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**ABSTRACT**

This paper investigates the economic and social consequences of sovereign default on external debt. We focus on the crises' impact on real per capita GDP, infant mortality, life expectancy, poverty headcounts, and calorie supply per capita. After methodological exclusions, the sample covers 221 default episodes over 1815-2020. The analysis adopts an eclectic empirical strategy that relies on an augmented synthetic control method and local projections. Our findings suggest that sovereign defaults lead to significant adverse economic outcomes, with defaulting economies falling behind their counterparts by a cumulative 8.5 percent of GDP per capita within three years of default. Moreover, output per capita remains nearly 20 percent below that of non-defaulting peers after a decade. Based on the trajectory of the health, nutrition, and poverty indicators we study, we assess that the social costs of sovereign default are significant, broad-based, and long-lived.

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## Introduction

The economics literature has long explored the various incentives for a sovereign to avoid default. An influential paper by Panizza, Sturzenegger, and Zettelmeyer (2009) groups the costs of default into four categories: Reputational costs, which impact future access to borrowing;[1] the costs of reduced international trade due to retaliatory trade embargoes and other sanctions;[2] costs arising from an impaired financial sector due to loss of access to capital markets and corporate credit, or declines in foreign investment;[3] and lastly, as highlighted in Borensztein and Panizza (2009), there are the political costs to the authorities. We contribute to this literature by presenting evidence that sovereign defaults also carry significant "social costs," as measured by the depth and duration of the economic contraction and its spillovers into poverty indicators and by the sustained and widening gap in essential health and nutrition indicators between defaulters and non-defaulters. While consistent, long-dated historical data on poverty levels remains elusive, the indicators presented in this study suggest a post-default rise in the incidence of poverty.

The empirical strand in this literature has primarily focused on quantifying the output (real GDP) losses that typically follow a sovereign default. Borensztein and Panizza (2009) studied defaults on external debt, while Reinhart and Rogoff (2009) examined the aftermath of both external and domestic debt defaults. They conclude that recessions following domestic defaults tend to be deeper than those when the default is confined to external debt. Marchesi and Mati (2021) compare external debt restructuring episodes with private creditors to those involving official (government-to-government) creditors. While there is little doubt that real GDP provides an essential measure of the economic damage associated with sovereign default, it is silent on how those costs may translate to other measures of well-being.

Our analysis focuses on 221 external private creditors' default episodes from 1815 to 2020. In addition to real per capita GDP, our indicators include child mortality, life expectancy, the number of households under the poverty line, and calorie supplies per capita.

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[1] Examples include Eaton and Gersovitz (1981), Dooley (2000), Amador and Phelan (2021), Aguiar and Amador (2021).
[2] See Bulow and Rogoff (1989), Mitchener and Weidenmaier (2010). 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.
[3] Rose (2005), Arteta and Hale (2008), Trebesch (2009), Mendoza and Yue (2012), Sandleris and Wright (2014), Arellano, Mateos-Planas, and Rios-Rull 2022).

Outside the literature on sovereign default, a relevant body of work stresses the regressive nature of inflation (and inflation crises). Based on survey data for 38 countries and cross-country and time series evidence, Easterly and Fischer (2004) conclude that inflation is a cruel tax, disproportionally impacting the poor. Diaz Alejandro (1963) suggested that currency crises were contractionary in emerging and developing countries because these redistributed income from workers (wages) with a high propensity to consume to exporters (profits and rents) with a lower propensity to consume. As sovereign default is often accompanied by higher inflation and weakening currencies (Reinhart and Rogoff, 2009), there are a priori reasons to expect that sovereign debt crises may also have a disproportionately adverse impact on the poor. Furthermore, lack of financing coupled with the fiscal adjustments needed to restore debt sustainability often leads to significant and sustained reductions in social programs.

There is a general understanding (supported by spotty evidence) that debt crises tend to increase poverty. Still, non-imputed poverty data remains scarce, and more accurate welfare measurements, such as multidimensional poverty indicators, are of recent vintage.[4] These data limitations undermine our understanding of the links between sovereign defaults and social welfare.

Our study aims to shed light on sovereign default's economic and social costs. First, we contribute to the literature by revisiting how costly sovereign defaults are in terms of output losses through a relatively unexploited lens. By applying the synthetic controls method, we assess the cumulative time-varying effects of sovereign defaults.[5] Second, we consider other welfare facets of the cost of default by incorporating life expectancy, infant mortality, total households under the poverty line, and calorie supply per capita into the analysis. Some studies (Nishiyama, 2011 and Wilkinson, 1992, among others) have suggested nonlinearities in the relationship between health indicators and income levels, with movements in and out of the lowest quintile having the most

[4] Non-imputed data refers to the years in which poverty measures are based on actual household surveys. As surveys are infrequent in many, if not most, countries, the poverty estimates for interim years are based on interpolation which rely heavily on real GDP.

[5] The synthetic control method algorithm provides a proxy for the missing counterfactual by constructing a combination of weighted controls from the "donor" pool countries. The "donor pool" is comprised of countries where the pretreatment outcomes and control variables are on average equal or at least very similar to those of the "treated unit" (the defaulting sovereign). The selection of the control group is done algorithmically and transparently, overcoming possible selection bias in the construction of the control group. Details are provided in the Methodology section of the paper.

significant impacts on health outcomes. Possibly, evidence from health and nutrition indicators may be more indicative of the regressive impact of defaults than real per capita GDP.

Our study leverages complementary empirical strategies. We apply the synthetic controls method (Abadie et al., 2003) modified to study multiple staggered events (Acemoglu et al., 2016) to an encompassing dataset that extends back to the 19th century and covers many low-income countries previously excluded from studies. The application of synthetic controls is comparatively novel in the sovereign default literature, as most studies predate the widespread use of this approach in the social sciences. Marchesi and Masi (2021), who apply this methodology to a sample of 23 countries that have restructured their external debt, represent the exception.

Moreover, while most existing studies have mainly been confined to the post-1970s sovereign default experience, our data allows us to study the wave of defaults during the 1930s and, more selectively, 19th-century episodes. As a robustness check, we reexamine the default-output-health nexus with the local projections approach (Jorda, 2005).

Our main findings can be summarized as follows:

First, we find that, on average, within three years of a sovereign default, the affected economies' real per capita GDP falls behind the control group by a cumulative 8.5 percent. While the slowdown in growth typically begins the year prior to the crisis, most of the cumulative effect owes to output losses during or post-default.

Second, recovery to the pre-default real per capita GDP level occurs, on average, by the fourth year after default. Over the post-default decade, an apparent structural shift in growth leads to an average growth deficit of around 1 percent per year. After a decade, defaulters' economic output per capita is nearly 20 percent below that of the non-defaulting control group.

Third, the output losses are largest for longer default episodes. The gap between defaulting countries and the counterfactual is two times larger for "long" defaults (above the median) relative to those resolved comparatively quickly. The median default duration over the entire 1815-2020 sample is six years.

Fourth, average infant mortality for the defaulting countries does not exhibit a spike or reversal during or after default. Yet progress slows, resulting in statistically significant cumulative gaps between defaulters and the counterfactual control group in the aftermath of default. On average, by year ten, defaulters have five more infant deaths per 1000 live births than the counterfactual.

This result is broadly consistent with Baird, Friedman, and Schady (2011), who find that infant mortality is sensitive to aggregate income shocks.

Fifth, infants who survive are expected to have shorter lives. There is a mild “double dip” in life expectancy in years four and six post-default. Ten years after the default, life expectancy is (on average) 1.1 years below that of the control group. All our exercises exclude cases where the default overlaps with armed conflict.[6]

Sixth, the aggregate daily supply of calories per capita peaks one year before default and contracts for five consecutive years. The peak-to-trough contraction is roughly one percent. The next six years are best categorized as an anemic and halting recovery. A decade after the default, the country’s per capita caloric supply stood about where it was prior to default.

Lastly, we explored the links between sovereign default and the incidence of poverty. Due to the series’ comparatively recent vintage (viz the other indicators) and the infrequent nature of household surveys, results must be interpreted with care. With this caveat in mind, we find that the number of households in poverty is roughly 6 percent higher by year five after a default, and a decade after the default, the gap has grown to 10 percent.

The paper proceeds as follows. The next section discusses the literature, which is largely populated by studies that focus on the link between sovereign default and economic growth. The scantier literature on the links between social indicators, poverty, and macroeconomic crises, which is more germane to this study, is also reviewed. Section 3 summarizes the data while pointing the reader to the detailed Data Appendix. Section 4 briefly describes the empirical strategy involving the synthetic control method and local projections. Section 5 provides the core results for the economic and social indicators we examine. Section 6 presents two sets of robustness exercises; the first accounts for potential sensitivities to our aggregation methodology, while the second attempts to address concerns over confounding variables, such as other types of crises (e.g., banking or currency) accompanying a default. Lastly, section 7 offers concluding remarks and discusses areas for future research.

## *2.* Literature

### *2.1 Output losses and sovereign default*

The literature that has studied the relationship between output and sovereign default has generally converged on the central finding that default is associated with output losses. However, there is far less consensus on the timing, magnitude, and duration of the economic costs.

As to the *timing* of the onset of recession, Levy-Yeyati and Panizza (2011), who cover 24 external default episodes in 14 countries over 1980-2004, stress that the output declines occur prior to and at the time of default and that economic recovery follows immediately on the heels of default. They interpret this result as indicative that it is the anticipation of a default that drives the recession. Reinhart and Rogoff (2009), who study 250 external default episodes over 1800-2008, also find that real per capita GDP begins to contract one year before the default and that the trough in output is in the year of the default. However, they stress that due to the depth of the recession relative to the anemic nature of the subsequent recovery, the *level* of real per capita GDP remains below the prior peak even three years after default (the point where their comparison ends). They define recovery as a return to the previous output peak; therefore, the post-default rebound is not considered a recovery.

As to the *magnitude* of the output losses and their *duration (persistence),* estimates vary considerably. Furceri and Zdzienicka (2012) control for the fact that sovereign defaults are often accompanied by other types of crises (banking and currency) and highlight episodes where these other crises were absent. They find evidence of both significant contemporaneous output losses (about 8 percent) as well as output losses over the medium term (eight years post default). Recent studies that also document significant output costs in the wake of default include Arellano Bai and Mihalache (2018), Medas et al. (2018), Kuvshinov and Zimmermann (2019), and Esteves Lennard and Kenny (2021).

Other studies, such as Sturzenegger and Zettelmeyer (2005) and Borenzstein and Panizza (2009), have found the impact of sovereign default on output to be significant but short-lived. A key message from Tomz and Wright (2007), who study the default-output connection for the period 1820-2004, is that while output falls (relative to trend) during and after default, the relationship is a weak one in that countries have also defaulted during comparatively "good times" and other countries facing output declines continued to service their debt.

Trebesch and Zabel (2017) emphasize that not all defaults are created equal and distinguish between “soft” and “hard” defaults. Their classification is based on a procedural index that tracks a government's payment and negotiation behavior vis-à-vis foreign creditors during a default spell. On the timing of the onset of recession, they also find that the downturn starts in the year prior to default. On the severity of the decline, they conclude that recovery is comparatively swift for the “soft” cases, akin to the pattern that Levy-Yeyati and Panizza (2011) describe. Not so for “hard” defaults, where real GDP remains more than five percentage points below its pre-crisis level five years after the outbreak of the crisis.

Marchesi and Masi (2021) examine the output effects of *individual* debt restructurings with private creditors (as well as with official creditors). This approach is a departure from the literature discussed here, which focuses on default spells or a debt crisis from default to its resolution (i.e., the final restructuring that cures the default). As documented in Graf von Luckner et al. (2023), a significant share of debt crises involves two or more individual debt restructurings. These interim restructurings occur while the country remains in default, making it difficult to compare these episodes with new defaults.

In this paper, we show that the recession begins in the year before default and that the output contraction is relatively short-lived post-default. Yet, our study also reveals staggering cumulative output losses relative to the control group of non-defaulters, thus highlighting the persistence of the economic costs.

These findings have important implications for the theoretical literature on sovereign default. The majority of dynamic equilibrium models assume that the cost of default is lump-sum. Calibration exercises typically assume 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 suggests a need to consider discount factors and default duration expectations inside a policymaker’s decision function.

### *2.2 Social indicators, sovereign default, and other financial crises*

The literature that links sovereign default to poverty, health, or other proxies of welfare is virtually nonexistent. However, other manifestations of economic distress often accompany sovereign default, be it a banking crisis, a currency crash, or a surge in inflation. Accordingly, we highlight some related research that focuses on the association between other types of distress and social indicators and outcomes.

Focusing on the bottom income quintile over the period 1973-2011, Rewilak (2018) concludes that currency crises are most harmful to the poor. This is followed by inflation and banking crises, while sovereign debt crises only have a statistically significant effect on the income of the poor in richer countries. Unfortunately, the definition of richer/poorer is idiosyncratic, as it is based on splitting the 61-country sample in two (31 richer/30 poorer), and no country list is provided.

Dollar and Kraay (2000) find that rising inflation and a fall in government spending (outcomes often associated with sovereign defaults) adversely impact the incomes of the bottom 20 percent. Easterly and Fischer (2004) similarly find that high inflation reduces the income share of the bottom quintile. Still, they also report declines in the real minimum wage and a tendency to increase poverty.

Financial crises, for their part, can lead to increases in poverty through their recessionary effects, increasing unemployment, falling real wages, and changes in relative prices. Baldacci et al. (2002) find that falling GDP per capita in the wake of financial crises is associated with increases in poverty and income inequality. Chen and Ravallion (2009) estimated that the financial crisis of 2009 would push 64 million people below the $2/day poverty line; they predicted the global poverty rate to fall from 42 percent to 39 percent in 2009, while the pre-crisis trajectory would have brought the poverty rate down to 38 percent.

Country studies such as Habib et al. (2010) estimate the adverse effects of financial crises on poverty in Bangladesh, Mexico, and the Philippines. These 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.

While our analysis is silent on the impact of sovereign debt crises on income distribution, the effects of the crisis on health indicators and poverty are suggestive of significant social costs.

## 3. Data

This section summarizes the main time series that comprise our database, while the Data Appendix provides greater detail regarding their coverage and sources. The Data Appendix also includes a list of the default episodes studied and the full country coverage, as non-defaulters are an integral part of the analysis, serving as a control or "donor" group. The variables fall into four categories: the dating of the sovereign external debt crisis; the time series that captures the costs of the crisis, output per capita, and various social indicators; the covariates used to construct the synthetic controls in our estimations; and the variables incorporated into the local projections exercise.

### *3.1 Sovereign defaults*

Our data on defaults to private external creditors comes from Farah Yacoub, Graf von Luckner, Reinhart, and Rogoff (2024) and takes the form of an annually assessed categorical variable (1 or 0). It covers 193 sovereigns since 1800. The beginning of a default spell (debt crisis) is marked by a new default, while the end is dated by a debt restructuring that cures the default for a period of at least two years. Debt restructurings that do not cure the default are not considered as "new" defaults. As shown in Graf von Luckner et al. (2023), these interim debt restructurings are quite common, particularly during longer default spells in the modern era.

The definition of a default follows how rating agencies qualify a default, which includes (1) missed payments beyond the grace period; (2) material changes to the contract adversely 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.[7] Farah Yacoub et al. (2024) adopt this definition, including for countries and periods when rating agencies did not exist or did not cover the debtor in question.[8] Compared to available historical sovereign ratings, the default dataset is broader in both the time and country dimensions, covering 299 episodes between 1800 and 2020.

To assess the impacts of sovereign default on social outcomes, we exclude cases from the pool of default episodes that coincide with one of the most important drivers of poverty and social costs: wars (Bianchi and Sosa-Padilla, 2022). Based on data from Sarkees and Wayman (2010) and Correlates of War (COW), we drop any observation where there is an ongoing armed conflict with a total number of war casualties exceeding 500 (over the course of the war), therefore 38 sovereign defaults are dropped from the sample. To apply the synthetic control method to our sample, we

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[7] See Ams et al. (2018) citing Moody's (2018).

[8] In some cases, Farah-Yacoub et. al. (2024) designation of default differs from that of credit agencies, because they incorporate information unavailable to these agencies at the time. In the modern era these discrepancies are more common among unrated sovereigns (typically LICs).

can only match the controls when we have data for 15 years prior to 10 years after, which further reduces the usable sample. Lastly, we exclude defaults whenever a prior default exists within a five-year window to avoid overlapping effects.

Thus, the final baseline number of defaults considered in the study is 221. The sample may be smaller depending on the variable of interest and other methodological constraints. Section 5 highlights the number of defaults considered for each exercise.

### *3.2 Costs of Default*

Real per capita GDP primarily relies on the Maddison Project Database, but numerous country studies also provide data for earlier periods than those covered by the Maddison data. These supplementary sources and their coverage are listed in the Data Appendix. As to variables that capture the social costs aspect, we rely on two "core" health measures: infant mortality and life expectancy. Infant mortality is defined as the number of children who die before reaching their first birthday per 1000 live births. The data comes primarily from the United Nations, but pre-1950 data is taken from Mitchell's *International Historical Statistics* (2013). Life expectancy is measured at birth in years for the total population and relies on the Gapminder and United Nations databases.

Other variables used to capture social costs include the number of households under the poverty line, calorie supply, and energy supply. The poverty count measures the number of households living below a $ 2.50-a-day threshold in 2005 PPP dollars, which comes from the Global Consumption and Income Project (Jayadev and Lahoti, 2016). 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.

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

We restrict the pool from which the synthetic control is constructed to countries structurally similar to the treated unit (the defaulting country). To capture structural similarities, we rely on three variables: geography, real per capita GDP, and Polity V. The Polity Project rates countries based on how democratic they are and is included here as a proxy for institutional quality. The main index produced by the Polity Project scores countries from -10 to +10, where the lowest score is assigned to countries that are strongly autocratic and the highest to countries that are strongly democratic. Compared with other indicators that could be used for the same purpose, these time series stand out in their length and breadth of coverage, allowing us to minimize the loss of data points. By geography, we mean adherence to a world region based on the official UN classification.

### *3.4 Data used as covariates in local projections*

The data on other varieties of crises, including domestic defaults, banking, currency, and inflation crises, are taken from Reinhart and Rogoff (2009) and the subsequent update and expansion of that database (see Graf von Luckner et al. 2024). Moreover, the Local Projections include a binary variable for cross-border wars, defined as described above, using data from Sarkees and Wayman (2010) and the Correlates of War (COW) data set.

## 4. Methodology

Our empirical approach is eclectic and employs complementary methodological approaches. In this section, we describe our two-pronged approach to data analysis.

### *4.1 Panel synthetic control method (SCM)*

To unambiguously identify the effect of sovereign default on socioeconomic outcomes, one would have to know what would have happened without the default. Absent the unobservable counterfactual, we apply a synthetic control method (Abadie and Gardeazabal, 2003) to attempt to construct a plausible counterfactual.

There are some important advantages to using this methodology relative to the popular panel data fixed effects approach. As is shown by Abadie, Diamond, and Heinmueller (2010), conditional on a good, time-consistent fit pre-treatment, the difference between the treatment unit and synthetic control post-treatment represents an unbiased estimation of the treatment effect unaffected by omitted variable bias. 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. Reducing that risk may require many covariates—a tall order for a study that aims to assess nearly two centuries of sovereign defaults. Also, the synthetic control method is especially apt to study time-varying effects and their statistical significance over time, which is well-suited to our task.

By combining the underlying strategies of difference-in-difference and matching methods, SCM systematically constructs a weighted combination of observations that minimize 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. 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.

Formally, the methodology assumes that in a panel of $j = 1, 2, \ldots, J + 1$ 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. The SCM thus aims to estimate the vector $(\delta_{i,T_0}, \ldots, \delta_{i,T})$, overcoming the absence of the counterfactual $Y_{it}(0)$. The estimation strategy thereby assumes a general model for the potential outcomes of all units, which depends on common factors, unit-specific characteristics, and transitory shocks. To construct the synthetic control unit, the SCM chooses a weight vector, $W = (w_1, \ldots, w_{J_{Controls}})'$ such that that $w_j \geq 0$ and $\sum w_j = 1$. The optimal weights w* are selected to minimize the difference between the pre-treatment characteristics of the treated unit and the aggregated synthetic control unit. Abadie et al. (2003) show that when the weights $w_j^*$ are chosen such that the root-mean-squared-prediction error of the outcome variable and observed covariates for the pre-treatment period is sufficiently close to zero, then

$$\widehat{\delta_{it}} = Y_{it} - \sum_{j=1}^{J_{Controls}} w_j^* Y_{jt}$$

represents an unbiased estimator of $\delta_{it}$. For proofs and further details on the SCM, we refer the reader to Abadie et al. (2021) and the sources cited therein.

To limit the risk of 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 three variables: United Nations region (Americas, Europe, Africa, Asia, and Oceania); economic development (proxied by GDP per capita) and institutional capacity, (proxied by the Polity II index). To be included in the "donor pool" (control group) for a given default episode, the potential donor country has to be from the same region and not lie further than 50 percentiles away from the defaulting country on the distribution of Polity II and GDP per capita. In other words, the median country can be matched with any country. However, we impose plausible restrictions; for example, advanced economies will not serve as a counterfactual to emerging and developing economies. [9] Finally, prior to 1950, the donor pool was restricted by the existence of fewer sovereigns (only 80 countries were independent by 1950) and by the relative scarcity of data on covariates. Hence, we apply only the same region restriction for the earlier part of the sample.

Unlike in the baseline methodology, in our analyses we face many countries, treated at different times. The subscript *i* henceforth denotes default-episodes, rather than countries. For each default episode (made up by a pair of a country and default year), we find the synthetic control, $k$, as described above, the outcomes of the treated unit and its synthetic control are respectively denoted $Y_i$ and $Y_k^i$.

To ***aggregate*** across, *I*, individual default event studies, we rely on the aggregation method introduced by Cavallo et al. (2013) while weighting the individual results by their pre-treatment fit, similar to what was first proposed by Acemoglu et al. (2016). The weighting is necessary to reduce the impact of cases where no suitable synthetic control exists and the control study would have little or no informational value. Such cases are identifiable by their poor pre-treatment fit. Yet, they can have significant post-treatment effects. We assign weights, $\omega_i$, equal to the inverse of the normalized pre-treatment root-mean-squared prediction error to each synthetic control estimation, *i*. To illustrate, the weighted average treatment effect, at time *t*, $\tau_t$, is :

$$\tau_t = \sum_{i=1}^{I} \omega_i (Y_{i,t} - Y_{k,t}^i) ,$$

with $\omega_i = \frac{1}{\sigma_i}$ ,

where the normalized root mean squared error, $\sigma,$ is:

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

[9] Given that we also use GDP per capita to match the pre-treatment trend, the GDP 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.

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 ∈ {T, …, $T_{0-1}$} : T< $T_0$}. Instances, where the synthetic control offers a poor fit, are hence assigned little weight in the aggregation. [10]

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, $p_t$,

$$p_t = \frac{\sum_{n=1}^{N} \theta_{n,t}}{N} \quad with\ \theta_{n,t} = \begin{cases} 1 & if\ |\tau_{t\,Default}| \leq |\tau_{n,t\,Pseudo-default}| \\ 0 & otherwise \end{cases}$$

Where τ is the difference between the $\omega_i$ -weighted aggregate of (pseudo-) treatments and their synthetic controls; or the (pseudo-) treatment effect. To further account for the smoothing effect from aggregation across many individual case studies, we run N = 100 iterations, each aggregating *I* pseudo-default event studies, leveraging randomly selected country-year pairs. For each aggregated normalized study, we match the number of pseudo defaults per permutation, *I*, to the number of defaults considered in the analysis of the actual defaults.

***Anticipation and SCM:*** Anticipation can lead to bias or even reverse causality. If sovereign default was caused by the anticipation of future economic downturns (as suggested by Levy-Yeyati and Panizza, 2011), there could be reverse causality at play. This is an empirical question, and evidence of anticipation is (at best) mixed. Economic downturns are routinely perceived to be transitory by debtor governments as well as multilateral institutions. This "optimism bias" is reflected in chronic forecast errors that systematically overestimate growth and fiscal adjustment and underestimate debt accumulation and debt stocks (see International Monetary Fund, 2017).

[10] To guard against overinflating the impact of individual event studies with miniscule *NRMSE, σ,* and large treatment effect, 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%.

Notwithstanding the track record of official economic projections, anticipated defaults should not be ruled out, as mounting risks impact domestic investment, international capital flows, and economic activity. As far as this type of anticipation exists, it should be included in the cost of sovereign default.

To account for the possibility of anticipation of default, a critical adjustment of the SCM is to backdate the matching algorithm pre-intervention to a moment before the default could have been potentially anticipated (Abadie, 2021). We apply the backdated matching approach and confirm that anticipation seems to be at play in some instances (as is indicated by diverging trends right before default in faster-moving variables, such as GDP per capita), though even in these cases, it is the actual occurrence of default that drives the bulk of the effects measured.

### *4.2 Local Projections (LP)*

There are several reasons why the LP method is useful in our analysis: Similar to the SCM, local projections allow for the estimation of potentially nonlinear, asymmetric, and time-varying effects of sovereign default. Unlike the SCM, it relies on the within-country variation over time around the time of default and is therefore not affected by the potential lack of suitable control units. Moreover, LPs allow for explicitly controlling potential omitted variables (e.g., banking crisis coinciding with defaults and/or default in the recent past). LP results that confirm the findings of the SCM can hence soothe concerns about inter- and extrapolation biases that might affect the SCM. In turn, the LP approach's key weaknesses are better addressed by the SCM: both the possible omitted variable bias and dynamic endogeneity bias in LP can affect results. The SCM can provide a robustness check for LP results by controlling for unobserved confounders and at least mitigating dynamic endogeneity bias, especially when there is a good fit in the synthetic counterfactual.

To complement the SCM analysis, we hence employ a local projections approach (Jordà, 2005). Given the tendency for different varieties of crises to occur concurrently (e.g., banking or currency crises accompanying a default), they present us with a method to control for potential confounding variables. LP offers a flexible and tractable alternative approach to estimating impulse response functions. They rely on the estimation of a series of regressions for a set of time horizons (h) following the occurrence of an event (in our case, a sovereign default). The regression model for each horizon h can be expressed as:

$$Y_{i,t+h} = \alpha_{i,h} + \beta_h D_{i,t} + \theta_h X_{i,t} + \epsilon_{i,t+h}$$

where $Y_{i,t+h}$ is the outcome variable of interest for country *i* at time $t + h$, $D_{i,t}$ is a binary variable indicating the occurrence of a sovereign default in country *i* at time t, $X_{i,t}$ is a vector of control variables, $\alpha_{i,h}$ are country fixed effects, and $\epsilon_{i,t+h}$ is the error term. The coefficient $\beta_h$ captures the effect of a sovereign default on the outcome variable at horizon $h$.

By running separate regressions for each horizon $h$, local projections allow for flexible estimation of the dynamic effects of sovereign defaults over time without imposing the restrictive assumptions of traditional VAR models, such as the linearity and symmetry of responses.

To implement the LP method within a time window similar to that of the SCM, we analyze h=10 leads. In the baseline setup with the largest sample size of the crisis analyzed, the vector of controls, *X,* includes five lags of the default variable, as well as three lags of the outcome variable as controls. In a second specification, we also control for currency-, inflation- and banking crises (Reinhart and Rogoff, 2009), and wars with three lags of each. Naturally, this significantly reduces our sample.

Comparing results between the SCM and the LP approaches is useful primarily as a robustness check and to compare the order of magnitude in the results. Differences in the methodology and the sample (for instance, whereas the local projections approach can control for wars in the sample, in the SCM, we need to drop these cases) limit the insightfulness of a one-to-one comparison.

## 5. Findings: Sovereign Default and its Economic and Social Costs

Our default database spans 1800-2020 and covers up to 193 sovereigns by the close of the sample, which yields a total of 221 default episodes on debt owed to private creditors. In the analysis summarized in this section, the number of default episodes included varies considerably across indicators and is determined by data availability, as there exists significant disparity in the starting dates across the indicators considered (see Data Appendix). The default episode count also varies across estimation strategies, as some exercises have more demanding data requirements. For example, our application of the Synthetic Control Method requires data for the indicators over a longer time frame prior to the default crisis.

### *5.1 Real per capita GDP*

Based on the application of SCM to a sample of 135 sovereign defaults on external private creditors since 1815, we find that, on average, sovereign default leads to a cumulative negative gap in real per capita GDP of 8.5 percent relative to the non-default control group after three years in default (Figure 1, top panel).[11] While GDP declines relative to the control group in the year prior to the default, about 4/5ths of the cumulative output loss occurs during and after the default. On average, positive GDP growth returns in the second year.

As to the medium- to long-term effects, over the decade following default, the average annual growth rate is one percentage point below that of the non-default group.[12] The widening gap is evident in Figure 1 (top panel), suggesting a structural shift in growth dynamics in the aftermath of default. Comparing studies with different samples, empirical approaches, and time horizons is complicated, as discussed in Section 2; with this caveat in mind, our results differ from, for example, Esteves, Lennard, and Kenny (2021), who find a return to trend after five years. However, our findings are broadly in line with Furceri and Zdzienicka (2012), who find that eight years after the occurrence of a debt crisis, output contracts by about 10 percent (compared to the country-specific output trend).

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[11] The 135 defaults include all cases for which we have GDP data, a match is available, and do not coincide with armed conflicts.

[12] Despite the information-based weights, these results are not dependent on the weighting scheme. As is shown in the section on robustness checks, after filtering results by a NRMSE threshold, the unweighted aggregate delivers a similar outcome.

The empirical literature has identified potential channels that may explain this pernicious dynamic. Evidence of brain drain following default (Garcia Zea, 2020; Theodoropoulos et al., 2014); the collapse of infrastructure investment during default (Kaminsky and Pereira, 1996); the contraction of domestic credit (Sandleris, 2008) and declining access to capital as the probability of default rises (Hebert and Schreger, 2017) figure prominently as plausible candidates. The channels may be even more potent for more protracted debt crises, as discussed below.

Figure 1. Sovereign default and real per capita GDP: Based on 135 defaults, 1828-2020

Figure 1.1. Sovereign default and real per capita GDP: Aggregated SCM results

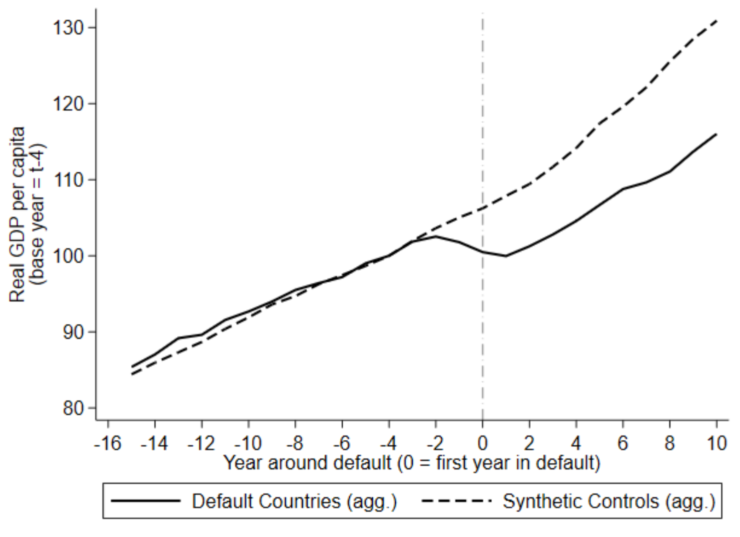



Figure 1.2. Sovereign default and real per capita GDP: Pseudo-treatment bootstrap test

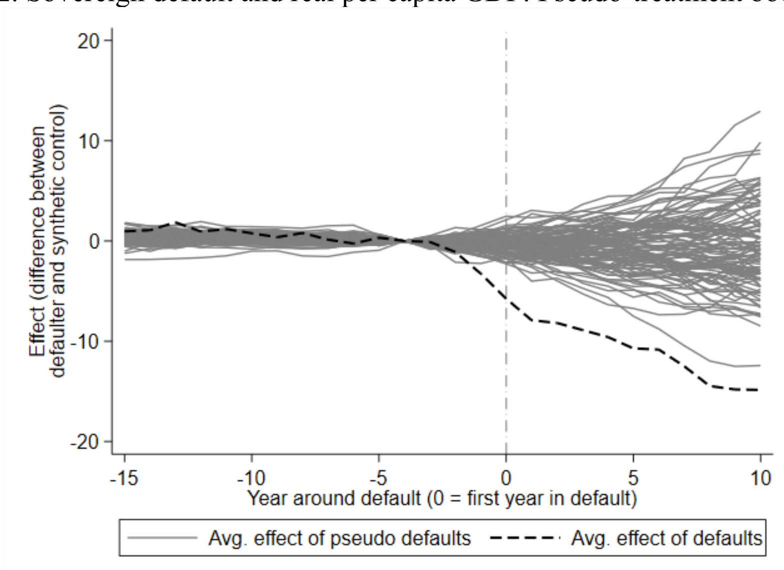



Figure 1.3. Sovereign default and real per capita GDP: Bootstrapped p-values

| **Period** | **t** | **t+1** | **t+2** | **t+3** | **t+4** | **t+5** | **t+6** | **t+7** | **t+8** | **t+9** | **t+10** |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **Pseudo p-value** | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |

Notes: Episodes are included based on data availability, no coinciding armed conflict, and subject to the availability of a match from the control group. Sources: See Data Appendix for sources on real per capita GDP, authors' calculations

The lower two figures (1.2 and 1.3) present the results of a significance test, where the null hypothesis is no difference between defaulters and non-defaulters. The exercise consists of randomly assigning defaults while returning the defaulting countries to the donor pool and observing whether the results from our original exercise are significantly different. Figure 1.2 presents the effect (black) in contrast to placebo average paths (gray). We ran 100 simulations, each one aggregating the weighted effects of 221 randomly assigned pseudo-defaults.[13] The number of pseudo-treated paths whose effects exceed the magnitude of the measured effect of default thus represents bootstrapped p-values, which are shown for all ten post-default years in Figure 1.3.

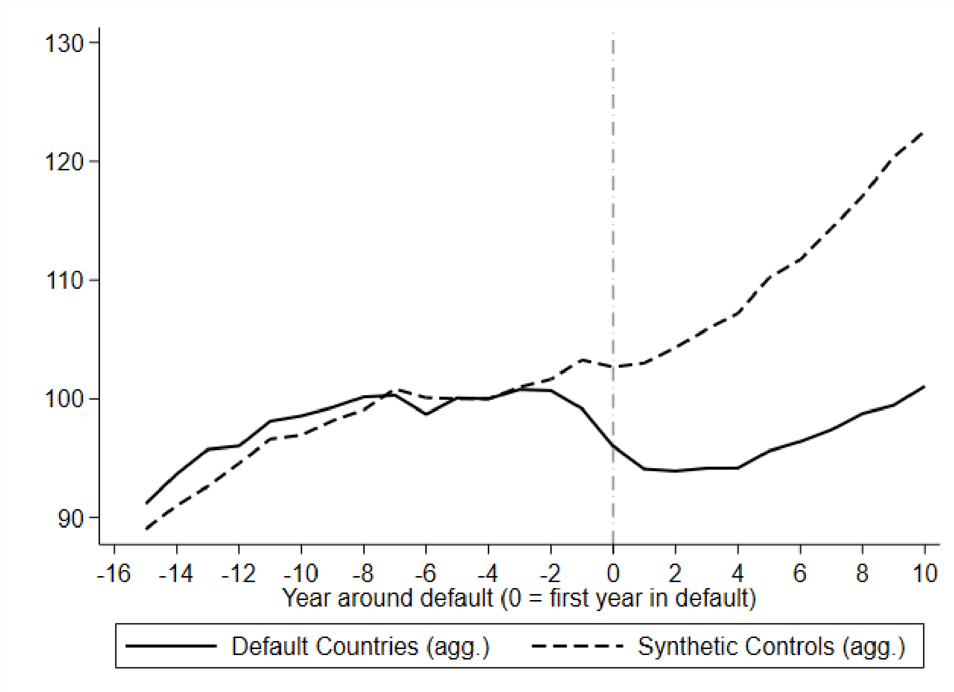
***Restricting the sample to compare with the existing literature:***

The average output decline depends on the sample period and country coverage. To facilitate comparisons, we apply a restricted sample tailored to match that used in a particular study. Borenzstein and Panizza (2009) studied 76 defaults between 1972 and 2000 and used a cross-country panel regression of GDP growth on default and relevant covariates, The authors find that default reduces annual GDP growth by 1.2 percentage points on average. Applying the SCM using the restricted sample, we find a growth differential of 1.3 percentage points per annum on average. Indeed, defaults over this period appear to have been particularly costly in terms of output losses: while the average economic contraction (measured from pre-default peak to post-default trough) was 2.5 percent across our whole sample since 1815, the comparable calculation amounts to a staggering 7.5 percent for the 1972 to 2000 subsample.[14] The lost decade of the 1980s was aptly named.

[13] The number of "pseudo defaults" per simulation is set equal to the total number of defaults in the sample, prior to excluding default episodes because of lack of data.

[14] 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.

Figure 2. Sovereign default and real per capita GDP: Subset of cases in the focus of existing literature 1976-2000 (n=63 defaults)




Sources: See the Data Appendix for sources on real per capita GDP and authors' calculations.

***Evaluating the impact of default duration on output:***

So far, we have treated all defaults as if they were homogenous. However, sovereign defaults differ markedly from one another (Asonuma and Trebesch, 2016). Cote d'Ivoire's default spell started in 1993 and remained unresolved until 2012; in the meantime, the country remained excluded from international capital markets. Others, like Uruguay in 2003, are triggered by a preemptive restructuring in anticipation of debt distress and are often short-lived. Some default episodes involve multiple failed debt restructurings before the default is cured (Graf von Luckner et al., 2021; Graf von Luckner et al., 2023). Regardless of the reason for the delay in exiting from default, these episodes could have very different impacts on economic output (Trebesch and Zabel, 2017; Benjamin and Wright, 2018).

The median default duration is six years for the full sample. When only considering defaults with durations above the median, we find defaults to be much more costly in terms of output loss. Over the decade post-default, "long" defaults have annualized growth rates 2.7 percentage points lower than the non-default counterfactual. Even after ten years, these "long" cases, on average, do not reach their pre-crisis per capita GDP level (Figure 3.2), with a per capita output level 20 percent below that of the synthetic control.

Figure 3. Sovereign default and real per capita GDP: The costs of "short" versus "long" default spells

Figure 3.1. "Short" defaults, 1828-2020 (n=62)

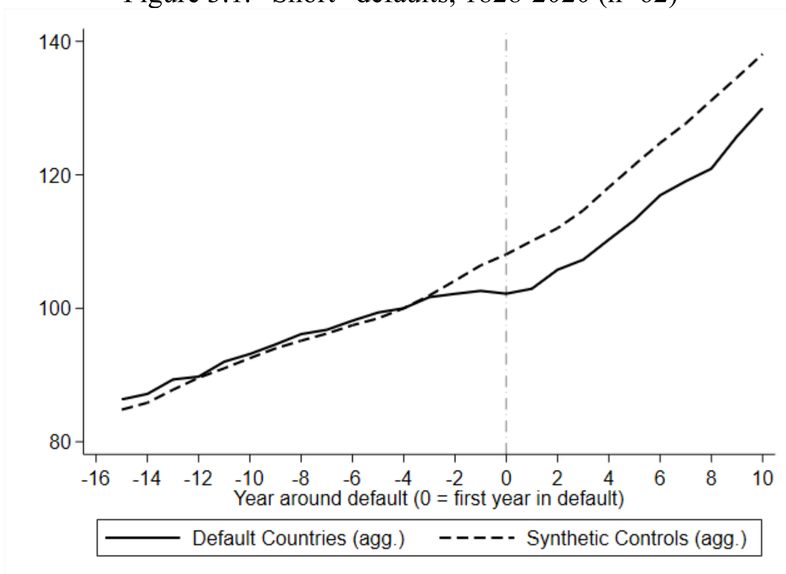
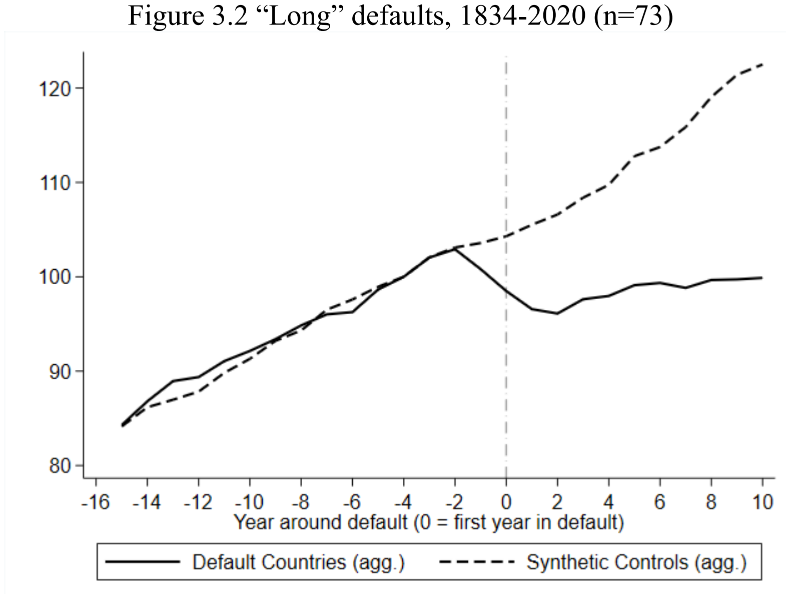



Figure 3.2 "Long" defaults, 1834-2020 (n=73)




Note: Figure 3.1 is a subsample of 62 cases with a default duration less than or equal to the full sample median of 6 years. These are dubbed "short" spells. Figure 3.2 is based on 73 cases where the duration of the default spell lasted more than six years. The median duration is calculated for the full sample (221 defaults); this explains why, for the subsample shown above, 46 percent of the observations lie below the median and 54 above. Episodes are included based on data availability, no coinciding armed conflict, and subject to the availability of a match from the control group. Sources: See Data Appendix for sources on real per capita GDP, authors' calculations

### *5.2 Social indicators*

Our results indicate that sovereign default leads to severe growth declines around default and a secular decline in growth, relative to the counterfactual, over the longer horizon. Dollar and Kraay (2002) present compelling evidence that growth is essential for poverty reduction. Their analysis suggests that the income of the poor rises (falls) equiproportionately with average incomes. Taken together, these observations lead to the conclusion that sovereign default can be expected to trigger a rise in poverty rates, if only due to the impacts on real per capita GDP growth.

Evidence from recent cases of default, where more indicators of welfare are available, lends support to this view. A survey study by Venezuela's leading universities and NGOs found that 64 percent of Venezuelans had lost an average of 11 kilograms in 2017, the year of their default.[15] Nearly 90 percent of respondents also reported that their household income was not sufficient to cover the purchase of food. In the wake of Argentina's default of late 2001, the poverty headcount ratio based on the international poverty line jumped from 38 percent to 53 percent.[16] Lebanon defaulted in 2020, and according to the United Nations, the proportion of people living under the international poverty line nearly tripled between 2019 and 2020, rising from 8 to 23 percent. Beyond the figures, these crisis episodes involve rolling blackouts along with shortages of crucial medical supplies. These outcomes are closely linked to the macroeconomic crisis, which extends well beyond the boundary of a sovereign default. In Argentina, Lebanon, and Venezuela, sovereign default was accompanied by a collapsing currency, a systemic banking crisis, and surging inflation—in Venezuela's case, hyperinflation.

Beyond real per capita GDP, we shift our focus to two "core" health indicators, infant mortality and life expectancy, for which comparatively long time series are available. There are compelling reasons to analyze these indicators. First, the public health literature suggests significant links between poverty and mortality for both infants and adults (Canudas-Romo, 2018). Nishiyama (2011) finds that economic contractions negatively and disproportionately affect infant mortality, with growth collapses leading to a significant deterioration. Second, even within advanced economies countries, significant income-driven heterogeneity in both measures has been documented. In the United States, for example, a 14-year gap in life expectancy has been reported

[15] Freitez, 2018. 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.
[16] World Development Report 2022, Chapter 5, Box 5.4

between the 99th and 1st percentile of the income distribution (Chetty et al., 2016). Third, Wilkinson (1992) finds that the relationship between incomes and life expectancy is nonlinear, with the most significant changes concentrated in moving from low- to middle-income and vice-versa, suggesting that a deterioration in observed outcomes for life expectancy is more likely related to households falling into poverty.

Taken in concert, these factors suggest that adverse outcomes for these two variables can be interpreted as regressive. In other words, even for an equiproportional shock to income, the deprivations related to mortality variables are experienced more acutely by the lowest tranche of the income distribution.

***Infant mortality*** SCM results are shown in Figure 4. The first observation is the lack of evidence of an outright reversal in the aggregate, with infant mortality decreasing for the default group. This is in contrast to the sharp reversals evident in real per capita GDP. There are, however, several individual default episodes (Argentina 2001, Bulgaria 1990, Chile 1931, among others) where mortality rates increase post-default. Though defaulters mostly keep with the broader downward trend in infant mortality rates of the past two centuries, the pace of improvement (decline in this case) is significantly less marked than for the control group. The gap widens over time, and the differences are statistically significant. The results reveal an additional 5.4 infant deaths per 1000 live births in the defaulting country, relative to the counterfactual, by the ten-year mark post-default.

Infant mortality is linked to various privations, some of which are proxied in this study (nutrition) and others for which we do not have adequate data (pre-natal care). Studies on Mexico's debt crisis in the 1980s (Frank and Finch, 2004 and Bronfman, 1992) have shown that while for some parts of society, infant mortality kept declining during the crisis, the parents' income became a crucial determinant of a newborn's chance of survival.

Figure 4. Sovereign default and infant mortality: Based on 104 default episodes, 1928-2020

Figure 4.1. Sovereign default and infant mortality: Aggregated SCM results

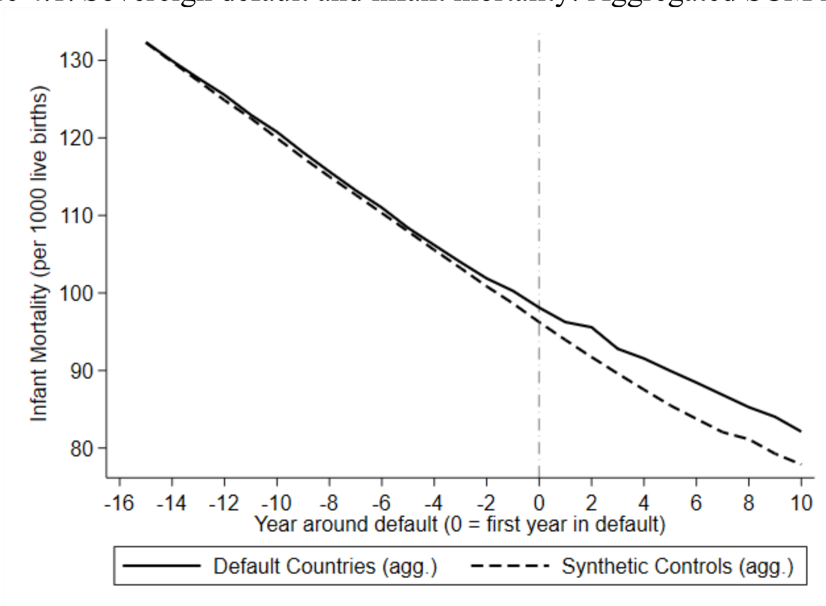



Figure 4.2. Sovereign default and infant mortality: Pseudo-treatment bootstrap test

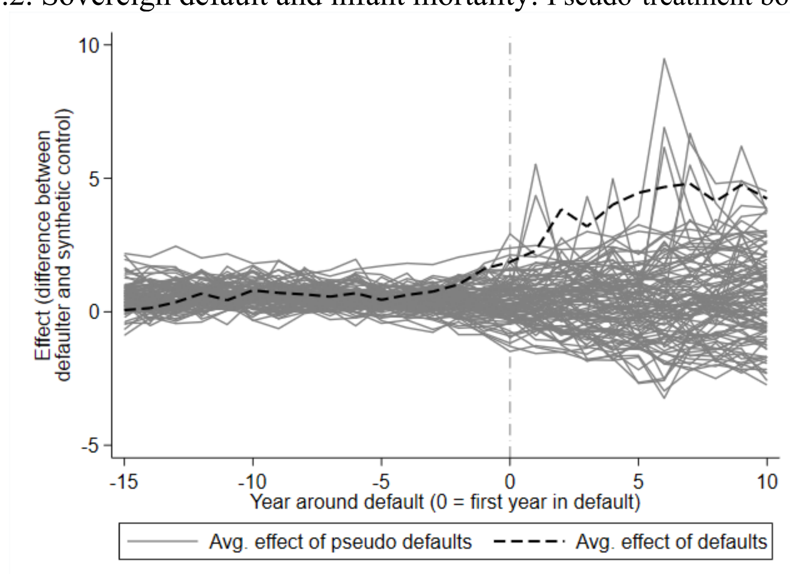



Figure 4.3. Sovereign default and infant mortality: Bootstrapped p-values

| **Period** | **t** | **t+1** | **t+2** | **t+3** | **t+4** | **t+5** | **t+6** | **t+7** | **t+8** | **t+9** | **t+10** |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **Pseudo p-value** | 0.04 | 0.03 | 0.00 | 0.00 | 0.01 | 0.01 | 0.00 | 0.03 | 0.03 | 0.03 | 0.01 |

*Notes*: Episodes are included based on data availability, no coinciding armed conflict, and subject to the availability of a match from the control group. Source: Mitchell (2013), United Nations, and Authors' Calculations.

***Life expectancy at birth,*** unlike infant mortality (Figure 4, top panel), reveals evidence of mild reversals (in the aggregate), as there are declines in years four and six post-default. Nonetheless, like child mortality, the overall trend remains one of improvement, albeit at a slower pace than the non-default control group. Over the post-default decade, the gap between defaulters and the non-default control group widens, reaching 1.1 years. While statistically significant at the 10 percent level of confidence, the results are not as robust as those for real GDP and child mortality.

As shown in Figure 5, defaults impact life expectancy primarily by reducing the rate of improvement post-default, but in extreme cases, outright reversal does occur. The defaults in Mexico in 1928, Peru in 1983, and Russia in 1998 are among several where declines in life expectancy were recorded.

Figure 5. Sovereign default and life expectancy at birth: Based on 127 default episodes, 1834-2020

Figure 5.1. Sovereign default and life expectancy: Aggregated SCM results

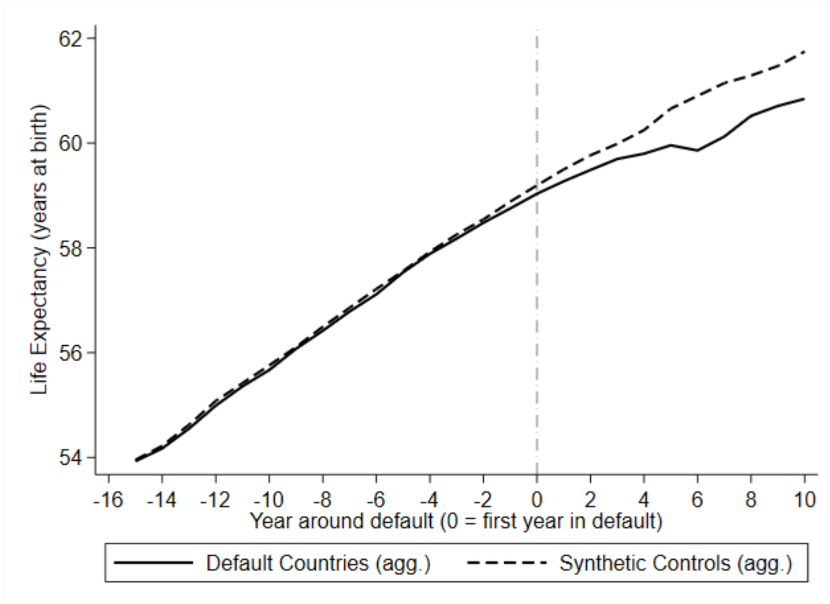



Figure 5.2. Sovereign default and life expectancy: Pseudo-treatment bootstrap test

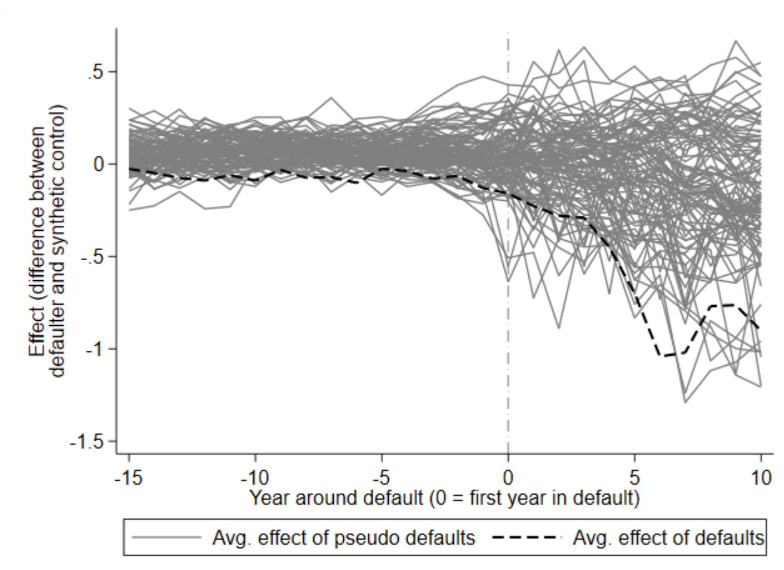



Figure 5.3. Sovereign default and life expectancy: Bootstrapped p-values

| **Period** | **t** | **t+1** | **t+2** | **t+3** | **t+4** | **t+5** | **t+6** | **t+7** | **t+8** | **t+9** | **t+10** |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **Pseudo p-value** | 0.11 | 0.11 | 0.09 | 0.13 | 0.03 | 0.03 | 0.00 | 0.02 | 0.05 | 0.06 | 0.06 |

*Notes*: Episodes are included based on data availability, no coinciding armed conflict, and subject to the availability of a match from the control group. *Sources:* Gapminder, United Nations, and authors' calculations

The ***poverty count*** is the number of households living on less than $2.50 a day at 2005 purchasing power-adjusted prices (PPP). Unfortunately, much of the traditional data on poverty headcount *ratios*, such as the World Bank's PIP (formerly Povcalnet), is only available starting in 1981. The GCIP offers a more comprehensive dataset of income- and consumption-based poverty measures drawn largely from surveys since 1960. GCIP's data presents other advantages for cross-country comparability. First, it maintains consistency in the welfare concept (income or consumption). Second, whenever it interpolates in the absence of surveys, it uses a time-weighted average of the consumption or income profiles in the two nearest survey years and then computes the means for each quantile. Other sources, such as the World Bank's PIP, apply the growth rate in private consumption per capita from national accounts to the survey mean income and then assume the profile to be the same as in the closest survey.

The SCM results are shown in Figure 6. The number of households living under the $ 2.50-a-day line appears to be on an upward trend for both groups. This is likely due to the long-run trend in population growth, which averaged 1.6 percent between 1960 and 2020. The gap in poverty counts between the defaulters and controls begins to widen moderately two years prior to default. In the years that follow a default, there is a surge in the number of households falling below the poverty line for defaulters. The gap widens by about two percentage points, its fastest increase, the year after default, and reaches its maximum by year nine at about 10 percent more households living in poverty for defaulting countries. The results for the years in which the gap is widest (*t+6* through *t+9*) appear to be significant at the 10 and 5 percent confidence levels. However, we caution against interpreting these results too confidently. We present a similar set of results using a $4.16 per day threshold in the appendix.

Figure 6. Sovereign default and poverty totals: Based on 78 default episodes 1975-2020

Figure 6.1. Sovereign default and poverty totals: Aggregated SCM results

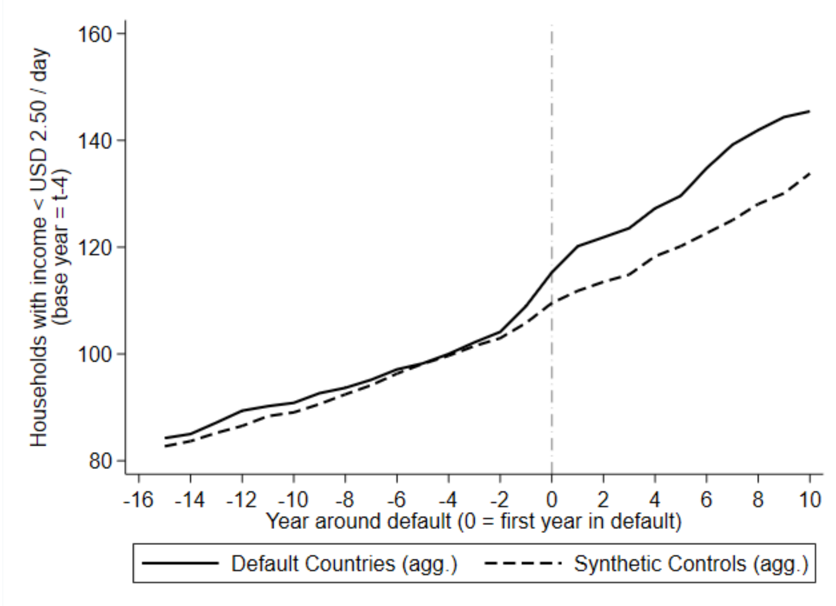



Figure 6.2. Sovereign default and poverty totals: Pseudo-treatment bootstrap test

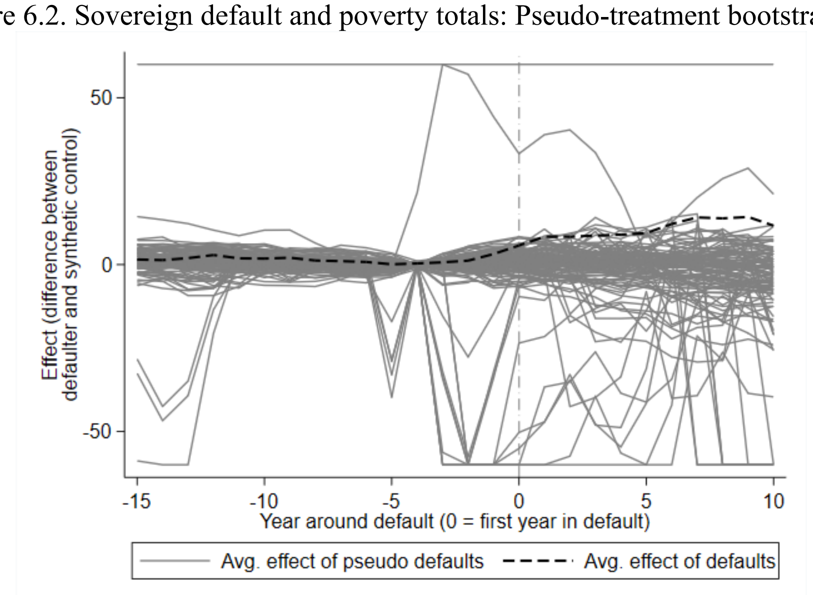



Figure 6.2. Sovereign default and poverty totals: Bootstrapped p-values

| **Period** | **t** | **t+1** | **t+2** | **t+3** | **t+4** | **t+5** | **t+6** | **t+7** | **t+8** | **t+9** | **t+10** |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **Pseudo p-value** | 0.06 | 0.03 | 0.03 | 0.07 | 0.04 | 0.03 | 0.03 | 0.02 | 0.01 | 0.01 | 0.02 |

Notes: The poverty count at $2.50 a day is the number of households living on less than $2.50 a day at 2005 purchasing power adjusted prices (PPP). Episodes are included based on data availability, no coinciding armed conflict, and subject to the availability of a match from the control group. Sources: Global Consumption and Income Project (GCIP) and authors' calculations.

***Caveats about poverty data***: Poverty headcounts are informative about welfare, but there are caveats. For instance, a decline in income for a person already below the poverty line has no effect on the indicator. The most important caveat, however, is that the dynamics of poverty headcounts are importantly driven by changes in real per capita GDP wherever interpolation is used due to the scarcity of household survey-based poverty data. Surveys are infrequent in the best of times, and crises may delay these further. Thus, the need for interpolation (oftentimes for many consecutive years) importantly erodes the independent information content of these data. As such, we cannot decisively estimate the degree to which these increases in poverty associated with default are more regressive than a poverty increase associated with any other output collapse due to the lack of accurate observed survey data. GCIP's methodology is potentially better than that of other data sources in accounting for this source of bias, but it is not immune to the same line of criticism.

General dissatisfaction with a strictly "monetary" definition of poverty motivated the development of multi-dimensional poverty indicators in recent years. This line of inquiry seeks to understand not just whether a household is poor but how poor and in which ways. Some of the better-known multi-dimensional poverty indices aggregate variables related to healthcare access (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, these data are not yet available for long periods of time. For example, one of the most widely used indices, Oxford's MPI, only started in 2010, and observations are not available every year. As such, these are excluded from our analysis.

***Aggregate daily calorie supply per capita.*** We proxy access to nutrition with the Food and Agriculture Organization's aggregate calorie daily supply per capita, which starts in 1961. Over the 1961-2020 period, global average daily calorie availability has increased steadily from roughly 2,200 per day to about 2,850. The global trend, however, is silent on the significant differences across countries, regions, and income groups. As we shall show, the incidence of sovereign default may help explain some of the observed cross-country variation.

As with other indicators, we apply the SCM to these data. As shown in Figure 7, the series was indexed to 100 in year four prior to default, facilitating the comparison between defaulting countries (73 episodes) and the control group. The contrast between the two groups is striking both around the default crisis as well as over the longer post-default horizon. Calorie supply peaks two years before the default and contracts for five consecutive years. The peak-to-trough contraction is relatively moderate at 1.2 percent. The next six years are best categorized as an anemic and halting recovery. A decade after the default, the country's per capita caloric supply stood about where it was prior to default. The gap with counterfactuals in year ten is 4 percent. The differences across the two groups are consistently significant, as shown in Figure 7.3. As discussed in our literature review, the relationship between nutrition and health outcomes is well-established. The evidence presented in this section appears to corroborate that relationship.

The reasons why this indicator of nutrition contracts around the default crisis is beyond the scope of this paper, and it is unlikely that a single explanation carries weight in all episodes, considering the heterogeneity in the pool of defaulters. However, a common thread across many debt crises is the urgent need to close the "twin deficits" (fiscal and current accounts) quickly. Closure of the current account deficit, as external financing dries up, is often achieved through an implosion in imports. Food accounts for an important share of total imports in many countries. In 2020, the year Lebanon defaulted, the volume of imports fell by almost 51 percent, while Sri Lanka's 2020 default is marked by a 22.4 percent contraction in income. These are not exceptions.

Figure 7. Sovereign default and calorie supply: Based on 73 defaults, 1961-2020

Figure 7.1. Sovereign default and calorie supply: Aggregated SCM results

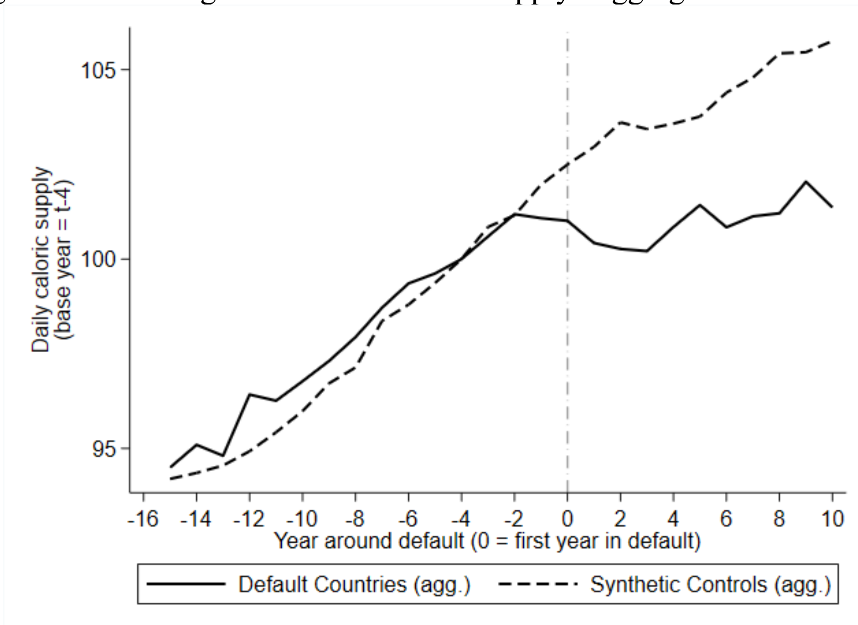



Figure 7.2. Sovereign default and calorie supply: Pseudo-treatment bootstrap test

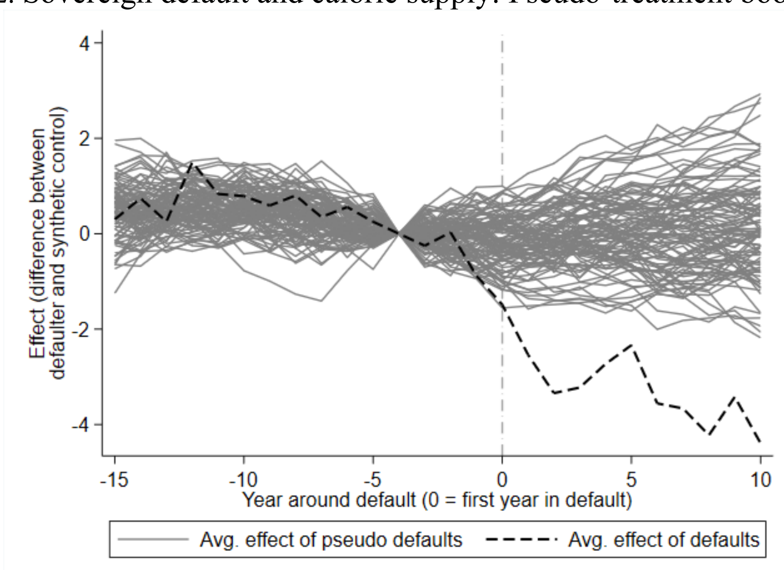



Figure 7.3. Sovereign default and calorie supply: Bootstrapped p-values

| **Period** | **t** | **t+1** | **t+2** | **t+3** | **t+4** | **t+5** | **t+6** | **t+7** | **t+8** | **t+9** | **t+10** |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **Pseudo p-value** | 0.01 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |

Notes*:* Episodes are included based on data availability and no coinciding armed conflict.
Sources: Food and Agriculture Organization (FAO) and authors' calculations.

*Robustness checks*

We now present a set of robustness checks. The first three aim to evaluate the potential for bias in our results stemming from our particular application of the SCM: Namely, (i) whether our weighing scheme drives the results, (ii) whether the donor pool should be restricted on debt-to-GDP ratios and region, as opposed to our baseline three variables; and (iii) whether the randomness inherent to the synthetic control's construction gives rise to additional uncertainty for our estimates. The fourth, the local projections method, addresses the potential for omitted variable bias stemming from concurrence between defaults and other types of macroeconomic crises, such as banking or currency crises.

*Five percent normalized root-mean-squared prediction error (NRMSE) threshold*

The first concern with our use of the SCM is rooted in our weighing methodology. Instead of comparing the treated country's path to a weighted counterfactual using the inverse of the NRMSE measure, we give equal weight to all donors that achieve at least 5 percent NRMSE. As is evident from the panel below, the results remain stable relative to our original approach.

Figure 8. Aggregated SCM results using a 5% normalized RMSE error for the pre-default period

Figure 8.1. Real per capita GDP (n=37 defaults)

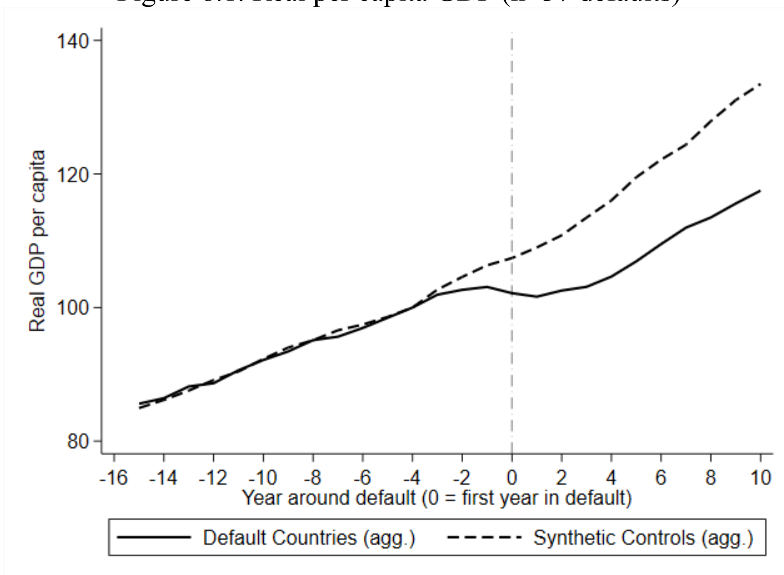



Figure 8.2. Life expectancy at birth (n = 106 defaults)

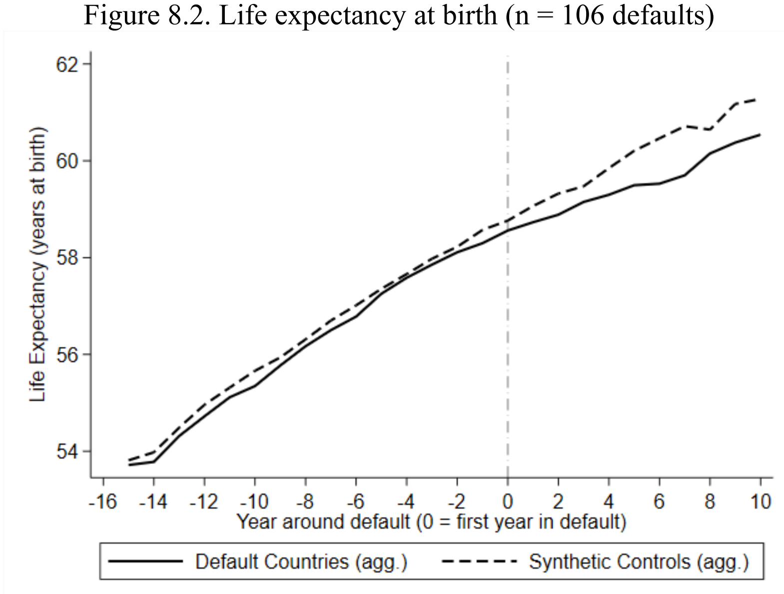


Figure 8.3. Infant mortality (n=43 defaults)

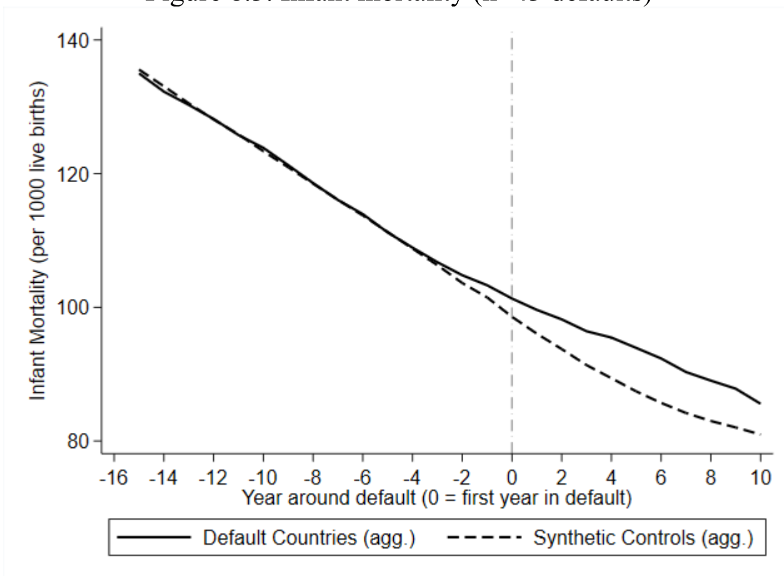



Figure 8.4. Poverty headcounts (n=43 defaults)

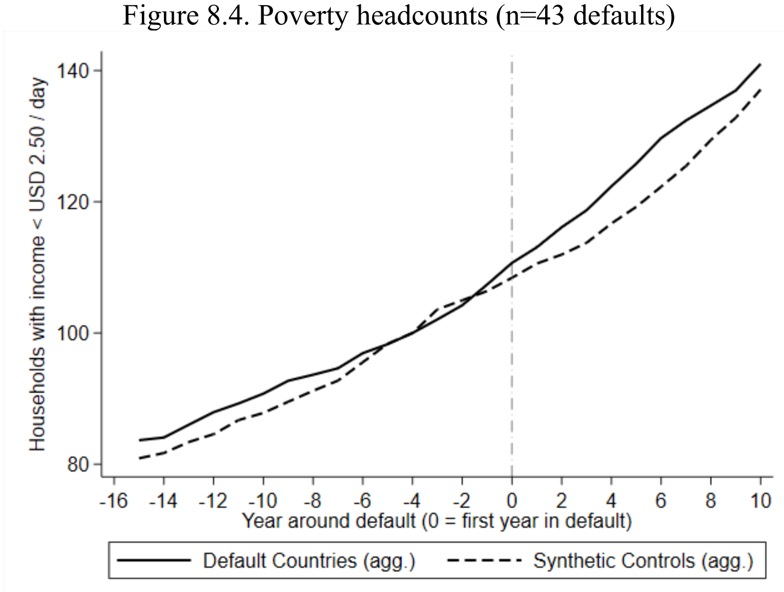
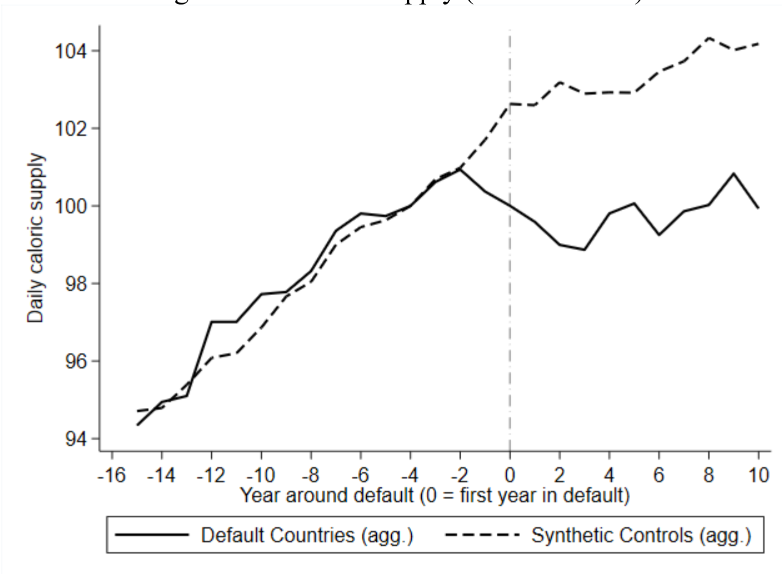


Figure 8.5. Calorie supply (n=43 defaults)




*Restricting Donor Pool with Debt/GDP*

The baseline specification restricts the donor pool of countries using the region, GDP per capita level, and institutional strength to arrive at synthetic controls that are structurally similar to the treated country. One objection to this approach could be that a more informative donor pool would consider the level of sovereign indebtedness because a debt overhang can be a drag on economic growth. We do not include this in our baseline specification because debt stocks risk being correlated with the treatment, which could, in turn, bias results. Here, we alter the donor pool constraints to include it as a robustness check, requiring the donor pool to include countries from the same UN world region with a debt-to-GDP within one interquartile range of the treated case. Though the number of defaults covered is smaller, driven by data availability, the trends found in our baseline do not disappear under this specification, indicating that debt as a drag to growth and social development is unlikely to be the driver of our findings.

Figure 9. Aggregated SCM results limiting the donor pool by region and debt-to-GDP levels

Figure 9.1. Real per capita GDP (n=84 defaults since 1890)

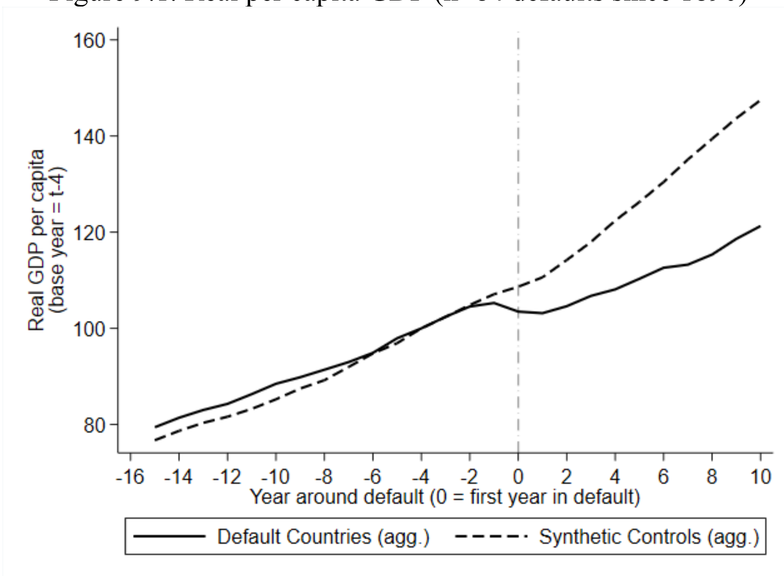



Figure 9.2. Life expectancy at birth (n = 84 defaults since 1890)

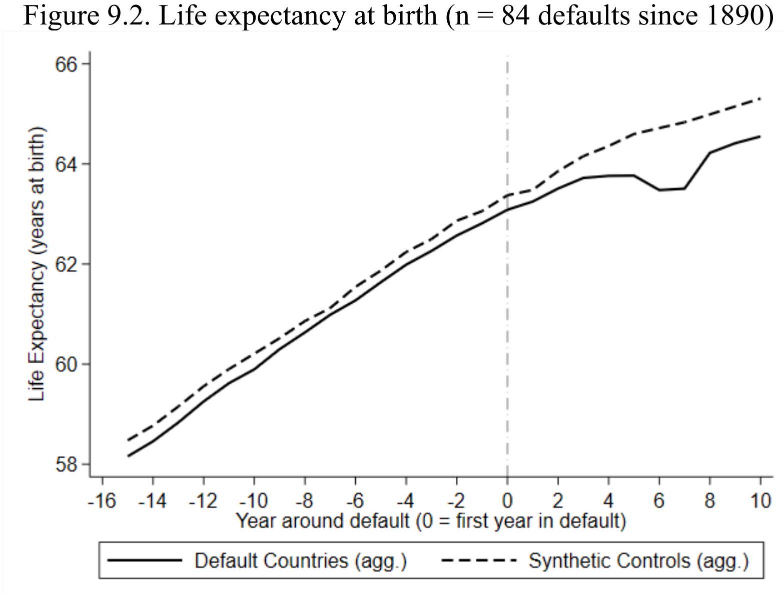


Figure 9.3. Infant mortality (n=74 defaults since 1928)

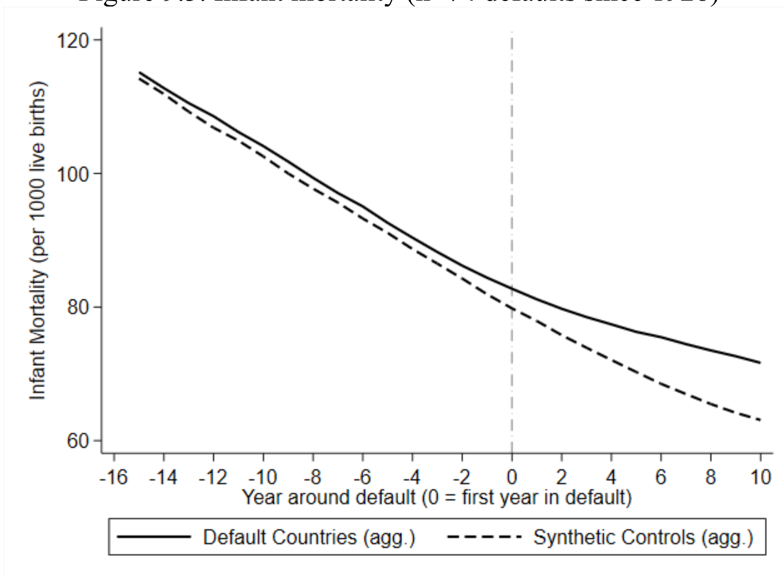



Figure 9.4. Poverty headcounts (n=63 defaults since 1976 )

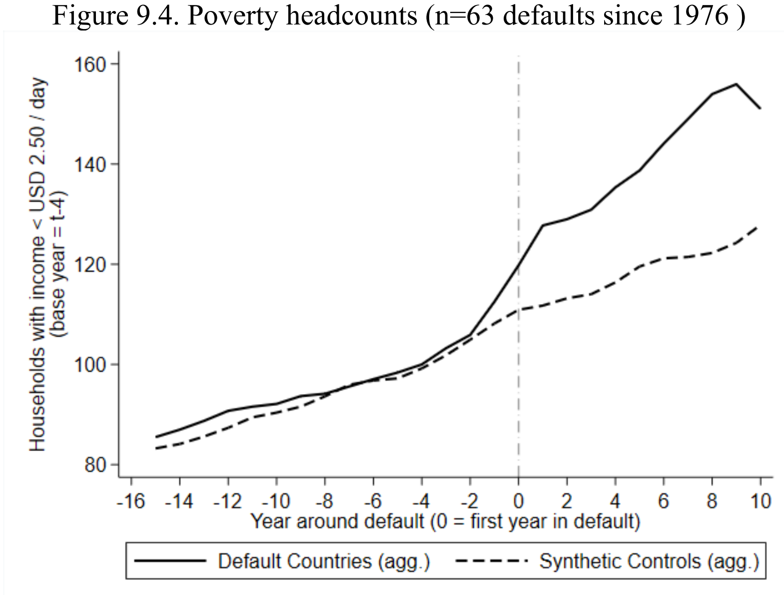
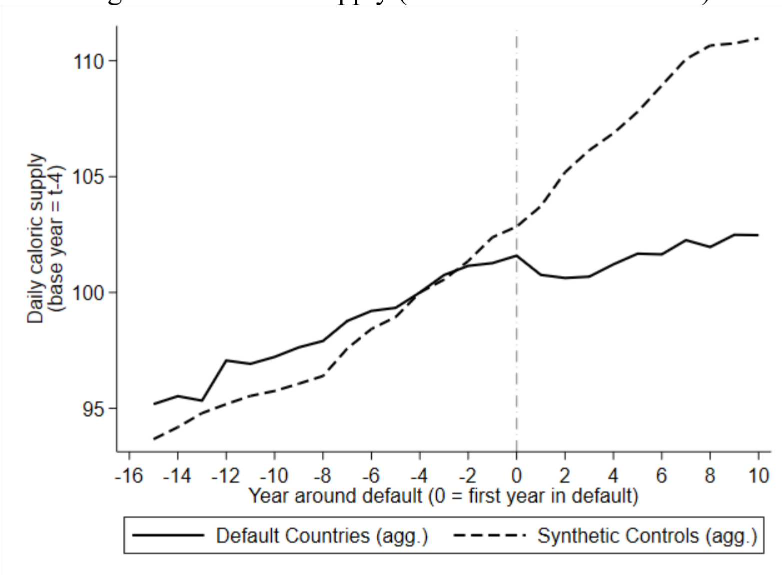


Figure 9.5. Calorie supply (n=61 defaults since 1976)




*Simulation-based prediction intervals*

To better quantify the uncertainty that can arise from in-sample uncertainty, that is, the randomness in the weights assignment during the synthetic control, Cattaneo, Feng, and Titiunik (2021) and Cattaneo, Feng, Palomba and Titiunik (2022) propose a method to estimate conditional simulation-based prediction intervals. Unlike typical confidence intervals, these prediction intervals set limits to a support region for a random variable, within which new realizations of that variable are likely to fall. Figure 10 shows the prediction intervals when applying this approach to our core specifications.[17] Note that these results are noisier since we follow Funke et al. (2023) in applying a non-weighted aggregation, so no RMSE-weighting is applied, and instances without a credible synthetic control remain in the sample at equal weights.

[17] We do not include the potential out-of-sample contribution to the prediction intervals here, since, as the authors highlight, quantifying the unobservable post-treatment error is less straightforward to handle nonparametrically. The authors suggest it should be used as a sensitivity analysis, making an aggregation over many synthetic control methods difficult.

Figure 10. Unweighted aggregated SCM results with simulated confidence intervals

Figure 10.1. Real per capita GDP (n=135 defaults)

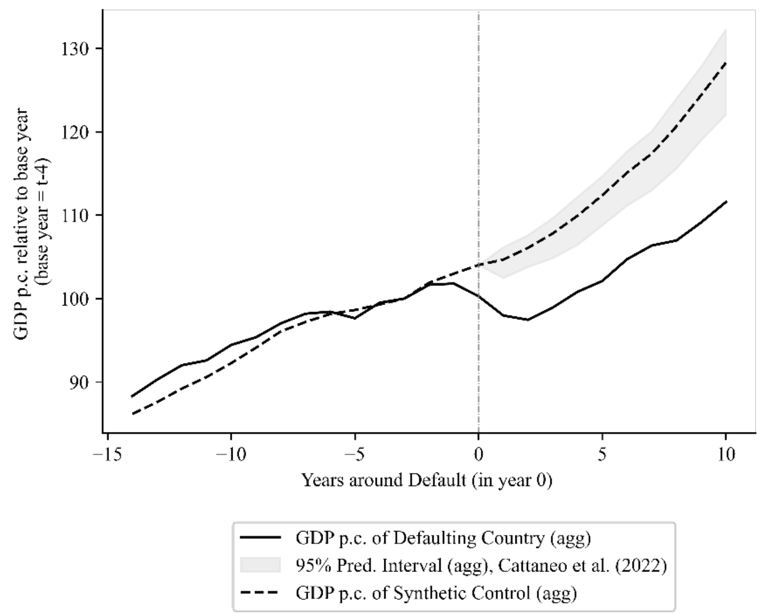



Figure 10.2. Life expectancy at birth (n = 127 defaults)

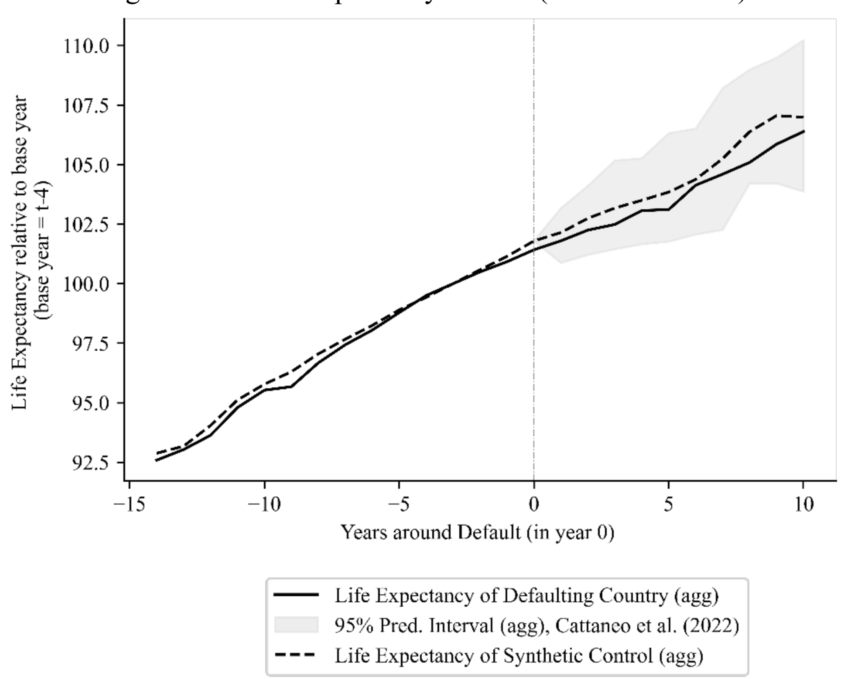


Figure 10.3. Infant mortality (n=104 defaults)

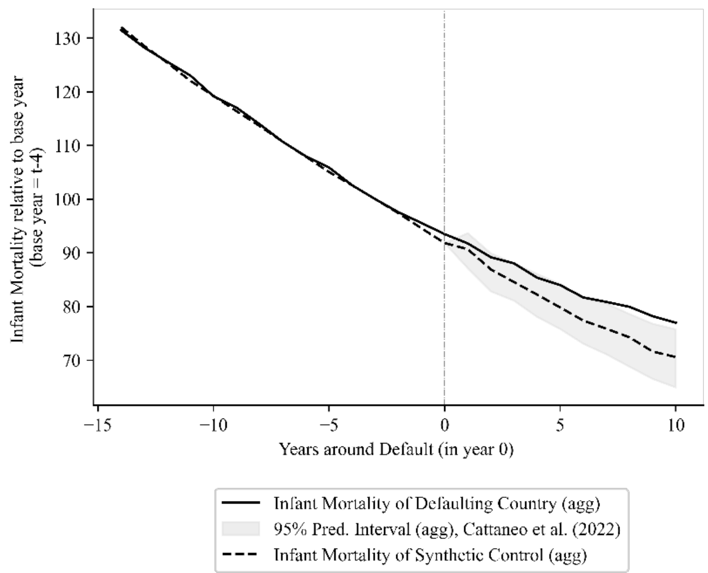



Figure 10.4. Poverty headcounts (n=78 defaults)

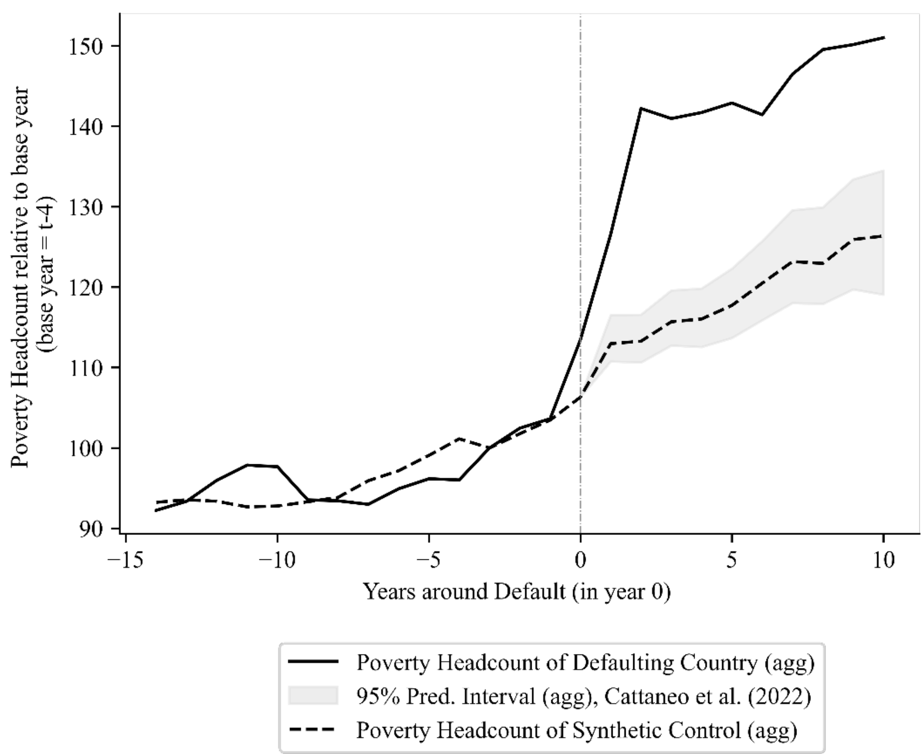
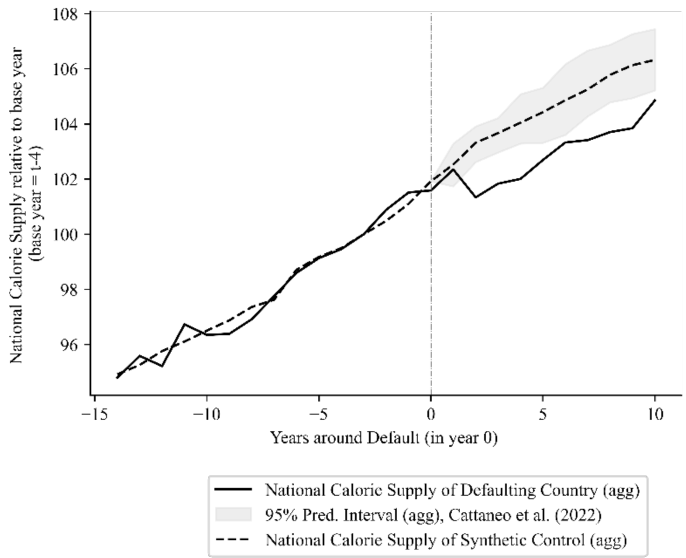


Figure 10.5. Calorie supply (n=73 defaults)




*Local Projections*

As is discussed in Section 4, the SCM leverages both within-country variation and cross-country comparisons, constructing a counterfactual from a weighted combination of control units. This renders SCM less susceptible to omitted variable bias but potentially more sensitive to changes in the relationship between outcomes and predictors. Conversely, LP relies primarily on within-country variation, making it robust to such changes but potentially vulnerable to omitted variable bias. As such, the LP's ability to estimate nonlinear, asymmetric, and time-varying effects presents a valuable complement to our baseline results. Additionally, it is able to explicitly control for the presence of alternate crises around the time of default.

The results from our baseline synthetic controls method and the local projections robustness checks reveal broad similarities in the adverse impacts of sovereign defaults on economic and social indicators. Both approaches suggest significant output losses, slowdowns in the improvement of health outcomes, and increases in poverty measures following sovereign debt crises. For example, the LP reinforces the notion of persistence in the default's output shock as growth remains far below trend for a decade. The deviations from trend changes in social cost variables are also broadly consistent with our findings.

Figure 11. Local projections impulse response functions

Figure 11.1 Real per capita GDP (n=176 defaults)

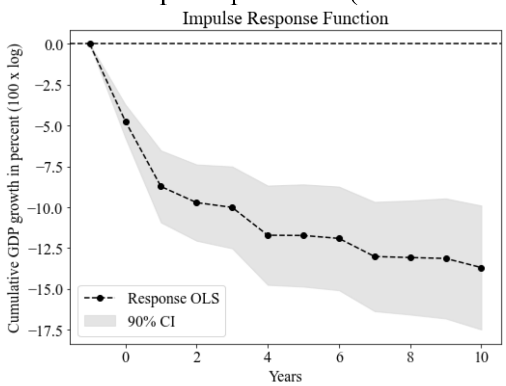



Figure 11.2. Life expectancy (n=214 defaults)

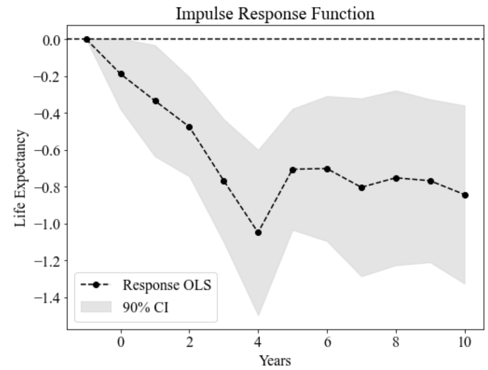



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Figure 11.3. Infant mortality (n=152 defaults)

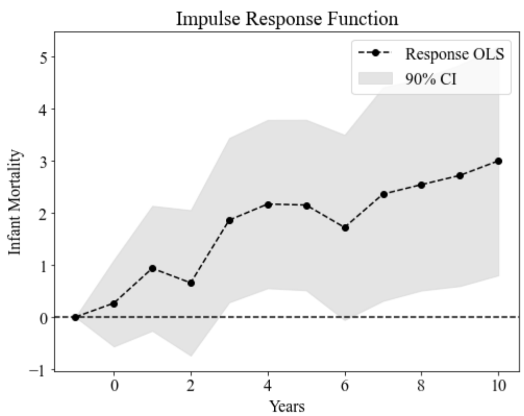



Figure 11.4 Poverty headcount (n=101 defaults)

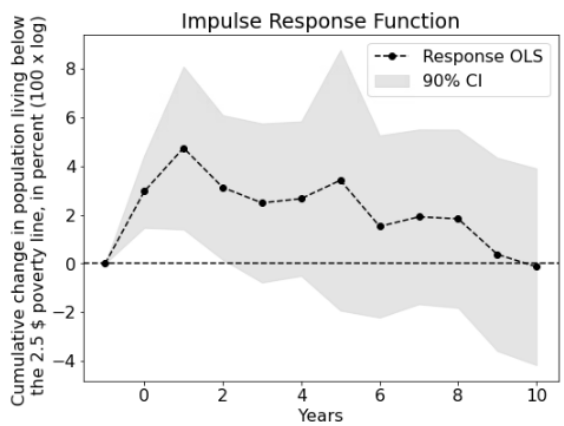
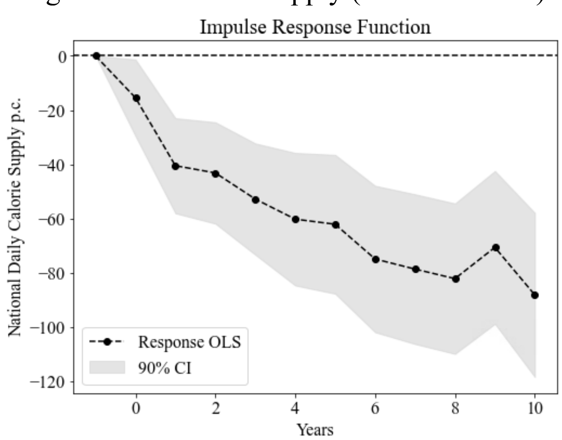



Figure 11.5. Calorie supply (n=104 defaults)




Notes: Real per capita GDP is expressed in logs. All regressions use three lags of the outcome variable and three legs of the default-onset variables as controls. Shaded areas represent 90% confidence intervals.
Sources: See Data Appendix for real GDP, Mitchell (2013), Food and Agriculture Organization (FAO), Gapminder, United Nations, and authors' calculations.

External sovereign defaults often coincide with banking crises, domestic defaults, inflation crises, and banking crises. Below, we compare the impulse response functions with and without these controls in the same reduced sample. The results are noisier due to the reduced sample, but the estimates of the magnitude of the costs remain similar to those in our baseline specification. Notably, whereas part of the output declines is explained by the presence of an alternate crisis, the same is not true for the social outcomes where controlling for other crises makes no difference at all. This suggests that sovereign default crises uniquely impact social outcomes in ways other crises do not.

Figure 12. Local projections impulse response functions with additional controls

Figure 12.1. Real per capita GDP (n=120 defaults)

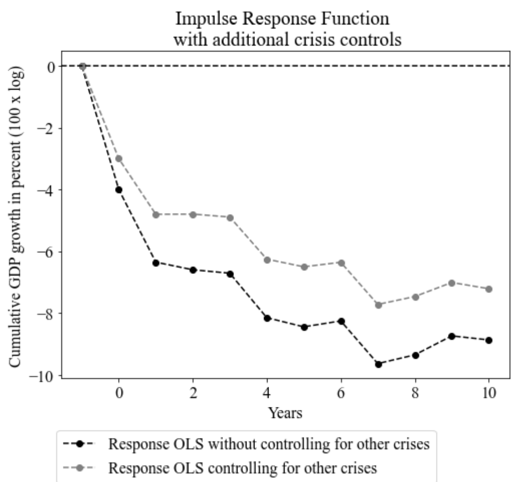



Figure 12.2. Life expectancy (n=146 defaults)

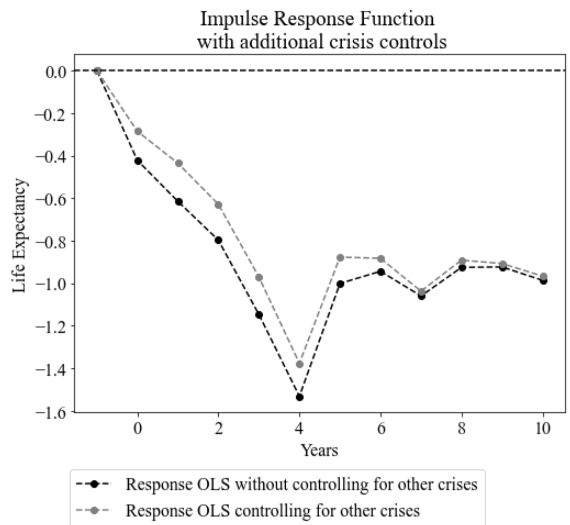



Figure 12.3. Infant mortality (n=90 defaults)

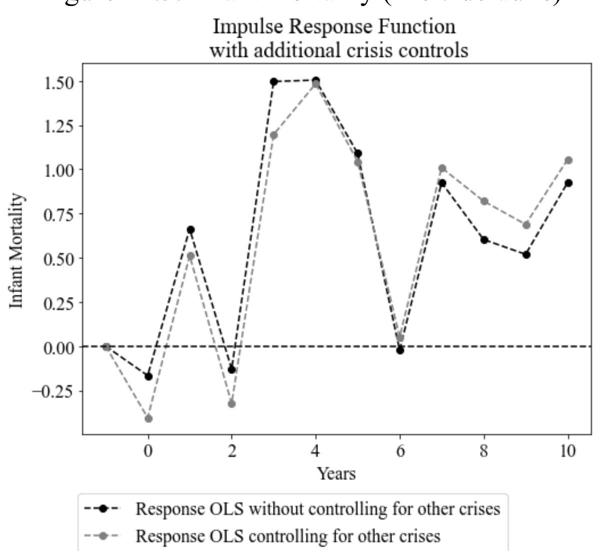



Figure 12.4 Poverty headcount (n=53 defaults)

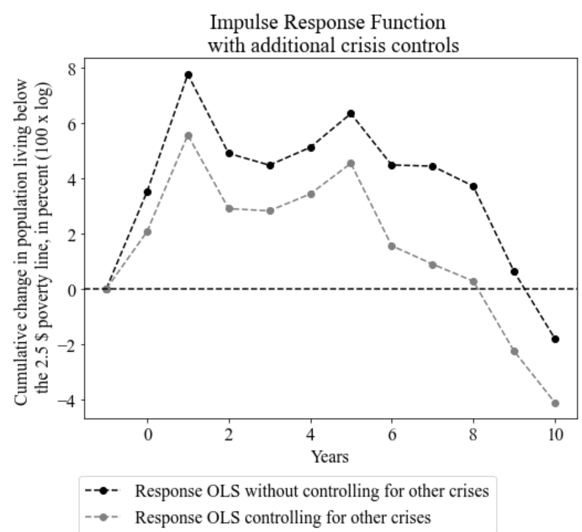
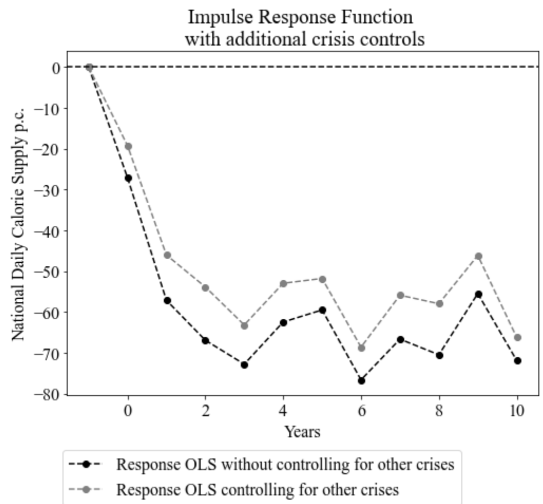


Figure 11.5. Calorie supply (n=52 defaults)




Notes: All regressions use three lags of the outcome variable and five legs of the default-onset variables as controls

**Concluding remarks**

This paper represents a first pass at systematically quantifying some of the social costs of sovereign default across the globe, subject to the challenges posed by the paucity and quality of available data. We have also revisited the measurement of economic costs, as proxied by the relationship between sovereign debt crises and per capita GDP. Our results indicate that sovereign default leads to severe growth declines around the crisis and a secular decline in growth, relative to the counterfactual, over the longer horizon. This finding has direct implications for the impact of defaults on poverty and the broader question of social costs, as earlier literature has suggested a strong negative connection between economic growth and poverty rates.

Beyond GDP, we find defaults adversely impact core social outcomes, as captured by infant mortality rates, life expectancy, poverty totals, and caloric supply, indicating that the most vulnerable segments of the population are heavily impacted by sovereign defaults. Whether the effects of poverty are predictably only driven by the deep recessions that accompany sovereign default is a question that will need better data and further inquiry. Our study considered this question, and we find that while there is ample reason to suspect that the impacts of default on incomes are regressive, there may be considerable heterogeneity in results. The literature from other disciplines outside economics points to nonlinearities and suggests a regressive association between incomes and the health/nutrition outcomes we explore. Our assessment is that the social costs of sovereign default are significant, broad-based, and appear to be long-lived.

We are unable to ascertain that a default-induced recession is more regressive than other output collapses or that the losses are not equiproportionally absorbed. However, it is likely that deterioration in infant mortality and life expectancy come chiefly from the poorer strata of a society.

Our paper is only a starting point to answer important questions. Many are left unanswered partly due to data availability. The social indicators we consider are not exhaustive, and we have not considered how (or if) macroeconomic policies during the default may (or may not) influence social outcomes. The decision to default almost always has a political dimension. It would be useful to examine the economic and social costs of sovereign default through the prism of different political regimes.

In an influential paper, Strauss and Duncan (1998) connect the dots between health, workers' productivity, growth, and development. They conclude, "Questions of the impact of health dynamics, particularly in response to negative shocks and aging, have been barely touched in low-income environments." We concur.

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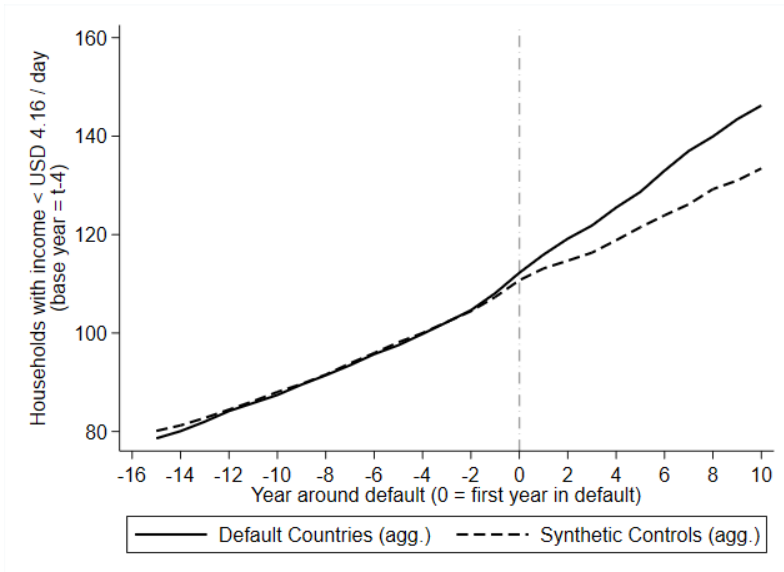
## Appendix 1 – Data

| Variable Name | Description | Number of observations | Sources |
|---|---|---|---|
| GDP per capita | Real GDP per capita constant prices (2011 US dollars) spliced using growth rates calculated from additional data sources where overlapping data matched to increase coverage. | Unbalanced panel from 1800 to 2018 (219 years) and coverage reaching up to 166 countries. | Maddison Project Database, (Bolt & van Zanden, 2020)<br>Additional sources: Statistics Iceland, New Zealand Statistics, Ferreres, (2005), Yousef (2002), Broadberry and Garner (2022), Bassino and van der Eng (2020). |
| Sovereign default | Binary variable taking the value one if the country is in default and 0 otherwise. | Panel from 1800 to 2020 (221 years) of 195 countries. | Farah Yacoub, Graf von Luckner, Reinhart and Rogoff (2024) |
| 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 1800 to 2020, with a minimum of 1 country and a maximum of 199. | United Nations Inter-Agency Group for Child Mortality Estimation, Mitchell (2013) |
| Population | The number of inhabitants measured mid-year in thousands | Unbalanced panel from 1800 to 2018, with coverage reaching up to 169. | Maddison Project Database, (Bolt & van Zanden, 2020), |
| Life expectancy | Life expectancy at birth in years for the total population | Unbalanced panel from 1800 to 2020 (121 years) of 194 countries. | Gapminder (2024) |
| Calorie supply | Per capita kilocalorie supply from all foods per day | Unbalanced panel from 1961 to 2020 (60 years), with a minimum of 143 countries and a maximum of 172 | Food and Agriculture Organization of the United Nations (FAO) |
| Poverty headcount | Percentage of people living below the $2.5 (4.16) per day poverty line (in 2005 PPP USD) | Unbalanced panel from 1981 to 2019 (40 years) with a minimum of 160 countries to a maximum of 165 | Global Consumption and Income Project (2021) |
| Polity II | A 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 | The Polity Project (Marshall and Gurr, (2020)) |

## Appendix 2 – Additional results

Sovereign default and poverty totals at \$4.16 a day: Based on 78 default episodes 1975-2020

Sovereign default and poverty totals at \$4.16 a day: Aggregated SCM results




Sovereign default and poverty totals at \$4.16 a day: Pseudo-treatment bootstrap test