Do Panels Help Solve the Purchasing Power Parity Puzzle?

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Abstract

While Rogoff (1996) describes the “remarkable consensus” of 3 to 5 year half-lives of purchasing power parity deviations among studies using long-horizon data, recent papers using panel methods with post-1973 data report shorter half-lives of 2 to 2.5 years. These studies, however, do not use appropriate techniques to measure persistence. We extend median-unbiased estimation methods to the panel context, calculate both point estimates and confidence intervals, and provide strong evidence confirming Rogoff’s original claim. While panel regressions provide more information on the persistence of real exchange shocks than univariate regressions, they do not help solve the purchasing power parity puzzle.

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1. Introduction

Rogoff's (1996) purchasing power parity puzzle involves the difficulty of reconciling very high short-term volatility of real exchange rates with very slow rates of mean reversion. In a much-quoted phrase, he describes the "remarkable consensus" of 3 to 5 year half-lives of purchasing power parity (PPP) deviations among various studies. The PPP puzzle has inspired a good deal of research, much of it directed at attempts to "solve" the PPP puzzle by reducing the half-lives.

The empirical work that Rogoff cites in support of his three to five year consensus mostly comes from univariate studies with long-horizon data.[1] More recent panel studies using quarterly, post-Bretton-Woods data with nominal exchange rates deflated by consumer price indexes and the United States dollar as the numeraire currency, including Wu (1996), Papell (1997, 2002), Fleissig and Strauss (2000), and Papell and Theodoridis (2001), find shorter half-lives of 2 to 2.5 years.

These shorter half-lives appear to be influencing perceptions of the magnitude of the PPP puzzle. Cheung, Chinn, and Fujii (2001) use 2 to 2.5 years to describe the results of the more recent studies. Engel and Morley (2001) use 2.5 to 5 years to describe the "typical" estimate of half-lives, followed by the quote from Rogoff (1996) which contains the 3 to 5 year consensus. Obstfeld (2001) writes "The best current estimates of real exchange rate persistence suggest that under floating nominal exchange rate regimes, the half-lives of real exchange rates shocks range from 2 to 4.5 years." If the point estimate of half-lives of PPP deviations is 2.5 rather than 4 (the average of Rogoff's 3 to 5) years,

[1] He does discuss two early panel studies. Frankel and Rose (1996) use annual data while Wei and Parsley (1995) use sectoral data.

it is much more likely that models with nominal rigidities (or future modifications of such models) will be able to "solve" the PPP puzzle.

This paper argues that the evidence of shorter half-lives from panel methods applied to post-1973 data is misleading. The reason is straightforward. These studies typically use panel versions of Augmented Dickey-Fuller (ADF) tests to investigate whether the null hypothesis of a unit root in real exchange rates can be rejected. The least squares estimate of the parameter of interest, α, the sum of the autoregressive coefficients, is significantly downward biased in models that contain an intercept or an intercept and a time trend. The bias becomes more severe as α gets larger, which has particular relevance for the case of real exchange rates. Moreover, the half-life, the expected number of years for a PPP deviation to decay by 50%, is a nonlinear function of α, which accentuates the bias.

These issues are addressed in the univariate context for first order AR models by Andrews (1993), who shows how to calculate exactly median-unbiased estimates, as well as exact confidence intervals, for half-lives in Dickey-Fuller (DF) regressions. Andrews and Chen (1994) show how to perform approximately median-unbiased estimation in ADF regressions. Median-unbiased estimators have the desirable properties that their point estimates remain median-unbiased and the coverage probabilities of their confidence intervals are invariant under monotonic transformations. This is important because the least squares estimate of the half-life, $\ln(0.5)/\ln(\alpha_{LS})$, is a monotonic transformation of the least squares estimate α_{LS}. If we replace α_{LS} with a median-unbiased estimate, α_{MU}, the half-life estimate will also be median-unbiased.

Murray and Papell (2002) use median-unbiased estimation methods with both annual long-horizon and quarterly post-1973 real exchange rate data. With long-horizon data, the median value of the point estimates of PPP deviations from the univariate regressions is 3.98 years, almost in the middle of Rogoff's 3-5 year range. With post-1973 data, the median half-life is 3.07 years, near the bottom, but still within, Rogoff's consensus. The median bounds of the confidence intervals, however, especially with the post-1973 data, are much too wide to be informative.

Murray and Papell (2004) use these methods to re-examine the evidence of slow mean reversion for the long-horizon dollar-sterling real exchange rate found by Lothian and Taylor (1996). Using their specification, we show that they underestimate the half-lives of PPP deviations, and thus overestimate the speed of mean reversion. When their specification is amended to allow for serial correlation, the speed of mean reversion falls even further. These results make resolution of the purchasing power parity puzzle more problematic.

The purpose of this paper is to extend median-unbiased estimation methods to the panel context and to investigate the implications of these methods for the persistence of deviations from PPP. We first compute least squares estimates of the half-life in panel ADF regressions. Using GS lag selection, the point estimate of the half-life is 2.35 years, with bounds on the 95% bootstrap confidence interval of 1.29 and 2.22 years. With MAIC lag selection, the half-life estimate is 2.90 years, with a confidence interval of [1.47, 2.74] years. While the point estimates seems reasonable, there is obviously something wrong when the upper bound of the 95% confidence interval lies *below* the point estimate. This failure of the bootstrap confidence interval reflects the bias inherent in least squares estimates of AR parameters, as well as an additional layer of bias, since the data generating process in the bootstrap is based on a biased estimate.

We proceed to compute approximately median-unbiased estimates of half-lives in panel ADF regressions. Our median point estimate is 3.55 years, with bounds on the 95% confidence interval of 2.48 and 4.09 years. We therefore conclude that panel methods applied to post-1973 data do not solve the purchasing power parity puzzle. Both the point estimates and the confidence intervals are consistent with Rogoff's 3 – 5 year consensus.

2. Persistence of PPP deviations

Purchasing Power Parity is the hypothesis that, following a disturbance, the real exchange rate reverts in the long run to a constant mean. We consider real exchange rates with the United States dollar as the numeraire currency, which are calculated as follows:

$$q = e + p^* - p, \tag{1}$$

where q is the logarithm of the real exchange rate, e is the logarithm of the nominal (dollar) exchange rate, p is the logarithm of the domestic CPI, and p^* is the logarithm of the U.S. CPI.

The Dickey-Fuller model regresses the real exchange rate on a constant and its lagged level:

$$q_t = c + \alpha q_{t-1} + u_t . \tag{2}$$

The null hypothesis of a unit root is rejected in favor of long run PPP if α is significantly less than unity.[2] A time trend is not included in equation (2) because such an inclusion would be theoretically inconsistent with long run PPP.[3]

The Augmented Dickey-Fuller regression adds *k* first differences of the real exchange rate to Equation (2) in order to allow for serial correlation:

$$q_t = c + \alpha q_{t-1} + \sum_{j=1}^{k} \psi_j \Delta q_{t-j} + u_t \ . \tag{3}$$

Again, the unit root null is rejected in favor of long run PPP if α is significantly less than one.

A panel extension of the DF regression which allows for heterogeneous intercepts would involve estimating the following equations,

$$q_{it} = c_i + \alpha q_{i,t-1} + u_{it} \ , \tag{4}$$

where the subscript i indexes the country and c_i denotes the country-specific intercept. Alternatively, a panel extension of the ADF regression model which allows for a heterogeneous intercept, as well as serially and contemporaneously correlated residuals, is written

$$q_{it} = c_i + \alpha q_{t-1} + \sum_{j=1}^{k_i} \psi_{ij} \Delta q_{i,t-j} + u_{it} \ . \tag{5}$$

We follow Levin, Lin, and Chu (2002) and restrict the value of α to be equal for every country in the panel. The null hypothesis is that all of the series contain a unit root and the alternative is that they are all stationary. This is in contrast with panel unit root tests,

[2] The literature on testing for PPP is voluminous. In comparison with unit root tests using CPI-based real exchange rates, evidence of stationarity is typically stronger using indices of traded goods, see Xu (2003), or testing for cointegration between the nominal exchange rate and relative prices, see Pedroni (2001).
[3] See Papell and Prodan (2004) for a further discussion of this issue.

such as Im, Pesaran, and Shin (2003), which allow α to vary across countries. In that case, the null hypothesis is that all of the series contain a unit root and the alternative is that at least one of the series is stationary.

In order to allow for contemporaneous correlation of the errors, Equations (4) and (5) are estimated using feasible GLS (seemingly unrelated regressions), with α equated across countries. In Abuaf and Jorion (1990), Equation (4) is used for long-horizon annual data and the value of k is set to 12 in Equation (5) for post-1973 monthly data. In Papell (1997), the values for k are heterogeneous and are taken from the results of univariate ADF regressions using the general-to-specific criteria.

Our concern in this paper is to calculate point estimates and confidence intervals of the speed of adjustment to PPP in the panel model, rather than to focus on the statistical question of whether or not the hypothesis of unit roots in real exchange rates can be rejected. The most commonly used measure of persistence is the half-life, the expected number of years for a PPP deviation to decay by 50%, calculated by $\ln(0.5)/\ln(\alpha)$. In the univariate DF regression, α is a complete scalar measure of persistence. In the more general ADF regression, it is preferable to calculate the half-life from the impulse response function, since $\ln(0.5)/\ln(\alpha)$ assumes a monotonic rate of decay which does not necessarily occur in higher order AR models. In the panel ADF model, with or without serial correlation, the only way to calculate the half-life is from α because the lag lengths (k_i) and the serial correlation coefficients (ψ_{ij}) in Equation (5) vary across countries.[4]

[4] An exception would be the model estimated by O'Connell (1998), where the k's and the c's are homogeneous across countries. As shown by Papell and Theodoridis (2001), however, these homogeneity restrictions are not supported empirically.

As described above, we restrict the value of α, and thus the half-lives, to be equal for every country in the panel. To lend credence to our homogeneity restriction, we conducted Wald tests for the null hypothesis that $\alpha_i = \alpha$, assuming that the panel is stationary. For both methods of lag selection that we consider, we fail to reject the null hypothesis and proceed under the assumption that homogeneity is reasonable for this sample.[5]

The problem with these half-life calculations is that the least squares estimates of α are significantly downward biased for the variants of the models that contain an intercept and have a value of α fairly close to unity.[6] Andrews (1993) constructs exactly median-unbiased estimates of α. The intuition behind median-unbiased estimation is as follows. Given the least squares estimate, α_{LS}, we find the value of α such that the median of the least squares estimate is α_{LS}. This is the median-unbiased estimator of α, denoted α_{MU}. One example from Andrews' (1993) tables is particularly relevant for PPP. Suppose that the true series had α equal to 1.0, so that the real exchange rate contained a unit root and PPP did not hold. With 100 observations (the approximate length of post-1973 quarterly data), the median of the least squares estimate of α is 0.957. The implied half-life is 3.94

[5] Imbs et. al. (2004) argue that cross-sectional aggregation can impart an upward bias to half-life estimates. The estimated bias in that paper, however, is entirely due to heterogeneity of convergence rates among the sectors that comprise national price indexes, not to heterogeneity of convergence rates among the countries that comprise their panels. Choi, Mark, and Sul (2004) fail to reject the homogeneity hypothesis for α with the same panel of real exchange rates as ours, but in an AR(1) context with annual data from 1948 – 1998, and conclude that cross-country heterogeneity in α does not constitute a significant source of bias.

[6] Taylor (2001) argues that, because of time aggregation bias, least squares estimates of α are upward biased. If time aggregation were a significant source of bias, estimates of half-lives of PPP deviations using quarterly data should be systematically larger than estimates of half-lives of PPP deviations using monthly data, but that does not seem to be the case. Papell (1997), for example, reports half-lives of about 2.5 years with data of both frequencies.

years, almost exactly in the middle of Rogoff's consensus, when in fact the true half life is infinite.

As discussed in Andrews and Chen (1994), in ADF regressions, the median-unbiased estimator of α is no longer exact, but approximate. This is because the median-unbiased estimator of α depends on the true values of the ψ_j terms in Equation (3), which are unknown. They propose an iterative procedure to obtain approximately median-unbiased estimates of α, as well as ψ_1, …, ψ_k. The intuition behind obtaining the approximately median-unbiased estimate, α_{AMU}, in Equation (2) is the same as in the exactly median-unbiased case. Conditional on the least squares estimates of ψ_1, …, ψ_k, we find the value of α such that the least squares estimator has α_{LS} as its median; call this $\alpha_{1,AMU}$. Conditional on $\alpha_{1,AMU}$, we obtain a new set of estimates of ψ_1, …, ψ_k and proceed to calculate a new median-unbiased estimate of α conditional on these coefficients; call this $\alpha_{2,AMU}$. The final estimate α_{AMU} is obtained when convergence occurs.

3. Extension of Median-Unbiased Estimation Methods to Panels

In this section, we propose an *ad hoc* extension of the Andrews (1993) and Andrews and Chen (1994) univariate median-unbiased estimation techniques to the case of more than one time series. In the exactly median-unbiased case, we seek to correct for the median-bias of α in the following panel DF regression:

$$q_{it} = c_i + \alpha q_{i,t-1} + u_{it} ,$$

where time is indexed from t = 1, 2, …, T and the number of series is indexed from n = 1, 2, … N. The above panel DF regression is estimated via feasible GLS subject to the restriction that α is equal across equations. This framework allows for both serially and contemporaneously correlated errors.

As a demonstration that median-bias is a relevant consideration in panel DF regressions, we have computed α_{MU} for $T+1=100$ and $N=20$. The artificial data are generated as AR(1) processes with zero intercept[7], common α, and serially and contemporaneously uncorrelated Gaussian errors.[8] Due to the high computational cost of calculating α_{MU} in panel DF regressions, we consider α = 1.0, 0.99, 0.97, 0.95, 0.93, 0.91, 0.90, 0.85, and 0.80, although in principal any value of $\alpha \in$ (-1,1] can be considered.

In our simulations, the median function is always monotonic, and the median-unbiased estimator is therefore well defined. However, the median function can be hump-shaped, or nonmonotonic, near unity for very small values of *T*. In a univariate context, Andrews (1993) reports that the 0.95 quantile function is hump-shaped in the vicinity of unity for small values of *T* when a constant is not included in the regression. Phillips and Sul (2003) discuss this issue in a panel context. Their simulation evidence suggests that for $N \geq 5$ and $T \geq 20$ nonmontonicity of the median function is not an issue. In our subsequent empirical example, our dataset far exceeds these values of *N* and *T*, and we will therefore not have to worry about nonmonotonicity of the median function.

[7] The value of the intercept does not affect the median-bias of α.

[8] In our subsequent empirical application, we allow the errors to be serially and contemporaneously correlated.

Our exactly median-unbiased estimators, as well as 90% and 95% confidence intervals, are reported in panel A of Table 1. For comparison, we have also reported Andrews' (1993) exactly median-unbiased estimator for $T + 1 = 100$ and $N = 1$.[9]

There are three interesting comparisons to be made between our panel median-unbiased estimator and Andrews' univariate median-unbiased estimator. First, as in the univariate case, the panel estimates are biased downward with the bias more severe the

[9] Andrews (1993) only reports 90% confidence intervals.

closer α is to unity. For example, when $\alpha = 0.93$, the panel estimator has a median-bias of -0.03 while, when $\alpha = 0.80$, the median-bias is -0.004. Second, the median-bias is not quite as severe in the panel DF regression as in the univariate DF regression. For example, when $\alpha = 0.97$, Andrews' estimator has a median-bias of -0.037, whereas our panel estimator has a median-bias of -0.034. Third, the confidence intervals for our panel estimator are tighter than the confidence intervals for Andrews' univariate estimator. These features accord with intuition. Correctly imposing the restriction that α is equal across equations is information not available in a univariate context and should lead to an estimate closer to the true value. Similarly, when we estimate a system of equations and impose correct cross equation restrictions, we achieve an increase in efficiency.

Turning now to approximately median-unbiased estimation of α in panel ADF regressions, we propose an *ad hoc* extension of the univariate Andrews and Chen (1994) approximately median-unbiased estimator. Conditional on ψ_{1i}, ψ_{2i}, …, $\psi_{k_i i}$ for i = 1, 2, …, *N*, we calculate the approximately median-unbiased estimator of α in the panel ADF regression, $\alpha_{1,AMU}$. Conditional on $\alpha_{1,AMU}$ we obtain new estimates of ψ_{1i}, ψ_{2i}, …, $\psi_{k_i i}$ which we use to calculate $\alpha_{2,AMU}$. α_{AMU} is obtained when convergence occurs.

4. Empirical Results

We now turn to the data. Our data consist of 20 quarterly U.S. dollar denominated real exchange rates from 1973.1 through 1998.2.[10] While there is a large literature on lag selection in univariate unit root tests, there is little guidance on how to choose the lag

[10] These are the same data used by Murray and Papell (2002). The 20 countries are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, The Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom. We end the data in 1998.2 when the nominal exchange rates among the Euro countries became irrevocably fixed.

length in panel unit root regressions so that the resulting test has good size and power. We employ the two most commonly used methods of choosing *k*: the general-to-specific (GS) procedure of Hall (1994) and Ng and Perron (1995), and the modified Akaike information criterion (MAIC) of Ng and Perron (2001). We set the maximum lag at 12, which is the convention with quarterly data.

Our results are presented in Table 2. For the panel ADF regression with GS lag selection, the least squares estimate of α is 0.929, implying a half-life of 2.35 years. When the MAIC is used, we find a least squares estimate of 0.942, which corresponds to a half-life of 2.90 years. We have also supplemented these point estimates with parametric bootstrap confidence intervals. In the bootstrap, the covariance matrix of the errors is set to equal the covariance matrix of the least squares residuals. The 95% bootstrap confidence interval for the least squares half-life is [1.29, 2.22] years with GS lag selection, and [1.47, 2.74] years when the MAIC is employed. Taken at face value, these confidence intervals *solve the PPP puzzle*. The upper bounds are below the lower end of Rogoff's consensus, and the lower bounds are consistent with half-lives implied by models with nominal rigidities. Notice however, that the failure of the bootstrap is so severe in this case that the 95% confidence intervals lie entirely below the point estimates. This reflects the bias inherent in least squares estimates of α when the true value is close to unity. In addition, these bootstrap confidence intervals have an additional layer of bias, since they reflect biased estimates based on a biased parameterization.[11] These results suggest that the practice of bootstrapping least squares half-life estimates in panel regressions with small samples should be avoided.

We now turn to correcting for median-bias in panel ADF regressions. To allow for contemporaneous correlation, the covariance matrix of the errors in our simulations is set equal to the covariance matrix of the least squares residuals. Since our ADF regressions capture serial correlation, this framework allows for both serially and contemporaneously correlated errors. These results are also reported in Table 2. When GS lag selection is used, we find an approximately median-unbiased estimate of α of 0.933, implying a half-life of 2.50 years. The 95% confidence interval is [2.25, 2.85] years. With MAIC lag selection, the approximately median-unbiased estimate of α is 0.963, implying a half-life of 4.60 years. The 95% confidence interval in this case is [2.71, 5.33] years. The differences between these confidence intervals is of course entirely due to the chosen lag lengths. For this dataset, GS typically leads to a higher value of k than MAIC. Eight of the twenty exchange rates have values of k in the range 6 – 8. The lowest value of k chosen by GS is 3. In contrast, the MAIC results in a much more parsimonious panel. Eleven of the twenty time series have $k = 0$, and the largest value of k chosen is 5. It is not surprising that the more parsimonious panel leads to a larger estimate of the half-life. Since there is no reason to prefer one of these confidence intervals over the other, we consider the median of the two confidence intervals, which is [2.48, 4.09] years.[12] This interval is very close to Rogoff's 3 – 5 year consensus. In particular, the lower bound is

[11] Kilian (1998) discusses this issue.

[12] Since we have an 2 confidence intervals, we calculate the median lower bound as the mean of the 2 lower bounds, etc.

large enough to rule out economic models with nominal rigidities as candidates for explaining the observed behavior of real exchange rates.

We again note that the estimated half-lives, and 95% confidence intervals, are calculated directly from α, and are not based on an impulse response function. The reason for this is that k_i and ψ_{ij} are not equal across countries in the panel. As such, there is no impulse response function for the entire panel. Calculating the half-life directly from α is potentially problematic, since this assumes that shocks decay at a monotonic rate. For AR processes with $k > 1$, shocks do not necessarily decay monotonically, so calculating half-lives based on α is potentially misleading. Nevertheless, previous research has demonstrated that for this data set, there are only minor differences between calculating the half-life from $\ln(0.5)/\ln(\alpha)$ or the impulse response function. Murray and Papell (2002) report univariate median-unbiased point estimates and confidence intervals for half-lives based on α and the impulse response function for the data set used here. The results are strikingly similar. The point estimates for both methods are very close, and the confidence intervals are virtually identical. Therefore, we think that computing the half-lives based on α for this data set is not misleading.

To quantify the benefit of working in a panel context, we compare our results to their univariate counterparts. For the same dataset, Murray and Papell (2002) compute univariate half-lives using GS lag selection. When the half-life is computed directly from α, their median half-life confidence interval is $[0.74, \infty)$ years. We have also redone Murray and Papell's (2002) univariate approximately median-unbiased estimates (their

Table 8) using the MAIC. We report these results in Table 3. The median 95% confidence interval when k is chosen by MAIC is $[1.36, \infty)$ years. Both of these confidence intervals are so wide that they are completely uninformative. The lower bounds of about 1 year are consistent with a rate of convergence to PPP that is predicted by models with nominal rigidities, and the upper bounds are consistent with the failure of PPP to hold in the very long run.

Why are our panel confidence intervals so much tighter than their univariate counterparts? We can think of two potential explanations. First, panel tests have higher power than univariate tests because they exploit cross-sectional, as well as time series, variability. Second, panel tests exploit the information contained in the contemporaneous correlation of the real exchange rates. We investigate these explanations by conducting our panel median-unbiased simulations with contemporaneously uncorrelated errors. The median-unbiased half-life intervals with uncorrelated errors are [2.66, 8.58] years and [3.60, 10.74] years for GS and MAIC respectively. While these confidence intervals are narrower than their univariate counterparts reported in Table 3, they are wider than the confidence intervals with contemporaneous correlation reported in Table 2. The gains in precision appear to come from both increased power and allowing the errors to be contemporaneously correlated.

5. Summary and Concluding Remarks

By exploiting cross-sectional as well as time series variability, panel methods offer the promise of sharpening the evidence of purchasing power parity over the post-Bretton Woods flexible exchange rate period. The purpose of this paper is to evaluate the evidence that these methods help solve the “Purchasing Power Parity Puzzle” by shortening estimates of half-lives of PPP deviations.

The focus of this paper is on the downward bias that least squares methods impart to half-life estimates. We extend the median-unbiased estimation methods of Andrews (1993) and Andrews and Chen (1994) to the panel context. We show that, in general, panel methods are subject to the same bias problems as univariate methods and, in particular, that these biases influence half-life estimates of PPP deviations. For panel ADF regressions that allow for serially and contemporaneously correlated errors, the bias in the least squares estimates is so severe that the 95% bootstrap confidence intervals for the half-life lie entirely below the point estimates.

Using approximately median-unbiased methods, the median 95% panel ADF confidence interval for the half-life is [2.48, 4.09] years, close to Rogoff’s 3 – 5 year consensus. It is worth remembering that Rogoff’s survey primarily consisted of studies applying univariate methods to long-horizon data. Using panel methods and post-1973 data, we provide strong confirmation of his evidence of long half-lives of PPP deviations. However, panels do not help solve the PPP puzzle. Even the median lower bound of our 95% confidence intervals, 2.48 years, is, in Rogoff’s words, “seemingly far too long to be explained by nominal rigidities.”

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Table 1a. Panel Median-Unbiased Estimator: N=20, T+1=100.

$$q_{it} = c_i + \alpha q_{i,t-1} + u_{it}$$

α/Quantile	0.025	0.05	0.50	0.95	0.975
1.0	0.948	0.951	0.966	0.977	0.979
0.99	0.935	0.938	0.953	0.966	0.967
0.97	0.916	0.919	0.936	0.949	0.951
0.93	0.876	0.881	0.900	0.918	0.921
0.90	0.846	0.851	0.873	0.892	0.897
0.85	0.795	0.800	0.825	0.849	0.853
0.80	0.745	0.750	0.776	0.801	0.805

Table 1b. Andrews (1993) Univariate Median-Unbiased Estimator: T+1=100.

$$q_t = c + \alpha q_{t-1} + u_t$$

α/Quantile	0.025	0.05	0.50	0.95	0.975
1.0	n.a.	0.863	0.957	0.999	n.a.
0.99	n.a.	0.854	0.950	0.994	n.a.
0.97	n.a.	0.834	0.933	0.981	n.a.
0.93	n.a.	0.788	0.897	0.956	n.a.
0.90	n.a.	0.754	0.869	0.936	n.a.
0.85	n.a.	0.697	0.821	0.900	n.a.
0.80	n.a.	0.641	0.773	0.862	n.a.

Table 2. Half-Lives in Panel Unit Root Regressions
Quarterly Data: 1973:1-1998:2

Lag Selection	α_{LS}	95% CI	HL_{LS}	95% CI	α_{MU}	95% CI	HL_{MU}	95% CI
GS	0.929	[0.874, 0.925]	2.35	[1.29, 2.22]	0.933	[0.926, 0.941]	2.50	[2.25,2.85]
MAIC	0.942	[0.889, 0.939]	2.90	[1.47, 2.74]	0.963	[0.938, 0.968]	4.60	[2.71, 5.33]

Table 3. Approximately Median Unbiased Half-Lives in Augmented Dickey-Fuller Regressions. Quarterly Data: 1973:1-1998:2

$$q_t = c + \alpha q_{t-1} + \sum_{i=1}^{k} \psi_i \Delta q_{t-i} + u_t$$

Country	k_{MAIC}	α_{LS}	HL_{LS}	α_{MU}	95% CI	HL_{MU}	95% CI
Australia	0	0.951	3.45	0.99	[0.91, 1.0]	17.24	[1.84, ∞)
Austria	0	0.929	2.35	0.97	[0.88, 1.0]	5.67	[1.36, ∞)
Belgium	3	0.932	2.46	0.96	[0.88, 1.0]	4.24	[1.36, ∞)
Canada	3	0.974	6.58	1.0	[0.95, 1.0]	∞	[3.38, ∞)
Denmark	3	0.923	2.16	0.95	[0.87, 1.0]	3.38	[1.24, ∞)
Finland	5	0.915	1.95	0.94	[0.86, 1.0]	2.80	[1.15, ∞)
France	0	0.932	2.46	0.97	[0.88, 1.0]	5.67	[1.36, ∞)
Germany	0	0.929	2.35	0.97	[0.88, 1.0]	5.67	[1.36, ∞)
Greece	5	0.928	2.32	0.96	[0.87, 1.0]	4.24	[1.24, ∞)
Ireland	0	0.897	1.59	0.93	[0.83, 1.0]	2.39	[0.93, ∞)
Italy	0	0.930	2.39	0.97	[0.88, 1.0]	5.67	[1.36, ∞)
Japan	0	0.955	3.76	1.0	[0.92, 1.0]	∞	[2.08, ∞)
Netherlands	0	0.925	2.22	0.96	[0.87, 1.0]	4.24	[1.24, ∞)
New Zealand	0	0.926	2.25	0.96	[0.87, 1.0]	4.24	[1.24, ∞)
Norway	0	0.912	1.88	0.95	[0.85, 1.0]	3.38	[1.07, ∞)
Portugal	0	0.956	3.85	1.0	[0.92, 1.0]	∞	[2.08, ∞)
Spain	1	0.946	3.12	0.98	[0.91, 1.0]	8.58	[1.84, ∞)
Sweden	4	0.932	2.46	0.96	[0.88, 1.0]	4.24	[1.36, ∞)
Switzerland	1	0.898	1.61	0.95	[0.83, 1.0]	3.38	[0.93, ∞)
United Kingdom	0	0.919	2.05	0.96	[0.86, 1.0]	4.24	[1.15, ∞)