HIGH FREQUENCY EVIDENCE ON THE DEMAND FOR GASOLINE

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Abstract

Daily city-level expenditures and prices are used to estimate the price responsiveness of gasoline demand in the U.S. Using a frequency of purchase model that explicitly acknowledges the distinction between gasoline demand and gasoline expenditures, we consistently find the price elasticity of demand to be an order of magnitude larger than estimates from recent studies using more aggregated data. We demonstrate directly that higher levels of spatial and temporal aggregation generate increasingly inelastic demand estimates, and then perform a decomposition to examine the relative importance of several different sources of bias likely to arise in more aggregated studies.

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1 Introduction

Over the last several decades gasoline prices in the United States have become increasingly volatile, attracting the ire of consumers and the attention of policymakers armed with various proposals to cushion the impact of price shocks on consumers and the broader economy. This price volatility has largely resulted from an increased uncertainty in the supply of oil and available refining capacity—conditions that are likely to persist (at times intermittently) for the foreseeable future. Understanding consumers’ ability to respond to such price fluctuations is crucial for predicting the potential impact of future supply disruptions and for estimating the value of proposed policy measures intended to limit the associated price volatility.¹

The elasticity of gasoline demand has been studied extensively over the last 40 years. However, the data and approaches used in these studies have not been well suited for evaluating response to the temporary price fluctuations that have become commonplace. Many utilize monthly, quarterly, or even annual aggregate measures of gasoline usage and average prices, often from a single national time series. Others rely on cross-sectional data in an attempt to identify a long-run demand elasticities based on price variation across regions or countries. Empirical models estimated at a high level of geographic aggregation relating monthly or annual gasoline volumes to average prices are likely to mask a significant share of the response by consumers in a given location to changes in the local gasoline price. Moreover, the use of highly aggregated data generally requires strong assumptions that restrict the demand relationship from varying across locations or over time. As a result, unobserved heterogeneity in underlying demand has the potential to contribute bias to estimates of demand elasticity.

Our study uses daily gasoline prices and citywide gasoline expenditures from 243 U.S. cities to provide an analysis of the impact of daily prices on daily gasoline demand. These high-frequency panel data have several important advantages. First, the expenditure

¹Notable examples of policy proposals include the “gas tax holiday” suggested by several presidential candidates during the 2008 elections and the release of oil from the strategic petroleum reserve which was utilized by President Obama during the summer of 2011 amid oil supply disruptions in Libya and other areas of the Middle East.
information comes from credit card transactions at the point of sale and, therefore, offers a much more direct measure of consumer demand. Second, the daily price variation that is masked by averaging in monthly, quarterly, or annual demand models can now be used to identify high frequency demand responses. Finally, our panel data can be exploited by including extensive fixed effects to better control for persistent differences in gasoline demand over time and across locations.

Unlike the existing literature our analysis also directly addresses the fundamental difference between gasoline demand (or usage) and gasoline expenditures—a difference that becomes even more pronounced when using daily data. Because consumers can use their car’s gas tank for short-term storage, a consumer’s demand for gasoline is different from its pattern of expenditures. Moreover, consumers may respond to price changes by altering both how much gasoline they use and when they decide to purchase. Traditional models that simply relate prices to gasoline expenditures cannot separate these two potential effects.

In order to model more accurately the demand for gasoline in a manner that recognizes the distinction between expenditure and consumption, we specify a two-equation model of the consumer’s probability of gasoline purchase and daily gasoline demand that separates the demand decision from the purchase decision in the most flexible manner possible given our city-level expenditure data. For example, consumers may be more likely to buy on certain days and the amount they purchase when they buy gasoline might fluctuate over time. Because we observe both the number of gasoline transactions occurring in a city on a given day as well as the total expenditures on gasoline, we are able to separately identify changes in consumers’ probability of purchase from changes in consumers’ underlying demand for gasoline. Aggregating our two-equation model of individual gasoline purchase and demand over all individuals in a metropolitan area yields a model of daily aggregate gasoline expenditures that we can use to recover a price of elasticity demand for the metropolitan area.

We obtain estimates of short-run demand elasticity ranging from $-0.27$ to $-0.35$, nearly an order of magnitude more elastic than other recent estimates (Hughes, Knittel and
Sperling, 2008; Small and Van Dender, 2007; Park and Zhao, 2010). To investigate the consequences of estimating demand using more highly aggregated data as is common in previous studies, we aggregate our data over time and across cities to varying degrees and repeat our analysis. The resulting demand estimates become increasingly less elastic as the level of data aggregation increases. Estimating the model using our data aggregated to a national time-series of monthly total expenditures and average prices results in elasticities that are indistinguishable from zero, suggesting that studies using aggregated data may underestimate consumers' responsiveness to short-term gas price fluctuations. To better understand the impacts of data aggregation, we then decompose the resulting bias into three different components. Our analysis reveals that the largest bias arises in time series models where time-period fixed effects can no longer be used to control for demand differences over time.

It is important to highlight that the use of daily data rather than, say, monthly data does not necessarily imply that our elasticity estimates describe a “shorter run” demand response. The relevant response horizon of any elasticity estimate depends on the variation in prices used to identify the response parameter and when these movements occur relative to when demand is observed. Prices change in this market on a daily basis and consumers make purchase decisions on a daily basis, so behavior is likely to be more accurately represented using a model of daily demand for gasoline. However, as in most studies, our baseline model is static and does not allow the degree of demand responsiveness to change depending on how long it has been since a price change occurred, so price changes occurring several months ago are just as important as price changes occurring days or weeks ago in terms of identifying demand elasticity. Specifically, our model of daily demand implies that if the daily price rose by 10 percent and remained at this higher level, daily gasoline demand would remain lower by an amount equal to 10 percent times our demand elasticity for as long as the daily price remained 10 percent higher. As a result, we believe that our main elasticity estimates reflect the same type of consumer response that other studies attempt to measure using more aggregated monthly or quarterly data.

We also consider a dynamic two-equation frequency of purchase model which incorporates lagged prices to allow the immediate response of demand to a price change to
differ from the longer run demand response. We find evidence that gasoline expenditures respond even more strongly in the days immediately following a price change, but this temporary additional response largely dissipates after 4 to 5 days. Including current and lagged prices in the purchase probability equation of our model reveals that much of the immediate and temporary response in gasoline expenditures results from a substantial change in consumers’ probability of purchase in the days following a price change. The probability of purchase on the day following a price increase tends to fall by 1.1% for every 1% change in the price of gasoline when evaluated at the mean purchase probability. However, this effect vanishes after a few days and the remaining impact of a price change on expenditures results entirely from changes in consumer’s gasoline consumption. Moreover, the implied elasticity once demand has completely responded to a given price change in the dynamic model is nearly identical to that estimated by our main (static) analysis, which is consistent with the estimates from the static model identifying a longer run response rather than the very-short run response that occurs in the days immediately following the shock.

We conclude from our analysis that the short-term response of U.S. gasoline demand to price fluctuations appears to be substantially more elastic than one might deduce from the estimates of existing studies relying on more aggregate data. The findings clarify the importance of demand response in mitigating the impacts of temporary gasoline supply shocks or disruptions that have become increasingly frequent in recent years. More accurate estimates should also improve the analysis and evaluation of public policies related to the gasoline market. For example, the more elastic estimates revealed here imply that temporary releases from the strategic petroleum reserve may have a smaller impact on the level of gasoline prices than previous estimates would suggest. Similarly, greater elasticity indicates that consumers would receive a smaller portion of the relief generated by temporary gasoline tax "holidays" (which have been implemented at various points in numerous states since 2000 and were proposed nationally in the summer of 2008) than would otherwise have been predicted. The impacts of aggregation documented here also highlight a potentially important challenge for future studies by showing that gasoline demand exhibits substantial geographic and temporal variation that can be difficult to control for when estimating empirical models using more aggregated data.
2 Approaches to Estimating Gasoline Demand

2.1 Common Estimation Strategies

There are a number of survey articles available (including Dahl and Sterner, 1991; Goodwin, 1992; Espey, 1998; Basso and Oum, 2007) that summarize and analyze the literature on gasoline demand estimation. Nevertheless, it is helpful to provide a brief overview of some of the benefits and limitations of a number of commonly used empirical approaches in order to motivate our analysis and highlight the contribution of our study.

Most studies of aggregate gasoline demand estimate a simple log-linear model of quantity as a function of the gasoline price and other variables such as average income to control for shifts in demand. This specification is chosen mostly for convenience, so that the coefficient on price represents an estimate of demand elasticity. A number of studies have investigated alternative functional forms, and some find alternative forms to have a better fit (e.g., Hsing, 1990), but generally the resulting elasticity estimates are not substantially impacted by specification (see Sterner and Dahl, 1992; Espey, 1998).

Such “static” demand models can be appropriate as long as the researcher believes that the price and quantity data in the sample represent observations from a stable relationship. In situations where it is thought that gasoline demand may take multiple periods to adjust to price changes, researchers often include a lagged dependent variable (the parsimonious approach) or specify a distributed lag model (the more flexible approach) in which lagged values of prices and other control variables are also included in the demand equation. Coefficients on lagged prices capture how demand temporarily deviates from the longer-run equilibrium relationship as adjustment occurs, allowing the coefficient on the current price level to capture more accurately the overall elasticity of demand in the longer run.

A wide variety of data have been used to estimate these aggregate demand models, including cross-sectional, time-series, and panel data. In general, demand estimates resulting from cross-sectional data are often interpreted to represent a long-run elasticity as differences in gasoline consumption will in part reflect the varying infrastructure investments
and vehicle choices made in the different regions or countries in light of the relative fuel price levels prevailing in those areas. Elasticity estimates using time-series data are typically interpreted as reflecting a somewhat shorter-run demand response as quantity changes are observed over time-periods in which these infrastructure investments and vehicle choices are relatively fixed or adjust very slowly. Studies using panel data tend to more closely resemble these time-series studies, as they rely heavily on temporal variation within a region to identify elasticity and usually include more extensive controls for persistent differences in demand across regions. It is perhaps not surprising that elasticity estimates from cross-sectional studies tend to be more elastic on average than those from time-series or panel studies (Espey, 1998). Individual level studies of gasoline demand based on cross-sectional survey data (e.g., Archibald and Gillingham, 1980; Hausman and Newey, 1995; Yatchew and No, 2001; Blundell et al., 2012) also tend to identify more price elastic demand estimates.

It is important to note that, in part due to a lack of credible instrumental variables, gasoline demand studies rarely attempt to directly address the possible endogeneity of the price variable resulting from supply side responses to unobserved demand shifts. As a result, the interpretation of price coefficients as accurate estimates of demand elasticity relies heavily on the ability of other control variables in the demand equation to explain any variation in quantity that is not the result of a change in price. Unfortunately, time-series and even panel-data studies rely almost exclusively on macroeconomic variables, like average income, and seasonal dummy variables (when relevant) which are certainly jointly determined with aggregate gasoline demand but do not perfectly predict fluctuations in gasoline usage. In this case, it is reasonable to expect this endogeneity bias to make price elasticity estimates more inelastic than they should be. Conversely, cross-sectional studies attempt to use observable characteristics of the countries, regions, or individuals to explain any variation in demand not resulting from differences in gasoline price. If this is unsuccessful and if consumers in areas with higher gasoline prices tend to use less gasoline either as a result of unobserved capital investments or regional characteristics, endogeneity may bias estimates to be overly elastic in these studies.

Several recent papers have attempted to overcome these issues by identifying and
utilizing instrumental variables to more accurately estimate demand. Hughes et al. (2008) estimate a specification of nationwide monthly gasoline demand using crude oil production disruptions as instruments for monthly gasoline prices, but find the resulting elasticity estimates to be nearly indistinguishable from those obtained in their baseline OLS specifications. Davis and Killian (2011) utilize monthly state-level aggregate gasoline consumption and average prices to estimate a first-differenced model in which changes in state gasoline tax rates serve as instrumental variables for changes in gasoline price. Their preferred IV estimate suggests a demand elasticity of $-0.46$ (s.e. $= 0.23$), while the corresponding OLS monthly state-level panel regression produces a substantially less elastic estimate of $-0.19$ (s.e. $= 0.04$) and an estimation using data aggregated to the national monthly time series level produces an even smaller elasticity of $-0.09$ (s.e. $= 0.04$).\footnote{Coglianese, Davis, Kilian and Stock (2015) point out that the IV estimate of Davis and Killian (2011) may be biased as a result of consumers anticipating the tax change and buying more gas in the month before the tax increase than in the month after. When one month leads and lags are included to control for this, the elasticity estimate falls to $-0.36$ (s.e. $= 0.24$), nearly identical to the estimate we obtain from our frequency of purchase model.} Davis and Killian’s IV estimate is much closer to our disaggregated elasticity estimates, suggesting that both their IV approach and our disaggregated panel fixed effects approach may be overcoming the potential simultaneity bias caused by baseline demand differences over time. However, their measure differs from ours in that it focuses on the demand response occurring during the month in which the corresponding state gasoline tax rate change occurs. As a result, it is likely to reflect a shorter run elasticity than is reflected in our baseline model.

### 2.2 Our Identification Strategy

Our identification strategy leverages the availability of the high-frequency panel data to include extensive sets of time and cross-sectional fixed effects in order to eliminate potential sources of endogeneity. Two main factors determine the severity of price endogeneity arising in OLS estimates of demand: the prevalence and magnitude of unobserved demand shocks relative to unobserved supply shocks and the elasticity of the supply curve. If demand shifts are relatively small or the relevant supply curve is fairly flat, then observed variation in price will mainly be the result of shifts in the supply curve. Correspondingly, our empirical specifications attempt to include fixed effects that will remove most of the
unobserved variation in demand while leaving a sufficient level of supply variation to accurately identify demand elasticity. Moreover, given the nature of the gasoline storage and distribution infrastructure, the daily supply of gasoline is likely to respond very elastically to any remaining daily demand shocks that are not absorbed by the included fixed effects.\footnote{Based on average inventory levels and daily consumption, there is typically enough gasoline stored at local distribution terminals and refineries to supply 4 weeks worth of demand (for more information see the U.S. Energy Information Administration’s Petroleum Supply Monthly). In addition, gas stations often have several days of inventory in their underground tanks. Day-to-day fluctuations in demand can easily be supplied from a combination of these sources, allowing intertemporal arbitrage that minimizes the likelihood of any substantial price response.}

In our daily city-level data, most of the variation in gasoline demand is likely to come from persistent differences across cities in the level of per-capita gasoline consumption and from economic changes that influence the level of gasoline demand over time nationwide. These demand shifts represent the primary sources potential endogeneity, and so our baseline demand specification controls for these by including both city and day-of-sample fixed effects. These fixed effects also remove a significant amount of supply variation including that generated by fluctuations crude oil prices over time or by persistent regional differences in costs, competition, environmental restrictions, or gasoline tax levels. However, temporary regional gasoline supply shocks resulting from refinery outages or pipeline disruptions create significant variation in the relative price levels across cities that will not be absorbed by fixed effects. Our identification of demand relies largely on these periodic shifts in local supply. While it is possible that demand shifts remain even with city and day-of-sample fixed effects included in the model, these will mostly come from predictable seasonal patterns or day-of-week purchase patterns which can be adequately planned for through adjustments to inventories, refinery production, or pipeline delivery schedules. Any additional idiosyncratic day-to-day fluctuations in local purchasing are likely small enough to be easily accommodated using local terminal inventories, so that relative price fluctuations not absorbed by city and day-of-sample fixed effects should entirely reflect localized supply-side cost shocks.

In other words, within the context of our model, the relevant daily supply curve is likely to be almost perfectly elastic, minimizing any potential for endogeneity bias in our OLS estimates. For robustness, however, we are also able to estimate alternative specifications that include city-specific sets of month-of-year and day-of-week fixed effects to control for
potential differences across cities in seasonal or weekly demand patterns yet still preserve the supply variation resulting from unexpected refinery or pipeline disruptions.

3 Retail Gasoline Price and Expenditure Data

Our data contains daily gasoline price and expenditure data for 243 metropolitan areas throughout the United States from February 1, 2006 to December 31, 2009. For each city average daily retail prices of unleaded regular gasoline are obtained from the American Automobile Association’s (AAA) Daily Fuel Gauge Report. The prices reported by AAA are provided by the Oil Price Information Service (OPIS) which constructs the city average prices using station-level prices collected from fleet credit card transactions and direct feeds from gas stations.4

Our expenditure data were obtained from the financial services company Visa Inc. The data reflect the total dollar amount of purchases by all Visa debit and credit card users at gas stations within a city on a given day. As with the price data, cities are defined based on geographic definition of the associated Metropolitan Statistical Area (MSA). In addition to total citywide expenditures, the Visa data also include the number of gasoline transactions or purchases taking place at gas stations in each city during each day. This allows us to separate the daily probability of purchase from the daily demand for gasoline. We also observe the total number of Visa cards that are actively purchasing (any product) within the current month. We use this as a measure of the total population of cards at risk of recording a gasoline purchase during each day of that month.5

Although the data has many advantages, Visa does not directly observe the price paid at the pump or the quantity of gasoline purchased by the customer. Instead, we construct a measure of the total quantity of gasoline purchased in a particular MSA on a particular day by dividing the total expenditures of Visa card customers at gas stations by the

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4The OPIS price survey is the most comprehensive in the industry and is commonly used in research on gasoline pricing.
5At two points during the sample period (August 1, 2006 and August 1, 2007) Visa made small adjustments to their merchant classification methodology which produce discrete jumps in our measure of gas station expenditures in some cities. To correct for this we estimate all models with additional city-specific data-period fixed effects allowing the average expenditures and transaction counts to differ before, between, and after these two dates.
average regular unleaded gasoline price in the city on that day. This approximation raises several potential issues which we explore and address below.

First, in dividing total gas station expenditures by the regular gasoline price we are ignoring the fact that around 15% of gas purchases are for mid-grade or premium gasoline which sell at higher prices. If the fraction of regular-grade purchases were fairly constant over time, we would not expect this unobserved price difference to impact our demand elasticity estimates. However, the results of Hastings and Shapiro (2013) suggest that some consumers may substitute from premium to regular grade gasoline when prices increase. In Appendix A we discuss this possibility in more detail, derive an expression for the potential bias, and use the estimates of Hastings and Shapiro (2013) to show that any such bias is very likely to be negligible in our context.

A second and potentially more important source of bias arises from the fact that total gas station expenditures are also likely to include some non-gasoline purchases. Simply dividing total expenditure by the gasoline price to produce a measure of quantity ignores this possibility. If non-gasoline purchases are present expenditures will appear more elastic to gasoline price changes. Even if the prices and demand for non-gasoline items are not correlated with the price of gasoline, dividing these expenditures by the price of gasoline will mechanically generate an elasticity of \(-1\) for the non-gasoline portion of the transaction.\(^6\) In general, the share of revenues generated by non-gasoline items is small but non-trivial. According to the 2007 U.S. Economic Census, the average gasoline station in the U.S. receives just over 21% of its total revenues from non-fuel sales.\(^7\) Fortunately, biases from non-fuel purchases are only a concern for in-store transactions, and our data includes the daily city-level expenditures and number of transactions separately for pay-at-pump and in-store purchases. Pay-at-pump purchases represent over 76% of total expenditures and over 64% of all transactions in our data.\(^8\)

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\(^6\)See Appendix B for a more complete discussion of the resulting bias.

\(^7\)Most of these non-fuel revenues come from food, cigarettes, and alcohol. Fuel sales often generate less than half of a station’s profits, but given the high volume sold it still represents the vast majority of station revenues.

\(^8\)On average pay-at-pump transactions are larger (in dollar value) than in-store transactions. The most likely explanation is that some in-store transactions include only non-fuel items which tend to be less expensive than the typical gasoline purchase. Unfortunately, our data do not allow us to examine the distribution of individual transaction amounts since we only observe the total expenditure for the day in each city.
transactions gives an alternative estimate of elasticity that is not subject to this bias and may give some indication of the magnitude of the bias for in-store transactions. In addition, in Appendix B we derive the magnitude of the bias that would be expected when non-gasoline expenditures are present in the data, and use outside estimates of non-gasoline expenditures to construct bias-corrected elasticity estimates. The results are consistent with the notion that estimating elasticities using pay-at-pump purchases fully eliminates (or perhaps even over-compensates for) any bias resulting from non-gasoline expenditures.

In the interest of completeness our analysis will present estimates utilizing all purchases as well as estimates using only pay-at-pump purchases. To be conservative however, we will mostly use pay-at-pump purchase data in our alternative specifications and supplementary analysis.

### 3.1 Descriptive Statistics

Before we begin our empirical analysis it is helpful to highlight some important features of the data. First, the price data reveal significant idiosyncratic fluctuation across cities. Though prices in all cities are impacted by common factors like world oil prices, there are many other factors that influence prices locally. Persistent price differences across states arise as a result of differences in gasoline tax rates or in the blends of gasoline that are required. More importantly, significant transitory differences in daily prices across the MSAs arise frequently during our sample period. Figure 1 compares retail price fluctuations in Los Angeles, Chicago, and New York over a typical 100 day period in 2007. It is clear that daily city-level prices provide much richer price variation than monthly data with which to study demand response.

Daily gasoline expenditures also follow different patterns across MSAs, presumably due to both independent retail price movements and other city-specific events. Note that daily expenditures necessarily change with retail prices because they represent the total quantity purchased multiplied by the price paid. As noted earlier, we create a measure of the total quantity of gasoline purchased each day using total daily expenditures divided by the daily average retail price for each MSA. Figure 2 presents a normalized seven-day moving
Figure 1: Daily Average Retail Gasoline Prices for Selected Cities

- Los Angeles, CA
- Chicago, IL
- New York, NY

Figure 2: Seven Day Moving Average of Total Quantity Purchased for Selected Cities Normalized by the City Average

- Los Angeles, CA
- Chicago, IL
- New York, NY
average of this measure of the daily quantity purchased for the same three cities depicted in Figure 1 over the same period.\textsuperscript{9} The daily quantities for each MSA are normalized by the average quantity purchased in that MSA over the sample period. Just as with the prices, the quantities move together at times but also exhibit significant differences.

The daily expenditure data allows us to examine high frequency features of gasoline purchase patterns. Gasoline demand is know to exhibit strongly seasonal variation, and our data reflects this general pattern. We are also able to document a very strong within-week pattern in gasoline purchasing behavior. Our data show that consumers buy roughly 17% more gasoline on Fridays than the daily average and buy 15% less on Sundays than the daily average. Figure 3 shows the averages by day of week of the total daily gasoline expenditures of Visa card customers across all 243 cities in our sample.\textsuperscript{10} Within each city this pattern varies to some extent but Friday is always the highest demand day and Sunday is always the lowest demand day.

Variation in the total expenditures across days can result either from fluctuations in the number of transactions that occur or from fluctuations in the amount people purchase per transaction. Figure 4 reports the average expenditure per transaction by day of week across all 243 cities in our sample.\textsuperscript{11} The within-week pattern in expenditure per transaction is notably different from that of overall expenditures, and it exhibits much less day-to-day variation overall. This reveals that the within-week pattern observed in total expenditures results largely from fluctuations in the number of transactions occurring in each day.

4 Model of Consumer Demand and Purchase Behavior

Because we are working with daily data, the effect of price on the amount of gasoline purchased may be very different from the effect on the amount of gasoline people are actually demanding at any given time. Consumers can buy and store gasoline in their car,

\textsuperscript{9}A moving average of daily quantity is used here to eliminate the strong within-week purchase patterns that are described below.
\textsuperscript{10}Day-of-week averages of the total quantity sold in the sample exhibit the exact same pattern.
\textsuperscript{11}This is a quantity weighted average which is equivalent to calculating (for each day of the week) the total expenditures divided by the total number of transactions over the sample period in all cities.
Figure 3: Day-of-Week Averages of Daily Nationwide Gasoline Expenditures by Visa Customers in our 243 Cities

Figure 4: Day-of-Week Averages of Nationwide Gasoline Expenditures per Transaction by Visa Customers in our 243 Cities
which implies that a consumer’s daily demand for gasoline can differ from its expenditures on gasoline. This section presents a theoretical model that recovers an estimate of the daily price elasticity of the unobserved demand for gasoline from data on the daily number of purchases and expenditures on gasoline for each MSA. A latent customer-level daily demand for gasoline and daily purchase probability give rise to an econometric model for customer-level daily gasoline expenditures that we then aggregate to the MSA level.

Suppose the daily demand for each customer in a city \( j \) on a day \( d \) takes the form:

\[
d_{jd} = \exp(\alpha_j + \lambda_d + \beta \ln(p_{jd}) + \epsilon_{jd}),
\]

where \( \alpha_j \) is a fixed-effect for MSA \( j \), \( \lambda_d \) is the fixed-effect for day-of-sample \( d \), \( p_{jd} \) is the price of gasoline for day \( d \) in region \( j \), and \( \beta \) is the price elasticity of demand. For each \( j \), the \( \epsilon_{jd} \) are a sequence of unobserved mean-zero random variables that may be heteroscedastic and correlated over time within each MSA but are distributed independently across MSAs and are independent of \( p_{jd} \). Consumers must periodically purchase gasoline to satisfy this level of daily usage. The probability that a consumer in MSA \( j \) purchases gasoline on a day \( d \) is assumed to equal:

\[
\rho_{jd} = \gamma_j + \delta_d.
\]

where \( \gamma_j \) is a fixed-effect for MSA \( j \) and \( \delta_d \) is the day-of-sample fixed effect for day \( d \). We assume that the expenditure on gasoline during day \( d \) by each customer in MSA \( j \), \( e_{jd} \), is related to the consumer’s daily purchase probability and daily gasoline demand through the following relationship:

\[
e_{jd} = \frac{p_{jd}d_{jd}}{\rho_{jd}}.
\]

This model implies that the actual quantity of gasoline purchased (if purchase occurs) times the daily probability of purchase is equal to the daily quantity demanded by that customer. Because our data is at the MSA level we aggregate the customer-level model of daily gasoline expenditures over the total number of customers in MSA \( j \) during day \( d \), \( N_{jd} \). The number of customers in MSA \( j \) during day \( d \) making a gasoline purchase is equal to \( n_{jd} \). Therefore, \( E_{jd} \), total gasoline expenditures during day \( d \) for MSA \( j \) can be expressed as:

\[
E_{jd} = e_{jd}n_{jd} = \frac{p_{jd}d_{jd}n_{jd}}{\rho_{jd}}.
\]
Because we observe the total number of active Visa cards \(N_{jd}\) in MSA \(j\) during day \(d\), and the total number of gasoline transactions \(n_{jd}\), \(n_{jd}/N_{jd}\) is an unbiased estimate of \(\rho_{jd}\), the probability of purchase for MSA \(j\) during day \(d\). Accordingly, we can estimate the parameters of equation 2 using OLS applied to:

\[
\frac{n_{jd}}{N_{jd}} = \gamma_j + \delta_d + \nu_{jd},
\]

where the \(\nu_{jd}\) are a sequence of mean-zero random variables that may be heteroscedastic and correlated with \(\epsilon_{jd}\) and over time within each MSA but are distributed independently across MSAs. We can use the fitted values \(\hat{\rho}_{jd} = \hat{\gamma}_j + \hat{\delta}_d\) to obtain a consistent estimates of the \(\rho_{jd}\). Substituting the estimated purchase probability into Equation 4 and taking logs generates our econometric model of gasoline expenditures:

\[
\ln(E_{jd}) = \alpha_j + \lambda_d + (\beta + 1)\ln(p_{jd}) + \ln(n_{jd}) - \ln(\hat{\rho}_{jd}) + \epsilon_{jd}.
\]

This model can alternatively be expressed in terms of the quantity purchased:

\[
\ln(Q_{jd}) = \alpha_j + \lambda_d + \beta\ln(p_{jd}) + \ln(n_{jd}) - \ln(\hat{\rho}_{jd}) + \epsilon_{jd},
\]

The empirical model in Equation 7 makes it possible to identify the underlying MSA-level elasticity of demand for gasoline \((\beta)\) using data on prices, the quantity purchased, and the number of transactions. In Equations 1 & 2 the demand and probability of purchase are both assumed to vary by city and day of sample, but different combinations of fixed effects can easily be used to generate alternative specifications for each of these functions. We will also consider a specification that includes both lagged and current prices in the demand and purchase probability equations.

## 5 Estimation of the Frequency of Purchase Model

We begin by estimating city-level demand for gasoline using the model expressed in Equation 7. Results are reported in Table 1. Following the discussion of data concerns in Section 3, our main specifications are estimated using pay-at-pump purchases only, but we also report the results when all purchases are used. The model implies that coefficients
Table 1: Estimates of Baseline Empirical Model of Demand

Dependent Variable = ln(quantity$_{jd}$)

<table>
<thead>
<tr>
<th></th>
<th>Pay-at-Pump Only</th>
<th>All Purchases</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>ln(price$_{jd}$)</td>
<td>-0.295</td>
<td>-0.364</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>ln(# of transactions$_{jd}$)</td>
<td>1</td>
<td>0.996</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td></td>
</tr>
<tr>
<td>ln(predicted probability of purchase$_{jd}$)</td>
<td>-1</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td></td>
</tr>
</tbody>
</table>

Note: Day-of-sample fixed effects and city fixed effects are included in all specifications. Standard errors in Columns 1 & 4 are robust and clustered to allow serial correlation within city. Standard errors for the remaining specifications are generated using a nonparametric bootstrap that allows errors to be serially correlated within a city and jointly distributed with the error term in the first-stage regression.

on ln(n$_{jd}$) and ln(ρ$_{jd}$) should be 1 and -1 respectively. We estimate the model with this restriction imposed and without it. To facilitate comparisons with earlier studies of gasoline demand we also include (in Columns 1 & 4) estimates from a basic log-linear aggregate demand model. For the basic model we report heteroskedasticity consistent standard error estimates that are clustered by city to allow for serial correlation. Standard error estimates for the purchase model are generated using a nonparametric bootstrap to account for the fact that the predicted probability of purchase is estimated in a first-stage regression.\textsuperscript{12}

When estimated using pay-at-pump transactions only, the model with all restrictions yields an elasticity estimate of -.36, while the unrestricted model produces a similar elasticity estimate of -.30. The unrestricted coefficient on ln(n$_{jd}$) is very close to 1, but the coefficient on ln(ρ$_{jd}$) is very close to zero; far from the -1 implied by the theory. This may be because the fixed effects absorb most of the variation in the probability of purchase (given the functional form specified in Equation 7), and any variation left may be measured with error. The estimated price elasticity is still similar to that from the model with restric-

\textsuperscript{12}The procedure first-re-samples the residuals from the purchase probability equation and the residuals from the expenditure equation and uses the original parameter estimates for both equations to compute re-sampled purchase frequencies and expenditure levels. The two-step estimation procedure is then repeated for the re-sampled purchase frequency and expenditure equation with the logarithm of the fitted value from the purchase frequency equation used as a regressor in the expenditure equation to obtain estimates for the parameter values for both equations for that re-sample. The sample variance of these re-samples is then used to compute the estimated standard errors for both parameter vectors.
tions imposed. Estimates of both the restricted and unrestricted models are somewhat more elastic when using data from all purchases as would be expected if the inclusion of in-store purchases results in some bias from non-gasoline transactions. Nevertheless, the pattern of estimates across specifications is similar for the pay-at-pump and all-transaction samples. Interestingly, the coefficient estimates from the basic demand model in Columns 1 & 4 are quite close to those generated by our frequency of purchase model.

Including time fixed effects helps to control for shifts in demand, but it can also mask supply shifts that could help to better identify demand elasticity. Hence, as an alternative specification we estimate the demand model using month-of-sample fixed effects rather than day-of-sample. Day-of-week fixed effects are also included to control for the weekly pattern in demand suggested by Figure 3. The resulting coefficient estimates, reported in Table 2, Column 1, are very similar to those of the benchmark pay-at-pump specification, with the price elasticity estimated to be \(-0.28\), suggesting that month-of-sample and day-of-sample fixed effects appear to work similarly in controlling for macroeconomic or gasoline market specific fluctuations that might impact gasoline demand at the national level.

In all the specifications estimated to this point, demand elasticities are identified off of city-specific variation in price and quantity purchased. However, large city-specific or regional demand shifts occurring over time have the potential to bias these elasticity estimates. Therefore, we estimate an additional set of specifications that include additional controls for city-specific demand patterns. Table 2, Column 2 reports the results of a regression including month-of-sample effects for each city as well as day-of-week fixed effects. Column 3 contains the estimates from a model including a full set of national day-of-sample fixed effects in addition to the city-specific month-of-year effects to allow seasonal patterns in demand to differ across cities. Column 4 adds further flexibility by including both day-of-sample and city-specific month-of-sample fixed effects. The estimated elasticities vary somewhat across these models, but are all of a similar magnitude to the baseline estimates in Table 1. It is important to note, however, that once separate month-of-sample fixed effects are included for each city they absorb any deviations from the national average that last longer than one month. As a result, the elasticity estimate in Column 4 only reflects the response in demand that occurs in the 4 weeks following a change in price. This estimate is
Table 2: Estimates from Alternative Specifications

\[
\text{Dependent Variable} = \ln(\text{quantity}_{jd})
\]

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>\ln(\text{price}_{jd})</td>
<td>-0.278</td>
<td>-0.271</td>
<td>-0.301</td>
<td>-0.351</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.012)</td>
<td>(0.006)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>\ln(# \text{of transactions}_{jd})</td>
<td>1.007</td>
<td>1.010</td>
<td>0.989</td>
<td>0.994</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>\ln(\text{predicted probability of purchase}_{jd})</td>
<td>0.009</td>
<td>0.011</td>
<td>-0.003</td>
<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
</tbody>
</table>

Fixed Effects:
- Day of Week X X
- Month of Sample X
- Day of Sample X X
- City X
- Month of Year \times City X
- Month of Sample \times City X

Note: Standard errors are generated using a nonparametric bootstrap that allows errors to be arbitrary serially correlated within a city and jointly distributed with the error term in the first-stage regression.

slightly more elastic than in the other specifications suggesting that very short run demand response is even more elastic than that which persists over a longer time horizon.\textsuperscript{13} In general, however, the results reveal that relatively elastic estimates of demand can be obtained using a variety of different sources of variation within the data and are not sensitive to a particular functional form.

Though the ability to include extensive fixed effects should minimize any potential endogeneity bias that might result from unobserved demand shocks, it is difficult to conclude with certainty that these demand shocks have been entirely eliminated. Unobserved city-specific demand shocks could still bias our elasticity estimates downward if local distribution terminals are not able to plan ahead or use inventories to costlessly absorb these differences (i.e., if the local supply curve is not highly elastic with respect to daily adjustments). As a final robustness check, we have estimated an instrumental variables

\textsuperscript{13}This result is confirmed when we explore the differences in longer-run vs shorter-run demand response in more detail in Section 7.
specification that utilizes spot market (wholesale) gasoline prices from large regional refining centers (New York Harbor, the Gulf Coast, or Los Angeles) as instruments for local retail prices. Market-wide fluctuations in wholesale gasoline prices resulting from a combination of changes in demand and changes in crude oil and refining costs are captured by day-of-sample fixed effects, but differences in wholesale gasoline prices between regions still exhibit significant variation largely driven by unexpected refinery shocks. Under the assumption that this regional variation in spot prices is relatively unaffected by temporary city-specific demand fluctuations, using this IV approach could help to eliminate any remaining endogeneity generated by correlation between city-level prices and local demand shocks.

The results of these IV specifications (described in more detail and reported in Appendix C) largely confirm that the robustness of the main OLS findings. The IV estimates of both the basic aggregate demand model and the frequency of purchase model are slightly more elastic but of a relatively similar magnitude to those reported in Tables 1 & 2.

What is most striking about these findings, in general, is that the elasticity estimates from both our frequency of purchase model and the basic log-linear aggregate demand model are consistently nearly an order of magnitude more elastic than those from comparable recent studies including Hughes et al. (2008) whose estimates range from −.034 to −.077 for the period 2000–2006 and Park and Zhao (2010) whose time-varying estimates ranges from around −.05 in 2000 to around −.15 in 2008. Our daily city-level purchase data clearly reveal a much greater degree of short run demand response than has been suggested by much of the literature.

6 Examining the Divergence from Previous Findings

Given the rather large discrepancy between our elasticity estimates and those found in other recent studies, we discuss in this section a number of differences in our analyses that could potentially explain this disparity.
6.1 Sources of Gasoline Consumption Data

Perhaps the biggest challenge in studying gasoline demand is finding an accurate measure of consumption. Nearly all available measures are recorded at a highly aggregated level and are likely to measure actual gasoline usage with a considerable amount of error. The most common source used in recent time series or panel studies (e.g., Hughes et al., 2008; Park and Zhao, 2010; Lin and Prince, 2013) is the U.S. Energy Information Administration’s (EIA’s) data on finished motor gasoline “product supplied”. These data are constructed from surveys of refineries, import/export terminals, and pipeline operators, and the volumes reported reflect the disappearance of refined product from these primary suppliers into the secondary distribution system (local distributors and storage facilities). Each month the EIA reports the product supplied in each of the nation’s five Petroleum Area Defense Districts (PADDs). Unfortunately, given distribution lags and storage capabilities, the amount of product flowing from secondary distributors to retailers and ultimately to consumers could differ substantially from the amount received by these suppliers in any given time period. In addition, to generate a measure that represents domestic gasoline usage, the EIA must net out from total production the estimated quantity of gasoline exported for use in other countries. This step provides yet another dimension for potential error, and created serious measurement issues in 2011 during a period of rapidly growing refined product exports.14

Another potential data source is the Federal Highway Administration (FHWA), which collects information from each state on the number of gallons of motor gasoline for which state excise taxes have been collected each month. This measure would appear to be more closely linked to consumption and it is available at the state level rather than the PADD level. However, a significant amount of measurement error is generated by the fact that each state has its own procedures and systems for collecting this information. The point in the supply chain at which the fuel is taxed also varies across states. Some require taxes to be paid when the distributor first receives the fuel, while others tax the volume of gasoline sold by the distributor. In fact, the FHWA includes in its publications the disclaimer that the reported volumes “may reflect time lags of 6 weeks or more between wholesale and retail

14See Cui (2012).
levels.”

In contrast to the EIA and FHWA data, our measure of gasoline expenditures from Visa is recorded at the final step of the distribution process—when the consumer purchases the product from the retailer. This eliminates the possibility that changes in consumer purchase volumes are masked by additions or withdraws from local storage. Moreover, the volumes are observed at a much more geographically and temporally disaggregated level, allowing changes in consumption to be linked much more directly to contemporaneous local price fluctuations.

6.2 Estimating Demand Elasticity Using Aggregated Data

In general, using highly aggregated data has the potential to mask much of the temporal and geographic co-movements in prices and quantities that result from consumer demand response. To illustrate this point, suppose the per-capita daily demand for gasoline in MSA $j$ during day $d$ can be represented as:

$$ q_{jd} = D_{jd}(p_{jd}, X_{jd}) + \epsilon_{jd}, \quad (8) $$

where $p_{jd}$ is price of gasoline in region $j$ on day $d$ and $X_{jd}$ is the vector of characteristics of region $j$ and day $d$ that enter the demand function for that region and day. These daily demand functions for each MSA imply that $Q_m$, the national average daily per-capita demand for gasoline during month $m$, is equal to

$$ Q_m = \frac{1}{A_m} \frac{1}{J} \sum_{d \in A_m} \sum_{j=1}^{J} D_{jd}(p_{jd}, X_{jd}) + \frac{1}{A_m} \frac{1}{J} \sum_{d \in A_m} \sum_{j=1}^{J} \epsilon_{jd}, \quad (9) $$

where $J$ is the total number of MSAs in the sample, $A_m$ denotes the set of days in month $m$, and $A_m$ denotes the number of days in $A_m$. This aggregation process implies that the national monthly demand for gasoline depends on the daily prices for all days during that month for all MSAs, rather than simply a single monthly national average price. Similarly, a monthly state-level average demand would depend on the daily prices for all days during that month for all MSAs in that state. However, this is not the model estimated in most empirical studies of aggregate demand. Typically these studies only have access to aggregated

---

15FHWA Highway Statistics 2010, Table MF-33GA, Footnote 1.
price measures as well, and, as a result estimate something like:

\[ Q_m = D(p_m, \bar{X}_m) + \bar{\epsilon}_m \]

where \( \bar{p}_m = \frac{1}{A_m} \frac{1}{J} \sum_{d \in A_m} \sum_{j=1}^{J} p_{jd} \).

(10)

The aggregated model expressed in Equation 10 is susceptible to several different types of bias. First, except in the special case when demand is linear, the demand function in Equation 10 can not be the same as in 8 because the quantity demanded at the average price will not be the same as the average quantity demanded at the actual price. Second, as highlighted by Pesaran, Pierse, and Kumar (1989), if there is heterogeneity in the parameters of \( D_{js}(p_{js}, X_{js}) \), say, across cities, then even when demand is linear the estimated parameters from the aggregate model in Equation 10 will represent consistent estimates of the average of the true city-specific parameter values only if the \( \epsilon_{js} \)s are contemporaneously uncorrelated across cities (or more generally, if the \( \epsilon_{js} \)s are uncorrelated across observations that are aggregated together in Equation 10). Third, aggregation can introduce endogeneity bias. Estimation of the disaggregated model requires that \( E[\epsilon_{jd}|D_{jd}(p_{jd}, X_{jd})] = 0 \). From Equation 9 it is clear that aggregation imposes a much stronger assumption for estimation, namely that \( E[\epsilon_{jd}d'|D_{jd}(p_{jd}, X_{jd})] = 0 \) for all observations \( jd \) and \( j'd' \) within the same month. The potential for aggregation to generate endogeneity becomes particularly strong when aggregation prevents the inclusion of fixed effects in the regression model. For example, it is possible to include both time-period and geographic-market fixed effects in the disaggregated model, but aggregation to a monthly time-series model (as in Equation 9) completely eliminates the panel variation and therefore prevents the inclusion of time-period fixed effects. In this case, any common shocks impacting demand across markets that was previously excluded from \( \epsilon_{js} \) by the inclusion of time-period fixed effects now generates correlation across contemporaneous \( \epsilon_{js} \)s, violating the zero conditional mean assumption.

Any or all of these aggregation issues may contribute to the large differences observed between our elasticity estimates and those from previous studies. Fortunately, we are able to investigate the impact of aggregation by using our data to create new data sets with varying levels of temporal and geographic aggregation. We construct daily data sets of state level and nationwide total quantity purchased and average price, as well three
monthly data sets at the city, state, and national levels. To facilitate a more direct comparison with other studies, we estimate basic log-log aggregate demand models and use aggregate per-capita quantities calculated as the corresponding sum of the daily quantity purchased divided by the total number of Visa customers in the combined area. Since prices may be averaged across cities (or days) with very different quantities purchased, we compute a quantity weighted average price. To match previous studies, the average prices in the national monthly specification are converted to constant 2005 dollars using the monthly GDP implicit price deflator.\footnote{Using real prices would not impact the other specifications because the day- or month-of-sample fixed effects would absorb any price adjustment.}

As in our main analysis, a complete set of time period and cross-sectional fixed effects are used whenever possible to control for shifts in demand. When using daily national time series data we include day-of-week and month-of-sample fixed effects. For the monthly national time series we are restricted to using month of year (i.e. seasonal) fixed effects, so per capita real personal disposable income is included as an additional control for demand shifts.\footnote{Data on per capita personal disposable income comes from the Bureau of Economic Analysis and are converted to constant 2005 dollars using the GDP implicit price deflator.} This final specification is identical to that of Hughes et al. (2008).

The demand estimates for each level of aggregation appear in Table 3. The top rows report the results when estimated using pay-at-pump purchases only while the bottom rows report the results for all purchases. The first column reports the daily city-level results again for comparison. The next three columns contain panel regressions with varying levels of temporal and/or geographic aggregation. Price elasticity estimates from these specifications are all very similar to each other (between $-0.22$ and $-0.25$ for pay-at-pump purchases and between $-0.30$ and $-0.34$ for all purchases) and are less elastic than corresponding estimates from the disaggregated regression in Column 1. The elasticity estimates from the two time series regressions in Columns 5 & 6 are even smaller in magnitude, being indistinguishable from zero for pay-at-pump purchases and ranging from $-0.12$ to $-0.14$ for all purchases; much closer to the elasticities reported by Hughes et al. (2008) in their national time-series study.\footnote{In Appendix D we replicate the exact model of Hughes et al. (2008) using their data source but from our 2006–2009 time period and show the results to be very similar to our pay-at-pump national time-series} Clearly, increasing levels of aggregation lead to less elastic estimates of demand,
### Table 3: Regressions Using Aggregated Data

**Dependent Variable = ln(quantity per capita)**

<table>
<thead>
<tr>
<th>Geography:</th>
<th>city daily (1)</th>
<th>city monthly (2)</th>
<th>state daily (3)</th>
<th>state monthly (4)</th>
<th>national daily (5)</th>
<th>national monthly (6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Periodicity:</td>
<td>Pay-at-Pump Purchases Only:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ln(price&lt;sub&gt;it&lt;/sub&gt;)</td>
<td>-0.295</td>
<td>-0.249</td>
<td>-0.245</td>
<td>-0.217</td>
<td>-0.020</td>
<td>0.004</td>
</tr>
<tr>
<td>(0.031)</td>
<td>(0.028)</td>
<td>(0.059)</td>
<td>(0.066)</td>
<td>(0.067)</td>
<td>(0.023)</td>
<td></td>
</tr>
<tr>
<td>ln(income&lt;sub&gt;it&lt;/sub&gt;)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.093</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.184)</td>
</tr>
<tr>
<td></td>
<td>All Purchases:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ln(price&lt;sub&gt;it&lt;/sub&gt;)</td>
<td>-0.378</td>
<td>-0.341</td>
<td>-0.325</td>
<td>-0.297</td>
<td>-0.143</td>
<td>-0.122</td>
</tr>
<tr>
<td>(0.030)</td>
<td>(0.028)</td>
<td>(0.065)</td>
<td>(0.073)</td>
<td>(0.065)</td>
<td>(0.018)</td>
<td></td>
</tr>
<tr>
<td>ln(income&lt;sub&gt;it&lt;/sub&gt;)</td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td>0.154</td>
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<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.160)</td>
</tr>
</tbody>
</table>

Fixed Effects:
- Day of Sample: X X
- Day of Week: X
- Month of Sample: X X
- Month of Year: X X
- City: X X
- State: X X

Note: Standard errors for panel specifications are robust and clustered at the level of the cross-sectional unit to allow for arbitrary serial correlation. Standard errors for time-series specifications are estimated using a Newey-West procedure and are robust to serial correlation.

particularly when moving from panel to time series data.

### 6.3 Sources of Aggregation Bias

Given that aggregation appears to bias elasticity estimates downward by up to an order of magnitude in our setting, we next investigate the different sources of potential aggregation bias to determine their relative impacts. To begin, we consider the possibility that bias results from aggregating with a log-linear demand specification and the fact that the sum across days or cities of the log price or quantity is not the same as the log of the sum. If the estimates.
true form of the city level demand is in logs then aggregating to the month-by-state level, for example, implies the following model:

\[
\ln(Q_{sm}) = \alpha_s + \lambda_m + \beta \ln(p_{sm}) + \epsilon_{sm} \quad (11)
\]

where

\[
X_{ms} = \frac{1}{A_m B_s} \sum_{d \in A_m} \sum_{j \in B_s} X_{cd},
\]

\(A_m\) denotes the set of days in month \(m\), \(B_s\) denotes the set of cities in state \(s\), \(A_m\) denotes the number of days in \(A_m\), \(B_s\) denotes the number of cities in \(B_s\). In contrast, our regressions in Table 3 adopt the approach more commonly used by aggregate studies in the literature, estimating a log-linear model using average price and quantity data:

\[
\ln(Q_{sm}) = \alpha_s + \lambda_m + \beta \ln(p_{sm}) + \epsilon_{sm}. \quad (12)
\]

To evaluate how much elasticity estimates change as a result of using the model in (12) rather than (11), we estimate both models at varying levels of aggregation. For comparison, we also estimate a linear demand model and report both coefficient estimates and the elasticity implied by the coefficient estimate when evaluated at mean values of price and quantity. The results are in reported in Table 4. Within each column of the table the estimated price coefficient is reported from each of these three demand models. The columns each represent a different level of data aggregation, and these levels correspond to those appearing in Table 3. All specifications utilize only pay-at-pump purchases, but the results exhibit similar patterns when estimated using all purchases.

Although the different forms of aggregation in Equations 11 & 12 have the potential to generate different elasticity estimates, in our data the levels of the estimates and the degree to which they change across increasing levels of aggregation are quite similar. Interestingly, the elasticities implied by the linear model also have a similar patterns across the different aggregation levels. We conclude from this that the aggregation bias apparent in our setting is relatively pervasive and is not the result of a particular functional form assumption or a particular mode of aggregation.
Table 4: Impact on Elasticity Estimates from Aggregating in Logs vs. Levels

(Pay-at-Pump Transactions Only)

<table>
<thead>
<tr>
<th>Geography:</th>
<th>city daily</th>
<th>city monthly</th>
<th>state daily</th>
<th>state monthly</th>
<th>national daily</th>
<th>national monthly</th>
</tr>
</thead>
<tbody>
<tr>
<td>Periodicity:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average of Logs (Eq. 11):</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\ln(price_{it})$</td>
<td>-0.295</td>
<td>-0.263</td>
<td>-0.250</td>
<td>-0.233</td>
<td>-0.004</td>
<td>-0.023</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.028)</td>
<td>(0.057)</td>
<td>(0.066)</td>
<td>(0.064)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Log of Averages (Eq. 12):</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\ln(price_{it})$</td>
<td>-0.295</td>
<td>-0.226</td>
<td>-0.247</td>
<td>-0.229</td>
<td>-0.007</td>
<td>-0.017</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.024)</td>
<td>(0.045)</td>
<td>(0.052)</td>
<td>(0.057)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Linear:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$price_{it}$</td>
<td>-0.046</td>
<td>-0.037</td>
<td>-0.037</td>
<td>-0.035</td>
<td>-0.006</td>
<td>-0.003</td>
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<tr>
<td></td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.006)</td>
<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Implied Elasticity</td>
<td>-0.296</td>
<td>-0.242</td>
<td>-0.242</td>
<td>-0.224</td>
<td>-0.039</td>
<td>-0.020</td>
</tr>
</tbody>
</table>

Fixed Effects:
- Day of Sample X X
- Day of Week X
- Month of Sample X X X
- Month of Year X
- City X X
- State X X

Note: Standard errors for panel specifications are robust and clustered at the level of the cross-sectional unit to allow for arbitrary serial correlation. Standard errors for time-series specifications are estimated using a Newey-West procedure and are robust to first-order serial correlation.

6.3.1 Decomposition of Aggregation Bias in a Linear Model

Restricting our attention to the linear model we can further decompose the amount of aggregation bias coming from several different sources. To begin, assume that the daily city-level per-capita demand for gasoline depends linearly on the price of gasoline with a slope that may differ across cities:

$$Q_{cd} = \alpha_d + \lambda_c + \beta_c p_{cd} + \epsilon_{cd}.$$
Suppose one were to consider a restricted regression model in which all cities are assumed to have the same price coefficient:

\[ Q_{cd} = \alpha_d^* + \lambda_c^* + \beta^* p_{cd} + \epsilon_{cd}^*. \]  

(14)

Using a residual regression approach, the estimated price coefficient from the restricted model (14) can be expressed as:

\[
\hat{\beta}^* = \left( \sum_{c,d} (p^r_{cd})^2 \right)^{-1} \left( \sum_{c,d} (p^r_{cd}) (Q^r_{cd}) \right)
\]  

(15)

where \( Q^r_{cd} \) and \( p^r_{cd} \) represent the residuals from regressions of \( Q_{cd} \) and \( p_{cd} \) respectively, on the day-of-sample fixed effects (\( \alpha_d \)) and city fixed effects (\( \lambda_c \)). From (13) we see that

\[ Q^r_{cd} = \beta_c p^r_{cd} + \epsilon^r_{cd}, \]

so:

\[
\hat{\beta}^* = \left( \sum_{c,d} (p^r_{cd})^2 \right)^{-1} \left( \sum_{c,d} (\hat{\beta}_c) (p^r_{cd}) \right) + \left( \sum_{c,d} (p^r_{cd})^2 \right)^{-1} \left( \sum_{c,d} (p^r_{cd}) (\hat{e}^r_{cd}) \right)
\]

(15)

where \( \hat{\beta}_c \) and \( \hat{e}_{cd} \) represent the coefficient estimate and the residual from the estimation of (13) and \( \hat{e}^r_{cd} \) represents the residual from the regression of \( \hat{e}_{cd} \) on day-of-sample and city fixed effects. The second term is equal to zero given the OLS moment condition.

To examine the impacts of aggregation we now consider a regression model aggregated to the month and state level, though different levels of aggregation take on a similar form. Define:

\[
Q_{sm} = \frac{1}{A_m B_s} \sum_{d \in A_m} \sum_{c \in B_s} Q_{cd} \quad \text{and} \quad p_{sm} = \frac{1}{A_m B_s} \sum_{d \in A_m} \sum_{c \in B_s} p_{cd}
\]

\[
\hat{D}_{sm} = \frac{1}{A_m B_s} \sum_{d \in A_m} \sum_{c \in B_s} (\hat{\alpha}_d + \hat{\lambda}_c) \quad \text{and} \quad e_{sm} = \frac{1}{A_m B_s} \sum_{d \in A_m} \sum_{c \in B_s} \hat{e}_{cd}
\]

where \( \hat{\alpha}_d, \hat{\lambda}_c \), and \( \hat{e}_{cd} \) represent the estimated fixed effects and residuals from the estimation of Equation 13. Notice that \( Q_{sm} \) can be expressed as:

\[
Q_{sm} = \hat{D}_{sm} + \frac{1}{A_m B_s} \sum_{d \in A_m} \sum_{c \in B_s} \hat{\beta}_c p_{cd} + e_{sm}
\]  

(16)
Based on these aggregate variable definitions, the aggregated regression model now takes the form:

\[ Q_{sm} = \alpha_m + \lambda_s + \beta^+ p_{sm} + \epsilon_{sm}. \]  

(17)

Once again, using a residual regression approach and the relationship in (16), the estimated price coefficient from the aggregated model (17) can be expressed as:

\[
\hat{\beta}^+ = \left( \sum_{s,m} (p_{sm}^r)^2 \right)^{-1} \left( \sum_{s,m} (p_{sm}^r) (Q_{sm}^r) \right) - \left( \sum_{s,m} (p_{sm}^r)^2 \right)^{-1} \left( \sum_{s,m} (p_{sm}^r) (Z_{sm}^r) \right) + \left( \sum_{s,m} (p_{sm}^r)^2 \right)^{-1} \left( \sum_{s,m} (p_{sm}^r) (e_{sm}^r) \right) + \left( \sum_{s,m} (p_{sm}^r)^2 \right)^{-1} \left( \sum_{s,m} (p_{sm}^r) (D_{sm}^r) \right)
\]

(18)

where \( Z_{sm}^r \) is the residual from regressing \( \sum_{d \in A_m} \sum_{c \in B_s} \hat{\beta}_c p_{cd} \) on month-of-sample and state fixed effects.

Each term in Equation 18 represents a different source of potential bias in the aggregated demand model. The first term can be used to assess the bias that results from aggregating across cities that differ in the price sensitivity of per-capita gasoline demand. If the slope of the per-capita demand curve is identical across cities or the prices observed in each city are completely independent of the slope of demand (i.e. \( Cov(\beta_c, p_{cd}) = 0 \)), then the first term in Equation 18 will be equal to both the mean value of \( \beta_c \) across cities and the \( \beta^* \) from the disaggregated model that restricts \( \beta \) to be the same in every city. However, if cities in which demand is more (less) responsive to price also tend to experience higher price levels, then the estimated slope of demand from the aggregated model will be biased upward (downward) in magnitude. The degree of this bias is represented by the difference between the first term in Equation 18 and the mean value of \( \beta_c \) from Equation 13.

The second term in Equation 18 represents bias that could arise from correlation between the aggregated prices and the aggregated day and city fixed effects from the disaggregate model. However, the aggregated fixed effects term \( (D_{sm}) \) is constant within a state across months and within a month across states, so it will be entirely colinear with the state and month fixed effects causing \( D_{sm}^r \) to be equal to zero. As a result, no bias will arise from
the second term of Equation 18 in any aggregated panel regression. This may not be true, however, in aggregated models using time-series or cross-sectional data.

When considering aggregated models that utilize time-series data the decomposition formula changes slightly because of the fact that a full set of fixed effects can no longer be included. For example, consider a model estimated with national daily average data that includes month-of-sample fixed effects as controls:

$$\hat{\beta} = \left( \sum_d (p_{rd}^m)^2 \right)^{-1} \left( \sum_d (p_{rd}^m)(Z_{rd}^m) \right) + \left( \sum_d (p_{rd}^m)^2 \right)^{-1} \left( \sum_d (p_{rd}^m)(D_{rd}^m) \right)$$

In this case the variables \( p_{rd}^m, Z_{rd}^m, D_{rd}^m, \) and \( e_{rd}^m \) now represent residuals from regressions of these daily averages on month-of-sample indicator variables. As a result, any within month-of-sample nationwide variation in quantity that was captured by the day fixed effect in the disaggregated model is now part of the error term. If these fluctuations are correlated with price but do not represent a demand response, then a classic endogeneity bias arises as a result of the inability to utilize a complete set of fixed effects to control for demand shocks when using aggregate (time-series) data. A similar potential for bias arises when using cross-sectional aggregate data.

The third term in Equation 18 represents bias that can arise from the introduction of correlation between the average prices and error term in the aggregated model. The OLS identification assumption in the disaggregated model requires \( p_{cd} \) to be uncorrelated with \( \epsilon_{cd} \). However, correlation between prices and demand shocks on other days or in other cities within the same state and month can cause \( p_{sm} \) and \( \epsilon_{sm} \) to be correlated, causing bias in estimates from aggregated panel data. If this correlation is positive (negative), estimates of demand response will be biased toward (away from) zero. Interestingly, if either the time or spatial dimensions are fully aggregated to either a time-series or cross-section, this bias no longer occurs. Since day-of-sample and city fixed effects are included in the disaggregated model, the errors from that model (\( \epsilon_{cd,s} \)) are orthogonal to the corresponding sample-wide city or day-of-sample average price. As a result, the third term of Equation 19 will be zero in time-series or cross-sectional models.
In Table 5 we examine the extent to which these three different sources of bias arise in our data. Each column presents the estimation results for a particular degree of aggregation. The top panel presents the price coefficient from the aggregate model and compares it to the price coefficient obtained using disaggregate data. Since a linear demand model is used in all specifications, we also report the elasticity implied by the coefficient when evaluated at the mean of the data. In the lower panel of Table 5 we decompose the bias by calculating the three different terms in Equation 18. In the aggregated panel regressions (first 3 columns), the first term in Equation 18 associated with a negative bias making demand appear up to 10% more elastic. This suggests that demand tends to be more responsive on the days and in the cities in which prices are higher, and that aggregation across days within each month and/or cities within each state generates bias due to correlation between aggregate prices and the aggregate demand response. In contrast, the third term in Equation 18 consistently contributes a positive bias in the aggregated panel regressions of up to 33%, making demand appear less elastic. Prices appear to have some positive correlation with unexplained demand shifts on other days within the month and in other cities within the state. In all cases this second component of bias overcomes the first, causing the aggregate panel regressions to underestimate demand elasticity.

In the aggregated time-series models (Columns 4 & 5 of Table 5) the source of bias is entirely different, resulting instead from the inability to include the time-period fixed effects that are present in the panel regressions. The incomplete set of time dummy variables are only partially able to control for macroeconomic fluctuations that may influence both gasoline demand and gasoline prices, and as a result, these models exhibit very inelastic estimates that are biased toward zero by 85% or more (from the second term in Equation 18).

We conclude from our decomposition that several important sources of bias may result from estimating gasoline demand using aggregate data, and that the source and degree of bias is likely to differ across the various levels of potential aggregation. Panel models of demand using geographically or temporally aggregated data can be biased either by an underlying correlation between the price responsiveness of demand in a location (or during a particular time) and the price level observed in that location (or at that time) or
Table 5: Decomposition of Aggregation Bias

*(Pay-at-Pump Transactions Only)*

<table>
<thead>
<tr>
<th>Geography:</th>
<th>Aggregated Panel Models</th>
<th>Time-Series Models</th>
</tr>
</thead>
<tbody>
<tr>
<td>Periodicity:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>city</td>
<td>(1)</td>
<td>national</td>
</tr>
<tr>
<td>monthly</td>
<td></td>
<td>daily</td>
</tr>
<tr>
<td>state</td>
<td>(2)</td>
<td>(4)</td>
</tr>
<tr>
<td>state</td>
<td>(3)</td>
<td>(5)</td>
</tr>
<tr>
<td>national</td>
<td></td>
<td></td>
</tr>
<tr>
<td>state</td>
<td></td>
<td></td>
</tr>
<tr>
<td>national</td>
<td></td>
<td></td>
</tr>
<tr>
<td>monthly</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Disaggregated:

| $\bar{\beta}_c$ (from Eq 13) | $-0.045$ | $-0.045$ | $-0.045$ | $-0.045$ | $-0.045$ |
|                             | (0.003)  | (0.003)  | (0.003)  | (0.003)  |

Implied Elasticity

| $-0.292$ | $-0.292$ | $-0.292$ |
|          |          |          |

Aggregated:

| $\hat{\beta}^+$ | $-0.037$ | $-0.037$ | $-0.035$ | $-0.006$ | $-0.003$ |
|                 | (0.003)  | (0.006)  | (0.007)  | (0.007)  |

Implied Elasticity

| $-0.242$ | $-0.240$ | $-0.224$ |
|          |          |          |

Bias from 1st Term of Eq. 18:

| $\bar{\beta}_c - \hat{\beta}^+_{\text{Term 1}}$ | $-0.0013$ | $-0.0036$ | $-0.0044$ | $0.0027$ | $0.0028$ |

Implied Elasticity Bias

| $-0.009$ | $-0.024$ | $-0.028$ |
|          |          |          |

Bias from Term 2 of Eq. 18:

| $\hat{\beta}^+_{\text{Term 2}}$ | $0$ | $0$ | $0$ |
|                                   |    |    |    |

Implied Elasticity Bias

| $0.0387$ | $0.0419$ |
|          |          |

Bias from Term 3 of Eq. 18:

| $\hat{\beta}^+_{\text{Term 3}}$ | $0.0090$ | $0.0116$ | $0.0148$ |
|                                   |         |         |          |

Implied Elasticity Bias

| $0.058$ | $0.075$ | $0.096$ |
|         |         |         |

Note: Included fixed effects in each specification are the same as those indicated in Table 4. Standard errors for panel specifications are robust and clustered at the level of the cross-sectional unit to allow for arbitrary serial correlation. Standard errors for time-series specifications are estimated using a Newey-West procedure and are robust to first-order serial correlation.
by a correlation between prices in one location (or time) and unobserved demand shocks in another location (or time) within the group of locations (or time-periods) being aggregated. In our application and sample the first type of correlation results in a negative bias while the second results in an even stronger positive bias causing overall elasticity estimates in these aggregated panel regressions to be biased toward zero. Time series models suffer from a different and potentially more serious potential bias due to the inability to use a complete set of time-period fixed effects to control for demand shifts that might occur as a result of macroeconomic or other market-wide shocks. If unexplained demand shifts do occur this will generate a positive correlation between price and the residual in the demand regression. In our sample this positive correlation is substantial enough to cause the elasticity estimates from our aggregated time-series models to be less than one-sixth the magnitude of those from our disaggregated model.

7 Short Run vs Longer Run Demand Elasticity

To this point our models have assumed that prices influence gasoline demand entirely through the current gasoline price. This restriction means that a change in price will be estimated to have the same impact on the demand for gasoline tomorrow as it will on the demand several months from now. In practice, however, it is not unusual for the demand curves to be more elastic in the short run than in the long run. Perhaps the most common of these situations occurs when consumers can hold inventories and in the short run choose to add to or withdraw from inventories in response to price changes even when they do not significantly change their consumption in the long run. Gasoline consumers obviously hold small inventories of gasoline in their vehicle's tank, so this behavior is feasible on a limited scale. Similarly, consumers may have the ability to postpone (or expedite) some necessary trips in response to a temporary increase (or decrease) in price, regardless of how they change their overall driving habits. These types of behavior imply that, for a given gasoline price today, the amount of gasoline purchased today might depend on whether the price has been at or near its current level for a while or whether it was significantly higher or lower a few days or a few weeks ago.
The elasticity estimate from most gasoline demand models (including our baseline model) represent some average of these shorter-run and longer-run responses. However, with daily data it is possible to separately identify these different responses by allowing demand to depend on past prices as well as current price levels. Moreover, by using the structure of our consumer purchase model and including past prices along with current prices in both the individual demand and purchase equations, we are able to decompose any potential short-run responses to examine whether consumers appear to be significantly altering gasoline usage or simply shifting when they make purchases in the days following a price change. If consumers are substituting away from driving in response to price increases then their daily demand may be influenced by past prices. If consumers are using their inventories of gasoline strategically, both current and past prices may influence a consumer’s probability of purchase. We alter Equations 1 & 2 to allow for these types of behavior. The demand for each customer in a city $j$ on a day $d$ can be specified as:

$$d_{jd} = \exp(\alpha_j + \lambda_d + \beta \ln(p_{jd}) + \sum_{l \in L} \zeta \ln(p_{jd-l}) + \epsilon_{jd}),$$

(20)

where $p_{jd-l}$ represents the price $l$ days prior to the current period and $L$ represents the set of lags lengths included in the specification. Similarly, the probability of purchase can be expressed as:

$$p_{jd} = \gamma_j + \delta_d + \psi \ln(p_{jd}) + \sum_{l \in L} \eta \ln(p_{jd-l}).$$

(21)

Leaving the consumer purchase model from Section 3 otherwise unchanged results in the following final representation of the aggregate quantity purchased in city $j$ on day $d$:

$$\ln(Q_{jd}) = \alpha_j + \lambda_d + \beta \ln(p_{jd}) + \sum_{l \in L} \zeta \ln(p_{jd-l}) + \ln(n_{jd}) - \ln(\hat{\rho}_{jd}) + \epsilon_{jd},$$

(22)

where the predicted purchase probability can be estimated from an OLS regression of:

$$\frac{n_{jd}}{N_{jd}} = \gamma_j + \delta_d + \psi \ln(p_{jd}) + \sum_{l \in L} \eta \ln(p_{jd-l}) + \nu_{jd}.$$

(23)

In both the demand equation and the purchase probability equation we include log of the current price and the lagged log prices from each of the previous 5 days as well as
Table 6: Purchase Model with Lagged Prices

<table>
<thead>
<tr>
<th></th>
<th>Traditional Model</th>
<th>Purchase Frequency Model</th>
<th>Demand Model</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Demand Equation</td>
<td>Purchase Equation</td>
<td>Demand Equation</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>ln(price$_{j,d}$)</td>
<td>-0.775 (0.081)</td>
<td>-0.009 (0.002)</td>
<td>-0.458 (0.009)</td>
</tr>
<tr>
<td>ln(price$_{j,d-1}$)</td>
<td>-0.607 (0.098)</td>
<td>-0.028 (0.002)</td>
<td>0.098 (0.008)</td>
</tr>
<tr>
<td>ln(price$_{j,d-2}$)</td>
<td>0.637 (0.091)</td>
<td>0.023 (0.001)</td>
<td>-0.002 (0.007)</td>
</tr>
<tr>
<td>ln(price$_{j,d-3}$)</td>
<td>0.393 (0.044)</td>
<td>0.014 (0.001)</td>
<td>0.052 (0.006)</td>
</tr>
<tr>
<td>ln(price$_{j,d-4}$)</td>
<td>0.184 (0.046)</td>
<td>0.003 (0.002)</td>
<td>0.020 (0.007)</td>
</tr>
<tr>
<td>ln(price$_{j,d-5}$)</td>
<td>0.055 (0.039)</td>
<td>0.003 (0.002)</td>
<td>-0.011 (0.007)</td>
</tr>
<tr>
<td>ln(price$_{j,d-10}$)</td>
<td>-0.076 (0.021)</td>
<td>-0.003 (0.001)</td>
<td>0.0002 (0.003)</td>
</tr>
<tr>
<td>ln(price$_{j,d-20}$)</td>
<td>-0.119 (0.021)</td>
<td>-0.001 (0.001)</td>
<td>0.004 (0.004)</td>
</tr>
<tr>
<td>ln(# of transactions$_{j,d}$)</td>
<td></td>
<td></td>
<td>0.995 (0.002)</td>
</tr>
<tr>
<td>ln(predicted probability of purchase$_{j,d}$)</td>
<td></td>
<td></td>
<td>-0.003 (0.001)</td>
</tr>
<tr>
<td>Total Implied Elasticity</td>
<td>-0.308</td>
<td>0.061</td>
<td>-0.296</td>
</tr>
</tbody>
</table>

20 Days After a Price Change

Note: City and day of sample fixed effects are present in all specifications. The dependent variable in Column 1 is the log of the average quantity purchased at the pump per capita by Visa customers in city $j$ on day $d$. Standard errors in Column 1 are robust and clustered to allow arbitrary serial correlation within a city. The dependent variables Columns 2 & 3 are the share of Visa customers purchasing at the pump and the log of the quantity purchased at the pump by Visa customers in city $j$ on day $d$. Standard errors in Columns 2 & 3 are generated using a nonparametric bootstrap that allows errors to be arbitrary serial correlated within a city and jointly distributed with the error term in the first-stage regression.
longer lags of 10 and 20 days. Lags longer than 20 days are omitted as their inclusion requires the use of a shorter sample for estimation, but when price lags of 40 and 60 days are included their coefficients are small in magnitude and do not substantially affect the estimates of the existing coefficients. For comparison we also estimate a similar version of the more traditional single-equation demand specification that includes lagged prices. Coefficient estimate from the traditional model are reported in in Column 1 of Table 6 and estimates from the demand and purchase probability equations from the frequency of purchase model are reported in Columns 2 & 3. All specifications are estimated using only pay-at-pump purchases. The final row of the table includes the total implied elasticity of the probability of purchase or of demand response after 20 days. Estimates from the traditional demand model and the demand equation in the purchase model are directly comparable to the corresponding specifications without lags in of Table 1.

In the traditional demand model (Column 1), the coefficients on the current and previous day’s log price are negative and much larger in magnitude than the corresponding elasticity estimated without lags. The sum of the first two coefficient estimates in Column 1 imply that the amount of gasoline purchased one day after a 1% price increase will will be 1.38% lower than it would have been without the price increase. During the following 3 to 4 days, however, the amount purchased tends to increase sharply, back towards its original level, canceling out much of the very strong initial response in purchasing. The 10- and 20-day lags reveal that the price response becomes slightly stronger once again, several weeks after the price change. Adding together the coefficients of all the price lags in the regression gives the response of demand 20 days after a permanent price change. This sum of coefficients is reported in the last row of Table 6 and implies that the elasticity of demand response after 20 days is −.31, nearly identical to the elasticities of −.30 identified in our baseline model with no lags.

Coefficient estimates from the frequency of purchase model reveal that the large response in the amount of gasoline purchased in the days following a price change results almost entirely from a temporary change in the probability of making a purchase rather

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19For the demand equations this is simply the sum of all the log-price coefficients. For the probability of purchase equation this is the sum of all the log-price coefficients divided by the mean probability of purchase.
than a change in gasoline demand or usage. The probability of purchase falls (rises) significantly on the day of and particularly on the day following a price increase (decrease). The coefficients on $\ln(p_{j,d})$ and $\ln(p_{j,d-1})$ imply that the purchase probability one day after a price change exhibits an elasticity with respect to price of around $-1.12$ all else equal.\footnote{The average probability of purchase by a cardholder on a given day is 0.033, so the elasticity of the probability of purchase is computed as $(-0.09 - 0.028)/0.033 = -1.12$.} However, this response in the probability of purchase in the day of and the day after a price change is entirely counteracted over the following few days to leave the elasticity of the overall response of purchase probability after 20 days to be small and slightly positive at .06.

In the demand equation (Column 3) the inclusion of lagged prices causes the coefficient on the current value of $\ln(p_{jd})$ to increase in magnitude, suggesting an even larger immediate demand response to price changes. As in the basic demand specification of Column 1, however, the sum of the coefficients on the current and lagged values of $\ln(p_{jd})$ in the demand equation are very similar to the coefficient estimates for $\ln(p_{jd})$ when no lagged prices are included (Table 1, Column 3). In other words, the total demand response to a price shock that lasts longer than a few days exhibits a demand elasticity of around $-0.30$, nearly identical to the estimates in our baseline purchase model. The results also reveal a small additional response within the first few days of a price shock, consistent with the idea that consumers delay/expedite gasoline usage by a few days in response to price fluctuations, but this effect does not appear to be as large as those resulting from changes in the probability of purchase.

The results of both the traditional demand model and the purchase frequency model provide evidence of a stronger responsiveness to price changes in the very short run, but also confirm that these responses occur in addition to a more persistent response which very closely resembles that implied by our earlier specifications. We conclude from this that the lower elasticity estimates in previous studies and in our monthly aggregated regressions were not a result of consumers being less responsive to price changes that persist over longer time periods. Instead it appears that the types of temporal aggregation bias and measurement error discussed in the previous section are responsible for reducing estimates
of demand response below those reflected in the daily data. This explanation is also consistent with the fact that additional aggregation of the analysis geographically produces a further reduction in estimated demand response despite the fact that there is little reason to suspect that consumers shift demand from state to state in response to relative price differences.

8 Conclusions

In this study we use high frequency panel data on gasoline prices and expenditures to re-examine the nature of gasoline demand in the U.S. We specify a model of gasoline purchase behavior that allows us to identify a measure of the short run elasticity of gasoline demand from data on gasoline expenditures. Our demand estimates are significantly more elastic than those of other recent studies. To investigate this discrepancy we aggregate our data, estimate demand models similar to those used in previous studies, and then perform a decomposition that identifies the degree of aggregation bias resulting from each of several possible sources. The results suggest that the sources of bias can differ depending on the degree and dimension of aggregation, but clearly show that the strongest bias occurs in time series models where a complete set of time-period fixed effects can not be included.

We also take advantage of the high frequency of our data to more carefully study how consumers respond immediately following a change in gasoline prices. Using our purchase frequency model we are able to separately identify the elasticity of gasoline usage or demand from the change in consumers’ probability of purchase. Our findings reveal a temporary response in the probability of purchase in the days following a price change as well as an immediate response in usage that does not dissipate over time. The resulting longer-run demand response implied by the dynamic model is consistent with the estimate from our static model.

Given that gasoline demand elasticity estimates are commonly used in policy evaluation and in broader economic research, our results provide valuable new evidence that gasoline demand may be more responsive to short term price fluctuations than was previously believed. Moreover, the results can substantially impact the inferences one draws
when evaluating market disruptions. For example, in early October of 2012 an Exxon re-
finery near Los Angeles experienced a power outage and was shut down for several weeks.
This refinery represents 15% of total gasoline production in the state and 25% of produc-
tion in Southern California. Several refineries in the state were already out of operation or
operating under full capacity and inventories were fairly low, so the unexpected outage led
prices in the Los Angeles area to increase by roughly 50 cents (an increase of around 13%) within a matter of days. According to our demand elasticity estimates ranging from -.27 to -.35, such a price increase might have caused the quantity demand to fall by as much as 4.5%, substantially contributing to the alleviation of the temporary supply shortfall. In contrast, using demand elasticity estimates closer to those generated by other recent studies (say for example -.05) would imply a negligible demand response of only 0.05%, suggest-
ing that almost the entire shortfall must have been made up through further withdraws from storage and expediting additional supplies from other markets. Having an accurate estimate of demand response is crucial for understanding how the market adjusts for such disruptions, and our results reveal that, particularly for temporary supply shocks, demand may play a much more important stabilizing role in the market than has recently been suggested.

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Appendices

A Exploring Potential Biases due to Substitution from Regular Grade to Premium Grade Gasoline

Our data include daily total revenues at gas stations aggregated to the city level and a city-level average of regular grade gasoline prices. Our measure of the quantity purchased in a city on a given day is generated by dividing the aggregate gasoline station revenue by the average price. One concern with this approach arises from the fact that roughly 15% of gasoline purchases are purchases of mid-grade or premium grade gasoline which sells at a higher price than regular grade. As a result, dividing total revenues (from sales of all grades) by the price of regular grade gasoline generates a slight overestimate of the number of total gallons purchased. If the relative shares of premium, mid-grade, and regular gasoline remains fairly constant over time the presence of premium and mid-grade purchases would not bias our elasticity estimates, as all identification is based on relative changes in prices and quantities over time. However, Hastings and Shapiro (2013) present evidence that gasoline consumers substitute from premium to regular grade fuel when gasoline prices increase. If the relative purchase shares of regular and premium gasoline fluctuate with price this substitution could bias our elasticity estimate. The following calculation approximates the potential size of such a bias.

Assuming that premium gasoline sells at a $\gamma \%$ premium over the regular grade price, the true quantity of gasoline purchased can be represented by:

$$Q_{true} = \frac{S_{reg} \cdot R}{P_{reg}} + \frac{(1 - S_{reg}) \cdot R}{P_{reg} \cdot (1 + \gamma)},$$

where $S_{reg}$ is the share of all gasoline purchased that is of regular grade and $(1 - S_{reg})$ represents the share of premium grade purchases, $P_{reg}$ represents the price of regular grade gasoline, $R$ is the total observed revenues from all gasoline sales. (For simplicity I am considering just regular and premium grades.)

The measure of quantity we use in the paper does not adjust for the price difference
between premium and regular grade sales, so our measure can be expressed as:

\[ Q_{\text{measured}} = \frac{R}{P_{\text{reg}}} = \frac{S_{\text{reg}} \times R}{P_{\text{reg}}} + \frac{(1 - S_{\text{reg}}) \times R}{P_{\text{reg}}}. \]

As a result, the bias in our measure of quantity will simply be:

\[ \text{Bias}_{Q} = Q_{\text{true}} - Q_{\text{measured}} = \frac{(1 - S_{\text{reg}}) \times R}{P_{\text{reg}}} - \frac{(1 - S_{\text{reg}}) \times R}{P_{\text{reg}} \times (1 + \gamma)} = \frac{\gamma}{1 + \gamma} (1 - S_{\text{reg}}) \times \frac{R}{P_{\text{reg}}}, \]

where \( \frac{\gamma}{1 + \gamma} \) reflects the percentage discount at which regular grade sells relative to premium grade, and \( \frac{\gamma}{1 + \gamma} (1 - S_{\text{reg}}) \) then represents the percent to which our quantity measure is biased (upward) from the true quantity as a result of assuming that all gasoline is sold at the regular-grade price.

Once again, if \( \gamma \) and \( S_{\text{reg}} \) are constant over time our demand elasticity estimates will not be affected by this bias. On the other hand, if the share of consumers buying regular grade gasoline (\( S_{\text{reg}} \)) increases when prices rise (as Hastings and Shapiro (2013) suggest) then some of the decrease in quantity we observe could be a result of people substituting to cheaper regular grade gasoline rather than people reducing gasoline consumption altogether. The magnitude of this effect, however, is almost sure to be rather small. Hastings and Shapiro (2013) estimate that a $1.00 increase in the gasoline price is associated with a 1.4 percentage point increase in the share of people buying regular grade gasoline. Given that prices average around $3.00 per gallon during our sample and the baseline share of people buying regular grade gasoline is around 85%, this would imply a price elasticity of substitution to regular grade gasoline of around 0.0005.

Measuring gasoline price in cents per gallon and assuming a mark-up on premium gasoline of around 10% over regular, this implies that:

\[ \frac{\partial \text{PercentBias}_{Q}}{\partial P_{\text{reg}}} = \frac{\gamma}{1 + \gamma} \left(- \frac{\partial S_{\text{reg}}}{\partial P_{\text{reg}}}\right) = 0.1 \times (-0.014) = -0.0014, \]

which in the absence of any actual demand response and at an average gasoline price of $3.00 would generate a false elasticity of demand of around \(-0.0014 \times 3 = -0.0042\). Given that our demand elasticity estimates are two orders of magnitude larger than this, any substitution from premium to regular gasoline associated with price changes will have a negligible impact on our analysis.
B Exploring the Bias in Estimated Gasoline Demand Elasticity Resulting from Non-Gasoline Purchases

As is discussed in Section 3, measuring quantity using the total revenues earned by gas stations in a city on a particular day divided by the city’s average price of gasoline will result in upward bias since around 21% of these revenues come from non-gasoline items. More importantly for our demand analysis, the estimated response of gasoline consumption to changes in prices will also biased upward (in absolute value). In our analysis we attempt to avoid this issue by using only pay-at-pump transactions in our quantity calculations. However, it is also possible to derive a reasonably accurate approximation of the magnitude of the bias that would result from using all transactions to calculate the quantity of gasoline sold. In this section we will derive the expected bias and show that it is fairly similar to the difference in estimated elasticities found when using only pay-at-pump transactions as opposed to all transactions.

Dividing total revenues by price results in a quantity measure
\[ \tilde{Q} = Q + \frac{K}{P}, \]
where \( Q \) represents the true quantity of gasoline sold, \( K \) represents the revenues from all non-gasoline sales, and \( P \) is the price of gasoline. Using this representation, the response of our measure of \( Q \) to a change in price will be:
\[ \frac{\partial \tilde{Q}}{\partial P} = \frac{\partial Q}{\partial P} \frac{K(P)}{P^2} + \frac{1}{P} \frac{\partial K(P)}{\partial P}. \]
Here we have allowed for the possibility that the demand for non-gasoline items may also be influenced by changes in the price of gasoline. As a result, the elasticity of our measure of \( Q \) with respect to price will be:
\[ \hat{\epsilon} = \frac{\partial \tilde{Q} P}{\partial P Q} = \frac{\partial Q P}{\partial P Q + K} - \frac{K(P)}{P^2} \frac{Q + K}{P} + \frac{1}{P} \frac{\partial K(P)}{\partial P} \frac{P}{Q + K}. \]
\[ = \epsilon_g \sigma_g - (1 - \sigma_g) + (1 - \sigma_g) * \epsilon_k \]
\[ = \epsilon_g \sigma_g + (1 - \sigma_g)(\epsilon_k - 1), \]
where \( \epsilon_g = \frac{\partial Q P}{\partial P Q} \) represents the true elasticity of demand for gasoline, \( \epsilon_k = \frac{\partial K(P)}{\partial P} \frac{P}{K} \) represents the elasticity of non-gasoline revenues with respect to the price of gasoline, and
\[ \sigma_g = \frac{PQ}{PQ+K} \] represents the share of total revenues coming from gasoline sales. Alternatively, the true elasticity of demand for gasoline can be expressed as a function of our all-transactions elasticity estimate:

\[ \epsilon_g = \frac{\tilde{\epsilon}}{\sigma_g} + \frac{1 - \sigma_g}{\sigma_g} (\epsilon_k - 1). \]

Based on this result, we can approximate the expected bias using estimates of parameters from previous studies. A survey of gas station consumers conducted by the National Association of Convenience Stores (NACS) estimates that around 27% of gas stations transactions are in cash, and only 44% of customers enter the store during their visit. Assuming that all cash paying customers enter the store, at most 17% of all customers (or 39% of those entering the store) are potentially making non-gasoline purchases with a credit card. Data from the U.S. Economic Census suggests that 21% of all gas station revenues come from non-gasoline items. Supposing that gasoline revenues can be attributed proportionally across all payer types and non-gasoline revenues attributed proportionally across customers entering the store, this would suggest that non-gasoline purchases represent roughly 6% of non-cash customers’ total spending.

There are no direct estimates of how the demand for non-gasoline items at the station responds to changes in the price of gasoline, but Gicheva et al. (2007) have examined how gasoline prices impact consumer spending more generally. They estimate that consumers reduce spending on \textit{food away from home} by 45-56% when gas prices increase by 100%. However, they also find that grocery spending actually increases as consumers cook at home more and that consumers substitute from more expensive to less expensive items within each category. In some ways, food and drink from a gasoline convenience store can be considered part of the food away from home category, but packaged items from the convenience store could also be considered as similar to grocery items (or less expensive options compared with other food away from home). As a result, it could be that spending

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\(^{21}\)See the 2014 NACS Retail Fuels Report ([http://www.nacsonline.com/YourBusiness/FuelsReports/GasPrices_2014/Pages/default.aspx](http://www.nacsonline.com/YourBusiness/FuelsReports/GasPrices_2014/Pages/default.aspx)). Survey data from previous years could not be used because the question determining the share of customers entering the store was not included.

\(^{22}\)The 6% results from multiplying the fraction those visiting the store that do not pay cash times the share of revenues that are non-gasoline (39% × 21%) and then dividing this by the the overall share of non-cash customers times the share of revenues from gasoline (73% × 79%).

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in convenience stores decreases when gas prices rise, but probably not to the same extent as other food away from home.

Considering these findings, suppose the share of total credit card customer revenues coming from gasoline sales ($\sigma_g$) is 94% ($= 1 - .06$), and the elasticity of non-gasoline revenues with respect to the price of gasoline ($\epsilon_k$) is 33%. Then, based on the bias derived above and the demand elasticity estimated using all purchases in our data, the true gasoline demand elasticity can be approximated as:

$$\epsilon_g = \frac{\bar{\epsilon}}{.94} - .064 \times (-1.33) = 1.064\bar{\epsilon} + .085.$$  

Interestingly, the bias-adjusted elasticity estimates implied by this relationship are relatively similar in magnitude to the elasticities obtained by estimating demand using only pay-at-pump transactions (reported in Table 1). For example, applying this bias-adjustment to the elasticity estimate of $-0.328$ from the traditional demand model estimated using all transactions (Column 4) would imply a true elasticity of gasoline of $\epsilon_g = 1.064(-.328) + .085 = -.264$, which is very close to the coefficient of $-0.259$ that we obtain when estimating the same model using only pay-at-pump transactions (Column 1). The estimate from our probability of purchase model (Column 6) would imply a bias-adjusted elasticity estimate of around $-0.336$ which is also much closer to (though not as low as) the estimate of $-0.288$ obtained using only pay-at-pump transactions (Column 3).

We conclude from the calculations above that biases resulting from the presence of non-gasoline purchases in our data are non-negligible in magnitude but are nowhere near large enough to explain the differences between our elasticity estimates and those obtained by other recent studies. Moreover, our ability to estimate demand using only pay-at-pump purchases appears to eliminate, or perhaps over-correct for, any bias resulting from non-gasoline purchases.
C Instrumental Variables Estimates

Table 7 reports the instrumental variables estimation results for both the basic model (Column 1) and the restricted and unrestricted versions of the consumer purchase model (Columns 2 & 3) presented in Section 4. All specifications are estimated using only pay-at-pump purchases. In an attempt to utilize exogenous variation refinery market conditions to identify supply-driven price fluctuations we use the log of the current "spot market" price of gasoline in the region as an instrument for local retail prices. While using measures of refinery outages directly as instruments might seem like attractive alternative, the relationship between outages and gasoline prices is complex and difficult to capture in a model because the impact depends also on the level of inventories in the market, the ease of importing from alternative sources, the available capacity at other refineries in the area, etc. Using spot market prices reveals the net impact of these outages on gasoline markets. Input prices further upstream are also not likely to be useful instruments here, as oil prices do not exhibit the regional variation necessary to identify fluctuations in relative gasoline prices across different parts of the country.

We use the spot market gasoline price data reported by the Department of Energy’s Energy Information Administration for either New York Harbor, the Gulf Coast, or Los Angeles depending on which of these large refining centers is most closely integrated into the city’s gasoline supply network. Petroleum Area Defense District (PADD) geographic definitions are used to classify this supply network integration. The New York Harbor spot price is used as an instrument for cities in the New England and Central Atlantic portions of PADD 1; the Gulf Coast spot price is used for cities in the Lower Atlantic region of PADD 1 and for cities in PADDs 2, 3, and 4; and the Los Angeles spot price is used for PADD 5 cities. Resulting estimates are directly comparable to the OLS estimates from Columns 4 through 6 of Table 1.
Table 7: IV Estimates of Baseline Empirical Model of Demand

\[ \text{Dependent Variable} = \ln(\text{quantity}_{jd}) \]

<table>
<thead>
<tr>
<th></th>
<th>Pay-at-Pump Only</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>( \ln(\text{price}_{jd}) )</td>
<td>-0.357 (0.050)</td>
</tr>
<tr>
<td>( \ln(# \text{ of transactions}_{jd}) )</td>
<td>1 0.996</td>
</tr>
<tr>
<td>( \ln(\text{predicted probability of purchase}_{jd}) )</td>
<td>-1 -0.003</td>
</tr>
</tbody>
</table>

Note: Day-of-sample fixed effects and city fixed effects are included in all specifications. Log retail gasoline prices in each city are instrumented for using the log of the spot price of gasoline from the either New York Harbor, the Gulf Coast, or Los Angeles depending on which of these refining centers is most closely linked with the city’s supply infrastructure. Standard errors in Columns 1 & 4 are robust and clustered to allow serial correlation within city. Standard errors for the remaining specifications are generated using a nonparametric bootstrap that allows errors to be serially correlated within a city and jointly distributed with the error term in the first-stage regression.

D Replication of Hughes et al. (2008) Model and Comparison to Visa Data

Once aggregated to a national time series our elasticity estimates are fairly close to those of Hughes et al. (2008) who estimate an identical specification using data from 2000–2006. Although our data is from a later period, we can generate a more accurate comparison by replicating the same specification using their data sources but for our later time period. As in their study, gasoline consumption is measured using the EIA’s monthly nationwide estimate of motor gasoline “product supplied”. Price is measured using the U.S. city average price for unleaded regular gasoline as reported in the U.S. Bureau of Labor Statistics’ CPI-Average Price Data and is converted to constant 2005 dollars using the GDP implicit price deflator.

To check our ability to replicate the Hughes et al. (2008) analysis we first estimate their baseline double-log specification (equivalent to Column 5 of Table 3 above) for the period 2001–2006. Results are reported in Table 8, Column 1. The estimate of price elasticity (−.042) is identical to that of Hughes et al. (2008).\(^{23}\) Estimating the same specification

\(^{23}\)Our estimate of the income elasticity is .32 as opposed to their estimate of .53. This discrepancy appears
Table 8: Elasticity Estimates from Replication of Hughes et al. (2008)

Dependent Variable = \( \ln(\text{quantity per capita}) \)

<table>
<thead>
<tr>
<th></th>
<th></th>
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Data Source:</td>
<td>EIA/BLS</td>
<td>EIA/BLS</td>
<td>Visa/AAA (all purchases)</td>
<td>Visa/AAA (pay-at-pump)</td>
</tr>
<tr>
<td>( \ln(\text{price}_t) )</td>
<td>-0.042</td>
<td>0.026</td>
<td>-0.122</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.024)</td>
<td>(0.016)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>( \ln(\text{income}_t) )</td>
<td>0.321</td>
<td>-1.272</td>
<td>0.154</td>
<td>0.093</td>
</tr>
<tr>
<td></td>
<td>(0.066)</td>
<td>(0.405)</td>
<td>(0.167)</td>
<td>(0.189)</td>
</tr>
<tr>
<td>Month-of-Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Note: Columns (1) and (2) use EIA data on “product supplied” to measure quantity and the CPI average price data for unleaded regular to measure price. Column (3) uses our Visa expenditure data to measure quantity and a weighted average of our AAA average price data to measure price. Standard errors are estimated using a Newey-West procedure and are robust to serial correlation.

Using data from 2006–2009 yields a price elasticity that is slightly positive and not significantly different from zero. This is very similar to our estimate from the same specification using aggregated Visa pay-at-pump purchase data (reported in Column 4) but is much less elastic than our estimate using all Visa purchases (Column 3). The use of an alternative data source may be partially responsible for differences between our elasticity estimates and those of previous studies, but overall the results above suggest that most of the difference likely a result of the level of data aggregation.

to have been caused by the fact that Hughes et al. (2008) use previously published estimates of disposable personal income that have since been revised by the BEA. We utilize the updated estimates so that our income measures are consistent with those available for the 2006–2009 sample period.