Robust Detrending in Empirical Macroeconomics*

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Abstract. Macroeconomists widely use the Hodrick–Prescott (HP) filter to measure business cycles as departures from a smooth trend. Relying on HP alone can make findings sensitive to its smoothing rule. We identify 1035 articles using HP in 50 economics journals from 2000 through November 11, 2026. Of these, 58 percent use only the HP family. We classify articles by research question across business cycles, labor markets and productivity, finance and fiscal policy, and inflation and monetary policy. Across the four areas, we revisit classic and recent findings from thirteen studies published between 1990 and 2026. We compare HP, boosted HP (BHP), Baxter–King (BK), Christiano–Fitzgerald (CF), and Hamilton (H). We compare their depth, timeliness, and revision as practical criteria for policy applications. Changing filters reverses some signs and rankings. In the pre-pandemic sample, switching from HP to CF reverses whether output or consumption moves together more closely across countries. The change remains statistically significant after adjustment for multiple comparisons. All three international models understate output comovement across filters. We propose choosing complementary filters in advance, assessing which findings hold, and applying the same filters to observed and simulated data.

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1. Introduction

The Hodrick–Prescott (HP) filter is one of the most widely used methods to measure business cycles as departures from a smooth trend (Hodrick and Prescott 1997). How large an upturn or downturn looks depends on how much of the rise or fall the filter assigns to the trend. Separating trend from cycle requires a judgment about which movements count as fluctuations (Canova 1998; Hamilton 2018). Since the invention of the HP filter, researchers have developed several alternative ways to separate trend from cycle over the years (Baxter and King 1999; Christiano and Fitzgerald 2003; Phillips and Shi 2021). In this paper, we do three things. First, we document extensively past literature which has either relied on only the HP filter or have used more than one filter. Second, we group the papers into four broad areas, compare how often they use HP alone, and revisit published findings from each area using different filters. Third, we show how using more than one filter reveals whether findings depend on a particular way of measuring the cycle.

Consider a classic problem in economics. Suppose the economy starts growing more slowly. Will it catch up in a few years, or will it keep producing less than we previously expected? Both stories can fit the same numbers at first. Economists try to tell them apart by estimating the economy's long-term path and measuring how far output has moved above or below it (Beveridge and Nelson 1981, Nelson and Plosser 1982, Canova 1998). Economists use a statistical tool called a filter to separate the long-term trend from temporary ups and downs. This step is called detrending. The filter estimates a path for the economy, and the distance between that path and actual output is the estimated cycle.

Canova (1998) shows that different filters can draw different paths through the same data, changing how large an upturn or downturn looks. The choice of filter can therefore change what a business cycle model is asked to explain. Many published studies rely on a single filter to detrend data, often HP.

We identify 1035 articles in 50 economics journals that use HP to detrend observed data. Of these, 603 (58 percent) use only the HP family. We first document other filters that have been created over the years. (1) Boosted HP (BHP) repeats the procedure on the fluctuations left over after each pass. (2) Baxter–King (BK) keeps swings of selected lengths using observations on both sides of each date. (3) Christiano–Fitzgerald (CF) also keeps swings of selected lengths and adjusts its weights near the ends of the series. (4) Hamilton (H) measures how far the data depart from what past values predict. For policy applications, we compare (1) depth, how far the cycle moves from trend, (2) timeliness, when the first estimate becomes available, and (3) revision, how much the estimate changes as new data arrive. The research question guides which properties matter and which filters provide complementary checks.

We revisit published findings in four areas of macroeconomic research. Within each study, we reconstruct the published comparison and change the filter while holding the data and dates fixed. Where longer series are available, we also test whether the results hold on the extended dates. The filter can change signs, rankings, or the size of an estimate. Other findings hold across filters.

We propose a detrending robustness protocol. Choose economically appropriate, complementary filters and settings before inspecting the results. Hold the input data and evaluation dates fixed while changing the filter. Specify which changes in the estimates would alter the economic conclusion and report uncertainty for those comparisons. When evaluating a model, apply the same measurement procedure to observed and

simulated data. This follows the logic of checking whether findings hold under alternative specifications (Lu and White 2014; Athey and Imbens 2017).

We contribute to research on how detrending shapes measured cycles and the conclusions drawn from them (Canova 1998; Chukwu et al. 2024; Panizza 2025; Wolf et al. 2026). We document how studies identified in our search of 50 economics journals use the HP filter, distinguishing studies that rely on the HP family alone from those that also use another method. We build on Canova's evidence of sensitivity by specifying how to choose alternatives, isolate the effect of detrending, and assess whether a substantive finding survives. For policy applications, we show how to guide that choice by asking how large an upturn or downturn looks, how soon an estimate is available, and how much that estimate changes as new data arrive.

We also contribute to research that uses measured cycles to evaluate economic findings and models (Backus et al. 1992; Heathcote and Perri 2002; Huo et al. 2025). We revisit published findings across four areas of macroeconomics and compare five filters on the same data and dates. Our international comparison builds on evidence that the ranking of cross-country output and consumption correlations depends on how the cycle is measured (Pakko 2004; Perron and Wada 2016; De Gorostiza-Roudnitski 2026). Applying the same filters to data and simulations, we find that the ranking can reverse in the data, while all three models understate how closely output moves together across countries.

2. Background and Data

2.1. HP Filter

The Hodrick–Prescott (HP) filter fits a smooth trend to a series and measures the cycle as the distance from that trend (Hodrick and Prescott 1997). Let y_t be log real output in period t and let τ_t be its trend. For T observations, HP chooses the trend path that solves

$$\widehat{\boldsymbol{\tau}} = \underset{\tau_1,\ldots,\tau_T}{\arg\min} \left\{ \sum_{t=1}^{T} (y_t - \tau_t)^2 + \lambda \sum_{t=2}^{T-1} (\tau_{t+1} - 2\tau_t + \tau_{t-1})^2 \right\}. \tag{1}$$

The first term keeps the trend close to the data. The second penalizes changes in the trend's growth rate. A larger smoothing parameter λ gives more weight to the second term and produces a smoother trend. The estimated cycle is $c_t = y_t - \widehat{\tau}_t$. Researchers can compare how much these deviations fluctuate and how closely they move across series, in both observed and simulated data (Kydland and Prescott 1982; Kydland and Prescott 1990).

The HP estimate depends on how much smoothing the researcher chooses. A trend that follows output closely can make upturns and downturns look smaller. The same smoothing parameter can also behave differently on annual and quarterly data (Ravn and Uhlig 2002). With persistent series, HP can produce apparent fluctuations that are absent from the process that generated the data (King and Rebelo 1993; Cogley and Nason 1995; Hamilton 2018). The standard two-sided filter uses observations after a date to estimate the trend at that date. A reading based on the data available at the time can therefore change substantially as later observations become available (Orphanides and van Norden 2002; Kamber et al. 2025).

Work on business cycles and real-time output gaps points to three practical questions for policy. How large is the cycle, when can it first be measured, and how much will that estimate change? Table 1b compares all five filters on the same output series. Cycle depth, speed, and stability describe different features of an estimate. Assessing accuracy requires a benchmark for the cycle of interest. Depending on the question, researchers may also care about frequency selectivity, or which swing lengths the filter retains, and phase distortion, or whether it shifts the timing of those swings (Baxter and King 1999; Schüler 2024).

Depth measures the largest percentage distance between output and its estimated trend during the same upturn or downturn. Morley and Piger (2012) show how the estimated cycle can look different in expansions and recessions. Schüler (2024) shows that HP and H can assign different amounts of variation to swings of different lengths. In a simulation of a COVID-like shock, Kamber et al. (2025) show that the choice of filter changes how large the drop below trend appears.

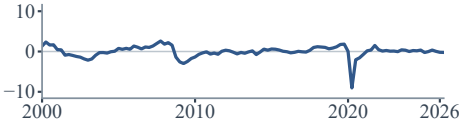
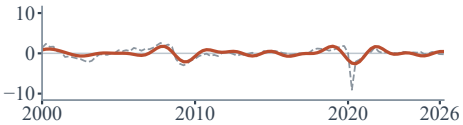
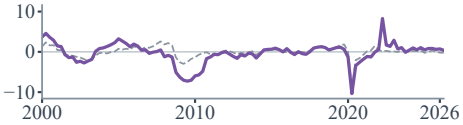
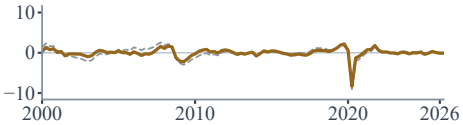
Timeliness measures how many quarters must pass before the filter can provide its first estimate. BK needs observations on both sides of the date being measured (Baxter and King 1999). CF adjusts its weights to give an estimate at the end of the available series (Christiano and Fitzgerald 2003). Wolf et al. (2026) adjust one-sided HP to preserve more of the two-sided filter's cyclical properties when only current and past observations are available.

Revision measures how much that first estimate changes as new observations arrive. Studies of the United States and other OECD countries find that adding observations can substantially revise earlier output gaps (Orphanides and van Norden 2002; Ince and Papell 2013). Quast and Wolters (2022) study the real-time reliability of a modified H filter. Quast and Wolters (2023) show that measured reliability can depend on which later vintage is used as the benchmark. We compare the first estimate for a quarter with its estimate three years later, drawing both samples from the same GNP vintage. This captures the revision from adding observations.

Table 1b also shows how to measure the time it takes output to return to trend, a question related to how long output losses persist after a downturn (Cerra and Saxena 2008).

A classic problem in business cycle research is deciding how much of a change in output reflects a temporary fluctuation and how much reflects a shift in the economy's longer-run path. Table 1a gives the formulas for all five filters and shows the cycles they estimate from the same U.S. real GNP series. BK and CF approximate the same target when they use the same range of swing lengths. H uses a forecast based on earlier values as its reference (Hamilton 2018). In BHP, each pass of HP allows more variation to enter the trend (Phillips and Shi 2021).

Table 1a. Five ways to measure the business cycle

Method	What it does	Formula	Example using U.S. output
HP ($\lambda = 1600$)	Fits a smooth trend and measures how far the series departs from it.	$\widehat{\boldsymbol{\tau}} = S_\lambda \mathbf{y}, \quad \mathbf{c} = \mathbf{y} - \widehat{\boldsymbol{\tau}},$ $S_\lambda = (I + \lambda D'D)^{-1}.$	
BK (6–32 quarters, $K = 12$)	Keeps swings of selected lengths using observations on both sides of each date.	$c_t = \sum_{j=-K}^{K} (B_j - \bar{B}_K) y_{t-j},$ $\bar{B}_K = \frac{1}{2K+1} \sum_{j=-K}^{K} B_j.$	
CF (8–24 quarters, no drift)	Keeps swings of selected lengths and changes the weights near the ends of the series.	$c_t = B_0 y_t + \sum_{\substack{2 \le s \le T-1 \\ s \ne t}} B_{\lvert s-t \rvert} y_s + A_t y_1 + F_t y_T,$ $A_t = -\frac{1}{2} B_0 - \sum_{j=1}^{t-2} B_j,$ $F_t = -\frac{1}{2} B_0 - \sum_{j=1}^{T-t-1} B_j.$	
H ($h = 8$ quarters, $p = 4$ lags)	Measures how far the series departs from a forecast based on earlier values.	$y_t = \alpha + \sum_{j=0}^{p-1} \beta_j y_{t-h-j} + u_t, \qquad c_t = \widehat{u}_t.$	
BHP ($\lambda = 1600$, BIC)	Repeats HP on the remaining fluctuations. Each pass moves more variation into the trend.	$R_\lambda = I - S_\lambda, \qquad \mathbf{c}^{(m)} = R_\lambda^m \mathbf{y},$ $\mathrm{BIC}(m) = \frac{\mathrm{var}(\mathbf{c}^{(m)})}{\mathrm{var}(\mathbf{c}^{(1)})} + \frac{\log T}{\mathrm{tr}(R_\lambda)} \mathrm{tr}(I - R_\lambda^m).$	

This table shows how five filters estimate the cycle in the same U.S. real GNP series from 1950:Q1 to 2026:Q2. The plots show log deviations times 100 from 2000:Q1 on a common scale. The dashed line is HP. BK ends in 2023:Q2 because it needs 12 later quarters. Here y_t is log output, T is the sample length, and D takes second differences. For a band of L to U periods, $a = 2\pi/U$, $b = 2\pi/L$, $B_0 = (b - a)/\pi$, and $B_j = [\sin(jb) - \sin(ja)]/(\pi j)$ for $j \neq 0$.

Table 1b. Practical criteria for comparing filters

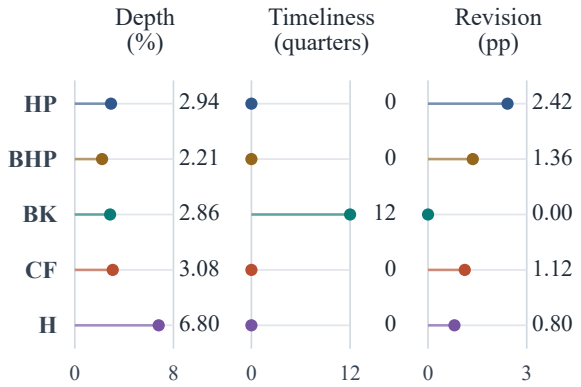
Panel A. What to measure

Metric	How to measure it	Related research
Depth	Largest percentage distance from the filter's reference path during the same upturn or downturn.	(Kamber et al. 2025; Morley and Piger 2012; Schüler 2024; Assunção and Fernandes 2025)
Time to close the gap	Quarters from the same trough or peak until output reaches or crosses the reference path for two consecutive quarters.	(Cerra and Saxena 2008; Zarnowitz and Ozyildirim 2006; Hall and Thomson 2024; Chen et al. 2024)
Timeliness and revision	Quarters to the first reading. Compare with the reading three years later. Report the median absolute revision in percentage points and the share that switch sign.	(Orphanides and van Norden 2002; Wolf et al. 2026; Quast and Wolters 2022; Ince and Papell 2013)

Panel B. How the filters can complement each other

Filter	Depth	Time to close the gap	Timeliness and revision
HP	Departure from a smooth output path.	Closing date relative to a smooth trend.	First reading from a fit through the latest release.
BHP	Departure when the underlying path may have shifted.	Checks the date against a more flexible trend.	Repeats HP through the latest release.
BK	Depth within a chosen range of swing lengths.	Dates historical swings using data on both sides.	Needs 12 later quarters at our setting.
CF	Uses the same swing lengths as BK.	Dates swings through the latest quarter.	Adapts weights at the end of the series.
H	Departure from a forecast based on earlier output.	Checks when output catches up to its forecast.	Uses only the current and earlier releases.

Panel C. Depth, timeliness, and revision



This table illustrates practical trade-offs for policy applications. Panel A defines the measures and Panel B shows how the filters can complement each other. The relevant criteria depend on the question. Assessing accuracy requires a benchmark for the cycle. Panel C shows the three measures for U.S. real GNP in the 2008–09 recession. Depth is the largest shortfall from trend in 2008:Q1–2009:Q2, using data through 2026:Q2. Timeliness counts quarters to the first estimate. Revision is the median absolute change in that estimate three years later, using the same GNP vintage at each cutoff. BK and CF use a 6–32 quarter band. Recession dates are from the NBER.

Canova (1998) and Burnside (1998) show that detrending can change measured cycles and what models are asked to explain. Pakko (2000) separates price–output comovement into swings of different lengths. Francis and Ramey (2009) show how slow movements in hours affect their estimated response to technology shocks. The choice of HP version affects estimates of credit volatility (Wolf et al. 2026). Laumer and Philipps (2024) compare fiscal multipliers under different definitions of expansion and recession. Kollmann (2013) finds that a model with independent shocks understates international output comovement even when HP is applied to both the data and the simulations. These studies led us to ask how often researchers check their results with another detrending method.

To answer that question, we follow Pietri (2026) and Lechevalier and Monfort (2025) in using the IDEAS/RePEc aggregate ranking described by Zimmermann (2013) to select economics journals. We searched the first 50 journal titles in the December 2025 ranking, skipping two Federal Reserve proceedings series. The resulting sample covers general-interest and field journals. Appendix A.1 lists the 45 journals outside the top five.

We searched these journals using keywords, citations, and text-reading tools. Figure 1 summarizes the 1035 articles identified from 2000 through November 11, 2026. Of these, 603 use only the HP family and 432 report a separate non-HP method on observed data. Public copies are harder to locate for older articles. Differences in the counts by year can therefore reflect differences in coverage. Appendix A.2 documents the search and screening rules.

2.2. Research Areas and Comparisons

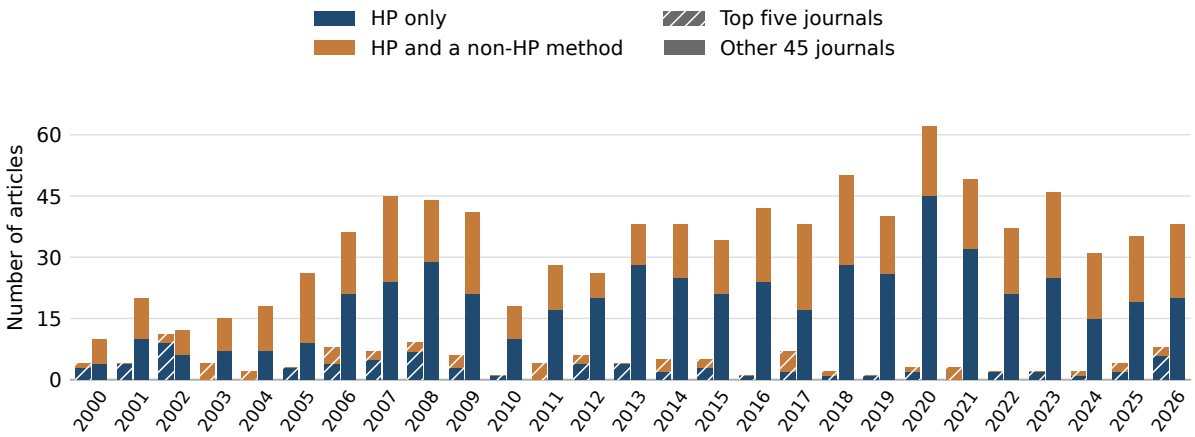
We use themes identified through natural language processing (NLP) clustering to classify the 1014 articles in the snapshot for Figure 1, Panel B. The four areas are business cycles, labor markets and productivity, finance and fiscal policy, and inflation and monetary policy. We assign each article by its main research question, using its title and checking available descriptions where needed. We assign 957 articles to the four areas. The other 57 in the snapshot and the 21 articles counted after it appear in Panel A.

Among the articles in these groups, 46% of the 243 business cycle papers use HP alone. The HP-only shares are 62% in labor markets and productivity, 64% in finance and fiscal policy, and 59% in inflation and monetary policy. Business cycle studies often use detrended volatility and comovement directly as targets for models. A filter can also supply just one input to a broader analysis, such as a predictor, a model parameter, or a rule for dating policy events.

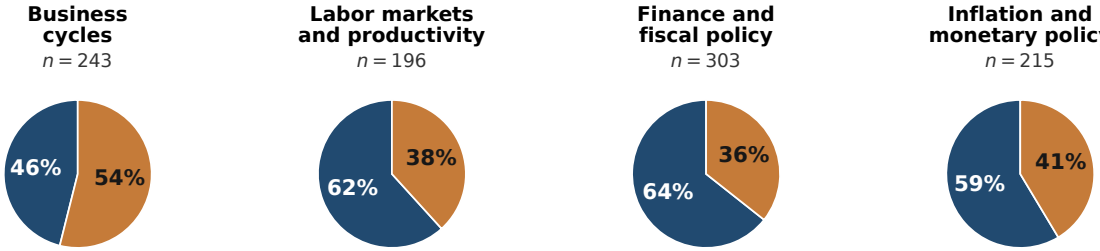
To see whether the choice of filter changes economic conclusions, we revisit thirteen studies across the four areas. Table 2 lists four studies on business cycles, two on labor markets and productivity, three on finance and fiscal policy, and four on inflation and monetary policy. For each study, it shows how the original paper used detrending and what changes when we use other filters.

Figure 1. Use of HP in economics journals, 2000–2026

Panel A. Annual article counts



Panel B. Shares by research area



This figure counts 1035 articles that our search identified as using HP on observed data. Blue shows HP alone, including different smoothing settings and one-sided, two-sided, adjusted, or boosted HP. Orange shows articles that also apply a non-HP method to observed data. In Panel A, striped bars cover the top five journals and solid bars cover the other 45 listed in Appendix A.1. Panel B shows shares within each area for the 957 articles assigned to one of the four areas in the 1014-article snapshot.

Table 2. Studies and findings across four research areas

Study	What the original paper tests and how	What changes and what holds
Panel A. Real business cycles		
Backus et al. (1992)	Uses HP output and consumption cycles in data and simulations to test international risk sharing.	The higher correlation can switch from output to consumption. All three models understate output comovement.
Backus and Kehoe (1992)	Uses HP output cycles to compare prewar and postwar volatility.	Japan's volatility ranking can reverse. U.S. output stays more volatile before World War I.
Kydland and Prescott (1990)	Uses HP cycles to measure the correlation of U.S. prices with output.	The correlation can change sign on the extended dates.
Hodrick and Prescott (1997)	Uses HP expenditure and output cycles to compare volatility.	Investment stays more volatile than output.
Panel B. Labor markets and productivity		
Haefke et al. (2013)	Uses first differences to estimate wage responses to productivity for new hires and all workers.	The estimated response is larger for new hires across filters. The 95% intervals include zero.
Berentsen et al. (2011)	Uses HP inflation and unemployment trends to evaluate the model. Also compares five-year averages.	The trends move together. The model explains 41–97% of the rise in unemployment.
Panel C. Finance and fiscal policy		
Wolf et al. (2026)	Uses three HP versions to compare credit-to-GDP and output volatility in the G7.	The annual ranking reverses in several countries. Credit-to-GDP stays more volatile in quarterly data.
Anundsen et al. (2016)	Uses HP credit and housing gaps to predict banking crises. Also checks annual differences.	The gap coefficient changes sign. The difference is imprecise with country-clustered errors. The credit growth coefficient stays positive.
Michaillat (2014)	Uses HP to set technology persistence in a model that compares public hiring across unemployment rates.	The hiring multiplier is higher at 6.5% than at 6% unemployment under all five calibrations.
Panel D. Inflation and monetary policy		
Lansing (2009)	Uses CF to compare U.S. inflation persistence and volatility over time. Checks with HP.	Persistence and volatility fall under all five filters.
Kim et al. (2005)	Tests for changes in the Fed's policy rule using both a CBO gap and an HP gap.	Evidence that the Fed responded differently across economic conditions before Volcker depends on the filter.
Favero et al. (2005)	Uses an HP output gap in models of inflation forecasts and the policy rule.	The gain in forecast fit holds. The estimated policy response to output varies.
Caldara et al. (2024)	Uses an HP global policy rate gap to check the dating of joint tightenings.	The GDP ranking can reverse at a fixed cutoff. It holds when the cutoffs select the same share of quarters.

This table describes the original detrending approach and the finding under alternative filters for each of thirteen studies. Panels A–D cover real business cycles, labor markets and productivity, finance and fiscal policy, and inflation and monetary policy. We compare filters on common dates, extend the samples for the four business cycle studies, and filter observed and simulated series the same way in the international model comparison.

We use the authors' archived data where available and reconstruct each published comparison as closely as the data allow. To isolate the effect of changing filters, we apply them to the same input series and calculate each result on dates available under all the filters. When we compare data with model simulations, we also match the length of the input series. The business cycle comparisons use 22 annual and 21 quarterly settings, including HP smoothing of 100 and 6.25 for annual data and 1600 for quarterly data.

We use the original authors' data or replication files for the nine studies in labor markets and productivity, finance and fiscal policy, and inflation and monetary policy. The labor studies use archived wage and productivity series and the original workbook for inflation and unemployment. The finance and fiscal policy studies use a G7 credit and output workbook, archived banking-crisis data, and the files for the public-hiring model. The inflation and monetary policy studies use archived inflation series, policy-rule and forecasting inputs, and a panel of monetary tightening episodes. We reproduce several published benchmarks directly, including the HP trends in Berentsen et al. (2011), the credit volatility comparisons in Wolf et al. (2026), and the policy-rule tests in Kim et al. (2005). The online appendix documents the benchmark checks, approximations, and common samples for all nine studies.

For the business cycle studies, we draw on historical and official data releases to reconstruct and extend the original series. The historical volatility exercise uses annual U.S. GNP from Balke and Gordon (1986), prewar Japanese GNP from Ohkawa et al. (1974), and postwar Japanese GDP from the Jordà–Schularick–Taylor database (Jordà et al. 2017). That database also supplies the ten-country historical comparison. The U.S. price and spending exercises use the Philadelphia Fed's Real-Time Data Set (Croushore and Stark 1999; Croushore and Stark 2001). For international comovement, we use archived OECD national accounts and aggregate the original seven European countries using their 1985 output and consumption weights (Summers and Heston 1988).

We test whether the business cycle results hold on later dates. The annual postwar series extend through 2025 using OECD GDP or BEA GNP. Quarterly U.S. output, spending, and price series come from the BEA and BLS through FRED. GNP ends in 2026:Q2 and GDP in 2026:Q3. Current OECD national accounts extend most international series through 2026:Q2. The September 2026 release from Statistics South Africa supplies the extended GDP and consumption series for that country.

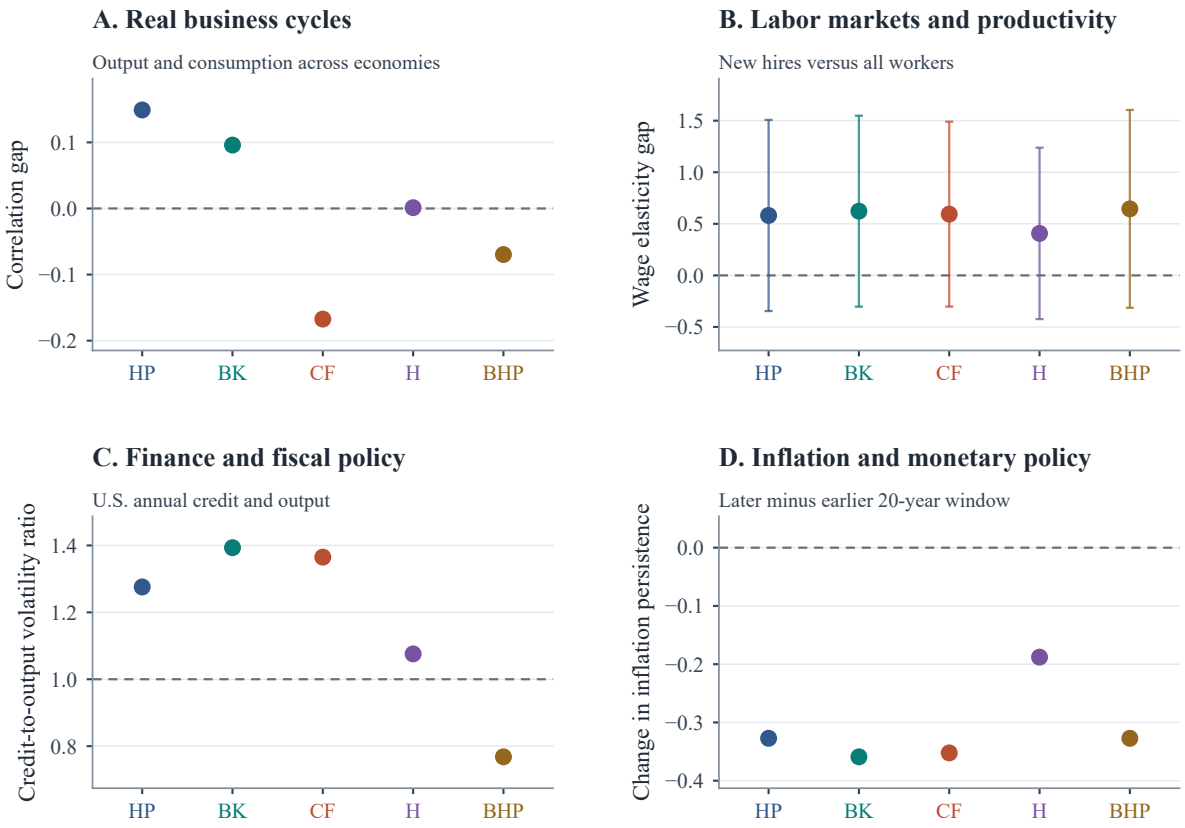
Figure 3 shows pointwise 95% intervals based on 999 bootstrap draws. We draw four-year blocks for historical volatility and eight-quarter blocks for the other comparisons. Paired international contrasts use 3,999 draws and adjust for multiple comparisons across settings. The dynamic response comparisons use 1,999 draws. All intervals condition on the fitted cycles.

Figure 2 previews one comparison from each area. The international correlation and annual credit volatility rankings can reverse across filters. The estimated wage response remains larger for new hires, with wide intervals, and inflation persistence falls across filters.

3. Results

We select classic published studies in each of the four areas (see Appendix B). We reconstruct each finding under the original method as closely as the available data allow. We then repeat each comparison with other

Figure 2. Detrending across four areas of macroeconomic research



This figure compares five filters on the same dates in four applications. Panel A subtracts the U.S.–Europe consumption correlation from the output correlation over 173 common quarters through 2016:Q4 (Backus et al. 1992). Panel B subtracts the wage-growth elasticity of all workers from that of new hires on 71 common quarters through 2003:Q1 (Haefke et al. 2013). Panel C divides U.S. annual credit-to-GDP volatility by output volatility (Wolf et al. 2026). Panel D gives the change in U.S. inflation persistence between 20-year windows ending in 1990:Q4 and 2001:Q4 (Lansing 2009). Whiskers in B give pointwise 95% intervals. The dashed lines mark equal correlations in A, equal elasticities in B, equal volatility in C, and no change in D.

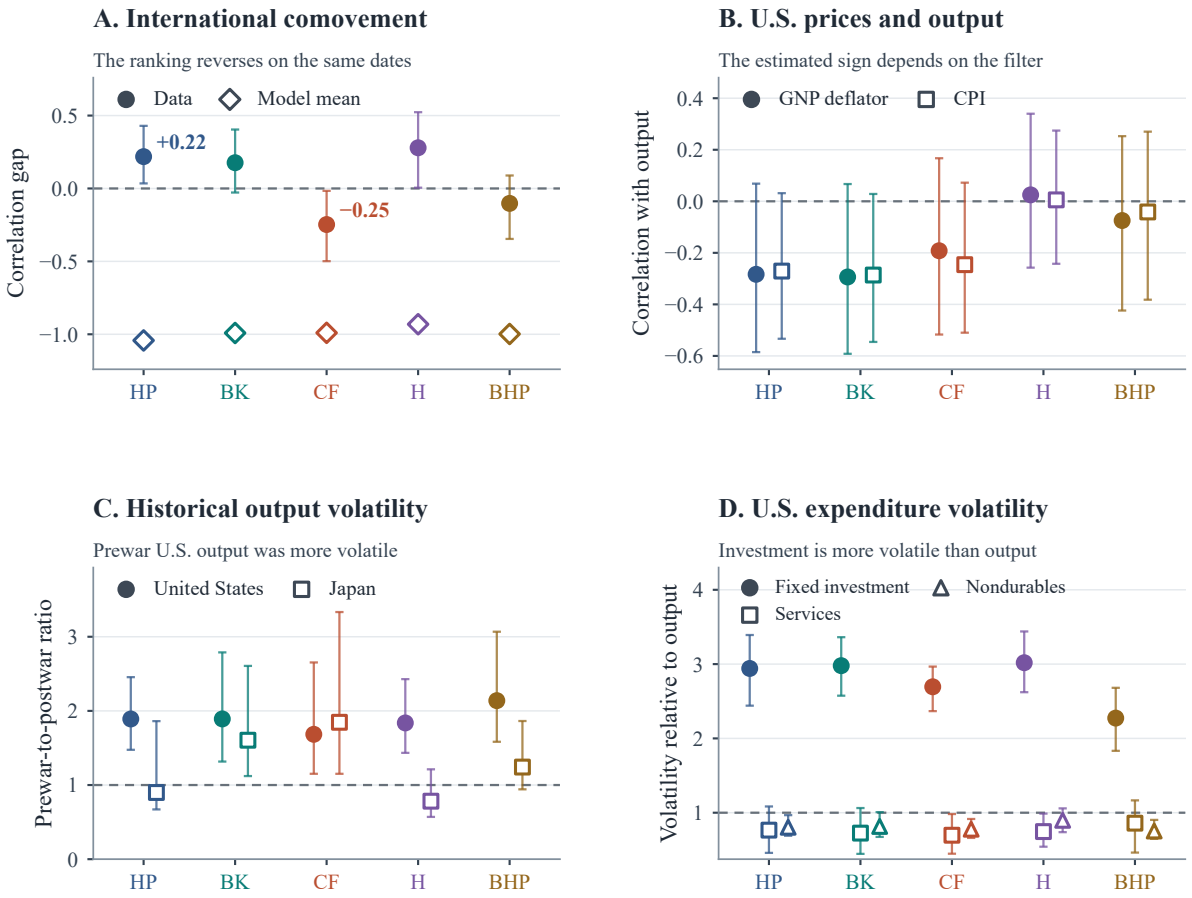
filters on the same data and dates to see whether the finding changes. When we evaluate a model, we apply each filter to the observed and simulated series in the same way. Table 3 reports the model or empirical design and the estimates under each filter for every study.

3.1. Real Business Cycles

We use four papers in this area. Each published comparison measures volatility or comovement directly from HP-filtered series. Figure 3 shows how these statistics change when we use other filters.

International comovement. Backus et al. (1992) apply HP to output and consumption in the data and in their model. Their HP estimates put the U.S.–Europe output correlation 0.24 above the consumption correlation. Our reconstruction gives a gap of 0.13 because the consumption correlation is higher (0.57

Figure 3. Four findings in real business cycle research



This figure compares five filters for four business cycle findings on international comovement, U.S. prices, historical output volatility, and U.S. expenditure volatility (Backus et al. 1992; Kydland and Prescott 1990; Backus and Kehoe 1992; Hodrick and Prescott 1997). Panel A gives the U.S.–Europe output correlation minus the consumption correlation over 1973:Q4–1983:Q4. Positive values mean output moves more closely together. Diamonds show means from 400 model simulations. Panel B correlates price indices with real GNP in the same quarter over 1957:Q4–2023:Q2. Panel C divides prewar by postwar output volatility on common dates in the original periods. Panel D divides spending volatility by GNP volatility over 1953:Q4–2023:Q2. Dashed lines mark zero in A and B and equal volatility in C and D. Whiskers show pointwise 95% block-bootstrap intervals for the observed data. Quarterly settings are in Table 1a. Annual settings are HP(100), BK(2–8 years, $K = 4$), CF(3–6 years, without drift removal), H ($h = 2, p = 2$), and BHP(100, BIC).

versus 0.46). The original inputs are unavailable, and the source of the difference remains unresolved. On the 41 common quarters, HP puts output ahead and CF puts consumption ahead, although the simultaneous interval for the difference includes zero. In the longer pre-pandemic sample, CF changes the output-minus-consumption gap from $+0.15$ under HP to -0.17, a difference of -0.32 with a simultaneous 95% interval of $[-0.56, -0.07]$. When the sample includes the pandemic and later quarters, the two correlations are close across filters (Table B2).

To check the model's fit, we apply the same filters to the data and to simulations from the original model and the two versions of Heathcote and Perri (2002). In their two-good model, removing international asset trade raises the output correlation toward the observed value. The version with complete markets better matches the observed gap between the output and consumption correlations. In the extended sample, the observed output correlation exceeds the 95th percentile of every model's simulated correlations at every setting. Changing the filter can change the observed ranking. It leaves the shortfall in model output comovement in place.

Historical output volatility. Backus and Kehoe (1992) compare prewar and postwar output volatility using HP cycles. For Japan, changing HP smoothing from 100 to 6.25 raises the prewar-to-postwar ratio from 0.89 to 1.46 on the same reconstructed sample. The published HP(100) benchmark is 0.80 and uses a different postwar series (Ravn and Uhlig 2002). Japan's ranking reverses within HP. U.S. output remains more volatile before World War I across filters and samples (Table B1).

U.S. prices. Kydland and Prescott (1990) use HP cycles to measure the correlation between prices and output. In the longer sample the correlation is negative under HP and slightly positive under some H settings. The positive estimates are imprecise, so the sign is a less stable target for models (Table B1).

U.S. spending. Hodrick and Prescott (1997) compare spending volatility with output volatility using HP cycles. Fixed investment remains more than twice as volatile as output under every setting in both samples. The size of investment fluctuations relative to output remains a target for models across filters (Table B1).

3.2. Labor Markets and Productivity

We use two papers in this area. They examine how wages respond to productivity and how much of a rise in unemployment a model can explain.

Wages and productivity. Haefke et al. (2013) estimate wage responses in first differences and also report HP and band-pass moments. We estimate the wage responses after removing trends with each filter. The estimated response is larger for new hires than for workers overall under all five filters. The difference ranges from 0.41 to 0.65 on the same 71 quarters. Every 95% interval includes zero (Figure 2).

Inflation and unemployment. Berentsen et al. (2011) use HP trends to ask how much of the 1972–1982 rise in unemployment their model explains. They also compare the trends with five-year averages. We hold the model calibration fixed and change how we estimate the trend in both the data and the simulation. Inflation and unemployment trends move together across filters. The model explains 42% of the rise under HP and 97% under H (Table B3).

3.3. Finance and Fiscal Policy

We use three papers in this area. They examine credit fluctuations, warning signs of banking crises, and the employment effects of public hiring.

Credit volatility. Wolf et al. (2026) use three versions of HP to compare credit-to-GDP and output volatility. When we compare filter families, the credit-to-GDP ratio remains more volatile than output in quarterly data. The ranking can reverse in annual data (Table B3).

Banking-crisis predictors. Anundsen et al. (2016) use HP to estimate credit and housing gaps and also check a specification with annual differences. We re-estimate the crisis model with gaps from each filter. The household credit gap coefficient changes sign, although the difference is imprecise with country-clustered errors. Real credit growth remains positively associated with banking crises across filters (Table B3).

Public hiring. Michaillat (2014) uses HP-filtered data to set the persistence of technology shocks in a model of public hiring. The filter changes the calibrated persistence. Across all five calibrations, the public-hiring multiplier is larger at 6.5% unemployment than at 6%.

3.4. Inflation and Monetary Policy

We use four papers in this area. They examine inflation persistence, the Federal Reserve's response to economic conditions, inflation forecasts, and joint central bank tightenings.

Inflation persistence. Lansing (2009) uses CF for the main estimates and checks them with HP. We repeat the comparison across all five filters. Inflation becomes less persistent and less volatile under every filter (Table B3).

The Federal Reserve's policy rule. Kim et al. (2005) test for changes in the Fed's response across economic conditions using both a Congressional Budget Office output gap and an HP gap. When we re-estimate the policy rule with alternative gaps, the evidence that the Fed responded differently across economic conditions before Volcker depends on the filter (Table B3).

Inflation forecasting. Favero et al. (2005) use an HP output gap in a forecast and a monetary policy rule. We replace the output gap in both exercises. Adding indicators improves inflation forecast fit under all five filters. The estimated policy response to output varies with the filter (Table B3).

Joint monetary tightening. Caldara et al. (2024) use an HP trend for the global policy rate in their check on the dating of joint tightenings. We change the filter used to date those episodes. With the same cutoff, H reverses the ranking of the two GDP declines, with a confidence interval that includes zero. When the cutoffs classify the same share of quarters as joint tightening, all five filters give a larger decline after joint tightening (Table B3).

Table 3. Study-level results across five filters

Study and design	Statistic	HP	BK	CF	H	BHP	Evidence and finding
Panel A. Real business cycles							
Backus et al. (1992) Two-country RBC model	Output minus consumption correlation (pre-pandemic)	+0.15	+0.10	**−0.17**	+0.001	**−0.07**	**Supported.** HP and CF give opposite rankings.
	Output correlation gap (data vs. model, extended)	+0.43	+0.42	+0.47	+0.32	+0.49	**Stable.** Models understate output comovement.
Backus and Kehoe (1992) Historical output moments	Prewar/postwar output volatility ratio (Japan)	0.90	**1.60**	**1.85**	0.78	**1.24**	**Descriptive.** The more volatile period changes.
	Prewar/postwar output volatility ratio (U.S.)	1.89	1.89	1.68	1.84	2.14	**Stable.** Prewar U.S. output is more volatile.
Kydland and Prescott (1990) U.S. cycle moments for RBC	Price–output correlation (extended)	−0.28	−0.29	−0.19	**+0.03**	−0.07	**Descriptive.** The price correlation flips sign.
Hodrick and Prescott (1997) U.S. expenditure moments	Investment/output volatility ratio (extended)	2.94	2.98	2.69	3.02	2.27	**Stable.** Investment is more volatile than output.
Panel B. Labor markets and productivity							
Haefke et al. (2013) Wage–productivity regressions	Wage elasticity gap (new hires minus all workers)	+0.58	+0.62	+0.59	+0.41	+0.65	**Stable.** New hires have a larger estimated response.
Berentsen et al. (2011) Monetary search model	Unemployment rise explained (1972–82)	42%	41%	43%	97%	44%	**Descriptive.** The share explained varies.
	Correlation of inflation and unemployment trends	+0.59	+0.55	+0.45	+0.26	+0.36	**Stable.** The trends move together.
Panel C. Finance and fiscal policy							
Wolf et al. (2026) U.S. credit and output moments	Credit/GDP-to-output volatility (annual)	1.28	1.39	1.37	1.08	**0.77**	**Descriptive.** The more volatile series changes.
	Credit/GDP-to-output volatility (quarterly)	4.10	4.72	4.23	3.39	2.17	**Stable.** Credit-to-GDP is more volatile quarterly.
Anundsen et al. (2016) Banking-crisis panel logit	Household credit gap coefficient	+0.27	**−0.04**	+0.40	+0.07	**−0.19**	**Descriptive.** The credit-gap sign changes.
	Real credit growth coefficient	+0.17	+0.30	+0.21	+0.23	+0.42	**Stable.** The estimated coefficient is positive.
Michaillat (2014) Public-hiring search model	Hiring multiplier difference (6.5% vs. 6% unemployment)	+0.042	+0.029	+0.031	+0.042	+0.023	**Stable.** The multiplier is higher at 6.5%.
Panel D. Inflation and monetary policy							
Lansing (2009) Rolling inflation moments	Change in inflation persistence	−0.33	−0.36	−0.35	−0.19	−0.33	**Stable.** Inflation persistence falls.
	Change in inflation volatility (pp)	−0.78	−0.77	−0.83	−1.37	−0.56	**Stable.** Inflation volatility falls.
Kim et al. (2005) Nonlinear Fed policy rule	Pre-Volcker linearity test (p-value)	0.127	0.179	**0.021**	0.529	0.509	**Descriptive.** Only CF rejects linearity at 5%.
Favero et al. (2005) Inflation forecast and GMM policy rule	Forecast fit gain (adjusted R^2)	+0.27	+0.28	+0.21	+0.31	+0.30	**Stable.** Adding indicators improves fit.
	Long-run policy response to the output gap	1.28	0.78	1.07	0.43	2.96	**Descriptive.** The positive response varies in size.
Caldara et al. (2024) International tightening events	GDP response gap (fixed cutoff, pp)	−2.96	−3.01	−2.62	**+1.35**	−1.96	**Descriptive.** H flips the fixed-cutoff ranking.
	GDP response gap (matched event share, pp)	−2.96	−2.27	−2.20	−0.32	−2.42	**Stable.** Matching event shares removes H's reversal.

This table distinguishes supported changes in findings, descriptive changes in estimates, and findings stable across the filters tested. The pre-pandemic CF-minus-HP correlation-gap contrast has simultaneous 95% interval $[-0.56, -0.07]$. The credit-gap contrast under country clustering and the fixed-cutoff H tightening gap have intervals that include zero. Positive price correlations are imprecise. Bold estimates cross zero, one for volatility ratios, or the 5% test threshold relative to HP. The model gap uses the highest simulated mean. Wage-gap intervals include zero. Credit coefficients are per percentage point. The GDP gap is joint minus separate tightening. Appendix B and the online appendix report settings and uncertainty.

4. Conclusion

Macroeconomists use HP to measure fluctuations in output, spending, and prices around a smooth trend. In our search of 50 economics journals, 58 percent of the 1035 articles that use HP on observed data rely on the HP family alone. We compare five filters across thirteen studies, holding the data and dates fixed within each comparison.

Whether output or consumption moves together more closely across countries depends on the filter. Across filters, all three models still predict that output moves together less closely than it does in the data. In the two-good model, removing international asset trade improves the fit of the output correlation. The version with complete markets comes closer to the observed gap between the output and consumption correlations. Which version fits better depends on the finding it is asked to match.

The estimated ranking of prewar and postwar output volatility in Japan can reverse, and the U.S. price–output correlation can change sign on the extended dates. Fixed investment remains more than twice as volatile as output in the longer sample. In the banking-crisis regressions, the estimated credit gap coefficient changes sign across filters, while the credit growth coefficient stays positive. Adding indicators improves the fit of inflation forecasts under all five filters.

In the United States and United Kingdom, net exports as a share of GDP tend to be below trend when output is above trend. This holds across filters on both the original and extended dates. In six of eleven countries on the extended dates, whether net exports and output move together or in opposite directions depends on the filter (Backus et al. 1994).

Building on Canova (1998), we ask how hours worked and investment respond when output is higher than past data would predict. A year later, the estimated responses are increases under HP and declines under BHP. The differences remain statistically significant after adjusting for both comparisons. HP and BHP measure output fluctuations differently, so they can disagree about how much of the same rise in output was unexpected.

We propose making detrending robustness a routine check. Choose complementary filters and plausible settings suited to the question before seeing the results. Hold the data and evaluation dates fixed. Report which economic findings change and the uncertainty around those changes. Apply the same filters to observed and simulated data when evaluating a model. The replication materials provide code and machine-readable results for researchers to rerun the comparisons, including with a coding assistant.

AI disclosure. OpenAI's ChatGPT (GPT-6 Luna) was used for proofreading and developing replication code.

Appendix A. Literature Survey

Appendix A.1. Journals in the RePEc Cohort

This table lists the 45 journals outside the top five in Figure 1 and their ranks in the December 2025 IDEAS/RePEc aggregate ranking. Following Pietri (2026, Appendix A), we use this ranking to give the literature search broad coverage of leading general-interest and field journals.

Rank	Journal	Rank	Journal
4	Journal of Financial Economics	29	Journal of Applied Econometrics
5	Journal of Economic Literature	30	Economic Policy
7	Journal of Economic Growth	31	Journal of Development Economics
8	Journal of Economic Perspectives	32	Journal of Economic Theory
9	American Economic Journal: Macroeconomics	33	Journal of Financial Intermediation
10	The Review of Financial Studies	34	Review of Economic Dynamics
11	Journal of Finance	35	Journal of Money, Credit and Banking
12	American Economic Journal: Applied Economics	36	European Economic Review
14	Journal of Monetary Economics	37	Research Policy
15	Journal of Econometrics	38	Journal of Human Resources
16	Annual Review of Economics	39	Journal of Business Venturing
17	Brookings Papers on Economic Activity	40	American Economic Review: Insights
18	The Review of Economics and Statistics	41	Journal of International Business Studies
19	Journal of the European Economic Association	42	Experimental Economics
20	The Economic Journal	43	International Economic Review
21	RAND Journal of Economics	45	Journal of Banking & Finance
22	Journal of Labor Economics	46	Organization Science
23	Journal of Accounting and Economics	47	Journal of Accounting Research
24	Journal of International Economics	48	Management Science
25	American Economic Journal: Economic Policy	50	Journal of Law and Economics
26	Journal of Business & Economic Statistics	51	Journal of Financial and Quantitative Analysis
27	Journal of Public Economics	52	Journal of Economic Dynamics and Control
28	Strategic Management Journal		

Appendix A.2. Journal Survey Methods

Journal roster and search. We use the first 50 journals in the December 2025 IDEAS/RePEc aggregate ranking, skipping Federal Reserve proceedings at ranks 44 and 49. We include journal articles dated January 1, 2000 through November 11, 2026. The Crossref catalog joins print and electronic ISSNs and deduplicates by published DOI. We search OpenAlex for variants of *Hodrick Prescott*, *Hodrick and Prescott*, *HP filter*, and *HP filtered* and for citations to papers on HP and its use. We expand the candidate list using Semantic Scholar citations, Crossref reference and abstract searches, alternate spellings and smoothing notation, and targeted full-text and archive searches. ProQuest and JSTOR searches supplement coverage for 2000–2009. Publisher, author, repository, and replication-archive links help recover article text. Working-paper and accepted versions are eligible as evidence and are linked to the published DOI. The file `hp_ranked_journals.csv` freezes the roster and `hp_search_coverage_by_year.csv` records coverage.

Inclusion and filter classification. Keyword hits and citations are leads. To enter the count, an article must apply HP to observed data, and the recovered materials must show whether a separate non-HP method is also applied to observed data. We count an article once even when multiple sources document its methods. One-sided, two-sided, adjusted, and boosted HP are part of the HP family. A non-HP method must estimate its own trend or cycle from data that have not already been HP-filtered. Descriptions of another study's method, simulations alone, and unresolved cases do not enter the count. The article decisions store the published DOI, evidence link, location in the source, and classification. The search identifies 1035 included articles. Of these, 603 use only the HP family and 432 also use a non-HP method. The resulting 58.3% is the HP-only share among the included articles.

Research areas and validation. NLP-assisted grouping identified four recurring topics. The saved labels assign one primary area using the article's main question, with titles and available descriptions used for review. Studies whose main question falls outside the four areas have a separate label. The initial clustering settings are unavailable. We retain the DOI-level assignments and use them to regenerate the reported shares. We checked that the topic snapshot contains unique DOIs and that all method labels agree with the final evidence file. We also selected five titles from each of the four areas and the outside group using a fixed DOI hash. A separate AI-assisted title review with the original labels hidden matched 22 of the 25 assignments. Two titles were unclear and one received a different label. This checks how clearly the saved labels follow from titles. The audit file records the decisions.

Which articles enter each panel. Panel A of Figure 1 counts all 1035 articles. The topic snapshot has 1014 of those articles. The other 21 enter Panel A only. Of the 1014 articles, 957 fall in the four areas and contribute to Panel B. The remaining 57 have an outside label. Each pie uses the count for its own area as its denominator. The evidence and topic files, title-audit decisions, and scripts reproduce the DOI joins, counts, and figure percentages. The online appendix reports search coverage and links to the article-level evidence.

Appendix B. Detailed Filter Estimates

Tables B1–B3 report the estimates underlying the comparisons in Table 3.

Table B1. Historical output volatility, U.S. prices, and investment

	HP on published dates		Five filters on common dates				
Outcome and window	Published	Rebuilt	HP	BK	CF	H	BHP
A. Prewar-to-postwar output volatility							
United States, original	1.90	1.89	1.89	1.89	1.68	1.84	2.14
United States, extended	–	–	2.18	2.37	2.28	2.20	2.21
Japan, original	0.80	0.89	0.90	1.60	1.85	0.78	1.24
Japan, extended	–	–	1.05	1.75	1.68	0.93	1.40
B. Correlation of U.S. prices with real GNP							
GNP deflator, original	−0.55	−0.56	−0.61	−0.58	−0.41	−0.13	−0.16
GNP deflator, extended	–	–	−0.28	−0.29	−0.19	0.03	−0.07
CPI, original	−0.57	−0.57	−0.56	−0.56	−0.50	−0.15	−0.33
CPI, extended	–	–	−0.27	−0.29	−0.25	0.01	−0.04
C. U.S. investment volatility relative to GNP							
Fixed investment, original	2.83	2.84	2.94	2.89	2.57	2.66	2.51
Fixed investment, extended	–	–	2.94	2.98	2.69	3.02	2.27

This table reports historical and postwar U.S. and Japanese business cycle comparisons. Panel A divides prewar output volatility by postwar output volatility. Published HP(100) ratios come from Ravn and Uhlig (2002, Table 2). Extended annual inputs run through 2025. On common dates, the U.S. postwar windows are 1954–1979 and 1954–2021. Japan's are 1956–1982 and 1956–2021. Panel B correlates U.S. prices with real GNP. Panel C divides U.S. fixed investment volatility by GNP volatility. Published quarterly estimates use HP(1600) (Kydland and Prescott 1990; Hodrick and Prescott 1997). Original common dates are 1957:Q4–1986:Q4 for prices and 1953:Q4–1976:Q2 for investment. Extended common dates end in 2023:Q2. Figure 3 gives the filter settings.

Table B2. International comovement in the data and three models

Panel A. Published and reconstructed HP benchmark

	United States and Europe			Simulated model		
Specification	Output	Consumption	Gap	Output	Consumption	Gap
Published HP	0.70	0.46	0.24	−0.18	0.88	−1.06
Reconstructed HP	0.70	0.57	0.13	−0.17	0.88	−1.05

Panel B. Output and consumption correlations on common dates

	Data		Backus et al.		Two goods, complete		Financial autarky	
Filter	Output	Cons.	Output	Cons.	Output	Cons.	Output	Cons.
Original sample								
HP(1600)	0.71	0.50	−0.17	0.87	0.20	0.66	0.26	0.85
BK, 6–32	0.75	0.57	−0.11	0.88	0.20	0.65	0.26	0.85
CF, 6–32	0.83	0.73	−0.12	0.87	0.19	0.65	0.25	0.85
BK, 8–24	0.56	0.67	−0.11	0.88	0.19	0.65	0.25	0.85
CF, 8–24	0.54	0.79	−0.12	0.87	0.17	0.64	0.23	0.84
H, 8 quarters	0.76	0.48	−0.07	0.87	0.20	0.60	0.26	0.80
BHP, BIC	0.38	0.48	−0.13	0.87	0.19	0.67	0.24	0.86
Extended sample								
HP(1600)	0.69	0.66	−0.19	0.87	0.19	0.66	0.26	0.85
BK, 6–32	0.68	0.65	−0.14	0.89	0.19	0.65	0.26	0.85
CF, 6–32	0.70	0.65	−0.17	0.88	0.19	0.66	0.25	0.85
BK, 8–24	0.67	0.71	−0.13	0.89	0.19	0.67	0.25	0.86
CF, 8–24	0.71	0.75	−0.16	0.87	0.18	0.66	0.24	0.85
H, 8 quarters	0.67	0.59	−0.01	0.90	0.27	0.67	0.34	0.85
BHP, BIC	0.73	0.78	−0.15	0.88	0.18	0.67	0.24	0.86

Panel C. Change in the observed gap when CF replaces HP

Window	Quarters	HP gap	CF gap	Difference
Original	41	0.22	−0.25	−0.47
				[−0.99, 0.06]
Pre-pandemic	173	0.15	−0.17	−0.32
				[−0.56, −0.07]
Extended	199	0.03	−0.04	−0.07
				[−0.35, 0.21]

This table compares U.S.–Europe output and consumption correlations in the data and in three models. Panel A reports the published HP estimates and our reconstruction (Backus et al. 1992). Panel B compares observed correlations with means over 400 paths per model. We calculate each correlation on the same dates in the data and each path, with 41 quarters through 1983:Q4 and 199 through 2023:Q2. BK uses twelve quarters on either side, and CF omits drift removal. Panel C shows how CF changes the output correlation minus the consumption correlation relative to HP, with simultaneous 95% intervals. The pre-pandemic series ends in 2019:Q4, with correlations on common dates through 2016:Q4.

Table B3. Detrending in labor, finance, and policy studies

Outcome	HP	BK	CF	H	BHP
A. Labor markets and productivity					
New-hire minus all-worker wage elasticity	0.58	0.62	0.59	0.41	0.65
Unemployment rise explained	42%	41%	43%	97%	44%
B. Finance and fiscal policy					
U.S. annual credit-to-output volatility ratio	1.28	1.39	1.37	1.08	0.77
Household credit gap coefficient	0.27	−0.04	0.40	0.07	−0.19
Real credit growth coefficient	0.17	0.30	0.21	0.23	0.42
Public-hiring multiplier at 6% unemployment	0.327	0.336	0.335	0.327	0.341
Public-hiring multiplier at 6.5% unemployment	0.369	0.365	0.366	0.369	0.363
C. Inflation and monetary policy					
Inflation persistence, early to late	0.59 to 0.26	0.56 to 0.20	0.61 to 0.26	0.81 to 0.62	0.36 to 0.04
Inflation volatility, early to late	1.53 to 0.75	1.49 to 0.72	1.58 to 0.75	2.43 to 1.05	1.21 to 0.65
Pre-Volcker linearity test (p-value)	0.127	0.179	0.021	0.529	0.509
Inflation forecast fit (gain in adjusted R^2)	0.27	0.28	0.21	0.31	0.30
GDP gap after joint tightening, fixed cutoff	−2.96	−3.01	−2.62	1.35	−1.96
GDP gap, matched event share	−2.96	−2.27	−2.20	−0.32	−2.42

This table compares results across five filters for nine labor, finance, and policy studies. Panel A reports the wage-growth elasticity for new hires minus that for all workers and the share of the rise in trend unemployment explained by the model. Panel B reports U.S. annual credit-to-GDP volatility divided by output volatility, changes in banking-crisis log odds per percentage point of each predictor, and employment gained per public job-week. Panel C compares inflation persistence and volatility in 20-year windows ending in 1990:Q4 and 2001:Q4, with volatility in percentage points. It also reports the policy-rule test, the gain in forecast fit from twelve factors, and the difference in GDP responses eight quarters after joint and separate tightenings. Dates are common across filters within each comparison.

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