Empirical Evidence on the Competitive Effects of Mergers in the Gasoline Industry

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Introduction

Horizontal mergers that significantly increase concentration can increase the ability of the merging firms to restrict their output and increase their prices and profits, even though competitors respond with their own output increases – a phenomenon sometimes termed "unilateral effects" since there is no explicit collusion. They can also improve the profitability of coordinated output restrictions. On the other hand, horizontal mergers can result in efficiencies (like scale economies) that reduce costs and prices. All three aspects may be present in a pending merger.

Vertical mergers do not increase concentration but change the behavior of the merging parties in both the upstream and downstream markets in which they compete. If an upstream and a downstream firm merge, it may be in the interest of the new firm to raise wholesale gasoline prices charged to downstream rivals. Depending on the structure of the wholesale market, this strategy could ultimately lead to foreclosure of that market to some rivals. In contrast, if a vertical merger reduces transactions costs or eliminates double marginalization then retail prices may fall, to the benefit of consumers.

The competitive effects of mergers in the gasoline industry has been a topic of great interest and controversy. For example, participants at the FTC Refined Petroleum Product Price hearings differed in their opinions about the effects of mergers on gasoline prices. There was general agreement that there has been substantial consolidation of the gasoline industry in the past two decades, but the extent to which this is attributable directly to mergers rather than the elimination of marginally competitive firms was less clear. There was much more disagreement about the competitive effects of mergers. Some participants believed mergers had reduced competition in an industry where there was too little competition to begin with. Others argued that mergers were part of an efficient, competitive response to changing cost and regulatory conditions.[1]

The competitive effects of mergers in the gasoline industry bear heavily on current policy-making. At the national level there have been several large mergers or acquisitions in recent years (British Petroleum and Amoco, British Petroleum and Arco, Exxon and Mobil, Chevron and Texaco, and Phillips and Conoco) and scores of smaller such transactions. Many of these were approved only with FTC-mandated divestiture

[1] The Senior Assistant Attorney General for California, Tom Greene, saw a "striking increase in concentration" and former Senator and Chairman of the Consumer Federation of America, Howard Metzenbaum, cited "the lack of meaningful competition in the oil industry" and stated, "a wave of mergers drove this consolidation and concentration." However, industry expert Phillip Verleger commented, "Petroleum products (prices) are more volatile and higher. Mergers in the industry are not—I repeat not—the primary cause. The proliferation of blends ordered by the EPA has reduced storage capacity and increased volatility." John Cook, the director of the Energy Information Administration of the Department of Energy, observed, "When people observe that prices seemingly increase faster than they decrease, this generally leads to the speculation that perhaps market forces don't explain all the variation so there must be some type of anticompetitive behavior at work. Our analysis suggests otherwise."

requirements in markets where the merger was deemed to have anti-competitive effects. At a more local level, nearly every state, and some large cities, have at least considered divorcement legislation that prohibits ownership of retail gasoline outlets by refineries. Such laws are currently in force in six states and the District of Columbia. Informed policy-making requires reliable evidence on the impacts of alternative policy choices on prices, and (ideally but more ambitiously) on consumer welfare.

This report summarizes nine studies providing such evidence and assesses the reliability of the empirical work in each. Nine is not a large number, but taken together this work addresses a wide variety of issues using different kinds of data and methodology. Four of these studies examine horizontal concentration or mergers, and the other five are concerned with vertical integration. Four of the studies utilize national, cross-section time-series data, while the other five examine specific states and events. With respect to methodology, five of the studies set out to measure the impact of past changes in structure, either mergers or divorcement legislation; three examine the relationship between measures of horizontal or vertical concentration, on the one hand, and gasoline prices, on the other; and one utilizes a structural model of consumer, retailer and refiner behavior to predict the impact of hypothetical but specific mergers. Each approach has its strengths and limitations, and studies vary in the degree to which they realize the potential of the methodology used.

Evidence on horizontal mergers

U.S. General Accounting Office Accounting Office (1986)

In 1986 the U.S. General Accounting Office (GAO) studied the relationship between price and horizontal concentration in the wholesale gasoline market. The work was undertaken because of concerns about the simultaneous increase in gasoline prices, decrease in crude oil prices and two major mergers in 1985. The mergers—Texaco's purchase of Getty and Chevron's purchase of Gulf—were allowed to proceed after the FTC required divestiture of refineries and wholesale and retail outlets in geographic regions with significant overlap.

Given the price volatility in petroleum markets, it would be very difficult at best to determine the impact of one or two specific mergers on wholesale or retail gasoline prices, and the GAO study does not take this approach. Instead, it models the wholesale gasoline price in each state as a function of that state's horizontal concentration in wholesale gasoline. Concentration is measured by the Herfindahl-Hirschman Index (HHI).[2] The study employs a regression model[3] and uses aggregate data for each of the 48

[2] This is the sum of squared percentage market shares of each wholesaler; see Carlton and Perloff (2000) p 247.

[3] The study also presents estimates of a structural model that distinguishes between demand and supply. This was done in an effort to isolate possible supply or demand shifts in different years, a question distinct from that of the relationship between HHI and price. The findings in these models were consistent with

continental states and the District of Columbia, for each of the 32 months from February 1983 through September 1985. (The report was issued in September 1986.) Thus the GAO data set has a classic cross-section time-series structure.

Assessment of the technical work in the GAO report is hampered by the fact that the report's documentation of data and estimation methods does not meet generally accepted academic standards.[4] State gasoline prices each month correspond to the first sale of gasoline in a state for consumption in that state, as compiled by the Energy Information Administration (EIA).[5] The report provides no indication of how prices were actually constructed, but it is clear that there are distinct prices for each state and month, and that they were seasonally adjusted by GAO.[6] HHIs were based on EIA wholesale market share data, but were prepared only annually—not monthly—by state.[7] In the study's regression model, wholesale gasoline price is also a function of the quantity of wholesale gasoline sales in the previous month, per capita income, the price of crude oil, and the difference between the price of home heating oil and the price of crude oil.[8] The report provides no sources for these data, nor does it indicate which of these measures are specific to a state and which are not, nor which are truly monthly measures and which are annual or are interpolated to monthly values. It appears, but is never stated explicitly, that many variables (including HHI and gasoline wholesale price) are transformed to logarithms. All of these details are critical in interpreting the implications of the reported estimates for the question of the relationship between concentration and price. In addition to these variables, the model in the GAO study also includes indicator variables for summer and for winter, and for the years 1984 and 1985.

The study estimates two variants of the model. The second variant includes, in addition to these explanatory variables, indicator variables for each state. Consistent with the econometrics literature, the study refers to this as an "error components" model. As this literature recognizes,[9] the relationship in an error components model differs in a fundamental way from that in a regression model. A regression model captures the impact of persistent geographical differences in the explanatory variables across states (like the HHI) on the outcome variable (wholesale gasoline prices, in the GAO study). In an error components model these persistent differences are explained away by the state indicator variables: the model captures only the systematic effects of explanatory variable changes over time on outcome variable changes over

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those in the regression and error components models, and they do not overcome the difficulties with those models discussed here.

[4] This may be due, in part, to time limitations imposed on the preparation of the GAO report; see GAO (1986), p 38.

[5] GAO (1986), p 39.

[6] GAO (1986), p 37.

[7] This fact emerges only in GAO's response to one of the FTC comments on their study; see GAO (1986), p 62.

[8] The last variable is included as a measure of the profitability of producing refined products other than gasoline (GAO (1986), p 41).

[9] Stock and Watson (2003), Section 8.3, provides a clear and simple exposition. For more technical details see Greene (2003), Sections 13.2-13.5.

time, in each state. Thus the coefficient on HHI in the regression model provides a summary answer to the question, "Controlling for other relevant factors, how has variation in HHI over states and time systematically affected the wholesale price of gasoline?" whereas in the error component model the question answered is, "Controlling for other relevant factors, how have variations in HHI over time within each state systematically affected the wholesale price of gasoline?" The advantage of an error components model is that it controls for omitted relevant factors that differ across states (but not over time). The disadvantage is that it there is less variation in the data available to accurately estimate the impact of variables of interest (like HHI) on the outcome (wholesale gasoline price).

These features of the model, by themselves, render any inference from the GAO study about the relationship between concentration and wholesale price rather difficult. The GAO regression model provides a coefficient estimate of 0.013 for HHI (Table I.1, p 46). Presuming, as the text of the report appears to do, that HHI and prices are measured in logs, this implies an "elasticity" of 1.3% of wholesale price with respect to HHI: for example, if HHI rises from 2000 to 3000 points, prices increase by 0.53%. This is an extremely small effect; according to the GAO report it is statistically significant, a point to which we shall return shortly. In the error components model the HHI coefficient is 0.041, over three times larger but still quite small. According to the reported standard errors, this value is statistically significantly greater than the estimate of 0.013 in the regression model. The interpretation of these results, however, is at best clouded by the nature of both the regression and error components models. Recall that both models remove systematic differences in the calendar years 1983, 1984 and 1985 by means of indicator variables, and that the HHI values are forced to be the same within each calendar year in each state. Thus both models exclude any nationwide effect of changes in HHI on changes in price in the 1983-1985 period. For reasons already discussed, the error components model also excludes systematic relations between HHI and wholesale prices across states. Thus, by construction, the GAO error components model focuses on the impact of year-over-year changes in state HHIs that are uncorrelated with year-over-year nationwide changes in HHIs. One would expect these effects to be small compared to the more important systematic changes in concentration and price over years or across states. Thus the very small impact of concentration on price found with the GAO model can therefore be ascribed primarily to the statistical methodology of that model.

All of the coefficient estimates in the GAO model are statistically significant (save one, the seasonal "winter" indicator variable in the error components model) and most are highly significant. For example, the elasticity of wholesale gasoline price with respect to income per capita is estimated to be 0.066 in the regression model, with a standard error of 0.007 (a t-ratio of almost 10), and in the error components model it is found to be 0.257 (standard error 0.017, t-ratio 15). Genuine statistical significance connotes informative data, but informative data appear lacking here. It is not even clear whether GAO was using

monthly or annual per capita income by state; there are no reliable monthly state data in any event.[10] Given the included indicator variables for state, calendar year, and season, the model has already eliminated most variation in income, and what is left is precisely that which cannot be measured with any accuracy. Yet the results claim to have estimated elasticity accurately to within a few percent.[11]

In interpreting the GAO findings, the possibility that the reported high statistical significance is spurious must be taken into consideration. The GAO report used conventional least-squares standard errors, which are valid if the error terms in a regression equation are uncorrelated. As in most cross-section time-series models, this assumption is presumptively untrue: an unusually high error term for one state in a given month will typically be accompanied by unusually high ones in other states (cross-section correlation), and for the same state error terms in adjacent months are almost certainly serially correlated (autocorrelation). The GAO report indicates that serial correlation exists, but dismisses the problem, noting that coefficient estimates are about the same when the problem is taken into account in a more sophisticated model.[12] As all econometrics texts point out, however, the issue is not one about coefficient estimates (which remain unbiased) but rather about standard errors[13], which are typically and often dramatically underestimated in these circumstances.[14] If, as is likely the case here, standard errors are understated by a factor of four or more, then a proper restatement of the empirical findings in the GAO report would indicate no significant relation between HHI and wholesale gasoline prices. The extent of the understatement in the GAO model could only be assessed beginning with the data used in the GAO report, and applying econometric methods that cope with cross-section correlation and autocorrelation. The outcome of that exercise would indicate whether there is, in fact, a significant relation between HHI and wholesale gasoline price in the GAO model.

An overriding reservation in any interpretation of the findings in GAO (1986) is that concentration is the endogenous outcome of the same economic forces—some measured and others not—that determine the wholesale price of gasoline. The impacts of this problem are difficult to disentangle from the way the model limits the impact of systematic geographic and persistent changes over time. One might expect, for example, that smaller and more isolated markets would attract fewer wholesalers, leading to higher concentration ratios, but that the characteristics of these markets themselves imply higher costs and prices not captured in the model. Given that the error components model sweeps away such changes, and that the geographical unit of the state is not the appropriate market in the first place, it is difficult to assess either

[10] State level income data are published quarterly, not monthly, by the Bureau of Economic Analysis.
[11] The fact that the estimates are dramatically different in the regression and error components models implies that there are omitted, time-invariant variables that are strongly correlated with persistent differences in state per capita incomes.
[12] GAO (1986), p 47.
[13] See, for example Stock and Watson (2003), pp 253-254, or Greene (2003), Section 10.2.
[14] See Davidson and MacKinnon (1993), Section 10.2, and references therein.

the extent or direction of the impact of endogenous HHI on the GAO model. The econometric problems posed by endogeneity can be addressed using instrumental variables methods. These methods presume the existence of other variables (the "instruments"), not yet included in the model, that are related to concentration but not to other unmeasured economic forces that determine the wholesale price of gasoline. These characteristics are essential to the efficacy and reliability of instrumental variables methods. However, neither the GAO report nor any of the other papers reviewed here have suggested appropriate instruments for concentration in a gasoline pricing model.

Chouinard and Perloff (2002)

Hayley Chouinard and Jeffrey Perloff utilize a cross-section time-series data set to investigate the impact of 8 producer mergers in 5 states and 27 retail mergers in 19 states on wholesale and retail gasoline prices. Like the GAO report, this study employs a reduced-form regression model estimated with monthly data (but for a longer period, January 1989 through June 1997) for the 48 contiguous states and the District of Columbia. The study utilizes only the error components form of the model:[15] that is, it incorporates an indicator variable for each state.

This study incorporates a more extensive list of explanatory variables than does the GAO investigation. There are variables driving demand (per capita income, vehicles per capita, share of population in metropolitan areas, miles per gallon, speed limits), costs (crude oil current and two lags), taxes, indicators for three kinds of pollution laws, and the fraction of stations leased and company operated. The last two variables are available only at the national level, and per capita income is interpolated from annual state data and national monthly data. Most important, there is an indicator variable for each of the 35 mergers. For a given merger and any "affected state"[16] the indicator variable is assigned the value zero before the completion of the merger and one in the month the merger is completed and for all months thereafter through the end of the sample; for all unaffected states the value is always zero.

The study includes these variables because its objective is to determine the relative importance of demand, costs, and market power in explaining variation in retail and wholesale gasoline prices over time and across geographic locations.[17] One of its principal conclusions is, "Tax variations and mergers contribute substantially more to geographic price differentials than do price discrimination, cost factors or pollution

[15] The study uses the term "fixed effects model," because of the way the model is estimated. Error components models may be estimated as fixed effects or random effects models (Greene (2003), Sections 13.3 and 13.4). Both the GAO and Chouinard-Perloff studies use error components models with a fixed effects estimator. A fixed effects model permits error components correlated with explanatory variables whereas a random effects model does not. A fixed-effects model is presumptively more appropriate; the Chouinard-Perloff study conducts a test that confirms this fact (Chouinard and Perloff (2002), p 7 fn 7).
[16] Chouinard and Perloff (2002) p 7; no exact definition of "affected" is given.
[17] Chouinard and Perloff (2002) abstract.

controls."[18] But given the error components structure included in its regression model—i.e., an indicator variable for each state[19] —the study can say nothing about variation over geographic locations and its conclusions about geographic price differentials are unwarranted. All systematic, time-invariant differences between states are absorbed by these indicators, leaving the model to explain only the impacts of changes in explanatory variables on changes in retail and wholesale gasoline prices in each state.[20] The study repeatedly fails to recognize this property of error components of models.[21]

Despite this limitation, the study could, in principle, yield some useful information about the impact of changes in its demand, cost and market power measures (including mergers) on retail and wholesale gasoline prices. For example, it indicates that crude oil price changes are passed through one-for-one to both wholesale and retail prices.[22] As currently written, however, the report does not list the mergers or the states affected, but provides the following summary[23] of the estimated impacts of 8 producers mergers and 27 retail mergers.

Table 1

Sign and statistical significance of estimated merger effect

	Positive		Negative		Range
	Significant	Insignificant	Insignificant	Significant	(Cents)
Wholesale prices					
Producer merger	1	2	3	2	20.0
Retail merger	4	17	4	2	12.0
Retail prices					
Producer merger	1	2	4	1	9.8
Retail merger	8	9	5	5	16.4

If the standard errors are reliable then these results provide some basis for concluding that mergers may have had some impact on prices. If this is the case, then it is also true that some mergers had positive impacts, and others negative, but the study provides no systematic analysis of characteristics of mergers or the market environment that account for the differing effects. Establishing the reliability of the standard errors must confront the same issues of serial and cross-section correlation that arose in the GAO study,

[18] Chouinard and Perloff (2002) abstract.
[19] Chouinard and Perloff (2002) p 4.
[20] There are a number of specific examples from Chouinard and Perloff (2002) that illustrate this point. For instance, "State specific taxes explained up to 22.2 cents of the difference in retail prices across states." Suppose, contrary to fact, that states had different gasoline taxes but never changed them. In a fixed effects model, like the one Chouinard and Perloff use, it would then be impossible to include state tax as a covariate because it would be perfectly collinear with the indicator variables for the states. Only if some of the states changed their taxes (as they in fact did) is it possible to include state tax and estimate a coefficient for it. What the model is then capturing is not the impact of differences in taxes across states, but the impact of changes in state taxes over time.
[21] See the discussion of the effect of different market power and demand conditions across states, p 4; the discussion of retail price effects, p 14; the discussion of mergers and retail price differentials across states, p 15; and Table 4, p 29, predicting price effects across states.
[22] This may seem unsurprising; by contrast, however, the GAO study found a 183% elasticity of gasoline wholesale price with respect to crude oil price (GAO, 1986, p 46).
[23] Information taken from Chouinard and Perloff (2002) pp 7, 8, and 17.

and when these issues are resolved it is entirely possible the conclusion would be that mergers have no detectable impacts on prices. In the interim, this study provides little information about these effects.

Hastings (undated)

Justine Hastings analyzes evidence of a very different kind about the relationship between horizontal concentration and prices in the retail gasoline market. Early in 1997 Atlantic Richfield Oil Company (ARCO) announced the long-term lease of 260 Los Angeles and San Diego service stations from the independent, unbranded retail chain Thrifty. Thrifty was by far the largest independent gasoline retailer in the two metropolitan areas, and the lease involved nearly all of its stations. As an independent, unbranded retail chain, Thrifty bought from many different wholesalers, including wholesalers of branded gasoline, but product was always sold under the "Thrifty" name. The takeover by ARCO significantly increased ARCO's already major presence in southern California. About two thirds of the Thrifty stations were converted to company-operated ARCO sites, and the others became ARCO lessee-dealer operations, or dealer-owned outlets supplied either by the company or by jobbers. Regardless of ownership details, all appeared the same to the consumer.

Hastings (undated) interprets this change in ownership as a natural experiment. The study supports this interpretation by noting that station locations and characteristics were determined prior to the ARCO long-term lease agreement, and that the transition was completed in a 60-day period following its announcement with no remodeling, expansion or other facility improvements. The study controls for station-specific fixed effects, and separate time effects for each city. It utilizes the retail census of gasoline stations to determine physical and ownership characteristics of stations, and combines these data with readings of sales volumes (from pump meters) and prices (from postings). The data set used includes, in addition to the 260 leased (former) Thrifty stations, about 670 stations that compete with the former Thrifty stations, but are not themselves (former) Thrifty stations. (A station is defined as competing with a (former) Thrifty station if it is one mile or less by roadway from that station. Hastings (undated) reports the results are substantively the same with a one-half mile criterion.) There are price and volume readings from four time periods for each of these stations.

The treatment group consists of the subsample of these stations in a (former) Thrifty retail market. All others—those not in a (former) Thrifty retail market are the control group. Hastings (footnote 12) reports that the distribution of brands in the treatment and control groups is the same, consistent with the interpretation of the ARCO long-term lease as a natural experiment. However, the paper does not present the actual distributions or formal tests, as is conventional in reporting the results of natural experiments. The only other similar information is a map, showing the distribution of (former) Thrifty stations in Los

Angeles. In particular, no information is provided about the distribution of ownership in the treatment and control groups.

The study finds that, before the lease agreement and rebranding, prices at Thrifty-competing stations were from two to three cents lower than in non-competing stations, and that, after rebranding, prices at these treatment stations were from three to four cents higher than at the control stations. Controlling for time and city effects, station and market characteristics, the point estimate of the difference is 5.0 cents, and the accompanying standard error of estimate is one cent. Whether or not the newly branded ARCO station is company- or lessee-operated has no discernible impact on local competitors' prices (Table IV, p 22 and Table V, p 26). The number of competitors in a market decreased if an ARCO station was already in that market, but this does not affect price changes systematically, either (Table VII, p 29). There was a statistically significant tendency for conversion of Thrifty to ARCO to raise prices more for low-share brands than for high-share brands, in the treatment group (Table VIII, p 32). Hastings interprets these results as supporting "the hypothesis that independent competitors decrease prices through increased price competition. When they are replaced with branded competitors, in a market with consumer brand-loyalty, price competition will be softened, and equilibrium prices will increase" (Hastings (undated), p 33).

Hastings (undated) is attractive on many grounds. It identifies very specific retail markets and a very specific change in market structure. It carefully measures price[24] and accounts for the most important covariates. There are no apparent complications with the behavior of error terms to compromise standard errors as there are in the cross-section time-series studies. Its finding of a five-cent change in price differentials is reliable, despite the fact that it is substantial relative to retail margins. At the same time, the specificity of the study limits its implications for larger questions about horizontal competition and prices. As the author has noted, the natural experiment simultaneously removed an independent brand that traditionally competes on price, and introduced a brand that traditionally builds loyalty. It is impossible to separate the effects of these two changes in Hastings' data. In addition, it would be difficult to argue that the price change discovered in Hastings (undated) is a difference between two equilibria. The ARCO takeover was a major event in many local retail gasoline markets, and there could be further responses in those markets, including modification of capital stock, further changes in ownership, and perhaps entry and exit. These market responses might ameliorate the large price change found in Hastings (undated). A re-examination of these same retail markets, several years after the ARCO takeover, would shed valuable light on these questions.

[24] Posted prices were observed at specific points in time, in December 1996 and February, June, October and December 1997. Since posted retail prices can change weekly (or even daily) the Hastings (2000) data set does not contain good information on average prices at any one station. But this is incidental to the main point of the study, which is to examine differences across stations. For this comparison, it is having price observations at the same time that is important.

Manuszak (2002)

Mark Manuszak constructs a structural model in which a small number of upstream refiners sell to a large number of downstream retailers, each of whom faces less than perfectly elastic demand because consumers have preferences over retailer locations. Retailers take wholesale price as given and maximize profits. Taking into account retailers' derived demand as a function of wholesale prices, the latter are then determined in a Nash-Bertrand equilibrium. The model can be extended to allow some retail outlets to be refiner-owned.

The empirical study examines detailed retail outlet data (including station locations and characteristics, as wells as prices and quantities sold broken down by station, level of service, and grade of gasoline) in Maui and Kauai (two of the Hawaiian Islands) over the period 1990 to 1995. There are no wholesale price data.

Using these data, the study estimates the parameters of the structural model. This requires fairly specific assumptions about the distribution of preferences. The paper uses state-of-the-art procedures in choosing these assumptions: many are required in order to produce demand functions that are suited to estimation, but little if anything is known about the sensitivity of results to these choices since there are few practical alternatives. The preference distributions contain unknown parameters, which the study estimates using generalized method of moments (GMM) procedures.

The structural model also incorporates the costs of the upstream refiners, which in turn partially determine wholesale prices. These costs are estimated employing an analysis of variance structure with factors for island, time, grade, service, and refiner. Estimated cost differences are substantial (those for branded wholesale on Kaui average almost twenty cents per gallon less than those for unbranded Kaui wholesale and all wholesale on Maui[25]) and are "implausible" according to the study.[26]

The GMM estimation procedure utilized in the paper has been employed widely in the past two decades and is well understood. It requires that the econometrician find secondary data (known as instruments) that are correlated with the observed random variables in the model, but uncorrelated with the model's unobserved random disturbances. In this study the observed random variables in the model are prices and quantities, and the unobserved random disturbances are the product-specific costs, and mean consumer valuations, of retail gasoline products.[27] (Each combination of station, level of service, and grade of gasoline constitutes a different gasoline product.) For example, the study assumes that the number of stations selling the same brand or product as a given station or the number of stations within a certain distance of a given station, is uncorrelated with consumers' mean preference for that station as well as that

[25] Manuszak (2002) Table 9.
[26] Manuszak (2002) p 30.
[27] Manuszak (2002) p 21.

station's unobserved costs.[28] It assumes that the level of service offered (e.g., two different service levels for the same grade of gasoline) is uncorrelated with consumer preferences.[29] These assumptions are highly questionable; indeed, the study emphasizes the endogeneity of location characteristics,[30] including the fact that locations are chosen by refiners in large part on the basis of aspects of consumer demand that are unobserved. There are conventional tests for the validity of instruments[31], but this study did not apply them.

A compelling advantage of structural models, like this one, as opposed to reduced form models, like the other studies of horizontal market structure reviewed here, is that they provide predictions of changes in prices and consumer welfare in response to any proposed merger. Manuszak (2002) provides predicted price increases of from 1.9 cents to 3.4 cents per gallon on Maui for the merging firms in various combinations of two refiners, with negligible price changes for non-merging refiners. Point estimates of offsetting compensation for reduction in consumer welfare range from 23 to 46 cents per month per registered vehicle. (There are no standard errors presented with these estimates.) A limitation common to even the best structural models is that results like these are contingent on correct specification of the model and reliable estimates. A thorough study can mitigate this limitation by investigating the sensitivity of the main conclusions to alternative specifications of the model, and by carrying out specification tests when possible. This study, which appears to be a work in progress, has not yet taken those steps. In the interim, its conclusions—which will always need to be qualified by the fact that it pertains to a specific and arguably unique American retail gasoline market—must be regarded as quite tentative.

Evidence on vertical integration

Hastings-Gilbert (2002, national study)

Justine Hastings and Richard Gilbert attack the question of the effect of vertical integration on wholesale and retail prices using two different data sets. The hypothesis entertained in each approach—described as the "raising rivals' costs model"—is that a vertically integrated firm that also sells to independent suppliers will post a higher wholesale price than one that is not vertically integrated. The posted wholesale price is merely an accounting entry to the vertically integrated entity, but a higher wholesale price will drive up the costs of competing retailers. As the market share of retailers integrated back to the refining stage increases, the spread between wholesale and retail prices should narrow.

[28] Manuszak (2002) p 23.
[29] Manuszak (2002) p 23.
[30] Manuszak (2002) p 20.
[31] Greene (2003), Section 18.4.1.

The first approach taken in Hastings and Gilbert (2002) is one of two studies (the other being Vita (2001)) using national cross-section time-series data to investigate the impact of vertical integration on gasoline price. This study uses quarterly data from January, 1993 through June, 1997, for 26 major metropolitan areas. It employs as its dependent variable the spread between the average unbranded wholesale price and the crude oil spot price.[32] The explanatory variables include four measures of vertical integration – the number and market share, respectively, of vertically integrated suppliers, and of unintegrated suppliers. These measures are assembled from retail census data, but the study provides only a sketchy description of how the variables are actually constructed. It documents considerable variation in these measures from one metropolitan area to the next.

Unfortunately, these are the only explanatory variables used in the Hastings and Gilbert (2002) cross-section time-series study.[33] This is in marked contrast to other longitudinal studies of wholesale or retail gasoline prices, and there are two notable consequences of this omission. One is that the goodness of fit of the Hastings-Gilbert model[34] is poorer than in other longitudinal studies. The second, and more important, consequence is that omitted variables (for example, those that drive demand) may well be correlated with the measures of vertical integration, resulting in biased estimates of the vertical integration variable coefficients.

The Hastings and Gilbert (2002) study provides regression model estimates with and without error components (the latter incorporated as fixed effects). Without error components, all four measures of vertical integration are statistically significant: increases in the number of vertically integrated and unintegrated suppliers both decrease the price spread, while increases in market share increase price spread in the case of integrated suppliers and decrease price spread for independent retailers. The effect appears to be two cents or less, moving from the first to third quartile.[35] With error components, three of the four coefficients are reduced in magnitude and are statistically insignificant. The components are jointly highly significant, indicating probably omitted variables bias in the model estimated without error components. The longitudinal study in Hastings and Gilbert (2002) does not, therefore, provide much evidence on the impact of vertical organization on price spreads.

[32] If changes in crude oil prices are passed through to wholesale prices one-for-one within a three month period (as is the case in Chouinard and Perloff (2002)) then the results in Hastings and Gilbert (2002) should be about the same using wholesale price as the dependent variable so long as crude oil price is included as an explanatory variable.

[33] Although the study offers no explanation, this may be due to the difficulty of organizing covariates by metropolitan area.

[34] See Hastings and Gilbert (2002), Table 4.

[35] This calculation entails matching variables in Tables 1 and 4 of Hastings and Gilbert (2002), which is not straightforward due to different nomenclature in the two tables.

Hastings and Gilbert (2002, local study)

The second study in Hastings and Gilbert (2002) examines the impact of the sale of Unocal's west coast refining and marketing assets to Tosco in November 1996. This transaction had negligible impact on horizontal concentration at the retail level, because there were no cities in which both companies had a significant presence. Since Tosco sold wholesale gasoline in all west coast markets whereas Unocal sold only in some, concentration increased in some wholesale markets but not in others. The raising rivals' costs model implies that Tosco should increase wholesale prices more in markets where it gains a greater downstream market share, and in markets where there is heavier competition from independent retailers.

Using retail census data, Hastings and Gilbert (2002) constructs the product of the increased downstream market share from the acquisition of Unocal retail outlets (i.e., Unocal's pre-acquisition share of the downstream market), and the percentage of those outlets that are geographically within one mile of an independent retailer.[36] This measure is one covariate in a regression equation explaining Tosco's weekly average wholesale price of unbranded gasoline. The other covariates are the number of refiners selling unbranded gasoline, the percentage of stations that are independent retailers, and Tosco's weekly average wholesale price in Phoenix. The latter variable is included as a proxy for costs: Phoenix terminals are supplied by common carrier pipeline from Los Angeles, and Phoenix was unaffected by the Unocal acquisition – in fact it had no change in upstream or downstream structure during the time period considered.

The structure of the data set is cross-section time-series, employing 12 cities and 128 weeks (July, 1996 through December, 1998). It utilizes an error components structure, and the usual tests favor fixed effects rather than random effects. The model also incorporates first-order autocorrelation. It does not accommodate correlation between shocks to wholesale prices in different cities, and examination of this feature would render standard errors more reliable – but, as it stands, the model represents a reasonably careful application of statistical inference using cross-section time-series.

The only significant covariate is the product of increased downstream market share and percentage of outlets competing with independents, with a coefficient estimate of about 0.4. The study interprets this finding as implying that "an integrated refiner's [wholesale] price is an increasing function of its competition with independent retailers."[37] This interpretation must be made cautiously, however, because

[36] It appears that this covariate is zero before the acquisition in all cities (see Hastings and Gilbert (2000) pp 15-16 and Table 5) but the study is not completely explicit on this point. This definition of the variable imparts changes in time that vary substantially across cities (Table 5) and are not absorbed in the variance components of the model.

[37] Hastings and Gilbert (2002), p 20. For example, given an increase in market share of 0.08, the median value in the sample, Tosco would increase wholesale prices by 0.7 cents if 20% of its acquired retail outlets were within a mile of an independent competitor, and by 1.7 cents if 50% of its retail outlets were so

in the sample there is greater variation in the increase in market share than there is in competition with rival independent retailers.[38] The raising rivals' costs hypothesis points directly to the interaction of Tosco's market share and the percentage of Tosco's outlets competing with independents as the driver of Tosco's wholesale prices. In the specification of the regression model used in Hastings and Gilbert (2000) this is the only way that Tosco's market share can affect wholesale prices, and one could interpret the results as simply indicating that Tosco's wholesale prices are an increasing function of its market share – i.e., the results simply reflect the change in Tosco's wholesale demand schedule. By entering market share separately in the regression Hastings and Gilbert could have used a simple *t*-test to sort out these competing interpretations.

Vita (2000)

State gasoline "divorcement" statutes restrict and in some cases proscribe the vertical integration of gasoline refiners and retailers. Divorcement laws are currently in effect in six states (Connecticut, Delaware, Hawaii, Maryland, Nevada and Virginia) and the District of Columbia, and have been proposed, at one time or another, in most state legislatures. To the extent these laws can be regarded as natural experiments, there are rich possibilities for empirical investigation of the impact of vertical integration on retail prices.

Historically, franchised retail gasoline dealers have supported divorcement legislation as a means of preventing predation by refiner-owned service stations. It is indeed the case that retail prices at refiner-owned stations are systematically lower than those at lessee or independent dealers,[39] but there is no economic case for predation of refiners upon their efficient dealers.[40] Simple microeconomics suggests that divorcement statutes may indeed increase retail prices, because of the efficiencies of integration and double marginalization in the determination of the wholesale prices paid and retail prices set by lessee or independent branded retailers. The case for divorcement statutes lowering retail prices is relatively weak, relying on specific forms of imperfect competition and obtaining at most ambiguous impacts on retail prices.[41]

Vita (2000) undertakes a cross-section time-series study utilizing monthly data for all 50 states, January 1995 through December 1997. The dependent variable is the retail price of unleaded regular gasoline net of taxes. The covariate of interest is an indicator for a divorcement law. Because such laws were in effect,

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situated. The largest predicted price increase in the sample is for Santa Barbara, amounting to about 3.6 cents per gallon.

[38] Hastings and Gilbert (2002), Table 5.

[39] See Barron and Umbeck (1984) and Shepard (1993), discussed subsequently.

[40] See Vita (2000) pp 217-218.

[41] See Salinger (1988), Ordover et al. (1990), Hart and Tirole (1991), Reiffen and Vita (1995).

or not, for the entire 1995-1997 period in each of the states, it is impossible to use a variance components model and estimate the impact of divorcement. This being the case, it is important to specify as completely as reasonably possible a set of covariates explaining systematic state-by-state differences in retail gasoline price. To this end the study incorporates demand shifters (income, driver and vehicle characteristics), cost shifters (wages, transportation prices, crude oil price, gasoline characteristics arising from environmental legislation, heating degree days, and regional transportation cost indicators for the northeast, US west of the divide, Alaska and Hawaii), yearly and monthly indicators. It also includes as covariates an indicator for the presence of a sales-below-cost law and the percentage of sales through self-service. The exclusion of the District of Columbia and the presence of the cost indicator for Hawaii implies that inference about the impact of divorcement laws is being drawn from five states – Connecticut, Delaware, Maryland, Nevada and Virginia.

The base model is estimated by least squares correcting for first-order autocorrelation. The main finding is that divorcement laws increase retail prices by 2.6 cents (95% confidence interval 1.2 cents to 4.0 cents). The estimation method presumes that all covariates, including divorcement laws, are exogenous. The suitability of this assumption is typically a central question in "natural experiment" studies like this one. As a check on assumptions, Vita (2000) also estimates the model taking both divorcement and sales-below-cost laws to be endogenous, and uses as instrumental variables the presence of state anti-takeover or minimum wage laws, the ADA rating of the state Congressional delegation, and an indicator for whether a large gasoline refiner is located in the state. Under this alternative set of assumptions the study finds that divorcement laws increase retail prices by 3.1 cents (95% confidence interval 0.7 cents to 5.5 cents). Prohibition of self-service sales (Oregon and Nevada) increases price by about 3.5 cents per gallon, while sales-below-cost laws have no statistically significant impact on price.

Vita (2000) is a well-executed time-series cross-section study that focuses on the impact of divorcement laws on the retail gasoline price in the states where they have been instituted. It finds this impact has been two-and-one-half cents, and it substantiates its interpretation of these laws as a natural experiment from which we may learn about the impact of the degree of vertical integration on retail gasoline prices. This study, and studies like it that utilize differences across states to identify the impact of horizontal or vertical organization on price, are reliable to the extent that they are able to identify all of the relevant factors accounting for state price differentials. Vita (2000) explains about 85% of the variation in gasoline prices, which is better than the GAO (1986) regression model that explained about 80%, but falls short of the variance components models in Chouinard and Perloff (2002) at 90% and GAO (1986) at 96%.

The increased explanatory power in the latter studies comes from the inclusion of state indicator variables . These indicator variables account for state-specific covariates that are important for gasoline prices but have not been included in the regressions in Chouinard and Perloff (2002), GAO (1986) or Vita (2000).

(Vita (2000) cannot include state indicators because they would be perfectly collinear with the divorcement indicator.) The implication is that there are unaccounted factors underlying variance across states in all of these studies, factors that may be correlated with the presence of divorcement laws and consequently bias the estimate of the impact of these laws. An important goal in future econometric cross-section time-series studies of this topic is reduction of the variation in prices that must be laid to unexplained differences in states (as is done in variance components models). Vita (2000) shows that one way to do this is through more deliberate accounting of cost differences

Barron and Umbeck (1984)

John Barron and John Umbeck utilize the Maryland divorcement law, which took effect in 1979, as a natural experiment whose outcome provides the impact of divorcement on retail gasoline prices. The theoretical foundation for the expected impact is a comparison of prices and hours of retail outlet operation when retail stations are company-owned, with these decisions when retail outlets purchase from refiners and set their own retail prices and hours of operation. The textbook double marginalization model implies that prices will be higher and hours shorter, given separation of upstream and downstream decision making.

As a consequence of the Maryland divorcement legislation, seven refiners were forced to sell or franchise a total of 170 stations, mostly during June or July 1979. Barron and Umbeck (1986) collected primary data from the seven refiners on hours of operation and retail prices at the affected stations, and at stations the refiners perceived as competitors of these stations. (Typically three or four such stations were identified for each formerly owned station.) They received responses for 144 stations and utilized 99 franchised stations, and their corresponding competitors, in their analysis.[42]

The study measures retail price relative to the BLS U.S. monthly gasoline price index, separately for self-serve and full-service gasoline. Table 2 summarizes the empirical findings in Barron and Umbeck (1986). The top panel indicates comparisons using the price and hours data directly; prices are expressed in real 1981 cents. The bottom panel provides comparisons controlling for station characteristics (service bay, convenience store, car wash, both full- and self-service, number of hoses, and acceptance of credit cards) and the number of stations identified by the leasing refiner as being in the market. The results are all consistent with the implications of the double marginalization model that company owned stations price lower than franchise or independent stations, and that divorcement will increase prices at both affected and competing stations. Affected stations reduce hours while competing stations have little or no discernible

[42] The alternative to franchise was outright sale, which changed the station brand. This consideration, plus the fact that many companies did not retain data for stations sold, led Barron and Umbeck (1986) to confine the study to franchised stations.

change in hours; however the comparison of hours between affected and competing stations depends on whether covariates are included.[43]

Table 2[a]

	Affected station before	Affected station after	Competing station
No controls:			
Full service price	-9.51	-2.86	+1.01
Self-serve price	-3.92	-2.53	+0.03
Hours	+13.76	+4.54	+2.99
With controls:			
Full service price	-5.55	-0.26	+1.01
Self-serve price	-2.42	-0.71	+0.30
Hours	-3.93	-12.31	+1.17

[a]Prices and hours are all shown relative to unaffected competing stations before divorcement. Standard error of estimate ranges from 0.25 to 0.40 for prices and 2 to 3 for hours.

Vita (2000) found that divorcement laws raise prices 2.6 cents per gallon, give or take 1.4 cents for a 95% confidence interval. In Maryland, 170 stations were affected by divorcement, there were about 600 competing stations[44] as defined in Barron and Umbeck (1984), and there were roughly 930 unaffected non-competing stations – a total of 1700 stations state-wide. We do not know the fraction of Maryland sales at company-owned stations; if it was only ten percent, then the study implies an increase of about 1 cent in full service and 0.2 cents in self-service, but if it was half then the increase is about 3 to 3.5 cents. Given that company owned stations are typically substantially larger than other stations,[45] and taking into account estimation error, the results in Vita (2000) and Barron and Umbeck (1984) are consistent. This is notable in view of the fact that Vita (2000) is based on state differences in the mid-1990s, whereas Barron and Umbeck (1984) is grounded in changes over time in one state in the late 1970s.

Shepard (1993)

Andrea Shepard provides some evidence on the relationship between the ownership of a gasoline station and retail gasoline price.[46] There are three kinds of ownership: open dealer contracts in which the station is owned and operated by an independent dealer who contracts with a refiner for gasoline supply; lessee dealer stations in which the capital at the station is owned by the refiner and leased by the self-employed dealer; and company outlets at which the capital is owned by the refiner and the station operator is employed by the refiner. The operator sets prices at open and lessee dealerships, the refiner under company ownership; the operator controls station characteristics in an open dealership, the refiner under a lessee dealership or company ownership.

[43] Barrnon and Umbeck (2000) takes no note of this discrepancy in findings.
[44] Barron and Umbeck (2000) p 321,
[45] Blass and Carlton (2001).
[46] An earlier summary version of this paper is Shepard (1990).

The principal focus of Shepard (1993) is on the relationship between station characteristics and ownership form. This is part of a larger literature (including Slade (1998) and Taylor (2000)) that uses classical principal-agent theory to optimize the contract between owner and operator. A secondary focus of this study is the relationship between ownership form and price, which falls within the purview of this survey. It is based on a primary data set collected from a cross-section of 924 branded gasoline stations in eastern Massachusetts in the first quarter of 1987. Of these stations, 38 were company owned, 452 were lessee-dealer and 434 were open dealer. In the empirical work on price, Shepard (1993) distinguishes only between company-owned and all other stations.

The study utilizes a conventional regression equation. The covariate of interest is an indicator variable for company ownership. The other covariates capture station characteristics (repair services, convenience store, number of cars that can be serviced simultaneously, whether both full and self-service are offered, whether the station was recently remodeled, whether it offers "mini-service", and an indicator for outlying area) and a "nearby capacity" variable that sums the number of cars that can be served simultaneously at other stations located within a one-mile radius. Six variants of the regression model are presented, corresponding to the six combinations of full- and self-service, and to leaded, unleaded regular, and unleaded premium gasoline. Across these six equations, five of the company ownership coefficients are statistically insignificant (ranging from –3.17 to 0.73); only that for full-service unleaded premium, –5.47 cents, is barely significant. The median estimate is –1.5 cents.

Shepard (1993) also presents a variant on this equation that is intended to control for local geographic effects. The documentation of this procedure is too sketchy to permit critical evaluation. The pattern of inconsistent and insignificant results is repeated in this variant.

The findings in Shepard (1993) are consistent with the findings in Vita (2000) and Barron and Umbeck (1984) that retail prices are lower at stations vertically integrated back to the refining stage. However, the estimates in the study are sufficiently imprecise (most likely due to the small data set) that it adds little information about the magnitude of the difference.

Conclusion

The empirical evidence on the competitive effects of mergers in the gasoline industry consistently supports the proposition that retail prices are lower with vertical integration than with separation of refining and retailing. State divorcement laws provide a plausible natural experiment measuring this price differential, and two well-executed independent studies utilizing different data and methods (Barron and Umbeck (1984) and Vita (2000)) indicate that it is substantial. Taken together, these studies support the proposition that retail gasoline prices at vertically integrated stations are from 1.5 cents to 5.0 cents lower than at leased

or independent stations, other things equal, and that prices at competing stations are also lower. The evidence from local posted prices in Shepard (1993) is much weaker, but is consistent with these estimates (as well as a much wider range). The Hastings and Gilbert (2002) national study examines the broader question of the relationship between various measures of vertical integration and retail gasoline prices, but fails to control for known, important retail price covariates, and obtains inaccurate estimates in any event. The Hastings and Gilbert (2002) local study addresses the change in wholesale prices following a single acquisition that increased vertical integration with little effect on horizontal retail concentration. It raises the possibility of slightly increased wholesale prices, but does not examine retail prices.

There is little, if any, reliable evidence on the competitive effects of horizontal mergers or concentration. One study (Hastings (undated)) plausibly interprets the conversion of independent to branded retail outlets in Southern California as a natural experiment. It finds that at stations competing with the stations that changed hands, prices increased—by three to seven cents—relative to prices generally, and without regard to ownership status of stations, in the five months following the transaction. This is a very large change, relative to retail-wholesale price differentials. However, Hastings (undated) does not track subsequent price changes or market adjustments. Another study (Manuszak (2002)) projects the impacts of mergers in one of the Hawaiian Islands in the context of a structural model that makes specific assumptions about preferences and costs. These assumptions could have been tested but were not, and the study itself regards the findings for costs as implausible.

Two studies utilize cross-section time series reduced form models. GAO (1986) examines the impact of the HHI measure of horizontal wholesale concentration on wholesale prices, using less than three years of monthly state data. Chouinard and Perloff (2002) estimates the impact of 35 different mergers on both wholesale and retail prices, using over eight years of monthly state data. The statistical methods used in both cases assume that variations in concentration (across both states and time) can be regarded as a "natural experiment" – for example, GAO (1986) implicitly assumes that concentration is uncorrelated with differential distribution costs that are not recorded, and Chouinard and Perloff (2002) requires that mergers not be related systematically to unexplained wholesale price differentials or movements over time. Both studies control for demand and cost shifting covariates that would plausibly enter a reduced form price model. In any event, taken at face value, Chouinard and Perloff (2002) finds no systematic relationship between mergers and prices. GAO (1986) reports a very weak link between concentration and prices, with wholesale prices rising by 0.5% (1.7% in the error components model) if the HHI increases from 2000 to 3000.

Given the importance and prominence of the motivating issues of economic policy, the dearth of reliable evidence on the competitive effects of horizontal mergers or concentration is unfortunate. Retail and wholesale gasoline markets differ substantially across time and place with respect to horizontal

concentration, entry, exit, and the impact of mergers, but to date no study has utilized good data or state-of-the-art methodology to examine this evidence. GAO (1986) and Chouinard and Perloff (2002) utilize data organized by state. State data are more readily available, but they are inconsistent with markets that tend to be organized around metropolitan areas, and around terminal clusters in metropolitan areas in particular.[47] Hastings and Gilbert (2002) constructed wholesale gasoline prices by metropolitan area, but apparently did not construct (and certainly did not use) most of the covariates that turn out to be important in other studies. Organization of data along market rather than state lines is an important topic in future research.

The failure of any study to appropriately utilize econometric methods for cross-section data that could be found in textbooks even at the time of GAO (1986) is harder to understand. Both GAO (1986) and Chouinard and Perloff (2002) draw implications that have no foundations given those studies' use of error components models, as detailed above. An ambitious yet worthy goal is to construct a cross-section time-series data base sufficiently rich in covariates that it accounts for geographic differentials in wholesale and retail prices of gasoline, so that cross-section time-series studies do not have to resort to error components methods (either fixed effects or random effects). Vita (2000) takes a step in this direction, and such a data base would be useful in studying not only the competitive effects of horizontal concentration and vertical integration, but also the vexing and politically relevant question of seemingly persistent differences in prices in different geographic regions. The implications of the petroleum physical distribution network for costs, incorporated if at all only with a few regional dummies in these studies, are well worth working out.

Better data and appropriate use of models notwithstanding, it is unlikely that relationships between horizontal concentration and vertical integration, on the one hand, and retail or wholesale prices of gasoline, on the other, will be self-evident. At the end of the day many questions will come down to the reliability of standard errors and hypothesis tests: an estimated price increase of five cents plus or minus one cent is not the same as an estimated price increase of five cents plus or minus ten cents, and so it is important to know whether the "one cent" or the "ten cents" (if either) is appropriate. This requires taking seriously properties of statistical models (like serial correlation) that are either ignored or simply go unreported in most of these studies. The natural sciences have long-established standards for full documentation and replicability. There is no reason why the same standards of evidence should not apply here.

[47] GAO (1986, p 40) recognizes this problem.

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