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Edgeworth Price Cycles in Gasoline:
Evidence from the U.S.

Paul R. Zimmerman
John M. Yun
Christopher T. Taylor

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WASHINGTON, DC 20580

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Paul R. Zimmerman†
U.S. Federal Trade Commission

John M. Yun
U.S. Federal Trade Commission

Christopher T. Taylor
U.S. Federal Trade Commission

Abstract

Studies of gasoline prices in multiple countries have found a repeated sequence of a sharp price increase followed by more gradual decreases. This pattern is linked to Maskin and Tirole's duopoly pricing game and is labeled Edgeworth price cycles. We examine data on average daily city-level retail gasoline prices for 350 cities in the U.S. from 1996-2007. Like others who have examined cycling, we show that a relatively small number of U.S. cities in contiguous upper Midwestern states evidence price cycling. However, our lengthy data set allows us to see that these cities began cycling in 2000. Thus, we can examine prices in cycling and non-cycling cites before and after cycling and, controlling for other factors, find prices are lower in cities that began cycling. We examine station ownership data for a sample of cycling and non-cycling cities and note some relationships between market structure and cycling.

JEL Codes: D4, L44, L81

Key Words: Retailing, Petroleum industry, Pricing, Gasoline, Dynamic oligopoly, Edgeworth price cycles

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† Contact author, 600 Pennsylvania Avenue NW, M-8059, Washington DC, 20580. Phone: (202) 326-3159. E-mail: pzimmerman@ftc.gov.

1 [illegible]

price cycling is a form of retail price war, as was the case with prior documented episodes of price cycling in the U.S. during the 1970's.[1]

Additionally, we address the question of what factors explain why city-level retail gasoline prices cycle. While we do not have a time series of market structure variables, we examine a cross-section of a sample of cycling and non-cycling cities for the period when cycling began. Prior work has examined factors such as the concentration of independents (Lewis, 2009) and possible price leadership by large retail chains (Speedway and QuikTrip) in the Midwest (Lewis, 2011). Other studies highlight the role of large, branded retailers (*e.g.*, Noel 2007a) or the concentration of major brands and independents with convenience stores (Doyle *et al*., 2010).

Previous research, however, has not separately examined the role of ownership structure amongst branded retailers. Within this group, ownership structures can vary from complete vertical integration (in the case of a refiner's company-owned-and-operated stations) to third-party control (*e.g.*, so-called "open dealer" or "jobber" stations). If, as previous research suggests, it is the centralization (or "coordination") of pricing decisions of branded retailers that facilitates cycling, then refiner company-ops should be correlated with the presence of cycling given that upstream refiners have *direct* control over the pricing patterns at these stations. Using data on ownership concentration, our results confirm this hypothesis; specifically, we find that the ownership concentration of direct refiner-operated stations (but not their raw market share) is correlated with more cycling. On the other hand, the raw share of "independent" retailers (but not their concentration) positively correlates with the presence of cycling. These results also appear consistent with the underlying theory of Edgeworth cycles.

The next section of the paper reviews the literature. The third section details the data and the methodology used to identify price cycles. The fourth section examines the price effects of cycling using a difference in differences estimator. The fifth section examines possible causes of cycling. The sixth section of the paper presents conclusions.

[1] See Allvine & Patterson (1974) and Castanias & Johnson (1993).

2 [illegible]

Most prior studies examining Edgeworth cycles in retail gasoline prices have looked at Australian, Canadian, or U.S. data. In some of the Australian and Canadian cases, researchers have found that cycling is associated with either suggested or confirmed tacit collusion (Wang, 2009) or explicit collusion (Wang, 2008; Erutku & Hildebrand, 2010).

In other cases, researchers have not linked price cycling patterns explicitly to collusion. Eckert (2003) and Noel (2007b) find that cycling

Doyle *et al.* (2010) examine 115 U.S. cities for cycling for a one year period from 2000-2001 and find that cycling tends to be concentrated in the Midwest. The authors focus on concentration of independent gas stations with convenience stores and the presence of brands as potential explanations for the prevalence of price cycling. Their main finding is that the most concentrated and the least concentrated markets are less likely to cycle. They also find some evidence that cities with at least two major brands present are more likely to cycle. Finally, Doyle *et al.* find price cycling cities are weakly associated with lower retail prices.

Finally, Lewis (2011) examines 280 U.S. cities for cycling with data from 2004-2010. He suggests that price leadership by independent gas stations with centralized city-wide pricing, Speedway and QuikTrip, generates the cycling pattern in many Midwestern cities. He also examines Speedway data to show that in a number of cities Speedway tends to lead the price increases.

3 [illegible]

cities will be negative. Additionally, the median change will be larger (in absolute value) when the typical daily price decline is larger.

Table 1 details the geographic coverage of our price data as well as providing summary statics on prices. The states are grouped by PADD (Petroleum Administration for Defense Districts) and subdivisions for PADD 1 (East Coast). The number of cities in each state is listed as well as the mean price and median price change for three periods: (1) the full sample period, 1996-2007; (2) the period before price cycling began, 1996-1999;[3] and (3) the period after price cycling began, 2001-2007. The median first difference for the pre-cycle period, 1996-1999, is mostly zero or close to zero for most states outside of PADD 2 with the exception of a few states in PADD 5. Even for PADD 2 and PADD 5, the median first difference is generally only a few hundredths of a cent less than zero. When examining the median difference for the post-cycle period, 2001-2007, most states outside PADD 2 still have median differences close to zero (although, few are actually equal to zero) while the median difference for many states in PADD 2 are well below zero including Illinois, Indiana, Kentucky, Michigan, Minnesota, and Ohio.

In Table 2, we list each of the 52 cities (grouped into 9 states) in our sample that cycled for at least one year. We use Doyle *et al.*'s (2010) median first difference cutoff value of -0.5 cents or lower to identify the existence of price cycling. We grouped years 1996-1999 together since no cycling occurred during this period. In 2000, 5 cities in Ohio had prices that cycled: Akron, Canton, Columbus, Dayton, and Toledo. Just one year later in 2001, 40 cities across 8 states had prices that cycled.

Broadly, in order to categorize a city as a "cycling" city for purposes of our difference in differences estimation in Section 4, we took the median value of the first difference over the entire post-cycle period, 2001-07, to classify a city as cycling or not.[4] This approach excludes

[3] See the discussion of Table 2 below for details on the identification of the start of price cycling.
[4]

cities that cycled for only a year or two. The r

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if the year is after 2000. The dummy variable [illegible] $_t$ is the interaction of those two dummy variables and takes on a value of one if the city is a cycling city and it is after 2000.[9] Thus, a negative estimate of the coefficient G_3 implies that the regime change from not cycling to cycling is correlated with a decrease in the average price in cycling cities relative to the non-cycling cities. The variables B_i, and F_{it} represent the constant term and the error term, respectively. For all the specifications we used standard errors clustered by state. The other dummy variables represent the year and state fixed effects. The number of years used changes the number of year fixed effects.

Columns (1) and (2) of Table 3 show the estimated effect of cycling using the shorter list of cycling cities for 1996-2010 and 1996-2007, respectively. The results show that prices decreased in cycling cities relative to non-cycling cities by approximately 1 cent per gallon. The effect using the shorter data set lacks statistically significant at conventional levels. Columns (3) and (4) use the longer list of cycle cites and show very similar results to that of the shorter list of cycle cities.

The results presented in columns (5), (6), (7), and (8) are calculated using a smaller control group. No longer are the prices in cycling cities, which in the case of the short list are all in the Midwest and the longer list including some Gulf Coast cities, compared with cities through out the entire country. In these regressions the control cities are located in the same PADD as the cycle cities. The results in column (5) and (6) compare the relative price change in the cycling cities identified using the five percent Markov criteria with the other cities in PADD 2 with the two different time periods. The results show a somewhat larger statistically significant decrease in prices, 1.4 and 1.3 cents per gallon, in cycling cities once they began cycling but it is generally the same order of magnitude as the previous results. The results in column (7) and (8) were

[9] We used the year 2001 for the beginning of cycling since it looks like cycling began in mid 2000 for most cities and all of the cycling cities were cycling by 2001. The analysis was not sensitive to the exact date. As a robustness check we used 2000 as the beginning of cycling and found very similar results.

generated by comparing the longer list of cycling cities to all the cities in PADDs 2 and 3. While the relative price decline is smaller, the effect of the change remains between ¾ of a cent and 1 cent per gallon.

The parameter estimates using the larger and the smaller data set are very similar but the effects are somewhat smaller using the shorter time period and are generally not statistically significant. This difference may be due to two factors. One, there are a little over 20 percent less observations in the smaller data set. Two, the later three years of data include the collapse of the price of crude oil in 2008. Lewis and Noel (2011), Noel (2009a), and Lewis (2009) point out that cycle cities have quicker pass thru of cost shocks than the non-cycle cities. The results using the longer data set would include this quicker pass through of the negative cost shock which would have shown up in the cycling cities.

These results strongly suggest that the advent of price cycling lead to a 1 cent per gallon reduction in relative prices in cycling cities. While other research, *e.g.*, Doyle *et al.* (2010), has suggested based on cross sectional variation that cycling leads to lower prices, we were able to analyze prices before and after cycling in cycling cities. In addition, as others such as Noel (2009b) have mentioned, these are average effects assuming that consumers make uniform purchases over time and do not take advantage of the price cycles. In addition, if consumers can take to be the firms that initiate and "lead down" the market during the undercutting phase of the cycle.

While the presence or concentration of independent gasoline stations may be an important determinant of gasoline price cycling, it is possible that the concentration of vertically integrated stations also plays a significant role. For example, while independent stations tend to drive undercutting, integrated stations might largely explain the other side of the coin: namely, initiation of the relenting phase. The ability to lead market prices upwards after hitting the bottom of a cycle may be a function of being able to set prices simultaneously at a large number of stations (*e.g.*, Noel 2007a), a characteristic that applies especially to fully integrated branded stations.[10] We expand on this theme by examining the influence of large, refiner company-owned-and-company-operated (COCO) networks of retail stations on a city's propensity to cycle. These are the stations at which upstream refiners are able to exert the most direct control over downstream retail prices. Accounting for the presence of both refiner-COCO stations and independent stations networks allows us to examine the separate relative contributions that each makes in determining the presence of city-level retail gasoline price cycling.

In order to examine causes of price cycling, we use data on station ownership characteristics. These data, which are obtained from New Image Marketing, Ltd. for 31 cities (18 cycling plus 13 non-cycling) provide information on brand market shares and ownership structure within the brands.[11] These data reflect a census of gasoline stations in the selected cities.[12] Three

[10] Gas stations that sell branded gas may be owned and operated by individuals who basically operate franchises (lessee-dealer stations); may be owned by the major oil company (refiner-COCO stations); or may be owned by the major oil company and leased to an operator that sets the retail price (open-dealers). Refiners only indirectly set the retail prices posted at their lessee-dealers stations (through the DTW) and open-dealer/jobber stations (through the branded rack price). As discussed below, the extent to which refiners can influence retail prices is almost certainly greater at the former.

[11] The 31 select cities (grouped by state) are as follows—AZ: Phoenix; CA: Los Angeles, San Francisco; CO: Denver; FL: Miami; GA: Atlanta; IL: Chicago, Peoria, Rockford, Springfield; IN: Terre Haute; KY: Lexington, Louisville; LA: New Orleans; MA: Boston; MI: Detroit, Grand Rapids, Kalamazoo; MN: Minneapolis; MO: St. Louis; NJ: Newark; OH: Cleveland, Toledo; TN: Knoxville, Memphis, Nashville; TX: Dallas, Houston; UT: Salt Lake City; VA: Fairfax; WA: Seattle.

[12] It was not possible to obtain the New Image data across all 350 cities used in the previous analyses as the company does not survey most cities. The 31 select cities correspond to all of the available surveys

ownership structures/groups, indexed by O, are reflected in the New Image data: (1) refiner-COCOs; (2) the sum of independent and branded jobber sites;[13] and (3) lessee-dealer sites. Because data on lessee dealer sites is not available for each of the 31 cities, we consider only refiner-COCO and independent stations in the following analysis.[14]

Let $s_{iO}^{(f)} \in (0,1]$ denote the share of total retail gasoline sales made in city i through stations of "flag" (or brand) $f = 1,\ldots,F$ that are operated under ownership structure O.[15] Define

$$H_{iO} = \sum_{f=1}^{F} (s_{iO}^{(f)})^2 \in (0,1] \qquad (2)$$

as the Herfindahl-Hirschman Index (HHI) of "within-group" (*i.e.*, stations of type O) retail gasoline sales in city i. $s_{iO}^{(f)}$ is the share of city-wide gasoline sales sold through stations operating under a given flag-ownership configuration.[16] The possible values of H_{iO} range from a maximum of 1.0 to a minimum based on the specific distribution of the relevant flag shares. The H_{iO} approaches one as the as the number of flags decreases or the disparity in the size between flags (holding the number of flags constant) increases.

Using the above HHIs we estimate the following cross-sectional probit regression:

$$\Pr(C_i = 1) = \Phi(\alpha_O' (\beta_H H_{iO}, \beta_s s_{iO}, \gamma X_i)), \qquad (3)$$

conducted by New Image that could be reasonably matched to our pricing data. For most select cities the census is from 2000 or 2001 with the remainder in 1999. Since our previous results suggest the cities were

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where Cy_i is an indicator taking a value of one if (based on the 10 percent Markov rule) city i is designated as a price cycling city and zero otherwise.[17] The variable

Column (2) shows that the refiner market share coefficient is positive but not significantly related to cycling. However, the independents' market share is positive and statistically significant. A one percentage point increase in this share is associated with a 1.3 percentage point increase in the probability of cycling. Note also that the magnitude of the point not necessarily imply that any given station is appreciably larger (or more significant) than any other, it may not be surprising that the raw share of integrated stations does not correlate with cycling.

Our finding that a greater presence of independent retailers—as measured by their overall market share—generally increases the propensity for cycling, but not so concentration, also aligns with prior research. These results are qualitatively similar to Lewis (2011) despite the somewhat differing classification of "independent" stations.[19] When controlling for state fixed effects and/or flag-specific shares, Lewis finds a marginally significant positive effect of the independents' HHI on cycling or a statistically insignificant negative effect. The overall share of independents, however, is positive and statistically significant in his most fully specified model.

6 [illegible]

Our analysis of U.S. retail price data confirms the finding in the literature that retail price cycling is generally a phenomenon of the upper Midwest. Our analysis is the first however, to detail when cycling started, mid-2000, and that it continues unabated.[20] Depending on the method/criteria for identifying price cycles, there are some cities outside the Midwest that have retail price cycles. In addition we show that the two main methods used in the literature to identify price cycles give very similar result.

With respect to the consequences of gasoline price cycles, we find that the average price in cycling cites declined relative to non-cycling cities once cycling commenced. Using multiple criteria for indentifying cities with price cycles and multiple control groups we show approximately a 1 cent per gallon decline in the average price in cycling cities relative to non-cycling cities once cycling begins. As Noel (2009b) points out, the average price difference for

[19] For instance, Lewis classifies Speedway as an independent dealer, whereas we classify it as refiner-COCO outlet since it is directly owned and controlled by a petroleum refiner (Marathon/Ashland).
[20] We have continued to analyze the retail prices and through the end of 2010 the Midwestern cities that we identify as cycling still have retail price cycles.

cycling cites may underestimate the consumer effects of price cycling since consumers may be able to take advantage of the cycling and make counter cyclical purchases.

With respect to the causes of retail price cycling, we find evidence that the concentration of branded refiner company-owned-and-operated stations is an important determinant of which cities experience gasoline price cycles. Since we have identified that cycling began in 2000, one place to look for an explanation of why cycling began in the Midwest would be events that occurred at or around that time that affected market structure. Lewis (2011) links cycling to the presence of QuikTrip and Speedway/SuperAmerica (SSA) in the region. SSA is a subsidiary of Marathon/Ashland petroleum and was formed when Marathon and Ashland merged in 1998.[21] In addition SSA is headquartered in Ohio and our results in Table 2 suggest that cycling may have begun slightly earlier in Ohio than in other parts of the Midwest. The beginning of cycling in the Midwest is also coincident with the price spike and the subsequent short lived unusually low prices in the region in the summer and fall of 2000.[22] It is possible that the combination of the change in market structure along with supply shocks may have lead to this change in pricing dynamic but it would be difficult to show a causal relationship.

Two facts that would have to be incorporated into the explanation of the origins of price cycling are why has cycling persisted for the last decade and why firms, especially the firms with larger market shares, would want to engage in this behavior since average prices declined with the advent of retail price cycling.

[21] Taylor and Hosken (2007).
[22] Bulow *et al.*, (2003).

R n e

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Noel, Michael D. (2007b), "Edgeworth Price Cycles, Cost-Based Pricing, and Sticky Pricing in Retail Gasoline Markets," *Review of Economics and Statistics* 89: 324-34.

We employ a Markov switching model based upon Neftçi (1984). Let p_t denote the retail price in a given city during week t, which over time is assumed to follow a mean-zero linearly regular stationary process. Define $\{I_t\}$ as a second-order ("two-state") Markov switching process such that

$$I_t \quad \begin{cases} 1 & \text{if} \quad {}_t p_t^{(\ )} \quad 0 \qquad \% \\ 1 & \text{if} \quad {}_t p_t^{(\ )} \quad 0, \qquad b \quad \% \end{cases} \tag{4}$$

where $\%_t$ denotes the first-difference operator.[23] The associated transition probabilities, denoted M_{ij} for $i, j \quad \{0,1\}$, are given by

$$\left.\begin{aligned} M_{11} &\quad \Pr(I_t \quad 1\,|\,I_{t\quad 1} \quad 1, {}_{t\quad 2} \quad 1) \\ M_{00} &\quad \Pr(I_t \quad 1\,|\,I_{t\quad 1} \quad 1, {}_{t\quad 2} \quad 1) \\ M_{10} &\quad \Pr(I_t \quad 1\,|\,I_{t\quad 1} \quad 1, {}_{t\quad 2} \quad 1) \\ M_{01} &\quad \Pr(I_t \quad 1\,|\,I_{t\quad 1} \quad 1, {}_{t\quad 2} \quad 1) \end{aligned}\right\} \tag{5}$$

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Neftçi argues that it is necessary to estimate Q_a when the number of observations contained in the relevant time series is small and when the initial state may contain useful information on the transition probabilities (*e.g.*, when the process I_t does not in fact start at $t \quad 1$, which is usually the case). Neftçi's paper develops a methodology for deriving the limiting probabilities of the initial conditions in terms of the transition probabilities.[24] If, however, the number of observations available in the sample is relatively large (*i.e.*, in an asymptotic sense) the initial state may be treated as a nuisance parameter (Billingsley, 1961). Since the number of daily city-specific price observations available in our dataset covers over a twelve year period (1996-2007), ignoring the influence of the initial condition is likely to be reasonable.[25] With $Q_a \quad 0$, the maximum likelihood estimates (MLEs) of the four unknown parameters $- \quad [\,_{00}M_{11},\ _{10}M_{01}]$ are obtained by setting the four score equations of the log-likelihood function equal to zero and solving the parameters in terms of the transition counts.[26] The general form of the score equations is given by

$$\frac{sL}{_j} \quad \frac{_j}{M_j} \quad \frac{G_j}{1s \quad _jM} \quad 0. \qquad Z \qquad M \tag{7}$$

Solving Eq. (7) in terms of M_j gives

$$\frac{_j}{_j} \quad \frac{_j}{1 \quad _j}$$

$$_j(1 \quad _j) \quad _j\,_j$$

$$_j(_j \quad _j) \quad _j$$

$$_j \quad \frac{_j}{_j \quad G_j} \quad \hat{}_j \quad fZ$$

Testing for the presence of Edgeworth price cycles (asymmetry) in gasoline prices involves testing the null hypothesis $H_0 : \lambda_{00} = \lambda_{11}$ against the (two-sided) alternative $H_1 : \lambda_{00} \neq \lambda_{11}$.

Hypothesis Testing

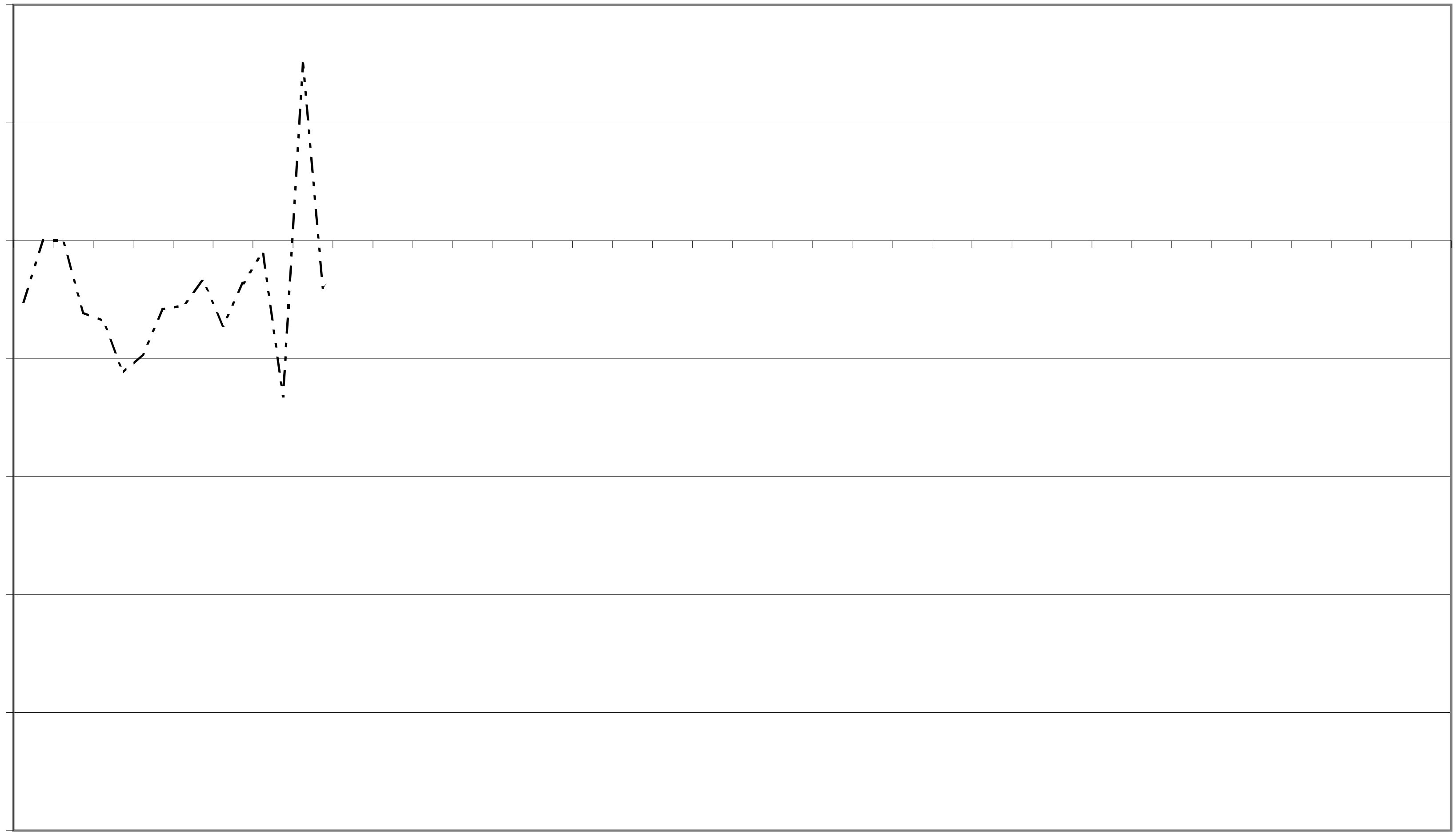
Neftçi demonstrates how the test for asymmetry can be evaluated by using the estimate of the transition probabilities to construct a confidence region (ellipsoid), the center of which corresponds to the MLEs of λ_{11} and λ_{00}. All points within the confidence ellipsoid represent the true value of the latter estimate for a given confidence level.[27] However, Sichel (1989) demonstrates that this procedure "has low power and is sensitive to noise" (p. 1259). Specifically, he shows that Neftçi's test may fail to identify asymmetry that is actually present, and instead applies an asymptotic *t*-test that appears to give higher power.

McQueen and Thorley (1991) test the symmetry hypothesis in their data by considering asymptotic Lagrange Multiplier, Likelihood Ratio, and Wald tests (all of which are approximately equal for large sample sizes). They note that: "The choice of test statistics is normally a matter of computational convenience" (p. 256). Again, the length of our time series data suggests that we can rely upon the direct analytical solutions for the MLEs and (asymptotic) variances of the Markov transition probabilities. This fact motivates the use of the Wald test since it uses the MLEs and asymptotic variance estimates of the *unconstrained* log-likelihood function, which correspond to the "unrestricted" estimates obtained by appealing to Eqs. (8) and (9). The computed value the Wald test under H_0 is given by:

$$\frac{(\hat{\lambda}_{00} - \hat{\lambda}_{11})^2}{\sigma^2(\hat{\lambda}_{11}) + \sigma^2(\hat{\lambda}_{00})} \sim \chi^2_1 . \qquad (10)$$

This test statistics is used to determine whether there is a statistically significant Edgeworth price cycling effect within a given city over the sample period.

[27] *See* Neftçi (1984, pp. 315-318) for the formula used to construct the confidence ellipsoid and further discussion of this test.



[illegible]	State	No. of Cities	Mean Price	Median 1st Dff	Mean Price	Median 1st Dff	Mean Price	Median 1st Dff
AL		[illegible]	124.84	r0.05	75.54	r0.01	154.48	r0.10
[illegible]		[illegible]	127.22	0.00	77.44	0.00	156.85	r0.01
[illegible]	CT	4	127.02	0.00	76.36	0.00	157.01	r0.02
	MA	4	130.32					

			0 r2 7		0 r9		02 7	
		No. of	Mean	Median	Mean	Median	Mean	Median
a	State	Cities	Price	1st Dff	Price	1st Dff	Price	1st Dff
a5	4	7	138.87	r0.03	88.83	r0.03	168.85	r0.03
(W)	AK	1	148.29	r0.01	98.22	0.00	178.86	r0.05
	AZ	5	137.37	r0.03	88.65	r0.02	166.97	r0.03
	CA	25	141.13	r0.04	89.55	r0.04	171.91	r0.05
	HI	1	148.37	0.00	102.11	r0.01	177.47	0.03
	NV	2	134.29	r0.02	83.97	0.00	164.32	r0.02
	OR	4	136.98	r0.03	89.64	r0.03	165.29	r0.03
	WA	9	133.18	r0.01	85.16	r0.01	161.92	r0.01

Bloomington rNormal	0.01	r0.24	r0.66	r0.48	r0.63	r0.50	r0.73	r0.70	r0.64	yes	**
Champaign rUrbana	r0.01	r0.11	r0.44	r0.47	r0.49	r0.33	r0.57	r0.52	r0.50		**
Chicago	0.01	r0.08	r0.50	r0.24	r0.34	r0.26	r0.27	r0.22	r0.26		*
Decatur	r0.03	r0.39	r0.39	r0.40	r0.43	r0.30	r0.46	r0.57	r0.52		**
Peoria rPekin	r0.04	r0.25	r0.77	r0.62	r0.59	r0.22	r0.40	r0.43	r0.30		**
Rockford	0.00	r0.13	r0.74	r0.72	r0.45	r0.35	r0.73	r0.43	r0.49	yes	**
Springfield	r0.04	r0.28	r1.13	r0.69	r0.90	r0.63	r0.74	r0.93	r1.21	yes	**
[illegible] A											
Bloomington	0.00	r0.13	r0.26	r0.44	r0.47	r0.66	r0.60	r0.69	r0.87	yes	**
Cincinnati	0.00	0.01	r0.11	r0.09	r0.34	r0.40	r0.57	r0.41	r0.33		*
Elkhart rGoshen	r0.01	r0.26	r0.59	r0.68	r0.73	r0.73	r0.89	r1.13	r0.98	yes	**
Evansville rHenderson	0.00	r0.32	r0.42	r0.21	r0.11	r0.20	r0.31	r0.46	r0.76		*
Fort Wayne	r0.01	r0.49	r0.84	r0.83	r0.65	r0.66	r0.80	r0.83	r0.92	yes	**
Gary	0.00	r0.29	r0.70	r0.54	r0.67	r0.64	r0.75	r0.67	r0.75	yes	**
Indianapolis	0.00	r0.24	r0.85	r0.95	r1.20	r1.29	r1.23	r1.31	r1.38	yes	**
Kokomo	0.03	r0.19	r0.56	r0.38	r0.47	r0.54	r0.60	r0.66	r0.87	yes	**
Lafayette	0.01	r0.19	r0.50	r0.61	r0.66	r0.60	r0.72	r0.79	r0.79	yes	**
Louisville	0.03	r0.24	r0.49	r0.37	r0.59	r0.33	r0.45	r0.54	r0.55		**
Muncie	r0.06	r0.32	r0.54	r0.53	r0.76	r0.93	r0.67	r1.10	r1.19	yes	**
South Bend	r0.01	r0.29	r0.69	r0.63	r0.59	r0.60	r0.88	r1.01	r0.97	yes	**
Terre BHaut											

[illegible]											
Minneapolis–St. Paul	r0.03	r0.23	r0.81	r0.77	r1.14	r0.97	r0.98	r1.05	r0.99	yes	**
Rochester	0.00	r0.01	r0.58	r0.37	r0.42	r0.42	r0.29	r0.24	r0.09		**
St. Cloud	0.01	r0.21	r0.69	r0.14	r0.19	r0.12	r0.15	r0.44	r0.45		

[illegible]

Kansas City											

[illegible]	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cycle * After	r1.14*	r0.74	r1.05*	r0.68	r1.42*	r1.32*	r0.94*	r0.74

[illegible]]	(1)	(2)	(3)	(4)
HHI r refiner company owned & operated sites	1.250**		1.424**	1.516**
	(2.50)		(2.01)	(2.08)
Market share r refiner company owned & operated sites		0.036	0.067	0.670
		(0.08)	(0.13)	(0.86)
HHI r independent and jobber sites	1.440		3.13438	4.250
	(0.41)		(0.87)	(1.09)
Market share r independent and jobber sites		1.270**	1.366**	2.075**
		(2.19)	(2.11)	(2.25)
Median household income				r2.96E r05
				(1.50)
Population density				1.290E r05
				(0.04)
Total population				4.647E r04
				(0.51)
Prob > Wald Chi squared (Null: Coefficients are jointly zero)	0.0434	0.0481	0.0912	0.0991

Pseudo R rsquared œí´ 4 !P 1 J 30W]ª&g´ 0

(2.50)2.96E