NBER WORKING PAPER SERIES

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Working Paper 7681
http://www.nber.org/papers/w7681

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2000

We thank James Harrigan for useful discussions. In addition, the paper has benefited from the comments of seminar participants at North Carolina State University, the University of Otago, and the April 1998 CEPR conference, Vienna Austria. Some of the Mark's work on this paper was supported by a 1998 Erskine Fellowship at the University of Canterbury, New Zealand. The views expressed herein are those of the authors and are not necessarily those of the National Bureau of Economic Research.

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NBER Working Paper No. 7681
May 2000
JEL No. F31, F37

ABSTRACT

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. These estimates provide an upper bound on speed of convergence that participants in European Monetary Union are likely to experience. 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 non-traded good prices in the overall price index.

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Introduction

Do prices 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 level? 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 level divergences across U.S. cities are temporary, but surprisingly persistent, with a half-life of nearly 9 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 levels 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 price-level 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 two percent. 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, 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 to be 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 of the hypothesis that deviations from PPP contain a unit root, implying that some portion of their variation is driven by a random walk.[1] This result implies that inflation differentials between countries, measured in terms of a common currency, can persist indefi-

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

nitely 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 4 and 5 years [Abuaf and Jorion (1990), Frankel and Rose (1986), Wu (1986), MacDonald (1996), Papell (1997), Lothian (1997) and Wei and Parsley (1995)]. 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 a number of explanations for incomplete relative price-level adjustment. These include: i) trade barriers, such as tariffs and quotas; ii) non-tariff barriers, including the bureaucratic difficulties of 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 non-traded goods in the general price level and the potential for differential growth in the level and efficiency of factors used in their production.[2] Some combination all of these factors is likely to impede adjustment toward PPP, as it seems improbable that any one factor in isolation is sufficiently important to explain the slow convergence.[3]

[2]Wei and Parsley (1995) find that deviations from PPP are positively related to nominal exchange-rate volatility (item iii), Engel and Rogers (1995) and Betts and Devereux (1997) 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), Wei and Parsley (1996), Engel and Rogers (1995) O'Connell and Wei (1997), 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* (1996) examine the implications of the Balassa-Samuelson hypothesis.

[3]For example, the effect of sticky nominal price adjustment as suggested by Dornbusch (1976) or Taylor (1977) should result in half-lives of a year or so, not the four to five year consensus estimate from

We can think of each of these factors 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 factos, and assuming that the tariff does not change, the relative price of goods in the regions will not change. The presence of non-traded goods, on the other hand, may generate deviations from PPP that are long-lasting, as differential improvements in the technology of producing traded and non-traded 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. Studying the relative price levels of cities in a common currency and trade area provides us with a type of natural experiment in which the impact of a number of these explanations are attenuated. Specifically, when examining the movements in relative prices say between Chicago and Detroit, tariff, non-tariff, 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 non-traded 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 U.S. 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 spans the relatively short period from 1975 to 1992, whereas our data spans 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

international data.

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 and not 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 emphasis on the HICP.[4]

To summarize our main results, we find price-level divergences across U.S. cities to be fairly large and surprisingly persistent. 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.

As in the international literature, it is no surprise 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. This result is reversed when we employ panel data procedures, as we find that relative prices do converge to a common trend, and we are able to reject the presence of a unit root. Using the full 78-year sample from 1918 to 1995, and assuming that relative prices 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.

What is responsible for the slow convergence? We examine three hypotheses: transportation costs, nonlinearities leading to slower adjustment to small shocks than to large ones, and the inclusion of non-traded goods prices in the general price index. As for transportation costs, 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 also find evidence that adjustment is faster when shocks are large. As for the presence of non-traded goods prices in the general price index, we study their role by looking at price behavior of commodities and services separately. Using thirty years of available data on

[4]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.

fourteen of the nineteen cities, we find that commodities and services prices converge to the cross-sectional average. As we expect, 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 paper is divided into four sections. Section 1 describes the data and presents some descriptive statistics. Section 2 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 3 we examine the importance of transportation costs and the presence of non-traded goods. Section 4 concludes with a discussion of the implications of our findings for the European Central Bank.

1 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.[5] These data were obtained from the Bureau of Labor Statistics and are the basis for the construction of the national consumer price index.

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 non-overlapping ten-year periods, beginning in 1926, computed for each of the 19 cities. We report the highest and lowest average annual inflation for each ten-year interval, as well as the differential. For example, from 1986 to 1995, New York City's inflation of 4.00 percent per year on average was the highest in the sample, while Houston's average annual inflation of 2.87 percent 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 ten to twenty years.

We draw several conclusions from these results. First, inflation differentials of one percentage point per year can persist over ten-year periods — a seemingly long period of time.

[5]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 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

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

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, on average the difference between the city with the highest and the lowest inflation is 1.11 percentage points, with relatively little variation from the 1920s to the 1990s. This is the first indication that there may have been little change in the dynamics of adjustment over the seventy plus years of the sample. Increasing the time span from ten to twenty years, and looking at three non-overlapping intervals, the average differential drops nearly in half to 0.57 percentage points annually, again suggesting very slow adjustment.

These inter-city inflation differentials, which are analogous to international real exchange-rate changes, 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 whereas 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. (pg. 36)'

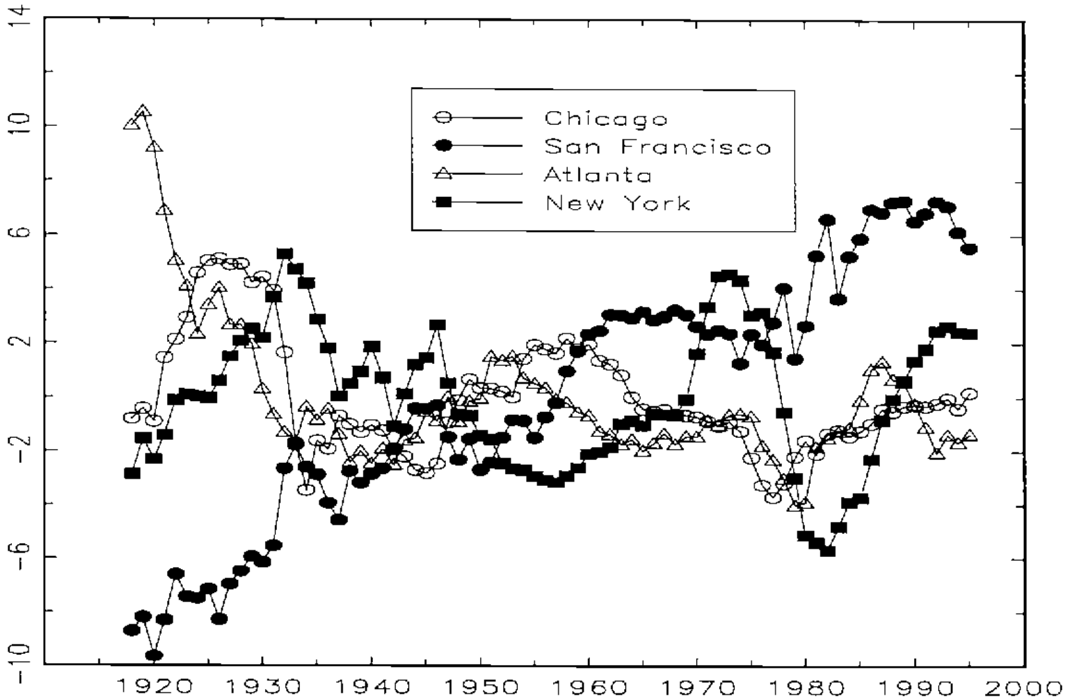

Figure 1: Log Price Levels, Relative to Cross-Sectional Average

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 price 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, respectively.

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 ten years. San Francisco's experience suggests the possibility of cycles around an upward trend, as its log price level shows no tendency to revert to the common mean.

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

2 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 real 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, and so they diverge from one another. The alternative hypothesis in our statistic tests is that the level of prices in various cities converge to a steady-state value in the long run.

In our panel econometric analysis we account for a common time effect (the cross-sectional mean), and so our results are invariant to the choice of a numeraire city. If the level of prices in San Francisco relative to the cross-sectional mean contains a unit root, it would mean that relative prices would wander apart indefinitely — the real exchange rate could become arbitrarily high or low. This result would be very troubling, as it would imply extreme factor immobility.

Univariate unit-root tests, of the type pioneered by Dickey and Fuller, have notoriously low power — it is 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 separate procedures: one due to Levin and Lin (1993) (LL) and the second derived by Im, Pesaran and Shin (1996) (IPS).[6]

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_{it} 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_{it}, $\beta_i \equiv \rho_i - 1$, and $\rho_i \equiv \sum_{j=1}^{k_i} \gamma_{ij}$. The approximate half-life of a shock to q_{it} is computed as $-\ln(2)/\ln(\rho_i)$.[7]

[6]See the Appendix for a description of both procedures.

[7]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 (2), 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.

It is important to include fixed effects in a panel setting. The variation of α_i across cities allows us to account for possible heterogeneity, such as differing income levels and sales taxes, which can lead to permanent differences in relative prices across cities. The common time effects, θ_t, which we cannot estimate in a univariate setting, capture the influence of macroeconomic shocks that induce cross-sectional dependence in real exchange rates. It is straightforward to account for these fixed effects by subtracting the cross-sectional mean of the real exchange rate each period and basing the tests on the transformed data. Computationally, this is identical to including common time dummy variables in the regression (1). We also note that the panel analysis makes it unnecessary for us to select a numeraire city since any movements in a numeraire price level are absorbed into the common time effect. This is potentially an advantage of the panel analysis since univariate results may not be robust to the choice of the numeraire.[8]

Our interest is in the parameters on the lagged log of the price level, q_{it}. These are the β_i's. The closer the estimates are to zero, the longer the estimated half-life of a shock and the more likely it is that the price data are nonstationary. The null hypothesis in both of the test procedures we employ are formulated such that each series contains a unit root. That is, $H_o : \beta_i = \beta = 0$ 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 some i. Bowman (1998) and Maddala and Wu (1997) 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 estimator of ρ, while the IPS procedure does not. In addition, the low power of the LL test allows us to err on the side of caution since it is unlikely that the behavior of only one or two outlier series will cause the unit-root null to be rejected.

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 only asymptotically, as the off-

[8]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.

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.[9]

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 tests 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.

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 $\bar{t}$.[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.

[9] O'Connell (1997) 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.

[10] The LL and IPS procedures and our parametric bootstrap are described in detail in the Appendix.

[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 2 and 4 years. The problem with these results is that there is no economic basis for expecting real exchange rates to trend over long periods.

Table 2: Panel Unit-Root Test Results

A. Levin and Lin					
Sample	τ	p-value	$\hat{\rho}$	adjusted $\hat{\rho}$	adjusted half-life
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
B. Im, Pesaran and Shin					
Sample	$\bar{t}$	p-value	$\hat{\bar{\rho}}$	adjusted $\hat{\bar{\rho}}$	adjusted half-life
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 19 U.S. cities. The methods are described in the text and the appendix.

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 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 sub-sample analysis, we examine 20-year subperiods extending from 1936–1955, 1956–1975, and 1976–1995. We continue to be able 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-WWII 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. Point estimates of ρ are quite large during 1956–1975. The implied half-life of convergence for the most recent period is between 8 and 9 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 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 9 years. It is interesting to ask why these estimates are so large. In the next section we pursue this line of inquiry.

[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.* (1996) perform a small Monte Carlo experiment from which they determined that Nickel's adjustment is reasonably accurate.

3 Additional Characteristics of the Data

In this section, we explore other features of these price-indices in an attempt to gain additional perspective into why convergence is so slow. In section 3.1 we examine the role of distance between two cities as a determinant not only of the size of the real exchange rate, but also of the persistence in the deviation from PPP. Section 3.2 examines the data for possible nonlinearities in the reversion towards the long-run real exchange-rate mean. Here, we explore the possibility that most of the time we are looking at slow responses to small deviations, and that, if and when large disturbances occur, responses would be more rapid.

3.1 Distance

We follow Engel (1993), 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.

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 conver-

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)	
$half$	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, $\hat{\rho}, \tau, half$ are estimated ρ, studentized coefficient, and implied half life from univariate ADF regressions. T-ratios in parentheses.

gence is indeed slower between cities of greater spatial separation, but the estimates are not statistically significant.

The evidence from this section is 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. We pursue this issue further in the next subsection.

3.2 Differential Adjustment Following Small and Large Deviations

Do the data suggest nonlinear reversion of log real exchange rates towards their means? In the presence of proportional transactions costs, the log real exchange rate behaves as a regulated Brownian motion within a neutral band created by the transportation costs. We expect that the exploitation of arbitrage opportunities, created when deviations from PPP are sufficiently large to move outside of the neutral band, cause these large deviations to be relatively short-lived.

Table 4: Nonlinear Adjustment

(Small) $\tilde{\rho}_s$	(Large) $\tilde{\rho}_\ell$	Wald Statistic (p-value)
0.955	0.896	2.557
(-1.234)	(-10.617)	(0.110)

Notes: Panel estimates of differential response of relative prices to large and small deviations from PPP, using the Levin and Lin procedure. Large deviations are defined as the largest 25 observations, in absolute value.

To investigate these issues, 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 consider the deviation from PPP to be large if it is among the largest 25 percent of observations in absolute value. The LL regression is then estimated on these 'small' and 'large' observations. The results are reported in table 4.

As can be seen, we estimate $\tilde{\rho}_\ell$ to be 0.896 on large deviations and $\tilde{\rho}_s = 0.955$ on small deviations. The p-value for the Wald test of the hypothesis $\tilde{\rho}_\ell = \tilde{\rho}_s = 0.955$ is 0.110, and so there is moderately strong evidence that large deviations are shorter-lived than small deviations, which is consistent with the hypothesis that convergence occurs up to a zero-arbitrage opportunity neutral band.

4 Non-traded Goods in the Price Index

The most natural explanation for our finding of slow inter-city relative price adjustment is the presence of non-traded-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

[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.

non-traded 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} i's city i's price of traded goods, P_{it}^{N} is city is price of non-traded goods, and ϕ is the share of non-traded 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 non-tradables across cities, causes similar behavior in the log real exchange rate.

In order to analyze the role of non-traded-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 non-traded-goods prices, and a similar price series on commodities as our measure of traded-goods prices. Unfortunately, these are only available for fourteen 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 non-traded goods inflation, and the traded/non-traded 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 non-traded-goods prices, the differences are even larger, rising to as high as 2 percentage points per year on average for ten years. Given the presumed high degree of labor and capital mobility in the U.S., these divergences strike us as extremely large.

The data are, however, generally consistent with the hypothesis that inflation is converg-

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

Table 5: Selected Annual Inflation Rates in Traded and Non-traded Goods

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

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

ing, albeit slowly. The 29-year samples show maximum differences of one-half to one-third those during the 10 year periods. Looking further, we see that non-traded-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 maximum divergence for the all-items CPI 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 non-tradables (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 non-traded-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

[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.

Table 6: Panel Unit-Root Tests on CPIs, Indices of Traded-Goods Prices, and Non-traded-Goods Prices 1967-1995.

A. Levin and Lin					
Variable	τ	p-value	$\hat{\rho}$	adjusted $\hat{\rho}$	adjusted half-life
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
B. Im, Pesaran, and Shin					
Variable	$\bar{t}$	p-value	$\hat{\bar{\rho}}$	adjusted $\hat{\bar{\rho}}$	adjusted half-life
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

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 is only available by state, and no regional productivity data is 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). The slow adjustment of the overall consumer price index is induced by the behavior of the prices of nontraded goods.

[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 U.S. 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 inter-city PPP convergence rates are approximately 9 years, or roughly 3 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.

We examined three possible explanations for the slowness of the movements in inter-city relative prices: transportation costs, nonlinearities leading to slower adjustment to small shocks than to large ones, and the presence of non-traded goods. We find evidence suggesting that all of these explanations play some role: distance slows adjustment, adjustment is faster when shocks are large, and non-traded goods prices converge more slowly than those of traded goods do.

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. 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 zero and eight percent or so, inflation differentials of one to two percentage points are quite large.[17] Furthermore, such persistent differentials in inflation mount up, resulting in price levels that differ by ten to fifteen percentage points — a sizeable amount.

Second, monetary policy operates by fixing nominal interest rates throughout the common currency area. This has several implications. 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

[17]See Chart 5 of King (1999) for information on the post-WWII U.S., for example.

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 two percent. If inflation in the Euro area is near the two percent 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 percent will increase the HICP by 0.1 percent. But it takes an increase in Irish inflation of 11.1 percent to lead to the same 0.1 percent rise in the HICP.[18] In other words, since Ireland's economy is less than one percent of the Euro area total, it's inflation 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 percent in the U.S. CPI, all other cities equal, would require a rise of about $2\frac{1}{2}$ percent in prices in Los Angeles, but about 15 percent if the increase were limited to the Cincinnati area.[19]

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 U.S. 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 U.S. 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.

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

[19] 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 county weights that are based on GDP.

Second, the U.S. 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. While 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.

A. Appendix

A1. 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. The bootstrap is described below.

A2. 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 LR-bar and t-bar statistics are nonstandard and do not have analytic expressions. IPS has tabulated critical values by 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 which we built by allowing for cross-sectional dependence.

A3. The Parametric Bootstrap

We generate our parametric bootstrap distributions for the unit-root test statistics with the data generating process (DGP),

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

Each q_{it} 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 eq.(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 or the IPS test on the pseudo-data. This yields a realization of τ and $\bar{t}$.

4. Repeat 2000 times and the collection of realized τ and $\bar{t}$ statistics form the bootstrap distribution of these statistics under the null hypothesis.

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