

Testing Human-Time Economics: Did the Engine of Growth Shift After 1973?

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Abstract

Human-Time Economics claims that the engine of advanced-economy growth changed character in the early 1970s: physical coupling stalled and the appropriation of human time carried what remained. This paper subjects that claim, and three others derived from it, to tests designed to fail. Four results follow. First, the break is not assumed but found: Bai–Perron selects a single break in the exergy-coupling margin at 1973 (sup-Wald $p = 0.0002$; 90 per cent confidence interval 1969–1978), the valuation residual switches with it (joint $p = 0.0002$), and no alternative date in 1968–2008 fits better. Second, the framework’s compensation mechanism fails: the appropriation margin exhibits no break at all (sup-Wald $p = 0.29$; Bai–Perron selects zero breaks; OLS-CUSUM 0.96 against a critical value of 1.358), so the engine shifted by the collapse of one margin rather than by the acceleration of another. Third, the shift is nevertheless located where the framework and no rival predicts: waged appropriation is trendless over seventy-five years (0.00002 per decade, $t = 0.04$) and shows no break, so for the wage relation to have carried the observed rise in appropriation, annual waged hours per adult would have had to rise 97 per cent rather than fall. Fourth, in a retrospective horse race over thirty-two economies, the demographic wedge separating temporal leverage from debt-to-output is orthogonal to the debt ratio ($r = 0.27$) and carries information about subsequent debt accumulation that the debt ratio does not ($t = 2.21$, placebo $p = 0.024$, while debt-to-output alone has a rank correlation of -0.03 with its own future path); but the gain does not survive robust estimation or out-of-sample evaluation at this sample size. We also state why the fertility prediction is not tested here rather than testing it badly. The paper is explicit about the one methodological hazard that destroys work of this kind: the six-margin decomposition is an identity, and we demonstrate numerically why it must never be estimated.

Keywords: human time; growth accounting; structural breaks; temporal leverage; debt sustainability; biophysical economics

JEL classification: C22; E01; O47; Q57; J11

1. Introduction

Something happened to advanced-economy growth in the early 1970s, and half a century of research has not settled what. Productivity growth fell and did not recover. Energy per unit of output stopped rising. The literature has offered the oil shocks, the end of Bretton Woods, the exhaustion of the post-war catch-up, a shift in the composition of output toward services, and the mismeasurement of an increasingly intangible product. What these explanations share is a common object: they are accounts of why the same engine ran more slowly. Two companion papers offer a different interpretation. The first suggests that biophysical economics, in the tradition running from Georgescu-Roegen (1971), has counted two low-entropy sources – the mineral stock and the solar flow – and missed a third, the finite span of human waking hours, and makes the accounting that this insight requires (Osmanov, 2026a). The second takes finite human lifetime as the temporal boundary of the economic system, derives an exact six-margin decomposition of output, a solvency condition denominated in hours, and a coupling between the appropriation of time and the reproduction of the fund that supplies it, and reports pilot measurement for the United States (Osmanov, 2026b). Taken together, they make a claim not about the speed of the engine but about which engine is running: that after the early 1970s the growth of output ceased to be carried by the exergy coupled to each appropriated hour and came to be carried by the number of hours the economic system organises and by the value it books per unit of useful work.

This paper is the test. Its purpose is not to illustrate the framework but to give it the best available opportunity to fail, and the design follows from a single methodological commitment: predictions that are stated directionally, tested on a date the theory nominates, in an equation whose left-hand side contains its own right-hand side, are not tests. Each of those three hazards is present in the natural way of doing this exercise, and Sections 2.4, 3 and 5 deal with them in turn.

Three things distinguish the claim from its neighbours, and the reader is entitled to have them stated before the evidence.

Against the mismeasurement literature. Syverson (2017), Byrne, Fernald and Reinsdorf (2016) and Brynjolfsson, Rock and Syverson (2021) argue that the productivity slowdown is partly an artefact of measuring output badly. That claim and this one sound similar and are orthogonal. The mismeasurement literature says the speedometer is misreading; the claim here is that the reading is attached to the wrong shaft. The distinction is empirically consequential rather than rhetorical, because the coupling margin tested in Section 5 is constructed on the exergy side of the accounts and does not use the output deflator at all. A deflator story cannot generate a sign reversal in a series that does not contain a deflator. It can, however, generate the mirror-image movement in the valuation residual, and Section 9 concedes that it may.

Against societal metabolism. The nearest existing accounting is the MuSIASEM programme, which allocates total human activity in hours across societal compartments and pairs each with its exergy throughput (Giampietro, Mayumi and Sorman, 2012; Giampietro et al., 2013), and which has recently argued that the profile of time allocation constitutes an internal biophysical limit to growth (Manfroni et al., 2021). The claim that human time matters biophysically is therefore not available to be made again, and this paper does not make it. Three differences are load-bearing. The first is a denominator: the exosomatic metabolic rate divides throughput by *total* human activity, while the coupling variable here divides it by *appropriated* hours; the ratio of the two is the appropriation rate, and Sections 5 and 6 show that essentially the entire post-war movement lives in it. The second is that societal metabolism treats human time as a fund allocated across compartments, whereas the framework tested here treats finite lifetime as an intertemporally non-transferable boundary reproduced with a two-decade lag — which is what allows a *solvency* condition to be posed at all. We deliberately drop the weaker version of this difference, sometimes stated as non-storability: that hours cannot be banked is true, well known and does no work. The third difference is the empirical payoff, and it is the subject of Section 7: a metabolic accounting produces no financial variable, whereas the lifetime boundary produces a leverage measure that can be raced against debt-to-output. That race is where a framework of this kind either earns a place in applied macroeconomics or does not.

Against the framework itself. The most useful thing an empirical paper can do for a young framework is to find out which of its claims are load-bearing and which are decoration. We report that the framework's central dynamic mechanism — that a system whose physical margin stalls defends its position by widening its claim on human time — fails its timing test cleanly and repeatedly. The appropriation margin did not accelerate after 1973 by any test we can construct, and we do not think this is a power problem: the same tests detect the coupling break at $p = 0.0002$ in the same sample. What did happen is compositional. Appropriation became the dominant margin of American growth not by rising faster but by continuing to rise at all while the physical margin turned negative. That is a weaker claim than the framework makes and a more defensible one, and Section 10 draws the theoretical consequence.

Section 2 restates the accounting core compactly, with a notation table, so that no reader needs the companion papers to follow the tests; it also confronts the identity problem. Section 3 states four refutable predictions and the observations that would refute them. Section 4 describes data and construction. Sections 5 to 8 report the four tests. Section 9 collects falsification and robustness. Sections 10 and 11 discuss and conclude.

2. The framework in compact form

2.1 Objects and notation

Let i index individuals, b_i birth date and $L_i < \infty$ lifetime, and let $w_i(t)$ be daily waking hours. The human-time flow of individual i is the measure $dh_i(t) = w_i(t) \mathbf{1}\{t \in [b_i, b_i + L_i]\} dt$, whose support is compact and translates with the birth date. The *time-fund* of a society is the aggregate of that flow over the living,

$$T_t = N_t \bar{w}_t \cdot 365, \quad (1)$$

with N_t the count of persons above the age of economic participation and $\bar{w}$ mean daily waking hours. The fund is partitioned exhaustively into hours appropriated by the economic system, hours devoted to forming the next cohort, physiologically irreducible hours, and a discretionary residual, with shares α , σ , ψ , δ summing to one. Appropriation is defined functionally rather than contractually: an hour is appropriated when it is organised for the reproduction of the system rather than of the person, so that the waged shift, the commute, the compliance hour, the metered platform session and the consumption work firms have offloaded onto customers all qualify, and the wage boundary is an institutional detail inside α rather than a wall around it.

Table 1. Notation.

Symbol	Object	Unit	Construction
N	adult cohorts	persons	civilian noninstitutional population 16+
$\bar{w}$	waking day	hours per day	convention, $\bar{w} = 16$
T	time-fund	person-hours per year	$N\bar{w} \cdot 365$
H	appropriated hours	person-hours per year	$H^w + H^u$
α	appropriation rate	share	H/T ; α^w waged, α^u unwaged
X	primary exergy	joules per year	energy accounts
κ	time capitalisation	joules per appropriated hour	X/H
ε	conversion efficiency	share	exergy-to-useful-work
λ	valuation	currency per joule of useful work	$Y/(X\varepsilon)$, residual
Y	real output	currency per year	national accounts
D	outstanding claims	currency	debt stock
m	monetisation rate	currency per appropriated hour	Y/H
$\hat{H}$	claimed hours	person-hours	D/m
$\Lambda(\theta)$	temporal leverage	share	eq. (4)
θ	settlement horizon	years	analyst's choice

2.2 The human-time identity

Writing useful work as $U = X\varepsilon$ and $\lambda \equiv Y/U$, chaining definitions gives an exact decomposition of output into six margins,

$$Y_t = \underbrace{N_t}_{\text{cohorts}} \times \underbrace{\bar{w}_t}_{\text{waking day}} \times \underbrace{\alpha_t}_{\text{appropriation}} \times \underbrace{\kappa_t}_{\text{coupling}} \times \underbrace{\varepsilon_t}_{\text{conversion}} \times \underbrace{\lambda_t}_{\text{valuation}}, \quad (2)$$

and in log-differences,

$$g_Y = g_N + g_{\bar{w}} + g_{\alpha} + g_{\kappa} + g_{\varepsilon} + g_{\lambda}. \quad (3)$$

Three of the margins are human-time margins (N , $\bar{w}$, α), two are physical (κ , ε), and one is a valuation residual (λ). Equation (2) is an identity in the sense that a Kaya decomposition (Kaya and Yokobori, 1997) is an identity, and it carries no substitution assumptions and no production function.

2.3 Temporal leverage

Because human time is interpersonally tradeable and intertemporally non-transferable, a claim maturing at a future date must be discharged out of the hours of persons alive at that date. Define claimed hours $\hat{H}_t \equiv D_t/m_t$ with $m_t \equiv Y_t/H_t$, and the *temporal leverage ratio* over a settlement horizon θ ,

$$\Lambda_t(\theta) \equiv \frac{\hat{H}_t}{\sum_{s=1}^{\theta} \bar{\alpha} T_{t+s}} = \frac{D_t/Y_t}{\sum_{s=1}^{\theta} (T_{t+s}/T_t)}. \quad (4)$$

Λ is the share of the next θ years of an economy's entire appropriable human time that outstanding claims already commit; the temporal solvency condition is $\Lambda \leq 1$, and the critical horizon θ^* solves $\Lambda(\theta^*) = 1$. The measure earns its keep through a single observation. If the projected fund is stationary, $T_{t+s} = T_t$ for all $s \leq \theta$, then $\Lambda_t(\theta) = (1/\theta)(D_t/Y_t)$ and θ^* equals the debt ratio expressed in years. Conventional debt sustainability ratios are therefore the stationary-fund special case of a temporal leverage measure, and the entire informational content of Λ relative to D/Y is the projected path of the appropriable base. Under a constant projected growth rate ρ of that base the denominator of (4) is $(1 + \rho)[(1 + \rho)^{\theta} - 1]/\rho$, and we define the *demographic wedge* as $100[\Lambda/\Lambda^{\text{bench}} - 1]$ with $\Lambda^{\text{bench}} = (D/Y)/\theta$. The wedge is a monotone function of ρ alone; the horse race of Section 7 therefore reduces to a well-posed question, namely whether the projected growth of the appropriable base carries information about fiscal outcomes conditional on the debt ratio.

2.4 What an identity can and cannot be asked to do

This subsection is not preliminary throat-clearing. It is the reason the paper takes the form it does, and a reader who skips it will misread everything that follows.

The obvious way to test (2) is to write it as a regression — to put $\ln Y$ on the left, the five measured margins on the right, add a dummy for 1973 and its interactions, estimate the long-run multipliers by an autoregressive distributed lag model with bounds testing, and read the coefficients as elasticities. (Pesaran, Shin & Smith, 2001) That design is fatally circular, and it is worth showing exactly how rather than merely asserting it. Because λ is recovered as $Y/(X\varepsilon)$, the right-hand side of (3) contains the left-hand side by construction. Estimating (3) on our annual United States data returns

$$R^2 = 1.000000, \quad \hat{\beta}_N = \hat{\beta}_{\alpha} = \hat{\beta}_{\kappa} = \hat{\beta}_{\varepsilon} = \hat{\beta}_{\lambda} = 1.000000000000, \quad \max|\hat{u}_t| = 4.5 \times 10^{-15},$$

which is not a finding about the American economy but a report on floating-point arithmetic. Nothing is repaired by dropping the residual margin: regressing g_Y on the four remaining margins yields $R^2 = 0.770$ with $\hat{\beta}_{\alpha} = 1.86$ ($t = 9.8$) and $\hat{\beta}_{\kappa} = 0.27$ ($t = 4.3$), numbers that describe how the omitted term covaries with the included ones and nothing else. The same trap catches correlations: the sample correlation between g_{λ} and g_{κ} is -0.805 , which looks like powerful evidence for a compensation mechanism and is in fact close to pure arithmetic, since X enters κ in the numerator and λ in the denominator. Any movement in exergy that output does not follow produces that correlation with no economics whatever.

We therefore adopt three rules and hold to them throughout.

Rule 1. *The identity is never estimated.* It is used to construct margin series, and the objects submitted to inference are the *properties of those series* — their means within regimes, the dates at which those means

change, their stability — never a regression of output on its own components. Testing whether the mean of g_κ shifted is a legitimate question about a data series; testing whether g_κ “explains” g_Y is not a question at all.

Rule 2. *Inference is confined to objects outside the identity.* The fiscal outcomes of Section 7 (subsequent debt accumulation, subsequent per-capita growth) and the demographic projections that enter Λ are not components of (2), so a regression relating them is a regression and not an accounting rearrangement.

Rule 3. *Every correlation among identity components is presumed mechanical until shown otherwise.* The -0.805 above is reported for exactly this reason, and it is reported as a warning rather than as a result.

Section 4.3 confirms that all series are integrated of order one even when an endogenous break is allowed, which is precisely why the analysis is conducted in log-differences, where the margins are stationary within regimes.

3. Four refutable predictions

The predictions below are derived from propositions already published in the companion papers, which fixes them independently of the estimation reported here. Each is stated with the observation that would refute it.

H1 (regime switch, and its location). *The physical coupling margin g_κ underwent a single structural break, and the data locate that break near the early 1970s without being told where to look.* This is a sharpened form of P1 of Osmanov (2026b). The sharpening matters: a test that imposes 1973 establishes nothing, because a dozen theories predict a break there, and the existence of a break therefore discriminates among none of them. **Refuted if** endogenous break estimation selects no break in g_κ , or selects a date whose confidence interval excludes the early 1970s, or if an alternative candidate date fits materially better.

H2 (compensation). *After the physical margin stalls, the appropriation margin accelerates.* This is the dynamic core of the framework — the mechanism by which a rent-defending system is said to widen the only margin with short-run elasticity — and it is the prediction that distinguishes the framework from a purely descriptive succession story. **Refuted if** g_α exhibits no break, or a break whose date is unrelated to the coupling break, or a break of the wrong sign.

H3 (location of the shift). *Whatever moved the appropriation margin did not move through the wage relation.* This is the discriminating prediction. The oil-shock account, the Bretton Woods account, the catch-up-exhaustion account and the mismeasurement account all have something to say about κ , Y or the deflator; none of them predicts that the share of the adult waking day organised by the economic system rises while the waged share of that day is flat. **Refuted if** the waged appropriation share exhibits a positive trend or a positive break of a magnitude capable of carrying the observed movement in α .

H4 (temporal leverage). *The demographic wedge carries information about subsequent fiscal outcomes that the debt ratio does not.* This is P3 of Osmanov (2026b) in the weakest form in which it is testable on available data, and it is the point at which the framework competes directly with an instrument macroeconomics already uses. **Refuted if** the wedge is orthogonal to subsequent outcomes conditional on D/Y , or if it is significant only in-sample and vanishes under robust estimation and out-of-sample evaluation.

A fifth prediction, on the hours channel in fertility, is discussed in Section 8 and deliberately not tested. We regard stating why as more useful than producing a coefficient we could not defend.

4. Data and construction

4.1 The United States annual series, 1950–2024

The identity requires five independently measured margins plus a residual. Real output is United States gross domestic product in chained 2017 dollars (Bureau of Economic Analysis). N is the civilian noninstitutional population aged sixteen and over. The waking-day convention is $\bar{w} = 16$, held constant, so that $g_{\bar{w}} = 0$ throughout and the identity has five moving margins. Primary exergy is United States total primary energy consumption (Energy Information Administration). Conversion efficiency is the aggregate exergy-to-useful-work series of Ayres and Warr (2009) as interpolated in Osmanov (2026a); the more recent useful-stage estimates of Aramendia et al. (2024) place the post-1970 path on a comparable plateau, and Supplementary Appendix A reports what becomes of every result when efficiency is instead held fixed at its 1950 value. Waged appropriated hours are constructed annually as the product of the employment-to-population ratio for the civilian population aged sixteen and over and average annual hours per worker on a Penn World Table-consistent basis (Feenstra, Inklaar and Timmer, 2015). Unwaged appropriated hours commuting, consumption and compliance work, and metered platform engagement are a calibrated path, not a measurement, and are treated as such throughout. The waged series can be checked against work outside this framework: Ramey and Francis (2009) reconstruct a century of American work and leisure and find market hours per capita close to flat across the post-war decades, which is the same qualitative statement as Section 6.2 derives here from an independent construction. Their series is not an input to ours, so the agreement is a check rather than a calibration.

Levels are benchmarked exactly to the twelve benchmark years of Osmanov (2026b, Table A.1) by proportional (Denton-type) adjustment, so that every reported margin reproduces the companion paper’s published value at 1950, 1960, 1970, 1973, 1980, 1990, 1995, 2000, 2007, 2010, 2019 and 2024, while annual variation between benchmarks comes from the published annual series. Three checks establish that the reconstruction is not circular with the framework it tests. First, the identity reproduces measured output to machine precision at every year. Second, primary exergy implied by the benchmark table matches published national energy totals to within 0.2 per cent for every benchmark year from 1950 to 2000, diverging to between two and four per cent after 2007, a discrepancy consistent with the revision of the convention for non-combustible renewables and reported rather than smoothed. Third, and most usefully, the calibrated unwaged path is *recovered* rather than imposed: subtracting our independently constructed waged series from the companion paper’s published α benchmarks yields unwaged appropriated time rising from 2.19 to 5.01 hours per adult per day, against the 2.0-to-5.0 path stated in Osmanov (2026a) from an entirely separate construction. Appendix A gives the full procedure.

The resulting annual growth contributions reproduce the companion paper’s benchmark decomposition without being fitted to it. Over 1950–2024 we obtain $g_N = 1.27$, $g_{\alpha} = 0.56$, $g_{\kappa} = -0.48$, $g_{\varepsilon} = 0.71$ and $g_{\lambda} = 1.09$ per cent per year against measured output growth of 3.15, and human-time, physical and valuation shares of log growth of 58.1, 7.3 and 34.6 per cent — identical to the published figures at the reported precision. We take this as evidence that the annual reconstruction is a faithful interpolation of the benchmark accounting rather than a new object.

4.2 The temporal leverage panel

Section 7 uses a retrospective cross-section of thirty-two advanced and emerging economies with base year 2000 and settlement horizon $\theta = 24$. Inputs are general government gross debt as a share of output in 2000 and 2024, the realised change in the population aged fifteen to sixty-four between those dates, real output per capita growth over the period, and initial output per capita as a convergence control. Every input is listed in Appendix B so that any figure can be replaced and the exercise re-run; results are reported with influence diagnostics precisely because a cross-section of this size is sensitive to individual

observations. A forward-looking table for thirty economies at 2025 with $\theta = 50$ is constructed on the same formula from published credit ratios (Bank for International Settlements, 2025) and the United Nations medium-variant projection of the working-age population to 2050, and reproduces Osmanov (2026b, Table 4) exactly for the ten economies common to both.

One objection to the retrospective design must be met at the outset, because it is the objection that would otherwise dispose of it. Using the *realised* path of the working-age population to compute leverage in 2000 appears to grant the measure perfect foresight. It does so only marginally, and for a reason internal to the framework. Everyone aged fifteen to sixty-four in 2024 was born by 2009; everyone aged twenty-four and over in that year was already alive in 2000. Roughly four-fifths of the 2024 appropriable base was therefore predetermined at the base date, and the residual uncertainty is concentrated in migration and in a single narrow cohort. This is Principle III of the framework — the fund is reproduced only with a maturation lag — restated as a forecasting property, and it is the reason a leverage measure denominated in future hours is constructible at all. A financial ratio has no comparable guarantee.

4.3 Integration properties

Augmented Dickey–Fuller tests with a constant and trend fail to reject a unit root for $\ln\kappa$ ($p = 0.084$), $\ln\alpha$ (0.232), $\ln\lambda$ (0.856), $\ln Y$ (0.878) and $\ln\alpha^w$ (0.457), and KPSS rejects stationarity for all five. Because an unmodelled break can masquerade as a unit root, (Perron, 1989) we repeat the exercise allowing an endogenously dated break in intercept and trend. Zivot–Andrews statistics (Zivot and Andrews, 1992) of -4.02 , -3.20 , -4.00 , -3.03 and -3.24 leave the unit-root null intact against a five per cent critical value of -5.08 . All series are treated as integrated of order one, and all analysis is conducted in log-differences.

The differenced series carry a further diagnostic worth reporting, because it anticipates the result of Section 5. An augmented Dickey–Fuller test on $\Delta\ln\kappa$ itself fails to reject a unit root ($p = 0.290$); after removing regime-specific means on either side of 1973 the same test rejects decisively ($p < 0.001$). The apparent non-stationarity of the differenced coupling margin *is* the break. The corresponding figures for $\Delta\ln\lambda$ are $p = 0.084$ and $p < 0.001$. For $\Delta\ln\alpha$ the test rejects both before and after regime demeaning ($p < 0.001$ in both cases): no break is needed to make the appropriation margin stationary.

5. Test 1: was there a break, and where?

5.1 Three estimators, one date

We do not impose 1973. Table 2 reports, for each margin, a bootstrapped sup-Wald test of a one-time mean shift with the break date estimated; Bai–Perron global estimation of up to three breaks with fifteen per cent trimming and the number selected by both the Bayesian and the Liu–Wu–Zidek information criteria; and the OLS-CUSUM statistic against its five per cent critical value of 1.358. The three procedures are those of Andrews (1993), Bai and Perron (1998, 2003) and Liu, Wu and Zidek (1997) respectively, and the reporting convention follows Hansen (2001): a date selected by a point estimator is worth little without the interval around it, so every break is accompanied by a bootstrapped confidence interval and, in Table 3, by the fit of the alternatives it was chosen over.

Table 2. Endogenous break tests on the margin growth series, United States, 1951–2024.

Margin	sup- Wald	date	p	Bai–Perron: breaks (BIC / LWZ)	date	90% CI	OLS- CUSUM	reject 5%
$\Delta\ln\kappa$	30.70	1973	0.0002	1 / 1	1973	1969– 1978	2.162	yes
$\Delta\ln\lambda$	32.46	1972	0.0002	1 / 1	1972	1968–	2.177	yes

Margin	sup- Wald	date	p	Bai–Perron: breaks (BIC / LWZ)	date	90% CI	OLS- CUSUM	reject 5%
						1976		
$\Delta \ln \alpha$	4.34	1975	0.287	0 / 0	—	1963– 2009	0.963	no
$\Delta \ln \alpha^w$	2.13	2000	0.687	0 / 0	—	1962– 2012	0.678	no
$\Delta \ln Y$	7.40	2000	0.084	0 / 0	—	1971– 2010	—	—

Note. Throughout the paper a break dated b means that b is the **last** observation of the pre-break regime, the Bai–Perron convention; the sup-Wald and Bai–Perron columns are therefore directly comparable. sup-Wald p -values from 5,000 permutations of the series under exchangeability; confidence intervals from 2,000 residual bootstrap replications; OLS-CUSUM five per cent critical value 1.358.

The coupling margin breaks once, and the estimators agree on when. Bai–Perron selects exactly one break under both information criteria and places it at 1973; the bootstrapped sup-Wald statistic, searching the same grid, selects the same year; and the OLS-CUSUM statistic attains its maximum in 1973 and exceeds its critical value by a wide margin. The valuation residual switches with it, at 1972, and a joint test of the hypothesis that the two margins shift together — which is the framework’s prediction and not a separate finding — returns $p = 0.0002$. The magnitudes are large. The mean contribution of g_κ moves from +1.56 to −1.40 per cent per year, and that of g_λ from −0.59 to +1.84. Before the break, output grew more slowly than the physical margins implied and valuation absorbed the difference as a drag; after it, the sign reverses and valuation becomes the largest single margin of American growth.

The date is not an artefact of the candidate set. Table 3 reports the sum of squared residuals from a mean shift imposed at each of eleven candidate dates spanning the plausible alternatives.

Table 3. Fit of the coupling break at alternative imposed dates (residual sum of squares, $\Delta \ln \kappa$).

Date	1968	1971	1973	1974	1976	1979	1981	1985	1990	2001	2008
RSS	.0369	.0342	.0326	.0350	.0353	.0366	.0403	.0424	.0420	.0426	.0441

The second oil shock (1979), the Volcker disinflation (1981), the information-technology episode (1990, 2001) and the financial crisis (2008) all fit worse, the last of them worst of all. This is the strongest form of H1 available: the framework nominated a date, the estimator was not told it, and the estimator found it.

5.2 What this does and does not establish

It establishes that a single, sharply dated, large regime change in the coupling between human hours and mobilised exergy occurred in the early 1970s, and that the valuation residual changed sign at the same moment. It does not establish that the framework explains why, and it discriminates against no rival, because the over-determination of 1973 is exactly the problem H1 was designed to concede rather than to solve. The oil shocks, the collapse of Bretton Woods and the exhaustion of post-war catch-up all predict a break at that date. Everything that distinguishes this framework from those accounts is therefore carried by Sections 6 and 7, and a reader who stops here has learned that the framework is consistent with a well-known fact.

Figure 1 displays the six margins and the rolling composition of growth. Panel (b) makes the compositional statement that Section 5.1 does not: on a fifteen-year rolling window, the physical margins carried 69 per cent of American log output growth to 1965 and −49 per cent by 2024, crossing zero in

1982, while the human-time margins moved from 40 to 58 per cent and the valuation residual from -10 to 92 per cent.

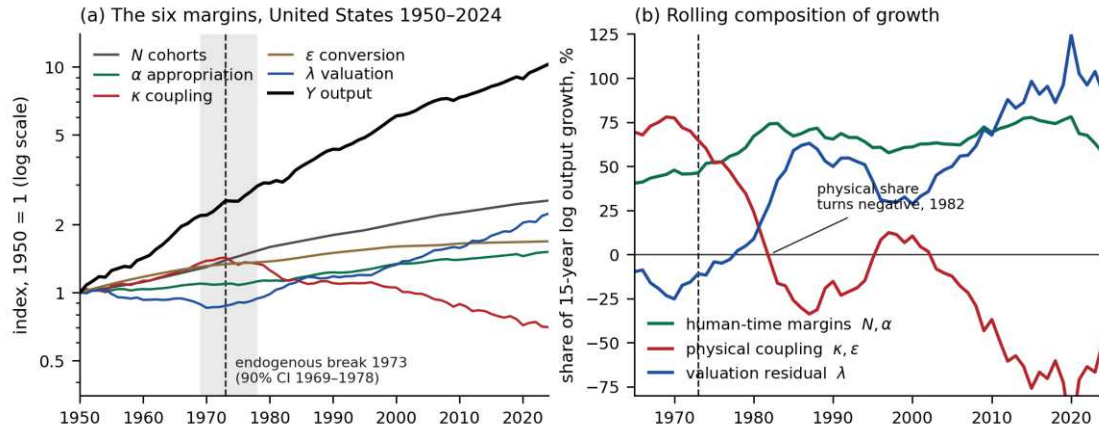


Figure 1. The engine of United States growth, 1950–2024. (a) The six margins of the human-time identity, indexed to 1950, log scale; the vertical line marks the endogenously estimated break in the coupling margin (1973) and the shaded band its ninety per cent confidence interval (1969–1978). (b) Composition of growth on a fifteen-year rolling window: shares of cumulative log output growth attributable to the human-time margins, the physical coupling margins and the valuation residual. Sources: Table A.1 of Osmanov (2026b); BEA; EIA; BLS; author’s calculations.

6. Test 2: compensation, and the discriminating margin

6.1 The compensation mechanism fails

H2 is the framework’s dynamic core: when the net-energy margin squeezes the growth of coupling, a system that behaves as if it defended entropic rent must widen the appropriation rate, because the fund is predetermined and α is the only margin with short-run elasticity. The prediction is that g_α rises at or shortly after the coupling break. It does not.

The mean contribution of the appropriation margin moves from 0.395 per cent per year before 1974 to 0.637 after, in the predicted direction. Every test we can construct says that difference is indistinguishable from noise. The bootstrapped sup-Wald statistic is 4.34 with $p = 0.287$. Bai–Perron selects zero breaks under both information criteria; when forced to one, it places it in 1975, but the ninety per cent confidence interval for that date runs from 1963 to 2009, which is to say the data cannot locate it at all. The OLS-CUSUM statistic reaches 0.963 against a critical value of 1.358. An exact permutation test over all partitions of the twelve benchmark growth observations — the small-sample inference used in the companion paper, reproduced here without interpolation — returns $p = 0.309$, against $p = 0.006$ for the coupling margin and $p = 0.012$ for valuation in the same eleven observations. The failure is not a power failure: the identical procedures detect the coupling break at $p = 0.0002$ in the identical sample.

We regard H2 as refuted in the form in which the framework states it, and Section 10 draws the consequence. The engine did not change because one margin was called upon to compensate for another. It changed because one margin collapsed while another continued at a rate it had maintained for two decades already. Appropriation became dominant by survival rather than by acceleration.

6.2 The discriminating margin

H3 asks where the appropriation that did occur is located, and it is the prediction that no rival account makes. Here the waged and unwaged components must be handled with different evidential weight, and we are explicit about which is which.

The waged component is measured. Over seventy-five years the waged share of the adult waking time-fund moves within the interval 0.173 to 0.202 — its minimum in the pandemic year 2020 and its maximum in 2000, so the range is cyclical rather than trending — with a fitted trend of 0.00002 per decade ($t = 0.04$, HAC standard errors, $p = 0.97$) and no detectable break (sup-Wald 2.13, $p = 0.6877$; OLS-CUSUM 0.678). Waged hours per adult per day were 3.03 in 1950 and 2.90 in 2024. Decomposing the growth of α into the contributions of its two components, the waged component contributes -0.027 percentage points per year over 1950–2024 and -0.004 over 1974–2024: not a small positive contribution but a small negative one.

The counterfactual states the point in a form that does not depend on the calibration at all. For the wage relation alone to have carried the observed rise in appropriation from 0.326 to 0.494, the waged share would have to have reached 0.357 by 2024, which is 5.71 waged hours per adult per day, or 2,086 waged hours per adult per year against an observed 1,058 — a required increase of 97 per cent, against an observed decline. Whatever raised the share of the adult day organised by the American economic system, it did not raise it through employment, and no measurement convention on the unwaged margin changes that, because the waged series does not contain the unwaged margin.

The unwaged component carries no independent evidential weight and we do not pretend otherwise. Its path is a calibration disciplined by time-use evidence for the recent period and by household-production reconstructions for earlier decades, and its apparent breaks in 1966, 1995 and 2010 are properties of the calibration rather than findings. What the calibration yields is a magnitude that is checkable outside this paper: closing the arithmetic requires unwaged appropriated time to have risen from about 2.2 to about 5.0 hours per adult per day over the period, and roughly 0.8 hours per adult per day over 1995–2007 alone. That second figure is recovered here rather than assumed: our independently constructed waged series, subtracted from the published appropriation benchmarks, yields an increase of 0.78 hours over exactly that window, which is the magnitude Osmanov (2026a) identifies as the framework's sharpest single test. Confirming or refuting that path against harmonised time-use archives is the sharpest single test the framework currently offers, and it is not a test this paper performs.

The honest statement of H3 is therefore conditional and still informative: the rise in appropriation cannot have occurred inside the wage relation, and the framework's account of where it did occur is a hypothesis with a stated, falsifiable magnitude.

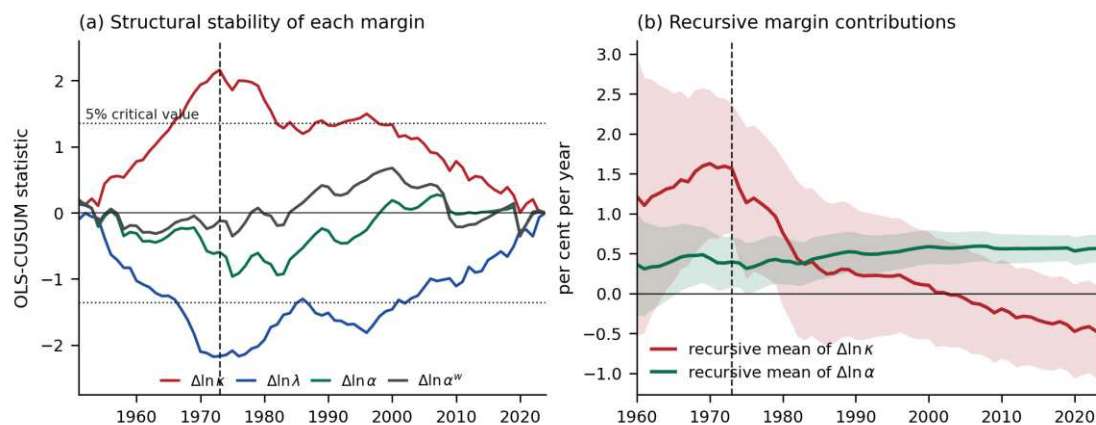


Figure 2. Break diagnostics. (a) OLS-CUSUM statistics for the growth of each margin against the five per cent critical value of 1.358; the coupling and valuation margins breach it and attain their maxima in 1973 and 1972, while total and waged appropriation do not breach it at all. (b) Recursive means of the coupling and appropriation contributions with ninety-five per cent bands.

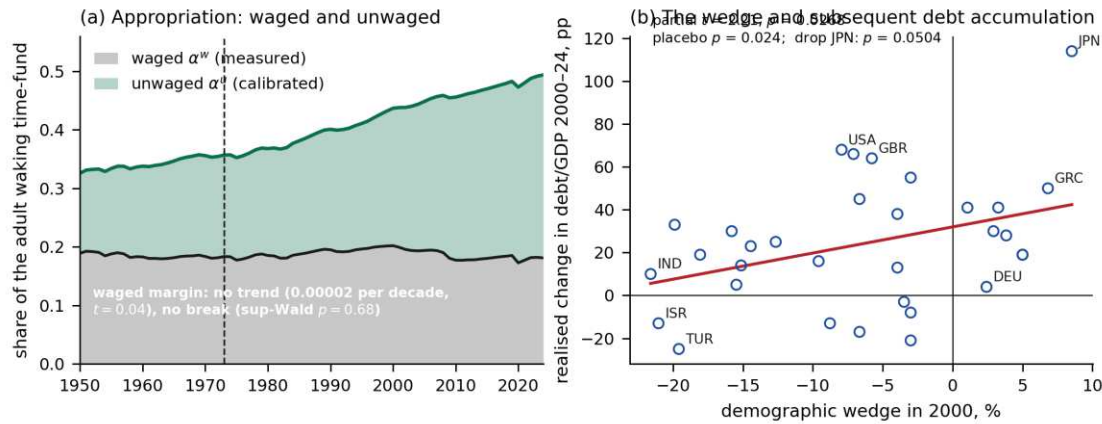


Figure 3. The discriminating margin and the temporal wedge. (a) The appropriation rate decomposed into its measured waged component and its calibrated unwaged component. (b) The demographic wedge in 2000 against the realised change in the debt ratio over 2000–2024, with the partial regression line from the encompassing specification of Table 4, Panel A.

7. Test 3: temporal leverage against debt-to-output

This is the test in which the framework competes with an instrument macroeconomics already uses, and it is the reason a metabolic accounting of human time and a *temporal* accounting of human time are different research programmes rather than the same one under two names. Societal metabolism produces no financial variable. The lifetime boundary produces one, and it either adds something to D/Y or it does not.

7.1 Is Λ a different measure or a relabelling?

The first question is whether temporal leverage is anything other than the debt ratio wearing a hat. By Proposition A of the companion paper it reduces exactly to $(D/Y)/\theta$ when the appropriable base is stationary, so the entire difference is the demographic wedge, and the wedge could in principle be a monotone function of the debt ratio itself — rich, indebted countries also being old ones.

It is not. Across the thirty-two economies of the retrospective panel, the correlation between the debt ratio in 2000 and the demographic wedge is 0.267 ($p = 0.139$): statistically indistinguishable from zero and economically small. The wedge is a second, largely orthogonal dimension of information, which is the necessary condition for the horse race to be worth running. The consequence for country orderings is real but bounded: the Spearman correlation between the D/Y ordering and the Λ ordering is 0.98, Kendall's τ is 0.90, and twenty-four of the four hundred and ninety-six country pairs are reordered. On the forward-looking 2025 table with a fifty-year horizon the reordering is larger — Italy moves seven places up the ranking relative to its debt ratio, the United States five places down, Germany four and a half up, Korea four — and the wedge ranges from +37 per cent for Japan to –50 per cent for Nigeria. Temporal leverage is thus not a relabelling, but neither is it a wholesale re-ranking; it is a correction whose size is the projected demography.

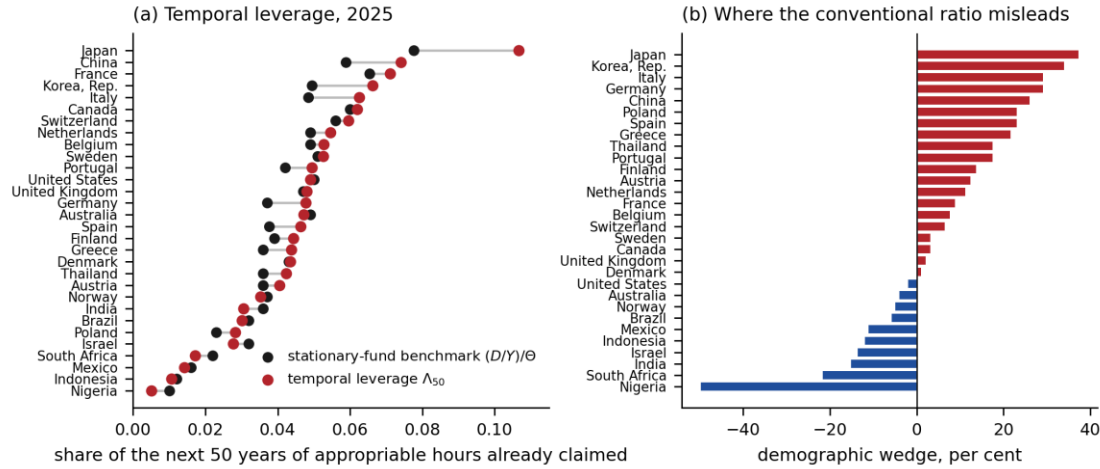


Figure 4. Temporal leverage at 2025, thirty economies, fifty-year settlement horizon. (a) Λ_{50} against the stationary-fund benchmark $(D/Y)/\theta$ of Proposition A; the distance between the two markers is the demographic wedge. (b) The wedge, ordered. Sources: BIS total credit statistics; United Nations World Population Prospects 2024, medium variant; author’s calculations. The ten economies common to Osmanov (2026b, Table 4) are reproduced exactly.

7.2 The horse race

We ask whether the wedge measured in 2000 carries information about outcomes realised over 2000–2024 that the debt ratio in 2000 does not. Two outcomes are used: the realised change in the debt ratio over the period, which is the direct measure of subsequent fiscal deterioration, and realised real output growth per capita, with initial output per capita as a convergence control. Three specifications are compared — the debt ratio alone, temporal leverage alone, and the encompassing specification containing the debt ratio and the wedge — with heteroskedasticity-robust standard errors, leave-one-out cross-validated root mean squared error, and a Diebold–Mariano comparison of the out-of-sample errors.

Table 4. Horse race: temporal leverage against debt-to-output, thirty-two economies, base year 2000.

Panel A — outcome: realised change in D/Y , 2000–2024, percentage points

Regressor	(A) D/Y	(B) Λ	(C) D/Y + wedge
D/Y_{2000}	0.185 (0.48)	—	0.082 (0.25)
Λ_{2000}	—	638.6 (0.70)	—
demographic wedge	—	—	1.221 (2.21), $p = 0.027$
R^2	0.030	0.069	0.149
leave-one-out RMSE	33.47	33.05	31.76
Diebold–Mariano vs (A)	—	0.67 [0.51]	1.14 [0.26]

Panel B — outcome: real output growth per capita, 2000–2024, per cent per year

Regressor	(A) D/Y	(B) Λ	(C) D/Y + wedge
D/Y_{2000}	−0.018 (−2.43)	—	−0.018 (−2.19)

Regressor	(A) D/Y	(B) Δ	(C) D/Y + wedge
Δ_{2000}	—	−40.2 (−2.48)	—
demographic wedge	—	—	0.005 (0.18), $p = 0.858$
R^2	0.474	0.473	0.475
leave-one-out RMSE	1.318	1.316	1.363
Diebold–Mariano vs (A)	—	0.08 [0.94]	−2.66 [0.012]

Note. Heteroskedasticity-consistent (HC3) t -statistics in parentheses; Diebold–Mariano statistics with p -values in brackets, positive values favouring the alternative specification over (A). All Panel B specifications include initial output per capita as a convergence control. $n = 32$.

Read the debt panel first. The most striking number in it is not ours: **the debt ratio in 2000 has essentially no predictive content for its own subsequent path**, with $R^2 = 0.030$, a t -statistic of 0.48, and a rank correlation with the realised change of -0.034 ($p = 0.85$). Whatever else is true, the conventional sustainability metric did not, over this quarter-century, forecast fiscal deterioration.

Against that baseline the demographic wedge does carry information. In the encompassing specification its coefficient is 1.22 with $t = 2.21$ and $p = 0.027$; an F -test of its exclusion gives $p = 0.035$; the fit rises from $R^2 = 0.030$ to 0.149; and a permutation placebo that reassigns the wedge across countries five thousand times returns $p = 0.024$, so the coefficient is not an artefact of the small sample's degrees of freedom. The sign is the predicted one: economies whose appropriable base was projected to shrink accumulated more debt over the following quarter-century, conditional on where they started. The rank-based version of the same statement is robust to functional form: the partial Spearman correlation between the wedge and subsequent debt accumulation, controlling for the debt ratio, is 0.327 ($p = 0.068$).

Now the qualifications, which are substantial and which we do not bury. Japan is enormously influential, with a Cook's distance of 1.24 against a maximum of 0.10 for every other economy in the panel. Dropping Japan moves the wedge coefficient to $t = 1.96$, $p = 0.050$; dropping Greece moves it to $p = 0.077$; dropping Japan, Italy and Greece *together* collapses it entirely ($t = 1.25$, $p = 0.212$). The result therefore survives the removal of any single observation, and of any pair — across all 496 two-country deletions the median t -statistic is 2.14 and 98 per cent exceed 1.645 — but not the simultaneous removal of the three ageing, highly indebted economies in which the mechanism is supposed to be concentrated. That is either the expected signature of a real effect located where the theory says it should be, or the statement that the effect *is* those three countries, and a cross-section of thirty-two cannot distinguish the two readings. Supplementary Appendix B reports the full diagnostics and every estimator-by-subsample combination. Under Huber M-estimation the coefficient falls to $t = 1.54$ ($p = 0.124$), and under median regression to $t = 0.89$ ($p = 0.383$): the in-sample significance depends materially on the influence of the extreme observations, which is another way of saying that the evidence for the wedge is largely the evidence of Japan, Greece and Italy on one side and India, Israel and Turkey on the other. Out of sample the picture is weaker still. Leave-one-out root mean squared error falls from 33.47 to 31.76 percentage points when the wedge is added, a five per cent improvement whose Diebold–Mariano statistic is 1.14 with $p = 0.264$. Restricting the sample to twenty-six advanced economies leaves the coefficient at $p = 0.055$.

The growth panel is a clean negative. Temporal leverage and the debt ratio perform identically (R^2 of 0.473 and 0.474), the wedge adds nothing in-sample ($p = 0.858$), and adding it makes out-of-sample forecasts significantly *worse* ($p = 0.012$). Whatever the demographic wedge is telling us about, it is not

telling us about growth in this window, and we report it because the framework's Proposition P3 does not distinguish between fiscal and real outcomes and should.

7.3 Verdict on H4

The framework claims that Λ dominates D/Y . That claim is not established. What is established is narrower and still worth having: the demographic wedge is orthogonal to the debt ratio, so temporal leverage is a genuinely distinct measure; it carries in-sample information about subsequent fiscal deterioration that the debt ratio does not, over a quarter-century in which the debt ratio carried none; and that information does not survive robust estimation or out-of-sample evaluation at a sample size of thirty-two. H4 is supported as an information claim and unsupported as a forecasting claim. A proper test requires a panel with multiple base years, instrument-level maturity profiles, and fiscal-stress outcomes measured independently of the debt ratios that define the regressor, and we regard constructing it as the first item on the agenda this paper leaves behind.

In the framework's own units the American position is as follows. At a monetisation rate of about \$39 per appropriated hour and total credit to the non-financial sector of 250 per cent of output, claimed hours stand at 1,930 billion, which is 2.50 years of the country's entire appropriated time-fund, or 7,215 hours per adult — about 1.24 waking-years of every adult American life already committed to the settlement of existing claims. Under a stationary fund that number is arithmetically identical to the debt ratio expressed in years, which is the point of Proposition A. What the framework adds is that the American fund is projected to grow while the Japanese, Korean, German and Italian funds are projected to contract by a fifth to a quarter, so that the same ratio denotes materially different obligations.

8. The reproduction constraint: why it is not tested here

The framework's most distinctive prediction concerns fertility: that conditional on income and on the monetary price of children, births track the residual discretionary hours of the sub-fund from which the biologically non-substitutable component of fund formation must be drawn. The arithmetic in the companion paper is arresting — the American replacement deficit is of the order of eleven minutes per adult per day, and the appropriation rate of the sub-fund of women aged twenty to thirty-nine exceeds the national rate — and the temptation to run a panel regression of period fertility on measured discretionary hours is considerable.

The appropriation rate of the fertile sub-fund is conceptually close to the opportunity cost of time in the tradition descending from Becker (1965), and a negative panel association between hours claimed and births would be consistent with both frameworks and would discriminate between neither. Producing that coefficient would not advance the framework; it would attach a new label to an old finding and would weaken the credibility of the tests that do discriminate.

What distinguishes the two accounts is not the sign of the association but the unit of the constraint. In the price-based account the binding object is the household's budget, so an instrument that lowers the money price of children relaxes it. In the framework tested here the binding object is a segment-specific hours constraint that money does not enter, so an instrument that returns hours should outperform an instrument that returns money at equal fiscal cost. That is a genuinely discriminating prediction and it requires a research design this paper does not have: a classification of pronatalist interventions by whether the instrument returns hours to the sub-fund — leave, working-time reduction, on-site care, schedule control — or lowers the money price, with effect sizes normalised by cost, and exogenous variation from staggered national changes in statutory working time and school-day length. The Nordic evidence makes the design urgent rather than optional: Finland combines universal subsidised childcare, extended paid leave and low public debt, a configuration in which price-based models predict fertility should be supported, and its total fertility rate fell from 1.87 in 2010 to 1.35 in 2019, with the economic-uncertainty

literature accounting for roughly a quarter of the decline (Hellstrand, Nisén and Myrskylä, 2021; Comolli et al., 2021; Nisén et al., 2024). The residual is what the hours channel would have to explain. Explaining it is a paper, and it is not this one.

9. Falsification and robustness

9.1 Scorecard

Table 5. Predictions, tests and outcomes.

	Prediction	Decisive statistic	Outcome
H1	Endogenous break in g_κ near the early 1970s	BP selects 1 break at 1973; sup-Wald $p = 0.0002$; CUSUM 2.16; CI 1969–1978; best-fitting date among eleven candidates	Supported
H1b	κ and λ switch jointly	joint permutation $p = 0.0002$	Supported
H2	g_α accelerates after the break	sup-Wald $p = 0.287$; BP selects 0 breaks; CUSUM 0.96; benchmark permutation $p = 0.309$	Refuted
H3	The shift is not located in the wage relation	α^w trend 0.00002 per decade ($t = 0.04$); no break ($p = 0.687$); required waged increase 97 per cent against an observed decline	Supported (waged side measured; unwaged side calibrated)
H4	The wedge beats D/Y	orthogonal ($r = 0.27$); in-sample $t = 2.21$, placebo $p = 0.024$; Huber $p = 0.124$; DM $p = 0.264$; growth outcome negative	Partially supported

Two of five survive intact, one is refuted, one is supported on its measured component only, and one is supported as an information claim and not as a forecasting claim. We take this to be the appropriate yield of a first empirical confrontation, and we would have been suspicious of a higher one.

9.2 Sensitivity

The largest single measurement uncertainty in the framework is the calibrated unwaged path, at ± 40 per cent. Rebuilding every series across that band leaves the coupling break at 1973 with a bootstrapped p -value of at most 0.0004 in all three branches, with Bai–Perron selecting exactly one break under both criteria in each, and leaves the appropriation margin without a significant break in any branch ($p = 0.52$, 0.28 and 0.17 at the low, central and high calibrations, with zero breaks selected throughout). The human-time share of log growth ranges from 53.5 to 61.2 per cent and the physical share from 4.3 to 12.0 per cent across the band, so the two never approach each other.

Two conventions turn out to be exactly rather than approximately neutral, and it is worth stating why. Appropriated hours are $H = \alpha T = [\alpha^w \bar{w} + u] N \cdot 365$ once the waged share and the unwaged path are expressed on the same day, so the waking-day convention cancels from H and therefore from κ identically: every result in Sections 5 and 6 is invariant to $\bar{w}$ by construction, not by robustness. Similarly, a proportional error in the level of primary exergy shifts $\ln \kappa$ by a constant and leaves every log-difference unchanged, so only errors in the *shape* of the exergy series can matter; a linear tilt of ± 2 per cent cumulated over the sample leaves the break at 1973 with $p = 0.0002$.

Supplementary Appendix A extends this exercise to eleven perturbations, including three alternative *shapes* for the unwaged path with the endpoints held fixed, a ± 3 per cent error in measured waged hours, and a counterfactual in which conversion efficiency is held at its 1950 value. The coupling break is selected at 1973 in every one of the eleven, with Bai–Perron choosing exactly one break and the OLS-CUSUM statistic rejecting throughout; the weakest branch is a front-loaded unwaged path, where the sup-

Wald p -value rises to 0.014. The waged appropriation series is invariant across all eleven. The appropriation margin shows no break in nine of the eleven; in the two branches that impose a strong curvature on the unwaged path, a break appears — at 1961 under a concave path and at 1975 under a convex one, that is, at whatever date the imposed curve bends. We read that as confirming rather than qualifying the position taken in Section 6.2: the timing of the unwaged margin is not identified by the data, and the refutation of H2 therefore rests on the central calibration and on every perturbation that does not re-specify the shape of a series the framework has not yet measured.

Three further sensitivities are reported rather than resolved. The exergy series diverges from published national energy totals by two to four per cent after 2007, which affects the level of κ in recent years and not the location of a break four decades earlier. Simultaneous activity — attention sold while other work is performed — means the unwaged component of appropriated hours is not additively clean, and pushes toward a higher α and a lower κ , reinforcing the reported pattern rather than weakening it. Trade in embodied human time is not corrected for here; the companion paper finds that territorial temporal leverage overstates every high-income economy by between fifteen and thirty-two per cent, and since the correction is largest for surplus economies whose demographic wedge is also largest, it works against the finding of Section 7.2 and should be applied before that finding is relied on.

9.3 What would refute the framework outright

We state this in advance of the work that would do it. The framework would not survive a harmonised time-use reconstruction showing that unwaged appropriated time in the United States rose by materially less than the two and a half hours per adult per day that the accounting requires between 1950 and 2024, because the waged margin is measured, flat, and cannot supply the difference; the appropriation account would then simply be wrong. It would not survive a demonstration that the demographic wedge is uninformative about fiscal outcomes in a properly powered panel with independently measured stress outcomes, because temporal leverage is the framework's only competitive empirical instrument. And it would not survive a finding that instruments returning hours and instruments returning money have indistinguishable effects on fertility at equal cost, because the non-substitutability of the reproduction sub-fund is what makes the framework something other than a relabelling of the time-allocation tradition. None of these is a remote contingency; all three are ordinary empirical projects, and we regard the framework's exposure to them as its principal recommendation.

10. Discussion

10.1 Succession, not compensation

The clearest theoretical result of this paper is negative, and it revises the framework. The companion papers derive the movement of appropriation from a rent-defence identity: with the fund predetermined and the coupling margin squeezed, a system that behaves as if it defended entropic rent must widen the only elastic margin. That is a compensation mechanism, and compensation mechanisms have testable timing. The timing is absent. Appropriation grew at 0.40 per cent a year before the coupling break and 0.64 after, and no procedure we can construct distinguishes those numbers, while the same procedures distinguish the coupling break at $p = 0.0002$.

The mechanism should therefore be restated. What the data support is *succession*: a margin that had been rising steadily since at least 1950 became the dominant carrier of growth because the margin that had previously carried it turned negative. The physical share of American log output growth fell from 69 per cent to -49 per cent while the human-time share rose from 40 to 58, and almost all of that reordering is the numerator of the first ratio collapsing rather than the numerator of the second accelerating. This is a weaker claim and it has three advantages. It does not require a selection argument about institutional arrangements defending rent. It is consistent with the appropriation margin having its own long-run

drivers — the diffusion of metering technologies, the extension of consumption work, the growth of the commute — that owe nothing to the energy transition. And it leaves room for an independent physical account of why the coupling margin turned when it did: the literature on the declining energy return on energy invested, and on the minimum return a society requires (Hall, Lambert and Balogh, 2014; Fizaine and Court, 2016), describes a squeeze on exactly the quantity in the numerator of κ , arrived at from the energy side rather than the time side. We do not test that account here, but nothing in our result competes with it, and a joint test would be a natural next step. And it survives the observation that would have refuted the stronger claim.

The “wrong engine” argument survives the revision, because that argument was never about acceleration. It is a claim about which margin is load-bearing, and the composition evidence is unambiguous: for four decades the physical margin has subtracted from American growth while the human-time margins have added to it, and a discipline that reads the productivity slowdown off a capital-and-labour decomposition is reading a gauge attached to the margin that stopped mattering. That is compatible with, and independent of, the mismeasurement literature’s claim that the output side is also badly measured. Both can be true. Only one of them is about the mechanism.

10.2 What temporal leverage is for

Section 7 gives a mixed verdict and a usable instrument. The usable part is this. The debt ratio is not a measure of leverage; it is a measure of leverage evaluated under the assumption that the appropriable base is stationary, and that assumption is now false, in the same direction, for most of the countries to which the ratio is applied. Reporting $\Lambda(\theta)$ alongside D/Y costs nothing — the additional input is a demographic projection that every finance ministry already possesses — and it makes the assumption visible. Where the wedge is small, as in the United States, the United Kingdom and Denmark, the conventional ratio is doing what it is thought to do. Where it is large, as in Japan, Korea, Italy and Germany, two economies with the same debt ratio are not equally levered, and the difference is between a fifth and a third.

The policy content of the framework, as tested here, is correspondingly modest and specific: a second sustainability statistic with a stated construction, not a new fiscal rule. We would resist the stronger reading, because Section 7.2 does not support it.

10.3 Limits

The empirical base is one economy in depth and thirty-two in cross-section, and the deep case is the United States, where the useful-work backbone exists. Extending the identity to eight or ten economies at the standard applied here is a substantial construction project. It requires exergy-to-useful-work reconstructions and time-use archives on a common denominator and it is the out-of-sample test that H1 and H3 most need, because a break found in one country at a date that country’s own oil shock also selects is a weaker fact than a break found in eight. The cross-section of Section 7 is a cross-section and not a panel, its outcome variables are dominated by two global shocks, and its verdict on the wedge rests materially on a handful of observations. A further limit is territorial rather than statistical. The appropriation rate measured here counts hours claimed *within* the United States, while a high-income economy also commands hours embodied in what it imports; the literature on ecologically unequal exchange, and its recent quantification of the net labour transfer from the global South (Hornborg, 1998; Hickel, Hanbury Lemos and Barbour, 2024), implies that a national appropriation rate understates the time-fund actually mobilised by a rich economy, and understates it by a share that has itself changed since 1973. Whether that omission dampens or amplifies the succession result depends on the time-path of embodied hours, which our construction cannot recover, and we flag it as the most consequential unmeasured margin in the accounting rather than as a caveat. Finally, the framework’s most distinctive prediction is untested here by choice, and until it is tested the framework’s claim to explain fertility alongside growth and solvency remains a claim.

11. Conclusion

We have endeavoured to test whether the engine of growth shifted after 1973 in the sense that Human-Time Economics demands, and to give the framework as good a chance as possible of failing. It has: a single structural break in the coupling between appropriated human hours and mobilised exergy is identified by three independent estimators at 1973 without the date being imposed, with a confidence interval of 1969–1978, and with a better fit than any of ten alternative candidate dates including 1979, 1981 and 2008. The valuation residual changed sign at the same moment. The physical margins have subtracted from American growth for four decades while the human-time margins have added to it. The framework's story fares less well. Its compensation mechanism, which posits an acceleration of appropriation where coupling stalls, is rejected: the appropriation margin exhibits no break whatsoever. What replaces it is a succession account, according to which appropriation became dominant by continuing rather than by accelerating. The framework's discriminating prediction – that the rise in appropriation is located outside the wage relation – holds on the side of the accounts that is measured rather than calibrated, and holds strongly: waged appropriation is trendless over seventy-five years, and carrying the observed movement through employment would have required a doubling of waged hours per adult that did not remotely occur. And the framework's competitive instrument, temporal leverage, is a genuinely distinct measure that carries information the debt ratio does not, in a quarter-century in which the debt ratio carried none, but not enough information to win a forecasting contest at this sample size. Two of the three companion papers claimed that finite human time is a distinct analytical object and that taking it as such gives an accounting, a solvency condition and a reproduction constraint. This one reports that the accounting locates a real and sharply dated regime change, that the solvency condition is informative but not yet decisive, that the dynamic mechanism as stated is wrong, and that the reproduction constraint remains to be tested properly. A framework that emerged from its first empirical confrontation with all of its claims intact would have been telling us something about the design of the test rather than about the world.

CRedit Authorship Contribution Statement: Tural Osmanov: Conceptualization, Methodology, Formal analysis, Writing – original draft, Writing – review & editing.

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Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author used Anthropic Claude as an assistive tool for structural organisation of the manuscript and for preparing the Supplementary Appendix. After using this tool the author reviewed and edited the content as needed and takes full responsibility for the content of the publication.

Data Availability

All data and code are openly available in Mendeley Data at doi: 10.17632/w9x35ssf6j.1 . The deposit includes the reconstructed annual series, the cross-country panels, estimation code in Python with independent R, Stata, MATLAB and EViews implementations of the structural-break tests, all figures, and a README mapping every table and figure to the code that produces it. (<https://data.mendeley.com/datasets/w9x35ssf6j/1>)

Declaration of interest

The author declares that he has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Construction of the United States annual series

The annual dataset is built in four steps and is fully reproducible from the accompanying code.

Step 1: benchmark levels. The twelve benchmark years of Osmanov (2026b, Table A.1) supply N , α , κ , ε , λ and Y . Primary exergy at each benchmark is derived as $X = \kappa \alpha N \bar{w} \cdot 365$ and used as the anchor for the annual energy series.

Step 2: annual shapes. Real output is cumulated from published annual growth rates of GDP in chained 2017 dollars; primary exergy from published annual total primary energy consumption; the employment-to-population ratio and average annual hours per worker from published annual series. The adult population and the conversion efficiency are interpolated between benchmarks, the first log-linearly and the second by a shape-preserving monotone cubic, both being smooth by nature.

Step 3: benchmarking. Each annual shape series is multiplicatively adjusted so that it equals the benchmark level at every benchmark year, with the adjustment factor interpolated log-linearly between anchors. This is a Denton-type proportional benchmarking and it preserves the annual growth profile of the published series between anchors while imposing the companion paper's levels at them.

Step 4: derived margins. Waged appropriation is $\alpha_t^w = e_t a_t / (\bar{w} \cdot 365)$ with e the employment-to-population ratio and a average annual hours per worker. Unwaged appropriated time per adult per day is recovered as $u_t = \bar{w}(\alpha_t - \alpha_t^w)$ at benchmark years and interpolated by monotone cubic, so that $\alpha_t = \alpha_t^w + u_t / \bar{w}$ holds annually and reproduces the published benchmarks exactly. Appropriated hours are $H = \alpha T$; $\kappa = X/H$; and $\lambda = Y/(X\varepsilon)$.

Validation. The identity reproduces measured output to machine precision at every year ($\max|Y/\hat{Y} - 1| < 10^{-14}$). Derived primary exergy matches published national totals to within 0.2 per cent at every benchmark from 1950 to 2000. The recovered unwaged path runs 2.19 to 5.01 hours per adult per day against the independently stated 2.0-to-5.0 calibration of Osmanov (2026a). Annual growth contributions reproduce the published benchmark decomposition for 1950–1973, 1974–2000, 2001–2024 and 1950–2024 at the precision reported in that paper.

Caveat. The decomposition of g_α into waged and unwaged contributions in Section 6.2 is a first-order log approximation; the exact identity is in levels, $\alpha = \alpha^w + \alpha^u$, and the approximation error never exceeds 0.08 percentage points per year.

Appendix B. Inputs to the temporal leverage panel

Table B.1. Retrospective panel, base year 2000, $\theta = 24$. D/Y is general government gross debt as a percentage of output; ΔWA is the realised change in the population aged 15–64, 2000–2024; g is average annual real output growth per capita, 2000–2024.

Economy	D/Y_{2000}	D/Y_{2024}	$\Delta D/Y$	ΔWA , %	Λ_{24}	benchmark	wedge, %	g , %
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Economy	D/Y_{2000}	D/Y_{2024}	$\Delta D/Y$	$\Delta WA, \%$	Λ_{24}	benchmark	wedge, %	$g, \%$
Japan	137	251	114	-14.7	0.062	0.057	+8.5	0.7
Greece	104	154	50	-12.0	0.046	0.043	+6.8	0.1
Hungary	55	74	19	-9.0	0.024	0.023	+5.0	2.7
Poland	36	55	19	-9.0	0.016	0.015	+5.0	3.9
Czechia	17	45	28	-7.0	0.007	0.007	+3.8	2.7
Portugal	54	95	41	-6.0	0.023	0.022	+3.3	0.8
Italy	105	135	30	-5.4	0.045	0.044	+2.9	0.0
Germany	59	63	4	-4.5	0.025	0.025	+2.4	1.0
Finland	42	83	41	-2.0	0.018	0.018	+1.1	1.0
Netherlands	52	44	-8	6.0	0.021	0.022	-3.0	1.0
France	58	113	55	6.0	0.023	0.024	-3.0	0.7
Denmark	52	31	-21	6.0	0.021	0.022	-3.0	1.1
Belgium	108	105	-3	7.0	0.043	0.045	-3.5	0.9
Korea, Rep.	17	55	38	8.0	0.007	0.007	-4.0	3.1
Austria	66	79	13	8.0	0.026	0.028	-4.0	0.9
United Kingdom	37	101	64	12.0	0.014	0.015	-5.8	1.0
Spain	57	102	45	14.0	0.022	0.024	-6.7	0.8
Sweden	51	34	-17	14.0	0.020	0.021	-6.7	1.4
China	22	88	66	15.0	0.008	0.009	-7.1	7.9
United States	53	121	68	17.0	0.020	0.022	-7.9	1.4
Switzerland	50	37	-13	19.0	0.019	0.021	-8.8	1.0
Norway	27	43	16	21.0	0.010	0.011	-9.6	0.9
Canada	80	105	25	29.0	0.029	0.033	-12.7	0.9
Brazil	65	88	23	34.0	0.023	0.027	-14.4	1.2
New Zealand	32	46	14	36.0	0.011	0.013	-15.1	1.2
Ireland	36	41	5	37.0	0.013	0.015	-15.5	3.4
Australia	20	50	30	38.0	0.007	0.008	-15.8	1.3
Mexico	39	58	19	45.0	0.013	0.016	-18.1	0.7
Türkiye	51	26	-25	50.0	0.017	0.021	-19.6	3.3
South Africa	42	75	33	51.0	0.014	0.018	-19.9	0.9
Israel	82	69	-13	55.0	0.027	0.034	-21.0	1.7
India	73	83	10	57.0	0.024	0.030	-21.6	4.9

Note. Debt ratios are stated to the nearest percentage point and demographic changes to the nearest half point; the table is provided so that any figure can be replaced and the exercise re-run, and Section 7.2 reports the influence diagnostics that a cross-section of this size requires.

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