WP/17/74

IMF Working Paper

The Effects of Data Transparency Policy Reforms on Emerging Market Sovereign Bond Spreads

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INTERNATIONAL MONETARY FUND

 WP/17/74

IMF Working Paper

Statistics Department

The Effects of Data Transparency Policy Reforms on Emerging Market Sovereign Bond Spreads[1]

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March 2017

Abstract

We find that data transparency policy reforms, reflected in subscriptions to the IMF's Data Standards Initiatives (SDDS and GDDS), reduce the spreads of emerging market sovereign bonds. To overcome endogeneity issues regarding a country's decision to adopt such reforms, we first show that the reform decision is largely independent of its macroeconomic development. By using an event study, we find that subscriptions to the SDDS or GDDS leads to a 15 percent reduction in the spreads one year following such reforms. This finding is robust to various sensitivity tests, including careful consideration of the interdependence among the structural reforms.

JEL Classification Numbers: F30, G10, G20

Keywords: data transparency, structural reforms, sovereign bond spreads, GDDS, SDDS, event study.

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[1] We are deeply indebted to Luca Errico for helping initiate this project and to Patrizia Tumbarello for her continued support. We are thankful to Greta Butaviciute for excellent research assistance. We are particularly grateful to Jaebin Ahn, Serkan Arslanalp, Romain Duval, Davide Furceri, and Yizhi Xu for their suggestions. We have also benefited from comments by Laurence Allain, Apostolos Aposolou, Zdravko Balyozov, Joe Crowley, Louis Marc Ducharme, Romain Duval, Thomas Elkjaer, Niels-Jakob Hansen, Guohua Huang, Yi Huang, Ramon Hurtado, Andrew Kitili, Umang Rawat, Louis Venter, Mark Van Wersch, Jeffrey Williams, Yingbin Xiao, and seminar participants in the IMF Jobs, Growth, Structural Reforms Seminar and the IMF Statistics Department Brownbag Seminar. Both authors worked on this project while working in the Strategy, Standards, and Review Division of the Statistics Department.

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"Nothing would help improve standards more than if countries that met higher standards were rewarded with lower borrowing costs… If this awareness translates into lower spreads for those meeting higher standards, the standards initiative will begin to pay off both for individual countries and for the system as a whole."

Stanley Fischer (2003)

I. INTRODUCTION

Do structural reforms to improve data transparency pay off? As Fischer (2003) emphasizes, the international financial system can reach an equilibrium with greater stability and resilience if the market rewards country efforts to improve data dissemination practices with a lower risk premium. We assess whether data transparency reforms pay off by gauging their impact—identified by IMF Data Standards Initiatives such as the Special Data Dissemination Standard (SDDS) and the General Data Dissemination System (GDDS)—on sovereign bond spreads in emerging economies; we use an event study to mitigate potential endogeneity issues.

Each past episode of financial turmoil has demonstrated that international rescue packages for crisis-hit countries have been compromised by data deficiencies. Specifically, these may have delayed preventive or corrective actions by the authorities and the international community that could have moderated the economic consequences of the events. These suspicions prompted an international effort to organize a working group to study data deficiencies. The result was the IMF's launch of the Data Standards Initiatives—the SDDS, established in April in 1996 in response to the Mexican financial crisis, and the GDDS, introduced the following year amid the Asian financial crises.[2]

More recently, in May 2015, the IMF introduced the Enhanced General Data Dissemination System (e-GDDS) as a part of its data standards initiatives aimed at promoting data transparency globally.[3] The financial crisis of recent years that affected both advanced and emerging/low-income economies revealed the complexity and integration of the world economy, especially its financial markets. As the G-20 Data Gaps Initiative emphasizes, this resulted in another layer of the data standards initiatives in 2012, the Special Data Dissemination Standard Plus, which aims to measure systemic risk and financial interconnectedness. Moreover, given that structural reforms have become central to the IMF's

[2] For example, in response to issues concerning reserve data, SDDS-subscribing countries are required to provide the Data Template on International Reserves and Foreign Currency Liquidity, which allows for distinguishing interest income and valuation changes in the stock of official reserves from the actively managed component of reserves (Dominguez and others 2012).

[3] All GDDS participation events in this study occurred before the introduction of the e-GDDS, so we refer to them as the GDDS.

mandate (IMF 2016b), prioritizing reforms in the data transparency area can be further leveraged to other areas of macro-structural issues.

The IMF's data standard initiatives have functioned as an information source to the international community, as intended. Improving data dissemination practices are expected to increase transparency about the status of participating economies,[4] thereby mitigating perceived risk by international investors.[5] The 2008 global financial crisis, which created heightened uncertainty about the actual condition of the global economy, has revived interest in understanding the link between data transparency and access to international financial markets (e.g., Moretti 2012, Marques and others 2013, Hashimoto and Wacker 2016). In particular, Marques and others (2013) find that a country with a higher level of transparency fared better during the global financial crisis.

Empirical evidence shows that these initiatives eventually helped improve countries' access to global capital markets by producing more transparent and reliable macroeconomic and financial data. Closely related to the work of the IMF, some studies used the adoption of the IMF data standards initiatives, the publication of the IMF Article IV Staff reports, or the Reports on the Observance of Standards and Codes as a proxy for data transparency and analyzed their effects on various financial variables.[6] These studies assessed the effect of improved data transparency on sovereign borrowing costs (Glennerster and Shin 2003, Cady 2005, Cady and Pellechio 2006, Moretti 2012), foreign exchange spreads (Tiffin and others 2003), exchange rate volatility (Cady and Gonzalez-Garcia 2007), international capital flows (Hashimoto and Wacker 2016), and so on.

[4] Mrkaic (2010) showed that the data transparency initiatives substantially improved the statistical quality of the IMF's World Economic Outlook Forecasts, thereby contributing to effective policy discussions. Tapsoba and others (2016) also found that improved statistical capacity attributable to IMF technical assistance helps reduce fiscal policy pro-cyclicality.

[5] More transparency does not necessarily translate into financial market stability as it may encourage herding behaviors (Furman and others 1998, Morris and Shin 2002, Walsh 2007). Moreover, Tong (2007) finds that public disclosure crowds out private investment in information by analyzing stock market analysts' forecasts in developing economies. Such theoretical ambiguity leads us to empirically test the link between data transparency and economic outcomes using exogenous events.

[6] Extensive studies have looked at the role of information frictions in explaining anomalies in international capital markets, such as a home bias puzzle (French and Poterba 1991, Tesar and Werner 1995, Portes and others 2001, Milesi-Ferretti and Lane 2004, Daude and Fratzscher 2008). These studies used bilateral telephone call traffic (Portes and others 2001), periodicals' trade, and the stock of immigrants from the source country in the host country (Daude and Fratzscher 2008) as a proxy for the degree of informational friction. A broader concept of transparency—the antonym of poor governance, political corruption, or fiscal opacity—is also known to attract investment, reduce capital flow volatility, and decrease sovereign risk premia (Goldstein 1998, Johnson and others 2000, Wei and Yu 2002, Gai 2003, Frenkel and Menkhoff 2004, Bernoth and Wolff 2008). We will not summarize the literature regarding the broader concept of transparency and economic performance. Rather, we limit the scope of transparency and focus exclusively on policy reforms to improve data transparency.

While these studies generally find that improvements in data transparency in line with IMF recommendations reduced sovereign borrowing costs, foreign exchange spreads, exchange rate volatility, and increased FDI inflows by a substantial degree, a recent study by de Resende and Loyola (2015) finds that the subscription to the SDDS or GDDS had no significant (or even a negative) impact on the subscribing country's access to international financial markets.[7] De Resende and Loyola (2015) point out that earlier studies of Cady (2005) and Cady and Pellechio (2006) assumed that reforms—such as a country's subscription decision to the SDDS or GDDS—are orthogonal to the state of the subscribing country. If not, a selection bias may invalidate the estimated effect identified in these studies.[8]

Using an event study as an alternative identification strategy to mitigate endogenous issues, we revisit the impact that subscription to the IMF's data standard initiatives had on sovereign bond spreads in a secondary market. We use J.P. Morgan's Emerging Market Bond Index Global (EMBIG) to measure sovereign bond spreads in emerging markets, which serves as a timely risk assessment by international investors. One might believe that from a policy perspective, spreads on new bond issuance at a primary market are more relevant than spreads at a secondary market. However, we take a broader view of a reform impact on sovereign bond spreads, which is likely seen first in the secondary market. As more investors gain confidence in the reform effect, it will eventually feed back into the primary market.

As many countries have adopted the SDDS or GDDS since the studies were conducted in the early 2000s, we believe it is time to assess how well data transparency policy reforms have been perceived by markets as reflected in bond spreads. We first show that subscriptions to the SDDS or GDDS are largely independent of relevant macroeconomic factors. In doing so, we apply a methodology from Gourinchas and Obstfeld (2012) and Catao and Milesi-Ferretti (2014) used to determine precursors of the various types of external crises.

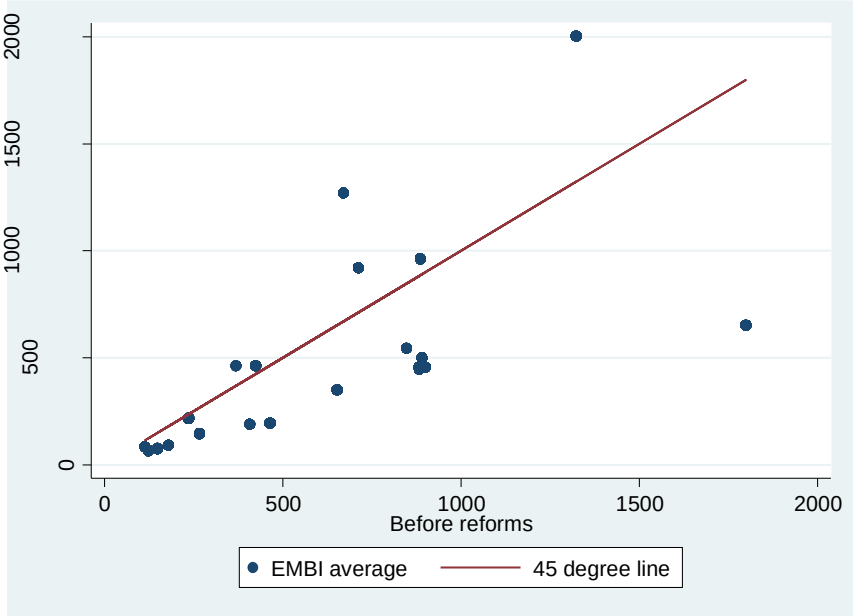
We then show that data transparency policy reforms substantially reduce sovereign bond spreads by using an event study methodology. We also show that our findings are robust to a battery of sensitivity tests. Although our findings relate to the short-run effect of the reforms—we do not try to quantify their long-run effect—our analysis has important implications for the effects of reform on bond issuance. The fact that the reforms reduce the

[7] de Resende and Loyola (2015) replicated the existing studies with updated data and questioned the effectiveness of data transparency policy reforms.

[8] An empirical test of the causal link from data transparency policy reforms to any economic outcome is not trivial owing to apparent reverse causality: a country that performs badly may have a stronger incentive not to disseminate its macroeconomic/financial data to the public. Nevertheless, it is not clear a priori about the direction of the endogeneity bias because it is also possible that these reforms take place during bad times as a part of the IMF-supported program.

spreads in the secondary market suggest a confidence in holding those sovereign bonds and will lead to more demand from international investors at a primary market.

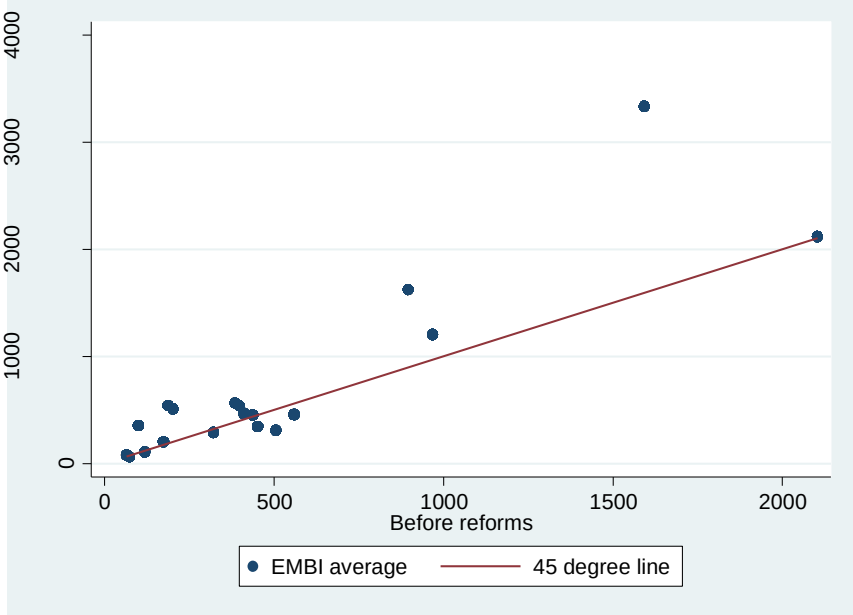
Figure 1. EMBIG Spreads Before and After Data Transparency Policy Reforms



Note: The EMBIG average is measured by the two-quarter average before (x-axis) and after (y-axis) the reform dates.

To obtain an initial view of the data, we plot the EMBIG spreads before the data transparency policy reforms against their levels after the reforms (Figure 1). Each dot in the graph represents the quarterly average over two quarters before and after the reforms. Even using a simple eyeball test, we find that the reforms are associated with a significant reduction in EMBIG spreads for most countries in the sample. A concern that these reforms simply captures a (downward) trend in the EMBIG spreads is mitigated by the observations from a placebo test: We find no systematic decline in EMBIG spreads after artificially moving the event dates one year ahead (Figure 2). Nevertheless, the eyeball test only establishes correlation, not causality, which will be examined in the following sections.

Figure 2. EMBIG Spreads Before and After Data Transparency Policy Reforms: Placebo Test



Note: The EMBIG average is measured by the two-quarter average before (x-axis) and after (y-axis) the false reform dates.

In the next section, we describe event studies and the data used in the paper. We then summarize the main empirical findings, provide sensitivity tests on the main findings and further empirical results, and then conclude.

II. ECONOMETRIC MODELS AND DATA

A. An event study

Two issues must be resolved in order to establish causality from the data transparency policy reforms to sovereign bond spreads (Figure 1). First, favorable (or unfavorable) economic conditions that encourage (or discourage) countries to adopt the IMF Data Standards Initiatives may also at the same time affect sovereign bond spreads (omitted variable bias). Similarly, causality may run from lower sovereign bond spreads through easy access to international financial markets to a country's increased capacity to adopt the IMF data standards initiatives (reverse causality). To address these issues, we first show that there were no particular sign of improvement or deterioration in macroeconomic variables known to determine sovereign bond spreads prior to the reforms.

Using external crises as an event, Gourinchas and Obstfeld (2012) and Catao and Milesi-Ferretti (2014) study a potential precursor of the crisis in both advanced and emerging economies. To the extent to which a crisis is an endogenous event driven by certain

macroeconomic factors, their studies are able to identify a set of such factors. Applying their methodology to the context of data transparency policy reforms, we study whether particular macroeconomic factors helped predict a country's decision to adopt reforms. If none of the macroeconomic variables helps predict the reforms, we may conclude that they are largely orthogonal events to the country's macroeconomic condition.

Formally, we estimate the following panel regression:

$$z_{i,t} = \alpha_i + \eta_t + \sum_{s=-k}^{k} \beta_s \delta_{i,s,t} + \varepsilon_{i,t}, \quad (1)$$

where $z_{i,t}$ is a set of macroeconomic variables; α_i and η_t are country- and time-fixed effects, respectively; $\delta_{i,s,t}$ denotes a dummy variable equal to one when country *i* is *s* periods away from a subscription in period *t*, and $\varepsilon_{i,t}$ is an error term. Therefore, the β_s coefficients capture how proximity to an event changes the behavior of a variable *z* within a *k*-quarter window surrounding the period of the reforms. Because of the first two terms on right-hand side of Equation (1) capture country and time-fixed effects, the β_s coefficients gauge how much an increase/decrease in the variables affect the adoption of reforms relative to the country-specific as well as the global mean. Unlike Gourinchas and Obstfeld (2012), who separately estimated Equation (1) for different types of crises, we pool the two types of reforms (the SDDS and GDDS) to prevent a small sample size from failing to reject any null hypothesis. Throughout the paper, robust standard errors are clustered by countries, following Catao and Milesi-Ferretti (2014).

As a next step, we compare sovereign bond spreads prior to and after the reforms, similar to the approach in de Resende and Loyola (2015). Unlike Equation (1), the following event study compares the prior and post mean of the variable of interest within a symmetric window, thereby elaborating suggestive evidence (shown in Figure 1). While the empirical models in Cady (2005) and Cady and Pellechio (2006) aimed to assess structural breaks in the parameters of interest before and after the reforms, this specification focuses on the reaction of a variable in a time period around a certain reform, thereby disentangling the effect of the reform from other compounding factors.[9] To isolate the effect, this analysis is limited to a sufficiently narrow event window $[t_0 - k, t_0 + k]$ around the time of the reforms, t_0.

[9] However, this event study requires observing the issuance of sovereign bonds before and after the reforms, which is not necessarily the case. This becomes even more unlikely if a shorter window (e.g., one quarter) is used to analyze the effects of the reforms on borrowing costs. To address this issue, we use secondary market spreads (EMBIG) as a measure of sovereign borrowing costs. By definition, EMBIG spreads are available only for emerging markets so we lose some observations from advanced economies. Theoretically, it is unclear whether primary or secondary market spreads are more relevant for our analysis. But Eichengreen and Mody (1998) find a tendency of primary market spreads to follow secondary market spreads with a one-year lag, reinforcing our selection of secondary market spreads for an event study.

Formally, we estimate the following equation:

$$z_{i,t} = \alpha_i + \eta_t + \gamma X_{i,t} + \lambda R_{i,t} + \varepsilon_{i,t}, \quad (2)$$

where $R_{i,t}$ is a dummy variable indicating whether country *i* has received treatment by time *t*, and $X_{i,t}$ is a vector of additional country-level control variables. In terms of global control variables, Gonzalez-Rozada and Yeyati (2008) find that global liquidity and global risk factors together account for more than 30 percent of the variation in sovereign borrowing spreads in emerging market economies. Similar to the empirical specification of Morreti (2012) and de Resende and Loyola (2015), we include the quarterly average of the U.S. federal funds rate and the VIX (Chicago Board Options Exchange Market Volatility Index) to capture global liquidity conditions and global risk, respectively, instead of controlling for a time-fixed effect.

The size of the window in an event study is an open choice. If the window is too short, it may preclude any proper identification of the effect of the event when it takes time to materialize. If the window is too long, other confounding factors would affect the dependent variable in addition to the relevant reforms. Therefore, we use a four-quarter window as a benchmark, considering the possibility that these reforms may be foreseen by market participants to some degree, and employ various event windows (1, 2, and 8 quarters) to check the robustness of the findings.[10]

Before estimating Equation (1), we first estimate the following panel regression using a full sample to study the empirical relationships between various macroeconomic factors and the EMBIG spreads:

$$z_{i,t} = \alpha_i + \eta_t + \gamma X_{i,t} + \varepsilon_{i,t}. \quad (3)$$

Once the conditional means of the macroeconomic variables around the reforms are estimated from Equation (1), the empirical relationships obtained from estimating Equation (3) help clarify the direction of potential bias in the estimates of main interest (λ), which is induced by the endogeneity in reform decisions. In other words, if reforms tend to take place during bad times (e.g., contingent reform plans accompanied by the IMF program following a crisis), it would go against our hypothesis that data transparency policy reforms reduce sovereign bond spreads and vice versa.

[10] If we use the whole sample period instead of the event window, our specification is similar to that of Glennerster and Shin (2003) and Morreti (2012).

B. Data description

We analyze the effect of the data transparency policy reforms on emerging market sovereign bond spreads using data from 52 emerging market economies where the EMBIG is available. The EMBIG tracks the value of country-specific portfolios of the dollar-denominated sovereign or quasi-sovereign debt instruments in secondary markets.[11] We download the EMBIG from Bloomberg at a quarterly frequency; all other macroeconomic variables are taken from Haver Analytics.[12] Unlike Cady and Pellechio (2006) and de Resende and Loyola (2015), we do not use launch yields or launch spreads, as sovereign bond issuances are not necessarily observed around the reforms, which makes an event study particularly unsuitable. Also, we look at responses of investors to reforms whose consequences show up more quickly in the secondary market than in the primary market. We use end-of-quarter daily spreads in the baseline estimation and also the quarterly average of daily spreads to check the robustness of our findings.

As of December 2016, 74 countries were subscribing to the SDDS (including 11 countries subscribing to the SDDS Plus, a platform of the most stringent dissemination standard) and 110 countries were participating in the GDDS.[13] Although a large portion of SDDS subscriptions occurred shortly after the Mexican crisis, we also find many more recent reforms. For the first analysis, we use a full set of data as we are interested in the difference in sovereign bond spreads between the period of reforms and non-reforms. In this case, non-reform events also add information. For the second analysis, we only use up to 26 countries that adopted a reform during the sample period in which the EMBIG spreads are available.

Despite the various approaches to control for potential factors influencing sovereign bond spreads other than the reforms, interdependently-made reform decisions responding to financial crises may undermine our identification strategy. To overcome this problem, we also use changes in the compliance status as an event. One can think of this event as full completion of the reforms, which substantially varies among those who subscribed to the SDDS simultaneously owing to the idiosyncratic nature of actual progress in satisfying the requirements.[14]

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[11] The EMBIG spread for a given country is defined as its EMBIG portfolio yield over a theoretical U.S. zero coupon curve.

[12] See Table A.1 in the appendix for details of the variables used in the paper.

[13] Table A.2 in the appendix summarizes the SDDS and GDDS subscription dates for the sample countries, together with the EMBIG coverage.

[14] For example, five countries (Argentina, Mexico, Philippines, South Africa, and Turkey) in the sample—used to estimate Equation (2)—subscribed to the SDDS at the same time (third quarter of 1996) as a collective response to the Mexican crisis. But each of them switched to the compliance status at different times, thereby mitigating the interdependence issue in reform decisions.

III. EMPIRICAL RESULTS

A. Data transparency policy reform decisions and the macroeconomic conditions

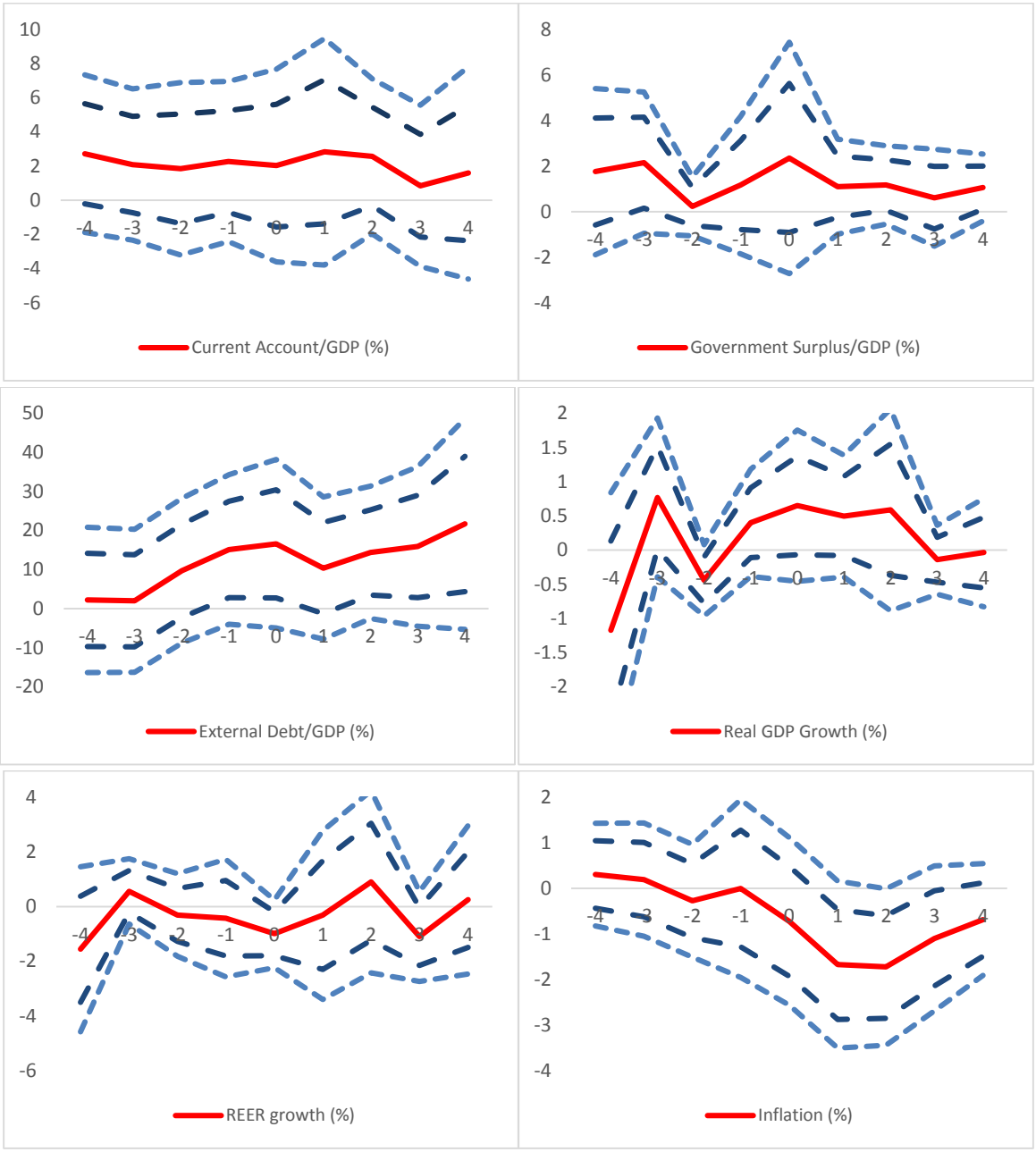
One of the problems of earlier studies is that they assumed that a country's decision to adopt reforms was strictly exogenous. In practice, many kinds of reforms are endogenous to macroeconomic conditions or policies (Drazen and Easterly 2001, Alesina and others 2006, Duval 2008, IMF 2016a, Ranciere and Tornell 2016). If not, a selection bias may invalidate the estimated effect found in these studies. In an attempt to establish causality between reform decisions and sovereign bond spreads, we show that subscriptions to the IMF data standard initiatives are not driven by the macroeconomic factors that simultaneously affect sovereign bond spreads.

We choose six variables known to be determinants of sovereign borrowing costs from the literature (Edwards 1984, Eichengreen and Mody 1998): current-account-to-GDP ratio, external-debt-to-GDP ratio, government-surplus-to-GDP ratio, real GDP growth, the growth rate of the real effective exchange rate, and the inflation rate. (Table 1 shows the results of estimating Equation (3)). We first include each of the macroeconomic variables in turn (Columns I to VI), together with the country- and time-fixed effects, and include them altogether (Column VII). Except for the current-account-to-GDP ratio, all the macroeconomic variables show the expected signs.[15] An improvement in the fiscal balance and an increase in real GDP growth and real exchange rate appreciation tend to narrow sovereign bond spreads, while an accumulation of external debt and an increase in inflation tend to widen the spreads.

We plot the estimates of β_s for each of these variables, together with the 80 percent and 95 percent confidence intervals (Figure 3). None of these macroeconomic variables shows a significant deviation from the pattern found in normal times (i.e., non-reform periods), indicating that reforms were unlikely to have been driven by any particular macroeconomic developments that potentially affect sovereign borrowing costs. However, this is a necessary, not a sufficient, condition of erogeneity.

[15] As we do not have sufficient observations on this variable around the reforms, we do not include it when estimating Equation (2).

Figure 3. Conditional Mean of Selected Variables Around Data Transparency Policy Reforms



Note: The dark (light) dashed lines indicate 80% (95%) confidence intervals using robust country-clustered standard errors.

Moreover, the relatively small sample in this paper, compared with those of Gourinchas and Obstfeld (2012) and Catao and Milesi-Ferretti (2014), may have prevented rejecting any null hypothesis despite the use of 80 percent confidence intervals. The endogeneity concerns are alleviated, however, owing to the randomness of the changes in the six macroeconomic variables before the reforms. While three variables (current-account-to-GDP ratio, external-debt-to-GDP ratio, and REER growth) move toward increasing the spreads, the other three variables (government-surplus-to-GDP ratio, real GDP growth, and the inflation rate) move in the opposite direction.

B. Do data transparency policy reforms reduce sovereign bond spreads?

After establishing exogeneity in decisions to subscribe to the SDDS or GDDS, we now estimate Equation (2) using data from countries that implemented data transparency policy reforms during our sample period. If subscriptions to the IMF data standard initiatives reduce sovereign bond spreads, we would find negative estimates of λ. We choose a four-quarter window as a benchmark, but also use one-, two-, and eight-quarter windows to check the robustness of our findings.

Except for the one-quarter window, we consistently find statistically significant effects of the reforms on sovereign bond spreads. (Table 2 shows the results of estimating Equation (2)). The size of the coefficient in the baseline estimation corresponds to about a 75-basis-point decline from the average of 550 basis points, which is not only statistically, but also economically, significant. In the next section, we check the robustness of our findings by applying a battery of sensitivity tests. For the sake of conciseness, we only report the results for the baseline four-quarter window for most cases.

IV. Robustness Checks and Further Results

A. Global control variables and the treatment of a dependent variable

As a first step in confirming the robustness of our findings, we replace time-fixed effects with our global control variables (U.S. federal funds rate and the VIX). This exercise substantially increases the magnitude of the main coefficient and the statistical significance (Table 3, Column I). This finding might have owed to the inability of the VIX to fully capture a risk factor for emerging economies. We discuss this possibility in Section E. The sign of the coefficients on the federal funds rate may seem odd at first glance, but this is because accommodative U.S. monetary policy is often a systemic policy response to U.S. economic downturns, implying tighter financing conditions for emerging market economies (Cline and Barnes 1997, Kamin and Kleist 1999).[16] The sign of the coefficients on the VIX is

[16] Once we further control for U.S. GDP growth, the statistical significance vanishes, leaving the size of λ virtually unaffected.

straightforward: an increase in global risk aversion or uncertainty is associated with higher borrowing costs from emerging markets through the so-called flight to quality or safety mechanism (Fratzscher 2012, Choi 2016).

We then also replace our dependent variable (daily EMBIG spreads at the end of the quarter) with the quarterly average of daily EMBIG spreads, as some countries in the sample subscribed to the SDDS or GDDS on the last of day of a quarter. This may overestimate the effect of the subscriptions by picking up short-run volatility related to the announcement rather than changes in risk premia induced by the reforms. This change does not affect our findings (Table 3, Column II).

B. Treatment of outliers and overlapping events

The reform effect on spreads may have resulted from few outlier observations, especially owing to the small sample size in an event study. To address this issue, we identify a country that experienced a sharp decline in EMBIG spreads after the subscriptions (Figure 1). Nigeria is clearly such an outlier; the EMBIG spreads narrowed to 800 basis points from about 1,700. It is difficult to attribute such a drastic narrowing in spreads to a data transparency reform alone. Thus, we re-estimate Equation (2) after dropping Nigeria (Column III).

Data transparency policy reforms sometimes occur under IMF-supported arrangements (the IMF program); a country may adopt new IMF data standards initiatives to fulfill the contingent reform plans typically accompanied by the IMF-backed program (Glennerster and Shin 2003, Bernal-Verdugo and others 2013). The IMF-backed program is arguably more important for international financial markets than data transparency policy reforms; an IMF-supported program often signals a country's commitment to implementing structural reforms, thereby masking the true effect of data transparency reforms. The other possibility is that because of market reactions to the macroeconomic conditions that required IMF financial assistance (Reinhart and Trebesch, 2016), implementation of the data transparency reform is not "seemingly" reflected in the spreads.

We identify the start and end dates of historical IMF-supported programs (http://www.imf.org/external/np/fin/tad/extarr1.aspx); we find 10 cases in which data transparency reforms (measured by SDDS subscriptions or GDDS participation) took place within the two-year window of the periods with the IMF-backed programs. We further identify five cases in which data transparency policy reforms took place within the two-year window of the periods of systematic banking crises using a database by Laeven and Valencia (2012). Given the uncertainty of how to control for these overlapping events using our framework, we simply drop all the countries satisfying the two conditions and re-estimate Equation (2)

(Column IV and V each). Dropping the outliers and the overlapping events hardly affects our conclusion (Table 3, Column III-V).

Another potential event driving both EMBIG spreads and decisions to undertake reforms is the adoption of inflation targeting. Inflation targeting typically improves central bank credibility, thus encouraging the authorities to adopt reforms relating to data dissemination and mitigating concerns about inflation financing (Mishkin 2000, Amato and Gerlach 2002). A statistically significant decline in the inflation rate following the reforms (Figure 3) further supports this possibility. But our event study with country-fixed effects only captures variation during the event window and the adoption of inflation targeting is a one-time irreversible event—as is data transparency policy reform. For this reason, we simply drop a country experiencing a change in its inflation targeting regime during the event window. It turns out that Brazil is the only case and deleting it from the sample does not alter our main results.[17]

C. Lagged dependent variable

Given the presence of serial correlation in EMBIG spreads, we include a lag of our dependent variable in Equation (2), despite a potential bias resulting from this inclusion in a dynamic panel model with fixed effects and a short time dimension (Nickell 1981). We do not use the approach proposed by Arellano and Bond (1991) here, owing to the short time-series property of an event study. Therefore, this sensitivity test constitutes only suggestive evidence rather than a rigorous econometric result. Although it becomes difficult to compare the size of the coefficients directly, in this case the effect on sovereign bond spreads is still statistically significant (Table 3, Column VI).

D. Controlling for additional macroeconomic factors

If the timing of the reforms is truly exogenous to a country's concurrent macroeconomic developments, it is not necessary to control for any macroeconomic variables. As Glennerster and Shin (2003) explain in a similar context, "to the extent that transparency leads to better macro policies, adding these variables would bias down our coefficients. However, to the extent that transparency only affects these variables over the medium term, any bias during our short sample would be limited." Clearly, our event study approach further

[17] According to the list of countries under inflation targeting in Hammond (2012), eight countries in our final sample (Brazil, Chile, Colombia, Mexico, Peru, Philippines, South Africa, and Turkey) had adopted inflation targeting by 2012.

lessens this concern. Nevertheless, the inclusion of other macroeconomic variables allows us to obtain more conservative estimates.

As in the first step of the event study, we include the following macroeconomic variables in Equation (2): government-surplus-to-GDP ratio, external-debt-to-GDP ratio, real GDP growth, real effective exchange rate growth, and the inflation rate. Such additional controls go well beyond those in Glennerster and Shin (2003) and Moretti (2012) using a similar approach. The results of the estimation are shown by adding each of macroeconomic variables in turn (Table 4, Columns I-VI) and the results of estimation are shown by including three of these macroeconomic variables simultaneously (Table 4, Column VII).[18] All the statistically significant variables entered into the estimation show a predicted sign and the inclusion of additional macroeconomic variables does not alter our main conclusion.

E. Alternative control variables

A substantially larger effect of the reforms using global controls instead of time-fixed effects may raise a concern that the VIX may not necessarily capture risks specific to emerging markets, thereby exaggerating the quantitative effect of the reforms. To address this issue, we control for the emerging market risk factor, measured by the weighted average of EMBIG spreads across countries in the sample.[19] This is indeed a valid concern (Table 5, Columns I- II): After controlling for the emerging market risk factor, the size of the coefficient is comparable to that of the baselines estimation.[20]

Other than country-specific macroeconomic factors studied above, sovereign ratings by credit agencies are an important determinant of the costs of sovereign debt (Sy 2002, Kaminsky and Schmukler 2002, Mora 2006). To the extent that credit ratings reflect the fundamentals of a given country, additional macroeconomic controls may not be needed. However, our event study with country-fixed effects only captures variation during the event window, and changes in the credit rating are not necessarily observed during the window. Because of this limitation, credit ratings are not used in the baseline estimation, but we still control them as a further robustness check.

To quantify changes in the credit ratings, we assign numerical values to Fitch's letter credit ratings. For letter credit ratings, we create a rating scale from 0 to 19 with an AAA rating

[18] We do not add all variables simultaneously given the constraint of the sample size.

[19] Weights are determined by their GDP in U.S. dollars.

[20] Not surprisingly, the emerging risk factor is far more important than the global risk factor in determining country-level EMBIG spreads. The estimated coefficients suggest that an increase in country-level EMBIG spreads is four-times larger for the same increase in the emerging risk factor than the global risk factor.

taking the highest value and D ("Default") the lowest (Table A.3). (Column III in Table 5 confirms the predicted sign of the credit rating variable.) Nevertheless, the inclusion of the credit ratings does not change our qualitative results, which indicate that data transparency reforms convey information to international investors beyond the rating of international credit agencies.

F. Placebo test: False dates of data transparency policy reforms

We further test the robustness of our findings by conducting a series of placebo tests. We intentionally advance the true reform dates forward and backward respectively. If we still find significant negative effects of data transparency reforms on sovereign bond spreads even with the false dates, our identification strategy would be invalidated. If a window size is k, then we artificially move the event dates $k+1$ quarter forward and backward, so there is no overlap between the new windows and the true reform dates. Looking at the coefficients of false reform dummies and their statistical significance for each of the event windows (Table 6), we find no evidence of the significant effects of the reforms on EMBIG spreads, suggesting that our results are not simply driven by luck.

G. Subscription to vs. compliance with the SDDS

To capture the causal effect of data transparency policy reforms on sovereign bond spreads, we have used SDDS subscription dates. Although subscriptions to a better data dissemination platform imply a country authorities' willingness to provide a high quality macroeconomic data, they do not necessarily guarantee successful reforms. Only after subscribing countries meet the SDDS' requirements is the quality of the data dissemination process truly improved. The IMF Statistics Department regularly monitors compliance with the standards, so we are able to identify the dates of actual compliance with the SDDS.

As a further robustness check, we replace all the SDDS subscription dates with the compliance dates.[21] Subscribers often meet all requirements of the SDDS specifications after subscription (because a new SDDS subscriber is allowed a transitional period given a large workload for the authorities). Moreover, compliance with the SDDS depends on the capacity of the national statistical agencies, whereas subscription owes largely to pressure from the international community, or by a decision of domestic policymakers. Using compliance dates further reduces endogeneity concerns. We still find that compliance with the Data Standards Initiatives is effective in reducing sovereign bond spreads (Table 7). Interestingly, the

[21] We use the same dates for GDDS countries to obtain the comparable estimates from the baseline estimation.

magnitude of the coefficients decreases across every event window, suggesting that the effect of initiating a reform is greater than completing it.

H. Non-linearity in data transparency policy reforms: SDDS vs. GDDS

So far, we have conducted our analysis using a pooled sample of both SDDS and GDDS subscribers—mainly to increase the sample size to minimize a small sample bias. By splitting the sample into SDDS and GDDS subscribers, we gauge the relative effectiveness of the two segments of Data Standards Initiatives on reducing sovereign bond spreads. When a country participates in the GDDS, it must provide a minimal level of data dissemination. Therefore, developing countries or low-income countries typically participate in the GDDS. On the other hand, subscription to the SDDS indicates a strong commitment to providing a comprehensive level of data dissemination; as a result, middle- and high-income countries are major users of this platform.[22]

The effect on EMBIG spreads is larger for the SDDS than the GDDS in every window (Table 8), which is consistent with findings of Cady and Pellechio (2006) using primary bond market spreads. Aside from a small sample bias in the GDDS sample, the relative effectiveness of SDDS subscription indicates the possibility of a non-linear effect of data transparency. Until a country satisfies high standards of data transparency, international investors may be reluctant to change their perception of the country's risk despite its effort to improve data transparency. But the difference may be equally driven by the inherent difference between the two groups of countries. Given only a few cases in which a GDDS-participating country graduates to the SDDS subscription in our sample, we cannot directly differentiate between the two possibilities.

V. Conclusion

We have provided robust empirical evidence that a country's efforts to disseminate data to the public through such IMF data platforms as SDDS and GDDS leads to a 15 percent reduction in sovereign risk premia one year following such reforms. Employing an event study with careful consideration of confounding macroeconomic factors and political events, we mitigate concerns related to the endogeneity in decisions to subscribe to the IMF data standard initiatives. The economically and statistically significant effects on sovereign bond spreads we find support the effort made by the IMF Statistics Department to promote the data standard initiatives and echo Fischer (2003) who emphasized the prominent role of data transparency in building a sound international financial system.

[22] See http://dsbb.imf.org/pages/GDDSDiffSDDS.aspx for a summary of the differences between the SDDS and GDDS.

Table 1. Impact of Macroeconomic Variables on Sovereign Bond Spreads

Dependent variable: Log of the EMBIG spread

	I	II	III	IV	V	VI	VI
Current Account/GDP	0.038						0.019
	0.024						0.011
Government Surplus/GDP		-0.009					-0.002
		0.008					0.005
External Debt/GDP			0.021***				0.023***
			0.002				0.002
Real GDP Growth				-0.037**			-0.025
				0.014			0.019
REER Growth					-0.006		-0.004
					0.004		0.004
Inflation Rate						0.036***	0.004
						0.009	0.009
Obs	1250	1365	1232	1626	1846	2180	630
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Robust country-clustered standard errors are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 2. Impact of Data Transparency Policy Reforms on Sovereign Bond Spreads: Baseline

Dependent variable: Log of the EMBIG spread

	k=4	k=1	k=2	k=8
λ	-0.142**	-0.067	-0.109*	-0.189***
	(0.063)	(0.064)	(0.064)	(0.062)
Obs	182	62	97	336
Time fixed effect	Yes	Yes	Yes	Yes
Country fixed effect	Yes	Yes	Yes	Yes

Note: Robust country-clustered standard errors are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 3. Data Transparency Policy Reforms on Sovereign Bond Spreads: Robustness Checks

Dependent variable: Log of the EMBIG spread (k=4)

	I	II	III	IV	V	VI	VI
λ	-0.424***	-0.110*	-0.130**	-0.306***	-0.161**	-0.122**	-0.128*
	(0.106)	(0.058)	(0.060)	(0.062)	(0.080)	(0.057)	(0.073)
VIX	0.020**						0.015***
	(0.008)						(0.004)
U.S. FFR	-0.077***						0.001
	(0.026)						0.020
lag of the log EMBIG						0.245**	0.680***
						(0.099)	(0.100)
Obs	182	182	173	95	137	181	176
Time fixed effect	No	Yes	Yes	Yes	Yes	Yes	No
Country fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Robust country-clustered standard errors are in parentheses. To save space, we only provide the results from robustness checks for the baseline window (four quarter) here. *, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

Table 4. Impact of data Transparency Policy Reforms on Sovereign Bond Spreads: Controlling for Macroeconomic Factors

Dependent variable: Log of the EMBIG spread (k=4)

	I	II	III	IV	V	VI
λ	-0.118***	-0.214**	-0.205***	-0.132*	-0.164**	-0.199***
	(0.010)	(0.087)	(0.033)	(0.075)	(0.068)	(0.043)
Government Surplus/GDP	-0.002					
	(0.006)					
External Debt/GDP		0.007				
		(0.007)				
Real GDP Growth			-0.004			-0.003
			(0.009)			(0.006)
REER Growth				-0.014***		-0.008**
				(0.004)		(0.004)
Inflation Rate					0.021*	0.031
					(0.011)	(0.028)
Obs	70	63	88	156	156	72
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effect	Yes	Yes	Yes	Yes	Yes	Yes

Note: Robust country-clustered standard errors are in parentheses. Due to data limitation, we only include real GDP growth, REER growth, and the inflation rate in a multivariate regression in Column VI. To save space, we only provide the results from robustness checks for the baseline window (four-quarter) here. *, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

Table 5. Impact of Data Transparency Policy Reforms on Sovereign Bond Spreads: Controlling for Additional Factors

Dependent variable: Log of the EMBIG spread (k=4)

	I	II	III	IV
λ	-0.141*	-0.142*	-0.075	-0.188**
	(0.076)	(0.073)	(0.060)	(0.085)
VIX		0.012*		0.006
		(0.006)		(0.004)
U.S. FFR	-0.028	-0.023		-0.032
	(0.022)	(0.023)		(0.019)
Average EMBIG	0.822***	0.785***		0.622***
	(0.061)	(0.075)		(0.120)
Sovereign rating			-0.084***	-0.092***
			(0.014)	(0.060)
Obs	182	182	108	108
Time fixed effect	No	No	Yes	No
Country fixed effect	Yes	Yes	Yes	Yes

Note: Robust country-clustered standard errors are in parentheses. To save space, we only provide the results from robustness checks for the baseline window (four-quarter) here. *, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

Table 6. Impact of Data Transparency Policy Reforms on Sovereign Bond Spreads: Placebo Test

Dependent variable: Log of the EMBIG spread

	k=4	k=1	k=2	k=8
λ (forward)	0.077	0.181	0.078	-0.050
	(0.123)	(0.371)	(0.192)	(0.175)
Obs	162	57	95	223
λ (backward)	0.082	0.006	0.025	-0.160
	(0.100)	(0.069)	(0.082)	(0.124)
Obs	171	60	99	322
Time fixed effect	Yes	Yes	Yes	Yes
Country fixed effect	Yes	Yes	Yes	Yes

Note: Robust country-clustered standard errors are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

Table 7. Impact of Data Transparency Policy Reforms: Subscriptions vs. Compliance

Dependent variable: Log of the EMBIG spread

	k=4	*k*=1	*k*=2	*k*=8
λ (Compliance)	-0.089**	-0.124	-0.095*	-0.159***
	(0.037)	(0.092)	(0.056)	(0.054)
Obs	247	83	138	455
Time fixed effect	Yes	Yes	Yes	Yes
Country fixed effect	Yes	Yes	Yes	Yes

Note: Robust country-clustered standard errors are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

Table 8. Impact of Data Transparency Policy Reforms: SDDS vs. GDDS

Dependent variable: Log of the EMBIG spread

	k=4	*k*=1	*k*=2	*k*=8
λ (SDDS)	-0.221**	-0.078	-0.257*	-0.281***
	(0.086)	(0.127)	(0.143)	(0.070)
Obs	117	41	62	217
λ (GDDS)	-0.137*	-0.057	-0.057	-0.180
	(0.076)	(0.089)	(0.074)	(0.182)
Obs	65	21	35	119
Time fixed effect	Yes	Yes	Yes	Yes
Country fixed effect	Yes	Yes	Yes	Yes

Note: Robust country-clustered standard errors are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

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Appendix

Table A.1. Data Description

Variable	Definition	Source
EMBIG spreads	J.P. Morgan's Emerging Market Bond Index Global (in basis point)	Bloomberg
Inflation rate	y-o-y growth rate of CPI	IFS, Haver Analytics
GDP growth	y-o-y growth rate of real GDP	IFS, Haver Analytics
REER growth	y-o-y growth rate of real effective exchange rate	BIS, Haver Analytics
Current account to GDP ratio	The ratio of current account to nominal GDP	IFS, Haver Analytics
External debt to GDP ratio	The ratio of external debt to nominal GDP	IFS, Haver Analytics
Government surplus to GDP ratio	The ratio of general government surplus to nominal GDP	IFS, Haver Analytics
Federal Funds rate	The effective Federal Funds rate	Federal Reserve Economic Data
VIX	The CBOE Volatility Index	Chicago Board Options Exchange
Reform dates	SDDS subscription/compliance, GDDS participation	http://dsbb.imf.org/pages/sdds/home.aspx
IMF program dates	Beginning and the ending dates of historical IMF programs	http://www.imf.org/en/data/imf-finances
Banking crisis dates	Systemic banking crises: a new database	Laeven and Valencia (2012)
Sovereign rating history	The history of changes in Fitch's sovereign ratings	https://www.fitchratings.com/site/sovereigns
Inflation targeting adoption dates	The adoption dates of an inflation targeting regime	Hammond (2012)

Note: The unbalanced sample of macroeconomic variables spans from 1994Q1 to 2015Q3.

Table A.2. SDDS Subscription and GDDS Participation Dates, and EMBIG Coverage

Country	Date of SDDS subscription	Date of SDDS compliance	Date of GDDS participation	EMBIG coverage
Argentina*	1996Q3	1999Q4		1994Q1-2015Q3
Belarus	2004Q4	2004Q4		2010Q3-2015Q3
Belize			2006Q3	2007Q1-2015Q3
Bolivia			2000Q4	2012Q4-2015Q3
Brazil*	2001Q1	2001Q1		1994Q2-2015Q3
Chile*	1996Q2	2000Q1		1999Q2-2015Q3
China*	2015Q3	2015Q3	2002Q2	1994Q1-2015Q3
Colombia*	1996Q2	2000Q2		1997Q1-2015Q3
Côte d'Ivoire*			2000Q2	1998Q2-2015Q3
Croatia*	1996Q2	2001Q1		1996Q3-2015Q3
Ecuador*	1998Q1	2000Q3		1995Q1-2015Q3
Egypt*	2005Q1	2005Q1		2001Q3-2015Q3
El Salvador	1998Q2	1999Q4		2002Q2-2015Q3
Gabon			2002Q4	2007Q4-2015Q3
Ghana			2005Q3	2007Q4-2015Q3
Guatemala			2004Q4	2012Q2-2015Q3
Honduras			2005Q3	2013Q2-2015Q3
Hungary*	1996Q2	2000Q1		1999Q1-2015Q3
India	1996Q4	2001Q4		2012Q4-2015Q3
Indonesia	1996Q3	2000Q2		2004Q2-2015Q3
Iraq*			2009Q4	2006Q1-2015Q3
Jamaica			2003Q1	2007Q4-2015Q3
Jordan	2010Q1	2010Q1	2000Q3	2011Q2-2015Q3
Kazakhstan	2003Q1	2003Q1	2001Q1	2007Q2-2015Q3
Latvia	1996Q3	1999Q3		2012Q3-2015Q3
Lebanon*			2003Q1	1998Q2-2015Q3
Lithuania	1996Q2	1999Q3		2009Q4-2015Q3
Malaysia*	1996Q3	2000Q3		1996Q4-2015Q3
Mexico*	1996Q3	2000Q2		1993Q4-2015Q3
Mongolia			2000Q3	2012Q2-2015Q3
Morocco*	2005Q4	20005Q4		1997Q4-2015Q3
Namibia			2002Q4	2011Q4-2015Q3
Nigeria*			2003Q2	1993Q4-2015Q3
Pakistan			2003Q4	2004Q2-2015Q3
Panama*			2000Q4	1996Q3-2015Q3
Paraguay			2001Q3	2013Q1-2015Q3
Peru*	1996Q3	1999Q3		1997Q1-2015Q3
Philippines*	1996Q3	2001Q1		1993Q4-2015Q3
Russia*	2005Q1	2005Q1		1997Q4-2015Q3
Senegal			2001Q3	2011Q2-2015Q3

Slovak Republic	1996Q3	1999Q4		2013Q3-2015Q3
South Africa*	1996Q3	2000Q3		1994Q4-2015Q3
Sri Lanka			2000Q3	2007Q4-2015Q3
Thailand*	1996Q3	2000Q2		1997Q3-2015Q3
Trinidad and Tobago			2004Q3	2007Q2-2015Q3
Tunisia	2001Q2	2001Q2		2002Q2-2015Q3
Turkey*	1996Q3	2001Q2		1996Q1-2015Q3
Ukraine*	2003Q1	2003Q1		2000Q2-2015Q3
Uruguay*	2004Q1	2004Q1		2001Q2-2015Q3
Venezuela*			2001Q1	1993Q4-2015Q3
Vietnam			2003Q3	2005Q4-2015Q3
Zambia			2002Q4	2012Q4-2015Q3

Note: All countries are used in the first event study of estimating Eq. (1). Countries with * are used in the second event study of estimating Eq. (2), because the EMBIG spreads are continuously observed before and after events only for these countries.

Table A.3. Assigning Numerical Values to Fitch's Letter Credit Ratings

Letter rating	Numeric value
AAA	19
AA+	18
AA	17
AA-	16
A+	15
A	14
A-	13
BBB+	12
BBB	11
BBB-	10
BB+	9
BB	8
BB-	7
B+	6
B	5
B-	4
CCC	3
CC	2
C	1
D (Default)	0