Comment on: Structural adjustment: damages, reparations and pathways to non-recurrence

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Abstract

Hickel *et al.* (2026) (HKBR) argue that structural adjustment programs (SAPs) of the IMF and World Bank caused declining real wages, increased rates of poverty, and worsened health in countries of the Global South. We discuss based on economic literature and original data analysis whether the different claims of this study are based on reliable data, can be interpreted as causal, and can be interpreted as liberalization. We conclude that the evidence is mixed and heterogeneous across countries, periods and programs.

Keywords: structural adjustment programs, economic development, liberalizations

JEL Codes: F33, O11, O19

AI use: Artificial intelligence tools (ChatGPT, Claude, Gemini) were used in the preparation of this manuscript for limited editorial assistance. These tools were employed to compress writing to express our points within the tight wordcount limit of a comment. All substantive theoretical claims, model construction, interpretation, and historical analysis were developed by the authors, who take full responsibility for the content of the paper.

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1 Introduction

In a recent contribution to *BMJ:Global Health*, Hickel *et al.* (2026) (hereafter HKBR) argue that structural adjustment programs (SAPs) imposed on countries in the Global South required economic liberalizations that led to decreases in incomes, increases in poverty, and deteriorating health outcomes. We dispute these claims on three grounds.

First, HKBR misdate SAPs—most did not start in 1980—and wrongly treat Sub-Saharan Africa as homogeneous, imposing a false common treatment date. Their reliance on aggregates masks country-level heterogeneity; at that level, the link between SAPs and economic decline is inconclusive.

Second, many Global South countries entered SAPs following fiscal and/or economic crises caused by global shocks or/and poor domestic institutions. Since participation in SAPs is non-random, there is a correlation between SAP treatment and poor socio-economic performance—a selection bias problem which is compounded by concurrent shocks, repeated and staggered program entry, and the heterogeneity of program content across countries. Thus, the apparent statistical correlations advanced by HKBR cannot be interpreted causally.

Finally, HKBR conflate SAPs with liberalization. Yet conditions were often not implemented, as the IMF and World Bank routinely waived non-compliance and extended new loans, leaving little overlap with actual liberalization. Moreover, stronger causal evidence from the liberalization literature generally finds gains in living standards, growth, and welfare (*contra* HKBR).

In addition to our main points, our appendix documents other flaws in HKBR regarding improper reading of source materials, factual errors of importance, and incorrect interpretation of data sources.[1]

2 Heterogeneity in outcomes and the timing of SAPs

HKBR (p. 2) claim that "[s]tructural adjustment was associated with a dramatic collapse in income and consumption across much of the global South after 1980," the year they assert that "most SAPs were signed." To support this claim, their Figure 1 uses a non-standard source[2] for GDP comparisons to document that real national income per adult in Sub-Saharan Africa rose until the early 1970s and then fell steadily until the late 1990s.[3] When one digs into the data underlying this figure, however, there is no systematic relationship at the country level between economic growth and the timing of SAPs.

Figure 1 plots real income per adult for each Sub-Saharan country normalized to 100 in 1980. Shaded years indicate periods when a country had an active structural adjustment loan from the IMF.[4] Dashed vertical lines indicate years when a country initially received a structural adjustment loan from the World Bank. In addtion to HKBR's real income measure, which comes from the World Inequality Database, we also plot a measure of real

[1] We thank J. P. Bastos of Texas Tech University for highlighting many of these issues while we were preparing this note.

[2] The World Inequality Database (WID) is a "non-standard" source of data on aggregate income growth across countries, as it is primarily designed to establish trends on income inequality within countries. The standard sources for aggregate income are the Maddison Project Database (MPD), the World Bank's World Development Indicators (WDI), and the Penn World Tables (PWT).

[3] The fact that the decline in aggregate Sub-Saharan African income begins in the mid-1970s—before HKBR's claimed onset of SAPs—raises endogeneity concerns. We address this in the next section.

[4] The IMF has many different loan programs. We classify the following as structural adjustment loans: Structural Enhancement Facility (SAF), Enhanced Structural Adjustment Facility (ESAF), and Extended Fund Facility (EFF). While the latter is not always classified as such in the literature, it requires recipients to "focus on structural reforms to address institutional or economic weaknesses, in addition to policies to maintain macroeconomic stability," which is clearly in the spirit of SAPs. Regardless, all of our main points and statistical analyses in this section go through when using SAFs and ESAFS only.

GDP per capita from the Penn World Tables, which is a more conventional source for macroeconomic data in economic research.

Three observations emerge from our country-level analysis that contradict HKBR's narrative. First, there is substantial variation across countries in SAP timing. Some Sub-Saharan countries' structural adjustment loan programs began around 1980, but others did not begin until a decade later. Some countries received only one adjustment loan, others five or more, and some none at all.

Second, living standards declined in most countries in the region during the 1980s regardless of SAP participation. Angola, Djibouti, Gabon, Liberia, Namibia, and South Africa all experienced large declines during this period without entering SAPs at all. Moreover, variation across countries in economic growth during this period is poorly correlated with the presence and timing of SAPs suggesting that global shocks, such as the 1970s energy crisis and subsequent inflation spike, are likelier culprits. The widespread economic downturn during this period was not limited to Africa or to developing countries in general.

Third, and most importantly, there is no systematic relationship between SAPs and economic growth: as we show in Appendix A, once one controls for the average Sub-Saharan growth rate in a given year, the correlation between growth and SAP presence is statistically insignificant in most specifications. Anecdotally, while some countries that received adjustment loans around 1980, like the Democratic Republic of the Congo and Nigeria, experienced subsequent economic declines, many countries that received loans later in the sample, such as Benin, Burkina Faso, Cameroon, Chad, Ghana, Mali, Rwanda, and Uganda all experienced large increases in living standards. Countries receiving multiple loans (e.g. Kenya, Senegal) experienced growth following some loans and decline following others.

3 Correlation versus causation

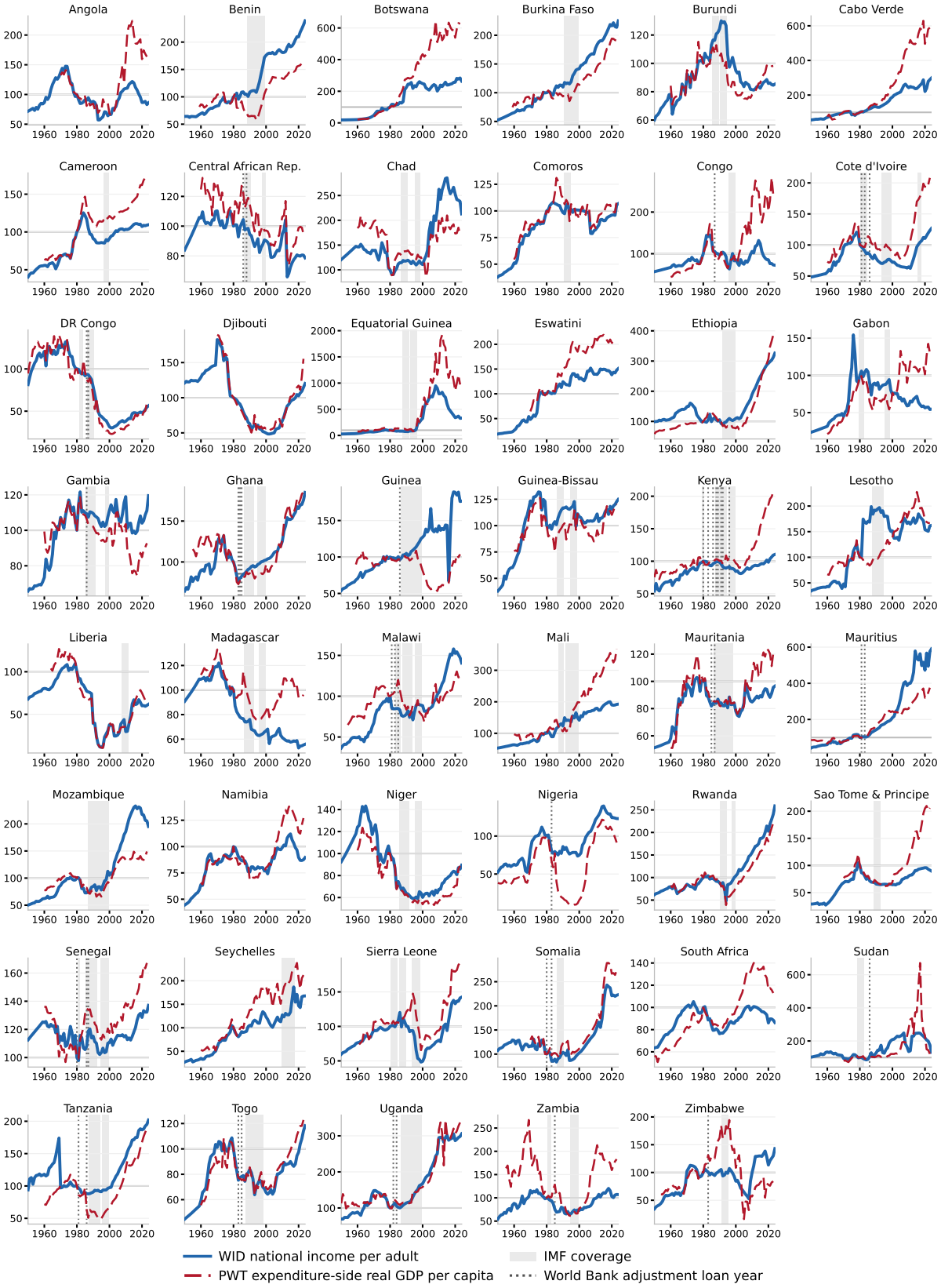
HKBR make no effort to isolate the causal effects of SAPs from the conditions that led countries to enter these programs or other unrelated shocks that affected these countries while these programs were underway. We discuss four key issues that complicate causal identification. First, SAPs start during economic crises (endogeneity). Second, there are other simultaneous shocks for which HKBR do not control (omitted variables). Third, many countries cycle in and out of SAPs (treatment cannot cleanly be measured). Fourth, SAPs markedly differ in content and intensity (treatment heterogeneity).

First, SAPs are not random treatments (Easterly, 2005; Gehring and Lang, 2020). Support from international financial institutions is extended during serious macroeconomic crises (fiscal crises, balance-of-payment crises, etc.) which would independently reduce living standards in the absence of this support.[5] HKBR's selected period (1970-90) coincides with major oil shocks, a sharp rise in international interest rates, monetary tightening, and a global recession. As SAP participation occurs during distress, participants and non-participants are not comparable, foiling causal assessment. Few sources cited by HKBR address this issue; well-cited contributions on the endogeneity of SAPs are overlooked (e.g., Dreher (2006)), and studies employing causal identification strategies rely on assumptions that are open to question.

Second, SAPs often coincide with other shocks that continue to affect outcomes after entry. In the 1980s, many developing countries faced worsening terms of trade, falling export revenues, reduced capital inflows, rising debt-service burdens, and domestic shocks such as droughts, conflict, HIV epidemics, or political instability. These

[5] Figure B.4 reproduces a central graph from Gehring and Lang (2020), which shows the relationship between sovereign credit ratings and the exact timing of IMF program entry. The figure suggests strong selection into treatment, which complicates the empirical identification of causal effects. Figure A.3 shows event study plots based on the data discussed in section 2 suggesting that treated countries are deviating in economic conditions from the control group; that is, the parallel trends assumption does not hold.

Figure 1: Country-level real income time series annotated with SAP program coverage



Notes: Real income per capita (solid blue) from World Inequality Database. Real GDP per capita (red dashed) constructed as rgdpe/pop from Penn World Tables v11. Shaded areas indicate years with active IMF adjustment loans (https://vreeland.scholar.princeton.edu). Dashed lines indicate World Bank adjustment-loan approval years (https://www.sourcewatch.org/index.php/Structural_Adjustment_Loan).

factors directly affected income, nutrition, public spending, and mortality. Even in the absence of any causal effects of SAPs, these shocks would still have had adverse effects on public-health outcomes during program periods, making before–after comparisons hard to interpret. Note that while the shocks mentioned above appear similar to those raised in the first concern, these issues are fundamentally distinct: the first explains why countries enter SAPs, while this one pertains to concurrent—but independent—effects during SAPs.

Third, the timing and repetition of program participation complicate identification (see Figure 1). As mentioned in the previous section, numerous countries cycled repeatedly through IMF and World Bank programs, and did so at very different times. These repetitive, staggered treatments complicate causal interpretation even when one uses frontier econometric methods, which HKBR do not.[6] Certainly, visual evidence based on a single approximate treatment date should not be interpreted as causal.

Fourth, SAPs were not all the same. They varied across time and countries, just as the severity of the initial conditions that caused participation in these programs varied. As a result, a single average "effect of SAPs" likely aggregates the effects of many substantively different interventions. In addition, key channels, such as subsidy removal, public employment cuts, and tax reforms, may affect outcomes indirectly through political unrest, reduced state capacity, or changes in household behavior. This variation in mechanisms in addition to program content further complicates the interpretation of average effects.

Previous studies have attempted to address some of these issues but have not yet conclusively resolved them. For example, Dreher (2006) and Oberdabernig (2013) address endogeneity by instrumenting program participation with UN voting alignment, finding that IMF loans reduce growth and increase poverty in the short run. However, their instrument's exclusion restriction is debatable, as UN voting may also affect trade and other aid flows. Stubbs *et al.* (2022) reach a similar conclusion using an alternative, but also imperfect, approach. Biglaiser and McGauvran (2022), Lang (2021), and Forster *et al.* (2020) use Bartik shift-share instruments that interact overall IMF liquidity with historical country-level exposure to IMF programs.[7] These studies also generally find adverse effects of SAPs, but the exclusion restriction is debatable in this context as well. Their identification hinges on comparing how countries with different histories of IMF involvement respond to changes in global IMF activity, and a crucial assumption is that countries with high and low exposure to IMF programs would have followed similar trends when global conditions changed in the absence of those programs. This assumption is hard to justify, as historical IMF exposure likely reflects underlying vulnerability, which also shapes responses to global crises.

4 Conflating Liberalizations with SAPs

The final substantial problem in HKBR is that, under the broad label of "neoliberal structural adjustment programmes," they conflate "austerity, privatisation and economic deregulation," claiming that "structural adjustment imposed neoliberal shock therapy" had "severe negative impacts on human welfare" (p. 1). This mixes distinct concepts: these policies (here, "liberalizations") may appear in SAPs, but not all SAPs include them, nor are all liberalizations themselves SAPs. As such, a more accurate question is whether "liberalizations," rather than SAPs per se, have led to the severe consequences described by HKBR.

The liberalization component of SAPs is the least likely to be implemented or enforced. Dollar and Svensson (2000) show that success depended on political factors (regime type, stability, ethnic fractionalization), not program design (loan size, conditionality, technical assistance). This implies limited IMF/World Bank control over reforms, undermining the link from SAPs to liberalizations assumed by HKBR. This finding is consistent with other recent works showing that there is no association between changes in economic freedom – an index of pro-market policies

[6] Recent econometric research shows that in such settings, standard before–after comparisons can be misleading, especially when treatment effects vary over time, across units, or in intensity (e.g., Callaway and Sant'Anna (2021); Goodman-Bacon (2021)).

[7] See Goldsmith-Pinkham *et al.* (2020) for an overview of Bartik instruments

– and receiving loans from these institutions (Meehan, 2025). Finally, Killick (1997, 2003) shows that compliance with IMF structural conditions was rare; instead, the Fund routinely waived or renegotiated unmet conditions, undermining credibility and encouraging strategic non-compliance. Easterly (2005) reinforces this critique by showing that the IMF and World Bank repeatedly extended new loans to countries that failed to implement prior conditions, effectively decoupling lending from reform. Over decades, the typical recipient received adjustment loans in most years with little progress on structural reforms, suggesting a preference for continued engagement over enforcement driven by institutional incentives rather than reform outcomes.

In other words, the"treatment of SAP" is not synonymous with the "treatment of liberalization" nor does the latter imply the former. There is no case for treating liberalizations and SAPs in the same breadth. They are different.

This is clear when revisiting Figure 1 of HKBR, which claims most SAPs were signed in 1980 in Sub-Saharan Africa. In fact, only 16 of 47 countries entered SAPs between 1980 and 1983 (see Figure 2). But how many actually liberalized? Using the Fraser Institute's Economic Freedom of the World (EFW) index—a standard proxy—helps answer this. The index ranges from 0 to 10, with higher values reflecting what HKBR call "neoliberal" policies. Grier and Grier (2021) define generalized liberalization as a sustained one-point increase; Rayamajhee and March (2025) use V-Dem data and apply the same threshold to state ownership. These yield two lists of liberalization episodes, to which we add Billmeier and Nannicini (2013). Table 1 reports, by five-year period, liberalizations, SAPs, and overlaps. We use two SAP sources: SourceWatch and the more comprehensive SSA dataset from James Vreeland (IMF and World Bank). Overlap is minimal: many SAPs simply came with no liberalizations.

Table 1: Episodes of SAP and Liberalization by five-year window

	1980–84	1985–89	1990–94	1995–99
SAPS (SourceWatch)	52	46	3	1
SAPS (SSA, Appendix A)	24	26	29	31
Liberalization (EFW)	6	9	18	17
Liberalization (EFW, SSA only)	2	1	5	6
Liberalization (VDEM)	3	11	23	13
Liberalization (VDEM, SSA only)	2	7	5	0
Liberalization (Billmeier and Nannicini)	2	15	17	8
Both (with EFW, Sourcewatch)	4	3	0	0
Both (with EFW for SSA)	2	1	2	6

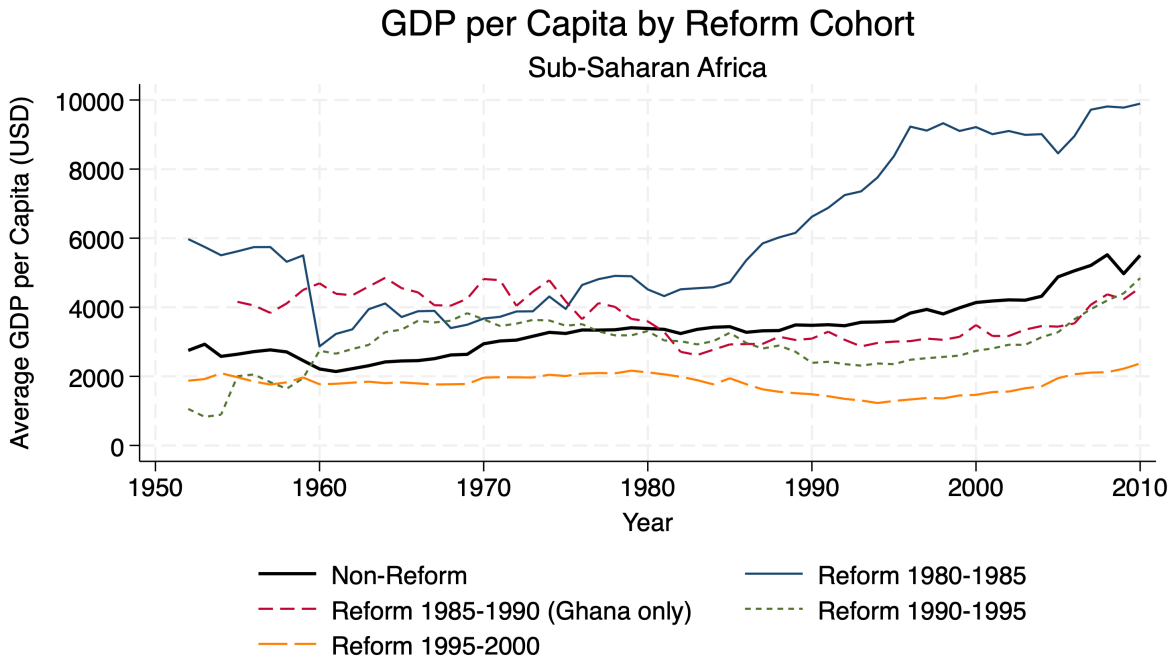
This distinction between liberalizations and SAPs is crucial because evidence on the causal effects of liberalization is extensive and consistent. We cite only studies using causal methods. Using matching, Grier and Grier (2021) show that countries reforming along Washington Consensus lines (sound monetary policy, property rights, deregulation) achieve 16% higher growth ten years after reforms relative to non-reformers.[8] Using similar strategies, Callais and Young (2023) find growth gains across the income distribution, including for trade liberalizations (Bergh *et al.*, 2026). Similarly positive outcomes are found with respect to environmental outcomes, women's rights, infant mortality and psychological well-being (Callais *et al.*, 2025; Rutar, 2025; Grier, 2025; Lawson, 2025).

Shifting to other causal methods, Yang (2011) find no effect on growth volatility and Onwachukwu *et al.* (2021) find reductions in some pollutants but increases in others. A large synthetic control literature on individual liberalizations generally finds improved outcomes (health, income, intergenerational mobility) (Billmeier and Nannicini, 2013; Barlow, 2018; Olper *et al.*, 2018; Lawson *et al.*, 2019; McCannon and Zhahadai, 2024; Callais *et al.*, 2024; Kantorowicz and Spruk, 2024; Grier and Grier, 2026; Marrazzo and Terzi, 2026; Amaya, 2020), though some exceptions exist (Mahmood, 2024; Escalante, 2022). This excludes studies of the inverse—greater regulation or nationalization—which consistently find harmful effects (Mora-Sanguinetti *et al.*, 2024; Grier and Maynard, 2016; Geloso and Pavlik, 2021).

[8] Alvarez *et al.* (2024) argue this is underestimated due to GDP fabrication in dictatorships. True effects are 1.1 to 1.62 times larger.

As such, HKBR conflate distinct phenomena, creating interpretative pitfalls—especially in Sub-Saharan Africa. Using a milder definition—a 0.5-point increase in the EFW index—Muhoza (2025) identifies more cases where SAPs and liberalizations occur close in time. Even under this "moderate" definition, results remain strong: liberalizing countries see per capita income growth 4.8 and 4.6 percentage points higher than comparable controls over five- and ten-year horizons.

Figure 2: GDP per capita, by reform period



To illustrate, we compare with Figure 1 in HKBR and use market-liberalization episodes identified by Grier and Grier (2021). Of the 49 reform cases, 14 are in Sub-Saharan Africa. Mauritius and Togo reformed earliest (1980–1985), followed by Ghana (1985–1990). Four countries reformed between 1990–1995, and the remaining seven between 1995–2000. Figure 2 plots GDP per capita by reform cohort alongside a series for non-reforming Sub-Saharan African countries. Early reformers (Mauritius, Togo) tracked others initially but surged from the 1980s onward, compounding gains and pulling far ahead by 1990. The 1990–95 cohort stagnated until reform, then rose. The 1995–2000 group remained poorest and flat ($1,500–2,000) for decades, with only a modest post-2000 uptick. Non-reformers grew slowly: though initially richer, they were overtaken by earlier reformers.

5 Conclusion

Data and methodological issues cast doubt on the average effects of SAPs; nothing as strong as HKBR's confidence is warranted. A more convincing interpretation focuses on the conditions under which SAPs succeed or fail—conditions that also govern liberalization. Not all liberalizations succeed; some disappoint (Callais and Lawson, 2024), often when implemented by autocratic regimes—a factor that overlaps more with SAPs. In Africa, the three cases identified by Callais and Lawson (2024) that failed to raise incomes (South Africa, Tanzania, Zambia) were not liberal democracies during reform. This is consistent with Grier and Grier (2026), who show that liberalizations increase incomes when accompanied by democratization or when democracy precedes reform. More broadly, Dollar and Svensson (2000) emphasize that SAP outcomes depend on political economy factors—precisely those that also determine the success of liberalization. The implication is that conclusions about SAPs must be conditional on institutional context, particularly in settings with weak institutions and autocratic governance. In light

of the literature, data, and methods discussed, HKBR may not be a suitable reference for drawing meaningful conclusions about SAPs.

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Appendix A Statistical relationships between SAPs and economic growth in Sub-Saharan Africa

We estimated contemporaneous correlations between economic growth and the presence of an adjustment loan, both contemporaneously (growth in the period where a loan is present) and with a lag (growth in the year following loan presence), annually and over five-year windows. As table A.3 shows, the only statistically significant correlation is in the contemporaneous 5-year specification in the WID data; the same correlation is not significant in the Penn World Tables, which is a much more widely accepted source for data on national incomes for this sort of exercise. As we discuss in section 3, this specification suffers from endogeneity concerns and cannot be interpreted causally, anyway. We also estimated event studies around a country's first adjustment loan, which show positive—not negative—effects on growth, although most coefficients are statistically insignificant (see figure A.3). There are also strong pre-trends, indicating that growth typically begins well before program entry and conclusively rules out a causal interpretation.

Table A.2: IMF and World Bank loan years for countries in the main Figure 1 sample

Country	IMF program years	WB adjustment loan years
Angola	None	None
Benin	1989–1999	None
Botswana	None	None
Burkina Faso	1991–1999	None
Burundi	1986–1989, 1991–1994	None
Cabo Verde	None	None
Cameroon	1997–1999	None
Central African Rep.	1987–1990, 1998–1999	1986, 1988
Chad	1987–1990, 1996–1998	None
Comoros	1991–1994	None
Congo	1996–1999	1987
Cote d'Ivoire	1981–1984, 1994–1999, 2016–2017	1981, 1983, 1986
DR Congo	1982–1983, 1987–1990	1986, 1987
Djibouti	None	None
Equatorial Guinea	1988–1991, 1993–1996	None
Eswatini	None	None
Ethiopia	1992–1999	None
Gabon	1980–1982, 1996–1998	None
Gambia	1986–1991, 1998–1999	1986
Ghana	1987–1992, 1995–1999	1983, 1984, 1985
Guinea	1987–1999	1986
Guinea-Bissau	1987–1990, 1995–1998	None
Kenya	1980, 1988–1994, 1996–1999	1980, 1983, 1986, 1988, 1989, 1991, 1992, 1996
Lesotho	1988–1994	None
Liberia	2008–2011	None
Madagascar	1987–1992, 1996–1999	None
Malawi	1984–1986, 1988–1993, 1996–1999	1981, 1983, 1985
Mali	1988–1990, 1992–1999	None
Mauritania	1987–1998	1985, 1987
Mauritius	None	1981, 1983
Mozambique	1987–1999	None
Namibia	None	None
Niger	1986–1991, 1996–1999	None
Nigeria	None	1983
Rwanda	1991–1994, 1998–1999	None
Sao Tome & Principe	1989–1992	None
Senegal	1981, 1986–1992, 1995–1999	1980, 1986, 1987
Seychelles	2010–2017	None
Sierra Leone	1981–1984, 1986–1989, 1994–1998	None
Somalia	1987–1990	1980, 1983

Country	IMF program years	WB adjustment loan years
South Africa	None	None
Sudan	1979–1982	1986
Tanzania	1988–1994, 1996–1999	1981, 1986
Togo	1988–1998	1983, 1985
Uganda	1987–1999	1982, 1984
Zambia	1981–1982, 1995–1999	1985
Zimbabwe	1992–1995	1983

IMF loan dates are from https://vreeland.scholar.princeton.edu. We classify the following IMF loans as structural adjustment loans: Structural Enhancement Facility (SAF), Enhanced Structural Adjustment Facility (ESAF), and Enhanced Fund Facility (EFF). World Bank loan dates are from https://www.sourcewatch.org/index.php/Structural_Adjustment_Loan.

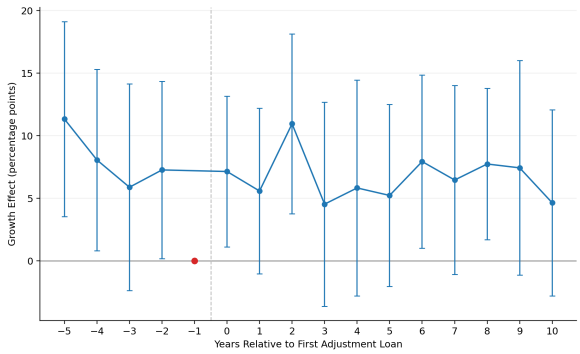
Table A.3: Growth regressions with country and time fixed effects

	Annual growth		5-year growth	
	Curr. yr.	Prev. yr.	Curr. 5-yr. window	Prev. 5-yr. window
Panel A. WID real income per adult				
Adjustment loan indicator	-1.140	0.074	-6.640**	1.509
	(0.920)	(0.893)	(3.303)	(3.788)
Observations	3478	3478	3290	3290
Country fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Panel B. PWT real GDP per capita				
Adjustment loan indicator	-0.630	-0.783	-6.039	-2.600
	(1.600)	(1.071)	(4.366)	(5.476)
Observations	2991	2991	2803	2803
Country fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes

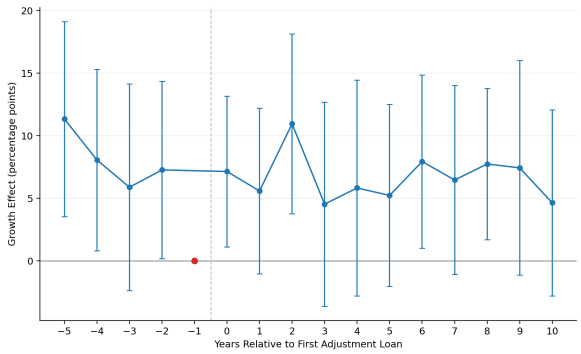
Standard errors clustered by country in parentheses. All specifications include country and time fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A.3: Event-study estimates around the first adjustment loan.

(a) WID real income per adult



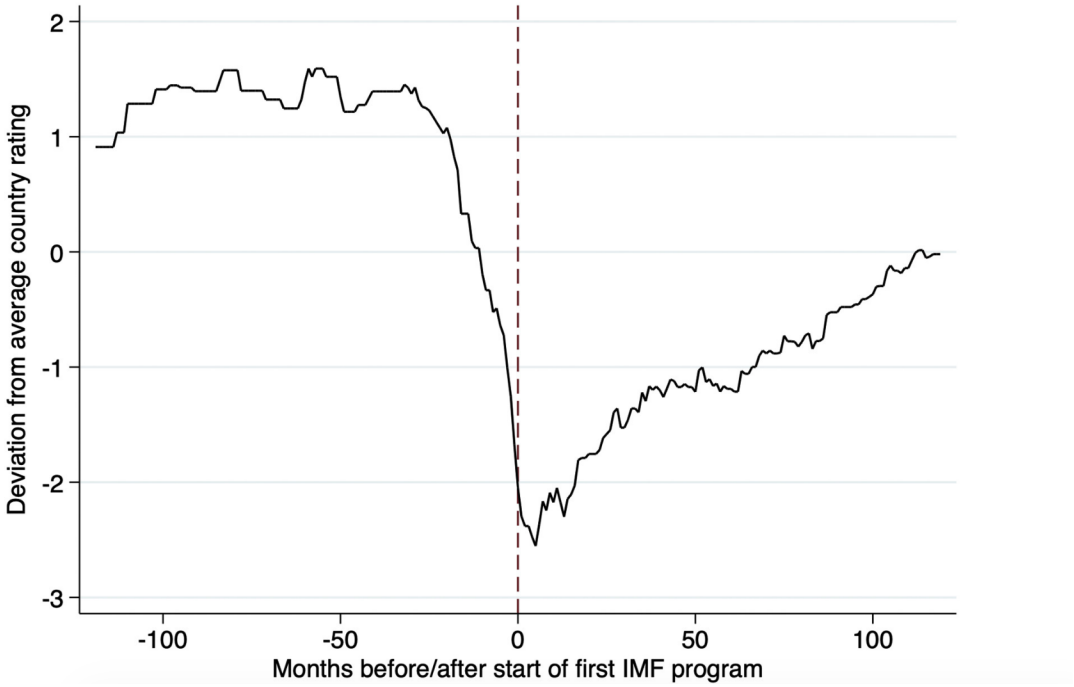
(b) PWT real GDP per capita



Lines indicate 95-percent confidence intervals using standard errors clustered by country. All specifications include country and time fixed effects.

Appendix B Endogenous selection into SAPs

Figure B.4: Country rating and timing of IMF programs



Notes: Reprint of Figure 2 in Gehring and Lang (2020) page 4.

Appendix C Comment on Figure 2

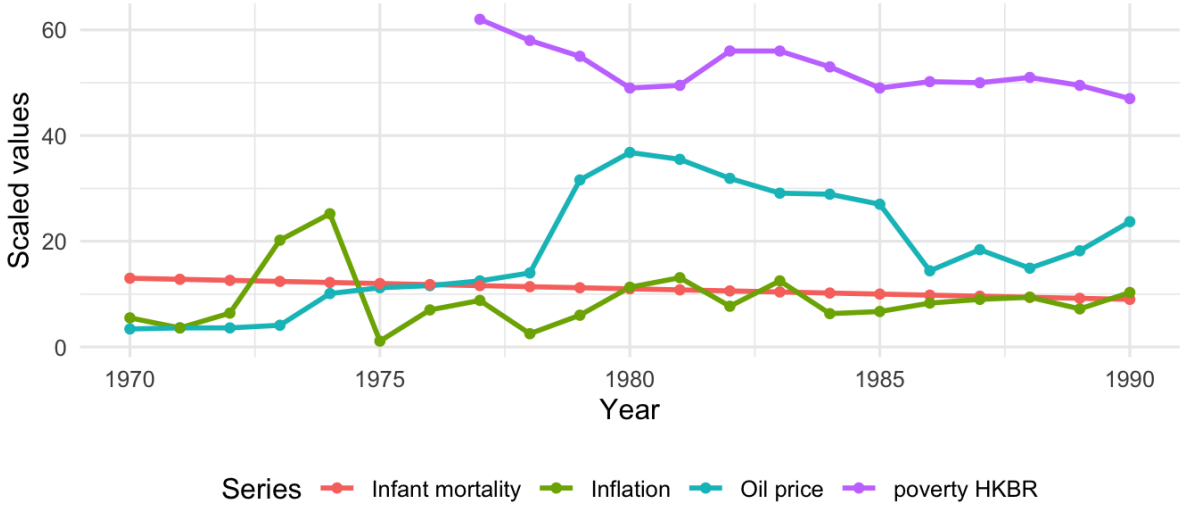
Figure 2 in HKBR suggests an increase in poverty in India following the introduction of structural adjustment programs. Figure C.5 reproduces this pattern and augments it with additional macroeconomic indicators.

Notably, the increase in poverty coincides with the second global oil shock (1979–1980), which led to a deterioration in macroeconomic conditions. This temporal overlap complicates the attribution of observed changes in poverty to SAPs alone.

At the same time, inflation rises sharply. This is particularly relevant because the basic needs poverty line (BNPL) places substantial weight on food and energy consumption. As a result, increases in oil prices can directly affect the measured poverty rate through their impact on the cost of basic consumption goods. Since both food and energy prices are closely linked to global oil prices, part of the observed increase in poverty may reflect these external price shocks rather than policy effects.

In contrast, infant mortality declines steadily over the entire period. Given the well-documented association between poverty and child health outcomes, this divergence suggests that the increase in poverty documented in HKBR is, at most, only weakly reflected in broader welfare indicators.

Figure C.5: Figure 2 replicated with additional data



Notes: The figure shows the poverty data reported in HKBR (in %) , infant mortality (per 10,000), the oil price (in USD), and CPI inflation (in %) as reported by the World Bank Development indicators.

In addition, it seems like Figure 2 from HKBR suffers from one of the problems highlighted in our paper as well as a form of cherry picking data. First, India did initiate mild economic reforms in 1981, but nothing that would constitute large-scale liberalization (Panagariya, 2005). The score of economic freedom produced by the Fraser Institute – again a proxy for how neoliberal (in the words of HKBR) an economy is – was 4.94 (out of 10) in 1980 and 4.81 in 1985 and 4.96 in 1990. It is only after 1990 that the score noticeably increases – given the 1991 reforms – to 5.55 in 1995 and 6.10 in 2000. Synthetic control analysis confirms that the 1991 reforms were the bigger reforms and it noticeably accelerated economic growth (Amaya, 2020). Again, this seems to be a case of conflating SAPs with liberalization especially as the 1991 liberalization came with a SAP as well – evidence that HKBR decide to ignore but which fit our general pattern.

As for cherry-picking, HKBR use the OECD's *How Was Life?* volumes which analyzed multiple dimensions of living standards over time since 1820 (van Zanden *et al.*, 2021). The portion concerning poverty actually reports *multiple*

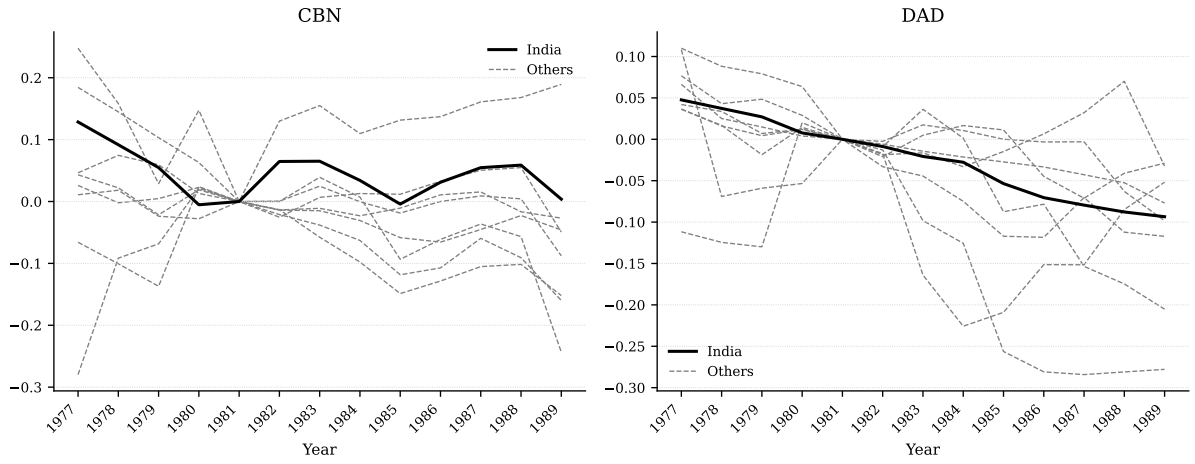


Figure C.6: Changes in Poverty in India and Neighboring Countries Relative to 1981 Levels According to Different Measures

poverty measures which are inventoried at Clio-Infra.eu. The two main measures are "CBN" which stands for the Cost of Basic Needs and "DAD" is the survey-based measure of 1.9$ per day poverty line. In figure **??** below, we showcase India and its near neighbors (Pakistan, Bangladesh, Nepal, Thailand, Sri Lanka, Myanmar, Bhutan, and Maldives) in reference to each's poverty rate level in 1981 (at the time of India's SAP). On the left panel is the CBN measure and the right panel shows DAD measure. The main takeaways here are twofold. First, India is (by 1989) evolving in ways not dramatically different from other countries with both measures suggesting that SAP might not have mattered in the way HKBR imply. Moreover, many of the other countries that secured some of the larger declines in poverty *did* receive SAP (namely Nepal, Pakistan and Thailand). Second, shifting to the DAD measures actually shows that India secured a moderate decline in poverty. Both takeaways suggest a far weaker case against SAP (even maybe the reverse) than HKBR did.

Appendix D Comment on Figure 3

The graphical analysis presented in Figure 3 in HKBR aims to illustrate the evolution of infant mortality in Kenya in relation to the onset of structural adjustment programs (SAPs). While visually compelling, the approach is methodologically problematic because it does not address the central identification challenge: the construction of a credible counterfactual. The dotted line in the figure is obtained by extrapolating pre-1980 trends, implicitly assuming that, absent SAPs, Kenya would have continued along this trajectory. This assumption is neither theoretically justified nor empirically tested, and it ignores the possibility that Kenya's post-1980 trajectory may have diverged for reasons unrelated to SAPs.

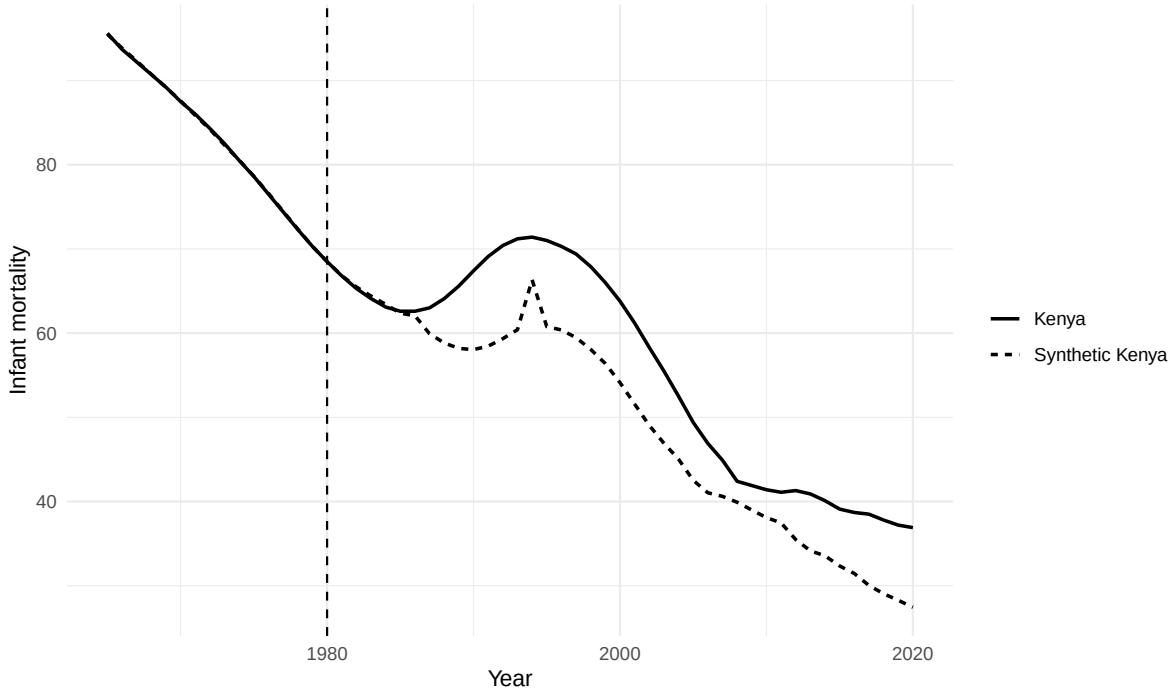
A key difficulty in constructing a counterfactual in this context is that the relevant comparison countries are subject to multiple, overlapping shocks. In particular, the period under study is characterized by the spread of HIV/AIDS—what HKBR mention as a problem—and, in several countries, by episodes of violent conflict or state collapse. These factors are independently known to affect infant mortality. As a result, both SAP exposure and these additional shocks complicate the selection of an appropriate donor pool for comparison. Any empirical strategy must therefore carefully consider which countries constitute a meaningful counterfactual for Kenya.

To address this issue, we implement a synthetic control approach.[9] In a first specification, we follow a purely data-driven strategy and allow all Sub-Saharan African countries to enter the donor pool. Figure D.7 shows the results.

[9]Data from the World Development Indicators.

While this approach produces a close pre-treatment fit, it assigns substantial weight to countries such as Botswana and Rwanda. This is problematic. Botswana, for instance, did not implement SAPs during the relevant period but experienced one of the most severe HIV epidemics globally. Rwanda, in turn, underwent a genocide in the mid-1990s, leading to a dramatic and exogenous disruption of health outcomes. The resulting synthetic control is therefore heavily influenced by countries whose post-treatment trajectories are driven by factors unrelated to SAPs. In this specification, deviations between Kenya and its synthetic counterpart are difficult to interpret causally, as they conflate the effects of SAPs with those of HIV and conflict.

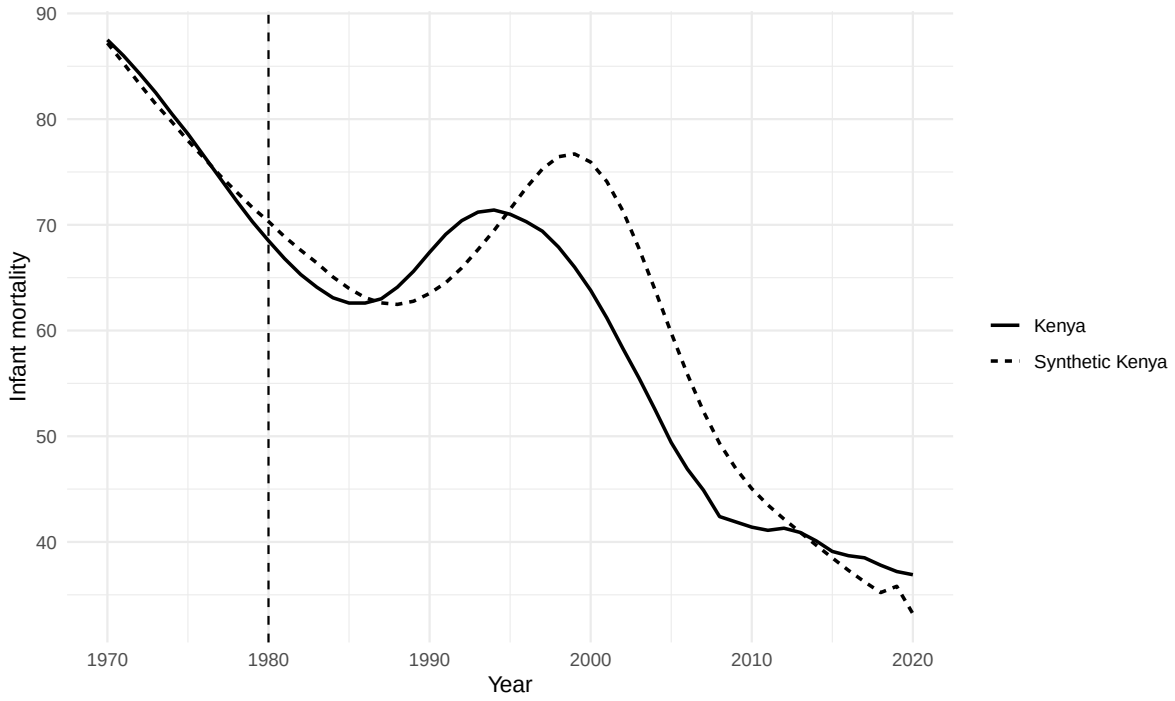
Figure D.7: SCM Kenya with all SSA countries in donor pool



Notes: Synthetic control estimates. The synthetic Kenya is primarily composed of Botswana (0.320), São Tomé and Príncipe (0.257), Cape Verde (0.147), Senegal (0.137), and Rwanda (0.056), with all remaining donor weights below 0.05. The synthetic control is constructed to match Kenya's pre-treatment infant mortality trajectory, using GDP per capita and fertility as additional predictors.

In a second specification, we restrict the donor pool to countries that were exposed to IMF programs or World Bank adjustment loans during the relevant period and exclude countries subject to extreme confounding shocks or with fundamentally different structural characteristics. This restriction is motivated by the need to construct a more policy-relevant comparison group. Results are reported in figure D.8. The resulting synthetic control yields a good pre-treatment fit but differs substantially in its post-treatment dynamics. In particular, Kenya's infant mortality does not increase relative to the synthetic control following the introduction of SAPs; if anything, it declines somewhat faster during the 1990s. This finding contrasts with the narrative suggested by the original figure in HKBR.

Figure D.8: SCM Kenya donor pool resticted to countries without SAPs



Notes: Synthetic control estimates. The donor pool assigns weights of 0.855 to Congo and 0.145 to Côte d'Ivoire. The synthetic control is constructed to match Kenya's pre-treatment infant mortality trajectory, using GDP per capita and fertility as additional predictors.

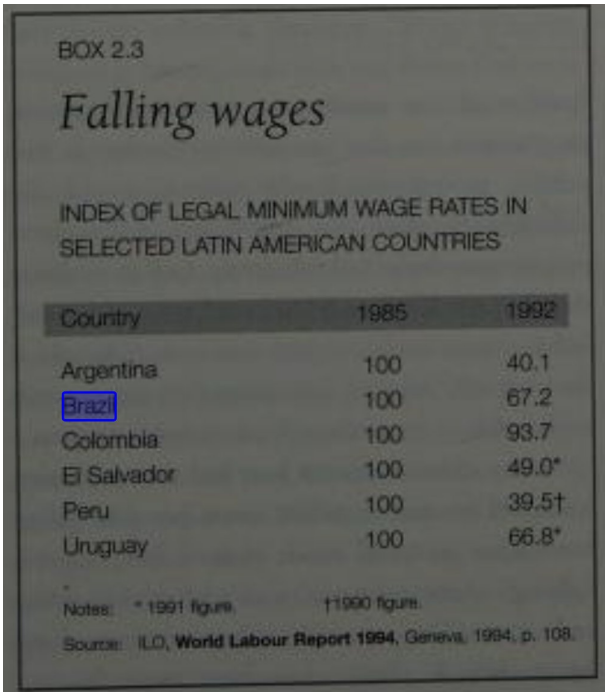
However, even this more carefully constructed specification does not fully resolve the identification problem. First, SAP exposure itself is heterogeneous in timing, intensity, and implementation across countries, making it difficult to interpret the estimated effect as a uniform treatment. Second, the period remains affected by other concurrent shocks, including the HIV epidemic, which cannot be fully accounted for given data limitations in earlier decades.[10] Third, the composition of the donor pool continues to matter for the results, and reasonable alternative restrictions may lead to different conclusions.

Taken together, these findings suggest that the simple projection-based counterfactual used in the original figure in HKBR is not a reliable basis. While synthetic control methods provide a more rigorous framework, their validity depends critically on the choice of donor pool and the broader empirical context. In the case of Kenya, once these considerations are taken into account, the evidence does not support a clear adverse effect of structural adjustment programs on infant mortality.

Appendix E Factual Errors

In their paper, HKBR cite wage collapse figures for Brazile and Columbia (67% and 84%) which are so large that they are implausible at first glance. An 84% real wage decline in Colombia means a worker earning $100 in 1985 earned $16 in real terms by 1995. A 67% decline in Brazil means workers retained 33 cents on the dollar. For comparison, the Great Depression reduced real wages in the United States by roughly 10–20% at its worst. The only peacetime contexts where real wages have fallen by 50–80% are episodes of hyperinflation so severe that

[10] Data on HIV infections is only available from 1990 onward, which is too late to be included in the model.



BOX 2.3

Falling wages

INDEX OF LEGAL MINIMUM WAGE RATES IN SELECTED LATIN AMERICAN COUNTRIES

Country	1985	1992
Argentina	100	40.1
Brazil	100	67.2
Colombia	100	93.7
El Salvador	100	49.0*
Peru	100	39.5†
Uruguay	100	66.8*

Notes: * 1991 figure. †1990 figure.

Source: ILO, **World Labour Report 1994**, Geneva, 1994, p. 108.

Figure E.9: Sources of Wage Data Cited by HKBR

nominal wages could not keep pace with prices. Brazil did experience severe inflation in the late 1980s and early 1990s, but real average wages in Brazil, while genuinely depressed, did not fall by two-thirds.

What explains the discrepecany? Figure E.9 shows the report they cite and it is easy to see that there are two major errors committed. First, the report is not about average wages but rather *minimum legal* wages. As such, it is not average wages that they cite – only minima (contra their claim). Second, there is an error of misunderstanding what index numbers are. They confuse an index level with a percentage change. The table sets 1985 equal to 100, so later values show the level relative to that base year. For example, Brazil at 67 means the minimum wage is 67% of its 1985 level, implying a 33% decline, not 67%. Likewise, Colombia at about 94 implies a decline of roughly 6%, not anything close to 84%. The mistake is to read the index itself as the size of the drop, rather than subtracting it from 100. This misinterpretation mechanically inflates the magnitude of the decline. This is a major factual error (and we thank JP Bastos for pointing it out to us).