Did U.S. Consumers Respond to the 2014–2015 Oil Price Shock? Evidence from the Consumer Expenditure Survey

Patrick Alexander[a] *and Louis Poirier*[b]

ABSTRACT

The impact of oil price shocks on the U.S. economy is a topic of considerable debate. In this paper, we examine the response of U.S. consumers to the 2014–2015 negative oil price shock using representative survey data from the Consumer Expenditure Survey. We propose a difference-in-difference identification strategy based on two factors, vehicle ownership and gasoline reliance, which generate variation in exposure to oil price shocks across consumers. Our findings suggest that exposed consumers significantly increased their spending relative to non-exposed consumers when oil prices fell, and that the average marginal propensity to consume (MPC) out of gasoline savings was above 1. Across products, we find that consumers increased spending especially on transportation goods and non-essential items.

Keywords: Oil prices, Oil shocks, Gasoline prices, U.S. economy, Consumer spending

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1. INTRODUCTION

The impact of the oil price decline of 2014–2015 on the U.S. economy has been a topic of considerable debate. Between June 2014 and December 2015, global oil prices fell by almost 50%. As a net oil importer, the U.S. should benefit economically from a negative oil price shock based on standard macroeconomic theory. However, popular commentary in the U.S. related to the 2014–2015 shock has lamented that this oil shock did not deliver the economic benefits that previous negative shocks provided.[1]

Quantitative exercises that have focused on this episode have also been somewhat conflicting. U.S. economic growth was weaker than expected throughout the 2014–2015 period, suggesting perhaps that the positive impact of the oil shock on consumer spending was unexpectedly weak (Furman, 2015; Leduc et al., 2016). Meanwhile, non-representative studies using micro data (Farrell and Grieg, 2015; Gelman et al., 2016) and studies using macro data (Baumeister and Kilian, 2016)

1. In 2015, the Council of Economic Advisers highlighted the surprisingly low response of U.S. consumption to low oil prices (Council of Economic Advisors, 2015). Also, a 2015 Gallup survey mentioned that most U.S. consumers did not spend their savings at the pump (Swift, 2016), and the *New York Times* quoted John C. Williams, president of the Federal Reserve Bank of San Francisco, who said that the impact of declining oil prices on consumption was misunderstood (Appelbaum, 2016).

a Corresponding author. Senior Economist, International Economic Analysis Department, Bank of Canada, 234 Wellington Street West, Ottawa, Ontario, Canada, K1A 0G9. E-mail: palexander@bankofcanada.ca.

b Economist, International Trade and Finance Branch, Department of Finance, Government of Canada, 140 O’Connor Street #19, Ottawa, Ontario, Canada, K1A 0G5.

suggest that consumers spent a considerable share of their gasoline expenditure savings. To date, we are aware of no study that examines the response of U.S. consumers to the 2014–2015 oil price shock using representative micro data for the entire U.S. population that cover all types of consumer purchases.

Understanding the mechanisms through which, and magnitude by which, oil price fluctuations impact the U.S. economy is of crucial importance, particularly for policy makers who rely on and communicate forecasts of U.S. economic growth. Given the increased significance of U.S. shale oil production in recent years, it is important to examine this topic using recent data to assess whether consumers still, on average, increase their spending when oil prices decline. As evident in reports by Furman (2015) and Yellen (2016), these issues were of key interest for U.S. policy makers throughout the 2014–2016 period, and will likely continue to be of interest as oil prices fluctuate in the future.

In this paper, we assess the impact of the 2014–2015 decline in oil prices on U.S. consumer behavior using survey micro data from the Consumer Expenditure Survey (CE).[2] The CE is particularly useful for studying this topic for two reasons. First, the survey is representative of the entire U.S. population and covers nearly all types of consumer purchases.[3] Second, the survey provides detailed information on various household characteristics, including motor vehicle ownership, that are useful for analyzing the topic.

We use a difference-in-difference type estimation strategy where we classify the treatment group as households that report that they own a vehicle, and the control group as those that do not report this. We also consider a secondary specification where the treatment group is households that rank above the 20th percentile in the distribution of gasoline spending propensity out of total expenditures, and the control group is households that rank below the 20th percentile in this distribution. We then assess the impact of the treatment, exposure to the gasoline savings provided by lower oil prices, on consumer spending behavior.

Our results suggest that the oil price shock had significant effects on consumer spending behavior. Gasoline savings were passed on to non-gasoline expenditures for consumers that gained the most in purchasing power due to the price shock. Based on our calculations, the average marginal propensity to consume (MPC) out of gasoline savings for U.S. consumers exposed to the shock was above 1. Overall, these findings suggest that oil price declines continue to induce growth in U.S. consumer spending as the standard theory predicts.

Our research contributes to several topics in the energy economics literature. First, our findings relate to recent studies that attempt to identify the mechanisms through which oil price changes affect the U.S. economy. An extensive literature, beginning with Edelstein and Kilian (2009), has argued for the importance of the *discretionary income effect* as a key transmission channel.[4] Recently, several studies have emphasized the significance of this channel for the U.S. in 2014–2015 (Baumeister and Kilian, 2016; Baumeister et al., 2018) while others have argued that

2. The CE is published by the Bureau of Labor Statistics (BLS).
3. According to the BLS, the data that we use cover roughly 95% of the typical consumer's expenditures.
4. Conceptually, the *discretionary income effect* represents the mechanism whereby a decline in oil prices leads to lower household spending on oil products and, therefore, frees up income to be spent on other products. For the typical household that does not earn income from an oil-related industry, this effect should generate increased real spending on non-oil products, assuming that oil products are demand inelastic and that households have an MPC out of income that is above zero. Moreover, given that the U.S. is a net importer of oil, in total these gains will outweigh total income losses from the U.S. oil industry, and hence one should expect higher aggregate consumption due to lower oil prices. For a more lengthy discussion of the *discretionary income effect*, see Baumeister et al. (2018). For examples of studies that examine the *discretionary income effect* in periods prior to 2013–2015, see Edelstein and Kilian (2009) and Hamilton (2009).

it was not important (Ramey, 2016). Notably, these studies rely on proxy measures for the average *discretionary income effect* applied to aggregate data, and thus fail to provide a contemporaneous control group. Our methodology is useful in identifying this channel because we consider plausibly exogenous variables that govern differential exposure to oil prices, and use this as a basis for a difference-in-difference estimation. Our results strongly support the importance of the *discretionary income* channel for transmitting the 2014–2015 oil price shock to U.S. private consumption.[5]

We also find that spending growth by exposed households was significantly higher than the standard *discretionary income* channel should have accounted for, suggesting that other mechanisms described by Edelstein and Kilian (2009) were important in 2014–2015. Specifically, our findings suggest that the *operating cost effect*, which captures substitution in consumption towards products that are complements to oil, was important, as households spent most of their gasoline savings on transportation goods. Since transportation goods are durable goods that require lumpy purchases (e.g., motor vehicles), the expenditure response to the 2014–2015 shock was significantly above the magnitude of the real income savings brought about by lower oil prices, which explains why we find that the MPC out of gasoline savings for U.S. consumers exposed to the shock was above 1.

Our results are consistent with non-representative studies from Farrell and Grieg (2015) and Gelman et al. (2016), which also find that consumers increased spending significantly due to savings from lower gasoline prices throughout the 2014–2015 period. Notably, neither of these other studies uses measures of consumer spending that capture spending on transportation goods. Our results suggest an even larger response from U.S. consumers than these studies do, seemingly due to the importance of increased spending on transportation goods.[6]

We also examine the types of goods and services that were purchased with the windfall savings from lower gasoline prices. As mentioned, most of the savings were spent on transportation goods, which is consistent with findings based on aggregate data from Edelstein and Kilian (2007, 2009). Besides transportation goods, we find that consumers generally spent their savings on non-essential items, including food away from home, apparel, entertainment, and alcohol, which is largely consistent with results found in Edelstein and Kilian (2007) and Farrell and Grieg (2015).[7]

Finally, we consider whether household spending out of gasoline price savings varied across households living in urban versus rural settings, households living in oil-producing versus non-oil-producing states, and across household mortgage statuses. We find that rural dwellers spent significantly more out of gasoline savings than urban dwellers, perhaps due to the fact that our rural treatment groups are especially gasoline dependent.[8] We find that households living in non-oil-producing states spent significantly more out of savings than households living in oil-producing states,

5. Ramey (2016) argues that the share of U.S. consumer spending on gasoline is, in principle, a misleading factor in explaining the impact of oil price shocks on the U.S. economy, and argues instead that the share of oil imports explains the response of private consumption to oil price shocks in recent decades. While our analysis does not refute or support the role of oil import share in determining the response of private consumption to oil price shocks, our findings do strongly suggest that household spending share on gasoline was a significant factor in explaining differential consumption responses across households after the June 2014 oil price shock.

6. Farrell and Grieg (2015) and Gelman et al. (2016) find that consumers spent roughly 80% and 100% of their gasoline savings in 2014–2015, respectively, whereas we find that consumers spent over 100% of their gasoline savings. Farrell and Grieg (2015) rely on credit card data that cover a sample of 25 million regular debit and credit card holders. As these authors note, their measure of spending does not cover purchases made with cash, checks or electronic transfers, which are common modes of payment for motor vehicle payments. Gelman et al. (2016) rely on smartphone application data that do not include purchases of consumer durables or housing, and therefore do not cover vehicle purchases.

7. Patterns in the data also suggest that consumers increased real expenditures on gasoline itself, which is consistent with findings from Gelman et al. (2016).

8. On average, rural residents spend a larger share of their total expenditures on gasoline than urban residents.

possibly due to the negative income and wealth effects that the latter group experienced due to the shock. Finally, we find that non-mortgage holders spent significantly more out of savings than mortgage holders. This is weakly consistent with evidence from Gelman et al. (2016), who find that mortgage holders did not spend more out of gasoline savings than non-mortgage holders after the June 2014 shock, which suggests that consumers treated the 2014–2015 shock as permanent rather than transitory. Our finding that non-mortgage holders increased spending *more* than mortgage holders could suggest that mortgage holders payed back mortgage debt rather than increased spending of gasoline savings, which is consistent with findings from Di Maggio et al. (2017) for household responses to mortgage rate adjustments. We note that further analysis is required before reaching any firm conclusions based on our findings in relation to household setting and mortgage status.

The rest of the paper is organized as follows. In section 2, we describe the data. In section 3, we describe our empirical approach. Section 4 describes the results. Section 5 concludes.

2. DATA

2.1 Handling the CE database

For this paper, we use Consumer Expenditure Survey (CE) micro data for 2013 to 2015 inclusive. The CE is the most comprehensive micro data source on household spending available for the U.S., and is primarily used for the construction of official consumer price index (CPI) weights. The CE data are derived from two separate surveys: the Interview Survey and the Diary Survey. In this paper, we only use data from the Interview Survey since the Diary Survey mostly reports day-by-day spending on small items. The Interview Survey collects expenditure data for up to 25,000 households per 3-month interval.[9] Each household is interviewed quarterly up to 5 times, reporting their spending over the previous 3 months. To have a clear representation of when spending occurred, we follow BLS suggestions and convert these measures into monthly values for every household. In addition to detailed information on household consumption expenditures, the CE also compiles information on various household characteristics, including household income, state of residence, etc. All expenditure data are nominal and non-adjusted for seasonality. U.S. population weights are provided for each household to accord with a representative sample of the U.S. population.

In general, aggregated consumption measures constructed from the CE micro data have been found to closely match official aggregate measures of personal consumption expenditures (PCE) constructed by the Bureau of Economic Analysis (BEA).[10] However, the CE also has well-known limitations compared to official aggregate consumption data (Garner et al., 2009). First, for some consumption items, the CE is prone to recall bias, hence some households misestimate their consumption of certain goods and services, and survey participants tend to round reported values up or down. Second, the CE has been documented to have an under-representation of high-

9. The Interview Survey provides information on up to 95% of the typical household's consumption expenditures. The unit of measurement in the CE is the so-called Consumer Unit (CU), which the BLS defines as "(1) all members of a particular household who are related by blood, marriage, adoption, or other legal arrangements; (2) a person living alone or sharing a household with others or living as a roomer in a private home or lodging house or in permanent living quarters in a hotel or motel, but who is financially independent; or (3) two or more persons living together who use their incomes to make joint expenditure decisions." To simplify the discussion in this paper, we will refer to Consumer Units as households.

10. This statement is true both in terms of levels and dynamic behavior over time (Bee et al., 2013). The official aggregate CE data are a combination of both the Interview Survey and Diary Survey data. Therefore, our aggregate annual numbers do not perfectly match the official aggregate CE data. Furthermore, the BLS performs some data cleaning to their micro data before aggregation. Nevertheless, the differences between the official aggregate data and our aggregate measures are relatively small.

er-income households. The BLS also top-codes consumption values for high-income households to avoid breaching the confidentiality of respondents. These flaws create an under-reporting problem for certain consumption categories.

To address these limitations, we follow Cloyne et al. (2015) and Coibion et al. (2017) and drop data points that are inconsistent or extreme.[11] We also drop observations for households in the top and the bottom percentile of income, where data are the most likely to be of poor quality. After cleaning, we are left with a database covering roughly 190,000 monthly household observations from January 2013 to December 2015.

One of our aims in cleaning the data was to derive a measure of household spending that was not influenced by idiosyncratic sectoral changes that occurred during our period of study that had nothing to do with the oil price shock. Accordingly, we created a "core" measure of spending that has several sub-components removed. First, we removed spending on health insurance due to the changes in the health insurance market in 2014. Second, we removed a category denoted "retirement savings" since this does not, in our view, conform to actual spending. Third, we removed education spending, which is extremely volatile and small. By removing these three categories, in addition to gasoline spending, we developed our measure of core spending.[12]

2.2 Description of the data

Table 1 shows the distribution of spending shares per sub-category and year aggregated across all households in our cleaned data. We categorize spending into three groups: "essential" products (food, shelter, transportation, baby care, health care, insurance, and utilities), "non-essential" products (alcohol, apparel, entertainment, personal care, education, books, food away from home, household expenses, and miscellaneous) and gasoline. The classification of spending into essential and non-essential products was inspired by previous work by Parker et al. (2013), who categorize all their types of goods and services into durables and non-durables spending, and work by Edelstein and Kilian (2007) that argues that gasoline price savings might be largely spent on smaller, discretionary items. Our goal is to distinguish smaller and/or non-essential purchases from larger and/or essential purchases. Our expectation is that, generally, spending out of gasoline price savings will be concentrated towards smaller and/or non-essential purchases. While we acknowledge that our classification could be considered somewhat subjective, it does not influence the interpretation of our results because we also look at which sub-components of the categories are driving our results.

2.2.1 Dynamics of consumption expenditures in CE data

As reported in Table 1, the gasoline nominal spending share decreased throughout our sample as gasoline prices fell. As Figure 1 panel 1 illustrates, U.S. gasoline prices went from an average of $3.55 USD per gallon in the first half of our sample (January 2013 to June 2014) to $2.73 USD per gallon in the second half (July 2014 to December 2015), a drop of 25 per cent.[13] These two periods will be referred to as before and after the oil price shock for the remainder of the paper. Figure 1 also reveals that consumers increased their real spending on gasoline beginning in the latter half of

11. For example, we drop households that report zero or negative income or consumption, or households that don't report owning a vehicle, but report spending money on gasoline.

12. It should be noted that our main results in Table 3 still hold if we don‘t remove education, health insurance, and retirement savings.

13. Meanwhile, over the same period oil prices fell 50 per cent, suggesting that the pass through of the oil price shock to gasoline prices was above zero but below 1.

Table 1: Aggregated Expenditure Shares Across Product Groups

	2013	2014	2015
Total spending	$49,827	$51,567	$52,798
	As a share of total spending		
Essential	70.4%	71.0%	71.5%
Food	10.2%	10.1%	10.0%
Shelter	20.6%	20.6%	19.9%
Transportation	12.7%	12.5%	13.5%
Baby care	0.6%	0.6%	0.7%
Health care	7.3%	8.2%	8.1%
Personal insurance	11.4%	11.3%	11.9%
Utilities	7.6%	7.7%	7.4%
Non-essential	24.3%	24.1%	24.5%
Alcoholic beverages	0.8%	0.8%	0.9%
Apparel	1.8%	1.9%	1.9%
Entertainment	4.5%	4.7%	4.7%
Personal care	0.6%	0.6%	0.6%
Education	2.0%	1.9%	1.7%
Books	0.2%	0.2%	0.2%
Food away from home	4.7%	4.8%	4.9%
House expenses	3.3%	3.2%	3.4%
Miscellaneous	6.4%	6.1%	6.3%
Gasoline	5.3%	4.9%	4.0%
Core	77.3%	76.9%	77.2%
Number of observations	62,864	63,071	64,917

Source: Derived by authors from CE micro data. See section 2 for details. Notes: We report the data using the same subcategories as the official CE aggregate values. Our measure of core spending excludes gasoline, education, health insurance, and retirement savings. All figures reported represent annual averages across households.

2014 when gasoline prices began to decline. Between August 2014 and January 2015, the average number of gallons consumed by U.S. households rose from 62 gallons to 82 gallons, and remained noticeably higher throughout 2015 compared to the first half of our sample period.

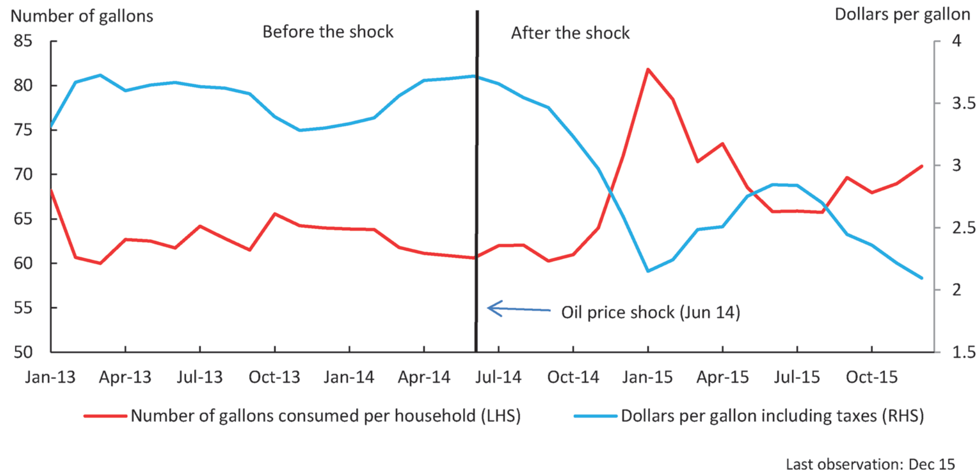
While gasoline price changes might generally be caused by demand factors, and therefore would perhaps not be exogenous from the perspective of U.S. households, the 2014–2015 gasoline price decline was widely considered to be mostly driven by supply-side factors. As documented by Gelman et al. (2016), the 2014–2015 oil price decline, which was responsible for the decline in gasoline prices, occurred contemporaneously with decisions by OPEC to abandon price support, and with the expansion of oil supply from the U.S. shale industry and Canadian Oil Sands, both of which are supply-oriented factors.[14]

The evidence in Figure 1, coupled with evidence from Table 1, suggests that the price elasticity of gasoline spending for U.S. consumers was between zero and –1 during the 2014–2015 period.[15] While we are reluctant to argue that this evidence provides a clean estimate of the elasticity

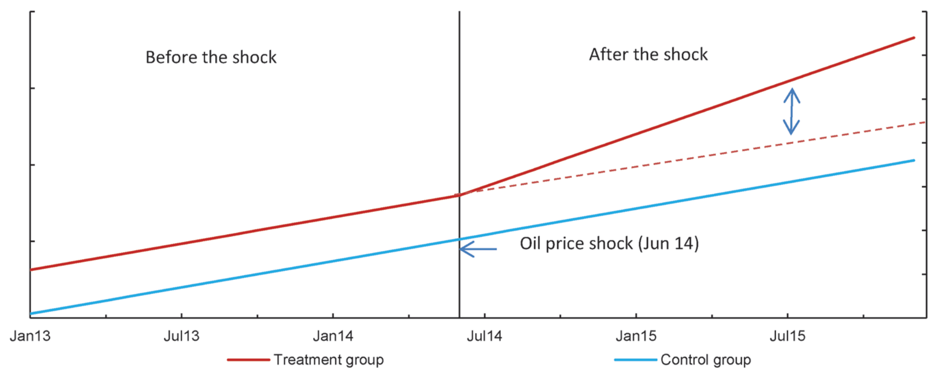
14. For more on the importance of supply factors in driving the 2014–2015 oil price decline, see Baffes et al. (2016) and Baumeister and Kilian (2016). Gelman et al. (2016) also provide strong evidence that the 2014–2015 oil price shock was unanticipated and treated as permanent by U.S. households.

15. If the price elasticity of gasoline were equal to or below –1, then we would expect nominal gasoline spending to remain constant or rise in a period of falling gasoline prices. If the price elasticity of gasoline were above 0, then we would expect real gasoline spending to fall in a period of falling gasoline prices. Estimates of the price elasticity of demand for gasoline sometimes differ across sample periods, perhaps due to differences in the sources of oil price shocks over time. For

Figure 1: Relationship Between Gasoline Prices and Real Expenditures



Notes: Values for gasoline prices are taken from U.S. Energy Information Administration data. Values for number of gallons consume per household are derived by dividing total spending on gasoline, based on CE microdata, by the price of gasoline. All figures are derived from monthly data.



Notes: The wedge between the solid and dotted lines depicts the treatment effect.

of demand for gasoline, evidence from other studies suggests elasticity estimates in this range for the U.S. For example, Edelstein and Kilian (2009) derive an estimate for the price elasticity of gasoline of –0.46 using aggregate U.S. data from 1970 to 2006. More recently, Gelman et al. (2016) find similar estimates using a non-representative panel of individual expenditures derived from mobile app data for the 2014–2016 period. For elasticities in this range, real gasoline expenditure partially rises as gasoline prices fall, leaving a significant share of windfall gains for either expenditure on non-gasoline products or for savings. Also, Table 1 reveals that no other categories of consumption exhibited as large of a decline in spending share as gasoline over the 2013–2015 period. In contrast to gasoline, spending share on both essential and non-essential products rose over this period.

example, in contrast to the 2014–2015 episode, U.S. real gasoline spending was quite stable as oil and gasoline prices fell in 2008 (Crain and Eitches, 2016), a decline that was widely believed to be driven by global demand. Hughes et al. (2008) also find evidence that the price elasticity of gasoline demand in the U.S. was low during the 2001–2006 period, when oil price fluctuations were, according to several studies, again driven significantly by global demand (Hamilton, 2009; Kilian, 2008). Meanwhile, evidence based on microdata from Byrne et al. (2015) suggests that consumers in Ontario, Canada were responsive to changes in local gasoline prices during the 2007–2008 period, suggesting that responsiveness varies across jurisdictions as well.

Spending share on transportation, a product group that is broadly complementary to gasoline, increased more significantly than any other sub-group, from 12.7% in 2013 to 13.5% in 2015.

To provide a more rigorous exploration of how U.S. households responded to the 2014–2015 oil price shock, we consider whether, and by how much, consumers who were especially exposed to the shock increased their spending. Intuitively, the impact should be larger for households that own a vehicle or who are relatively dependent on gasoline spending.[16] To examine this, Table 2 reports differences in spending behavior throughout our sample between vehicle owners and non-vehicle owners and low and high gasoline spenders.[17] The set of households that report owning a vehicle outnumber the set that do not by a ratio of nearly ten to one (173,341 versus 17,524).[18] By construction, the ratio of high gasoline to low gasoline spenders is five to one.

Table 2: Differences in Consumption Patterns, Before and After the Oil Price Shock

	Core monthly spending				
	All sample	With a car	Without a car	High gas	Low gas
Before shock (Jan2013-Jun2014)	$3,216.49	$3,386.48	$1,582.37	$3,481.26	$2,060.77
After shock (Jul2014-Dec2015)	$3,390.65	$3,569.95	$1,613.90	$3,741.31	$2,111.22
Difference (%)	5.40%	5.40%	2.00%	7.50%	2.40%
	Gasoline spending				
	All sample	With a car	Without a car	High gas	Low gas
Before shock (Jan2013-Jun2014)	$218.75	$240.87	.	$261.49	$29.03
After shock (Jul2014-Dec2015)	$184.75	$203.36	.	$227.25	$28.54
Difference (%)	–15.50%	–15.60%	.	–13.10%	–1.70%
Number of observations	190,852	173,341	17,524	152,691	38,174

Source: Derived by authors from CE micro data. See section 2 for details. Notes: Our measure of core spending excludes gasoline, education, health insurance, and retirement savings. All figures reported represent monthly averages across households.

Comparing the pre-shock (before July 2014) and post-shock (after June 2014) samples, core spending increased for our entire sample, as reported in Table 2. This evolution of spending is consistent with the robust improvements the U.S. economy experienced between 2013 and 2015. Indeed, between our two sample dates, aggregate consumption, as measured by the BEA, increased 6.5 per cent. More interestingly, the increase in spending was noticeably higher for vehicle owners and high gasoline spenders than their control counterparts.

This finding could be due to the fact that vehicle owners and high gasoline spenders tend to have higher incomes; several studies have documented that higher-income households benefited the most in the post-crisis period in the U.S.[19] Intuitively, these income gains could have been passed

16. Theoretically, non-gasoline producers that use oil products as inputs should also benefit from lower oil prices, which could lead to either direct impacts for business owners or indirect impacts for consumers through lower prices beyond gasoline. However, existing work suggests that there is little evidence to support that these channels are quantitatively important (Edelstein and Kilian, 2009).

17. These two groups are defined with indicator variables. For vehicle owners, we identify each household with a vehicle with an indicator of 1, and those without a vehicle with an indicator of 0. For this exercise, we exclude any households in the sample that become a vehicle owner while being interviewed to address endogeneity concerns. For high gasoline spenders, we identify households who fall above the 20th percentile in the distribution of gasoline expenditure share with an indicator of 1, and those that fall below the 20th percentile with an indicator of 0.

18. Evidence from the American Community Survey suggests that 9.1% of U.S. households did not own a vehicle in 2011–2015, which closely resembles the share that we find for 2013–2015.

19. See, for example, Semega et al. (2017) or Saez (2016) on the evolution of top incomes in the United States in the post-crisis period.

on to higher spending, which explains why relative spending by vehicle owners and high gasoline spenders increased after June 2014.

Meanwhile, vehicle owners and high gasoline spenders differ significantly from their control group counterparts along several other dimensions as well. On average, both groups have higher core monthly spending over our entire sample, have more people per household, and are much more likely to have a mortgage, than their respective control groups. These differences, as well as household income, should be controlled for when estimating the response of consumer spending to variations in gasoline prices. We describe our approach to doing this in the next section.

3. EMPIRICAL APPROACH

Our primary aim in this analysis is to quantify the change in U.S. core expenditures that occurred in response to the 2014–2015 oil price shock. To do so, we perform a difference-in-difference estimation, the theoretical basis for which is illustrated in Figure 1 panel 2. Our identification strategy considers two groups, one that experiences savings brought by lower oil prices (the treatment group, depicted by the red line) and one that does not (the control group, depicted by the blue line). The treatment effect is defined as the difference between the change in spending from the pre-shock to the post-shock period for the treatment group and the control group. In Figure 1, this wedge is represented by the difference between the dotted red line and the solid red line (since the dotted blue line lies on top of the solid blue line for the control group).

Ideally, a difference-in-difference estimation exercise should have similar pre-treatment trends for control and treatment groups, as depicted in Figure 1. In Figure 2, we reproduce similar figures to Figure 1 panel 2 using actual data from both of our two specifications: where the treatment group is (1) vehicle owners and (2) households above the 20th percentile in the distribution of gasoline expenditure share. A visual inspection of the upper panel reveals a different evolution of spending for the treatment and control group before the shock under the main specification, which is concerning from the perspective of validity for our test. Note that this appears to be less of a problem under our secondary specification in the lower panel, which is one reason why this specification is provides an important robustness check for our main results.

The difference-in-difference regression specification for household i and month-year t is the following:

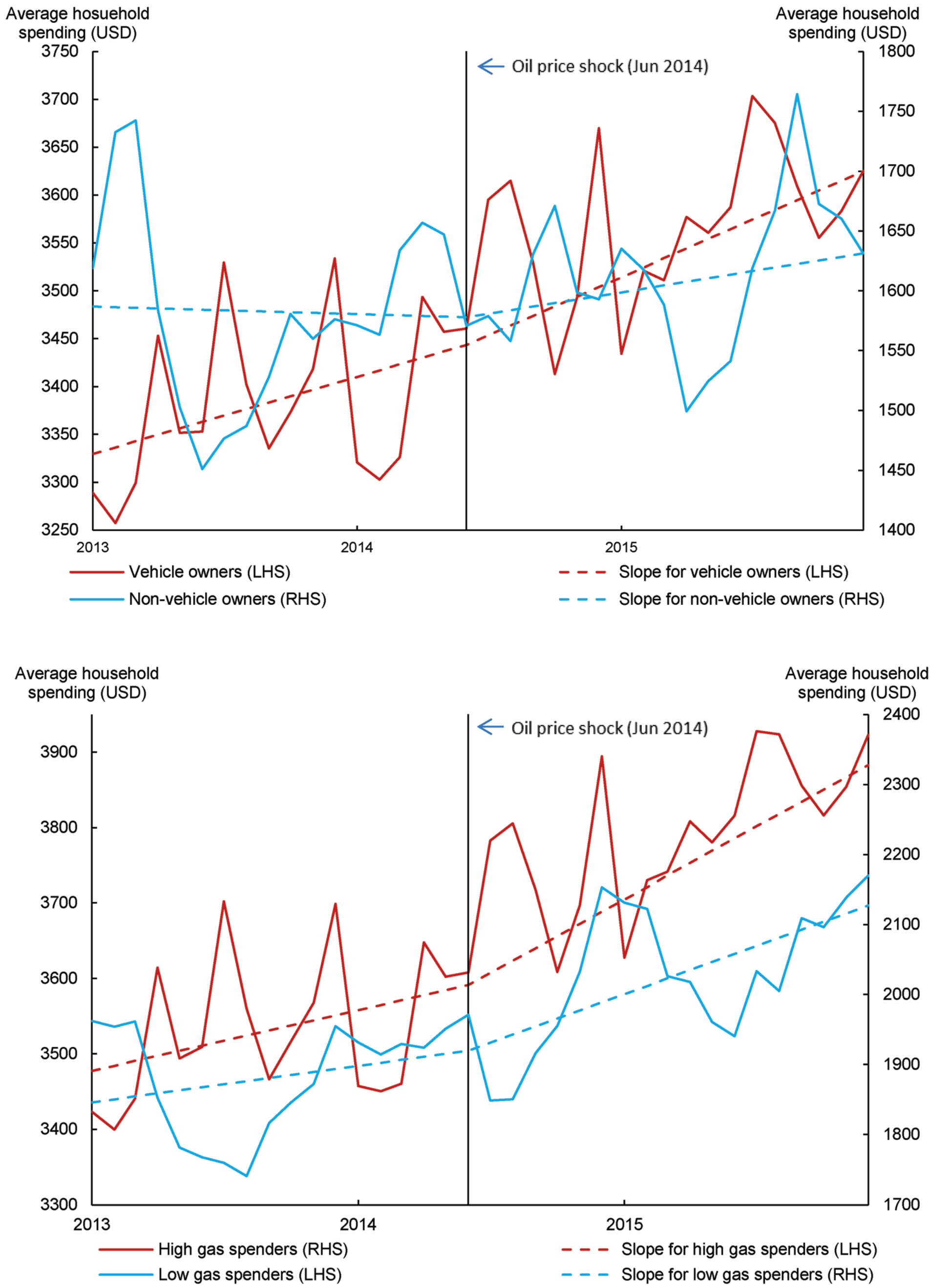
$$Y_{i,t}=\beta_0+\beta_1 treated_i+\beta_2 shock_t+\beta_3 treated_i\times+\beta_4 controls_{i,t}+\lambda_t+\epsilon_{i,t}, \quad (1)$$

where $Y_{i,t}$ represents a spending variable of interest, $treated_i$ represents an indicator variable for the treatment group, and $shock_t$ represents an indicator variable for after the oil price shock (post June 2014).

The coefficient of interest, β_3, captures the impact of the interaction term, treated and shock, which indicates the average treatment effect. We also include additional control variables ($controls_{i,t}$) to adjust for differences in observable characteristics across groups,[20] and month-year

20. Our list of control variables includes measures of households income, household size, age of the household head, and several 0/1 indicators, including: a variable that equals 0 for households that reported having a head that is female and 1 for those that reported having a head that is male; a variable that equals 0 for households that reported living in a rural area and 1 for those that reported living in an urban area; a variable that equals 0 for households that reported not owning a home with a mortgage and 1 for those that did report this; and a variable that equals 0 for households where the head is reported as being unemployed and 1 for those where the head is not reported as being unemployed.

Figure 2: Pre- and Post-Shock Trends in Household Expenditures Across Treatment and Control Groups



fixed effects λ_t.[21] Finally, $\epsilon_{i,t}$ represents an error term that is assumed to be identically and independently distributed.

21. We include month-year fixed effects to control for any time-specific macroeconomic changes that might affect consumption across all types of U.S. households, and to adjust for a linear trend in average U.S. consumption growth over our

Overall, our two specifications offer several advantages and disadvantages. Our main specification has the advantage that moving from the control group to the treatment group requires a significant investment (buying a vehicle), hence it offers perhaps a more convincing identification for the impact of oil price changes on consumption based on predetermined exposure to the shock. Meanwhile, our secondary specification has the advantage that the number of observations is more balanced between the treatment and control groups, and observable household characteristics tend to be more balanced as well. The secondary specification also follows similar approaches taken in other studies (Edelstein and Kilian, 2009; Farrell and Grieg, 2015), so our results under this specification are perhaps more easily contextualized in the related literature. In the end, our view is that it is best to consider both specifications and compare the results as a robustness check.

Our identification strategy assumes that the oil price shock of 2014–2015 was unanticipated and predetermined with respect to the changes in consumption between our treatment and control groups. As mentioned earlier, evidence provided by Gelman et al. (2016) and others suggests that this shock was mainly driven by supply-side factors, exogenous from the perspective of U.S. consumers, and that the shock was unanticipated and treated as permanent by U.S. consumers. Our identification strategy also assumes that selection into treatment and control groups was predetermined with respect to the shock, adjusting for our list of control variables. Again, movement from the control to the treatment group under our main specification requires a significant investment (buying a vehicle), and hence is likely predetermined with respect to gasoline prices for most households. Under our secondary specification, while most households might well spend more on gasoline (in real terms) when prices fall, there is no clear reason why the ranking of gasoline expenditure across households should change, and hence our assumption of pre-determinedness is not unrealistic. Moreover, to address potential endogeneity issues here, we drop all households that moved between our control and treatment groups over our period of study. This reduces our sample size to 189,538.

4. RESULTS

4.1 Main results

Our main set of regression results, based on (1) with core spending as the dependent variable, are reported in columns 1 and 2 in Table 3. We show results for the impact on core spending under both of our main vehicle ownership and secondary gasoline spending share specifications. As the first row indicates, controlling for the set of variables included in the regression, there is evidence that all consumers increased core spending after June 2014 under both of our specifications: the point estimates for the "shock" variable (after June 2014) are positive and statistically significant in both columns 1 and 2. This result appears to suggest that the broad growth in U.S. consumption after June 2014 depicted in Table 2 cannot entirely be explained by variation in our control variables.

Controlling for our set of control variables, consumers with vehicles and/or high gasoline spending share had significantly higher core spending throughout our entire sample, as indicated in row 2, where the estimate for "treated" is positive and statistically significant in both columns 1 and 2. This finding is not surprising, since consumers that own vehicles or spend more on gasoline might generally have higher spending propensities than our control groups.

period of study. Note that the fixed effect for the final month-year in our sample is dropped to permit identification of the β_3 coefficient on the $shock_t$ variable.

In the third row of Table 3, we report estimates of the coefficient on the interaction variable, which captures the differential response of the treatment group to the shock. Our estimates indicate a large, positive, and statistically significant increase in core expenditures, suggesting that households that owned vehicles and/or had a high share of gasoline in their consumption basket spent their savings from foregone gasoline expenditures on non-gasoline products. More specifically, car owners increased their core expenditures by roughly \$103 per month in nominal terms after the 2014–2015 oil price shock (column 1); high gasoline spenders increased their spending on core products by roughly \$92 per month in nominal terms (column 2).[22]

Table 3: Main Results from Difference-in-Difference Estimations

	Vehicle Ownership	Gasoline Reliance	Vehicle Ownership	Gasoline Reliance
Shock	166.2**	220.1***	–35.89***	–32.97***
	(61.58)	(63.53)	(3.049)	(2.835)
Treated	561.9***	541.1***	173.9***	191.3***
	(16.72)	(19.32)	(0.902)	(0.824)
Treated × Shock	103.4***	91.63***	–40.34***	–37.80***
	(22.53)	(25.61)	(1.187)	(1.064)
Hhld. income	22.60***	22.39***	0.616***	0.542***
	(0.229)	(0.230)	(0.00986)	(0.00949)
Hhld. size	188.5***	176.0***	28.56***	24.44***
	(5.001)	(5.067)	(0.514)	(0.467)
Age	3.740***	4.706***	–0.0638*	0.177***
	(0.452)	(0.450)	(0.0287)	(0.0264)
Male	99.96***	91.03***	10.77***	7.711***
	(14.66)	(14.66)	(0.823)	(0.789)
Urban	300.7***	286.6***	–25.49***	–27.76***
	(28.51)	(28.33)	(1.824)	(1.719)
Mortgage	461.4***	448.6***	29.93***	24.12***
	(17.89)	(17.82)	(0.987)	(0.945)
Employed	–135.0***	–159.4***	24.12***	14.80***
	(18.66)	(18.77)	(1.055)	(1.004)
Obs.	189,538	189,538	189,538	189,538
Adj. R-squared	0.211	0.213	0.259	0.315

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1). Columns 1 and 2 report results with core spending as dependent variable. Definition of core spending is described in section 2. Columns 3 and 4 report results with gasoline spending as the dependent variable. Columns 1 and 3 report results where "treated" group is households that report owning a vehicle. Columns 2 and 4 report results where "treated" group is households above the 20th percentile in the gasoline expenditure share distribution. Included as control variables are household income, household size, age of the household head, male indicator for the household head, urban and mortgage indicators, and employed indicator for the household head. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

In the Online Appendix, we provide further robustness tests. These include estimation under our two specifications with (i) month fixed effects, (ii) clustered standard errors by household, (iii) later beginning dates for the treatment, and (iii) propensity scores included in the regressions. These tests provide strong confirmation of our results.

22. Results reported in Table 3 also indicate that, controlling for other included variables, core spending is positively related to household income, household size, age of the household head, and higher for households that are headed by men, households living in urban areas, households that own a home with mortgage debt, and households where the head is unemployed. For the results reported in Tables 4–10, we include similar control variables, but do not report their corresponding estimates in the tables.

4.1.1 Discussion

To assess whether these magnitudes plausibly represent the real income gains characterized by Edelstein and Kilian (2009) as the *discretionary income effect*, we consider a similar regression specification as in equation (1), but with gasoline expenditure as the dependent variable. This specification should yield an estimate of β_3 that represents the average foregone spending on gasoline among our treatment group, indicating the maximum amount that the typical household could spend on other items due to additional discretionary income.

Column 3 in Table 3 reports results from this regression under our primary specification, where the treatment group is represented by vehicle owners. The point estimate for the interaction term suggests that, among vehicle owners, the average household lowered its gasoline spending by roughly \$40 per month after the oil price shock, which is less than half our estimated value of \$103 per month that these households additionally spent on core products after the shock. Column 4 reports results from this regression under our secondary specification, where the treatment group is represented by high gasoline spenders, and implies similar conclusions. The point estimate for the interaction term indicates that, among high gasoline spenders, the average household lowered its gasoline spending by roughly \$38 per month, which is again less than half our estimated value of \$93 per month that these households additionally spent on core products after the shock. These results suggest that households that were exposed to the 2014–2015 oil price shock increased this spending by over twice as much as the maximum amount that could be attributed to the *discretionary income effect*.[23]

In theory, these large responses could reflect additional mechanisms besides the *discretionary income effect*. The *precautionary savings effect* and the *operating cost effect* are potential factors that could deliver augmented expenditure responses to gasoline price shocks. In fact, Edelstein and Kilian (2009) present findings that suggest the expenditure response due to oil price fluctuations could be roughly three times as large as what the maximum effect due to *discretionary income* savings would suggest. They attribute this larger magnitude as partly a reflection of these additional two effects.[24]

Through the *precautionary savings effect*, consumers might interpret lower gasoline prices as a higher future real income, and hence the consumption response could be larger than the direct discretionary savings. Through the *operating cost effect*, consumers might react to lower gasoline prices by increasing their purchases of gasoline-related goods. Since these goods are durable and, in many cases, require lumpy expenditures, the empirical response could be very large when con-

23. Another way to reach a similar conclusion without relying on results in columns 3 and 4 is from the following back-of-the-envelope calculation. To begin, U.S. gasoline prices fell from an average of \$3.55 before July 2014 to \$2.73 after June 2014 according to BEA data, which marks a 25% decline. Moreover, estimates from the literature suggest that the elasticity of demand for gasoline in the U.S. is in the region of –0.3 to –0.5. Suppose we take the lower bound of this range, –0.3, and assume the total MPC out of gasoline price savings is 1. Together, these parameters imply that the cross-price elasticity of demand for non-gasoline products is 0.7. Given that the average U.S. vehicle owner spends roughly \$200 to \$250 per month on gasoline compared to 0% for non-vehicle owners (see Table 2), the maximum possible expenditure wedge between vehicle and non-vehicle owners induced by the gasoline price shock, which represents the maximum possible size for the *discretionary income effect*, should be roughly (0.25)*(0.7)*(\$250) = \$43.75. Under our secondary specification, the average high gasoline spender spends roughly \$227 to \$262 per month on gasoline, compared to roughly \$30 per month for low gasoline spenders. Accordingly, a similar back-of-the-envelope calculation reaches a maximum possible size for the *discretionary income effect* of roughly (0.25)*(0.7)*(\$262 − \$30) = \$40.60. Both of these approximations are close to the point estimates reported in Table 3, which provides some reassurance that these point estimates are reasonable.

24. Another possible effect described by the authors is the *uncertainty effect*, but this would, in theory, put downward bias on our results, so it would not explain the large effect that we find.

sumers make purchases. In other words, this response might appear as a sort of investment in gasoline-related consumption, where purchases are a large discreet event but consumption is smoothed over the future.

To further examine these avenues, in the next section we decompose expenditures into product categories, and consider where gasoline savings were spent.

4.2 Decomposition of the results by detailed expenditure categories

Tables 4 and 5 report results from the specification in (1), but with spending on essential products as the dependent variable. For vehicle owners, depicted in Table 4, the interaction effect is positive, and statistically significant with a point estimate of approximately \$67 (column 1, row 3). Given that the interaction effect for all core spending was around \$103, essential goods are absorbing a substantial share of these additional expenditures. Looking at specific sub-categories, we find no evidence of a differential response to the shock for the vehicle owners in spending on food, shelter, health care, personal insurance, or utilities. In contrast, we do find evidence of a strong differential response for transportation goods, which includes automotive goods (column 4, row 3).[25] Notably, the sum of all estimated treatment effects across the essential sub-components add up to the aggregate treatment effect reported in column 1 of Table 4 (i.e. $6.47-8.04+66.17+5.65-5.06-2.56+4.17 \approx 66.81$), which is also true for all remaining Tables 5–8.

For high gasoline spenders, depicted in Table 5, the differential spending response on essential goods after the shock is not as statistically significant and the magnitude is lower than under the specification in Table 4.[26] Again, looking at specific sub-categories, we find no evidence of a positive differential response to the shock for the high gasoline spenders in spending on food, shelter, health care, personal insurance, or utilities. And again, we do find evidence of a differential response for transportation goods, although the magnitude and statistical significance are lower than under the specification in Table 4. Under both specifications, sub-category results reveal that nearly all of these additional expenditures on essential products are spent on transportation goods.[27]

The magnitude and statistical significance of the interaction term for transportation goods under both specifications is comparatively very large. This result is in line with results found in Edelstein and Kilian (2007, 2009), and seemingly indicative of the *operating cost effect* identified by these authors. Intuitively, consumers appear to have reacted to lower gasoline prices by spending more on transportation goods. The fact that these purchases tend to be bulky and are consumed over a longer horizon could explain why the magnitude of our coefficient estimates in row 3 of Table 3 are so large. In fact, if we net out the estimates for transportation products, then the coefficient estimates in row 3 of Table 3 are reduced to roughly \$37 (\$103-\$66) and \$45 (\$92-\$47) for the main specification and secondary specification, respectively. These figures are remarkably close to our estimated treatment effects in columns 3 and 4 of Table 3, which represents the maximum potential

25. Sub-components included in transportation goods are automotive goods (new and used), vehicle financing, maintenance and repair, insurance, rental, and public transportation. In separate unreported results, we found that over 75% of the estimated treatment effect for transportation goods is due to additional spending on automotive goods.

26. Note that the magnitude of the interaction coefficient for core expenditures is slightly smaller under this specification than under our main specification (\$92 compared to \$103), so it is not surprising that the magnitude for sub-categories is also smaller.

27. We also find, under both specifications, that baby care spending increased significantly for the treatment group after the shock. This category includes spending for babysitting, and other expenses for day care centers and nursery schools. CPI inflation for child care and nursery schools was especially strong in 2015, which might have affected these groups more than their control counterparts. Since this effect is small and likely not related to lower oil prices, it is not warranted to put emphasis on it.

Table 4: Detailed Regressions for Essential Core Spending and its Sub-Components with Vehicle Ownership Specification

	Essential	Food	Shelter	Transport
Shock	12.58	4.302	47.78*	–7.024
	(47.18)	(5.712)	(18.73)	(40.41)
Treated	351.1***	30.20***	–97.01***	286.1***
	(12.93)	(2.611)	(7.556)	(9.066)
Treated × Shock	66.81***	6.465	–8.042	66.17***
	(17.45)	(3.424)	(10.80)	(11.82)
Control Vars.	X	X	X	X
Obs.	189,538	189,538	189,538	189,538
Adj. R-squared	0.145	0.275	0.179	0.021

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1). Column 1 reports results with essential spending as dependent variable. Definition of essential spending is described in section 2. Columns 2–4 report results where dependent variable is spending on food, shelter, and transportation, respectively. All columns report results where "treated" group is households that report owning a vehicle. Control variables include the same set included in results reported in Table 3. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p < 0.01$, $p < 0.05$, and $p < 0.001$, respectively.

Table 4: (Continued) Detailed Regressions for Essential Core Spending and its Sub-Components with Vehicle Ownership Specification

	Baby care	Health care	Pers. insur.	Utilities
Shock	–4.592	–10.15	–3.296	–14.44***
	(3.697)	(6.846)	(3.457)	(4.077)
Treated	–8.449***	51.27***	6.275***	82.65***
	(1.224)	(2.351)	(1.153)	(1.786)
Treated × Shock	5.651***	–5.055	–2.555	4.173
	(1.555)	(3.189)	(1.506)	(2.446)
Control Vars.	X	X	X	X
Obs.	189,538	189,538	189,538	189,538
Adj. R-squared	0.055	0.027	0.008	0.309

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1). Columns 1–4 report results where dependent variable is spending on baby care, health care, personal insurance, and utilities, respectively. All columns report results where "treated" group is households that report owning a vehicle. Control variables include the same set included in results reported in Table 3. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p < 0.05$, $p < 0.01$, and $p < 0.001$, respectively.

size for the *discretionary income effect* and is consistent with the MPC estimate of 1 that is found by Gelman et al. (2016). Moreover, the difference between the point estimates for total essential products and transportation products is essentially zero under both specifications, which suggests that the response in consumption of essential products excluding transportation due to the oil price shock was negligible.

Tables 6 and 7 report results from the specification in (1), but with spending on non-essential products as the dependent variable. Again, overall spending on these products increased disproportionately for vehicle owners after the gasoline price shock (Table 6: column 1, row 3), although the magnitude is less than that for transportation goods in Table 6. Across different spending categories, we find a positive and significant differential response for vehicle owners after the shock for spending on alcohol, apparel, entertainment, household expenses, and food away from home. We find no evidence of a positive response for spending on books, appliances, or miscellaneous products.

Under our secondary specification, reported in Table 7, the estimated treatment effect for overall non-essential goods (column 1, row 3) is, again, positive and statistically significant. Across

Table 5: Detailed Regressions for Essential Core Spending and its Sub-Components with High Gasoline Spenders Specification

	Essential	Food	Shelter	Transport
Shock	60.91	9.419	55.05**	29.77
	(47.98)	(5.197)	(17.63)	(41.84)
Treated	340.4***	46.83***	–47.55***	238.1***
	(14.95)	(2.027)	(6.317)	(12.27)
Treated × Shock	40.76*	4.638	–22.00*	47.21**
	(20.62)	(2.604)	(9.948)	(16.59)
Control Vars.	X	X	X	X
Obs.	189,538	189,538	189,538	189,538
Adj. R-squared	0.146	0.277	0.179	0.021

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1). Column 1 reports results with essential spending as dependent variable. Definition of essential spending is described in section 2. Columns 2–4 report results where dependent variable is spending on food, shelter, and transportation, respectively. All columns report results where "treated" group is households above the 20th percentile in the gasoline expenditure share distribution. Control variables include the same set included in results reported in Table 3. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

Table 5: (Continued) Detailed Regressions for Essential Core Spending and its Sub-Components with High Gasoline Spenders Specification

	Babycare	Healthcare	Pers.insur.	Utilities
Shock	–5.325	–14.72*	–5.533	–7.746*
	(3.613)	(6.802)	(3.654)	(3.726)
Treated	–12.18***	35.54***	6.387***	73.20***
	(1.257)	(2.706)	(1.462)	(1.491)
Treated × Shock	6.776***	2.389	0.374	1.371
	(1.506)	(3.518)	(1.804)	(1.914)
Control Vars.	X	X	X	X
Obs.	189,538	189,538	189,538	189,538
Adj. R-squared	0.055	0.027	0.009	0.314

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1). Columns 1–4 report results where dependent variable is spending on baby care, health care, personal insurance and utilities, respectively. All columns report results where "treated" group is households above the 20th percentile in the gasoline expenditure share distribution. Control variables include the same set included in results reported in Table 3. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

sub-categories, the treatment effect for high gasoline spenders is positive and significant for spending on alcohol, apparel, entertainment, and food away from home, and either negative or statistically insignificant for spending on books, appliances, household expenses, and miscellaneous products.

Overall, results in Tables 4–7 suggest that, under both specifications, the 2014–2015 oil price shock induced additional spending on transportation goods, alcohol, apparel, entertainment, and food away from home. How do these results compare to results from other studies? Edelstein and Kilian (2007) and Farrell and Grieg (2015) provide detailed studies on U.S. responses in consumption due to gasoline price shocks. Edelstein and Kilian (2007) also find particularly large positive responses for transportation-related goods, including motor vehicles and parts, pleasure boats, pleasure air crafts, and recreational vehicles.[28] Beyond transportation products, they find negligible evidence of significant responses for other durable goods, which is consistent with what we

28. Since Farrell and Grieg (2015) rely on evidence from credit card data, they are unable to capture spending on most durable goods, including transportation-related goods.

Table 6: Detailed Regressions for Non-Essential Core Spending and its Sub-Components with Vehicle Ownership Specification

	Non-essential	Alcohol	Apparel	Entertain	Books
Shock	153.6***	1.692	42.42***	49.25***	2.104**
	(33.33)	(1.712)	(5.339)	(8.089)	(0.764)
Treated	210.9***	4.712***	–6.683***	46.77***	4.202***
	(8.513)	(0.632)	(1.916)	(2.271)	(0.227)
Treated × Shock	36.55**	1.852*	8.774***	10.68**	–1.536***
	(11.91)	(0.923)	(2.183)	(3.617)	(0.330)
Control Vars.	X	X	X	X	X
Obs.	189,538	189,538	189,538	189,538	189,538
Adj. R-squared	0.147	0.097	0.028	0.042	0.029

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1). Column 1 reports results with non-essential spending as dependent variable. Definition of non-essential spending is described in section 2. Columns 2–5 report results where dependent variable is spending on alcohol, apparel, entertainment, and books, respectively. All columns report results where "treated" group is households that report owning a vehicle. Control variables include the same set included in results reported in Table 3. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

Table 6: (Continued) Detailed Regressions for Non-Essential Core Spending and its Sub-Components with Vehicle Ownership Specification

	Appliances	Household expenses	Food away from home	Misc.
Shock	6.649*	0.749	4.020	52.35*
	(2.879)	(13.64)	(5.656)	(23.90)
Treated	6.248***	21.78***	32.71***	103.3***
	(0.751)	(4.188)	(2.655)	(4.630)
Treated × Shock	0.348	12.97*	11.58***	–9.049
	(1.236)	(5.315)	(3.343)	(7.416)
Control Vars.	X	X	X	X
Obs.	189,538	189,538	189,538	189,538
Adj. R-squared	0.006	0.033	0.153	0.037

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1). Columns 1–4 report results where dependent variable is spending on appliances, household expenses, food away from home, and miscellaneous products, respectively. All columns report results where "treated" group is households that report owning a vehicle. Control variables include the same set included in results reported in Table 3. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

find. Both Edelstein and Kilian (2007) and Farrell and Grieg (2015) find significant and persistent increased spending on food in restaurants and apparel/department store items, which is consistent with our findings in relation to food away from home and apparel. For entertainment, our results are somewhat out of line with those from Edelstein and Kilian (2007), who find no evidence of significant response, but consistent with results from Farrell and Grieg (2015) who also find that consumers spent some proportion of gasoline savings on entertainment.[29]

Overall, our findings lend support for the importance of both the *discretionary income effect* and the *operating cost effect* in transmitting the 2014–2015 oil price shock to the U.S. economy.

29. Edelstein and Kilian (2007) find evidence of decreased spending on alcohol at home, but increased spending on alcohol away from home, in response to lower gasoline prices. Since our measure of spending on alcohol does not specify where consumption takes place, our results are difficult to compare. Farrell and Grieg (2015) do not report evidence for spending on alcohol.

Table 7: Detailed Regressions for Non-Essential Core Spending and its Sub-Components with High Gasoline Spenders Specification

	Non-essential	Alcohol	Apparel	Entertain	Books
Shock	159.2***	1.892	42.56***	52.86***	2.394**
	(35.90)	(1.607)	(5.212)	(8.512)	(0.756)
Treated	200.8***	4.318***	0.608	36.94***	3.320***
	(10.45)	(0.550)	(1.740)	(4.179)	(0.232)
Treated × Shock	50.88***	2.260**	10.45***	10.56*	–1.988***
	(12.88)	(0.752)	(2.027)	(4.919)	(0.314)
Control Vars.	X	X	X	X	X
Obs.	189,538	189,538	189,538	189,538	189,538
Adj. R-squared	0.148	0.097	0.028	0.042	0.029

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1). Column 1 reports results with non-essential spending as dependent variable. Definition of non-essential spending is described in section 2. Columns 2–5 report results where dependent variable is spending on alcohol, apparel, entertainment, and books, respectively. All columns report results where "treated" group is households above the 20th percentile in the gasoline expenditure share distribution. Control variables include the same set included in results reported in Table 3. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

Table 7: (Continued) Detailed Regressions for Non-Essential Core Spending and its Sub-Components with High Gasoline Spenders Specification

	Appliances	Household expenses	Food away from home	Misc.
Shock	8.576**	10.16	8.623	39.35
	(2.957)	(13.18)	(5.068)	(27.70)
Treated	6.337***	25.83***	44.79***	80.04***
	(0.916)	(3.482)	(1.935)	(7.535)
Treated × Shock	–1.665	5.007	11.21***	11.91
	(1.294)	(4.573)	(2.477)	(9.192)
Control Vars.	X	X	X	X
Obs.	189,538	189,538	189,538	189,538
Adj. R-squared	0.006	0.033	0.155	0.037

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1). Columns 1–4 report results where dependent variable is spending on appliances, household expenses, food away from home, and miscellaneous products, respectively. All columns report results where "treated" group is households above the 20th percentile in the gasoline expenditure share distribution. Control variables include the same set included in results reported in Table 3. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

4.3 Decomposition of the results across household types

Another benefit of the CE data is that we can examine spending responses across different household types that might be of interest to economists and policy makers. In this section, we focus specifically on the importance of urban versus rural residence, oil-producing versus non-oil-producing state residence, and mortgage ownership for the determination of our results.[30] Considering urban and rural residents separately, it might be expected that rural residents, who are generally more dependent on motor vehicles than urban residents, would benefit relatively more from a decline in gasoline prices. Indeed, within our sample, rural residents spent 6.4% of their total expenditures on gasoline, on average, compared to 4.6% for urban residents.

30. We also examined differences across income quintiles and household sizes, but found little evidence of robust patterns across our two specifications.

Table 8 presents results that are consistent with this intuition. Looking at results from estimating equation (1) for urban and rural residents separately, the coefficient on the interaction term—which captures the impact of the treatment according to our model—suggests that treated urban residents who owned a vehicle spent roughly $92 per month more in the period after the gasoline price shock, relative to the control group. In contrast, rural residents spent over three times this much, roughly $334 per month, according to our results. Under the secondary specification, where the treatment group is households above the 20th percentile in the gasoline expenditure share distribution, we find similar results. The impact of the treatment is roughly $83 per month for urban residents, but $189 per month for rural residents. Overall, these results suggest that rural residents increased spending more in response to the gasoline price shock than urban residents, perhaps because the rural treatment group tends to be more reliant on gasoline than the urban treatment group.[31]

Table 8: Separate Regressions for Core Spending for Urban and Rural Populations

	Vehicle Ownership		Gasoline Reliance	
	Urban	Rural	Urban	Rural
Shock	180.8**	–101.2	229.2***	114.0
	(64.79)	(195.7)	(67.12)	(186.5)
Treated	568.0***	464.5***	534.6***	630.6***
	(17.22)	(71.56)	(20.42)	(57.47)
Treated × Shock	91.63***	334.1***	82.85**	189.2*
	(23.21)	(96.53)	(26.84)	(84.82)
Control Vars.	X	X	X	X
Obs.	177,873	11,665	177,873	11,665
Adj. R-squared	0.213	0.122	0.214	0.129

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1) where dependent variable is core spending as defined in section 2. Columns 1 and 2 report results where "treated" group is households that report owning a vehicle. Columns 3 and 4 report results where "treated" group is households above the 20th percentile in the gasoline on population of rural residents. Control variables include the same set included in results reported in Table 3. All regressions expenditure share include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

Looking separately at oil-producing and non-oil-producing state dwellers might also be informative. As described in Baumeister and Kilian (2016), oil-related investment and production declined significantly in response to the 2014–2015 oil price shock, and hence one might expect that households living in oil-producing states would experience losses in future income and/or wealth that would lead to lower consumption propensity after the shock.[32] To test this theory, we ran our main core spending regressions separately for households that reported living inside and outside of oil-producing states.[33] Results are reported in Table 9. According our main specification, vehicle owners living in oil-producing states did not significantly increase their spending after the shock,

31. Note that the standard error from the rural regression is very large under both specifications, hence the precision of these results is lower for the rural groups than the urban groups. This is in large part because our sample size is much larger for the urban than the rural group. Note, meanwhile, that all results are statistically significant at the 5% level.

32. Although these income and wealth effects should impact the control group as well as the treatment group, our specification is suited to test whether the treatment group spent their discretionary income gains from lower oil prices. Hence, a insignificant result suggests that these households saved rather than spent their income gains.

33. Our set of oil-producing states, chosen based on a ranking of primary energy production per capita derived from data provided by the U.S. Energy Information Administration (EIA), includes Alaska, Colorado, Louisiana, Oklahoma, Pennsylvania, Texas, and Utah. Unfortunately, there are several other oil-producing states, including Wyoming and North Dakota, for which the CE does not collect data, hence these are not included. We consider all other states reported in the CE data as "non-oil-producing". Households that do not report their state of residence are excluded.

while vehicle owners that lived in non-oil-producing states increased their spending by almost $140 per month. Similarly, according to our secondary specification results, gasoline-reliant households living in oil-producing states did not significantly increase their spending after the shock, while gasoline-reliant households living in non-oil-producing states increased spending by roughly $144 per month. These results suggest that, for the typical household living in an oil-producing state, the propensity to increase spending due to gasoline price savings after the 2014–2015 oil price shock was countered by the propensity to save gasoline savings due to losses in income and/or wealth.[34]

Table 9: Separate Regressions for Core Spending for Populations Living In and Outside of Oil-Producing States

	Vehicle Ownership		Gasoline Reliance	
	Oil state	Non-oil state	Oil state	Non-oil state
Shock	81.29	318.9***	90.42	364.1***
	(0.67)	(3.93)	(0.74)	(4.31)
Treated	759.0***	535.0***	637.0***	520.0***
	(21.05)	(25.11)	(14.99)	(20.76)
Treated × Shock	1.120	139.5***	29.87	144.3***
	(0.02)	(4.86)	(0.52)	(4.46)
Control variables	X	X	X	X
Obs.	35,720	118,606	35,720	118,606
Adj. R-squared	0.207	0.217	0.209	0.218

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1) where dependent variable is core spending as defined in section 2. Columns 1 and 2 report results where "treated" group is households that report owning a vehicle. Columns 3 and 4 report results where "treated" group is households above the 20th percentile in the gasoline expenditure share distribution. Columns 1 and 3 report results based on population residing in oil-producing states. Columns 2 and 4 report results based on population not residing in oil-producing states. Control variables include the same set included in results reported in Table 3. All regressions include month-year fixed effects. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

Another criterion that is interesting to consider is household mortgage status. Kaplan et al. (2014) document that U.S. households that own primarily illiquid assets, such as housing, tend to have a high marginal propensity to consume out of transitory income shocks, and hence operate in a similar hand-to-mouth fashion as households at the bottom end of the wealth distribution. Cloyne et al. (2015) present evidence, based on CE data, that U.S. households that own mortgages react to monetary policy shocks by increasing consumption, whereas homeowners without mortgage debt are much less responsive. This evidence is interpreted to reflect hand-to-mouth behavior on the part of mortgage owners, as consistent with the evidence from Kaplan et al. (2014).

To test the role of these mechanisms for responses to the 2014–2015 oil price shock, we separately estimate equation (1) for mortgage holders and non-mortgage holders. Results are reported in Table 10. Interestingly, our results suggest that non-mortgage holders increased their spending significantly in response to the treatment, whereas the response among mortgage holders was muted: the estimated response among non-mortgage holders is roughly $91, whereas the response among mortgage holders is not statistically distinguishable from zero. Similarly, under our secondary specification, the estimated response for non-mortgage holders is, again, roughly $91,

34. Results related to oil and non-oil states, while intuitive, come with several caveats. Population weights provided in the CE data are intended to be representative of the national population, not state populations, hence regression results at the state level are not accurately representative. In addition, a sizable proportion of oil and gas royalties flow out of major oil-producing states (Fitzgerald and Rucker, 2016), hence some of the income and wealth losses from the 2014–2015 oil price shock would have been experienced outside of these states.

Table 10: Separate Regressions for Core Spending for Populations With and Without Mortgages

	Vehicle Ownership		Gasoline Reliance	
	With mortgage	W/o mortgage	With mortgage	W/o mortgage
Shock	350.3*	109.4	398.2**	156.2*
	(148.0)	(74.07)	(121.7)	(76.93)
Treated	732.7***	553.5***	647.8***	527.4***
	(74.03)	(17.69)	(55.50)	(21.11)
Treated × Shock	25.90	91.01***	10.73	91.36**
	(109.0)	(24.24)	(73.02)	(28.14)
Control variables	X	X	X	X
Obs.	68,950	120,588	68,950	120,588
Adj. R-squared	0.165	0.174	0.166	0.176

Source: Derived by authors from CE micro data. Notes: This table reports results from estimation of equation (1) where dependent variable is core spending as defined in section 2. Columns 1 and 2 report results where "treated" group is households that report owning a vehicle. Columns 3 and 4 report results where "treated" group is households above the 20th percentile in the gasoline expenditure share distribution. Columns 1 and 3 report results based on population of mortgage holders. Columns 2 and 4 report results based on population of non-mortgage holders. All regressions include month-year fixed effects. Control variables include the same set included in results reported in Table 3. Robust standard errors are reported in brackets. *, **, *** indicate $p<0.05$, $p<0.01$, and $p<0.001$, respectively.

while the corresponding estimate for mortgage holders is, again, not statistically distinguishable from zero.

These results might be explained by several factors. First, if households regard oil price changes as permanent rather than transitory, then we should expect households that do not own homes with mortgage debt to increase consumption as much as households that are hand-to-mouth mortgage holders. Indeed, Gelman et al. (2016) find evidence that owning a mortgage did not increase the propensity of households to spend out of savings from lower gasoline prices in 2014–2015. Our finding that non-mortgage holders increased spending more than mortgage holders could be on account of mortgage holders paying back mortgage debt rather than spending out of gasoline savings. Indeed, this type of behavior is supported by evidence from Di Maggio et al. (2017) for household responses to mortgage rate adjustments.

We note that these results should also be considered with some degree of caution. Although 39% of vehicle owners in our sample reported owning a mortgage, only 5% of non-mortgage holders reported owning a vehicle, hence our results in Table 10 rely on relatively few observations. Nevertheless, these results might indeed be accurate, and suggest, if nothing else, that the MPC out of gasoline savings among mortgage holders was not higher than that among non-mortgage holders during the 2014–2015 episode.

5. CONCLUSION

In this paper, we examine the impact of the 2014–2015 decline in global oil prices on U.S. consumer spending. We use a difference-in-difference identification strategy, comparing spending responses of vehicle owners to non-vehicle owners, and also spending responses of high gasoline spenders to low gasoline spenders. We interpret the difference in these responses as the direct impact of the oil price shock on consumer spending.

Our results reveal that spending for vehicle owners and high gasoline spenders grew significantly more than spending for control groups, which suggests that the 2014–2015 oil price shock led to significant growth in U.S. household consumption. In terms of magnitude, our findings suggest that the average marginal propensity to consume out of gasoline price savings was above 1, driven by disproportionate growth in lumpy spending on transportation products and non-essential products.

These findings suggest that the demand channel remains important for the transmission of oil price shocks to the U.S. economy.

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