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PRICE INDEX CONVERGENCE AMONG UNITED STATES CITIES*

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We study the dynamics of price indices for major U.S. cities using panel econometric methods and find that relative price levels among cities mean revert at an exceptionally slow rate. In a panel of 19 cities from 1918 to 1995, we estimate the half-life of convergence to be approximately nine years. The surprisingly slow rate of convergence can be explained by a combination of the presence of transportation costs, differential speeds of adjustment to small and large shocks, and the inclusion of nontraded goods prices in the overall price index.

1. INTRODUCTION

Do price indices in major U.S. cities share a common trend, and if so, how quickly do they revert to that trend following a local shock to the price index? To answer this question, we study the dynamics of consumer price indices for 19 major U.S. cities over the period from 1918 to 1995. The panel time-series methods we employ are now commonly used for studying real output growth rates and levels of real exchange rates across countries. We estimate that price index divergences across U.S. cities are temporary, but surprisingly persistent, with a half-life of nearly nine years.

Our research has two primary motivations. First, we hope to gain a better understanding of the sources of persistence in the deviations from purchasing power parity (PPP) found in studies of national price indices and exchange-rate data. Second, and more importantly, we see the European Monetary Union as having many similarities to the United States, and believe that studying the behavior of prices across U.S. cities will help us in understanding the likely nature of inflation convergence in the Euro area. The European Central Bank's stated inflation objective is a year-on-year change in the Harmonized Index of Consumer Prices (HICP) of not more than 2%. But how large might we expect regional deviations from this Euro-area-wide average to be, and how long are they likely to persist? The lack of data prevents us from answering this question directly using European prices under monetary union. Instead, we look to the United States, a mature common currency area of similar regional diversity, size, and industrial development,

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to estimate the degree of relative price dispersion and the rate of convergence that we expect to see within the Euro area.

The primary antecedents to our work are found in the literature comparing price movements across international borders. When examined over the post-1973 period of floating exchange rates, pairwise comparisons of countries using univariate methods typically do not reject the hypothesis that deviations from PPP contain a unit root.[2] This result implies that inflation differentials between countries, measured in terms of a common currency, can persist indefinitely or, equivalently, that the common currency price level in one country can deviate from that in another by an arbitrarily large amount. Recently, researchers employing multivariate tests that combine numerous countries in panel unit-root testing procedures have rejected the unit-root hypothesis, implying that relative prices revert to a common mean. However, the rate at which this mean reversion occurs is evidently quite slow. Consensus estimates of the half-life of a deviation from PPP range between four and five years (Abuaf and Jorion, 1990; Frankel and Rose, 1996; Wu, 1996; MacDonald, 1996; Papell, 1997; Lothian, 1997; Wei and Parsley, 1995).[3] This finding leads us to our first question: To what extent do these international results hold for regions within a common currency area? Our prior expectation is that we would observe more rapid price convergence across regions within a single country than across countries, since within-country markets for products, labor, and capital are presumably better integrated.

International PPP researchers have suggested several explanations for incomplete relative price-level adjustment. These include: (i) trade barriers, such as tariffs and quotas; (ii) nontariff barriers, including the bureaucratic difficulties in establishing foreign distribution systems for traded goods; (iii) the failure of nominal exchange rates to adjust to relative price-level shocks; (iv) firms exercising local monopoly power through differential pricing to segmented markets; (v) sticky nominal price-level adjustment arising from imperfectly competitive product markets where price changes are costly; (vi) transportation costs associated with moving goods from one region to another; and (vii) the presence of nontraded goods in the general price level and the potential for differential growth in the level and efficiency of factors used in their production.[4]

Each of these factors can be thought of as creating permanent deviations from PPP, influencing transitional dynamics, or both. For example, tariffs will drive a wedge between prices in different regions. But in the absence of any other

[2] For excellent surveys on the literature up through the early 1990s, see Bruer (1994) and Froot and Rogoff (1995).

[3] The effect of sticky nominal price adjustment as suggested by Dornbusch (1976) or Taylor (1979) should result in half-lives of a year or so, not the four- to five-year consensus estimate from international data.

[4] Wei and Parsley (1995) find that deviations from PPP are positively related to nominal exchange-rate volatility (item iii), Betts and Devereux (1996) study the implications of pricing to market (item iv), Mussa (1986) and Engel (1993) attribute the higher volatility of real exchange-rate changes during the float to sticky price adjustment (item v), O'Connell and Wei (1997) and Papell and Theodoridis (1997) study the role of transportation costs using distance as a proxy measure (item vi), Chinn (1997), Kakkar and Ogaki (1994), and Canzoneri et al. (1999) examine the implications of the Balassa–Samuelson hypothesis.

factors, and assuming that the tariff does not change, the relative price of goods in the regions will not change. The presence of nontraded goods, on the other hand, may generate deviations from PPP that are long-lasting, as differential improvements in the technology of producing traded and nontraded goods will lead to real exchange-rate movements that can only be erased by movements in labor and capital from one region to another. By analogy, transportation costs will both allow relative prices to differ and affect the rate at which they are observed to converge. Adjacent regions, with low costs of moving goods between them, will be more likely to adjust quickly to a given relative price disturbance than regions that are far apart.

Attempts to disentangle the marginal effects of each of the seven broad explanations for deviations from PPP have posed a challenge. Some combination of all of the above-mentioned factors is likely to be responsible for impeding adjustment toward PPP, as it seems improbable that any one element in isolation is sufficiently important to explain the slow convergence.

The study of relative price indices for cities within a common currency and trade area provides us with a type of natural experiment in which the impact of many of these explanations is attenuated. Specifically, when examining the movements in relative prices say between Chicago and Detroit, tariff, nontariff, and nominal exchange-rate effects are surely minimized as explanations for persistence. The remaining factors are more difficult to rule out: The role of pricing-to-market remains to the extent that transportation costs prohibit effective arbitrage across regions, sticky price adjustment can be important if adjustment speeds vary across regions, and biased technological growth combined with the presence of nontraded goods may also slow convergence.

Our work is closest to Parsley and Wei (1996) and Engel and Rogers (1997). Both examine violations of the law of one price within the United States using consumer price data. There are, however, significant differences between their studies and ours. First, while Parsley and Wei do examine the dynamic convergence of prices among cities, their data span the relatively short period from 1975 to 1992, whereas our data span a long historical period that begins in 1918. Engel and Rogers use data from 1986 to 1994; they do not study its dynamic properties. A second major difference is our focus on the behavior of aggregate price indices, which contain a broader coverage of goods and services sold in various locations. It is this aspect of our work that makes the results applicable to the problems faced by monetary policy makers, whose attention is generally focused on measures of aggregate inflation, with less emphasis on the behavior of the price of individual commodities. This is surely the case of the set of countries that target consumer price inflation measures explicitly, as well as the European Central Bank, with its focus on the HICP.[5]

To summarize our main results, using panel data procedures, we find that relative prices do converge to a common trend, and we are able to reject the presence of

[5] Our focus on U.S. data has the added advantage that, in the spirit of the methods used to construct the HICP, the consumer price measures are based on the same basket of goods across regions. This is in contrast to international comparisons of national consumer price data.

a unit root.[6] Using the full 78-year sample from 1918 to 1995, and assuming that relative price levels contain no deterministic trend, we estimate the half-life of convergence to be approximately 9 years. One might expect that this result could be a consequence of relatively low factor mobility in the pre-World War II period, suggesting that the convergence rate should be more rapid in the more recent sample, but we find no indication that the convergence rate has changed over time. Annual inflation rates measured over 10-year intervals can differ by as much as 1.6 percentage points. While differentials of this size may not seem large by current international standards, the real interest rate differentials they create within a common currency zone could have a substantial impact on resource allocations.

What is responsible for the slow convergence? We examine three hypotheses: the role of distance, nonlinear adjustment leading to slower adjustment to small shocks than to large ones, and the inclusion of nontraded goods prices in the general price index. As for distance, our point estimates suggest that convergence is faster between cities that are closer together, but the effects are both small in magnitude and statistically insignificant. We find no evidence that adjustment is faster when shocks are large.

As for the presence of nontraded goods prices in the general price index, we study their role by looking at price behavior of commodities and services separately. Using 30 years of available data on 14 of the 19 cities, we find that commodities and services prices converge to the cross-sectional average. We obtain only fragmentary evidence that shocks affecting service prices die out more slowly than those hitting commodity prices, suggesting that the slow convergence in overall price indices is a consequence of the difficulty in trading some goods.

The remainder of the article is divided into four sections. Section 2 describes the data and presents some descriptive statistics. Section 3 reports the main empirical findings, including univariate and multivariate time-series results based on unit-root tests, as well as estimates of the convergence rates. In Section 4, we examine the importance of distance, nonlinear adjustment, and the role of nontraded goods in the price index in explaining slow convergence. Section 5 concludes with a discussion of the implications of our findings for the European Central Bank.

2. THE DATA AND DESCRIPTIVE STATISTICS

Our primary dataset is a panel of annual observations on the consumer price index (CPI) for 19 cities over the period 1918–1995.[7] These data were obtained from the BLS and are the basis for the construction of the national CPI.

We begin with a very preliminary and coarse examination of these data. The results in Table 1 are based on annualized inflation rates calculated for seven

[6] As in the international literature, we found that standard univariate testing procedures generally are unable to reject the hypothesis that the log real exchange rate between pairs of U.S. cities is characterized by a process with a unit root.

[7] The cities in the sample are Atlanta, Baltimore, Boston, Chicago, Cincinnati, Cleveland, Detroit, Houston, Kansas City, Los Angeles, Minneapolis, New York City, Philadelphia, Pittsburgh, Portland, San Francisco, Seattle, St. Louis, and Washington D.C. The regular publication of the CPI began in 1921. Observations for preceding years were estimated by the Bureau of Labor Statistics (BLS).

TABLE 1

SELECTED ANNUAL INFLATION RATES

Sample	Maximum	City	Minimum	City	Differential
1926:1935	−1.70	Washington D.C.	−3.25	Los Angeles	1.55
1936:1945	3.44	Portland	2.25	Boston	1.20
1946:1955	4.52	Chicago	3.60	New York City	0.92
1956:1965	2.13	San Francisco	1.19	Detroit	0.94
1966:1975	5.69	New York City	4.98	Los Angeles	0.71
1976:1985	7.64	Cleveland	6.35	New York City	1.29
1986:1995	4.00	New York City	2.87	Houston	1.13
1936:1955	3.96	Seattle	3.41	Boston	0.55
1956:1975	4.11	New York City	3.54	Chicago	0.56
1976:1995	5.76	Seattle	5.15	Houston	0.61

NOTE: Highest and lowest average inflation during each sample period.

nonoverlapping 10-year periods, beginning in 1926, computed for each of the 19 cities. We report the highest and lowest average annual inflation for each 10-year interval, as well as the differential. For example, from 1986 to 1995, New York City's inflation of 4.00% per year on average was the highest in the sample, while Houston's average annual inflation of 2.87% was the lowest. The differential was 1.13 percentage points per year on average. As one might expect, these differentials become smaller when we lengthen the horizon from 10 to 20 years.

We draw several conclusions from these results. First, inflation differentials of one percentage point per year can persist over 10-year periods – a seemingly long period of time. But even this very crude look at the data suggests that these differences reverse themselves, as New York City's high inflation from 1986 to 1995 is preceded by relatively low inflation in the previous decade. These reversals suggest that the differentials die out, but on a decadal time scale. Second, the average difference between the city with the highest and the lowest inflation is 1.11 percentage points, and there is relatively little variation from the 1920s to the 1990s. This is the first indication that there has been little change in the dynamics of adjustment over the 70-plus years of the sample. Increasing the time span from 10 to 20 years, and looking at three nonoverlapping intervals, the average differential drops nearly in half to 0.57 percentage points annually, again suggesting very slow adjustment.

These intercity inflation differentials, which are analogous to percentage changes in international real exchange rates, are of the same order of magnitude as real exchange-rate adjustments within Europe. For example, Canzoneri et al. (1998) report that annual changes of real exchange rates relative to the German deutschemark between 1973 and 1991 range from 0.1 percentage points per year for Belgium to −2.0 percentage points per year for Italy. Over this same period, the maximum average inflation differential across U.S. cities was 0.52 percentage points per year. Furthermore, as noted in the October 1999 *ECB Bulletin*, the size of inflation differentials across the Euro area during 1999 was "around 2 percentage points between the highest and lowest rate of HICP increase"(p. 36).

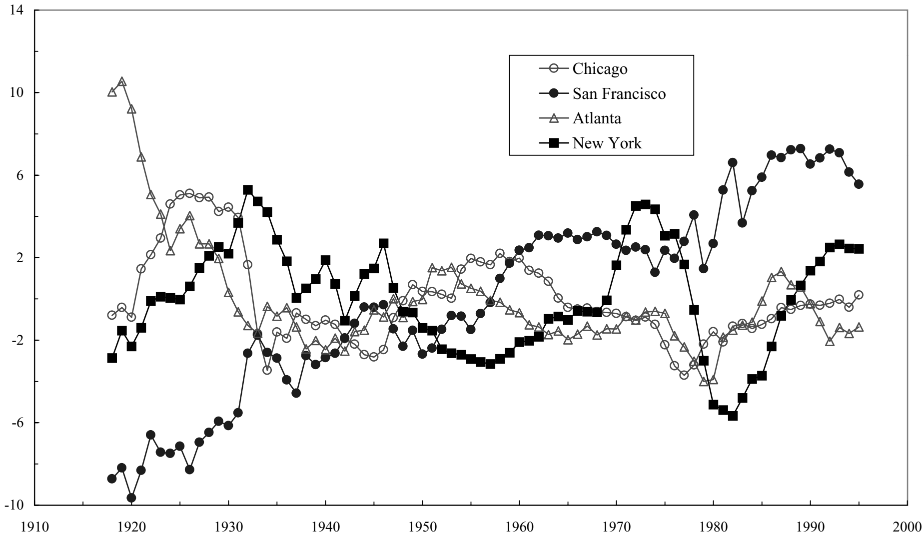

FIGURE 1

LOG PRICE INDICES, RELATIVE TO CROSS-SECTIONAL AVERAGE

Next, we plot the data to give a graphical impression of the convergence in relative prices. To do this, we need some sort of base. To foreshadow the more detailed work in the next section, we compute the log CPI in each city relative to the cross-sectional mean. Figure 1 displays the deviations from this mean of the log price in Chicago, San Francisco, Atlanta, and New York.

The impression one gets from the figure is that deviations from PPP between U.S. cities are at least as persistent as those observed between nations. Beginning with Chicago and New York City, cumulative deviations in excess of five percentage points are common, and appear to occur in cycles lasting on the order of 10 years. San Francisco's experience suggests the possibility of cycles around an upward trend, as its log price index shows no tendency to revert to the common mean.

This preliminary examination of the data suggests that U.S. intercity real exchange rates exhibit significant movements that persist for many years. We now proceed with a detailed examination of their time-series properties.

3. ECONOMETRIC ANALYSIS

The purpose of the analysis of this section is to study two properties of the city price data. First we are interested in whether or not relative prices between cities are unit-root processes. That is to say, we ask whether the real exchange rates between cities contain a stochastic trend, or unit root, under which they will diverge from one another. The alternative hypothesis in our statistical tests is that the level of relative prices in various cities converges to a steady-state value in the long run.

Univariate unit-root tests of the type pioneered by Dickey and Fuller are known to have low power in certain circumstances—it is often difficult to reject the unit-root null when it is in fact false. One way that researchers have confronted this problem has been to exploit the panel dimension of data available in certain applications. We employ two panel procedures: one due to Levin and Lin (1993) [LL] and the second suggested by Im et al. (1997) [IPS]. The two procedures are described in the Appendix.

We examine the following characterization of the data:

$$\Delta q_{i,t} = \alpha_i + \theta_t + \beta_i q_{i,t-1} + \sum_{j=1}^{k_i} \gamma_{ij} \Delta q_{i,t-j} + \epsilon_{i,t} \tag{1}$$

where $q_{i,t}$ is the log-price level of city i at time t, α_i is a city-specific constant to control for non-time-dependent heterogeneity across cities, and θ_t is a common time effect. The γ_{ij}s are lag coefficients in the process characterizing $q_{i,t}$, $\beta_i \equiv \rho_i - 1$, and $\rho_i \equiv \sum_{j=1}^{k_i} \gamma_{ij}$. The approximate half-life of a shock to $q_{i,t}$ is computed as $-\ln(2)/\ln(\rho_i)$.[8]

It is important to include fixed effects in a panel setting. The variation of α_i across cities allows us to control both for deviations from PPP in the base year for the price index as well as for possible heterogeneity arising from differing income levels and sales taxes that lead to permanent differences in relative prices across cities. The common time effect, θ_t, captures the influence of macroeconomic shocks that induce cross-sectional dependence in real exchange rates. We also note that it is unnecessary for us to select a numeraire city, since any movements in a numeraire price index are absorbed into the common time effect.[9]

Our primary interest is in the β_is, the coefficients on the lagged log of the price index, q_{it}. The closer β_i is to zero, the longer is the estimated half-life of a shock. The null hypothesis in both of the test procedures we employ is formulated such that each series contains a unit root, $H_0 : \beta_i = 0$ for all i. Where the LL and IPS tests differ is in their treatment of β_i under the alternative hypothesis. In the LL test, the alternative is $H_a : \beta_i = \beta < 0$, whereas in the IPS test the alternative permits heterogeneity across the individuals, with $H_a : \beta_i < 0$ for at least one i. Bowman (1998) and Maddala and Wu (1999) find that the IPS test has more power than LL. The LL procedure, on the other hand, has the advantage of providing us with a panel estimate of ρ, while the IPS procedure does not.

The asymptotic distributions derived by LL and IPS for their test statistics assume that the error term is independent across individuals and time. Our strategy of including common time effects can account for the cross-sectional dependence

[8] Estimation of (1) requires a choice for k_i, which we determine by Campbell and Perron's (1991) top-down t-test approach. We start with $k_i = 6$, estimate Equation (1), and then, if the absolute value of the t-ratio for $\hat{\gamma}_{i6}$ is less than 1.96, we reset $k_i = 5$ and reestimate the equation. The process is repeated until the t-ratio of the estimated coefficient with the longest lag exceeds 1.96.

[9] Panel analyses of international PPP cannot get away from the numeraire problem because national real exchange rates all require the use of the nominal exchange rate in their construction. Papell and Theodiridis (1997) show how international tests of PPP are dependent on the choice of the numeraire currency.

TABLE 2
PANEL UNIT-ROOT TEST RESULTS

Sample	τ	p-value	$\hat{\rho}$	Adjusted $\hat{\rho}$	Adjusted half-life
		A. Levin and Lin			
Full (1918–1995)	−11.518	0.000	0.894	0.922	8.535
1918–1955	−8.314	0.137	0.884	0.950	13.513
1956–1995	−10.812	0.002	0.858	0.916	7.900
1936–1955	−9.663	0.127	0.790	0.912	7.525
1956–1975	−8.532	0.002	0.848	0.987	52.972
1976–1995	−9.789	0.021	0.800	0.925	8.891
Sample	$\bar{t}$	p-value	$\hat{\bar{\rho}}$	Adjusted $\hat{\bar{\rho}}$	Adjusted half-life
		B. Im, Pesaran, and Shin			
Full (1918–1995)	−2.686	0.000	0.883	0.931	9.695
1918–1955	−1.927	0.147	0.859	0.959	16.557
1956–1995	−2.440	0.007	0.837	0.917	8.000
1936–1955	−2.177	0.117	0.729	0.868	4.896
1956–1975	−2.024	0.005	0.785	0.930	9.551
1976–1995	−2.131	0.043	0.774	0.918	8.101

NOTES: Panel unit-root tests and estimates of convergence rates for the log-price level of the 19 U.S. cities. The methods are described in the text and the Appendix.

only asymptotically, as the off-diagonal elements of the residual covariance matrix of the panel system, that is the $E(\epsilon_i \epsilon_j)$ for $i \neq j$, are of $O(N^{-1})$. To control for residual dependence across cities, we calculate p-values of the LL and IPS test statistics from a parametric bootstrap consisting of 2000 replications using the estimated error-covariance matrix in the data-generating process.[10]

Table 2 displays the results of the LL and IPS tests. We examine both the full sample and a number of subsamples. We omit a deterministic trend as being inconsistent with the PPP hypothesis we wish to examine.[11] Overall, the tests allow us to reject the unit-root null in a vast majority of the cases. That is to say, regardless of the procedure or the sample period, there is very little evidence of a stochastic trend in the city price data.

Having obtained evidence that relative prices converge across cities, we are now interested in the speed of convergence based on the persistence parameters: the ρ_i. Since the LL model is based on restricting ρ_i to be equal across all cities, we simply report the estimated value. For the IPS model, ρ_i differs across cities, and so we report results based on the average across i. Since the estimated serial correlation

[10] O'Connell (1998) suggests a generalized least squares estimator by adopting a parametric model of the cross-sectional dependence. That procedure also requires the serial correlation across individuals to be homogeneous ($k_i = k$) for all i, which is not true in our data.

[11] We replicated the results in Table 2 for the case in which a deterministic trend was included in the specification. As one would expect, allowing for a trend in the real exchange rates between cities reduces the estimated half-lives substantially. When the trend is included, the estimates fall by more than half, to between two and four years. The problem with these results is that there is no economic basis for expecting real exchange rates to trend over long periods.

coefficient is biased down in small samples, we bias-adjust the panel estimates of ρ using the formula suggested by Nickell (1981).[12] We label the resulting estimate as "adjusted $\hat{\rho}$." For the IPS procedure, we compute the average of the bias-adjusted $\hat{\rho}_i$s, which we denote "adjusted $\hat{\bar{\rho}}$."

From the adjusted $\hat{\rho}$ and the adjusted $\hat{\bar{\rho}}$, we compute the adjusted half-life of divergences from PPP for cities in our sample. The results are reported in the far right column of Table 2. Beginning with the full-sample estimates, we find that the half-life to convergence is estimated to be in the neighborhood of 9 years – 8.5 years using LL and 9.7 years using IPS.

In our subsample analysis, we examine 20-year subperiods extending from 1936 to 1955, 1956 to 1975, and 1976 to 1995. We continue to reject the unit-root null in most cases. The pattern of the adjusted half-life estimates is somewhat puzzling, however. One would expect that convergence rates would be faster in more recent years than in the pre-World War II period, but the data do not show a clear pattern – the estimated adjusted half-lives do not decline as the sample moves closer to the present. This may reflect the fact that the United States has been a common currency area for two centuries with relatively high factor mobility.

Looking at the details of some of the results, we see that the point estimates of ρ are quite large during 1956–1975. The implied half-life of convergence for the most recent period is between eight and nine years, approximately the same as both the full sample and the earlier period. This last period corresponds roughly to the period studied in international PPP studies and the Parsley and Wei study, where estimated half-lives of two to four years are the norm.

To summarize our results, regardless of the econometric method, we strongly reject the hypothesis that all real exchange rates between the U.S. cities in our sample contain a unit root. While relative price levels are stationary, their deviations are very persistent. We estimate half-lives to convergence of approximately nine years. It is interesting to ask why these estimates are so large. In the next section we pursue this line of inquiry.

4. INVESTIGATING EXPLANATIONS FOR SLOW CONVERGENCE

We now move on to try to uncover explanations for why convergence may be so slow. In this section, we study the role of distance, asymmetric adjustment to large and small deviations, and the role of nontraded goods prices in the CPI as possible explanations for the persistence in PPP deviations.

4.1. *Distance.*

We follow Engel and Rogers (1996) and Parsley and Wei (1996) by using distance to proxy for unobservable transportation costs. Table 3 reports the results of several cross-sectional regressions in which the independent variable is either the logarithm of distance between city "*i*" and the numeraire city of Chicago or the double log of distance.

[12] Nickell's formula is, $\text{plim}_{N\to\infty}(\hat{\rho}-\rho) = (A_T B_T)/C_T$, where $A_T = -(1+\rho)/(T-1)$, $B_T = 1 - (1/T)(1-\rho^T)/(1-\rho)$, and $C_T = 1 - 2\rho(1-B_T)/[(1-\rho)(T-1)]$. Canzoneri et al. (1999) perform a small Monte Carlo experiment from which they determined that Nickell's adjustment is reasonably accurate.

TABLE 3
DISTANCE AS AN EXPLANATORY VARIABLE

Dependent Variable	Regressor ln (distance)	$\bar{R}^2$	ln (ln(distance))	$\bar{R}^2$
$V(q)$	0.647	0.164	4.254	0.158
	(2.080)		(2.049)	
$V(\Delta q)$	0.827	0.084	0.541	0.080
	(1.601)		(1.571)	
$\hat{\rho}$	3.18×10^{-4}	−0.062	−0.005	−0.062
	(0.013)		(−0.031)	
τ	−0.149	−0.042	−1.077	−0.038
	(−0.557)		(−0.069)	
half-life	1.061	−0.002	6.653	−0.009
	(0.979)		(0.921)	

NOTES: $V(q)$ = volatility of log real exchange rate relative to Chicago in percent per annum, $V(\Delta q)$ = volatility of annual percent change in log real exchange rate, $\hat{\rho}$, τ, and half-life are estimated ρ, studentized coefficient, and implied half-life from univariate ADF regressions. T-ratios in parentheses.

The dependent variable in the first regression is the volatility of the log real exchange rate, denoted $V(q)$, which is measured as the time-series sample standard deviation of q_{it}. As in Engel–Rogers and Parsley–Wei, we find that locations that are farther apart exhibit statistically significantly higher volatility in the log of their relative price levels. The point estimates of the slope coefficient in regressions of the volatility of the log relative price on our measures of distance are positive and statistically significantly different from zero (at the 5% level). The point estimate implies that the New York–Chicago real exchange rate will be approximately 0.65 percentage points more volatile per annum than the St. Louis–Chicago real exchange rate because New York is approximately 2.718 times farther from Chicago than is St. Louis, and $\ln(2.718) = 1$.

Is there any evidence that real exchange-rate adjustment is impeded by distance? To examine this question, we regress alternative measures of real exchange-rate persistence – our univariate estimates of the persistence parameter (ρ), the t-ratio associated with the persistence parameter (τ), and the implied half-lives toward convergence – on the measures of distance. The estimated slope coefficients from these regressions indicate that convergence is indeed slower between cities of greater spatial separation, but the estimates are not statistically significant.

4.2. *Differential Adjustment following Small and Large Deviations.* The point estimates from Section 2 are consistent with the hypothesis that proportional transportation costs induce a neutral band within which the log relative price between two locations can fluctuate without generating unexploited arbitrage opportunities. This would mean that when deviations from PPP are sufficiently large to move outside of the bands, adjustment will be rapid.

TABLE 4
NONLINEAR ADJUSTMENT

Threshold (in percent)	(Small) $\tilde{\rho}_s$	(Large) $\tilde{\rho}_\ell$	Wald Statistic	p-value
10	0.897	0.893	0.493	0.483
20	0.887	0.888	0.001	0.973
30	0.877	0.890	0.280	0.597

NOTES: Panel estimates of asymmetric response of relative price indices to large and small deviations from PPP, using the Levin and Lin procedure. Large deviations are defined respectively as the largest 10%, 20%, and 30% of observations in absolute value. p-value is for the Wald test of the hypothesis $\tilde{\rho}_s = \tilde{\rho}_\ell$.

To investigate the possible nonlinearity of the adjustment process, we employ a modified LL panel regression in which the lagged level of the real exchange rate (the regressor) is stratified by size into two groups – small and large.[13] We first consider the deviation from PPP to be large if it is among the largest 10% of observations in absolute value. The LL regression is then estimated on these "small" and "large" observations. We also run the regression using 20% and 30% as the threshold to determine small and large observations. The results are reported in Table 4.

As can be seen, the panel point estimates produce virtually no evidence of asymmetric adjustment.

4.3. *Nontraded Goods in the Price Index.* Perhaps the most compelling explanation for our finding of slow intercity relative price adjustment is the presence of nontraded-goods prices in the price indices we employ. If the price level of city i is represented as a geometrically weighted average of the price of traded goods and nontraded goods, the log real exchange rate can be expressed as

$$q_{it} = (1-\phi)\ln\left(\frac{P_{it}^T}{P_{0t}^T}\right) + \phi\ln\left(\frac{P_{it}^N}{P_{0t}^N}\right) \tag{2}$$

where P_{it}^T is city i's price of traded goods, P_{it}^N is city i's price of nontraded goods, and ϕ is the share of nontraded goods in the overall price level, which for simplicity is assumed to be homogeneous across cities. The empirical analysis controls for a common time effect, equivalent to θ_t in Equation (1), and so again we are not required to specify a numeraire city per se.

If PPP holds for traded goods, the first term in (2) is $I(0)$. Nonstationarity, or high persistence in the relative price of nontradables across cities, causes similar behavior in the log real exchange rate.

[13] Our method is admittedly ad hoc, and it might be preferable to let the data inform us as to whether a particular deviation is large or small. This is done in O'Connell and Wei (1997) and Taylor and Peel (1998), who apply threshold autoregression models in their investigations of nonlinearities in real exchange-rate adjustment.

TABLE 5

SELECTED ANNUAL INFLATION RATES IN TRADED AND NONTRADED GOODS

Sample	Maximum	City	Minimum	City	Differential
All Items Consumer Price Inflation					
1976:1985(10 years)	7.64	Cleveland	6.35	New York	1.29
1986:1995(10 years)	4.00	New York City	2.87	Houston	1.13
1967:1995(29 years)	5.56	Cleveland	5.22	St. Louis	0.34
Traded-Goods (Commodity) Price Inflation ($\Delta \ln P^T$)					
1976:1985(10 years)	6.56	Dallas	5.50	Philadelphia	1.06
1986:1995(10 years)	2.85	New York City	2.31	Houston	0.53
1967:1995(29 years)	4.85	Baltimore	4.43	Detroit	0.42
Nontraded-Goods (Service) Price Inflation ($\Delta \ln P^N$)					
1976:1985(10 years)	9.38	Cleveland	7.37	New York City	2.01
1986:1995(10 years)	4.87	New York City	3.32	Dallas	1.55
1967:1995(29 years)	6.56	Cleveland	5.98	St. Louis	0.58

NOTE: Highest and lowest average inflation during each sample period.

In order to analyze the role of nontraded-goods prices in the price level we examine the components of the real exchange rate in Equation (2) using a BLS price series on services as our measure of nontraded-goods prices, and a similar price series on commodities as our measure of traded-goods prices. Unfortunately, these are only available for 14 cities beginning in 1966, and so we restrict the remainder of our analysis to this reduced sample.[14]

As we did in Sections 1 and 2, we present both descriptive information on the inflation divergence within our sample and statistical evidence on the stationarity and speed of convergence for the various price series. Table 5 presents information analogous to that in Table 1 for the all-items CPI, traded and nontraded goods inflation, and the traded/nontraded goods relative price. We note several key features of the data. First, as was the case with the longer time series, the inflation differences are again quite large. Even for traded goods, inflation differences are as high as an average one percentage point per year for a decade. For nontraded-goods prices, the differences are even larger, rising to as high as two percentage points per year on average for 10 years. Given the presumed high degree of labor and capital mobility in the United States, these divergences strike us as extremely large.

The data are, however, generally consistent with the hypothesis that inflation is converging, albeit slowly. The 29-year samples show maximum differences of one-half to one-third of those during the 10-year period. Looking further, we see that nontraded-goods price inflation has larger divergences than both traded-goods price inflation and the all-items CPI. This is as we would expect. The only anomaly in the table is that the full-sample maximum difference for traded-goods price inflation exceeds that for the overall index. Over the 1967 to 1995 sample the

[14] These cities are Chicago, New York, Philadelphia, Boston, Pittsburgh, Detroit, St. Louis, Cleveland, Washington D.C., Dallas, Baltimore, Houston, Los Angeles, and San Francisco.

TABLE 6

PANEL UNIT-ROOT TESTS ON CPI, INDICES OF TRADED-GOODS PRICES, AND NONTRADED-GOODS PRICES 1967–1995

Variable	τ	p-value	$\hat{\rho}$	Adjusted $\hat{\rho}$	Adjusted half-life
		A. Levin and Lin			
CPI	−8.671	0.001	0.844	0.925	8.891
P^T	−6.678	0.035	0.866	0.951	13.796
P^N	−7.901	0.013	0.855	0.938	10.830
Variable	$\bar{t}$	p-value	$\hat{\hat{\rho}}$	Adjusted $\hat{\hat{\rho}}$	Adjusted half-life
		B. Im, Pesaran, and Shin			
CPI	−2.319	0.053	0.807	0.914	7.708
P_T	−2.037	0.109	0.768	0.886	5.727
P_N	−2.060	0.092	0.829	0.954	14.719

maximum divergence for the all-items CPI is an average of 0.34 percentage points per year between Cleveland and St. Louis. For traded-goods prices, the maximum is 0.42 percentage points.

Moving to the formal statistical tests, Table 6 reports a set of results for the aggregate price index (CPI), the price of tradables (P^T), and the price of nontradables (P^N). We are able to reject the presence of a unit root in nearly all of the price series using both the LL and the IPS procedures.[15]

One mystery emerges from these results. We expect that traded-goods prices should adjust more rapidly than both nontraded-goods prices and the overall index. Here the evidence is decidedly mixed. If one takes the results of the IPS test, then the theory is validated. Using the results from the LL procedure, however, the deviations from PPP are more persistent for the component parts of the index than for the CPI as a whole.

We simply note that data availability hampered our ability to examine whether these results could be explained by either real wage or productivity differentials. Real wage data are only available by state, and no regional productivity data are collected.[16] As a result, we are unable to test the extent to which either productivity or income differentials can account for test results.

To summarize the results of this section, we find that there is long-run adjustment toward PPP for both traded (commodities) and nontraded goods (services), but there is only fragmentary evidence that the slow adjustment of the overall consumer price index is induced by slower adjustment in the prices of nontraded goods.

[15] As was the case in Section 2, we do not include a time trend in the estimation. When we do add a time trend, the half-lives are reduced significantly.

[16] Recent work by Alberola-Ila and Tyrväinen (1999) on European data suggests that one needs both wage and productivity data to provide an adequate test of the Balassa–Samuelson hypothesis.

5. LESSONS FOR THE EUROPEAN CENTRAL BANK

Our analysis of price-level behavior across cities within the United States has raised a number of puzzles. While we find persuasive evidence to reject the hypothesis that the real exchange rate between two cities contains a unit root, the deviations from city PPP are substantially more persistent than deviations from international PPP. Our estimated intercity PPP convergence rates are approximately nine years, or roughly three times the cross-national estimates. Moreover, the deviations from city PPP are substantially more persistent than estimates of the deviation from the law of one price found by other researchers.

What does this all mean for the European Central Bank? One issue that confronts the ECB is the impact and persistence of regional inflation divergence.[17] As noted by Walton and Déo (1999a, 1999b), large inflation differentials among regions cause a number of difficulties. First, they create real interest-rate differences. Given that under normal circumstances, the real interest rate fluctuates in a range of between 0 and 8% or so, inflation differentials of one to two percentage points are quite large.[18] Furthermore, such persistent differentials in inflation mount up, resulting in price levels that differ by 10 to 15 percentage points – a sizable amount.

Second, monetary policy operates by fixing nominal interest rates throughout the common currency area. Since third-party arbitrageurs operating outside of the monetary union will ensure equalization of nominal interest rates on debt (e.g., sovereign debt) of identical default risk, heterogeneity of inflation rates will imply vastly different real interest rates across nations, affecting their ability to service their debts.

Beyond this, areas that are doing well, with high levels of aggregate demand, will tend to have higher levels of inflation than regions with low levels of activity. Higher local demand leads to higher inflation and lower real interest rates, driving demand up even more. As a result, the policy that fixes nominal rates has the potential to be procyclical.

The U.S. Federal Reserve generally ignores these regional inflation differences. It is nearly impossible to find evidence in the deliberations of the Federal Open Market Committee of any consideration being given to such issues.

The ECB is likely to ignore these differences as well. To see why, consider the fact that the ECB's stated inflation objective is a year-on-year change in the HICP of not more than 2%. If inflation in the Euro area is near the 2% maximum, then how big would a change in inflation in an individual country have to be to trigger ECB action? The answer clearly depends on the size of the country. An increase in German inflation of 0.3% will increase the HICP by 0.1%. But it takes an increase in Irish inflation of 11.1% to lead to the same 0.1% rise in the HICP.[19] In other words, since Ireland's economy is less than 1% of the Euro area total, its inflation

[17] Since the United States is a mature currency union, the idea of using the U.S. cities results to predict European relative price index dynamics would be even more plausible if the initial transition toward European price-level convergence had already occurred. In a recent article, Rogers (2001) reports that the cross-sectional volatility of relative prices across European countries declined between 1990 and 1999, yielding evidence that such a transition may already have occurred.

[18] See Chart 5 of King (1999) for information on the post-World War II United States, for example.

[19] See Walton and Déo (1999a, Table 2).

can diverge from the average by a factor of 100 before anything would be done. This range is a bit wider than that implied by population weights for the U.S. cities, where a rise of 0.1% in the U.S. CPI, all other cities equal, would require a rise of about 2.5% in prices in Los Angeles, but about 15% if the increase were limited to the Cincinnati area.[20]

Given that monetary policy will not be able to react to the imbalances that result from inflation differences across countries of the Euro area, what will? First, factors will move, but gradually. Capital will flow in response to differences in real interest rates, and labor will move in response to differences in the cost of living. Casual observation certainly leaves the impression that both labor and capital is more mobile within the United States than they are within Europe. While these factor market characteristics may be changing in Europe following the implementation of monetary union, for the time being, the apparently higher degree of mobility in the United States leads us to view our estimates of the speed of price-level convergence across American cities as an upper bound on the rates that members of the European currency union are likely to experience.

Second, the United States has a centralized fiscal authority that is better equipped than its European counterpart to offset such shocks through regional transfers. For example, the American unemployment insurance system is primarily a federal program that serves to redistribute income from relatively more to relatively less prosperous regions of the country. The U.S. federal fiscal system reduces the pressure on domestic monetary policy to resolve conflicting demands arising from regional differences. Although the mechanism does exist for redistribution of resources across European national boundaries, at this point the amounts involved continue to be very small.

We close by noting that the countries of the Euro area face an additional challenge in the transition following monetary union. Initially, there may be wide inflation differences across countries that are justified by fundamentals. In particular, the conversion rates chosen for the fixing of exchange rates at the inception of the euro, as well as changes in local regulation and taxation, will create a need for one-time changes in price levels. Our results suggest that these adjustments may occur very slowly.

APPENDIX

The LL procedure is computationally equivalent to estimating (1), allowing for differential degrees of serial correlation across individuals (different k_i), while constraining $\beta_i = \beta = 1 - \rho$ to be identical. Their procedure also controls for heteroskedasticity across individuals and provides us with a panel estimate of persistence, ρ. LL suggest two test statistics: one based on the panel estimate of β and the other on the studentized coefficient of $\hat{\beta}$, which we label τ. LL go on to show that the sampling properties of τ are superior to those of $\hat{\beta}$, and so we base our inferences only on τ.

[20] These estimates are based on the 1996 population levels, as the BLS does not publish city expenditure weights that would be the exact analog to the HICPs country weights that are based on GDP.

To do the IPS test, we run the augmented Dickey–Fuller (ADF) regression for each i individually and let τ_i be the studentized coefficient from the ith ADF regression. Since the ϵ_{it} are assumed to be independent across individuals, the τ_i are also independent. IPS show that the cross-sectional average $\bar{t} = (1/N)\sum_{i=1}^{N}\tau_i$ is asymptotically normally distributed. However, as in the LL test, our tests are based on a parametric bootstrap distribution of $\bar{t}$. The LL and IPS procedures and our parametric bootstrap are described in detail in next sections.

A.1. *The Levin and Lin Test.* The LL test proceeds as follows:

1. Eliminate the common time effect θ_t by subtracting the cross-sectional mean from the data. The basic unit of analysis is $\tilde{q}_{i,t} = q_{i,t} - (1/N)\sum_{i=1}^{N} q_{i,t}$.
2. For each city,
   (a) Regress $\Delta\tilde{q}_{i,t}$ on a constant, (possibly) a trend, and k_i lagged values of $\Delta\tilde{q}_{i,t}$, where the lag lengths k_i are determined by Campbell and Perron's (1991) procedure as discussed in Footnote 6. Let $\hat{e}_{i,t}$ denote the residuals from the regression.
   (b) Regress $\tilde{q}_{i,t-1}$ on the same variables in part (2a) above and let $\hat{v}_{i,t-1}$ denote the residuals from this regression.
   (c) Regress $\hat{e}_{i,t}$ on $\hat{v}_{i,t-1}$ (no constant). Denote the residuals from this third regression by $\hat{\epsilon}_{i,t}$. Use the standard error of this regression, $\hat{\sigma}_{ei} = \sqrt{(T-k_i-1)^{-1}\sum_{t=k_i+2}^{T}\hat{\epsilon}_{i,t}^2}$ to normalize $\hat{e}_{i,t}$ and $\hat{v}_{i,t-1}$. Denote the normalized values by $\tilde{e}_{i,t} = \hat{e}_{i,t}/\hat{\sigma}_{ei}$ and $\tilde{v}_{i,t-1} = \hat{v}_{i,t-1}/\hat{\sigma}_{ei}$.
3. Run the *panel OLS* regression $\tilde{e}_{i,t} = \beta\tilde{v}_{i,t-1} + u_{i,t}$. In our analysis of nonlinear adjustment, it is the values of $\tilde{v}_{i,t-1}$ that we stratify into groups in estimating the adjustment following "large" and "small" deviations from PPP.
4. The LL test statistic, τ, is the studentized coefficient from the panel OLS regression (the reported t-statistic). The asymptotic distribution of τ is nonstandard and LL provide adjustments to τ that result in an asymptotically standard normal variate under the null hypothesis and under the assumption that the errors are contemporaneously uncorrelated. We do not use their adjustment since we allow for contemporaneous correlation across individual cities and bootstrap τ directly.

A.2. *The Im, Pesaran, and Shin $\bar{t}$-Test.* To conduct the IPS $\bar{t}$-test, first remove the common time effect by performing step 1 of the LL test. For each city, run the augmented Dickey–Fuller regression of $\Delta\tilde{q}_{i,t}$ on $\tilde{q}_{i,t-1}$, a constant, (possibly) a trend, and k_i lagged values of $\Delta\tilde{q}_{i,t}$ with lag lengths k_i determined by Campbell and Perron's (1991) procedure. Let t_i denote the studentized coefficient (the "t-statistic" for the coefficient on $\tilde{q}_{i,t-1}$) from the univariate ADF test. The IPS test statistic is $\bar{t} = (1/N)\sum_{i=1}^{N} t_i$.

Under the null hypothesis that each of the series contains a unit root and that they are cross-sectionally independent, IPS show that the asymptotic distributions of the t-bar statistics are nonstandard and do not have analytic expressions. IPS have tabulated critical values by the Monte Carlo simulation assuming that the cross-sectional correlation of the errors are zero. We rely on the parametric bootstrap distribution of the $\bar{t}$-statistic that we built by allowing for cross-sectional dependence.

A.3. *The Parametric Bootstrap.* We generate our parametric bootstrap distributions for the unit-root test statistics with the data generating process (DGP),

$$\Delta q_{i,t} = \mu_i + \sum_{j=1}^{k_i} \Delta q_{i,t-j} + \epsilon_{it} \tag{A.1}$$

Each $q_{i,t}$ is modeled as a unit-root process in which its first difference follows a univariate autoregression. Ideally, one might prefer to specify the DGP as an unrestricted vector autoregression for all 19 cities, but estimating such a large system turns out not to be feasible.

The individual equations of the DGP are fitted by least squares with k_i determined by the Campbell–Perron rule. When linear trends are included in the test equations, constants are included in Equation (A.1). We account for dependence across cross-sectional units by estimating the joint error covariance matrix $\Sigma = E(\epsilon_t \epsilon_t')$ where $\epsilon_t = (\epsilon_{1t}, \ldots, \epsilon_{Nt})$ from the OLS residuals.

The bootstrap distribution for τ and $\bar{t}$ is built as follows:

1. Draw a sequence of length $T + 100$ innovation vectors from $\tilde{\epsilon}_t \sim N(0, \hat{\Sigma})$.
2. Generate pseudo-observations $\{\hat{q}_{it}\}, i = 1, \ldots, N, t = 1, \ldots, T + 100$ according to (A.1) using estimated values of the coefficients.
3. Drop the first 100 pseudo-observations, then run the LL and the IPS tests on the pseudo-data. This yields a realization of τ and $\bar{t}$.
4. Repeat 2000 times. The collection of realized τ and $\bar{t}$-statistics form the bootstrap distribution of these statistics under the null hypothesis.

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