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Life After Default: Private vs. Official Sovereign Debt Restructurings

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Abstract: This paper studies the relationship between sovereign debt default and annual GDP growth taking into account the depth of a debt restructuring and distinguishing between private and official deals, as well as between debt flow and stock reduction. Analyzing 520 restructuring episodes, over the period 1975-2013, we find that private and official defaults may have different growth outcomes. Most importantly, controlling for the severity of the debt crisis, we are able to detect a more lasting and negative relationship between default and growth. While private defaults are generally associated with lower growth during the crisis and over the long run (mitigated by the amount involved), for official defaulters we do not observe a growth contraction throughout the years of the crisis and they are associated with higher growth over the long run (independently of the amount involved). When debt relief operations involve debt write offs, however, the negative relationship between private default and growth becomes blurred, while official defaulters strongly benefit in terms of growth from the face value reduction. Using the Synthetic Control Method, we present further evidence for the heterogeneity of the economic impact of debt restructurings, confirming that official and private defaults may have different effects on GDP growth and should then be treated differently.

Keywords: Haircuts, Output losses, Sovereign defaults

JEL Classification: F34, G15, H63

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1 Introduction

Sovereign defaults and debt restructuring are not costless as a sovereign's unilateral decisions to stop servicing its debt implies important economic costs. At least this is what the sovereign debt literature has commonly assumed as a government's main incentive to honor its debt obligations.[1] The (empirical) literature on sovereign defaults, however, has generally found that default costs are difficult to quantify and short lived. Only more recently, by paying more attention to the specific analysis of debt renegotiations, very different results have been obtained with respect to the previous literature. In particular, thanks to a more precise measurement of a country's repayment record, longer term effects can be detected, which are more in line with the effects of a default according to the theory.[2]

In this paper we focus on the relationship between annual GDP growth and both "private" and "official" debt restructurings applying a similar methodology to Cruces and Trebesch (2013a) to the analysis of the relationship between debt default and economic growth.[3] From now on in the paper, by "*private restructuring*", we will denote a restructuring deal with private creditors (foreign banks and bondholders), while "*official restructuring*" will stand for agreements reached with official creditors (in the Paris Club).

Although the overall evidence in the literature indicates that default episodes are negatively correlated with growth, the decision of a default has usually been modelled as a binary decision, thus ignoring the large variation in restructuring outcomes.[4] We add to prevoius works by considering both the amount of debt treated in the restructuring as well as the actual amount of debt write off involved in the deal, as proxy of the severity of the default episode. Moreover**,** we verify if higher private, or official, restructurings, are correlated with a significant variation of (annual) economic growth over a period of ten years.

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

[2] 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 instead investigated the macroeconomic impact of sovereign debt restructurings with official-sector creditors.

[3] Defined narrowly, 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.

[4] This circumstance implies, *de facto*, no distinction between the different degrees of severity of default episodes and could (at least partially) explain why previous works has so far detected negligible medium-run effects of debt defaults on growth.

Debt restructuring 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 then 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, despite the role that official creditors have historically played in the resolution of sovereign debt crises (e.g., IMF 2013), little is known on the implications of debt restructurings involving these creditors. 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. To the best of our knowledge, this is the first attempt to disentangle the different effect of private vs. official restructuring.

Analyzing 520 restructuring episodes over the period 1975-2013, we find that commercial and official defaults are associated with different growth outcomes. Most importantly, controlling for the severity of the default, we are able to detect a more lasting and negative relationship between default and growth. While private defaults are generally associated with lower growth during the crisis and over the long run (mitigated by the amount involved), for official defaulters we do not observe a growth contraction throughout the years of the crisis and they are associated with higher growth over the long run (independently of the amount involved). When debt relief operations involve debt write off, the negative relationship between private default and growth becomes blurred, while official defaulters strongly benefit in terms of growth from the face value reduction, thereby improving recovery (as in Arslanalp and Henry 2005).

Using the Synthetic Control Method (Abadie and Gardeazabal 2003, Abadie *et al.* 2010), we provide further evidence for the heterogeneous effect of default on commercial and official debt. 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). Our findings, although revealing some differences among countries involved in the same type of debt restructuring, point to confirm that private restructurings are associated with

output losses that persist over time. Conversely, official defaulters do not show a permanent drop in GDP per capita, and are even able to grow more than their synthetic counterparts at the end of the debt crisis (although the effect of the restructurings is not significant).

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. The importance of the way in which restructuring are actually arranged is confirmed by the results of both Asonuma and Trebesch (2016) and Trebesch and Zabel (2017) who find that less confrontational (or preemptive) restructurings are associated with a lower output loss as compared to soft (non-preemptive) defaults.

The rest of this paper is organized as follows. Section 2 briefly describes the related literature. Section 3 introduces our data and the empirical model. In Section 4 we present our results, while Section 5 contains some robustness checks. Section 6 describes the findings obtained through the Synthetic Control Method (hereafter SCM). The final Section 7 concludes.

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).[5]

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

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

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.* (2006) and (2009).[6] In all these cases, however, the effects specifically associated with a default (on the top of those related to the crisis 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.[7]

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

[6] 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.

[7] 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.

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 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.[8]

As the official sector is concerned, Cheng *et al.* (2018) have focused on 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.[9]

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 (as well as debt flow vs. stock effects) 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 Reinhart and Trebesch (2016) our main contribution is to conduct a comparison of official vs. private restructurings 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. On the other hand, our contribution to the paper of

[8] The absence of a significant relationship between restructuring and growth, in the aftermath of the crisis, may be explained as the average effect of two (i.e., private and official) countervailing effects on growth.

[9] 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.

Cheng *et al.* (2018), is to compare official vs. private restructuring as well as debt flow vs. stock effects.

The analysis of sovereign debt defaults has been neglected in the literature applying the SCM, with the notable exception of Jorra (2011).[10] Nevertheless, 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).

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).[11] The results of this paper are actually more consistent with the

[10] 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), natural resource discoveries (Smith 2015, Masi and Ricciuti 2016), and civil war (Costalli *et al.* 2017). Jorra (2011) has used the SCM to analyze the heterogeneity of default costs, considering only five countries, but without distinguishing between private and official defaults.

[11] 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

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 unexcusable" 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.[12]

Following Grossman and van Huyck (1988), the intuition behind our analysis is that the collateral damage of a sovereign default is likely to be lower in restructurings which involve the official sector and are orchestrated by the Paris Club, as opposed to private defaults which are, at least on average, likely to be more confrontational.[13] For example, in 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 he financial sector working differently for official and private defaulters.[14]

3 Data and empirical model

In this Section, we present our empirical approach to analyze the effects of a default on economic growth, by controlling for the severity of the default episode. We will first describe the sample and then introduce our empirical model.

adds to the cost of a default.

[12]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).

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

[14]In a companion paper (Marchesi and Masi 2018) we actually confirm that commercial and official defaults are also associated to different outcomes in terms of credit ratings.

3.1 Default coding and sample composition

Our analysis spans the years between 1975 and 2013 and includes 117 developing and emerging market economies.[15] We have selected this sample as follows. First, we excluded from the sample small countries with a population of less than one million (as measured at the end of the sample period in 2013) and all advanced economies, 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 Herzegovina and Serbia). The resulting set of 117 countries includes 73 defaulting countries, which experienced at least one debt crisis during the sample period as well as 44 non-defaulters. Among defaulters, 51 countries had both private and official debt restructurings, 18 countries had only official debt restructurings (through the Paris Club) while 4 countries had only private haircuts. Table A1a in the Online Appendix A shows all the defaulting countries in the sample, the type of restructurings, and the debt crisis periods, whereas Table A1b lists non-defaulters.

We consider two measures of debt restructuring, that is the total amount of debt affected by the restructuring (as a share of total external debt) and the corresponding face value reduction (as a percentage of the amount of debt treated in the restructuring deal), when available. Following Reinhert and Trebesch (2016) and Cheng et al. (2018), we compare the effects of a "simple" debt restructuring with that of a face value reduction. What is more, we take these two specific measures to be able to compare the same type of intervention for restructuring involving private and official creditors. From now on, in the paper, by the term "haircut" we will denote the amount of (private or official) face value reduction associated to the restructuring deal.

We relied on the original dataset by Cruces and Trebesch (2013b) for the data on debt restructurings with commercial creditors.[16] 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

[15] More specifically, following the 2013 World Bank Country classification, we included low, middle income and high income (non OECD) countries.

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

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).[17] The new data made available by Cheng *et al.* (2018) allowed us to compare the impacts of (i) flow treatment and (ii) stock treatment. What is more, the authors report, for each agreement, the corresponding terms of treatment and the face value reduction provided (if any), which allowed us to take into account the actual face value reduction for official deals and to compare this with the corresponding private face value reduction reported by Cruces and Trebesch (2013b).[18]

Table 1a shows summary statistics for different subsamples in the full sample of 520 restructurings.[19] We find that the average amount of debt affected by private restructurings, between 1975 and 2013, is about 18 percent (simple mean). Looking at the three different subperiods, we detect a sizeable increase in this amount over time. Average size of debt affected by private restructuring is almost double during the last subperiod (2002-2013), as compared to the initial period (1975-1988), and about 50 percent higher with respect to the intermediate one (1989-2001). When comparing the size of private haircut, we see that only 2 restructurings involved face value reduction in the first subperiod.[20] One reason is that almost all the settlements up to the beginning of the Brady plan (1989-1994) mainly implied maturity extensions without an actual face value reduction. Nevertheless, their amount exceeds, on average, the reductions granted in the second subperiod, but not those in the last subperiod, which account, on average, for the 65 percent of the debt treated.

As official restructuring are concerned, we find that the average amount of debt affected by official restructurings, over the full period, is about 17 percent, slightly lower than the average private amount. Looking at the three different subperiods, we find a sizeable increase in the size of official restructurings over time. Average size during the last subperiod (2002-2013) is about 5 times the average restructuring implemented during the initial period (1975-1988), and almost three times

[17] As low-income countries are concerned, Paris Club creditors agreed to provide them concessional reschedulings (conditional on the adoption of an IMF program) under the Toronto (1988), Trinidad (1990), Naples terms (1994). 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.

[18] We could not use the *Preferred Haircut HSZ* used by Cruces and Trebesch (2013a), as this variable was not available in the dataset on official deals.

[19] Among those, 156 default episodes involved restructuring with private creditors, while 364 involved deals with official creditors.

[20] The two episodes of private debt reduction listed in Table 1a refer to the Bolivian buyback and to the Mexican "Morgan Bond plan", both taking place in 1988.

the average size of the intermediate period (1989-2001). Similarly to private haircuts, and as documented by the different debt relief initiatives described above, we detect a sizeable increase in the size of official haircuts over time, too. Figures 1 and 2 show the distribution of both private and official restructuring by the amount of debt affected and by the haircut size.

Table 1b shows summary statistics for different subsamples according to a country's income. As the number of countries is concerned, overall, we do not find big differences between countries having private or official restructurings. The number of high income countries defaulting with private creditors is very similar to those having an official debt restructuring. Whereas, on average, both low and middle income countries benefitted more from official agreements.

The difference is bigger when considering the number of countries involved in a face value reduction. In this case, we can observe that high income countries never experienced an official haircut. Moreover, while middle income countries tend to benefit more from private haircut (24 vs. 9, respectively), low income countries tend to benefit more from official ones (27 vs. 12, respectively). Finally, as the amount of debt affected by a restructuring is concerned, we find that middle income countries obtain the highest percentage of private restructurings, while the average size of official restructurings is the highest for low income countries. On the other hand, low income defaulters receive the highest proportion of both private and official haircuts.

TABLES 1a & 1b HERE

FIGURES 1 & FIGURE 2 HERE

3.2 Method

We analyze the relationship between private and official restructuring and annual per capita GDP growth using a fixed-effects GLS estimator in order to correct for heteroskedasticity across countries and obtain efficient estimates.[21]

Specifically we test:

$$y_{it} = \alpha + \gamma_j C_{it} + \delta_j R_{it} + \theta_j FC_{it-j} + \lambda_j FR_{it-j} + \beta X_{it} + \eta_i + \tau_t + u_{it}, \qquad j = 0, ..., 10 \quad (1)$$

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

[21] 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.

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 restructuring (haircut), FR_{it-j} denotes the amount of private/official debt affected (haircut) in the last restructuring.[22] Finally, η_i and τ_t denote country and time dummies, respectively, which allow us to control for both unobservable country characteristics and time common trends.[23]

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 size of the haircut, i.e. "occurrence" versus "magnitude."

We have chosen to consider both the duration of the debt crisis, and up to ten periods after the last restructuring, 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 (as in the next Section) in which we examine separately the growth pattern *during the crisis* and up to ten years *after its end*. Thus, to obtain comparable results, we apply our baseline specification from the start of the debt crisis (after the default), and using duration data for both private (Asonuma and Trebesch 2016) and official debt restructuring (Cheng et al. 2018). We then include 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 output 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 total population (both in log), 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).

Table A2 in the Online Appendix A provides a detailed description of each variable and its source while Table A3 and A4 show some summary statistics.

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

[23] 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).

4 Empirical results

The results of the model of equation (1) are presented in Tables 2 and 3 below. While Table 2 presents the results obtained including the amount of debt affected by private/official restructurings, Table 3 shows the results obtained controlling for the private/official face value reduction.

4.1 Debt Restructuring

In columns 1-2 of Table 2, we apply our baseline specification during the private/official debt crisis and in column 3 we also include the amount of private/official debt, which was restructured during the crisis (i.e., excluding final deal restructuring). Since column 4, we start also to include dummy variables indicating the event of the last restructuring for both private and official deals. In particular, columns 4-5 include up to 3 and 5 years after the last restructuring. Finally, since column 6, we start to include the actual amount of both private and official debt affected by the restructuring. More specifically, columns 6-8 include up to 5, 7 and 10 years after the last restructuring, respectively, by controlling for both the occurrence and the restructured amount.[24] While all these results are reported for comparison, we largely base the discussion on the fully specified model of column 8.

As can be seen, most of the control variables have the expected sign. Growth rates significantly increases with higher investments and civil liberties, while it decreases with higher population growth and its level (in log), higher public expenditure and after the occurrence of a banking crisis. Quite surprisingly it also strongly decreases with the level of secondary education. The coefficients of terms of trade and openness are not significant.[25]

As our variables of interests are concerned, during the debt crisis, we can observe that the relationship between private default and growth is negative but not always statistically significant.[26] This negative effect is partly reduced, however, by the actual amount of debt involved in the restructuring, which has always a positive and significant (but indeed small) coefficient.[27] This

[24] The best way to interpret the findings of Table 2 is to consider the expected variation in growth as conditional on the restructuring size, that is $\lambda_j FR_{it-j} + \theta_j$.

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

[26] In the last column of Table 2, the coefficient of private default duration is (almost) significant at the ten percent level (P-value is 0.106). During a private default, growth decreases, on average, by 0.8 percent.

[27] When we start to include the dummy denoting the final restructuring (since column 4 of Table 2), the variables private/official default duration are built excluding the upper bound extreme of the interval. The same apply to the variables private/official restructuring after column 6, that is when we start to include the final restructuring amount.

result is to some extent similar to Trebesch and Zabel (2017) that find evidence of a negative relationship between default and growth during the default years only.

On the other hand, evidence of a relationship between official default and growth, during the debt crisis, is more blurred and, if anything, in the last column of Table 2, the coefficient of official default duration is positive but insignificant at conventional levels. The coefficients denoting the amount of debt involved in official restructurings are also very small but never significant.

After the end of the debt crisis, in column 8, we find that the coefficient of the final restructuring dummy is positive and significant, at the five percent level, two years after the end of the default (growth increases by about 2.34 percent). However, immediately afterwards, this coefficient turns and remains negative (but not always significant) up to ten years after the end of the debt crisis. For example, after six and eight years since the last restructuring growth decreases by 1.7 and 1.6 percent, which is clearly an economically relevant magnitude.[28] The negative effect of a default is partly compensated by the actual amount of the restructured debt. For example, six years after the last restructuring, an increase of one standard deviation in the amount of the restructuring increases growth by 0.25 percent.

The results are strongly different when examining the aftermath of official defaults. In this case, the coefficients denoting the final restructuring itself and its (up to ten) lags are always positive and generally (but not always) significant. More specifically, at the time of a final restructuring and up to one, two, six and eight years after, we find that the coefficients of the final restructuring dummy are always positive and statistically significant. For example, two years after the end of the default growth increases by almost 1.6 percent. On the other hand, in this case, the actual amount of debt involved in the restructuring is irrelevant.

In figure 3, we summarize the results with two graphs tracking the evolution over time of the size, sign and significance of the coefficient denoting both private and official final restructuring deals.

To sum up, when considering the amount of debt affected by the restructuring, private restructurings are generally associated with lower growth both during the crisis and in the long run (somehow mitigated by the amount involved), while official restructurings do not seem to reduce growth throughout the crisis years and are associated with higher growth in the long run (independently of the amount).

As in Asonuma and Trebesch (2016) and Trebesch and Zabel (2017), we suggest that a plausible

[28] After ten years since the last restructuring, the coefficient of restructuring dummy is still negative and significant at the ten percent level (P-value 0.101).

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.

TABLE 2 HERE

FIGURES 3 HERE

4.2 Debt reduction

The first two columns of Table 3, as we did before, report our baseline specification controlling for the duration of the crisis, while in column 3 we also include the amount of debt which was forgiven,during the debt crisis. Since column 4, we start also to include dummy variables indicating the event of the last restructuring, for both private and official deals, when it is specifically associated with a face value reduction. In particular, columns 4-5 include up to 3 and 5 years after the last debt forgiveness. Finally, since column 6, we start to include the actual amount of both private and official face value reduction. As above, we largely base the discussion on the fully specified model of column 8.

Most of our control variables have the expected impact on growth as above. As our variables of interests are concerned, the coefficient of private default duration is always negative, but it is statistically significant only in the baseline specification (without controls), the coefficient of official default duration is always positive but insignificant. Since a face value reduction generally comes as the "final" restructuring in a series (i.e., the agreement that ends each of the debt crisis spells), the actual amount of debt forgiven during the debt crisis (i.e., without considering the final year) is actually negligible and hence its coefficient is never found to be statistically significant.

After the end of the debt crisis, in column 8, we find that the coefficient of the final private haircut dummy is positive and significant, at the ten percent level, two years after the end of the default (growth now increases only by about 1.79 percent). Immediately afterwards, this coefficient turns and remains negative, without (almost) ever being significant, up to nine years after the end of the debt crisis.

The results are strongly different when examining the aftermath of official defaults. In this case,

the coefficients denoting the final haircut itself and its (up to ten) lags are always positive and generally significant up to eight years since the last restructuring associated. For instance, one year after the end of default, growth increases already by almost 2.2 percent.

The actual size of the haircut seems irrelevant for both private and official defaults.[29] Figure 4 shows more clearly the evolution over time of the size, sign and significance of the coefficients of private and official final haircut dummy.

In summary, we find that when debt relief operations involve debt write off, the negative relationship between private default and growth becomes blurred while official defaulters strongly benefit in terms of growth from the face value reduction These results are 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 a nominal haircut. 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 official defaults, without obtaining similar results, on average, for private defaulters.[30]

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 coefficient of a private haircut. On the other hand, official defaulters seem to benefit from the debt relief effect of a higher haircut (as in Arslanalp and Henry 2005) and no "collateral damage" seems associated with the default in this case.

TABLE 3 HERE

FIGURES 4 HERE

5 Robustness checks

This Section aims to test the robustness of our main model of equation (1). The results are shown in Tables 4, 5a, and 5b. More specifically, we try to control for the presence of (i) autocorrelated standard errors (columns 1-2 and 4-5 of Table 4) (ii) omitted variable bias (columns 3 and 6 of

[29] The coefficient of the private haircut is found to be negative and significant only one year after the end of the crisis.

[30] When focusing only on Brady deals, however, as in Reinhart and Trebesch (2016), we confirm that final haircuts are associated to a boost in growth. In fact, similarly to Paris Club agreements, Brady deals can also represent an example of a "centrally organized" sovereign deal. These results are available on requests.

Table 4), as common shocks could affect both output and haircuts, and (iii) reverse causality (Tables 5a and 5b), since changes in output can potentially explain the type of default.

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-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 $T = 39$, 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 both columns one and four of Table 4 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 both columns two and five of Table 4.

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).[31] 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).[32]

In both columns three and six of Table 4 we have then included 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,

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

[32] 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.

the results are 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 5a we test the influence of lagged growth on the current level of debt affected by a private restructuring. Then, in columns 4-6 we test the influence of lagged growth on the amount of official debt affected by the restructuring. Finally, in Table 5b, we test the influence of lagged growth on the current level of debt affected by a private (columns 1-3) and an official (4-6) haircut. In all specifications we do not find any evidence that lagged growth is a good predictor for either private or official restructuring (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 these two sections 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. In the next Section we present some evidence of causality between default and growth adopting an alternative specification method.

TABLE 4 & 5 HERE

6 Synthetic Control Method

In this Section we investigate the heterogeneity of the economic impact of debt restructurings by constructing a counterfactual of the path of the GDP per capita for each country that had only either private or official debt restructurings. The missing counterfactual outcome is estimated with 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.[33] 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 GDP per capita of a sample of non-defaulters.

[33] 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).

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 (hereafter 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-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.

A formal presentation of the method is provided in the Online Appendix C, whereas in the following subsections, first, we describe the selected case studies. Then, we present the results obtained for each private/official default. Finally, we aggregate the country-specific effects of defaults into average effects, distinguishing between the two types of restructurings.

6.1 Sample and data

In order to disentangle the effect of private and official defaults, we consider 18 countries that had only official debt restructurings (through the Paris Club), and 4 countries that had only private restructurings, as described in Section 3.1.[34] Among private defaulters, we have to exclude Venezuela, since, as an oil exporter country, it proves difficult to find a credible comparison unit that reproduces the volatile path of its GDP per capita, whereas we include Argentina, given the relevance of its default episode in 2001. Although Argentina has also experienced in the eighties (1982-93) a default involving both private and public creditors, the interval between the two episodes is long enough to prevent us from capturing the effect of the first episode.

[34] 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.

As official restructurings are concerned, we should exclude seven countries (out of 18) for different reasons. We exclude Angola and Cambodia due to data availability constraints.[35] 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, whose RMSPE was too high to guarantee a credible estimation of the treatment effect. 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 the debt crisis.

Therefore, we overall discuss four and eleven cases of private and official debt crises, 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. Moreover, manteining the same time specification of equation (1), it allows us to compare the SCM results with those presented in Section 4. On the other hand, in this setting we are unable to take into account the heterogeneity across default episodes, that is by taken into account either the amount of the restructured debt or the face value reduction.

Following previous macroeconomic applications of the SCM (e.g., Abadie and Gardeazabal 2003), 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.[36]

We use two sets of predictors. The first one encompasses the same variables used in the regression analysis (investment, openness, government consumption, population growth and total population, secondary education, terms of trade, civil liberties, and banking crisis). The second one includes only those variables that are typically used in growth regressions, such as the level of investments, openness, population growth and total population, and secondary education. We add to both of these specifications the average GDP per capita calculated in the 10 years preceding the crisis.[37] Although the two sets of predictors lead to similar results, we take into account, for each case study,

[35] As already highlighted, 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 Angola and Cambodia are available from 1986 (three years before the default) and 1993 (two years before the default), respectively.

[36] The results are unchanged considering different pools of control units. In particular, we consider countries that had official restructurings as additional comparison units for each private restructuring episode, and countries that had private restructurings as additional controls for each official restructuring episode. Results are available upon request.

[37] 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.

the set that guarantees a better fit in the pre-treatment period, in order to increase the confidence on the post-treatment projection of the synthetic outcome.[38] The predictors are averaged over a 10-year pre-event period, and the path of the outcome variable is observed until 2014.[39]

6.2 Private default

Table 6 reports the weights assigned to each country that constitutes the synthetic, and the predictor balance obtained through the optimization procedure 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.[40] As can be seen, the synthetic control groups are able to reproduce the pre-treatment characteristics of the treated countries. 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 -0.01 percent (in the case of Argentina) to 0.29 percent (in the case of Pakistan). Instead, the degree of similarity is smaller for Uruguay and Argentina when judged by openness and population growth and by openness and terms of trade, respectively. However, these variables have a relative low predictive power for the outcome variable prior to the debt default.[41] Overall, we can argue that the control units constructed by the SCM are not statistically different from their respective treated countries.

The ability of the SCM to produce a reliable control unit can be also assessed from Figure 5, which displays the path of GDP per capita in each country that had private defaults (solid 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.

[38] Figures B1 and B2 in Appendix B present the graphs obtained using the alternative set of predictors.

[39] 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 6 and 8 show, for each country, which variables are actually used to conduct the optimization procedure.

[40] 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.

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

TABLE 6 HERE

FIGURES 5 HERE

Although Figure 5 shows that private defaults affect defaulting countries differently, each of the four 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).[42] Some striking differences, however, are detectable afterwards. In particular, while the output losses increase in the case of Paraguay and South Africa, Uruguay 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 ten years from the start of the debt crisis.[43]

Furthermore, we tested 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).[44] Blue lines in Figure B3 in the Online Appendix B depict the leave-one-out synthetic outcomes, while black and red lines reproduce actual and synthetic outcomes, respectively, as in Figure 5. This sensitivity test confirms that the results presented above are fairly robust to changes in the composition of the control group. Argentina represents a remarkable exception since its synthetic counterpart would have been significantly below the path of its GDP per capita excluding Hungary from the control group. However, the divergence from the results presented above may be explained by the lesser ability of the algorithm to match the pre-event outcome of our case study. Indeed, the leave-one-out procedure entails a lower fit between the treated and the synthetic unit in the pre-event period.

Table 7, then, quantifies the economic impact of a private default up to ten years since the beginning of the default.[45] As can be seen, the negative gap between the actual and the synthetic GDP per capita consistently increases in the case of Paraguay and South Africa, while it starts to decrease five years after the Argentinean default. Finally, the negative effect is somehow constant in Uruguay during the period considered. On average, the annual GDP gap induced by the default ranges between -10.17 percent (Paraguay) and -16.66 percent (South Africa). Estimates over a larger time span provide even a stronger difference between defaulters and their synthetic units. On the other hand, Table 8 shows that the average negative effect in Paraguay and South Africa

[42] Notice that the duration of the debt crisis differs quite substantially, ranging from four years in the case of Argentina to eight years for South Africa and Uruguay.

[43] Interestingly, unlike Paraguay and South Africa, these countries benefitted from a haircut associated with a face value reduction.

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

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

is far higher considering the ten years following the last deal, reaching the values of -22.20 percent and -32.13 percent, respectively. During the same time span, the output of Argentina is on average 5.12 percent lower with respect to the synthetic. Uruguay was able to overcome the output loss completely seven years after the end of the debt crisis (i.e., fifteen years after the initial deal), even if its GDP per capita dropped again in the following year.

TABLES 7-8 HERE

The SCM does not allow for a validation of our results using the traditional modes of statistical inference because of the small-sample nature of the data. We overcome this problem by implementing in-space placebo tests, which 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.

Figure B5 in the Online Appedix B shows the results of these experiments: 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. The placebo tests prove that private haircuts negatively affect the GDP of the defaulters, although heterogeneously. Considering the ten years after the end of the debt crisis, the negative effect on the output of Argentina is never statistically significant, but in the fourth and fifth leads, while it is significant up to five (and from nine to ten) years after the end of the debt crisis in the case of Uruguay. P-values of Paraguay and South Africa are below the 10% level throughout the period considered.[46]

6.3 Official Default

Table 9 reports the weights assigned to each country that constitutes the synthetic, and the predictor balance obtained through the optimization procedure 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. In this case, the difference in the average GDP per capita in the period before the debt crisis ranges between -0.89 percent

[46] The negative effect is significant up to four years after the start of the debt crisis in Argentina, while it becomes statistically different from zero after six years in the case of Paraguay. In the same time span, P-values of South Africa and Uruguay are always below the 10% level. Graphs are available upon request.

(Chad) and 0.39 percent (Burkina Faso). The synthetic counterparts of Burkina Faso, Chad and Mali show considerable higher values for secondary education, but this not invalidate our analysis, given the low prediction power attributed to this specific variable. Thus, we are again confident that the SCM reduces the possible bias arising from control units that do not provide a satisfying fit in terms of pre-treatment variables.

Figure 6 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.[47]

TABLE 9 HERE

FIGURES 6 HERE

Defaults on official debt show also heterogeneous effects across countries. However, none of the defaulters has a reduction in the output that lasts after the end of the debt crisis, and, for some of them, the default seems to have a positive effect on growth.[48] More precisely, Benin and Chad were able to recover the output losses, achieving a GDP per capita higher than their counterparts at the end of the debt crisis (indicated by the second vertical line). Indonesia and Sri Lanka show a negative outcome gap after the default but they catch up with their synthetic units within six years after the resolution of the crisis. Mali displays both negative and positive values of the outcome gap, but its GDP per capita starts to increase consistently seven years after the default. In the case of Ghana, there is no evidence of an effect of the default, whereas this event seems to have a positive impact on growth for El Salvador and Georgia. Finally, the GPD per capita of Burkina Faso, Egypt and Kyrgyz Republic is constantly below its counterfactual in the post-treatment period, but it did not drop significantly after the default. In particular, while the output path is somewhat flat in the case of Egypt, the GDP of Burkina Faso and Kyrgyz Republic smoothly increases.

The leave-one-out procedure presented in Figure B4 in the Online Appendix B confirms that these results are not driven by the composition of the control group. Exceptions are Georgia and Mali, for which the paths of the synthetic outcomes would be above the actual outcomes, and Sri Lanka,

[47] Even when the SCM does not provide a perfect fit for the outcome variable prior to the default, the divergence is negligible, especially considering the years right before the event, and the two paths of GDP per capita start to diverge significantly only after the beginning of the crisis.

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

whose synthetic counterpart would be below its GDP per capita. It is worth saying that these results may be explained by the higher RMSPE obtained by the procedure.

Table 10 sets out the economic impact of official defaults in the ten years following the initial deal.[49] During this time window, Chad, Egypt, Indonesia, Kyrgyz Republic, and Mali all show an increasing negative gap between the actual and the synthetic GDP per capita. Such negative effect, however, decreases substantially from the last debt restructuring in the case of Chad, Indonesia, and Mali. As shown in Table 11, the average effects calculated over ten years after the end of the crisis are -1.98 percent, -3.67 percent, and -1.18 percent, respectively. On the contrary, El Salvador and Georgia show an increasing positive gap. On average, the annual outcome gap induced by the default ten years after the last restructuring ranges between -32.80 percent (Egypt) and +33.40 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. Moreover, they are far smaller than the one observed in the case of private haircuts ten years after the first deal, which is consistent with the findings presented in Section 4.

This is also confirmed by the placebo tests presented in Figure B6 in the Online Appendix B, which also show that the effect of official default is not significant at conventional levels, with the exceptions of Egypt, which is significantly negatively affected throughout the period considered.[50]

TABLES 10-11 HERE

6.4 Average effects

In this subsection, we aggregate the country-specific effects of private and official defaults into average effects in order to improve the comparability of the results obtained through GLS. Following Cavallo *et al.* (2013), first, we 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 7 presents the average impacts of debt crises 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

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

[50] Egypt is negatively affected starting from 5 years after the start of the debt crisis, whereas the impact on Indonesia is positive and significant three years after the default. Graphs are available upon request.

below the synthetic counterpart, the magnitude of the economic effect on private defaulters is far higher. Indeed, after a decline which precedes the default and is short-lived 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 country had finalized its last private restructuring, GDP per capita is, on average, 12% higher than it was at the time of default, whereas it would be 35% higher in the counterfactual scenario. This gap further increases in the aftermath, reaching about 40% in the following ten years.

Conversely, official defaulters not only do not show a drop in the output, but they are also able to catch up their synthetic counterpart. When official restructurings lead all defaulters out of the crisis, 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.

FIGURE 7 HERE

In summary, consistently with the results obtained from the regression analysis, we find that countries involved in only private restructuring are not able to recover their output losses in the medium-long run. Differently than before, however, we do not find evidence of a boost in growth, in the long run, 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.

7 Conclusions

This paper studies the relationship between debt default and (annual) GDP growth, over a period of ten years, by taking into account both the depth of a debt restructuring and the face value reduction, and by distinguishing between private and official creditors. More specifically, the amount of restructured (forgiven) debt is used as a proxy for the severity of the default episode. Analyzing 520 restructuring episodes over the period 1975-2013, we find that private and official defaults are associated with different outcomes in terms of growth.

Controlling for the severity of the debt crisis, we are able to detect a more lasting and negative relationship between default and growth. While private defaults are generally associated with

lower growth both during the crisis and over the long run (mitigated by the amount involved), for official defaulters we do not observe a growth contraction throughout the years of the crisis and they are associated with higher growth over the long run (independently of the amount involved). When debt relief operations involve debt write offs, however, the negative relationship between private default and growth becomes weaker, while official defaulters now strongly benefit in terms of growth from the face value reduction.

Therefore, the trade-off concerning the effects of sovereign debt restructurings seems to be associated with opposite outcomes for private and official defaulters. While we find no effect of face value reduction on growth for the former, an official haircut increases growth, over the long term, by an annual average of about 2 percent. This evidence seems to suggest that for private defaulters the positive and negative spillovers of a debt reduction overall compensate each other, while for official defaulters positive spillovers seems to prevail. Taken together, the results point to confirm that official and private defaults may have different effects and should then be treated differently.

Using the SCM, we adopt a case study analysis to investigate this heterogeneous response of GDP per capita to private and official restructurings. Consistently with the results obtained from the regression analysis, 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. The difference between the results obtained in the regression analysis is due to the fact that the SCM isolates the effects of a specific type of deals and evaluates private and official defaulters considering a counterfactual sample made by non defaulters. That should explain the stronger (weaker) negative (positive) effects we detect for private (official) defaults using SCM with respect to GLS.

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, especially after a face value reduction. These results might then provide important insight for the current debate on providing Greece with an official debt relief, when its third bailout program will be soon finalized (Eichengreen *et al.* 2018, European Commission 2018, Eurogroup 2017 and IMF 2017).

The analysis is of course limited in several respects. We do not claim to draw causal inferences from

the empirical analysis, given the nature of the data available. 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 low growth can only be indicative of a correlation between the two variables. We could observe punishment effects, reputational effects or none of the two. Lower growth might not be the consequence of a default per se but of other factors affecting debt sustainability as well. Thus, 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 this difference. In a companion paper (Marchesi and Masi 2018), however, we confirm that commercial and official defaults are associated with different outcomes also in terms of credit ratings (both when provided by credit agencies and institutional investors). While private defaults seem to involve some reputational costs up to the long run, official defaults are associated with a contraction in credit rating only in the default year. After that official defaulters seem not affected (or even benefit) by the restructuring episodes. To the extent that credit ratings is a good proxy for borrowing costs, this evidence may suggest that the positive growth prospects for official defaulters, after the end of the default, might also be due to the absence of a negative stigma in the credit market.

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Tables and figure

Table 1a: Restructurings and Haircuts over time (in %)

	Observations	Mean	SD	Min	Max
Private restructuring					
1975-1988	85	12	15	0.39	60
1989-2001	57	18	22	0.32	108
2002-2013	14	23	20	2	67
Official restructuring					
1975-1988	120	7	6	0.40	33
1989-2001	176	10	12	0.03	82
2002-2013	68	33	52	0.04	96
Private Haircut					
1975-1988	2	58	40	30	86
1989-2001	38	47	33	1	97
2002-2013	11	65	33	4	97
Official Haircut					
1975-1988	5	33	0	33	33
1989-2001	150	71	23	22	98
2002-2013	52	91	13	22	98

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

Private Restructurings *(Average size %)*		
High Income	Middle Income	Low Income
10	19	6
Private Restructurings (# of countries)		
High Income	Middle Income	Low Income
4	35	16
Official Restructurings *(Average size %)*		
High Income	Middle Income	Low Income
9	11	15
Official Restructurings (# of countries)		
High Income	Middle Income	Low Income
3	38	29
Private Haircuts *(Average size %)*		
High Income	Middle Income	Low Income
26	41	84
Private Haircuts (# of countries)		
High Income	Middle Income	Low Income
3	24	12
Official Haircut *(Average size %)*		
High Income	Middle Income	Low Income
0	69	69
Official Haircut (# of countries)		
High Income	Middle Income	Low Income
0	9	27

Table 2: Private and Official Restructurings and Growth, 1975-2013, GLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Default Duration	-1.017***	-0.459	-0.682*	-0.655	-0.537	-0.530	-0.447	-0.840
	(-4.427)	(-1.277)	(-1.840)	(-1.543)	(-1.230)	(-1.201)	(-0.951)	(-1.617)
Official Default Duration	0.142	-0.396	-0.398	-0.454	-0.392	-0.402	-0.242	0.570
	(0.663)	(-1.287)	(-1.252)	(-1.242)	(-1.029)	(-1.046)	(-0.596)	(1.295)
Private Restructurings			0.037**	0.060**	0.053**	0.053**	0.048*	0.045*
			(2.064)	(2.249)	(2.025)	(2.008)	(1.835)	(1.667)
Official Restructurings			-0.001	-0.014	0.009	0.007	0.007	0.012
			(-0.066)	(-0.403)	(0.252)	(0.202)	(0.195)	(0.347)
Final Priv. Restr. Dummy				0.871	0.886	1.155	1.172	0.415
				(1.204)	(1.233)	(1.204)	(1.210)	(0.441)
Final Priv. Restr. Dummy (-1)				0.470	0.523	0.316	0.122	-0.515
				(0.720)	(0.776)	(0.353)	(0.135)	(-0.583)
Final Priv. Restr. Dummy (-2)				1.749**	1.864**	2.883***	2.994***	2.337**
				(2.431)	(2.515)	(3.017)	(3.066)	(2.466)
Final Priv. Restr. Dummy (-3)				0.421	0.461	0.022	-0.035	-0.639
				(0.647)	(0.698)	(0.025)	(-0.040)	(-0.739)
Final Priv. Restr. Dummy (-4)					0.736	0.357	0.227	-0.382
					(1.133)	(0.426)	(0.264)	(-0.447)
Final Priv. Restr. Dummy (-5)					-0.417	0.171	0.037	-0.773
					(-0.674)	(0.213)	(0.046)	(-0.952)
Final Priv. Restr. Dummy (-6)							-0.582	-1.722**
							(-0.750)	(-2.178)
Final Priv. Restr. Dummy (-7)							-0.132	-0.863
							(-0.148)	(-0.980)
Final Priv. Restr. Dummy (-8)								-1.579*
								(-1.795)
Final Priv. Restr. Dummy (-9)								-1.323
								(-1.533)
Final Priv. Restr. Dummy (-10)								-1.325
								(-1.640)
Final Private Restructuring						-0.014	-0.013	-0.006
						(-0.434)	(-0.421)	(-0.190)
Final Private Restructuring (-1)						0.010	0.016	0.023
						(0.311)	(0.517)	(0.773)
Final Private Restructuring (-2)						-0.053*	-0.052*	-0.042
						(-1.904)	(-1.854)	(-1.501)
Final Private Restructuring (-3)						0.026	0.030	0.037
						(1.020)	(1.184)	(1.466)
Final Private Restructuring (-4)						0.023	0.029	0.041
						(0.915)	(1.130)	(1.603)
Final Private Restructuring (-5)						-0.027	-0.026	-0.014
						(-1.057)	(-1.012)	(-0.555)
Final Private Restructuring (-6)							0.026	0.044*
							(1.031)	(1.729)
Final Private Restructuring (-7)							0.016	0.021
							(0.578)	(0.764)
Final Private Restructuring (-8)								0.052*
								(1.925)
Final Private Restructuring (-9)								0.027
								(1.000)
Final Private Restructuring (-10)								0.043*
								(1.671)
Final Off. Restr. Dummy				0.153	0.221	0.370	0.596	1.485*
				(0.246)	(0.355)	(0.508)	(0.807)	(1.948)
Final Off. Restr. Dummy (-1)				0.964	0.900	0.575	0.679	1.505**
				(1.602)	(1.478)	(0.782)	(0.930)	(2.061)
Final Off. Restr. Dummy (-2)				0.130	0.126	0.425	0.757	1.634**
				(0.222)	(0.212)	(0.566)	(1.015)	(2.201)
Final Off. Restr. Dummy (-3)				-0.719	-0.574	-0.555	-0.186	0.559
				(-1.090)	(-0.861)	(-0.680)	(-0.228)	(0.692)
Final Off. Restr. Dummy (-4)					0.350	0.007	0.274	1.081
					(0.554)	(0.009)	(0.342)	(1.361)
Final Off. Restr. Dummy (-5)					-0.120	0.462	0.620	1.043
					(-0.206)	(0.658)	(0.879)	(1.504)
Final Off. Restr. Dummy (-6)							0.679	1.269*

							(0.862)	(1.665)
Final Off. Restr. Dummy (-7)							0.353	0.886
							(0.387)	(1.012)
Final Off. Restr. Dummy (-8)								1.612**
								(2.015)
Final Off. Restr. Dummy (-9)								0.339
								(0.393)
Final Off. Restr. Dummy (-10)								-0.311
								(-0.349)
Final Official Restructuring						-0.007	-0.008	-0.010
						(-0.336)	(-0.381)	(-0.504)
Final Official Restructuring (-1)						0.015	0.016	0.012
						(0.696)	(0.732)	(0.598)
Final Official Restructuring (-2)						-0.016	-0.017	-0.020
						(-0.500)	(-0.560)	(-0.752)
Final Official Restructuring (-3)						0.005	0.005	0.008
						(0.182)	(0.187)	(0.306)
Final Official Restructuring (-4)						0.016	0.018	0.012
						(0.541)	(0.613)	(0.478)
Final Official Restructuring (-5)						-0.048*	-0.042	-0.029
						(-1.650)	(-1.463)	(-1.154)
Final Official Restructuring (-6)							0.010	0.014
							(0.277)	(0.505)
Final Official Restructuring (-7)							0.037	0.041
							(0.725)	(0.906)
Final Official Restructuring (-8)								0.042
								(1.486)
Final Official Restructuring (-9)								0.034
								(1.177)
Final Official Restructuring (-10)								0.029
								(0.987)
Investment		0.183***	0.182***	0.195***	0.190***	0.188***	0.188***	0.182***
		(10.678)	(10.635)	(10.593)	(9.909)	(9.696)	(9.405)	(8.841)
(delta) Population		-0.642***	-0.630***	-0.532**	-0.472**	-0.469**	-0.481**	-0.583***
		(-3.156)	(-3.102)	(-2.514)	(-2.237)	(-2.153)	(-2.186)	(-2.611)
Secondary Edu		-0.047***	-0.047***	-0.047***	-0.043***	-0.044***	-0.046***	-0.052***
		(-3.724)	(-3.718)	(-3.451)	(-3.063)	(-3.151)	(-3.187)	(-3.508)
(log) Population		0.247*	0.245*	-3.730***	-3.639***	-3.444***	-2.782**	-2.799*
		(1.957)	(1.937)	(-3.111)	(-2.931)	(-2.754)	(-2.109)	(-1.927)
Government Cons.		-0.163***	-0.160***	-0.179***	-0.182***	-0.185***	-0.185***	-0.200***
		(-5.316)	(-5.236)	(-5.246)	(-5.555)	(-5.614)	(-5.319)	(-5.529)
Civil Liberties		0.196*	0.204*	0.290**	0.324***	0.341***	0.284**	0.392***
		(1.739)	(1.812)	(2.450)	(2.584)	(2.705)	(2.118)	(2.745)
(delta) Terms of Trade		-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
		(-0.955)	(-0.949)	(-1.139)	(-1.130)	(-1.046)	(-1.132)	(-1.071)
Openness		-0.012**	-0.012**	-0.014**	-0.012*	-0.011	-0.007	-0.009
		(-1.976)	(-2.038)	(-2.130)	(-1.696)	(-1.554)	(-1.009)	(-1.184)
Banking Crises		-1.418***	-1.407***	-1.218***	-1.132***	-1.157***	-1.350***	-1.070***
		(-4.774)	(-4.742)	(-4.060)	(-3.779)	(-3.845)	(-4.418)	(-3.388)
Observations	4,020	1,559	1,559	1,470	1,411	1,411	1,345	1,238
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES
Number of id	117	72	72	72	72	72	72	72

t-statistics in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3: Private and Official Haircuts and Growth, 1975-2013, GLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Default Duration	-1.017***	-0.459	-0.518	-0.494	-0.378	-0.394	-0.384	-0.508
	(-4.427)	(-1.277)	(-1.409)	(-1.168)	(-0.870)	(-0.903)	(-0.818)	(-0.990)
Official Default Duration	0.142	-0.396	-0.433	-0.452	-0.334	-0.338	-0.256	0.307
	(0.663)	(-1.287)	(-1.354)	(-1.248)	(-0.886)	(-0.900)	(-0.647)	(0.708)
Private Haircut			0.019	0.010	0.007	0.012	0.017	0.007
			(1.257)	(0.127)	(0.091)	(0.164)	(0.231)	(0.105)
Official Haircut			0.004	0.005	0.005	0.001	0.002	0.001
			(0.632)	(0.542)	(0.587)	(0.124)	(0.250)	(0.108)
Final Priv. Haircut Dummy				0.890	0.907	0.600	0.410	0.149
				(1.226)	(1.259)	(0.647)	(0.437)	(0.166)
Final Priv. Haircut Dummy (-1)				0.528	0.562	1.444	1.310	1.245
				(0.807)	(0.832)	(1.472)	(1.342)	(1.342)
Final Priv. Haircut Dummy (-2)				1.723**	1.855**	1.187	1.317	1.657*
				(2.387)	(2.498)	(1.150)	(1.249)	(1.675)
Final Priv. Haircut Dummy (-3)				0.486	0.499	-0.183	-0.244	-0.487
				(0.746)	(0.756)	(-0.204)	(-0.264)	(-0.544)
Final Priv. Haircut Dummy (-4)					0.757	0.300	0.192	0.336
					(1.167)	(0.325)	(0.203)	(0.361)
Final Priv. Haircut Dummy (-5)					-0.438	-0.637	-0.982	-1.372
					(-0.707)	(-0.709)	(-1.053)	(-1.497)
Final Priv. Haircut Dummy (-6)							-0.616	-1.221
							(-0.642)	(-1.261)
Final Priv. Haircut Dummy (-7)							0.649	-0.153
							(0.656)	(-0.156)
Final Priv. Haircut Dummy (-8)								-0.978
								(-0.983)
Final Priv. Haircut Dummy (-9)								-1.792*
								(-1.837)
Final Priv. Haircut Dummy (-10)								-0.794
								(-0.855)
Final Private Haircut						0.007	0.012	0.009
						(0.398)	(0.666)	(0.513)
Final Private Haircut (-1)						-0.025	-0.024	-0.028*
						(-1.360)	(-1.308)	(-1.663)
Final Private Haircut (-2)						0.014	0.013	0.003
						(0.658)	(0.603)	(0.176)
Final Private Haircut (-3)						0.021	0.021	0.022
						(1.179)	(1.184)	(1.320)
Final Private Haircut (-4)						0.007	0.005	-0.003
						(0.401)	(0.283)	(-0.150)
Final Private Haircut (-5)						0.005	0.007	0.010
						(0.298)	(0.389)	(0.581)
Final Private Haircut (-6)							0.003	-0.004
							(0.184)	(-0.239)
Final Private Haircut (-7)							-0.029	-0.021
							(-1.322)	(-1.016)
Final Private Haircut (-8)								0.007
								(0.343)
Final Private Haircut (-9)								0.019
								(1.008)
Final Private Haircut (-10)								0.008
								(0.457)
Final Off. Haircut Dummy				0.154	0.250	0.252	0.415	0.737
				(0.248)	(0.402)	(0.305)	(0.501)	(0.908)
Final Off. Haircut Dummy (-1)				1.007*	0.951	1.587*	1.735**	2.217***
				(1.677)	(1.566)	(1.876)	(2.118)	(2.848)
Final Off. Haircut Dummy (-2)				0.124	0.144	1.347*	1.762**	2.219***
				(0.211)	(0.241)	(1.681)	(2.261)	(2.970)
Final Off. Haircut Dummy (-3)				-0.698	-0.546	-0.402	0.179	0.879
				(-1.055)	(-0.818)	(-0.501)	(0.226)	(1.146)
Final Off. Haircut Dummy (-4)					0.397	0.508	1.037	1.654**
					(0.627)	(0.658)	(1.314)	(2.148)
Final Off. Haircut Dummy (-5)					-0.114	1.269*	1.486**	1.931***
					(-0.195)	(1.733)	(2.009)	(2.668)
Final Off. Haircut Dummy (-6)							1.542*	2.260***

							(1.908)	(2.886)
Final Off. Haircut Dummy (-7)							1.681*	2.297**
							(1.689)	(2.297)
Final Off. Haircut Dummy (-8)								1.886**
								(2.395)
Final Off. Haircut Dummy (-9)								1.511*
								(1.888)
Final Off. Haircut Dummy (-10)								0.785
								(0.906)
Final Official Haircut						-0.006	-0.007	-0.005
						(-0.435)	(-0.579)	(-0.413)
Final Official Haircut (-1)						-0.016	-0.018	-0.018
						(-1.307)	(-1.513)	(-1.556)
Final Official Haircut (-2)						-0.031**	-0.036***	-0.037***
						(-2.560)	(-3.029)	(-3.082)
Final Official Haircut (-3)						-0.001	-0.010	-0.011
						(-0.106)	(-0.691)	(-0.764)
Final Official Haircut (-4)						-0.003	-0.011	-0.015
						(-0.199)	(-0.805)	(-1.086)
Final Official Haircut (-5)						-0.043***	-0.047***	-0.046***
						(-3.430)	(-3.684)	(-3.741)
Final Official Haircut (-6)							-0.023*	-0.028**
							(-1.714)	(-2.138)
Final Official Haircut (-7)							-0.019	-0.023
							(-1.410)	(-1.629)
Final Official Haircut (-8)								0.006
								(0.484)
Final Official Haircut (-9)								-0.021
								(-1.409)
Final Official Haircut (-10)								-0.013
								(-0.883)
Investment		0.183***	0.184***	0.195***	0.191***	0.197***	0.195***	0.180***
		(10.678)	(10.708)	(10.575)	(9.906)	(10.090)	(9.766)	(8.766)
(delta) Population		-0.642***	-0.639***	-0.534**	-0.479**	-0.471**	-0.467**	-0.553**
		(-3.156)	(-3.167)	(-2.542)	(-2.283)	(-2.173)	(-2.134)	(-2.460)
Secondary Edu		-0.047***	-0.047***	-0.045***	-0.041***	-0.041***	-0.044***	-0.052***
		(-3.724)	(-3.664)	(-3.348)	(-2.972)	(-2.977)	(-3.103)	(-3.635)
(log) Population		0.247*	0.240*	-3.535***	-3.464***	-3.279***	-2.091	-1.930
		(1.957)	(1.914)	(-2.951)	(-2.792)	(-2.652)	(-1.565)	(-1.206)
Government Cons.		-0.163***	-0.158***	-0.181***	-0.185***	-0.185***	-0.185***	-0.205***
		(-5.316)	(-5.118)	(-5.295)	(-5.637)	(-5.645)	(-5.403)	(-5.782)
Civil Liberties		0.196*	0.199*	0.278**	0.319**	0.296**	0.232*	0.305**
		(1.739)	(1.761)	(2.348)	(2.533)	(2.366)	(1.738)	(2.139)
(delta) Terms of Trade		-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
		(-0.955)	(-0.941)	(-1.118)	(-1.115)	(-1.068)	(-1.136)	(-1.025)
Openness		-0.012**	-0.011*	-0.013**	-0.011*	-0.012*	-0.009	-0.012
		(-1.976)	(-1.875)	(-2.069)	(-1.655)	(-1.786)	(-1.284)	(-1.572)
Banking Crises		-1.418***	-1.394***	-1.179***	-1.099***	-1.014***	-1.164***	-0.928***
		(-4.774)	(-4.686)	(-3.907)	(-3.648)	(-3.350)	(-3.791)	(-2.899)
Observations	4,020	1,559	1,559	1,470	1,411	1,411	1,345	1,238
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Number of id	117	72	72	72	72	72	72	72

t-statistics in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 4: Robustness check for Private and Official Restructurings and Haircuts

	Private and Official Restructurings			Private and Official Haircuts		
	(1)	(2)	(3)	(4)	(5)	(6)
Private Default Duration	-0.634	-1.005*	-1.123*	-0.443	-0.654	-0.354
	(-1.248)	(-1.763)	(-1.925)	(-0.874)	(-1.163)	(-0.614)
Official Default Duration	0.766*	0.635	1.149**	0.557	0.366	0.759
	(1.791)	(1.314)	(2.402)	(1.303)	(0.777)	(1.562)
Private Restructuring/Haircut	0.038	0.047*	0.044	-0.002	0.008	-0.069
	(1.411)	(1.811)	(1.501)	(-0.026)	(0.119)	(-0.968)
Official Restructuring/Haircut	0.011	0.010	0.003	-0.002	-0.002	-0.005
	(0.336)	(0.287)	(0.078)	(-0.255)	(-0.197)	(-0.602)
Final Priv. Restr./Haircut Dummy	0.138	0.183	-0.567	0.043	0.008	0.208
	(0.153)	(0.195)	(-0.575)	(0.049)	(0.008)	(0.222)
Final Priv. Restr./Haircut Dummy (-1)	-0.608	-0.349	-1.250	1.061	1.002	0.178
	(-0.730)	(-0.385)	(-1.266)	(1.196)	(1.063)	(0.188)
Final Priv. Restr./Haircut Dummy (-2)	2.279**	2.213**	0.972	1.058	1.504	0.269
	(2.464)	(2.291)	(1.009)	(1.089)	(1.490)	(0.262)
Final Priv. Restr./Haircut Dummy (-3)	-0.954	-0.698	-2.123**	-1.056	-0.488	-1.107
	(-1.124)	(-0.786)	(-2.212)	(-1.213)	(-0.534)	(-1.196)
Final Priv. Restr./Haircut Dummy (-4)	-0.631	-0.477	-0.595	-0.059	0.173	0.754
	(-0.750)	(-0.546)	(-0.674)	(-0.065)	(0.183)	(0.830)
Final Priv. Restr./Haircut Dummy (-5)	-0.689	-0.864	-0.962	-1.510*	-1.489	-2.314***
	(-0.855)	(-1.040)	(-1.201)	(-1.703)	(-1.598)	(-2.766)
Final Priv. Restr./Haircut Dummy (-6)	-1.911**	-1.921**	-2.207***	-0.971	-1.204	-1.401
	(-2.494)	(-2.375)	(-2.977)	(-1.032)	(-1.230)	(-1.571)
Final Priv. Restr./Haircut Dummy (-7)	-0.968	-0.923	-1.872**	-0.286	-0.220	-0.367
	(-1.112)	(-1.030)	(-2.172)	(-0.299)	(-0.224)	(-0.409)
Final Priv. Restr./Haircut Dummy (-8)	-1.705**	-1.497*	-1.567*	-1.094	-1.014	-1.091
	(-1.967)	(-1.674)	(-1.761)	(-1.145)	(-1.022)	(-1.075)
Final Priv. Restr./Haircut Dummy (-9)	-1.116	-1.282	-0.510	-1.669*	-1.773*	-0.829
	(-1.317)	(-1.458)	(-0.593)	(-1.787)	(-1.814)	(-0.818)
Final Priv. Restr./Haircut Dummy (-10)	-1.215	-1.188	-1.667**	-0.433	-0.715	-0.846
	(-1.515)	(-1.480)	(-2.064)	(-0.482)	(-0.783)	(-0.895)
Final Private Restructuring/Haircut	0.002	-0.001	0.014	0.006	0.009	-0.005
	(0.063)	(-0.043)	(0.436)	(0.359)	(0.525)	(-0.366)
Final Private Restructuring/Haircut (-1)	0.019	0.010	0.014	-0.028*	-0.024	-0.015
	(0.645)	(0.341)	(0.462)	(-1.685)	(-1.447)	(-1.031)
Final Private Restructuring/Haircut (-2)	-0.047*	-0.041	-0.023	0.013	0.004	0.026
	(-1.650)	(-1.473)	(-0.836)	(0.703)	(0.189)	(1.486)
Final Private Restructuring/Haircut (-3)	0.035	0.037	0.054**	0.025	0.020	0.013
	(1.370)	(1.459)	(2.079)	(1.554)	(1.193)	(0.813)
Final Private Restructuring/Haircut (-4)	0.033	0.039	0.042	-0.004	-0.001	-0.013
	(1.270)	(1.508)	(1.614)	(-0.239)	(-0.062)	(-0.869)
Final Private Restructuring/Haircut (-5)	-0.029	-0.016	-0.014	0.009	0.010	0.026*
	(-1.114)	(-0.636)	(-0.545)	(0.550)	(0.594)	(1.709)
Final Private Restructuring/Haircut (-6)	0.052**	0.045*	0.051**	-0.011	-0.006	-0.007
	(2.051)	(1.767)	(2.089)	(-0.655)	(-0.392)	(-0.484)
Final Private Restructuring/Haircut (-7)	0.020	0.022	0.039	-0.021	-0.020	-0.029*
	(0.710)	(0.792)	(1.486)	(-0.997)	(-0.943)	(-1.683)
Final Private Restructuring/Haircut (-8)	0.053*	0.049*	0.048*	0.010	0.010	0.017
	(1.934)	(1.794)	(1.789)	(0.523)	(0.471)	(0.880)
Final Private Restructuring/Haircut (-9)	0.022	0.028	0.026	0.023	0.021	0.020
	(0.815)	(1.026)	(0.978)	(1.211)	(1.104)	(1.168)
Final Private Restructuring/Haircut (-10)	0.043*	0.041*	0.038	0.001	0.008	-0.003
	(1.680)	(1.650)	(1.495)	(0.069)	(0.449)	(-0.164)
Final Off. Restr./Haircut Dummy	1.673**	1.545**	1.428*	1.312	0.727	0.840
	(2.246)	(2.013)	(1.882)	(1.587)	(0.893)	(1.030)
Final Off. Restr./Haircut Dummy (-1)	1.457**	1.569**	1.191*	2.338***	2.320***	1.458*
	(2.096)	(2.119)	(1.705)	(3.167)	(2.963)	(1.899)
Final Off. Restr./Haircut Dummy (-2)	1.712**	1.661**	2.351***	2.024***	2.217***	1.850**
	(2.374)	(2.197)	(3.180)	(2.830)	(2.928)	(2.501)
Final Off. Restr./Haircut Dummy (-3)	0.297	0.506	0.423	0.671	0.954	0.511
	(0.375)	(0.618)	(0.560)	(0.908)	(1.230)	(0.679)
Final Off. Restr./Haircut Dummy (-4)	1.250	1.118	0.558	1.740**	1.745**	1.144
	(1.589)	(1.394)	(0.708)	(2.331)	(2.244)	(1.456)
Final Off. Restr./Haircut Dummy (-5)	1.015	1.117	0.877	1.753**	1.956***	1.242
	(1.540)	(1.573)	(1.192)	(2.548)	(2.681)	(1.622)
Final Off. Restr./Haircut Dummy (-6)	1.320*	1.354*	1.493*	2.179***	2.336***	2.319***

	(1.748)	(1.763)	(1.928)	(2.924)	(2.990)	(2.819)
Final Off. Restr./Haircut Dummy (-7)	0.851	0.878	0.752	1.768*	2.094**	1.461
	(0.950)	(0.986)	(0.844)	(1.871)	(2.109)	(1.482)
Final Off. Restr./Haircut Dummy (-8)	1.416*	1.473*	0.967	1.674**	1.708**	0.599
	(1.813)	(1.814)	(1.307)	(2.225)	(2.158)	(0.815)
Final Off. Restr./Haircut Dummy (-9)	0.369	0.436	0.380	1.274*	1.427*	0.602
	(0.436)	(0.503)	(0.455)	(1.664)	(1.783)	(0.775)
Final Off. Restr./Haircut Dummy (-10)	-0.410	-0.576	-0.907	0.333	0.587	0.687
	(-0.467)	(-0.649)	(-0.999)	(0.406)	(0.692)	(0.814)
Final Official Restructuring/Haircut	-0.011	-0.014	-0.021*	-0.013	-0.006	-0.020
	(-0.623)	(-0.721)	(-1.734)	(-0.991)	(-0.502)	(-1.636)
Final Official Restructuring/Haircut (-1)	0.012	0.011	-0.010	-0.021*	-0.022*	-0.025**
	(0.697)	(0.568)	(-0.823)	(-1.865)	(-1.816)	(-2.152)
Final Official Restructuring/Haircut (-2)	-0.028	-0.022	-0.062**	-0.034***	-0.038***	-0.032***
	(-1.095)	(-0.793)	(-2.446)	(-2.901)	(-3.164)	(-2.758)
Final Official Restructuring/Haircut (-3)	0.013	0.009	-0.024	-0.008	-0.014	-0.018
	(0.517)	(0.345)	(-0.979)	(-0.581)	(-0.968)	(-1.427)
Final Official Restructuring/Haircut (-4)	0.007	0.013	-0.010	-0.015	-0.016	-0.027**
	(0.305)	(0.486)	(-0.416)	(-1.137)	(-1.160)	(-2.173)
Final Official Restructuring/Haircut (-5)	-0.033	-0.033	-0.032	-0.048***	-0.050***	-0.035***
	(-1.397)	(-1.270)	(-1.303)	(-3.812)	(-3.906)	(-2.783)
Final Official Restructuring/Haircut (-6)	0.015	0.015	-0.002	-0.027**	-0.030**	-0.029**
	(0.540)	(0.517)	(-0.066)	(-2.025)	(-2.235)	(-2.237)
Final Official Restructuring/Haircut (-7)	0.033	0.044	0.039	-0.017	-0.020	-0.019
	(0.746)	(0.962)	(0.733)	(-1.241)	(-1.455)	(-1.396)
Final Official Restructuring/Haircut (-8)	0.039	0.046	0.030	0.003	0.007	0.010
	(1.398)	(1.550)	(1.106)	(0.244)	(0.547)	(0.786)
Final Official Restructuring/Haircut (-9)	0.020	0.033	0.014	-0.024	-0.019	0.001
	(0.705)	(1.088)	(0.497)	(-1.581)	(-1.250)	(0.060)
Final Official Restructuring/Haircut (-10)	0.018	0.035	0.023	-0.013	-0.014	-0.054***
	(0.625)	(1.143)	(0.791)	(-0.825)	(-0.883)	(-3.184)
Growth (-1)	0.223***			0.216***		
	(7.608)			(7.294)		
Investment	0.128***	0.187***	0.168***	0.133***	0.185***	0.172***
	(6.114)	(8.399)	(7.457)	(6.329)	(8.428)	(7.796)
(delta) Population	-0.505**	-0.590**	-1.267***	-0.500**	-0.582**	-1.122***
	(-2.224)	(-2.415)	(-4.164)	(-2.183)	(-2.386)	(-3.636)
Secondary Edu	-0.042***	-0.050***	-0.055***	-0.042***	-0.050***	-0.062***
	(-2.868)	(-3.040)	(-3.362)	(-2.977)	(-3.161)	(-3.930)
(log) Population	-0.484	-2.853*	-1.032	0.359	-1.924	-0.955
	(-0.344)	(-1.787)	(-0.542)	(0.234)	(-1.101)	(-0.485)
Government Cons.	-0.159***	-0.216***	-0.162***	-0.167***	-0.221***	-0.152***
	(-4.541)	(-5.393)	(-3.337)	(-4.823)	(-5.655)	(-3.237)
Civil Liberties	0.316**	0.344**	-0.094	0.246*	0.264*	-0.180
	(2.293)	(2.226)	(-0.567)	(1.766)	(1.715)	(-1.076)
(delta) Terms of Trade	-0.000*	-0.000	-0.000	-0.000*	-0.000	-0.000
	(-1.715)	(-1.223)	(-0.896)	(-1.710)	(-1.214)	(-0.947)
Openness	-0.006	-0.008	-0.001	-0.008	-0.011	-0.005
	(-0.826)	(-1.021)	(-0.067)	(-1.149)	(-1.413)	(-0.648)
Banking Crises	-0.783**	-0.935***	-0.947***	-0.639**	-0.821**	-0.833**
	(-2.510)	(-2.839)	(-2.682)	(-2.033)	(-2.470)	(-2.425)
Currency Crises			-3.706***			-3.673***
			(-6.394)			(-6.446)
Debt to GDP			-2.500***			-2.660***
			(-4.884)			(-5.203)
Gov. Change			-0.545**			-0.594**
			(-2.194)			(-2.445)
Inflation			6.644***			4.948**
			(3.276)			(2.487)
(Absence of) Political risk			0.026			0.022
			(1.485)			(1.251)
Observations	1,230	1,236	853	1,230	1,236	853
Number of id	72	70	56	72	70	56
Country FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Note: In columns 1-3 the interest variable refers to debt restructuring, while in columns 4-6 to NPV reduction. t-statistics in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 5a: Reverse Causality, Debt Restructuring, GLS

	Dependent variable: Priv. Restructuring			Dependent variable: Off. Restructuring		
	(1)	(2)	(3)	(4)	(5)	(6)
Growthpc (t-1)	0.000	0.001	0.001	-0.005	-0.001	0.001
	(0.039)	(0.176)	(0.146)	(-0.320)	(-0.042)	(0.068)
Growthpc (t-2)		-0.002	-0.002		-0.013	-0.014
		(-0.403)	(-0.325)		(-0.753)	(-0.745)
Growthpc (t-3)			0.000			0.002
			(0.035)			(0.087)
Observations	1,821	1,789	1,753	1,821	1,789	1,753
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	69	69	69	69	69	69

Note: In columns 1-3 the dependent variable is the private restructuring, while in columns 4-6 it is official restructuring.
t statistics in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 5b: Reverse Causality, Haircut, GLS

	Dependent variable: Priv. Haircut			Dependent variable: Off. Haircut		
	(1)	(2)	(3)	(4)	(5)	(6)
Growthpc (t-1)	0.006	0.006	0.007	0.007	0.006	0.005
	(0.565)	(0.575)	(0.614)	(0.244)	(0.200)	(0.170)
Growthpc (t-2)		-0.001	-0.002		0.000	-0.005
		(-0.078)	(-0.176)		(0.010)	(-0.148)
Growthpc (t-3)			0.002			0.013
			(0.219)			(0.439)
Observations	1,821	1,789	1,753	1,821	1,789	1,753
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	69	69	69	69	69	69

Note: In columns 1-3 the dependent variable is private haircut, while in columns 4-6t is official haircut.
t statistics in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Private defaults: predictor balance, RMSPE and country weights

	Paraguay (1986)		South Africa (1985)		Uruguay (1983)		Argentina (2001)	
	Treated	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*
Average pre-treatment GDP pc	2183.23	2189.46	6767.94	6766.39	6504.10	6512.23	7899.80	7898.98
Investment	-	-	26.88	22.60	18.76	20.13	18.50	26.61964
Openness	-	-	55.86	55.65	34.99	67.47	21.44	79.39372
(delta) Population	2.74	2.74	2.32	2.32	0.46	2.54	1.18	0.6407553
(log) Population	14.99	14.97	17.12	17.09	14.87	16.92	17.39	15.57105
Secondary Education	26.60	26.58	-	-	61.52	43.66	79.13	87.40235
Government Cons.	-	-	-	-	-	-	13.01	18.82909
(delta) Terms of trade	-	-	-	-	-	-	1.23e+08	-3.62e+09
Banking Crisis	-	-	-	-	-	-	0.29	0
Civil Liberties	-	-	-	-	-	-	2.86	3.437
RMSPE	180.25		281.72		174.31		329.63	
Control group	Bangladesh 0.028		Bangladesh 0.001		China 0.098		Hungary 0.521	
	Botswana 0.25		China 0.125		Colombia 0.57		Lebanon 0.479	
	China 0.016		Colombia 0.417		Hong Kong 0.243			
	Colombia 0.021		Hong Kong 0.001		Saudi Arabia 0.089			
	Hong Kong 0.02		India 0.001					
	India 0.02		Iran Islamic Rep. 0.194					
	Iran Islamic Rep. 0.019		Malaysia 0.001					
	Lesotho 0.178		Nepal 0.001					
	Malaysia 0.022		Papua New Guinea 0.001					
	Mauritius 0.13		Puerto Rico 0.22					
	Myanmar 0.03		Saudi Arabia 0.034					
	Nepal 0.035		Thailand 0.001					
	Papua New Guinea 0.059		Zimbabwe 0.001					
	Saudi Arabia 0.011							
	Swaziland 0.05							
	Thailand 0.024							
	Tunisia 0.033							
	United Arab Emirates 0.002							
	Zimbabwe 0.051							

Table 7: Private defaults: country-specific and average effects of debt restructurings *n* years after the default

	t_1	t_2	t_3	t_4	t_5	t_6	t_7	t_8	t_9	t_{10}
Paraguay	-1.08	-4.94	-5.70	-9.16	-11.73	-14.60	-13.89	-13.36	-11.94	-15.28
South Africa	-3.94	-6.70	-7.61	-9.61	-14.46	-18.43	-24.22	-26.05	-27.12	-28.44
Uruguay	-15.71	-13.20	-11.26	-9.71	-13.62	-13.66	-17.16	-17.85	-15.50	-15.78
Argentina	-25.04	-20.94	-17.99	-14.17	-10.55	-6.36	-6.77	-12.07	-5.94	-1.04

Table 8: Private defaults: country-specific and average effects of debt restructurings *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}
Paraguay	-13.36	-11.94	-15.28	-16.62	-17.45	-22.56	-28.23	-30.21	-32.98	-33.42
South Africa	-27.12	-28.44	-28.27	-29.38	-33.02	-32.80	-33.08	-36.48	-36.12	-36.57
Uruguay	-15.50	-15.78	-12.92	-15.61	-11.72	-7.62	-0.20	-1.49	-8.33	-11.82
Argentina	-10.55	-6.36	-6.768	-12.07	-5.94	-1.04	-0.70	1.167	-3.84	-

Table 9: Official defaults: predictor balance, RMSPE and country weights

	Benin (1989)		**Burkina Faso (1991)**		**Chad (1989)**		**Egypt (1987)**		**El Salvador (1990)**	
	Treated	Synthetic	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*
Average pre-treatment GDP pc	613.25	612.48	339.36	340.70	473.69	469.47	1277.97	1277.55	2151.20	2153.14
Investment	15.17	17.51	17.72	17.75	4.27	17.02	26.48	29.75	12.76	17.36
Openness	52.30	37.76	38.20	28.05	42.71	25.30	63.04	63.01	50.97	62.83
(delta) Population	2.85	2.75	2.56	2.406	2.53	2.53	2.51	1.96	1.42	2.66
(log) Population	15.23	16.31	15.87	17.17	15.41	17.50	17.62	18.32	15.40	15.50
Secondary Education	16.31	26.01	4.46	25.35	5.95	21.86	49.24	40.96	35.19	32.31
Government Cons.	-	-	-	-	-	-	-	-	-	-
(delta) Terms of trade	-	-	-	-	-	-	-	-	-	-
Banking Crisis	-	-	-	-	-	-	-	-	-	-
Civil Liberties	-	-	-	-	-	-	-	-	-	-
RMSPE	14.72		13.47		27.47		22.36		103.61	
Control group	Bangladesh 0.003		Bangladesh 0.301		Bangladesh 0.543		China 0.591		Colombia 0.071	
	Nepal 0.638		Lesotho 0.001		Nepal 0.447		Malaysia 0.193		Namibia 0.437	
	Papua N. Guinea 0.043		Nepal 0.699		Oman 0.01		Mauritius 0.014		Nepal 0.492	
	Zimbabwe 0.315						Swaziland 0.202			

Table 9 (cont'd): Official defaults: predictor balance, RMSPE and country weights

	Georgia (2001)		Ghana (1996)		Indonesia (1995)		Kyrgyz Republic (2002)		Mali (1988)		Sri Lanka (2005)	
	Treated	Synthetic	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*	*Treated*	*Synthetic*
Average pre-treatment GDP pc	1395.41	1388.77	831.10	832.95	1551.87	1548.34	642.30	643.05	440.63	441.16	1752.42	1746.70
Investment	15.96	15.96	15.45	14.74	26.68	24.75	16.07	16.18	16.86	18.52	23.90	23.82
Openness	79.10	101.67	47.32	43.74	49.13	49.80	83.22	107.29	46.99	40.36	79.86	65.93
(delta) Population	-0.83	0.76	2.76	2.70	1.89	2.07	1.02	1.03	1.85	2.33	0.71	1.57
(log) Population	15.36	15.34	16.51	16.53	18.99	17.72	15.36	15.62	15.82	16.33	16.74	16.63
Secondary Education	81.68	81.65	36.91	26.77	45.29	29.05	87.50	82.77	8.19	21.76	76.57	57.06
Government Cons.	-	-	-	-	9.80	9.13	-	-	-	-	10.95	11.62
(delta) Terms of trade	-	-	-	-	-3.6e+12	-5.7e+08	-	-	-	-	1.52e+10	8.22e+09
Banking Crisis	-	-	-	-	0	0.02	-	-	-	-	0	0.0163
Civil Liberties	-	-	-	-	5.5	4.12	-	-	-	-	4	4.33
RMSPE	349.88		13.38		13.28		30.27		22.39		32.14	
Control group	Armenia 0.299		Bangladesh 0.403		Bangladesh 0.42		Armenia 0.184		Nepal 0.843		Azerbaijan 0.163	
	Lao PDR 0.1		Lao PDR 0.365		Colombia 0.012		Bangladesh 0.001		Papua N. Guinea 0.157		Hungary 0.029	
	Oman 0.04		Namibia 0.058		Malaysia 0.032		Lao PDR 0.048				India 0.039	
	Tajikistan 0.561		Swaziland 0.059		Thailand 0.381		Tajikistan 0.659				Lebanon 0.051	
			Zimbabwe 0.116		Tunisia 0.154		Uzbekistan 0.108				Nepal 0.525	
											Tunisia 0.192	

Table 10: Official defaults: country-specific and average effects of debt restructurings *n* years after the default

	t_1	t_2	t_3	t_4	t_5	t_6	t_7	t_8	t_9	t_{10}
Benin	-8.19	-10.67	-6.48	-5.66	-12.50	-9.33	-13.60	-12.23	-11.90	-9.62
Burkina Faso	-8.89	-9.60	-14.46	-13.35	-8.88	-8.17	-5.66	-3.92	-8.47	-7.99
Chad	-1.73	1.29	3.19	-17.24	-13.83	-17.40	-20.23	-21.21	-20.44	-25.07
Egypt	-9.77	-12.73	-16.73	-21.31	-24.23	-28.44	-31.32	-33.89	-35.92	-36.66
El Salvador	5.47	8.16	18.51	23.95	28.80	29.20	31.73	35.37	39.26	38.83
Georgia	-0.86	7.29	9.66	14.37	17.39	23.40	20.11	23.41	31.03	42.57
Ghana	-2.13	-1.25	-1.05	-1.14	-1.46	-0.94	0.56	-0.84	-0.71	-1.69
Indonesia	2.19	3.52	7.89	-2.41	-6.13	-6.26	-6.06	-6.39	-7.84	-9.10
Kyrgyz Republic	-16.10	-18.58	-26.26	-31.06	-32.65	-32.20	-27.01	-30.68	-30.83	-35.94
Mali	6.90	2.97	7.40	-3.29	-8.77	-11.84	-11.79	-11.32	-8.66	-2.60
Sri Lanka	-3.72	-6.99	-7.20	-7.88	-4.23	4.30	12.18	13.00	15.75	-

Table 11: Official defaults: country-specific and average effects of debt restructurings *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}
Benin	12.47	12.31	13.91	17.37	24.26	19.01	11.80	6.37	2.98	4.67
Burkina Faso	-4.49	-6.28	-4.16	-4.31	-6.67	-8.78	-12.09	-11.02	-10.88	-11.59
Chad	-24.59	-17.21	4.24	14.64	7.28	2.99	-2.31	-4.60	2.01	-2.25
Egypt	-24.23	-28.44	-31.32	-33.89	-35.92	-36.66	-33.75	-33.63	-34.96	-35.24
El Salvador	5.48	8.16	18.51	23.95	28.80	29.20	31.73	35.37	39.26	38.83
Georgia	14.37	17.39	23.40	20.11	23.41	31.03	42.57	47.76	53.23	60.69
Ghana	-0.71	-1.69	-4.08	-0.17	-0.59	0.06	6.58	8.64	8.82	6.11
Indonesia	-8.60	-9.11	-6.95	-3.78	-4.58	-0.67	-1.54	0.02	2.14	-
Kyrgyz Republic	-31.06	-32.65	-32.20	-27.01	-30.68	-30.83	-35.94	-33.18	-34.55	-
Mali	7.45	7.95	7.74	4.69	1.28	-0.90	-2.85	-6.56	-13.74	-16.82
Sri Lanka	-3.72	-6.99	-7.20	-7.88	-4.23	4.30	12.18	13.00	15.75	-

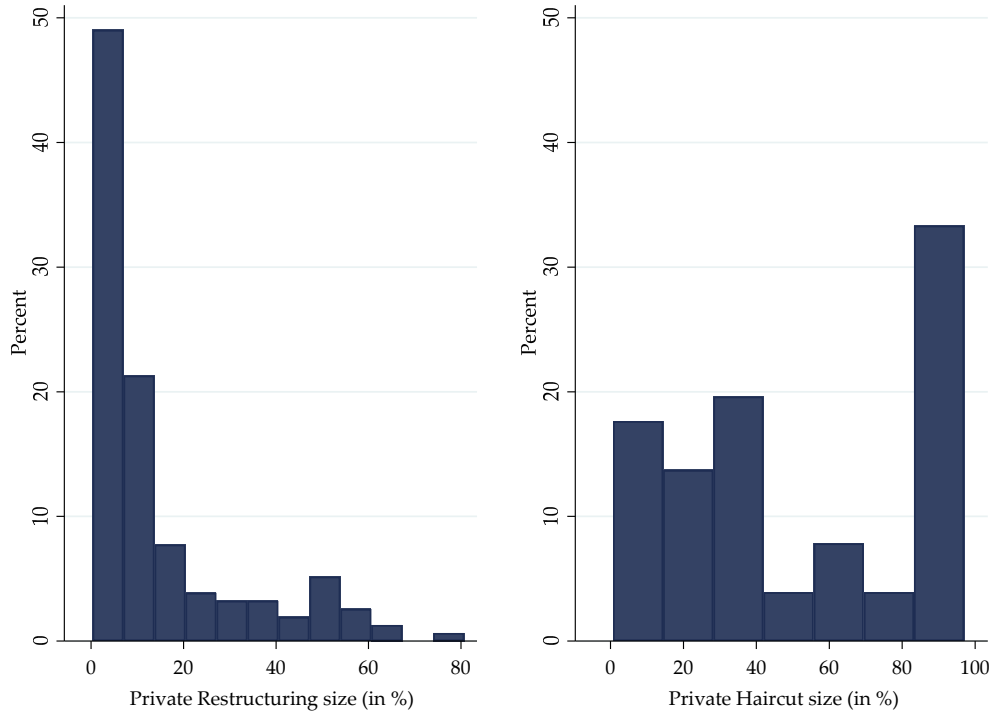


Figure 1: Frequency by size (percent) of both Private Restructurings and Haircuts

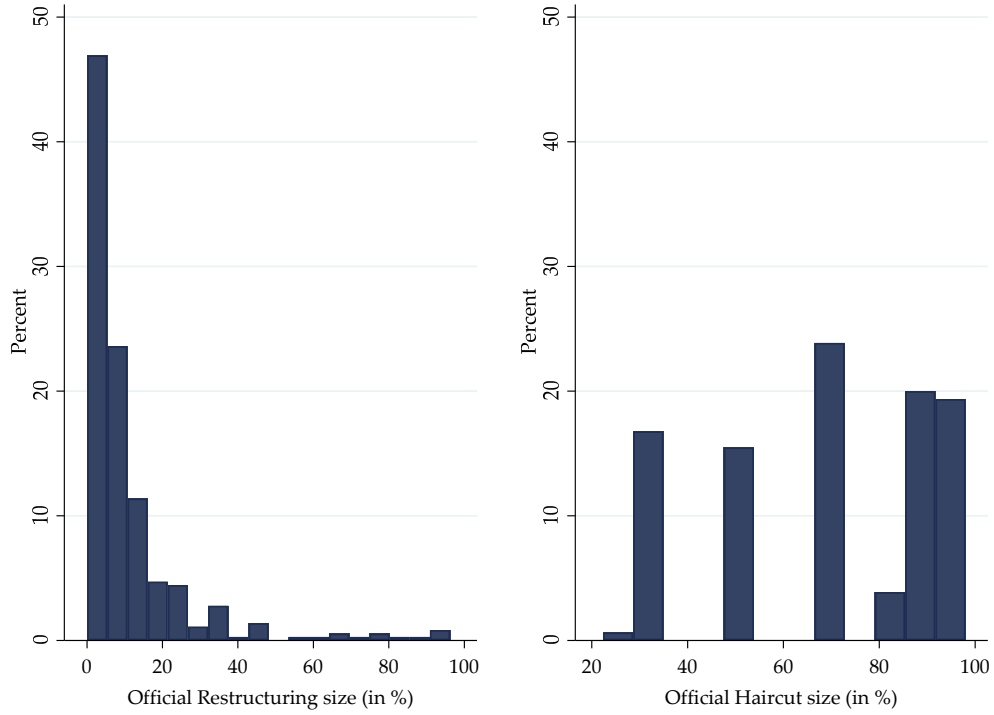


Figure 2: Frequency by size (percent) of both Official Restructurings and Haircuts

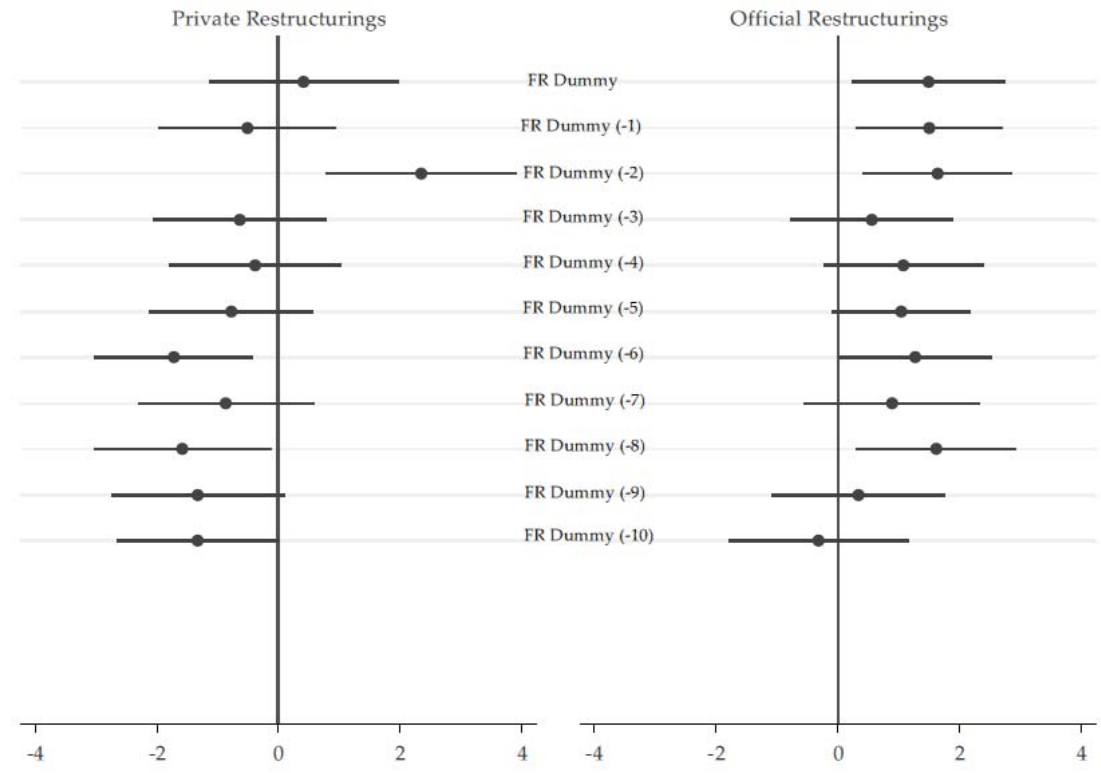


Figure 3: Restructurings and Growth, Private and Official

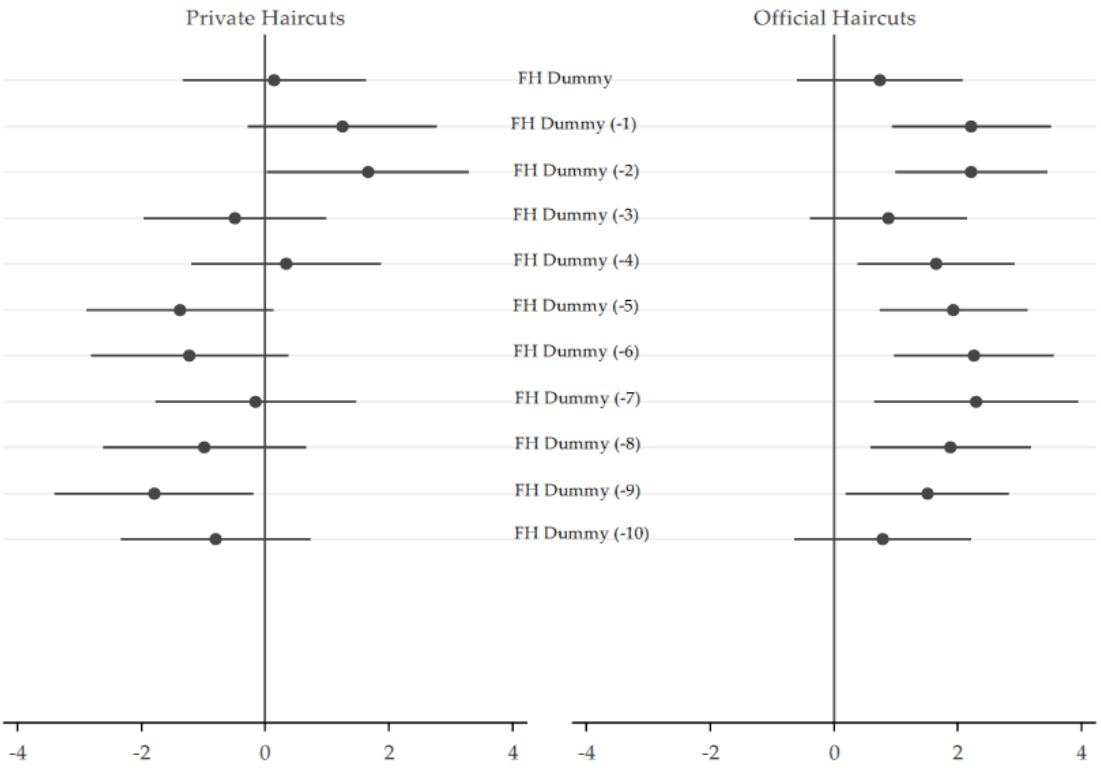


Figure 4: Haircuts and Growth, Private and Official

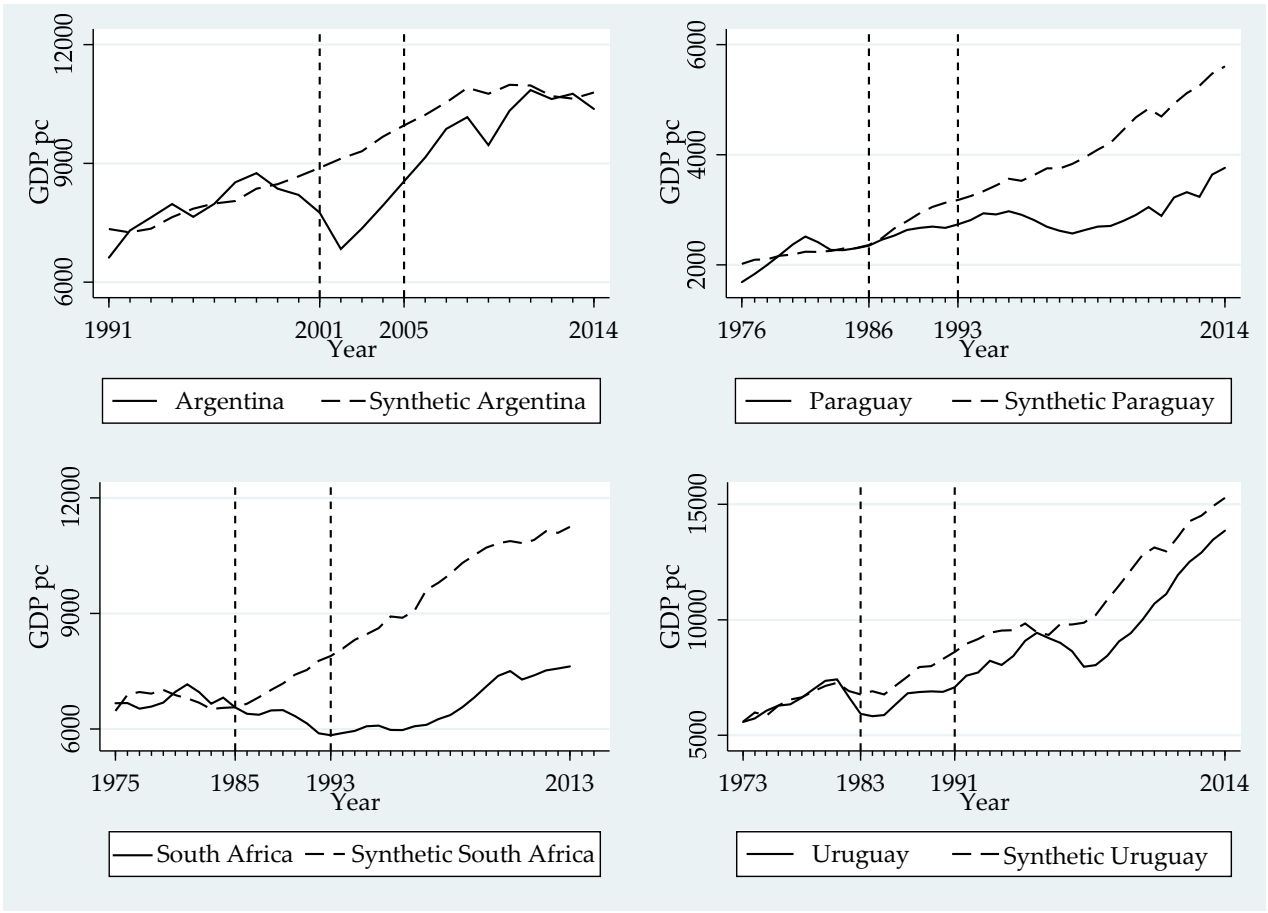


Figure 5: Private defaults: evolution of GDP pc, treated versus synthetic

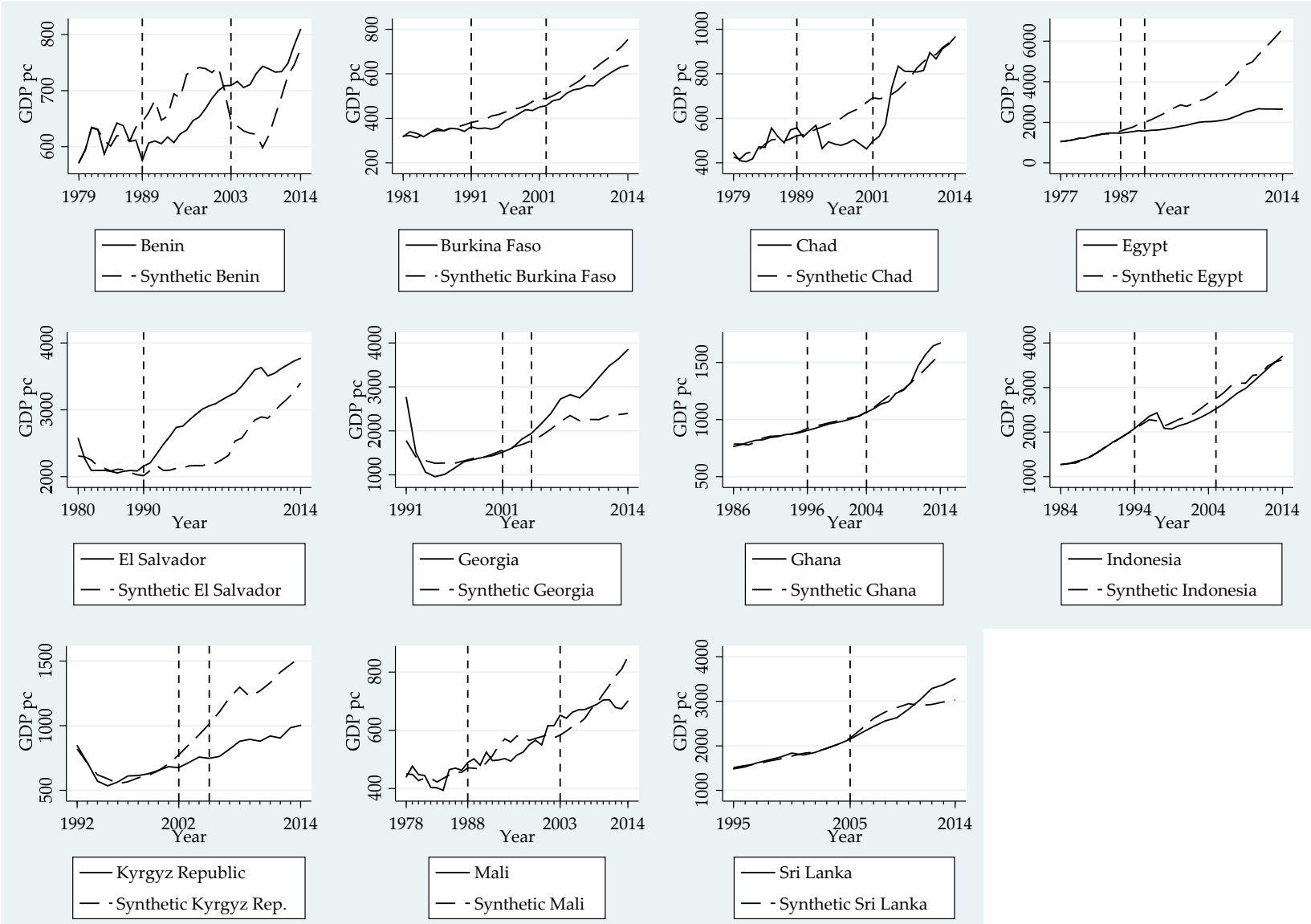


Figure 6: Official default: evolution of GDP pc, treated versus synthetic

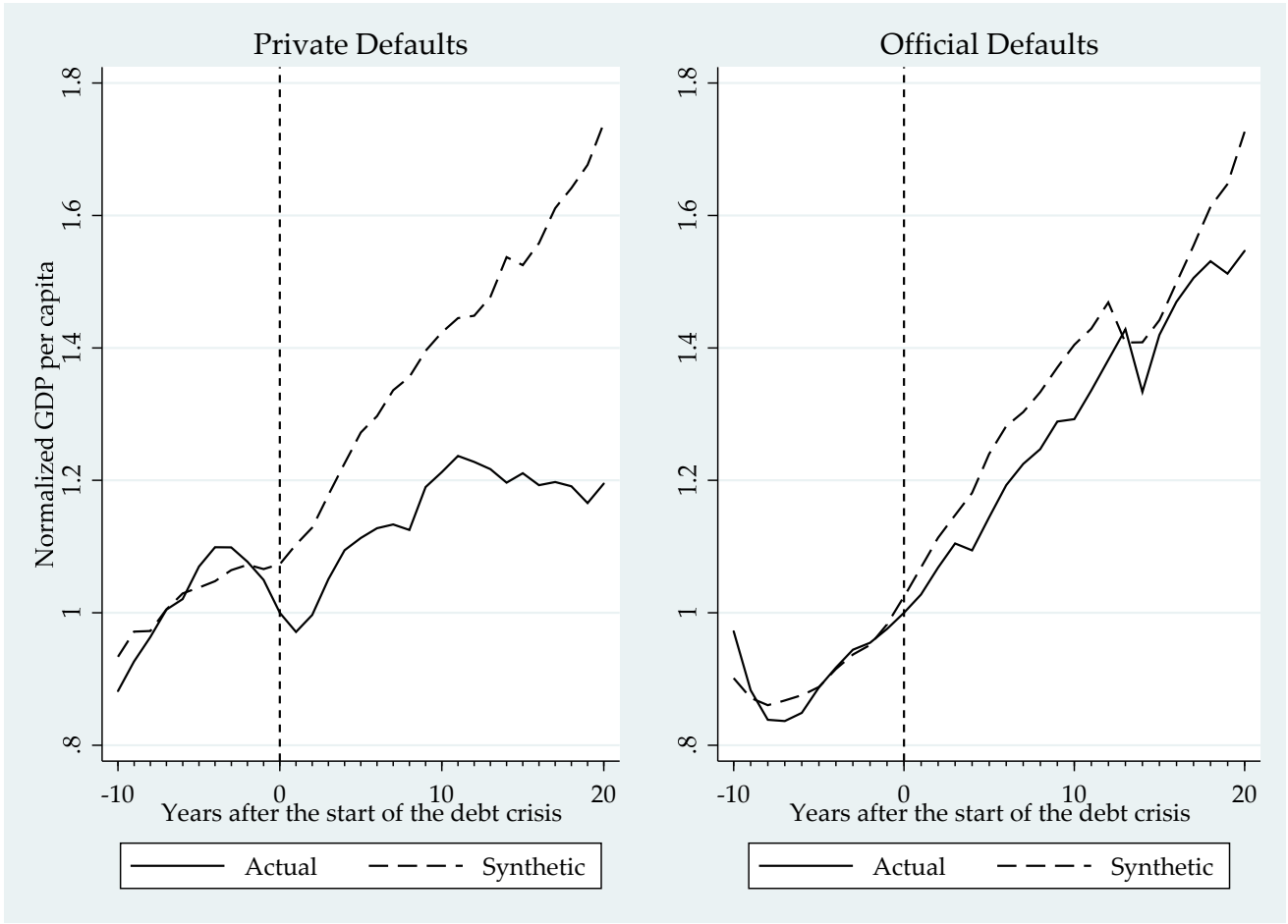


Figure 7: Average effects of private and official defaults

Online Appendices

Appendix A: Sample and variable description

Table A1a: Country sample, defaulters

	Private Restructurings			Official restructurings	
Albania	1991-1995			1993-2000	
Algeria	1991-1996			1994-1995	
Angola				1989	
Argentina	1982-1993	2001-2005		1985-1992	2014
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	
Congo, Dem. Rep.	1975-1989			1976-1989	2002-2010
Congo, Rep.	1983-1988	2007		1986-2010	
Costa Rica	1981-1990			1983-1993	
Cote d'Ivoire	1983-1998	2000-2012		1984-1994	1998-2012
Cuba	1983-1985			1985-1986	
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	
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	
Guinea	1985-1998			1986-2001	2008-2012
Guinea-Bissau				1987-2001	2010-2011
Haiti				1995	2006-2009
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	
Morocco	1983-1990			1983-1992	

Mozambique	1983-1991	2007	1984-2001	
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	
Senegal	1981-1985	1990-1996	1981-2004	
Sierra Leone	1980-1995		1977-2007	
South Africa	1985-1993			
Sri Lanka			2005	
Sudan	1975-1985		1979-1984	
Tanzania	1981-2004		1986-2002	
Togo	1987-1997		1979-1995	2008-2010
Turkey	1976-1982		1978-1980	
Uganda	1979-1993		1981-2000	
Ukraine	1998-2000		2001	
Uruguay	1983-1991	2003		
Venezuela, RB	1982-1990			
Vietnam	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 haircuts, while countries in italics are those with only official restructuring.

Table A1b: Country sample, not defaulters

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

Table A2: Variable definitions and sources

Variable	Definition	Source
DEPENDENT VARIABLE		
GDP growth	Per capita GDP (constant 2005 US$), Annual rate of change (percent)	WDI (2015)
GDP per capita	GDP per capita (constant 2010 US$)	WDI (2017)
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)
Private Restructuring	Private debt restructurings, percent of total external debt	Cruces and Trebesch (2013)
Private Restr. Dummy	Dummy =1 in case of a private restructuring	Authors' compilation based on Cruces and Trebesch (2013)
Official Restructuring	Official debt restructurings, percent of total external debt	Cheng, Diaz-Cassou, Erce (2016)
Official Restr. Dummy	Dummy =1 in case of an official restructuring	Authors' compilation based on 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 face value reduction of official debt	Built by the author
Private Haircut	Face value reduction of private debt (percent)	Cruces and Trebesch (2013)
Private Haircut Dummy	Dummy =1 in case of a face value reduction of private debt	Built by the author
CONTROL VARIABLES		
Investment	Gross fixed capital formation, ratio to GDP	WDI (2015)
Gov. Consumption	Gen. government final consumption expenditure, ratio to GDP	WDI (2015)
Openness	Exports plus imports of goods and services, ratio to GDP	WDI (2015)
Inflation	Consumer price index (2010 = 100), Annual rate of change	WDI (2015)
External debt to GDP	Ratio of external debt to GDP	WDI (2015)
Political Risk	ICRG Political Risk Index	ICRG (2013)
Government change	Dummy=1 in years with a change in the executive	Database of Political Institutions (2012)
(delta) Population	Rate of population growth, annual	WDI (2015)
(log) Population	Log of total population	WDI (2015)
Secondary Education	Percentage of the population that completed secondary education	WDI (2015)
(delta) Terms of Trade	Annual change in terms-of-trade (in million)	WDI (2015)
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 (2015)

Table A3: Summary statistics, last column of Table 2

Variable	N	Mean	SD	Min	Max
Growth	1238	2.12	5.26	-40.75	33.03
Private Default Duration	1238	0.15	0.36	0	1
Private Restructurings	1238	0.43	4.06	0	59.73
Final Priv. Restr. Dummy	1238	0.03	0.16	0	1
Final Priv. Restr. Dummy (-1)	1238	0.03	0.16	0	1
Final Priv. Restr. Dummy (-2)	1238	0.02	0.15	0	1
Final Priv. Restr. Dummy (-3)	1238	0.03	0.17	0	1
Final Priv. Restr. Dummy (-4)	1238	0.03	0.16	0	1
Final Priv. Restr. Dummy (-5)	1238	0.03	0.16	0	1
Final Priv. Restr. Dummy (-6)	1238	0.03	0.16	0	1
Final Priv. Restr. Dummy (-7)	1238	0.02	0.15	0	1
Final Priv. Restr. Dummy (-8)	1238	0.02	0.15	0	1
Final Priv. Restr. Dummy (-9)	1238	0.03	0.16	0	1
Final Priv. Restr. Dummy (-10)	1238	0.03	0.16	0	1
Final Private Restructuring	1238	0.54	4.77	0	80.71
Final Private Restructuring (-1)	1238	0.56	4.81	0	80.71
Final Private Restructuring (-2)	1238	0.57	5.59	0	108.91
Final Private Restructuring (-3)	1238	0.69	5.95	0	108.91
Final Private Restructuring (-4)	1238	0.61	5.63	0	108.91
Final Private Restructuring (-5)	1238	0.61	5.63	0	108.91
Final Private Restructuring (-6)	1238	0.65	5.68	0	108.91
Final Private Restructuring (-7)	1238	0.5	5.13	0	108.91
Final Private Restructuring (-8)	1238	0.54	5.3	0	108.91
Final Private Restructuring (-9)	1238	0.59	5.55	0	108.91
Final Private Restructuring (-10)	1238	0.64	5.77	0	108.91
Official Default Duration	1238	0.23	0.42	0	1
Official Restructurings	1238	0.74	3.84	0	76.93
Final Off. Restr. Dummy	1238	0.03	0.17	0	1
Final Off. Restr. Dummy (-1)	1238	0.03	0.17	0	1
Final Off. Restr. Dummy (-2)	1238	0.03	0.17	0	1
Final Off. Restr. Dummy (-3)	1238	0.03	0.16	0	1
Final Off. Restr. Dummy (-4)	1238	0.03	0.17	0	1
Final Off. Restr. Dummy (-5)	1238	0.03	0.16	0	1
Final Off. Restr. Dummy (-6)	1238	0.02	0.15	0	1
Final Off. Restr. Dummy (-7)	1238	0.02	0.15	0	1
Final Off. Restr. Dummy (-8)	1238	0.02	0.15	0	1
Final Off. Restr. Dummy (-9)	1238	0.02	0.14	0	1
Final Off. Restr. Dummy (-10)	1238	0.02	0.13	0	1
Final Official Restructuring	1238	0.65	7.48	0	169.43
Final Off. Restructuring (-1)	1238	0.98	11.99	0	326.13
Final Off. Restructuring (-2)	1238	0.48	5.3	0	146.84
Final Off. Restructuring (-3)	1238	0.52	5.75	0	146.84
Final Off. Restructuring (-4)	1238	0.58	5.93	0	146.84
Final Off. Restructuring (-5)	1238	0.51	5.71	0	146.84
Final Off. Restructuring (-6)	1238	0.35	3.63	0	82.06
Final Off. Restructuring (-7)	1238	0.28	3.07	0	82.06
Final Off. Restructuring (-8)	1238	0.32	3.5	0	82.06
Final Off. Restructuring (-9)	1238	0.3	3.45	0	82.06
Final Off. Restructuring (-10)	1238	0.26	3.38	0	82.06
Investment	1238	22.28	7.06	0.3	60.16
(delta) Population	1238	1.64	1.29	-2.39	11.18
Secondary Edu	1238	59.24	28.27	3.81	110.48
(log) Population	1238	16.58	1.29	14.15	20.91
Government Cons.	1238	13.79	4.94	3.14	43.48
Civil Liberties	1238	3.77	1.41	1	7
(delta) Terms of Trade (bn)	1238	-23.35	9313.24	1.66E+05	93085.91
Openness	1238	70.73	35.15	12.01	220.41
Banking Crises	1238	0.11	0.32	0	1

Table A4: Summary statistics, last column of Table 3

Variable	N	Mean	SD	Min	Max
Growth	1238	2.12	5.26	-40.75	33.03
Private Default Duration	1238	0.15	0.36	0	1
Private Haircut	1238	0.06	1.25	0	30
Final Priv. Haircut Dummy	1238	0.03	0.16	0	1
Final Priv. Haircut Dummy (-1)	1238	0.03	0.16	0	1
Final Priv. Haircut Dummy (-2)	1238	0.02	0.15	0	1
Final Priv. Haircut Dummy (-3)	1238	0.03	0.17	0	1
Final Priv. Haircut Dummy (-4)	1238	0.03	0.16	0	1
Final Priv. Haircut Dummy (-5)	1238	0.03	0.16	0	1
Final Priv. Haircut Dummy (-6)	1238	0.03	0.16	0	1
Final Priv. Haircut Dummy (-7)	1238	0.02	0.15	0	1
Final Priv. Haircut Dummy (-8)	1238	0.02	0.15	0	1
Final Priv. Haircut Dummy (-9)	1238	0.03	0.16	0	1
Final Priv. Haircut Dummy (-10)	1238	0.03	0.16	0	1
Final Private Haircut	1238	0.92	8.22	0	95.5
Final Private Haircut (-1)	1238	0.96	8.06	0	95.5
Final Private Haircut (-2)	1238	0.91	8.1	0	97
Final Private Haircut (-3)	1238	1	8.38	0	95.5
Final Private Haircut (-4)	1238	1	8.34	0	92.32
Final Private Haircut (-5)	1238	0.84	7.36	0	92.32
Final Private Haircut (-6)	1238	0.92	7.8	0	92.32
Final Private Haircut (-7)	1238	0.66	6.36	0	92.32
Final Private Haircut (-8)	1238	0.8	7.3	0	92.32
Final Private Haircut (-9)	1238	0.82	7.33	0	92.32
Final Private Haircut (-10)	1238	0.92	7.8	0	92.32
Official Default Duration	1238	0.23	0.42	0	1
Official Haircut	1238	2.08	11.52	0	90
Final Off. Haircut Dummy	1238	0.03	0.17	0	1
Final Off. Haircut Dummy (-1)	1238	0.03	0.17	0	1
Final Off. Haircut Dummy (-2)	1238	0.03	0.17	0	1
Final Off. Haircut Dummy (-3)	1238	0.03	0.16	0	1
Final Off. Haircut Dummy (-4)	1238	0.03	0.17	0	1
Final Off. Haircut Dummy (-5)	1238	0.03	0.16	0	1
Final Off. Haircut Dummy (-6)	1238	0.02	0.15	0	1
Final Off. Haircut Dummy (-7)	1238	0.02	0.15	0	1
Final Off. Haircut Dummy (-8)	1238	0.02	0.15	0	1
Final Off. Haircut Dummy (-9)	1238	0.02	0.14	0	1
Final Off. Haircut Dummy (-10)	1238	0.02	0.13	0	1
Final Official Haircut	1238	1.11	10.11	0	98
Final Off. Haircut (-1)	1238	1.4	11.47	0	98
Final Off. Haircut (-2)	1238	1.09	10.07	0	98
Final Off. Haircut (-3)	1238	0.91	9.2	0	98
Final Off. Haircut (-4)	1238	1.11	10.11	0	98
Final Off. Haircut (-5)	1238	0.85	8.72	0	98
Final Off. Haircut (-6)	1238	0.81	8.61	0	98
Final Off. Haircut (-7)	1238	0.99	9.66	0	98
Final Off. Haircut (-8)	1238	0.76	8.47	0	98
Final Off. Haircut (-9)	1238	0.52	6.98	0	98
Final Off. Haircut (-10)	1238	0.44	6.31	0	98
Investment	1238	22.28	7.06	0.3	60.16
(delta) Population	1238	1.64	1.29	-2.39	11.18
Secondary Edu	1238	59.24	28.27	3.81	110.48
(log) Population	1238	16.58	1.29	14.15	20.91
Government Cons.	1238	13.79	4.94	3.14	43.48
Civil Liberties	1238	3.77	1.41	1	7
(delta) Terms of Trade (bn)	1238	-23.35	9313.24	1.66E+05	93085.91
Openness	1238	70.73	35.15	12.01	220.41
Banking Crises	1238	0.11	0.32	0	1

Appendix B: Additional figures

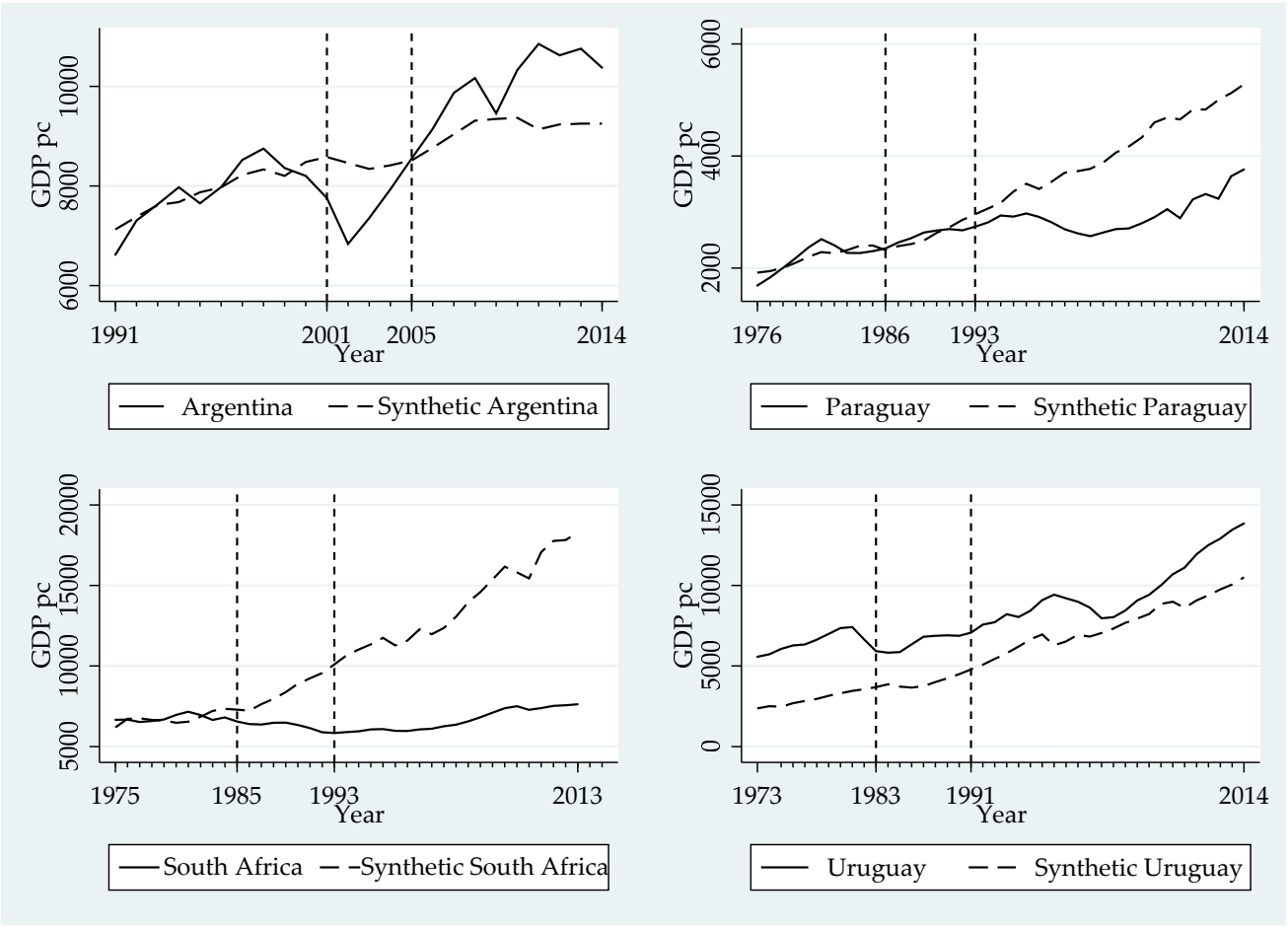


Figure B1: Private defaults: evolution of GDP pc, treated versus synthetic.
Alternative set of predictors

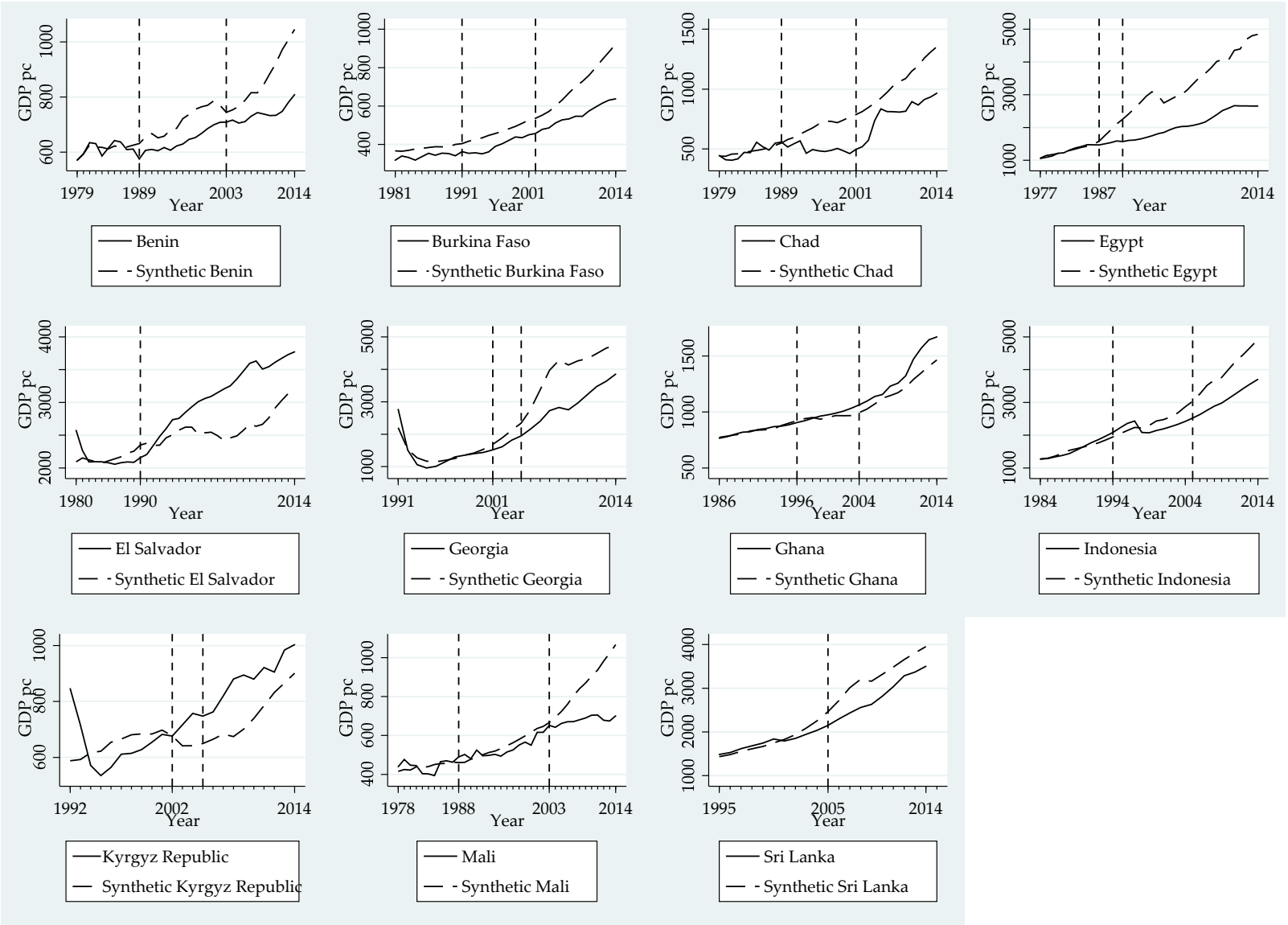


Figure B2: Official defaults: evolution of GDP pc, treated versus synthetic. Alternative set of predictors

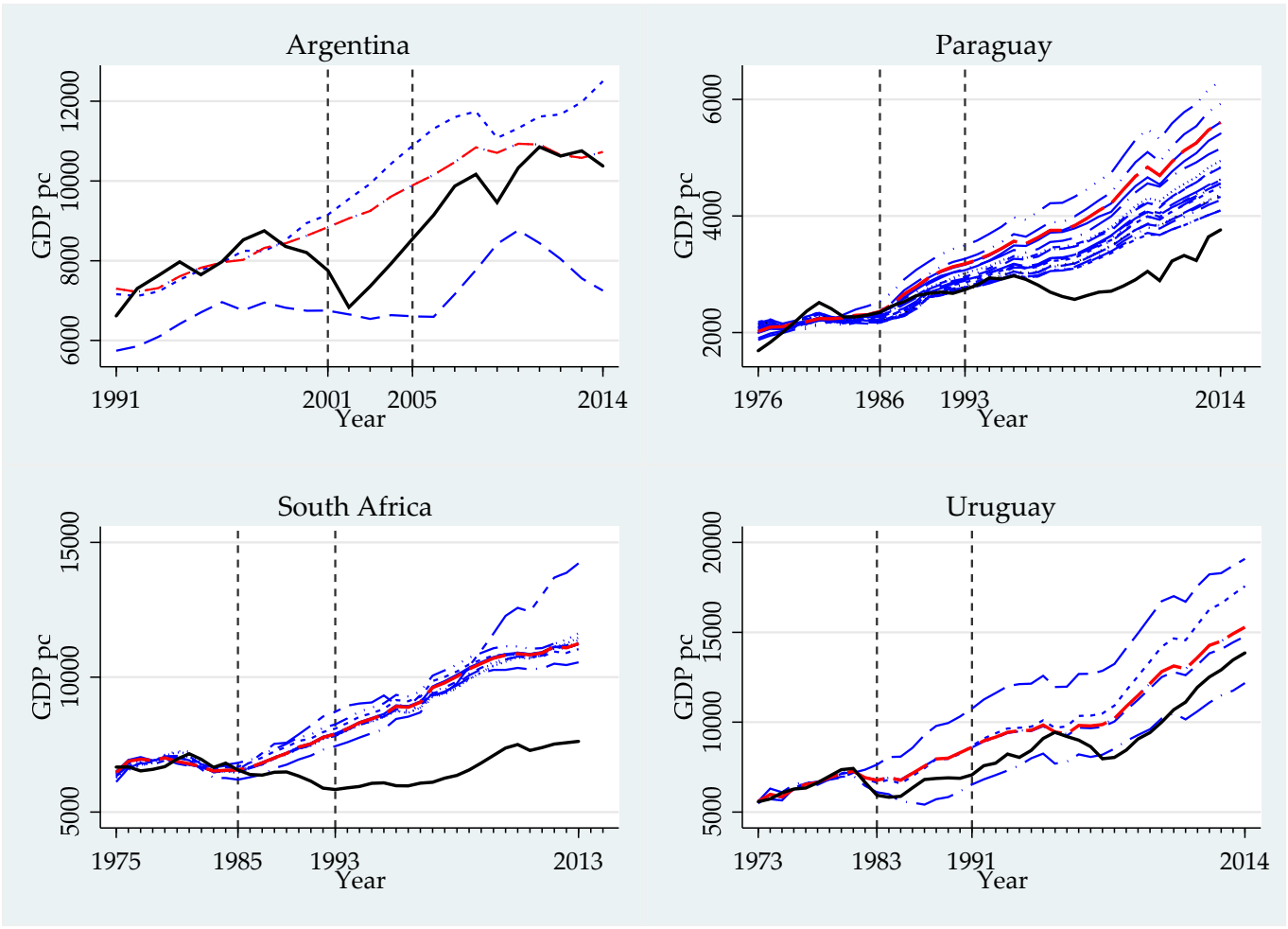


Figure B3: Private defaults: leave-one-out distribution of the synthetic control

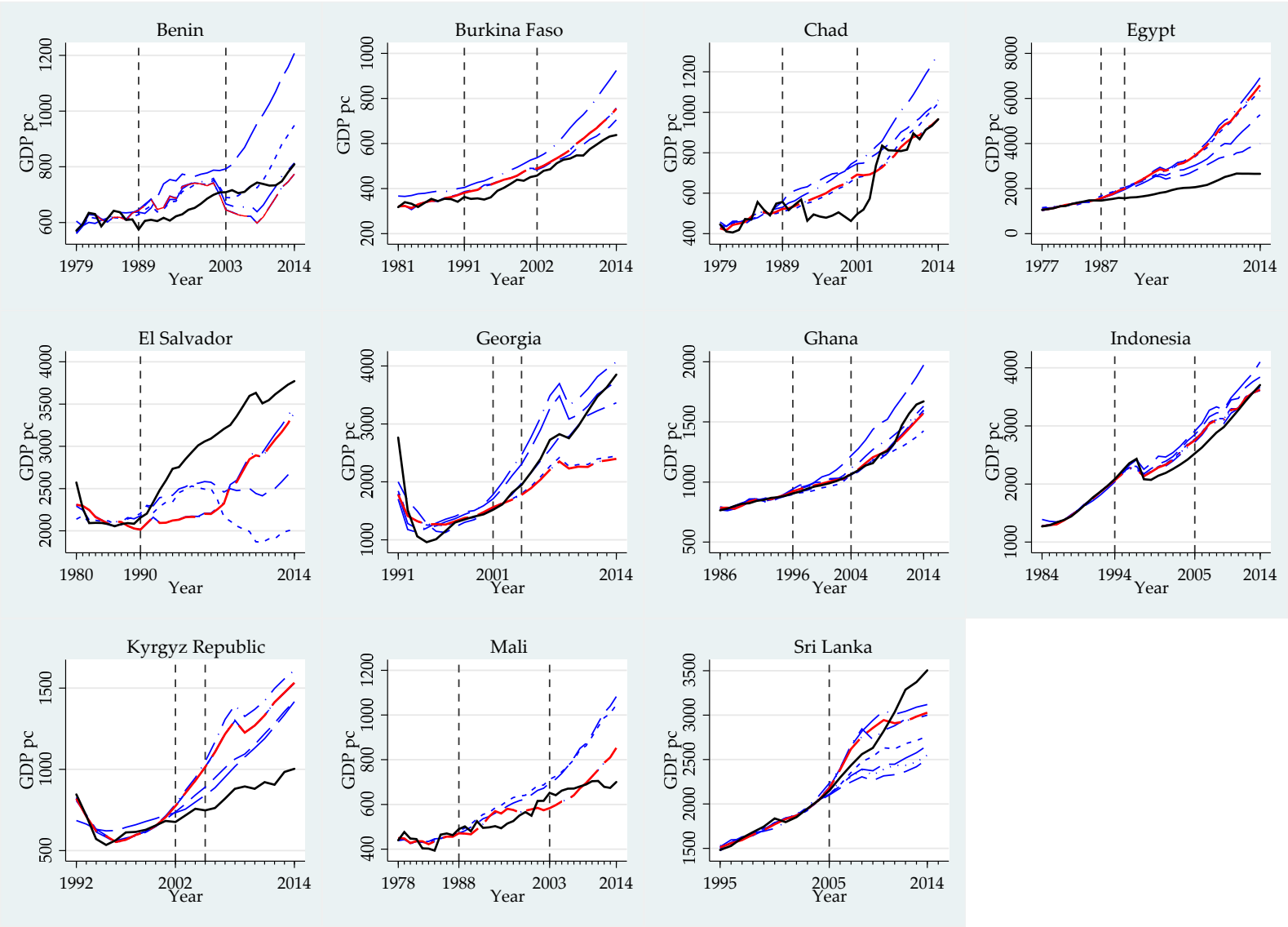
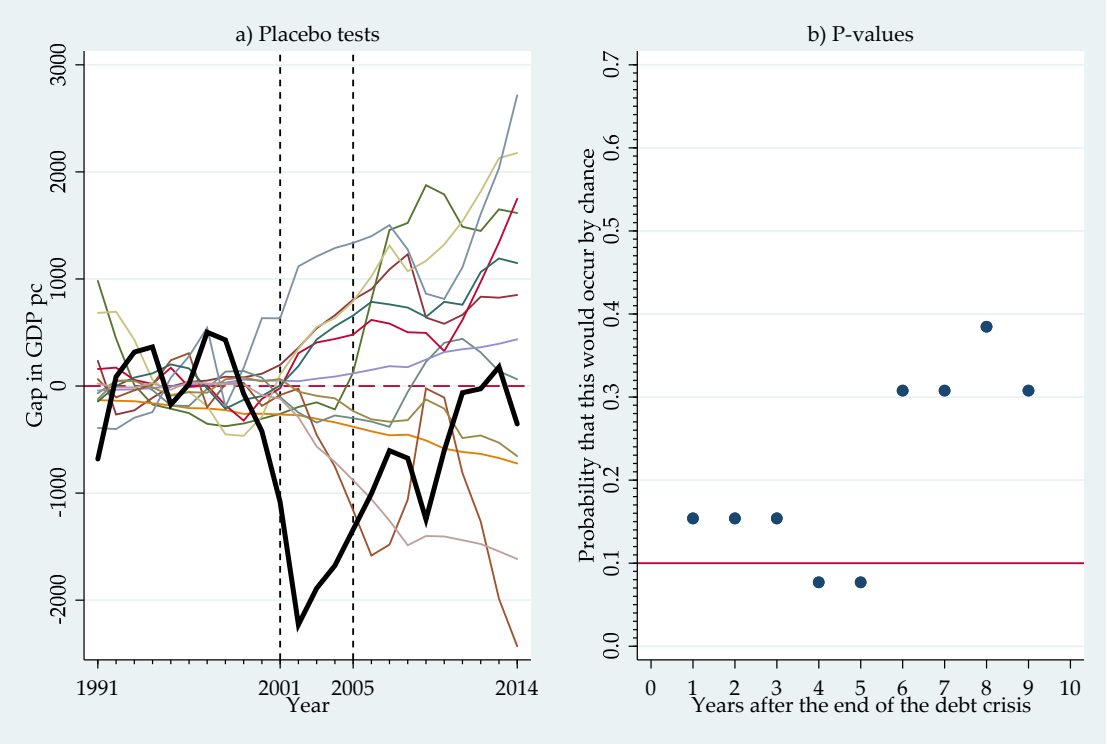
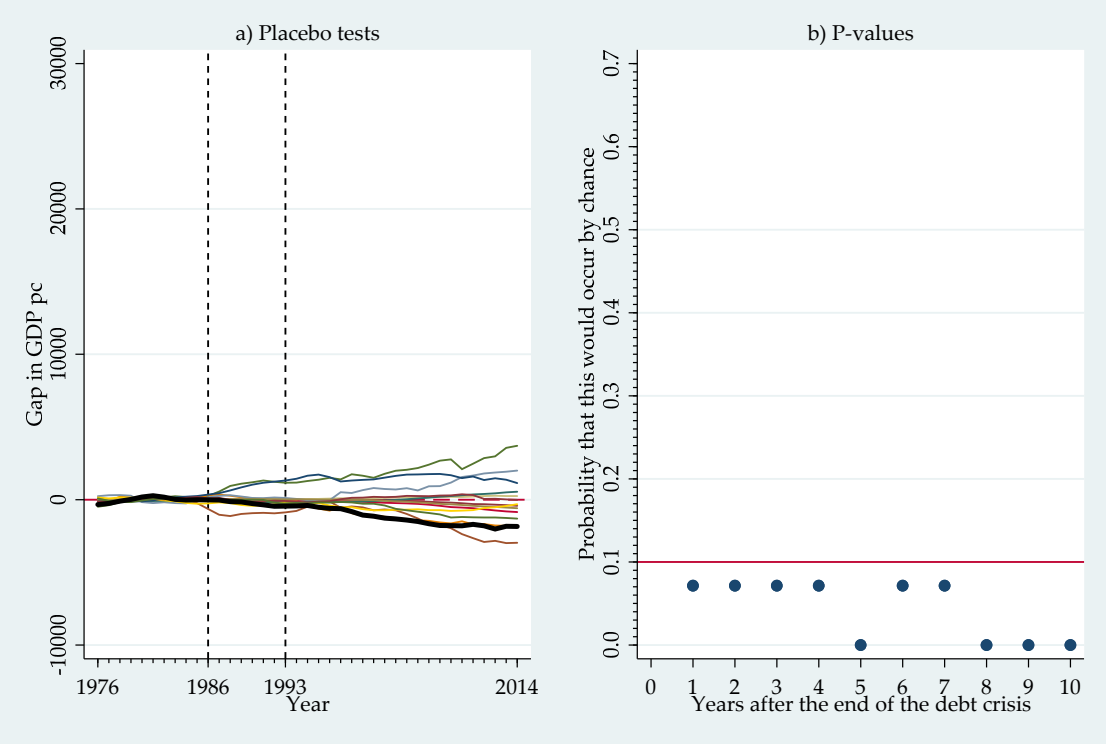


Figure B4: Official defaults: leave-one-out distribution of the synthetic control

Argentina



Paraguay



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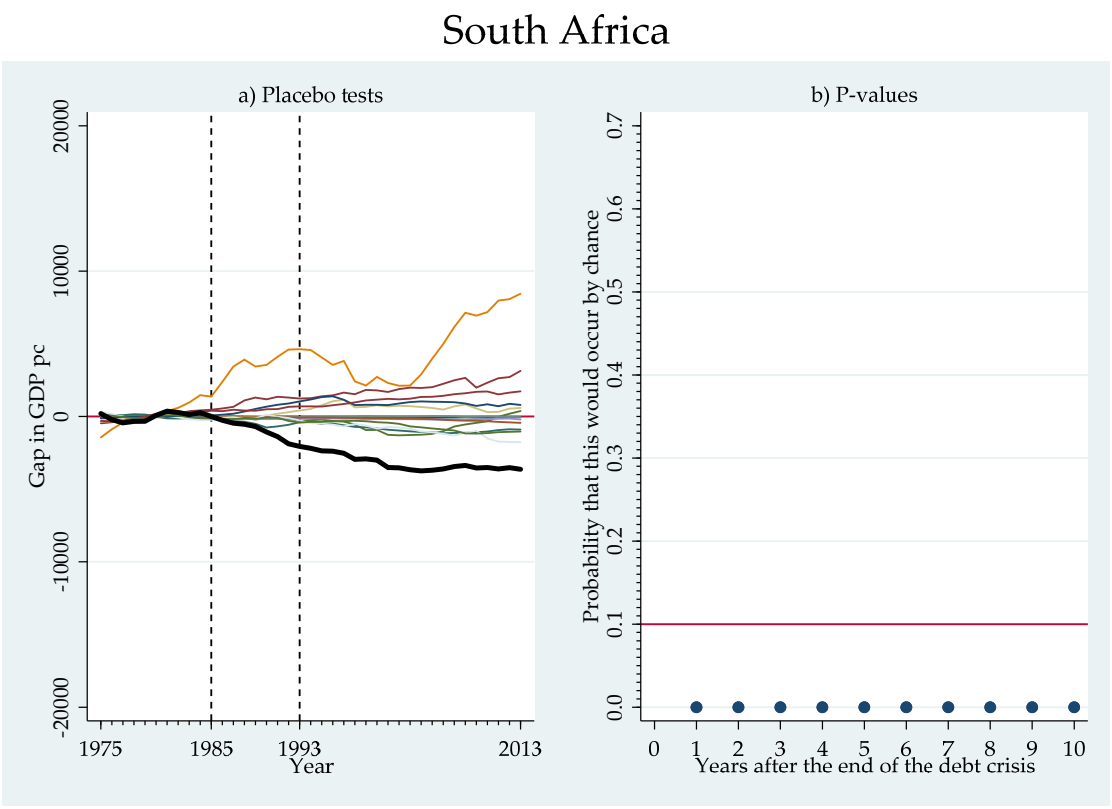


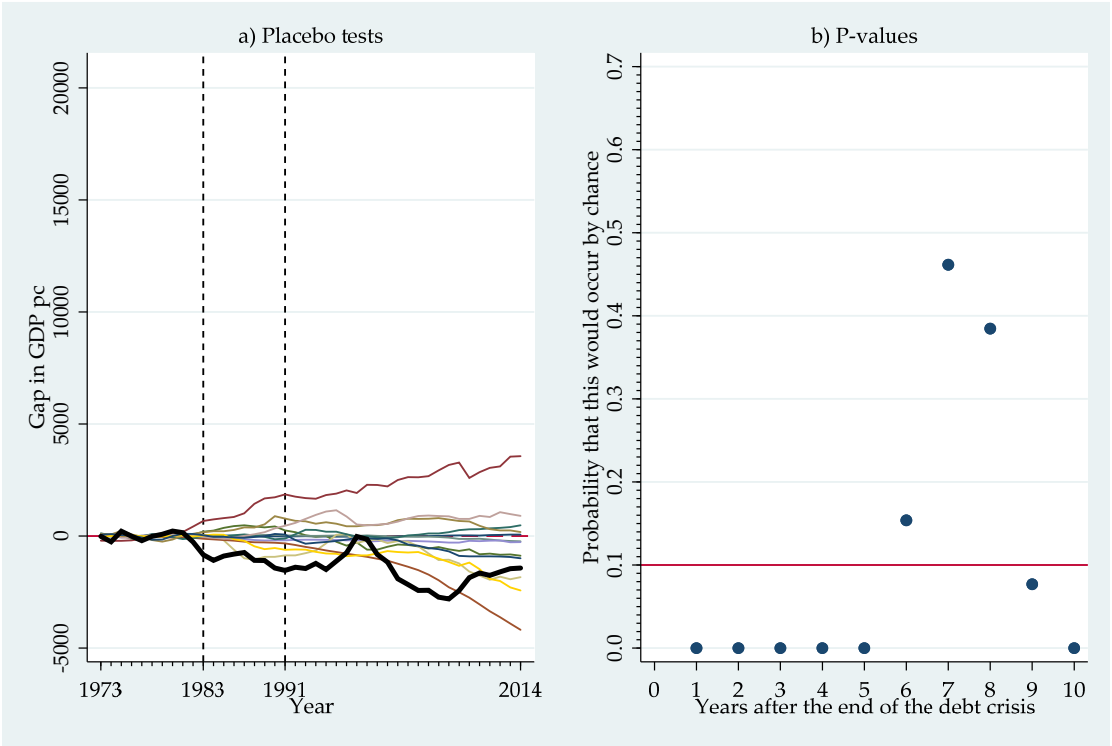
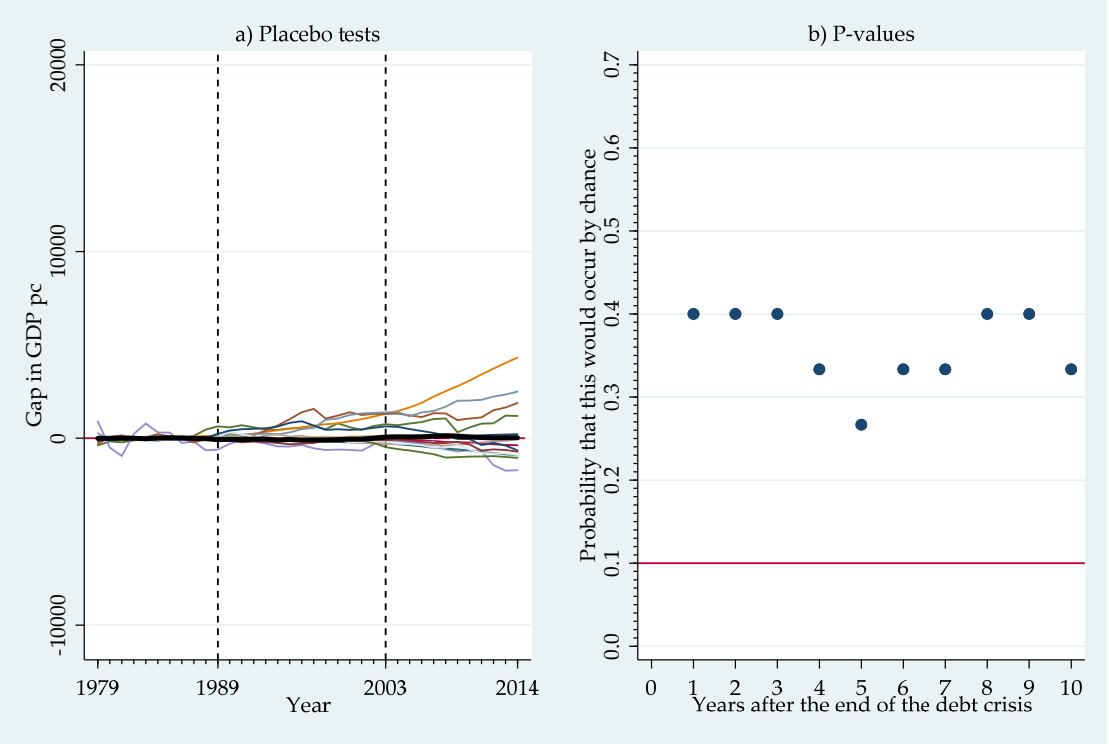
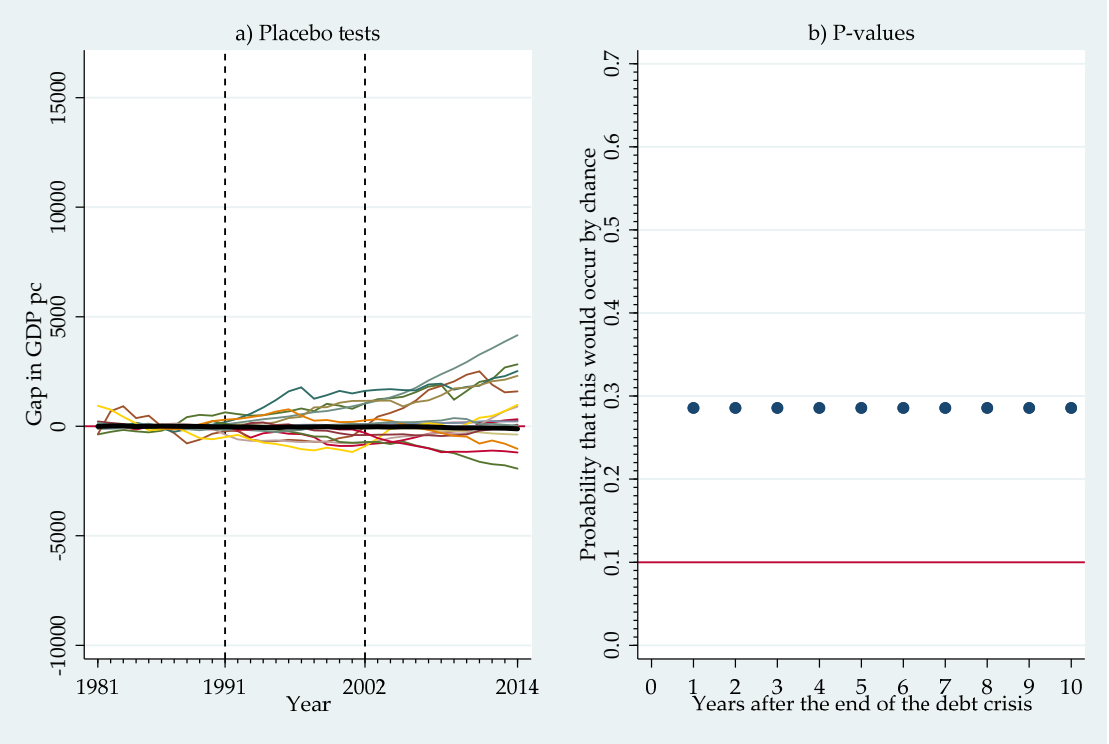


Figure B5: Private default: a) placebo tests b) p-values

Benin



Burkina Faso



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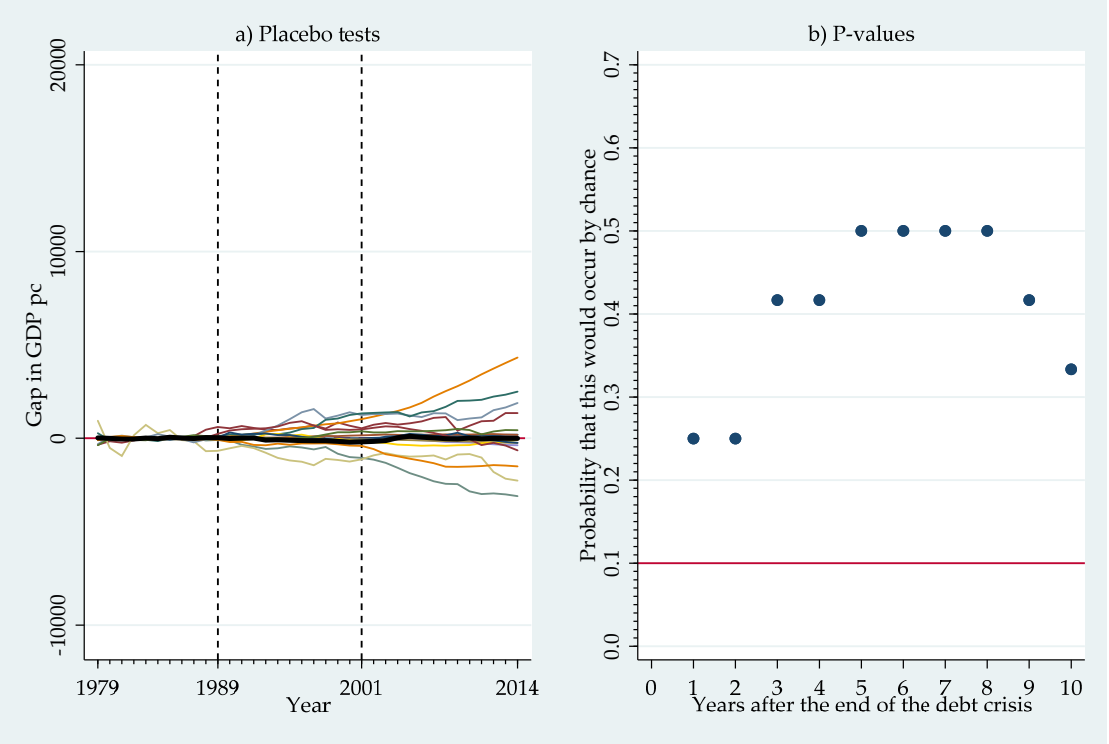
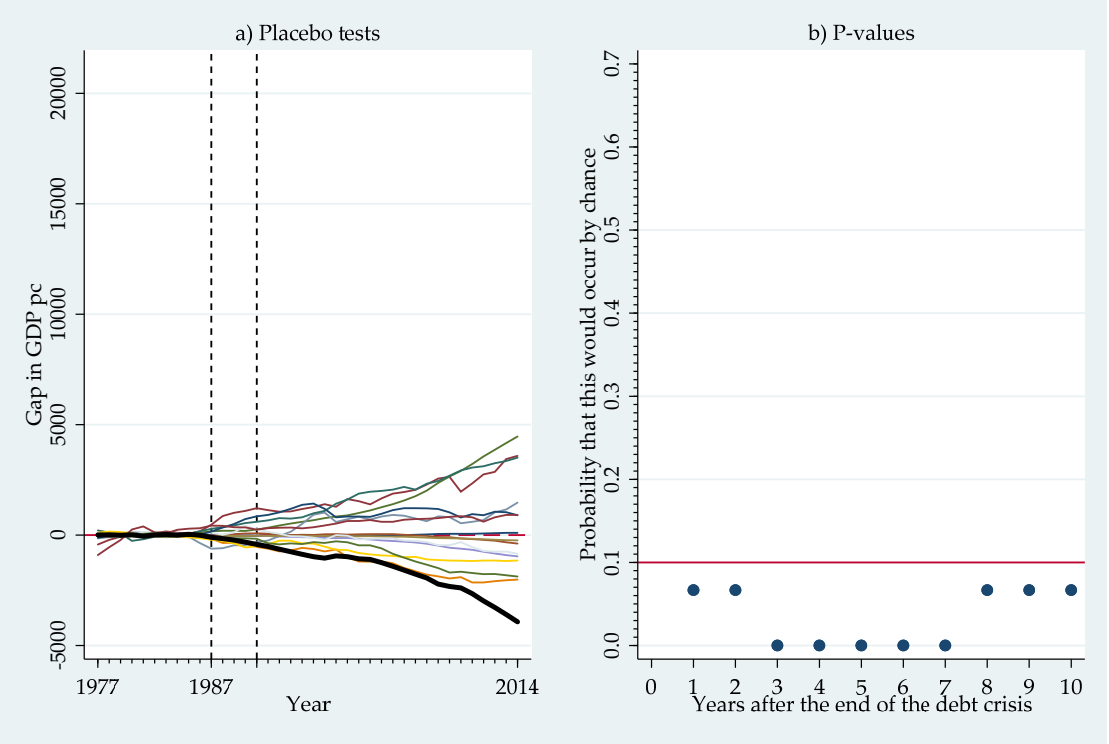
Chad



Egypt



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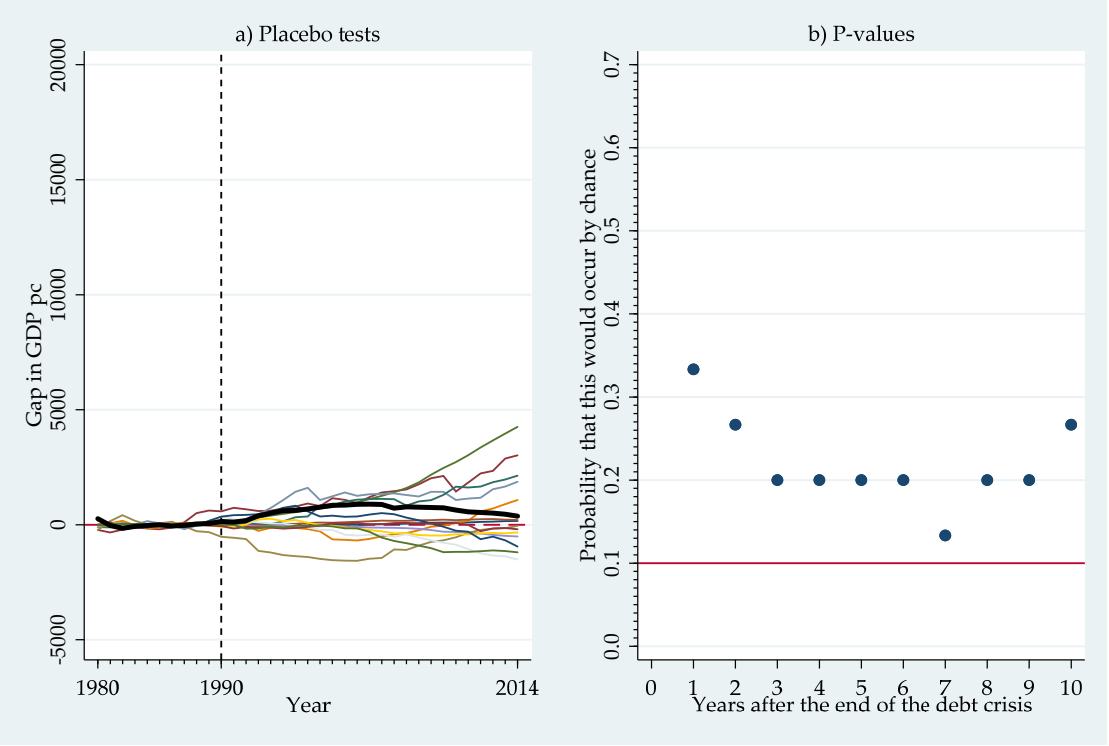
El Salvador



Georgia

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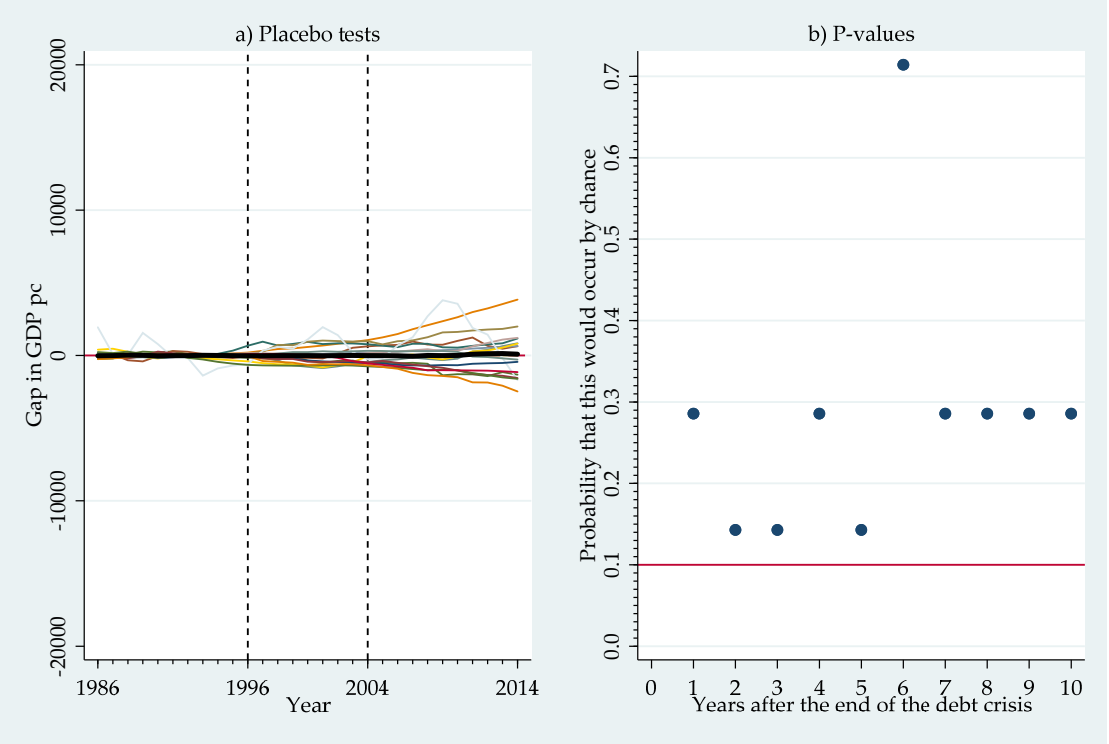
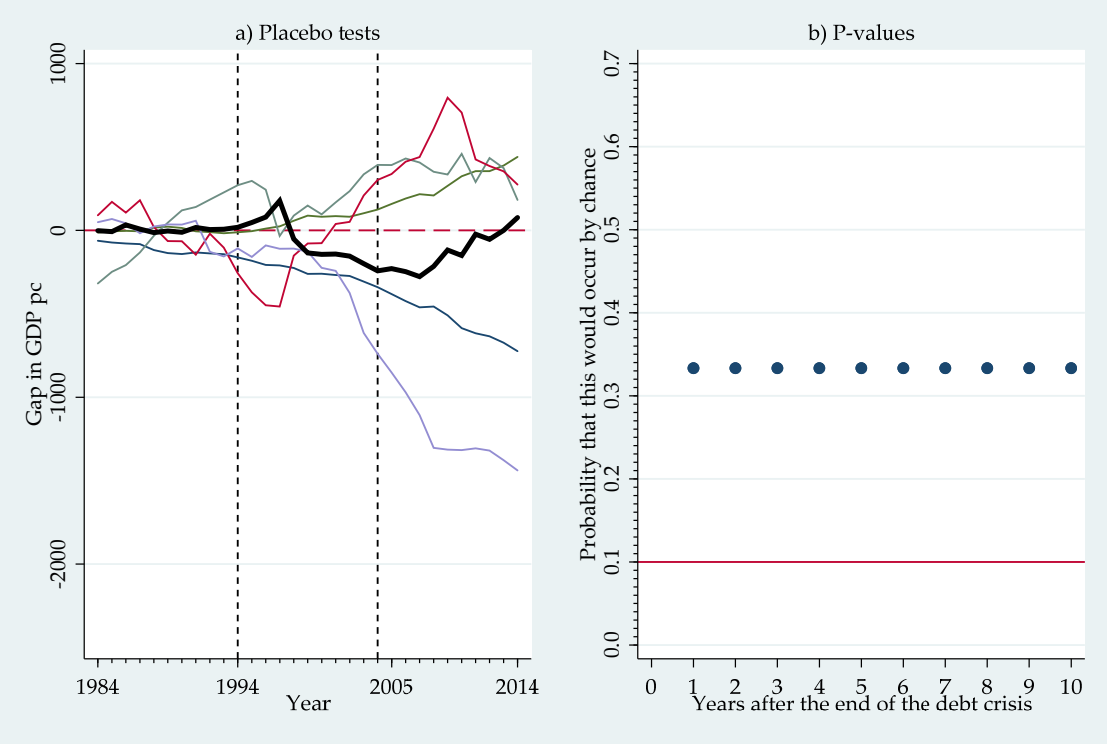
Ghana



Indonesia



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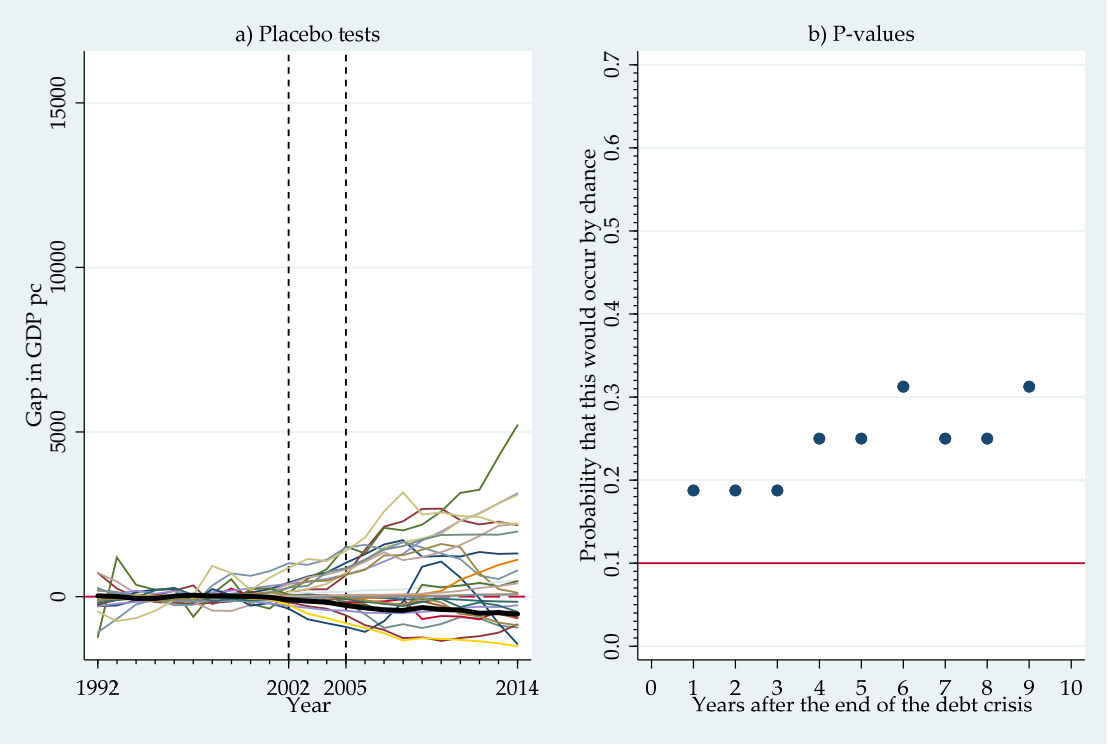
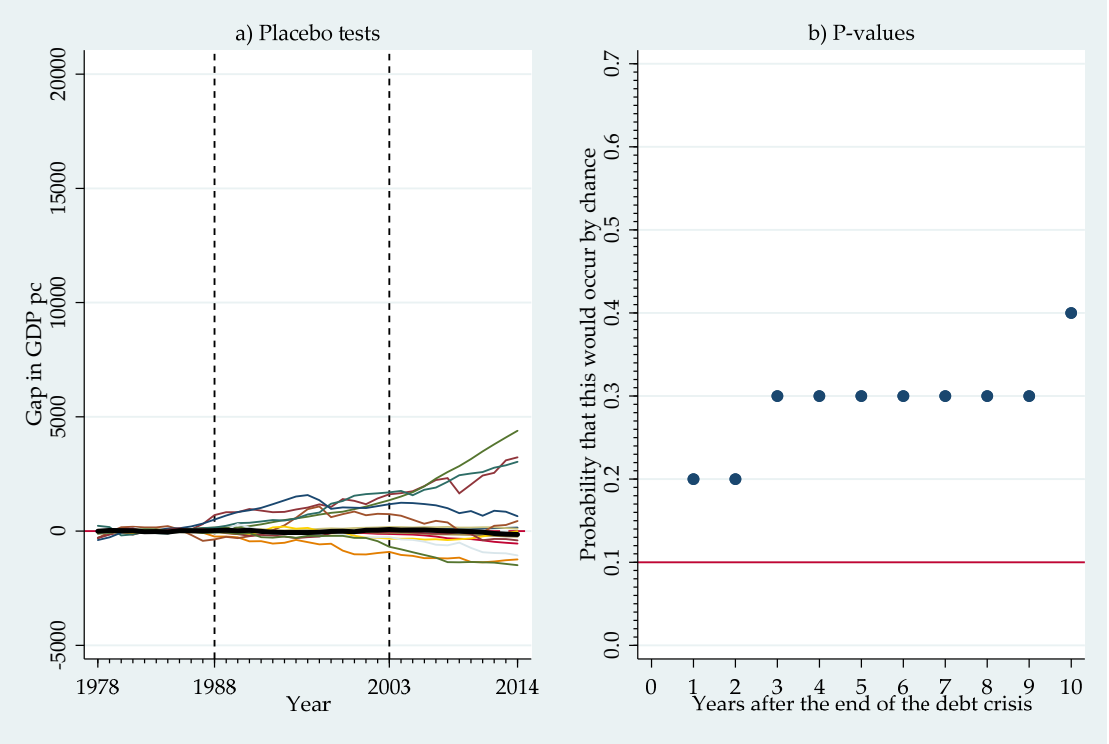
Kyrgyz Republic



Mali



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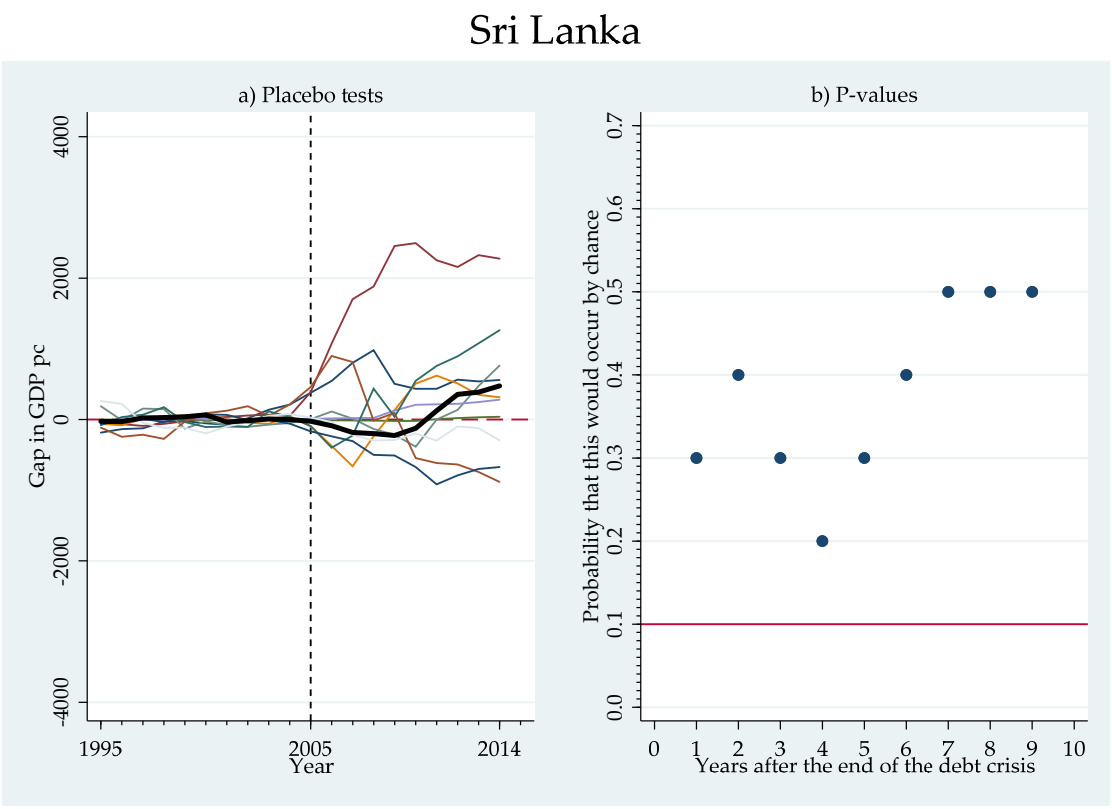

Figure B6: Official default: a) placebo tests b) significance levels

Appendix C: A formal discussion of the Synthetic Control Method

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:

$$\beta_{1t} = Y_{1t} - Y_{1t}^N \tag{C1}$$

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:

$$\widehat{\beta}_{1t} = Y_{1t} - \sum_{i=2}^{I} w_i Y_{it} \tag{C2}$$

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:

$$\sum_{k=1}^{K} v_k (X_{1k} - X_{0k}W)^2, \qquad s.t.\ w_i \geq 0 \text{ and} \sum_{i=2}^{I} w_i = 1 \tag{C3}$$

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,

$$MSPE = \frac{1}{T_0} \sum_{t<T_0} (Y_{it} - \sum_{i=2}^{I+1} w_i Y_{it})^2 \tag{C4}$$

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.