Borrowing Costs after Sovereign Debt Relief *

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December 3, 2021

Abstract

Can debt moratoria help countries weather negative shocks? We exploit the Debt Service Suspension Initiative (DSSI) to study the bond market effects of deferring official debt repayments. Using daily data on sovereign bond spreads and synthetic control methods, we show that countries eligible for official debt relief experience a larger decline in borrowing costs compared to similar, ineligible countries. This decline is stronger for countries that receive a larger relief, suggesting that the effect works through liquidity provision. By contrast, the results do not support the concern that official debt relief could generate stigma on financial markets.

JEL Codes: F34, H63, O23

Keywords: Debt relief; Sovereign debt; Developing countries; Sovereign bond spreads; Debt Service Suspension Initiative

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"*The G20 debt relief initiative does not offer optimal benefit [. . .]. We fear we might unnecessarily create a crisis.*"

Ukur Yatani, Treasury Secretary of Kenya (May 15, 2020)

1 Introduction

Unexpected global and domestic shocks can trigger sovereign debt crises (Easterly, 2001; Kaminsky and Vega-Garcia, 2016; Fernández *et al.*, 2017). In early 2020, the Covid-19 pandemic started putting government finances of developing countries under severe strain, increasing the risk of a new wave of debt crises (Arellano *et al.*, 2020). Revenues declined because economic activity contracted and spending rose in order to tackle the pandemic.[1] As financing needs surged, the international community agreed to help low-income countries respond to the crisis by offering a suspension of debt service due in the second half of 2020. We exploit this policy intervention to test how such a simple debt moratorium can help countries weather negative shocks.

The Debt Service Suspension Initiative (DSSI) is a form of debt relief that eases financing constraints through liquidity provision by deferring debt repayments to official creditors without aiming to affect the net present value (NPV) of public debt.[2] The size of the liquidity provision under the DSSI is non-trivial. For all eligible countries it amounts to USD 11.5 billion in 2020 and accounts for more than one fifth of the fiscal shortfall due to the Covid-19 shock.[3] As such, the DSSI is attractive for both creditors and debtors. Official creditors would incur no or minimal losses in NPV-terms and may benefit from better economic conditions and improved debt sustainability in debtor countries. Debtors enhance their liquidity and can protect social and health spending. Nevertheless, many eligible countries have opted not to join the DSSI. This may seem a puzzling response to what at first sight seems to be free money at time of great needs. As indicated by

[1] Economic growth in low-income developing countries was around 0% in real terms in 2020 (down from 5.3% in 2019). The overall fiscal deficit will widen to 5.5 percent of GDP from 3.8 percent in 2019. The public debt-to-GDP ratio is projected to increase by almost 5 percentage points (IMF, 2021b).

[2] The DSSI Term Sheet states that "the suspension of payments will be NPV-neutral" and a joint IMF-World Bank Staff Note specifies that "the 2020 DSSI provides an NPV-neutral debt rescheduling with a one-year grace period and four-year maturity, using the interest rate set in the original loan contract" (p. 32). However, it is plausible to assume that the contractual interest rates were lower than the market interest rates in the distressed market conditions during the spring of 2020. In that case, the standstill would not be NPV-neutral but imply a haircut for the official creditors, whereas private creditors continued to be repaid on time and in full.

[3] The size of the fiscal shortfall for DSSI-eligible countries amounts to about USD 49 billion. We compute this shortfall by comparing the fiscal balance, as projected in the June 2020 IMF World Economic Outlook, with the last pre-pandemic projections released in October 2019.

the initial quote by the Treasury Secretary of Kenya, a key motivation behind the reluctance to participate in the debt relief initiative is the reputational concern that sovereign borrowing costs could increase (IMF, 2020b; The World Bank, 2020).[4]

Our goal in this paper is to empirically test how this moratorium on official debt affected sovereign borrowing costs for poor countries in private bond markets. A theoretical perspective does not unambiguously suggest the direction of the effect. In a frictionless setting, an NPV-neutral debt relief should not affect spreads. Moreover, the lack of spillover effects of *official* debt relief on *private* debt markets could also explain a neutral effect.[5] However, joining a debt moratorium could trigger stigma effects as markets may perceive participation as a signal of previously unobserved debt vulnerabilities. Also, sovereign bond spreads can increase if debt service reprofiling exacerbates debt service burdens in the coming years, implying future debt sustainability problems.[6] But official debt relief could also translate into lower spreads through a liquidity effect. A debt service reduction can free up resources and make debtors better equipped to deal with negative shocks and to promote macroeconomic stability. This can help mitigate the shock's economic consequences and help avoid costly debt restructuring processes. Especially in a context where countries are under pressure to increase spending, liquidity provision can thus reduce the risk of debt distress and restore market confidence.[7] From this perspective, both creditors and debtors can benefit from the debt service suspension. However, differently from the standard concept of mutually beneficial debt relief (see, e.g., Krugman, 1988; Sachs, 1989), the main benefits of the initiative on the creditor side would then not accrue to those creditors that are offering the suspension (the official sector), but to those that did not grant any relief (the private sector). Without private sector participation in the initiative, the debt suspension would then effectively be a subsidy from official creditors to

[4] See also numerous articles in the press, e.g., by The Economist and Reuters.

[5] See Dellas and Niepelt (2016) for a sovereign debt model with private and official creditors.

[6] In fact, the debt sustainability assessment by the IMF and the World Bank depends on breaching debt service (and debt stock) thresholds. A reprofiling of debt service can generate spikes in years ahead leading to threshold breaches and worse risk ratings, with implications for market access and borrowing costs (Lang and Presbitero, 2018). This point is also discussed in the IMF-World Bank Staff Note (Box 2). In the theoretical literature, Yue (2010) shows how debt regenotiations determine recovery rates and Asonuma (2016) explains endogenously a large increase in bond spreads after restructurings with low recovery rates. Fink and Scholl (2016) suggest that bailouts can increase the default probability in the long run. Hatchondo *et al.* (2020) explicitly model a debt standstill on private creditors and find that it results in higher spreads unless combined with a haircut.

[7] Gourinchas *et al.* (2020) and Roch and Uhlig (2018) show, in a monetary union setting, that bailouts can be beneficial for both debtors and creditors. Bianchi *et al.* (2018) show in a quantitative model of sovereign default that accumulating reserves can be optimal for indebted governments.

private ones.[8]

The main empirical challenge to identifying the effect of debt relief on sovereign bond spreads is that the provision of debt relief is usually not random. The case of the DSSI, however, allows us to construct plausible counterfactuals. In contrast to most debt restructurings, the DSSI was announced and implemented simultaneously for all 73 eligible countries and not tailored to the needs of individuals countries.[9] The eligibility criteria were based on preexisting country lists that countries could not select into. This setting allows us to employ a research design based on synthetic control (SC) and difference-in-difference (DiD) methods. Developed by Abadie and Gardeazabal (2003) and Abadie *et al.* (2015), the SC method is now used in the macroeconomic and public finance literature to deal with shocks that affect several countries (Campos *et al.*, 2019; Essers and Ide, 2019; Marchesi and Masi, 2020). In our baseline, we construct a SC for each DSSI-eligible country with daily data on sovereign bond spreads combining countries from the donor pool of ineligible countries. In addition to individually analyzing these country-specific SCs, we also apply synthetic DiD (Arkhangelsky *et al.*, 2021) and the generalized SC method (Xu, 2017) to jointly analyze all treated countries.

Comparing sovereign bond spreads of DSSI-eligible countries with their SCs shows that spreads significantly declined after debt relief was granted, suggesting that official debt relief has a positive spillover effect on private bond markets. The size of this effect differs across countries, but it goes in the same direction for *all* of them. Placebo tests in space and time show that the effect on spreads is due to the DSSI and cannot be explained by contemporaneous IMF programs. A large set of robustness tests show that our finding is not sensitive to changes in the composition of the donor pool and in the set of variables used to construct the synthetic controls. Applying synthetic DiD and the generalized SC method—which both unify SC methods with two-way fixed-effect models—confirms this result, pointing to an average decline of about 200 basis points (bps).

[8]In that sense, the DSSI is related to debt buybacks where official financing is used to pay off bonds held by private creditors (termed a "boondoggle" by Bulow and Rogoff, 1988). The DSSI, however, requires a commitment to use the freed-up resources to tackle the pandemic and its limited size makes it unlikely that countries can use a substantial amount of resources to pay off bonds held by private creditors. While it is thus quantitatively unlikely that such a "boondoggle" will have substantial economic effects in the context of the DSSI, it could still contribute to a negative effect on spreads by improving the borrowers' position to serve the debt.

[9]A similar argument is used, for instance, by Arslanalp and Henry (2005) and Reinhart and Trebesch (2016) to identify the impact of coordinated debt relief under the Baker plan and the Brady Initiative.

Motivated by the sharp deterioration of market liquidity with the unfolding of the Covid-19 crisis, we also test whether the DSSI had any effect on liquidity risk by replicating the baseline analysis and comparing the bid-ask spreads of DSSI-eligible countries with those of their SCs. We do not observe any significant improvement in market liquidity for DSSI-eligible countries compared to their SCs. This result is consistent with the evidence on the primary market, where bond issuances contracted sharply in 2020, when only four DSSI-eligible countries issued bonds after the pandemic hit (The World Bank, 2021).

We then replicate our main findings in a standard DiD setting that traces heterogeneous dynamics of bond spreads after debt relief based on the local projection method (Jordà, 2005). To distinguish between two mechanisms that could drive the main result, we allow for different responses depending on the size of the DSSI relief, the time to maturity of outstanding sovereign bonds, the share of private creditors in debt service, the country's decision to participate in the DSSI and the initial fiscal position. The decline in bond spreads is larger for eligible countries that have a larger share of debt service due in the eligibility period and for those with a shorter debt maturity. By contrast, the decline in spreads does not depend on the amount of repayments due to private creditors. We also find that the effect of the DSSI on spreads is stronger in countries with weaker ex-ante fiscal positions. The absence of spread increases after debt relief—including for countries owing a large share of repayments to private creditors and for those which joined the initiative—does not support the presence of a negative stigma effect that outweighs the positive effects. Instead, results are consistent with a positive liquidity effect resulting from the postponement of debt service.

Our analysis contributes to the empirical literature on the consequences of sovereign debt restructurings (Tomz and Wright, 2013; Reinhart and Trebesch, 2016; Trebesch and Zabel, 2017; Cheng *et al.*, 2019; Asonuma *et al.*, 2021). While most of this literature looks at implications for growth and investment, our work focuses on estimating causal effects on borrowing costs and follows a recent literature that moves beyond treating restructurings as binary treatments (Asonuma and Trebesch, 2016; Trebesch and Zabel, 2017; Meyer *et al.*, 2021). In a seminal paper, Cruces and Trebesch (2013) consider a large sample of sovereign debt restructurings and find haircut sizes to be associated with subsequently higher bonds spreads and longer market exclusion. Our results are consistent with and specify their finding that bond spreads do not increase after debt restructurings with minimal haircuts.[10] We also add to the sovereign debt literature on heterogeneous creditors (Boz, 2011; Dellas and Niepelt, 2016; Corsetti *et al.*, 2018; Marchesi and Masi, 2021, 2020) by looking at the spillover effect of *official* debt restructuring on *private* debt markets.

Furthermore, our paper contributes to the development literature on sovereign debt relief, building on two papers that investigate the reaction of financial markets to debt relief for developing countries. Consistent with the presence of debt overhang, Arslanalp and Henry (2005) document a large appreciation of stock markets in countries that received debt relief under the Brady Plan. Similarly, Raddatz (2011) shows that multinational firms operating in countries that received multilateral debt relief through the Heavily Indebted Poor Countries (HIPC) initiative and the Multilateral Debt Relief Initiative (MDRI) recorded positive abnormal returns in the days after the announcement of debt relief. Compared to these two studies, we look at the direct effect of sovereign borrowing costs and focus on an episode in which only debt service repayments are deferred, suggesting that liquidity support alone can trigger a positive market reaction.

When interpreting our findings, it is important to keep in mind that our sample is limited to frontier markets with limited access to international capital markets. Furthermore, the exogeneity of the policy intervention may mitigate potential stigma effects, given that debt relief was not granted in response to country-specific vulnerabilities. Even considering these caveats, we believe that there are important lessons which can be drawn to design more effective debt restructuring policies, especially for countries relying on official finance. First, our results lend support to the view that early and fast debt restructurings can be beneficial and less costly for creditors and debtors (IMF, 2014; Asonuma and Trebesch, 2016). Second, they suggest that simple state-contingent debt instruments can help countries mitigate their exposure to macroeconomic volatility and to natural disasters.[11] Third, concerns by policymakers that such forms of official debt service suspension will trigger stigma on financial markets are probably misplaced. More generally, our analysis indicates

[10]In Cruces and Trebesch (2013), the adverse effect becomes stronger when haircuts to lenders reach 40% or more. In contrast, the rescheduling from DSSI is unlikely to create haircuts beyond 10%, even when using the same approach of calculating haircuts at market discount rates.

[11]In particular, bonds with a floating grace period (Cohen *et al.*, 2008), which can be triggered by the debtor in case of adverse shocks, could provide flexibility and simplify contract negotiations in response to short-term financial distress. In this context, see also a proposal by Ross and Ulukan (2020). Moreover Gelpern *et al.* (2020) recently argued for the inclusion of standstill clauses in sovereign debt contracts, while Bolton *et al.* (2021) proposed to deal with sovereign debt distress by providing temporary legal protection to debtor countries while they are diverting financial resources toward crisis response policies.

that the rapid provision of short-term liquidity through debt service suspension can be an effective form of financial support to deal with temporary liquidity crises.

2 The Debt Service Suspension Initiative

As the spread of the Covid-19 virus evolved into a global pandemic, concerns about its impact on the world's poorest countries grew. These countries not only have less adequate health infrastructure to deal with the pandemic, but also lack financial resources to mitigate the economic fallout caused by lockdowns, trade disruptions, and falling aggregate demand. In response to these concerns, G-20 leaders committed to do "whatever it takes" to "provide help to all countries in need of assistance." On April 15, a month after the World Health Organization (WHO) had declared Covid-19 a pandemic, the G-20 leaders announced their plan to financially support the world's poorest countries through the DSSI.

The DSSI provides a time-bound suspension of principal and interest payments due by 73 eligible developing countries to bilateral government lenders. It was initially set to last between May 1 and December 31, 2020 and the repayment period for the suspended debt is set to three years, with a one-year grace period.[12] The list of eligible countries was defined to include all countries that are eligible to receive resources from the World Bank's International Development Association (IDA) and all least developed countries under the UN classification, while excluding countries that were in arrears to the World Bank or the IMF (Eritrea, Sudan, Syria, Zimbabwe). In practice, this covers a mix of low and middle-income countries including some that are richer than some excluded countries (e.g., Angola vs. Vietnam).

Eligible countries are also heterogeneous in their access to finance, with many confined to concessional financing, but others issuing bonds on international capital markets. Moreover, eligibility is not conditional on immediate financing needs and debt repayment problems. Half of eligible countries were, according to the IMF and the World Bank low income countries debt sustainability

[12]In October 2020, the G-20 agreed to extend the DSSI by 6 months and then it has been further extended to June 2021 and, on April 7, 2021, the G20 announced a final extension of the DSSI through end-December 2021, also agreed by the Paris Club. In these cases, the repayment period will be five years, with one-year grace period; see http://www.g20.utoronto.ca/2021/210407-finance.html and Appendix A.1 for a timeline. The description of the DSSI draws on the G20 communiqué.

framework, at low or medium risk of debt distress prior to the crisis (IMF-World Bank Staff Note), while other developing countries that were already in debt renegotiations prior to the crisis were not eligible (e.g., Ecuador). Finally, as the design of the DSSI does not take the country-specific severity of the Covid-19 shock into account, the amount of debt service eligible for suspension is orthogonal to key measures of the country-specific shock. We show in Figure A2 that this is true for the intensity of the fiscal shock, the economic shock, and the health shock (as measured by the number of Covid-19 cases and deaths).

The initiative differs from traditional multilateral financing as it comes with few conditions. Unlike IMF programs, there are no structural benchmarks or prior actions that governments need to undertake. Nevertheless, country authorities do have to take some steps. They have to make a formal request, as participation is not automatic. Authorities also have to commit to using the created fiscal space to tackle the crisis and to disclose public debt data. Finally, they have to request IMF financing, including through the emergency facilities with limited conditionality, or already have an existing IMF-supported program in place. As the latter may, in theory, endanger our identification strategy, we discuss this issue in Section 4.1.

While eligibility is based on pre-defined country lists and, thus, has a quasi-exogenous component, the decision to participate in the DSSI is an endogenous policy choice of debtor countries. To look at what drives participation in the DSSI, we consider the entire sample of 73 DSSI-eligible countries—43 of which joined the DSSI by September 2020—and estimate a simple probit model in which the probability of joining the DSSI (as of September 2020) is a function of a set of macroeconomic variables. The results, reported in Table A1, show that lower GDP per capita and higher levels of public debt service are associated with a higher likelihood to participate in the DSSI, suggesting that DSSI participation is driven by economic needs and expected benefits.[13] Hence, our analysis will take DSSI eligibility as the treatment, considering the day of the policy implementation (May 1, 2020), which coincides with the first country (Pakistan) joining the initiative.

[13]Table A2 provides descriptive statistics of country-level macroeconomic indicators for the eligible participants and non-participants in our sample.

3 Data and Stylized Facts

Data. We collect daily data on sovereign bond spreads from January 2, 2019 to August 31, 2020 for all the 68 low- and middle-income countries for which data are available (Table A3). Data on bond spreads are complemented with data on bid-ask spreads and on the time to maturity of outstanding bonds. As most of the DSSI-eligible countries have no regular market access, data on sovereign bonds are available only for 18 countries, which are the focus of our analysis and constitute the group of *treated* countries (Figure A3).[14] The *donor pool* used to construct SCs includes the remaining 52 non-eligible middle-income countries with sovereign bond data. Data on sovereign bonds are retrieved from Bloomberg. We use the EMBIG Index for all countries except Benin and Rwanda, for which we rely on a weighted average spread computed by IMF staff from bond-level data retrieved from Bloomberg.[15] Data on bid-ask spreads and time to maturity are computed as a weighted average of the bond-level data.

We merge the sovereign bond data with macroeconomic and socio-economic indicators (e.g., public debt over GDP, real GDP growth, fiscal and current account balances, life expectancy, education, political stability) from the IMF World Economic Outlook (WEO) and the World Bank World Development Indicators (WDI) and World Governance Indicators (WGI). Data on the size of the DSSI-eligible debt service are drawn from a novel World Bank dataset on monthly debt service due.[16] We also collect data on external public debt from private creditors from the World Bank International Debt Statistics (IDS), which we complement with information collected from national authorities and IMF staff reports for 16 countries not covered by the IDS (see Appendix A.2 for details). Finally, we code the date of each country's participation in the DSSI based on information from the World Bank, the IMF weekly update, and communications with country authorities.

[14]These countries are: Angola, Benin, Cameroon, Cote d'Ivoire, Ethiopia, Ghana, Honduras, Kenya, Mongolia, Mozambique, Nigeria, Pakistan, Papua New Guinea, Rwanda, Senegal, Tajikistan, Uzbekistan and Zambia. Among these countries, Benin, Ghana, Honduras, Kenya, Mongolia, Nigeria, Rwanda and Uzbekistan did not request to join the initiative in the sample period.

[15]The latter source only includes bonds issued under foreign law and denominated in US dollar or Euro. Nevertheless, the series is almost perfectly correlated with the EMBIG Index for countries for which both sources are available.

[16]The data cover 68 of the 73 DSSI-eligible countries (thus excluding Kiribati, Marshall Islands, Micronesia, South Sudan and Tuvalu) and is available at: https://datatopics.worldbank.org/debt/ids/.

Stylized facts. The daily data show that average sovereign bond spreads in the 18 DSSI-eligible countries with access to international capital markets sharply increased when the Covid-19 pandemic broke out in March 2020. Spreads jumped from about 550 bps in late-February to almost 1200 bps in early-April, slightly decreased before the DSSI announcement on April 15, and increased again in the two weeks after that. On May 1, however, when the DSSI officially started with the first countries participating, sovereign bond spreads began a constant, albeit decelerating, downward path over the entire summer (Figure A4).

The decline in sovereign spreads, especially after the first countries joined the DSSI, would suggest that debt relief has not triggered an adverse market reaction, contrary to what the concern regarding reputational costs would predict. However, as shown in Figure A5, sovereign bond spreads were also declining in many ineligible developing countries, pushed by the improvement in the global economic environment (IMF, 2021a). More generally, a simple comparison between eligible and ineligible countries can be biased if they are systematically different and their spreads react differently to global shocks other than the DSSI.

We compare the characteristics of eligible and ineligible countries in Table A4 along multiple dimensions. As a direct consequence of the eligibility rule, eligible countries have, on average, a lower GDP per capita and score lower on some country-level variables typically associated with it (life expectancy, political stability, trade openness, access to private creditors). However, the two groups of countries do not show significant differences when comparing their external debt stock and debt service ratios, current and fiscal accounts, international reserves, foreign direct investment and remittances (all scaled by GDP). In spite of the symmetry between the two groups along many relevant dimensions, the average differences between the two groups along other dimensions call for a more rigorous empirical strategy than a simple comparison between eligible and ineligible countries. In the following, we opt for the synthetic control method, which allows for the construction of plausible counterfactuals for each individual eligible country by selecting the combination of ineligible countries that best fits each eligible country in the pre-treatment period.

4 Empirical Analysis

4.1 Synthetic Controls

In a context in which an intervention affects a small number of units (countries in our case), the use of SC methods can provide advantages over more traditional regression analyses (Abadie, 2021). By taking a synthetic combination of untreated countries (the donor pool), SCs often match the characteristics of treated countries in the pre-intervention period better than single unaffected countries and thereby provide more appropriate counterfactuals. Compared to the DiD design, the SC method does not give all untreated units the same weight in the comparison, but generates a weighted average of the untreated countries in the donor pool (often including weights of zero for many units). Then, the outcomes for the SCs are projected into the post-treatment period using the same weights.

Baseline analysis. In the baseline, we construct separate SCs for each of the treated countries. Following the notation in Abadie (2021), the treated country is the first unit ($c = 1$); the donor pool is the set of potential comparisons, $c = 2, \ldots, C + 1$, i.e., the ineligible countries. Y_{ct} is the outcome variable, in our case bond spreads. The synthetic control estimator of τ_{1t}, i.e., the treatment effect on the treated country ($c = 1$) at time t, is then estimated by

$$\hat{\tau}_{1t} = Y_{1t} - \sum_{c=2}^{C+1} w_c Y_{ct}. \tag{1}$$

Following Abadie and Gardeazabal (2003) and Abadie *et al.* (2010), the weights, $\mathbf{W} = (w_2, \ldots, w_{C+1})'$, are restricted to sum to one and to be nonnegative. They are chosen to minimize

$$\|\mathbf{X_1} - \mathbf{X_0 W}\| = \left(\sum_{h=1}^{k} v_h (X_{h1} - w_2 X_{h2} - \cdots - w_{C+1} X_{hC+1})^2 \right)^{1/2}. \tag{2}$$

Minimizing this expression ensures that the synthetic control best resembles the pre-intervention values of the outcome predictors for the treated unit ($\mathbf{X_1}$). The matrix $\mathbf{X_0} = [\mathbf{X_2} \ldots \mathbf{X_{C+1}}]$ collects the values of the predictors for the untreated units. $\mathbf{X}$ may include pre-intervention values of the outcome, Y. The positive constants, $v_1, \ldots, v_k$, reflect the relative importance of the k predictors

$X_{11}, \ldots, X_{k1}$ for predicting Y_{1t}. See Abadie (2021) for further details.

In our setting, we consider countries with available data on bond spreads and construct individual synthetic controls for all 18 DSSI-eligible countries using all $C = 52$ ineligible countries as the donor pool.[17] The baseline treatment is the official implementation of the DSSI on May 1, 2020.[18] We construct a SC for each treated country using a set of pre-treatment characteristics ($\mathbf{X}$) that include standard macroeconomic variables and the value of sovereign spreads (Y) on specific dates. As macroeconomic variables, we take the 2019 values of real GDP growth, public debt, the fiscal balance, the current account balance, international reserves (all expressed as a share of GDP) and the share of external public debt owed to private creditors (in total public external debt), as they are key determinants of sovereign bond yields. To match countries based on initial market responses to the pandemic shock prior to the start of the DSSI, we match them on pre-treatment spreads using four specific dates that correspond to important events of the Covid-19 pandemic: February 4, March 11, March 26 and April 6.[19] Finally, we take differences in the intensity of the Covid-19 crisis into account by adding the number of cases per million people. To allow for sufficiently long and symmetric pre- and post-intervention periods, our sample starts on January 2 (the first trading day in 2020) and ends on August 31, 2020.

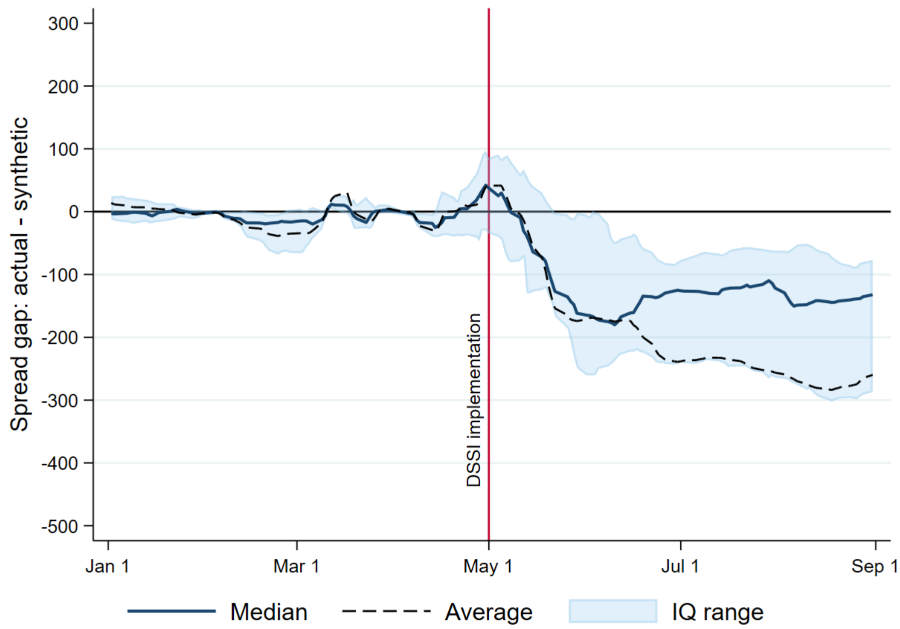
When using the SC method, two main assumptions must hold (see Abadie, 2021). First, *no anticipation*. The variables chosen to construct the SCs should be able to approximate the path of sovereign bond spreads of the DSSI-eligible countries, but they should not anticipate the event. In our setting, this assumption is likely to be satisfied. The start of the DSSI can be considered a plausibly exogenous shock, as it happened in response to the impacts of the Covid-19 global pandemic rather than in response to the debt build-up of individual countries. Also, debt relief was offered simultaneously to all eligible countries in a centralized way, irrespective of their economic circumstances. Second, *no interference*. The countries in the donor pool used to estimate the synthetic controls should not be affected by the event. This implies that there are no spillover

[17] Table A4 provides summary statistics of various country-level statistics for both eligible and ineligible countries.

[18] In a robustness test, we also use the announcement of the DSSI on April 15, 2020, as the treatment.

[19] On February 4, the WHO asked the UN Secretary-General to activate the UN crisis management policy following the outbreak in China. On March 11, the WHO declared Covid-19 a pandemic. On March 26, the G20 Leaders declared they were "committed to do whatever it takes to overcome the pandemic." April 6 is the first trading day after the WHO reported that over 1 million cases of Covid-19 had been confirmed worldwide. See: https://www.who.int/news-room/detail/29-06-2020-covidtimeline.

Figure 1: Sovereign bond spreads in DSSI-eligible countries vs. their synthetic controls



Notes: The figure plots the difference between the actual sovereign bond spreads and those of the respective synthetic control (*spread gap*). The solid line is the median value of the spread gaps in the sample of DSSI-eligible countries; the dashed line is the average. The shaded area indicates the interquartile (IQ) range. The vertical line indicates the DSSI implementation on May 1, 2020. Country-level sovereign bond spreads are 7-day moving averages. Sources: Bloomberg, Our World in Data, World Bank IDS, IMF World Economic Outlook and National Authorities.

effects from sovereign bond yields of DSSI-eligible countries to those of ineligible countries. While spillover effects of sovereign bond yields exist (Gande and Parsley, 2005), even among emerging markets (Dell'Erba *et al.*, 2013), they are larger in economies that are strongly financially integrated (e.g., the euro area, Bruyckere *et al.*, 2013), they are much stronger from richer to poorer countries rather than vice versa (Arezki and Liu, 2020), and they stem from global factors (e.g., US monetary policy, global risk aversion) and contagion from systemic events (González-Rozada and Yeyati, 2008; Miranda-Agrippino and Rey, 2020). Thus, it is unlikely that bonds spreads in the donor pool, which are mostly dependent on economic conditions in advanced and emerging economies, are affected by dynamics of sovereign bond markets in eligible low-income countries.

Baseline results. Table 1 reports the weights used to construct the SCs for each treated country and the Fit Index (Adhikari and Alm, 2016), a goodness-of-fit measure of the constructed counterfactual for each treated country. This index is computed as the ratio of the pre-treatment normalized root

Table 1: Synthetic controls

Country	Composition of the Synthetic control	RMSPE	Fit Index
Angola	Sri Lanka (42.70%), Bahamas (28.70%), Gabon (13.10%), Ecuador (9.80%), Lebanon (5.60%)	94.44035	0.0002
Benin	Ukraine (39.00%), Kazakhstan (27.30%), Belize (14.20%), Armenia (7.50%), Russia (6.60%), Belarus (2.50%), Lebanon (2.20%), Venezuela (0.80%), Guatemala (0.10%)	15.32492	0.000255
Cameroon	Armenia (26.60%), Paraguay (21.90%), Iraq (21.50%), Vietnam (10.70%), Guatemala (8.60%), Lebanon (6.30%), Kazakhstan (2.90%), Bolivia (1.50%)	42.52291	0.000385
Cote d'Ivoire	Ukraine (36.10%), Russia (25.10%), Belize (19.00%), Kazakhstan (11.10%), China (8.70%)	15.68356	0.000628
Ethiopia	Armenia (44.30%), Philippines (16.50%), Vietnam (13.80%), Egypt (12.50%), Georgia (7.00%), Lebanon (4.50%), Gabon (1.50%)	43.70391	0.00062
Ghana	Egypt (47.10%), China (26.10%), Ukraine (14.60%), Guatemala (6.60%), Lebanon (5.00%), Venezuela (0.60%)	33.82309	0.000476
Honduras	Bahamas (54.60%), Russia (17.70%), Bolivia (10.40%), Serbia (6.60%), Gabon (6.30%), Iraq (2.40%), Suriname (2.00%)	17.69936	0.000513
Kenya	Egypt (63.40%), China (15.80%), Belize (13.00%), Ukraine (5.80%), Kazakhstan (2.00%)	36.96895	0.001211
Mongolia	Armenia (46.60%), Croatia (18.70%), Kazakhstan (9.60%), Tunisia (8.70%), Ecuador (8.60%), Jamaica (7.80%)	57.11242	0.000628
Mozambique	Belize (32.60%), Tunisia (29.40%), Armenia (19.70%), Ukraine (7.30%), Barbados (6.40%), Jordan (3.20%), Lebanon (1.40%)	30.21086	0.000753
Nigeria	Ukraine (41.50%), Guatemala (19.80%), Belize (17.40%), Turkey (9.20%), Paraguay (7.60%), Lebanon (4.10%), Russia (0.50%)	30.65403	0.000398
Pakistan	India (30.60%), Sri Lanka (21.90%), Bahrain (19.70%), Egypt (15.20%), Tunisia (11.90%), Suriname (0.40%), Lebanon (0.30%)	17.49369	0.000314
Papua New Guinea	Russia (30.50%), Turkey (23.40%), Vietnam (18.00%), Belize (17.70%), Bolivia (9.10%), Venezuela (1.40%)	28.58984	0.001106
Rwanda	Georgia (39.40%), Bolivia (30.40%), Armenia (11.70%), Gabon (8.20%), Ecuador (6.40%), Vietnam (3.90%)	37.004	0.000343
Senegal	Philippines (34.90%), Belize (25.70%), Egypt (21.30%), Ukraine (12.30%), Kazakhstan (5.70%)	16.2614	0.000878
Tajikistan	Vietnam (23.50%), Armenia (21.60%), Guatemala (20.20%), Iraq (16.70%), Kazakhstan (8.30%), Lebanon (7.30%), Venezuela (2.30%)	56.6462	0.000485
Uzbekistan	Kazakhstan (45.60%), Armenia (28.10%), Belarus (10.20%), Guatemala (7.30%), Tunisia (4.80%), Argentina (3.10%), Lebanon (1.00%)	24.38655	0.000728
Zambia	Bahamas (36.10%), Suriname (35.90%), Lebanon (25.00%), Venezuela (2.90%)	138.5992	0.000164

Notes: The table lists the composition of the synthetic control for each DSSI-eligible country. The last two columns report the root mean square prediction error (RMSPE) of the outcome variable in the pre-DSSI period and the *Fit Index*, i.e., the ratio between the normalized RMSPE and the variance of the outcome variable in the pre-DSSI period. The synthetic control estimates refer to those plotted in Figures 1 and 2.

mean squared prediction error (RMSPE) and the variance of the bond spreads in the pre-treatment period (e.g., the RMSPE of a zero-fit model). The index is close to zero across all countries, suggesting that our SCs perform well in approximating the pre-treatment dynamics of the bond spreads.

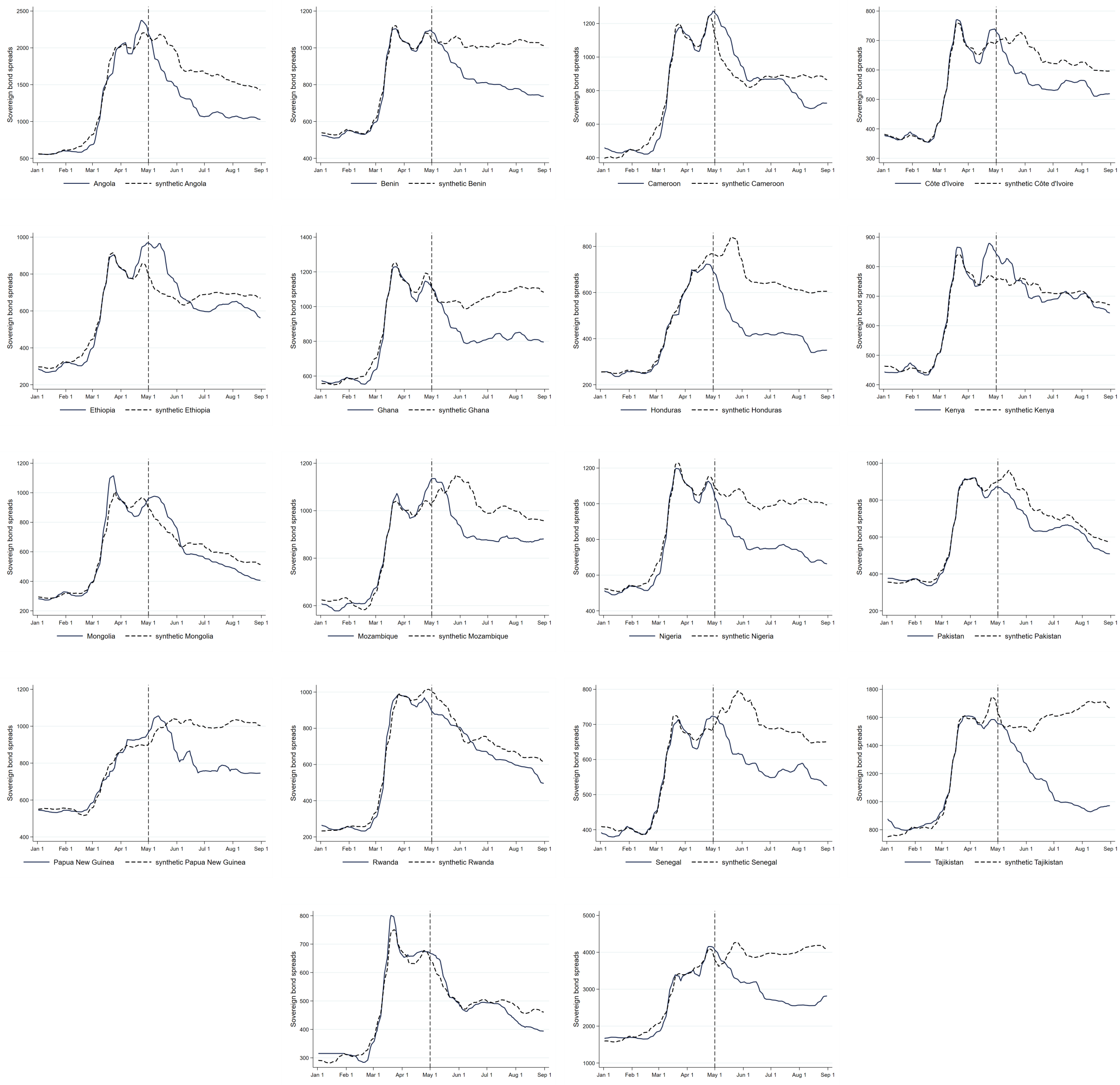
Figure 1 plots the difference between an eligible country's actual spread and the spread of its SC (the *spread gap*) around the treatment date May 1—the day when the DSSI was implemented and when the first country officially participated. The two lines show the median (solid line) and average (dashed line) spread gap of the 18 eligible countries, while the shaded area represents the interquartile range. In the four months before the treatment, the spread gap is close to zero. Then, after May 1, the gap starts declining, with the median gap stabilizing at around 150 bps in June, while the average gap declines by more than 200 bps.

Looking separately at each of the 18 treated countries strengthens the finding that there was no negative market reaction after the DSSI was implemented. None of the 18 DSSI-eligible countries experienced an increase in spreads compared to its SC. (See both Figure A6, which plots all 18 spread gaps, and Figure 2, which plots actual and SC spreads for each country separately.) While for a few countries (e.g., Ethiopia, Kenya, and Uzbekistan) the relative decline in spreads is small, for others (e.g., Angola, Nigeria, Senegal, and Tajikistan) the decline in spreads is large, persistent, and economically meaningful.

In an additional empirical test, we set the treatment to the day of the DSSI *announcement* two weeks earlier. The results, reported in Figure A7, are qualitatively similar results and show a lag in the fall in spreads consistent with the effect being driven by the DSSI *implementation*. One reason explaining why the DSSI's effect is observable after the DSSI implementation rather than after its announcement is the uncertainty surrounding the implementation of the DSSI between the two dates. Only on May 1, the policy was officially implemented, the first country (Pakistan) officially joined, and it became clear that private creditors would not have to participate in debt relief (IIF, 2020). Thus, in the remainder of the paper we will consider May 1 as the treatment date.

Falsification tests. We run several falsification tests to enhance the validity of this result: a) two in-time placebo tests in which we backdate the timing of the treatment, and b) an in-space placebo

Figure 2: Synthetic control method for DSSI-eligible countries

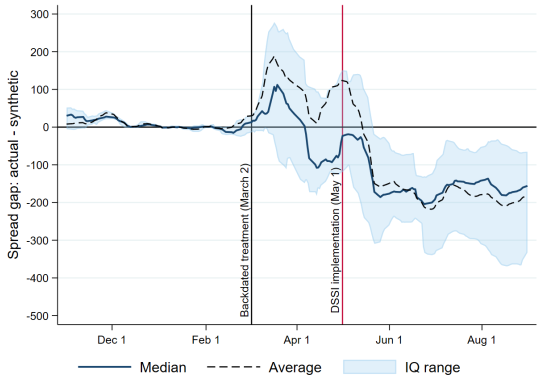


Notes: The charts plot, for each DSSI-eligible country, both actual sovereign bond spreads and those of the respective synthetic control. Dhe DSSI implementation on May 1, 2020 is the treatment and is indicated by the vertical line. See description in the main text. Ghana, Honduras, Kenya, Mongolia, Nigeria and Uzbekistan did not formally request to join the initiative in the observation period. Data sources: Bloomberg, Our World in Data, World Bank IDS, IMF World Economic Outlook and National Authorities.

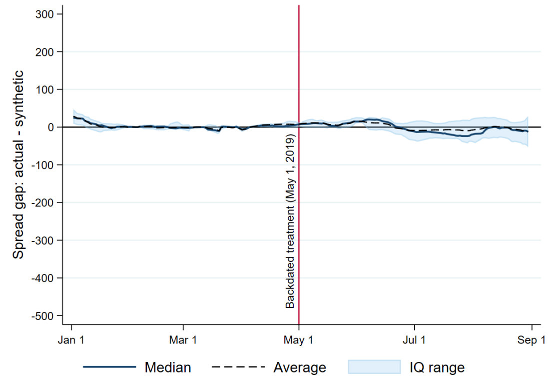
test in which we assign the treatment to similar, non-DSSI eligible countries.

For the first placebo in time, we start by backdating the treatment to March 1, before the UN declared Covid-19 a pandemic. In the period between the placebo treatment and the actual

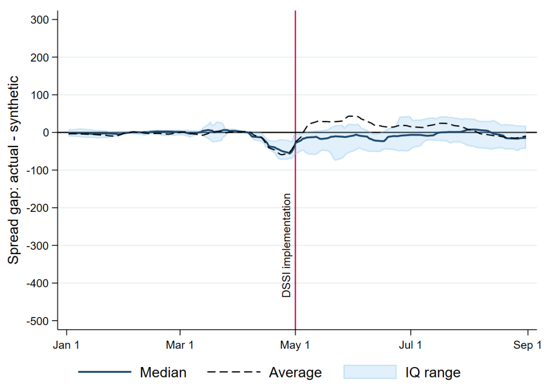
Figure 3: Falsification tests



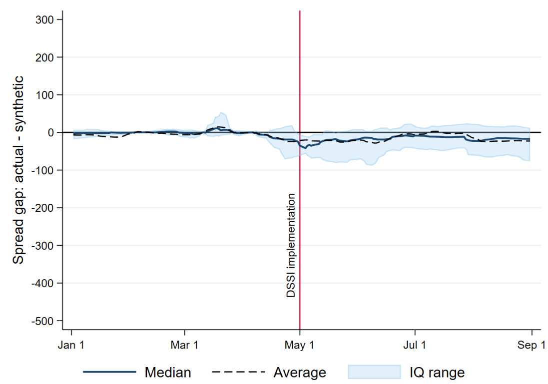
(a) Backdating to March 2, 2020



(b) Backdating to May 1, 2019



(c) In-space placebo



(d) IMF programs

Notes: The figures show the placebo tests of the DSSI's effect on bond spreads. The in-time placebos shift the treatment to March 2, 2020 in panel (a) and one year back in time (to May 1, 2019) in panel (b). They use the group of DSSI-eligible countries as treated units. In panel (a), the sample starts on November 1, 2019 to maintain a 4-month pre-treatment period as in the baseline. The pre-period matching is done on the same set of macroeconomic variables used in the baseline, while the four dates used to match the spreads are December 16, January 2, January 15 and February 3. In panel (b), the pre-period matching is done by excluding the number of Covid-19 cases per million people and by using the same set of macroeconomic variables as in the baseline, but measured in 2018, and the same four dates, but in 2019. The sample excludes Mozambique (in default), Zambia (in severe distress) and Uzbekistan (for lack of data on bond spreads). The first in-space placebo in panel (c) considers as treated the 18 poorest countries that are not eligible for the DSSI (GDP per capita $<$ 5,500 USD). This sample includes Armenia, Belize, Bolivia, Egypt, El Salvador, Georgia, Guatemala, India, Indonesia, Jordan, Morocco, Namibia, Paraguay, Philippines, Sri Lanka, Tunisia, Ukraine, and Vietnam. Panel (d) plots the *spread gap* for countries that are not eligible for the DSSI but signed an IMF lending arrangement between April 15 (DSSI announcement) and June 22 (the day when the last country joined the initiative in the observation period). The sample includes Armenia, Bahamas, Bolivia, Chile, Colombia, Costa Rica, Dominican Republic, Egypt, Georgia, Guatemala, Jamaica, Jordan, Panama, Peru, Paraguay, and Ukraine. See the description in the main text. Each panel plots the difference between the actual sovereign bond spreads and those of the respective synthetic control (*spread gap*). The solid line is the median value of the spread gaps in the sample of DSSI-eligible countries; the dashed line is the average. The shaded area indicates the interquartile range. The vertical line indicates the DSSI implementation on May 1, 2020. Country-level sovereign bond spreads are 7-day moving averages. Sources: Bloomberg, Our World in Data, World Bank IDS, IMF World Economic Outlook and National Authorities.

treatment, the spread gaps show no consistent patterns. As the interquartile range indicates, there are both positive and negative spread gaps across countries. For the median country the spread gap turns from slightly positive to slightly negative before the actual treatment date. This is consistent with the high volatility of financial markets during the unfolding of the pandemic (i.e., the CBOE Volatility Index increased by a factor of 3 between February and March, close to the level reached at the peak of the 2008 crisis) and the uncertainty about the design and implementation of the DSSI. More importantly, the spreads between March 2 and May 1 do not show any evidence of systematic anticipation effects (Figure 3, panel a). It is only after the actual DSSI implementation that the spread gap widens and becomes negative, replicating the baseline result.

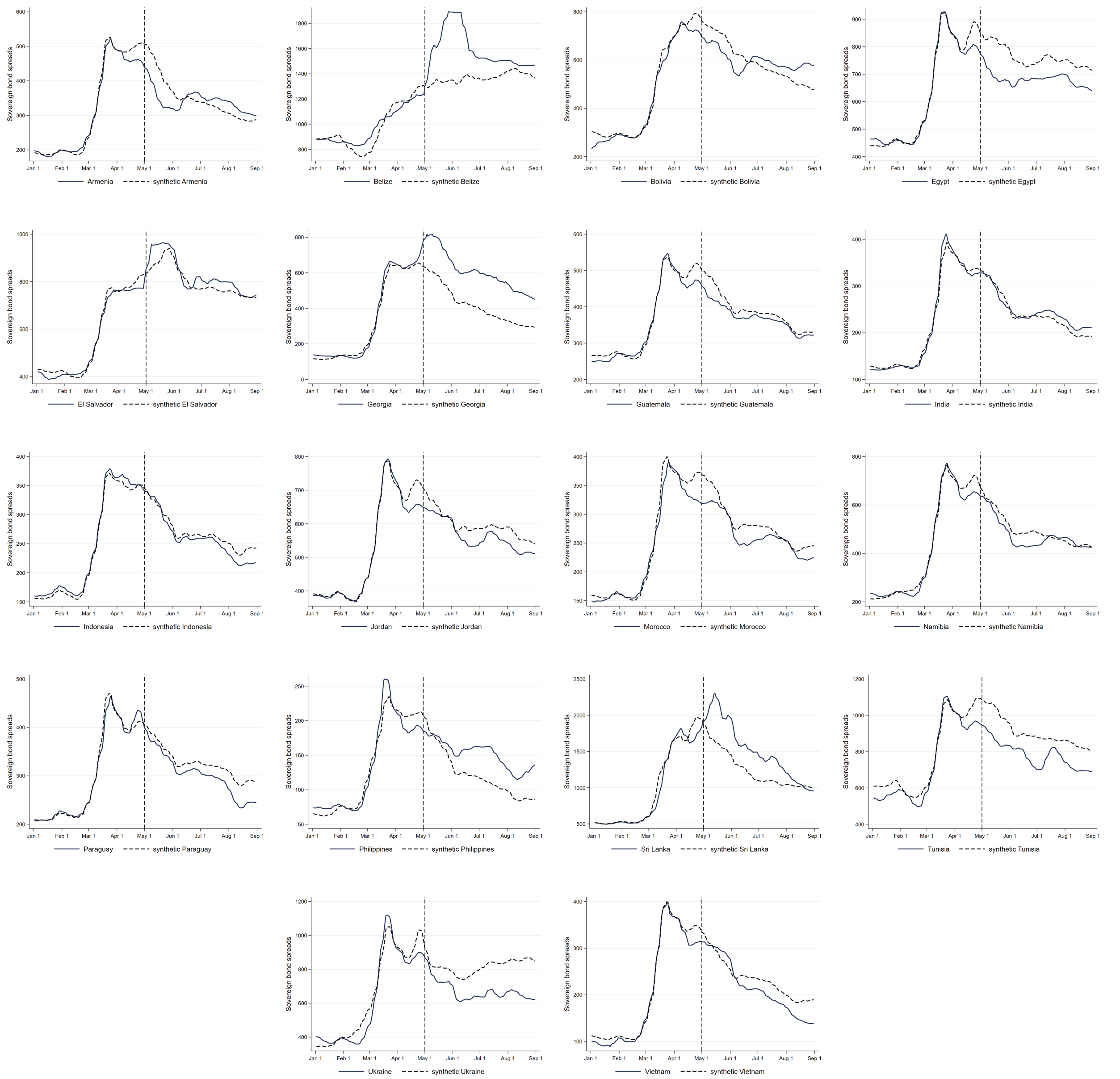
In the second in-time placebo, we backdate the treatment by one year (to May 1, 2019) and construct the SC for each of the DSSI-eligible countries (except Uzbekistan for lack of spread data, as well as Mozambique and Zambia, which were in default and severe distress, respectively, in 2019) using 2018 macroeconomic variables. The results (Figure 3, panel b) show that the gaps between the actual spreads and their SCs are very close to zero, demonstrating that the SCs are able to reproduce the dynamics of spreads one year before the policy was implemented. The exercise does not produce any difference in 2019 between countries that became DSSI-eligible in 2020 and their synthetically constructed counterfactuals.

For the placebo in space, we consider the 18 countries of the donor pool with the lowest incomes (all with GDP per capita lower than 5,500 USD, as listed in the notes to Figure 3). If the decline in sovereign spreads is the result of a common trend among countries with low incomes, this placebo test should show results similar to our baseline. Instead, we see that the median spread gap is close to zero for the entire post-DSSI period and the trajectories of individual countries' spread gaps are broadly split below and above zero (Figure 3, panel c). Figure 4 plots the respective country-specific charts and shows no common pattern across these 18 countries.[20]

Debt relief or IMF program. A potential concern with our research design is that the effect may not be driven by debt relief *per se*, but could be the result of the conditions attached to DSSI participation (see Section 2). In particular, the presence of an IMF program, coupled with more

[20]For most countries, trends in spreads do not diverge from the trends of their respective synthetic control after the treatment. For a few countries, some divergence is visible (positive for Belize, Georgia, and Sri Lanka; negative for Tunisia and Egypt), but these estimated differences point in opposite directions, do not coincide exactly with the treatment date, and are, on average, zero. For these countries, we interpret these divergences from their SCs as noisy deviations from an average zero effect and, thus, as Type II errors.

Figure 4: Synthetic control method for non DSSI-eligible countries



Notes: The charts plot, for the 18 ineligible countries with the lowest income, the actual sovereign bond spreads (solid line) and those of the respective synthetic control (dashed line). The vertical line indicates the DSSI implementation on May 1, 2020. See description in the main text. Data sources: Bloomberg, Our World in Data, World Bank IDS, IMF World Economic Outlook and National Authorities.

transparency towards financial markets, may drive down bond spreads (Gehring and Lang, 2020). To disentangle the effect of debt relief from that of requesting an IMF program, we run another placebo test in which the treatment group includes developing countries that are not eligible for the

DSSI but signed an IMF emergency financing program soon after the announcement of the DSSI.[21] If the fall in spreads that we observe in the baseline is the effect of IMF programs, then we should observe a similar decline when running this placebo test. The results, however, show a different picture (Figure 3, panel d). While the average and median gaps are mildly negative, countries are almost equally split between those with positive and negative spread gaps. Thus, while there is some evidence suggesting that participation in an IMF program for some countries could reduce sovereign bond spreads, this is unlikely to explain the large drop that we observe for almost all eligible countries in response to the DSSI.

Robustness. We run a large battery of robustness tests to mitigate potential concerns that our main findings could be sensitive to the choice of i) the countries included in the donor pool, and ii) the variables used to construct the SCs. As regards the first point, we start by excluding the three countries in default from the donor pool (Argentina, Barbados and Venezuela, based on updated data from Asonuma and Trebesch (2016)) and find consistent results that are very similar to the baseline (Figure A8). Then, we provide more systematic evidence that our results are not sensitive to the composition of the donor pool by dropping one country at a time from the donor pool and re-estimating 52 versions of the SCs for each treated country. Figure A9 plots the 52 median spread gaps for each of these restricted donor pools along with the baseline median spread gap obtained when using the entire donor pool (as in Figure 1). The chart shows that the median spread gap is always negative, distributed around the median value estimated in the baseline, and in most cases very close to it, suggesting that our main result is not particularly sensitive to the composition of the donor pool.

As regards the second point, we start by dropping April 6 from the set of matching dates, as its proximity to the treatment date could drive the evolution of the spread gap in the pre-treatment period (Figure A10). Then, we use the average pre-treatment bond spreads rather than their value at specific dates to construct the SCs (Figure A11). In both cases, results are qualitatively similar to the baseline and, if anything, the estimated effect on spreads is larger (with the caveat that using

[21] We consider the time window from April 15 (the date of the DSSI announcement) to June 22 (the date when the last DSSI eligible country joined the initiative). Results are robust to alternative choices. The list of IMF emergency lending programs is available at: https://www.imf.org/en/Topics/imf-and-covid19/COVID-Lending-Tracker. We exclude Ecuador as it restructured its debt post Covid-19.

the average spreads leads to more volatile results).

The final set of robustness tests consider the set of macroeconomic variables used to construct the SCs. In separate exercises, we include: i) sovereign credit ratings, to control for the possibility that DSSI countries are less creditworthy than the countries in the donor pool (Figure A12); ii) a measure of the extent of the Covid-19 growth shock in 2020,[22] to allow for a potentially heterogeneous impact of the pandemic which could be reflected in the sovereign spread dynamics (Figure A13); and iii) the full set of macroeconomic and socio-demographic variables that differ across DSSI-eligible countries and the donor pool, as reported in Table A4 (Figure A14). Finally, we run the opposite exercise by matching only on the lagged values of the bond spreads in the four pre-treatment dates (Figure A15), thereby minimizing the risk of cherry-picking the set of predictors used in the SC (Ferman *et al.*, 2020). In all cases, our results are qualitatively similar and point to a decline of sovereign spread for the median (average) country of around 100 (200) bps.

Synthetic DiD. DSSI-eligible countries have, on average, a lower per capita GDP, weaker institutions and a lower access to private creditors than countries in the donor pool (see Table A4). Moreover, the countries may also differ along other characteristics which may drive spreads (e.g., economic policies) and are not always observable and measurable. To deal with such concerns more explicitly, we draw on recent methodological advances that allow for the inclusion of unit and time fixed effects under the SC framework. Specifically, we implement the "synthetic DiD" estimator (Arkhangelsky *et al.*, 2021) and, for robustness, the "generalized synthetic control method" (Xu, 2017). Both approaches allow to absorb the effect of unobservable country characteristics, as long as they do not vary over the short period of time of our observation window. Similarly, day fixed effects control for the effect of global shocks that are uniform across countries.

Synthetic DiD combines features of SC and DiD methods. Like SC methods, it weights observations to match pre-treatment trends; like DiD methods it accounts for unit fixed effects (α_c) and time fixed effects (β_t). The method, first, finds unit weights ($\hat{w}_c^{sdid}$) that align pre-treatment trends of treated and untreated units and time weights ($\hat{\lambda}_t^{sdid}$) that balance the pre-treatment and the post-treatment period for untreated units. These weights are then used in a two-way fixed-effects

[22] This measure is computed as the difference between the projections in the June 2020 release of the IMF World Economic Outlook and the last pre-pandemic projections released in October 2019.

regression to estimate the average effect (τ) of the treatment (D_{ct}):

$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = arg \min_{\tau,\mu,\alpha,\beta} \left\{ \sum_{c=1}^{N} \sum_{t=1}^{T} (Y_{ct} - \mu - \alpha_c - \beta_t - D_{ct}\tau)^2 \hat{w_c}^{sdid} \hat{\lambda}_t^{sdid} \right\}. \quad (3)$$

The corresponding standard error can be estimated via a unit-wise jackknife procedure.[23] The sample that we use in this estimation consists of all 18 eligible and all 52 ineligible countries for which we have daily data for all 165 trading days between January 2, 2020 and August 31, 2020, amounting to 11,550 country-day observations.

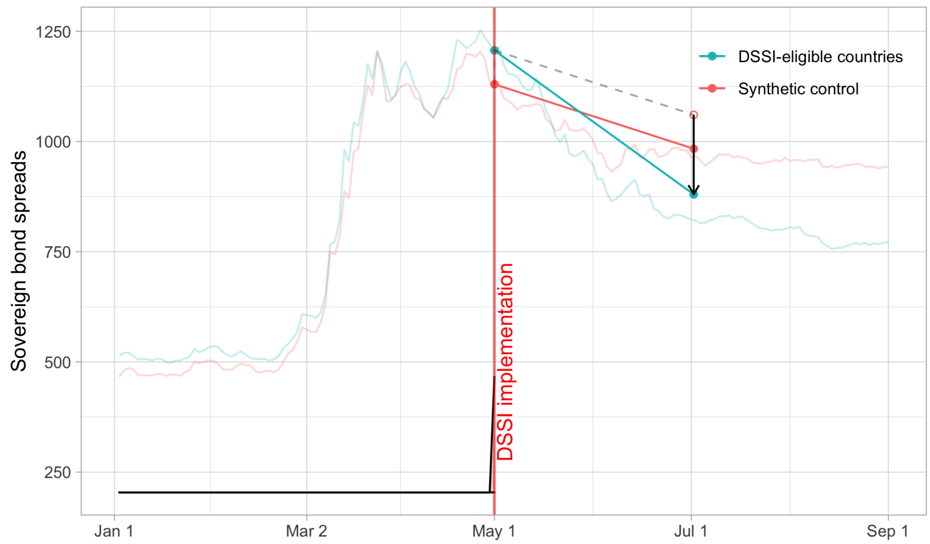
Figure 5 plots the result of applying the synthetic DiD estimator. The SC that this method constructs is able to closely match the path of treated countries in the pre-intervention period when the Covid-19 shock increased bond spreads in most countries. When the DSSI is implemented on May 1, the bond spreads of treated countries depart from this parallel trend and decline much more and faster than those of their SCs. A gap of about 200 bps remains visible until the end of the observation period. The estimated treatment effect is statistically significant and equal to 180 bps. Applying the generalized SC method produces very similar results with a slightly larger effect size (Appendix A.3 and Figure A1).

4.2 Credit Risk and Liquidity Risk

So far our attention has been on the effect of the DSSI on sovereign bond spreads as a measure of credit risk. However, the unfolding of the pandemic also saw a sharp deterioration of market liquidity in both advanced and emerging markets (IMF, 2020a). The bid-ask spread on sovereign bonds—a common measure of market liquidity—for the median DSSI-eligible country increased from about 20 bps before the pandemic crisis to around 60 bps between mid-March and the DSSI implementation, when it slowly started declining (Figure A16). Thus, lower spreads could reflect a reduction in both credit and liquidity risk. Disentangling the two effects in bond prices is challenging, especially in our context, as it requires comparing bonds that are similar along many characteristics (including, critically, credit risk), but differ in terms of market liquidity (see, for instance, Monfort and Renne, 2013; Schwarz, 2019, on the euro area). Although it is not possible

[23]For further details on synthetic DiD, see Arkhangelsky *et al.* (2021).

Figure 5: Synthetic difference in differences



Notes: The figure plots the results of applying the synthetic difference in differences (SDiD) estimator proposed by Arkhangelsky *et al.* (2021). The vertical line indicates the DSSI implementation on May 1, 2020. The figure shows the parallel trends of bond spreads in eligible countries and their synthetic controls before the DSSI implementation and the eligible countries' departure from this parallel trend after the DSSI is implemented. The black arrow indicates the treatment effect, i.e., the difference between actual spreads in eligible countries and the projected counterfactual. The treatment effect and its 90% confidence interval are estimated at $\tau = 180.4, [-350.8, -10.0]$. Data sources: Bloomberg, Our World in Data and IMF World Economic Outlook.

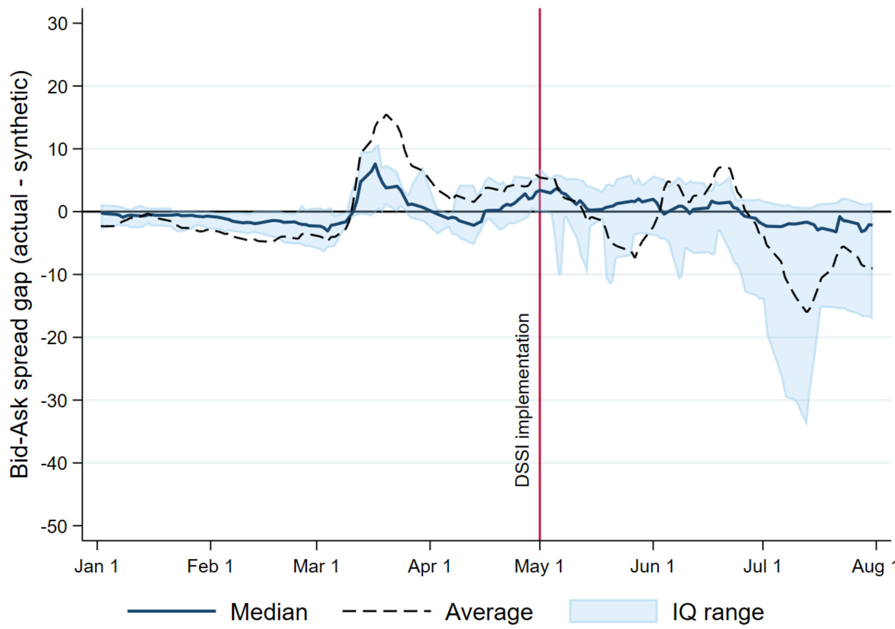
to replicate a similar comparison in our sample of DSSI countries, which have a limited number of outstanding bonds, we can extend our SC approach to test whether the DSSI not only reduced sovereign bond spreads, but also improved market liquidity in the treated countries' sovereign bonds, i.e., through lower bid-ask spreads than the SC would predict.

For this purpose, we collect daily data on bid-ask spreads (computed as differences between traded bid and ask yields) on sovereign bonds for 67 countries in our sample (including all 18 DSSI-eligible countries) and use those as an outcome variable in our baseline specification. We take the 7-day moving average of bid-ask spreads and construct the SC for each treated country using the same pre-treatment variables used in the baseline analysis for bond spreads, plus the bid-ask spreads on the four specific dates used in the baseline.[24]

The results are shown in Figure 6. Different from what we observed for sovereign bond spreads,

[24] The analysis differs from the baseline as the post-DSSI period ends on July 31 (rather than August 31), given that data for Papua New Guinea in August are not available. We also include the sovereign bond spreads on the usual four dates in the set of pre-treatment variables, although results are robust to excluding them.

Figure 6: Sovereign bond bid-ask spreads in DSSI-eligible countries vs. their synthetic controls



Notes: The figure plots the difference between the actual sovereign bond bid-ask spreads and those of the respective synthetic control (*spread gap*). The solid (dashed) line is the median (average) value of the bid-ask spread gaps in the sample of DSSI-eligible countries. The shaded area indicates the interquantile range. The vertical line indicates the DSSI implementation on May 1, 2020. Country-level sovereign bond bid-ask spreads are 7-day moving averages. Sources: Bloomberg, Our World in Data, World Bank IDS, IMF World Economic Outlook and National Authorities.

we do not observe any discernible improvement in market liquidity for DSSI-eligible countries compared to their SCs. The bid-ask spread gap for the median country is close to zero in the post-DSSI period and, if we consider the end point on July 31, we observe that the bid-ask spread gap is positive in 8 countries and negative in 10 countries, with no clear pattern in either direction.[25] Keeping the caveat in mind that we cannot precisely disentangle credit and liquidity risks, the lack of robust evidence that the DSSI affected market liquidity would suggest that its positive effect on sovereign bond spreads is likely to reflect a decline in credit risk.

[25]Starting in late June the average spread gap shows a visible decline, which is mostly driven by two countries (Mozambique and Zambia). On the other hand, Papua New Guinea shows a positive change in the bid-ask spread compared to its SC. In addition, even considering the maximum decline in the average bid-ask spread gap (around 10 bps)that is observable on single post-treatment days, this is still a relatively small improvement compared to the 60 bps increase in the run up to the pandemic (Figure A16).

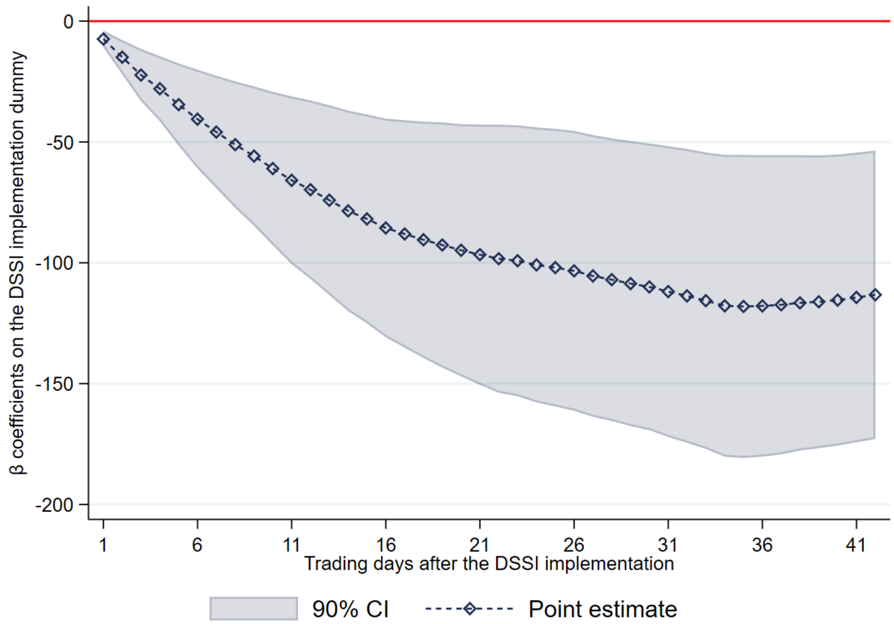
4.3 Heterogeneous Effects

The evidence that bond spreads decline more in DSSI-eligible countries than in their SCs does not support the presence of a stigma resulting from debt relief and is consistent with a positive effect due to liquidity provision. To further support this claim, we extend our analysis in a standard DiD setting and trace the dynamic response of sovereign spreads after the DSSI using the standard local projection method (Jordà, 2005). While this approach is less suited to construct a good counterfactual for the treated sample, it allows us to look at state-dependent responses to the treatment (Auerbach and Gorodnichenko, 2013; Jordà and Taylor, 2016; Ramey and Zubairy, 2018). We run six additional tests in which we allow for differences in the effect of the treatment depending on: 1) the size of debt relief, 2) the residual maturity of outstanding debt, 3) the initial risk of debt distress, 4) the initial public debt-to-GDP ratio, 5) the relative importance of private creditors, and 6) the decision to participate in the DSSI. To minimize the differences between DSSI-eligible countries and the donor pool, we consider the same sample of 36 countries used in the in-space placebo (the 18 DSSI-eligible ones and the 18 countries in the donor pool with a GDP per capita lower than 5,500 USD, see panel (c) of Figure 3), and estimate the following baseline equation:

$$\Delta Spread_{c,t+h} = \beta_h DSSI_c \times Post_t + \alpha_1 Spread_{c,t-1} + \alpha_2 Spread_{c,t-2} + \tau_t + \gamma_c + \epsilon_{c,t}, \tag{4}$$

where the dependent variable denotes the change in sovereign bond spreads of country c from day t to $t+h$ and the key explanatory variable is the interaction between the $DSSI$ indicator, which identifies DSSI-eligible countries, and the $Post$ indicator, which is equal to 1 from the DSSI implementation day onwards and 0 before. The lagged values of sovereign spreads control for potential mean reversion or persistence in the dynamics of spreads. Day fixed effects (τ_t) and country fixed effects (γ_c) absorb global daily shocks and country-specific time-invariant unobservable characteristics which may drive differences in bond spreads. Thus, the coefficients β_h trace the differential effect of the DSSI on bond spreads between eligible and ineligible countries over a 42-trading day window (from May 1 to July 1). In the second step, we allow the coefficient β to vary depending on country characteristics adding a triple interaction with: 1) a binary variable equal to 1 for countries where the amount of debt service relief under the DSSI is larger than 0.5 percent

Figure 7: DiD evidence from impulse response functions



Notes: The charts plot the impulse response function (IRF) and the associated 90% confidence intervals, obtained by estimating equation 4 and clustering standard errors at the country level. The IRF traces the effect of the DSSI implementation ($t = 0$) on daily sovereign bond spreads. Data source: International Debt Statistics (World Bank), Bloomberg and IMF World Economic Outlook.

of GDP; 2) a binary variable equal to 1 for countries where the residual maturity of outstanding international sovereign bonds is 5 years or less; 3) a binary variable equal to 1 for countries at high risk of debt distress and equal to 0 for those with moderate or low risk;[26] 4) a binary variable equal to 1 for countries with a debt-to-GDP ratio larger than 70 percent of GDP; 5) a binary variable equal to 1 for countries where the share of debt service due to private creditors between May and December 2020 is at least 50 percent of total debt service relief under the DSSI; and 6) a binary variable equal to 1 for countries which joined the DSSI within our sample period.

Results confirm—in a standard DiD setting—the statistically significant decline in spreads after the DSSI implementation. The effect size reaches approximately 120 bps after about 1 month and persists around that level thereafter (Figure 7). Splitting the average effect shows that the

[26] We use the IMF/World Bank risk rating under the low-income countries Debt Sustainability Framework (LIC DSF). This information is not available for 4 eligible countries which are not part of the LIC DSF (Angola, Mongolia, Nigeria, and Pakistan) and which we thus exclude from this analysis. See https://www.worldbank.org/en/topic/debt/brief/covid-19-debt-service-suspension-initiative.

decline in spreads is twice as large in countries that received a large DSSI relief compared to those where debt relief was 0.5 percent of GDP or less (Figure 8, panel a). Also, the effect on sovereign spreads is stronger and statistically significant only for countries with shorter residual maturities on outstanding sovereign bonds (panel b). Both of these results suggest that debt relief has an effect on market prices via liquidity provision, as the debt service suspension affects sovereign spreads more the larger the amount of debt service due and the higher the share of debt due in the short term.[27]

Next, we test whether the initial fiscal position matters for the market reaction to debt relief. Results show that the decline in spreads is stronger and significant only for high-debt and high-risk countries, suggesting that the debt suspension is more likely to bring benefits to countries with weaker fiscal positions (Figure 8, panels c and d). This is consistent with the analysis by Arellano *et al.* (2020) on non-linear effects of debt relief conditional on the initial level of public debt.

By contrast, splitting the sample along the share of debt service due to private creditors shows that there is virtually no difference in the decline in spreads between countries with large and small shares (panel e). Moreover, if the DSSI were to affect bond yields through stigma, one should observe an increase in sovereign spreads at least for countries which joined the DSSI. Instead, the decline in bond spreads is significant for both country groups (panel f), strengthening our interpretation that debt relief does not trigger a negative stigma effect that outweighs the positive effects of liquidity provision.

A limitation of the latter exercise is that it does not take into account the different dates on which governments officially announced their participation in the DSSI. To look at the potential implication of DSSI *participation* on borrowing costs, we run an additional test based on the generalized SC method (see Appendix A.3 for details on this method), as it allows the treatment date to vary by country. We consider the sample of DSSI-eligible countries and code the day on which the country publicly announced its participation in the DSSI as the country-specific treatment. We plot the results in Figure 9. Comparing participants to non-participants (both eligible and ineligible)

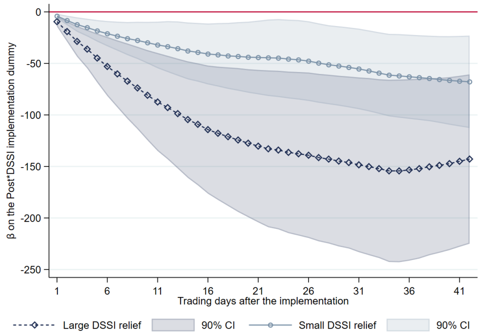
[27] A caveat in interpreting this result is that the point estimates across the two sub-samples are not statistically different. However, this is to be expected given the small sample size and the difference in the point estimates, at least in panel a), is large enough to suggest that the size of liquidity provisions substantively matters for the market reaction after the DSSI.

shows that the baseline result can be replicated using *country-specific* participation rather than the *common* DSSI implementation as treatment (panel a).[28] However, when comparing DSSI participants only to eligible countries that did not participate, we find a precisely estimated null effect (panel b). Two lessons can be drawn from this exercise. First, bond spreads exhibit similar dynamics across countries that participated and those that did not. Consistent with Figure 8 panel (f), those that did not join (in the observation period) seem to have benefited from the positive *common* effect of the DSSI.[29] Second, the lack of a statistically and economically significant increase in spreads for participants *after their participation* relative to eligible non-participants further indicates that there is no stigma effect that results from a country's decision to join the DSSI.

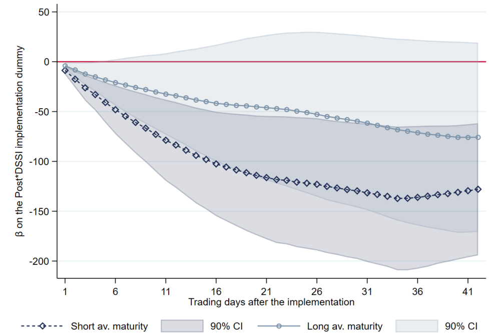
[28]As many countries announced their participation relatively soon after the central announcement, it is not possible to clearly differentiate between the common effect of the DSSI and the country-specific decision to participate. Furthermore, we urge caution when interpreting this result as the country-specific decision to participate is potentially endogenous.

[29]The policy leaves countries the possibility to sign up at a later stage. Kenya, for instance, was always eligible but only joined in November 2020.

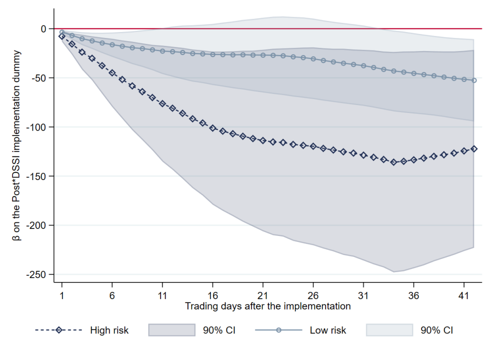
Figure 8: Heterogeneous effects: DiD evidence from impulse response functions



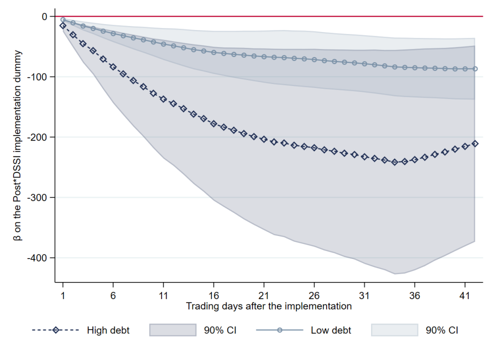
(a) Size of the DSSI relief



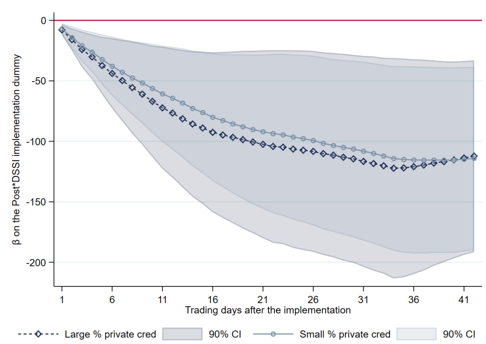
(b) Time to maturity



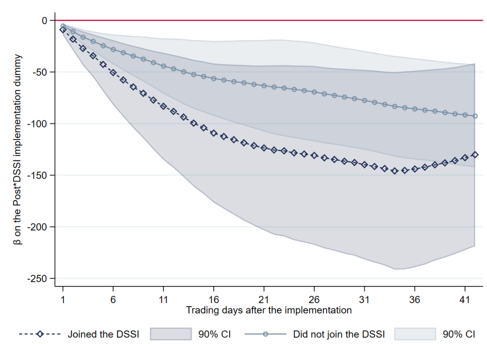
(c) Risk of debt distress



(d) Public debt



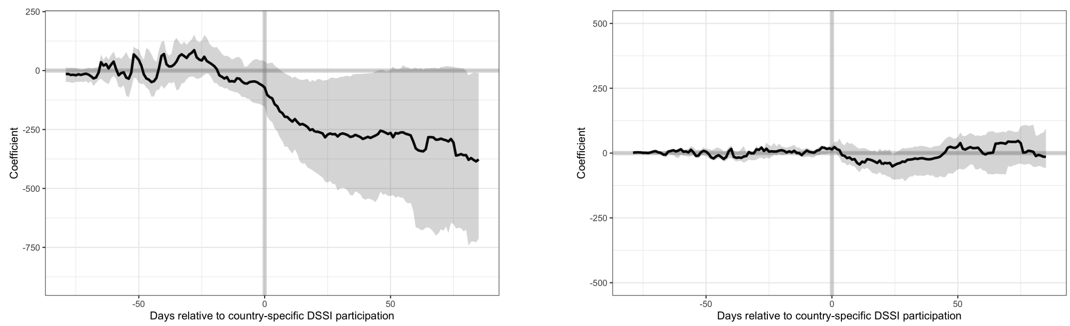
(e) Share of private creditors



(f) DSSI participation

Notes: The charts plot two separate impulse response functions of the effect of the DSSI implementation ($t = 0$) on daily sovereign bond spreads for two sub-samples of countries. Panel (a) differentiates between eligible countries that received a debt relief larger or smaller than 0.5 percent of GDP. Panel (b) splits the sample depending on the time to maturity of the average outstanding bonds being below or above 5 years. Panel (c) only considers the 12 DSSI-eligible countries with a LIC-DSF "risk of debt distress" rating and differentiates between countries at high risk and those at low or moderate risk. Panel (d) differentiates between eligible countries with public debt above and below 70 percent of GDP. Panel (e) differentiates between countries with debt service due to private creditors above or below 50 percent of total debt service due under the DSSI; Panel (f) separately considers countries that joined the DSSI by September 2020 and those that did not (see footnote 14). All panels plot the impulse response functions and the associated 90% confidence intervals, obtained by estimating equation 4 and clustering standard errors at the country level. See description in the main text. Data source: International Debt Statistics (World Bank), Bloomberg and IMF World Economic Outlook.

Figure 9: Effects of DSSI participation



(a) Participants vs. **all** non-participants (b) Participants vs. **eligible** non-participants

Notes: The charts plot the average treatment effect on the treated (ATT) applying the generalized synthetic control method proposed by Xu (2017). Also shown are 90% confidence intervals, which are estimated based on a bootstrap procedure. The treatment is the country-specific participation in the DSSI. Panel (a) compares participants to eligible and ineligible non-participants. Panel (b) compares participants only to eligible non-participants. See the description in the main text and in Appendix A.3. Data sources: Bloomberg, Our World in Data, and IMF World Economic Outlook.

5 Discussion

Can a simple debt moratorium—without a haircut for creditors—help countries weather a negative shock? While pressing in the context of the current pandemic, this is an important question for short-term debt crises more generally.

On the one hand, a debt moratorium can provide much needed liquidity. In particular, if the crisis is perceived as temporary, liquidity provision can promote macroeconomic stability, restore market confidence, and reduce the risk of debt distress, leading to a decrease in borrowing costs. On the other hand, such debt relief could be perceived as a signal that public debt is unsustainable, triggering an increase in borrowing costs.

Our results, based on the short-term market response to a moratorium on official debt service during the Covid-19 pandemic, point to a significant and economically meaningful ($\sim$ 200 bps) decline in sovereign bond spreads for eligible countries. The evidence suggests that this effect is due to liquidity provision and cannot be explained by the contemporaneous request of IMF financing. These results imply that rapid and unconditional provision of official debt rescheduling to countries facing short-term liquidity shocks can be an effective form of financial support. In addition, our results would support the design and adoption of simple state-contingent debt instruments with floating grace periods to help poor countries mitigate their exposure to adverse shocks.

Two qualifications are important for the interpretation of our results. While the DSSI helped address the liquidity shock at the onset of the pandemic, this does not imply that it is effective in dealing with a long-lasting crisis. Repeated maturity extensions (the DSSI was extended twice after our study period) typically beget serial restructurings rather than decisively resolving a crisis (e.g., Reinhart and Trebesch, 2016). To the extent that the pandemic weighs on long-term growth prospects, the liquidity crisis can gradually evolve into a solvency crisis, particularly in the most vulnerable countries. Such a scenario would be more likely to require a debt stock reduction to reduce debt overhang and restore debt sustainability.

Second, our analysis looks at debt relief provided by the official sector resulting in a positive spillover effect on private bond markets. In October 2020 (after our study period), the same group of countries became eligible for a debt restructuring scheme with mandatory participation of the private sector. Whether a scheme with private creditor participation will also be effective in reducing borrowing costs remains an open question for future research.

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