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Growth, Interrupted:

How Crises delay Global Convergence

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Growth, Interrupted: How Crises delay Global Convergence
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ABSTRACT: During a major crisis, the transitional dynamics of conditional convergence are unlikely to apply. In this paper, we introduce a Markov chain approach which integrates the study of crises and convergence. We allow upwards and downwards mobility to change when a country enters a crisis regime. We find that conflict and debt crises help to explain the persistence of low relative income, and that the convergence process has changed over time. Faster global convergence in the early 2000s can be attributed partly to fewer and shorter crises, so the multiple shocks after 2020 are likely to have slowed income convergence.

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WORKING PAPERS

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Contents

I. Introduction

This paper seeks to explore, and help to remedy, a striking disjunction between development commentary and econometric research on convergence. Informal commentary routinely discusses fragile states, long-term conflict, debt crises, and concepts such as ‘lost decades’ and ‘growth tragedies’. Yet these ideas have played little role in more than thirty years of empirical convergence research. Many of the econometric studies appeal, explicitly or implicitly, to the idea that countries are continually adjusting towards long-run growth paths, which are themselves described by stable functions of a small set of variables. Even major shocks are experienced as no more than random, short-lived departures from these transitional dynamics; conditional convergence is the main event. This approach seems poorly suited to explaining the recent experiences of, say, Venezuela or Zimbabwe, to give just two possible examples that can stand in for many more.

To address this, we seek to integrate the study of crises and convergence. Using a finite-state Markov chain, we derive transition probabilities between relative development levels, but also allow for crisis-induced regime shifts. The probabilities of upward and downward mobility change when a country is experiencing a major crisis such as political violence or sovereign debt distress. In this way, we extend the approach of Quah (1993) and Kremer et al. (2001). Given differences in mobility between peaceful and crisis periods, overall convergence prospects are influenced by crises. Our work contrasts with growth regressions, which often assume a single growth regime, and with the simpler versions of Quah’s approach, in which movements between income categories are quantified but rarely influenced by other events or variables.

One test of the usefulness of an empirical framework is how many questions it can address. From our simple extension of Quah’s framework, we gain a better sense of how downwards mobility arises, the varying incidence and duration of crises, the difference they make to long-run prospects, and their influence on changes in the convergence process. In all these respects, the approach can deepen understanding of how the world income distribution has evolved and how it can be expected to evolve in the future.

Of course, there is already a vast literature on recessions, debt crises, and conflict, and their consequences; see, for example, Cerra et al. (2023) and Luo et al. (2021) on the scarring effects of recessions. There is also much work on how shocks influence mediumterm growth; see, for example, Dieppe (2020) and Rodrik (1999). Our main contribution is not to add to these existing lines of research, but to ask what crises imply for the evolution of cross-country inequality. Put differently, we seek to extend the econometric study of global convergence in ways that acknowledge the importance of major crises.

Our estimates confirm, in line with a large literature, that crises are more common and last longer at low levels of development. A more novel finding is that crises may help to explain the persistence of low relative income, and even the bimodality or 'twin peaks' result associated with Quah (1993). We also find that recent changes in the convergence process may be closely connected to a lower prevalence and shorter duration of crises.

This points to the importance of studying convergence and crises jointly. It also suggests that recent convergence may be undermined or even reversed by the lingering effects of the global financial crisis of 2007-9, the Covid-19 pandemic, trends in conflict, rising geopolitical tensions and debt distress, and an intensifying trade war.[1]

A crisis can interrupt or halt the growth process of 'normal times', rendering conditional convergence temporarily less relevant. Rather than treating different types of crisis as independent sources of variation in growth, we see a major crisis — from one or more of several possible sources — as switching off the growth process of normal times and sending a country down a different path.

Addressing this empirically in a general way is a challenge, but perhaps the simplest possible approach is to start from Quah (1993) and then enlarge the state space. To be more specific, we extend Quah's framework by making the state space two dimensional: we supplement relative income categories with information on crises. We can then transform the state space into one that

[1] For an analysis of the Covid-19 pandemic and convergence, see Brussevich et al. (2022).

has a single dimension, with a larger number of composite states and extended state definitions. With this device, the analysis becomes straightforward. We can study mobility and future prospects using the same tools as Quah (1993), Kremer et al. (2001) and Feyrer (2008).

The Markov chain approach contrasts with linear regressions. In the case of crises, researchers estimating cross-country regressions often assume that the transitional dynamics of a growth model will continue to apply, with the effects of a crisis then overlaid on top. In practice, in a country experiencing a major crisis, it seems likely that output and its proximate determinants will follow dynamic processes that differ from those in textbook growth models. This is related to another disadvantage of growth regressions, which typically assume that growth determinants can smoothly substitute for one another.[2]

As indicated above, we deliberately aggregate several different types of crises, precisely because we assume that their onset can interrupt the growth process of normal times. In regimes associated with crises, this is sufficient to alter upwards and downwards mobility. This contrasts with a more conventional approach in which crises of different types each have simple additive effects, as if they are independent variables causing their own respective shortfalls from an underlying balanced growth path that continues to influence the dynamics.

As in past work using Quah's approach, several interpretations are possible, and perhaps best held in view simultaneously. At one level, transition matrices are one form of descriptive statistics: they can show, for example, the proportion of countries that experienced a new crisis in the time period considered, along with the proportion in crisis which then restored stability. These descriptive statistics are more informative when allowed, as here, to vary across levels of development.

More ambitiously, we can interpret the estimated probabilities of transitions between states as estimates of underlying structural parameters: they capture the prospects for upwards and downwards mobility and how these prospects vary across ranges of relative income and between

[2] We could draw an analogy between a linear regression and a CES production function in which different growth determinants are perfect substitutes at fixed ratios. For some relevant discussion, see Sirimaneetham and Temple (2009).

crisis states and ‘normal times’. It is then interesting to ask what the estimates of these structural parameters ultimately imply, at least as a thought experiment. As in Quah (1993), we can compute a stationary or long-run distribution over outcomes that would be attained if the underlying process remained stable and ran for a sufficiently long time. Not everyone will be convinced that this structural interpretation is the right one, but even then, the Markov chain approach can complement other ways of describing the data. It reveals tendencies in the recent data that more conventional approaches, tied as they often are to a single regime, may hide.

To construct our crisis regimes, we begin with a binary indicator. Although this may seem restrictive, as if crises are all alike, the approach is richer in this, and consistent with crises that vary widely in severity and duration. If we have four relative income categories, we will have an additional four crisis regimes. This allows the probabilities of transitions into and out of crisis to vary with development levels, as do the effects of crises on mobility. It is this which allows us to capture the stylized fact that, on average, crises are more likely, last longer, and have more severe effects at lower levels of development.[3] And even for a given development level, working with transition probabilities means that crises are allowed to vary in length. Some crises will be brief and have only a short-lived effect on mobility, while others will last much longer and lead to more serious output losses.

The approach can be used to analyze the recent changes in convergence documented in Dieppe (2020), Kindberg-Hanlon and Okou (2020), Kinfemichael and Morshed (2019), Kremer et al. (2022), Patel et al. (2021), Roy et al. (2016) and Startz (2020). We find that even a flexible model, with many parameters, shows evidence of parameter instability over time. By comparing results across subperiods, we confirm that convergence is more evident in the recent data than formerly. A newer result attributes this, at least partly, to a lower incidence and shorter expected duration of crises.[4] But since 2020 and the Covid-19 pandemic, the world has been hit by multiple further shocks, including geoeconomic fragmentation, new wars and conflicts, and most recently an

[3] In this respect, note that we will consider indicators of both relative development (relative income and TFP) and absolute development (the capital-output ratio).

[4] In work on growth accelerations and reversals, rather than convergence, Gruss et al. (2020) find that growth benefited from a more benign external environment.

escalation in trade tensions. These could work to inhibit convergence, in line with the earlier effects of crises that we quantify in this paper.

The paper has the following structure. The next section provides some background and relates our work to the literature. Section 3 describes the methods and section 4 the data. We take a first look at upwards and downwards mobility in section 5, followed by the main results in sections 6 and 7. Aspects of robustness are explored in section 8 before section 9 concludes. The appendix includes lists of countries in the main samples that we use.

II. Background

Few questions in growth econometrics have been studied more intensively than convergence. Its study is not merely descriptive, or a way to understand recent trends. It matters for development policy, the long-term strategies of aid donors and global institutions, and scenarios for future emissions of greenhouse gases. It also casts some indirect light on the usefulness of alternative models of aggregate development, and how they might be revised or extended in the light of the data.

In the first wave of growth regressions, the most common framework was ultimately based on the transitional dynamics of the capital-output ratio, as in neoclassical growth models with exogenous or endogenous saving; see, for example, McQuinn and Whelan (2007). Later work recognized that conditional convergence may be just one regime among several; periods of stagnation or even outright collapse are common, as Pritchett (2000) forcefully pointed out. Researchers adopted increasingly eclectic approaches, seeking to isolate the determinants of discrete growth acceleration episodes, and studying collapses or reversals as in, for example, Gruss et al. (2020), or using duration models to study how growth spells can be sustained; see Temple (2021) for a review and references.

This work made clear that, when studying how an entire distribution is changing, the answer should not simply be reduced to whether convergence is present or absent. In practice, it is often more interesting to ask how many countries are converging, how fast, and why relative income may be persistent over some of its range (as in the hypothesis of a middle-income trap). One stylized fact is that, at the lower end of the scale, relative income is especially persistent: the poorest countries have rarely improved their position relative to the US or other leading economies.[5]

Past work has sometimes examined what Barro and Sala-i-Martin (1990, 1991) called sigma convergence: the term for a downwards trend in cross-country inequality, as tracked over time by a statistic such as the standard deviation of log income per head.[6] Although less prominent than beta convergence, this provides a more direct summary of what is happening to the dispersion of income per head and other outcomes. Dispersion may decline depending on the initial conditions, movements towards steady-state growth paths, or narrowing dispersion of those paths.[7]

Over the last sixty years, sigma convergence has not been the norm. Kremer et al. (2022) find (their figure 2) sigma divergence between 1960 and 2010, before some convergence in the 2010s associated with faster growth in the developing world and slower growth in the rich countries. For a set of 100 countries, Jones (2016, figure 27) finds that the standard deviation of log GDP per person rose by about 35% between 1960 and the late 1990s, before declining slightly. Between 1960 and 2007, Kremer et al. (2022, figure 3) find that growth for the countries below the first quartile was often slower than for countries above it.

One reason for divergence seems to be the persistence of relative income and TFP for the poorest countries (Imam and Temple 2025). Perhaps the default response would cite low-quality institutions, deterring investments of various forms. But even a country with good institutions may grow only slowly, or lose ground, when facing a crisis such as external war, civil war, or a sovereign debt crisis. And as we noted in the introduction, the informal commentary on development often points to the potential importance of crises in explaining medium-term trends, but this emphasis has not been mirrored in econometric research on convergence.

[5] Papers on the persistence of relative income include Canova and Marcet (1995), Arias and Wen (2016), Kindberg-Hanlon and Okou (2020), Mountford (2025) and Imam and Temple (2024b, 2025). See also Kremer et al. (2001, p. 290) and Müller et al. (2022). In line with much of the literature, but with some loss of precision, we use the term income as shorthand for output per head.

[6] For discussions of convergence concepts and/or estimation, see Galor (1996), Johnson and Papageorgiou (2020), Johnson et al. (2025) and Smith (2024).

[7] For discussion see, for example, Smith (2024). On changing steady-state paths, see Mountford (2025).

We may not know how to engineer growth where it is absent, but as Easterly (2001) noted, we certainly do know how governments can kill growth through mismanagement. When this happens, the economy will lose ground relative to frontier economies. The capital-output ratio will not be moving smoothly towards a long-run equilibrium, and an empirical model more flexible than a conditional convergence regression will be needed. In such a world, conditional convergence regime is likely to be just one regime among several.

In search of a more general approach, Quah (1993) applied finite-state Markov chains to data on relative GDP per head. This allows flexibility in the dynamics and patterns of mobility, rather than modeling only the conditional mean as in a linear regression. The approach can be used to quantify the extent of various forms of mobility within the distribution, project how the marginal distribution over the states will evolve, and formally test whether the convergence process has changed. Building on Quah's work, Feyrer (2008) used Markov chains to study transitions for the proximate determinants of GDP per head: relative TFP, the capital-output ratio, and human capital. Recently, Imam and Temple (2024b, 2025) updated that analysis, taking advantage of another thirty years of data and proposing new summary statistics. Capital intensity and human capital were found to be converging, but this has disguised a lack of improvement in relative TFP.[8]

Another strand of literature uses models with more than one growth regime but treats the current state as unobservable. This work includes Jerzmanowski (2006), Kerekes (2012), Morier and Teles (2016), and Startz (2020), among others. In these papers, the growth paths feature discrete shifts between fundamentally different regimes, according to a stochastic process that is not observed directly. In the long term, when we will have many more years of data, this approach is likely to dominate. But it is data-hungry, so at present it is typically implemented using annual data, which risks conflating medium-run and long-run growth phenomena with short-run cyclical dynamics. A second drawback is that treating the state as unobservable limits the ability to consider a large state space.[9]

[8] This tallies with the earlier findings of Gallardo-Albarrán and Inklaar (2021), based on long-term development accounting: they found that differences in TFP account for an increasing share of the international variation in output per worker.

[9] Morier and Teles (2016, p. 1563) note that, in their framework, allowing four states appears to ask too much of the available data. Jerzmanowski (2006) and Kerekes (2012) study models with four states: crisis, stagnation, steady growth and 'miracle' growth.

For the questions we are interested in, there may be a case for experimenting with observable states, as in Quah (1993) and much of the work that followed, such as Kremer et al. (2001), Fiaschi and Lavezzi (2003, 2007) and Feyrer (2008). Given that we can observe the incidence of crises directly, at least to some extent, we can then adopt models with a larger state space and focus on transitions over four or five-year intervals. This approach is easy to implement, report and interpret. It yields a flexible but transparent framework for exploring how major crises influence the path of the world cross-country distribution.

One paper which uses unobservable states, but is otherwise close in spirit to ours, is Startz (2020). His Bayesian regime-switching model considers three possible states for growth, one of which is divergence from the frontier, corresponding to a 'growth disaster' regime. Our work complements his, given some differences between the two analyses. Startz models output as a univariate process, while we link its dynamics to data on conflict and sovereign debt crises; in this respect we substitute data and economic structure for statistical structure. Second, to keep the number of parameters manageable, Startz (2020, p. 7) assumes that — conditional on the current state — catch-up and divergence speeds, and the probabilities of switching between states, are common for all countries. In contrast, because we have as many crisis regimes as development levels, we allow the probabilities of transitions into and out of crisis to vary with development levels. We also allow the effects of crises on mobility to vary. In the longer term, as more data become available, there will be scope to combine ideas from the two papers, and with methods from other recent work such as Müller et al. (2022).

Our analysis will identify crisis regimes as times with either political violence or a sovereign debt crisis. For readers more familiar with the experiences of developed countries, it may seem surprising that we do not give more emphasis to financial crises, notably banking crises and currency crises. In the robustness section of the paper, we will examine results based on defining crisis regimes more broadly.

III. Methods

Growth regressions have well-known problems, including restrictive parametric assumptions and sensitivity to outliers and measurement error (Durlauf et al. 2005, Temple 2021). All of these problems are likely to limit the usefulness of beta-convergence regressions. As in Quah (1993), we instead study income mobility using a finite state Markov chain. Since the basics of Markov chains are well known, we describe them only briefly, drawing on Imam and Temple (2024a), which in turn closely follows Stachurski (2009).

Consider a series $\{X_n, n \geq 0\}$ in discrete time, with a discrete state space S with states 1, … S. We consider a transition matrix P=[p_{ij}] where $p_{ij} = P\{X_n = j | X_{n-1} = i\}$ for all $i, j \in S$. The elements of P are non-negative and each row sums to one; the individual elements are probabilities of transitions between states. The maintained assumption in a first-order Markov chain, known as the Markov property, is that the transition probabilities depend only on the current state and not on the earlier history of the process. We discuss this in more detail shortly.

Denote the marginal or unconditional distribution over the states at time t by a row vector ψ_t.[10] Over time the evolution of this marginal distribution can be described by

$$\psi_{t+1} = \psi_t P$$

It can be shown (for example, Stachurski, 2009, theorem 4.3.5) that every Markov chain on a finite state space has at least one stationary distribution. Each stationary distribution will satisfy $\psi^* = \psi^* P$. When ψ^* is unique, this will be the long-run outcome. The individual elements of the row vector ψ^* indicate the proportions of time the process will spend in each state if the process runs for a long time. But depending on the elements of the transition matrix P, the process will converge slowly or quickly to the long-run equilibrium, and the nature of the stationary distribution will be more or less sensitive to the individual transition probabilities. Also note that relative income can be stationary even if the individual output series are non-stationary; for a related analysis, see Evans (1996).

[10] For a more rigorous treatment, see Stachurski (2009, pp. 74-76).

In their study of output per head, Kremer et al. (2001) suggested that the Markov property did not hold in annual data, but they found no evidence to reject it for five-year intervals. In our case, since we introduce crises into the analysis, the Markov property may be less likely to hold, and we address this in several ways. We allow the incidence and duration of crises — reflected in the transition probabilities into, and out of, crisis states — to vary with relative development levels. But this may not be enough to sustain the Markov property, and it is perhaps an open question whether repeated crises in a given country arise because, say, low-income countries are especially prone to them, or because a crisis today raises the chances of a sequel in later decades. It is the latter that would undermine our analysis. With this in mind, we will examine the support for the Markov property for each outcome variable we consider.

Relative to the early studies, we now have a lot more data. Quah's data ended forty years ago, in 1985; Feyrer's data ended in 1989; and the data of Kremer et al. (2001) ended in 1996. In our case, even though we use four-year intervals rather than annual data, our results often draw on more than a thousand transitions. This means that the probabilities of even quite rare transitions can be estimated with some precision.

To allow straightforward interpretation of the results, we translate relative GDP per head (and other outcomes, such as relative TFP) into discrete categories. One drawback of the approach is that translating a continuous variable like income or TFP into discrete categories can distort the findings (Bulli 2001). An alternative approach uses stochastic kernels as in Quah (1997), Johnson (2005), and Barseghyan and DiCecio (2011). But such kernels are themselves noisily estimated in cross-country samples and, for the economic questions of most interest here, the results are much easier to report and interpret if we use discrete categories.

In both Quah (1993) and Feyrer (2008), development categories are defined relative to the world mean. As Pearlman (2003) pointed out, this creates scope for an internal inconsistency, since the unconditional distribution over states may tend towards one in which all countries would — impossibly — be above the world mean. Kremer et al. (2001) also discussed this problem, and responded by measuring GDP per head relative to the mean of the five richest economies. Along similar lines, we avoid the issue by measuring output per head and TFP relative to the mean of the G7 economies. An advantage of using the G7, rather than simply the richest countries, is that the latter sometimes include countries that specialize in oil and gas, and see major swings in their national accounts as energy prices change.

We then need to choose threshold levels that define the categories. As Imam and Temple (2024b) argue, a natural stipulation is that a country growing at a constant relative rate should take the same amount of time to traverse each of the intermediate income categories (not the highest or lowest). We use these 'constant growth thresholds' in the analysis of output per head. In the cases of relative TFP and the capital-output ratio, we use a different approach to help ensure that each state is well represented in the data.

As mentioned earlier, if we are willing to treat the estimated transition probabilities as structural parameters, we can project outcomes over long horizons. Past work has suggested that such projections can be sensitive to small changes in the transition probabilities, especially for variables where convergence is slow.[11] To get a sense of whether our estimates of the long-run distribution are noisy, we will report bootstrapped standard errors for this distribution, as in Feyrer (2008).

In addition, we think it is good practice to report the absolute numbers of transitions. But we should dispel some potential misconceptions about transition counts, and low counts in particular. As noted in Imam and Temple (2024a), these are not always a problem. If a state x is observed many times in the data, but is followed by state y only a handful of times, this should be reliable evidence that the probability of moving from state x to state y is low, and this probability can be precisely estimated. A more serious problem arises when a given state is observed only rarely in the data, and so we select our category thresholds so that each state does arise quite frequently.

To understand which transition probabilities can be precisely estimated, we report asymptotic standard errors based on Anderson and Goodman (1957). They derived the asymptotic variances of estimated transition probabilities p_{ij} for a Markov chain; for a transition from state i to j, they showed that

[11] See Imam and Temple (2024a) for discussion and references.

$$\sqrt{n_i}(\hat{p}_{ij} - p_{ij}) \longrightarrow N(0, p_{ij}(1 - p_{ij}))$$

where n_i is the number of observations of state i prior to the final period.[12] Since we often have more than a thousand transitions, we will see that these asymptotic standard errors usually imply quite precise estimates of the transition probabilities.

Quah (1993) identified an emerging tendency towards 'twin peaks' in the stationary distribution of relative GDP per head. For that variable, upwards and downwards mobility was found to be limited, and convergence to a stationary distribution rather slow (Kremer et al. 2001). But, as seen in Imam and Temple (2024b), convergence is typically faster for some of the proximate determinants of GDP per head. Hence, the stationary distributions for those variables should be more robust. As well as reporting stationary distributions, we report 20-year and 100-year projections, which will typically be less sensitive to individual transition probabilities (Kremer et al. 2001).

IV. Data

We first discuss the length of the time intervals we consider. Papers in the Quah (1993) tradition sometimes use annual data, but Kremer et al. (2001) argued that the Markov property for cross-country data is more likely to hold with five-year intervals. For the same reason, Imam and Temple (2024b, 2025) also used five-year intervals. In this paper, the dataset spans a shorter time, because of the shorter span of the financial crisis data we use. Then, for our purposes, moving to four-year intervals has two advantages. It gives us more transitions to work with and allows us to compare results with four-year and eight-year intervals to help evaluate the Markov property. Later in the paper, we will compare some of the results to those using five-year intervals.

Our indicator of a major crisis is a binary variable coded as one for a time interval containing either an episode of conflict or a sovereign debt crisis (or both) and zero otherwise. The conflict data are from the Polity dataset on Major Episodes of Political Violence, 1946-2018, from the Center

[12] In the growth literature, this result was previously used by Proudman et al. (1998) and also noted in Kremer et al. (2001).

for Systemic Peace. In that dataset, such episodes are defined by 'the systematic and sustained use of lethal violence by organized groups that result in at least 500 directly-related deaths over the course of the episode' (Marshall 2019). We use the variable 'actotal' to create a binary variable which is equal to one for a time interval containing such an episode, whether arising from interstate war or societal war (such as civil war) and zero otherwise.

The debt crisis variable is taken from Nguyen et al. (2022), who build on the banking crisis database of Laeven and Valencia (2020) and add information on sovereign debt crises and currency crises. The Nguyen et al. database is not a balanced panel and has uneven coverage of different types of crisis. Moreover, we have to assess the presence or absence of a crisis over each time interval (for example, each interval of four years) over which we track output transitions. The conventions we adopt are as follows. Where either the debt crisis data or the conflict data (or both) record a crisis for at least one year within the time interval, we code that as unity for our crisis indicator. Where the debt crisis and conflict variables are both zero for all years of the interval, we code the indicator as zero for that interval. In all other cases, our indicator is coded as missing.

The final state classifications will collapse the two-dimensional state space into one dimension, using composite states with expanded state definitions. In what follows, typically this has four peaceful states (corresponding to four possible output categories) and four crisis states (corresponding to the same four output categories) so that we have eight states in total.

Since we are combining data on output and crises, and given that we use time intervals longer than a year, the timing assumptions need care. We have to decide how to arrive at a crisis indicator based on annual data.[13] Our approach is easiest to understand with a concrete example: say, for the intervals 2011-15 and 2015-19 when using a four-year time interval. We assess the output category at the start of each interval. In this case, a country will move from an output category (say, the lowest) measured in 2011 to one measured in 2015. If there is no crisis in 2011-14 or 2015-18, the country will be classified as in a peaceful state throughout. If there is a crisis

[13] For a parallel choice see, for example, Rancière et al. (2006, p. 3335).

at some point within 2011-14 but none within 2015-18, there will be a transition from a crisis state to a peaceful state; and if there is no crisis within 2011-14 but at least one within 2015-18, there will be a transition from a peaceful state to a crisis state. This approach ensures that particular country-year observations (including crisis years) are not used more than once. It also means that, for a transition into a peaceful state, the peace should be sustained for four years; similarly, for a transition to be from a peaceful state, the peace should have held for four years.

The data on GDP are taken from version 10.01 of the Penn World Table (PWT; Feenstra et al. 2015).[14] The panels we use are balanced, usually for the time period 1975-2015. This means that we can use four-year or eight-year subperiods. When using four-year intervals, ending the analysis in 2015 means that we use output data no later than 2015 and crisis data no later than 2018; hence the results are not affected by the Covid-19 pandemic.

When studying outcomes in the PWT data, rather than use real GDP per head, we use real GDP per adult of working age (15-64), where the data on the working-age population are taken from the World Development Indicators. This approach was used in Mankiw et al. (1992) and Imam and Temple (2024b), and may provide a better measure of productivity for our purposes than using either GDP per head or GDP per worker. Although data on GDP per worker are available, the way to define a 'worker' appropriately is often unclear in developing economies with a large informal sector.

Our samples for the various analyses include most of the world's larger countries; the balanced panel we use for output per head covers countries representing almost 89% of world population in 2015. The main omissions are Russia and the other successor states of the USSR. Lists of countries in the main samples can be found in the appendix.

[14] In some of our earlier work, we also used the Maddison Project Database, but its longer historical coverage brings no advantage here; it is data availability for financial crises which is typically the binding constraint.

V. A first look

In the later analysis, we will look at transition matrices for GDP per head. As in Imam and Temple (2024b, 2025), we follow Feyrer (2008) and also examine the convergence of some proximate determinants of output per head, here relative TFP and the capital-output ratio.[15] Since the results are detailed, in this section we first look at some simpler summaries. These can be seen as purely descriptive, but informative about our questions of interest. Since this is just a first look, we defer a full discussion of the setup until the next section. Our aim here is to examine the extent to which the presence of a crisis changes the prospects for either upwards or downwards mobility, and how this varies across different categories of development.

Briefly, we take output per working-age adult (in PPP terms) relative to the average of the G7 economies, the Penn World Table measure of relative TFP, and the capital-output ratio. We divide each of these outcome variables into four categories or levels. These range from level 1 (the lowest level of development) to level 4 (the highest). We then compute the probabilities of moving downwards from each of levels 2-4, and the probabilities of moving upwards from each of levels 1-3. But in making these calculations, we separate the observations depending on whether or not there is a crisis, so that we can compare upwards and downwards mobility between crisis and non-crisis states.

Table 1: Mobility and Crisis, 1975-2015

	↓2	↓3	↓4	↑1	↑2	↑3
Relative Y/L						
No crisis	0.051	0.071	0.032	0.091	0.220	0.152
Crisis	0.121	0.156	0.172	0.054	0.098	0.067
Relative TFP						
No crisis	0.109	0.236	0.280	0.160	0.156	0.142
Crisis	0.333	0.383	0.481	0.071	0.090	0.149
K/Y ratio						
No crisis	0.011	0.085	0.048	0.400	0.359	0.346
Crisis	0.031	0.105	0.132	0.262	0.264	0.215

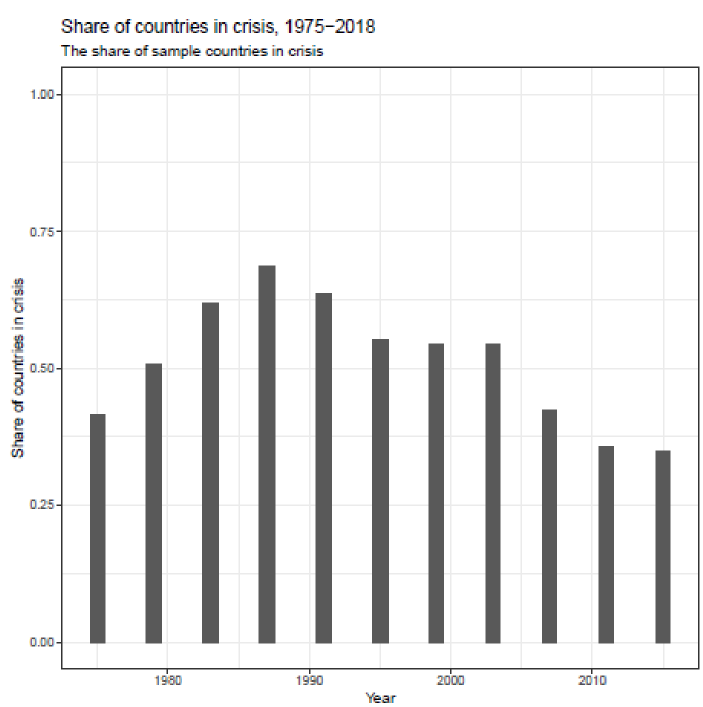
A preview of later results, for mobility downwards and upwards between development categories from 1 (lowest) to 4 (highest). For more details see section 5 of the text.

[15] We do not consider human capital outcomes, since their time paths are likely to be determined largely by long-term government education policies rather than short-run or medium-run crises.

The results of this simple exercise can be seen in Table 1. In the case of relative income, crises increase the risk of downwards mobility in all three cases (that is, downwards from levels 2-4) and similarly reduce the prospects of upwards mobility. Similar patterns obtain for relative TFP (the highest category aside) and the capital-output ratio. These simple descriptive statistics already suggest that crises are likely to influence convergence outcomes. The later sections of the paper will investigate this further.

Given the effects of crises on mobility, it would be natural to ask how many countries are classed as in crisis for each of the four-year intervals we consider. This is shown in Figure 1, for the balanced panel of countries used for the results on output per head. It can be seen that the incidence of crises peaks in the late 1980s, and more than half the sample countries are in crisis in the 1980s and 1990s. More recently, the share has declined, and that will be important in our later analysis of whether the convergence process has changed.

Figure 1: Share of countries in crisis in each four-year interval



That said, we should acknowledge a limitation of the approach. The Markov chains we estimate relate development categories and a crisis indicator, the latter based on four years of data, to outcomes for both in the previous period. This is open to the criticism that, contemporaneously, mobility affects crises as well as vice versa; this makes it harder to isolate the effect of crises on mobility. For example, a decline in GDP per head may be one force that quickly precipitates a crisis, such as a debt crisis or civil war. The association we find between crises and downwards mobility may conflate two very different types of effect.

This argument is harder to make when we look at the prospects for upwards mobility, however. Although growth in output may be associated with a credit boom, that would typically result in a domestic banking crisis rather than the wars and debt crises that determine our crisis indicator.[16] Results on upwards mobility are more likely to be causal rather than merely descriptive.

When we turn to the transition matrices, in which relative development levels and crises evolve jointly, we may be overstating the costs of crises for downwards mobility, but less so for upwards mobility. Convergence outcomes depend on both forms of mobility. Given the prevalence of crises, and that crises can interrupt the growth process for years or even decades at a time, that mechanism deserves more attention in the convergence literature than it has received to date, even if we risk overestimating its role.

One form of downwards mobility in relative TFP is especially worth noting. Adverse world price shocks for resource-dependent countries could lower measured relative TFP and also precipitate a major crisis; in this case, it is the price shock, not the later crisis, which first drives downwards mobility. We will investigate differences between resource-rich and resource-poor countries later in the paper. Perhaps surprisingly, our main results do not seem greatly affected by this distinction.

VI. Output results

In this section, we look in detail at transitions for real GDP per working-age adult. As before, the time period is between 1975 and 2015, so the final output observation is from 2015 and the last crisis observations from 2015-2018. Since we use four-year intervals, we have eleven

[16] On credit booms see, for example, Castro and Martins (2019). The robustness section will discuss the role of financial crises.

observations of states per country. This leads to data on a total of NT =118 × (11 − 1) = 1180 transitions. Before we report the results, we note a qualification: since output fluctuates at short horizons, our results will typically overstate the mobility of *potential* output, the concept of most interest to growth economists. But since we find that mobility is in any case limited, smoothing out short-run fluctuations would tend to reinforce that conclusion.

Similarly to our companion papers on the middle-income trap and the persistence of low relative income, Imam and Temple (2024b, 2025), we often work with constant growth thresholds, here (0.08, 0.16, 0.32) where the numbers are relative to the average of the G7 economies. One difference from our earlier work is that we use four states rather than five for output per head. Given the second dimension of the original state space — the binary crisis indicator — this implies 4×2 = 8 states in total, which is more manageable for ease of reporting and interpretation than working with 5 × 2 = 10 states.

The first results are shown in Table 2. The entry in a row and column indicates the probability of moving from the row state to the column state. The individual transition probabilities are derived by asking what proportion of countries in a given row state at time t are found in a given column state at time $t + 1$. By a standard argument, these are the maximum likelihood estimates, as used in Quah (1993) and Kremer et al. (2001) among others.[17] We report the transition matrix in a grid, in which absent transitions (transition probabilities equal to zero) are represented by a blank cell, while probabilities of at least 0.10 are shown in bold.

Table 2 and later tables also report the final observed distribution (that is, the one in the final period of the sample) and the marginal distributions to be expected 20 years and 100 years later, based on iterating the transition matrix. The 100-year projections have the advantage that they will be less sensitive than the long-run distribution to individual transition probabilities. As in Quah (1993), we should say that these projections are not serious attempts at long-term forecasts— those would require more variables and contextual factors to be taken into account — but rather ways of revealing tendencies within the recent dynamics, as a form of thought experiment.

[17] For the derivation of the ML estimator see, for example, Norris (1997).

Table 2: Four-year GDP transitions, PWT data, 1975-2015

Transition matrix	<0.08	<0.16	<0.32	<∞	<0.08C	<0.16C	<0.32C	<∞C
<0.08	**0.682**	0.091			**0.227**			
	(0.070)	(0.043)			(0.063)			
<0.16	0.017	**0.458**	**0.169**		0.034	**0.271**	0.051	
	(0.017)	(0.065)	(0.049)		(0.024)	(0.058)	(0.029)	
<0.32		0.036	**0.661**	**0.143**		0.036	**0.116**	0.009
		(0.018)	(0.045)	(0.033)		(0.018)	(0.030)	(0.009)
<∞			0.023	**0.909**		0.003	0.006	0.058
			(0.008)	(0.016)		(0.003)	(0.004)	(0.013)
<0.08C	0.077	0.014			**0.869**	0.041		
	(0.018)	(0.008)			(0.023)	(0.013)		
<0.16C		0.069	0.017		**0.121**	**0.711**	0.081	
		(0.019)	(0.010)		(0.025)	(0.034)	(0.021)	
<0.32C		0.022	**0.141**	0.037		**0.133**	**0.637**	0.030
		(0.013)	(0.030)	(0.016)		(0.029)	(0.041)	(0.015)
<∞C			0.022	**0.172**		0.011	**0.140**	**0.656**
			(0.015)	(0.039)		(0.011)	(0.036)	(0.049)
$\gamma = 65.3$								
Last period	0.076	0.076	0.153	0.347	0.119	0.102	0.076	0.051
20 years ψ_{T+20}	0.045	0.040	0.104	0.348	0.170	0.126	0.099	0.068
100 years ψ_{T+100}	0.052	0.040	0.092	0.319	0.211	0.127	0.094	0.065
Stationary ψ^*	0.059	0.042	0.090	0.292	0.235	0.131	0.092	0.060
(s.e.)	(0.027)	(0.011)	(0.020)	(0.074)	(0.075)	(0.029)	(0.019)	(0.018)
$NT = 1180$	$N = 118$	$T = 10$						

Table shows transitions for GDP per working-age adult relative to G7 average, using PWT version 10.01. Transitions from row state to column state; in state labels, 'C' indicates the presence of a crisis. Anderson-Goodman asymptotic standard errors shown in parentheses. Bootstrapped standard errors for the stationary distribution also shown. For more details see the text.

Looking at Table 2, we can first use the estimated matrix to assess the incidence and duration of crises. If we start with the upper-right quadrant, these transitions reflect new entries into a crisis. By summing, row-by-row, the transition probabilities in the four columns on the right, it can be shown that transitions into a crisis are most likely for the two lowest categories of relative income, followed by the third lowest, and then lowest in the top category (see Table 3). In other words, low-income countries are much more prone to crises, a familiar stylized fact from the literature.

Table 3: Four-year Y/L transition counts, 1975-2015

Counts	<0.08	<0.16	<0.32	<∞	<0.08C	<0.16C	<0.32C	<∞C
<0.08	30	4			10			
<0.16	1	27	10		2	16	3	
<0.32		4	74	16		4	13	1
<∞			8	311		1	2	20
<0.08C	17	3			193	9		
<0.16C		12	3		21	123	14	
<0.32C		3	19	5		18	86	4
<∞C			2	16		1	13	61
Mobility		↓2	↓3	↓4	↑1	↑2	↑3	
No crisis		0.051	0.071	0.032	0.091	0.220	0.152	
Crisis		0.121	0.156	0.172	0.054	0.098	0.067	

Transition counts for output per head, from row state to column state, for a balanced panel 1975-2015. 'C' indicates the presence of a crisis. For other notes see Table 2.

The transition matrix also contains information on the duration of crises. The bottom left quadrant reflects transitions out of crisis into stability. By summing the transition probabilities row-by-row for this submatrix, it can be shown that the probability of exiting a crisis is lowest for the two lowest relative income categories. In other words, low-income countries are not only more prone to the onset of crises, but those crises last longer.

We also report an asymptotic measure of convergence speed, following Kremer et al. (2001, p. 290). The measure is defined as:

$$\gamma \equiv -\frac{\log(2)}{\log|\lambda_2|}$$

where λ_2 is the second largest eigenvalue (after 1) of the transition matrix. This gives the number of periods needed to halve the norm of the difference between the current distribution and the stationary distribution; the lower this number, the faster is convergence. Note that, as an asymptotic rate, this does not take the initial distribution into account, and so actual convergence will sometimes be faster than this. We adjust γ for the fact that our intervals are four years apart.

The stationary distribution of relative income, reported at the foot of the table, suggests a degree of bimodality: the mass is mainly distributed across the fourth state (highest relative income, no crisis) and the fifth (lowest relative income, crisis). At first glance, this suggests that crises could help to explain the ‘twin peaks’ pattern that Quah (1993) identified as a long-run tendency of the data. Exposure to repeated crises may be part of the reason that low relative income is so persistent, and why some countries fail to catch up.

Note, however, that although our framework allows crises to be more likely, and last longer, at lower levels of development, we do not explore the causal effects at work. The perspective taken in Collier (2007) is that some developing countries are trapped in various ways. Our framework allows low-income countries to be more crisis-prone, but does not acknowledge risk factors other than low income, or disentangle them from the effect of crises on income. Investigating this in more depth would require alternative empirical models to the ones we present here, although the robustness section will consider the role of dependence on natural resources.

Given that we have introduced crises into the analysis, and that a crisis today may influence the probability of a crisis in the future, it is natural to ask whether the first order Markov property holds here, as we have assumed thus far. In principle, we could test whether the process is second-order Markov rather than first-order by computing transition probabilities which use outcomes at $t-2$ as well as $t-1$. The problem with this approach is that it greatly increases the number of parameters: with eight states, our three-dimensional transition array would have 83 = 512 parameters. This means that a formal test will be hard to implement reliably until more years of data are available.

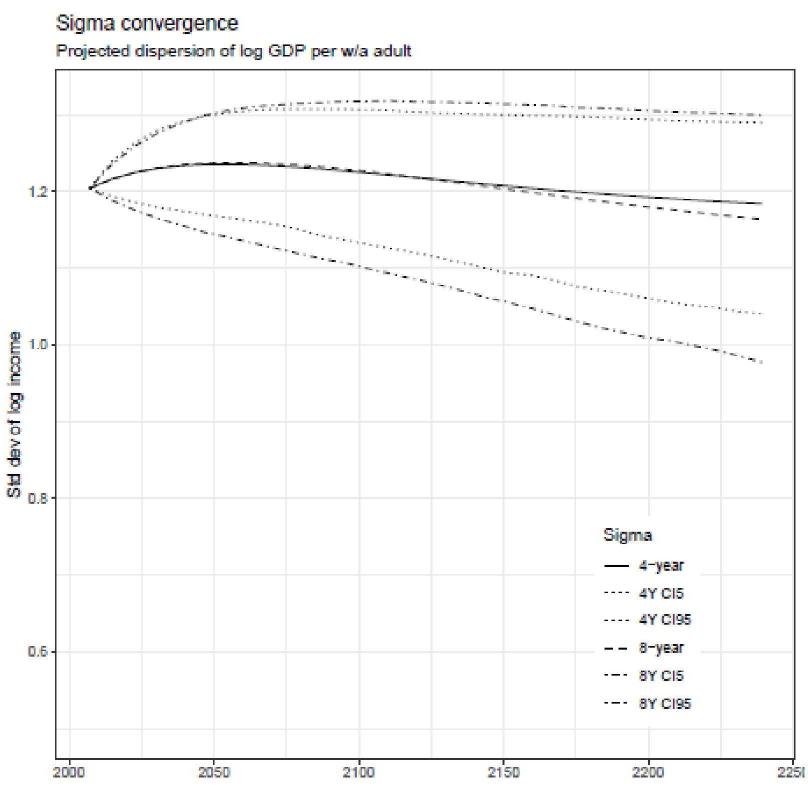
With this in mind, we instead draw on the approach used by Kremer et al. (2001). They estimate Markov chains from five-year and ten-year intervals, and then compare informally the square of the five-year transition matrix with the ten-year matrix. If the two are similar, this suggests (though does not prove) that five-year intervals are sufficient to ensure that the process is first-order Markov. In our case, we do this for four-year and eight-year intervals, using output data between 1975 and 2007.[18] The implied eight-year transition matrices are similar, but rather than report the details, we present a graphical summary of what the numbers imply. Again we borrow an idea

[18] The shorter period, of 32 years, is divisible by four and eight, and we end in 2007 rather than 2015 because we do not have crisis data for the whole period 2015-2022. See section 4 for more on the timing assumptions.

from Kremer et al. (2001, pp. 290-2) and examine sigma convergence: the path of cross-country inequality, as tracked by a statistic such as the standard deviation of log GDP per head.

At this point, for both four-year and eight-year intervals, we convert the eight-state transition matrix into one with four states, corresponding to the four income categories, no longer distinguishing states by the presence or absence of a crisis. We then use this four-state transition matrix to project dispersion forwards, in our case over more than two hundred years. This is easily done if, for each country, we translate the four income categories into relative income levels using the average relative income of each category in the final year of output data we use, here 2007. We can then calculate the projected standard deviation of log income at each date.

Figure 2: Transition path for cross-country income inequality



The results of the exercise are shown in Figure 2. This plots the projected dispersion of log relative income over time, and also shows bootstrapped 90% confidence bands for this projection.[19] The

[19] In the bootstrap, we resample using the eight states observed in the data, but then again carry out each projection by converting the eight-state matrix into one with four states.

bands are slightly wider for eight-year transitions, probably because the number of transitions used to estimate the transition probabilities is then smaller. It can be seen that the path of cross-country inequality implied by four-year transitions, the central solid line, is very close to that implied by eight-year transitions, the central dashed line. This is not definitive proof that the first-order Markov property holds, but it helps to shift the burden of proof towards those who question that assumption. Interestingly, the 90% confidence bands suggest that a range of outcomes is possible for this measure of cross-country inequality, which may increase for a long span of time before it begins to decline.[20]

The next issue to consider is whether the transition matrix is temporally homogeneous, which requires the transition probabilities to be constant over time. We have enough data to estimate transition matrices for two subperiods, 1975-1995 and 1995-2015, again using four-year intervals within each subperiod. When this is done, it provides striking evidence that the convergence process and what it implies have changed quite dramatically. A Pearson goodness-of-fit test of a restricted model, in which the transition probabilities are constant across the two subperiods, rejects the null at the 1% level.[21] This indicates a clear change in the dynamics of convergence, even when using a more flexible model than has been the norm in the recent convergence literature; a finite state Markov chain with eight states has 56 independent parameters.[22] This suggests that the convergence process has genuinely changed.

To show the differences in an economical way, we contrast the equilibrium distributions implied by the matrices for the two subperiods (see Table 4).

Table 4: Stationary distributions compared

	<0.08	<0.16	<0.32	$<\infty$	$<0.08C$	$<0.16C$	$<0.32C$	$<\infty C$
1975-1995	0.072	0.007	0.013	0.044	**0.701**	0.116	0.036	0.011
1995-2015	0.015	0.033	0.160	**0.537**	0.028	0.058	0.071	0.098

[20] This result need not be inconsistent with theory; see, for example, Lucas (2000).

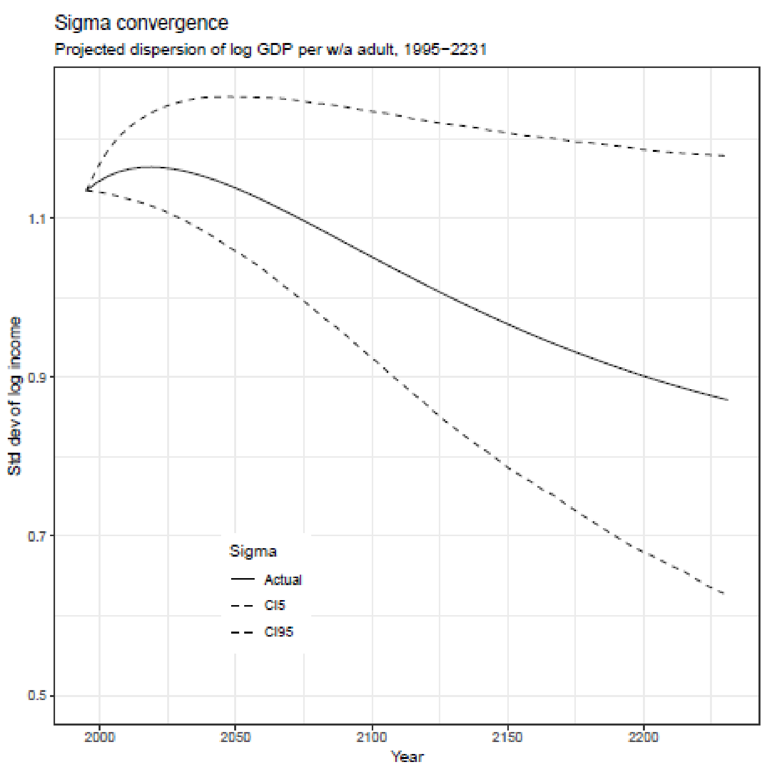
[21] For the relevant test statistic, see Bickenbach and Bode (2003).

[22] There are eight parameters in each row of the transition matrix, but each row must sum to one, so we are estimating $8 \times (8 - 1) = 56$ independent parameters.

We highlight the two entries shown in bold in the table. In the earlier subperiod, 1975-1995, much of the probability mass is concentrated on a low-income, crisis state (the fifth of the eight). In the later subperiod, 1995-2015, much of the mass is instead concentrated on a high-income, crisis-free state (the fourth of the eight).

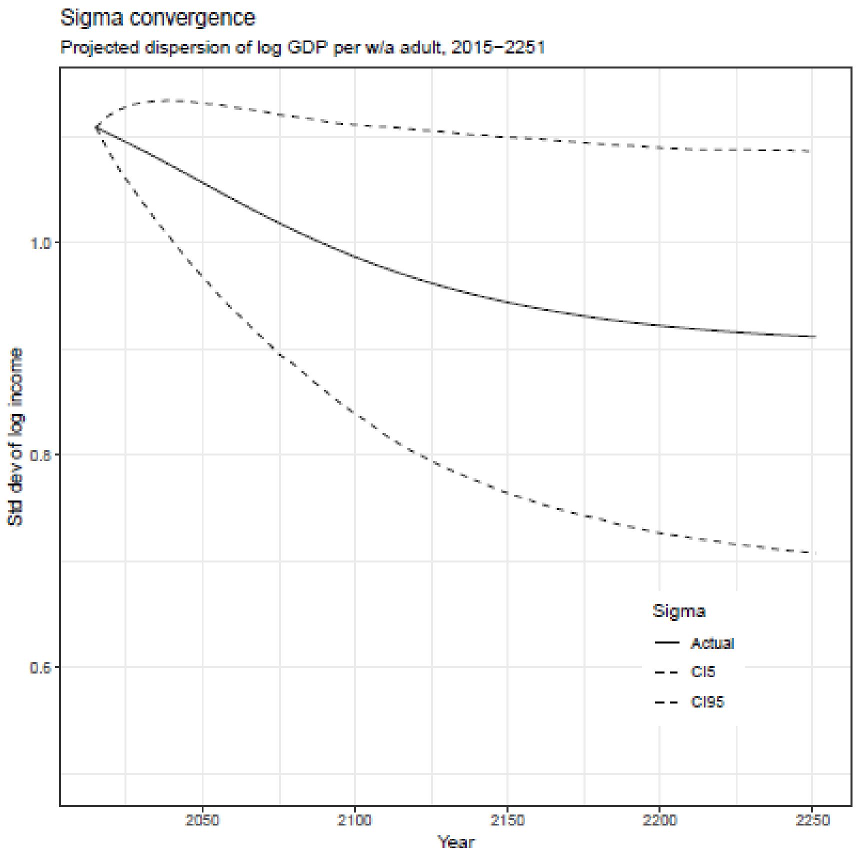
The asymptotic convergence speeds, not reported, are also different in the two cases; convergence to the stationary distribution is faster in the later subperiod. When we project from the first subperiod, ending in 1995, the experience of the 1970s and 1980s suggested that divergence was likely; see Figure 3.

Figure 3: Transition path based on first subperiod



That this did not come to pass suggests that the processes for growth and crises subsequently changed. Indeed, when we project from the later subperiod, convergence is predicted (see Figure 4). The contrasts with the previous two figures, for the whole period and the first subperiod, suggest that the convergence process changed markedly in the later subperiod. This is consistent with the recent research discussed earlier, notably Patel et al. (2021) and Startz (2020), but using a different approach.

Figure 4: Transition path based on later subperiod



Based on Figure 3 we also note that our framework, in accounting for extended periods of divergence, may help to explain what Grier and Grier (2007) called the 'neoclassical anomaly'. They pointed out that income diverged over 1961–1999, even though the standard neoclassical growth determinants converged. When seeking to explain this anomaly, the role of crisis regimes in convergence dynamics may have been under-explored in the econometric literature.

These results have a natural interpretation. Comparing individual elements of the respective transition matrices (not reported) shows that, in the later period, crises were less likely to arise and shorter in duration, confirming the result of Ćorić (2012) that the Great Moderation was not confined to developed countries. This seems enough to make convergence to high income a more likely outcome, and to lessen the persistence of low relative income. Put differently, if we integrate the study of convergence with crises, we find that the prospects for convergence improved because of changes in the crisis process. The earlier persistence of low relative income is associated with frequent and lengthy debt crises and wars. At an informal level this has long been understood, but we find that such effects may be large enough to explain why the convergence process has strengthened in recent years. This also implies that recent global convergence will not be sustained, if crises begin to recur at a higher rate, or last longer when they do occur.

VII. Proximate causes

In this section, we examine the mobility of two proximate determinants of output: relative TFP and the capital-output ratio. We again work with four categories. In the case of TFP, we will measure it relative to the average of the G7 economies. To help ensure that we have good representation of each state, we set the category thresholds using the quartiles in the first period.

In Table 5 we report four-year transitions for relative TFP, again for 1975-2015. This uses the measure of TFP that is provided in version 10.01 of the Penn World Table. That measure is computed from a measure of real output deflated by a Törnqvist quantity index of factor endowments; for more details see Feenstra et al. (2015, Section V). Hence, it is a more general measure than has been the norm in the development accounting literature, the majority of which assumes a Cobb-Douglas aggregate production function.

The transition matrix for relative TFP is based on 79 countries, so we have $79 \times (11-1) = 790$ transitions. A noteworthy feature of the results is that the long-run distribution of relative TFP is widely dispersed, although the fifth state (lowest relative TFP and a crisis) accounts for more than a quarter of the probability mass. This tallies with the emphasis of Imam and Temple (2024b, 2025) on the lack of convergence in relative TFP, but now suggesting that the incidence and duration of crises play a substantial role in this lack of convergence.

Table 5: Four-year TFP transitions, 1975-2015

Transition matrix	1	2	3	4	1C	2C	3C	4C
1	**0.691**	**0.149**			**0.149**	0.011		
	(0.048)	(0.037)			(0.037)	(0.011)		
2	0.075	**0.653**	**0.150**	0.007	0.034	0.082		
	(0.022)	(0.039)	(0.029)	(0.007)	(0.015)	(0.023)		
3	0.009	**0.217**	**0.566**	**0.142**		0.009	0.057	
	(0.009)	(0.040)	(0.048)	(0.034)		(0.009)	(0.022)	
4		0.097	**0.151**	**0.677**		0.011	0.022	0.043
		(0.031)	(0.037)	(0.048)		(0.011)	(0.015)	(0.021)
1C	**0.116**	0.020	0.005		**0.813**	0.040	0.005	
	(0.023)	(0.010)	(0.005)		(0.028)	(0.014)	(0.005)	
2C	0.051	**0.103**	0.013		**0.282**	**0.474**	0.077	
	(0.025)	(0.034)	(0.013)		(0.051)	(0.057)	(0.030)	
3C		0.043	0.085		0.043	**0.298**	**0.383**	**0.149**
		(0.029)	(0.041)		(0.029)	(0.067)	(0.071)	(0.052)
4C			0.037	**0.111**	0.037	0.074	**0.333**	**0.407**
			(0.036)	(0.060)	(0.036)	(0.050)	(0.091)	(0.095)
$\gamma = 16.4$								
Last period	0.253	0.304	0.139	0.038	0.215	0.025	0.013	0.013
20 years ψ_{T+20}	0.186	0.230	0.118	0.060	0.283	0.081	0.030	0.011
100 years ψ_{T+100}	0.188	0.208	0.104	0.054	0.321	0.083	0.031	0.012
Stationary ψ^*	0.189	0.208	0.103	0.054	0.322	0.083	0.031	0.012
(s.e.)	(0.036)	(0.034)	(0.024)	(0.018)	(0.057)	(0.017)	(0.010)	(0.005)
$NT = 790$	$N = 79$	$T = 10$						

Transitions from row state to column state. For other notes see Table 2.

We can get a clearer sense of this from the lower panel of Table 6. A crisis increases the risk of a downwards movement in relative TFP, and in two cases out of three, lowers the prospects for an upwards movement. Some of these differences are quite marked, suggesting that crises can help to explain movements within the worldwide distribution of relative TFP.

Table 6: Four-year TFP transition counts, 1975-2015

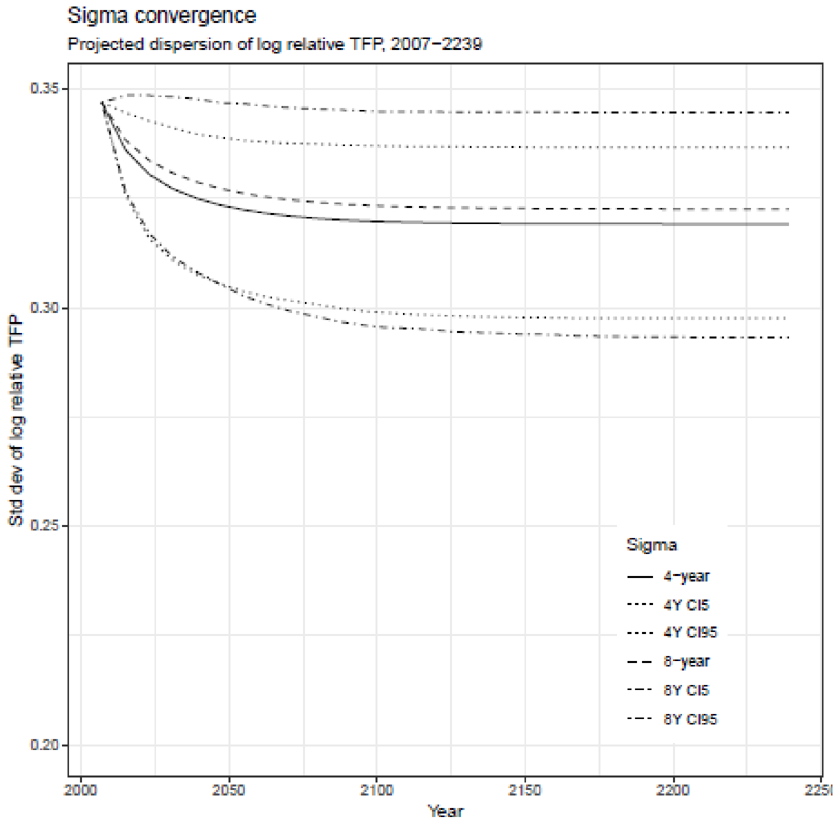
Counts	1	2	3	4	1C	2C	3C	4C
1	65	14			14	1		
2	11	96	22	1	5	12		
3	1	23	60	15		1	6	
4		9	14	63		1	2	4
1C	23	4	1		161	8	1	
2C	4	8	1		22	37	6	
3C		2	4		2	14	18	7
4C			1	3	1	2	9	11
Mobility		$\downarrow 2$	$\downarrow 3$	$\downarrow 4$	$\uparrow 1$	$\uparrow 2$	$\uparrow 3$	
No crisis		0.109	0.236	0.280	0.160	0.156	0.142	
Crisis		0.333	0.383	0.481	0.071	0.090	0.149	

Transitions from row state to column state. For other notes see Table 3.

In Figure 5, we again compare projections based on four-year and eight-year time intervals, for a slightly shorter time period than the reported transition matrix.[23] The two projections — the central solid and dashed lines — are fairly close together. The transition matrix approach suggests that relative TFP will converge over the next hundred years, although if we consider the numbers on the vertical scale, it is clear that the remaining predicted decline in dispersion is modest. In other words, dispersion in relative TFP seems set to be an enduring, but not worsening, feature of the data.

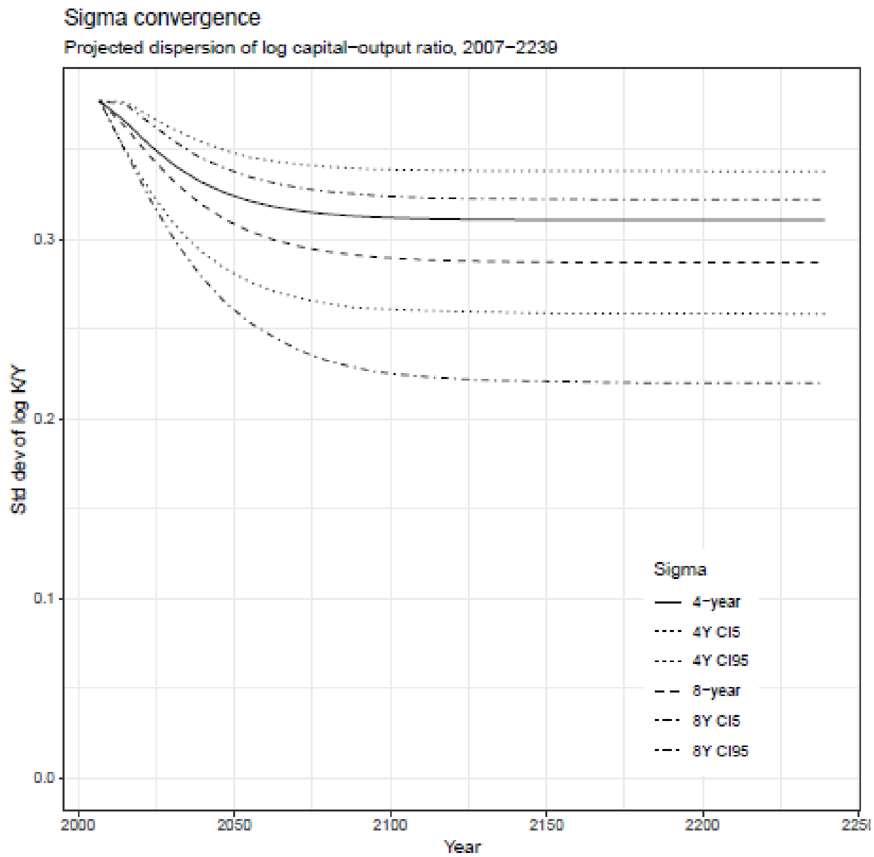
[23] The shorter period arises since we want to use eight-year intervals as well as four.

Figure 5: Transition path for cross-country dispersion in log relative TFP



Next, we look at the capital-output ratio, again using data from version 10.01 of the PWT (see Figure 6). There are two points to note here. First, we do not need to benchmark against other countries, as the absolute value of the capital-output ratio is meaningful and its long-run distribution will be stable given convergence to balanced growth paths (this is not true of TFP or output, which in principle can both grow indefinitely). Second, many crises are initially likely to lower output proportionately more than the fixed stock of capital, in which case their mechanical effect on impact will be to *increase* the capital-output ratio. This means that the effect of a crisis on downwards mobility in the capital-output ratio may be muted, and would be discernible only over much longer horizons, corresponding to long periods of depressed investment.

Figure 6: Transition path for cross-country dispersion in log capital-output ratio



If we look at the long-run distribution reported in Table 7, we can see that countries will spend more than 70% (0.506+0.219) of the time in the highest category for the capital-output ratio. These results tally with the convergence of the capital-output ratio found in Imam and Temple (2024b, 2025), and in the work of Caselli and Feyrer (2007) on the marginal product of capital, but with more recent data and better country coverage than the latter.

Table 7: Four-year K/Y transitions, 1975-2015

Matrix	1	2	3	4	1C	2C	3C	4C
1	**0.486**	**0.229**	0.029		**0.114**	**0.143**		
	(0.084)	(0.071)	(0.028)		(0.054)	(0.059)		
2		**0.543**	**0.239**	0.011	0.011	0.087	**0.109**	
		(0.052)	(0.044)	(0.011)	(0.011)	(0.029)	(0.032)	
3		0.077	**0.469**	**0.315**		0.008	**0.100**	0.031
		(0.023)	(0.044)	(0.041)		(0.008)	(0.026)	(0.015)
4			0.042	**0.874**			0.006	0.077
			(0.011)	(0.019)			(0.005)	(0.015)
1C	0.036	0.024			**0.702**	**0.214**	0.024	
	(0.020)	(0.017)			(0.050)	(0.045)	(0.017)	
2C		0.069	0.031		0.031	**0.635**	**0.208**	0.025
		(0.020)	(0.014)		(0.014)	(0.038)	(0.032)	(0.012)
3C		0.017	0.094	0.033		0.088	**0.586**	**0.182**
		(0.009)	(0.022)	(0.013)		(0.021)	(0.037)	(0.029)
4C			0.021	**0.153**		0.021	0.090	**0.714**
			(0.010)	(0.026)		(0.010)	(0.021)	(0.033)
$\gamma = 15.3$								
Last period	0.000	0.051	0.110	0.500	0.000	0.076	0.093	0.169
20 years ψ_{T+20}	0.000	0.030	0.086	0.502	0.006	0.054	0.110	0.211
100 years ψ_{T+100}	0.000	0.026	0.083	0.506	0.007	0.051	0.108	0.219
Stationary ψ^*	0.000	0.026	0.083	0.506	0.007	0.051	0.108	0.219
(s.e.)	(0.000)	(0.008)	(0.015)	(0.052)	(0.004)	(0.015)	(0.021)	(0.033)
$NT = 1180$	$N = 118$	$T = 10$						

Transitions from row state to column state. For other notes see Table 2.

Looking at the mobility summary in the lower panel of Table 8 we can see that the effect of a crisis on downwards mobility is present but muted, as expected. But crises are clearly associated with worse prospects for upwards mobility, suggesting that crises play a role in the evolution of this variable: absolute convergence in capital-output ratios would be even more pronounced in the absence of crises.

Table 8: Four-year K/Y transition counts, 1975-2015

Counts	1	2	3	4	1C	2C	3C	4C
1	17	8	1		4	5		
2		50	22	1	1	8	10	
3		10	61	41		1	13	4
4			13	271			2	24
1C	3	2			59	18	2	
2C		11	5		5	101	33	4
3C		3	17	6		16	106	33
4C			4	29		4	17	135
Mobility		$\downarrow 2$	$\downarrow 3$	$\downarrow 4$	$\uparrow 1$	$\uparrow 2$	$\uparrow 3$	
No crisis		0.011	0.085	0.048	0.400	0.359	0.346	
Crisis		0.031	0.105	0.132	0.262	0.264	0.215	

Transitions from row state to column state. For other notes see Table 3.

Again, we look at projections from transition matrices based on four-year and eight-year intervals. This time the dashed and solid lines are a little further apart, which suggests that the first-order Markov property may be harder to defend for the capital-output ratio than for relative output per head and TFP. We can see that further convergence is predicted, although the remaining reduction in dispersion from its starting point is again quite modest.

VIII. Robustness

In this section, we turn to robustness, starting with the potential role of natural resources. Countries which derive income from resource rents will be especially prone to booms and slumps driven by changing world prices. The scope for differing patterns of mobility suggests that combining resource-poor and resource-dependent countries in the same sample could be problematic. To investigate this, we have studied subsamples: those countries that are not dependent on natural resources, and those that are. Kremer et al. (2001), in their analysis, excluded 20 countries with high shares of the mining and quarrying sector in GDP, based on sectoral data from the United Nations. We add to this list Equatorial Guinea, which discovered oil in 1996.

We use this list of resource-dependent countries to generate two subsamples, with and without resource dependence. We then compare the stationary distributions implied by the transition matrices for the two different samples. Since we have just 19 resource-dependent countries in our original sample, we have to do this with a smaller set of income categories than before. We amalgamate the lowest two relative income categories into one (and again for the crisis states) so that we now have six states rather than eight.

The results are presented in Table 9, which shows stationary distributions and their bootstrapped standard errors. The most important result is that the stationary distribution in the first row — for the whole sample — is very close to the stationary distribution for the non-resource-dependent sample. This strongly suggests that our earlier results are not greatly affected by heterogeneity in natural resource dependence.

Table 9: Long-run distributions for subsamples

	<0.16	<0.32	$<\infty$	$<0.16C$	$<0.32C$	$<\infty C$
All countries	0.082	0.099	0.323	0.329	0.101	0.066
(s.e.)	(0.019)	(0.019)	(0.068)	(0.067)	(0.019)	(0.018)
Resource-poor	0.094	0.097	0.312	0.330	0.105	0.062
(s.e.)	(0.025)	(0.022)	(0.082)	(0.076)	(0.023)	(0.020)
Resource-dependent	0.009	0.093	0.416	0.314	0.077	0.092
(s.e.)	(0.046)	(0.370)	(0.226)	(0.275)	(0.054)	(0.053)

This table shows long-run distributions for output per head, for the whole sample and two subsamples. Note the similarity of the results for the whole sample to the results for resource-poor countries.

It is true that resource-dependent countries do show somewhat different dynamics: see the final two rows of the table. The point estimates suggest that resource-dependent countries are more likely to be in the highest crisis-free income category. But given the small sample, the long-run distribution is very noisily estimated. A Pearson test of spatial homogeneity rejects the null at the

5% level, so there is clear evidence for heterogeneity. But the closeness of the long-run distributions for the whole sample and the resource-poor sample suggests that either resource-dependent countries are too few in number, or the differences in their dynamics not stark enough, to explain away our earlier findings.

Thus far, our crisis regimes have been derived from indicators of debt crises and conflict. We have argued that either form of crisis may be sufficient to halt growth, so that it makes sense to aggregate them rather than try to estimate independent, additive effects; in other words aggregation is a strength here rather than a weakness. It recognizes that growth prospects may be only as strong as their weakest link, as discussed in Sirimaneetham and Temple (2009).

But it could be argued that financial crises, notably domestic banking crises and currency crises, will also inhibit convergence. Against this, these forms of crisis may sometimes be too short-lived to greatly influence the medium-term dynamics of variables like TFP or the capital-output ratio. In addition, the literature on banking and currency crises suggests that their risks and longer-term effects will be contingent on factors such as the strength of domestic institutions, the extent of financial development, and differences in leverage.[24] Our setup is not easily adapted to take this into account: we allow the likelihood and expected duration of crises to vary with development levels, but allowing them to vary with institutional quality or leverage would be harder.

With all these considerations in mind, we investigate robustness to an alternative definition of crisis regimes. We present a table equivalent to our earlier Table 1, but now where the crisis regimes can arise from more forms of crisis — a sovereign debt crisis or conflict, as before, but now also a banking crisis or a currency crisis. The data on the latter two again come from the financial crisis database of Nguyen et al. (2022), and we reduce four years of data on crises to a binary indicator as before.

The results with this wider definition are shown in Table 10. Although including banking and currency crises could attenuate the effects of crises on mobility patterns, the effects are in line

[24] On financial crises and context dependence see, for example, Rancière et al. (2008), and on exchange rate volatility, Aghion et al. (2009).

with those found earlier, with few systematic differences between the two. This suggests that our results are quite robust to how crises are defined, which could be explained in several ways. Either the new definition does not greatly increase the incidence of crises (given that they sometimes overlap) or upwards and downwards mobility are not much affected by the presence or absence of these additional forms of crisis. The latter seems most likely, so our earlier focus on debt crises and conflict may be a reasonable choice.

Table 10: Mobility and all forms of crisis, 1975-2015

	$\downarrow 2$	$\downarrow 3$	$\downarrow 4$	$\uparrow 1$	$\uparrow 2$	$\uparrow 3$
Relative Y/L						
No crisis	0.059	0.062	0.025	0.118	0.235	0.188
Crisis	0.119	0.143	0.139	0.052	0.097	0.062
Relative TFP						
No crisis	0.110	0.222	0.203	0.175	0.165	0.156
Crisis	0.265	0.431	0.561	0.070	0.094	0.137
K/Y ratio						
No crisis	0.013	0.089	0.064	0.414	0.355	0.356
Crisis	0.028	0.108	0.106	0.261	0.267	0.258

Mobility downwards and upwards between development categories, based on four-year intervals between 1975 and 2015, but now adding banking and currency crises to the definition of a crisis regime. For more details see section 8 of the text.

Another possible concern relates to our use of four-year intervals. Kremer et al. (2001) and the wider growth literature often use five-year intervals. As we mentioned earlier, we adopted four-year intervals so that we can compare the results with those from eight-year intervals, allowing an informal assessment of the Markov property for each of the outcome variables we consider. But for readers interested in this choice, we again present a table equivalent to our earlier Table 11, now based on five-year intervals for the period 1975-2010. Despite the shorter overall timespan and the use of five-year intervals, the results are largely in line with those presented earlier.

Table 11: Mobility, five-year intervals, 1975-2010

	↓2	↓3	↓4	↑1	↑2	↑3
Relative Y/L						
No crisis	0.054	0.108	0.013	0.095	0.243	0.169
Crisis	0.154	0.196	0.208	0.068	0.118	0.069
Relative TFP						
No crisis	0.110	0.267	0.403	0.224	0.187	0.213
Crisis	0.379	0.400	0.591	0.093	0.052	0.200
K/Y ratio						
No crisis	0.000	0.061	0.098	0.433	0.318	0.476
Crisis	0.053	0.135	0.140	0.343	0.348	0.286

Mobility downwards and upwards between development categories, based on five-year intervals between 1975 and 2010. For more details see section 8 of the text.

IX. Conclusions

Earlier work on convergence indicates that low-income countries have been slow to improve their relative position. Although informal commentary often alludes to lost decades and growth tragedies, these have played only a minor role in the convergence literature. This paper integrates the study of crises and convergence, allowing relative income and some of its proximate determinants to evolve differently at times of crisis, and then studying what this implies for the trajectory of the cross-country distribution.

We find evidence that these dynamics can indeed explain the persistence of relative income. Crises are both more common and last longer at low levels of development, and contribute to explaining patterns for mobility in relative output per head, relative TFP, and the capital-output ratio. For all three variables, we find that crises raise the risk of downwards mobility and reduce the prospects for upward mobility, across several levels of development. More strikingly, we also find that the prospects for convergence to high income have improved over time, associated with crises that are shorter and fewer in number. Policies that limit crisis exposure would, by improving growth prospects and lowering the risk of stagnation or deterioration, help to narrow cross-country dispersion.

At first glance, and in line with Patel et al. (2021), our results may seem optimistic: the experience of recent decades suggests that many countries will achieve a high level of development. This optimism may be misplaced, however. Some countries in Africa are experiencing new or renewed conflicts, and there may be a heightened risk of debt distress and crises (Rogoff 2022). If severe crises become more common again, convergence is likely to slow down, or even give way to the divergence associated with the 1980s. If this view is correct, proactive prevention and robust recovery strategies will be needed.

For countries at risk, crisis prevention and management should be mainstays of development policy; other measures are unlikely to succeed without them. The recent decline in the incidence of crises is likely to have resulted partly from improved financial governance. IMF surveillance and rapid-response liquidity mechanisms can help to maintain stability and prevent reversals in convergence.

This points to the importance of institutional resilience. One of the most fundamental benefits of strong institutions may be reduced crisis risk, and so development strategies that integrate crisis prevention with institutional strengthening are likely to have the greatest impact. Institutions that secure property rights, maintain fiscal and monetary stability, and provide mechanisms for peaceful conflict resolution, could be critical. Strengthening state capacity and governance structures is likely to yield long-term growth benefits that extend beyond immediate gains in investment or productivity. External support mechanisms, including rapid-response liquidity provision by international organizations, can complement domestic efforts, although this depends on the quality of domestic institutions and political stability.

As we have stressed throughout, the study of global convergence raises many interesting questions, beyond whether convergence is present or absent. There are benefits in flexible approaches that can accommodate a range of possible dynamics, and that can better reflect the complexity of an evolving distribution. Here, by introducing multiple crisis regimes that influence the chances of upwards and downwards mobility, we cast some light on how cross-country distributions have evolved and what may happen in the future. Many of the results suggest that crisis prevention remains central to growth promotion.

Annex I.

1.1 Quantiles and mobility

For some of our analyses, we use quartiles in the first period of the data to determine the category thresholds. An alternative would be to base the categories on continually updating quantiles, but this would have significant drawbacks. The implicit thresholds would be varying over time, and the stationary distribution would be uninformative by construction. For these reasons, we prefer our chosen approach.

1.2 Timing assumptions

Given that we use time intervals longer than a year, we have to decide how to arrive at a crisis indicator based on annual data; for a parallel choice see, for example, Rancière et al. (2006, p. 3335). We illustrate with a four-year interval. For the state classification in, say, 1975, we use output at that date, while the crisis indicator is unity if there was a crisis in any of the years 1975, 1976, 1977 or 1978. This means that upwards or downwards mobility between, say, 1975 and 1979 is measured by comparing output in 1975 and 1979, while crisis outcomes are those arising between 1975 and 1978 inclusive. This ensures that particular crisis years are not used more than once.

1.3 Bootstrap

We use a bootstrap with 2001 replications. The bootstrapped standard errors for the transition probabilities, not reported, are typically close to the Anderson-Goodman asymptotic standard errors we report.

1.4 Country samples

Country names are those given in version 10.01 of the PWT.

Sample for Tables 2 and 3

Albania, Algeria, Angola, Argentina, Australia, Austria, Bahrain, Bangladesh, Belgium, Benin, Bolivia (Plurinational State of), Botswana, Brazil, Burkina Faso, Burundi, Côte d'Ivoire, Cabo Verde, Cambodia, Cameroon, Canada, Central African Republic, Chad, Chile, China, Colombia, Congo, Costa Rica, Cyprus, D.R. of the Congo, Denmark, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eswatini, Ethiopia, Fiji, Finland, France, Gabon, Gambia, Ghana, Greece, Guatemala, Guinea-Bissau, Guyana, Haiti, Honduras, India, Indonesia, Iran (Islamic Republic of), Iraq, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kenya, Kuwait, Lao People's DR, Lebanon, Lesotho, Liberia, Luxembourg, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Morocco, Mozambique, Myanmar, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Oman, Pakistan, Panama, Paraguay, Peru, Philippines, Portugal, Qatar, Republic of Korea, Rwanda, Saudi Arabia, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sri Lanka, Sudan, Suriname, Sweden, Switzerland, Thailand, Togo, Trinidad and Tobago, Tunisia, Turkey, Uganda, United Arab Emirates, United Kingdom, United States, Uruguay, Venezuela (Bolivarian Republic of), Viet Nam, Zambia, Zimbabwe.

Sample for Tables 5 and 6

Angola, Argentina, Australia, Austria, Bahrain, Belgium, Bolivia (Plurinational State of), Botswana, Brazil, Burkina Faso, Côte d'Ivoire, Cameroon, Canada, Chile, China, Colombia, Costa Rica, Cyprus, Denmark, Dominican Republic, Ecuador, Egypt, Finland, France, Gabon, Greece, Guatemala, Honduras, India, Indonesia, Iran (Islamic Republic of), Iraq, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kenya, Kuwait, Luxembourg, Malaysia, Mauritius, Mexico, Morocco, Mozambique, Netherlands, New Zealand, Niger, Nigeria, Norway, Panama, Paraguay, Peru, Philippines, Portugal, Qatar, Republic of Korea, Rwanda, Saudi Arabia, Senegal, Singapore, South Africa, Spain, Sri Lanka, Sudan, Sweden, Switzerland, Taiwan, Thailand, Trinidad and Tobago, Tunisia, Turkey, United Kingdom, United States, Uruguay, Venezuela (Bolivarian Republic of), Zambia, Zimbabwe.

Sample for Tables 7 and 8

Albania, Algeria, Angola, Argentina, Australia, Austria, Bahrain, Bangladesh, Belgium, Benin, Bolivia (Plurinational State of), Botswana, Brazil, Burkina Faso, Burundi, Côte d'Ivoire, Cabo Verde, Cambodia, Cameroon, Canada, Central African Republic, Chad, Chile, China, Colombia, Congo, Costa Rica, Cyprus, D.R. of the Congo, Denmark, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eswatini, Ethiopia, Fiji, Finland, France, Gabon, Gambia, Ghana, Greece, Guatemala, Guinea-Bissau, Haiti, Honduras, India, Indonesia, Iran (Islamic Republic of), Iraq, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kenya, Kuwait, Lao People's DR, Lebanon, Lesotho, Liberia, Luxembourg, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Morocco, Mozambique, Myanmar, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Oman, Pakistan, Panama, Paraguay, Peru, Philippines, Portugal, Qatar, Republic of Korea, Rwanda, Saudi Arabia, Senegal, Sierra Leone, Singapore, South Africa, Spain, Sri Lanka, Sudan, Suriname, Sweden, Switzerland, Taiwan, Thailand, Togo, Trinidad and Tobago, Tunisia, Turkey, Uganda, United Arab Emirates, United Kingdom, United States, Uruguay, Venezuela (Bolivarian Republic of), Viet Nam, Zambia, Zimbabwe.

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