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ABSTRACT

We formulate a test of the fungibility of money based on parallel shifts in the prices of different quality grades of a commodity. We embed the test in a discrete-choice model of product quality choice and estimate the model using panel microdata on gasoline purchases. We find that when gasoline prices rise consumers substitute to lower octane gasoline, to an extent that cannot be explained by income effects. Across a wide range of specifications, we consistently reject the null hypothesis that households treat "gas money" as fungible with other income. We evaluate the quantitative performance of a set of psychological models of decision-making in explaining the patterns we observe. We also use our findings to shed light on extant stylized facts about the time-series properties of retail markups in gasoline markets.

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1 Introduction

Neoclassical households treat money as fungible: a dollar is a dollar no matter where it comes from. But many households keep track of separate budgets for items like food, gas and entertainment (Zelizer 1993). Some even physically separate their money into tins or envelopes earmarked for different purposes (Rainwater, Coleman and Handel 1959). In hypothetical choices, participants routinely report different marginal propensities to consume out of the same financial gain or loss depending on its source (Heath and Soll 1996). Mental budgeting has been linked to the effects of public policies such as income tax withholding (Feldman 2010), tax-deferred retirement accounts (Thaler 1990), and the effect of fiscal stimulus (Sahm, Shapiro and Slemrod 2010). Despite these links and despite a large body of anecdotal and laboratory evidence on mental budgeting, there is little empirical evidence measuring its importance in the field.

In this paper we study mental budgeting in the field using data on consumer purchase decisions. Our empirical test is based on the following thought experiment (Fogel, Lovallo and Caringal 2004). Consider a household with income M. The household must purchase one indivisible unit of a good that comes in two varieties: a low-quality variety with price P_L and a high-quality variety with price P_H, where $P_H > P_L$. Now consider two scenarios. In the first scenario, the prices of the two varieties each increase by Δ dollars to $P_L + \Delta$ and $P_H + \Delta$ while household income remains constant at M. In the second scenario, the household's income declines by Δ dollars to $M - \Delta$ while prices remain constant (at P_L, P_H). Both scenarios lead to the same budget constraint and hence to the same utility-maximizing behavior. However, the household may not see it that way.

Suppose the household has a mental budget for the product category in question. In the price-increase scenario, the mental budget for the category in question will be strained if Δ is large when viewed against category expenditures. In contrast, in the income-loss scenario, the "pain" of the equivalent income decline can be spread across many categories. The psychology of mental accounting means that the household will be more likely to substitute from the high- to the low-quality variety under the price-increase scenario than under the income-loss scenario, even though for a utility-maximizing household the two are equivalent.

We test the mental accounting hypothesis using data on purchases of gasoline. Gasoline comes in three octane levels—regular, midgrade, and premium—which differ in price and perceived quality. When global supply and demand conditions cause an increase in the price of oil, the prices of all three grades of gasoline tend to increase in parallel. The psychology of mental accounting predicts that such price increases will result in significant substitution towards regular gasoline and away from premium and midgrade varieties, whereas correspondingly large changes in income from other sources will induce far less substitution.

We demonstrate the effect of gasoline prices on quality choice in both aggregate data from the Energy Information Administration, covering the period 1990-2009, and panel microdata on households' purchases of gasoline from a large grocery retailer with gas stations on site, covering the period 2006-2009. In both data sources there is a clear positive effect of gasoline prices on the market share of regular gasoline.

Two facts suggest that the relationship between gasoline prices and octane choice cannot be explained by income effects. First, in the second half of 2008 gasoline prices fell due to the deepening of the financial crisis and associated recession. During this period, although almost all indicators of consumer spending and well-being were plummeting, households substituted to higher-octane gasoline. Second, the magnitude of the income effects necessary to explain the time-series relationship between gasoline prices and octane choice is inconsistent with cross-sectional evidence. We find that a \$1 increase in the price of gasoline increases a typical household's propensity to purchase regular gasoline by 1.4 percentage points. Because the average household buys about 1200 gallons of gasoline per year, that is also the implied effect of a \$1200 loss in income. However, cross-sectional estimates imply that a \$1200 reduction in household income increases the propensity to buy regular gasoline by less than one tenth of one percentage point.

To formally test the null hypothesis that consumers treat money as fungible, we develop a discrete-choice model of gasoline grade demand. In the model, households trade off the added utility of more expensive grades against the marginal utility of other consumption goods. As the household gets poorer, either through a loss of income or an increase in gasoline prices, the marginal utility of other consumption goods rises relative to the marginal utility of higher-octane gasoline, leading to substitution towards lower octane levels. Under standard utility-maximization, the model implies fungibility in the sense of our thought experiment: a parallel shift in the prices of all grades is behaviorally equivalent to an appropriately scaled change in income. We translate this implication into a formal statistical test of the null hypothesis that households treat money from different sources as fungible.

We estimate the model on our retailer panel, which contains data on over 10.5 million gasoline transactions from 61,494 households. The panel structure of the data permits us to observe the purchases of the same household over time, and hence to address possible confounds from household heterogeneity. We compare the effect of changes in the gasoline price to the effect of comparable variation in household income, both in the cross-section and over time. Across a range of specifications we confidently reject the null hypothesis that households treat money as fungible regardless of its source, in favor of the prediction of the psychology of mental accounting.

We consider a number of alternative explanations for the observed pattern, including changes over time in the composition of households buying gasoline, misspecification of the marginal utility function, correlation between gasoline prices and other prices, measurement error and transitory shocks to income, and supply-side responses to gasoline price increases. None of these alternatives can account for the large deviations from fungibility that we observe.

To further check our identification strategy, we conduct a placebo exercise in which we test whether gasoline money and other money are treated as fungible when households make a quality choice in a non-gasoline domain. In particular, we re-estimate our baseline specification on data on households' choice of orange juice and milk brands. We find that poorer households buy less expensive brands of orange juice and milk, but that gasoline prices exert a weak (and statistically insignificant) positive effect on the quality of brands chosen in these categories. We cannot reject the null hypothesis that consumers treat gasoline money and other money as fungible when choosing among milk or orange juice brands.

Having established that a discrete-choice model with fungibility cannot explain our findings, we turn to an evaluation of several alternative models of decision-making. We consider two models that might plausibly explain our findings: a loss-aversion model based on Köszegi and Rabin (2006) and a salience model based on Bordalo, Gennaioli, and Shleifer (2012). For each model, we formally estimate the model's parameters on our panel, compute choice probabilities at the estimated parameters, and compare the model's prediction for the path of octane choice to the observed data.

Finally, we consider the implications of our findings for retailer behavior. Our findings indicate that consumers will put a higher premium on saving money on gas in high-price times than in low-price times. This implies that retailers should face more intense competition during high-price times, and hence that retail markups should fall. We use a stylized model of retailer pricing to show that our estimated model can partly (but not fully) account for the inverse relationship between gasoline prices and retailer markups documented in Lewis (2011).

The primary contribution of this paper is to provide evidence of mental accounting "in the wild." Most evidence on mental accounting (Thaler 1999) or the closely related phenomenon of choice bracketing (Read, Loewenstein and Rabin 1999) comes from hypothetical choices or incentivized laboratory behaviors (Fogel, Lovallo and Caringal 2004). Important exceptions include Kooreman's (2000) study of child care benefits in the Netherlands, Milkman and Beshears' (2009) study of the marginal propensity to consume out of a coupon in an online grocery retail setting, and related work by Abeler and Marklein (2008) and Feldman (2010). To our knowledge, ours is the first paper to test for mental accounting in the response to prices and the first to illustrate the effect of price-induced variation in "category income" on purchase decisions.

To our knowledge, ours is also the first paper to estimate Köszegi and Rabin's (2006) or Bordalo, Gennaioli, and Shleifer's (2012) model using data on retail purchases. In that sense, the paper contributes to a growing literature that uses consumer microdata to structurally estimate the parameters of psychological models of decision-making (Conlin, O'Donoghue and Vogelsang 2007, Barseghyan et al 2011, Grubb and Osborne 2012). The paper also contributes to research on supply-side responses to consumers' psychological biases (DellaVigna and Malmendier 2004).

Methodologically, we follow Allenby and Rossi (1991), Petrin (2002) and Dubé (2004) in enriching the role of income effects in discrete-choice models of household purchase decisions. We show that incorporating mental accounting significantly improves model fit. In that sense, we also contribute to a literature in marketing that incorporates psychological realism into choice models with heterogeneity (Chang, Siddarth and Weinberg 1999).

Two existing literatures predict the opposite of what we find. First, a literature following Barzel (1976) exploits tax changes to test the Alchian-Allen conjecture that higher category prices result in substitution to *higher* quality varieties (Sobel and Garrett 1997). In the context of gasoline, Nesbit (2007) and Coats, Pecquet and Taylor (2005) find support for the Alchian-Allen conjecture; Lawson and Raymer (2006) do not. Second, a literature in psychology and economics examines "relative thinking" in which consumers focus on ratios when normative decision theory implies that they should focus on differences (Azar 2007 and 2011). In section 7 we discuss a possible reconciliation of our findings with those of the relative thinking literature.

The remainder of the paper is organized as follows. Section 2 provides background information on grades of gasoline. Section 3 describes our data. Section 4 presents our model of consumer choice and discusses our empirical strategy for testing fungibility. Section 5 presents a descriptive analysis of gasoline grade choice. Section 6 presents estimates of our model. Section 7 presents evidence on alternative psychological mechanisms underlying our findings. Section 8 discusses implications for retailer behavior. Section 9 concludes.

2 Background on Gasoline Grade Choice

Gasoline typically comes in three grades, with each grade defined by a range of acceptable octane levels: regular (85-88), midgrade (88-90), and premium (90+) (EIA 2010). A higher octane level increases gasoline's combustion temperature so that it can be used in high-compression engines (which yield higher horsepower for a given engine weight) without prematurely igniting (also known as "knocking").

Typically, a gasoline retailer maintains a stock of regular and premium gasoline on site, and midgrade is produced by mixing regular and premium at the pump. Regular and premium gasoline are, in turn, produced at refineries by blending intermediate product streams with different chemical properties so that the resulting blend matches the desired specifications, including octane level. Typically there are multiple ways to arrive at an acceptable final product, and refineries use programming models to decide on the profit-maximizing mix given spot prices for various input, intermediate, and output streams. Changing the output of the refinery to include, say, more premium and less regular gasoline would involve changing the mix of intermediate streams used in gasoline production (Gary and Handwerk 2001), which can be achieved seamlessly for small changes in the product mix.

A large proportion of high-octane gasoline sales go to cars that do not require it, with most consumers justifying their purchase of premium gasoline on "vague premises" (Setiawan and Sperling 1993). Most modern cars have knock sensors that prevent knocking at any octane level. Perhaps because auto makers often recommend premium gasoline for sports cars, the most frequently stated reason for using high-octane gasoline is a performance gain, for example in the time to accelerate from 0 to 60 miles per hour (Reed 2007). *Consumer Reports* (2010) and other consumer advocates have questioned whether such performance gains are real. Buyers of high-octane gasoline may also believe that using above-regular grades helps promote long-term engine cleanliness and health, but because detergents are required for all grades of gasoline, using above-regular grades does not in fact help an engine "stay clean" (Reed 2007). In addition, any supposed gains in fuel economy from using high-octane grades are "difficult to detect in normal driving conditions" (API 2010; see also Click and Clack 2010). Thus, according to Jake Fisher at *Consumer Reports*, "There are two kinds of people using premium gas: Those who have a car that requires it, and the other kind is a person who likes to waste money" (Carty 2008).

It is well known that higher octane gasolines tend to lose market share when the price of gasoline goes up (Lidderdale 2007), a phenomenon that gasoline retailers call "buying down" (Douglass 2005). Due to their association with good performance, high-octane varieties are perceived as a luxury good that the consumer can do without. However, industry analysts have noted that buying down is surprising in light of the small stakes involved: "It really doesn't add up to very much... It's more of a psychological thing. You're at the pump, and it seems like every time you hit a certain threshold, you cringe" (industry analyst Jessica Caldwell, quoted in Lush 2008). The commonly held psychological interpretation of buying down is consistent with experimental evidence on mental accounting, and motivates the analysis that follows.

3 Data

3.1 Panel Microdata

Our main data source is a transaction-level file from a large U.S. grocery retailer with gasoline stations on site. The data include all gasoline and grocery purchases made from January 2006 through March 2009 at 69 retail locations, located in 17 metropolitan areas in 3 different states.

For each gasoline transaction, the data include the date, the number of gallons pumped, the grade of gasoline (regular, midgrade, or premium), and the amount paid. We use these data to construct a price series by store, grade, and date equal to the modal price across all transactions, where transaction prices are calculated as the ratio of amount spent to number of gallons, rounded to the nearest tenth of a cent. The majority of transactions are within one tenth of one cent of the daily mode, and 88 percent of transactions are within one cent of the daily mode.

The data allow us to match transactions over time for a given household using a household identifier linked to a retailer loyalty card. Approximately 87 percent of gasoline purchases at the retailer can be linked to a household identifier through the use of a loyalty card.

Our main measure of household income is supplied by the retailer, and is based on information given by the household to the retailer when applying for the loyalty card, supplemented with data, purchased by the retailer from a market research firm, on household behaviors (e.g., magazine subscriptions) that are correlated with income.

For comparison and sensitivity analysis we also make use of two geography-based measures of income. For the large majority of households in our sample, the retailer data include the census block group of residence. We use this to obtain 2000 U.S. Census income data at the block group level. We further match block groups to zipcodes using 2000 Census geography files provided by the Missouri Census Data Center (2011). For each zip code, we obtain annual measures for 2006, 2007, and 2008 of the mean adjusted gross income reported to the IRS (Mian and Sufi 2009).

For estimation we use a subsample comprised of purchases by households that make at least 24 gasoline purchases in each year of 2006, 2007, and 2008, and for whom we have a valid household income measure. We exclude some outlier cases from the estimation sample.[1] The final sample we use in estimation includes $10,548,175$ transactions by $61,494$ households.

[1] These are: households that purchase more than 665 times over the length of the sample, households that ever purchase more than 210 times in a given year, households that ever purchase more than 10 times in a given week, and a small number of transactions that involve multiple gasoline purchases. We also exclude from the sample a small number of store-days in which reported prices are too large by an order of magnitude, and a small number of store-days in which stockouts or reporting errors mean that only one grade of gasoline is purchased. Together, these exclusions represent 4.78 percent of transactions.

To estimate the effect of gasoline prices on non-gasoline consumption, we exploit the fact that our data allow us to match gasoline transactions to grocery transactions by the same household. As an overall measure of household consumption, we compute total grocery expenditures by household and week.

We also examine two categories of grocery expenditure in more detail: refrigerated orange juice and milk. We focus on these categories as they are perishable, relatively high in volume, and involve clear quality and price delineations (for example, between conventional and organic varieties.) We aggregate individual UPCs in these categories into products grouped by size and brand and construct a weekly price series for each store and product. Appendix B contains additional details on how we group UPCs into products and how we construct the price series. For estimation, we use data on households that purchase at least once in the category in each sample year. We exclude households that purchase 200 or more times in a given category in any sample year. In the online appendix, we present estimates of our key results using even tighter restrictions on frequency of purchase and show that our substantive conclusions are unchanged.

3.2 Aggregate Data

To confirm that the key patterns in the retailer panel are representative, we use monthly data from 1990-2009 on retail prices and sales volume by grade of gasoline for the 50 states (and the US total) obtained from the Energy Information Administration (EIA) at eia.doe.gov in June 2010. Portions of our analysis also make use of national and regional weekly price series obtained from the EIA in April 2012. The EIA collects price and volume data from a sample survey of retailers and a census of prime suppliers, essentially large firms that deliver a significant volume of petroleum products to "local distributors, local retailers, or end users" (EIA 2009). The online appendix reports estimates of our model using the state-level EIA data.

We supplement the EIA data with data from the Consumer Expenditure Survey (CEX) Interview Files, 2006-2009. We use the Consumer Expenditure Survey data to evaluate the representativeness of grocery expenditures in our sample and to project the total annual expenditures of sample households.

3.3 Sample Representativeness

Table 1 evaluates the representativeness of our sample on key dimensions of interest. The first column presents statistics for all households in the retailer database. The second column presents statistics for households in our estimation sample. The third column presents representative state-level statistics for the three states our retail sites are located in. Thus comparing columns (1) and (2) reveals differences between all households purchasing gasoline and those purchasing gasoline at least 24 times per year during our 3-year period, and comparing columns (1) and (3) reveals differences between the retailer's customers and state populations.

Given our requirement that households in the estimation sample purchase gasoline at the retailer at least 24 times per year for a little over 3 consecutive years, the majority of households are excluded from our estimation sample. During our sample period, households could move, stop in to one of our retail stores even if they live in other areas, discard their loyalty cards, or purchase their gasoline primarily at other gasoline retailers. However, while the households in our estimation sample are a minority of the households in the full retailer database, on most dimensions the two samples look similar. Census block group incomes, commute times, and public transportation usage are similar between the two samples, with estimation sample households living in slightly higher-income block groups. Estimation sample households earn somewhat more income per year than households in the full retailer sample. Estimation sample households buy a similar amount of gasoline per trip to households in the full sample. The main points of distinction between estimation sample households and those in the full sample result directly from our selection rule. Estimation sample households make more gasoline trips per purchase month and buy more groceries at the retailer than do households in the full sample. Importantly, estimation sample households live much closer to their most-frequently-visited retailer site than the average retailer patron, which may in turn explain their greater propensity to buy gasoline and groceries from the retailer.

The third column of the table shows means for all households in the three states from which we draw our retailer data, with each state weighted according to its number of households in the full retailer database. Relative to the average household, households from the retailer data live in higher-income block groups. Households in the retailer sample buy slightly less regular gasoline than reported in the EIA data for the same states, and also pay about 4-5 cents less per gallon of gasoline than the state average as reported by the EIA. The lower average price per gallon at retailer sites presumably arises because the retailer does not sell a major brand of gasoline, whereas the EIA average price series is based on data that include (higher) major-brand prices. Sample households spend less on groceries at the retailer than the average household in the state spends on groceries overall, presumably reflecting the fact that sample households buy some groceries at other retailers.

3.4 Validity of Income Measures

The geographic variation in our main household income measure corresponds well with data from other sources. The median of our household income measure at the Census block group level has a correlation of 0.82 with median household income from the 2000 Census. The mean of our household income measure at the zipcode level has a correlation of 0.77 with mean adjusted gross income in the zipcode, as reported to the IRS in 2008.

A drawback of our main household income measure is that it is only available at a single point in time. To address this limitation, we use our measure of household grocery expenditures to proxy for time-varying shocks to household income. Existing literature shows that food expenditure responds to variation in income in the cross-section and over time, predicting about 40 percent of the cross-sectional variation in total expenditure (Skinner 1987) and responding significantly to shocks to current and future household income (Stephens 2001, 2004, Japelli and Pistafferi 2010).

Table 2 shows that, in our data, food expenditures are related to income variation in the cross-section and over time. Across households, we estimate an income elasticity of grocery expenditure of 0.17, which closely matches the analogous estimate of 0.17 from the Consumer Expenditure Survey. Across zipcodes, we estimate an elasticity of 0.14. Importantly, the zipcode-level relationship remains similar in magnitude (at 0.09) and marginally statistically significant in a model with zipcode fixed effects, indicating that changes in income at the zipcode level are correlated with changes in food expenditure at our retailer. These findings lend credibility to food expenditures as a proxy for shocks to income over time, especially in light of the large existing literature establishing the responsiveness of food expenditures to shocks.

4 Econometric Framework

4.1 Model

Suppose that household i chooses among gasoline grades indexed by $j \in \{0, ..., J\}$ where $j = 0$ denotes regular gasoline and p_{jt} is the price per gallon of grade j at time t. The household must buy $q_{it} > 0$ gallons of gasoline in period t.

Following convention (see, e.g., Berry, Levinsohn and Pakes 1995, Nevo 2000), money not spent on gasoline is spent on other goods. Other goods deliver indirect utility $\Lambda(m_{it} - q_{it} p_{jt})$, where m_{it} is the household's total per-period expenditures. We normalize $\Lambda(m_{it} - q_{it} p_{0t}) \equiv 0$.

Let U_{ijt} be household i's utility from purchasing grade j at time t, and let $u_{ijt} = U_{ijt}/q_{it}$ be utility per gallon of gasoline. We assume that

$$U_{ijt} = v_{ijt} q_{it} + \Lambda(m_{it} - q_{it} p_{jt}) \tag{1}$$

where v_{ijt} is a taste parameter. The specification in (1) has the fungibility property described in the introduction: an increase in gasoline prices of \$1 is equivalent to a decrease in non-gasoline expenditures of q_{it}

dollars.

We take a first-order approximation to $\Lambda(m_{it} - q_{it}p_{jt})$ around $m_{it} - q_{it}p_{0t}$, which gives per-gallon utility

$$u_{ijt} = v_{ijt} - \lambda_{it}(p_{jt} - p_{0t}) \tag{2}$$

where λ_{it} is household i's marginal utility of non-gasoline expenditures at time t and is a function of $m_{it} - q_{it}p_{0t}$.

We assume that tastes are given by

$$v_{ijt} = \alpha_{ij} + \varepsilon_{ijt}, \tag{3}$$

where α_{ij} is a household-specific, time-invariant taste intercept and ε_{ijt} is an unobservable, i.i.d. taste shock distributed type I extreme value independently of the other terms. In appendix C, we present estimates from a model in which v_{ijt} includes an aggregate preference shock.

We assume that λ_{it} is linear in non-gasoline expenditures:

$$\lambda_{it} = \mu_i - \eta(m_{it} - q_{it}p_{0t}). \tag{4}$$

Here, μ_i is a household-specific marginal utility term and η measures the extent of diminishing marginal utility in non-gasoline expenditures. The common assumption that utility is quasilinear in money corresponds to $\eta = 0$.

We estimate the model via maximum likelihood under alternative assumptions about α_{ij} and μ_i. To test the hypothesis that households treat money as fungible, we estimate an unrestricted model:

$$\lambda_{it} = \mu_i - \eta^M m_{it} + \eta^G q_{it}p_{0t}. \tag{5}$$

We then test the restriction that $\eta^M = \eta^G = \eta$.

4.2 Discussion

Our model follows Houde (forthcoming) in taking gasoline quantities q_{it} as exogenous. We view this as a reasonable approximation at high frequencies given the relative insensitivity of gasoline quantities to gasoline prices in the short run. We note, however, that our specification is consistent with some common discrete-continuous models of demand. For example, the "cross-product repackaging" model of Willig (1978) and Hanemann (1984) corresponds to a special case of our model when $\eta = 0$.

There are two ways in which relaxing this assumption could affect our conclusions. The first is compositional change: if higher gasoline prices induce households who prefer premium gasoline to drive disproportionately less than those who like regular gasoline, then aggregate data could show evidence of substitution across octane levels even if there is none. In our descriptive analysis, we show directly that compositional change of this kind is extremely small, and in formal estimation we show that our findings are robust to allowing for unobserved heterogeneity in tastes α_{ij}.

The second way in which relaxing the assumption of exogenous quantities could affect our conclusions is if higher gasoline consumption is complementary to higher octane levels. In our descriptive analysis, we discuss and rule out several explanations for such a relationship. In appendix C, we show that controlling for gallons purchased tends, if anything, to strengthen our conclusions, because households tend to buy more regular gasoline when they purchase more gasoline overall.

Our model also follows the convention in the discrete-choice literature of considering a unitary household. It is well-known that violations of fungibility can arise from strategic behavior within the household (Lundberg and Pollak 1993). Although it is not clear how such forces would result in a violation of fungibility in our context, in appendix C we show that our estimates are similar when we restrict the sample to households with only one adult member, where strategic considerations are unlikely to be at work.

4.3 Implementation

We construct empirical measures of m_{it} and $q_{it}p_{0t}$. The variable m_{it} should measure the household's total expenditure on all goods. We construct two measures of m_{it}. Our main measure, m_i, does not vary over time. To construct it, we estimate a regression of total annual expenditure on total annual family income using the 2006-2009 Consumer Expenditure Survey interview files. We apply the coefficients from this model to the retailer-supplied household income measure to compute a measure of predicted total expenditure. In appendix C, we present results from a specification in which we predict total expenditure from Census block income.

We also construct a time-varying measure, m_{it}. To construct it, we estimate a regression of total annual expenditure on total monthly expenditure on food at home using the 2006-2009 Consumer Expenditure Survey interview files. We apply the coefficients from this model to the total expenditure on grocery items by the household in the four weeks prior to the transaction to compute a measure of predicted total expenditure.

In all specifications, we report standard errors adjusted for the fact that m_{it} is estimated in a first-step model following Murphy and Topel (1987). The adjustment makes little difference to the standard errors we report.

We show in section 3.4 above that the retailer-supplied measure of household income that forms the basis of measure m_i is highly correlated with other income measures available at an aggregate level. Nevertheless, the income measure doubtless contains both transitory income variation and measurement error. By forming measure m_i from a regression of total expenditure on self-reported income, we use only the component of reported income that is predictive of total expenditure, thus minimizing bias due to measurement error. In appendix A, we formalize the intuition that our two-step procedure addresses measurement error concerns. We also discuss results from a specification in which we explicitly model measurement error in both income and total expenditures, as well as transitory shocks to income. In that specification, our results are, if anything, stronger than in our baseline model.

The variable $q_{it}p_{0t}$ should capture the extent to which higher gasoline prices reduce income available for other purchases and should be measured with the same periodicity as m_{it}. Because our data only include household gasoline purchases at a single retailer, using annual gasoline expenditures computed from our microdata panel would understate the true household budget share of gasoline, which in turn would make us more likely to reject the null hypothesis of fungibility. Instead we measure q_{it} as average annual US gasoline consumption during our sample period (from the EIA), divided by the number of US households in 2006. We measure p_{0t} as the weekly average national retail price of gasoline (from the EIA). The number of gallons of gasoline per household that we estimate (1183) is greater than average annual purchases in our panel for all but 4.7 percent of households. In appendix C, we show that our results are robust to measuring q_{it} from spending at the retailer and to allowing that the prices of energy goods other than gasoline are correlated with the price of gasoline.

4.4 Identification

To develop intuition for the identification of our model it is helpful to consider our utility specification:

$$u_{ijt} = \alpha_{ij} - \left(\mu_i - \eta^M m_{it} + \eta^G q_{it} p_{0t}\right)\left(p_{jt} - p_{0t}\right) + \varepsilon_{ijt} \tag{6}$$

Consider a special case of this random utility model in which there is no heterogeneity in tastes or gasoline consumption and there are only two grades–regular and premium–with the prices between grades staying constant over time at some level, which we normalize to unity. In this special case we can drop subscripts j and write our model as a binary logit with utility

$$u_{it} = (\alpha - \mu) + \eta^M m_{it} - \eta^G q p_{0t} + \varepsilon_{it}. \tag{7}$$

In this special case, the null that $\eta^M = \eta^G$ corresponds tightly to the notion of fungibility that we discuss in the introduction. A parallel increase of \$1 in the price of all gasoline grades should decrease the propensity to purchase premium gasoline (or, equivalently, increase the propensity to purchase regular gasoline) by the same amount as a decrease of \$$q$ in total expenditure m_{it}. Put differently, income effects should be the same whether they come from "gas money" or other money.

We identify η^M from variation in income across households in our sample and from variation in income over time, as proxied by grocery expenditures.[2] We show in appendix C that our results are robust to identifying η^M from variation in income across Census block groups. We also show in appendix C that our results survive allowing the parametrization of marginal utility to differ across households of different income levels (Petrin 2002).

We identify η^G from variation in the national price of gasoline. Variation in national gasoline prices is driven by global supply and demand shocks that are plausibly unrelated to tastes for octane levels. To the extent that shocks to income drive demand for gasoline, this confound will tend to lessen our estimate of η^G and hence to bias our test in a conservative direction. We show in appendix C that our results are similar if we identify η^G from the portion of gasoline price variation that is attributable to fluctuations in the spot price of crude oil.

5 Descriptive Evidence

5.1 Gasoline Prices and Grade Choice

Figure 1 plots, separately by decade, the regular-grade share of total US gasoline sales as well as the (real) US average price for regular unleaded gasoline, from the EIA data. Figure 2 plots the regular-grade share and average price by week for transactions in our retailer panel. Both figures show a clear pattern: the share of regular gasoline tends to increase (at the expense of premium and midgrade) when the price of gasoline rises, and to fall when the price of gasoline falls. We show in the online appendix that the effect persists for several months after an initial increase in the price of gasoline, with no sign of a decay in the longer term.

Qualitatively, income effects would appear to be able to explain the correlation between gasoline prices and octane choice. All else equal, higher gasoline prices reduce household wealth and should therefore induce substitution to lower-quality goods. However, two facts strongly suggest that income effects cannot

[2] As equation (6) shows, in practice η^M and η^G are also identified by the relationship between income and the sensitivity of purchase probabilities to variation in $p_{jt} - p_{0t}$. During our sample period the retailer engaged in significant experimentation with grade price gaps, providing a credible source of identification of the effect of the price gaps $p_{jt} - p_{0t}$ on purchase behavior. We show in appendix C that our results survive on a subsample in which the price gaps do not vary at all.

alone provide a good explanation of the observed correlation between gasoline prices and octane choice.

First, the positive relationship between gasoline prices and the propensity to buy regular gasoline persists even in a period when income effects predict the opposite. The decline in world oil prices during the second half of 2008 coincided with, and is typically attributed to, a massive decline in realized (and expected future) demand for oil due to the worsening of the 2008 financial crisis (Taylor 2009). During this period, households generally acted poorer: automobile and retail sales plunged (Linebaugh and Dolan 2008, Zimmerman, Saranow and Bustillo 2008), and the growth in spending on luxury items such as organic products halted dramatically (NielsenWire 2009). One would therefore expect households to have substituted toward regular gasoline, yet they did the opposite, increasing their propensity to buy premium or midgrade gasoline by almost 4 percentage points. The evidence from the second half of 2008 is therefore difficult to reconcile with a model in which the correlation between gasoline prices and octane choice is driven by income effects.

Second, the income effects required to explain the relationship between gasoline prices and octane choice are extremely large. During the price spike from January to June of 2008, gasoline prices increased from \$2.98 to \$4.10 per gallon. During that same period, the share of transactions going to regular gasoline increased by 1.4 percentage points, from 80.2 percent to 81.6 percent. With annual consumption of 1183 gallons per household, the 2008 spike generated a \$1313 loss in income for a typical household. Figure 3 shows the cross-sectional relationship between household income and the propensity to buy regular gasoline. An OLS regression line fit to the plot implies that an income loss of \$1313 would result in an increase of 0.02 percentage points in the share of regular gasoline: two orders of magnitude below the observed change. To explain a 1.4 percentage point increase in the share of regular gasoline, the gasoline price spike in 2008 would have had to decrease household incomes by almost $\$100,000$.

When gasoline prices increase, households' choice of octane level shifts dramatically. Households act as if they have become much poorer, when in fact they have only become slightly poorer. Before turning to formal estimation, we pause to consider some alternative explanations for our findings.

5.2 Alternative Explanations

5.2.1 Changes in the Composition of Households Buying Gasoline

In the above discussion, we interpret the effect of gasoline prices on the share of regular gasoline as evidence that gasoline price changes induce households to substitute across grades. In principle, such changes could arise in the aggregate even absent cross-grade substitution, if higher gasoline prices induce larger reductions in the demand for gasoline among households that typically buy premium and midgrade than among households that typically buy regular.

In fact, compositional change does not explain our findings. One way to see this is to estimate the relationship between gasoline prices and the propensity to buy regular gasoline on a household-by-household basis in our retailer panel. We find that a positive relationship between gasoline prices and the propensity to buy regular is present for the majority of households. Of the households in our panel, 26.3 percent always buy regular, and 1.1 percent always buy either midgrade or premium gasoline. Among the remaining households who sometimes buy regular gasoline and sometimes buy premium gasoline, the empirical correlation between buying regular and the price of gasoline is positive for 59.4 percent.

Another way to see this is to decompose the changes in grade shares over time into a component that is due to compositional change and a component that is not. Such an exercise is presented in figure 4. The blue (solid) line shows the time series of the market share of regular gasoline from figure 2. The red (long-dashed) line plots the predicted share of regular at the retailer if we assume that, at each purchase occasion, each household's probability of buying regular gasoline is equal to its mean probability over the entire sample period. The series in the red line thus reflects changes over time in the types of households are buying gasoline at the retailer. The green (short-dashed) line simply plots the difference between the blue and red lines, normalized to have the same mean as the blue line for comparability. The green line thus reflects only changes due to within-household substitution over time. The figure shows that compositional change explains almost none of the variation in the share of regular gasoline over time.

Finally, calculations based on aggregate facts suggest that compositional change is likely to be too small to explain the variation in the share of regular gasoline that we observe over time. For example, figure 1 shows that the 1990 oil price spike raised gasoline prices by about 34 percent. Given a short-run elasticity of demand for gasoline of about 0.05 (Hughes, Knittel and Sperling 2008; Smith 2009), we would expect a decline in gasoline purchased of less than 2 percent. Even in the extreme scenario in which the entire decline in demand came from buyers of premium and midgrade gasoline, a 2 percent decline in gasoline demand would change the regular share by less than 1 percentage point, as against an observed change of 7 percentage points.

As a further check on the sensitivity of our findings to compositional change, in our formal analysis below we show that our findings are unchanged if we allow explicitly for cross-household preference heterogeneity.

5.2.2 Changes in the Price Gaps Among Gasoline Grades

The thought experiment in the introduction assumes that the price gap between high and low quality grades remains constant. In practice the assumption of constant price gaps between regular, midgrade, and premium gasoline is a good approximation but does not hold exactly. Our formal model explicitly allows for variation in price gaps, and in appendix C we show that our findings are unchanged if we estimate on the subsample of transactions in which the price gaps between grades are exactly 10 cents each.

In the online appendix, we show in aggregate data that an increase in the price of regular gasoline induces a small and temporary decline in the price gap between premium and regular gasoline. This direction of change works against our finding of a shift in quantities toward regular gasoline, and is consistent with a shift in demand towards regular gasoline coupled with a supply of octane levels that is imperfectly elastic in the very short run but highly elastic in the long run. Such a supply structure is, in turn, consistent with the relative ease of shifting the mix of refinery output from premium to regular gasoline. (Note that the observed patterns are not consistent with an explanation for our findings driven entirely by shocks to the relative supply of octane levels, because the relative price and relative demand for regular gasoline both move in the same direction.)

5.2.3 Vehicle Substitution and Vehicle Maintenance

Another potential confound is within-household change in vehicle usage. When gasoline prices rise, households may substitute toward driving more fuel-efficient vehicles. If more fuel-efficient vehicles are also those that recommend a lower-octane fuel, changes in which vehicles are being fueled could explain a portion of the time-series variation in octane choice.

Vehicle substitution is unlikely to explain our findings for two reasons. First, the empirical correlation between fuel economy and octane recommendations is weak. Across vehicles in model years 2003-2008, the correlation between fuel economy (combined miles per gallon) and an indicator for recommending or requiring premium gasoline is -0.11 (Environmental Protection Agency 2011).

Second, the extent of vehicle substitution is too small to explain the patterns we observe at high (weekly) frequencies. There are three main channels through which vehicle substitution might occur. The first is a relative increase in the market share of fuel-efficient vehicles among new purchases. In an average week in 2006, new car sales represented about one-tenth of one percent of the automobile stock (United States Census 2009). Applying Busse, Knittel and Zettelmeyer's (2010) estimate of the change in market share by quartile of fuel economy to the estimated fraction of vehicles in each quartile that recommend regular gasoline, we estimate that a $1 increase in the price of gasoline increases the share of the vehicle stock recommending regular gasoline by less than one-hundredth of one percentage point over one week. This predicted change is several orders of magnitude below the effects we estimate.

The second channel is disproportionate scrappage of less-fuel-efficient vehicles. As with new car purchases, the share of the vehicle stock scrapped in any given week is too small to allow for a significant change in the stock of vehicles on the road. In addition, Knittel and Sandler (2011) find that vehicle age is a more important determinant of scrappage rates than fuel economy per se.

The third channel is changes in the intensity of driving of different types of vehicles. Knittel and Sandler (2011) find that, at annual horizons, less fuel efficient cars are driven less than fuel efficient cars when gasoline prices rise. Adjusting their estimates to apply to short-run changes by matching to the short-run elasticity estimates in Hughes, Knittel and Sperling (2008), we estimate that a $1 increase in the price of gasoline increases the (mileage-weighted) share of vehicles recommending regular gasoline by two-hundredths of one percentage point. This predicted change is two orders of magnitude below the effects we estimate.

Even holding constant the set of vehicles on the road, an increase in gas prices may induce owners of less fuel efficient vehicles to devote less effort to vehicle maintenance. To the extent that high-octane gasolines are perceived (perhaps incorrectly) as an investment in vehicle maintenance, this force could potentially explain some substitution from high- to low-octane gasolines. A prediction of this explanation is that the effects we estimate will be more pronounced for more fuel-efficient vehicles. Although we do not observe fuel efficiency, we can proxy for it with gas tank size, estimated using the household's maximum fill amount during the sample period.[3] In appendix C, we show that, if anything, the effect of gasoline prices on gasoline grade choice is larger for households with smaller tank sizes (and hence more fuel-efficient vehicles), although effects are similar between households with large and small gas tanks.

Another possibility is that drivers adjust how they drive when gas prices are high, perhaps driving slower or in a less "sporty" manner. If drivers perceive higher octane levels as complementary to sporty driving, they might substitute to regular gasoline when gasoline prices are high. In the online appendix we present evidence from vehicle accident data on the relationship between driving speeds and the price of gasoline. We find no evidence of a relationship between the two.

Finally, we note that if households (incorrectly) perceive premium gasoline to be more fuel-efficient, this force will work against the direction of findings that we observe.

[3]Using data from Reuters (2007) and www.fueleconomy.gov, we estimate that for the top 20 selling vehicle models in January-July 2007, the correlation between tank size (in gallons) and combined fuel efficiency (in miles per gallon) is -0.76. Calculations kindly performed for us by staff at fueleconomy.gov show that among all vehicles in 2010 the correlation between tank size and combined fuel efficiency is -0.73.

6 Model Estimates

6.1 Main Results

Table 3 presents our main results.

For each specification we present estimates of the effect on marginal utility of a \$1000 decrease in gasoline expenditures or a \$1000 increase in total expenditures (parameters η^G and η^M, respectively). We also present the average marginal effect on regular share of three experiments: increasing the price of regular gasoline by \$1, decreasing gasoline expenditures by \$1000, or increasing total expenditures by \$1000. As a test for fungibility we present the p-value from a Wald test of the hypothesis that $\eta^M = \eta^G$.

In column (1), we present our baseline specification. In this model we use our cross-sectional measure of household expenditures m_i and we assume that there is no heterogeneity in taste parameters α_{ij} and μ_i. This model is a conditional logit model (McFadden 1973).

In our baseline specification in column (1) we find that a \$1 increase in the price of regular gasoline increases the regular share by 1.4 percentage points, which, in turn, implies that a \$1000 decrease in household gasoline expenditures decreases the regular share by 1.2 percentage points. By contrast, a \$1000 increase in total household expenditures decreases the regular share by 0.08 percentage points. The Wald test rejects the equality of the effects of gasoline and total expenditures with a high level of confidence.

In column (2), we use our time-varying measure of household expenditure m_{it}. We allow that μ_i is a linear function of m_i to eliminate all cross-sectional identification of η^M. If anything, using time variation to identify η^M tends to weaken the estimated income effect, strengthening our rejection of fungibility.

In column (4) we present a specification in which we allow for unobservable variation in α_{ij}. We assume that α_{ij} are normally distributed independently across households and choices, and independently of m_i. For computational reasons we estimate the model on a subsample consisting of every 10th transaction for each household. In column (3) we re-estimate the model from column (1) on the subsample to illustrate its comparability to the full sample, and in the online appendix we present results from a specification with heterogeneity in α_{ij} estimated on the full sample. Comparing columns (3) and (4), we find that allowing for household-specific unobservable tastes tends, if anything, to strengthen the estimated effect of the gasoline price level on the propensity to buy regular-grade gasoline. We continue to confidently reject the null hypothesis of fungibility.

In appendix C, we show that the estimates in table 3 are robust to identifying the model using variation in world crude oil prices, splitting the sample into high- and low-income households, allowing for a correlation between gasoline prices and other energy prices, using several alternative estimates of household gasoline and total expenditures, and allowing for aggregate preference shocks. In the online appendix, we present estimates from a model in which we allow for unobserved heterogeneity in μ_i and a model in which we allow for heterogeneity in both α_{ij} and μ_i without imposing distributional assumptions. Across these specifications we consistently reject the null hypothesis of fungibility.

6.2 Interpretation of Magnitudes

The violation of fungibility that we estimate is economically significant. Our baseline estimates imply that households respond almost 20 times more to a reduction in income due to an increase in gasoline prices than to equivalent variation in income from other sources. At our point estimates, a $1 increase in the price of gasoline would have to reduce a typical household's non-gasoline expenditures by more than $20,000 per year to reconcile the observed increase in the propensity to purchase regular gasoline.

Figure 5 illustrates the violation of fungibility in a different way. The figure shows weekly averages for three series. The first is the observed share of transactions going to regular gasoline. The second is the predicted share of transactions going to regular gasoline from our baseline model. The third series is the predicted share of transactions going to regular gasoline from a model estimated with the constraint that $\eta^G = \eta^M$, equivalent to imposing fungibility. The first two figures track each other closely: our model fits the large swings in the market share of regular gasoline fairly well. But the third figure, which imposes fungibility, fits very poorly, predicting almost no variation over time in the market share of regular gasoline.

We can also evaluate the magnitude of the violation of fungibility by asking how often households would choose differently if they were forced to obey fungibility. To perform this calculation, for each transaction in our dataset we randomly draw utility shocks ε_{ijt} from their assumed distribution. We then compute the utility-maximizing choice of octane level according to both our baseline model and an alternative model in which we impose $\eta^G = \eta^M$ and adjust μ_i so that each household's mean marginal utility of income is the same as in the baseline model. We compute statistics of interest averaged over five such simulations.

We estimate that 60.4 percent of households make at least one octane choice during the sample period that they would have made differently if forced to obey fungibility. Forcing households to treat gas money as fungible with other money would change octane choices in 0.6 percent of transactions overall.

6.3 Placebo Tests

We interpret our findings as evidence that, when purchasing gasoline, consumers are especially sensitive to the size of their gas budget, and therefore treat changes in gasoline as equivalent to very large changes in income when deciding which grade of gasoline to purchase. A prediction of this interpretation is that the effect of gas prices on non-gasoline purchases should be commensurate with income effects. That is, we would expect that gasoline and other income would be fungible in decisions about non-gasoline purchases.

Table 4 presents an estimate of our model applied to sample households' choice of orange juice and milk rather than gasoline grade. Here consumers choose between brand-size combinations in each category instead of grades of gasoline. We allow the marginal utility of money to vary separately with gasoline prices and income, just as we did in our baseline model estimated on gasoline purchases.

We find that higher incomes result in a shift in demand away from the private label and towards higher-quality brands. We find that higher gasoline prices tend, if anything, to induce shifting towards higher-quality brands, although the effect is not statistically significant. The counterintuitive sign may result from the fact that some gasoline price shocks are themselves due to income variation (such as the recession), which is a source of conservative bias in our main tests.

In contrast to our findings for gasoline grade choice, we cannot reject the equality of gasoline and total expenditure effects in these cases. That is, consistent with Gicheva, Hastings and Villas-Boas (2007), we find that gasoline and other income are fungible in decisions about grocery purchases. In the online appendix, we show that our findings are similar even when we restrict attention to orange juice or milk purchases that occur on the same day as a gasoline purchase, when the salience of gasoline prices is presumably at its greatest. The online appendix also presents a visual representation of our findings, showing that when gasoline prices rise, consumers act much poorer when buying gasoline but not when buying orange juice or milk.

The lack of evidence of a violation of fungibility in these placebo categories does not result from a lack of power. Table 4 presents p-values from a test that the ratio η^G/η^M for brand choice in the placebo category is equal to the analogous ratio for gasoline grade choice (using the baseline parameters for gasoline estimated in table 3). For both orange juice and milk we confidently reject the hypothesis that the ratio for the placebo category is equal to that for gasoline. In this sense, we can statistically reject the hypothesis that fungibility is violated as much in placebo categories as in gasoline grade choice.

Note, however, that power would be an issue if we were to attempt to test whether an increase in, say, the price of milk (as opposed to gasoline) causes substitution to lower-quality milk varieties. Milk and orange juice prices do not exhibit the large swings that gasoline prices do, and the prices of different brand-size combinations do not move in close parallel. Milk and orange juice purchases therefore do not afford a good laboratory for testing the effect of own-category price variation on quality substitution, although they do serve as a valid test of the specification of our gasoline models.

7 Psychological Mechanisms

In this section we consider a set of models that capture different psychological intuitions for the violation of fungibility that we observe. We estimate each model and evaluate its performance in predicting the empirical time series of octane choice.

7.1 Model Specification and Estimation

7.1.1 Loss Aversion

We estimate a model of loss aversion based on Köszegi and Rabin (2006). In the model, households obtain direct consumption utility as well as "gain-loss" utility when consumption departs from a reference level. We assume that gain-loss utility exhibits loss aversion but not diminishing sensitivity, and we show in the online appendix that allowing for diminishing sensitivity slightly improves model fit. We depart from Köszegi and Rabin (2006) in treating the reference consumption level as a degenerate distribution with value equal to the expected consumption level.

We assume that there are two consumption dimensions: gasoline consumption and non-gasoline consumption. Non-gasoline consumption delivers utility $\mu(m_{it} - p_{jt}q_{it})$. Gasoline consumption delivers utility $\theta g_j q_{it}$ where g_j is the octane level of grade j. We assume that purchase prices $\{p_{jt}\}$ are unknown prior to the time of purchase and that all other payoff-relevant state variables are known in advance. We write household i's per-gallon utility from purchasing grade j at time t as

$$u_{ijt} = \alpha_j - \mu p_{jt} + \gamma\theta(g_j - \tilde{g}_{it})1_{g_j < \tilde{g}_{it}} - \gamma\mu(p_{jt} - \tilde{p}_{it})1_{p_j > \tilde{p}_{it}} + \varepsilon_{ijt} \tag{8}$$

where γ is a multiplier that corresponds to the extent and importance of loss aversion, $\tilde{g}_{it}$ is the reference octane level, and $\tilde{p}_{it}$ is the reference price.[4] We include a utility intercept α_j and shock ε_{ijt} to ensure that the model has sufficient flexibility to fit the empirical mean and variability of grade shares.

To operationalize the model, we assume that households form expectations of future grade choice and transaction price based on their forecasts of future gasoline prices, which in turn are based on current price levels (Anderson, Kellogg and Sallee 2011). We estimate $\tilde{g}_{it}$ and $\tilde{p}_{it}$ as the predicted values from regressions of realized octane level and transaction price, respectively, on a cubic polynomial in the national regular price as of either one or four weeks prior to purchase. We use national prices rather than purchase prices to avoid conflating loss aversion with household heterogeneity (Bell and Lattin 2000). We use one- and four-

[4] Equation (8) suppresses the gain portions of the gain-loss utility functions and the consumption utility from octane, both of which are mechanically unidentified.

week horizons for expectation formation to illustrate the range of plausible values. Because households in our sample buy gasoline 4.6 times in an average purchase month, it is unlikely that households' expectations are based on prices more than four weeks old. We set $g_0 = 87$, $g_1 = 89$, $g_2 = 91$, reflecting typical octane levels of regular, midgrade, and premium, respectively.

7.1.2 Price Salience

We estimate a model of salience based on Bordalo, Gennaioli, and Shleifer (2012). In the model, households place greater weight on product attributes which are salient at the moment, where salience is determined by the degree to which an attribute varies within an "evoked set" of options.

We assume that each grade of gasoline has two attributes: octane level g_j and price p_{jt}. Let $\bar{g}_{it}$ and $\bar{p}_{it}$ be the mean octane level and price in household i's evoked set at time t. Let $\sigma(x_{jt}, \bar{x}_{it}) = \frac{|x_{jt} - \bar{x}_{it}|}{|x_{jt}| + |\bar{x}_{it}|}$ denote the salience function defined in Bordalo, Gennaioli, and Shleifer (2012), and let $z_{ijt} = 1_{\sigma(g_j, \bar{g}_{it}) < \sigma(p_{jt}, \bar{p}_{it})}$ be an indicator for whether price is more salient than octane level in the evaluation of good j by household i at time t. We write household i's per gallon utility from purchasing grade j at time t as

$$u_{ijt} = \alpha_j - \mu p_{jt} + \theta g_j (1 - z_{ijt}) - \gamma p_{jt} z_{ijt} + \varepsilon_{ijt} \tag{9}$$

where θ and γ are functions of the decision weights on the two attributes and of the extent to which the decision-maker over-weights the salient attribute.[5] We include a utility intercept α_j and shock ε_{ijt} to ensure that the model has sufficient flexibility to fit the empirical mean and variability of grade shares.

We operationalize the model in parallel with the loss-aversion model. We assume that the evoked set includes all grades at current prices, and all grades at national mean prices as of either one or four weeks prior to purchase. We set $g_0 = 87$, $g_1 = 89$, $g_2 = 91$.

7.2 Results

Figure 6 presents the models' predictions. Each panel presents results for a different model. For a given model, we compute the predicted probability of purchasing regular gasoline at each purchase occasion. We average these predictions across transactions to compute the predicted regular share. For each model we present results using both a one-week and a four-week horizon.

Panel A shows results for loss aversion. When prices rise, more households find that they are in danger of spending more than expected on gasoline. To partially alleviate that loss, households switch to regular

[5] Equation (9) suppresses the baseline consumption utility from octane, which is mechanically unidentified.

grade, as in the observed data. Once prices have increased enough that essentially all households are in the losses region on all grades of gasoline, the model predicts little further increase in the regular share as prices continue to rise, a prediction that is counter to the observed data. The model also counterfactually predicts that if prices remain high but steady for an extended period, households' expectations adapt, leading the regular share to fall. Because this latter prediction is sensitive to the length of the forecasting horizon, the model fit improves when we switch from a one-week to a four-week horizon.

Panel B shows results for price salience. When prices rise, the gap between present and past prices increases, resulting in more attention to price, less attention to octane, and hence more purchases of regular gasoline. As with the loss-aversion model, the salience model exhibits a counterfactual "leveling off" of the regular share when prices rise for a long period, as well as adaptation to periods of sustained price increases. The model also predicts that rapidly falling prices–which makes prices more salient than octane–can induce a brief shift to regular gasoline. Unlike the loss-aversion model, the salience model's fit is better with a shorter horizon. The reason is that the salience of prices depends on whether the percentage variation in today's prices relative to past prices is greater than the percentage variation in octane levels across grades. The percentage variation in octane levels is small, and at a four-week horizon the volatility in prices is almost always larger, so price is almost always more salient than octane. With a shorter horizon, the relative salience of price and octane vary more often, leading to richer dynamics.

7.3 Discussion

Both of the models we consider show some degree of consistency with our primary evidence.

In the online appendix, we present further results from an ad-hoc model meant to capture the psychology of category budgeting. The model fits the data well, but it is not comparable to the two specifications we discuss above, in that it does not draw on an existing body of theory.

We omit some models whose predictions do not accord with our findings. Most notably, models with "relative thinking" (Azar 2007 and 2011) predict that, when all prices increase, price differences become less salient (because they are smaller in relative magnitude), leading to quality upgrading. We find the opposite. Saini, Rao and Monga (2010) offer a possible reconciliation of relative thinking evidence with our findings. They employ a hypothetical choice methodology in which the participant must choose whether to drive for five minutes to obtain a $10 discount on an item. As in Tversky and Kahneman (1981), they find that the willingness to drive to get a discount is lower for a more expensive item. However, they show that when the participant is surprised by a higher-than-expected price the willingness to drive for a discount goes up. Their interpretation is that expected variation in prices evokes relative thinking (i.e., diminishing sensitivity) but unexpected variation in prices evokes "referent thinking" (i.e., loss aversion). Because households cannot predict the path of future gasoline prices (Anderson, Kellogg and Sallee 2011), it is reasonable to assume that referent thinking would dominate relative thinking in our context. Indeed, Saini, Rao and Monga (2010) employ a gasoline-related vignette in their study, and show that higher-than-expected gasoline prices can induce consumers to drive further to seek out discounts.

Although we focus on the predictions of the models we consider for gasoline purchases, we can also consider their ability to match the placebo tests we present in section 6 above. It is transparent that the price salience model predicts no effect of gasoline prices on non-gasoline purchases beyond those implied by income effects. The same is true of the loss aversion model, provided that a household's sensation of loss from an increase in gasoline prices ends when the gasoline transaction ends.

8 Implications for Retail Markets

Existing evidence suggests that the retail markup on gasoline tends to fall when the oil price rises (Peltzman 2000, Chesnes 2010, Lewis 2011). To illustrate, Panel A of figure 7 reproduces figure 1 of Lewis (2011), which shows the pre-tax retail price and wholesale (spot) price of regular reformulated gasoline in Los Angeles in 2003 and 2004 as measured by the EIA. When the spot price rises, the markup–the gap between the wholesale and retail prices–compresses.

Lewis (2011) provides a search-based account of this effect. When prices rise, consumers cannot tell how much of the increase is retailer-specific, so they increase the intensity with which they search for better prices at other retailers, thus putting downward pressure on retailer margins.

Our findings offer a complementary explanation. We show above that when prices rise, consumers act as if they have a high marginal utility of money in the gasoline domain. If this force operates when consumers decide which retailer to purchase from, it will result in greater price sensitivity and hence lower retail markups.

To illustrate, consider the following toy model of retail pricing. The market consists of a large number of identical retailers selling regular grade gasoline to a unit mass of households. (Formally, we consider the limit case as the number of retailers grows large.) Each household's utility is quasilinear in money with marginal utility ρ_t and is subject to an additive type-I extreme value error i.i.d. across households and retailers. Retailers set prices simultaneously, taking the marginal utility ρ_t as given, and face a common and exogenous wholesale price c_t. Then in the unique equilibrium (Anderson, de Palma and Thisse 1992) all retailers charge the same price p_{0t}^*:

$$p_{0t}^* = c_t + \frac{1}{\rho_t}. \tag{10}$$

Given this model of equilibrium pricing, we can ask how much of the tendency of markups to fall when wholesale prices rise can be explained by the variation in marginal utility that we estimate in our model of grade choice. We assume that

$$\rho_t = \bar{\rho}\left(\mu - \eta^M m + \eta^G q p_{0t}^*\right) \tag{11}$$

where m is average annual household expenditure, q is average annual gallons of gasoline per household, and $\left\{\bar{\rho}, \mu, \eta^G, \eta^M\right\}$ are preference parameters. Equations (10) and (11) uniquely define p_{0t}^* as a function of parameters. We estimate $\bar{\rho}$ via nonlinear least squares using the EIA data shown in figure 7, matching the predicted markup to the observed series. We take the other preference parameters from our baseline model of grade choice.

Panel B of figure 7 shows the observed markup, the markup predicted using preference parameters from our baseline model, and the markup predicted from a model of grade choice in which we constrain $\eta^G = \eta^M$. The markup tends to increase when the spot price decreases. Using the preference parameters from our baseline model, our model of retail pricing explains some, though not all, of this pattern. By contrast, using preference parameters that impose fungibility $\left(\eta^G = \eta^M\right)$ the retail pricing model predicts essentially no variation in the markup.

9 Conclusions

A significant body of experimental and laboratory evidence shows that households maintain separate mental budgets for different categories. In contrast to standard utility models, mental budgeting predicts excess sensitivity to small income shocks induced by category-level price changes.

We test for this form of excess sensitivity in rich panel data on household gasoline purchases. Households substitute from higher to lower octane levels when gasoline prices rise. The substitution we observe cannot be explained by income effects, compositional changes, or changes in the price differences between grades.

We formulate and estimate a discrete-choice model of the demand for octane level. Using the model, we show that we can confidently reject the null hypothesis that consumers respond equally to changes in gasoline prices and to equivalent changes in income from other sources. The model provides a better fit to the observed data when the fungibility constraint is relaxed, and formal statistical tests reject the null hypothesis that consumers treat gas money and other money as fungible.

Placebo tests using choices of non-gasoline products show that gasoline prices do not exert a disproportionate effect on purchases in non-gasoline domains. This finding is consistent with the mental budgeting story, which predicts that within-category price changes exert a disproportionate influence on within-category purchase decisions.

Using our data, we formally estimate and evaluate a model with loss aversion based on Köszegi and Rabin (2006) and a model of salience based on Bordalo, Gennaioli, and Shleifer (2012). In this sense our findings inform the growing theoretical literature in psychology and economics by illustrating the quantitative predictions of alternative models in a novel setting and dataset.

Finally, we show using a toy model that our estimates may help explain extant evidence on time-series variation in the retail markup for gasoline, suggesting a possible channel of supply-side response to the violation of fungibility that we document, and contributing to a growing literature on firms' responses to consumer psychology (DellaVigna and Malmendier 2004).

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Appendices

A Allowing for Measurement Error and Transitory Income Shocks

Let z_i be a household's true current income and let $\hat{z}_i$ be the household's self-reported income. Let m_i be a household's true permanent income, which we assume is equivalent to its total expenditure on goods and services. Let $\hat{m}_i$ be a household's reported total expenditures.

For households in our retailer panel we measure reported current income $\hat{z}_i$. For households in the Consumer Expenditure Survey we measure reported current income $\hat{z}_i$ and reported expenditure $\hat{m}_i$. Suppose that the measurement errors in both m_i and z_i are classical in the sense that these errors are normally distributed independently of one another and of m_i, z_i, and the other exogenous variables in our model. Suppose further that m_i and z_i are jointly normally distributed with some nonzero covariance.[6]

Then from a regression of $\hat{m}_i$ on $\hat{z}_i$ in the Consumer Expenditure Survey we obtain an estimate of the conditional expectation $\mathrm{E}(m_i|\hat{z}_i)$. The marginal utility function in equation (5) is

$$\lambda_{it} = \tilde{\mu}_i - \eta^M \mathrm{E}(m_i|\hat{z}_i) + \eta^G q_{it} p_{0t} \tag{12}$$

where $\tilde{\mu}_i = \mu_i - \eta^M (m_i - \mathrm{E}(m_i|\hat{z}_i))$ is a function both of underlying structural heterogeneity in marginal utility μ_i and of deviations between the true permanent income m_i and the econometrician's conditional expectation $\mathrm{E}(m_i|\hat{z}_i)$. By construction, the deviation $(m_i - \mathrm{E}(m_i|\hat{z}_i))$ is orthogonal to the expectation $\mathrm{E}(m_i|\hat{z}_i)$.

The model in equation (12) is formally equivalent to one in which the econometrician perfectly measures permanent income ($\mathrm{E}(m_i|\hat{z}_i) = m_i$) and the marginal utility parameter μ_i is normally distributed independently of $\hat{z}_i$. In the online appendix, we present estimates of such a model and show that, if anything, our results are stronger than in our baseline estimates.

B Product Aggregation in Grocery Categories

Transaction data in grocery categories include a universal product code (UPC) for each item purchased. Our data include 91 orange juice UPCs and 129 milk UPCs purchased during the sample period. We exclude small-format and single-serving UPCs and those that sell fewer than 100 units over the sample period.

We aggregate the UPCs into products for estimation purposes in two steps.

[6] Implicitly, we also assume that the extent of measurement error (and transitory shocks) in household income does not differ between our retailer panel and the Consumer Expenditure Survey. The results in section 3.4 provide important evidence that this assumption is a reasonable approximation.

First, we recode as identical any two UPCs that are substantially the same. In orange juice, this means recoding as identical any two UPCs that have the same brand, size, pulp content, and enrichment (e.g., calcium added). In milk, this means recoding as identical any two UPCs that have the same brand, size, and fat content.[7] UPCs that are recoded in this way typically do not differ on any attributes recorded by the retailer; in many cases differences will reflect technical changes in the retailer's database or small differences in packaging.

Second, we aggregate UPCs into products by size and brand. For UPCs in the same size, brand, and store, the Pearson correlation in the change in log prices is 0.89 for orange juice and 0.88 for milk. Following the composite commodity theorem (Deaton and Muellbauer 1980), the high correlation in prices within brand and size gives some support to the level of aggregation we choose in our analysis.

We construct a price series for each product as follows. We rescale prices to be in units of dollars per half gallon. For each product and each store, we construct the overall market share of each component UPC and use these market shares to construct a fixed-weight average price in each calendar week. We use the universe of store purchases (rather than only those purchases made by sample households) to construct the product price series.

In orange juice, there are 19 products (brand-size groups). The most popular is half-gallon Tropicana, which has a market share of 22 percent. Prices range from $2.30 per half gallon for one-gallon private label juice to $4.06 per half gallon for 89 oz format Simply Orange organic juice. (A few smaller formats have even higher prices when measured in price per half gallon.)

In milk, there are 9 products. The most popular is the one-gallon private label with a market share of 70 percent. The one-gallon private label is also the least expensive option, costing $1.48 per half gallon as against $3.66 per half gallon for the half-gallon format Horizon organic milk.

C Robustness Checks

Appendix table 1 presents a series of alternative specifications. In each case we present the marginal effect on the regular share of a $1 change in gasoline prices, a $1000 change in total expenditure, and a $1000 change in gasoline expenditure, as well as the p-value from a test of the null hypothesis that $\eta^G = \eta^M$. Row (1) reproduces the baseline specification from column (1) of table 3 for comparison.

In row (2) we restrict attention to households that have a single adult member according to demographic

[7] Note that in the case of milk, common brands for conventional milk vary regionally. For estimation purposes we treat all non-private-label conventional milk as a single brand. Because most of the variation in conventional milk brands is across stores rather than within stores, this aggregation should not greatly distort our picture of the consumer's choice set.

information supplied by the retailer.

In row (3) we allow grade preferences to depend on the amount of gasoline purchased. We include in v_{ijt} an interaction between grade and the difference between the household's gasoline purchases in the transaction month and the household's mean monthly purchases over the sample period.

In rows (4) and (5) we estimate the model separately for households with below- and above-median household income. Following Petrin (2002), this approach allows for greater flexibility in the parametrization of the marginal utility function λ_{it}.

In row (6) we construct m_i as the predicted value from a regression of our baseline measure of m_i on per capita income in the household's Census block group.

In row (7) we adjust estimated gasoline expenditures $q_{it}p_{0t}$ to account for the correlation of gasoline prices with other energy prices. We do this by rescaling our estimate of household gasoline expenditures so that its ratio to household energy expenditures over the period we study is equal to the coefficient in an OLS regression of the annual change in log energy prices on the annual change in log gasoline prices (using data from the NIPA).

In row (8) we estimate gasoline expenditures $q_{it}p_{0t}$ as the product of the household's average annual gasoline purchases (in gallons) at the retailer and the price of regular gasoline at the retail location at the time of purchase.

In row (9) we identify η^G from variation in the spot price of oil at Cushing, OK instead of the US average price of regular gasoline. We do this by running a first-stage regression of gasoline expenditures $q_{it}p_{0t}$ on the oil price and allowing both v_{ijt} and λ_{it} to contain a linear term in the residual from the first-stage regression.

In row (10) we restrict attention to purchases in which the price gap between midgrade and regular gasoline is 10 cents and the price gap between premium and regular gasoline is 20 cents, rounded to the nearest cent.

In row (11) we aggregate the data to the store-week level. We estimate our model letting all variables equal their store-week mean. We transform the market share of each grade of gasoline into the mean utility of that grade in a given store-week, relative to the share of regular gasoline (Berry 1994). We estimate via OLS, allowing for a store-week-grade utility shock that is mean zero conditional on the included variables.

In row (12) we allow that tastes v_{ijt} have an additive component distributed independently normal across choices and store-weeks. We estimate via maximum likelihood on a one percent sample of the data, approximating the likelihood via sparse grid integration (Heiss and Winschel 2008) with accuracy 4.

In rows (13) and (14) we estimate the model separately for households with below- and above-median gas tank size, where we proxy for gas tank size with the maximum amount of gasoline purchased by the household across all transactions in the sample period.

Table 1: Descriptive statistics for retailer data

	(1)	(2)	(3)
Sample	All Retailer Households	Estimation Sample	All Households in Same State
Household income measure provided by retailer (in 2008 $US)	$86,968	$97,173	
In household's Census block group:			
Average household income (in 2008 $US)	$95,421	$98,355	$81,252
Average commute time among workers	26.292	26.941	26.780
Fraction of workers commuting using public transportation	0.028	0.024	0.038
Number of gasoline trips per month (conditional on at least one trip)	1.664	4.601	
Average gallons per purchase occasion	11.740	12.323	
Average distance from block group centroid to most visited store (in miles)	20.579	4.100	
Fraction of gallons purchased which are regular grade	0.800	0.792	0.822
Average retail price paid per gallon	$2.836	$2.848	$2.892
Average annual grocery expenditure	$411	$2503	$4295
Number of households	1,306,748	61,494	—

Notes: In columns (1) and (2) the table shows the mean across households in each sample. In column (3) the table shows the mean across US states, where states are weighted by the proportion of households in the full sample who reside in each state. Census block group characteristics are missing for 5.5 percent of households in the full sample and 5.0 percent of households in the estimation sample. Distance to most visited store is treated as missing for households living in a different state from the most visited store (<8% of all households and <1% of households in estimation sample). The state equivalent measure for fraction of gallons purchased of regular grade and average price paid is based on 2006-2009 EIA data for the state of the household's most visited store. The state equivalent measure for average grocery expenditure is from the 2006-2009 Consumer Expenditure Survey Interview Files for the state of the household's most visited store.

Table 2: Income elasticity of grocery expenditures

Dependent variable: Log(Average monthly grocery expenditures)				
	(1)	(2)	(3)	(4)
Log(Household income)	0.1686	0.1682		
	(0.0014)	(0.0076)		
Log(Average adjusted gross income in zipcode-year)			0.1379	0.0893
			(0.0207)	(0.0528)
Unit of Analysis	Household	Household	Zipcode-year	Zipcode-year
Data source	CEX	Retailer	Retailer	Retailer
Year dummies?			X	X
Zipcode dummies?				X
Number of observations	122483	61333	3820	3820
R^2	0.1072	0.0079	0.0151	0.9725

Note: Data for specification (1) are from the Consumer Expenditure Survey 2006-2009 interview files and use family income before tax as the household income concept. Data for specifications (2) through (4) are from the retailer. All specifications exclude households/zipcode-years with zero expenditure on groceries during sample period. Regressions at the zipcode-year level are weighted by the number of sample households in the zipcode-year.

Table 3: Model of gasoline grade choice

Dependent variable: Choice of gasoline grade

	(1)	(2)	(3)	(4)
Effect on marginal utility of:				
\$1000 increase in gasoline expenditures	0.4306	0.4327	0.4132	0.7145
(Parameter η^G)	(0.0314)	(0.0305)	(0.0335)	(0.0317)
\$1000 decrease in total expenditures	0.0293	0.0127	0.0297	0.0416
(Parameter η^M)	(0.0008)	(0.0005)	(0.0010)	(0.0042)
Average marginal effect on regular share of:				
\$1 increase in price of regular gasoline	0.0142	0.0143	0.0136	0.0140
	(0.0010)	(0.0010)	(0.0011)	(0.0006)
\$1000 decrease in gasoline expenditures	-0.0120	-0.0121	-0.0115	-0.0118
	(0.0009)	(0.0009)	(0.0009)	(0.0005)
\$1000 increase in total expenditures	-0.0008	-0.0004	-0.0008	-0.0007
	(0.0000)	(0.0000)	(0.0000)	(0.0001)
p-value of Wald test for fungibility $\left(\eta^G = \eta^M\right)$	0.0000	0.0000	0.0000	0.0000
Sample	All	All	1/10th	1/10th
Time-varying expenditure measure?		X		
Household-level random coefficients?				X
Number of transactions	10548175	10548175	1082486	1082486
Number of households	61494	61494	61494	61494

Note: Data are from retailer. Table reports estimates of the model described in section 4. Standard errors in parentheses allow for correlation in residuals by month. Models are estimated via maximum likelihood. In specification (1) we measure total expenditures with our time-constant measure m_i. We assume that α_{ij} and μ_i are constant across households. In specification (2) we measure total expenditures with our time-varying measure m_{it} and allow that μ_i is a linear function of m_i. Specification (3) repeats specification (1) on a sample of every 10th transaction for each household. Specification (4) uses the sample in specification (3) and allows that α_{ij} are distributed independently normal across households and choices. To estimate the mixed logit model in specification (4), we approximate the likelihood using sparse grid integration with accuracy 9 (Heiss and Winschel 2008) and maximize the likelihood using KNITRO's active-set algorithm for unconstrained problems (Byrd et al. 2006). We validated our implementation of the mixed logit model by replicating benchmark Monte Carlo exercises reported in Heiss and Winschel (2008).

Table 4: Placebo model of non-gasoline choice

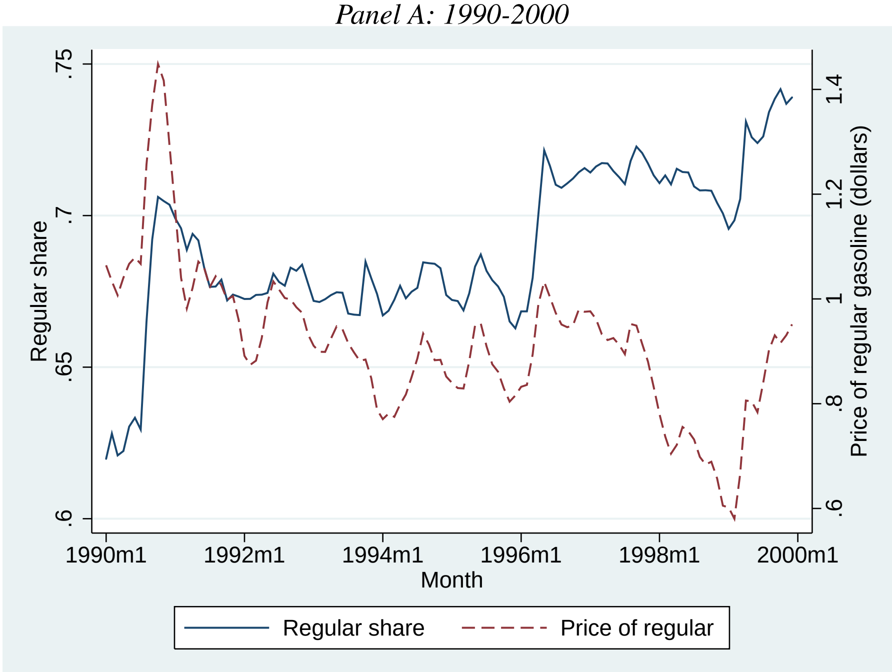
Dependent variable: Choice of brand

	(1)	(2)
Effect on marginal utility of:		
\$1000 increase in gasoline expenditures	-0.0141	-0.0128
(Parameter η^G)	(0.0250)	(0.0197)
\$1000 decrease in total expenditures	0.0044	0.0034
(Parameter η^M)	(0.0002)	(0.0001)
Average marginal effect on private label share of:		
\$1 increase in price of regular gasoline	-0.0169	-0.0055
	(0.0299)	(0.0084)
\$1000 decrease in gasoline expenditures	0.0143	0.0046
	(0.0252)	(0.0071)
\$1000 increase in total expenditures	-0.0045	-0.0012
	(0.0002)	(0.0000)
p-value of Wald test for fungibility $(\eta^G = \eta^M)$	0.4571	0.4115
p-value of test that η^G/η^M for category is equal to η^G/η^M for gasoline grade choice	0.0000	0.0000
Category	OJ	Milk
Number of transactions	411161	2210312
Number of households	13493	34128

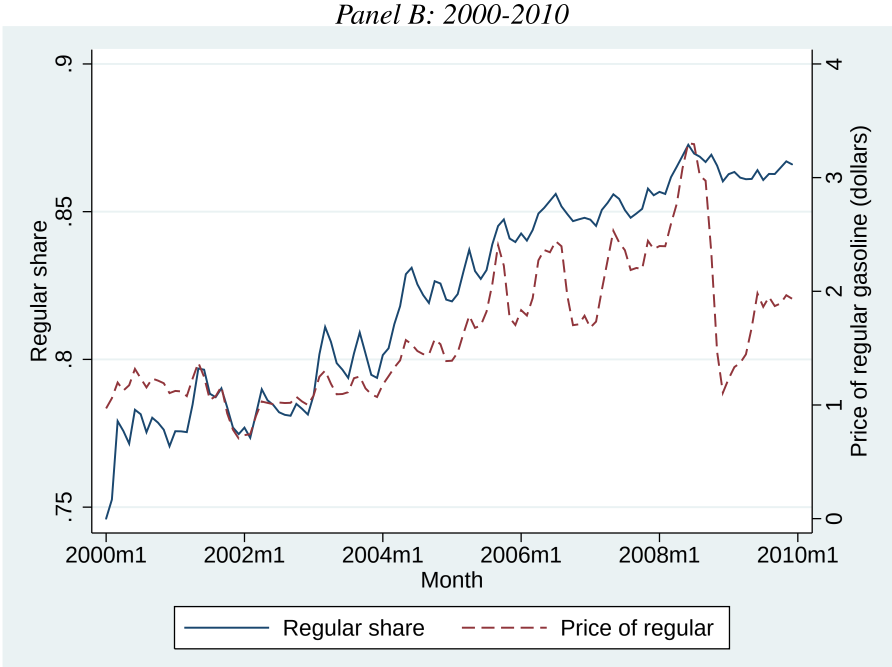
Note: Data are from retailer. Table reports estimates of the model described in section 4 but applied to choice of orange juice or milk brand rather than choice of gasoline grade. Standard errors in parentheses allow for correlation in residuals by month. We assume that α_{ij} and μ_i are constant across households. The test that η^G/η^M for the category is equal to is equal to η^G/η^M for gasoline grade choice is performed via a jackknife over months, accounting for the correlation in η^G/η^M across models. The test uses our baseline specification from column (1) of table 3.

Figure 1: Regular share and price of regular gasoline

Panel A: 1990-2000

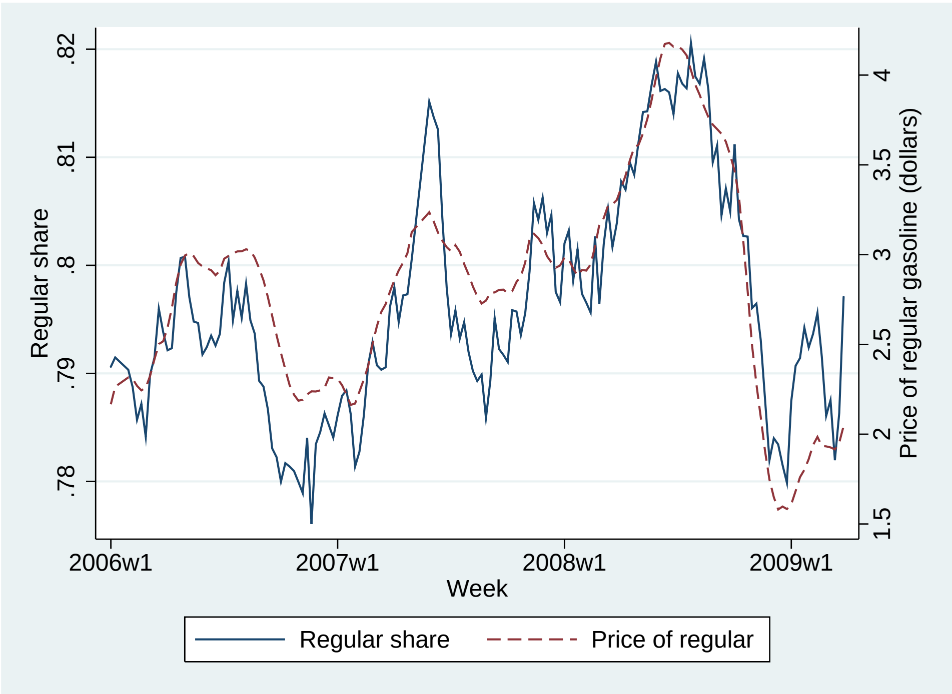


Panel B: 2000-2010



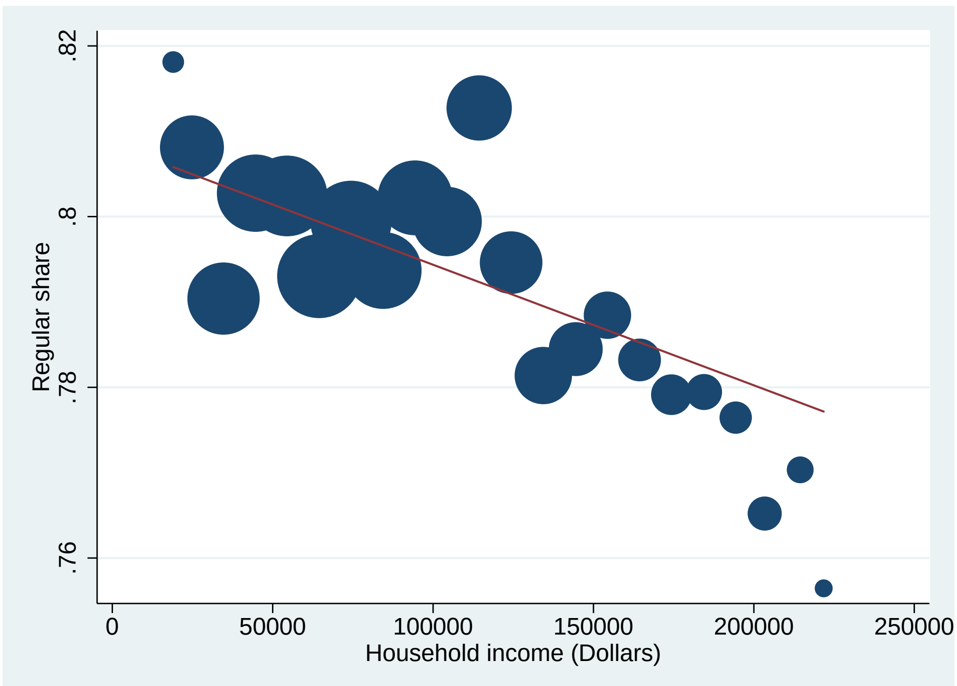
Notes: Data are from the EIA. Each panel plots the monthly US market share of regular gasoline and the monthly US average price of regular gasoline (in 2005 US dollars). The level shift in the share of regular gasoline at the beginning of 1996 coincides with a change in the EIA survey instrument.

Figure 2: Regular share and the price of regular gasoline



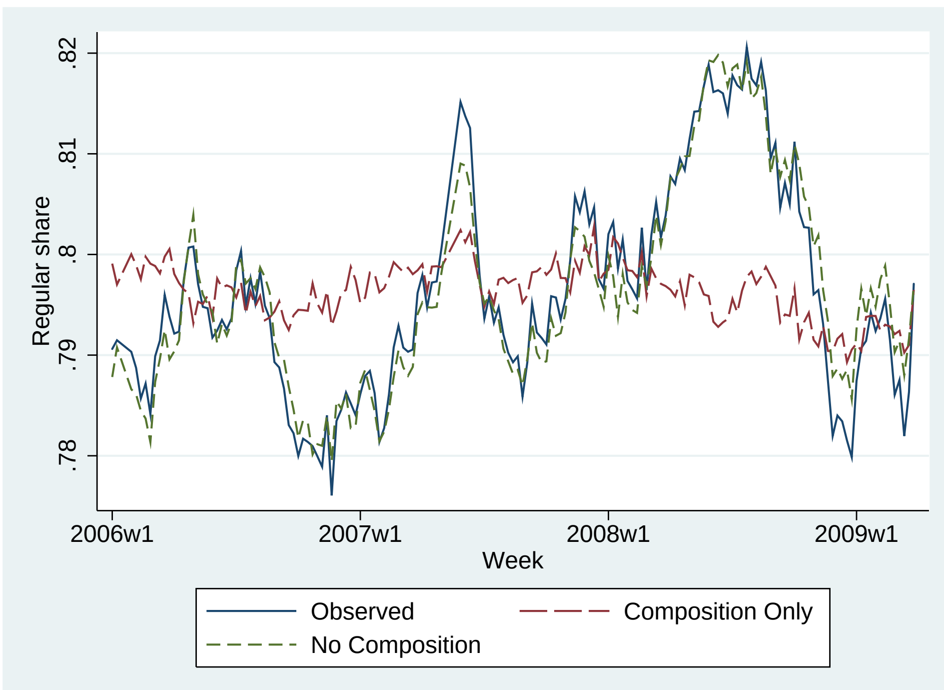
Notes: Data are from the retailer. The plot shows the weekly share of transactions that go to regular gasoline and the weekly average transaction price of regular gasoline (in current US dollars).

Figure 3: Regular share and household income



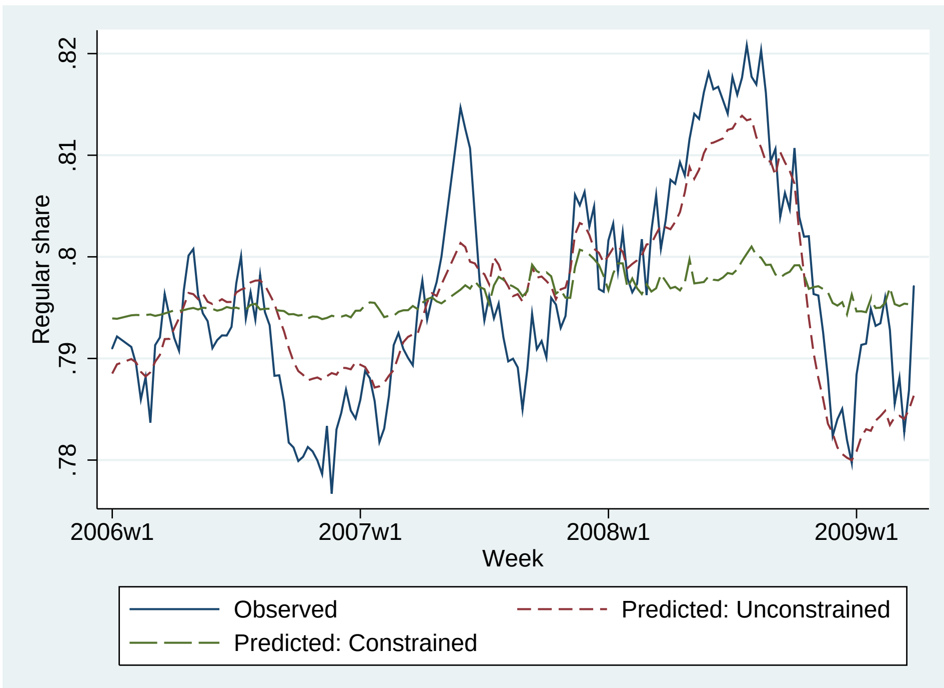
Notes: Data are from the retailer. The plot shows the average share of transactions that go to regular gasoline against the average household income for different groups of households. Households are grouped into bins of width $10,000, i.e. into bins $20,000-29,999, $30,000-$39,999, etc. Figure excludes households with income below the 5th percentile or above the 95th percentile. The area of each symbol is proportional to the number of sample households in each income bin.

Figure 4: Compositional change in gasoline purchasers



Notes: Data are from the retailer. The line labeled "observed" shows the weekly share of transactions that go to regular gasoline. The next two lines are based on a transaction-level regression of an indicator for purchase of regular gasoline on household fixed effects. The line labeled "no composition" is the weekly average residual from the regression, normalized to have the same mean as the observed series. The line labeled "composition only" is the weekly average predicted value from the regression.

Figure 5: Model fit, with and without fungibility

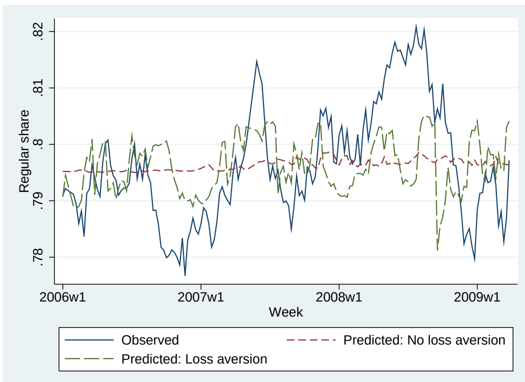


Notes: Data are from the retailer. The line labeled "observed" shows the weekly share of transactions that go to regular gasoline. The line labeled "predicted: unconstrained" shows the average predicted probability of buying regular gasoline from the baseline model in column (1) of table 3. The line labeled "predicted: constrained" shows the average predicted probability of buying regular gasoline from the same model, re-estimated imposing the constraint that $\eta^G = \eta^M$.

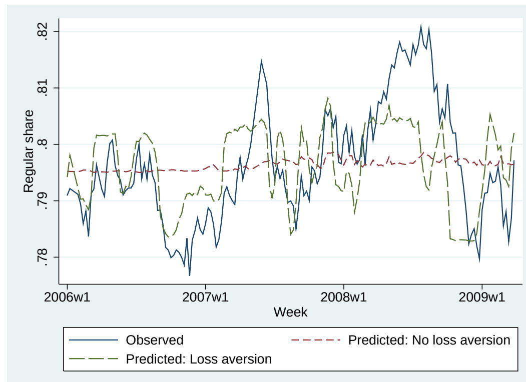
Figure 6: Alternative psychological mechanisms

Panel A: Loss aversion

One-week horizon

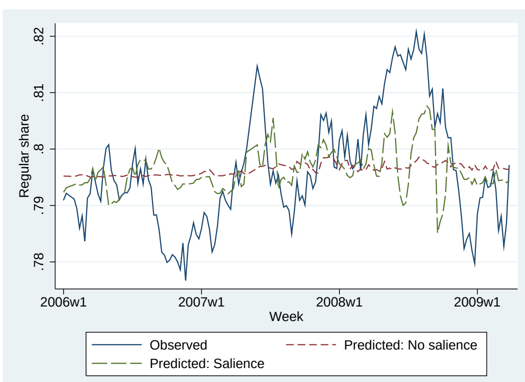


Four-week horizon

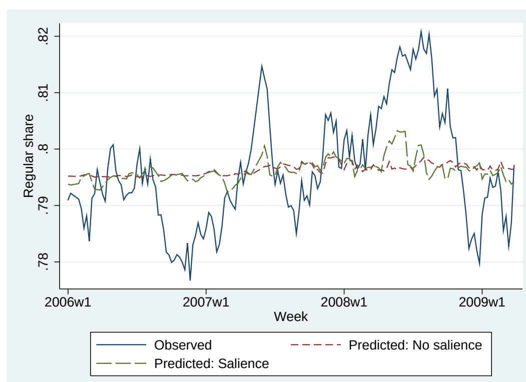


Panel B: Price salience

One-week horizon



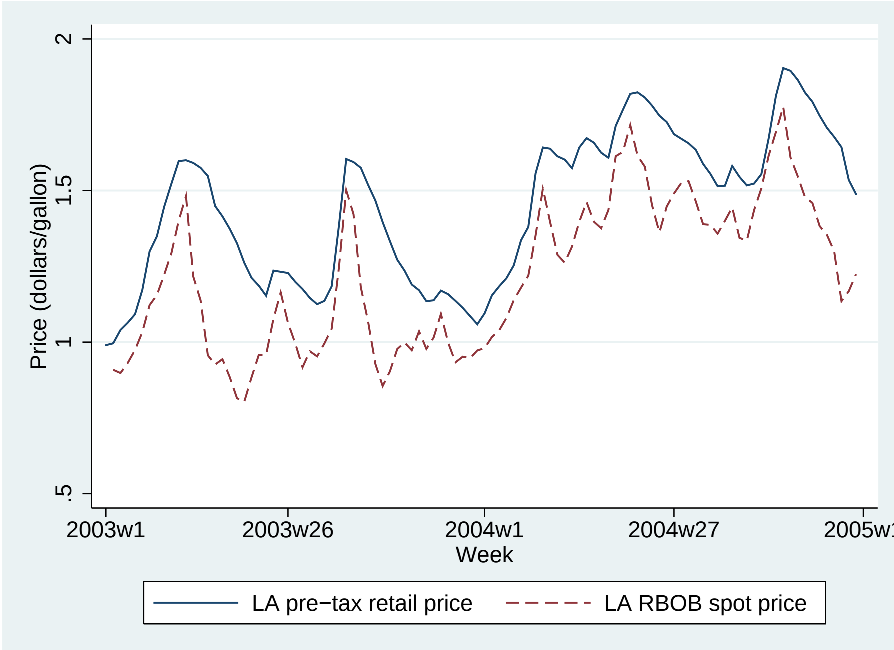
Four-week horizon



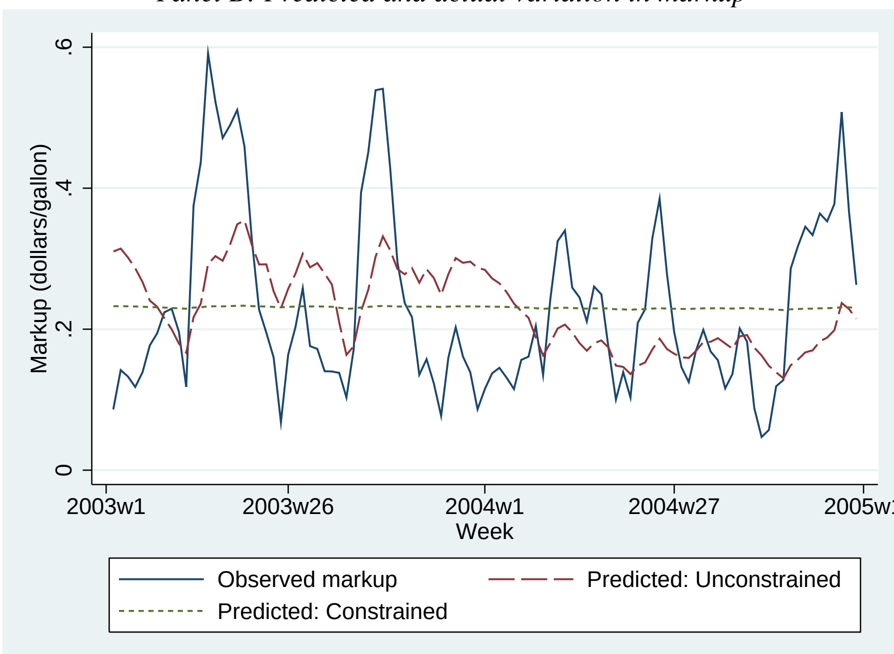
Notes: Data are from the retailer. The line labeled "observed" shows the weekly share of transactions that go to regular gasoline. In Panel A, the lines labeled "predicted: no loss aversion" and "predicted: loss aversion" show the average predicted probability of buying regular gasoline from the model in equation (8) estimated with γ constrained to 0 and with γ unconstrained, respectively. Household expectations are assumed to be based on national gasoline prices [one week/four weeks] prior to purchase. In Panel B, the lines labeled "predicted: no salience" and "predicted: salience" show the average predicted probability of buying regular gasoline from the model in equation (9) estimated with θ and γ constrained to 0 and with θ and γ unconstrained, respectively. The evoked set is assumed to include all gasoline grade at national prices [one week/four weeks] prior to purchase. See section 7 for additional details.

Figure 7: Implications for retail prices

Panel A: Wholesale and retail prices



Panel B: Predicted and actual variation in markup



Notes: Data are from the EIA. Panel A shows the weekly average pre-tax retail price of regular reformulated gasoline in Los Angeles and the previous week's average daily spot price of reformulated gasoline. Panel B shows the observed markup (equal to the difference between the pre-tax retail price and the spot price) and the markup predicted by the model of retailer behavior described in section 8 under two different assumptions about the determinants of household marginal utility of money. The line labeled "predicted: unconstrained" uses the estimates of the marginal utility function from our baseline model in column (1) of table 3. The line labeled "predicted: constrained" uses the estimates of the marginal utility function from same model, re-estimated imposing the constraint that $\eta^G = \eta^M$.

Appendix Table 1: Alternative specifications

		Average Marginal Effect on Regular Share of:			Fungibility
		Increase in Gas Price ($1)	Increase in Gas Expenditure ($1000)	Decrease in Total Expenditure ($1000)	p-value
(1)	Baseline	0.0142	-0.0120	-0.0008	0.0000
		(0.0010)	(0.0009)	(0.0000)	
(2)	Restrict to single-adult households	0.0149	-0.0126	-0.0013	0.0000
		(0.0010)	(0.0009)	(0.0000)	
(3)	Control for gallons purchased	0.0146	-0.0123	-0.0008	0.0000
		(0.0010)	(0.0009)	(0.0000)	
(4)	Below-median income	0.0151	-0.0128	-0.0010	0.0000
		(0.0010)	(0.0008)	(0.0000)	
(5)	Above-median income	0.0138	-0.0117	-0.0010	0.0000
		(0.0012)	(0.0010)	(0.0000)	
(6)	Predict total expenditure from Census block income	0.0143	-0.0120	-0.0014	0.0000
		(0.0010)	(0.0009)	(0.0000)	
(7)	Account for correlation with energy prices	0.0142	-0.0104	-0.0008	0.0000
		(0.0010)	(0.0008)	(0.0000)	
(8)	Estimate gasoline expenditure from retailer data	0.0151	-0.0211	-0.0008	0.0000
		(0.0012)	(0.0017)	(0.0000)	
(9)	Identify from variation in world oil price	0.0137	-0.0116	-0.0008	0.0000
		(0.0012)	(0.0010)	(0.0000)	
(10)	Restrict to transactions with 10-cent price gaps	0.0162	-0.0137	-0.0008	0.0000
		(0.0023)	(0.0020)	(0.0000)	
(11)	Aggregate to store-week level	0.0147	-0.0125	-0.0012	0.0000
		(0.0011)	(0.0009)	(0.0000)	
(12)	Allow store-week-level preference shock	0.0121	-0.0103	-0.0008	0.0000
		(0.0024)	(0.0020)	(0.0001)	
(13)	Below-median tank size	0.0150	-0.0127	-0.0010	0.0000
		(0.0011)	(0.0009)	(0.0000)	
(14)	Above-median tank size	0.0133	-0.0113	-0.0008	0.0000
		(0.0010)	(0.0009)	(0.0000)	

Notes: The baseline specification is from column (1) of table 3. Other specifications are variants on specification (1). See appendix C for details.