

WORKING PAPER 32

**Price Level Convergence Among United States Cities:
Lessons for the European Central Bank**

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Editorial

On April 3 -4, 1998 the Oesterreichische Nationalbank hosted a joint Euroconference with the CEPR on „Real Exchange Rates: Recent Theories and Evidence“. A number of papers presented at this conference is being made available to a broader audience in the Working Paper series of the Bank. This volume contains the sixth of these papers. The first five papers were issued as Working Papers 27-31.

ABSTRACT

We study the dynamics of price indices for major U.S. cities. Using panel econometric methods, we find that relative price levels among cities mean revert, but at a surprisingly slow rate. In a panel of 15 cities from 1918 to 1995, we estimate the half life of convergence to be approximately 9 years. The following hypotheses are investigated as explanations for the slow convergence: (i) Arbitrage impediments induced by transportation costs, and (ii) and the inclusion of nontraded goods prices in the overall price index as suggested by the Balassa-Samuelson hypothesis. Our estimates provide an upper bound on convergence rates that participants in European Monetary Union may experience.

Keywords: Purchasing power parity, Convergence, European Monetary Union

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Note: The views expressed in this Working Paper are strictly those of the authors and do not, in any way, commit the Oesterreichische Nationalbank nor the CEPR.

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 inflation shock? In order to answer this question, we study the dynamics of consumer price indices for a panel of 19 major U.S. cities over the period from 1918 to 1995 using panel time-series methods now in common use for studying real output growth rates and logarithms of real exchange rates across countries. Our finding is that price level divergences across U.S. cities are surprisingly persistent, with a half-life of nearly 9 years.

Our research has two principal motivations, the first concerning the sources of persistence in the deviations from purchasing power parity (PPP) found in studies of national price levels and exchange rate data, and the second related to understanding the likely nature of price-level convergence in the European Monetary Union (EMU). When examined over the post-1973 float, pairwise comparisons of countries using univariate methods typically do not allow rejection 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 would imply that inflation differentials between countries can persist indefinitely and that the price level in one country can deviate from that in another by an arbitrarily large amount. Recently, researchers that have exploited more powerful multivariate tests to simultaneously combine numerous countries in panel unit root testing procedures have rejected the unit root hypothesis, implying that relative prices revert to a common mean. But the rate at which this mean reversion occurs is 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 (1996), MacDonald (1996), Papell (1997), Lothian (1997), Wei and Parsley (1995)]. This 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

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¹ The references are too numerous for us to cite here. For excellent surveys on the literature up through the early 1990s, see Breuer (1994) and Froot and Rogoff (1995).

single country than across countries since within-country markets for products, labor, and capital are presumably better integrated.

International PPP researchers have suggested a broad set of factors that may prevent complete adjustment in relative price levels. 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 rate adjustment; iv) firms exercising local monopoly power and pricing to segmented markets; v) sticky nominal adjustment arising from imperfectly competitive product markets where price changes are costly; vi) transportation costs associated with moving goods from one region to another; 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 of all of these factors is likely to prevent rapid adjustment toward PPP, as it seems improbable that any one in isolation is sufficiently important explain the slow convergence in its entirety.³

We can think of each of these factors as creating permanent deviations from PPP, influencing transitional dynamics, or both. For example, the tariffs will, unless changed, simply drive a wedge between prices in different regions. But in the absence of any other influence, 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 very long lasting, as differential changes 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

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) attributes the higher volatility of real exchange rate changes during the float to sticky price adjustment (item v), Wei and Parsley (1996), Engel and Rogers (1996), 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), Canzoneri *et al.* (1996) examine implications of the Balassa-Samuelson hypothesis.

³ For example, 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 four to five years and Chari, *et al.* find that calibrations of sticky price models cannot replicate the persistence of the real exchange rate found in the data.

converge. Regions that are near, with low costs of moving goods between them, will be more likely to adjust quickly to a given size relative price disturbance than regions that are far apart.

Attempts to disentangle the marginal affects of each of the six broad candidate explanations for deviations from PPP has 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 are attenuated. Specifically, when examining the movements in relative prices say between Chicago and Detroit, tariff, non-tariff and nominal exchange rate affects 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 second motivation for undertaking this research is to provide a foundation for understanding price level convergence in a future European monetary union (EMU) by drawing on the US experience as a laboratory. One issue that confronts the European Central Bank (ECB) is how long the effects of a regional price shock will persist and how best to deal with such a shock. The job of the ECB is likely to be more difficult than that of the U.S. Federal Reserve for at least two reasons. First, the U.S. has a centralized fiscal authority that can implement regional transfers to offset such shocks. 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 makes it less likely that regional divergences will place conflicting demands on domestic monetary policy. In addition, slow convergence implies that differences in regional inflation rates will persist for relatively long periods. 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 may imply vastly different real interest rates and hence sizable differentials in real tax liabilities in the servicing of that debt across nations.

Beyond this, casual observation certainly leaves the impression that both labor and capital is more mobile within the U.S. than it is within Europe. While many of these factors may change following the implementation of monetary union, for the time being, the apparently higher degree of factor mobility in the U.S. leads us to view our estimates of the speed of price-level convergence across American cities as a likely upper bound on the rates that members of the currency union are likely to experience.

Our work differs in several dimensions from research conducted by Parsley and Wei (1996) and Engel and Rogers (1997) and others who study violations of the law of one price within the U.S. First, the longer of the two data sets, studied by Parsley and Wei, spans the relatively short period from 1975 to 1992, whereas our data spans a long historical period that begins in 1918. Second, we examine the behavior of price indices, which contain broad coverage over many goods sold in various locations. A potential problem with the Parsley-Wei or Engel-Rogers data is that the price of different brands can be sampled in different cities. Consequently, the failure of the law of one price between two cities that they document may be exactly like the failure of the law of one price that can be seen on a single store shelf at the grocery store because two brands of coffee charge different prices. While our price indices are aggregate measures built up from prices of individual goods, the broad coverage of commodities may provide more homogeneous coverage across cities. Third, our focus is on the behavior of price indices, which is more appropriate when coming from a macroeconomic perspective since inflation is measured by the percent change in such indices and not by changes in individual commodity prices.

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 percent. 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 substantial impacts on resource allocations.

As in the international literature, employing standard univariate testing procedures, we are generally unable to reject the hypothesis that the log real exchange rate between pairs of

U.S. cities are characterized by a process with a unit root. This result is reversed when we employ more powerful 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 the 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. We find no indication that the convergence rate has changed over time.

What might be causing the slow convergence? We examine two potential explanations: transportation costs and the presence of nontraded goods. For the first, we find that converge is faster between cities that are closer together, but the effects are small. We study the implications of nontraded goods by looking at the behavior of the prices goods and services separately. Using data for the thirty years beginning in 1966, we find that the deviation from PPP for commodities converge at about the same rate as the deviation from PPP for all goods, and substantially more quickly than the commodity-services relative price differential. Surprisingly, the deviation from PPP defined over services converges at about the same rate as that of the overall index.

The remainder of the paper is divided into four sections. Section 1 describes the data and presents some simple descriptive statistics. Section 2 reports the main empirical findings, including univariate and multivariate time-series results based on unit root tests, as well as the estimates of the convergence rates. In Section 3 we proceed to examine the importance of transportation costs and the presence of nontraded goods. Section 4 concludes.

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 1918–1995.⁴ These data were obtained from the Bureau of Labor

⁴ The cities in the sample are, Atlanta, Baltimore, Boston, Chicago, Cincinnati, Cleveland, Detroit, Houston, Kansas City, Los Angeles, Minneapolis, New York, Philadelphia, Pittsburgh, Portland, San Francisco, Seattle, St. Louis, and Washington D.C. The regular publication of the CPI occurred in 1921. Observations for preceding years were estimated by the BLS.

Statistics and are the basis for the construction of the national consumer price index.

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	0.92
1956:1965	2.13	San Francisco	1.19	Detroit	0.94
1966:1975	5.69	New York	4.98	Los Angeles	0.71
1976:1985	7.64	Cleveland	6.35	New York	1.29
1986:1995	4.00	New York	2.87	Houston	1.13

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 nonoverlapping ten year periods beginning in 1926 computed for each of the 19 cities. We report the highest and lowest average annual inflation for each of ten year interval, as well as the differential. For example, from 1986 to 1995, New York City's inflation 4.00 percent 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 percent per year on average.

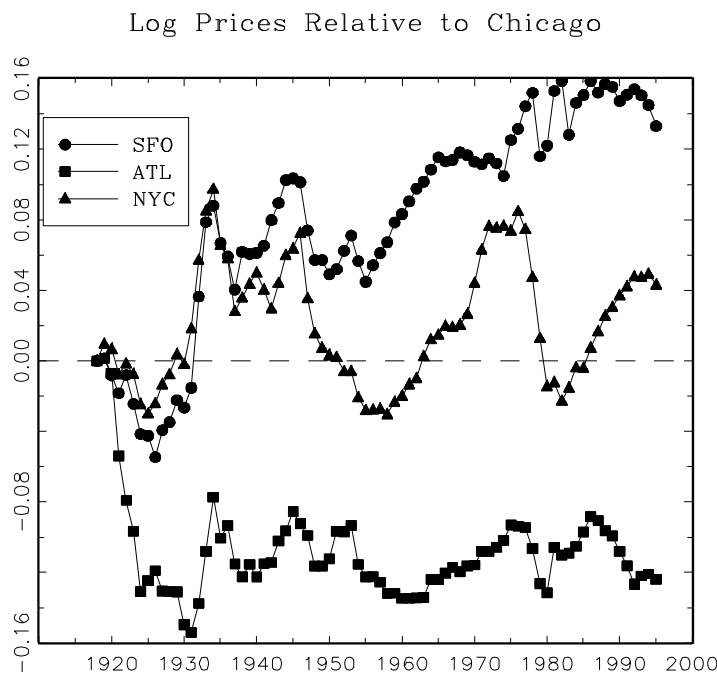
We draw several interesting conclusions from these results. First, inflation differentials of one percent per year can persist over 10 year periods — a seemingly long period of time. But even this very crude look at the data suggests that these differences reverse themselves, as New York City's high inflation from 1986 to 1995 is preceded by relatively low inflation in the preceding decade. These reversals suggest that the differentials die out, but on a decadal time scale. Second, on average the difference between the highest and lowest inflation city is 1.11 percent, 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

Original expenditure weights were calculated from studies of consumption patterns of urban families in 32 cities over the period 1917-1919. Since that time, the weights have been updated on a number of occasions.

plus years of the sample. Increasing the time span from ten to twenty years, and looking at four nonoverlapping intervals, the average differential drops nearly in half to 0.64 percent annually, again suggesting very slow adjustment. Third, the real exchange rate adjustments are smaller, but of the same order of magnitude as real exchange rate adjustments within Europe. Canzoneri *et al.* (1998) report that annual changes of real exchange rates relative to Germany from 1973 to 1991 range from 0.1 percent per year for Belgium to -2.0 percent per year for Italy.

Next, we plot the data to give a graphical impression of the convergence in relative prices. Since our interest is in the rate at which the price in one city relative to that in another reverts to some long-run level, we must choose a "numeraire city." Because of its size and its central geographical location, we assign Chicago this role.⁵ Figure 1 displays the log real exchange rate, between Chicago and San Francisco, New York, and Atlanta, respectively. We set 1918 as the base year so that the three cities have a common initial observation.

Figure 1: Log Relative Price Levels. 1918=1.



The impression one gets by from the figure is that deviations from PPP between U.S.

⁵ In the international context, Papell and Theodoris (1997) show that the choice of numeraire is not innocuous. Because the behavior of our intra-national data is much more homogeneous than the international real exchange rate data, we expect our results to be robust to using Chicago as the numeraire.

cities is as persistent as those observed between nations. Focusing on the center of the three lines, which plots the behavior of prices in New York relative to those in Chicago, we see that cumulative deviations of 8 to 10 percent occur in cycles that last on the order of ten years. The cases of San Francisco and Atlanta are slightly different. Between 1918 and 1981, the price level in San Francisco rose relative to Chicago by 15.3 percent while over the same period the price level of Atlanta declined relative to Chicago by 10.6 percent. The cumulative appreciation in San Francisco relative to Atlanta is 25.9 percent. But the Atlanta case is one in which there was a depreciation early in the sample followed by modest decadal cycles. The path of the San Francisco-Chicago exchange rate suggests the possibility of cycles around an upward trend.

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.

2 Econometric Analyses

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 exchange rates between cities are stationary about their mean. That is to say, we ask whether the level of prices in the various cities, relative to the Chicago numeraire, converge in the long run to a steady state value. The alternative is that they contain a stochastic trend, or unit root. If the level of prices in San Francisco relative to that in Chicago contain a unit root, it would mean that they would wander apart indefinitely — the real exchange rate could become arbitrarily high or low. This would be very troubling, as it would imply extreme factor immobility. We rule out deterministic trends for the same reason.

This section is divided into two parts. The first examines the univariate time-series properties of the data using traditional unit root tests pioneered by Dickey and Fuller. In section 2.2, we exploit recently developed time-series econometric methods that allow us to examine the joint behavior of a panel dataset. These procedures allow testing simultaneously for

unit roots in all of the cities price (relative to the numeraire, Chicago).

2.2 Univariate Unit Root Tests

We begin with a series of individual tests for unit roots in the eighteen series measuring the price in each city relative to Chicago. Let $q_{it} = \ln P_i/P_{0t}$ be the log ‘real exchange rate’ between city i and Chicago where P_i is the CPI for city i (Chicago is city ‘0’). We assume that each q_{it} can be written as a (k_i+1) -th ordered autoregression:

$$q_{it} = \alpha_i + \sum_{j=1}^{k_i+1} \gamma_{ij} q_{it-j} + u_{it} \quad (1)$$

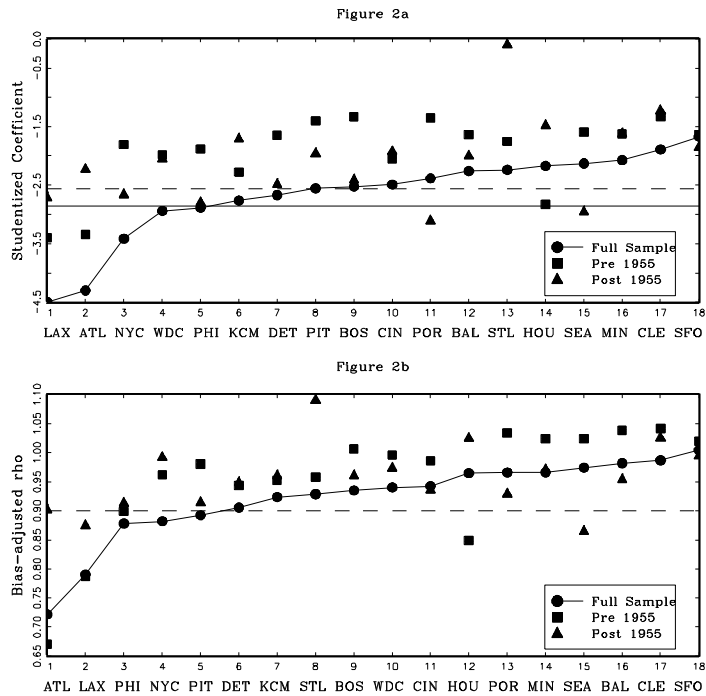
The γ_j s are the partial autocorrelation coefficients that characterize the q_{it} process. Subtracting q_{it-1} from both sides of (1) and collecting terms, we rewrite this as

$$\Delta q_{it} = \alpha_i + \beta_i q_{it-1} + \sum_{j=1}^{k_i+1} \delta_{ij} \Delta q_{it-j} + u_{it} \quad (2)$$

where $\beta_i \equiv \rho_i - 1$, $\rho_i \equiv \sum_{j=1}^{k_i} \gamma_{ij}$, and $\delta_{ij} \equiv -\sum_{r=j+1}^{k_i} \gamma_{ir}$.

Equation (2) is the formulation proposed by Dickey and Fuller for testing the null hypothesis that a unit root is present, $H_0: \beta_i = 0$. While it is common to perform unit root tests with a linear, deterministic trend on the right hand side of equation (2), we have omitted that case here, as PPP implies that there be no trend in the log real exchange rate. The test requires a choice for k_i , the lag number of lags in the autoregression. We follow Campbell and Perron (1991) and determine k_i using the top-down t-test approach, beginning with six lags. That is, we start with $k = 6$, 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 to be five and reestimate the equation. The process is repeated until the t-ratio of the estimated coefficient with the longest lag exceeds 1.96.

Figure 2: (a) Studentized coefficients (t statistics) and (b) bias-adjusted estimates of ρ from ADF regressions: 1918–1995.



The results of the univariate tests are presented in Figure 2a where we plot the studentized coefficients from the regressions. We present three sets of results that differ based on the sample period. The three samples are (1) the full 1918-1995 sample, with results plotted as solid circles; (2) the 1918-1955 sample, with results plotted as solid squares and labeled ‘pre-1955’; and (3) the 1956-1995 sample, with results plotted as solid triangles and labeled ‘post-1955’. For each city, labeled 1 to 18 on the horizontal axis, and each sample, we plot the value of the Dickey-Fuller t -ratio using the scale on the vertical axis. A dashed horizontal line is drawn at the 10 percent critical value of -2.57 and a solid horizontal line is drawn at the 5 percent critical value, -2.86 , both for the full sample.⁶ For the full-sample case, the unit root hypothesis can be rejected in 7 of 18 cases at the 10 percent level and 5 of 18 at the 5 percent level. While for the early period, the rejection rate at the 10 percent level is 3 of 18 for the early period, and 5 of 18 over the latter half of the sample. Overall, there is little evidence against the unit root hypothesis when the series are examined individually.

⁶ The critical values change for the subsamples. In each of the subsamples, the 10 percent critical value is approximately -2.60 and the 5 percent critical value is approximately -2.93 .

Beyond the unit root tests, the regressions yield measures of persistence in relative price movements. This is summarized by the quantity $\rho_i = \beta_i + 1$, which is the autocorrelation coefficient when $q_{i,t}$ follows an AR(1). These results are plotted in Figure 2b with the cities along the horizontal axis and the $\hat{\rho}$ along the vertical axis. Since these estimates are known to be downward biased, we adjust them using Kendall's (1954) formula.⁷ Again, we present results for the full sample, the pre-1955 sample and the post-1955 sample. The (adjusted) point estimates are generally quite large, with the vast majority exceeding 0.90. This implies that deviations from PPP across cities are very persistent — for $\rho = 0.9$ the half-life of a shock is 6.6 years.⁸ In looking at the changes across sample periods, one might expect that the convergence rates would be faster in the more recent period than in the earlier one. Interestingly, the results are nearly the opposite. In 13 of the 18 cases, ρ is higher in the later period than in the earlier one.

2.2 Panel Unit Root Tests

Univariate unit root tests 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 (1997) (IPS).

The tests are based on the simple error-components model specification:

$$\Delta q_{i,t} = \alpha_i + \theta_t + \beta_i q_{i,t-1} + \sum_{j=1}^{k_i} \gamma_{ij} \Delta q_{i,t-j} + \varepsilon_{i,t} \quad (3)$$

where we have allowed for city-specific fixed effects, α and common time effects, θ .

⁷ Kendall shows that the bias of the least squares estimator is $E(\hat{\rho}_i) - \rho_i \simeq -(1 + 3\rho_i)/T$. The figure plots estimates adjusted with this formula.

⁸ The half-life is computed as $-\ln(2)/\ln(\rho_i)$.

It is important to include fixed effects in a panel setting. The variation of α across cities allows us to account for possible heterogeneity in income levels and sales taxes, for example, which can lead to permanent differences in relative prices across cities. Common time effects, the θ , which we cannot estimate in a univariate setting, captures 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 off the cross-sectional mean of the real exchange rate each period and employing the transformed observations as the data, which is computationally equivalent to including common time dummy variables in the regression (3).

We employ panel unit root tests proposed by LL and IPS.⁹ The two test procedures differ in their treatment of β_i . LL restrict $\beta = \beta_i$, for all i , while IPS allow the β_i 's to differ across i . But in both cases, the test is for the presence of a unit root in all of the series against the alternative that either $\beta \neq 0$ (LL) or (some) $\beta_i \neq 0$. Maddala and Wu (1997) find that the IPS test has more power than LL. The LL procedure, on the other hand, has the advantage that it provides us with a panel estimator of ρ , which the IPS procedure does not.

The distributional results derived by LL and IPS assume that the error term is independent across individuals and time. Because we model common time effects as a deterministic process, the cross-sectional dependence is removed only asymptotically as the off-diagonal elements of the covariance matrix are of $O(1/N)$. To control for residual dependence across individuals, 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

⁹ See the Appendix for a description of both procedures.

¹⁰ O'Connell and Wei (1997) suggest 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.

The LL procedure is computationally equivalent to estimating (3) by fixed-effects and common time dummies, allowing for differential degrees of serial correlation across individuals k_i , while constraining $\beta = \rho - 1$ to be identical where where $\rho = \sum_{j=1}^{k_i} \gamma_{ij}$. 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 one based on the panel ‘t-statistic’ associated with $\hat{\beta}$, which we label τ . However, since they showed that the sampling properties of τ were superior to that of β , we base our inferences on τ .

The other test that we employ is the group-mean t-statistics suggested by Im, Pesaran, and Shin, which we denote by \bar{t} . The IPS procedure is based on a comparison of Equation (3) estimated with and without all of the β_i ’s restricted to equal zero. For each equation in the panel, the restricted and unrestricted form are estimated yielding the equivalent of a likelihood ratio test and a t-test for $H_0: \beta_i = 0$. These are then averaged across i .

Table 2 displays the results of the LL and IPS tests. We examine both the full sample and a number of subsamples.¹¹ Overall, the tests lead 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.

¹¹ As in the univariate case, we omit a deterministic trend as being inconsistent with the PPP hypothesis we wish to examine.

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)	-10.904	0.000	0.897	0.925	8.891
1918-1955	-7.623	0.002	0.890	0.960	16.980
1956-1995	-10.246	0.000	0.862	0.913	7.615
1936-1955	-9.576	0.002	0.783	0.878	5.327
1956-1975	-9.089	0.000	0.830	0.935	10.313
1976-1995	-9.921	0.000	0.791	0.888	5.835
B. Im, Pesaran and Shin					
Sample	\bar{t}	p-value	$\hat{\rho}$	Adjusted $\hat{\rho}$	Adjusted half-life
Full (1918-1995)	-2.689	0.000	0.882	0.930	9.506
1918-1955	-1.917	0.009	0.860	0.959	16.604
1956-1995	-2.483	0.000	0.833	0.912	7.556
1936-1955	-2.292	0.002	0.718	0.855	4.412
1956-1975	-2.111	0.000	0.779	0.924	8.742
1976-1995	-2.227	0.000	0.764	0.907	7.113

Having obtained evidence that the 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 . As we noted previously, the estimated serial correlation coefficient is biased down in small samples. This leads us to bias-adjust the panel estimates of ρ using a procedure suggested

by Nickel (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 — 8.9 years using LL and 9.5 years using IPS.

Turning to the sub-sample analysis, we note that we continue to be able to reject the unit root null in most cases. This is especially true when we employ the IPS procedure. But the pattern of the adjusted half-life estimates is somewhat puzzling. As we mentioned in the context of the univariate analysis, 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.

It is worth examining the tests over 20 year subperiods extending from 1936–1955, 1956–1975, and 1976–1995 a bit more closely. Point estimates of ρ are largest during 1956–1975. The implied half-life of convergence for the most recent period ranges from 4.3 to 10.3 using LL procedure, and 8.7 years to 4.4 years from the IPS results, but in both cases it is the early period that exhibits the fastest convergence. This last period corresponds roughly to the period studied in international PPP studies and the Parsley and Wei study, and here we estimate half-lives from 5 to 7 years.

To summarize the panel results, regardless of the econometric model, strongly reject the hypothesis that all real exchange rates between the U.S. cities in our sample relative to Chicago contain a unit root. While the unit root hypothesis is rejected, relative prices levels are

¹² Nickel's formula is $\text{plim}_{N \rightarrow \infty} (\hat{\rho} - \rho) = (A_T B_T) / C_T$, 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.

very persistent. We estimate half-lives to convergence of approximately 9 years. It is interesting to ask why these estimates are so large. The next section pursues this line of inquiry.

3 Additional Characteristics of the Data

In this section, we explore other features of these price-indices to gain additional perspective on why convergence is so slow. In section 3.1, we examine the role of distance between two cities as a determinant not only for the size of the real exchange rate, but possibly as a determinant 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) in 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 locations that are farther apart exhibit statistically significantly higher volatility of log relative price levels. The point estimates of the slope coefficient in regressions of the volatility of log relative price changes on our measures of distance are positive, but not statistically significantly different from zero. Figure 3 plots the volatility of the real exchange rate for city i against its distance to Chicago.

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 ρ , τ , and 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.

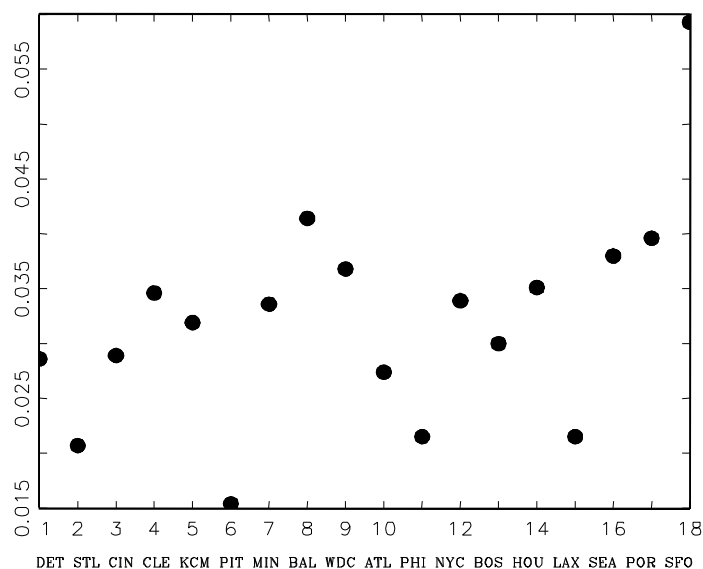
Table 3: Distance as an Explanatory Variable

Dependent Variable	Ln(distance)		Regressor	
		R^2	ln(ln(distance))	R^2
$V(q)$	$6.47 \cdot 10^3$ (2.080)	0.164	0.043 (2.049)	0.158
$V(\Delta q)$	$8.28 \cdot 10^4$ (1.601)	0.084	$5.41 \cdot 10^3$ (1.571)	0.080
$\hat{\rho}$	$4.36 \cdot 10^3$ (0.152)	-0.061	0.025 (0.129)	-0.061
τ	-0.195 (-0.070)	-0.062	-0.177 (-0.095)	-0.062
h	1.979 (1.409)	0.055	12.828 (1.358)	0.047

Notes: $V(q)$ =volatility of log real exchange rate relative to Chicago, $V(\Delta q)$ =volatility of change in log real exchange rate, $\hat{\rho}$, τ , h are estimated ρ , studentized coefficient, and implied half life from univariate ADF regressions.

Figure 3: Volatility of Relative Price Levels

Volatility of Log Real Exchange Rate
Chicago is Numeraire



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

In this section, we examine the data for evidence of 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 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.

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 displayed in table 4.

Table 4: Nonlinear Adjustment

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

As can be seen, we estimate $\tilde{\rho}_l$ 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 and $\tilde{\rho}_l = \tilde{\rho}_s = 0.955$ is 0.110. 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 Nontraded Goods in the Price Index

In this section, we investigate the role of non-traded goods prices in the overall price index in generating real exchange rate persistence. 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

$$q_{it} = \ln \left(\frac{P_{it}^T}{P_{0t}^T} \right) + \phi \left(\ln \frac{P_{0t}^T}{P_{0t}^N} - \ln \frac{P_{it}^T}{P_{it}^N} \right) \quad (4)$$

¹³ The modified LL regression,

$$\tilde{e}_{i,t} = \delta_1 \tilde{v}_{i,t-1}^{small} + \delta_2 \tilde{v}_{i,t-1}^{large} + \tilde{\varepsilon}_{i,t}$$

where $\tilde{v}_{i,t-1}^{large}$ is the largest x percent of the observations and $\tilde{v}_{i,t-1}^{small}$ are the other $100-x$ percent of the observations. 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.

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.

If PPP holds for traded goods, the first term in (4) is $I(0)$. Nonstationarity or high persistence in the relative price of tradables to nontradables across cities cause similar behavior in the log real exchange rate.

The Balassa-Samuelson approach explains the behavior of the relative price of tradables to nontradables by biased technological progress. Technical progress results in higher productivity growth in the traded goods sector which is comprised more heavily of capital intensive manufactured commodities while nontraded goods are predominately labor-intensive services. Productivity growth raises the real wage. But because non-traded goods production is labor intensive, the relative price of traded goods falls.

This point can be made explicit by the following example. Suppose that the sectoral production function is Cobb-Douglas. Competition in regional factor markets imply that sectoral real wages are equated to the sectoral marginal product of labor. Equating the nominal wage across sectors implies

$$\ln \left(\frac{P_{it}^T}{P_{it}^N} \right) = \ln \left(\frac{\alpha_N}{\alpha_T} \right) + \ln \left(\frac{X_{it}^N}{X_{it}^T} \right) \quad (5)$$

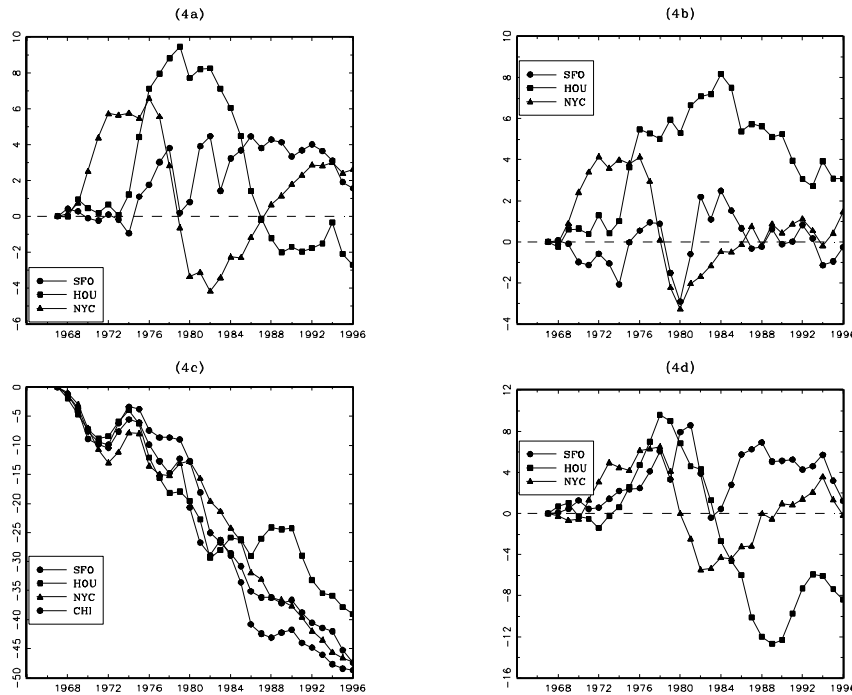
where α_j is labor's share in sector $j = N, T$, and X_{it}^j is average labor productivity in sector $j = N, T$. The Balassa-Samuelson approach implies that real exchange rate behavior can be explained by regional differences in relative labor productivity.

We begin our analysis of the role of nontraded goods prices in the price level by examining the components of the real exchange rate in equation (4). Here, we employ BLS price series on services as our measure of nontraded goods prices, and prices on commodities as our measure of traded goods prices. The availability of these component prices begins in 1966.

Selected price series for San Francisco, Houston, New York, and the numeraire Chicago are illustrated in figure 4. Figure 4a displays the log real exchange rate for Chicago over the available sample period. Casual observation from figure 4b reveals that the deviation from PPP for traded goods appears to be as persistent as the deviation from PPP defined over the general price level. Figure 4c shows a pronounced trend in the relative price of tradables for each city. This is exactly the pattern one would expect to see as income levels rise over time. Finally, figure 4d plots the relative price of traded to nontraded goods differential— the last term in equation (4)— with Chicago as the numeraire from which it can be seen that these series display correlated movements with the real exchange rates in figure 4a.

Table 5 reports results from LL and IPS tests. ‘Traded’ is the log deviation from PPP using traded goods prices with Chicago as numeraire. ‘Nontraded’ is the analogous deviation defined over the price of services. ‘Traded/Nontraded’ is the log relative price for each city, whereas ‘Relative Traded/Nontraded’ is the second term in 4.

**Figure 4: (4a): Chicago Real Exchange Rate;
 (4b): Chicago Traded Goods Real Exchange Rate;
 (4c): Log Relative Price of Traded to Nontraded Goods;
 (4d): City Differentials in Log Relative Price of Traded to Nontraded Goods. 1966–1995.**



We recalibrate our estimates of convergence over the sample extending from 1966 to 1995 by performing the LL and IPS tests over this sample on q_{it} . Both the LL and IPS tests continue to reject the unit root over the shortened sample period. The deviation from PPP defined over traded goods is quite persistent and the unit root cannot be rejected under the LL test. Canzoneri *et al.* (1997) found similar results in their international study of OECD countries. Surprisingly, the unit root can be rejected in the deviation from PPP defined over services as it were (labeled nontraded). The unit root cannot be rejected in the differential of this relative price relative to Chicago (Relative Traded/Nontraded), however. While both components of the real exchange rate are persistent, evidently, the behavior of the traded/nontraded goods component does not appear to be sufficient to dominate the general properties of the real exchange rate.

Table 5: Panel Unit Root Tests on CPIs, Indices of Traded Goods Prices, and Nontraded Goods Prices 1966-1995.

Sample	τ	p-value	$\hat{\rho}$	Adjusted $\hat{\rho}$	Adjusted half-life
A. Levin-Lin					
Real Exchange Rate	-8.573	0.000	0.843	0.924	8.769
Traded	-6.196	0.019	0.778	0.963	18.385
Nontraded	-8.306	0.000	0.850	0.932	9.843
Traded/Nontraded ^a	-7.172	0.000	0.812	0.888	5.835
Relative Traded/Nontraded	-5.473	0.199	0.903	0.999	692.80
Sample	\overline{LR}	\bar{t}	$\hat{\rho}$	Adjusted $\hat{\rho}$	Adjusted half-life
C. Im, Pesaran, Shin, no trend					
Real Exchange Rate	-2.398	0.000	0.803	0.909	7.265
Traded	-1.953	0.009	0.778	0.897	6.360
Nontraded	-2.205	0.001	0.826	0.904	6.868
Traded/Nontraded ^a	-1.953	0.001	0.757	0.871	5.021
Relative Traded/Nontraded	-1.500	0.367	0.871	0.999	n.a.

^a: Regression includes linear trend.

5 Conclusion

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, we find that deviations from city PPP are substantially more persistent than deviations from international PPP. 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.

Our results differ from the international evidence on another dimension. Mussa (1986)

argued that real exchange rate volatility between cities within a common currency area behaved like national real exchange rates under a fixed exchange rate regime. Recently, Parsley and Popper (1997) found that international convergence rates towards PPP were invariant to the nominal exchange rate regime. Understanding the source of these discrepancies will form the focus of future research.

Appendix

A1. The Levin and Lin Test

The Levin and Lin (LL) test proceeds as follows:

1. Control for 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. Run the following univariate regressions for each city
 - (a) $\Delta \tilde{q}_{i,t}$ on a constant, trend, and k_i lagged values of $\Delta \tilde{q}_{i,t}$. Choose the k_i lag lengths by Campbell and Perron's (1991) top-down procedure. Call the residuals from the regression $\hat{e}_{i,t}$.
 - (b) $\tilde{q}_{i,t-1}$ on the same variables in part (2a) above. Call the residuals from this regression $\hat{v}_{i,t-1}$.
 - (c) $\hat{e}_{i,t}$ on $\hat{v}_{i,t-1}$ (no constant). Call the residuals from this regression $\hat{\epsilon}_{i,t}$. 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 $\hat{v}_{i,t-1}$. Call the normalized values $\tilde{e}_{i,t} = \hat{e}_{i,t} / \hat{\sigma}_{ei}$ and $\tilde{v}_{i,t-1} = \hat{v}_{i,t-1} / \hat{\sigma}_{ei}$.
3. Run the *panel OLS* regression $\tilde{e}_{i,t} = \beta \tilde{v}_{i,t-1} + \tilde{e}_{i,t}$.

Let τ be the studentized coefficient from the panel OLS regression (the reported t-statistic).

The asymptotic distribution of τ is nonstandard. LL show how to make 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.

A2. The Im, Peseran and Shin Test

Im, Peseran, and Shin (IPS) propose a simple test for the panel based on the grouped mean of independent univariate tests. Their test proceeds in three steps:

1. Control for common time effects by subtracting the cross-sectional mean from the observations. The basic unit of analysis is again, $y_{i,t} - (1/N) \sum_{i=1}^N y_{i,t}$.
2. Run unrestricted and restricted versions of eq. (3). Denote the estimates by

$$\text{(Unrestricted)} \quad \Delta q_{i,t} = \hat{\alpha}_i + \hat{\delta}_i t + \hat{\beta}_i q_{i,t-1} + \sum_{j=1}^{k_i} \hat{\gamma}_{ij} \Delta y_{i,t-j} + \hat{\varepsilon}_{i,t}$$

$$\text{(Restricted)} \quad \Delta q_{i,t} = \tilde{\alpha}_i + \tilde{\delta}_i t + \sum_{j=1}^{k_i} \tilde{\gamma}_{ij} \Delta q_{i,t-j} + \tilde{\varepsilon}_{i,t}$$

Estimate the error variances by $\hat{\sigma}_i^2 = (1/T) \sum_{t=1}^T \hat{\varepsilon}_{i,t}^2$ and $\tilde{\sigma}_i^2 = (1/T) \sum_{t=1}^T \tilde{\varepsilon}_{i,t}^2$.

3. The likelihood ratio for individual i is $LR_i = T(\ln \tilde{\sigma}_i^2 - \ln \hat{\sigma}_i^2)$. Form the statistics

$$\text{LR - bar: } \overline{LR} = \frac{1}{N} \sum_{i=1}^N LR_i$$

$$\text{t - bar: } \bar{t} = \frac{1}{N} \sum_{i=1}^N \tau_i$$

where τ_i is the studentized coefficient from the ADF test.

Under the null hypothesis, 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 t-bar statistic which were built allowing for cross-sectional dependence.

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