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Life after default. Private and Official Deals

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Abstract: This paper studies the relationship between sovereign debt default and annual GDP growth distinguishing between private and official deals. Using the Synthetic Control Method to analyze 23 official and private defaulters from 1970 to 2017, we find that private defaults generate output losses both during the crisis and persisting over time. Conversely, official defaulters do not show a permanent drop in GDP per capita, neither during the crisis nor in its aftermath. Using panel data analysis to control for the creditors' loss (haircut), we confirm that official and private defaults may have different effects on GDP growth.

Keywords: Sovereign defaults, Output losses, Synthetic control method

JEL Classification: F34, G15, H63

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1 Introduction

Sovereign defaults and debt restructuring are not costless, as a sovereign's unilateral decision to stop servicing its debt implies important economic costs.[1] This is reflected in most of the sovereign debt literature, which has commonly assumed defaults costs as a government's main incentive is to honor its debt obligations.[2] The (empirical) literature on sovereign defaults, however, has generally found that costs of default are difficult to quantify and possibly short lived. Only more recently, with an emphasis on the specific aspects of debt renegotiations, new perspectives have emerged. In particular, thanks to a more precise measurement of a country's repayment record, more persistent effects of default have be detected, which are more in line with the effects of a default according to the theoretical predictions.[3]

This paper focuses on the heterogeneity of the effect of sovereign debt restructurings on economic growth. In particular, we distinguish, between *private and official debt restructurings*. By *private restructuring*, we denote a restructuring deal with private creditors (foreign banks and bondholders), while *official restructuring* stands for agreements reached with official creditors (in the Paris Club). To compare the effects of these two types of agreements, we use the Synthetic Control Method (Abadie and Gardeazabal 2003, Abadie *et al.* 2010), hereafter SCM. This method allows us to estimate the level of GDP per capita that defaulting countries would have reached in the absence of the default, by considering a weighed combination of non-defaulters (synthetic). Observing the trend of the outcome over the duration of the debt crisis, and in its aftermath, we

[1] Narrowly defined, default occurs when the debtor violates the legal terms of the debt contract (e.g., the debtor might fail to pay interest or principal within the specified grace period). This narrow definition, however, overlooks situations in which the sovereign threatens to default and creditors respond by "voluntarily" revising the contract. In recognition of this problem, credit ratings agencies like Standard and Poor's define a default as beginning either when the sovereign breaks the contract, or when the sovereign "tenders an exchange offer of new debt with less favorable terms than the original issue" (Beers and Chambers 2007). This broader definition is usually preferred and this the one we adopt in this paper.

[2] For a survey see Panizza *et al.* (2009) and Tomz and Wright (2013).

[3] Asonuma and Trebesch (2016), Asonuma *et al.* (2016), Benjamin and Wright (2009), Cruces and Trebesch (2013a), Forni *et al.* (2016), Reinhart and Trebesch (2016) and Trebesch and Zabel (2017) focused on debt crisis resolution and renegotiation from a private sector perspective. Cheng *et al.* (2017) (2018) have investigated the macroeconomic impact of sovereign debt restructurings with official-sector creditors instead.

provide measures of the effects for each country experiencing either private or official restructurings, up to ten years after the last agreement.

In the second part of the paper, applying a similar methodology to Cruces and Trebesch (2013a) to the analysis of the relationship between debt default and economic growth, we take the heterogeneity of the default episode into account by also controlling for its severity. While the SCM allows us to contrast the growth outcome of either private or official defaulters, the panel data analysis allows us to enlarge the sample by considering countries defaulting with both types of creditors. What is more, we can take the "magnitude" of the default into account. Specifically, we consider the actual present value reduction (or *haircut*) involved in the deal, as proxy for the severity of the crisis. We add to previous works by comparing the growth outcome of official and private restructuring by estimating the impact of both types of restructuring.

Debt restructurings could affect growth in at least two alternative ways. Higher private, or official, restructurings may have negative effects on growth, as the adverse spillovers of a default are likely to be more severe in hard defaults (i.e., involving higher haircuts) as compared to soft defaults (see Trebesch and Zabel 2017). Alternatively, there is the channel of debt relief operating in the opposite direction. Since higher haircuts reduce the level of government's debt substantially, such debt reduction might allow countries to exit a debt overhang, thereby improving growth prospects, as described by Krugman (1988). Thus, the overall impact of a debt restructuring on growth is theoretically ambiguous and remains an empirical question.

Our analysis contributes to the emerging literature focusing on the characteristics and the economic relevance of debt restructuring. Our specific contribution is to contrast the outcomes on growth between official and private debt agreements. In fact, little is known on the implications of debt restructurings involving official creditors, despite the role that these creditors have historically played in the resolution of sovereign debt crises (e.g., IMF 2013) and the fact that official debt accounts for a substantial share of total sovereign debt, especially in developing countries.

In particular, given the different characteristics of private and official defaulters (most importantly their different ability to access the credit market), we expect that the above mentioned trade-off between the "reputational" and the "debt-relief effect" of a debt restructuring may act differently for sovereign defaulters on private and official debt. As defaults are typically partial and vary in their duration (e.g., Arellano, Mateos-Planas and Ríos-Rull 2019), showing the heterogeneous determinants of default, as well as the heterogeneous treatment of creditors in the event of default, is important as it could help shedding light on what precisely are the costs of default to a sovereign country. To the best of our knowledge, this is the first attempt to disentangle the different effect of private vs. official restructuring.

Using the Synthetic Control Method to analyze 23 official and private defaulters over the period 1970-2017, we find that commercial and official defaults are associated to different growth outcomes. Private restructurings are associated with output losses during the debt crisis and persisting over time. In particular, up to ten years after the event, GDP per capita is, on average, 14% higher than it was at the time of default, whereas it would be 40% higher in the counterfactual scenario. Conversely, official defaulters do not show a permanent drop in GDP per capita, neither during the crisis nor in its aftermath. They may even grow more than their synthetic counterparts at the end of the debt crisis (although the effect of the restructuring is not significant).

Controlling for the severity of the default in 351 restructuring episodes, over the period 1970-2013, we confirm that private and official defaults may have different effects on GDP growth. While private defaults are generally associated with lower growth during the crisis but not in the long run (as in Trebesch and Zabel 2017), for official defaulters we do not observe a growth contraction throughout the years of the crisis and they are, on average, associated with higher growth in the long run.

We argue that the main explanation for this difference depends on the different circumstances in which private and official restructurings are provided. In particular, official restructuring are arranged within the Paris club umbrella, which is supposed to guarantee a relatively smoother

approach to the way in which deals are actually orchestrated than private ones, hence lowering the collateral damage of a default.[4] The importance of the way in which restructuring are actually arranged is confirmed by the results of both Asonuma and Trebesch (2016), Trebesch and Zabel (2017) and Asonuma *et al.* (2019), who find that, at least in the case of private deals, less confrontational (or preemptive) restructurings are associated with a lower output loss as compared to hard (non-preemptive) defaults.

Our results then point to the importance of the way in which debt restructurings are actually orchestrated, namely whether or not they may lead to some "collateral damage" on the domestic economy, such as the deterioration of a country reputation in the credit markets, which might have persistent effects. After the Greek debt restructuring of 2012, private sovereign debt has been replaced by official debt. Last June a debt relief agreement for Greece was negotiated by euro area governments without face-value reduction but leaving the door open to further debt relief (Financial Times 2018). Our results may then provide important insight for the debate on granting Greece further official debt relief in the future (Eichengreen *et al.* 2018, European Commission 2018, Eurogroup 2017) as well as for the importance of finding the right amount of face-value debt reduction to help Greece, and other economies, recover.[5]

The rest of this paper is organized as follows. Section 2 briefly describes the related literature. Section 3 introduces our data while Section 4 presents the SCM. In Section 5 we present the empirical model which takes into account the haircut size of each restructuring. Finally, Section 6 concludes.

[4] As argued by Tomz (2007) any measure that would help to reinforce the reputational mechanism between debtors and creditors are particularly important as they would be to help investors distinguish excusable defaults and inexcusable ones (e.g., Grossman and Van Huyck 1988).

[5] More generally, in response to the sovereign debt crises that shook the euro area since the end of 2009, the governments of Cyprus, Greece, Ireland and Portugal all received official funds from both the International Monetary Fund and the European Stability Mechanism (ESM). Given the limitatations in the architecture of debt in the construction of the euro area, it becomes crucial to prevent future crises (e.g., Basu and Stiglitz 2015).

2 Related Literature

The (empirical) literature analyzing sovereign defaults has mainly looked at their effects on international trade, international credit market and GDP growth. There is evidence documenting trade cost of defaults in particular for export-oriented industries (Rose 2005, Borensztein and Panizza 2010). Apparently, the access to credit market is influenced by more recent repayments but not by distant repayment history (e.g., Ozler 1993), which is also confirmed in more recent papers documenting a short-lived effect of default on spreads and market access (Borensztein and Panizza 2009, Gelos *et al.* 2011 and Panizza *et al.* 2009).[6]

Only more recently, Cruces and Trebesch (2013a) came to different conclusions, which are more in line with the effects of a default according to the theory. More specifically, by including in their analysis a measure of investors' losses (or "haircuts"), they show that restructuring involving higher haircuts are associated with significantly higher subsequent bond yield spreads and longer periods of capital market exclusion (that is credit markets do not seem to "forgive and forget," as in Bulow and Rogoff 1989b). Such different result with respect to the previous literature is remarkable and it is attributed to a more precise measurement of a country's repayment record. Therefore, their analysis does suggest that it is crucial to consider the magnitude of a default and not only its occurrence.

As the direct link between debt default and economic growth is concerned, a strong but *short-lived* negative contemporaneous effect on GDP growth is found by Sturzenegger (2004) and later confirmed by Borensztein and Panizza (2009) and De Paoli *et al.* (2009).[7] In all these cases, however, the effects specifically associated with a default (on the top of those related to the crisis

[6] Studies that instead provide empirical evidence in support to the "reputation view" include English (1996) and Tomz (2007).

[7] Using higher frequency data, Levy Yeyati and Panizza (2011) actually show that output contraction precedes default and that default episodes seem actually already to mark the beginning of the economic recovery. Furceri and Zdzienicka (2012) and Kuvshinov, and Zimmermann (2016) find, instead, long-lasting output losses after debt crises, while Tomz and Wright (2007) find a negative but surprisingly weak relationship between economic output and default on loans from private foreign creditors.

itself) are quite difficult to identify. Therefore, while there is evidence that sovereign debt defaults are negatively correlated with economic growth, there is no study finding a causal relationship going from default to growth.

Before the seminal contribution of Cruces and Trebesch (2013), the (empirical) literature on sovereign defaults had adopted a dichotomous treatment of sovereign defaults generally finding short lived effect of sovereign defaults. More recently, a new and emerging literature is devoting more attention to the heterogeneity in sovereign debt crises and to the specific analysis of debt restructuring strategies.

From a private sector perspective, Asonuma and Trebesch (2016), Forni *et al.* (2016), Reinhart and Trebesch (2016) and Trebesch and Zabel (2017) have investigated the economic consequences of debt restructurings, focusing in particular on their outcomes in terms of economic growth. Asonuma and Trebesch (2016) consider the asymmetric output costs between preemptive -that can be implemented prior to a payment default- and post-default restructurings. They find that preemptive restructurings are more frequent and quicker to negotiate, being associated with both lower haircuts and output losses.[8]

Reinhart and Trebesch (2016) focus on the effects of debt restructuirng by comparing episodes during the 1930s (official restructuring for European nations) and the 1990s (private restructuring for Latin American countries through the Brady Plan). Using a difference-in-difference approach, they find that softer forms of debt relief (e.g., obtained through maturity extensions or interest rate reductions) are not generally followed by higher economic growth, while only debt write-offs are able to improve the economic situation of debtor countries. Forni *et al.* (2016) study the impact of private agreements distinguishing between "bad" and "good" debt restructurings in terms of their impact for growth. They find that restructurings are, in general, bad for growth

[8]Similarly, Asonuma *et al.* (2016) have considered the impact of preemptive vs. post-default restructuring on the dynamics of imports and exports. They document that countries with post-default restructurings experience, on average, a more severe and protracted decline in imports and a larger fall in exports. They find additional evidence of a smaller and less prolonged decline in investment and real exchange rate in preemptive cases than in countries with post-default restructurings.

unless they allow a country to exit a default period (if they are final). In particular, debt relief is found to have the largest growth impact for countries that exit default with relatively low debt levels. Trebesch and Zabel (2017), by distinguishing between hard defaults (more confrontational) and soft defaults (adopting a consensual crisis resolutions), show that hard defaults are associated with a much steeper drop in output as compared to soft defaults. Surprisingly, however, after five years, neither high haircuts nor debtor coerciveness are associated with lower growth.[9]

As the official sector is concerned, Cheng *et al.* (2018) consider the macroeconomic impact of official restructurings. Interestingly, they build a new dataset on official debt restructurings conducted through the Paris Club, which allows them to include information on face value reduction losses for creditors and on the extent of provision of nominal debt relief in official deals (see Cheng *et al.* 2017). Their results are in line with those of Reinhart and Trebesch (2016), more specifically they show that Paris Club treatments can have a significant impact on economic growth but only in the case of debt treatment involving nominal haircuts. Moreover, their results show that countries not receiving nominal debt relief turn out to be more likely to pursue a prudent fiscal policy after the restructuring than those receiving a nominal haircut.[10]

With respect to these empirical models, even though some papers have already considered the economic consequences of restructuring involving the private sector and some others have empirically investigated the outcomes of official sector restructurings, we are the first to assess and compare the outcomes of official and private external debt restructurings altogether. More specifically, we compare official vs. private restructuring in a specification that allow us to disentangle the specific effect of private vs. official deals by simultaneously estimating the occurrence of both types of restructuring to the same country. With respect to both Cheng *et al.* (2018) and Reinhart and Trebesch (2016) our main contribution is to conduct a comparison of official vs. private restruc-

[9] More recently, Gordon and Guerron-Quintana (2018) theoretically show that growth shocks are the main determinant of whether default is hard or soft. In particular they decompose how much of the empirical correlation between default intensity and output growth is selection (i.e., persistently low output growth leads to hard defaults) and how much is causal (i.e., hard defaults literally reduce output).

[10] From a policy perspective, their results provide additional evidence to support the idea that the official sector faces a trade-off between the objectives of stimulating economic growth and of promoting fiscal prudence.

turings for a larger (and different) sample than the advanced economies in the 30's and the "Brady countries" in the '90s. In our setting we are able to confirm the positive effect on growth of a debt reduction only in the case of official defaults but we do not obtain similar results, on average, for defaulters on private debt.

The analysis of sovereign debt defaults has been neglected in the literature applying the SCM, with the notable exception of Jorra (2011), who, however, has used the SCM to analyze the heterogeneity of default costs without distinguishing between private and official defaults and considering only five countries.[11] Hence, this is the first paper which implements this method to analyze the heterogenous cost of private an official defaults.

Finally, our results would also contribute to the recent policy debate on debt restructurings (e.g., Brookings-CIEPR 2013; Eichengreen *et al.* 2018, IMF 2013, 2015a, 2015b, 2017). In particular, if defaulting on private or official debt is not found to be the same, this circumstance could be particularly instructive, for example, in the case of Greece, where private debt has been replaced by official debt.

2.1 Theoretical considerations

According to the classic theory of sovereign debt (Eaton and Gersovitz 1981, Bulow and Rogoff 1989a) defaults maybe costly due to direct punishment (mainly trade sanctions), capital market exclusion or higher cost of borrowing (the so called reputational effect). More recent models focus on the domestic effects of the defaults, which could be interpreted as bad news about the sovereign and, as a result, lead defaults to be associated with negative spillovers on investments, productivity and corporate access to both foreign credit and banking sector (Arteta and Hale 2008, Sandleris 2008, Mendoza and Yue 2012, Gennaioli *et al.* 2014).

[11] This method has been firstly applied by Abadie and Gardeazabal (2003) to study the economic cost of terrorism in the Basque countries. Other studies have analyzed the effect of liberalizations (Campos and Kinoshita 2010, Billmeier and Nannicini 2011), institutional integration (Campos *et al.* 2018), natural resource discoveries (Smith 2015, Masi and Ricciuti 2016), and civil war (Costalli *et al.* 2017).

In the theoretical literature of sovereign debt, a question whether theoretical models embed cyclical or trend GDP shocks still remains. For example, Arellano (2008) and Bi (2008) argue that the output costs of default should be like cyclical shocks (or equivalent to the short-lived effects we referred to in the Introduction). On the other hand, Aguiar and Gopinath (2006), Benjamin and Wright (2009), Yue (2010), Boz *et al.* (2011) argue for the existence of trend shocks, which are confirmed by some recent empirical and theoretical contributions (Gornemann 2014 and Paluszynski 2017).[12] The results of this paper are actually more consistent with the hypothesis of trend shocks, that is output costs which are highly persistent or even permanent.

The theoretical literature also suggests explanations for the existence of larger output costs, in the case of defaults associated with a more confrontational government behavior. More specifically, Grossman and van Huyck (1988) introduced the distinction between "excusable and inexcusable" types of defaults. High creditor losses which are not justified by a bad state of the economy could thus lead to a deterioration of a country reputation and hence to collateral damage on the domestic economy.[13] Even if private and official deals may have become, to some extent, "intertwined" (e.g., due to the "comparability of treatment" clause), defaulting on private debt is much more "visible".[14] On the contrary, an official default, which often occurs without much media coverage, is less likely to determine negative spillovers on a country's domestic economy (Shlegl *et al.* 2019).

In particular, official restructuring that are arranged within the "Paris club umbrella" are supposed to guarantee a relatively smoother approach to the way in which deals are actually orchestrated than private ones, hence lowering even further the collateral damage of a default.[15] For example, in

[12] Gornemann (2014) shows empirically that the costs of defaults are long-lived: even ten years after a default, GDP is roughly six percentage points lower than it would have been without a default. Based on this observation, he develops a small open economy model, in which a sovereign default triggers a persistent loss in GDP relative to trend through a temporary reduction in technology adoption and investment. This persistence of the GDP losses adds to the cost of a default.

[13] Trebesch and Zabel (2017) empirically find that "confrontational defaulters" are associated, on average, with a 3 to 5 percentage points lower growth rate during the crisis (depending on the sample and estimation method).

[14] The Paris Club has a comparability of treatment clause which requires a country benefitting from debt relief from Paris Club creditors to seek similar treatment from other creditors, including private ones.

[15] There might be exceptions of course. For example, in the late 1980s (1989–1994), Brady deals addressed

line with Gennaioli *et al.* (2014), who show that the spillovers of a default on domestic and foreign banks are larger the higher the haircut, there could be a channel operating through the financial sector working differently for official and private defaulters. In a companion paper (Marchesi and Masi 2019), we actually confirm that commercial and official defaults are associated to different outcomes in terms of credit risk. Contrarily to Marchesi and Masi (2019), however, here we do not find that high *private* haircuts are followed by lower growth in the post-default period, while Marchesi and Masi (2019) document that higher *private* haircuts are associated with lower agency ratings and higher sovereign bond yields up to seven years since the final deal.[16]

3 Data

In the paper, we focus on restructurings with foreign creditors, thus excluding debt restructurings that mainly affected domestic creditors. Figure1a shows the evolution over time of the relative shares of official and private external debt for all countries in our sample. As we can see, official debt accounts for a substantial share of total sovereign (external) debt. Moreover, the shares of official and private debt have remained stable over the last forty years. In light of this observation, there is still too little research on the relative treatment of official versus private defaults.

Our analysis includes developing and emerging market economies. We have selected this sample as follows. First, we excluded from the sample all advanced economies (with the exception of Greece), in order to make the sample as homogeneous as possible. Moreover, we dropped countries whose debt restructurings took place in the context of wars and state dissolution, such as Iraq, and successor states of the Socialist Republic of Yugoslavia (i.e., Kosovo, Macedonia, Bosnia and

commercial bank lending to sovereign debtors (mostly middle-income countries) involving a combination of an IMF agreement, debt-service reduction and rescheduling from commercial banks and reform effort on the debtors' side. Considering the amount of debt reduction (about 15% of the original debt) and of the "new money" ($3.62) which were actually granted, it seems quite reasonable to stress the role of the "credibility gains" associated with the adhesion to the plan (both in terms of debt reduction and of the acceptance of IMF adjustment programmes) in the improved Latin American countries financial conditions.

[16] As argued by Trebesch and Zabel (2017), emerging markets often see a quick recovery of output after financial crises, even if credit and capital flows remain depressed and market access is unfavourable.

Herzegovina and Serbia).

The resulting set of 130 countries includes 87 defaulting countries, which experienced at least one debt crisis during the sample period as well as 43 non-defaulters. Among defaulters, 57 countries experienced both private and official debt restructurings, 23 countries had only official debt restructurings (through the Paris Club) while 7 countries had only private deals. Table C1a, in the Online Appendix C, illustrates all the defaulting countries in the sample, the type of restructurings, and the debt crisis periods, whereas Table C1b lists non-defaulters.

We relied on the original dataset by Cruces and Trebesch (2013b) for the data on debt restructurings with commercial creditors.[17] This dataset provides a list of 187 distressed sovereign debt restructurings with external banks and bondholders occurred between 1970 and 2013. It includes information on the amount of debt restructured, the face value reduction, and a measure of debt relief (*Preferred Haircut HSZ*) computed by the authors considering the present value of both old and new debt instruments.

For official debt restructurings, we relied on the original dataset built by Cheng *et al.* (2017), which contains 429 sovereign debt restructurings with the Paris Club, between 1956 and 2015. Paris Club creditors may provide (official) debt treatments to debtor countries in the form of rescheduling (i.e., debt relief by postponement of debt service payments) or, in the case of concessional rescheduling, reduction in debt service obligations during a defined period (flow treatment) or as of a set date (stock treatment).[18] What is more, the authors report, for each agreement, the corresponding

[17] In August 2014, the authors provided an update of their data covering the year 2013 as well.

[18] The Paris Club is an informal forum of the most important official creditor countries and was designed to deal with the payment difficulties of debtors. The restructuring approach of the Paris Club has evolved over time. In the 1980s, negotiations took place on a case-by-case basis and focused on short-term liquidity problems, mostly implementing maturity extensions without nominal debt reduction. During the 1990s and 2000s, especially for low income countries, restructurings became increasingly concessional, including debt stock cancellations. Specifically, as low-income countries are concerned, the possibility of a partial debt stock cancellation of non-ODA debt was gradually extended from 33% of the eligible debt in 1988 (Toronto Terms) to 50% in 1991 (London Terms) and 66% in 1994 (Naples Terms). In 1996, the World Bank and the IMF have implemented the Heavily Indebted Poor Countries (or HIPC) Debt Initiative, which was first strengthened in 1999, and more recently in 2005, when, under the Multilateral Debt Relief Initiative (MDRI) multilateral institutions were encouraged to increase their specific contribution to debt reduction. Debt relief at completion point under the HIPC Initiative is provided within the HIPC Exit Terms.

terms of treatment and the face value reduction provided (if any). Following Cheng *et al.* (2017), by looking at the terms of treatment, we were able to compute the present value reduction for official deals (or haircut) and to compare this value with the corresponding haircut measure in the case of private agreements (or *Preferred Haircut HSZ)* used by Cruces and Trebesch (2013a).[19]

Since the data on private debt restructurings are available only up to 2013, our panel will span the period 1970-2013. Table 1a shows summary statistics for different subperiods in the full sample of 351 restructurings.[20]

While the average size of a private haircut, expressed in percentage points, is about 40 percent over the full sample (simple mean), when looking at the three different subperiods, we detect a sizeable increase in this amount over time. Average private haircut is more than double during the last subperiod (2002-2013), as compared to the initial period (1970-1988), and almost double with respect to the intermediate one (1989-2001). On the other hand, the average official haircut, over the full period, is 64 percent, about 33 percent higher than the average private one.[21] Looking at the three different subperiods, we also find a sizeable increase in the size of official haircuts over time. Average size during the last subperiod (2002-2013) is about 1.5 times the average haircut implemented during the initial period (1970-1988), and almost double the average size of the intermediate period (1989-2001). Similarly to private haircuts, and as documented by the different debt relief initiatives, we detect a sizeable increase in the size of official haircuts over time too. Figure 1b shows the evolution over time of private and official haircuts, while Figure 2 illustrates their frequency by size.

Table 1b presents summary statistics for different subsamples, according to a country's income.

[19] Cheng *et al.* (2017) provide a detailed overview of the different terms and report the net present value relief associated to the different Paris Club Terms of Treatment over the years. We calculated the net present value relief associated to the "ad hoc" agreements by directly looking at the Paris Club documentation. (http://www.clubdeparis.org/en/traitements).

[20] Among those, 168 episodes involved restructuring with private creditors, while 183 involved deals with official creditors.

[21] In order to make a better comparison between the two types of defaulters, we consider official restructurings up to 2013, that is the last years in which we have data on the size of private restructurings.

As the average size is concerned, we do not find big differences between countries with private or official haircuts. Low income countries obtain the most generous private and official haircuts, even if the difference in the average size, between middle and low income countries, is the highest in the case of private deals.[22] The difference is bigger when considering the number of countries involved in private or official defaults. In this case, while the number of middle income countries benefiting from some debt relief is the same considering private and official creditors, middle income countries tend to benefit much more from a private haircut than low income ones. Moreover, only one high income country (Seychelles) obtained official present value reduction, in our sample.

INSERT FIGURES 1a, 1b & FIGURE 2

INSERT TABLES 1a & 1b

3.1 Sample composition in SCM

As we explained in the introduction, in the paper we use two estimation methods to estimate the costs of default, which to some extent complement each other. The SCM allows us to find some causal evidence for the effects of default on growth, but comes at the cost of (i) not allowing us to take into account the magnitude of the default, (ii) reducing the number of countries in the sample. In fact, it does not allow us to include in the sample countries defaulting with both types of creditors, which represents the majority of episodes.[23] On the other hand, using the panel data analysis we can take the severity of the episode into account and enlarge the sample by considering countries defaulting with both types of creditors. This comes however, at the cost of finding only conditional correlations.

In the next Section, we will use the SCM to compare the outcome in growth of either private or official defaulters. Hence, in order to disentangle the effect of private and official defaults, we

[22] With the exception of only one country, Seychelles, which obtained an official haircut of 100 percent in 2009.

[23] For countries that default, at different times, with both types of creditors, it would not be possible to find the appropriate counterfactual. In principle, this would be possible only for countries that default simultaneously with both type of creditors, but they represent a too small fraction of our sample.

will examine separately the 23 countries with only official debt restructurings and the 7 countries having only private deals, respectively.[24]

As private restructurings are concerned, we added Argentina to private defaulters, given the relevance of its (private) default episode in 2001. Despite the fact that Argentina had also experienced a default involving both private and public creditors in the eighties, the interval between the two episodes is long enough to prevent us from capturing the effect of the first episode.

Among official defaulters, we had, unfortunately, to exclude eight countries from the original pool of 23 countries, for different reasons. More specifically, we excluded Cambodia and Equatorial Guinea due to data availability constraints.[25] Furthermore, since the credibility of the SCM hinges on its ability to match the pre-treatment outcome of the treated and the synthetic unit, we do not present results for Burundi and Myanmar, for which the roots of the mean square prediction errors (RMSPE) were too high to guarantee a credible estimation of the treatment effect, as explained in the next Section. Moreover, the SCM is not suited to deal with additional shocks that potentially affect the outcome in the period after the event under analysis. Thus, we cannot consider Central African Republic, Guinea-Bissau and Rwanda, whose economies were harmed by internal conflicts, and Haiti, which was stricken by a terrible earthquake just at the end of its debt crisis.

Therefore, we overall discuss eight and fifteen cases of private and official default, respectively. We consider the first year of the debt crisis as the starting point of the treatment period. This timing assumption enables us to observe what would have happened in the absence of the default both during the debt crisis and in its aftermath.

[24] We depart from the previous analysis by Jorra (2011), that considers debt crisis involving both types of restructurings, thereby discarding the heterogeneous effects coming from these two different types of defaults.

[25] The SCM requires a sufficiently long pre-treatment period with no missing values in the outcome variable for the entire period of analysis. GDP data for Cambodia and Equatorial Guinea are available from 1993 (two years before the default), and 1980 (five years before the default), respectively.

4 Synthetic Control Method

In this Section, we investigate the heterogeneity of the economic impact of private and official restructurings by constructing a counterfactual of the path of the GDP per capita for each country that dealt with either private or official creditors only. The missing counterfactual outcome is estimated using the SCM developed by Abadie and Gardeazabal (2003) and later improved by Abadie *et al.* (2010).

Contrary to other econometric approaches used to solve the "fundamental problem of causal inference," the SCM provides for the identification of heterogeneous responses of macro-policies or events (treatments) that affect macro-units in small-sample comparative studies.[26] The SCM compares the outcome of the case of interest (treated unit) with that of the control unit (synthetic). In our analysis, we evaluate the GDP per capita of defaulting countries with respect to the (weighted) GDP per capita of a sample of non-defaulters.

One of the value added features of this method is that it reduces discretion in the choice of the comparison units. Indeed, the synthetic is constituted by a weighted combination of all potential comparison units that best reproduces the characteristics of the treated country. More precisely, the SCM is a data-driven procedure that assigns a weight to each unit in the control group in order to minimize the pre-treatment differences between the treated unit and its synthetic counterpart, taking into account a set of pre-intervention variables that are relevant to predict the outcome variable (predictors). The ability to match the pre-event outcome of the treated country with that of the synthetic control is measured by the root of the mean squared prediction error (RMSPE). The lower the RMSPE, the more the synthetic resembles the characteristics of the treated country.

As an additional benefit, the SCM deals with endogeneity from omitted variable bias by accounting for the presence of unobservable time-varying confounders. When there is a large number of pre-

[26] The fundamental problem of causal inference arises when, for a given unit exposed to treatment, the alternative state of affairs in the absence of the intervention is unobservable, and therefore its effect is unidentifiable (Holland 1986).

event periods, only those units that are similar in both observed and unobserved characteristics would produce similar paths for the outcome under analysis. Thus, if the path of the outcome variable of the treated and the synthetic unit are alike over a sufficiently long pre-treatment period, the difference (gap) between the GDP per capita of a defaulting country and the synthetic in the aftermath is an unbiased estimation of the effect of the default.

Following previous macroeconomic applications of the SCM (e.g., Abadie and Gardeazabal 2003, Campos *et al.* 2018), we use GDP per capita as our outcome variable instead of GDP growth. For each treated country, the pool of potential control units encompasses no-defaulters for which data are available. We test to what extend our results are driven by any particular control country by iteratively re-estimate the synthetic outcomes, omitting in each iteration one of the country that received a positive weight (leave-one-out synthetic control).[27]

The set of predictors encompasses investment, openness, government consumption, population growth and total population, education, and terms of trade. We also add the average GDP per capita calculated in the 10 years preceding the crisis.[28] The predictors are averaged over a 10-year pre-event period, and the path of the outcome variable is observed until 2017.[29]

Finally, we validate our results by implementing in-space placebo tests, which allow us to compare the estimated treatment effect for each defaulter with all the (fake) treatment effects of control countries (Abadie *et al.* 2010). More precisely, we estimate the treatment effect assuming that each control country was affected by the default in the same year as the treated country. If the estimated effect on the defaulting country is larger than most of the effects obtained by the (fake) experiments, we can conclude that the SCM results are not driven randomly by chance.

A formal presentation of the method is provided in the Online Appendix A, whereas in the

[27] See Abadie *et al.* (2015).

[28] Our findings are robust to the exclusion of pre-treatment average of GDP per capita. However, the inclusion of these data ensures a better pre-treatment fit.

[29] The SCM requires at least one observation for each of the predictors in the pre-treatment period. To not further restrict our sample, if data are not available for a treated country, we exclude the variable from the set of predictors. Tables B1 and B2, in the Online Appendix B, show, for each country, which variables are actually used to conduct the optimization procedure.

following sub-sections, first, we describe the selected case studies. Then, we present the results obtained for each private and official defaulter. Finally, we aggregate the country-specific effects into average effects, distinguishing between the two types of restructurings.

4.1 Private default

Table B1 in the Online Appendix B reports the weights assigned to each country that constitutes the synthetic, the predictor balance obtained through the optimization procedure, and the RMSPE for sovereign defaulters on private debt. While the last rows in the table ensure transparency and reduce concerns about interpolation biases (Abadie *et al.* 2010), a comparison of the predictors in the pre-treatment period provides an appraisal of the quality of the synthetic control.[30] As can be seen, the synthetic control groups are able to reproduce the pre-treatment characteristics of the treated countries.[31] Overall, we can argue that the control units constructed by the SCM are not statistically different from their respective treated countries.[32]

The ability of the SCM to produce a reliable control unit can be also assessed from Figure 3, which displays the path of GDP per capita in each country that had private defaults (continuous lines) and its synthetic counterpart (dashed lines). Each graph shows that the synthetic unit tracks well the trajectory of GDP per capita in the country under analysis when we look at the years before the start of the debt crisis (indicated by the first vertical line). Combined with the high degree of predictor balance, this suggests that the synthetic units would continue to track the outcome of the defaulting countries if the debt crises had not happened. That is, the synthetic units provide rational estimates of the level of GDP per capita that these countries would have reached in the absence of the default.

[30] The list of weights assigned to each potential control makes it explicit that countries characterized by geographical proximity or similar risk of default contribute relatively more to the counterfactual outcome.

[31] As we explain in the Online Appendix A, the SCM assigns to each predictor a weight v, which reflects the predictive power of the variable. The values obtained from the data-driven procedure are available upon request.

[32] This is especially true considering the average GDP per capita in the period before the default, for which the difference between the treated country and its counterpart ranges from 0.001 percent (in the case of Argentina) to -0.99 percent (in the case of Paraguay). The degree of similarity is actually lower when considering the other predictors, which, however, have a lower predictive power for the outcome variable prior to the debt default.

INSERT FIGURE 3

Although Figure 3 shows that private restructurings affect defaulting countries differently, most of the cases displays a negative gap between the actual and the synthetic GDP per capita after the debt crisis, which persists (or even increases) until the final deal (indicated by the second vertical line). Dominica represents an exception, since its GDP per capita starts to diverge from the synthetic outcome at the end of the debt crisis. However, its debt crisis lasts only one year, that is the shortest duration among private defaulters.[33] Some striking differences are detectable in the post-crisis period, too. In particular, while the output losses increase in the case of Belize, Paraguay, South Africa, and Venezuela, Greece and Argentina were able to catch up with their synthetic units, even though their GDP per capita remains below the synthetic outcome for at least five and eight years from the initial restructuring, respectively.

Figure B1 in the Online Appendix B shows the results obtained through the leave-one-out procedure (l-o-o): continuous lines depicts the l-o-o synthetic outcomes, while bold and dashed lines reproduce actual and synthetic outcomes, respectively, as in Figure 3. This sensitivity test confirms that the results presented above are fairly robust to changes in the composition of the control group.[34]

Tables 2a and 2b quantifies the economic impact of a private restructuring during the debt crisis and up to ten years after the final restructuring, respectively.[35] As can be seen, the negative gap between the actual and the synthetic GDP per capita consistently increases in the case of Belize, Paraguay, and Uruguay, while it starts to decrease after the final restructuring in Argentina. On average, the annual GDP gap induced by the default during the debt crisis ranges between -21.6

[33] The duration of the debt crisis differs quite substantially, ranging from one year in the case of Dominica to eight years for South Africa and Uruguay. We consider the Greek debt crisis ongoing until the end of the sample period.

[34] The GDP per capita of Dominica, Uruguay, and Venezuela may exceed their l-o-o synthetic counterparts. However, such divergences can be explained by the lower ability of the algorithm to match the pre-event outcome of our case studies. Indeed, the leave-one-out procedure entails a lower fit between the treated and the synthetic unit in the pre-event period.

[35] The effect is calculated as the percentage difference between the observed GDP per capita and its synthetic counterfactual.

percent (Belize) and -0.6 percent (Dominica). Estimates over a larger time span provide even a stronger difference between defaulters and their synthetic units. Indeed, ten years after the final restructuring the average effect is between -32.6 percent (Belize) and -3.1 percent (Argentina). At the end of this time span, the GDP per capita of Argentina and Greece was higher than the synthetic outcomes, although by a small percentage (1.6 and 2.5 percent, respectively).[36]

INSERT TABLES 2a & 2b

Then, Figure B3, in the Online Appendix B, shows the results of the placebo tests: Panels a) depict the distributions of the placebo tests, while panels b) present the P-values for the hypothesis that the effect occurred by chance.[37] The placebo tests prove that private restructurings negatively affect the GDP of the defaulters, although heterogeneously. Considering up to ten years after the end of the debt crisis, the negative effect on the output of Argentina is statistically significant only in the fourth leads, while it is always significant during the debt crisis. The defaults seem to significantly affect Dominica and Greece only up to three and four years after the final restructuring, respectively.

4.2 Official default

Table B2, in the Online Appendix, reports the weights assigned to each country that constitutes the synthetic, and the predictor balance obtained through the optimization procedure, and the RMSPE for sovereign defaulters on official debt. As before, the comparison of the variables used to construct the control units proves that the SCM provides a good estimate of the counterfactual outcome, hence we can be confident that the SCM reduces the possible bias arising from control

[36] It is worth noting that the SCM does not take into account the plausible contagion effects originating from crisis countries on those representing their synthetic counterparts (e.g., trade partners). Therefore, the gap between the actual and the synthetic GDP per capita represents a lower bound of what would have happened in the absence of the debt crisis.

[37] Figure B4, in the Online Appendix B, shows the p-values obtained through the in space-placebo procedure during the private debt crisis.

units that do not provide a satisfying fit in terms of pre-treatment variables.[38]

Figure 4 provides a graphical confirmation of the ability of the synthetic units to match the pre-treatment outcome of the defaulting countries. For most of the cases, the synthetic GDP per capita very closely tracks the trajectory of this variable in the treated country, and the actual and the synthetic outcome start to diverge only at the end of the pre-treatment period.

INSERT FIGURE 4

As in the previous cases, defaults on official debt show heterogeneous effects across countries, too. However, none of the defaulters has a permanent reduction in the output, and, for some of them, the default seems to have a positive effect on growth.[39] More precisely, Benin, Burkina Faso, Ghana, Guatemala, Indonesia, and Mali show a level of GDP per capita almost equal than their counterparts at the end of the debt crisis (indicated by the second vertical line). Angola, Chad, and Sri Lanka catch up with their synthetic units after the resolution of the crisis. The GDP per capita of Antigua and Barbuda, Comoros, Egypt and Kyrgyz Republic is constantly below its counterfactual in the post-treatment period, but it did not drop significantly after the default, and, especially in the case of Antigua and Barbuda, the outcome smoothly increases. The level of GDP is even higher than the synthetic outcome in the case of El Salvador and Georgia. The leave-one-out procedure presented in Figure B2 in the Online Appendix B confirms that these results are not driven by the composition of the control group.[40]

Table 3a presents the economic impact of official defaults during the debt crisis.[41] In this period,

[38] In this case, the difference in the average GDP per capita in the period before the debt crisis ranges between -0.48 percent (Chad) and 0.55 percent (Georgia). The synthetic counterparts of Angola, Burkina Faso, Chad and Mali show considerable higher values for education, but this does not invalidate our analysis, given the low prediction power attributed to this specific variable.

[39] The length of the default period varies from one year (El Salvador and Sri Lanka) to fifteen years (Mali).

[40] Exceptions are Georgia, for which the path of the l-o-o synthetic outcome would be above the actual outcome, and El Salvador and Guatemala, for which the positive gap would be even higher. In this case also, it is worth saying that these results may be explained by the higher RMSPE obtained by the procedure.

[41] The effect is calculated as the percentage difference between the observed GDP per capita and its synthetic counterfactual. Figure B6, in the Online Appendix B, show the p-values obtained through the in space-placebo procedure during the official debt crisis.

Chad, Comoros, Egypt, Indonesia, and Kyrgyz Republic show an increasing negative gap between the actual and the synthetic GDP per capita. As shown in Table 3b, on average, the outcome gap induced by the default ten years after the last restructuring ranges between -33.6 percent (Kyrgyz Republic) and +32.3 percent (Georgia), with a higher degree of variation with respect to the cases of private haircuts. We should emphasize, however, that the negative gaps observed between defaulters and their synthetic units are mainly due to the better economic performance of the latter rather than to a drop in the outcome of official defaulters. Finally, the placebo tests presented in Figure B5, in the Online Appendix B, show that the (negative) effect of an official restructuring is consistently significant in the aftermath of the debt crisis only in the case of Antigua and Barbuda and Egypt.[42]

INSERT TABLES 3a & 3b

4.3 Average effects

In this sub-section, we aggregate the country-specific effects of private and official defaults into average effects in order to improve the comparability with the results obtained by applying panel data analysis. Following Cavallo *et al.* (2013), we first normalize the estimates by setting equal to 1 the GDP per capita of each treated country in the starting year of the debt crisis. Then, we pool the country specific effects of private and official defaulters, separately.

Figure 5 presents the average impacts of the default on GDP per capita of private and official defaulters. As can be seen, although both types of defaulters show a path of output that lies below the synthetic counterpart, the magnitude of the economic effect on private defaulters is far higher. Indeed, after a short-lived decline preceding the default -as observed by Levy Yeyati and Panizza (2011)- GDP per capita of private defaulters starts to increase, but less than what would have happened in the absence of the crisis. In particular, eight years after the event, when each

[42] In particular, Egypt is negatively affected starting from 5 years after the initial restructuring, whereas the impact on Antigua and Barbuda is significant throughout the duration of the debt crisis.

country had finalized its last private restructuring, GDP per capita is, on average, 10% higher than it was at the time of default, whereas it would be 33% higher in the counterfactual scenario. This gap reaches about 40%, on average, after the following ten years.

Conversely, official defaulters not only do not show a drop in the output, but they are even able to catch up their synthetic counterpart. When official agreements lead all defaulters out of the crisis, which corresponds at most to 15 years after the event, GDP per capita of affected countries is, on average, 40% higher than it was at the start of the crisis. Almost the same output would have been reached in the counterfactual scenario. In summary, we find that only countries involved in private restructurings are not able to recover their output losses in the medium-long run, while official defaulters do not show a permanent drop in GDP per capita, neither during the crisis nor in its aftermath.

INSERT FIGURE 5

5 Panel data analysis

In this Section we analyze the relationship between private and official restructuring and annual per capita GDP growth taking into account the severity of the restructuring episode. More specifically, in the panel data analysis, we use a measure of the investor losses (haircuts), in all restructurings with foreign banks and bondholders.[43] Our analysis spans the years between 1970 and 2013 and includes 130 countries. We use a fixed-effects GLS estimator in order to correct for

[43]The two measures of private and official haircut come from two different sources and are computed in two different ways. For this reason, as a robustness check, we will also consider the private and official nominal haircut, which are computed, in both cases, as the ratio of face value debt reduction to the amount of debt treated in the restructuring deal (see Reinhert and Trebesch 2016; and Cheng *et al.* 2018). Results are available on request.

heteroskedasticity across countries and obtain efficient estimates.[44] Specifically we test:

$$y_{i,t} = \alpha + \beta C_{i,t} + \gamma R_{i,t} + \delta_j FC_{i,t-j} + \theta_j FR_{i,t-j} + \lambda X_{i,t} + \eta_i + \tau_t + u_{i,t} \qquad j = 1, ...-3, -4\&5, -6\&7 \tag{1}$$

where y_{it} represents per capita growth in country i at period t, C_{it} is a dummy equal to one during the private/official debt crisis, while R_{it} denotes the amount of the private/official debt affected by each restructuring (haircut) during the crisis. FC_{it-j} is a dummy equal to one when a country has finalized its last private/official deal, while FR_{it-j} denotes the amount of private/official haircut associated to the last restructuring.[45] Finally, η_i and τ_t denote country and time dummies, respectively, which allow us to control for both unobservable country characteristics and common trends.[46]

The advantage of including both official and private restructurings in the same specification is that it allows us to detect their effects by avoiding an omitted variable bias. Moreover, we are also able to distinguish the growth variation associated with the default *per se* from that associated with the amount of the debt affected (i.e., "occurrence" versus "magnitude").

We have chosen to consider both the duration of the debt crisis and up to seven periods after the last restructuring (following Cruces and Trebesch 2013a), for at least two reasons. First, we want to be able to detect more permanent effects of a default, in line with the hypothesis of persistent output costs (e.g., Gornemann 2014). Second, we want to make as comparable as possible the results obtained using GLS to those obtained using the SCM in which we examine separately the growth pattern *during the crisis* and up to ten years *after its end*.[47] Thus, we apply our baseline

[44] A groupwise likelihood ratio heteroskedasticity test, performed on the residuals of the baseline model estimated by OLS, led to a rejection of the null hypothesis of homoskedasticity across groups (countries) for all regressions. The results (available on request) are robust to a OLS specification with standard errors clustered at the country level.

[45] Following Cruces and Trebesch (2013a), we define last restructuring as those that were not followed by another agreement within the subsequent four years.

[46] In this way we can also accounts for global factors that might have influenced the simultaneous dating choice of debt restructuring events (e.g., Baker or Brady plan in the two periods, 1985-88, and, 1989-94, respectively).

[47] In an older version of this paper (Marchesi and Masi 2018), besides using different proxies for the severity of the default, we considered up to ten lags of private/official final restructuring variables, obtaining similar results. In the

specification from the start of the debt crisis, and using duration data for both private (Asonuma and Trebesch 2016) and official debt restructuring (Cheng *et al.* 2018). We then include up to seven year lags of both the occurrence and the magnitude of the last restructuring, for both official and private deals.

Finally, our choice of control variables follows the literature on the impact of default on economic growth. More specifically, adopting the same specification as in Trebesch and Zabel (2017) (which is in turn the same of Levy Yeyati and Panizza 2011), we control for investments as a percentage of GDP, a measure of openness (exports and imports over GDP), government expenditure, annual rate of growth of population and (log of) total population, rate of variation of annual terms of trade, the percentage of the population that completed secondary education, the Freedom House index of civil liberties and a dummy for a banking crises (Laeven and Valencia 2013).

Furthermore, we include the amount of IMF net lending to control for the possibility that the different results, between private and official agreements, may depend on additional financing from the IMF that are associated with official restructurings. If an IMF programme is a *sine quo non* condition for Paris Club creditors to provide relief, not all private restructurings were associated with IMF programmes. Table C2 in the Online Appendix C provides a detailed description of each variable and its source, while Table C3 shows summary statistics.

The results of the model of equation (1) are presented in Table 4 below. In columns 1-2, we control for both the duration of private and official debt crisis, and the amount of private and official haircut. In columns 3-4, we include both private and official haircuts, expressed in percentage points, up to seven years after the final restructuring (with and without control variables, respectively).

As can be seen in column 4 of Table 4, while the coefficients of the private haircut variable are never statistically significant, the official haircut coefficients are positive and significant up to five years since the final agreement. In particular, a one standard deviation increase in official

current version, however, we opted for a more parsimonious specification (i) to avoid concerns of multicollinearity, and (ii) to follow more closely the original specification of Cruces and Trebesch (2013a).

haircut (25 percentage points in this sample) is associated to a growth rate which is about 25 percent higher in year 1 after the final restructuring, and still 23 percent higher in year 4 and 5, after controlling for our macro variables and country and time fixed effects. This is clearly an economically relevant magnitude.

In columns 5-6 of Table 4, we consider only the dummy indicating the occurrence of the private and official haircut, with and without control variables respectively. Finally, the last two columns contain the full specification, which includes both the lagged haircut and the lagged haircut dummies, with and without control variables. While all these results are reported for comparison, we largely base the discussion on the fully specified model of column 8.

INSERT TABLE 4

As our variables of interests are concerned, *during the debt crisis*, we can observe that prolonged private debt crises are associated with a significant contraction of GDP growth of about 1 to 1.8 percentage point per year, depending on the specification (all coefficients are significant at the one percent level). As the average duration of a private defaults is about 10 years in this sample, this result implies that the average GDP loss associated to private default is about 18 percentage points in total.[48]. On the other hand, we do not find any evidence of a statistically significant relationship between official default and growth, during the debt crisis. The coefficients denoting the amount of debt involved in official restructurings are also never significant at conventional levels. Thus, the different growth outcomes, after a private or an official default, are in line with those obtained using the SCM, in the previous Section.[49]

After the end of the debt crisis, in column 8, we find that, considering private agreements, the coefficients of both the lagged final haircut dummies and their lagged size are never statistically significant up to seven years since the last restructurings. This result is to some extent similar

[48] The size of this loss is similar to the one we obtained using the SCM, which was about 20 percentage points.

[49] In both cases, however, the size of the haircut (granted during the debt crisis) seems to attenuate such negative effect.

to Trebesch and Zabel (2017), who, focusing only on private restructurings, find evidence of a negative relationship between default and growth only during the default years, but not in its aftermath. The results differ, however, when examining the aftermath of an official default. In this case, the coefficients denoting the lags of the final haicut are always positive and generally significant up to seven years after the final agreement.[50]

When one looks at the *size* of the private restructuring, it should be kept in mind that the coefficients shown in the fully specified model (column 8 of Table 4) have to be interpreted conditionally, as in any interaction model. The best way to interpret the findings of Table 4 is to look at Figures 6 and 7, which show the expected variation in growth of a restructuring conditional on the haircut size, that is $\delta_j + \theta_j FR_{i,t-j}$, from equation 1 above. The different panels correspond to how many years after the restructuring growth is being measured, and the dotted lines show 90 percent confidence bands. The effects are calculated from the most demanding specification (column 8). Besides easier interpretation, this joint estimate and the resulting graphs are important because the high correlation between FC and FR complicates making inference about their individual effects, but facilitates inference about their sum (see Cruces and Trebesch 2013a).[51]

Figure 6 shows that restructurings involving private haircuts are not statistically significant in the long term. The growth increase after a private haircut would be significant for levels of haircut at which the lower confidence band is above the zero horizontal line. Since this is never the case from one to the seven years after the final private deal, we can conclude that such effect is never statistically significant.

Figure 7 reports the expected effect on growth of an official restructuring conditional on its size. The bottom line of Figure 7 is that the growth increase after an official haircut is always positive and statistically significant for years 1–7, and when the proportion of debt affected is above 40

[50] The coefficient of the third lag is the only one which fails to be significant at conventional levels.

[51] As pointed out by Cruces and Trebesch (2013a), multicollinearity does not bias least squares estimates, but the high correlation between FC and FR will tend to increase the estimated standard errors. The high correlation between FC and FR (about 0.8 in our sample) actually lowers the variance of the estimated effect of interest, $\delta + \theta FR$.

percent (i.e., the mean of this sample being about 60 percent). Thus, from year one and up to five years after final official agreements, we can see that haircuts greater than 40 percent can be associated with significantly higher growth. For year six to seven years after the restructuring, the growth increase would be significant only for much smaller haircuts (i.e., smaller than 20 percent). Indeed, the magnitude of this positive effect is declining over time and becomes almost irrelevant in the last period.

Finally, as can be seen, most of the control variables have the expected sign. Growth rates significantly increase with higher openness and decreases with higher population (in log), higher public expenditure, after the occurrence of a banking crisis and a (negative) shock to the terms of trade. The coefficients of investment, population growth, education civil liberties and IMF net loans are generally not significant.[52]

To sum up, when considering the haircut size, private restructurings are generally associated with lower growth during the crisis but not in the long run, while official restructurings do not seem to reduce growth throughout the default years, and are associated with higher growth in the long run.

We suggest that a plausible explanation for the different growth outcome of official and private defaulters may be due to the different way in which the restructurings are actually provided for the two types of defaulters. For example, using Trebesch and Zabel's terminology, official restructurings might be more similar to a "soft" default than private deals and, as such, be associated with lower economic costs. Contrarily to Trebesch and Zabel, however, who can only find an average effect, we are able to disentangle between the specific growth variation arising from these two type of defaulters. These results are also in line with both Reinhart and Trebesch (2016) and Cheng *et al.* (2018), who both find that debt treatments can have a significant impact on economic growth only when they are associated with some nominal haircut.[53]

[52] These results are similar to those obtained by Levy Yeyati and Panizza (2011).

[53] With respect to Reinhart and Trebesch (2016), however, we are able to confirm the positive effect on growth of a debt reduction only in the case of an official default, without obtaining similar results, on average, for private

As discussed above, there is a trade-off concerning the effect on growth of the amount of forgiven debt: a positive "debt relief effect" and a negative "reputational effect." Since these two effects would typically go into two opposite directions, their net effect could then explain the insignificant effect of a private haircut. On the other hand, official defaulters seem to benefit from the debt relief effect of a present value reduction (as in Arslanalp and Henry 2005).[54]

Differently than in the SCM analysis, we do not find evidence of a growth contraction, after the debt crisis, for private defaulters, while we find evidence of a boost in growth for official defaulters. This difference may depend on the fact that in the SCM we are able to isolate the effect of the specific type of default, avoiding possible confounding effects. Moreover, the SCM compares private/official defaulters with a counterfactual made by non-defaulters only, which might explain the stronger (weaker) negative (positive) effects we detect for private (official) defaults using SCM with respect to GLS.

INSERT FIGURES 6 & 7

5.1 Robustness checks

This Section aims to test the robustness of our main model of equation (1). More specifically, we try to control for the presence of (i) autocorrelated standard errors (ii) omitted variable bias, as common shocks could affect both output and haircuts, and (iii) reverse causality, since changes in output can potentially explain the type of default. The results are shown in Tables 5 and 6 below.

Autocorrelated standard errors. We address concerns of serially correlated errors by both including lagged growth in our specification and by estimating the model correcting for AR(1) autocorrelation within panels and cross-sectional heteroskedasticity across countries. In a dynamic panel with country fixed effects the lagged dependent variable is correlated with the country-

defaulters.

[54] Since most cases of official haircut concern the countries which are eligible for HIPC, such result then provides evidence of the effectiveness of official creditors' deals, at least in terms of higher GDP growth.

specific component of the error term and, thus, the OLS fixed-effects estimator produces biased estimates. However, Nickell (1981) shows that, in the AR(1) case, the bias declines as the time series dimension of the panel, T, increases. Judson and Owen (1999) testing the performance of the fixed-effects estimator on panels with typical macroeconomic dimensions find that the fixed-effects estimator performs well when $T = 30$. As in our sample $T = 34$, we expect any bias introduced by the inclusion of the lagged dependent variable to be very small. We then include growth at time $(t-1)$, in column 1 of Table 5 and, as can be seen, both sign and significance of the restructuring variables remain overall the same. The same holds when we correct for AR(1) autocorrelation within panels and cross-sectional heteroskedasticity across countries in column 3 of Table 5.

Additional controls. The results could still be biased due to the omission of time-varying country-specific variables correlated with both growth and the government payment behavior and growth, despite controlling for time and country fixed effects and standard macro controls. Following Trebesch and Zabel (2017), we include political risk (as debtor payment attitude may be affected by political crises) and control for the occurrence of currency crises (as well as the occurrence of banking crises).[55] Thus, we add the ICRG political risk indicator as well as a dummy for changes in the executive (taken from the Database of Political Institutions, DPI). Moreover, we also include inflation and the debt to GDP ratio, both taken form the World Development Indicators (WDI).[56]

In columns 2 of Table 5 we have then included the additional controls, which, however, are available only for a reduced sample. As the number of observations drops dramatically (by almost half) these results are hardly comparable as changes in the coefficients of the interest variables might be due to changes in sample size rather than to their different effect. Nevertheless, the results are

[55] Both indicators are taken from Laeven and Valencia (2013).

[56] More specifically, this specification allows us to control for several factors that may be associated to the occurrence of a sovereign debt crisis, such as proxies for countries' ability and willingness to repay (see Manasse and Roubini 2009). In particular, solvency crises are characterized by high level of external debt to GDP, together with monetary or fiscal imbalances, while liquidity crises are identified by moderate debt levels but greater political uncertainty, which, at least to some extent, can capture a country's willingness to pay.

overall similar to those of previous specifications.

Reverse causality. Reverse causality can indeed be one of the main objection to comment our result. Therefore, we test the influence of lagged growth on our explanatory variables. More specifically, in columns 1-3 of Table 6, we test the influence of lagged growth on private haircuts, while in columns 4-6 we test the influence of official ones. In all specifications we do not find any evidence that lagged growth is a good predictor for either a private or an official haircut.

Taken together, we find no evidence for reverse causality and no evidence for a confounder driving our main results. We should emphasize, however, that the results in this Section should be taken cautiously, as we explained, identification is difficult and there are competing channels, which are hard to disentangle in the data at hand. Hence, we are able to detect only strong conditional correlations but not any causal effect.

INSERT TABLES 5 & 6

6 Conclusions

This paper studies the heterogeneous effect of debt default on GDP growth by distinguishing between private and official creditors, and by taking the magnitude of the debt crisis into account. In the first part of the paper, using the SCM, we adopt a case study analysis of 23 official and private defaulters from 1970 to 2017 to investigate the heterogeneous response of GDP per capita to private and official restructurings. We find that countries involved in only private restructurings are not able to recover their output losses in the medium-long run, whereas official restructurings do not have an impact on defaulters' GDP per capita.

In the second part of the paper, by estimating a panel of 130 countries, over the years 1970-2013, we confirm that official and private defaults may have different effects on GDP growth. Controlling for the haircut size, we find that private defaulters are penalized in terms of lower growth during

the debt crisis, but not in its aftermath. For official defaulters, instead, growth is not affected during the crisis and it is generally increasing up to seven years since the final haircut. Hence, the trade-off concerning the effects of sovereign debt restructurings seems to be associated with opposite outcomes for private and official defaulters. For the former, we find no effect on growth of a present value reduction, for the latter, an official haircut generally increases long term growth. This evidence then suggests that for private defaulters positive and negative spillovers of a debt reduction overall compensate each other, while for official defaulters positive spillovers seem to prevail. Taken together our results point to confirm that official and private defaults may have different effects and should then be treated differently.

In line with Asonuma and Trebesch (2016) and Trebesch and Zabel (2017), our results points to the importance of the way in which debt restructurings are actually, orchestrated, namely whether or not they are associated with more or less confrontational relationship between creditors and debtors, which might have persistent effects. To the extent that Paris Club deals (but also Brady deals) may represent an example of a "soft" default, this evidence suggests that they are associated with higher growth rates over the long term. Since most cases of official haircut concern the countries which are eligible for the Heavily Indebted Poor Country (HIPC) Initiative, this paper then provides evidence of the effectiveness of official creditors' deals, at least in terms of higher GDP growth. These results might also provide important insight for the current debate on providing Greece with an official debt relief in the future.

The analysis is of course limited in several respects. In the panel analysis, we do emphasize that the direction of causality in the relationship between sovereign defaults and growth raises some questions and thus a robust association between debt defaults and lower (or higher) growth can only be indicative of a correlation between the two variables. Therefore, both the determinants and the effects of a debt restructuring should be more carefully investigated. Nevertheless, these concerns are, at least partially, overcome by the results obtained using the SCM, which specifically deals with the endogeneity due an omitted variables bias by accounting for the presence of

unobservable time-varying confounders.

Finally, in the paper we do not actually analyze the underlying channels which should explain the difference between private and official defaults. We plan to explore the reasons for these differences and the mechanisms through which growth may be affected, such as trade, domestic costs or access to credit markets. In a companion paper (Marchesi and Masi 2019), we take different proxies for credit risk measures, such as rating agencies as well as bond yield spreads (EMBIG). By controlling for both the occurrence and the magnitude of debt defaults, we find that private defaults seem to involve some reputational costs up to seven years since the last agreement, while official defaulters are not affected (or may even benefit) by the restructuring episodes.

This evidence may suggest that the positive growth prospects for official defaulters (in particular after the end of the debt crisis), might be due to the absence of a negative stigma in the credit market. On the other hand, private defaults, being more visible and more confrontational (at least on average) than official ones, are more likely to badly affect debtors' reputation in the credit markets, in line with the distinction between "excusable and inexcusable" types of defaults (Grossman and van Huyck 1988, Tomz 2007).

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Tables

Table 1a: Private and Official Restructurings and haircuts over time (in %)

	Observations	Mean	SD	Min	Max
Private Haircut					
1975-1988	85	26	20	0	100
1989-2001	62	51	29	0	100
2002-2013	21	57	29	6	97
Official Haircut					
1975-1988	4	33	0	33	33
1989-2001	117	58	20	12	100
2002-2013	62	81	23	4	100

Table 1b: Private and Official Restructurings and Haircuts by country's income

Private Haircuts *(Average size %)*		
High Income	Middle Income	Low Income
31	36	54
Private Haircuts (# of countries)		
High Income	Middle Income	Low Income
7	43	14
Official Haircut *(Average size %)*		
High Income	Middle Income	Low Income
100	64	66
Official Haircut (# of countries)		
High Income	Middle Income	Low Income
1	24	24

Table 2a: Private restructurings: effects during the debt crisis

	t_1	t_2	t_3	t_4	t_5	t_6	t_7	t_8
Argentina	-21.44	-16.30	-12.94	-8.48				
Belize	-14.82	-17.65	-19.07	-21.11	-23.92	-25.53	-29.10	
Dominica	-0.63							
Greece	-10.07	-5.54	-2.65	-3.11	2.49	-	-	-
Paraguay	-1.42	-5.99	-6.93	-10.43	-13.33	-15.94	-15.03	
South Africa	6.70	5.32	1.87	1.97	-5.91	-13.39	-19.90	-21.47
Uruguay	-17.16	-15.55	-14.69	-13.44	-17.28	-17.77	-21.24	-21.99
Venezuela	0.44	4.52	-1.53	-3.76	-7.93	-17.41	-18.99	

Notes: For each country, the percentage effect is given by the percentage difference between the observed GDP per capita and its synthetic counterfactual during the debt crisis. Dashes indicate no estimation is available.

Table 2b: Private restructurings: effects *n* years after the end of the debt crisis

	t_1	t_2	t_3	t_4	t_5	t_6	t_7	t_8	t_9	t_{10}
Argentina	-4.41	-1.74	-3.85	-11.63	-6.76	-1.00	-0.71	1.58	-2.02	-0.34
Belize	-29.80	-30.26	-33.62	-36.63	-	-	-	-	-	-
Dominica	-4.45	-6.17	-5.58	-1.78	2.83	1.75	-0.68	-5.83	-10.05	-9.30
Greece	-10.07	-5.54	-2.65	-3.11	2.49	-	-	-	-	-
Paraguay	-14.62	-13.41	-16.77	-18.27	-19.12	-24.16	-29.78	-31.62	-34.26	-34.67
South Africa	-22.58	-22.73	-21.48	-22.43	-21.55	-18.88	-20.25	-18.77	-16.85	-18.89
Uruguay	-19.73	-20.01	-17.42	-20.17	-16.40	-12.58	-5.57	-6.46	-13.07	-16.41
Venezuela	-18.87	-19.00	-21.80	-26.70	-25.20	-26.95	-25.86	-23.90	-29.36	-31.99

Notes: For each country, the percentage effect is given by the percentage difference between the observed GDP per capita and its synthetic counterfactual up to ten years after the end of the debt crisis. Dashes indicate no estimation is available.

Table 3a: Official restructurings: effects *during* the debt crisis

	t_1	t_2	t_3	t_4	t_5	t_6	t_7	t_8	t_9	t_{10}	t_{11}	t_{12}	t_{13}	t_{14}	t_{15}
Angola	-0.27	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Antigua	-16.60	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Benin	-7.92	-9.34	-5.93	-4.62	-10.02	-8.12	-11.91	-11.06	-11.92	-9.88	-7.01	-6.57	-1.61	4.86	-
Burkina Faso	-8.88	-9.62	-14.41	-13.33	-8.87	-8.17	-5.71	-3.91	-8.48	-7.96	-6.35	-	-	-	-
Chad	-5.69	-2.57	-1.72	-22.01	-18.47	-22.52	-25.67	-26.31	-26.57	-31.12	-37.21	-34.22	-	-	-
Comoros	-10.49	-15.61	-20.46	-22.99	-	-	-	-	-	-	-	-	-	-	-
Egypt	-14.34	-19.31	-21.91	-26.83	-	-	-	-	-	-	-	-	-	-	-
El Salvador	3.59	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Georgia	-0.58	7.25	9.41	-	-	-	-	-	-	-	-	-	-	-	-
Ghana	-0.51	0.54	0.39	-0.60	-0.63	-1.47	-2.18	-4.74	-	-	-	-	-	-	-
Guatemala	3.05	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Indonesia	3.61	4.29	4.24	-9.73	-12.93	-14.85	-13.65	-13.22	-13.97	-15.19	-14.26	-	-	-	-
Kyrgyz Republic	-16.72	-19.37	-27.30	-	-	-	-	-	-	-	-	-	-	-	-
Mali	5.48	-2.17	4.04	-5.61	-7.48	-8.97	-12.29	-11.28	-11.13	-8.33	-6.78	-11.91	-3.92	-4.81	-2.36
Sri Lanka	-16.88	-	-	-	-	-	-	-	-	-	-	-	-	-	-

Notes: For each country, the percentage effect is given by the percentage difference between the observed GDP per capita and its synthetic counterfactual during the debt crisis. Dashes indicate no estimation is available.

Table 3b: Official restructurings: effects *n* years after the end of the debt crisis

	t_1	t_2	t_3	t_4	t_5	t_6	t_7	t_8	t_9	t_{10}
Angola	-0.27	-5.87	-18.01	-39.86	-41.04	-34.39	-28.61	-26.21	-24.91	-25.75
Antigua	-16.60	-17.36	-21.16	-20.25	-19.63	-17.58	-17.95	-	-	-
Benin	6.85	4.89	4.00	4.66	8.64	2.94	-3.50	-9.85	-14.14	-13.84
Burkina Faso	-4.36	-6.08	-3.93	-4.06	-5.04	-5.84	-9.24	-10.76	-10.68	-11.46
Chad	-32.51	-27.59	-10.43	-2.05	-10.31	-15.16	-19.33	-21.45	-17.26	-23.57
Comoros	-24.98	-27.17	-28.74	-30.78	-	-	-	-	-	-
Egypt	-28.28	-30.68	-32.56	-34.74	-35.76	-34.08	-28.73	-29.19	-28.79	-28.09
El Salvador	3.59	5.54	13.60	17.47	19.77	18.99	19.88	21.49	23.35	21.70
Georgia	14.08	17.29	23.58	20.56	24.36	31.38	41.56	45.16	49.35	55.96
Ghana	-5.19	-6.56	-9.13	-5.87	-5.47	-4.53	2.69	5.21	5.55	3.27
Guatemala	3.05	3.64	3.51	4.07	6.75	10.43	9.80	9.65	9.39	7.40
Indonesia	-15.92	-16.57	-15.49	-14.17	-14.90	-14.07	-12.59	-11.74	-12.19	-12.41
Kyrgyz Republic	-32.32	-34.19	-33.90	-28.29	-31.85	-32.02	-37.08	-34.25	-35.49	-36.60
Mali	-6.17	-6.95	-9.73	-14.06	-17.05	-18.30	-20.29	-24.16	-30.45	-32.96
Sri Lanka	-16.88	-20.39	-21.95	-18.94	-17.51	-14.41	-10.03	-10.18	-8.92	-5.66

Notes: For each country, the percentage effect is given by the percentage difference between the observed GDP per capita and its synthetic counterfactual up to ten years after the end of the debt crisis. Dashes indicate no estimation is available.

Table 4: Private and Official Haircut and Growth, 1970-2013, GLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Default Duration	-1.510***	-1.284***	-1.767***	-1.245***	-1.716***	-1.119***	-1.796***	-1.161***
	(-6.517)	(-4.172)	(-6.860)	(-3.563)	(-6.657)	(-3.214)	(-6.869)	(-3.271)
Private Haircut	0.026***	0.030***	0.024***	0.029***	0.025***	0.029***	0.024***	0.028***
	(3.898)	(3.477)	(3.542)	(3.261)	(3.563)	(3.231)	(3.478)	(3.164)
Official Default Duration	-0.452**	-0.268	-0.180	0.054	-0.264	-0.237	-0.167	-0.010
	(-2.153)	(-1.041)	(-0.784)	(0.184)	(-1.184)	(-0.849)	(-0.714)	(-0.032)
Official Haircut	0.016***	0.017***	0.017***	0.018***	0.017***	0.019***	0.017***	0.020***
	(3.553)	(3.192)	(3.541)	(3.293)	(3.500)	(3.540)	(3.411)	(3.523)
Final Priv. Haircut Dummy (-1)					-0.007	-0.001	-0.022	-0.010
					(-0.885)	(-0.072)	(-1.281)	(-0.431)
Final Priv. Haircut Dummy (-2)					-0.003	0.010	-0.005	0.015
					(-0.398)	(1.072)	(-0.263)	(0.710)
Final Priv. Haircut Dummy (-3)					0.002	0.013	0.020	0.034*
					(0.211)	(1.386)	(1.126)	(1.661)
Final Priv. Haircut Dummy (-4&5)					-0.010	0.004	0.015	0.022
					(-1.635)	(0.596)	(1.161)	(1.417)
Final Priv. Haircut Dummy (-6&7)					-0.010*	0.003	0.000	0.012
					(-1.718)	(0.400)	(0.004)	(0.818)
Final Off. Restr. Dummy (-1)					0.014	0.006	0.000	-0.005
					(1.643)	(0.833)	(0.027)	(-0.465)
Final Off. Restr. Dummy (-2)					0.014	0.012	-0.001	0.000
					(1.568)	(1.635)	(-0.125)	(0.011)
Final Off. Restr. Dummy (-3)					0.015*	0.026***	0.022*	0.037***
					(1.694)	(3.700)	(1.884)	(3.470)
Final Off. Restr. Dummy (-4&5)					0.003	0.012*	-0.002	0.001
					(0.395)	(1.756)	(-0.226)	(0.152)
Final Off. Restr. Dummy (-6&7)					0.005	-0.004	-0.003	-0.019**
					(0.689)	(-0.623)	(-0.271)	(-1.998)
Final Private Haircut (-1)			-0.264	-0.036			0.808	0.420
			(-0.556)	(-0.060)			(0.778)	(0.292)
Final Private Haircut (-2)			-0.320	0.218			-0.066	-0.369
			(-0.669)	(0.387)			(-0.062)	(-0.297)
Final Private Haircut (-3)			-0.271	0.243			-1.365	-1.533
			(-0.566)	(0.435)			(-1.266)	(-1.239)
Final Private Haircut (-4&5)			-0.978***	-0.210			-1.814**	-1.406
			(-2.733)	(-0.490)			(-2.332)	(-1.479)
Final Private Haircut (-6&7)			-0.755**	-0.034			-0.761	-0.745
			(-2.143)	(-0.085)			(-0.992)	(-0.866)
Final Official Haircut (-1)			1.129***	0.989**			1.085*	1.163*
			(2.606)	(2.058)			(1.849)	(1.830)
Final Official Haircut (-2)			1.154***	0.926**			1.266**	1.163*
			(2.594)	(2.088)			(2.134)	(1.806)
Final Official Haircut (-3)			0.336	0.782*			-0.312	-0.490
			(0.756)	(1.772)			(-0.525)	(-0.768)
Final Official Haircut (-4&5)			0.334	0.928***			0.454	1.037**
			(0.993)	(2.640)			(1.032)	(2.192)
Final Official Haircut (-6&7)			0.464	0.486			0.550	1.173**
			(1.377)	(1.343)			(1.261)	(2.401)
Investment (-1)		0.012		0.011		0.013		0.011
		(0.675)		(0.638)		(0.706)		(0.625)
(delta) Population (-1)		-0.125		-0.198		-0.180		-0.212
		(-0.742)		(-1.171)		(-1.064)		(-1.250)
Secondary Education (-1)		0.010		0.011		0.008		0.010
		(1.422)		(1.539)		(1.036)		(1.338)
(log) Popolation (-1)		-3.257***		-3.472***		-3.525***		-3.408***
		(-2.972)		(-3.162)		(-3.155)		(-3.073)
Government Consumption (-1).		-0.068***		-0.068***		-0.067***		-0.066***
		(-3.221)		(-3.230)		(-3.192)		(-3.154)
Civil Liberties (-1)		-0.051		-0.033		-0.036		-0.029
		(-0.584)		(-0.377)		(-0.410)		(-0.321)
(delta) Terms of Trade(-1)		-0.0001***		-0.0001***		-0.0001***		-0.0001***
		(-2.607)		(-2.595)		(-2.613)		(-2.644)
Openness (-1)		0.029***		0.032***		0.031***		0.033***
		(4.739)		(5.170)		(5.017)		(5.249)
Banking Crises (-1)		-2.081***		-2.071***		-2.164***		-2.107***
		(-4.769)		(-4.736)		(-4.908)		(-4.781)
IMF net loans (-1)		-0.079		-0.062		-0.045		-0.052
		(-1.244)		(-1.064)		(-0.835)		(-0.944)
Observations	4,905	2,239	4,905	2,239	4,905	2,239	4,905	2,239
Number of country_id	130	91	130	91	130	91	130	91
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

Notes: This table shows coefficients of an unbalanced panel data regression with GLS fixed effects at the country-year-level. Country and year-fixed effects are included. t statistics are in brackets. Significance levels: *0.10, ** 0.05, *** 0.01.

Table 5: Robustness check for Private and Official Haircut, 1970-2013, GLS

	(1)		(2)		(3)	
Private Default Duration	-0.959***	(-2.837)	-1.019**	(-2.519)	-1.112***	(-2.836)
Private Haircut	0.024***	(2.793)	0.014*	(1.842)	0.022**	(2.573)
Official Default Duration	0.094	(0.330)	0.201	(0.638)	-0.069	(-0.212)
Official Haircut	0.014***	(2.658)	0.011**	(2.130)	0.015***	(2.885)
Final Priv. Haircut Dummy (-1)	0.682	(0.490)	-0.556	(-0.433)	0.039	(0.028)
Final Priv. Haircut Dummy (-2)	-0.732	(-0.621)	-1.188	(-1.209)	-0.491	(-0.403)
Final Priv. Haircut Dummy (-3)	-1.390	(-1.192)	-1.801*	(-1.862)	-1.745	(-1.436)
Final Priv. Haircut Dummy (-4&5)	-1.331	(-1.455)	-2.158***	(-2.809)	-1.621	(-1.602)
Final Priv. Haircut Dummy (-6&7)	-0.073	(-0.089)	-0.395	(-0.581)	-0.661	(-0.731)
Final Private Haircut (-1)	-0.014	(-0.640)	0.005	(0.274)	-0.004	(-0.179)
Final Private Haircut (-2)	0.021	(1.031)	0.032*	(1.898)	0.017	(0.849)
Final Private Haircut (-3)	0.031	(1.640)	0.038**	(2.330)	0.039*	(1.939)
Final Private Haircut (-4&5)	0.021	(1.374)	0.038***	(2.960)	0.028*	(1.659)
Final Private Haircut (-6&7)	0.003	(0.190)	0.006	(0.506)	0.013	(0.789)
Final Off. Restr. Dummy (-1)	1.074*	(1.749)	0.892	(1.524)	1.051*	(1.682)
Final Off. Restr. Dummy (-2)	0.815	(1.320)	0.857	(1.489)	0.975	(1.525)
Final Off. Restr. Dummy (-3)	-0.757	(-1.237)	-0.338	(-0.588)	-0.414	(-0.656)
Final Off. Restr. Dummy (-4&5)	0.947**	(2.087)	0.930**	(2.253)	0.928*	(1.829)
Final Off. Restr. Dummy (-6&7)	0.871*	(1.865)	1.118***	(2.686)	0.850*	(1.671)
Final Official Haircut (-1)	-0.004	(-0.375)	0.003	(0.240)	-0.004	(-0.393)
Final Official Haircut (-2)	0.008	(0.742)	0.005	(0.433)	0.001	(0.127)
Final Official Haircut (-3)	0.041***	(3.924)	0.021*	(1.805)	0.035***	(3.191)
Final Official Haircut (-4&5)	-0.004	(-0.474)	-0.004	(-0.500)	0.001	(0.064)
Final Official Haircut (-6&7)	-0.016*	(-1.711)	-0.025***	(-2.987)	-0.015	(-1.588)
Investment (-1)	-0.023	(-1.294)	-0.079***	(-3.358)	-0.004	(-0.179)
(delta) Population (-1)	-0.078	(-0.469)	-0.923***	(-3.807)	-0.150	(-0.775)
Secondary Education (-1)	0.008	(1.037)	0.007	(0.741)	0.007	(0.863)
(log) Popolation (-1)	-2.364**	(-2.237)	4.425***	(2.980)	-3.272**	(-2.555)
Government Consumption (-1)	-0.036*	(-1.954)	-0.036	(-1.452)	-0.052**	(-2.325)
Civil Liberties (-1)	-0.036	(-0.431)	-0.238*	(-1.931)	-0.051	(-0.514)
(delta) Terms of Trade (-1)	-0.0001***	(-2.735)	-0.0001**	(-2.008)	-0.0001***	(-2.656)
Openness (-1)	0.024***	(4.193)	0.022***	(3.037)	0.031***	(4.548)
Banking Crises (-1)	-1.934***	(-4.526)	-2.008***	(-4.527)	-2.002***	(-4.820)
IMF net loans (-1)	-0.002	(-0.047)	-0.008	(-0.143)	-0.001	(-0.012)
Currency Crises (-1)			-0.080	(-0.207)		
Gov. Change (-1)			-0.611***	(-3.236)		
Inflation (-1)			8.606***	(7.291)		
(Absence of) Political risk (-1)			0.019	(1.345)		
Debt to GDP (-1)			0.002	(1.231)		
Growth (-1)	0.245***	(12.568)				
Observations	2,652		1,393		2,649	
Number of country_id	111		75		107	
Country FE	YES		YES		YES	
Year FE	YES		YES		YES	

Notes: We include the lagged dependent variable (in column 1) and further control variables (in column 2), while in column 3 we correct for AR(1) autocorrelation within panels and cross-sectional heteroscedasticity across countries. t-statistics are in brackets.
Significance levels *** p<0.01, ** p<0.05, * p<0.1

Table 6: Reverse Causality, GLS

	Dependent variable: Priv. Haircut			Dependent variable: Off. Haircut		
	(1)	(2)	(3)	(4)	(5)	(6)
Growthpc (-1)	0.000	0.000	0.000	-0.002	-0.001	0.000
	(0.012)	(0.052)	(0.052)	(-0.349)	(-0.100)	(0.050)
Growthpc (-2)		-0.002	-0.002		-0.005	-0.007
		(-0.279)	(-0.283)		(-0.965)	(-1.199)
Growthpc (-3)			-0.000			0.003
			(-0.028)			(0.497)
Observations	1,764	1,759	1,754	1,764	1,759	1,754
Country FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Macro controls	YES	YES	YES	YES	YES	YES
Number of id	99	99	99	99	99	99

Notes: In columns 1-3 the dependent variable is private haircut, while in columns 4-6 it is official haircut. t statistics are in brackets. Significance levels *** p<0.01, ** p<0.05, * p<0.1.

Figures

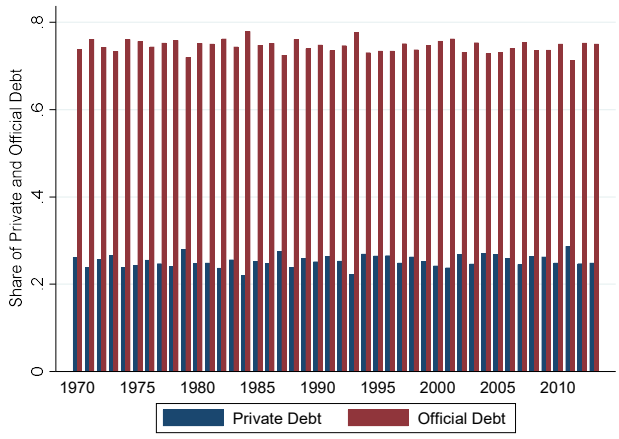


Figure 1a: Share of private and official debt

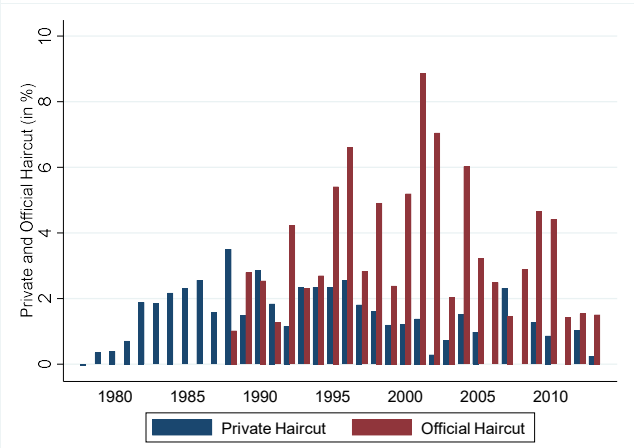


Figure 1b: Share of private and official haircut

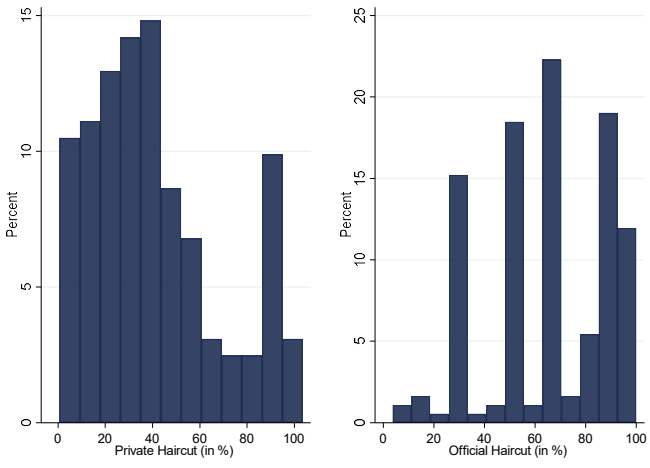


Figure 2: Frequency by size of private and official haircut

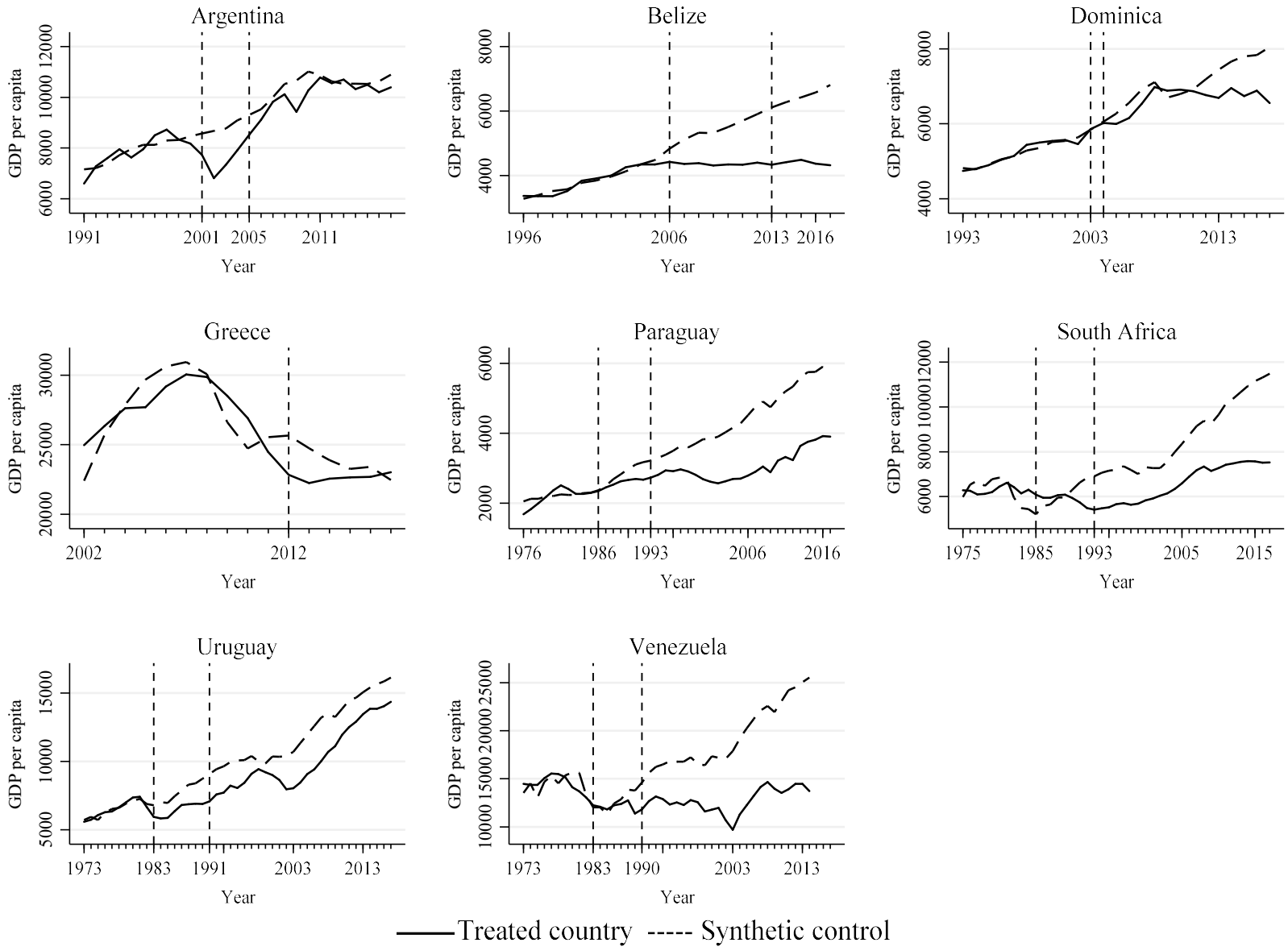


Figure 3: Private defaulters: evolution of GDP per capita, treated vs synthetic.

Notes: In each graph, the continuous line represents the trend of GDP per capita for the defaulting country, while the dashed line shows the trend for the synthetic control. The composition of each synthetic unit is reported in Table 2.

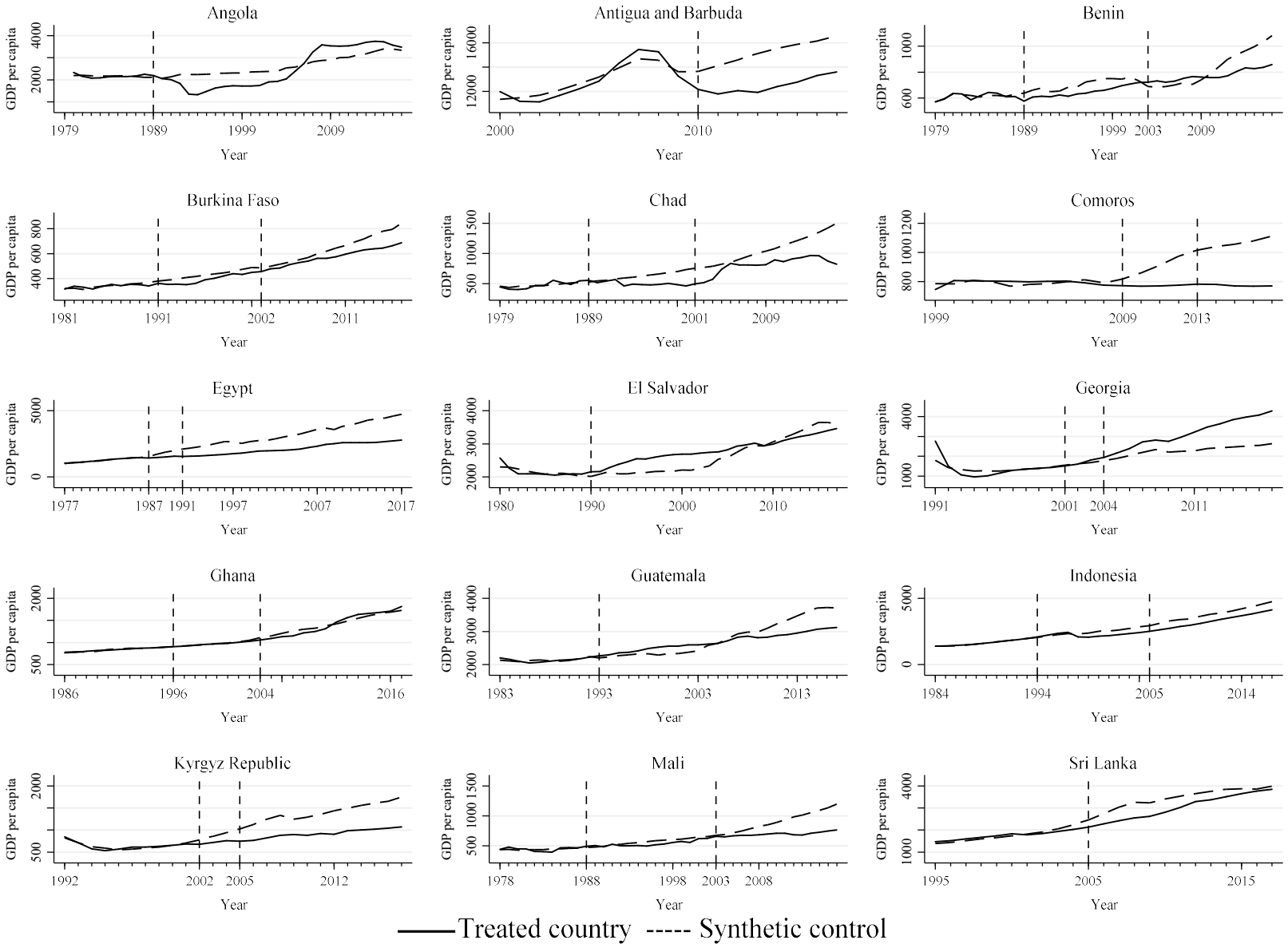


Figure 4: Official defaulters: evolution of GDP per capita, treated vs synthetic.

Notes: In each graph, the continuous line represents the trend of GDP per capita for the defaulting country, while the dashed line shows the trend for the synthetic control. The composition of each synthetic unit is reported in Table 4.

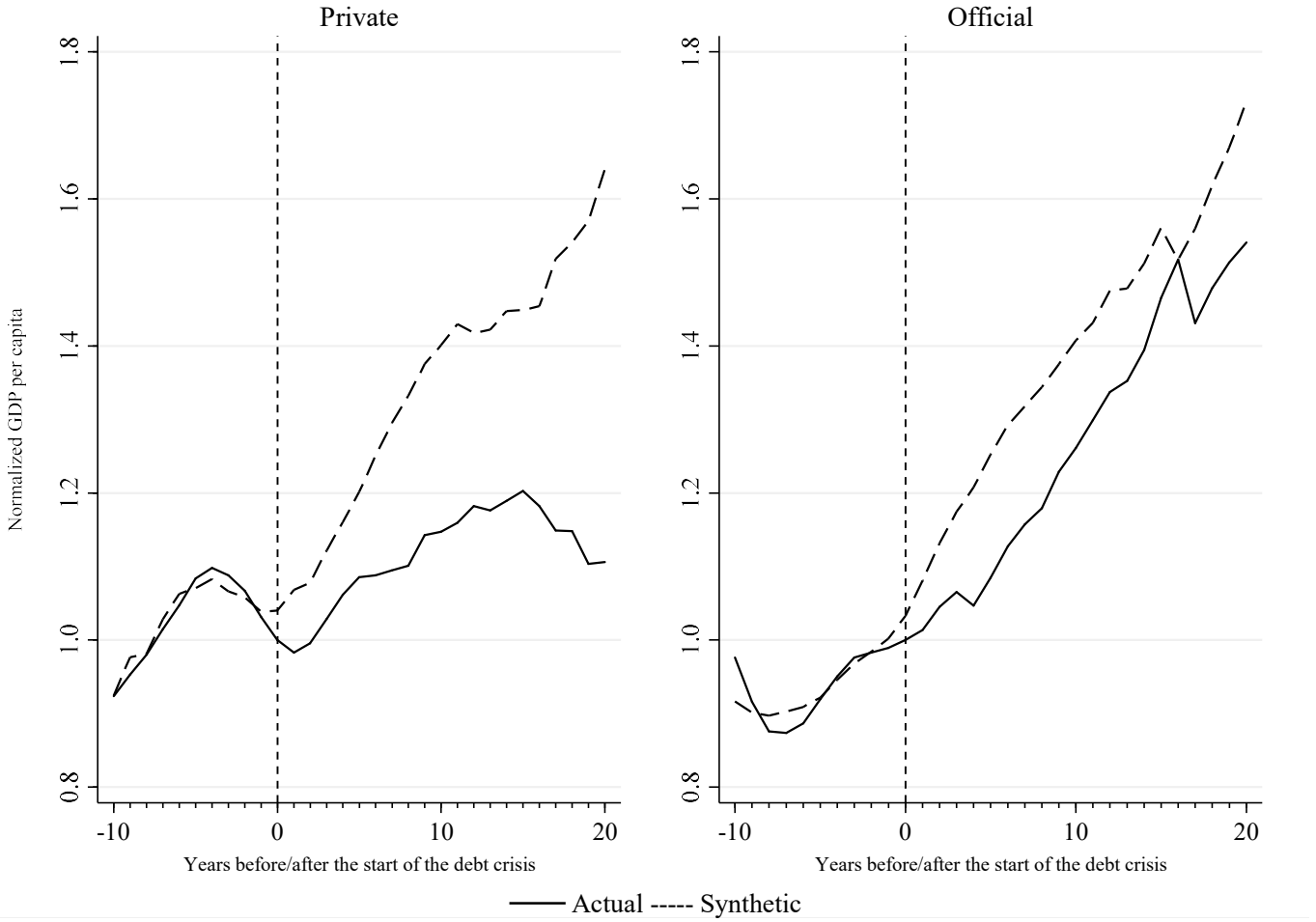


Figure 5: Average effects on private and official defaulters.

Notes: In each graph, the continuous line represents the average GDP per capita for the defaulting countries, while the dashed line shows the average outcome for the synthetic countries. GDP per capita is normalized to 1 in period 0.

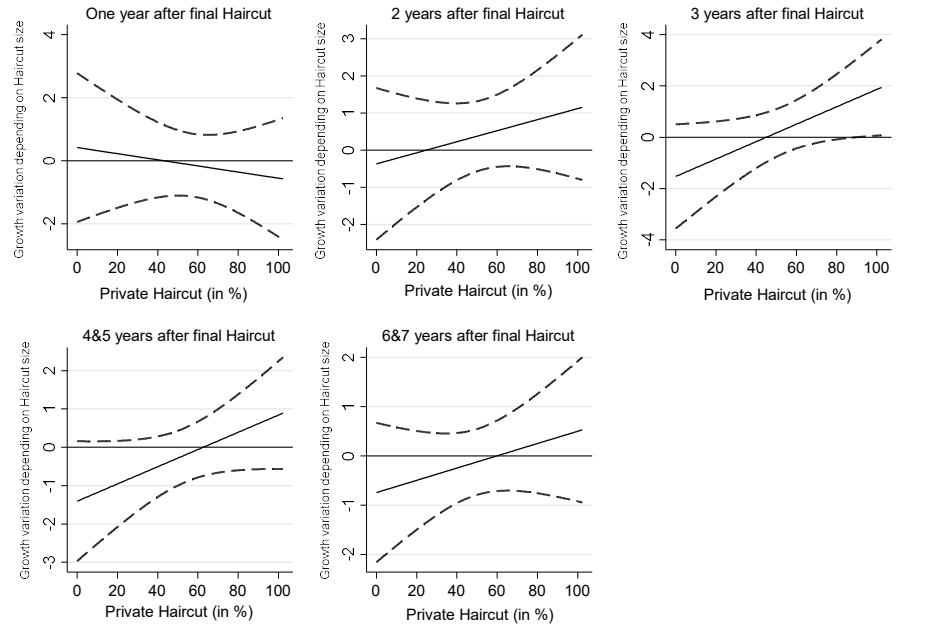


Figure 6: Expected effect on growth for different levels of private haircuts.

Notes: Each graph shows the marginal effect of private haircuts on growth for different haircut size and at different lag lengths. The dashed lines show 90 percent confidence bands. The effects are calculated using the coefficients from Table 6, column 8.

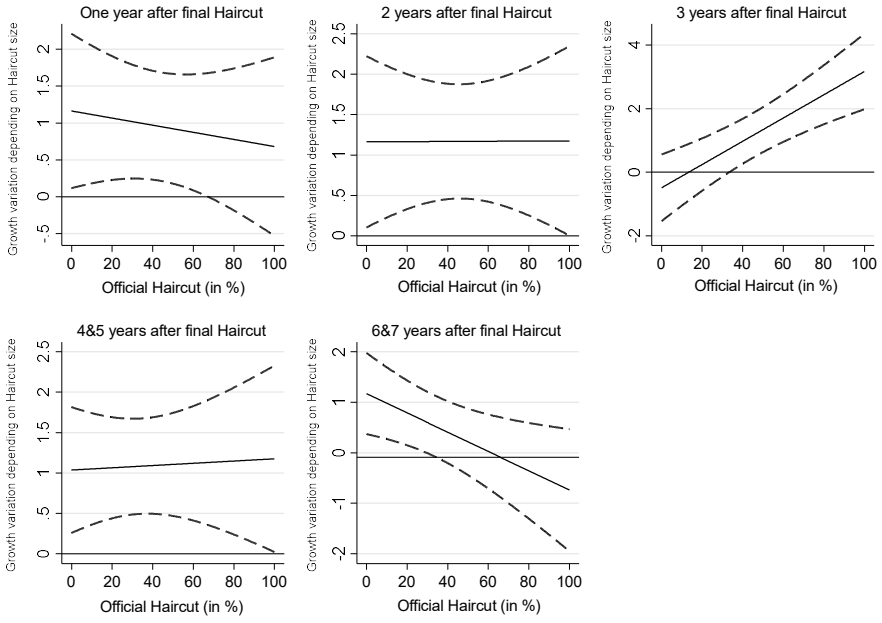


Figure 7: Expected effect on growth for different levels of official haircuts

Notes: Each graph shows the marginal effect of official haircuts on growth for different restructuring size and at different lag lengths. The dashed lines show 90 percent confidence bands. The effects are calculated using the coefficients from Table 6, column 8

Online Appendices

Online Appendix A: A Formal Discussion of the Synthetic Control Method

Online Appendix B: Additional Tables and Figures

Online Appendix C: Sample and Variable Description

Online Appendix A

The SCM provides quantitative inference in small-sample comparative studies by estimating the counterfactual situation of one or several aggregate entities in the absence of an event or intervention (Abadie and Gardeazabal 2003; Abadie *et al.* 2010). To frame the SCM in the context of our study, assume that there is a balanced panel of $I+1$ countries indexed by i and observed over T years. Among these, country $i=1$ has a debt restructuring (treated country) at time $T_0 < T$, whereas the remaining I countries are non-defaulters (control group). The effect of this event is given by:

$$\beta_{1t} = Y_{1t} - Y_{1t}^N \tag{A1}$$

where $t > T_0$, Y_{1t} is the observed (actual) outcome of country $i=1$ for a post-default period t, and Y_{1t}^N is the unobservable potential (synthetic) outcome, that is the GDP per capita that would have been observed in the absence of the debt restructuring. The SCM estimates Y_{1t}^N by defining a weighted average of all countries in the control group (synthetic), and the estimator of β_i at time t is given by the difference between the actual and the synthetic outcome at that period:

$$\widehat{\beta}_{1t} = Y_{1t} - \sum_{i=2}^{I} w_i Y_{it} \tag{A2}$$

The weights w_i attached to each country in the control group are chosen such that the characteristics of the defaulting country in the pre-event period are best reproduced by the characteristics of the synthetic unit. Formally, the vector W^* containing the weights assigned to each control unit minimises the following sum:

$$\sum_{k=1}^{K} v_k (X_{1k} - X_{0k}W)^2, \qquad s.t.\ w_i \geq 0 \text{ and} \sum_{i=2}^{I} w_i = 1 \tag{A3}$$

where X_{1k} and X_{0k} are vectors the pre-event variables (predictors) that are relevant to predict

the GDP per capita, for the defaulter and non-defaulter, respectively, and v_k is a weight that reflects the predictive power of variable k. The weights v_k are chosen to minimise the mean squared prediction error (MSPE), that is the expected squared distance between the outcome of the treated country and the outcome of the synthetic in the pre-event period,

$$MSPE = \frac{1}{T_0} \sum_{t<T_0} (Y_{it} - \sum_{i=2}^{I+1} w_i Y_{it})^2 \tag{A4}$$

To achieve lower MSPE, we implemented the nested optimisation procedure that searches among all the positive semi-definite and diagonal matrices V and all the sets of W for the best fitting convex combination of the units in the control group. The nested optimization procedure is implemented by the Stata module synth (Abadie *et al.* 2011). To ensure that the global minimum in the parameter space has been found, we run the nested optimisation using three different starting points of V: the regression-based V, the equal V weights, and a third procedure that uses the Stata maximum likelihood search.

Online Appendix B

Table B1: Private restructurings: predictor balance, RMSPE, and country weights

	Argentina		Belize		Dominica		Greece	
	Treated	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*
Average pre-default GDP pc	7878.72	7878.62	3827.81	3827.88	5212.76	5204.74	27566.21	27420.46
Investment	17.99	26.63	21.51	21.52	-	-	22.13	18.60
Education	80.7	82.92	72.41	69.23	105.94	88.62	97.03	96.39
(delta) Population	1.24	1.24	3.14	0.73	-0.16	-0.13	0.22	2.90
(log) Population	17.38	15.34	12.42	14.16	11.16	14.52	16.22	15.96
Openness	19.48	80.08	115.15	122.05	108.97	99.86	52.75	67.44
(delta) Terms of trade	6.87E+08	-2.17E+10	-	-	-	-	-	-
RMSPE	**310.98**		**91.95**		**90.62**		**1525.54**	
Control group	Colombia 0.02; Hong Kong 0.044; Hungary 0.277; Lebanon 0.659		Armenia 0.202; Mauritius 0.451; Mongolia 0.031; Swaziland 0.316		Armenia 0.671; Bahrain 0.158; Mauritius 0.17		Colombia 0.427; Kuwait 0.573	

	Paraguay		South Africa		Uruguay		Venezuela	
	Treated	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*
Average pre-default GDP pc	2183.24	2205.07	6292.62	6301.38	6504.07	6490.28	14534.53	14553.95
Investment	-	-	28.87	23.46	18.76	20.72	-	-
Education	26.76	26.73	-	-	62.02	40.81	44.13	44.19
(delta) Population	2.74	2.74	2.55	2.55	0.46	2.52	2.76	3.13
(log) Population	14.99	14.97	17.20	17.16	14.87	16.52	16.48	16.52
Openness	-	-	54.15	54.12	35.00	70.05	-	-
(delta) Terms of trade	-	-	-	-	-	-	-	-
RMSPE	**188.68**		**507.80**		**189.28**		**1002.28**	
Control group	Bangladesh 0.032; Botswana 0.276; China 0.018; Colombia 0.023; Hong Kong 0.02; India 0.024; Iran 0.019; Lesotho 0.224; Malaysia 0.025; Mauritius 0.133; Nepal 0.039; Papua New Guinea 0.014; Saudi Arabia 0.008; Zimbabwe 0.071; Swaziland 0.012; Thailand 0.026; United Arab Emirates 0.002; Tunisia 0.033		Bangladesh 0.016; Botswana 0.009; China 0.131; Colombia 0.502; Hong Kong 0.102; India 0.015; Iran 0.014; Lesotho 0.011; Malaysia 0.016; Oman 0.008; Nepal 0.012; Papua New Guinea 0.012; Saudi Arabia 0.095; Singapore 0.001; Zimbabwe 0.011; Swaziland 0.009; Thailand 0 .02; Tunisia 0.014		Colombia 0.677; Hong Kong 0.256; Oman 0.023; Saudi Arabia 0.044		Bangladesh 0.002; Botswana 0.001; China 0.171; Colombia 0.005; Hong Kong 0.518; India 0.002; Iran 0.004; Lesotho 0.003; Malaysia 0.005; Oman 0.001; Nepal 0.007; Papua New Guinea 0.002; Saudi Arabia 0.273; Zimbabwe 0.001; Swaziland 0.001; Thailand 0.003; Tunisia 0.004	

Notes: For each defaulting country, we report i) the predictor balance (i.e., for each predictor used for the construction of the counterfactual, we report the pre-default average of the defaulter and the pre-default average of the synthetic control); ii) the root of the mean square prediction error; iii) the control countries with a weight higher than 0. For some countries, some predictors are not used due to missing data.

Table B2: Official restructurings: predictor balance, RMSPE, and country weights

	Angola		Antigua and Barbuda		Benin		Burkina Faso		Chad	
	Treated	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*
Average pre-default GDP pc	2170.35	2167.22	12942.17	12935.37	614.13	613.90	339.03	340.75	473.79	476.08
Investment	0.00	18.00	-	-	15.17	17.25	17.72	17.68	4.27	16.36
Education	9.43	23.44	92.76	92.61	16.57	22.20	4.46	24.68	5.95	20.38
(delta) Population	3.19	2.88	1.34	1.34	2.82	2.91	2.56	2.41	2.53	2.63
(log) Population	16.14	16.22	11.39	14.22	15.23	17.38	15.87	17.19	15.41	18.11
Openness	85.48	62.22	115.50	115.31	52.30	33.83	38.20	27.68	42.71	23.19
(delta) Terms of trade	-	-	-	-	-2.68E+09	-1.58E+07			6.70E+09	1.51E+07
RMSPE	**83.30**		**513.33**		**15.68**		**13.42**		**35.19**	
Control group	Bangladesh 0.467; Oman 0.032; Namibia 0.321; Papua New Guinea 0.181		Bahrain 0.264; Hong Kong 0.025; Latvia 0.449; Lithuania 0.002; Mauritius 0.255; Qatar 0.003		Bangladesh 0.659; Lesotho 0.065; Mauritius 0.001; Papua New Guinea 0.003; Zimbabwe 0.272		Bangladesh 0.318; Nepal 0.682		Bangladesh 0.955; Mauritius 0.045	

	Comoros		Egypt		El Salvador		Georgia		Ghana	
	Treated	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*
Average pre-default GDP pc	793.12	791.25	1256.73	1257.54	2152.47	2154.47	1393.64	1386.03	831.26	834.49
Investment	11.09	11.48	26.48	23.80	12.76	16.31	15.96	15.96	15.45	16.07
Education	38.48	66.20	2.53	2.53	37.30	28.70	82.07	82.06	36.91	29.88
(delta) Population	2.43	1.71	50.60	28.21	1.42	2.84	-0.83	0.78	2.76	2.66
(log) Population	13.29	15.82	17.64	17.66	15.40	16.40	15.36	15.35	16.51	16.51
Openness	51.24	109.52	63.04	52.15	50.97	55.24	79.10	102.99	47.32	47.37
(delta) Terms of trade	-1.31E+09	-1.57E+08	2.89E+08	3.44E+09	-1.28E+08	-1.67E+10	-	-	-	-
RMSPE	**21.36**		**16.80**		**104.93**		**342.71**		**12.00**	
Control group	Lesotho 0.001; Zimbabwe 0.309; Swaziland 0.043; Tajikistan 0.646		Bangladesh 0.214; Botswana 0.164; India 0.219; Malaysia 0.073; Swaziland 0.011; Thailand 0.32		Bangladesh 0.5; Colombia 0.078; Namibia 0.422		Armenia 0.301; Lao PDR 0.074; Oman 0.04; Tajikistan 0.585		Bangladesh 0.32; Lao PDR 0.238; Namibia 0.073; Nepal 0.269; Swaziland 0.084; Tajikistan 0.017	

…continued

	Guatemala		Indonesia		Kyrgyz Republic		Mali		Sri Lanka	
	Treated	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*
Average pre-default GDP pc	2132.97	2132.76	1619.28	1620.54	642.30	642.25	440.65	441.58	1739.41	1735.11
Investment	12.14	15.93	26.68	25.14	16.07	16.25	16.86	20.52	23.90	23.86
Education	19.09	33.13	44.58	43.48	88.48512	82.68197	7.75	18.98	76.47	76.31
(delta) Population	2.38	2.81	1.89	2.00	1.02	1.02	1.84	2.60	0.68	0.68
(log) Population	15.98	16.76	18.99	18.78	15.36	15.72	15.82	17.07	16.75	16.72
Openness	35.80	44.77	49.48	48.78	83.22	106.23	46.99	51.89	79.86	79.69
(delta) Terms of trade	2.17E+08	-4.39E+10	-4.91E+12	-6.95E+11	-2.60E+08	1.90E+07	7.81E+09	3.00E+08	1.52E+10	1.61E+10
RMSPE	**43.29**		**9.86**		**30.44**		**26.87**		**104.06**	
Control group	Bangladesh 0.44; Colombia 0.195; Namibia 0.287; Zimbabwe 0.078		India 0.651; Iran 0.036; Malaysia 0.114; Mauritius 0.172; Thailand 0.027		Bangladesh 0.049; Armenia 0.201; Tajikistan 0.664; Uzbekistan 0.085		Bangladesh 0.704; Lesotho 0.285; Papua New Guinea 0.011		Azerbaijan 0.084; Bangladesh 0.014; Armenia 0.181; Botswana 0.005; Belarus 0.207; Colombia 0.004; Hong Kong 0.002; India 0.158; Iran 0.003; Lao PDR 0.048; Lebanon 0.003; Malaysia 0.005; Mauritius 0.005; Oman 0.002; Namibia 0.004; Nepal 0.009; Papua New Guinea 0.005; Slovak Republic 0.003; Zimbabwe 0.007; Swaziland 0.005; Tajikistan 0.039; Thailand 0.009; United Arab Emirates 0.001; Tunisia 0.007; Uzbekistan 0.181; West Bank and Gaza 0.007	

Notes: For each defaulting country, we report i) the predictor balance (i.e., for each predictor used for the construction of the counterfactual, we report the pre-default average of the defaulter and the pre-default average of the synthetic control); ii) the root of the mean square prediction error; iii) the control countries with a weight higher than 0. For some countries, some predictors are not used due to missing data.

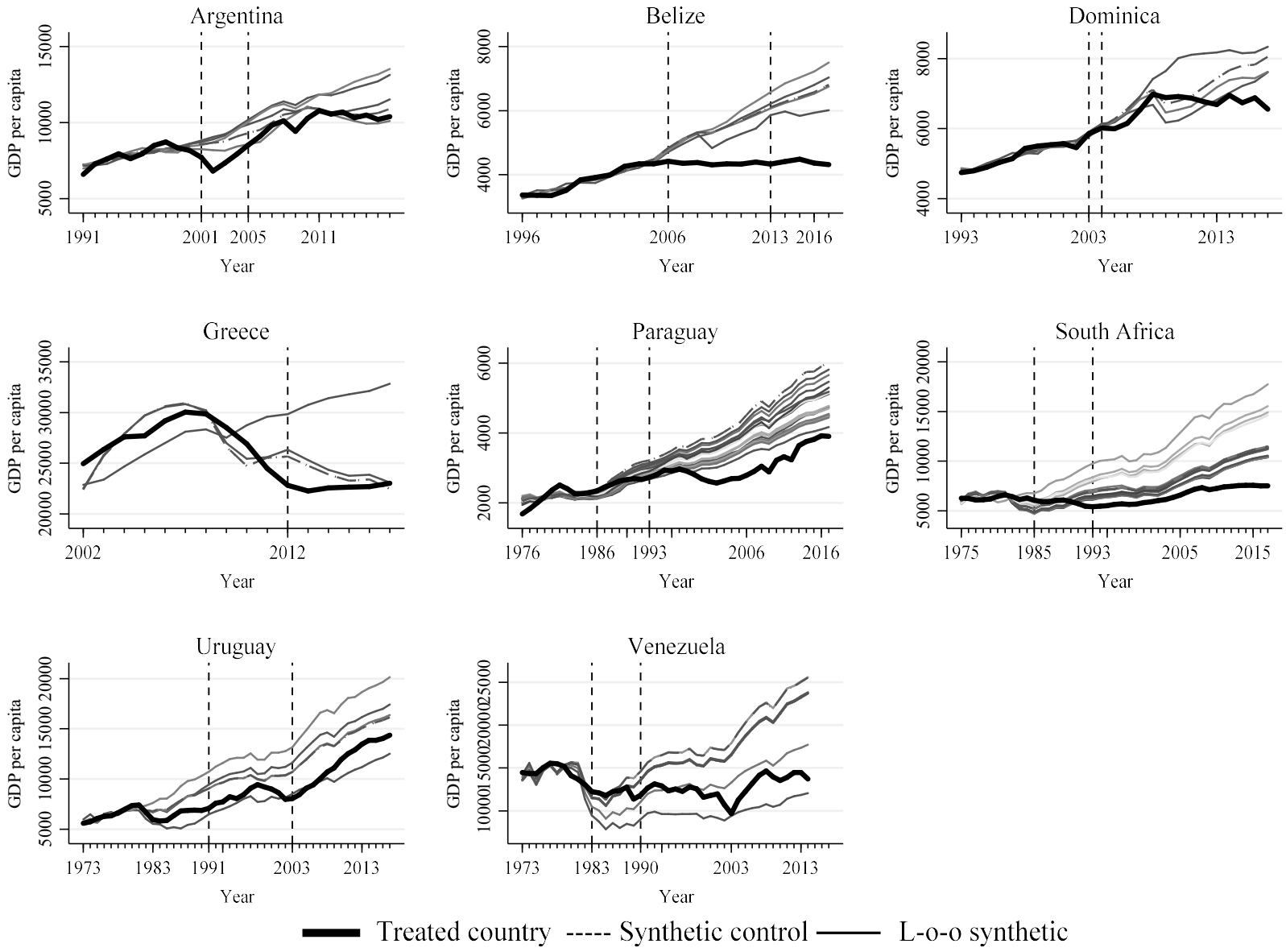


Figure B1: Private defaulters: leave-one-out distribution of the synthetic control

Notes: In each graph, the bold line represents the trend of GDP per capita for the defaulting country, the dashed line shows the trend for the synthetic country, and the continuous lines represent the synthetic countries obtained through the leave-one-out procedure.

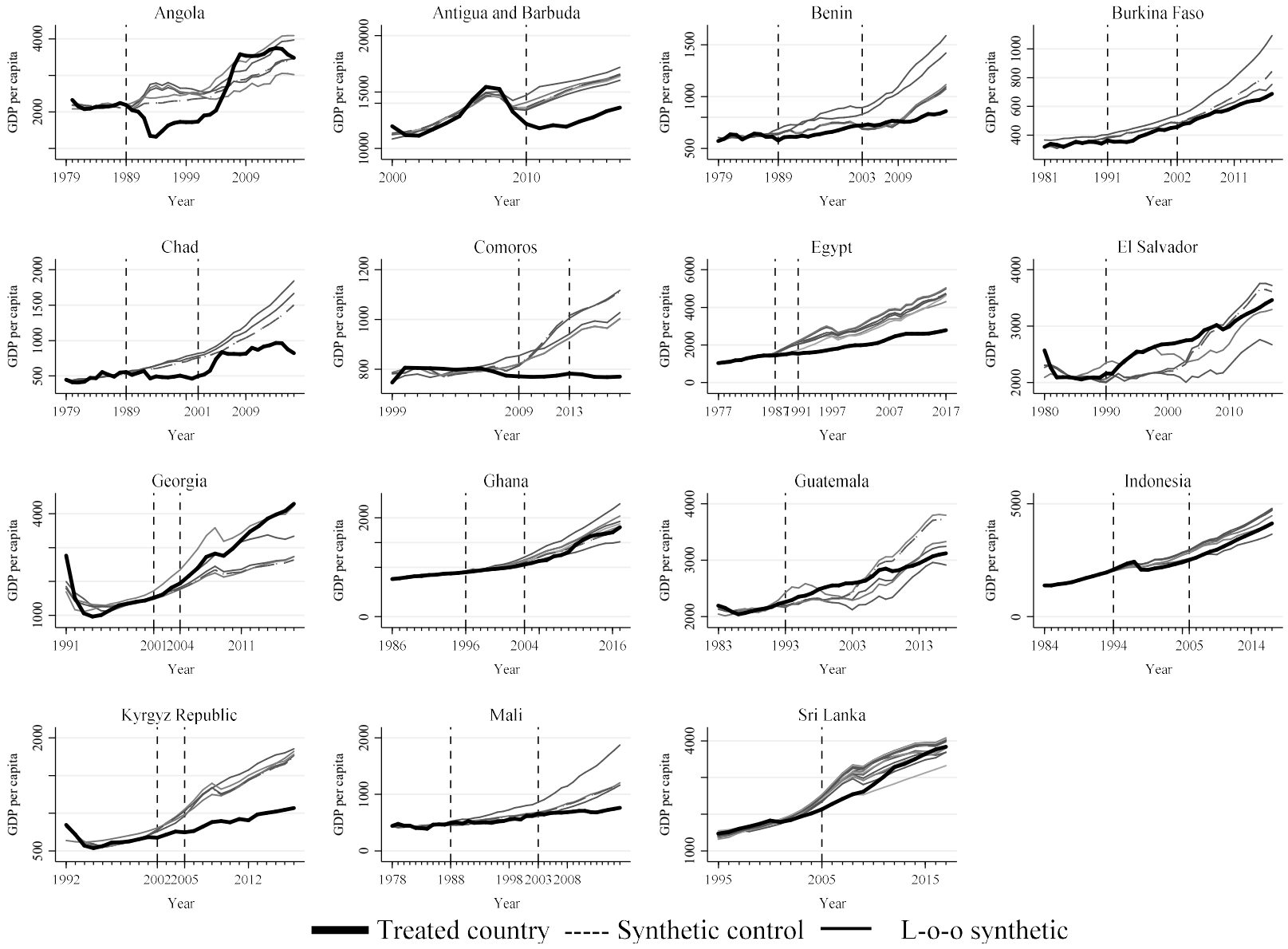


Figure B2: Official defaulters: leave-one-out distribution of the synthetic control

Notes: In each graph, the bold line represents the trend of GDP per capita for the defaulting country, the dashed line shows the trend for the synthetic country, and the continuous lines represent the synthetic countries obtained through the leave-one-out procedure.

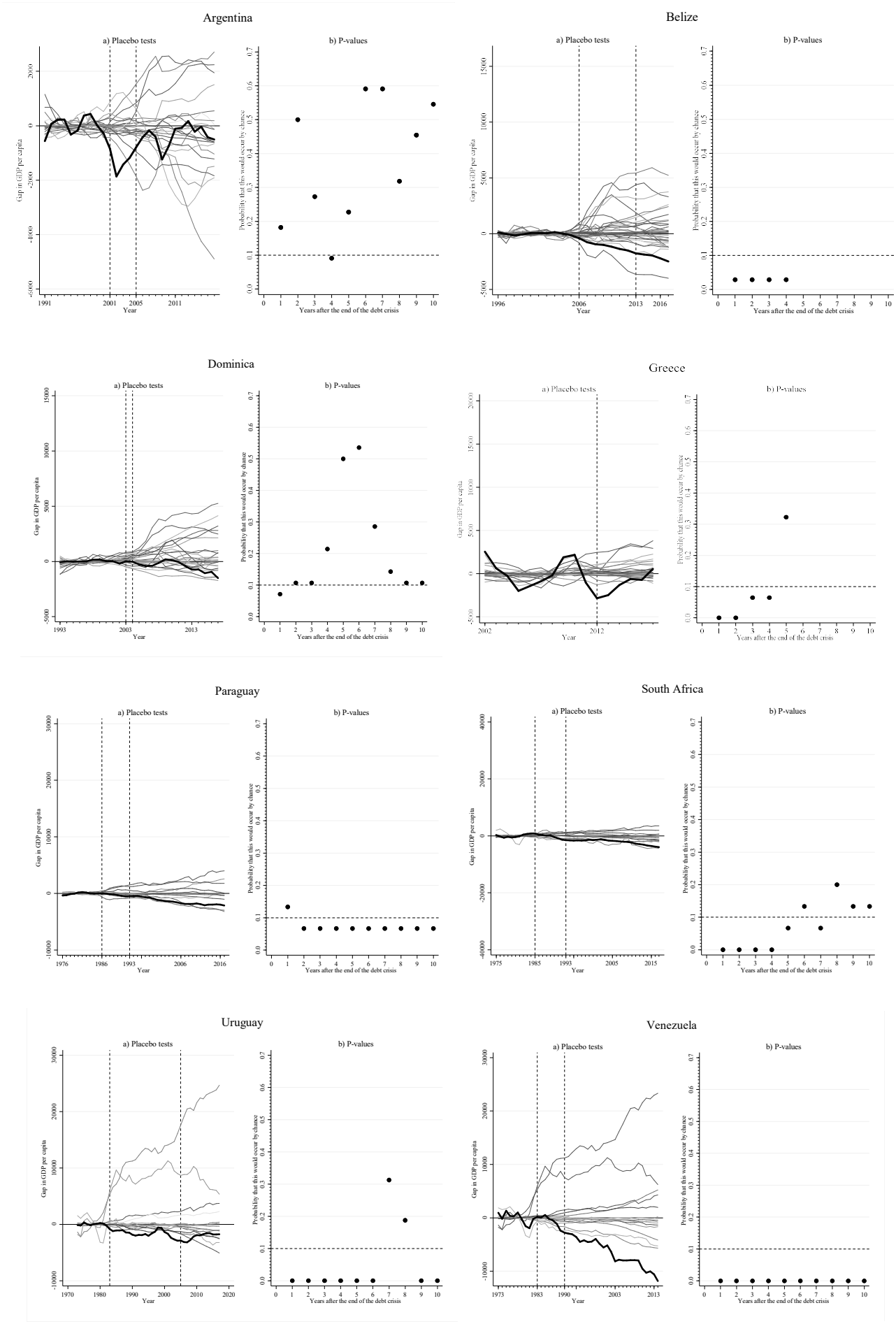


Figure B3: Private defaulters: a) placebo tests b) p-value

Notes: In each graph, panel a) shows the placebo tests in which the bold line represents the gap between the GDP per capita of the defaulting country and its synthetic counterpart, and the continuous lines represent the same gap obtained through the in space-placebo procedure. Panel b) shows the corresponding p-values up to ten years after the end of the debt crisis.

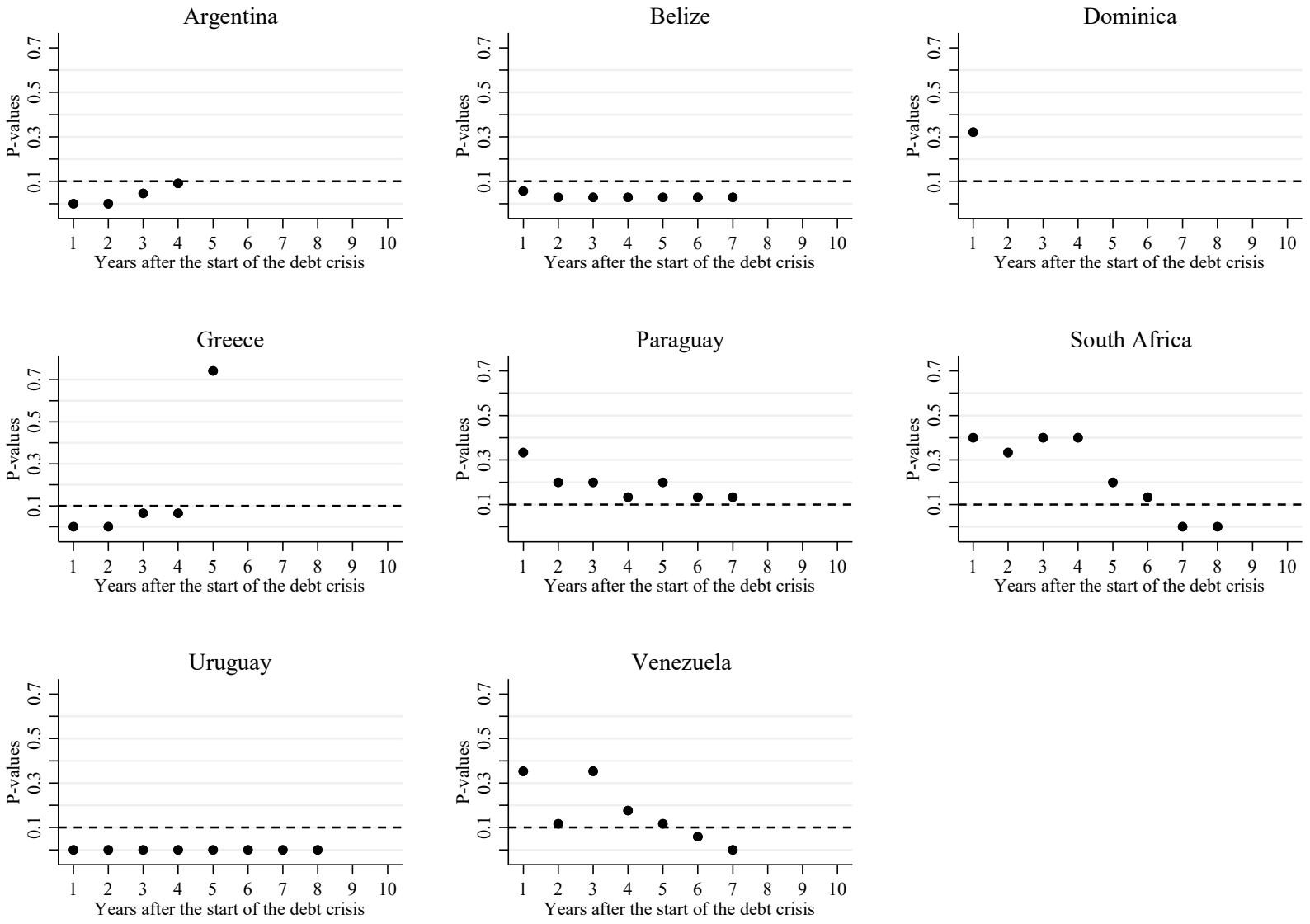
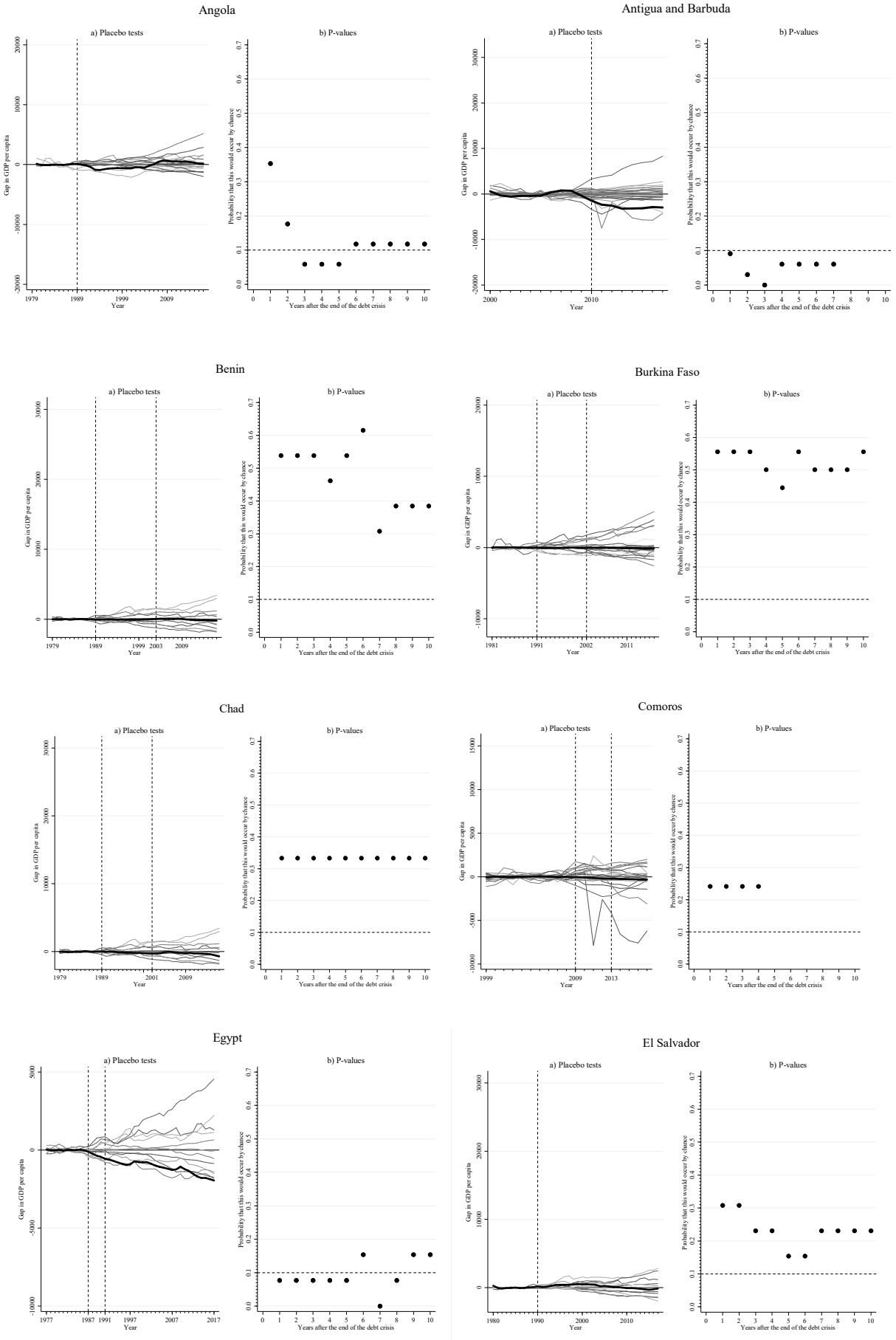


Figure B4: Private defaulters: p-values during the debt crisis

Notes: Each graph shows the p-values obtained through the in space-placebo procedure during the debt crisis.


Angola
a) Placebo tests
b) P-values
Gap in GDP per capita
Year
Probability that this would occur by chance
Years after the end of the debt crisis
Antigua and Barbuda
a) Placebo tests
b) P-values
Gap in GDP per capita
Year
Probability that this would occur by chance
Years after the end of the debt crisis
Benin
a) Placebo tests
b) P-values
Gap in GDP per capita
Year
Probability that this would occur by chance
Years after the end of the debt crisis
Burkina Faso
a) Placebo tests
b) P-values
Gap in GDP per capita
Year
Probability that this would occur by chance
Years after the end of the debt crisis
Chad
a) Placebo tests
b) P-values
Gap in GDP per capita
Year
Probability that this would occur by chance
Years after the end of the debt crisis
Comoros
a) Placebo tests
b) P-values
Gap in GDP per capita
Year
Probability that this would occur by chance
Years after the end of the debt crisis
Egypt
a) Placebo tests
b) P-values
Gap in GDP per capita
Year
Probability that this would occur by chance
Years after the end of the debt crisis
El Salvador
a) Placebo tests
b) P-values
Gap in GDP per capita
Year
Probability that this would occur by chance
Years after the end of the debt crisis

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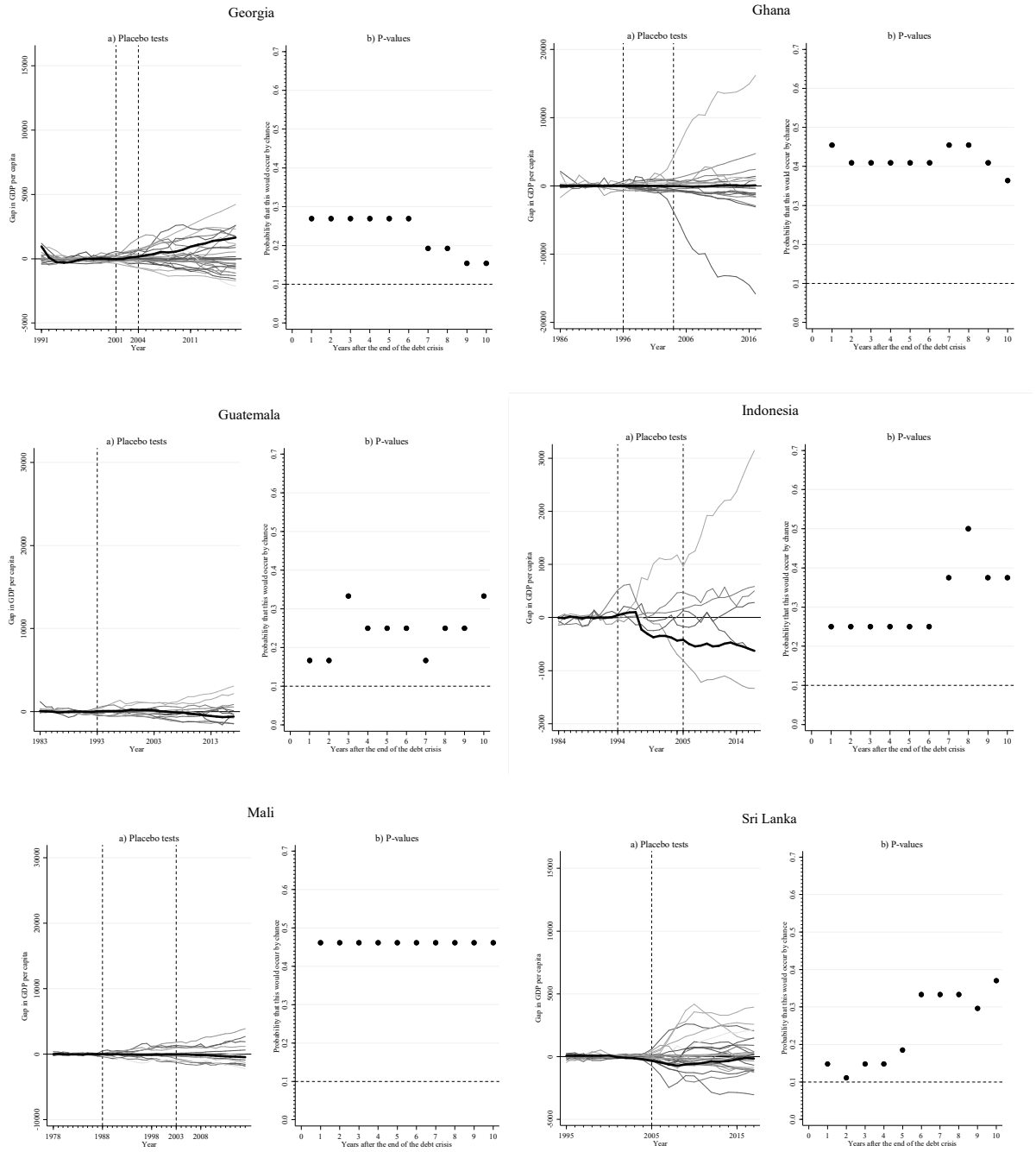

Figure B5: Official defaulters: a) placebo tests b) p-values

Notes: In each graph, panel a) shows the placebo tests in which the bold line represents the gap between the GDP per capita of the defaulting country and its synthetic counterpart, and the continuous lines represent the same gap obtained through the in space-placebo procedure. Panel b) shows the corresponding p-values up to ten years after the end of the debt crisis.

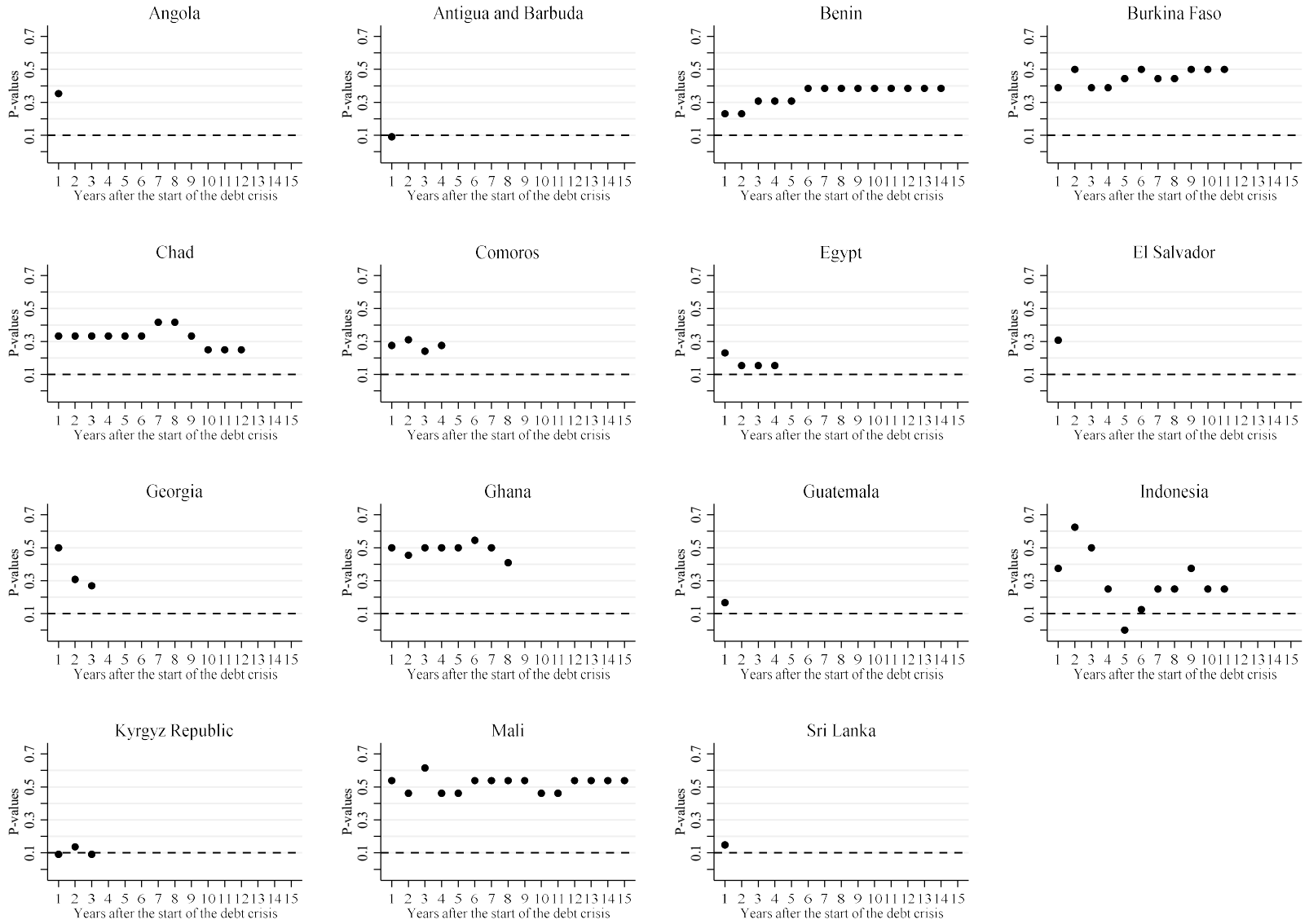

Figure B6: Official defaulters: p-values during the debt crisis

Notes: Each graph shows the p-values obtained through the in space-placebo procedure during the debt crisis.

Online Appendix C

Table C1a: Country sample, defaulters

	Private restructurings			Official restructurings	
Albania	1991-1995			1993-2000	
Algeria	1991-1996			1994-1995	
Angola				1989	
Antigua and Barbuda				2010	
Argentina	1982-1993	2001-2005		1985-1992	2014
Belize	2006-2013				
Benin				1989-2003	
Bolivia	1980-1993			1986-2001	
Brazil	1983-1994			1983-1992	
Bulgaria	1990-1994			1991-1994	
Burkina Faso				1991-2002	
Burundi				2004-2009	
Cambodia				1995	
Cameroon	1985-2003			1989-2006	
Central African Republic				1981-2009	
Chad				1989-2001	
Chile	1983-1990			1975-1987	
Comoros				2009-2013	
Congo, Dem. Rep.	1975-1989			1976-1989	2002-2010
Congo, Rep.	1983-1988	2007		1986-2004	2010
Costa Rica	1981-1990			1983-1993	
Cote d'Ivoire	1983-1998	2000-2012		1984-1994	1998-2012
Cuba	1983-1985			1985-1986	
Dominica	2003-2004				
Dominican Republic	1982-1994	2004-2005		1985-1991	2004-2005
Ecuador	1982-1995	1999-2000	2008-2009	1983-2003	
Egypt, Arab Rep.				1987-1991	
El Salvador				1990	
Equatorial Guinea				1985-1994	
Ethiopia	1990-1996			1992-2004	
Gabon	1986-1994			1987-1995	2000-2004
Gambia, The	1984-1988			1986	2003-2008
Georgia				2001-2004	
Ghana				1996-2004	
Greece	2012				
Grenada	2004-2005			2006	
Guatemala				1993	
Guinea	1985-1998			1986-2001	2008-2012
Guinea-Bissau				1987-2001	2010-2011
Guyana	1982-1999			1989-2004	
Haiti				1995-2009	
Honduras	1981-2001			1990-2005	
Indonesia				1994-2005	
Jamaica	1977-1990			1984-1993	
Jordan	1989-1993			1989-2002	
Kenya	1992-1998			1994-2004	
Kyrgyz Republic				2002-2005	
Liberia	1980-1982	2009		1980-1984	2008-2010
Madagascar	1981-1990			1981-1990	1997-2004
Malawi	1982-1988			1982-1988	2001-2006
Mali				1988-2003	
Mauritania	1992-1996			1985-2002	
Mexico	1982-1990			1983-1989	
Moldova	2001-2004			2006	

….continued

Morocco	1983-1990		1983-1992	
Mozambique	1983-1991	2007	1984-2001	
Myanmar			2013	
Nicaragua	1978-1995	2007	1991-2004	
Niger	1983-1991		1983-2004	
Nigeria	1982-1991		1986-1991	2000-2005
Pakistan	1998-1999		1981	1999-2001
Panama	1984-1996		1985-1990	
Paraguay	1986-1993			
Peru	1978-1997		1978-1996	
Philippines	1983-1992		1984-1994	
Poland	1981-1994		1981-1991	
Romania	1981-1983	1986	1982-1983	
Russia	1991-2000		1993-1999	
Rwanda			1998-2005	
Saint Kitts and Nevis	2012		2012	
Senegal	1980-1985	1990-1996	1981-2004	
Seychelles	2008-2010		2009	
Sierra Leone	1980-1995		1977-2007	
South Africa	1985-1993			
Sri Lanka			2005	
Sudan	1975-1985		1979-1984	
Tanzania	2004		1986-2002	
Togo	1987-1997		1979-1995	2008-2010
Trinidad and Tobago	1988-1989		1989-1990	
Turkey	1976-1982		1978-1980	
Uganda	1979-1993		1981-2000	
Ukraine	1998-2000		2001	
Uruguay	1983-1991	2003		
Venezuela, RB	1983-1990			
Viet Nam	1982-1997		1993	
Yemen, Rep.	1983-2001		1996-2001	
Zambia	1983-1994		1983-2005	

Notes: Countries in bold correspond to are those with only private restructurings, while countries in italics are those with only official restructurings.

Table C1b: Country sample, not defaulters

Armenia	Hungary	Lithuania	Saudi Arabia	Uzbekistan
Azerbaijan	India	Malaysia	Singapore	West Bank and Gaza
Bahrain	Iran, Islamic Rep	Mauritius	Slovak Rep	Zimbabwe
Bangladesh	Kazakhstan	Mongolia	Swaziland	
Belarus	Kuwait	Namibia	Syrian Arab Rep.	
Botswana	Lao PDR	Nepal	Tajikistan	
China	Latvia	Oman	Thailand	
Colombia	Lebanon	Papua New Guinea	Tunisia	
Eritrea	Lesotho	Puerto Rico	Turkmenistan	
Hong Kong	Libya	Qatar	United Arab Em.	

Table C2: Variable definitions and sources

Variable	Definition	Source
DEPENDENT VARIABLE		
GDP growth	Annual percentage growth rate of GDP	WDI (2018)
GDP per capita	GDP per capita (constant 2010 US$)	WDI (2018)
VARIABLES OF INTEREST		
Private default duration	Dummy=1 for each year of the private debt crisis	Asonuma and Trebesch (2016)
Official default duration	Dummy=1 for each year of the official debt crisis	Cheng, Diaz-Cassou, Erce (2016)
Official Haircut	Face value reduction of official debt (percent)	Cheng, Diaz-Cassou, Erce (2016)
Official Haircut Dummy	Dummy =1 in case of a present value reduction of official debt	Authors' compilation based on Cheng, Diaz-Cassou, Erce (2016)
Private Haircut	Face value reduction of private debt (percent)	Cruces and Trebesch (2013)
Private Haircut Dummy	Dummy =1 in case of a present value reduction of private debt	Authors' compilation based on Cruces and Trebesch (2013)
CONTROL VARIABLES		
Investment	Gross fixed capital formation, ratio to GDP	World Development Indicators, World Bank (2018)
Gov. Consumption	Gen. government final consumption expenditure, ratio to GDP	World Development Indicators, World Bank (2018)
Openness	Exports plus imports of goods and services, ratio to GDP	World Development Indicators, World Bank (2018)
Inflation	Consumer price index (2010 = 100), Annual rate of change	World Development Indicators, World Bank (2018)
External debt to GDP	Ratio of external debt to GDP	World Development Indicators, World Bank (2018)
Political Risk	ICRG Political Risk Index	International Country Risk Guide, The PRS Group (2018)
Government change	Dummy=1 in years with a change in the executive	Database of Political Institutions, World Bank (2017)
(delta) Population	Rate of population growth, annual	World Development Indicators, World Bank (2018)
(log) Population	Log of total population	World Development Indicators, World Bank (2018)
Education	Percentage of the population that completed secondary education	World Development Indicators, World Bank (2018)
(delta) Terms of Trade	Annual change in terms-of-trade (in million)	World Development Indicators, World Bank (2018)
Banking crisis	Dummy=1 in the case of a banking crisis, 0 otherwise	Laeven and Valencia (2013)
Currency crisis	Dummy= 1 in the case of a currency crisis, 0 otherwise	Laeven and Valencia (2013)
Civil Liberties	The Freedom House index of civil liberties, range goes from-1 to 7	Freedom House. 2018. Freedom of the Press Index.
IMF net loans	IMF net loans, ratio to GDP	World Development Indicators, World Bank (2018)

Table C3: Summary statistics

Variable	N	Mean	SD	Min	Max
Per capita Growth	2239	1.93	5.4	-40.74	36.84
Private Default Duration	2239	0.15	0.35	0	1
Private Haircut	2239	1.51	9.46	0	100
Official Default Duration	2239	0.23	0.42	0	1
Official Haircut	2239	3.05	14.66	0	100
Final Priv. Haircut Dummy (-1)	2239	0.02	0.13	0	1
Final Priv. Haircut Dummy (-2)	2239	0.02	0.13	0	1
Final Priv. Haircut Dummy (-3)	2239	0.02	0.14	0	1
Final Priv. Haircut Dummy (-4&5)	2239	0.03	0.18	0	1
Final Priv. Haircut Dummy (-6&7)	2239	0.04	0.18	0	1
Final Private Haircut (-1)	2239	1.02	8.43	0	100
Final Private Haircut (-2)	2239	1.03	8.31	0	100
Final Private Haircut (-3)	2239	1.09	8.63	0	100
Final Private Haircut (-4&5)	2239	2.01	11.53	0	100
Final Private Haircut (-6&7)	2239	1.87	10.95	0	100
Final Off. Haircut Dummy (-1)	2239	0.02	0.15	0	1
Final Off. Haircut Dummy (-2)	2239	0.02	0.15	0	1
Final Off. Haircut Dummy (-3)	2239	0.02	0.15	0	1
Final Off. Haircut Dummy (-4&5)	2239	0.04	0.21	0	1
Final Off. Haircut Dummy (-6&7)	2239	0.04	0.2	0	1
Final Off. Haircut (-1)	2239	0.87	8.41	0	100
Final Off. Haircut (-2)	2239	0.95	8.75	0	100
Final Off. Haircut (-3)	2239	0.81	7.85	0	100
Final Off. Haircut (-4&5)	2239	1.7	11.49	0	100
Final Off. Haircut (-6&6)	2239	1.45	10.51	0	100
Investment	2234	21.62	7.22	0	60.56
(delta) Population	2239	2.06	1.15	-6.18	7.92
Education	2139	96.98	22	16.82	149.31
(log) Popolation	2239	16.34	1.49	11.93	20.96
Government Cons.	2235	14.44	6.4	0	88.98
Civil Liberties	2239	4.21	1.43	1	7
(delta) Terms of Trade (bn)	2233	488.99	32785.35	4.74E+05	1.04E+06
Openness	2237	68.24	32.83	9.03	311.36
Banking Crises	2161	0.03	0.17	0	1
IMF net loans	2238	0.12	0.74	-4.21	6.05

Notes: Summary statistics are calculated from Table 6, column 8