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Poverty Mobility in Developing Countries: Stylized Facts, Data Constraints, and Subnational Illustrations

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Poverty Mobility in Developing Countries: Stylized Facts, Data Constraints, and Subnational Illustrations*

Abstract

A considerable degree of poverty mobility can occur in countries even though overall, aggregate, poverty remains unchanged. This paper reviews studies on poverty mobility in developing countries, using concepts organized by four choices: the unit of analysis, the time frame, the reference distribution, and the data structure. Ten stylized facts are drawn from the literature. An illustrative application asks whether similar patterns appear in the dynamics of places, using the Subnational Poverty and Inequality Database (SPID) for 1,949 regions in 135 economies over 2000–2024. We find that at the $4.20-a-day line, movement across fixed poverty bands points consistently towards lower poverty. Persistent high poverty is geographically concentrated in mostly African economies, and disproportionately in fragile ones, in which measurement is also considerably more problematic. Income-group upgrades go together with broader and faster regional poverty reduction. Movements in monetary and multidimensional dynamics are generally consistent. An apparent convergence pattern among initially poorer regions is sensitive to illustrative measurement-error assumptions. We further highlight data limitations and future research directions.

JEL classification

I32, D31, O10, O15, C81

Keywords

poverty mobility, poverty dynamics, persistent poverty, subnational data, measurement error, developing countries

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1. Introduction

Poverty headcounts show how many people are poor at a given time, but not whether the same households remain poor, enter poverty, or escape it. At the regional level, a lower headcount may reflect lasting exits from poverty or gains in some regions alongside persistent deprivation in others. These dynamics require different policy responses: long-term investment in human capital, infrastructure, productive assets, or geographic targeting where poverty persists; and insurance and shock-responsive social protection where poverty is transient (Baulch and Hoddinott 2000; Carter and Barrett 2006; Jalan and Ravallion 2000).

This distinction has become more important as the global poverty landscape has changed. Decades of progress have reduced extreme poverty, but the pace of reduction has slowed. A considerable population still remains below less austere poverty standards and close enough to them to be vulnerable to shocks (World Bank 2024a). Policy debates have accordingly broadened from lifting people above a single poverty line to sustaining escapes, preventing descents, and expanding the middle class; while a parallel middle-income-trap debate asks whether national progress is sufficiently broad and durable for all population groups (Ferreira *et al.* 2013; World Bank 2024b).

The evidence needed to answer them is uneven. Repeated household cross-sections are common, but fewer than two in five economies have fielded a qualifying household panel since 2000, and many available panels are small in size, and follow households for only a short period (Dang 2026). Data scarcity is only one problem. Attrition, changes in questionnaires or recall periods, and measurement error can exaggerate or result in spurious movement even when linked observations do exist. The practical challenge is therefore twofold: panel data availability limits the dynamics that can be observed, while comparability and reliability limit what can be concluded.

Employing the general data tier framework from Dang (2026), this chapter offers an accessible, poverty-focused synthesis for developing countries. Concepts and measures are organized around four choices: what unit moves, over what period, against which reference, and with what data. The literature is then distilled into ten stylized facts, together with two observations on vulnerability and intergenerational persistence that the chapter's empirical material cannot test. Each claim is paired with the data needed to support it and the main threat to its interpretation. This approach helps provide further supportive evidence for these stylized facts.

A second purpose is to bring geography into the discussion without confusing places with people. Household poverty mobility concerns whether the same households enter, exit, or remain in poverty. Regional poverty dynamics similarly concern whether the poverty rate of the same place rises, falls, or remains high. The Subnational Poverty and Inequality Database (SPID) provides data on 1,949 regions in 135 economies over 2000–2024 *et alet al*that helps investigate the latter (Dang *et al*. 2026). It joins a wider movement to assemble comparable mobility data across countries, of which the global opportunity and mobility database of Ferreira *et al*. (2026) is a recent example. While these data cannot identify poverty mobility at the household level, or whether migration changed regional composition, they can show whether progress was widespread or concentrated, whether high poverty persisted in the same places, and whether monetary and multidimensional poverty moved together. Regional evidence therefore complements the stylized facts based on analysis of household survey data.

The subnational illustration yields a connected message. Over the 2010s, movement across fixed poverty-rate bands was predominantly toward lower poverty. Yet, absolute progress coexisted with strong relative persistence: many regions improved against fixed thresholds without changing much in the geographic ranking. Persistent high poverty was concentrated in a limited

group of mostly African economies, and disproportionately in fragile and conflict-affected ones, while missing surveys left several large poor countries outside the persistence classification. Country averages nonetheless conceal deep pockets: the widest gaps between a country's worst region and its own average are found outside Africa, in Panama, Iran, and Colombia. National income-group upgrades were associated with broader and faster regional poverty reduction, although the comparison is descriptive.

Monetary and multidimensional poverty moved in the same direction in about four spells out of five, close enough to corroborate a broad trend but not to make the measures interchangeable. An apparent convergence pattern among initially poorer regions is sensitive to illustrative measurement-error assumptions. Together, these findings clarify which statements can be estimated or illustrated with the available data, and show that various choices—the unit, benchmark, poverty line, and error structure—should be considered together for better understanding of poverty dynamics.

This chapter complements two related contributions in this handbook. Genicot *et al.* (2025) focus on income mobility in the upward direction, including panel-free measures centered on the growth experienced by those starting lower in the distribution. Jolliffe and Lakner (2023) examine the more aggregated construction and static comparability of global poverty measure. The synthesis also adds to earlier reviews on the developing-country poverty-dynamics literature represented by Baulch and Hoddinott (2000) and Iversen *et al.* (2019), particularly by offering a new angle based on subnational evidence that connects household-focused findings to the geography of poverty.

The chapter is organized as follows. Section 2 sets out the main concepts, measure families, and data requirements, emphasizing the distinction between household and place-based dynamics.

Section 3 summarizes ten stylized facts from the literature and states which can be tested directly, which have only place-level analogues, and which lie outside the regional data. Section 4 uses the SPID to illustrate transitions across poverty-rate bands, persistent high poverty and its concentration in fragile settings, the breadth of regional progress, measurement-error sensitivity, and the relationship between monetary and multidimensional dynamics. Section 5 summarizes the argument and draws together the policy implications and priorities for data and research.

2. Concepts, units, and data

2.1 Four choices behind any mobility statement

Any mobility statement rests on four choices, made explicitly or not. The unit of analysis fixes what moves: an individual or household, a cohort or group, a place, or a country. The time frame separates intra-generational movement, a household's trajectory over years, from intergenerational transmission between parents and children. The reference distribution fixes what movement is measured against: a national poverty line, the national income ranking, or thresholds held fixed for the whole world. The data structure determines which of these statements can actually be produced.

A single sentence shows how much the choices carry. The claim that "mobility rose in country X" can mean that households re-ranked faster, that more crossed the national line, that more crossed a global line, or that children's outcomes depended less on their parents'. These are four claims with different data demands and different policy content. The unit choice is the one most often left implicit, and the one that may matter most. For example, the results shown later about regional poverty rates are useful for policy advice (Section 4.3), but they are not exactly a statement about household poverty transitions.

A first family of measures is non-directional, treating mobility as movement per se. The workhorse is the transition matrix, whose cell (j, k) records the probability of moving from state j to state k. Scalar summaries compress the matrix into an index, the best known being the trace-based measure of Shorrocks (1978). Distance-based indices aggregate individual changes regardless of sign (Fields and Ok 1996, 1999), which can be organized using an axiomatic superstructure (Cowell and Flachaire 2018).

A second family is directional, valuing movement up ordered categories and penalizing movement down. A recent example is the upward-mobility measure of Ray and Genicot (2023), a weighted aggregate of individual income growth with weights declining in the starting level. Between the two sits the growth incidence curve. Its anonymous version plots the growth of each quantile. The non-anonymous version of Bourguignon (2011) plots the growth of those who started at a given position.

Poverty dynamics proper add specific structure related to a poverty line that generic income mobility does not carry. The spells approach classifies households by the pattern of their poverty episodes over a specific time window (Hulme and Shepherd 2003). The components approach decomposes welfare into a permanent part and fluctuations around it, labeling poverty chronic when the permanent component falls below a poverty threshold (Duclos *et al.* 2010; Jalan and Ravallion 1998). These two approaches embody different views of what chronic means, persistent observation versus persistent capacity, and can classify the same household differently.

Three cross-cutting distinctions discipline debates. Absolute movement, meaning the crossing of a fixed real threshold, differs from relative movement in the ranking. For example, a growing economy can display much of the first with none of the second. Exchange mobility, meaning pure re-ranking within an unchanged distribution, differs from structural mobility that

shifts the distribution itself. Anonymous growth incidence differs from its non-anonymous counterpart precisely by the amount of re-ranking. Last but not least, measurement error takes an important role with mobility measures. Classical error inflates non-directional mobility mechanically and can also bias directional estimates through spurious mean reversion, because units measured as too poor at the baseline tend to exhibit fast (measured) improvement.

2.2 Measures and the data they require

The framework crosses the four choices with four data tiers: D1 to D4. Tier D1 is a single cross-section. D2 is repeated cross-sections, the modal data situation in developing countries. D3 is an actual panel. D4 is linked administrative registers, which can reduce attrition and enlarge samples but can bring selective coverage, linkage error, and incomplete welfare information. Registers also capture formal-sector activity better than the informal work in which many poor households engage. Table A1 lists the measure families, the unit each is defined on, the minimum tier at which it can be estimated, and whether the estimand is directly observed or recovered under assumptions. The fourth column carries the distinction most often blurred in applied work.

At tier D2, household transitions are not observed but modeled. The synthetic panel method is the most widely used, with applications spanning a wide range of countries and both poverty and vulnerability questions. Synthetic panels model the joint distribution of welfare across rounds using time-invariant characteristics and assumptions about cross-round dependence. *et alet al*The approach descends from the pseudo-panel literature founded by Deaton (1985). Bounds are available under weak assumptions, and point estimates under bivariate normality with some additional assumption about the error terms (Cruces *et al*. 2015; Dang *et al*. 2014; Dang and

Lanjouw 2023; Rongen *et al*. 2026). Moreno *et al.* (2021) further extend the approach to income mobility.[1] (Fact 5 below reports how well these methods validate against true panels).

Other methods differ in where the identifying assumption sits. Ray and Genicot (2023) dispense with panel data by construction. If individuals who cross paths at identical incomes are interchangeable, tracking identities adds nothing, so their upward-mobility measure can be computed from cross-sectional marginals alone. The price is that it records directional growth rather than who moved where. Berman (2022) instead borrows the dependence structure, showing that absolute mobility estimates are relatively insensitive to the copula linking two distributions. Kraay and van der Weide (2022) assume a lognormal autoregressive income process and use only the means and variances of repeated cross-sections to bound persistence. A fourth route combines different data sources. Chetty *et al.* (2017) join a copula estimated from administrative records to survey-based marginals using U.S. data. Finally, a developing-country analogue is to borrow a dependence structure from a small panel (which would be less expensive to implement than a full panel) and apply it to nationally representative cross-sections to measure poverty mobility (Gibson 2001).

Two practical outputs follow. The first is a tier-contingent core reporting set, comprising the nine families of mobility measures that a given tier supports together with an explicit measurement-error sensitivity (Table A1).[2] It is chosen so that any two studies reporting it can be

[1] Pseudo-panels, which track cohort means rather than households, lay the earlier foundation for synthetic panels (Antman and McKenzie 2007; Deaton 1985; Inoue 2008). Garcés-Urzainqui *et al.* (2021) supply a practitioner primer.

[2] Table A1 also separates a tenth entry from the nine statistical families, because it is a choice of reference distribution rather than a new statistic. Movement across fixed global welfare classes uses thresholds that are absolute, expressed in PPP dollars, and held constant for every unit and every year. Crossing one is therefore distinct from positional movement in the world ranking and from movement relative to any national line. The example that matters for this chapter is a region whose poverty headcount falls from 70% to 40% while every other region in its country improves as fast. Its position in the national ranking is unchanged, yet it has crossed fixed thresholds that carry real meaning for the people living there. Fixed global thresholds are the natural frame for subnational analysis, where units are too numerous and too heterogeneous for country-specific lines to aggregate cleanly.

compared. The second is a decision rule from question and data to method, set out in Figure 1. The figure runs left to right through the four choices, then to the feasible measure and the failure mode that most threatens it. The shaded route is the one this chapter takes: the unit is the place, the horizon is within a lifetime, and the reference is a set of fixed global thresholds. Notably, while the data are repeated cross-sections (tier D2) at the household level, they form an actual panel (tier D3) at the regional level and so permit richer analysis of regional aggregates. That route supports analysis of transitions across fixed poverty bands, non-anonymous growth incidence, and persistence across observed surveys.

3. What the literature shows

3.1 Ten stylized facts

The literature yields ten stylized facts. They are drawn from national panels where these exist, from long-run village studies (Himanshu *et al.* 2018), from retrospective methods (Krishna 2004, 2010), and from synthetic panels where panels are absent. Two further observations, equally well established in the literature but not supported with the subnational data, close the section.

Fact 1. Poverty is fluid. Over horizons of five to ten years, the ever-poor population is typically a multiple of the poor at any single date. Modest net changes therefore conceal large gross flows in both directions (Baulch and Hoddinott 2000; Dercon and Shapiro 2007). Recent multi-country panel evidence points the same way: across five economies that together account for most of the global decline in poverty since 1990, both poverty exits and entries are common, and escaping poverty resembles a slippery slope more than a long-term trap (Armentano *et al.* 2026). Long-run village and life-history studies find the same churning at much finer resolution (Krishna 2010). Synthetic-panel evidence supports similar patterns in more countries without panel data, in India

and across Africa and the Middle East (AlAzzawi *et al.* 2025; Dang and Dabalen 2019; Dang *et al.* 2021). Protection and promotion should therefore be sized to the flows rather than to the stock. The principal qualification is that some measured churning may just be noise rather than genuine movement (as Fact 7 records).

Fact 2. Poverty entries remain consequential, and generally have different drivers from exits. Health shocks and the expenses that follow them are an often prominent descent channel, alongside weather shocks and life-cycle events, although their relative importance varies by setting and by the risks households already face. While poverty escapes are led by income diversification, jobs, education, and migration (Dercon 2004; Krishna 2004; Woolard and Klasen 2005), rising catastrophic health spending could facilitate poverty entry and even result in chronic poverty (Garcia-Diaz *et al.* 2024; Wagstaff *et al.* 2018). Climate and disaster shocks are often observed to push households below the line and slow their recovery (Dang *et al.* 2024; Hallegatte *et al.* 2020). Internal migration and urban wage premia remain a principal route out of poverty, although how much of the premium survives adjustment for urban disamenities is contested (Gollin *et al.* 2021; Lagakos 2020). Poverty entry rates can differ from poverty exit rates; whether they are equally frequent can depend on the setting and the window.

Fact 3. Geography strongly conditions poverty levels and trajectories. Two distinct bodies of evidence support this, and they should be kept separate. In the global distribution, country of residence accounts for a large share of the variance of global, individual, incomes (Milanovic 2015). Within countries, a separate literature finds geographic poverty traps at the micro level, with consumption growth depending on area characteristics after conditioning on household ones (Jalan and Ravallion 2002). Neighborhood exposure effects can be now estimated with administrative data at fine spatial scale, particularly in high-income countries (Chetty and Hendren

2018). High-resolution poverty maps built from satellite and mobile data have made the within-country geography of deprivation visible in settings without dense survey coverage (Chi *et al.* 2022; Smythe and Blumenstock 2022). The between-country and within-country strands are distinct, and it is the second strand that makes the dynamics of places a complementary object of study.

Fact 4. Both chronic and transient poverty are often substantial, but their measured shares can depend on the definition. Transient poverty accounted for roughly half of measured poverty in post-reform rural China (Duclos *et al.* 2010; Jalan and Ravallion 1998). Chronic poverty accounts for about half or more across much of sub-Saharan Africa (Dang and Dabalen 2019), a pattern also visible in the asset-based literature, where structural and stochastic poverty are separated using asset rather than flow measures (Carter and Barrett 2006). But neither category can be assumed dominant without careful measurement, and the split carries a policy assignment. Reported shares move with the poverty line, the welfare aggregate, the classification method, and the length and timing of the observation window (Hulme and Shepherd 2003). Multidimensional measures similarly show sensitivity to aggregation choices and reference periods (Alkire *et al.* 2017, 2023). It is thus useful to place attention on the definitions and various associated choices when studying poverty dynamics.

Fact 5. Panel scarcity limits direct evidence, but alternative methods recover selected transitions under explicit assumptions. The panel inventory summarized in Section 1 is the starting point: most economies cannot follow households for long, and many cannot follow them at all (Dang 2026; Dang *et al.* 2019; Serajuddin *et al.* 2015; World Bank 2024a). Survey coverage itself remains thin in the poorest countries, and survey noncomparability is a further obstacle. The record supports investing in survey availability and comparability, while still requiring attention

to residual dependence, model choice, and validation. As discussed above (Section 2.2), where a true panel does not exist, synthetic panels built from the same country's repeated cross-sections can offer an alternative. A synthesis of the validation studies puts the mean absolute deviation for synthetic panel estimates from actual panel estimates at about 1.4 percentage points (Dang 2026; Hérault and Jenkins 2019). Other useful methods make different modelling assumptions (Berman 2022; Chetty *et al.* 2017; Kraay and van der Weide 2022; Lucchetti *et al*. 2025; Ray and Genicot 2023).

Machine-learning estimates from satellite imagery now fill some spatial gaps, but they estimate levels rather than household transitions (Yeh *et al.* 2020), and small-area methods that combine surveys with auxiliary data face the same limit, improving precision on levels while leaving transitions unidentified (Merfeld *et al.* 2025).

Fact 6. Poverty traps are not ubiquitous, yet large transfers can shift trajectories for some very poor households. Direct tests for the S-shaped dynamics that trap theory predicts mostly fail, since single-equilibrium, slow-convergence dynamics fit most household panel data (Kraay and McKenzie 2014), and the recent multi-country panel record is likewise better described by churning than by a low-level equilibrium (Armentano *et al.* 2026). The asset-based tradition nonetheless identifies thresholds in asset rather than flow space, which is where trap dynamics would be expected to show (Carter and Barrett 2006). Asset-heavy interventions have been found to be able to shift trajectories durably for the very poorest.

Multi-country evidence shows persistent gains (Banerjee *et al.* 2015), long-run follow-ups confirm that they endure for a decade or more (Banerjee *et al.* 2021), and threshold behavior appears among ultra-poor beneficiaries (Balboni *et al.* 2022; Carter and Barrett 2006). Adding a psychosocial component to a capital transfer raises consumption further, which suggests that the

binding constraint is not capital alone (Bossuroy *et al.* 2022). The two propositions are compatible. Failure to detect a trap in pooled data is not evidence that thresholds are absent for the subgroups that programs target, and the success of large transfers is not evidence that the average poor household is trapped.

Fact 7. Measurement error can substantially inflate measured mobility and apparent convergence. Corrections for reporting error cut estimated one-year poverty-exit rates by roughly a third (Lee *et al.* 2017). Accounting for noise substantially reduces measured consumption mobility (Glewwe 2012). Pseudo-panel averaging yields far lower mobility than noisy individual data (Antman and McKenzie 2007). Survey-design experiments show how large the underlying error can be, since recall periods and item lists move measured consumption by double-digit percentages (Beegle *et al.* 2012; Gibson *et al.* 2015). Errors of this size also afflict targeting: proxy-means scores built from mismeasured consumption misclassify a substantial share of households (Gazeaud 2020). The distortion is also uneven across statistics rather than uniform. The size and direction of these distortions are specific to the error model and setting, and the general lesson is that it is useful to check sensitivity rather than assume them away, especially when working with surveys of differing quality.

Fact 8. Mobility conclusions depend on the welfare concept, threshold, and measure. Consumption mobility is often lower than income mobility over short horizons, although the ranking can reverse with the horizon, the welfare construction, and the amount of measurement error. Spells and components approaches classify the same households differently. How persistence is summarized matters as much as which welfare concept is chosen, since alternative mobility indices order the same panels differently (Creedy and Gemmell 2022). Monetary and multidimensional poverty dynamics overlap only partially (Alkire *et al.* 2017; Jäntti and Jenkins

2015). The choice among multidimensional aggregation rules may not have universal agreement, and different rules rank the same populations differently (Alkire and Foster 2011; Ravallion 2011). Relative and societal lines add a further layer, since a household can escape an absolute line while falling behind a relative one (Jolliffe and Prydz 2021).

Fact 9. Evidence on poverty convergence is mixed and specification-dependent. Countries that start poorer grow faster, at least in recent decades (Kremer *et al.* 2022; Patel *et al.* 2021). Countries that start with more poverty do not reduce poverty faster. Ravallion (2012) documented this poverty convergence puzzle across some 90 developing countries and traced it to two channels: a high initial poverty rate depresses subsequent growth, and it also lowers the elasticity of poverty to growth. Later work qualifies rather than overturns the finding. Convergence appears within sub-Saharan Africa over an extended sample, where a strong growth effect dominates the adverse initial-poverty effect (Ouyang *et al.* 2019). It appears once nonlinearity in the relation between initial poverty and subsequent change is allowed for (Crespo Cuaresma *et al.* 2022). It appears within countries, across regions of Mexico, more clearly than between countries (López-Calva *et al.* 2022).

Marrero *et al.* (2025) find club convergence rather than absolute convergence, with the highest-poverty club accounting for nearly half the world's poor and its average poverty barely moving over four decades. Whether poverty converges is therefore an open and specification-dependent question rather than a settled regularity, and it is included here as a debate a reader might navigate. (Section 4 offers a preliminary exploration into its subnational version and finds a pattern that is sensitive to illustrative measurement-error assumptions).

Fact 10. Conflict and fragility are now a dominant correlate of persistent poverty. The geography of extreme poverty has shifted toward fragile and conflict-affected settings, which now

account for a rising share of the global poor and are projected to account for more (Corral *et al.* 2020). The mechanism has the descent signature of Fact 2. Conflict destroys assets and disrupts markets, and the anticipation of violence alone depresses investment and consumption even where violence does not materialize (Rockmore 2020). Welfare losses persist well beyond the end of hostilities, so recovery is slow rather than automatic (Baliki *et al.* 2024; Brück *et al.* 2019).

Cross-country panel evidence for Africa finds the association robust to disaggregating conflict by type and intensity (Okunlola and Okafor 2022), and the global multidimensional poverty estimates now report acute deprivation concentrated in conflict-affected settings (UNDP and OPHI 2024). Fragility also compounds Fact 5, because the settings where poverty is most persistent are those where surveys are hardest to field, so the evidence base is thinnest exactly where it is most needed. Section 4.3 reports a place-level analogue: regions in economies on the World Bank's fragility list are more than three times as likely to be persistently high-poverty, and they are also markedly less likely to be measurable at all.

3.2 Two further observations

The two observations below are as well established in the literature as the ten regularities above. Both rest on the cited literature because the SPID can speak to neither.[3]

Observation 1. Vulnerability extends well above the poverty line. Vulnerability asks who faces a non-trivial probability of falling below the line in the immediate future, rather than who is below it now, and can be formalized as expected poverty or low expected utility (Ligon and Schechter 2003). The poverty line and the vulnerability line (a higher threshold) together bound a "vulnerable" class that is distinct from and sandwiched by the poor and the secure middle class (Dang and Lanjouw 2017; Ferreira *et al.* 2013; López-Calva and Ortiz-Juarez 2014). Policy aimed

[3] Regional welfare aggregates carry no information on the risk an individual household faces, and no dataset of places can follow one generation into the next.

only at the currently poor therefore leaves that stratum unaddressed. Furthermore, poverty traps supply the dynamic theory beneath poverty persistence: threshold externalities holding households below a critical asset level, against the null of slow, single-equilibrium convergence (Carter and Barrett 2006; Kraay and McKenzie 2014).

Observation 2. Intergenerational persistence is higher in poorer and more unequal economies. *et alet al*The Great Gatsby curve extends to the developing world, and educational mobility is lowest in sub-Saharan Africa and South Asia (Corak 2013; Narayan *et al*. 2018; van der Weide *et al*. 2024). Emran and Shilpi (2021) review the measures and the identification problems specific to poorer countries, where co-residence and missing parental records complicate the standard estimators. Within-country variation is large enough that regional gaps can rival the gaps between countries (Alesina *et al.* 2021; Asher *et al.* 2024) (which connects this observation to Fact 3).

4. Illustrative application: what repeated cross-sections reveal about places

Section 4 asks whether the patterns discussed above appear in regional poverty rates. Because the unit of analysis there is the place rather than the household, the new evidence provides direct evidence for some regularities and place-level analogues for others.

4.1 Scope of the illustration

Everything in this section is a statement about places rather than about household transitions, and Figure 1 fixes the boundary. The route traced there ends in three feasible measures: transitions of regional headcounts across poverty bands, persistence above a threshold across observed surveys, and non-anonymous growth incidence for tracked regional aggregates.

What the SPID observes is the poverty headcount of the same region at multiple survey dates. What that supports directly is movement across bands, persistence above a threshold, association with national income-class change, and comparison of monetary and multidimensional regional change. Four things lie outside its reach. It cannot say which households entered or exited poverty, nor what caused those transitions. It cannot establish whether a household-level poverty trap exists. It cannot separate genuine poverty change from migration and demographic composition.[4] Table 1 states the mapping and the necessary qualifications. The term "persistent high-poverty regions" is used throughout in place of "chronically poor regions", to keep the household vocabulary of Fact 4 distinct from the place-level classification computed here.

4.2 Data

The SPID data are harmonized welfare aggregates and poverty measures for 1,949 subnational regions (typically the first administrative level) in 135 economies over 2000–2024, expressed in 2021 purchasing power parity (PPP) dollars (Dang *et al.* 2026). Welfare aggregates constructed by national teams are standardized, spatially and temporally deflated, flagged for comparability, and are based on official household (income or consumption) surveys. Regional headcounts are therefore similar to the global counts but applied one administrative level down. Poverty is measured at $3.00, $4.20, and $8.30 a day in 2021 PPPs. Two companions complete the toolkit: the World Bank's historical income-classification panel, and the subnational Multidimensional Poverty Measure (MPM). The sample spans all income groups, including 190 regions in 18 high-income economies, which are retained as a benchmark. (Table A2 reports every

[4] Recovering any of these would require household-level data. Migration and demographic composition are particularly consequential, since a region can record falling poverty because poor households left rather than because they became better off. The SPID does not identify migrants, and this analysis makes no attempt to separate the two channels.

headline result for developing economies alone). The design and the robustness checks are defined in the appendix (Table A2).[5]

4.3 Results

Table 1 maps the results discussed below to the ten stylized facts discussed above (Section 3.1), and each result below is also tagged with the fact it speaks to. Table 2 reports the transition matrix across bands of the initial headcount distribution. Movement is common and predominantly in one direction. Over a median of 12 years, 37.6% of regions ended in a lower-poverty band and 5.6% in a higher one, while 56.8% stayed. Both tails are the most persistent, with roughly seven in eight of the least-poor fifth and seven in ten of the poorest fifth still where they began (Fact 1). Persistence in the low-poverty tail reflects an achieved floor, whereas persistence in the high-poverty tail is the place-level counterpart of chronic poverty.

Fifteen regions entered the poorest band, so movement toward higher poverty is a real if minority flow (Fact 2). Of the 248 regions in the initial poorest band, all but 21 are in sub-Saharan Africa using the World Bank regional grouping.[6] The band widths are deliberately unequal, since the least-poor band covers headcounts up to 1% while the poorest band is open above 66.4%, reaching 97.7% in this sample. Diagonal persistence at the tails is therefore partly mechanical, and the summary columns rather than individual cells carry the message.

Two qualifications bound the reading. The ratio of downward to upward movement is about seven to one in the baseline. It falls to roughly four to one under country weighting and to

[5] Four points are notable. Weighting is a choice of estimand rather than a ranking of methods: equal-region weighting describes the typical administrative region, equal-country weighting the typical country, and population weighting the typical person. The first two are reported here and do not bound the third, which is unavailable because the SPID does not carry region populations. In addition, comparability breaks are pervasive. Only 569 of the 1,239 anchored spells, in 50 economies, begin and end within the same comparability spell. Just over half of the transition sample therefore crosses a flagged break. The country bootstrap resamples the 91 economies with replacement, with all of a drawn country's regions entering together.

[6] The World Bank grouping places Djibouti in the Middle East and North Africa.

about six to one on comparability-clean spells. It rises to nearly ten to one among developing economies alone, and to thirteen to one when regions whose headcount lies within two percentage points of a band cutoff at either date are dropped (Table A2).

The direction of the result is robust; its magnitude is not. Because the bands are absolute, a region can also move to a lower-poverty band through national growth alone. The positional counterpart makes the contrast sharp. When regions are re-ranked into quintiles at each date, two-thirds remain in their initial quintile and more than five in six of the initially poorest fifth stay poorest. Broad absolute progress against poverty therefore coexists with strong relative persistence among places, and the two facts answer different questions (Fact 8). The matrix at the $3.00 and $8.30 lines is reported in the appendix (Table A3).

Figure 2 relates regional trajectories to the national context. Economies are grouped by whether their World Bank income classification was upgraded, unchanged, or downgraded between 2010 and 2024, which yields twenty-three, sixty-seven, and one of the 91 sample economies. The figure reports the within-country share of regions whose headcount fell and the mean annualized change. Income-group upgrades go together with broader and faster regional poverty reduction (Fact 3). In the average upgraded economy, about five in six sampled regions recorded falling poverty, against two in three in the average unchanged economy, with mean reductions of 0.9 against 0.5 percentage points per year. Both statistics are averages across countries, not across regions. Panel A shows the underlying country-level variation, which is wide within both groups, so the group means should not be read as describing homogeneous blocs. The association survives restriction to developing economies and to comparability-clean spells, and it widens at higher poverty lines (Figure A1).

No causal reading is available in either direction. Upgrades and broad regional progress are joint outcomes of national growth, and countries that upgrade are selected by their starting proximity to a classification threshold. The single downgraded economy appears as one labeled point in Panel A and is excluded from the group averages, since a bar drawn from one observation invites comparison with groups of 23 and 67. One timing concern deserved a check rather than a caveat. Economies are classified by their income group in calendar 2010 and 2024, whereas the poverty spells end between 2017 and 2023, so an upgrade recorded after a country's final survey could be attached to an earlier trajectory. Reclassifying each economy by its income group in its own baseline and final survey years agrees with the calendar classification for 91.2% of the 91 economies. It reduces the upgraded group from 23 economies to 17, and it widens rather than narrows the gap: an average within-country share of 89.8% of regions with falling poverty against 66.9%, and mean reductions of 1.12 against 0.47 percentage points per year. The association therefore remains under survey-aligned classification.

Figure 3 maps the persistence classification. Of the 1,949 SPID regions, 1,294 in 101 economies have the three surveys spanning eight years that classification requires. Among them, about one-fifth were persistently above a 50% headcount at every observed survey, another one-fifth were above at some surveys but not all, and the remaining three-fifths stayed below the threshold at every survey. The persistent share is stable across design choices (Table A4 reports in full) (Fact 4). It is 17.9% under country weighting, 22.4% among developing economies, and 14.4% on internally comparable survey runs. A balanced three-observation design gives 19.2%, and moving the threshold from 60% to 40% spans 14.8% to 22.3%.

The episodic share is not stable, and two checks show why. Under the balanced design, which keeps each eligible region's first, middle, and last observation so that every region

contributes exactly three, the episodic share falls from 19.2% to 17.3%. A region observed six times has more opportunities to cross the threshold than one observed three times. More strikingly, when regions with any observation between 45% and 55% are dropped as borderline, the episodic share collapses by two-thirds to 6.4%, while the persistent share moves from 19.1% to 20.9%. This shows that the classification depends strongly on near-threshold observations, but it does not distinguish genuine crossings from measurement error, since a trajectory of 48, 52 and 58% would be dropped along with a noisy estimate near 50%. The near-equality of the two groups in the baseline should therefore not be treated as a finding. The persistent classification, which requires every observation to clear the threshold, is the robust half of the exhibit.

Persistent high poverty is geographically concentrated in 28 economies, led by Malawi, Rwanda, Madagascar, and Tanzania. These lie in eastern and southern Africa more than in the Sahel, which runs against the prior held at the outset of this exercise. Line dependence is strong, since the persistent share is 8.7% at $3.00 and 37.4% at $8.30 (Figure A2). The map has two limitations worth stating. It shades countries by the share of their classifiable regions, so within-country pockets are invisible and near-zero shares are visually close to no-data countries. The coverage gaps are themselves a result (Fact 5). India, the Democratic Republic of Congo, and Sudan have too few qualifying surveys, so some of the largest poor populations are absent from the classification altogether.

Figure 4 maps those pockets directly, using the subnational boundary files released with the SPID. Shading a country by the share of its classifiable regions treats a country with one deeply poor province inside otherwise prosperous territory much like a country with no poor regions at all, which hides the cases of most policy interest. Across the 114 economies with at least five regions in their latest survey, the gap between the worst region and the country average averages

10.1 percentage points outside sub-Saharan Africa and 18.5 within it. Among economies outside sub-Saharan Africa whose country average is below 30% (where a pocket is distinguishable from general poverty) the largest gaps are in Panama at 37.3 points, Iran at 35.8, and Colombia at 33.0, followed by Indonesia, the Philippines, and India. In each of these six economies the average region has a headcount poverty rate between 7% and 21% at the $4.20 line, while at least one region sits above 40%, and in Panama, Colombia, and Indonesia above 50%. The map names them: Guna Yala in Panama, Sistan and Baluchestan in Iran, La Guajira in Colombia, East Nusa Tenggara in Indonesia, Zamboanga del Norte in the Philippines, and Chhattisgarh in India. These are not artifacts of small administrative units. Across all 114 economies the highest-poverty region sits at the median of its own country's size distribution, and in these six it sits at the 68th percentile by area and holds a median 3.3% of national land.

Figure A3 sets these economies against the rest: panel (a) shows the full spread of regions behind each, and panel (b) places all 114 economies, confirming that pockets of this kind are a middle-income phenomenon, since where national poverty is very high the distinction does not arise and where it is very low the gap closes.[7]

Figure 5 decomposes the classification by the country's 2010 income group. The composition shifts steeply with development (Fact 4). Almost three-quarters of low-income countries' regions are persistently high-poverty, against 15.2% in lower-middle-income and 1% in upper-middle-income countries, and none in high-income ones. The episodic share peaks among lower-middle-income regions, at about two in five, in the middle of the income ladder rather than at the bottom, because episodic status requires straddling the 50% threshold. That peak is not a

[7] National inequality is a weak guide to these pockets. Among economies outside sub-Saharan Africa with average regional poverty below 30%, the correlation between the national Gini coefficient and this gap is 0.34, and Iran and India post two of the six largest gaps at Gini coefficients near the bottom of the distribution. Spatial concentration of poverty and interpersonal inequality are related but distinct (Fact 3).

fixed feature of places. At $3.00 it sits among low-income regions and at $8.30 among upper-middle-income ones, since the set of straddling regions climbs the ladder with the line (Figure A4). The rightmost bar reports all classifiable SPID regions, which is an unweighted and incomplete sample of 1,294 regions in 101 economies rather than a world total.

Fragility sharpens the geography (Table A5 reports the cross-tabulation in full). Among classifiable regions, 48.8% of those in economies on the World Bank's fragility list are persistently high-poverty, against 14.5% elsewhere, and their poverty fell at less than half the pace with reversals more than twice as frequent (Fact 10).

The coverage gap makes that an understatement. Only 13 of the 38 fragile economies have the surveys classification requires, and 43.7% of their regions can be classified against 72.2% elsewhere; Afghanistan, Somalia, South Sudan, Sudan, Syria, and the Republic of Yemen are all absent. Persistent high poverty is therefore more common among the classifiable regions of these economies, and coverage there is markedly lower. Because the unclassifiable regions may differ systematically from the observed ones, the estimate should not be generalized to all fragile settings, and the direction of the selection bias is unknown. Facts 5 and 10 work together here, and the combination provides a clear illustration of the chapter's thesis: the evidence is thinnest exactly where poverty is most persistent.

Figure 6 adapts the non-anonymous growth incidence curve of Bourguignon (2011) from incomes to poverty rates (Fact 9). Panel A plots the annualized change in the headcount, with negative values denoting poverty reduction, against each region's initial percentile in the distribution of regional headcounts. Dots are ventile means and the dashed line is the anonymous counterpart. Regions with higher initial poverty recorded larger subsequent reductions. Changes are flat at zero through the bottom quarter, where headcounts start at the floor, and reach 1.2 to 1.4

percentage points of reduction per year in the upper ventiles. The fitted slope is −0.0157 per initial percentile, with a standard error of 0.0035 clustered on the economy. A second and distinct regression, on the initial headcount itself rather than on its percentile, gives −0.0125 per initial percentage point, with a standard error of 0.0036 (from the country bootstrap that Panel B also uses). The two are different specifications, not the same coefficient rescaled, and the measurement-error correction below applies to the second.

This pattern should be read with more caution than any other in the chapter, and it is reported as descriptive rather than as evidence about traps or convergence. Three problems compound. Low initial headcounts leave little room for a further decline, since for a spell of length T the annualized change is bounded by $-h_0/T$, so the floor mechanically compresses change among the initially least poor, and the relationship is in any case visibly nonlinear, which a single line summarizes poorly. The regressor is the baseline value of the outcome, so classical measurement error in the initial headcount produces mean reversion that mimics convergence (Fact 7). Panel B traces that correction across assumed reliability ratios, applying it within each country-bootstrap replication and using each spell's own duration rather than the median. The corrected slope is −0.0086 at a reliability ratio of 0.95, with a 95% interval that still excludes zero. It is −0.0040 at 0.90, with an interval that no longer does, and the line drawn between the plotted points crosses zero at about 0.86.

No particular value is claimed to be the true reliability of a regional headcount. No published estimate covers this object, which compounds household reporting error with survey sampling error and comparability error, so the exercise is illustrative. What it shows is that error of a size no one can currently rule out is enough to remove the result. A baseline-change regression of this kind is not a test for poverty traps at any level of aggregation, and the specification pools

regions across economies rather than estimating a within-country relationship. The pattern is consistent with convergence in regional poverty rates but is not evidence against traps, and at $8.30 the slope is indistinguishable from zero (Figure A5). Read against Fact 9, the subnational evidence sits with the qualified rather than the affirmative side of that literature: the pooled association between initial regional poverty and subsequent change is sensitive to the illustrative measurement-error assumptions. One observation does not depend on the slope at all. The non-anonymous curve follows the same regions, whereas the anonymous curve compares positions in the two cross-sectional distributions. The two track each other closely until about the 75th percentile and then diverge. Differences may reflect re-ranking; with unequal survey intervals, they can also reflect the way changes are annualized.

Figure 7 compares monetary and multidimensional dynamics over matched spells (Fact 8). Monetary poverty is the SPID headcount at $4.20. Multidimensional poverty is the subnational MPM headcount, matched to the same region and survey year so that both series come from the same underlying surveys. Spells run at least five years, giving 1,210 regions in 80 economies with a median span of 10 years. The two often agree on direction. The Pearson correlation of annualized changes is 0.79 and the Spearman rank correlation 0.76. Taking all 1,210 spells as the denominator, both measures fall in 60.3% and both rise in 17.9%, so 78.2% move in the same direction. A further 19.2% are discordant and 2.6% show no change in one measure.[8]

The discordant fifth is the substantive point. The measures should not be treated as substitutes, because in roughly one spell in five a region's progress depends on which is consulted. Agreement itself depends on the line, falling from 0.83 at $3.00 to 0.28 at $8.30 (Figure A6). Part

[8] Agreement is robust to the restrictions applied elsewhere, with the correlation between 0.74 and 0.84 and the same-direction share between 74.4% and 81.6% under country weighting, developing-only, comparability-clean, and longer-spell samples (Table A6).

of that gradient is mechanical, since the multidimensional index embeds a monetary dimension defined at $3.00 a day. Isolating the non-monetary component of the index would show how much of the agreement is mechanical, but the SPID release used here does not expose the component indicators, so the question is left open. Correlation and sign agreement establish co-movement, not interchangeability.

Four caveats bound the section, each already stated above. These are dynamics of places rather than of people. Regions are unweighted. Just over half the transition spells cross a comparability break, though the headline results hold on the clean subsample. The analysis is descriptive throughout.

5. Summary and Policy Implications

Three empirical takeaways are useful. Among regions that changed bands over the 2010s, movement was predominantly toward lower poverty, by about seven to one in the baseline, but movement toward higher poverty did not disappear. The ratio ranges from about four to one under country weighting to about thirteen to one when regions near a band cutoff are dropped, so its direction is robust and its magnitude is not. Persistent high poverty is geographically concentrated and affects about a fifth of classifiable regions at the $4.20 line, a share ranging from 14.4% to 22.4% across the design checks reported in Table A4. But those regions sit in only 28 economies, mostly in eastern and southern Africa. Fragility is a strong correlate: regions in economies on the World Bank's fragility list are more than three times as likely to be persistently high-poverty, and their regions are about 40% less likely to be classifiable at all, a gap of 28.5 percentage points. Monetary and multidimensional measures agree on the direction of regional change about four times in five, a share that holds between 74 and 82% across every restriction tested. That is close

enough to be reassuring, and far enough from unanimity that the choice of measure remains a choice of question.

Some caveats are in order. Convergence among initially poorer regions, which a naive reading of Figure 6 would report as a headline, loses statistical significance once an illustrative reliability of about 0.90 is assumed for the baseline headcount. The near-equality of persistent and episodic regions is similarly fragile, since the episodic share falls by two-thirds when borderline observations near the threshold are excluded, although the persistent share moves only from 19.1% to 20.9%. The data coverage gaps are notable: 655 of 1,949 regions cannot be classified at all, and the missing economies include several of the largest poor populations in the world, along with most of the fragile and conflict-affected ones.

Several policy implications are useful. The first concerns the balance between promotion and protection. Facts 1 and 2 together imply that a fixed beneficiary list drawn from one survey will be inadequate, because the population touched by poverty over a decade is a multiple of the stock of poor at any date and the households about to descend are, by definition, non-poor when the shock arrives. Social protection therefore needs mechanisms to update eligibility and to expand rapidly after shocks, alongside support for persistent deprivation. Health coverage and shock-responsive transfers are thus useful anti-poverty instruments.

The second concerns place. Fact 3 and the results of Section 4 together imply that national averages are a poor guide to where poverty policy should act, and that a narrow set of regions in eastern and southern Africa accounts for a large share of persistent high poverty. Pockets of deep poverty inside otherwise progressing countries are a second and distinct target, and the widest of them are outside Africa. Geographic targeting is therefore worth considering, though counts of administrative regions are not counts of poor people, and place-based and household-based

approaches are best treated as potentially complementary. The coverage gaps documented above mean that the regions most likely to need it are the ones least likely to be measurable. Fact 10 sharpens the point. Fragile settings combine the highest persistence with the thinnest data, so a targeting rule that treats missing observations as evidence of low need risks overlooking them, and survey investment in those settings is itself a distributional choice rather than a technical one. The third concerns measurement itself. Fact 7 and Figure 6 together imply that an anti-poverty program evaluated on regional poverty statistics could be credited with convergence that is largely mean reversion, so reliability assumptions belong in the evaluation rather than in a footnote to it.

The data agenda follows from Fact 5 rather than from any conceptual dispute, and three priorities stand out in order. First, rotating panel components attached to existing national surveys would add direct household transition evidence to infrastructure that already exists, wherever survey programs can sustain the tracking. Second, repeat or validation measures of the same welfare construct within a survey wave would allow the reliability ratios that error corrections require to be estimated rather than assumed. Income and consumption modules are different constructs rather than replicate measures, so this requires either genuine repeat measurement or an explicit model linking the two. Third, region populations and boundary files should be published alongside subnational welfare aggregates, since without them no subnational statistic can be made representative of people rather than of administrative units.

We end with some thoughts on further research. While the SPID offers useful analysis for poverty dynamics at the regional level, more accurate policy interventions still require a better understanding at the household (or individual) level. Furthermore, various seemingly innocuous choices made regarding the unit of analysis, reference benchmark, poverty line, and error structure,

can affect the estimation results. We should take these choices into careful consideration for better understanding of poverty dynamics.

6. Cross-References

- Upward Mobility in Developing Countries
- Measuring Global Poverty in a Changing World
- Multidimensional Poverty: Measurement, Analysis, Applications
- Income, Consumption, Wealth

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Table 1. What the new evidence can and cannot say about each stylized fact

Fact	What the SPID exhibit provides	Status
1. Poverty is fluid	Movement of regional headcounts across fixed bands (Table 2)	Place-level analogue; household churning not observed
2. Entries consequential, drivers differ	Frequency of movement to higher-poverty bands (Table 2)	Partial analogue for frequency; drivers not tested
3. Geography conditions trajectories	Regional trajectories and national income-class change (Table 2, Figure 2)	Direct, and descriptive rather than causal
4. Persistent and episodic poverty both large	Regions above a 50% headcount at all, some, or no surveys (Figures 3 and 5)	Place-level persistence analogue, not household chronic poverty
5. Panel scarcity binds	Coverage gaps that leave 655 regions unclassifiable (Figure 3, Table A5)	Illustrates the constraint; does not validate synthetic panels
6. Traps not ubiquitous; big transfers work	—	Not tested; Figure 6 is expressly not a trap test
7. Measurement error inflates mobility	Reliability-ratio sensitivity of the Figure 6 slope (Table A2)	Illustrative sensitivity analysis; the convergence interpretation is not robust across the assumed reliability values
8. Conclusions depend on the measure	Matched monetary and multidimensional regional changes (Figure 7)	Direct, qualified by the shared monetary component
9. Evidence on poverty convergence is mixed and specification-dependent	Subnational counterpart of the convergence regression (Figure 6)	Subnational analogue; fragile to measurement error
10. Conflict and fragility correlate with persistent poverty	Persistence and coverage by fragility status (Table A5)	Place-level analogue; classifiable fragile economies are a selected subset

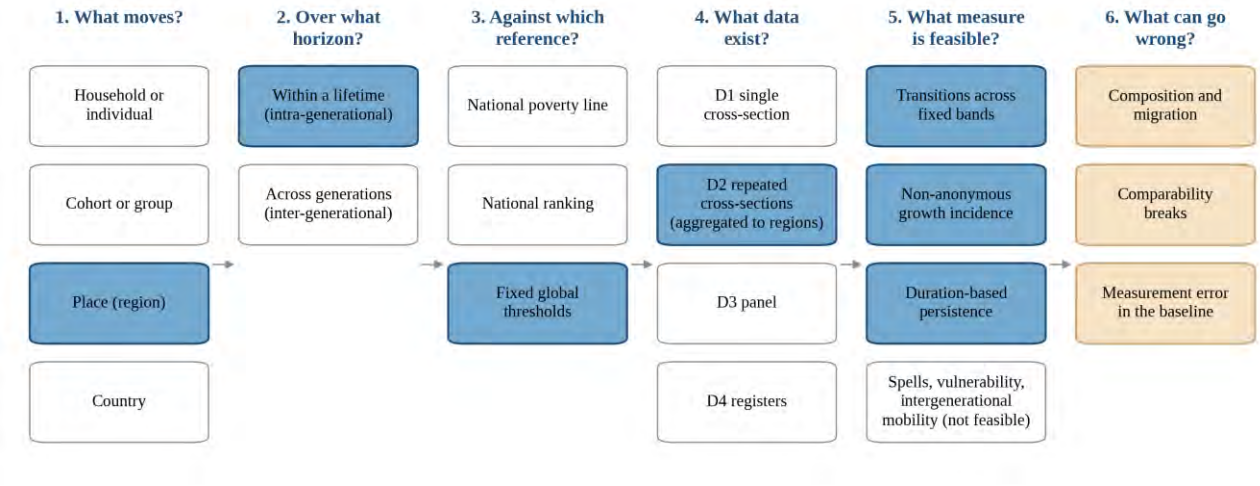
Note: “Direct” means the exhibit estimates the regularity for the unit on which it is stated. “Analogue” means the exhibit estimates a place-level counterpart of a household-level regularity, which is informative but not equivalent. Regularities 1, 2, and 4 are stated for households in the literature and can only be tested here for places.

Table 2. Among regions that changed bands, movement was predominantly toward lower poverty

Initial quintile (circa 2010)	To: Q1	To: Q2	To: Q3	To: Q4	To: Q5	Lower / same / higher band (%)
Panel A. Number of regions						
Q1 (least poor)	215	32	1	0	0	
Q2	131	103	14	0	0	
Q3	37	92	111	7	0	
Q4	4	7	120	102	15	
Q5 (poorest)	0	0	5	70	173	
Panel B. Row percentages (resampling dispersion in parentheses)						
Q1 (least poor)	86.7	12.9	0.4	0.0	0.0	— / 86.7 / 13.3
	(4.5)	(4.2)	(0.5)	(0.0)	(0.0)	
Q2	52.8	41.5	5.6	0.0	0.0	52.8 / 41.5 / 5.6
	(8.8)	(7.9)	(2.3)	(0.0)	(0.0)	
Q3	15.0	37.2	44.9	2.8	0.0	52.2 / 44.9 / 2.8
	(6.5)	(4.9)	(5.9)	(1.5)	(0.0)	
Q4	1.6	2.8	48.4	41.1	6.0	52.8 / 41.1 / 6.0
	(1.2)	(1.0)	(6.8)	(5.6)	(2.8)	
Q5 (poorest)	0.0	0.0	2.0	28.2	69.8	30.2 / 69.8 / —
	(0.0)	(0.0)	(1.0)	(6.1)	(6.5)	

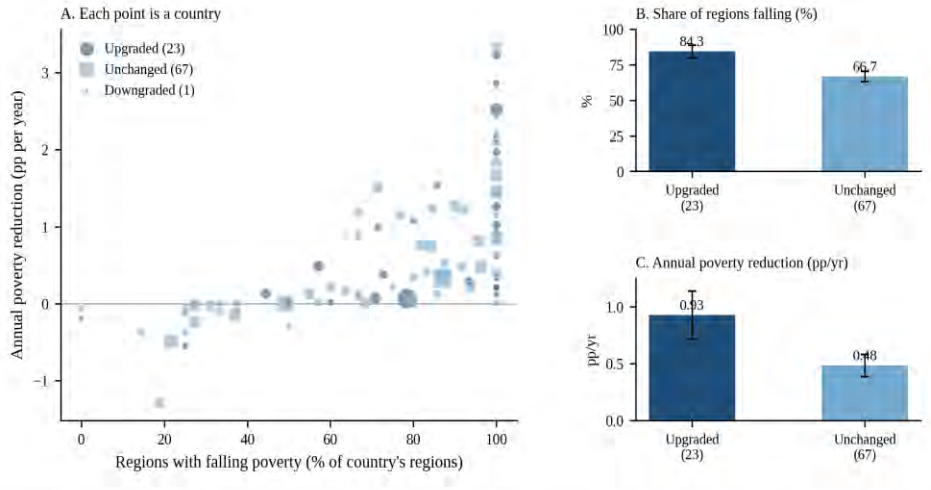
Note: 1,239 SPID regions in 91 economies with a survey in both 2008–2012 and 2017–2023, median span 12 years. Quintiles are defined on the initial regional headcount at the $4.20-a-day (2021 PPP) line, with cutoffs of 1.0%, 6.0%, 27.9%, and 66.4% applied at both dates, so the table records absolute rather than positional movement. Parentheses in Panel B are resampling dispersion, drawing the 91 countries with replacement 1,000 times. Re-ranking regions at each date instead leaves 66.0% in their initial quintile and 85.5% of the poorest fifth still poorest. Regions are unweighted. Table A2 reports weighting and comparability alternatives; Table A3 repeats the matrix at the $3.00 and $8.30 lines. Source: authors' calculations using the SPID.

Figure 1. From question to measure: the six choices behind a mobility statement



Note: The six choices of Section 2. Shaded boxes trace the route taken by the evidence of Section 4: the unit is the place, the horizon is within a lifetime, the reference is a set of fixed global thresholds, and the data are repeated cross-sections that form an unbalanced panel of regional aggregates. The three shaded measures in column 5 are those the route supports; spells, vulnerability, and intergenerational mobility require units the data do not track. Column 6 names the failure modes that Section 4.3 and Table A1 address. Source: authors' construction.

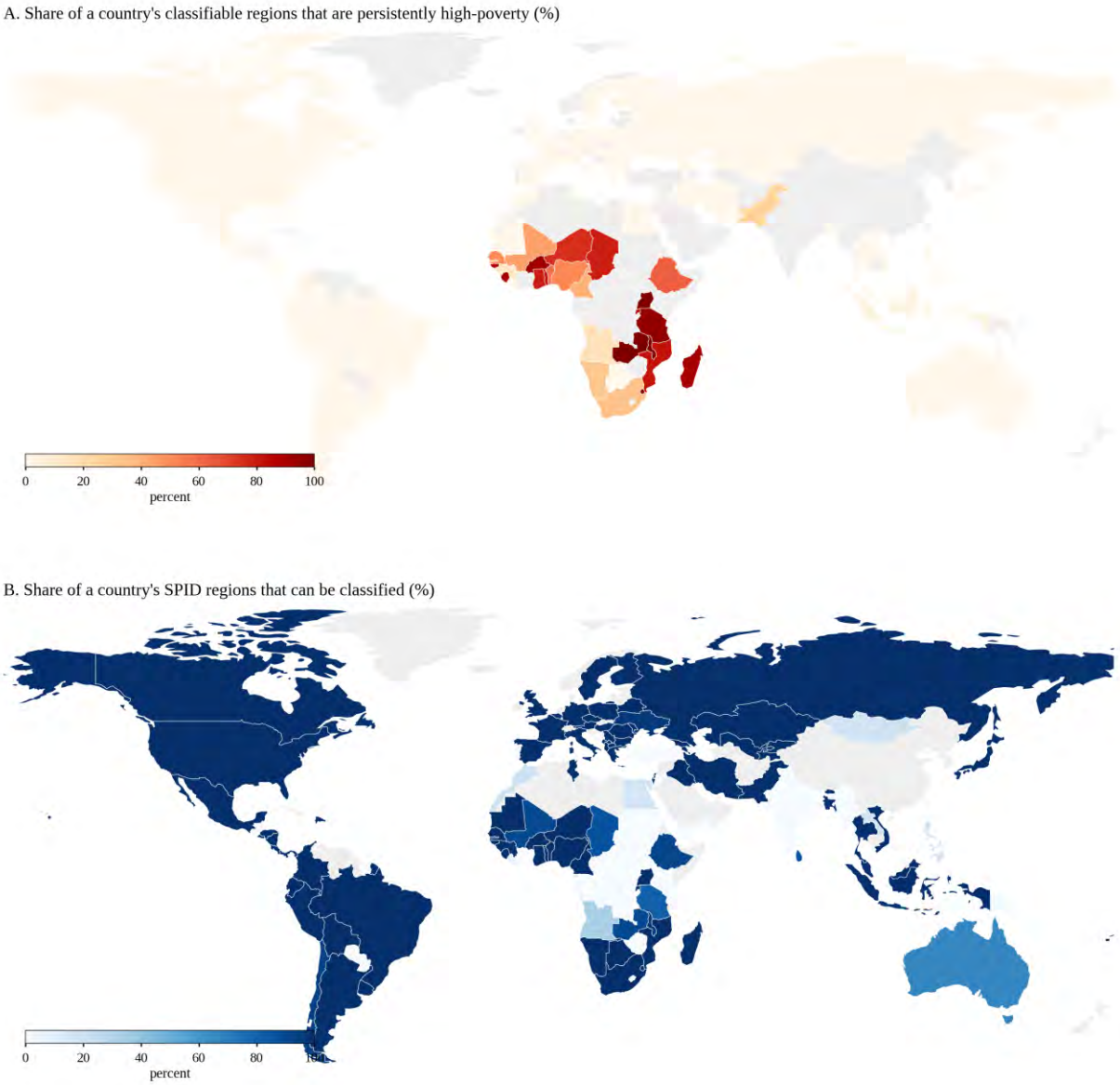
Figure 2. Income-group upgrades are associated with broader and faster regional poverty reduction, 2010–2024



Panels B and C are averages across the countries of each group, with standard errors of the country mean. The single downgraded economy is a triangle in Panel A and is omitted from B and C. Higher is better in every panel.

Note: Country classifications are World Bank income groups from the official historical classification (fiscal years corresponding to 2010 and 2024 data). Poverty is the regional headcount at the $4.20-a-day (2021 PPP) line. Bars are unweighted averages across the countries in each group, computed from country-level means; whiskers are standard errors across countries and are omitted for the downgraded group, which contains a single country. The sample is that of Table 2, and every sample country has at least two regions. pp = percentage points. Panel A shows the underlying country-level observations and Figure A1 the same comparison at the $3.00 and $8.30 lines. Source: authors' calculations using the SPID and the World Bank income-classification panel.

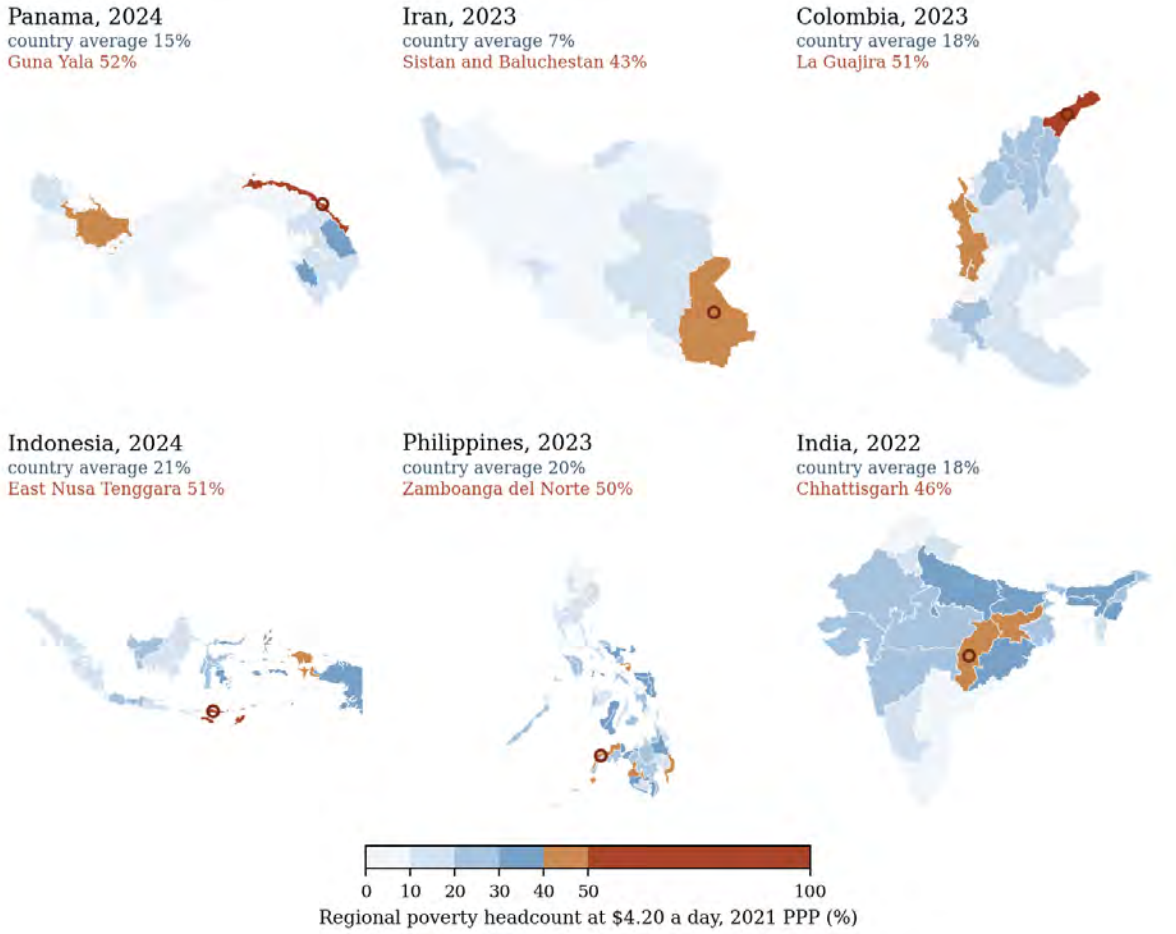
Figure 3. Country shares of classifiable regions with persistent high poverty, and classification coverage



Grey denotes no SPID data or no classifiable region. Shading is at country level rather than region level, so within-country pockets are not visible.

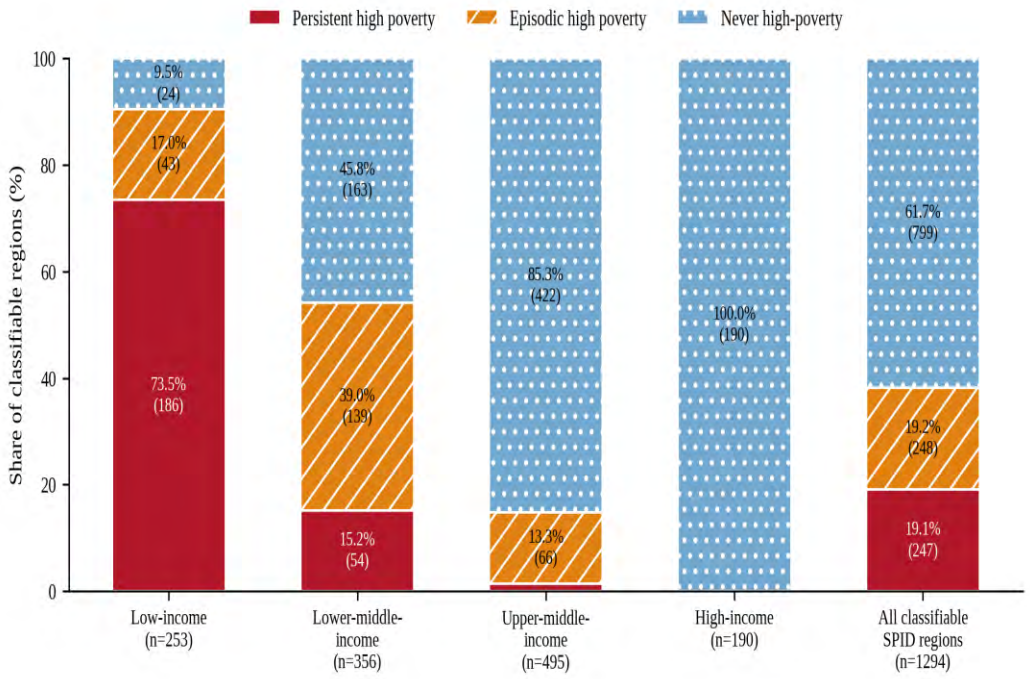
Note: A region is classified from all its surveys over 2000–2024 at the $4.20-a-day (2021 PPP) line, with high poverty a headcount above 50%: persistent if above at every survey, episodic if above at some but not all. Classification requires three surveys spanning eight years, which 1,294 of the 1,949 SPID regions meet, in 101 economies; 19.1% are persistent, 19.2% episodic, 61.7% never high-poverty. Countries are shaded by the share of classifiable regions that are persistent, so within-country pockets are invisible; boundary files are available for most economies, and Figure 4 maps regions directly for six of them. Panel B shows coverage, since a high share among few classifiable regions can mislead. Figure A2 repeats the map at the $3.00 and $8.30 lines. Source: authors' calculations using the SPID.

Figure 4. Deep pockets of poverty sit inside otherwise progressing countries



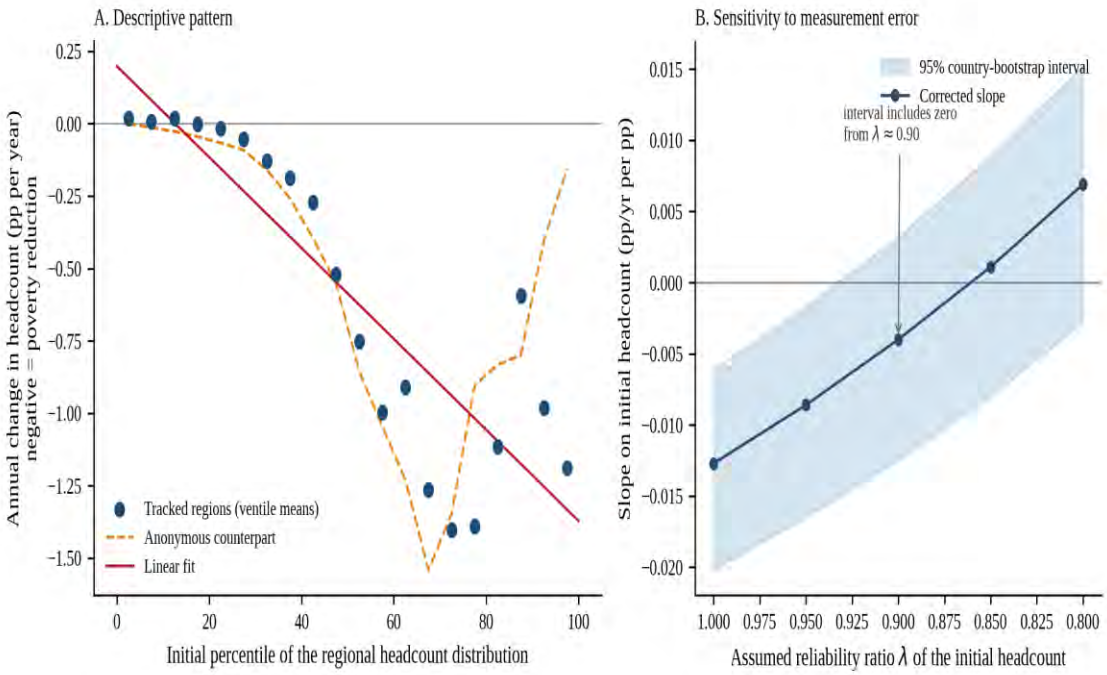
Note: Regional poverty headcounts at the $4.20-a-day (2021 PPP) line in each economy's latest survey, from the SPID with the boundary files of the same release. The six economies are those named in the text: outside sub-Saharan Africa, with a country average below 30%, and with the largest gap between the highest-poverty region and that average. The circle marks the highest-poverty region, named beneath the country. Panama's comarca appears in the SPID as Kuna Yala and is given here in the current spelling, Guna Yala. Regions are unweighted, so a pocket in a small province and one in a populous state count equally; the highest-poverty region is not systematically small, sitting at the 68th percentile of its own country's regions by area in these six and holding a median 3.3% of national land. Figure A3 shows the full distribution of regions within these economies and the pocket gap across all 114 economies. Source: authors' calculations using the SPID and the AM25 subnational boundary files.

Figure 5. The persistent–episodic composition shifts sharply with country income group and poverty line



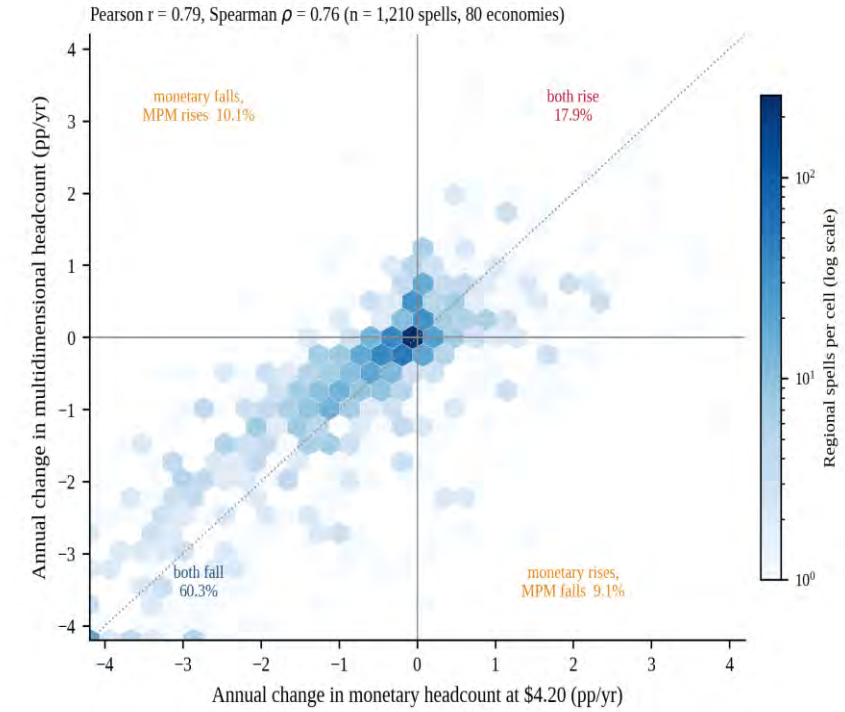
Note: The classification is that of Figure 3, at the $4.20-a-day (2021 PPP) line; income groups are World Bank classifications for 2010. Bars show unweighted shares of each group's classifiable regions, with region counts inside the bars; the rightmost bar pools all classifiable regions. Figure A4 reports the same decomposition at the $3.00 and $8.30 lines, where the episodic share peaks in low-income and upper-middle-income regions respectively: episodic status requires straddling the 50% threshold, so the peak shifts up the income ladder as the line rises. Source: authors' calculations using the SPID and the World Bank income-classification panel.

Figure 6. The apparent convergence pattern is sensitive to illustrative measurement-error assumptions



Note: Panel A is non-anonymous in the sense of Bourguignon (2011), so regions are tracked and the horizontal axis is each region's initial percentile among the 1,239 regional headcounts of Table 2. Dots are ventile means of the annualized change, negative values denoting reduction; the dashed line is the anonymous counterpart. The fitted slope is −0.0157 per initial percentile (standard error 0.0035, clustered on the economy). Panel B corrects a separate regression, of the annualized change on the initial headcount itself, for classical error in that regressor across assumed reliability ratios. The uncorrected slope is −0.0125, with a country-bootstrap standard error of 0.0036 and a clustered standard error of 0.0035; the 95% interval excludes zero at $\lambda = 0.95$ but not at $\lambda = 0.90$, and the line between plotted points crosses zero at about $\lambda = 0.86$. No published reliability estimate exists for regional headcounts, so the range is illustrative. Figure A5 repeats Panel A at other lines. Source: authors' calculations using the SPID.

Figure 7. Monetary and multidimensional poverty often move together but are not interchangeable



Note: Monetary poverty is the regional headcount at the $4.20-a-day (2021 PPP) line; multidimensional poverty is the subnational Multidimensional Poverty Measure headcount, matched to the same region and survey year. The index embeds a monetary dimension defined at $3.00 a day, so only the horizontal axis moves across lines. Spells span at least five years: 1,210 regions in 80 economies, median span 10 years. Axes are truncated at ±4.2 pp per year, leaving 48 spells outside the frame, but all shares and correlations use the full 1,210. Both measures fall in 60.3% of spells and rise in 17.9%, they diverge in 19.2%, and the remaining 2.6% show no change in one measure. The correlation is 0.79. Figure A6 repeats the figure at other lines. Source: authors' calculations using the SPID and the subnational MPM.

Appendix A: Additional tables and figures

The main text reports every exhibit at the $4.20-a-day (2021 PPP) line. This appendix repeats each of them at the $3.00 and $8.30 lines, and reports the country-level observations underlying Figure 2. Robustness of the headline results is reported in Tables A2, A4, and A5; population-weighted variants are unavailable because the SPID does not carry region populations.

Table A1. Measure families, units, and data requirements

Measure family	Unit	Minimum tier	Directly observed or model-based
1. Poverty status and profiles	Household	D1	Observed
2. Anonymous growth incidence	Quantile	D2	Observed
3. Poverty transition matrices	Household	D3 (D2 model-based)	Observed at D3; bounds or point estimates at D2
4. Non-directional mobility indices	Household	D3	Observed, but inflated by error
5. Directional upward mobility	Household or quantile	D2	Computable from repeated cross-sectional marginals; does not recover individual transitions
6. Non-anonymous growth incidence	Household or place	D3 for people; D2 for places	Observed where the unit is tracked
7. Spells and components (chronic/transient)	Household or place	D3 for households; D2 for tracked place aggregates	Spells observed where the unit is tracked; components require a permanent-income model
8. Vulnerability to poverty	Household	D3, or D2 with assumptions	Model-based
9. Intergenerational persistence	Parent–child pair	D3/D4, or D1 retrospective	Observed with links; otherwise model-based
Reference-distribution choice (not a tenth statistical family)			
10. Movement across fixed global classes	Place or quantile aggregate	D2 for tracked aggregates and anonymous change; D3, or D2 with modeling, for household transitions	Observed for tracked aggregates; model-based for households

Note: Tiers: D1, single cross-section; D2, repeated cross-sections; D3, household panel; D4, linked administrative registers. "Minimum tier" is the least demanding data structure at which the estimand can be produced, not the structure at which it is best estimated. Rows 1 to 9 are statistical families. Row 10 is separated because it is a choice of reference distribution rather than a new statistic: it applies the transition and growth-incidence machinery of families 3 and 6 to thresholds held constant across units and years, and its data requirement therefore follows the underlying measure.

Table A2. Design choices and robustness of the headline results

	Movement to a lower-poverty band (%)	Movement to a higher-poverty band (%)	Regions falling: upgraded vs unchanged (%)	Slope of annual change on initial headcount
Baseline: region-weighted, all economies	37.6	5.6	84.3 vs 66.7	−0.0125
Country-weighted	35.3	8.3	—	−0.0142
Developing economies only (excl. high income)	42.0	4.4	84.3 vs 74.3	−0.0111
Spells within one comparability run	33.4	6.0	89.7 vs 61.8	−0.0098
Regions within 2 pp of a band cutoff dropped	34.8	2.6	—	—
Reliability ratio $\lambda = 0.95$ / 0.90 / 0.85	—	—	—	−0.0086 / −0.0040 / 0.0011

Note: Bands are quintiles of the initial regional headcount at the $4.20-a-day (2021 PPP) line, with cutoffs applied at both dates so that the matrix records absolute movement. The transition sample is 1,239 regions in 91 economies anchored in 2008–2012 and followed to 2017–2023. Country-weighted figures average within countries before averaging across them. The last column is the slope of the annual change in the headcount on the initial headcount, with standard errors clustered on the economy, which run from 0.0030 to 0.0049 across the four rows and are 0.0035 in the baseline; the 95% interval excludes zero at $\lambda = 0.95$ but not at $\lambda = 0.90$, and the point estimate crosses zero at about $\lambda = 0.86$. The regions-falling column averages within-country shares. Tables A4 and A6 report persistence and monetary–multidimensional robustness. Source: authors' calculations using the SPID.

Table A3. Regional transitions across quintiles of poverty incidence at alternative poverty lines

Initial quintile (circa 2010)	To: Q1	To: Q2	To: Q3	To: Q4	To: Q5	Lower / same / higher band (%)
Panel A. $3.00 a day (2021 PPP): row percentages (resampling dispersion in parentheses)						
Q1 (least poor)	79.0	18.5	2.4	0.0	0.0	
	(5.9)	(5.1)	(1.3)	(0.0)	(0.0)	
Q2	43.1	48.4	8.5	0.0	0.0	
	(8.8)	(8.3)	(2.3)	(0.0)	(0.0)	
Q3	15.0	40.5	40.1	4.5	0.0	
	(6.4)	(5.1)	(5.4)	(2.3)	(0.0)	
Q4	1.2	3.2	52.0	37.9	5.6	
	(0.7)	(1.4)	(6.4)	(6.0)	(3.0)	
Q5 (poorest)	0.0	0.0	3.2	27.0	69.8	
	(0.0)	(0.0)	(1.4)	(6.7)	(7.5)	
Panel B. $8.30 a day (2021 PPP): row percentages (resampling dispersion in parentheses)						
Q1 (least poor)	99.2	0.8	0.0	0.0	0.0	
	(1.1)	(1.1)	(0.0)	(0.0)	(0.0)	
Q2	47.2	49.2	3.6	0.0	0.0	
	(9.0)	(8.1)	(2.0)	(0.0)	(0.0)	
Q3	6.1	54.7	37.2	2.0	0.0	
	(2.5)	(6.0)	(7.4)	(1.0)	(0.0)	
Q4	0.4	4.4	32.3	54.0	8.9	
	(0.4)	(2.2)	(5.8)	(5.5)	(3.3)	
Q5 (poorest)	0.0	0.0	0.4	23.4	76.2	
	(0.0)	(0.0)	(0.5)	(4.9)	(4.9)	

Note: As Table 2, with quintiles defined on the initial distribution of the regional headcount at the line named in each panel. The cutoffs are headcounts of 0.3%, 1.8%, 12.7%, and 43.0% at the $3.00 line and 9.2%, 36.9%, 67.6%, and 92.5% at the $8.30 line. Panel A cutoffs are compressed near zero because most regional headcounts at the $3.00 line are very small, so the Q1–Q2 distinction there falls within measurement error. The share of regions moving to a lower-poverty quintile group is 37.0% at $3.00, 37.6% at $4.20 (Table 2), and 33.7% at $8.30; the share moving to a higher one is 7.9%, 5.6%, and 3.1%. Source: authors' calculations using the SPID.

Table A4. Robustness of the persistence classification

Specification	Regions	Economies	Persistent (%)	Episodic (%)	Never high-poverty (%)
Baseline: >50% headcount, ≥3 surveys, ≥8 years	1,294	101	19.1	19.2	61.7
Panel A. Weighting and sample					
Equal weight per country	1,294	101	17.9	19.3	62.8
Developing economies only (2010 groups)	1,104	83	22.4	22.5	55.2
Longest internally comparable survey run	1,042	78	14.4	10.0	75.6
Panel B. Classification design					
Headcount threshold 40%	1,294	101	22.3	22.6	55.1
Headcount threshold 60%	1,294	101	14.8	17.5	67.8
Minimum 4 surveys	1,138	84	15.7	19.1	65.2
Minimum 12-year span	1,114	84	17.6	20.8	61.6
Balanced: first, middle, and last observation	1,294	101	19.2	17.3	63.4
Borderline regions excluded (any observation 45–55%)	1,031	99	20.9	6.4	72.7
Panel C. Poverty line					
$3.00 a day (2021 PPP)	1,294	101	8.7	17.2	74.1
$8.30 a day (2021 PPP)	1,294	101	37.4	26.2	36.4

Note: A region is persistent if its headcount is above the threshold at every observed survey, episodic if above at some but not all, and never high-poverty otherwise. The line is $4.20 a day (2021 PPP) except in Panel C, and each row varies one choice from the baseline. The balanced row keeps each region's first, middle, and last observation, since a region observed six times has more opportunities to cross the threshold than one observed three times. The developing-only row uses the 2010 income classification, as in Figure 5. The borderline row drops regions with any observation between 45% and 55%. The comparable-run row keeps, for each region, the run with the longest span among runs of at least three surveys, and applies the eligibility screen to that run. Selecting the run with the most surveys instead would drop two regions of West Bank and Gaza, where the run with more surveys covers fewer years, leaving 1,040 regions and the same three shares. Across Panels A and B the persistent share stays between 14.4% and 22.4%, while the episodic share falls to 6.4% once borderline observations are excluded. Source: authors' calculations using the SPID.

Table A5. Persistent high poverty is more common, and far less measurable, in fragile economies

	Fragile and conflict-affected	All other economies	Ratio
Panel A. Measurability			
Economies in the SPID	28	107	—
Economies with classifiable regions	13	88	—
Regions that can be classified (%)	43.7	72.2	0.61
Panel B. Persistence among classifiable regions			
Regions classified	172	1,122	—
Persistently high-poverty (%)	48.8	14.5	3.36
Episodically poor (%)	18.6	19.3	0.97
Never high-poverty (%)	32.6	66.2	0.49
Panel C. Dynamics over the anchored spells			
Moved to a lower-poverty band (%)	20.0	40.4	0.49
Moved to a higher-poverty band (%)	10.6	4.8	2.22
Annual poverty reduction (pp per year)	0.26	0.64	0.40

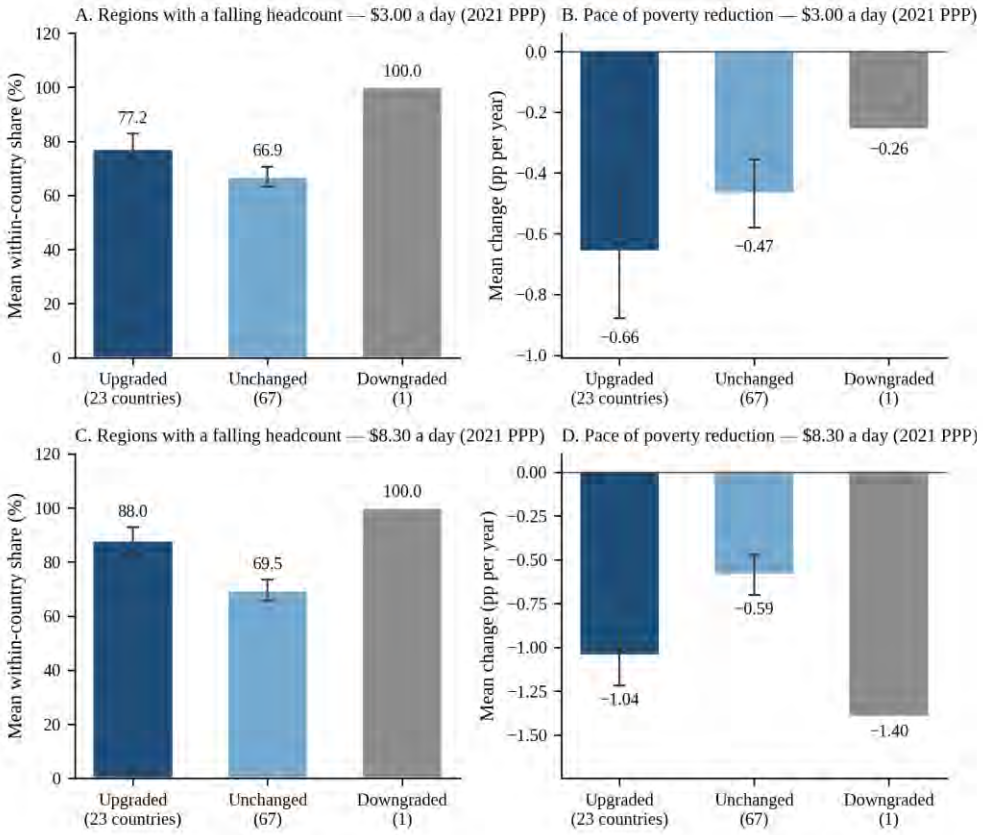
Note: Fragility follows the World Bank's FY26 list of fragile and conflict-affected situations, which names 38 economies, of which 28 appear in the SPID. Poverty is the regional headcount at $4.20 a day (2021 PPP). Panels A and B use the 1,294 classifiable regions of Figure 3; Panel C uses the 1,239 anchored spells of Table 2. Panel A is a selection warning rather than a result: only 13 of the 38 fragile economies have the surveys classification requires, and Afghanistan, Somalia, South Sudan, Sudan, Syria, and the Republic of Yemen are absent, so Panel B should not be generalized to fragile settings the surveys do not reach. Economies are grouped by their FY26 classification applied retrospectively. Source: authors' calculations using the SPID and the World Bank fragility classification.

Table A6. Robustness of the monetary–multidimensional comparison

Specification	Regions	Economies	Pearson r	Spearman ρ	Same direction (%)
Baseline: matched spells, ≥5 years	1,210	80	0.79	0.76	78.2
Correlation across country-mean changes (80 country-level observations)	1,210	80	0.84	0.82	85.0
Developing economies only	1,042	64	0.79	0.77	79.2
Spells within one comparability run	483	44	0.74	0.74	81.6
Minimum 10-year spell	677	45	0.84	0.75	74.4
Monetary line $3.00 a day	1,210	80	0.83	0.80	78.3
Monetary line $8.30 a day	1,210	80	0.28	0.45	74.0

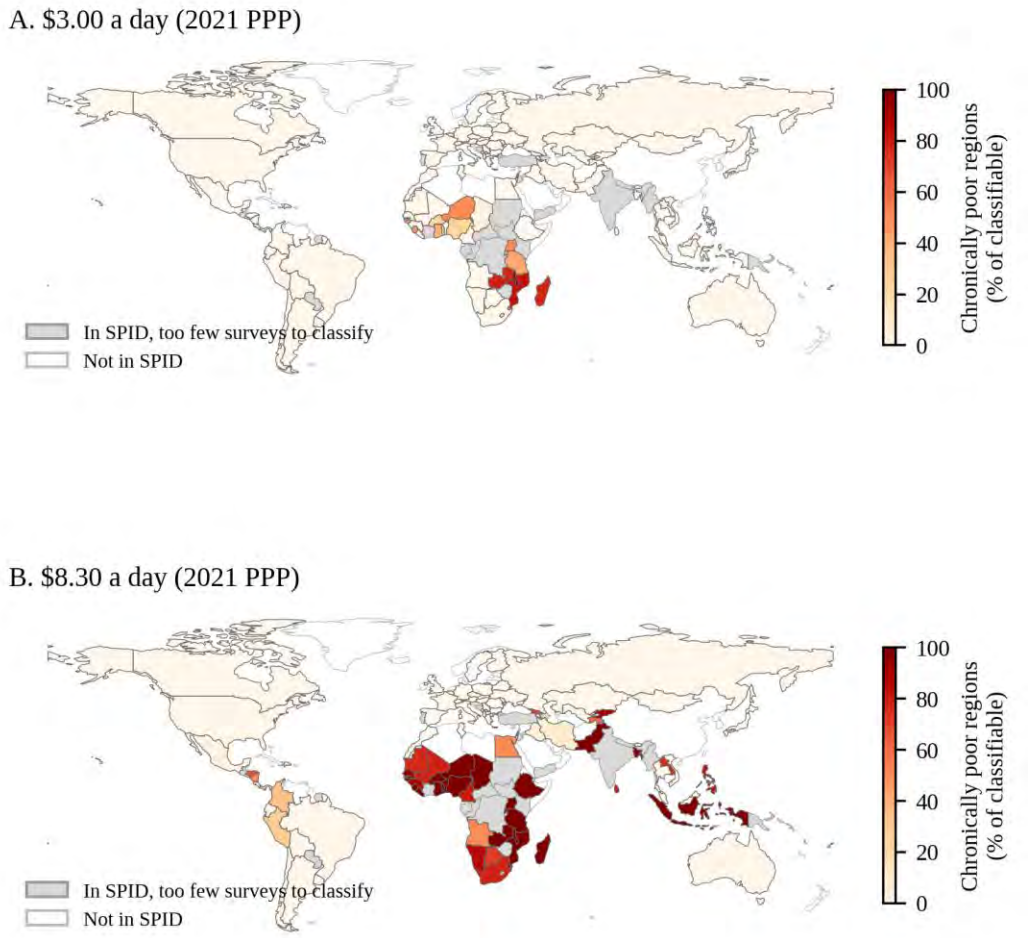
Note: Correlations are between annualized changes in the regional monetary headcount and in the subnational Multidimensional Poverty Measure headcount over the same spell. "Same direction" is the share of spells in which both measures move the same way. Country-weighted figures average within countries before correlating. The developing-economy row keeps spells whose economy is not high-income in the anchored file, retaining regions that the matched sample leaves unclassified; applying the 2010 classification to every economy instead gives 1,035 spells in 63 economies and the same correlations. The correlation and the same-direction share vary little across the first five rows, so the agreement reported in Figure 7 is not an artifact of weighting, scope, comparability, or spell length. The last two rows vary the monetary line: because the multidimensional index embeds a monetary indicator defined at $3.00 a day, agreement is mechanically closest there. Source: authors' calculations using the SPID and the subnational MPM.

Figure A1. Regional poverty dynamics and national income-classification transitions at alternative poverty lines



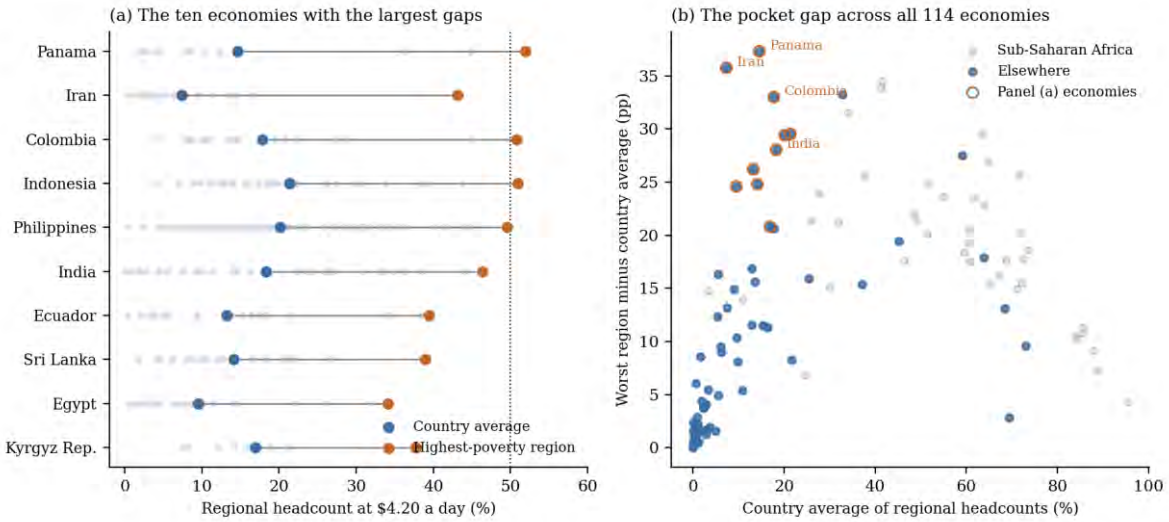
Note: As Figure 2, at the $3.00-a-day line in Panels A and B and the $8.30-a-day line in Panels C and D (2021 PPP). The gap between upgraded and unchanged economies widens as the line rises: the mean within-country share of regions with a falling headcount is 77.2% against 66.9% at $3.00, 84.3% against 66.7% at $4.20 (Figure 2), and 88.0% against 69.5% at $8.30. Source: authors' calculations using the SPID and the World Bank income-classification panel.

Figure A2. Persistence of high regional poverty at alternative poverty lines, 2000–2024



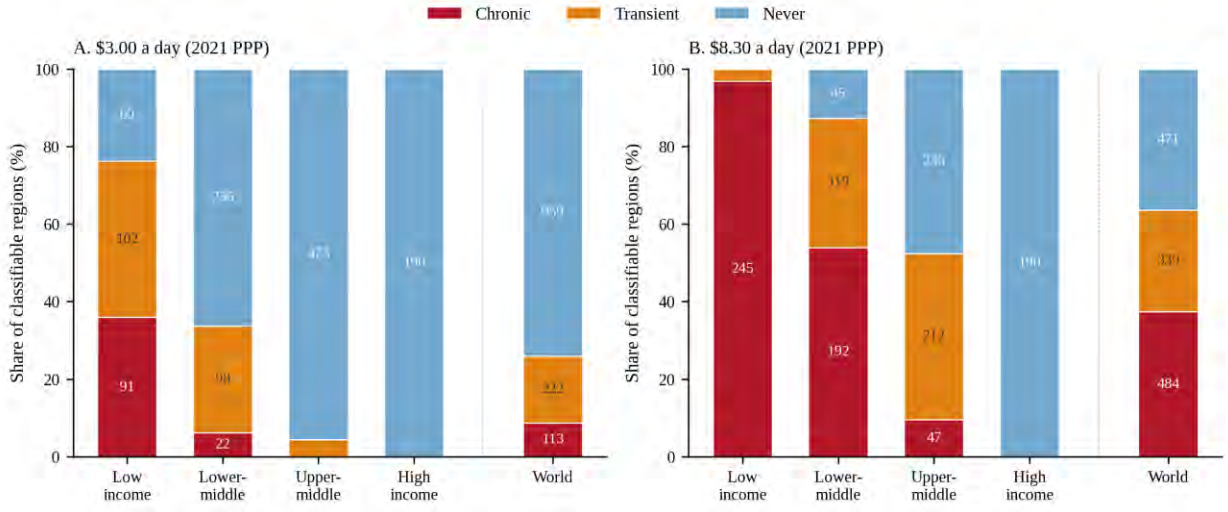
Note: As Figure 3, with high poverty defined as a headcount above 50% at the $3.00-a-day line in Panel A and the $8.30-a-day line in Panel B (2021 PPP). The classifiable sample is the same 1,294 regions in 101 economies throughout. Persistently poor regions are 8.7% of classifiable regions at the $3.00 line, 19.1% at $4.20, and 37.4% at $8.30, in 16, 28, and 53 economies respectively. Source: authors' calculations using the SPID.

Figure A3. The distribution behind the pockets, and the pocket gap across all economies



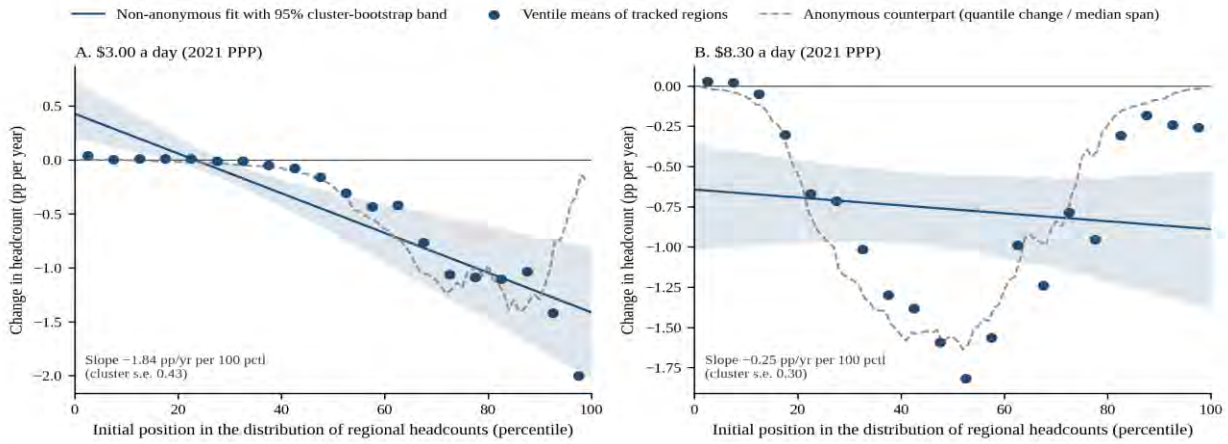
Note: Panel (a) shows every region of the ten economies with the largest pocket gaps outside sub-Saharan Africa and a country average below 30%, ordered by that gap; pale dots are individual regions, and the line runs from the unweighted country average to the highest-poverty region. Panel (b) plots the gap against the country average for all 114 economies with at least five regions in their latest survey, with the panel (a) economies circled. Poverty is the headcount at the $4.20-a-day (2021 PPP) line. Source: authors' calculations using the SPID.

Figure A4. Persistent and episodic poverty of places at alternative poverty lines, by country income group



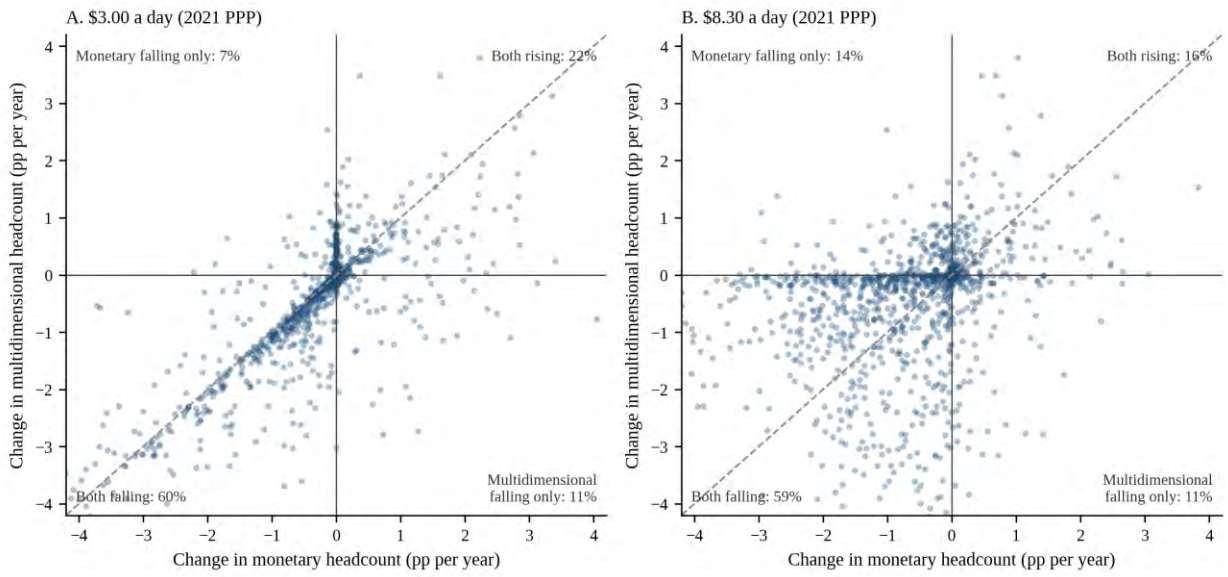
Note: As Figure 5, at the $3.00-a-day line in Panel A and the $8.30-a-day line in Panel B (2021 PPP). The episodic share peaks among low-income regions at the $3.00 line (40.3%), among lower-middle-income regions at $4.20 (39.0%, Figure 5), and among upper-middle-income regions at $8.30 (42.8%): a region is episodically poor only if it straddles the 50% threshold, so the peak moves up the income ladder as the line rises. Source: authors' calculations using the SPID and the World Bank income-classification panel.

Figure A5. Non-anonymous growth incidence of regional poverty reduction at alternative poverty lines



Note: As Figure 6, at the $3.00-a-day line in Panel A and the $8.30-a-day line in Panel B (2021 PPP). The fitted slope falls in absolute size as the line rises: −1.84 pp per year per 100 percentiles at $3.00 (cluster s.e. 0.42), −1.57 at $4.20 (0.35, Figure 6), and −0.25 at $8.30 (0.30), the last indistinguishable from zero. At the $8.30 line the ventile profile is U-shaped rather than monotone: regions in the middle of the initial distribution reduced poverty fastest, while those starting highest were close to the upper bound of the headcount. The uncorrected negative association appears at the lower lines, while Figure 6 illustrates its sensitivity at $4.20. A correction shown only at $4.20 does not establish the corrected result at $3.00. Source: authors' calculations using the SPID.

Figure A6. Monetary versus multidimensional regional poverty dynamics at alternative poverty lines



Note: As Figure 7, with the monetary headcount measured at the $3.00-a-day line in Panel A and the $8.30-a-day line in Panel B (2021 PPP); the multidimensional measure is unchanged across panels. The correlation of annualized changes is 0.83 at $3.00, 0.79 at $4.20 (Figure 7), and 0.28 at $8.30, and the discordant share rises from 15.5% to 19.2% to 24.5%. The remainder in each case shows no change in one of the two measures, which is 6.2% of spells at $3.00, 2.6% at $4.20 and 1.6% at $8.30. The multidimensional index embeds a monetary dimension defined at $3.00, so agreement is mechanically closest in Panel A and the two measures increasingly describe different populations as the monetary line rises. Source: authors' calculations using the SPID and the subnational MPM.