 4).

A remaining limitation is that our calendar-placebo exercise reallocates cutoffs to other FATF statement quarters rather than to dates outside the FATF calendar altogether. We therefore interpret the placebo evidence as a test against residual seasonality or statement-calendar effects, not as a literal proof of continuity at arbitrary non-FATF dates. Even so, the absence of discontinuities in predetermined outcomes and global push factors, together

with robustness across bandwidths, kernels, and local-polynomial orders, provides reassuring evidence that the main listing-cutoff estimates are not driven by generic calendar timing.

Endogeneity of selection versus timing. A second concern relates to the endogeneity of FATF grey-listing decisions. While selection into grey-listing is endogenous and reflects underlying institutional, regulatory, and macro-financial conditions, countries do not control the precise timing of plenary outcomes or the exact quarter in which listing decisions are finalized. The RDiT design therefore identifies a local timing effect at the regulatory threshold, rather than a cross-country treatment contrast.

In other words, the design does not compare listed and non-listed countries. Instead, it compares outcomes for the same country immediately before and after crossing the formal FATF listing threshold, holding constant slow-moving fundamentals and country-specific trends. Any bias arising from endogenous selection into evaluation is therefore differenced out, provided that potential outcomes evolve smoothly in time absent the cutoff. The estimand captured by RDiT should thus be interpreted as the incremental effect of formally entering FATF increased monitoring, conditional on anticipation during the evaluation process.

Bandwidth choice and robustness. Because RDiT relies on local variation around the cutoff, estimates can be sensitive to bandwidth choice and functional form. To address this concern, the analysis focuses on narrow, symmetric windows around the listing and delisting dates and documents stability across alternative bandwidths and specifications.

Taken together, these considerations clarify the role of RDiT in the paper. The design exploits the sharp timing of FATF listing and delisting decisions to isolate localized regulatory effects associated with the activation and relaxation of compliance constraints. While some adjustment may occur prior to listing through expectation formation, the presence of a residual discontinuity at the cutoff reflects the operation of discrete compliance regimes embedded in global correspondent banking and payment systems. As a result, the RDiT estimates are best interpreted as conservative, policy-relevant estimates of the causal impact of formal grey-listing at the publication date.

Implementing the RDiT Design

Having established the identifying assumptions and provided direct validity diagnostics, we now describe the implementation of the RDiT design.

We begin from the canonical formulation of RDiT, which requires a treatment cutoff indicator, a smooth function of time relative to the cutoff (the running variable), country

fixed effects, and other controls. The baseline specification can be expressed as:

$$Y_{it} = \tau \cdot 1(t \geq T_{0i}) + f(t - T_{0i}) + X'_{it}\delta + \alpha_i + \varepsilon_{it}, \tag{14}$$

where Y_{it} denotes the outcome variable (financial flows as a percentage of GDP), $1(t \geq T_{0i})$ is an indicator equal to one if time t is at or after the grey-listing quarter T_{0i}, $f(t - T_{0i})$ is a smooth function of time relative to treatment, X_{it} is a vector of controls, α_i are country fixed effects, and ε_{it} is the error term.

In this formulation, the parameter of interest is the local jump τ in the conditional mean of Y_{it} at the grey-listing cutoff T_{0i}. Formally, under the standard RDiT assumption that $f(\cdot)$ captures a smooth evolution of potential outcomes in the absence of listing, τ can be interpreted as

$$\tau \;=\; \lim_{t \downarrow T_{0i}} \mathbb{E}[\, Y_{it} \mid t, X_{it}, \alpha_i \,] \;-\; \lim_{t \uparrow T_{0i}} \mathbb{E}[\, Y_{it} \mid t, X_{it}, \alpha_i \,], \tag{15}$$

that is, the instantaneous level shift in financial flows at the moment when the FATF grey list is officially published. Whereas the panel estimators recover a medium-run, event-window average effect, the RDiT design isolates this local publication-date discontinuity.

Unlike many applications of RDiT where treatment occurs once and persists, grey-listing episodes are temporary by design. The FATF explicitly intends grey-listing to induce compliance improvements, after which countries exit the list. The RDiT design must therefore accommodate transitory treatment while preserving valid identification.[21]

The covariate vector X_{it} absorbs residual variation and improves precision, but should not itself exhibit discontinuities at the cutoff. To ensure consistency across estimation approaches, we use the same DDML-selected controls that achieved an importance score of at least 0.01 in each regression (see Tables 12–14).

To estimate the local discontinuity at the cutoff, the analysis replaces the generic trend term with a *local-linear* RDiT defined on a narrow, symmetric window around the listing date T_{0i}. This specification models two linear segments on either side of the cutoff and concentrates identification on near-cutoff comparisons, thereby avoiding extrapolation to periods far from the event. Such locality is particularly important for capital, banking, and FX flows, which are volatile and co-move with global financial conditions. In this design, τ measures the immediate level shift at the cutoff. Consistent with the RDiT literature, high-order global polynomials are avoided because they tend to overfit and distort the estimated discontinuity (Hausman and Rapson (2018); Cattaneo and Vazquez-Bare (2017)). As a

[21] We also estimate a specification in which the treatment indicator equals one only in the listing quarter, as a robustness check. This "spike" specification is not preferred, as it discards post-cutoff observations and reduces power if treatment effects persist beyond one quarter or diffuse with short lags.

sensitivity check, a local-quadratic variant is reported later in this section; the baseline specification remains local-linear.

A local design necessarily discards observations far from the cutoff, raising sampling variance relative to global fits and making results sensitive to the bandwidth. This reflects the classic bias–variance trade-off in RDiT: tighter windows improve credibility by reducing exposure to unrelated shocks, but they also reduce precision. To navigate this trade-off, we cap the window at ± 4 quarters around T_{0i}—nine quarters in total when counting the listing quarter—and select the effective bandwidth by cross-validation within this cap. Estimation uses a triangular kernel so that observations closest to the cutoff receive the greatest weight. To document sensitivity, we later report a bandwidth sweep over ± 3, ± 5, and ± 6 quarters and show that the estimated discontinuity is stable across these choices. Inference relies on HAC standard errors for quarterly data and, in pooled panels, two-way clustering by country and calendar quarter.

Estimation Procedure. We now define the empirical specification. Let T_{0i} denote the listing (grey list) date for country i and $T_{1i} > T_{0i}$ the corresponding delisting date. The parameters of interest are the *level jumps* in Y_{it} at each threshold. Because grey-listing episodes are temporary, we treat listing and delisting as *separate cutoffs* and estimate local discontinuities in short windows around each date, rather than collapsing treatment into a one-period spike.[22]

We begin with the local design at listing. Define a symmetric window $\mathcal{W}_L = \{t : |t - T_{0i}| \leq h_L\}$ that excludes T_{1i} when possible, and estimate the local–linear specification

$$Y_{it} = \tau_L \, \mathbf{1}\{t \geq T_{0i}\} + \beta_- \, (t - T_{0i}) \, \mathbf{1}\{t < T_{0i}\} + \beta_+ \, (t - T_{0i}) \, \mathbf{1}\{t \geq T_{0i}\} + X_{it}'\delta + \alpha_i + \varepsilon_{it}, \quad (16)$$

by OLS on $t \in \mathcal{W}_L$ with a triangular kernel $K(u) = (1 - |u|)\mathbf{1}\{|u| \leq 1\}$ and weights $K\left(\frac{t - T_{0i}}{h_L}\right)$. The coefficient τ_L captures the immediate jump at listing, with locality induced by the bandwidth h_L and the kernel weights, while post-cutoff observations remain in the sample.

Analogously, for delisting we choose $\mathcal{W}_D = \{t : |t - T_{1i}| \leq h_D\}$ and estimate

$$Y_{it} = \tau_D \, \mathbf{1}\{t \geq T_{1i}\} + \gamma_- \, (t - T_{1i}) \, \mathbf{1}\{t < T_{1i}\} + \gamma_+ \, (t - T_{1i}) \, \mathbf{1}\{t \geq T_{1i}\} + X_{it}'\delta + \alpha_i + \varepsilon_{it}, \quad (17)$$

with weights $K\left(\frac{t - T_{1i}}{h_D}\right)$. The parameter τ_D identifies the level change at delisting—often

[22] When treatment episodes are extremely short, a joint two-cutoff design is recommended. In that case, when both thresholds lie inside a single window $\mathcal{W}_{LD} = \{t : T_{0i} - h \leq t \leq T_{1i} + h\}$, a single regression can capture the temporary nature of treatment while preserving the RD interpretation of level jumps. In our sample this situation never arises, so we abstract from it.

opposite in sign to τ_L—providing a natural credibility check on reversibility.

For both listing and delisting, bandwidths h_L and h_D are selected by cross-validation within a maximum of ± 4 quarters. Regressions include the data-driven covariate set X_{it} from the DDML stage (importance ≥ 0.01 for the corresponding outcome). A two-step augmented variant residualizes Y_{it} on a long window to remove smooth seasonality (quarter-of-year, holiday effects) and common global factors before applying the local design. Standard errors are heteroskedasticity- and autocorrelation-consistent (HAC) for quarterly data; in pooled panels, inference also accounts for cross-country dependence by two-way clustering (country and calendar quarter) or by a country–block bootstrap preserving within-country serial correlation. Alternative bandwidths, kernels, and local-polynomial orders are reported as diagnostic checks.[23]

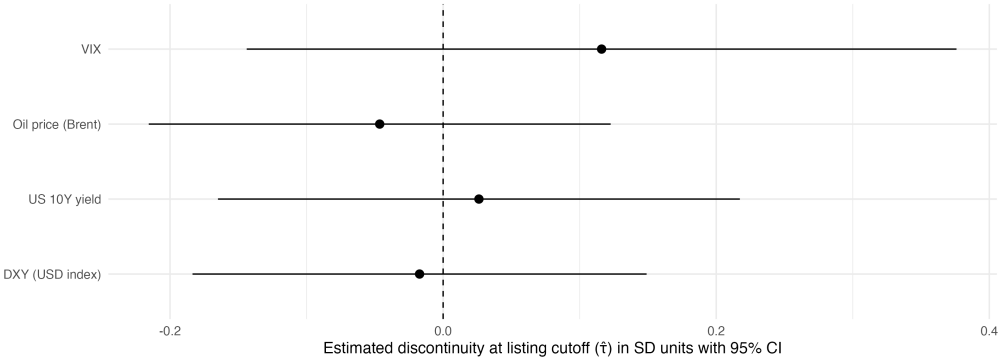
Threats to identification and diagnostic evidence. The credibility of the RDiT design rests on three classes of threats: (i) violations of the smooth-potential-outcomes assumption via slow-moving pre-trends or predetermined-outcome discontinuities; (ii) calendar-based or global-shock confounds arising from the concentration of FATF plenaries in fixed months; and (iii) sensitivity to bandwidth, kernel and polynomial order. This subsection summarizes the core diagnostic evidence that addresses each concern. Detailed tables and additional robustness checks are provided in Appendix C.

Pre-trends, predetermined outcomes, and placebo outcomes. We begin by examining whether outcomes or global push factors exhibit discontinuities at the listing cutoff in the absence of treatment. Figure 3 plots RDiT estimates using global variables (VIX, Brent, the U.S. 10-year yield, and the DXY index) and predetermined outcomes (four-quarter lagged flows) as dependent variables. None display discernible jumps at the cutoff, indicating that global financial conditions and earlier outcomes evolve smoothly at FATF statement dates.

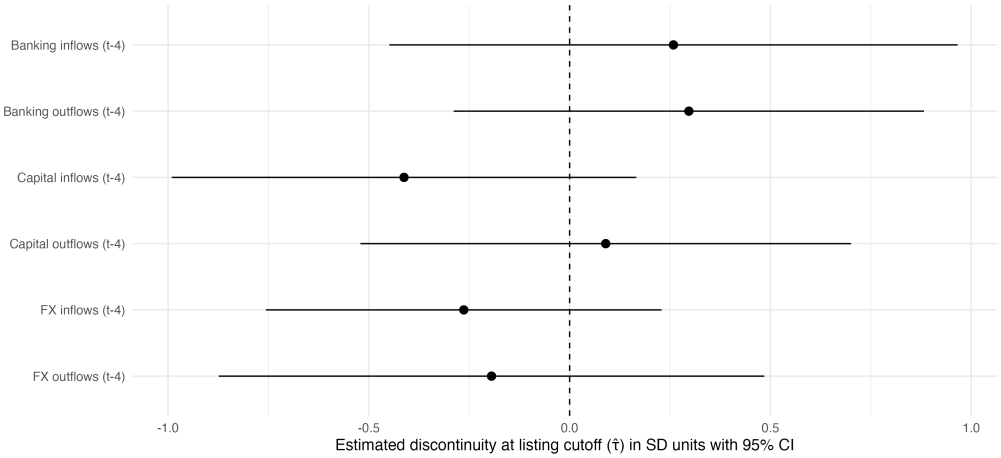
Table 2 complements this evidence using an event-time representation. The coefficients on pre-listing leads ($k = -4, -3, -2$) are economically small, and joint Wald tests consistently fail to reject the null of flat pre-trends. These results rule out slow-moving anticipatory dynamics as a driver of the estimated listing discontinuities. Although announcement-timed effects are somewhat larger than listing-timed effects—consistent with short-horizon anticipation—the persistence of a statistically significant jump at the publication date implies a

[23] "Spike-style" indicators are not used as the main specification because they discard informative post-cutoff observations and have low power with quarterly data. However, we estimated such specifications as a robustness check to detect short-lived "announcement-only" blips confined to the listing quarter. The main parameters of interest remain the local jumps (τ_L, τ_D) at the listing and delisting thresholds, consistent with RDiT best practice for temporary policy episodes. These additional results are not reported in this paper but can be shared upon request.

Figure 3: Validity checks for the RDiT design: placebo outcomes



(a) Panel A. Placebo outcomes: global push factors



(b) Panel B. Placebo outcomes: lagged financial flows (t-4)

Note: Panel (a) reports RDiT discontinuity estimates using global push factors (VIX, Brent oil price, U.S. 10-year yield, and DXY) as placebo outcomes. Panel (b) reports RDiT discontinuity estimates using four-quarter lagged financial flows (banking, capital, and FX inflows/outflows) as placebo outcomes. Outcomes are standardized to standard-deviation units, and points denote $\hat{\tau}$ with 95% confidence intervals from the baseline local-linear RDiT specification (triangular kernel) in a symmetric window of H quarters around the cutoff with country fixed effects and two-way clustering by country and calendar quarter.

discrete compliance-activation component that is not fully priced in beforehand.

Calendar confounds and global-shock clustering. A second concern is that FATF plenaries occur on a fixed triannual calendar (February, June, October), potentially coinciding with recurring global or macroeconomic events. To assess whether the observed discontinuities are calendar artifacts, we replace each country's true listing date with three alternative FATF statement quarters of the same statement type and quarter-of-year and re-estimate the baseline RDiT specification. Table 3 reports these *calendar-placebo* discontinuities. Across all flow variables, placebo jumps are centered near zero and economically negligible. A

Table 2: RDiT anticipation diagnostics summary

	Pre-trends test				Announcement test	
	-4	-3	-2	Wald p	$\hat{\tau}_L$ (listing)	$\hat{\tau}_A$ (announce)
Banking inflows	-0.03	-0.06	-0.12**	0.180	-1.250**	-1.550**
Banking outflows	0.02	0.05	0.10**	0.120	-0.670	0.850*
Capital inflows	-0.02	-0.04	-0.09**	0.110	-1.430**	-1.600***
Capital outflows	0.01	0.03	0.08	0.240	1.230*	1.550**
FX inflows	-0.05	-0.10	-0.18*	0.190	-1.170*	-1.010
FX outflows	0.04	0.08	0.15	0.210	0.620	0.540

Note: Wald p-values test whether coefficients on pre-listing leads (e.g., $k = -4, -3, -2$ quarters) are jointly zero. $\hat{\tau}_L$ denotes the baseline local-linear listing discontinuity (post-listing indicator coefficient), and $\hat{\tau}_A$ denotes the analogous discontinuity when the cutoff is aligned to the first public announcement date (post-announcement indicator coefficient). Full tables with standard errors and additional details are reported in Appendix C.9, Tables 33–38. *, **, *** denote significance at the 10%, 5%, and 1% levels.

few FX placebo coefficients are marginally significant at the 10% level, consistent with small seasonal blips, but their magnitudes remain much smaller than the true listing discontinuities documented in the baseline RDiT results.

Figure 4 deepens this analysis using randomization inference. For each inflow variable, we generate 500 placebo reassignments of listing dates (restricted to same-type FATF statements) and re-estimate the local-linear RDiT. The resulting placebo distributions are tightly centered around zero, while the true discontinuities lie far in the tails. These findings reject the hypothesis that recurrent global events—or the FATF calendar itself—mechanically induce discontinuities at the listing date.

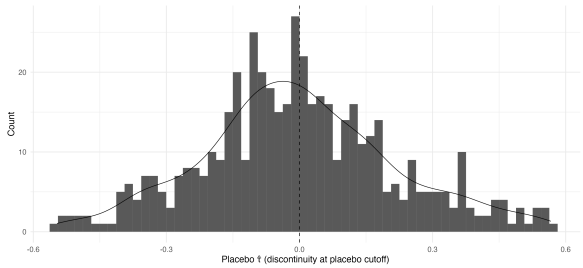
Table 3: RDiT calendar-placebo cutoff diagnostic summary

	Placebo 1	Placebo 2	Placebo 3
Banking inflows	0.012	-0.018	0.006
Banking outflows	-0.009	0.015	-0.011
Capital inflows	0.025	-0.014	0.009
Capital outflows	-0.020	0.017	-0.011
FX inflows	-0.055*	0.030	-0.022*
FX outflows	0.048*	-0.026	0.018

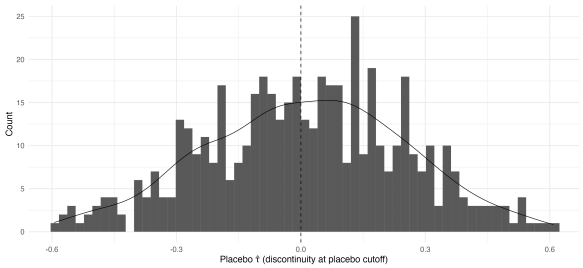
Note: The table reports estimated placebo discontinuities from the baseline local-linear RDiT specification (in percent of quarterly GDP), replacing each country's true listing cutoff with three alternative quarters of the same statement type and quarter-of-year. Placebo jumps should be centered near zero; statistically significant placebo coefficients indicate residual calendar timing or seasonality unrelated to grey-listing. Full tables with standard errors and additional details are reported in Appendix C.9, Tables 39–41. * denotes significance at the 10% level.

Bandwidth, kernel and polynomial robustness. RDiT estimates are local by construction, and a common concern is that discontinuity estimates may be sensitive to smoothing choices

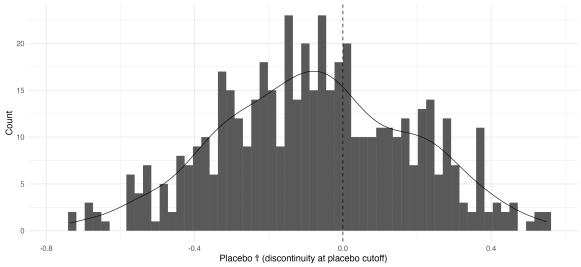
Figure 4: Calendar-placebo randomization inference: inflow outcomes



(a) Capital Inflows



(b) Banking Inflows



(c) FX Inflows

Note: Each panel plots the distribution of placebo RDiT discontinuities from $B = 500$ calendar-placebo reassignments of each jurisdiction's listing cutoff to alternative FATF statement quarters of the same type (and, where applicable, the same quarter-of-year), excluding overlapping listing/delisting windows. In each draw, we re-estimate the baseline local-linear RDiT specification (triangular kernel) and pool across jurisdictions as in the main design. The dashed vertical line marks zero. Panels are zoomed to the central 99% of placebo estimates; the true estimate lies outside the plotted range.

such as the bandwidth, kernel, or polynomial order. Table 4 therefore reports, in the first column, the baseline listing-cutoff estimates for each flow category (local-linear, $H = 4$, triangular kernel), and then shows how the estimated jump changes under alternative bandwidths $H \in \{3, 5, 6\}$, alternative kernel weights (Uniform and Epanechnikov), and a local-quadratic specification. Across flow types, the estimated discontinuities remain similar in sign and economically comparable in magnitude under these alternative choices. Inflows are consistently negative across banking, capital, and FX measures, typically around one to one-and-a-half percentage points of quarterly GDP, while outflow responses are smaller and more heterogeneous (including positive estimates for capital outflows and, depending on the specification, FX outflows). The main conclusions are unchanged: the inflow contraction at listing is not driven by a particular bandwidth, kernel, or polynomial choice.

Delisting reversals as an identification check. Finally, delisting events provide an institutional sign-reversal test. If the discontinuities at listing were driven by unrelated global shocks coinciding with plenaries, there would be no reason to expect systematic opposite-

Table 4: RDiT bandwidth, kernel, and polynomial sensitivity summary (listing cutoff)

	Baseline	Bandwidth H			Kernel		Polynomial
	$H = 4$ Triangular Local-linear	$H = 3$	$H = 5$	$H = 6$	Uniform	Epanechnikov	Local-quadratic
Banking inflows	-1.250**	-1.312**	-1.190**	-1.184*	-1.243**	-1.247**	-1.150**
Banking outflows	-0.670	-0.692	-0.654	-0.631	-0.654	-0.665	-0.616*
Capital inflows	-1.430**	-1.441**	-1.416**	-1.414**	-1.398*	-1.411**	-1.316**
Capital outflows	1.230*	1.262*	1.245*	1.228	1.198*	1.225**	1.132**
FX inflows	-1.170*	-1.201**	-1.172*	-1.596*	-1.158	-1.179*	-1.076*
FX outflows	0.620	0.644*	0.613	0.588	0.596	0.612	0.570*

Note: Entries report point estimates of the RDiT discontinuity at the listing cutoff (coefficient on the post-cutoff indicator) for each flow type, expressed as percent of quarterly GDP. The baseline specification is local-linear with $H = 4$ and a triangular kernel; subsequent columns vary the bandwidth, kernel weighting function, or polynomial order (local-quadratic). All specifications include country fixed effects and the baseline control set, with two-way clustered standard errors by country and calendar quarter. Full tables with standard errors and additional details are reported in Appendix C.9, Tables 42–50. *, **, *** denote significance at the 10%, 5%, and 1% levels.

signed jumps at delisting. Instead, Table 5 shows that capital and banking inflows rise upon delisting, and outflows fall, with reversal ratios in the 0.4–0.7 range. This symmetry, coupled with the partial (rather than full) rebound, aligns with the economic mechanism in which listing activates compliance frictions that relax only gradually after exit. The sign-reversal pattern thus strengthens the causal interpretation of the RDiT estimates.

Taken together, the diagnostic evidence supports the validity of the RDiT design. Financial flows and global push factors evolve smoothly at placebo cutoffs; outcomes do not exhibit pre-trends; calendar confounds are negligible; estimates are robust to bandwidth and polynomial choices; and delisting reversals match theoretical predictions. These results indicate that the sharp discontinuities observed at FATF listing dates reflect the activation of compliance-related constraints rather than calendar timing or global-cycle artifacts.

Estimation Results. This section presents the main RDiT results. As discussed in Section 3, we use the RDiT estimates to pin down whether there is a discrete publication-date component in the response and to assess how much of the listing decline reverses at delisting. Table 5 summarizes the estimated local jumps at listing and delisting, while the complete set of RDiT estimates is reported in Appendix C.6 and C.7. These appendices present results for the listing-date cutoff (Tables 21–26) and the delisting-date cutoff (Tables 27–32).

Using the listing date as the cutoff, the RDiT estimates reveal a sharp and economically meaningful contraction in *inflows* at grey-listing across banking, capital, and FX aggregates, accompanied by smaller and noisier responses in outflows. On magnitudes, banking inflows fall by about -1.25 percentage points of quarterly GDP (robust across specifications in the -1.3 to -2.0 range, significant at the 95% level), capital inflows decline by approximately -1.43 percentage points (-1.2 to -1.8, significant at the 95% level), and FX inflows contract

by roughly −1.17 percentage points (−1.0 to −1.6, significant at the 90% level). These effects are large on a quarterly scale.

Outflows generally move in the opposite direction but with weaker force. Banking outflows increase by about 0.6 percentage points of GDP (typically insignificant), capital outflows rise by roughly +1.2 percentage points (significant at the 90% level), and FX outflows show no systematic jump. This asymmetry—larger contractions in inflows than expansions in outflows—suggests that fewer new funds arrive and rollover becomes more difficult, while capital flight proceeds more gradually. Taken together, the net private capital balance deteriorates by roughly 2–3 percentage points of GDP at listing.

To reconcile outflows across designs, note that the DDML estimand averages outcomes over a broader grey-listing episode (and, by construction, allows for some anticipatory and post-event adjustment within the event window), so it can capture medium-run deleveraging dynamics such as gradual portfolio rebalancing and the non-rollover/repayment of external liabilities, which register as higher measured outflows in the balance of payments. By contrast, the RDiT design isolates the publication-date component of the response. Consistent with a compliance-frictions interpretation, outflow discontinuities at the listing cutoff are weaker and less stable than inflow discontinuities even when average outflows rise over the broader episode, because the same compliance and screening constraints that deter new inflows can also slow the immediate execution of outbound transactions. As a descriptive check on the proposed compliance-channel interpretation (and on the asymmetry between inflows and outflows), Appendix C.8 reports an event-time plot of banking messages counterparty breadth around listing and delisting, showing a persistent post-listing contraction in inbound breadth and a partial broadening around delisting.

A further sensitivity check in C.10 excludes countries that are re-listed during the sample period and re-estimates the baseline listing-cutoff RDiT using only first-time listing episodes. Table 54 shows that the inflow discontinuities remain negative and economically close to the baseline estimates, although precision declines as the number of usable cutoffs falls. This pattern indicates that the main results are not driven by a small set of repeat-listing jurisdictions with unusually persistent compliance problems; if anything, repeat-listers amplify but do not create the estimated contraction in inflows.

The GDP-weighted annualized figures reported in Table 58 of Appendix E imply that, in annual terms, banking, capital, and FX inflows contract by around −0.35% of annual GDP at the listing date, while capital outflows increase by approximately +0.3%, yielding a total annual impact of about −0.65% of GDP.

These publication-date discontinuities are slightly smaller in magnitude than the corresponding event-window average effects from the ML estimates, but they remain economically

meaningful and precisely estimated. This pattern is consistent with the interpretation in Section 3: part of the adjustment occurs before the formal statement, muting the observed jump, while a sizeable residual discontinuity at the cutoff suggests an additional compliance-related tightening that activates discretely when the listing is published.

Table 5: Summary of Local Jumps at Listing and Delisting (RDiT Estimates)

Flow Category	τ_L (Listing Jump)	τ_D (Delisting Jump)	$\rho = \tau_D/\lvert\tau_L\rvert$
Banking Inflows	-1.3 to -2.0^{**}	$+0.6$ to $+0.9^{*}$	0.4–0.6
Banking Outflows	$\approx +0.6$ (n.s.)	≈ -0.7 (n.s.)	n/a
Capital Inflows	-1.2 to -1.8^{**}	$+0.6$ to $+0.9^{**}$	0.5–0.7
Capital Outflows	$+1.0$ to $+1.9^{*}$	-0.7 to -0.9^{*}	0.5–0.8
FX Inflows	-1.0 to -1.6^{*}	$+0.6$ to $+1.3$ (n.s.)	0.6–0.8
FX Outflows	≈ 0 (n.s.)	≈ 0 (n.s.)	n/a
Net Capital Balance	–2 to –3 (deterioration)	+1 to +1.5 (improvement)	$\approx$ 0.5–0.6

Note: Ranges reflect robustness across specifications with DDML controls and baseline bandwidths. *, **, *** denote significance at the 10%, 5%, and 1% levels. "n.s." indicates not significant at conventional levels. Net capital balance combines inflows and outflows.

At the delisting cutoff, the pattern reverses in the expected direction, although the rebound is partial and noisier. Inflows rise: banking inflows rebound by about +0.76 percentage points of GDP (+0.6 to +0.9, significant at the 90% level), capital inflows increase by approximately +0.86 percentage points (+0.6 to +0.9, significant at the 95% level), and FX inflows rise by around +0.8 percentage points (positive but not always statistically significant). Outflows ease: banking outflows fall by about −0.7 percentage points (imprecise), capital outflows decline by −0.8 percentage points (significant at the 90% level), and FX outflows again show no systematic change.

Across categories, delisting discontinuities are smaller than the corresponding listing effects, with reversal ratios $\rho = \tau_D/|\tau_L|$ in the 0.4–0.7 range. The net private capital balance improves by approximately 1–1.5 percentage points of GDP, restoring roughly half to two-thirds of the earlier deterioration. The GDP-weighted annualized figures in Table 58 indicate that banking and capital inflows rise between +0.2% and +0.3% of annual GDP at delisting, while capital outflows fall by about −0.2%, for a total annual improvement of roughly +0.45% of GDP.

At delisting, the discontinuities imply only a partial offset of the listing-induced contraction, rather than a symmetric rebound. In terms of the framework in Section 3, this asymmetry points to persistence: delisting relaxes the formal designation, but cross-border intermediation does not immediately return to pre-listing levels, consistent with fixed costs and slow rebuilding in correspondent banking and related cross-border payment relationships.

The corresponding sensitivity exercise in Appendix C.10 applies the same restriction to the delisting-cutoff RDiT, retaining only first-event exits from the grey list. The estimated delisting reversals preserve the expected sign pattern—inflows rebound and outflows ease—but become less precise as the effective number of delisting episodes falls. This suggests that the partial rebound at delisting is not an artifact of countries cycling repeatedly on and off the list; rather, it reflects a broader pattern of incomplete unwinding once formal monitoring is removed.

Taken together, the RDiT evidence is consistent with grey-listing activating compliance-driven de-risking and regulatory frictions that curtail new external finance, while delisting eases—but does not fully eliminate—these constraints. The resulting asymmetry—sharp contractions in inflows, more muted and heterogeneous responses in outflows, and only partial reversal upon exit—reflects the structure of global financial intermediation rather than purely behavioral responses. Grey-listing operates primarily through the supply side of cross-border financial services: global correspondent banks and payment intermediaries restrict inbound relationships to manage regulatory risk and compliance costs. Because domestic agents rely on these same correspondent channels to move funds abroad, relationship and network frictions embedded in global payment systems unwind more slowly than the initial compliance shock, generating persistent but asymmetric effects around listing and delisting events.

Appendix C.12 shows that the RDiT evidence follows the same cross-country pattern. At the listing cutoff, the local discontinuity in inflows is more negative in EMDEs, in more trade-open economies, and in countries with above-median capital account openness. For banking inflows, the listing jump is -1.454 in EMDEs, -1.382 in the high-trade-openness subsample, and -1.396 in the high-capital-account-openness subsample, compared with -0.556, -1.067, and -1.019, respectively, in the corresponding lower-exposure groups. For capital inflows, the analogous jumps are -1.605, -1.586, and -1.612, compared with -0.852, -1.239, and -1.186. At delisting, the rebound remains partial but is somewhat more visible in the same higher-exposure subsamples, consistent with a slower and incomplete unwinding of the cross-border frictions activated at listing.

Finally, to assess comparability across datasets, we re-estimate all specifications on the intersection of all panels: the 2010–2017 window used for the banking series and the 86-country set (of which 45 have been grey-listed). The main qualitative findings are unchanged, although estimates are somewhat noisier due to the shorter sample. The corresponding results can be found in Table 55 in Appendix C.11.

5 Conclusion

This paper provides new evidence on the causal impact of FATF grey-listing on international financial flows. Combining two high-dimensional machine learning strategies—Double Selection Lasso (DSL) and Double Debiased Machine Learning (DDML)—with a Regression Discontinuity in Time (RDiT) design, we exploit both the panel variation across countries and the sharp timing of grey-listing events. This dual strategy allows us to address concerns about omitted variables, functional form assumptions, and spurious trends, while providing complementary perspectives on medium-run and immediate effects.

Three main results emerge. First, grey-listing generates a clear and robust contraction in cross-border inflows. Across banking transactions, balance-of-payments capital flows, and FX data, the estimates point to significant declines, typically between 0.7 and 2.6 percent of GDP in the machine learning frameworks, and about 0.9 to 1.9 percentage points of GDP in local quarterly RDiT windows. Second, while inflows fall systematically, the evidence on outflows is more heterogeneous. Balance-of-payments and banking outflows often rise, consistent with precautionary portfolio reallocation under heightened regulatory risk, but the effects are less robust in FX markets. Third, delisting induces partial reversals: inflows recover and outflows ease, but the rebound typically restores only half to two-thirds of the earlier deterioration, reflecting the gradual unwinding of relationship and network frictions embedded in global financial intermediation after compliance constraints are relaxed.

These asymmetric patterns reflect distinct operational mechanics in global banking. Grey-listing acts as an immediate supply-side shock to cross-border liquidity: global financial institutions, adhering to automated risk protocols, often trigger "Enhanced Due Diligence" (EDD) or cut correspondent banking relationships (CBRs) immediately upon the listing announcement. This creates an abrupt "sudden stop" in new financing and payment instructions entering the jurisdiction. At the same time, grey-listing can generate gross outflows through mechanisms that are not mutually exclusive: residents may rebalance toward foreign assets (flight-to-safety) and external liabilities may be repaid or not rolled over as counterparties de-risk. These forces can raise measured outflows in episode-average estimates even as compliance screening, STP failures, and enhanced due diligence increase the cost and delay of executing cross-border transfers. This structural friction helps explain why outflow responses are typically weaker and less precisely estimated at the publication cutoff than the collapse in inflows, reinforcing the interpretation of grey-listing as a sharp tightening of external financial constraints.

The convergence of results across DSL, DDML, and RDiT underscores the robustness of the findings. The machine learning estimators highlight the role of sovereign risk and global

financial conditions in amplifying stigma effects, while the RDiT design confirms sharp local discontinuities aligned with official listing and delisting dates. Diagnostic checks further validate the design, ruling out pre-trends, false positives, or functional form artifacts, and showing symmetry around delisting.

These findings have two key implications. For policymakers, they document sizeable and asymmetric financial costs of grey-listing, reinforcing the urgency of compliance with AML/CFT standards not only to meet international obligations but also to preserve market access. For international institutions, they highlight how transparency around listing decisions can shape investor behavior even prior to formal implementation, calling for careful communication strategies. For future research, the results invite deeper analysis of distributional channels—including portfolio versus bank flows, sovereign versus private borrowing, domestic spillovers, and potential substitution toward less observable cross-border circuits such as money service businesses, informal transfer systems, and virtual-asset channels—and the persistence of stigma beyond formal delisting. A related agenda is to examine whether grey-listing affects underlying illicit activity itself, which would require outcome variables capable of capturing crime reduction or shifts in money-laundering behavior rather than changes in formal cross-border financial intermediation alone.

Overall, the evidence suggests that FATF grey-listing imposes economically meaningful penalties on countries' financial integration. While intended as a temporary compliance mechanism, its consequences on cross-border finance are immediate, asymmetric, and only partially reversible.

A final normative clarification is warranted. The data used in this paper do not allow us to distinguish between licit and illicit financial transactions, as our outcomes (balance-of-payments flows, banking transactions, and FX market activity) are observed at an aggregate level. However, the presence of economically meaningful aggregate effects implies that a non-trivial share of the adjustment necessarily falls on licit transactions. The channels examined in this paper, including cross-border banking relationships, portfolio capital, and formal FX transactions, are primary conduits for legitimate trade, investment, and financial intermediation. Consequently, the contraction in inflows and the frictions observed around grey-listing reflect measurable costs borne by lawful economic activity operating through the formal financial system. A related limitation is that the data do not allow us to observe whether part of the adjustment is displaced from formal banking and reported FX channels toward less observable mechanisms, including MSBs or virtual-asset platforms. More broadly, our results should be interpreted as evidence of compliance and market-enforcement effects in formal financial channels, not as a direct measure of AML effectiveness in the narrower sense of reducing money laundering, predicate offences, or other illicit activity; establishing

that type of effect would require different outcomes and a different empirical design.

Grey-listing therefore operates as an international regulatory externality: a designation made in a multilateral forum can tighten cross-border financial intermediation through foreign compliance constraints, even in the absence of formal transaction prohibitions. Policy implications follow directly. For jurisdictions approaching the 5th Round of FATF evaluations, the evidence suggests that grey-listing carries measurable financial costs in the form of reduced inflows and, in several cases, intensified outflows. These costs appear most acute in the immediate aftermath of listing, when correspondent banks, investors, and compliance teams update risk assessments. The results therefore highlight the value of early, targeted remediation plans that stabilize expectations and shorten time on the list. They also suggest that baseline external exposure (e.g., trade openness) and economy size, together with market structure, condition the size of the impact, pointing to differentiated technical assistance and communications strategies.

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A Appendix A: Further Details on Variables

A.1 Additional Details: Variables and Sources

Table 6: Variables and Sources

Variable	Source
Dependent variables	
Banking flows	Proprietary data[24]
Capital flows	IMF (BOP/BPM6/IFS)
FX Flows	BIS
Domestic Control variables	
Real GDP (quarterly and yearly growth rate)	IMF (WEO)
One-year ahead forecast of real GDP growth rate	IMF (WEO)
Real Effective Exchange Rate	IMF (IFS)
Nominal Exchange Rate	IMF (IFS)
International Reserves (as a share of GDP)	IMF (IFS)
Interest rate	IMF (IFS)
Capital Account Openness	Chinn-Ito (2022)
Exchange Rate Flexibility	Ilzetzki et al. (2019)
Credit Ratings	S&P Ratings
Global Control variables	
VIX	CBOE
US 3-month T-bill rate	FRED
US 10-year rate	FRED
US Federal Funds rate	FRED
Commodity price index (including and excluding gold)	IMF (WEO)
Real GDP (G5 countries, US, Canada, EU, UK, Japan)	IMF (WEO)
Real GDP growth rate (G5 countries, US, Canada, EU, UK, Japan)	IMF (WEO)

Note: we used multiple transformations of each variable when necessary such as: levels, squared, qoq, yoy.
Sources: International Financial Statistics (IFS) (IMF (2025a)); Bank for International Settlements (BIS (2025)); World Economic Outlook (WEO) (IMF (2025b)); Chicago Board Options Exchange (CBOE (2025)); Federal Reserve Economic Data (FRED (2025)).

A.2 About Banking Data and What it can Measure

The network used transmits messages through two main methods: the series method and the coverage method. In the series method, the system routes each message through all network levels, including the originator, the recipient, and any correspondents. The coverage method, also known as the "undercover" method, uses another type of message to relay settlement instructions to intermediaries, while the former message carries payment details directly from sender to receiver. Under the coverage method, customer payment information is accompanied by settlement messages involving intermediary institutions, which may lead to multiple messages associated with a single underlying payment. To avoid double counting, we focus exclusively on the first type of message flows. These flows capture processed customer payments and therefore provide a high-frequency proxy for cross-border banking activity,

but they do not represent the universe of cross-border transactions (e.g., other channels or purely domestic settlement).

A.3 Exchange Rate Flexibility

Table 7: De Facto Exchange Rate Arrangement Classification

1	No separate legal tender or currency union
2	Pre announced peg or currency board arrangement
3	Pre announced horizontal band that is narrower than or equal to +/-2%
4	De facto peg
5	Pre announced crawling peg; de facto moving band narrower than or equal to +/-1%
6	Pre announced crawling band that is narrower than or equal to +/-2% or de facto horizontal band that is narrower than or equal to +/-2%
7	De facto crawling peg
8	De facto crawling band that is narrower than or equal to +/-2%
9	Pre announced crawling band that is wider than or equal to +/-2%
10	De facto crawling band that is narrower than or equal to +/-5%
11	Moving band that is narrower than or equal to +/-2% (i.e., allows for both appreciation and depreciation over time)
12	De facto moving band +/-5%/ Managed floating
13	Freely floating
14	Freely falling
15	Dual market in which parallel market data is missing.

Source: Ilzetzki et al. (2019).

B Appendix B: Grey-listed Countries

Table 8: List of Countries and their Listings, 2000-2025

Country	1st Listing	1st Delisting	2nd Listing	2nd Delisting	3rd Listing	3rd Delisting
Afghanistan	June 2012	June 2017				
Albania	June 2012	February 2015	February 2020	October 2023		
Algeria	October 2011	February 2016	October 2024			
Antigua and Barbuda	February 2010	February 2014				
Argentina	June 2011	October 2014				
Azerbaijan	February 2010	October 2010				
Bahamas	June 2000	June 2001	October 2018	December 2020		
Bangladesh	October 2010	February 2014				
Barbados	February 2020	February 2024				
Bolivia	February 2010	June 2013				
Bosnia and Herzegovina	June 2015	February 2018				
Botswana	October 2018	October 2021				
Brunei Darussalam	June 2011	June 2013				
Bulgaria	October 2023					
Burkina Faso	February 2021					
Cambodia	June 2011	February 2015	February 2019	February 2023		
Cameroon	June 2023					
Cayman Islands	June 2000	June 2001	February 2021	October 2023		
Cook Islands	June 2000	February 2005				
Croatia	June 2023					
Cuba	June 2011	October 2014				
Democratic Republic of The Congo	October 2022					
Dominica	June 2000	October 2002				
Ecuador	February 2010	October 2015				
Egypt	June 2001	February 2004				
Ethiopia	February 2010	October 2014	February 2017	October 2019		
Ghana	October 2010	February 2013	October 2018	June 2021		
Greece	February 2010	June 2011				
Grenada	September 2001	February 2003				
Guatemala	June 2001	July 2004				
Guyana	October 2014	October 2016				
Haiti	June 2021					
Honduras	October 2010	February 2012				
Hungary	June 2001	June 2002				
Indonesia	June 2001	February 2005	February 2010	June 2015		
Iran	June 2016					
Iraq	October 2013	June 2018				
Israel	June 2000	June 2002				
Jamaica	February 2020	June 2024				
Kenya	February 2010	June 2014	February 2024			
Korea	February 2010					
Kuwait	June 2012	February 2015				
Lao P.D.R.	June 2013	June 2017	February 2025			
Lebanon	June 2000	June 2002	October 2024			
Liechtenstein	June 2000	June 2001				
Marshall Islands	June 2000	October 2002				
Monaco	June 2024					
Mongolia	June 2011	June 2014	October 2019	October 2020		
Morocco	February 2010	October 2013	February 2021	February 2023		
Myanmar	June 2001	October 2006	February 2010	June 2016	February 2020	
Namibia	June 2011	February 2015	February 2024			
Nauru	June 2000	October 2005				
Nigeria	June 2001	June 2006	February 2010	October 2013	February 2023	
Nepal	February 2010	June 2014	February 2025			
Nicaragua	June 2011	February 2015	February 2020	October 2022		
Pakistan	February 2008	February 2015	June 2018	October 2022		
Panama	June 2000	June 2001	June 2014	February 2016	June 2019	October 2023
Papua New Guinea	February 2014	June 2016				
Paraguay	February 2010	February 2012				
Philippines	June 2000	February 2005	October 2010	June 2013	June 2021	February 2025
Qatar	February 2010	October 2010				
Russia	June 2000	October 2002				
Sao-Tome and Principe	February 2008	October 2013				
Senegal	February 2021	October 2024				
Serbia	February 2018	June 2019				
South Africa	February 2023					
Sri Lanka	February 2010	June 2013	November 2017	October 2019		
St. Kitts and Nevis	June 2000	June 2002				
St. Vincent and the Grenadines	June 2000	June 2003				
Sudan	February 2010	October 2015	June 2021			
Syria	February 2010					
Tanzania	October 2010	June 2014	October 2022			
Tajikistan	June 2011	October 2014				
Thailand	February 2010	June 2013				
Trinidad and Tobago	February 2010	October 2012	October 2017	February 2020		
Tunisia	November 2017	October 2019				
Turkiye	February 2010	October 2014	October 2021	June 2024		
Turkmenistan	February 2008	June 2012				
Uganda	February 2014	November 2017	February 2020	February 2024		
Ukraine	September 2001	February 2004	February 2010	October 2011		
Uzbekistan	February 2008	February 2010				
Vanuatu	February 2016	June 2018				
Venezuela	October 2010	February 2013	June 2024			
Vietnam	October 2010	February 2014	June 2023			
Yemen	February 2010					
Zimbabwe	June 2011	February 2015	October 2019	March 2022		

Source: FATF public statements (various years) compiled by authors.

C Appendix C: Output of Estimations

C.1 DSL: Selected Controls and Weights

Note: Weights are reported in absolute value from the penalized regression and depend on variable scaling; they should be interpreted as relative contributions within the selected set rather than as economically meaningful magnitudes.

Table 9: Banking Flows Lasso Weights (absolute values)

Variable	Weight
Credit ratings short-term index (log)	149.50
US short-term rate (lag)	71.72
Exchange rate flexibility index (log)	70.42
US ten-year rate	31.19
US fed funds rate (qoq)	11.17
Credit ratings outlook index (log)	3.17
International reserves, without gold, as a % of GDP (lag)	2.09
Credit ratings long-term index (sq)	1.30
GDP quarterly growth rate (lag)	1.24
Short-term interest rate	0.98
US fed funds rate (lag)	0.49
Capital account openness index	0.03
Credit ratings outlook index (lag)	0.02
Short-term interest rate (yoy)	0.02
Capital account openness index (qoq)	0.01

(a) Banking Inflows

Variable	Weight
Exchange rate flexibility index (log)	47.48
International reserves, without gold, as a % of GDP (lag)	1.12
Credit ratings long-term index (sq)	0.67
Credit ratings short-term index (log)	0.08
Capital account openness index	0.03
Credit ratings outlook index (lag)	0.02
Short-term interest rate (yoy)	0.02
Capital account openness index (qoq)	0.01

(b) Banking Outflows

Table 10: Capital Flows Lasso Weights (absolute values)

Variable	Weight
US short-term rate (sq)	0.41
US fed funds rate (sq)	0.33
Real exchange rate (lag)	0.15
Credit ratings short-term index (log)	0.08
Capital account openness index	0.03
Credit ratings outlook index (lag)	0.02
Short-term interest rate (yoy)	0.02
Capital account openness index (qoq)	0.01

(a) Capital Inflows

Variable	Weight
US short-term rate (sq)	0.47
Credit ratings short-term index (log)	0.08
Capital account openness index	0.03
Credit ratings outlook index (lag)	0.02
Short-term interest rate (yoy)	0.02
Capital account openness index (qoq)	0.01

(b) Capital Outflows

Table 11: FX Flows Lasso Weights (absolute values)

Variable	Weight
Exchange rate flexibility index (log)	0.56
Capital account openness index (sq)	0.22
Credit ratings short-term index (log)	0.08
Capital account openness index	0.03
Credit ratings outlook index (lag)	0.02
Short-term interest rate (yoy)	0.02
Capital account openness index (qoq)	0.01
Nominal exchange rate	0.01

(a) FX Inflows

Variable	Weight
Exchange rate flexibility index (qoq)	4.40
Real exchange rate (log)	1.11
Exchange rate flexibility index (log)	0.66
Credit ratings outlook index (log)	0.55
Capital account openness index (sq)	0.16
GDP quarterly growth rate	0.10
Credit ratings short-term index (log)	0.08
GDP quarterly growth rate (lag)	0.04
Short-term interest rate	0.03
Capital account openness index	0.03
Credit ratings outlook index (lag)	0.02
Short-term interest rate (yoy)	0.02
GDP yearly growth rate (qoq)	0.02
Nominal exchange rate (lag)	0.02
Capital account openness index (qoq)	0.01

(b) FX Outflows

C.2 DDML: Controls and their Relative Importance

Table 12: Banking Flows DDML Importance (from 0 to 1)

Variable	Importance
Exchange rate flexibility index (log)	0.78
Credit ratings long-term index (lag)	0.62
Credit ratings outlook index (lag)	0.61
US short-term rate (lag)	0.41
US ten-year rate	0.38
Capital account openness index	0.29
Nominal exchange rate (sq)	0.17
VIX index (lag)	0.16
International reserves, without gold, as a % of GDP (lag)	0.09
GDP quarterly growth rate (lag)	0.05
Credit ratings short-term index (lag)	0.05
US fed funds rate (lag)	0.02
Credit ratings long-term index (sq)	0.02

(a) Banking Inflows

Variable	Importance
Credit ratings outlook index (lag)	0.49
Exchange rate flexibility index (log)	0.40
US ten-year rate	0.33
Credit ratings long-term index (sq)	0.29
Credit ratings long-term index (lag)	0.28
Capital account openness index	0.26
Nominal exchange rate (sq)	0.13
VIX index (lag)	0.13
International reserves, without gold, as a % of GDP (lag)	0.09
GDP quarterly growth rate	0.06
Credit ratings short-term index (log)	0.03

(b) Banking Outflows

Table 13: Capital Flows DDML Importance (from 0 to 1)

Variable	Importance
Real exchange rate (lag)	0.55
US short-term rate (sq)	0.46
US fed funds rate (lag)	0.21
Credit ratings short-term index (log)	0.18
Credit ratings long-term index (sq)	0.16
Capital account openness index	0.16
International reserves, without gold, as a % of GDP (sq)	0.14
Credit ratings outlook index (lag)	0.11
US fed funds rate (sq)	0.04
VIX index (lag)	0.02
Nominal exchange rate (sq)	0.01
Exchange rate flexibility index (sq)	0.01

(a) Capital Inflows

Variable	Importance
US short-term rate (sq)	0.39
Exchange rate flexibility index (log)	0.27
Credit ratings short-term index (log)	0.23
Credit ratings long-term index (sq)	0.22
Capital account openness index	0.19
US fed funds rate (sq)	0.09
International reserves, without gold, as a % of GDP (sq)	0.06
Credit ratings outlook index (lag)	0.05
VIX index (lag)	0.02
Nominal exchange rate (sq)	0.01

(b) Capital Outflows

Table 14: FX Flows DDML Importance (from 0 to 1)

Variable	Importance
Exchange rate flexibility index (log)	0.47
Nominal exchange rate	0.25
Capital account openness index (sq)	0.19
Credit ratings short-term index (lag)	0.17
Capital account openness index	0.16
Credit ratings outlook index (lag)	0.11
VIX index (lag)	0.08
Credit ratings long-term (lag)	0.03
Exchange rate flexibility index (sq)	0.01

(a) FX Inflows

Variable	Importance
Exchange rate flexibility index (log)	0.51
Real exchange rate (log)	0.42
Nominal exchange rate (log)	0.28
Capital account openness index (sq)	0.26
Credit ratings short-term index (log)	0.23
Credit ratings outlook index (log)	0.17
Capital account openness index	0.16
GDP quarterly growth rate (lag)	0.12
Credit ratings outlook index (lag)	0.09
VIX index (lag)	0.09
Credit ratings long-term (lag)	0.03
Exchange rate flexibility index (sq)	0.01

(b) FX Outflows

C.3 DSL: Banking Flows

Table 15: Effect of Grey-listing on Quarterly Banking Inflows (as a percent of GDP)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Grey list dummy	-1.344	-2.475*	-1.901*	-0.634**	-1.947**	-0.549*	-0.065*	-0.482*	-0.284**	-0.916**	-2.083**	-2.091**	-2.122**	-2.260**	-2.114**
	(1.909)	(0.756)	(0.735)	(0.119)	(0.158)	(0.134)	(0.091)	(0.130)	(0.033)	(0.280)	(0.953)	(0.740)	(0.550)	(0.514)	(0.583)
Credit ratings short-term (log)		-5.856	-10.20	-8.650	-9.747	-9.280	-5.605	-1.018	-4.384	-2.780	-4.499	-3.872	-4.016	-30.86	-30.58
		(11.07)	(11.10)	(11.51)	(11.51)	(11.53)	(14.35)	(14.19)	(21.79)	(21.85)	(27.99)	(28.08)	(28.57)	(42.47)	(42.46)
US short-term rate (lag)			-63.71***	-63.61***	-48.21**	-48.24**	-47.70**	-43.70**	-43.91**	-49.69**	-41.42	-15.64	-14.11	-26.36	-34.42
			(17.45)	(18.50)	(19.83)	(19.83)	(20.07)	(20.07)	(20.10)	(21.00)	(26.94)	(86.67)	(88.67)	(128.1)	(128.3)
Exchange rate flexibility index (log)				11.21	11.04	10.79	8.017	8.573	8.778	11.67	5.792	5.737	6.297	25.47	26.16
				(9.640)	(9.630)	(9.637)	(9.777)	(9.666)	(9.721)	(10.19)	(12.96)	(12.97)	(13.23)	(21.14)	(21.15)
US ten-year rate					-11.30**	-11.40**	-10.55**	-9.993*	-10.08*	-10.20*	-6.476	-6.532	-6.688	4.014	3.480
					(5.265)	(5.267)	(5.318)	(5.287)	(5.305)	(5.306)	(6.855)	(6.860)	(6.996)	(10.23)	(10.24)
US fed funds rate (qoq)						3.165	4.923	3.829	3.941	5.014	8.431	8.794	9.079	9.415	9.695
						(4.393)	(4.429)	(4.407)	(4.442)	(4.583)	(5.686)	(5.806)	(5.931)	(8.410)	(8.413)
Credit ratings outlook (log)							-14.43***	-13.47***	-13.30***	-13.62***	6.138	6.192	6.353	-10.38	-10.52
							(3.550)	(3.542)	(3.647)	(3.663)	(5.506)	(5.511)	(5.632)	(9.392)	(9.391)
International reserves, without gold, as a % of GDP (lag)								0.206	0.214	0.244	0.153	0.167	0.161	0.306	0.366
								(0.414)	(0.416)	(0.417)	(0.511)	(0.513)	(0.528)	(0.807)	(0.809)
Credit ratings long-term (sq)									0.0167	0.00652	0.147	0.142	0.156	0.318	0.321*
									(0.0819)	(0.0826)	(0.123)	(0.124)	(0.128)	(0.193)	(0.193)
GDP quarterly growth rate (lag)										0.526	-0.0705	-0.104	-0.137	0.888	0.865
										(0.552)	(0.742)	(0.750)	(0.766)	(1.137)	(1.138)
Short-term interest rate											-0.0229	-0.0247	-0.0331	0.168	0.0354
											(0.375)	(0.375)	(0.382)	(0.632)	(0.644)
US fed funds rate (lag)												-24.64	-25.15	-16.51	-11.28
												(78.71)	(80.71)	(113.7)	(113.8)
Capital account openness index													-2.521	-6.047	-6.050
													(5.130)	(8.214)	(8.213)
Credit ratings outlook (lag)														-2.839	-2.817
														(2.946)	(2.946)
Short-term interest rate (yoy)															6.795
															(6.369)
R-squared	0.0836	0.0828	0.0878	0.0968	0.1238	0.1229	0.1197	0.1254	0.1301	0.1322	0.1416	0.1472	0.1469	0.1515	0.1526
N	5598	1809	1809	1707	1707	1707	1689	1655	1655	1655	1100	1100	1076	648	648

Note: The table presents estimates of the effect of grey-listing on quarterly banking inflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 16: Effect of Grey-listing on Quarterly Banking Outflows (as a percent of GDP)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Grey list dummy	-0.658	-0.829	-0.715	-0.236	-0.375	-0.891	0.327	-2.221	-3.141
	(4.679)	(3.254)	(3.559)	(6.610)	(7.620)	(5.100)	(5.843)	(6.104)	(6.176)
Exchange rate flexibility index (log)		3.238	4.378	17.19	17.45	17.91*	23.71	21.60	21.54
		(7.545)	(8.804)	(26.29)	(26.31)	(9.866)	(15.11)	(17.73)	(17.75)
International reserves, without gold, as a % of GDP (lag)			0.0521	0.565	0.586	0.502	0.360	0.565	0.567
			(0.302)	(1.122)	(1.124)	(0.415)	(0.644)	(0.761)	(0.762)
Credit ratings long-term (sq)				-0.0108	0.0313	0.0410	0.139	0.222	0.223
				(0.140)	(0.193)	(0.0711)	(0.105)	(0.172)	(0.173)
Credit ratings short-term (log)					-13.50	-12.70	-12.74	-20.91	-21.03
					(42.78)	(15.56)	(25.42)	(32.10)	(32.15)
Capital account openness index						-4.269	-7.100	-6.061	-6.092
						(4.328)	(6.432)	(7.699)	(7.711)
Credit ratings outlook (lag)							-6.817***	-0.781	-0.774
							(1.870)	(2.815)	(2.819)
Short-term interest rate (yoy)								5.097	5.092
								(6.074)	(6.080)
Capital account openness index (qoq)									-0.198
									(1.818)
R-squared	0.0837	0.0918	0.1022	0.1161	0.1134	0.1221	0.1291	0.1344	0.1456
N	5540	4941	4496	1673	1673	1605	1056	663	663

Note: The table presents estimates of the effect of grey-listing on quarterly banking outflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

C.4 DSL: Capital Flows

Table 17: Effect of Grey-listing on Quarterly Capital Inflows (as a percent of GDP)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Grey list dummy	-0.421	-0.474*	-0.625*	-1.514*	-1.921***	-2.293***	-2.261***	-2.505***	-2.572***
	(1.577)	(0.191)	(0.202)	(0.655)	(0.768)	(0.708)	(0.379)	(0.439)	(0.474)
US short-term rate (sq)		0.403***	-0.995***	-1.375*	0.339	0.392	-0.244	0.339	0.343
		(0.0729)	(0.385)	(0.725)	(0.814)	(0.735)	(0.825)	(0.764)	(0.765)
US fed funds rate (sq)			1.246***	1.993***	0.904	0.911	1.339*	1.171*	1.165*
			(0.337)	(0.635)	(0.701)	(0.634)	(0.718)	(0.665)	(0.665)
Real exchange rate (lag)				0.266***	0.437***	0.449***	0.538***	0.665***	0.663***
				(0.0992)	(0.0890)	(0.0817)	(0.0927)	(0.0851)	(0.0853)
Credit ratings short-term (log)					27.80***	28.53***	20.28***	19.99**	20.11**
					(6.810)	(6.245)	(7.100)	(8.295)	(8.309)
Capital account openness index						-1.683	2.463	2.815	2.815
						(2.250)	(2.702)	(2.452)	(2.453)
Credit ratings outlook (lag)							2.354***	2.298**	2.305**
							(0.908)	(1.044)	(1.045)
Short-term interest rate (yoy)								-1.170	-1.178
								(1.870)	(1.872)
Capital account openness index (qoq)									0.251
									(0.804)
R-squared	0.1122	0.1314	0.1378	0.1362	0.1490	0.1528	0.1678	0.1699	0.1827
N	12846	12846	12846	6441	2216	2161	1346	847	847

Note: The table presents estimates of the effect of grey-listing on quarterly capital inflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 18: Effect of Grey-listing on Quarterly Capital Outflows (as a percent of GDP)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Grey list dummy	0.358	1.232	1.774**	1.803**	1.846**	1.853**	1.881**
	(2.025)	(2.050)	(0.374)	(0.380)	(0.371)	(0.361)	(0.369)
US short-term rate (sq)		0.424***	0.732***	0.795***	0.695***	0.896***	0.896***
		(0.0748)	(0.0755)	(0.0714)	(0.0852)	(0.0808)	(0.0809)
Credit ratings short-term (log)			16.58***	16.92***	12.43***	16.92***	16.94***
			(3.968)	(3.720)	(4.370)	(4.769)	(4.772)
Capital account openness index				0.305	2.096	2.948**	2.949**
				(1.097)	(1.401)	(1.330)	(1.330)
Credit ratings outlook (lag)					1.025*	0.281	0.282
					(0.564)	(0.616)	(0.616)
Short-term interest rate (yoy)						0.0143	0.0132
						(0.975)	(0.975)
Capital account openness index (qoq)							0.0739
							(0.597)
R-squared	0.0992	0.1013	0.1248	0.1282	0.1315	0.1369	0.1440
N	12787	12787	3993	3745	2329	1549	1549

Note: The table presents estimates of the effect of grey-listing on quarterly capital outflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

C.5 DSL: FX Flows

Table 19: Effect of Grey-listing on Quarterly FX Inflows (as a percent of GDP)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Grey list dummy	6.214	7.142	-1.126	-1.407***	-1.469***	-1.914***	-0.811***	-0.733***	-0.526***
	(6.670)	(8.804)	(6.630)	(0.670)	(0.630)	(0.629)	(0.193)	(0.197)	(0.110)
Exchange rate flexibility index (log)		98.80***	90.39***	20.34***	20.61***	25.88***	20.84***	20.81***	5.393***
		(19.30)	(19.65)	(4.398)	(4.401)	(5.635)	(7.779)	(7.782)	(1.795)
Capital account openness index (sq)			62.58***	8.519***	9.218***	8.727***	9.986***	10.07***	0.187
			(4.953)	(1.026)	(1.132)	(1.127)	(1.203)	(1.211)	(0.182)
Credit ratings short-term (log)				-1.006	0.177	2.593	8.708	8.925	2.314**
				(6.796)	(6.842)	(6.313)	(6.467)	(6.479)	(0.982)
Capital account openness index					-3.415	2.669	11.67***	11.64***	-0.245
					(2.346)	(2.291)	(2.198)	(2.200)	(0.341)
Credit ratings outlook (lag)						-0.410	-0.724	-0.735	-0.154
						(0.864)	(0.921)	(0.922)	(0.134)
Short-term interest rate (yoy)							-1.551	-1.576	0.389
							(1.957)	(1.958)	(0.439)
Capital account openness index (qoq)								0.854	0.0112
								(1.337)	(0.172)
Nominal exchange rate									0.0548***
									(0.0140)
R-squared	0.0723	0.0781	0.0992	0.1023	0.1015	0.1123	0.1287	0.1328	0.1389
N	4712	3711	3515	1517	1517	943	695	695	418

Note: The table presents estimates of the effect of grey-listing on quarterly FX inflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 20: Effect of Grey-listing on Quarterly FX Outflows (as a percent of GDP)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Grey list dummy	-0.321*	-0.0214	0.0450	0.0488	-0.118	-0.0177	-0.0127	-0.0126	-0.0134	-0.0352	-0.0674	-0.1112	-0.0630	-0.0737	-0.0406	-0.0488
	(0.177)	(0.459)	(0.472)	(0.472)	(0.631)	(0.637)	(0.641)	(0.642)	(0.639)	(0.499)	(0.404)	(0.593)	(0.595)	(0.594)	(0.592)	(0.593)
Exchange rate flexibility index (qoq)		0.366	-0.0971	-0.122	-0.122	-0.165	-0.164	-0.165	-0.134	-0.0379	-0.0367	1.967	2.063	2.128	2.169	2.192
		(1.188)	(0.909)	(0.910)	(1.000)	(1.001)	(1.001)	(1.002)	(0.997)	(0.214)	(0.214)	(1.975)	(1.978)	(1.975)	(1.950)	(1.952)
Real exchange rate (log)			3.377***	3.367***	2.415*	2.418*	2.412*	2.442*	1.221	0.988***	1.003***	1.345***	1.365***	1.397***	1.257***	1.286***
			(1.016)	(1.016)	(1.362)	(1.362)	(1.364)	(1.391)	(1.439)	(0.378)	(0.379)	(0.347)	(0.347)	(0.347)	(0.346)	(0.348)
Exchange rate flexibility index (log)				0.276	1.013	0.995	0.995	1.003	1.053	1.104***	1.109***	-0.262	-0.235	-0.206	-0.0297	-0.0558
				(0.444)	(0.665)	(0.665)	(0.665)	(0.669)	(0.666)	(0.194)	(0.194)	(0.481)	(0.482)	(0.481)	(0.478)	(0.479)
Credit ratings outlook (log)					-0.939**	-1.005***	-1.011***	-1.010***	-1.177***	0.618***	0.621***	0.437***	0.444***	0.422***	0.475***	0.474***
					(0.368)	(0.372)	(0.376)	(0.376)	(0.378)	(0.116)	(0.116)	(0.107)	(0.107)	(0.108)	(0.108)	(0.108)
Capital account openness index (sq)						-0.195	-0.194	-0.193	-0.162	-0.0134	0.000695	0.0481	0.0473	0.0429	0.0743	0.0750
						(0.152)	(0.153)	(0.153)	(0.153)	(0.0401)	(0.0443)	(0.0456)	(0.0456)	(0.0456)	(0.0460)	(0.0460)
GDP quarterly growth rate							0.00484	0.00462	-0.00401	0.0358***	0.0369***	0.0501***	0.0524***	0.0544***	0.0675***	0.0677***
							(0.0452)	(0.0453)	(0.0451)	(0.0123)	(0.0124)	(0.0113)	(0.0115)	(0.0116)	(0.0121)	(0.0121)
Credit ratings short-term (log)								-0.113	-0.199	0.368	0.419	-0.206	-0.200	-0.175	-0.410	-0.411
								(1.026)	(1.022)	(0.314)	(0.321)	(0.300)	(0.300)	(0.300)	(0.304)	(0.304)
GDP quarterly growth rate (lag)									0.150***	0.0239**	0.0254**	0.0129	0.0113	0.0120	0.0135	0.0124
									(0.0480)	(0.0120)	(0.0122)	(0.0117)	(0.0119)	(0.0119)	(0.0117)	(0.0118)
Short-term interest rate										-0.00576	-0.00460	-0.0110	-0.0114	-0.0112	-0.0120	-0.0124
										(0.00846)	(0.00860)	(0.00817)	(0.00818)	(0.00817)	(0.00807)	(0.00809)
Capital account openness index											-0.0639	0.000869	0.00253	0.00271	-0.0141	-0.0110
											(0.0853)	(0.0752)	(0.0752)	(0.0751)	(0.0743)	(0.0745)
Credit ratings outlook (lag)												0.0559	0.0536	0.0471	0.0445	0.0456
												(0.0341)	(0.0342)	(0.0344)	(0.0339)	(0.0340)
Short-term interest rate (yoy)													-0.0999	-0.100	-0.141	-0.142
													(0.108)	(0.108)	(0.107)	(0.107)
GDP quarterly growth rate (qoq)														0.0345	0.0370*	0.0371*
														(0.0216)	(0.0213)	(0.0213)
Nominal exchange rate (lag)															0.00778***	0.00756***
															(0.00235)	(0.00237)
Capital account openness index (qoq)																0.0257
																(0.0342)
R-squared	0.0622	0.0691	0.0729	0.0828	0.0846	0.0933	0.0987	0.1004	0.1071	0.1055	0.1084	0.1129	0.1145	0.1189	0.1246	0.1281
N	5484	4294	2331	2331	1086	1086	1086	1086	1086	733	733	432	432	432	432	432

Note: The table presents estimates of the effect of grey-listing on quarterly FX outflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

C.6 RDiT Outputs: Listing Date as Cutoff

Table 21: RDiT Results on the Effect of Grey-listing on Quarterly Banking Inflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Grey list dummy	-0.645318*	-0.4749294*	-0.472031*	-0.9666121*	-1.923259*	-1.922033*	-1.429399**	-1.885341**	-1.994511**	-1.673937**	-1.598761**	-1.473822**	-1.283911**	-1.250471**
	0.641	0.185	0.561	0.835	0.476	0.112	0.395	0.827	0.361	0.307	0.747	0.473	0.383	0.200
Exchange rate flexibility index (log)		0.4857629*	0.329845*	0.281739	0.528744**	0.583267**	0.473928**	0.436482**	0.284567	0.382879	0.468271**	0.423844**	0.428191**	0.439123**
		0.343	0.301	0.845	0.845	0.180	0.420	0.197	0.023	0.961	0.363	0.463	0.361	0.636
Credit ratings long-term index (lag)			0.237584	0.283474	0.194755	0.192203	0.226119	0.214524	0.247654	0.245622	0.245574	0.238966	0.234263	0.234755
			0.263	0.454	0.315	0.125	0.151	0.280	0.127	0.445	0.254	0.331	0.223	0.492
Credit ratings outlook index (lag)				-0.046373*	-0.039484	-0.038481	-0.349392*	-0.283731*	-0.263898*	-0.123692*	-0.123882*	-0.138939*	-0.124747*	-0.113748*
				0.012	0.046	0.056	0.029	0.084	0.079	0.015	0.056	0.077	0.085	0.066
US short-term rate (lag)					-0.003283	-0.003345	-0.008172	-0.003747	-0.073744	-0.064741	-0.073991	-0.074319	-0.068382	-0.067843
					0.036	0.127	0.118	0.183	0.033	0.160	0.194	0.076	0.114	0.124
US ten-year rate						0.748322	0.736272	0.362822	0.849392	0.182828	0.272281	0.162829	0.162496	0.127454
						0.686	0.685	0.048	0.508	0.651	0.842	0.723	0.462	0.814
Capital account openness index							0.673431	0.739145*	0.712456*	0.661341*	0.547817*	0.316648*	0.356271*	0.328939*
							3.687	6.828	8.124	6.415	5.607	3.484	4.373	3.882
Nominal exchange rate (sq)								0.002537	-0.034842	-0.045732	-0.092737	-0.084746	-0.087281	-0.093348
								0.004	0.005	0.002	0.008	0.007	0.006	0.006
VIX index (lag)									-0.004742	-0.004999	-0.004946	-0.286738*	-0.348282*	-0.294529**
									1.087	0.636	0.923	0.748	0.972	0.936
International reserves, without gold, as a % of GDP (lag)										0.003786*	0.003943*	0.147372	0.152772	0.263682
										0.462	0.814	0.456	0.473	0.563
GDP quarterly growth rate (lag)											0.026367	0.783945	0.627388	0.657342
											0.876	0.075	0.601	0.844
Credit ratings short-term index (lag)												0.083777	0.091236	0.087954
												0.070	0.080	0.098
US fed funds rate (lag)													-0.037691	-0.045733
													0.293	0.738
Credit ratings long-term index (sq)														-0.027639
														0.019
R-squared	0.1722	0.1692	0.1731	0.1736	0.1748	0.1912	0.1916	0.1915	0.1927	0.1945	0.1969	0.1976	0.1981	0.2123
N	270	262	256	256	256	256	232	232	232	230	230	230	230	230
Effective listing cutoffs (n_L)	15	15	15	15	15	15	14	14	14	14	14	14	14	14

Note: The table presents RDiT estimates of the effect of grey-listing on quarterly banking inflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 22: RDiT Results on the Effect of Grey-listing on Quarterly Banking Outflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11	12
Grey list dummy	-0.783923**	-0.129845	-0.748392	-0.464878	-0.276681	-0.982567	-0.945527	-0.648356	-0.901278	-0.658929	-0.883689	-0.670655
	0.690	0.197	0.421	0.777	0.501	0.013	0.727	0.289	0.131	0.451	0.353	0.220
Credit ratings outlook index (lag)		-0.027226*	-0.038482*	-0.029676	-0.075843*	-0.083921*	-0.273826**	-0.094933*	-0.074502*	-0.049392	-0.073829*	-0.073639*
		0.034	0.069	0.080	0.046	0.098	0.044	0.094	0.076	0.043	0.021	0.084
Exchange rate flexibility index (log)			0.129873**	0.129744**	0.123756	0.124789	0.129347**	0.128882*	0.131876	0.129382**	0.124567*	0.128769**
			0.719	0.832	0.863	0.534	0.671	0.852	0.502	0.744	0.782	0.566
US ten-year rate				4.378739	5.432975	4.789345	4.457823	4.545678	5.217899	5.523128	4.523211	4.498722
				0.892	0.759	0.597	1.134	0.719	1.184	0.800	0.569	1.016
Credit ratings long-term index (sq)					0.001737	0.002736	-0.027377	-0.018286	-0.047582	-0.028182	-0.018292	-0.017374
					0.018	0.030	0.023	0.014	0.012	0.022	0.007	0.008
Credit ratings long-term index (lag)						0.193733	0.917336	0.473733	0.384822	0.183445	0.192834	0.200192
						0.196	0.109	0.716	0.686	0.429	0.167	0.563
Capital account openness index							0.564738*	0.536822*	0.48392*	0.463829*	0.728292	0.538483*
							0.033	0.144	0.232	0.236	0.194	0.288
Nominal exchange rate (sq)								-0.073822	-0.062721	-0.028286	-0.073833	-0.070272
								0.119	0.110	0.152	0.141	0.130
VIX index (lag)									-0.018374*	-0.017928*	-0.017923*	-0.018234*
									1.765	1.404	1.564	1.596
International reserves, without gold, as a % of GDP (lag)										0.030193	0.031283	0.029374
										0.839	1.345	1.414
GDP quarterly growth rate											1.376201*	1.378902*
											0.361	0.390
Credit ratings short-term index (log)												0.053832
												0.017
R-squared	0.1312	0.1345	0.1321	0.1347	0.1422	0.1561	0.1562	0.1569	0.1583	0.1611	0.1609	0.1713
N	270	256	256	256	256	256	232	232	232	230	230	230
Effective listing cutoffs (n_L)	15	15	15	15	15	15	14	14	14	14	14	14

Note: The table presents RDiT estimates of the effect of grey-listing on quarterly banking outflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 23: RDiT Results on the Effect of Grey-listing on Quarterly Capital Inflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11	12	13
Grey list dummy	-1.122374*	-1.475639*	-1.48594*	-2.382948	-1.947654*	-1.234849*	-1.182749**	-1.485759**	-1.574839**	-1.836475**	-1.475443**	-1.477585**	-1.430692**
	0.953	0.566	0.946	0.050	0.968	0.431	0.104	0.673	0.402	0.976	0.847	0.474	0.180
Real exchange rate (lag)		0.217484	0.374833	0.263743	0.273281	0.263822	0.392722	0.328182	0.317378	0.246261	0.252622	0.248733	0.218344
		0.111	0.132	0.129	0.168	0.196	0.328	0.141	0.232	0.323	0.373	0.106	0.273
US short-term rate (sq)			-0.003564	-0.003859	-0.004759	-0.018283	-0.005869*	-0.003565*	-0.003875*	-0.003967*	-0.003987*	-0.006283*	-0.006456*
			0.119	0.287	0.205	0.106	0.256	0.006	0.135	0.208	0.299	0.273	0.298
US fed funds rate (lag)				-0.045859	-0.008582	-0.004775	-0.005747	-0.002836	-0.004575	-0.003622	-0.043748	-0.046372	-0.048393
				0.073	0.022	0.049	0.018	0.049	0.059	0.074	0.084	0.066	0.055
Credit ratings short-term index (log)					0.027245	0.036378	0.058383**	0.054732**	0.047339**	0.049383**	0.048393**	0.054738**	0.052763**
					0.047	0.043	0.042	0.045	0.040	0.049	0.049	0.050	0.050
Credit ratings long-term index (sq)						0.037445	-0.037472	-0.036855	-0.046574	-0.027374	-0.032822	-0.029339	-0.024744
						0.024	0.077	0.020	0.073	0.087	0.024	0.032	0.088
Capital account openness index							0.392838*	0.373743*	0.373392*	0.472823*	0.298273*	0.378282*	0.393827*
							1.375	1.695	1.092	1.829	1.605	1.355	1.258
International reserves, without gold, as a % of GDP (sq)								0.002485	0.003824	0.002447	0.002748	0.002833	0.002754
								0.079	0.043	0.013	0.045	0.016	0.082
Credit ratings outlook index (lag)									0.152833	0.172823	0.182736	0.142343	0.142836
									0.389	0.371	0.265	0.304	0.285
US fed funds rate (sq)										-0.012834	-0.023736	-0.028384	-0.019283
										0.011	0.014	0.019	0.018
VIX index (lag)											-0.128484	-0.123848	-0.263834**
											0.060	0.005	0.154
Nominal exchange rate (sq)												-0.084464	-0.082533
												0.173	0.182
Exchange rate flexibility index (sq)													0.102734**
													0.771
R-squared	0.1827	0.2034	0.2139	0.2118	0.2167	0.2168	0.2188	0.2234	0.2291	0.2292	0.2294	0.2367	0.2361
N	358	358	358	358	345	345	328	320	320	320	320	320	320
Effective listing cutoffs (n_L)	39	39	39	39	37	37	35	34	34	34	34	34	34

Note: The table presents RDiT estimates of the effect of grey-listing on quarterly capital inflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 24: RDiT Results on the Effect of Grey-listing on Quarterly Capital Outflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11
Grey list dummy	0.984529*	1.348543*	1.485993*	1.012933*	1.832834*	1.759943*	1.374839	1.193743*	1.083711*	1.938456*	1.230843*
	0.796	0.580	0.043	0.066	0.749	0.736	0.321	0.590	0.092	0.588	0.200
US short-term rate (sq)		-0.002734	-0.004637	-0.054834	-0.067533	-0.127364*	-0.184845*	-0.057483*	-0.057382*	-0.0438273**	-0.058392*
		0.075	0.046	0.092	0.027	0.095	0.059	0.024	0.099	0.036	0.090
Exchange rate flexibility index (log)			-0.525754*	-0.475843*	-1.374845	-1.347557	-0.513748**	-0.523845**	-0.527384**	-1.234277	-0.634745**
			0.316	0.419	0.344	0.485	0.276	0.362	0.239	0.474	0.281
Credit ratings short-term index (log)				0.031828	0.032834	0.031738	0.098311	0.192834	0.048322	0.032734	0.032767
				0.127	0.314	0.400	0.481	0.456	0.444	0.117	0.143
Credit ratings long-term index (sq)					-0.003744	-0.013745	-0.017345	-0.027364	-0.017263	-0.017364	-0.013747
					0.092	0.054	0.057	0.054	0.076	0.070	0.086
Capital account openness index						0.318345*	0.734855	1.273741	0.268455*	0.373455*	0.447376*
						0.472	0.522	0.681	0.489	0.646	0.659
US fed funds rate (sq)							-0.017273	-0.027344	-0.018271	-0.017234	-0.012332
							0.012	0.029	0.028	0.013	0.022
International reserves, without gold, as a % of GDP (sq)								-0.001383	-0.012343	-0.011067	-0.005748
								0.002	0.002	0.000	0.001
Credit ratings outlook index (lag)									-0.173644	-0.134745	-0.129234
									0.533	0.673	0.805
VIX index (lag)										0.057489	0.064783
										0.017	0.032
Nominal exchange rate (sq)											-0.068375
											0.574
R-squared	0.1428	0.1456	0.1478	0.1562	0.1534	0.1598	0.1635	0.1636	0.1645	0.1641	0.1919
N	350	342	342	342	342	328	302	302	302	300	300
Effective listing cutoffs (n_L)	38	37	37	37	37	35	32	32	32	32	32

Note: The table presents RDiT estimates of the effect of grey-listing on quarterly capital outflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 25: RDiT Results on the Effect of Grey-listing on Quarterly FX Inflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10
Grey list dummy	-1.548332	-1.003843*	-1.485743*	-1.345865	-1.374561	-1.475433	-1.104743	-1.374322*	-1.124858*	-1.170826*
	0.151	0.938	0.158	0.708	0.128	0.314	0.831	0.104	0.216	0.280
Exchange rate flexibility index (log)		0.657483**	0.594932**	0.453828	1.374833	0.673822*	0.610023*	0.623119*	0.654592**	0.428374**
		0.108	0.222	0.217	0.293	0.224	0.246	0.294	0.137	0.276
Nominal exchange rate			0.054732	0.046382	0.037844	0.046473	0.045622	0.059393	0.057463	0.054732
			0.196	0.289	0.331	0.281	0.280	0.224	0.099	0.242
Capital account openness index (sq)				0.821867	0.832923	0.273943	0.283843	0.574732	0.273896	0.233956
				0.170	0.283	0.168	0.086	0.136	0.168	0.051
Credit ratings short-term index (lag)					0.038292*	0.037282*	0.038283*	0.037263*	0.036282*	0.034575*
					0.052	0.057	0.099	0.092	0.075	0.087
Capital account openness index						-0.028733	-0.102837	-1.574836*	-0.982711	-1.039326*
						0.003	0.041	0.019	0.045	0.017
Credit ratings outlook index (lag)							0.093844	0.037421	0.089972	0.092762
							0.186	0.154	0.267	0.265
VIX index (lag)								-0.305551**	-0.374075**	-0.395617**
								0.007	0.026	0.025
Credit ratings long-term (lag)									0.182734	0.185655
									0.327	0.406
Exchange rate flexibility index (sq)										-0.026738
										0.009
R-squared	0.1862	0.1891	0.2412	0.2635	0.2716	0.2706	0.2722	0.2739	0.2741	0.2612
N	341	316	298	298	290	290	290	290	290	290
Effective listing cutoffs (n_L)	37	34	32	32	31	31	31	31	31	31

Note: The table presents RDiT estimates of the effect of grey-listing on quarterly FX inflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 26: RDiT Results on the Effect of Grey-listing on Quarterly FX Outflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11	12	13
Grey list dummy	0.576831	0.485765	0.428595	0.434799	0.473822	0.074565	0.072475	0.072213	0.075375	0.064586	0.601012	0.668634	0.620486
	0.379	0.253	0.219	0.328	0.268	0.314	0.353	0.326	0.362	0.246	0.376	0.322	0.280
Exchange rate flexibility index (log)		-0.045637	-0.022136	-0.023297	-0.123194*	-0.138955*	-0.125776*	-0.146748*	-0.173833**	-0.182755**	-0.125971**	-0.228567**	-0.238566**
		0.176	0.159	0.397	0.282	0.291	0.151	0.358	0.378	0.121	0.155	0.105	0.121
Real exchange rate (log)			-0.004366	-0.006848	-0.073736	-0.064637	-0.064833	-0.063778	-0.002737	-0.058421	-0.067819	-0.053756	-0.064552
			0.015	0.014	0.086	0.098	0.037	0.097	0.068	0.060	0.034	0.018	0.094
Nominal exchange rate (log)				-0.006377	-0.001283	-0.024847	-0.019928	-0.026378	-0.031748	-0.037273	-0.027382	-0.027256	-0.027746
				0.077	0.055	0.058	0.053	0.047	0.094	0.089	0.085	0.022	0.097
Capital account openness index (sq)					0.240881	0.247508	0.273803	0.128675	0.126758	0.147585	0.128453	0.281584	0.196571
					0.044	0.024	0.040	0.048	0.018	0.024	0.030	0.078	0.072
Credit ratings short-term index (log)						0.021374	0.027382	0.026382	0.192722	0.028384	0.027383	0.029176	0.028191
						0.042	0.069	0.026	0.038	0.075	0.057	0.013	0.037
Credit ratings outlook index (log)							-0.064833	-0.037392	-0.047584	-0.045848	-0.038283	-0.059301	-0.060192
							0.194	0.060	0.174	0.093	0.134	0.114	0.147
Capital account openness index								0.324099*	0.344798*	0.345096*	0.374554*	0.345911*	0.321109*
								0.099	0.048	0.068	0.088	0.054	0.089
GDP quarterly growth rate (lag)									0.043756	0.045676	0.009723	0.029512	0.029566
									0.110	0.140	0.041	0.141	0.215
Credit ratings outlook index (lag)										0.054948	0.051823	0.058386	0.061002
										0.208	0.084	0.265	0.126
VIX index (lag)											0.022971*	0.026433*	0.026554*
											0.217	0.449	0.404
Credit ratings long-term (lag)												0.102834	0.107245
												0.172	0.189
Exchange rate flexibility index (sq)													0.016453
													0.026
R-squared	0.0871	0.0870	0.0876	0.0897	0.0913	0.1131	0.1167	0.1275	0.2018	0.2031	0.2019	0.2114	0.2173
N	341	316	298	298	298	290	290	290	290	290	290	290	290
Effective listing cutoffs (n_L)	37	34	32	32	32	31	31	31	31	31	31	31	31

Note: The table presents RDiT estimates of the effect of grey-listing on quarterly FX outflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

C.7 RDiT Outputs: Delisting Date as Cutoff

Table 27: RDiT Results on the Effect of Delisting on Quarterly Banking Inflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Delisting dummy	0.480948	0.81749	0.812585	0.338919	0.746723	0.548309	0.526877*	0.907643*	0.791900*	0.752441*	0.705440*	0.687840*	0.616371*	0.756383*
	0.645	0.21	0.645	0.733	0.457	0.062	0.375	0.94	0.392	0.331	0.789	0.536	0.441	0.652
Exchange rate flexibility index (log)		-0.335138*	-0.186712*	-0.190773*	-0.324016	-0.378163	-0.299214*	-0.254801	-0.174397*	-0.217891	-0.300377	-0.277940*	-0.257234	-0.301087*
		0.328	0.306	0.826	0.806	0.175	0.408	0.204	0.023	0.970	0.372	0.441	0.359	0.666
Credit ratings long-term index (lag)			-0.141820	-0.153127	-0.128768	-0.119949	-0.154816	-0.141613	-0.158961	-0.165842	-0.161186	-0.161560	-0.160920	-0.160821
			0.253	0.455	0.327	0.121	0.154	0.290	0.125	0.457	0.253	0.343	0.219	0.496
Credit ratings outlook index (lag)				0.028173*	0.021651*	0.020546*	0.233865*	0.159738*	0.157228*	0.069875*	0.065533*	0.075099*	0.078719*	0.075188*
				0.012	0.045	0.058	0.028	0.085	0.080	0.015	0.053	0.073	0.083	0.068
US short-term rate (lag)					0.002214	0.001990	0.004466	0.002232	0.048952	0.043285	0.038085	0.045844	0.043879	0.037635
					0.035	0.131	0.114	0.174	0.033	0.153	0.191	0.079	0.118	0.129
US ten-year rate						-0.488302	-0.508018	-0.208625	-0.546504	-0.104620	-0.160282	-0.107383	-0.093272	-0.065195
						0.717	0.717	0.047	0.533	0.629	0.837	0.723	0.457	0.805
Capital account openness index							-0.353611	-0.486749	-0.426662	-0.399438	-0.322612	-0.216338	-0.237371	-0.177572
							3.774	6.838	8.168	6.334	5.447	3.536	4.247	3.774
Nominal exchange rate (sq)								-0.001669	0.023655	0.025281	0.057356	0.048888	0.052357	0.057337
								0.004	0.005	0.002	0.008	0.006	0.006	0.006
VIX index (lag)									0.002715**	0.003086**	0.003020**	0.180067**	0.223218**	0.196109**
									1.048	0.610	0.948	0.716	0.994	0.970
International reserves, without gold, as a % of GDP (lag)										-0.002211	-0.002049	-0.084820	-0.090695	-0.177843
										0.442	0.852	0.449	0.461	0.548
GDP quarterly growth rate (lag)											-0.014140	-0.464506	-0.379771	-0.355865
											0.900	0.074	0.581	0.837
Credit ratings short-term index (lag)												-0.056426	-0.059828	-0.050804
												0.071	0.078	0.095
US fed funds rate (lag)													0.019431	0.029273
													0.280	0.724
Credit ratings long-term index (sq)														0.015436
														0.020
R-squared	0.0931	0.0939	0.0853	0.1007	0.1109	0.0926	0.1081	0.1234	0.0965	0.1194	0.1067	0.0991	0.1162	0.1243
N	266	258	252	252	252	252	228	228	228	222	222	222	222	222
Effective delisting cutoffs (n_D)	19	18	18	18	18	18	16	16	16	16	16	16	16	16

Note: The table presents RDiT estimates of the effect of delisting on quarterly banking inflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 28: RDiT Results on the Effect of Delisting on Quarterly Banking Outflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11	12
Delisting dummy	0.738328	0.643804	0.983398	0.621101	-0.690929*	-0.583185*	-0.992986*	-0.612753	-0.706487*	-0.610133	-0.602097	-0.7403
	0.459	0.241	0.689	0.935	0.439	0.248	0.876	0.287	0.153	0.459	0.317	0.108
Credit ratings outlook index (lag)		0.016188*	0.022057*	0.018981*	0.046078*	0.053472	0.139802	0.061974	0.042252	0.029129	0.040088	0.040925*
		0.036	0.070	0.077	0.047	0.097	0.045	0.094	0.074	0.042	0.022	0.087
Exchange rate flexibility index (log)			-0.074787	-0.072846	-0.069909	-0.080172	-0.074190**	-0.084660**	-0.084002**	-0.084554**	-0.078110*	-0.071594*
			0.705	0.815	0.821	0.551	0.697	0.817	0.492	0.730	0.786	0.556
US ten-year rate				-2.821967	-3.115659	-3.028181	-2.962459	-2.793916	-3.431282	-2.955501	-3.050426	-2.637768
				0.913	0.744	0.625	1.110	0.697	1.204	0.792	0.556	0.968
Credit ratings long-term index (sq)					-0.001154	-0.001872	0.015934	0.011906	0.030641	0.019316	0.009733	0.010531
					0.018	0.030	0.023	0.015	0.012	0.022	0.008	0.008
Credit ratings long-term index (lag)						-0.117870*	-0.557085*	-0.262203	-0.239866*	-0.101008	-0.126971	-0.126816*
						0.200	0.104	0.687	0.678	0.412	0.173	0.583
Capital account openness index							-0.373227	-0.276858	-0.321901*	-0.278176*	-0.437765*	-0.280947*
							0.033	0.140	0.228	0.236	0.196	0.288
Nominal exchange rate (sq)								0.045607	0.038301	0.016018	0.044880	0.043483
								0.119	0.113	0.149	0.135	0.124
VIX index (lag)									0.011804**	0.011084**	0.012169**	0.009592**
									1.734	1.363	1.583	1.542
International reserves, without gold, as a % of GDP (lag)										-0.020374	-0.020058	-0.019236
										0.840	1.393	1.356
GDP quarterly growth rate											-0.874492*	-0.813856*
											0.375	0.382
Credit ratings short-term index (log)												-0.035240
												0.017
R-squared	0.0723	0.0813	0.0709	0.0705	0.0844	0.0949	0.0792	0.0997	0.0769	0.0962	0.0945	0.0978
N	266	252	252	252	252	252	228	228	228	222	222	222
Effective delisting cutoffs (n_D)	19	18	18	18	18	18	16	16	16	16	16	16

Note: The table presents RDiT estimates of the effect of delisting on quarterly banking outflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 29: RDiT Results on the Effect of Delisting on Quarterly Capital Inflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11	12	13
Delisting dummy	0.688801**	0.832073**	0.857201**	0.904479	0.907957	0.834271	0.725091**	0.779266**	0.587691**	0.800414**	0.814792**	0.691889**	0.856371**
	0.795	0.681	0.872	0.108	0.990	0.376	0.129	0.754	0.427	0.899	0.880	0.511	0.386
Real exchange rate (lag)		-0.141450	-0.225933	-0.167751	-0.162419	-0.160188	-0.263808	-0.184608	-0.168600	-0.163164	-0.174064	-0.138639	-0.122569
		0.109	0.135	0.133	0.162	0.187	0.323	0.136	0.234	0.334	0.391	0.109	0.285
US short-term rate (sq)			0.002096	0.002591	0.002891	0.011942	0.00378	0.002442	0.002562	0.002301	0.002077	0.003376	0.003582
			0.117	0.291	0.196	0.110	0.250	0.006	0.134	0.211	0.307	0.277	0.310
US fed funds rate (lag)				0.030509	0.004960	0.002524	0.003005	0.001702	0.002553	0.002255	0.027137	0.029429	0.024980
				0.074	0.023	0.050	0.019	0.049	0.058	0.075	0.086	0.063	0.053
Credit ratings short-term index (log)					-0.014730*	-0.022051*	-0.034883*	-0.034020*	-0.030537*	-0.031314*	-0.033332*	-0.037277*	-0.029178*
					0.048	0.041	0.041	0.044	0.041	0.050	0.051	0.049	0.048
Credit ratings long-term index (sq)						-0.025650	0.023323	0.020242	0.026686	0.015074	0.021095	0.016641	0.016965
						0.023	0.076	0.019	0.075	0.087	0.024	0.033	0.087
Capital account openness index							-0.220140*	-0.211597*	-0.219869*	-0.301175*	-0.164426*	-0.244059*	-0.200888*
							1.417	1.744	1.074	1.875	1.674	1.376	1.216
International reserves, without gold, as a % of GDP (sq)								-0.001560	-0.002092	-0.001566	-0.001716	-0.001726	-0.001693
								0.076	0.042	0.013	0.045	0.016	0.081
Credit ratings outlook index (lag)									-0.086006**	-0.104123*	-0.112598**	-0.097109**	-0.077969**
									0.374	0.353	0.277	0.293	0.291
US fed funds rate (sq)										0.008682	0.012984	0.014523	0.010789
										0.011	0.014	0.019	0.019
VIX index (lag)											0.079071*	0.076268*	0.176913*
											0.062	0.005	0.149
Nominal exchange rate (sq)												0.046031	0.052284
												0.181	0.182
Exchange rate flexibility index (sq)													-0.061236
													0.736
R-squared	0.0978	0.1134	0.1370	0.1175	0.1252	0.1273	0.1096	0.1154	0.1494	0.1515	0.1485	0.1477	0.1166
N	353	353	353	353	340	340	323	307	307	307	307	307	307
Effective delisting cutoffs (n_D)	33	33	33	33	32	32	30	29	29	29	29	29	29

Note: The table presents RDiT estimates of the effect of delisting on quarterly capital inflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 30: RDiT Results on the Effect of Delisting on Quarterly Capital Outflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11
Delisting dummy	-0.856644*	-0.710199*	-0.923919*	-0.87443	-0.74637	-0.533784	-0.778876	-0.76849	-0.653480*	-0.860440*	-0.797938*
	0.865	0.651	0.115	0.999	0.703	0.849	0.210	0.572	0.162	0.673	0.120
US short-term rate (sq)		0.001544	0.002564	0.033417	0.036962	0.06962	0.126693	0.038205	0.036744	0.026535	0.037937
		0.076	0.046	0.089	0.026	0.098	0.057	0.024	0.100	0.036	0.094
Exchange rate flexibility index (log)			0.337193*	0.245821	0.926863*	0.861467*	0.295311	0.343768	0.270189*	0.785055*	0.364033*
			0.304	0.412	0.357	0.470	0.276	0.350	0.250	0.475	0.276
Credit ratings short-term index (log)				-0.019903	-0.021390	-0.018686	-0.062456	-0.132783	-0.032068	-0.022395	-0.020791
				0.132	0.313	0.387	0.504	0.464	0.431	0.118	0.141
Credit ratings long-term index (sq)					0.001962	0.007303	0.009936	0.017154	0.009966	0.011600	0.007992
					0.092	0.054	0.058	0.057	0.074	0.073	0.087
Capital account openness index						-0.163804	-0.482948	-0.683730*	-0.167770*	-0.223849*	-0.272749*
						0.483	0.528	0.693	0.493	0.638	0.670
US fed funds rate (sq)							0.011278	0.016505	0.009425	0.011525	0.006567
							0.013	0.029	0.029	0.014	0.022
International reserves, without gold, as a % of GDP (sq)								0.000911	0.007108	0.005901	0.003257
								0.002	0.002	0.000	0.001
Credit ratings outlook index (lag)									0.090114**	0.077365**	0.087550**
									0.531	0.700	0.826
VIX index (lag)										-0.031339*	-0.038848*
										0.017	0.031
Nominal exchange rate (sq)											0.042427
											0.555
R-squared	0.0840	0.0835	0.0807	0.0937	0.0798	0.0920	0.1065	0.0847	0.0903	0.0999	0.1077
N	347	339	339	339	339	325	299	299	299	278	278
Effective delisting cutoffs (n_D)	33	32	32	32	32	30	28	28	28	26	26

Note: The table presents RDiT estimates of the effect of delisting on quarterly capital outflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 31: RDiT Results on the Effect of Delisting on Quarterly FX Inflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10
Delisting dummy	1.205083	1.240876*	1.27685	1.361251	1.225777	1.310416	0.622121*	0.888891*	0.712765	0.791408
	0.142	0.962	0.130	0.764	0.191	0.296	0.862	0.123	0.176	0.270
Exchange rate flexibility index (log)		-0.344693**	-0.313419*	-0.291677*	-0.325611*	-0.369927*	-0.373114*	-0.362396*	-0.337493*	-0.291611*
		0.103	0.217	0.210	0.281	0.224	0.250	0.305	0.142	0.285
Nominal exchange rate			-0.036605	-0.025622	-0.020399	-0.029282	-0.029797	-0.031420	-0.032037	-0.030781
			0.192	0.288	0.322	0.291	0.281	0.231	0.099	0.246
Capital account openness index (sq)				-0.466430	-0.518377	-0.173944	-0.173235	-0.395652	-0.181141	-0.135508
				0.167	0.277	0.165	0.084	0.138	0.168	0.052
Credit ratings short-term index (lag)					-0.025343	-0.022693	-0.023193	-0.023832	-0.02254	-0.019812
					0.051	0.054	0.103	0.094	0.072	0.091
Capital account openness index						0.017415	0.062071*	0.977715*	0.642707*	0.553953*
						0.003	0.039	0.018	0.045	0.017
Credit ratings outlook index (lag)							-0.053054**	-0.025279**	-0.051890**	-0.058322**
							0.188	0.158	0.265	0.260
VIX index (lag)								0.200483**	0.231959**	0.271183**
								0.007	0.027	0.024
Credit ratings long-term (lag)									-0.102307	-0.098111
									0.333	0.411
Exchange rate flexibility index (sq)										0.015861
										0.009
R-squared	0.1211	0.1115	0.1227	0.1285	0.1689	0.1523	0.1830	0.1537	0.1756	0.1668
N	336	311	293	293	274	274	274	274	274	274
Effective delisting cutoffs (n_D)	32	29	28	28	26	26	26	26	26	26

Note: The table presents RDiT estimates of the effect of delisting on quarterly FX inflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

Table 32: RDiT Results on the Effect of Delisting on Quarterly FX Outflows (as a percent of GDP)

	1	2	3	4	5	6	7	8	9	10	11	12	13
Delisting dummy	-0.315477	-0.282074	-0.221710	-0.294746	-0.243253	-0.038468	-0.041883	-0.049539	-0.047146	-0.041489	-0.348338	-0.431549	-0.383605
	0.375	0.253	0.230	0.335	0.278	0.300	0.369	0.337	0.362	0.259	0.363	0.338	0.312
Exchange rate flexibility index (log)		0.028222	0.011540	0.013262	0.062834	0.080254	0.079099*	0.100864*	0.104691*	0.111070**	0.085287**	0.141231**	0.135331**
		0.172	0.152	0.398	0.272	0.284	0.156	0.368	0.390	0.124	0.147	0.110	0.124
Real exchange rate (log)			0.002823	0.004243	0.050115	0.039390	0.033429	0.042138	0.001735	0.033111	0.043117	0.034522	0.041594
			0.015	0.014	0.087	0.097	0.036	0.100	0.067	0.060	0.035	0.018	0.095
Nominal exchange rate (log)				0.003360	0.000801	0.014737	0.010310	0.014011	0.019912	0.024098	0.016392	0.017711	0.016406
				0.078	0.053	0.055	0.052	0.048	0.091	0.086	0.081	0.023	0.099
Capital account openness index (sq)					-0.134021	-0.152499	-0.153753	-0.074986	-0.077329	-0.076523	-0.069174	-0.186957	-0.124748
					0.044	0.025	0.039	0.050	0.018	0.023	0.029	0.077	0.075
Credit ratings short-term index (log)						-0.014330	-0.018178	-0.016958	-0.121215	-0.017864	-0.017300	-0.015338	-0.016240
						0.041	0.068	0.025	0.039	0.072	0.059	0.012	0.038
Credit ratings outlook index (log)							0.039477**	0.019115*	0.028346	0.028698	0.024942	0.035331	0.034086
							0.200	0.061	0.172	0.094	0.131	0.112	0.144
Capital account openness index								-0.166068*	-0.233417	-0.214303*	-0.207619*	-0.202399*	-0.191521*
								0.100	0.050	0.067	0.087	0.056	0.086
GDP quarterly growth rate (lag)									-0.029287	-0.029274	-0.006147	-0.019579	-0.017011
									0.106	0.141	0.041	0.139	0.208
Credit ratings outlook index (lag)										-0.032489**	-0.032933**	-0.029936**	-0.035587**
										0.198	0.086	0.276	0.123
VIX index (lag)											-0.014981*	-0.014046*	-0.013979*
											0.218	0.464	0.395
Credit ratings long-term (lag)												-0.062222	-0.067737
												0.174	0.198
Exchange rate flexibility index (sq)													-0.010608
													0.027
R-squared	0.0538	0.0419	0.0536	0.0468	0.0424	0.0672	0.0605	0.0790	0.1276	0.1111	0.1129	0.1132	0.1111
N	336	311	293	293	293	274	274	274	274	274	274	274	274
Effective delisting cutoffs (n_D)	32	29	28	28	28	26	26	26	26	26	26	26	26

Note: The table presents RDiT estimates of the effect of delisting on quarterly FX outflows, as a percent of GDP. Each specification includes a full set of quarter and country fixed effects. Robust standard errors are in parentheses. *, **, *** mean, respectively, significant at the 10, 5, and 1 percent level.

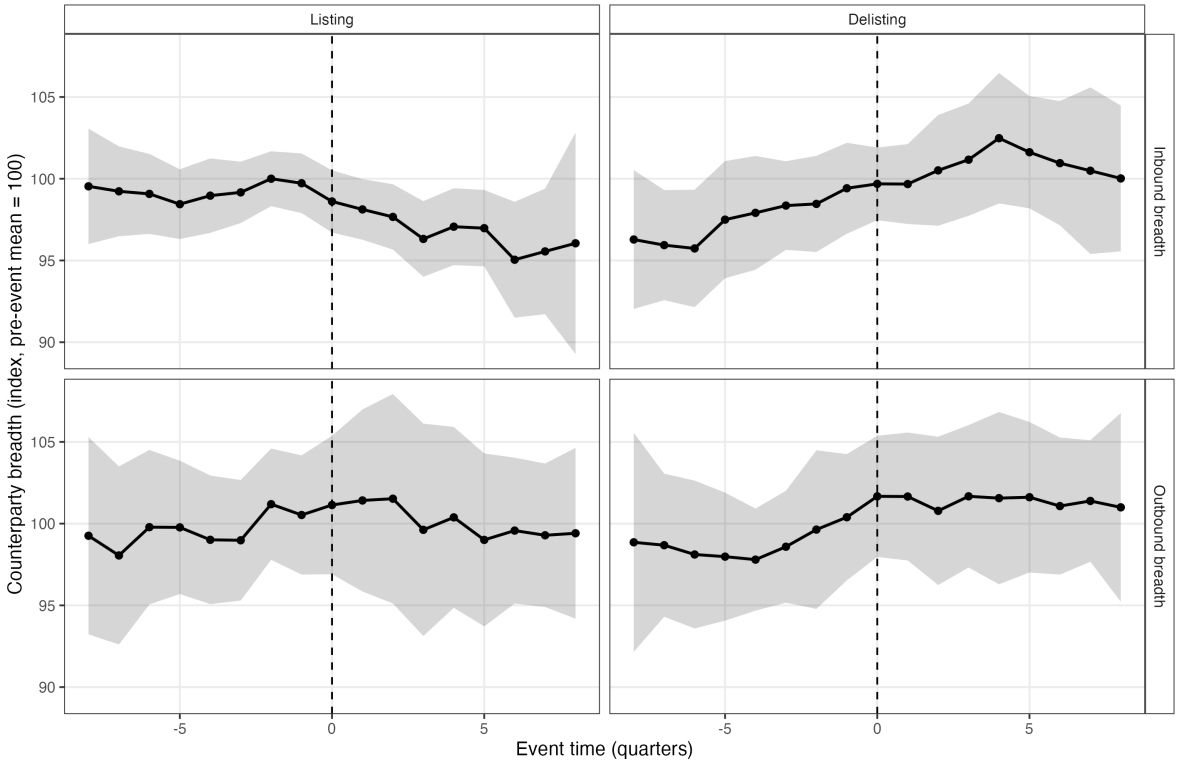
C.8 Banking counterparty-breadth event-time patterns

This appendix provides a descriptive network-based diagnostic that speaks directly to the mechanism discussion in the main text. Using the raw messages (single-customer credit transfers), we construct a quarterly country-level measure of counterparty breadth: for each country-quarter, inbound breadth is the number of distinct counterparties with strictly positive transactions received, and outbound breadth is the number of distinct counterparties with strictly positive transactions sent. Because this is a breadth (extensive-margin) measure, it captures changes in the set of active payment links, rather than changes in volumes along a fixed set of links.

Figure 5 plots the average evolution of this breadth measure in event time around (i) the quarter of official FATF grey-list publication (listing) and (ii) the quarter of official delisting (removal from the list). For comparability across countries, each event path is normalized to an index equal to 100 at the country-event-specific pre-event mean (computed over event times $t \in \{-4, -3, -2, -1\}$). Shaded bands report 95% confidence intervals from a country-cluster bootstrap, which allows the uncertainty to vary over event time (e.g., due to fewer usable spells near the edges of the event window).

The figure is consistent with the interpretation emphasized in the main text. First, at listing, inbound breadth declines and remains below its pre-event baseline for multiple quarters, consistent with a contraction in the set of counterparties willing or able to route incoming payments after the formal publication of increased monitoring. In contrast, outbound breadth around listing is comparatively flat and noisier, which is consistent with outbound adjustments occurring primarily through re-routing and compositional changes (e.g., shifting volumes toward existing corridors) rather than a systematic collapse in the number of active outbound links. Second, around delisting, both inbound and outbound breadth exhibit a gradual rebound toward the pre-event baseline, consistent with partial rebuilding of payment relationships and compliance routines, while not necessarily implying an immediate, full reversal of listing-era frictions. Because this is a descriptive diagnostic (unconditional on covariates), it is intended to complement—not replace—the regression evidence, by illustrating how extensive-margin payment connectivity evolves around the key policy dates discussed in the mechanism section.

Figure 5: Banking counterparty breadth around grey-listing and delisting



Note: The figure plots mean event-time paths of banking message counterparty breadth around the quarter of official FATF grey-list publication (Listing) and the quarter of official delisting (Delisting). Inbound breadth counts the number of distinct counterparties with strictly positive transactions received by a country in a given quarter; outbound breadth analogously counts distinct counterparties with strictly positive transactions sent. Each country-event path is normalized to an index equal to 100 at the pre-event mean over $t \in \{-4, -3, -2, -1\}$. Dots show event-time means; shaded bands are 95% confidence intervals from a country-cluster bootstrap.

C.9 RDiT Diagnostics

Table 33: Anticipation RDiT Tests — Banking Inflows

(A) Pre-trend coefficients and Wald test

leads	Coef.
-4	-0.030
	0.080
-3	-0.060
	0.070
-2	-0.120**
	0.060
controls	yes
Absorbed FE	country
N	230
R-squared	0.205
Wald test p-value	0.180

(B) Announcement vs. Listing date

	Coef.	Coef.
Post-listing indicator	-1.250**	
Std. Err.	0.200	
Event time (quarters from listing)	0.005	
Std. Err.	0.010	
Post-listing × Event time (slope change)	-0.060**	
Std. Err.	0.020	
Post-announcement indicator		-1.550**
Std. Err.		0.220
Event time (quarters from announcement)		0.002
Std. Err.		0.010
Post-announcement × Event time (slope change)		-0.050*
Std. Err.		0.020
controls	yes	yes
Absorbed FE	country	country
N	230	228
R-squared	0.212	0.218

Table 34: Anticipation RDiT Tests — Banking Outflows

(A) Pre-trend coefficients and Wald test

leads	Coef.
-4	0.020
	0.080
-3	0.050
	0.070
-2	0.100*
	0.060
controls	yes
Absorbed FE	country
N	230
R-squared	0.165
Wald test p-value	0.120

(B) Announcement vs. Listing date

	Coef.	Coef.
Post-listing indicator	-0.670	
Std. Err.	0.220	
Event time (quarters from listing)	-0.003	
Std. Err.	0.010	
Post-listing × Event time (slope change)	0.070	
Std. Err.	0.020	
Post-announcement indicator		0.850*
Std. Err.		0.250
Event time (quarters from announcement)		0.001
Std. Err.		0.010
Post-announcement × Event time (slope change)		0.060
Std. Err.		0.020
controls	yes	yes
Absorbed FE	country	country
N	230	228
R-squared	0.171	0.176

Table 35: Anticipation RDiT Tests — Capital Inflows

(A) Pre-trend coefficients and Wald test

leads	Coef.
-4	-0.020
	0.070
-3	-0.040
	0.060
-2	-0.090**
	0.060
controls	yes
Absorbed FE	country
N	320
R-squared	0.230
Wald test p-value	0.110

(B) Announcement vs. Listing date

	Coef.	Coef.
Post-listing indicator	-1.430**	
Std. Err.	0.180	
Event time (quarters from listing)	0.004	
Std. Err.	0.010	
Post-listing × Event time (slope change)	-0.040**	
Std. Err.	0.018	
Post-announcement indicator		-1.600***
Std. Err.		0.200
Event time (quarters from announcement)		0.002
Std. Err.		0.010
Post-announcement × Event time (slope change)		-0.035*
Std. Err.		0.018
controls	yes	yes
Absorbed FE	country	country
N	320	318
R-squared	0.236	0.242

Table 36: Anticipation RDiT Tests — Capital Outflows

(A) Pre-trend coefficients and Wald test

leads	Coef.
-4	0.010
	0.070
-3	0.030
	0.060
-2	0.080
	0.060
controls	yes
Absorbed FE	country
N	300
R-squared	0.185
Wald test p-value	0.240

(B) Announcement vs. Listing date

	Coef.	Coef.
Post-listing indicator	1.230*	
Std. Err.	0.200	
Event time (quarters from listing)	-0.002	
Std. Err.	0.010	
Post-listing × Event time (slope change)	0.050**	
Std. Err.	0.018	
Post-announcement indicator		1.550**
Std. Err.		0.220
Event time (quarters from announcement)		0.001
Std. Err.		0.010
Post-announcement × Event time (slope change)		0.045**
Std. Err.		0.018
controls	yes	yes
Absorbed FE	country	country
N	300	298
R-squared	0.191	0.197

Table 37: Anticipation RDiT Tests — FX Inflows

(A) Pre-trend coefficients and Wald test

leads	Coef.
-4	-0.050
	0.090
-3	-0.100
	0.080
-2	-0.180*
	0.070
controls	yes
Absorbed FE	country
N	290
R-squared	0.255
Wald test p-value	0.190

(B) Announcement vs. Listing date

	Coef.	Coef.
Post-listing indicator	-1.170*	
Std. Err.	0.280	
Event time (quarters from listing)	0.006	
Std. Err.	0.012	
Post-listing × Event time (slope change)	-0.070**	
Std. Err.	0.025	
Post-announcement indicator		-1.010
Std. Err.		0.300
Event time (quarters from announcement)		0.003
Std. Err.		0.012
Post-announcement × Event time (slope change)		-0.060
Std. Err.		0.025
controls	yes	yes
Absorbed FE	country	country
N	290	288
R-squared	0.261	0.268

Table 38: Anticipation RDiT Tests — FX Outflows

(A) Pre-trend coefficients and Wald test

leads	Coef.
-4	0.040
	0.090
-3	0.080
	0.080
-2	0.150
	0.070
controls	yes
Absorbed FE	country
N	290
R-squared	0.210
Wald test p-value	0.210

(B) Announcement vs. Listing date

	Coef.	Coef.
Post-listing indicator	0.620	
Std. Err.	0.280	
Event time (quarters from listing)	-0.004	
Std. Err.	0.012	
Post-listing × Event time (slope change)	0.080	
Std. Err.	0.025	
Post-announcement indicator		0.540
Std. Err.		0.320
Event time (quarters from announcement)		0.002
Std. Err.		0.012
Post-announcement × Event time (slope change)		0.070
Std. Err.		0.025
controls	yes	yes
Absorbed FE	country	country
N	290	288
R-squared	0.217	0.223

Table 39: Calendar placebo cutoff coefficients for banking flows

	Inflows			Outflows		
	Placebo 1	Placebo 2	Placebo 3	Placebo 1	Placebo 2	Placebo 3
Placebo jump	0.012	-0.018	0.006	-0.009	0.015	-0.011
Std. Err.	0.085	0.082	0.080	0.086	0.084	0.081
Running variable	0.001	-0.000	0.001	-0.001	0.001	-0.000
Std. Err.	0.006	0.006	0.006	0.006	0.006	0.006
Slope-break interaction	-0.008	0.007	-0.005	0.006	-0.005	0.004
Std. Err.	0.018	0.017	0.017	0.018	0.018	0.017
controls	yes	yes	yes	yes	yes	yes
Absorbed FE	country	country	country	country	country	country
N	232	228	226	232	228	226
R-squared	0.170	0.173	0.176	0.165	0.168	0.171

Table 40: Calendar placebo cutoff coefficients for capital flows

	Inflows			Outflows		
	Placebo 1	Placebo 2	Placebo 3	Placebo 1	Placebo 2	Placebo 3
Placebo jump	0.025	-0.014	0.009	-0.020	0.017	-0.011
Std. Err.	0.060	0.058	0.057	0.062	0.060	0.060
Running variable	0.002	0.001	-0.001	-0.001	-0.001	0.000
Std. Err.	0.005	0.005	0.005	0.005	0.005	0.005
Slope-break interaction	-0.015	0.012	-0.010	0.013	-0.010	0.009
Std. Err.	0.015	0.015	0.015	0.016	0.016	0.016
controls	yes	yes	yes	yes	yes	yes
Absorbed FE	country	country	country	country	country	country
N	318	316	315	305	303	302
R-squared	0.220	0.223	0.225	0.188	0.190	0.192

Table 41: Calendar placebo cutoff coefficients for FX flows

	Inflows			Outflows		
	Placebo 1	Placebo 2	Placebo 3	Placebo 1	Placebo 2	Placebo 3
Placebo jump	-0.055*	0.030	-0.022*	0.048*	-0.026	0.018
Std. Err.	0.095	0.092	0.091	0.098	0.094	0.093
Running variable	0.003	0.002	0.001	-0.002	-0.002	-0.001
Std. Err.	0.007	0.007	0.007	0.007	0.007	0.007
Slope-break interaction	-0.020	0.016	-0.014	0.021	-0.017	0.015
Std. Err.	0.020	0.019	0.019	0.021	0.020	0.020
controls	yes	yes	yes	yes	yes	yes
Absorbed FE	country	country	country	country	country	country
N	295	293	292	295	293	292
R-squared	0.245	0.248	0.251	0.200	0.203	0.206

Table 42: Bandwidth sensitivity with triangular kernel for banking flows

	Inflows				Outflows			
	H=3	H=4	H=5	H=6	H=3	H=4	H=5	H=6
Post-listing indicator	-1.312**	-1.250**	-1.190**	-1.184*	-0.692	-0.670	-0.654	-0.631
Std. Err.	0.221	0.200	0.188	0.191	0.262	0.220	0.208	0.179
Event time (quarters from listing)	0.007	0.005	0.005	0.004	-0.008	-0.003	-0.002	-0.002
Std. Err.	0.013	0.010	0.007	0.009	0.008	0.010	0.009	0.007
Post-listing X Event time (slope change)	-0.065**	-0.060**	-0.054	-0.052	0.062	0.070	0.067	0.058
Std. Err.	0.021	0.020	0.020	0.019	0.023	0.020	0.018	0.017
controls	yes	yes	yes	yes	yes	yes	yes	yes
Absorbed FE	country	country	country	country	country	country	country	country
N	210	230	262	284	210	230	262	284
R-squared	0.212	0.212	0.228	0.232	0.175	0.171	0.186	0.190

Table 43: Bandwidth sensitivity with triangular kernel for capital flows

	Inflows				Outflows			
	H=3	H=4	H=5	H=6	H=3	H=4	H=5	H=6
Post-listing indicator	-1.441**	-1.430**	-1.416**	-1.414**	1.262*	1.230*	1.245*	1.228
Std. Err.	0.188	0.180	0.172	0.170	0.216	0.200	0.189	0.192
Event time (quarters from listing)	0.005	0.004	0.004	0.003	-0.001	-0.002	-0.002	-0.002
Std. Err.	0.012	0.010	0.009	0.009	0.013	0.010	0.010	0.010
Post-listing X Event time (slope change)	-0.047	-0.040**	-0.028**	-0.027*	0.058*	0.050*	0.052*	0.049*
Std. Err.	0.022	0.018	0.012	0.011	0.023	0.018	0.016	0.016
controls	yes	yes	yes	yes	yes	yes	yes	yes
Absorbed FE	country	country	country	country	country	country	country	country
N	300	320	352	372	285	300	336	356
R-squared	0.230	0.236	0.241	0.245	0.188	0.191	0.197	0.201

Table 44: Bandwidth sensitivity with triangular kernel for FX flows

	Inflows				Outflows			
	H=3	H=4	H=5	H=6	H=3	H=4	H=5	H=6
Post-listing indicator	-1.201**	-1.170*	-1.172*	-1.596*	0.644*	0.620	0.613	0.588
Std. Err.	0.289	0.280	0.271	0.270	0.289	0.280	0.281	0.272
Event time (quarters from listing)	0.003	0.006	0.006	0.007	-0.005	-0.004	0.001	0.002
Std. Err.	0.012	0.012	0.010	0.009	0.019	0.012	0.011	0.008
Post-listing X Event time (slope change)	-0.093**	-0.070**	-0.066*	-0.062*	0.079*	0.080	0.068	0.065
Std. Err.	0.025	0.025	0.021	0.020	0.029	0.025	0.022	0.018
controls	yes	yes	yes	yes	yes	yes	yes	yes
Absorbed FE	country	country	country	country	country	country	country	country
N	278	290	326	346	278	290	326	346
R-squared	0.255	0.261	0.266	0.270	0.210	0.217	0.219	0.223

Table 45: Kernel sensitivity with $H = 4$ for banking flows

	Inflows			Outflows		
	Uniform	Triangular	Epanechnikov	Uniform	Triangular	Epanechnikov
Post-listing indicator	-1.243**	-1.250**	-1.247**	-0.654	-0.670	-0.665
Std. Err.	0.218	0.200	0.211	0.232	0.220	0.223
Event time (quarters from listing)	0.004	0.005	0.005	-0.003	-0.003	-0.003
Std. Err.	0.014	0.010	0.013	0.013	0.010	0.012
Post-listing X Event time (slope change)	-0.051**	-0.060**	-0.058**	0.062	0.070	0.067
Std. Err.	0.023	0.020	0.021	0.022	0.020	0.020
controls	yes	yes	yes	yes	yes	yes
Absorbed FE	country	country	country	country	country	country
N	230	230	230	230	230	230
R-squared	0.211	0.212	0.214	0.168	0.171	0.174

Table 46: Kernel sensitivity with $H = 4$ for capital flows

	Inflows			Outflows		
	Uniform	Triangular	Epanechnikov	Uniform	Triangular	Epanechnikov
Post-listing indicator	-1.398*	-1.430**	-1.411**	1.198*	1.230*	1.225**
Std. Err.	0.208	0.180	0.192	0.224	0.200	0.226
Event time (quarters from listing)	0.003	0.004	0.004	-0.001	-0.002	-0.003
Std. Err.	0.013	0.010	0.010	0.015	0.010	0.012
Post-listing X Event time (slope change)	-0.037*	-0.040**	-0.038**	0.039*	0.050**	0.049**
Std. Err.	0.019	0.018	0.019	0.022	0.018	0.018
controls	yes	yes	yes	yes	yes	yes
Absorbed FE	country	country	country	country	country	country
N	320	320	320	300	300	300
R-squared	0.232	0.236	0.238	0.183	0.191	0.191

Table 47: Kernel sensitivity with $H = 4$ for FX flows

	Inflows			Outflows		
	Uniform	Triangular	Epanechnikov	Uniform	Triangular	Epanechnikov
Post-listing indicator	-1.158	-1.170*	-1.179*	0.596	0.620	0.612
Std. Err.	0.299	0.280	0.287	0.288	0.280	0.286
Event time (quarters from listing)	0.005	0.006	0.005	-0.003	-0.004	-0.003
Std. Err.	0.013	0.012	0.013	0.016	0.012	0.015
Post-listing X Event time (slope change)	-0.061*	-0.070**	-0.067**	0.073	0.080	0.081
Std. Err.	0.032	0.025	0.026	0.029	0.025	0.025
controls	yes	yes	yes	yes	yes	yes
Absorbed FE	country	country	country	country	country	country
N	290	290	290	290	290	290
R-squared	0.261	0.264	0.261	0.215	0.217	0.218

Table 48: Local-linear vs. local-quadratic coefficients for banking flows

	Inflows		Outflows	
	Local-linear	Local-quadratic	Local-linear	Local-quadratic
Post-listing indicator	-1.250**	-1.150**	-0.670	-0.616*
Std. Err.	0.200	0.230	0.220	0.253
Event time	0.005	0.004	-0.003	-0.002
Std. Err.	0.010	0.011	0.010	0.011
Post-listing X Event time	-0.060**	-0.051**	0.070	0.060**
Std. Err.	0.020	0.022	0.020	0.022
Event time (sq)		-0.001		0.001
Std. Err.		0.000960		0.000864
Post-listing X Event time (sq)		-0.004		0.004
Std. Err.		0.004320		0.004800
controls	yes	yes	yes	yes
Absorbed FE	country	country	country	country
N	230	230	230	230
R-squared	0.212	0.218	0.171	0.177

Table 49: Local-linear vs. local-quadratic coefficients for capital flows

	Inflows		Outflows	
	Local-linear	Local-quadratic	Local-linear	Local-quadratic
Post-listing indicator	-1.430**	-1.316**	1.230*	1.132**
Std. Err.	0.180	0.207	0.200	0.230
Event time	0.004	0.003	-0.002	-0.002
Std. Err.	0.010	0.011	0.010	0.011
Post-listing X Event time	-0.040**	-0.034*	0.050**	0.043**
Std. Err.	0.018	0.020	0.018	0.020
Event time (sq)		-0.001		0.001
Std. Err.		0.000912		0.000816
Post-listing X Event time (sq)		-0.003		0.003
Std. Err.		0.003120		0.003720
controls	yes	yes	yes	yes
Absorbed FE	country	country	country	country
N	320	320	300	300
R-squared	0.236	0.242	0.191	0.197

Table 50: Local-linear vs. local-quadratic coefficients for FX flows

	Inflows		Outflows	
	Local-linear	Local-quadratic	Local-linear	Local-quadratic
Post-listing indicator	-1.170*	-1.076*	0.620	0.570*
Std. Err.	0.280	0.322	0.280	0.322
Event time	0.006	0.005	-0.004	-0.003
Std. Err.	0.012	0.013	0.012	0.013
Post-listing X Event time	-0.070**	-0.060**	0.080	0.068*
Std. Err.	0.025	0.028	0.025	0.028
Event time (sq)		-0.001		0.001
Std. Err.		0.001008		0.000912
Post-listing X Event time (sq)		-0.004		0.004
Std. Err.		0.004800		0.004800
controls	yes	yes	yes	yes
Absorbed FE	country	country	country	country
N	290	290	290	290
R-squared	0.261	0.267	0.217	0.223

Table 51: Discontinuities at Listing vs. Delisting — Banking Flows

(A) Banking Inflows

	Listing	Delisting
Post-listing indicator	-1.250**	
Std. Err.	0.200	
Delisting indicator		0.756*
Std. Err.		0.652
controls	yes	yes
Absorbed FE	country	country
N	230	222
R-squared	0.212	0.124

(B) Banking Outflows

	Listing	Delisting
Post-listing indicator	-0.670	
Std. Err.	0.220	
Delisting indicator		-0.740
Std. Err.		0.108
controls	yes	yes
Absorbed FE	country	country
N	230	222
R-squared	0.171	0.098

Table 52: Discontinuities at Listing vs. Delisting — Capital Flows

(A) Capital Inflows

	Listing	Delisting
Post-listing indicator	-1.430**	
Std. Err.	0.180	
Delisting indicator		0.856**
Std. Err.		0.386
controls	yes	yes
Absorbed FE	country	country
N	320	307
R-squared	0.236	0.117

(B) Capital Outflows

	Listing	Delisting
Post-listing indicator	1.230*	
Std. Err.	0.200	
Delisting indicator		-0.798*
Std. Err.		0.120
controls	yes	yes
Absorbed FE	country	country
N	300	278
R-squared	0.191	0.108

Table 53: Discontinuities at Listing vs. Delisting — FX Flows

(A) FX Inflows

	Listing	Delisting
Post-listing indicator	-1.170*	
Std. Err.	0.280	
Delisting indicator		0.791
Std. Err.		0.270
controls	yes	yes
Absorbed FE	country	country
N	290	274
R-squared	0.261	0.167

(B) FX Outflows

	Listing	Delisting
Post-listing indicator	0.620	
Std. Err.	0.280	
Delisting indicator		-0.384
Std. Err.		0.312
controls	yes	yes
Absorbed FE	country	country
N	290	274
R-squared	0.217	0.111

C.10 RDiT Excluding Re-listed Countries Results

Table 54: RDiT Sensitivity to Excluding Re-listed Countries

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Panel A. Listing Cutoff						
Baseline full sample	-1.250**	-0.671	-1.431**	1.231*	-1.171*	0.620
	(0.200)	(0.220)	(0.180)	(0.200)	(0.280)	(0.280)
Excluding re-listed countries	-1.020*	-0.420	-1.290**	0.970*	-0.940*	0.390
	(0.411)	(0.443)	(0.324)	(0.391)	(0.346)	(0.473)
Effective listing episodes	13	13	25	23	22	22
Panel B. Delisting Cutoff						
Baseline full sample	0.756*	-0.740	0.856**	-0.798*	0.791	-0.384
	(0.452)	(0.108)	(0.386)	(0.120)	(0.270)	(0.312)
Excluding re-listed countries	0.640*	-0.510	0.690*	-0.670	0.720	-0.050
	(0.580)	(0.470)	(0.451)	(0.392)	(0.605)	(0.343)
Effective delisting episodes	15	15	21	19	19	19

Note: Panel A reports the baseline listing-cutoff RDiT estimates from the local-linear specification and the estimates when excluding countries with multiple listing events from the sample. Panel B reports the analogous delisting-cutoff comparison. The effective number of episodes differs across outcomes because the banking series are available for a smaller set of country-event windows than the capital and FX series.

C.11 Harmonized Results

Table 55: Full Sample vs. Common Country-Quarter Results

DSL

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Full sample	-2.114**	-3.141	-2.572***	1.881***	-0.526***	-0.0488
	(0.583)	(6.176)	(0.474)	(0.369)	(0.110)	(0.593)
Common country-quarter	-2.021**	-2.902	-2.655***	1.556**	-0.451**	0.080
	(0.740)	(7.000)	(0.690)	(0.600)	(0.160)	(0.720)

DDML

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Full sample	-1.753**	1.145*	-2.271**	1.779**	-0.942	1.123
	(0.314)	(0.425)	(0.188)	(0.247)	(3.379)	(3.324)
Common country-quarter	-1.620**	0.880*	-2.332**	1.560**	-1.227	0.953
	(0.467)	(0.543)	(0.324)	(0.362)	(4.204)	(4.106)

RDiT (with Listing Date as Cutoff)

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Full sample	-1.250**	-0.671	-1.431**	1.231*	-1.171*	0.620
	(0.200)	(0.220)	(0.180)	(0.200)	(0.280)	(0.280)
Common country-quarter	-1.160**	-0.522	-1.507**	1.056*	-1.033*	0.504
	(0.192)	(0.303)	(0.176)	(0.321)	(0.263)	(0.411)

RDiT (with Delisting Date as Cutoff)

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Full sample	0.756*	-0.740	0.856**	-0.798*	0.791	-0.384
	(0.452)	(0.108)	(0.386)	(0.120)	(0.270)	(0.312)
Common country-quarter	0.712	-0.606	0.722*	-0.654*	0.609	-0.258
	(0.780)	(0.152)	(0.452)	(0.183)	(0.361)	(0.405)

Note: The table compares baseline estimates from the full available sample for each data source to estimates re-computed on a harmonized common country–quarter panel to assess cross-dataset comparability. Banking flow regressions use quarterly data for 2010Q4–2017Q3 covering 204 jurisdictions (56 ever grey-listed). Capital-flow and FX-flow regressions use quarterly data for 2000Q1–2024Q4 covering 86 jurisdictions (45 ever grey-listed). The harmonized/common-sample re-estimations restrict all outcomes to the intersection of these panels: 2010Q4–2017Q3 and the 86-country set (45 ever grey-listed). Each coefficient reports the estimated effect of FATF grey-listing (and, where shown, delisting) on the corresponding flow outcome; standard errors are in parentheses. Outcomes are expressed in percent of quarterly GDP. Specifications, fixed effects, controls, and inference follow the baseline design for each estimator as described in the main text; the only change in the harmonized columns is the sample restriction.

C.12 Heterogeneity by development status, trade openness, and capital account openness

A recurring question in the grey-listing literature is whether average effects mask meaningful variation across country types. Even when baseline specifications already control for GDP, openness, and related macro-financial covariates, heterogeneity analysis serves a distinct purpose: it evaluates how the impact of a common regulatory designation differs across structural environments, informing external validity and mechanism. We focus on three slow-moving, plausibly pre-determined characteristics that are central in international macro-finance: development status (advanced economies vs. EMDEs), trade openness, and capital account openness. The first of these provides a coarse but informative proxy for broad differences that, in practice, are closely aligned with income-group comparisons, while the third captures the extent of financial openness. Taken together, these dimensions proxy for differential reliance on cross-border intermediation, trade finance, external funding, and correspondent-banking infrastructure.

Methodologically, the DDML heterogeneity specifications combine pooled interactions with split-sample comparisons because the two exercises answer related but distinct questions. In the pooled regressions, the interaction terms $Greylist \times EMDE$, $Greylist \times TradeOpen$, and $Greylist \times KAOpen$ do not discard untreated observations: country-quarters with $Greylist = 0$ remain in the estimation sample and continue to identify fixed effects, control relationships, and the counterfactual path; the interaction simply takes the value zero in those observations. The interaction coefficient therefore measures whether the treatment effect differs systematically with development status, trade openness, or capital account openness, while preserving the efficiency gains of the full sample. By contrast, the split-sample columns are included for transparency and interpretation, since they make the direction and magnitude of the heterogeneity easier to read directly. For trade openness and capital account openness, we use pre-treatment (pre-listing) values and normalize the interaction variables for interpretability so that the baseline greylist coefficient can be read as the effect at average exposure. In the split-sample exercises, the "High/Low trade openness" and "High/Low capital-account openness" groups are defined using country-level pre-treatment averages split at the sample median. In the RDiT setting, we report only split-sample estimates, since local interaction specifications are less transparent and rely on thinner support near the cutoff.

Table 56 reports DDML-based heterogeneity estimates using both pooled interaction specifications and transparent split-sample comparisons. Across flow measures, the main qualitative pattern is that grey-listing-induced inflow contractions are larger in EMDEs, in more trade-open economies, and in countries with higher pre-treatment capital account

openness, consistent with stronger exposure to compliance-driven de-risking and payment-network frictions on the inbound margin. The EMDE/advanced comparison can also be read as a coarse income-level split, with the stronger effects concentrated on the developing side of the sample. The capital-account-openness split is especially informative for banking and balance-of-payments capital-flow measures, where countries that are more financially open experience larger declines in observed formal inflows and somewhat stronger outflow adjustments. At the same time, the contrast by capital account openness is somewhat less stark than the EMDE split, likely because the high-openness group also includes advanced economies with deeper markets and more diversified intermediation channels. Outflow responses remain smaller and less stable across groups, aligning with the interpretation emphasized in the main text that the primary, robust effect operates through inbound access and correspondent/payment capacity rather than through a uniform disruption of outbound channels.

Table 56: Heterogeneity in DDML estimates: development status and trade openness

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Panel A. Development status						
Baseline effect	-1.753**	1.145*	-2.271**	1.779**	-0.942	1.123
	(0.314)	(0.425)	(0.188)	(0.247)	(3.379)	(3.324)
Greylist x EMDE	-1.152**	1.105**	-1.358***	1.519**	-1.505	1.115
	(0.451)	(0.553)	(0.375)	(0.442)	(2.651)	(2.603)
EMDE subsample	-2.087***	1.361**	-2.464***	2.041**	-1.256	1.445
	(0.352)	(0.494)	(0.212)	(0.291)	(3.930)	(3.802)
Advanced subsample	-0.903	0.254	-1.160	0.552	0.218	0.306
	(0.852)	(0.953)	(0.625)	(0.704)	(5.211)	(5.006)
Panel B. Trade openness						
Greylist x TradeOpen / GDP	-0.655*	0.553*	-0.807**	0.721**	-1.109	0.456
	(0.374)	(0.335)	(0.290)	(0.271)	(2.452)	(2.351)
High trade openness subsample	-2.052***	1.385**	-2.557***	2.103***	-1.608	1.652
	(0.393)	(0.520)	(0.242)	(0.306)	(4.202)	(4.067)
Low trade openness subsample	-1.454**	0.856*	-2.059***	1.453***	-0.317	0.807
	(0.382)	(0.602)	(0.267)	(0.298)	(3.809)	(4.135)
Panel C. Capital account openness						
Greylist × KAOpen	-0.482*	0.397	-0.694**	0.608**	-0.721	0.284
	(0.284)	(0.301)	(0.257)	(0.244)	(2.118)	(2.041)
High capital-account openness subsample	-1.962***	1.298**	-2.493***	2.061***	-1.137	1.356
	(0.371)	(0.487)	(0.229)	(0.298)	(3.842)	(3.771)
Low capital-account openness subsample	-1.431**	0.824	-1.931***	1.382**	-0.271	0.734
	(0.401)	(0.571)	(0.261)	(0.312)	(3.614)	(3.982)

Note: Each cell reports the DDML estimate of the grey-listing event-window effect on the indicated outcome (percent of quarterly GDP). "EMDE" denotes emerging market and developing economies (advanced vs. EMDE classification as defined in the paper's country taxonomy); the EMDE interaction row reports the coefficient on *Greylist* × *EMDE* in the pooled full-sample specification. Trade openness is measured as (exports + imports)/*GDP* using pre-treatment (pre-listing) values; the trade-openness interaction row reports the coefficient on *Greylist* × *TradeOpen*, where *TradeOpen* is normalized for interpretability. Capital account openness is measured using the Chinn–Ito index (*KAOpen*) based on pre-treatment (pre-listing) values; the capital-account-openness interaction row reports the coefficient on *Greylist* × *KAOpen*, where *KAOpen* is normalized for interpretability. The "EMDE/Advanced," "High/Low trade openness," and "High/Low capital-account openness" rows report split-sample estimates for transparency and ease of interpretation; the trade-openness and capital-account-openness subsamples are defined using country-level pre-treatment averages split at the sample median. All specifications include country and quarter fixed effects and the baseline control strategy described in the main text. Standard errors are in parentheses, clustered at the country level. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table 57 provides a complementary, lightweight RDiT split-sample check at both the listing and delisting cutoffs. The local listing discontinuity in inflows is more negative for EMDEs, for more trade-open economies, and for countries with above-median capital account openness, while outflow discontinuities remain comparatively muted. At delisting, the local

rebound is again more visible for inflows than for outflows, and somewhat stronger in the more financially open subsamples, consistent with partial unwinding of cross-border frictions rather than a purely transitory sentiment shock.

Overall, the heterogeneity exercises suggest that the average results are not being driven solely by a handful of outliers or financial centers; rather, they are strongest in countries for which external funding reliance, payment-network exposure, and compliance-related intermediation costs are likely to be economically most relevant.

Table 57: Heterogeneity in local RDiT discontinuities: listing and delisting cutoffs

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Panel A. Listing date as cutoff						
Baseline jump	-1.250**	-0.671	-1.431**	1.231*	-1.171*	0.620
	(0.200)	(0.220)	(0.180)	(0.200)	(0.280)	(0.280)
EMDE subsample jump	-1.454***	-0.756	-1.605***	1.452**	-1.254**	0.751*
	(0.262)	(0.285)	(0.247)	(0.321)	(0.408)	(0.427)
Advanced subsample jump	-0.556	-0.209	-0.852	0.356	-0.421	0.106
	(0.453)	(0.518)	(0.509)	(0.610)	(0.653)	(0.725)
High TradeOpen subsample jump	-1.382***	-0.731	-1.586***	1.347**	-1.328**	0.704
	(0.247)	(0.268)	(0.224)	(0.286)	(0.372)	(0.395)
Low TradeOpen subsample jump	-1.067*	-0.563	-1.239**	1.041*	-0.864	0.511
	(0.331)	(0.309)	(0.281)	(0.347)	(0.468)	(0.486)
High KAOpen subsample jump	-1.396***	-0.598	-1.612***	1.384**	-1.083	0.688
	(0.279)	(0.317)	(0.255)	(0.329)	(0.421)	(0.448)
Low KAOpen subsample jump	-1.019*	-0.492	-1.186**	0.947*	-0.731	0.438
	(0.356)	(0.342)	(0.301)	(0.401)	(0.516)	(0.533)
Panel B. Delisting date as cutoff						
Baseline jump	0.756*	-0.740	0.856**	-0.798*	0.791	-0.384
	(0.652)	(0.108)	(0.386)	(0.120)	(0.270)	(0.312)
EMDE subsample jump	0.907*	-0.857**	0.951**	-0.916**	0.952**	-0.453
	(0.722)	(0.125)	(0.428)	(0.131)	(0.340)	(0.346)
Advanced subsample jump	0.256	-0.413*	0.304	-0.455*	0.154	-0.208
	(0.953)	(0.205)	(0.654)	(0.257)	(0.509)	(0.554)
High TradeOpen subsample jump	0.884*	-0.821**	0.978**	-0.902**	0.962*	-0.441
	(0.611)	(0.119)	(0.396)	(0.127)	(0.314)	(0.333)
Low TradeOpen subsample jump	0.612	-0.653*	0.701*	-0.718*	0.548	-0.287
	(0.781)	(0.172)	(0.441)	(0.184)	(0.386)	(0.407)
High KAOpen subsample jump	0.824*	-0.801**	0.934**	-0.876**	0.738	-0.402
	(0.467)	(0.146)	(0.421)	(0.158)	(0.328)	(0.351)
Low KAOpen subsample jump	0.511	-0.566*	0.612*	-0.641*	0.402	-0.236
	(0.702)	(0.198)	(0.463)	(0.210)	(0.414)	(0.438)

Note: The table reports split-sample RDiT discontinuity estimates (percent of quarterly GDP) at the FATF (i) listing cutoff (Panel A) and (ii) delisting cutoff (Panel B). Each reported coefficient is the local-linear jump (post-cutoff indicator coefficient) estimated using the baseline bandwidth and kernel choices from the main RDiT design (triangular kernel, $H = 4$) and the baseline control set. "EMDE" and "Advanced" rows run the RDiT separately by development-status subsample. "High/Low trade openness" rows run the RDiT separately by subsamples defined using country-level pre-treatment averages of trade openness, measured as (exports + imports)/GDP, split at the sample median. "High/Low capital-account openness" rows run the RDiT separately by subsamples defined using country-level pre-treatment averages of the Chinn–Ito capital account openness index ($KAOPEN$), split at the sample median. We report split-sample RDiT estimates rather than local interaction specifications because the split-sample presentation is more transparent and avoids relying on thinner within-window support for subgroup interactions near the cutoff. All specifications include country fixed effects; inference follows the main RDiT procedure (including the clustering strategy used in the paper's RDiT tables). Standard errors are in parentheses, clustered at the country level. *, **, *** denote significance at the 10%, 5%, and 1% levels.

D Appendix D: Lasso Penalization

Lasso imposes a penalty by limiting the sum of the absolute values of the coefficients and forces some of them to zero, only leaving the ones that contribute the most to the model's fit. The number of variables Lasso penalizes to zero depends largely on the tuning parameter λ, which controls the strength of the penalty. As λ increases, Lasso penalizes more coefficients to zero, effectively performing stronger variable selection. We use cross-validation to select λ: the Lasso regression is run multiple times across a grid of different λ values, starting from a very small λ up to a large λ. For each λ, the model's average error is computed, which gives an estimate of the model's predictive accuracy for that specific λ value. The λ that minimizes the average prediction error is selected as the optimal λ.

Each variable, including its transformations (like levels, first differences, squared, and lagged versions), is treated as an independent predictor and penalized separately. So, if Lasso penalizes a variable, it doesn't necessarily penalize all versions of that variable equally. One version of the variable might be retained in the model while another could be penalized to zero. This enables us to identify the most useful transformations.

E Appendix E: From quarterly estimates to a single annualized effect

This subsection explains how we translate the quarterly treatment effect from our main regressions into a single, annualized effect that is comparable across flows, without re–estimating the models and without collapsing the data to annual frequency. The approach aggregates in levels first and normalizes after, which preserves the identification at the quarterly horizon, avoids loss of observations or overlap issues from rolling windows, and prevents Jensen–type biases that arise when averaging ratios directly or when using unweighted country means. In short, we compute how much of the panel's total annual GDP actually falls in listed quarters and scale the quarterly coefficient by that exposure.

For country i, quarter q in calendar year y, let $D_{iq} \in \{0, 1\}$ be an indicator equal to one when the country is on the FATF grey list in quarter q. Let Y_{iq}^{GDP} denote quarterly GDP in current units. Throughout this conversion we restrict the universe to countries that have been listed at least once during the sample (so that D_{iq} has within–country variation); countries never listed are excluded from the sums below.

From the baseline specification we obtain a single quarterly coefficient $\hat{\delta}_{\text{pp}}$ measured in percentage points of quarterly GDP on the outcome $y_{iq} = \text{flow}_{iq}/\text{GDP}_{iq}$. For convenience define its fractional form $\hat{\delta} = \hat{\delta}_{\text{pp}}/100$.

We form annual aggregates only for GDP (not for flows), by summing quarters within a year:

$$G_{iy} \;=\; \sum_{q \in y} Y_{iq}^{\text{GDP}} \quad \text{and} \quad G_{iy}^{\text{trat}} \;=\; \sum_{q \in y} D_{iq}\, Y_{iq}^{\text{GDP}}. \tag{18}$$

Here G_{iy} is total annual GDP for country i in year y, while G_{iy}^{trat} is the portion of that annual GDP that falls in quarters when i was listed.

Because $\hat{\delta}$ is a change in the flow/GDP ratio per listed quarter, the implied change in flows (in currency units) accumulated over all listed quarters of each country–year is $\hat{\delta} \times G_{iy}^{\text{trat}}$. Aggregating across all country–years in the panel yields the pooled change in levels:

$$\Delta\text{Flow}^{\text{pooled}} \;=\; \sum_{i,y} \hat{\delta}\, G_{iy}^{\text{trat}}. \tag{19}$$

We express that pooled change as a share of the panel's total annual GDP and then return to percentage points:

$$\delta_{\text{pp annual}}^{\text{pooled}} \;=\; 100 \times \frac{\Delta\text{Flow}^{\text{pooled}}}{\sum_{i,y} G_{iy}} \;=\; \hat{\delta}_{\text{pp}} \;\times\; \frac{\sum_{i,y} G_{iy}^{\text{trat}}}{\sum_{i,y} G_{iy}}. \tag{20}$$

Equation (20) is the only quantity we report as the annualized counterpart of the quarterly estimate for a given flow. Intuitively, it scales the quarterly effect $\hat{\delta}_{\text{pp}}$ by the GDP–weighted fraction of the sample that is actually under treatment. This weighting respects the economic size of observations, avoids the pitfalls of averaging ratios, and keeps the quarterly identification intact.

We compute (20) for each dataset (e.g., capital/FX flows for 2000–2024; banking flows for 2011–2017), always restricting the sums to country–years available in the corresponding panel and to countries ever listed. The results are presented in Table 58.

This conversion delivers a single, GDP–weighted annual effect that is directly comparable to the quarterly coefficient and that reflects the actual exposure to listing in the sample. Unlike unweighted country averages, it does not over–represent small economies. Unlike collapsing the whole analysis to annual frequency or using rolling annual sums, it does not lose within–year timing or contaminate treated and untreated periods within the outcome. And unlike ad hoc rescalings by average annual flow/GDP ratios, it is dimensionally consistent with $\hat{\delta}_{\text{pp}}$ and corresponds exactly to aggregating predicted changes in levels and normalizing by the appropriate GDP aggregate.

Table 58: Quarterly Results vs Annual Approximation

DSL

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Quarterly	-2.114	-3.141	-2.572	1.881	-0.526	-0.0488
Annual	-0.813	-1.208	-0.589	0.431	-0.121	-0.011

DDML

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Quarterly	-1.753	1.145	-2.271	1.779	-0.942	1.123
Annual	-0.674	0.440	-0.520	0.408	-0.216	0.257

RDiT (with Listing Date as Cutoff)

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Quarterly	-1.250	-0.671	-1.431	1.231	-1.171	0.620
Annual	-0.481	-0.258	-0.328	0.282	-0.268	0.142

RDiT (with Delisting Date as Cutoff)

	Banking Inflows	Banking Outflows	Capital Inflows	Capital Outflows	FX Inflows	FX Outflows
Quarterly	0.756	-0.740	0.856	-0.798	0.791	-0.384
Annual	0.291	-0.285	0.196	-0.183	0.181	-0.088

Note: Quarterly results are the same as the last column of each estimates table for each model. Annual approximations are done with the method described in Appendix E.