

# The Social Costs of Sovereign Default

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## Abstract

This paper estimates the costs of sovereign defaults to a broader extent than has been done in the literature. Applying the synthetic control method to a sample of 131 defaults since 1900, it finds that, on average, growth in the first two years falls 3.6 and 2.4 percentage points short of the counterfactual. Still, after a decade, defaulters' economic output per capita is nearly 17 percent below that of the counterfactual. Poverty headcounts—available since the 1980s—exceed their pre-crisis levels by roughly 30 percent

shortly after default and remain elevated a decade later. Variables proxying access to nutrition, energy, and health outcomes—available since the 1960s—suggest that standards of living decline sharply after sovereign defaults. For instance, on average, by year 10 after default, defaulters have 13 percent more infant deaths every year than the synthetic control. And surviving infants are expected to have shorter lives: life expectancy drops to 1.5 percent below the counterfactual.

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# The Social Costs of Sovereign Default

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## I. Introduction

The literature on the macroeconomic costs of default has its origin in the theoretical literature studying why sovereigns do or do not default. On the one hand, sovereigns arguably face few strictly binding financing constraints given their capacity to tax and expropriate so they should have no issues servicing the debt.<sup>1</sup> On the other hand, there are few obvious reasons to repay debt, given that the legal framework leaves creditors with a difficult task in collecting against governments. Much of the theoretical literature explored the tension between these two camps, pointing to reputational costs as the major disincentive to the default decision (e.g. Eaton and Gersovitz 1981, Dooley 2000, Amador and Phelan 2021, 2021a, Aguiar and Amador 2021).<sup>2</sup> Other influential papers cast doubt on the validity of reputational costs as the main disincentive; pointing instead to direct sanctions such as trade embargoes (e.g. Bulow and Rogoff 1989, Mitchener and Weidenmaier 2010).

Our study falls within a more recent strand of the literature that has largely bypassed this debate. It turns instead to the assessment of the economic costs associated with default and considers them, perhaps unwittingly, as disincentives to default. Studies have documented multiple negative impacts stemming from default: loss of access to corporate credit, declines in foreign investment and productivity are a few examples (Rose, 2005; Arteta and Hale, 2008; Trebesch, 2009; Mendoza and Yue, 2012; Sandleris and Wright, 2014, Arellano et al. 2018, Arellano et al 2022).<sup>3</sup> Yet, to our knowledge, no study documents the social costs – distributional impacts of output costs, incidence of poverty, and multi-dimensional aspects of poverty – of sovereign defaults. Our study seeks to fill this conspicuous gap to the greatest extent feasible due to the glaring data constraints in this field.

We consider two related questions. First, we contribute to the literature by studying *how costly sovereign defaults are* in terms of output using the synthetic controls method. Second, a more holistic evaluation of the (dis-)incentives to default begs the question: *who bears the brunt*

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<sup>1</sup> This view is for example reflected in a famous quote by Walter Wriston, at the time chairman of Citibank, who is often cited for saying “Countries don’t go bankrupt”.

<sup>2</sup> See also M.L.J. Wright (2011) for a survey of the literature studying the incentive structure of the default decision.

<sup>3</sup> See *ibid*. Note also that these findings are not without controversy, Tomz (2007) finds that direct military intervention – arguably the strongest form of “sanction” imaginable – was not as prevalent or effective as other authors suggest.

*of this cost?* There are various reasons to turn the literature’s focus in this direction. For example, the distribution of losses has obvious effects on political incentives (especially for democratically elected decision makers). Yet, the empirical literature has been largely silent around this issue. Existing works concentrate on the relationship between inequality and sovereign default, predicting that increased inequality increases the probability of default since both austerity and tax increases are politically costly (Andreasen et al., 2019; Jeon and Kabukcuoglu, 2018). Both models are motivated and parameterized by the case of Argentina, 2001.<sup>4</sup> We seek to fill that gap and inform further development in this literature by providing findings on the social cost of default, as well as stylized facts on the heterogeneous effects of default on different segments of the income distribution. In this paper, we test whether sovereign default leads to social costs, including those associated with inequality, while previous research focused on the role of inequality as a driver of default.

Our study uses a sample of 131 sovereign defaults since 1900. Data coverage naturally varies depending on the exercise due to both data availability for the variables of interest and the time-variance inherent to the cornerstone *sovereign* default variable – the number of sovereigns covered in our database in 1900 was 50, while it increases to 193 by the 2010s. The exercises on poverty and distributional impacts have the smallest sample since the World Bank’s Poverty and Inequality Platform (PIP) data that we rely on are only available since the 1980s and data is best only for the most recent decades, which saw only limited sovereign defaults. Additionally, for some exercises the PIP require further exclusions, as we explain in the relevant sections below. The analyses of proxies of multi-dimensional poverty rely on longer annual data series that often start in the 1960s. Our analysis of per capita output has the most complete data coverage starting in 1900. However, the sample changes over time owing to waves of independence along with any gaps in the per capita GDP data series from the Maddison Project. Lastly, it is important to

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<sup>4</sup> This in itself is somewhat problematic, since Argentina had been in default for more than half of the twenty years prior to the 2001 default. We provide stylized facts using a wider, though still not representative sample. Note also that some related literature exists considering the political economy of inflation inducing decisions and the policies’ differential impact: Albanesi (2007) argues that low-income households are more negatively affected by inflation. As inequality increases the bargaining power of low-income households diminishes, and the government follows policy decisions that are less likely to favor the group with diminished bargaining power, thus raising equilibrium inflation.

highlight that we drop from our sample cases enmeshed in wars (we consider both civil wars and cross-border conflicts) at the time of the default, or during the five years thereafter.<sup>5</sup>

To shed light on these questions we required a modification to apply the synthetic controls method (Abadie et al. 2003; Abadie et al. 2010) to a panel. We develop an aggregation procedure for the SCM and apply it on a broad data set, which includes many previously unexamined low-income countries. To the best of our knowledge, this is the first paper that applies this quasi-experimental design in the sovereign default literature, since most of it precedes widespread use of synthetic controls in the social sciences.<sup>6</sup> Applying the synthetic controls method comes with significant advantages over the panel data fixed effects approaches typically used in the literature:

- (1) As is shown by Abadie, Diamond & Heinmueller (2010), conditional on a good, time-consistent fit pre-treatment, the difference between treatment unit and synthetic control post-treatment represents an unbiased estimation of the treatment effect, unaffected by omitted variable bias. Meanwhile, panel regression fixed effect models' estimates are unbiased only when there are no unmeasured (or unmeasurable) time-variant variables that could affect the outcome variable. An assumption that in contrast to the SCM condition is de facto impossible to verify (Abadie, 2021; Billmeier and Nannicini, 2013).
- (2) To reduce the risk of omitted variable bias, panel fixed effect models require a large number of covariates, for which long time series do not exist. The synthetic control is not reliant on long time series for many covariates, which allows us to assess a broader and longer period of sovereign defaults going back to 1900.
- (3) The synthetic control method is especially apt to study time-varying effects and their statistical significance over time, which, to the best of our knowledge, makes us the first to systematically assess the time-varying costs of default (Costalli et al., 2016).<sup>7</sup>

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<sup>5</sup> In sum, 28 sovereign defaults are dropped from the sample, as they coincide with armed conflicts.

<sup>6</sup> The Synthetic Control Method's algorithm provides a proxy for the missing counterfactual, by constructing a combination of weighted controls from the donor pool so that the pretreatment outcomes and control variables are on average equal or at least very similar to those of the treated unit. Importantly, the selection of the control is done algorithmically and transparently, overcoming possible selection bias in the construction of the control.

<sup>7</sup> In the existing literature, the cost of default is typically estimated by regressing GDP growth on default dummies (and lags thereof) as well as a set of covariates. This binary approach renders the unbiased estimation of time-specific effects with statistical significance levels impossible, as multicollinearity lags of default and other RHS variables in the panel is unavoidable.

We apply the synthetic control method methodology on GDP per capita, poverty headcounts and a set of proxy variables for elements of multi-dimensional poverty indicators to measure if and by how much they are affected by sovereign defaults.

We find that, on average, within three years of a sovereign default, the affected economies have fallen behind by a cumulative 8 percent of GDP p.c. relative to the synthetic counterfactual. Whereas growth slows down the year prior to default, a large part of the effect is driven by GDP p.c. growth of the defaulting country falling 3.6 (2.4) percentage-points behind the counterfactual in the year of default (one year after default). On average, sovereigns recover to the pre-default GDP p.c. peak four years after default. *Recovery* defined in this manner, however, does not preclude serious scarring: Over the decade post default, the structural effect on growth leads to a deficit of around 1.5% per year. After one decade, defaulters' economic output per capita is nearly 17% or one-sixth below the output reached by the non-defaulting counterfactual. We also show that the costs are larger for longer default episodes. To render a comparison with existing findings in the literature easier, we apply our methodology to the same sample used in leading prior works (namely Borenstein and Panizza 2009). Allowing for time-variant effects leads to significantly larger and more long-lasting effects of default on GDP growth.

Still, output has been criticized as a measure of welfare because, among other things, it can ignore the distribution of income (See e.g. [Fleurbaey 2009](#); [Stiglitz, Durand & Fitoussi 2018](#); [Aitken 2020](#)). This paper offers initial elucidation on *the second question* by continuing the evaluation of the costs of default beyond GDP per capita. The first variable we examine in this subsection is thus poverty headcount ratios indexed to 100 in the year prior to default.<sup>8</sup> We find that these exceed their pre-crisis levels by 30 percent, and the counterfactual's level by a staggering 70 percent one year after the default. The gap between the defaulter and its control's indexed poverty headcount level remains wide even a decade after default.

But poverty headcounts are not without caveats, the literature on multidimensional poverty (e.g. [Alkire 2015](#), [Ravallion 2011](#)), which could be traced to A.K. Sen's 1976 seminal paper, highlights multiple issues with them, such as their inability to measure the intensity of poverty

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<sup>8</sup> Unfortunately, the best data set available on poverty, PIP data from the World Bank, has a short history and most individual observations are based on imputed values. While these data contain survey observations, these are sporadic and cover a short period. Therefore, for this part of the analysis we are limited to the use of PIP data since 1988 and to rely on many observations which are imputed from growth in that data set.

for households and their lack of granularity in assessing the ways in which they are poor.<sup>9</sup> Unfortunately, we are again limited by the availability and informational content of the existing data with respect to the outcomes we would like to measure, as the best known multi-dimensional poverty indicators are available only since 2010.<sup>10</sup> We thus perform this part of the analysis using indicators that proxy some of the components in multi-dimensional poverty indicators, for which longer time series exist.

Indicators proxying access to nutrition and electricity, as well as health outcomes also suggest that defaults lead to marked declines in standards of living. All of our outcome variables are indexed to 100 the year before default. Calorie availability consistently lags in defaulting countries with the cumulative gap relative to synthetic controls growing to 4 percentage points ten years after default. Our findings on energy supply, though not statistically significant, also suggest that defaulters lag their controls by about 10 percentage points cumulatively after four years. Infant mortality and life expectancy show significant cumulative gaps for defaulters relative to their controls. On average, by year ten, defaulters have 13% more infant deaths than the counterfactual. This result is consistent with Baird et al. (2011) who find that infant mortality is sensitive to aggregate income shocks. And those infants that survive are expected to have shorter lives: with the life expectancy dropping to 1.5 percent below the synthetic counterfactual – the equivalent of 1.2 years – below that in the synthetic control. This result is consistent with Baird et al. (2011) who find that infant mortality is sensitive to aggregate income shocks.

The last set of findings presented here take a more detailed view of how poverty and the incomes of the poor are affected by defaults – albeit with a reduced sample that should be interpreted as a broad case study. Based on the 11 cases for which we have sufficient survey-sourced data, we find that there is considerable heterogeneity in “excess poverty” outcomes. In four cases there are sizable increases in poverty over what would be implied by the growth shock. Another four cases remain close to zero excess poverty and two show negative excess poverty. As for incomes, we identify peak-to-trough windows in per capita GDP levels and look at the cumulative change in income going to different segments of the population. We find that the bottom 10 percent’s income level dropped on average by a cumulative 9 percent while the

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<sup>9</sup> It is worth noting that this literature is extensive and though there is agreement that poverty headcounts may be insufficient, there is ample debate about the right methodology to construct a multi-dimensional poverty index (Alkire 2015) or multiple indices (Ravallion 2011).

<sup>10</sup> See [OPHI GMPL](#).



top 10 percent's income level increased by roughly 8.7 percent cumulatively despite an average GDP cumulative collapse of roughly 8.4 percent. This is based on a reduced sample of 8 cases for which data on income shares by percentile were available from surveys.

Our results echo with parts of different strands of the literature studying the relationship between output declines and sovereign default. Some works have found defaults and debt crises to be succeeded by significant and lasting economic contractions (Reinhart and Rogoff 2009, Furceri and Zdzienicka 2012, Medas et al. 2018, Rewilak, 2018, Kuvshinov and Zimmermann 2019, Esteves et al. 2021, Arellano et al. 2018), others find the impact of sovereign default on output to be less so (Borenzstein and Panizza 2009; Sturzenegger and Zettelmeyer 2005; Tomz and Wright, 2007) or find sovereign defaults to occur following a decline in economic output or at the trough of a recession (Levy-Yeyati and Panizza 2011). Trebesch and Zabel (2017) build on an earlier literature differentiating between types of default (Eichengreen, 1991; Obstfeld and Taylor, 2003) and show that part of this divergence is likely driven by fundamental differences between defaults. We find that the output collapse in level terms is relatively short-lived with growth returning in short order, yet, and importantly, our study also finds declines relative to the counterfactual to be of a staggering magnitude on average.

Our findings thus have important consequences for the theoretical literature of default. The vast majority of the dynamic equilibrium models on sovereign default assume the cost of default to be lump-sum, typically calibrating models assuming an output loss of 2 percent for each year of default (Arellano and Ramanarayanan, 2012; Aguiar and Gopinath, 2006; Yue, 2010; Hatchondo and Martinez, 2012; Chatterjee and Eyigungor, 2012; Aguiar et al., 2013; Cole et al., 2016, among others). Showing that the cost of default is time-variant and significantly greater in the first years of default implies the need to consider discount factors and default duration expectations inside a policy maker's decision function.

We also contribute to the literature on synthetic controls in the context of multiple treatment units and heterogeneous treatment events by further developing on the aggregation methodology first proposed by Cavallo et al. (2013), introducing weights based on the pre-treatment fit of

control and treated unit, thus weighing individual synthetic control studies by their informational value.<sup>11</sup>

With respect to the impacts along the distribution, we were unable to identify studies directly linking sovereign defaults and poverty – perhaps the lowest hanging fruit we pick with this study – but some research focuses on the association between other types of distress and poverty. And it is worth noting that crises often travel together thus making this literature relevant to our present endeavor (Reinhart and Rogoff 2009, World Bank 2022). Our results are broadly compatible with the findings of the literature addressing the relationship between the different guises of macroeconomic distress and poverty. Dollar and Kraay (2000) find that rising inflation and a fall in government spending, both outcomes intuitively associated with defaults, adversely impact the incomes of the bottom 20 percent. Financial crises, for their part, can lead to increases in poverty through slowdowns in economic activity, increasing unemployment, falling real wages, or changes in relative prices. Baldacci et al. (2002) find, in a cross-country setting, that falling GDP per capita in the wake of financial crises is associated with increasing poverty and increasing income inequality. Chen and Ravallion (2009) estimate the financial crisis of 2009 to have plunged 53 million people below the \$1.9/day poverty line, largely on account of falling growth rates of consumption per capita.<sup>12</sup>

Lastly, Kraay (2006) finds that growth is the main driver of poverty reduction and that gains from growth are captured equiproportionally across the income distribution. We test this finding in the inverse direction, asking whether poverty increases by more than expected if the loss from an output collapse associated with a default was distributed equiproportionally along the country's income distribution. Though our sample is greatly reduced for this exercise, the cases we examine suggest that there is heterogeneity in the impact of default-associated output

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<sup>11</sup> When aggregating across multiple treated units, Abadie et al (2010) suggest turning the treated units into a single unit (e.g. move from municipal level to regional averages). Because treatment moments differ in our case this is hardly applicable. Facing a similar setup, Cavallo et al (2013) develop an unweighted aggregation methodology, suitable in their specific case, where all individual synthetic control trials find suitable synthetic controls. In our case, the former is not the case, so we further extend their methodology to consider the quality-of-fit pretreatment when aggregating unique synthetic control trials. Alternative existing approaches to assign weights to treatment units, such as that weighting units by their interior sample sizes (Zeng et al. ,2021) are not applicable in our case.

<sup>12</sup> Country specific studies such as Habib et al. (2010) similarly estimate adverse effects of financial crises on poverty in Bangladesh, Mexico and the Philippines. These cross-country results are corroborated by micro-evidence from the Mexican financial crisis of 1994-1995, with a rise in both the poverty rate and the poverty gap, increasing unemployment and falling per capita income and consumption, with the poverty effects being stronger in urban areas. Similarly, fiscal retrenchment post-crisis is associated with a deterioration of the income distribution.

collapses on both poverty and cumulative income changes along the distribution. Still, the average outcome is regressive relative to expectations based on an assumption of proportional distribution of losses.

The remainder of the paper proceeds as follows. Section II presents a short overview of the data used. Section III presents the methodology and discusses potential issues, as well as how we address them. Section IV presents our findings. Section V concludes.

## **II. Data**

To better understand the social costs of default, we use three main types of variables: those measuring the social costs, those measuring the default crisis events, and those used to construct the synthetic controls in our estimations.

We use different variables to assess the social costs associated with sovereign default. Some of these variables measure demographic aspects such as infant mortality, life expectancy and population. Infant mortality is defined as the number of children who die before reaching their first birthday per 1,000 live births. Life expectancy is measured at birth in years for the total population as published by the World Development Indicators.

Other variables measure access to resources or income, such as poverty headcount, calorie supply, and energy supply. The poverty headcount measures the percentage of people living below the \$1.90 per day poverty line as published by the World Bank (Poverty and Inequality Platform - PIP). Calorie supply measures the average availability of food in the country (not the actual consumption or its distribution) as computed by the Food and Agriculture Organization. Energy supply assesses the amount of energy usage in a country from any source as published by the OECD World Energy Balances.

Our data on sovereign defaults to private external creditors comes from Farah Yacoub, Graf von Luckner and Reinhart (2022) and takes the form of an annually assessed categorical variable – that is, in default or not (1 or 0). It covers 193 sovereigns and 3 non-sovereigns since 1800, but we use it only from 1900. Default spells are generally considered over once the default signal has been cured for two years. Substantively, it captures defaults as perceived by financial market participants and rating agencies and consequently the revealed inability of the sovereign to meet its commitments as contracted. Thus, the definition hews closely to how rating agencies qualify a

default, which includes: (1) missed payments beyond the grace period; (2) material changes to the contract affecting creditors, including distressed debt exchanges that reduce the debtor's obligations; or (3) unilateral changes imposed by the debtor resulting in diminished financial obligation.<sup>13</sup>

Farah Yacoub et al. (2023) strive to categorize defaults based broadly on this definition even for countries and time periods when rating agencies did not cover the debtor in question.<sup>14</sup> Compared to available historical sovereign ratings, the default data set is broader both in the time and cross-country dimensions. All core results presented here remain significant and of similar magnitude when instead using a smaller data set that only considers defaults signaled by the three leading credit rating agencies merged with a study by Standard and Poor's applying its methodology in retrospect (Standard and Poor's, 2006).

Because we strive to assess the impacts of sovereign default on social outcomes, we aim at limiting possible confounding drivers of poverty, such as wars (Bianchi and Sosa-Padilla 2022). We thus drop all those defaults, which coincide with inter-country wars, using data from Sarkees and Wayman (2010).

### **Data used to restrict the pool for synthetic controls**

In order to limit the risk of interpolation and overfitting, we restrict the pool from which the synthetic control is constructed to countries that are structurally similar to the treated unit (Abadie, 2021). To capture structural similarities, we rely on two variables: GDP per capita and Polity V. GDP per capita is measured in real terms and published by the Maddison Project, and serves as an alternative to splitting the sample by income group, as is often done in the literature. The Polity Project rates countries based on how democratic they are and chosen here as a proxy for institutional strength. The main index produced by the Polity Project score countries from -10 to +10, where the lowest score is attributed to countries that are strongly autocratic and the highest score to countries that are strongly democratic. Compared with other indicators that

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<sup>13</sup> See Ams et al. (2018) citing Moody's (2018).

<sup>14</sup> However, in some cases our default designation differs from that of credit agencies because we may have uncovered information unavailable to these agencies at the time – for example considering a debt or dispute not captured by the agencies – or because in our judgment there are important reasons to signal or not a default. Such as creditors' unwillingness to "accelerate" the contract because they are engaged in conversations, the presence of a legal dispute, or the technical nature of a default not capturing a sovereign's inability to honor its commitments. See Appendix A.1.3 for an example of how the default data was compiled (Farah Yacoub et al. 2021).

could be used for the same purpose, the two chosen variables stand out in their length and breadth of coverage, allowing us to minimize the loss of datapoints.

### III. Methodology

To unambiguously identify the effect of a sovereign default on socioeconomic outcomes, one would have to know what would have happened without the default. Absent the axiomatically unobservable counterfactual, we apply a Synthetic Control Method (Abadie and Gardeazabal, 2003, Abadie et al., 2010). By combining the underlying strategies of difference-in-difference and matching methods, the Synthetic Control Method (SCM) systematically constructs a weighted combination of observations that minimizes the difference of the chosen control with respect to the pre-treatment trend in the variable of interest, as well as a set of relevant predictors, prior to treatment. Importantly, unlike linear regression models often used in the existing literature on the cost of defaults, the SCM does not rely on extrapolation, as unlike standard regression weights, the synthetic control weights never exceed unity (Abadie et al. 2015). Further, because the SCM does not restrict the time variation in the effect of an intervention, we can estimate how the effect of default changes over the years after default. Which contrasts with the prior literature using panel data models, where in most cases the effect of defaults were restricted to be constant over time post default.

Formally, the methodology assumes that in a panel of  $I + 1$  units over  $T$  periods only one unit  $i$  receives the treatment and that treatment occurred within the time observed, so at time  $T_0 < T$ . In the case at hand, we observe units at the country level. The effect of treatment for country  $i$  at time  $T_0$  is thus

$$\delta_{it} = Y_{it}(1) - Y_{it}(0) = Y_{it} - Y_{it}(0),$$

Where  $Y_{it}(1)$  and  $Y_{it}(0)$  represent the (potential) outcome of  $Y$  with and without treatment. What the SCM thus aims to estimate is the vector  $(\delta_{i,T_0}, \dots, \delta_{i,T})$ , overcoming the absence of the counterfactual  $Y_{it}(0)$ . The strategy first developed by Abadie et al. (2003, 2010) thus relies on a way to estimate the treatment effect described under a general model for potential outcomes of all units,

$$Y_{jt}(0) = \eta_t + v_{jt}$$

$$Y_{jt}(1) = \delta_{jt} + \eta_t + v_{jt},$$

Where  $J = 1, \dots, I_{Control} + 1$ . Other than the dynamic treatment effects  $\delta_{jt}$ , potential outcomes also depend on a common factor  $\eta_t$  and the error term  $v_{jt}$ . The error,  $v_{jt}$  is assumed to be subject to the factor model

$$v_{jt} = Z_j \theta_t + \lambda_t \mu_j + \epsilon_{jt},$$

with  $Z_j$  being a vector of relevant observed covariates that are unaffected by the policy change.  $\theta_t$  represents a vector made up of time specific parameters,  $\mu_j$  is a country-specific unobservable,  $\epsilon_{jt}$  are transitory shocks with a zero-mean and  $\lambda_t$  is an unknown common factor. Importantly, while standard Difference-in-Difference models impose  $\lambda$  to be constant over time, the SCM allows for time-variant heterogeneity.  $Z_j$  is unrestricted in terms of number of covariates that can be applied. However, for  $\widehat{\delta}_{it}$  to be unbiased the covariates (or predictors) need to be independent from the treatment. In our case, as we are exclusively applying predictors from well before the treatment, this independence likely holds.<sup>15</sup> Defining  $W = (w_1, \dots, w_{I_{Controls}})'$  as a generic  $(I_{Controls} \times 1)$  vector of weights such that  $w_j \geq 0$  and  $\sum w_j = 1$ , any possible  $W$  represents a potential synthetic control for the treated country  $i$ . The SCM defines  $Y_j^k = \sum_{s=1}^{T_0} k_s Y_{js}$  as a generic linear combination of pretreatment outcomes. As is shown by Abadie et al. (2010), when one chooses  $w^*$  such that

$$\bar{Y}_i^k = \sum_{j=1}^{I_{Controls}} w_j^* \bar{Y}_j^k,$$

as well as

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<sup>15</sup> The possible bias from anticipation is discussed further below.

$$Z_i = \sum_{j=1}^{I_{Controls}} w_j^* Z_j,$$

then

$$\widehat{\delta}_{it} = Y_{it} - \sum_{j=1}^{I_{Controls}} w_j^* Y_{jt}$$

is an unbiased estimator of  $\delta_{it}$ . For further details on the SCM, we refer the reader to Abadie et al. (2010), as well as Abadie and Gardeazabal (2003).

Note that in order to limit the risk of interpolation and overfitting, as well as to build controls using structurally similar countries, we restrict the pool from which the synthetic control is constructed on the basis of two variables: GDP per capita and institutional strength, proxied by the Polity V index. To be included in the donor pool for a given synthetic control study, the potential donor country has to fall within one quartile of the distribution of each of these two variables.<sup>16</sup>

To move from one individual event study to the average effect of sovereign defaults on the outcome variables across the vast sample of sovereign defaults in our data, we primarily rely on the aggregation methodology introduced by Cavallo et al. (2013). We extend that methodology in that rather than calculating the unweighted (normalized) average effect, we aim to further account for the fact that not all individual analyses bear the same informational value. Which stems from not all sovereigns that default necessarily having a suitable synthetic control constructable from the donor pool (Abadie et al. (2010, 2015)). To illustrate, imagine an island-state without any close by neighbors that is structurally different to any other economy in the world. Especially since we restrict the pool of possible samples to reduce the risk of overfitting, one would not be able to construct a suitable synthetic control for such a country.<sup>17</sup> Hence, as is implied by the very nature of event studies, the results from that individual synthetic control

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<sup>16</sup> Given that we also use GDP per capita to match the pre-treatment trend, this pool-restriction is seldomly binding. We thus add the measure of institutional strength, to capture the underlying structural differences of countries from the same income group.

<sup>17</sup> We restrict the donor pool to countries that are structurally similar to the treated unit, by only considering countries within one quartile of the distribution of GDP and institutional strength, proxied by the Polity IV.

study would have little to no informational value. Since, formally, when the vector of covariates and pre-intervention outcomes of the treated unit does not fall sufficiently close to the convex hull of the set of points that entail the covariates and pre-treatment outcome variable of the donor pool, we cannot approximate the counterfactual using a synthetic control. Such cases are identifiable by their poor pre-treatment fit. Yet, they can have large post-treatment effects, even when the distance between the defaulter and its synthetic control simply remains constant over time. To take the heterogeneity in informational value into account when aggregating the event studies, we assign weights,  $\omega_m$ , equal to the inverse of the normalized pre-treatment root-mean-squared prediction error to each synthetic control,  $m$ :<sup>18</sup>

$$\omega_m = \frac{1}{NRMSE} ,$$

where the normalized root mean squared error (NRMSE) is

$$NRMSE = \frac{\sqrt{\frac{\sum_t^{T_0-1} (Y_{i,t} - Y_{k,t})^2}{\overrightarrow{T_{0-1}}}}}{\frac{\sum_t^{T_0-1} Y_{i,t}}{\overrightarrow{T_{0-1}}}}$$

Where  $i$  and  $k$  represent the treated unit and its synthetic control and  $\overrightarrow{T_{0-1}}$  is the number of time periods in the pretreatment period so that  $\{T \in \{T, \dots, T_{0-1}\} : T < T_0\}$ . Instances where the synthetic control offers a poor fit are hence assigned little weight in the aggregation. We can thus refrain from applying further bias correction (e.g. Arkhangelsky et al. (2019); Abadie and L'Hour (2021)) for these cases in particular. Against overinflating the impact of individual event studies with miniscule NRMSE and large treatment effect, we implement twofold: First, we set a lower bound to the NRMSE at 0.01. Further, we implement a robustness check in which we compare the results to those using an adjusted methodology, where we apply a *NRMSE* cut-off at 5% and assign equal weights to all observations with *NRMSE* < 5%.

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<sup>18</sup> This would of course lead to bias, if there existed a significant correlation between pre-treatment RMSE and the treatment affect. However, we do not find any evidence for this affecting our results.



Following Abadie et al. (2010) and Cavallo et al. (2013), to arrive at the significance of the estimated average effect of sovereign default, we apply permutation tests, to compare the observed and aggregated normalized treatment effect with the aggregated, normalized effect observed when randomly selecting placebo treatment units and years. The proportion of these placebo-standardized-effects that are at least as large as the observed standardized effect for each post-treatment period can be read as the probability of the observed effects occurring by chance and can hence be understood as time-period-specific quasi p-values,  $p_t$ :

$$p_t = \frac{\sum_{i=1}^N \theta_{it}}{N} \quad \text{with } \theta = \begin{cases} 1 & \text{if } Y_{itD} \leq Y_{itND} \\ 0 & \text{otherwise} \end{cases}$$

To further account for the smoothing effect from aggregation across many individual case studies, we run  $N = 100$  rounds of  $k$  pseudo-default event studies, leveraging randomly selected country-year doublets. For each aggregated normalized study, we match the number of pseudo defaults per permutation,  $k$ , to the number of defaults considered in the analysis of the actual defaults.

### **A note on anticipation**

As is the case in any event study leveraging time variation in a variable of interest to measure treatment effects, anticipation can lead to bias or even reverse causality. If sovereign default was caused by the perfect anticipation of future economic downturns, there could be reverse causality at play. It is important to note however that neither the theoretical (see the introduction for an excerpt), nor the empirical literature support that being the typical path of events. Instead, there is both ample amount of evidence of the “This-time-is-different”-syndrome (Reinhart and Rogoff, 2009) leading to governments and agents in an economy turning a blind eye to even the most obvious warning signs; and economic downturns beginning just before the default (Levy-Yeyati and Panizza, 2011). The latter finding, in fact, hints at a different type of anticipation – the anticipation of sovereign financial distress and eventually default by investors and businesses, that could slow down GDP because of disinvestment: As far as this type of anticipation exists, it of course should be included in the cost of sovereign default (Costalli et al., 2016). To account for the possibility of anticipation of default, an important adjustment of the SCM is to backdate the matching algorithm pre-intervention to a moment before the default

could have been confidently anticipated (Abadie, 2021). Because when applying the SCM there is no restriction on time variation in the effect of the treatment, de facto treating pre-treatment similar to post-treatment periods does not bias the estimators but allows one to see the difference of the effect of anticipation (if and when it sets in) and the effects of the actual treatment. We apply the backdated matching approach and confirm at least some anticipation seems to be at play in some instances, though even in these cases it is the actual occurrence of default that drives the lion share of the effects measured.<sup>19</sup>

## **IV. Findings**

### **Output**

Based on a sample of 131 defaults since 1900, we find that, on average, sovereign default causes an economy to fall behind by a cumulative 8.4 percent gap relative to the synthetic control within the first three years of the default. This is mainly driven by growth-rates in the first two years of defaults being 3.6 and 2.4 percentage points below the counterfactual. And while GDP growth slows down relative to the control prior to the default – which likely includes effects from default-anticipation on investor- and business confidence, inter alia – the marked decline in output only begins with the default year. On average, GDP per capita growth returns in the third year – though it remains below the trend observed prior to default, on which the synthetic control continues forward. After four years, sovereigns reach their pre-default output level. However, over the decade following default the average annual growth rate is 1.7% below that of the control. So, there appears to remain significant scarring that precludes reconvergence to the prior trend. Note that despite the information-based weight, these results are not driven primarily by the weighting. As is shown in the appendix, filtering results by a NRMSE threshold and not weighting individual findings results in similar findings. Additionally, our results are robust to using the event study methodology pioneered by Gourinchas and Obstfeld (2012), which results in similar findings for growth dynamics.<sup>20</sup> As for all other aggregated results in this paper,

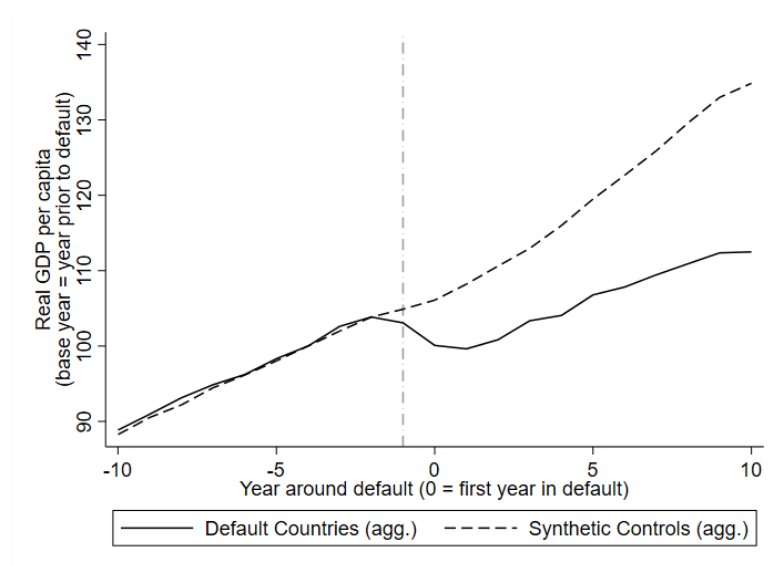
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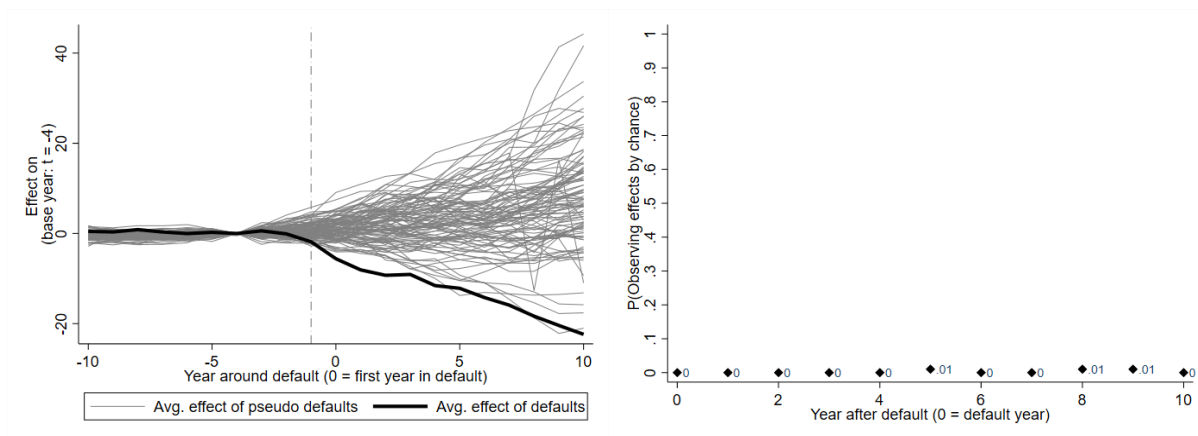
<sup>19</sup> Note that because limiting the matching to periods well before the default requires longer time-series, in the case of poverty head count data, where the length of the time series is limited, we match on the seven periods immediately prior to the default instead.

<sup>20</sup> See the appendix for a brief discussion and presentation of those results.

country-specific synthetic control studies (including weights used to construct the individual controls, and those used to weigh individual studies in the aggregation) are available upon request.

Our results hence differ from previous findings (e.g. Esteves et al., 2021) because they do not rely on extrapolations from fixed-effects models, as well as not facing the former's multicollinearity problems, positions us to better identify what appears to be a break in the growth trend, following sovereign default. The widening gap evident in Figure 1 shows this change in trend, suggesting a structural shift in growth dynamics in association with sovereign default. Empirical evidence of brain-drain following default (Garcia Zea, 2020; Theodoropoulos et al., 2014) the lack of investment in infrastructure due to fiscal austerity and the effect of sudden stops as international investment recedes (Sandleris, 2016; Calvo, 1998) suggest a wide range of contributing channels to explain this change. The investigation into when and which of these or other factors plays the prevalent role is left to future research.





**Figure 1: Effect of default on real GDP per capita. Based on 131 defaults since 1900, where data exists and that are not filtered because of coinciding armed conflict. Source: Maddison GDP Data, Authors' Calculations.**

The lower two graphs of Figure 1 show the statistical significance of our findings. The panel of the lower left presents that the effect we identify (black) is significantly worse than when applying the same methodology estimating the average effects of pseudo-defaults (gray). Here, we ran 100 simulations, each one aggregates the weighted effects of 169 randomly assigned pseudo-defaults.<sup>21</sup> The number of pseudo-treated paths whose effects exceed the magnitude of the measured effect of default thus represent bootstrapped p-values, and are shown for all ten post-default years in the lower right panel of Figure 1.

When seeking to compare our findings to those found in prior studies, the average decline in GDP depends on the sample period. When rather than the full sample of 131 defaults (for which we have sufficient data) we apply a restricted sample equal to that used in prior works on this question (i.e. the 64 defaults between 1972 and 2000 analyzed by Borenzstein and Panizza 2009), we arrive at point estimates that indicate more profound impacts of sovereign default on economic output. Using a cross-country panel regression of GDP growth on default and relevant covariates, Borenzstein and Panizza (2009) find that default reduces annual GDP growth by 1.2 percentage points on average, with the largest effect in the first year (2.6 percentage points). By contrast, applying our methodology to the same sample we find that the first year alone GDP growth is 5.5 percentage points lower, before levelling off to an average annual differential in GDP growth of 2 percent over the first six years of default. The difference in the effect on long-

<sup>21</sup> The number of pseudo defaults per simulation is set equal to the number of defaults in the sample, prior to filtering because of data availability and the coincidence of default and armed conflict, as this filtering is also applied to the pseudo-defaults.

term growth is likely explainable by fixed effect models not capturing time-variant country characteristics and structural changes due to default, which are captured by the SCM's counterfactual. The striking difference in the first year of default is more puzzling. Importantly, the finding is not primarily driven by the SCM algorithm, or our information-weighting: Using no weights and instead dropping cases with a fit worse than a given threshold (conservatively, we applied 5%;10% and 20% NRMSE thresholds), as well as using a fixed-effect event study methodology ([Gourinchas and Obstfeld, 2012](#)). The difference might instead be due to subdued “structural” growth rates during the period studied by Borenzstein and Panizza (2009). And indeed, defaults over this period appear to have been particularly painful: while the average growth rate in the year of default of economies over our whole sample since 1900 is 1 percent, the same was -3.2 percent for the 1972 to 2000 subsample.<sup>22</sup> Instead, in line with the existing literature ([Levy-Yeyati and Panizza, 2011](#)), our results confirm that, likely driven by anticipation of the crisis affecting the private sector, a slowdown in growth of 1.7 percentage points compared to the counterfactual precedes the default.

So far, we have treated all defaults as if they were equal. However, sovereign defaults differ significantly from one another ([Asonuma and Trebesch, 2016](#); [Trebesch and Zabel, 2017](#)). Some, such as Côte d'Ivoire's default spell starting in 1993, remained unresolved and excluded the sovereign from capital markets for decades. Others, like that of Uruguay in 2003, are triggered by a preemptive restructuring in anticipation of debt distress, and are often short-lived, without significant exclusion from capital markets and international trade. And, though less common, there exist *hard* default episodes that end swiftly. In the more typical case, default episodes are spells of recurring defaults, with multiple failed attempts at restructuring the debt burden ([Graf von Luckner et al., 2021](#)). Regardless of the reason for the duration of the period, these episodes could have very different impacts on economic output ([Benjamin and Wright 2018](#)). And especially the average effects in the later part of the sample are thus lowered in magnitude by some countries exiting default and benefitting from the positive impact of debt relief on growth, as is documented by [Reinhart and Trebesch \(2016\)](#). When only considering defaults with durations above the median (6 years), we find defaults to be much more costly in terms of economic output. Over the decade post default, longer defaults have annualized growth rates

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<sup>22</sup> See also World Bank World Development Report 2022, Chapter 5, p. 210 showing the great extent to which the lost decade damaged growth in countries entering default between 1980-85.

2.4% lower than the counterfactual. Even after ten years, these cases on average do not reach their pre-crisis GDP per capita levels, with a national per capita output level more than 24 percent below that of their synthetic control.<sup>23</sup>

### **Social costs: Poverty and its facets**

Our results make the case that default leads to severe growth declines. We now take on the task of measuring the direct impact on poverty headcount and the ways in which living standards may decline in the face of a default.<sup>24</sup>

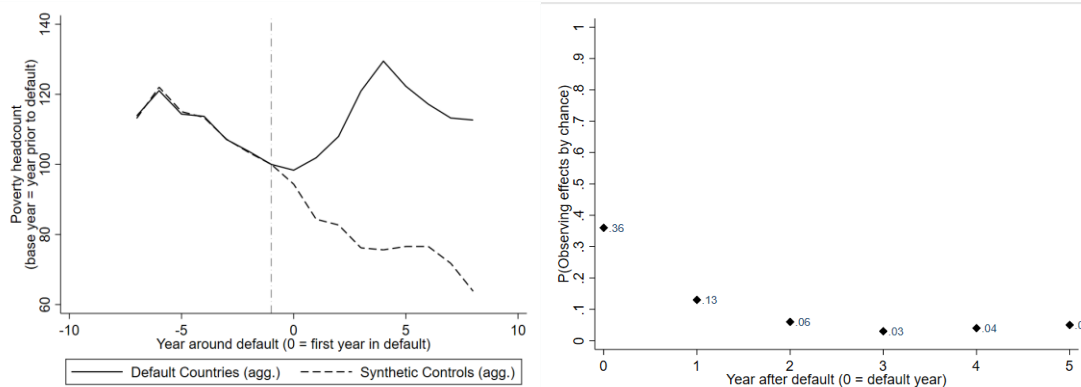
We start by estimating the effects of default on poverty headcounts, which our results suggest are large. Poverty ratios peak in the fourth year after default at 30 percent more individuals living on less than USD 1.9 a day than pre-crisis levels, which amounts to 70 percent more than in the synthetic counterfactual scenario. The gap in poverty headcounts between the defaulting country and synthetic controls remains large with poverty headcounts still 70 percent above the counterfactual seven years after default. Unfortunately, much of the data on poverty headcount ratios is only available starting in 1981 and many of the observations come from imputations performed by the World Bank's Poverty and Inequality Platform (PIP) as opposed to survey observations.<sup>25</sup> Yet, the findings are significant, particularly during the first two years after the default and their magnitude suggests that they are relevant to both the question we set out to explore and policy making.

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<sup>23</sup> Of course, this selection suffers from endogeneity. In what has been referred to as *gambling for resurrection* creditors that observe sovereigns with steep declines in economic activity seem to portray a tendency to hold up with restructuring talks (Uribe, 2006; Benjamin and Wright, 2009; Kaminsky and Vega-Garcia, 2016). Possibly knowing that a restructuring at the depth of a recession will likely mean greater creditor losses. Not solving the debt crisis in turn prolongs the economic downturn.

<sup>24</sup> The results of this section appear robust when using a fixed-effects event study methodology (Gourinchas and Obstfeld 2012). The results of that exercise broadly confirm our findings on poverty headcounts, infant mortality, calorie and energy supply; which can be found with a more detailed discussion in the appendix. Overall, the direction and timing of the effects confirm the findings presented here using SCM.

<sup>25</sup> Data starting in 1981 implies we can only consider defaults after 1988, given the pre-treatment matching requirement, which we reduce to seven years for the poverty headcount analysis, given the data constraints.



**Figure 2: Effect of default on poverty head count (i.e. individuals per 1000 inhabitants that earn less than USD 1.9 PPP) Based on 20 default episodes since 1988, where data exists and that are not filtered because of coinciding armed conflict. Source: PIP; Authors' Calculations.**

Poverty headcounts are informative about welfare, but there are caveats. For instance, an income reduction for a person already below the poverty line has no effect on the indicator. In this sense, our findings above present a lower bound estimate of the effects of default on poverty: A default increases the number of individuals considered in extreme poverty by almost one-third when compared to pre-crisis levels. Additionally, the welfare of an individual above the poverty line could still be hypothetically worse than that of a “poor” person if, for example, the “richer” person is physically disabled and in an environment with little accessibility. The most important caveat, however, is that much of the dynamics of poverty at this aggregate measurement level are driven by changes in output due to the scarcity of survey-based poverty data and that the line used for cross-country measurements is fixed. So, beyond documenting this increase and providing a rough estimate of the magnitude of the change, these findings say little about the differential impact of defaults across the income distribution. Our findings suggest defaults and the concomitant output collapses push people below the poverty line and that they possibly many stay there, but we cannot ascertain what the impact was on those already under the poverty line. Though we attempt to shed some more light around the effects on poverty in the following section through exercises targeted at a reduced sample for which survey observations are available.

This leaves us wanting for further measures that can help illuminate the welfare effects of default. There is a rich literature on multi-dimensional poverty indicators. Some of the better known multi-dimensional poverty indices aggregate variables related to access to health (nutrition and child mortality), education (years of schooling and school attendance), and basic

goods that proxy standards of living (cooking fuel, drinking water, and electricity, among others). While these indices are a step-up from poverty headcounts in that they measure the “intensity” of the poverty and allow for granularity in exploring “how the poor are poor,” these data are also ill-suited for our task. The most widely used index, Oxford’s MPI, only starts in 2010 and observations are not available every year; this is the same issue we face with poverty headcount data if we try to rely only on survey data as opposed to imputations.

We therefore turn to alternative indicators that proxy some of the components of these indicators, but for which more complete time series exist to try to complement the current picture. Taken in concert, these indicators suggest that defaults have persistent negative effects on benchmark indicators and therefore the general welfare of the population. Food availability is curtailed, electricity consumption drops, and even child mortality and life expectancy seem to have worse-than-control outcomes more than a decade after default. The picture of welfare that emerges from an exploration of these variables is one of stagnation with respect to the counterfactuals, and thus with respect to the world.

The recent case of the República Bolivariana de Venezuela after its 2017 default puts in stark relief the insidious association between a major macroeconomic crisis and the welfare of the population. One study by the country’s leading universities and NGOs found that 64 percent of Venezuelans had lost an average of 11 kilograms in 2017 ([Encovi 2017](#)).<sup>26</sup> Nearly 90 percent of respondents also reported that their households’ income was not sufficient to cover the purchase of food and infant mortality had grown to about 70 percent, among other deprivations. Additionally, in 2019, the country suffered nation-wide blackouts ([Morales-Arilla 2021](#)). If only to state the obvious, curtailed access to power in a modern society can prove disastrous, not least because access to other services like water is dependent on it.

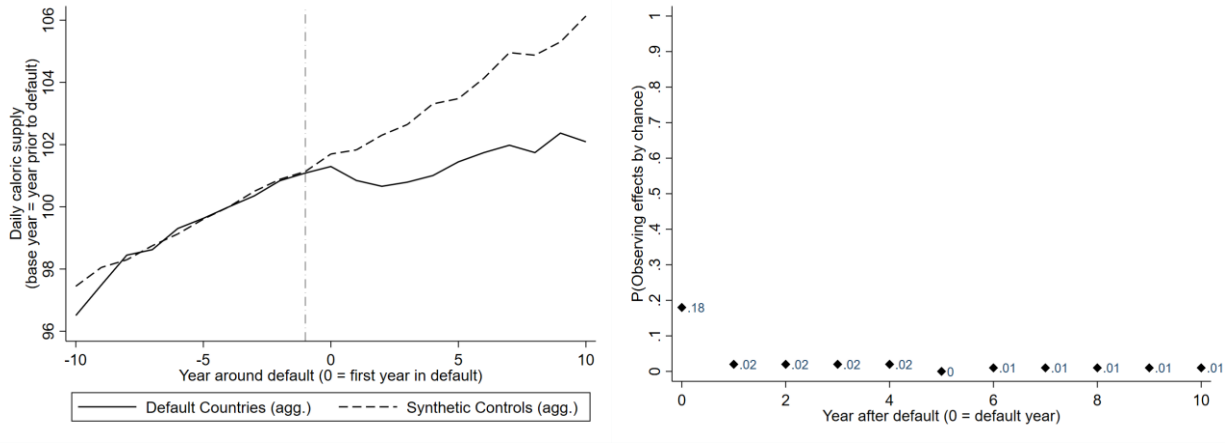
We begin by proxying access to nutrition with the calories available for consumption by the population – aggregate calorie supply per capita indexed to 100 for the year before default. Over the 1961-2015 period, global average daily calorie availability has increased steadily from roughly 2,200 per day to about 2,850. Yet, when we compare defaulting countries to their controls, calorie supply stagnates relative to the synthetic control. The gap grows steadily to

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<sup>26</sup> Deriving the overall weight loss for the entire sample from the responses, one still finds that the population lost an average of 6.8 kilograms.



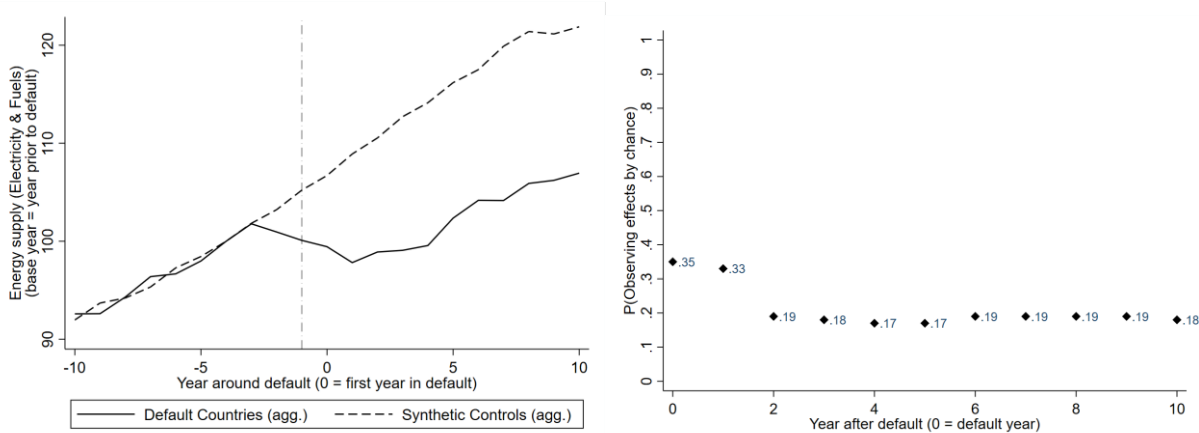
about 4 percentage points 10 years after default. Note also that the relationship between nutrition and other health outcomes is well-established (Strauss and Duncan 1998).



**Figure 3: Effect of default on calorie supply. Based on 73 defaults since 1961, where data exists and that are not filtered because of coinciding armed conflict. Source: FAO. Authors’ Calculations.**

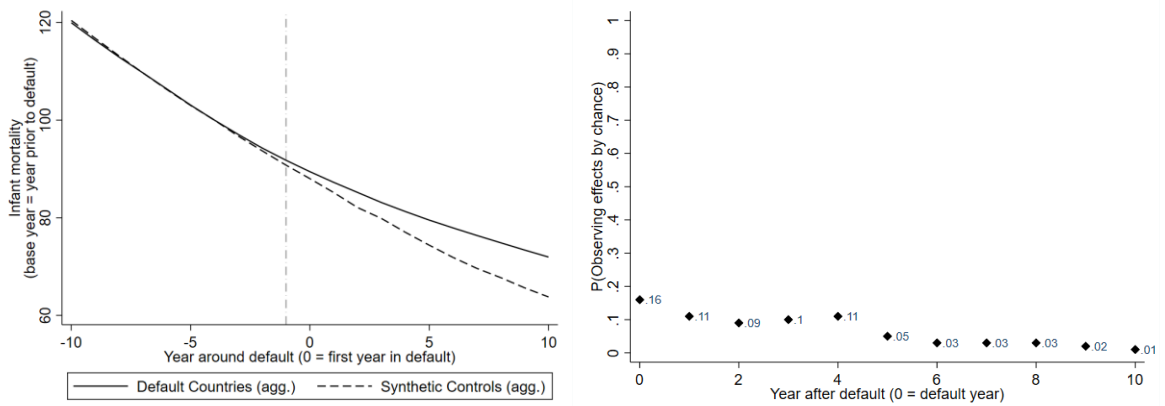
While not statistically significant, our exercise suggests that energy supply per capita levels severely lag in defaulting countries relative to their synthetic control. Therefore, this exercise should be read as a stylized fact.<sup>27</sup> The cumulative estimated gap grows to leave the defaulter roughly 10 percent below the synthetic counterfactual by year three of the default. To be sure, energy consumption can be driven by economic activity via industrial outputs since those tend to consume a considerable amount of energy. So, it is conceivable that consumers can maintain their access level, but in extreme cases it is hard to imagine a scenario in which only industry is affected. And, even in that case, such a marked gap would have effects on development and eventually on welfare. Going back to recent examples, both Venezuela and Lebanon have suffered significant deprivations in access to energy after their respective defaults. In Lebanon’s capital, Beirut, the government had to reduce the provision of electricity to around 30 minutes per day in the summer of 2021 (following the country’s default in early 2020), critically affecting those unable to afford privately run generators.

<sup>27</sup> Yet, it is worth mentioning that the event studies we performed as a robustness check present significant declines in the growth of energy availability with respect to countries’ long term average growth of energy supply. See the appendix for details.



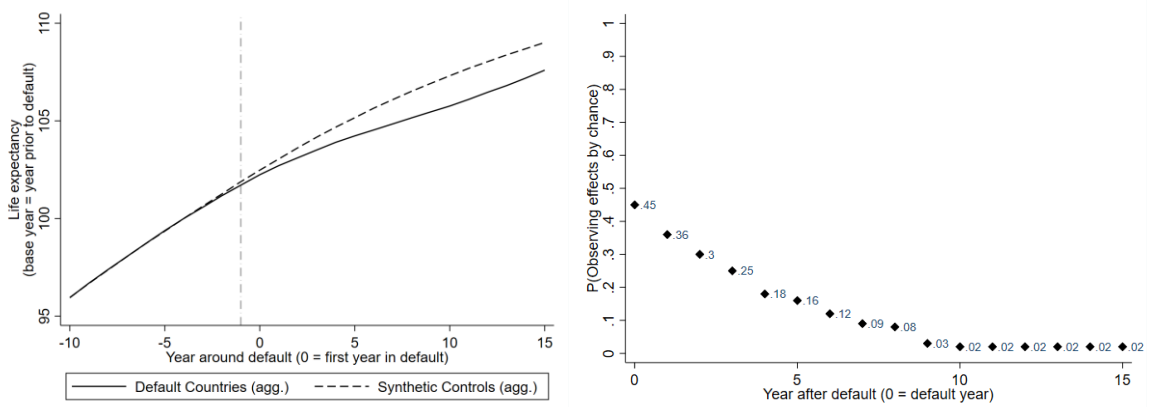
**Figure 4: Effect of default on energy supply.** Based on 48 default episodes since 1971, where data exists and that are not filtered because of coinciding armed conflict, Energy supply per capita indexed to 100 at t-4. Source: OECD Economic, Environmental and Social Statistics, IEA World Energy Statistics and Balances, Authors' Calculations.

Infant mortality also presents intuitive results. Though defaulters keep with the broader downward trend of the 1960-2015 period, the decline is somewhat less marked than for controls. The differences become starker and statistically significant over time, with 13% more infant deaths in the defaulting country, relative to the counterfactual, by year ten after default. Infant mortality is linked to various privations, some of which are proxied here (nutrition) and others for which we do not have sufficient data (pre-natal care). Take the example of Venezuela during the year of its default, 2017. According to the 2017 Encovi study, the number of women at risk (risky pregnancies) for lack of timely medical pregnancy control doubled between 2012 and 2017. Risky pregnancies make for higher mortality of both mother and infant. However, our infant mortality results are fairly heterogeneous, with some countries even accelerating the reduction of infant mortality relative to their peers, soon after they defaulted: Myanmar (default in 1997) and Kenya (default in 1992) are such examples.



**Figure 5: Effect of default on infant mortality. Based on 81 default episodes since 1960, where data exists and that are not filtered because of coinciding armed conflict. Source: United Nations, Authors' Calculations.**

Lastly, life expectancy,<sup>28</sup> an indicator that could be regarded as the long-run total of human welfare for a society, shows a worrying divergence between defaulters and controls. The study window for this indicator is extended to 15 years, as it is the cumulative effect of deprivations over the years since a default that are most likely to affect outcomes. While no statistically significant gap is discernible during the first five years, the divergence is evident and statistically significant after a decade. The cumulative gap between defaulting countries and their controls grows to a striking 1.5 percent – or 1.2 years.<sup>29</sup> To be sure, life expectancy presents an upward trend on average for defaulters, but improvements lose pace considerably in the decade after a default.



**Figure 6: Effect of default on life expectancy. Based on 77 default episodes since 1960, where data exists and that are not filtered because of coinciding armed conflict. Source: FAO. Authors' Calculations.**

<sup>28</sup> Measured as the life expectancy at birth of a newborn that year.

<sup>29</sup> Based on average global life expectancy in 2020 of 73 years (United Nations).

### **Excess Poverty: Is the effect on poverty driven by more than “the size of the pie?”**

As has been shown in the literature (e.g. Borenzstein and Panizza, 2009) and confirmed in this paper, sovereign defaults coincide with significant output collapses. The question is then: Does a sovereign default worsen the impact of an output decline on a society’s poorest?

The change of poverty headcounts around default appears as a key metric to turn to when trying to answer this question. However, much of the dynamics of poverty headcounts that we presented in the previous section are driven by changes in aggregate income because of two factors. First, mechanically, for a given shift of the income distribution, an output collapse will increase the poverty headcount. Second, while the World Bank’s Poverty and Inequality Platform, where we take our data from, often presents time series data on poverty, most of the observations available are imputations derived from income growth dynamics and an assumed log normal income distribution. In other words, it assumes the impact of an output collapse to be distribution-neutral, which ties both poverty and growth outcomes together for most cross-country analyses. As is well established in the literature (Clementi and Gallegati, 2005, Souma 2001, McDonald 2008), income size distributions are typically lognormal, with the density

$$f_x(x) = \frac{1}{x\sigma\sqrt{2\pi}} e^{-\frac{(\ln(x) - \mu)^2}{2\sigma^2}},$$

mean  $e^{\left(\mu + \frac{\sigma^2}{2}\right)}$  and median  $e^\mu$ . With knowledge of the parameters  $\mu$  and  $\sigma$ , one can arrive at the poverty headcount,  $x$ , using the cumulative distribution function:

$$F_X(x) = \Phi\left(\frac{\ln(x) - \mu}{\sigma}\right),$$

where  $\Phi$  is the cumulative distribution function of the standard normal distribution. Using two moments of the distribution prior to default, e.g. the median and the mean, we derive the parameters  $\mu$  and  $\sigma$  before the default. Because distribution neutral growth implies that each member of the society is affected the same, relative to their prior income, we can estimate the impact on the share of the population falling below the poverty line,  $x$ , by shifting the

distribution, holding  $\mu$  and  $\sigma$  constant. Hence, the share of the population that falls below the poverty line,  $x$ , for each year,  $t$ , starting in the default year,  $d$ , is

$$F_X(x_{d+t}) = \Phi \left( \frac{\ln(x_{d-1} * \frac{1}{(1+\delta_{d+t})}) - \mu}{\sigma} \right) \quad \text{for } t = 0, \dots, 4$$

Where  $\delta_{d+t}$  is the cumulative change in GDP relative to the year prior to default.

Having found the expected poverty headcount if growth impacted all individuals across the income distribution equally, we compare this figure with observed poverty headcounts sourced directly from surveys: *Excess poverty*. After filtering PIP data for observations coming from compatible surveys (i.e. same type of coverage: rural, national, urban) and observations only up to 6 years apart, we obtain 649 observations for change in *excess poverty*.<sup>30</sup> Of these, only 30 observations occur in the  $t$  to  $t+3$  period of a default spell, we use these data for our first descriptive exercise. When looking at the effects of growth in average per capita income on growth, Kraay (2006) finds that much of the variation in poverty can be attributed to “growth in average incomes,” while little is attributable to a high sensitivity of poverty to the same. Dollar and Kraay (2002) find that mean income for the bottom quintile of the distribution grows equiproportionally to overall per capita income.

Yet, the cases we can explore below suggest that, at least on occasion, poverty increases beyond what would be expected for a given GDP decline. Figure 7 below suggests that poverty headcounts are somewhat elastic to income during the first few years of a default. The slight downward slope of the observations within the default window implies that in these cases growth translated to larger than expected changes in excess poverty between surveys. This elasticity is estimated at -0.03% per every 1% change in mean income and it is statistically significant at the

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<sup>30</sup> Note that the observations for change in excess poverty can thus be taken from occurrences up to 6 years apart, but the window is not fixed. For example, an observation of country X’s change in excess poverty may be the result of comparing excess poverty at  $t$  to excess poverty at  $t-1$ ; while another observation for the same country, or a different one, may come from comparing excess poverty at  $t+3$  to excess poverty at  $t$ . The observations are adjusted as an annualized average change in excess poverty for differences  $> 1$  year apart. The distances between surveys for the 649 observations are distributed as follows: 54.73% of the differences come from observations 1 year apart, 13.26% are 2 years apart, 12.8% are 3 years apart, 6.25% are 4 years apart, 7.47% are 5 years apart, and 5.49% are 6 years apart. For the 30 observations within the  $t$  to  $t+3$  default window, 70% are 1 year apart, 16.67% 2 years, 10% 3 years, 3.33% 6 years.

10% level. This is not a large effect, but it differs from the non-defaulting sample that suggests excess poverty is inelastic to growth and thus that changes in poverty after a default are only a result of the concomitant change in growth. A similar exercise not shown here looking at point estimates for excess poverty versus change in mean incomes paints a similar picture. Admittedly, however, even after maximizing the number of survey observations available by using a flexible 6 year window, data constraints impede a more conclusive statement.

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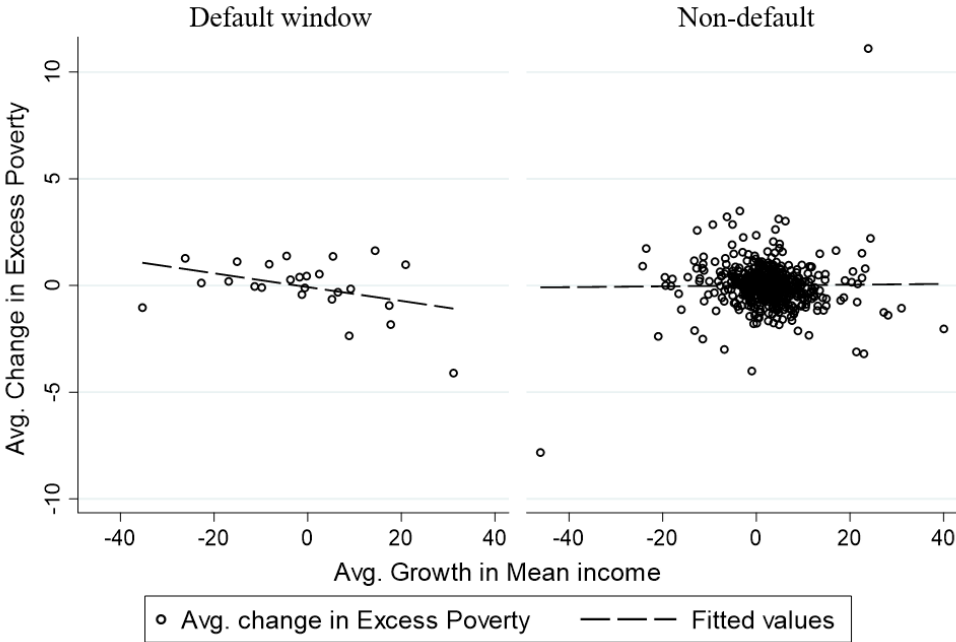


Figure 7: Average change in excess poverty around default,  $t$  to  $t+3$  and outside of this window. Based on 649 observations, of which 30 occur within default windows since 1980. Source: PIP, Authors' Calculations.

Only in 11 instances is poverty headcount surveyed *every* year of the t-5 to t+5 period of a default. We explore these cases in more detail using the same excess poverty methodology here. For example, after its 1998 default, Moldova’s poverty headcount peaked with an additional 20 percentage points over what would be implied if the growth collapse had been absorbed proportionally by individuals across the entire income distribution.

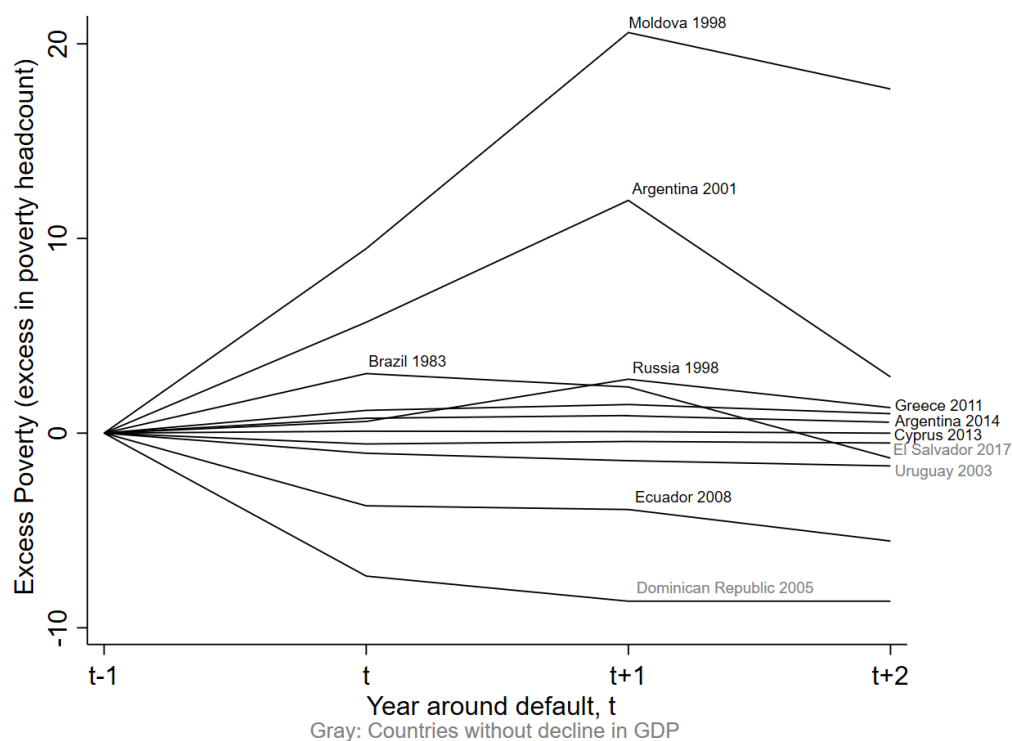


Figure 8: Average excess poverty around default, t, by country. Includes the 11 default episodes since 1980, where annual survey-based income distributions are available. Source: PIP, Authors’ Calculations.

The results show considerable heterogeneity. Four countries, Moldova, Argentina (2001), Brazil and the Russian Federation, present sizable increases in poverty over what would be implied by the growth shock. Greece, Cyprus, Argentina (2014), El Salvador and Uruguay remain close to zero excess poverty, which means changes in poverty headcount are driven mainly by average income reduction. Lastly, Ecuador and the Dominican Republic present negative excess poverty figures. These two cases also continued to grow consistently during the default period. While this is somewhat puzzling, some potential explanations exist. For example, both of these cases were of pre-emptive restructuring rather than a “hard” default (Asonuma and Trebesch, 2016), which can free up resources for alternative budgetary measures (e.g. social

spending). In the Dominican Republic's case (as well as the other cases marked in gray), the default was not accompanied by recession and inflation seems to have been dangerously high in the year prior to default but controlled during the default. Ecuador's default was accompanied by a relatively benign one-year decline in GDP of around 2 percent. Uruguay for its part suffered contagion from Argentina and responded swiftly with macroeconomic measures and swift restructuring.

An elasticity of poverty to growth larger than one could be due to various channels or initial conditions. At the lower end of the income distribution, job security may be lower, households may have little ability to save, and be generally more exposed to erosion from concomitant macroeconomic crises such as inflation. Dollar and Kraay (2002) find that control of inflation is one of the few policies that improve income capture by the bottom quintile beyond the equiproportional growth that makes for the main finding in their paper. It is not a stretch then to hypothesize that one of the main channels in which defaults can lead to excessive poverty increases is through the runaway inflation that accompanies them in many cases. Argentina's 2001 default presented excess poverty of about 10 percentage points, lending some support to this thesis, but at the same time Brazil's 1983 default shows somewhat muted excess poverty during a time of triple-digit inflation. For their part, Ecuador and El Salvador were dollarized economies at the time of these defaults. Since inflation and currency devaluation are drivers for poverty increases, being dollarized prior to the default could make for a less regressive distribution of socialized losses. Of course, the proximate cause of the poverty increase in this scenario would be inflation and currency collapses, but dollarized systems control these risks to a greater extent than the typical monetarily autonomous country. Another factor worth exploring to explain the different outcomes is the political leaning of the governments in charge of the crisis response. Ecuador's 2008 restructuring was pursued by Rafael Correa's government explicitly as a rebuttal to Wall Street and with the stated objective of freeing up Ecuadorian resources for social expenditure.

The cases above signal that a significant number of countries experience income distribution shifts around the international poverty line of 2011-US\$ 1.90 per day. This is a threshold question that may prove more informative in some cases than in others, but it provides initial evidence that there may be more than only growth to the story of poverty and default.



Unfortunately, this is the best data available in terms of coverage, which could be driven by structural bias affecting the availability of survey data during crisis times – Surveys are not necessarily known to be a priority during macroeconomic crises.

If we then take a more strictly distributional view and ask how the income of households at different levels of the income curve is affected during output collapses associated to a default, our cases point to a regressive relationship. The cumulative drop in the level of national income going to the bottom 10 percent<sup>31</sup> of the distribution is a whopping 9 percent when measured from peak to trough during a t-1 to t+2 window around default.<sup>32</sup> For the bottom half of the distribution (including bottom 10 percent), it drops roughly 6.4 percent. But, on the flip side, households in the top 10 percent gained roughly 8.7 percent – all despite an average cumulative GDP p.c. collapse of roughly 8.5 percent.

These summary statistics are based on 8 cases for which we have data and which present GDP downturns along with the default, since we are trying to better ascertain how regressive an output collapse happening concurrently with a default is.<sup>33</sup> Some heterogeneity remains. In two cases, Russia 1998 and Argentina 2014, the share of income going to the bottom 10 percent increased. For all others, it decreased. In cases like Greece 2011 and Argentina 2001, the share of income received by the least fortunate plunged by far more than overall GDP. In Greece's case, the bottom 10 percent's share of income fell by 44% while the output collapse amounted to nearly 18%; for Argentina the bottom 10 percent lost roughly 86% of their share of income while the output collapse amounted to 16.7%.

## V. Conclusion

This paper uses an augmented synthetic control method to show that sovereign defaults are very costly. The associated drop in GDP p.c. is large and long lived. The results are larger than

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<sup>31</sup> Measured as the share of income captured by the bottom 10 percent of the income distribution times real GDP in constant 2011 USD.

<sup>32</sup> Peak is defined as the highest GDP p.c. figure present at either t-1 or t, while trough is defined as the bottom GDP p.c. figure present between t, if the peak is at t-1 and the default happens in the early months of year t, and t+2. Consequently, peak-to-trough windows can be as short as 1 year and as long as 3 years.

<sup>33</sup> In practice, this means we ignore the cases of El Salvador 2017, Dominican Republic 2005, and Uruguay 2003 since the countries' GDP p.c. continued to grow during the default period.

what was previously found in the literature, but still of comparable magnitude with a more robust econometric method. Furthermore, the synthetic control method allows for time-variant effects which previously used methods in the literature were unable to estimate. We show that the cost of default is heavily frontloaded.

More importantly, to our knowledge, this is the first paper to systematically assess some of the key social costs of sovereign default in a large panel. We find significant effects of sovereign default on poverty, nutrition, electricity and health outcomes, indicating that the most vulnerable segments of the population are heavily impacted by sovereign defaults and the decade that succeeds them. Whether the effects on poverty are predictably only caused by plummeting growth is a question that will need better data and further inquiry, but our study showed that there may be considerable heterogeneity in results. Additionally, the main association stands: Sovereign defaults impose a great burden on a society, pushing about 30% more people into poverty and depriving the population of access to basic needs like nutrition and energy.

The social cost results are informative despite data limitations of some of the outcome variables. In particular, using data for the 8 countries where we observe output collapses after default and where annual data on the income distribution is available, we find that on average, lower income segments of the population bear the brunt of the default cost. This result is consistent with the results from the less granular poverty data. In other words, just like all roads lead to Rome, all variables here point to a decline in standards of living of those whose standards of living were already sub-standard.

The link between social outcomes and sovereign default has been under-researched. A notable exception is Andreasen et al. (2019), as well as Jeon and Kabukcuoglu (2018). Both works develop a theoretical model to predict sovereign default based on the level of inequality. In their models, as inequality increases, sovereign default becomes more likely. We contribute to this strand of the literature by studying the flip side of that relationship. Our study shows that social outcomes (such as poverty or infant mortality) are importantly suffering after the default event.

Yet, our paper is only a starting point to answer important questions. Many research questions are left unanswered mainly due to data availability. For instance, the interaction with other types of crises and how that affects social costs. As discussed above, inflation is a driver for poverty increase and it often occurs during default crises. It would be useful to understand

how these two types of crises interact and their effect on poverty – Potentially whether there is sequencing or if inflation, for example, is the channel through which much of the social costs are borne. Furthermore, while we look at differential impacts depending on the length of default, there are other characteristics of a default episode that can also affect the results, such as the size of the haircut and the number of restructurings. It is our suspicion as well that with a more granular study controlling for war, as opposed to excluding incidents of war, the results would show interesting heterogeneity – though it is hard to imagine household survey data being available for war years. Lastly, the decision to default is always a political decision. It would be useful to understand how different political regimes fare in case of default and how the social costs of default differ depending on the political regime.

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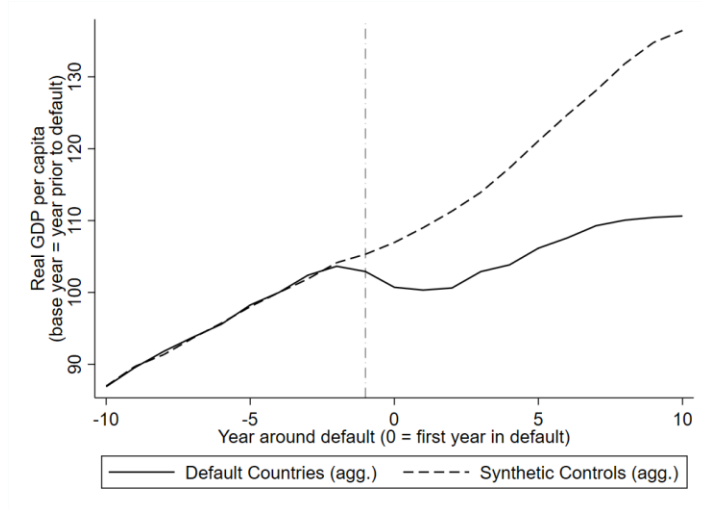
## A1. Appendix 1 - Variable descriptions

Variable Name	Description	Number of observations	Source
GDP pc	Real GDP per capita constant prices (2011 US dollars)	Unbalanced panel from 1800 to 2018 (219 years) with a minimum of 20 countries to a maximum of 169	<a href="#">Maddison Project Database, version 2020</a>
Sovereign default	Binary variable taking the value 1 if the country is in sovereign default and 0 otherwise	Panel from 1800 to 2020 (221 years) of 195 countries	Farah Yacoub et al. (2021)
Infant mortality	Number of children who die before reaching their first birthday per 1000 live births. Some of the data points are extrapolated using the past trend in a country and the global trend.	Unbalanced panel from 1960 to 2020 (61 years) with a minimum of 79 countries to a maximum of 199	<a href="#">United Nations Inter-Agency Group for Child Mortality Estimation</a>
Population	Number of inhabitants measured mid-year in thousands	Unbalanced panel from 1900 to 2018 (119 years) with a minimum of 53 countries to a maximum of 169	<a href="#">Maddison Project Database, version 2020</a>
Life expectancy	Life expectancy at birth in years for the total population	Panel from 1960 to 2020 (61 years) of 208 countries	<a href="#">World Development Indicators</a>
Energy supply	Basic supply and demand data for all fuels are required to compare the contribution that each fuel makes to the economy and their interrelationships through the conversion of one fuel into another.	Panel from 1971 to 2019 of 146 countries	OECD Economic, Environmental and Social Statistics, IEA World Energy Statistics and Balances

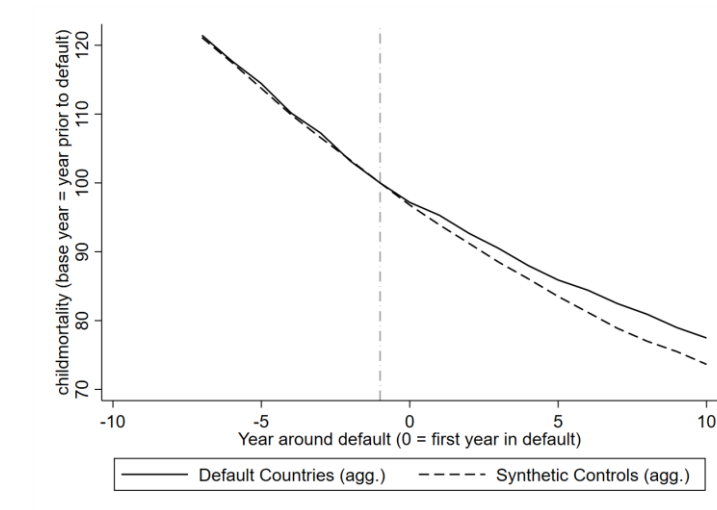
Calorie supply	Per capita kilocalorie supply from all foods per day	Unbalanced panel from 1961 to 2018 (58 years) with a minimum of 143 countries to a maximum of 172	<a href="#">Food and Agriculture Organization of the United Nations (FAO)</a>
Poverty headcount	Percentage of people living below the \$1.90 per day poverty line	Unbalanced panel from 1981 to 2019 (40 years) with a minimum of 160 countries to a maximum of 165	<a href="#">Poverty and Inequality Platform (PIP)</a>
Polity V	Score ranging from +10 (strongly democratic) to -10 (strongly autocratic)	Unbalanced panel from 1800 to 2018 (219 years) with a minimum of 22 countries to a maximum of 168	<a href="#">Polity Project</a>

## A2. Appendix 2 – Results with 5% threshold

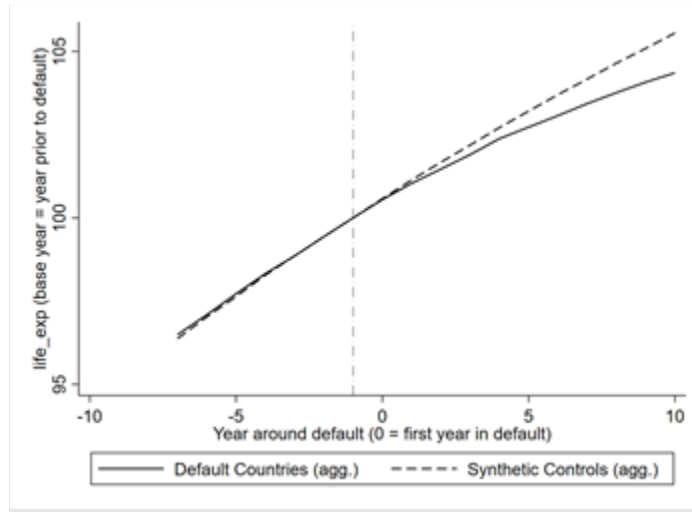
### GDP p.c. (Maddison since 1900)



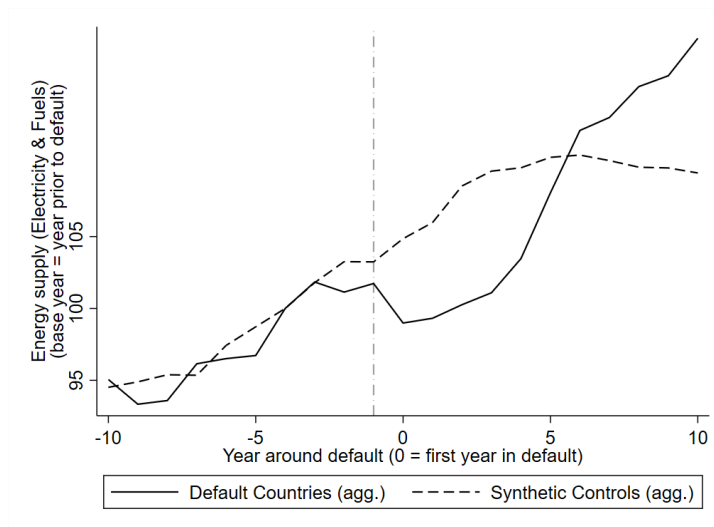
### Child mortality (since 1950)



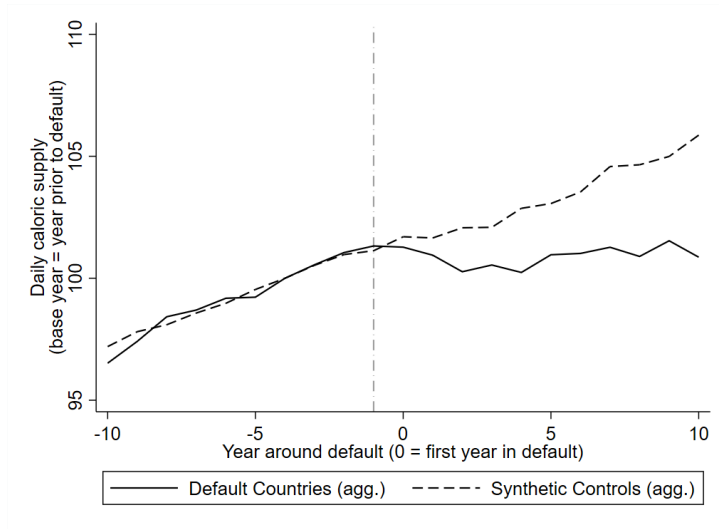
## Life Expectancy (since 1960)



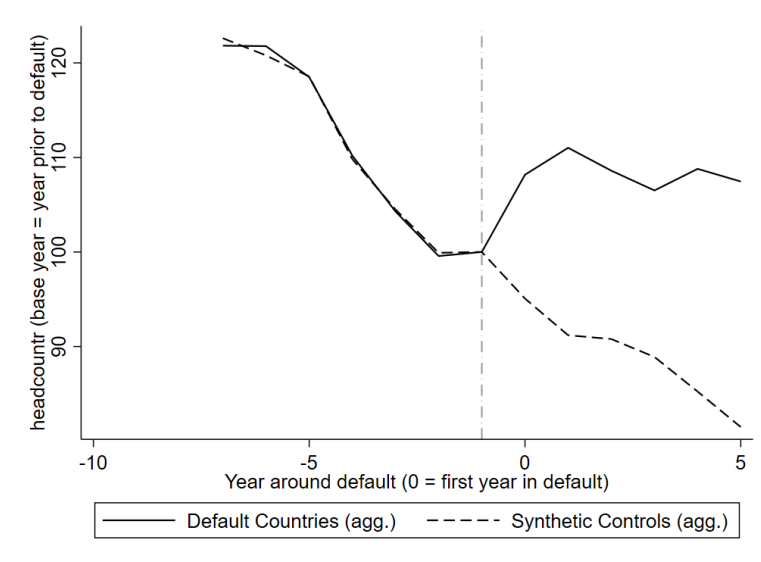
## Energy supply (since 1970)



## Caloric Supply (since 1961)



## Poverty Headcount (since 1988)



### A3. Appendix 3 – Event Studies T-5 to T+5

The below are country-fixed-effects event studies following the methodology of Gourinchas and Obstfeld (2012). They should be read as deviations from country long term means of the enunciated variable. All but one of the variables are first differences (in percentage terms), so the appropriate interpretation for them is X percentage points below or above country long-term average percent change. For Poverty Headcounts, the variable is already in ratio form. Therefore, the appropriate interpretation should be X percentage points over country long-term average poverty headcount ratio. The results from this exercise are compatible with our findings above using SCMs.

