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Essays in Policy

By

KELSEY FORTUNE
DISSERTATION

Submitted in partial satisfaction of the requirements for the degree of

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Acknowledgments

I am immensely grateful for the support system that made this dissertation possible.

The barriers were great.

We are greater.

Abstract

I explore the effect of two distinct types of marijuana legislation, medical or recreational legalization, on alcohol and over-the-counter drug purchases. Using the Nielsen Scanner Data and a differences-in-differences approach, I find significant differences in the way marijuana interacts with other markets depending on the type of legislation. Alcohol purchases decreased by an estimated 11.8% with medical marijuana legalization and increased by 38.9% with recreational legalization.

For the remainder, I focus on the largest clean transportation program in California, the Clean Vehicle Rebate Project. The issue of equity in clean energy incentive programs led to the implementation of means testing the this program in 2016. Analysis highlights the importance of modeling choices and concludes that means testing led to increased electric vehicle adoption by low income households, estimating demand elasticity of -6.8 and identifying important interactions between equity and environmental impacts.

Finally, using linear probability and Probit models, I find that those purchasing more expensive vehicles and buyers living in areas with a higher proportion of low income households were less likely to apply for the California Clean Vehicle Rebate Project. Buyers living in the same zip code as the dealer where they purchased the vehicle all applied for a rebate. Purchasing an electric vehicle from a dealer that is larger or further away from the buyers registration location both were correlated with higher likelihood of applying for the California Clean Vehicle Rebate Project. These results are then put into the context of policy implications.

Contents

1	Acknowledgments	ii
2	Abstract	iii
3	List of Figures	vi
4	List of Tables	vi
5	Marijuana Laws and the Consumption of Other Goods	1
5.1	Introduction	2
5.2	Literature Review	6
5.3	Data	7
5.4	Methods	9
5.5	Results	11
5.6	Conclusion	14
5.7	Appendix A	18
5.8	Appendix B	19
5.9	Tables and Figures	20
6	Equity in Clean Energy Rebates: Evidence from California’s Clean Vehicle Re- bate Project	34
6.1	Introduction	35
6.2	Program Details	38
6.3	Data	40
6.4	Methods	43
6.5	Results	45
6.5.1	Subgroup and Additional Analysis	47
6.5.2	Demand Elasticities	49
6.5.3	Robustness Checks	50
6.6	Policy Evaluation and Implications	51
6.6.1	Increased Equity	52
6.6.2	Environmental and Equity Interactions	53
6.7	Conclusion	55
6.8	Appendix C: Other Specifications	59
6.9	Tables and Figures	62
7	Free Money: When Subsidy Take-Up is Less than 100%	81
7.1	Introduction	82
7.2	Data	86
7.3	Methods	90
7.4	Results	91
7.5	Conclusion	95
7.6	Appendix D: Missing Experian Data in Early 2016	100

7.7 Tables and Figures 101

List of Figures

1	Alcohol Sales in California	20
2	Identifying Variation	20
3	Cumulative EV Transactions	62
4	Cumulative New EVs: US and CA	63
5	New PHEV Transactions by Make	63
6	New BEV Transactions by Make	64
7	Geographic Dispersion of EV Transactions (through 2015)	65
8	Number of CVRP Applications Over Time	66
9	Borenstein and Davis	67
10	CVRP Funding Concentration Curve	68
11	Air Quality (Left) and Low Income Rebates (Right)	69
12	Application Data by Presence in DMV Data	101
13	DMV Data by Presence of CVRP Application	102
14	Categories of Matching by Make	103
15	Presence of Applications Before Means Testing	103
16	Presence of Applications After Means Testing	103
17	Time to Application	104
18	Time to Application by Make	104
19	Time to Application over Time	104
20	Application and Vehicle Price Data and Linear Fit	105
21	Application and Zip Code Median Income Data and Linear Fit	105
22	Application and Zip Code Proportion of the Population under 300% of the Federal Poverty Guideline Data and Linear Fit	106
23	Application and Dealer Size Data and Linear Fit	106
24	Application and Dealer Distance from Buyer Data and Linear Fit	107
25	Vehicles with Applications but no Registration	108
26	Matched Vehicles by Application Month	108
27	Histogram of the Distance between Dealer and Buyer Zip Code (km)	109
28	Histogram of the Size of Dealers by EV Sales	110

List of Tables

1	Module Categories	21
2	Summary Statistics	22
3	Log Alcohol Quantities	22
4	Log Alcohol Category Quantities	23
5	Log Medication Quantities	24
6	Log Alcohol Revenue	25
7	Log Alcohol Category Revenue	26
8	Log Medication Revenue	27
9	Log Alcohol Quantities	28
10	Log Alcohol Category Quantities	29

11	Log Medication Quantities	30
12	Log Alcohol Revenue	31
13	Log Alcohol Category Revenue	32
14	Log Medication Revenue	33
15	CVRP Rebate Amounts	70
16	Maximum Income to Qualify for Standard Credit	71
17	Maximum Income to Qualify for Low Income Credit	71
18	EV Data Tabulation	72
19	Monthly Data Tabulation	72
20	Data Summary for Zip Codes By PPLI _{300%} <i>Quartile</i>	73
21	Main Regressions for all EVs	74
22	Regressions with all EVs and Finer Income Categories	75
23	Regressions with EVs by Price Quartile	76
24	Regressions by EV and Transaction Types	77
25	Elasticity Estimates for Low Income Group	78
26	Donut Regressions	79
27	Event Study	80
28	CVRP Rebate Amounts	111
29	Matching by VIN	112
30	Summary Statistics	113
31	Linear Probability Models	114
32	Probit Models	115
33	Conditional Marginal Effects	116
34	Linear Probability Models through March 2016	117
35	Probit Models through March 2016	118

Marijuana Laws and the Consumption of Other Goods

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Abstract

This paper explores the effect of two distinct types of marijuana legislation, medical or recreational legalization, on alcohol and over-the-counter drug purchases. Using the Nielsen Scanner Data and a differences-in-differences approach, I find significant differences in the way marijuana interacts with other markets depending on the type of legislation. Alcohol purchases decreased by an estimated 11.8% with medical marijuana legalization and increased by 38.9% with recreational legalization. Pain medication purchases appear to be less effected by marijuana policy. However, I were surprised to see no significant decreases in purchases and even a weakly significant increase under recreational legalization.

Researcher(s)' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

5.1 Introduction

This paper will investigate the effect of marijuana legislation on the sales of alcohol and over the counter medication. We hypothesize that medical and recreational marijuana legalization will decrease sales of pain medication and have different effects on the sale of alcohol, as they target different populations. These substitutabilities and complementarities are important to evaluating the cost and benefits of legislation. Using the “Neilson Scanner Data” and difference-in-difference methods to evaluate these policies, we find little effect on sales of over the counter medication and that marijuana acts as a substitute for alcohol, specifically liquor, under medical marijuana legalization and a complement under recreational marijuana legalization.

Discussions to legalize marijuana in the US continue to intensify and have reached a critical inflection point. In November 2016, four states passed a recreational marijuana law (RML), joining four other states to have legalized the use, sale, and consumption of recreational marijuana. Lawmakers in 17 states have introduced measures to legalize recreational marijuana in 2017 alone. Additionally, over half of the states in the US have already passed some form of a medical marijuana law (MML), whereby patients can receive marijuana with a recommendation or prescription from a doctor. The merits for states to legalize marijuana are ample. The medical community has found conclusive and substantial evidence that cannabis can help patients with chronic pain.¹ RMLs can be huge generators of tax revenue for states; in 2016, both Colorado and Washington brought in over \$200 million each in tax revenue from RMLs.² Proponents also argue that marijuana legalization reduces the costs for law enforcement and criminal justice systems associated with investigating crimes related to marijuana.

Despite the burgeoning amount of support for marijuana legalization, policymakers remain in serious deliberation about the potential drawbacks of legalizing weed. For

¹<http://www.latimes.com/nation/la-na-legal-marijuana-future-2017-story.html>

²<http://www.latimes.com/nation/la-na-legal-marijuana-legislation-2017-story.html>

instance, since the Rohrabacher-Farr amendment passed in 2003, the Justice Department has been prohibited from using federal funds to prevent states from implementing their own MMLs; now, however, current US Attorney General Jeff Sessions is seeking to revoke these protections so that medical marijuana providers can be federally prosecuted.³ It is thus of severe importance that researchers and policymakers continue to develop an understanding of the potential costs and benefits of legalizing marijuana.

In this paper, we present causal evidence on how medical and recreation marijuana laws in the US influenced the purchases of other goods - namely, the purchases of over-the-counter (OTC) drugs and alcohol. Understanding how marijuana legalization influences the consumption of other goods is particularly important given past concern that MMLs may impact outcomes such as drug and alcohol abuse (Wen, Hockenberry and Cummings, 2015) and driving under the influence (Anderson, Hansen and Rees, 2013). Since marijuana is seen as a potential remedy for pain, one may expect cannabis and OTC medicine to act as substitutes; an exogenous positive shock to the supply of marijuana would lead to a decrease in demand for OTC drugs. Similarly, given the belief alcohol can serve as a painkiller,⁴ marijuana legalization could reduce the consumption of alcohol. On the other hand, drugs and alcohol could potentially be complements in consumption with marijuana, particularly since all three are oftentimes consumed in leisure and consumers may extract additional utility through joint⁵ consumption.⁶ In summary, we hypothesize that any type of marijuana legalization will decrease OTC purchases, and alcohol purchases will decrease as a result of MMLs and increase as a result of RMLs.

To investigate the causal impact of marijuana legalization on the purchases of OTC drugs and alcohol, we utilize state-month variation in the adoption of MMLs and RMLs across the US, paired with the Nielsen scanner data. This data include weekly sales at

³https://www.washingtonpost.com/news/wonk/wp/2017/06/13/jeff-sessions-personally-asked-congress-to-let-him-prosecute-medical-marijuana-providers/?utm_term=.3eaff50703d5

⁴<https://www.ncbi.nlm.nih.gov/pubmed/27919773>

⁵Pun intended.

⁶Of course, a substitution effect could occur in consumption due to leisure as well.

the barcode level spanning over 35,000 stores and approximately 90 retail chains in the US, and cover over 50% of the total volume of sales in US grocery stores. In our primary specifications, we collapse this data to the state-month-module level to estimate models with store, month, and module fixed effects,⁷ which control for a variety of confounding variables that potentially correlate with the location and timing of marijuana legalization and the level of alcohol or OTC drug purchases.

Results from these regressions support our hypothesis that MMLs and RMLs effect consumer purchases of alcohol differently. We find that that while MMLs appear to have small, insignificant negative effects on most consumption, RMLs appear to have a significant positive effect on alcohol consumption. Additionally, we break up alcohol consumption into liquor, beer, and wine. We hypothesize that negative effect of MMLs is coming from the substitutability of liquor and medical marijuana as self medication for pain. There is indeed a significant negative effect of MMLs on liquor purchases. Whereas the effect on beer and wine is a precise zero. When we parse through this break down for RMLs, we find the effect also manifesting primarily in liquor purchases. The sign of these estimates suggest that alcohol, specifically liquor, and marijuana act as complements in states with RMLs. While our estimates are not statistically distinguishable from zero, they are significantly different from the estimated effects of MMLs. We attribute the lack of statistical significance to the fact that MMLs are acting through a smaller portion of the population.

In addition to estimating differential effects on alcohol purchasing, we explore the relationship between this legislation and OTC medication purchases. We group these modules into three categories: pain medication, contraception, and other medication. We expect to only see a negative relationship between marijuana legislation and pain medication in both situations. While analysis confirms this hypothesis with no effect on

⁷“Module” is the finest level of grouping of barcodes provided by Nielsen. The Nielsen data in its entirety contain approximately 1,200 modules, while our sample of OTC medicine and drugs covers a total of 40 modules. Examples of modules include “Vodka” and “Rum” (see Table 1).

contraception or other medication, our results are inconsistent with our hypothesis that marijuana legalization will categorically decrease pain medication purchases. We find neither type of legislation to have a negative effect on pain medication purchases, and, in fact, there is some evidence that RMLs may increase pain medication purchases.

This paper makes several important contributions to the literature. To our knowledge, it is the first paper to observe the direct consumption response of OTC drugs to marijuana legalization. While previous studies have investigated responses in alcohol consumption, they have relied entirely on self-reported behavior via surveys. Not only do these surveys typically suffer from relatively small sample sizes, but there may also be major concerns of a “social desirability” bias, where survey-takers tend to give responses that are generally viewed favorably (Grimm, 2010). The potential for this bias could be especially prominent in this setting given the stigmatic natures of marijuana and alcohol consumption. While the direction of this bias is theoretically ambiguous, it could plausibly be a driver behind the highly variant and mixed results found from previous studies (see Background section). Moreover, given the vast spread and high volume of purchases covered by the Nielsen data, this study can precisely estimate even small responses to changes in consumption. Lastly, with the exception of a working papers from Dragone et al. (2017) and Hao and Cowan (2017), this paper is the first to investigate the impacts of RMLs, laws of which have gained the most popularity and discussion recently; given RMLs and MMLs affect very different people, it is likely not the case that results for responses to MMLs can be directly extrapolated to RMLs.

The paper continues as follows: section 2 provides an overview of the current state of the relevant literature, section 3 presents our data, section 4 describes the identified variation and methods on which we rely, section 5 presents and interprets our results, and section 6 concludes.

5.2 Literature Review

The legalization of marijuana has increasingly received traction across the United States. Since 1996, 29 states and Washington D.C. have passed some form of a medical marijuana law (MML), whereby patients can receive marijuana with a recommendation or prescription from a doctor. In just the past five years, eight states and Washington D.C. have approved a recreational marijuana law (RML) where residents can attain marijuana without a doctor's note. Given RMLs are relatively new in the US, the majority of prior studies have focused on the introduction of MMLs to investigate the impacts of marijuana legalization on various outcomes.

The prior literature generally finds that MMLs increased the consumption of marijuana (Anderson, Hansen and Rees, 2013; Pacula et al., 2015; Wen, Hockenberry and Cummings, 2015). The literature also suggests that MMLs may have increased consumption among individuals who were not prescribed the marijuana. For example, Anderson, Hansen and Rees (2013) found a reduction in the street price of marijuana in response to the introduction of a MML, while Chu (2014) found that MMLs led to increases in marijuana possession arrests. With respect to adolescents, however, studies mostly find that MMLs did not increase marijuana consumption (Lynne-Landsman, Livingston and Wagenaar, 2013; Anderson, Hansen and Rees, 2015).

Given the strong evidence of an impact of MMLs on marijuana consumption, studies have utilized a variety of surveys to investigate associations between MMLs and the consumption of other goods. MMLs have been found to have a negative impact on cigarette consumption (Choi, Dave and Sabia, 2016) and no impact on the consumption of hard drugs (Wen, Hockenberry and Cummings, 2015; Chu, 2015), while the evidence on alcohol consumption has been mixed. For instance, Anderson, Hansen and Rees (2013) found reductions in alcohol consumption in response to the introduction of MMLs, while Wen, Hockenberry and Cummings (2015) found increased drinking among those aged 21 and

older, with no effect on drinking for those under 20. Other studies have utilized variation in the availability/price of alcohol to investigate the substitutability/complementarity of alcohol and marijuana, and these studies are generally mixed as well (Williams et al., 2004; Crost and Guerrero, 2012; Crost and Rees, 2013).

Finally, a handful of studies have investigated the impact of MMLs on other outcomes through which the substitutabilities or complementarities in the consumption of marijuana and other goods would be the driving forces. For instance, studies have found that MMLs led to a reduction in traffic fatalities and drunk driving (Anderson, Hansen and Rees, 2013), suicides (Anderson, Hansen and Rees, 2015), and absences due to sickness (Ullman, 2017), with no impact on crime (Brinkman and Mok-Lamme, 2017; Adda, McConnell and Rasul, 2014; Morris et al., 2014).

5.3 Data

The data used in this paper are commonly referred to as the “Nielsen Retail Scanner Data”, which was obtained from the Kilts Marketing Data Center at the Booth School of Business at the University of Chicago. The raw files contain, among other variables, the total number of weekly sales from 2006 to 2015 across participating stores at the Universal Product Code (UPC) level (i.e. barcode level). Approximately 90 retail chains are contained in the data, spanning over 35,000 stores across the 48 continental United States and Washington D.C. The data contain over 2.5 million UPCs, each of which are grouped into one of several possible categories of classification called *modules*. Modules, in turn, are lumped into *groups*, and finally, groups are bunched together to form one of ten possible *departments*. For instance, “Alcoholic Beverages” is one of the ten departments, which contains three groups: “Beer”, “Liquor”, and “Wine”. Liquor contains 14 modules, including “Vodka”, “Rum”, and “Tequila”. Our primary analysis collapses the raw data (store-week-UPC level) and calculates the total quantity sold at the state-month-module level. Modules considered in this paper are described in Table 1, and the summary statistics

for these state-month-module observations can be found in Table 2. Additional analysis considers store level aggregation instead of state level, with corresponding store fixed effects. We supplement these data with state level covariates. We can control for the proportion of the state population currently in school, the proportion under the age of 25, the proportion of high school dropouts, and the proportion of college graduates. Our results, however, are robust to including these state level controls and to the level of data aggregation.

These data are not perfect. Drawbacks include the clear trends and cyclicity (e.g. Figure 1) as well as a lack of balance in the panel. The cycles are due to seasonality of consumption, and we will use date by module fixed effects to deal with the cyclical nature of sales. This module specific time fixed effect will also deal with any module level changes in sales common across states. Additionally, to explain these trends which may concern the reader, we would like to provide some potential explanations for this pattern. First, coverage by the Nielsen data is increasing over time. There are items and stores which do not appear throughout the entire dataset. As a robustness check, we will run all of our analysis on a balanced panel to ensure this is not an issue. Additionally, this could be a story of market share. Over time, fewer larger, chain stores are capturing an increasing amount of the market. We will address this by running all analysis not only on the full dataset, but also on a balanced panel. Our results are robust to this change, but if you would like more information on our balanced panel analysis, see Appendix A.

While there are drawbacks to using these data, it has advantages over the data used in previous studies on this topic. First, these data are not survey based. When studying controversial topics, surveys data are subject to “social desirability” bias wherein subjects are likely to lie about socially questionable behavior. These data allow us to avoid this potential issue. Second, this dataset covers over 50% of grocery sales in the contiguous United States. Finally, this dataset is large. Compared to survey data or state specific data used in the existing literature, we have a much larger dataset with broader coverage and

therefore, have wider scope and more power than previous work.

Before describing the identification, Figure 2 visually represents our data on MMLs and RMLs. This is the variation in MMLs and RMLs on which our analysis depends. It is clear from this data that MMLs are much more prevalent during this period. At the beginning of 2006, the start of our data, there are already eight states with MMLs, whereas the first RML is passed in mid-2012. The variation in timing of these laws will allow us to identify the effect of these laws on alcohol and over the counter drug use. RMLs are slightly more complex, as the variation is much more limited. Only four states currently allow the recreational use of marijuana. However, we include these in our analysis as well to examine how these two types of legislation effect different people in different ways.

5.4 Methods

I analyze the effect of MMLs and RMLs on OTC medication and alcohol purchases through a series of regressions. In the main specifications, I will use the data described previously to estimate the following difference in differences models at the state level.

$$Outcome_{mst} = \alpha + \mathbf{Policy}_{st}\beta + \rho_{ms} + \sigma_{mt} + \epsilon_{mst} \quad (1)$$

$$Outcome_{mst} = \alpha + \mathbf{Policy}_{st}\beta + \mathbf{Demographics}_{st}\delta + \rho_{ms} + \sigma_{mt} + \epsilon_{mst} \quad (2)$$

$$Outcome_{mst} = \alpha + \mathbf{Policy}_{st}\beta + \mathbf{Demographics}_{st}\delta + \gamma_{st} + \rho_{ms} + \sigma_{mt} + \epsilon_{mst} \quad (3)$$

In equation (1), $Outcome_{mst}$ is $\log(Units_{mst})$ measured at the module state month level. As a robustness check, Appendix A presents these analyses with the outcome $\log(Revenue_{mst})$. Results are robust to the choice of outcome variable. \mathbf{Policy}_{st} are the variables of interest in these regressions; MML_{st} and RML_{st} . Therefore β is a vector of two values capturing the estimated effect of these policies. Fixed effects are included in all regressions to control for state differences in levels by module, ρ_{ms} and time spe-

cific differences in level by module σ_{mt} . Including these fixed effects allows us to isolate the within state change due to the policy variation. Finally, ϵ_{mst} is an independent and identically distributed random error term which captures any unobservables effecting the outcomes.

Variations on this specification include the addition of state level covariates and state specific time trends show in equations (2) and (3). In both equations, *Demographic_{st}* is a matrix of demographic controls at the state level over time. The results are robust to including the following controls: proportion of the state population under 25 years of age, proportion in school, proportion who dropped out of high school, and the proportion who graduated from college. In equation (3), a state specific time trend, γ_{st} , is also included. The addition of state specific time trends attenuates the estimates, and, in most cases, estimates lose statistical significance. This can be attributed to the roll-out of these policies.

Estimates in equations (1) and (2) pick up a level shift in sales of alcohol. These specifications will pick up changes in sales that happen months after the policy change. We believe this is the effect we would like to pick up, as it can take time to build up the infrastructure for a new market. However, this comes with the strong assumption that nothing else effecting alcohol and OTC medication sales is changing differentially in states that pass marijuana legislation. Equation (3) allows us to weaken this assumption, but comes with a trade-off. The assumption upon which equation (3) depends is that there is nothing effecting alcohol and OTC medication changing differentially in states that pass marijuana legislation at the same time as the law change. However, estimates from this equation only pick up the immediate discontinuity effect of the policy, ignoring potential changes in sales as the infrastructure is built up over the following months. We, therefore, interpret these estimates as lower bounds for the true effects of these policies.

5.5 Results

Methods described in the previous section are used to estimate the effect of MMLs and RMLs on alcohol and OTC medication. These categories are split further into liquor, wine, beer, pain medication, contraception, and other medication. Results from our main specifications can be found in Tables 3, 4, and 5. Alternative specifications can be seen in Tables 6 through 14 and Appendix A.

In this section, we will consider the effects of marijuana policies on $\log(Units_{mst})$ of alcohol and OTC drugs. Our Table 3 shows our preferred specification for our first category of interest, alcohol, in column 1. These estimates suggest that MMLs decrease alcohol sales by approximately 11.8%, and RMLs increase alcohol sales by approximately 38.9%. These results are significant at the 10% and 1% level, respectively. We attribute the difference in magnitude of these effects to the proportion of the population influenced by these policies. These are very large effects.

In column 2 of Table 3, demographics are added. These control are the proportion of the population under 18 years of age, proportion of the population in school, proportion of the population who dropped out of high school, and the proportion of the population who graduated from college. Their inclusion does not have a significant effect on our estimates or standard errors. We posit this is due to the inclusion of state level fixed effects. These demographic measures do not vary much from 2005 to 2015.

Column 3 of Table 3 adds a state specific time trend which could be picking up something different and important about alcohol purchases in those states that choose to adopt marijuana legislation. With the addition of a state specific time trend, we estimate a 3.61% decrease in alcohol sales with the passage of MMLs and a 16.6% increase in alcohol sales with the passage of RMLs. While neither are statistically significant at any reasonable significance level, we believe that including these state specific trends is capturing part of the effect of these policies. A change in trend is also an interesting and important effect

of these policies, and including a state specific trend biases our estimates toward zero. We attribute the lack of a sharp discontinuity to a time lag in the actual roll-out of infrastructure necessary for these new markets to form. While both estimates are attenuated in this specification, they are still opposite signed and statistically different from one another. Our conclusion that these policies effect alcohol consumption in different ways holds even under these rigid conditions.

Next, we break the Alcohol category into three subcategories: Liquor, Beer, and Wine. We perform this analysis because we believe that marijuana is primarily substituted for liquor as a form of self-medication for pain. These results, in Table 4, confirm the hypothesis. Focusing first on our main specification for each subcategory, specification (1), the effect of MMLs on alcohol consumption is almost entirely seen in a 17.9% decrease in liquor sales, with zeros as the estimated effect of MMLs on beer and wine consumption. Similarly, the effect of RMLs on alcohol consumption is almost entirely due to a 63.8% increase in liquor sales. The addition of state demographics in specification (2) does not have a significant effect on our estimates. In specification (3), estimates are attenuated with the addition of state specific time trends.

We also explore the effect of marijuana policy on OTC medication. We split OTC medication into three subcategories; pain medication, contraception, and other medication, because we expect marijuana policy to have an effect only on pain medication. Results from these regressions are in Table 5. While we do observe a negative effect of MMLs on pain medication sales, it is insignificant and very small; similar in magnitude to the estimated effect of contraception and other medication. Our hypothesis of medical marijuana as a substitute for OTC pain medication is not confirmed by this result. The estimated effect of RMLs on pain medication, however, is a statistically significant (at the 10% level) 6.06% according to specification (1). Again our hypothesis is not confirmed. This result is suggestive of a complementarity between marijuana and OTC pain medication under RMLs. This result is robust to adding state demographics, but not state

specific time trend. Results are attenuated with this addition and become insignificant. This is unsurprising as we would not expect to see a sharp discontinuity.

Overall, our main results provide evidence that RMLs and MMLs interact with other markets in substantially different and in some cases, surprising ways. MMLs decrease alcohol sales by approximately 11.8% acting primarily through liquor sales. On the other hand, RMLs actually increase alcohol sales by approximately 38.9% also due primarily to increased liquor sales. These estimates, from specification (1), rely on the level change from before to after and therefore pick up not only the immediate change, but any change that continues over time following these policy changes. While other changes may be occurring in states that pass marijuana legislation which bias our estimates, we believe these estimates are close to the true effect. To eliminate this potential bias, we include state specific time trends in specification (3). While these results restrict our identifying variation to the sharp discontinuity at the time of legislation, which we interpret as a lower bound for the true effects, the difference between the effect of the two policy types on alcohol consumption is still significant. This suggests that MMLs and RMLs target different populations and the effect of marijuana consumption on other markets is strongly dependent on the type of legislation.

In addition to our main specifications, we use $\log(\text{Revenue})$ as the outcome variable and balance our panel of data. While significance levels vary slightly with these changes, none effect our main conclusions. To see these regression results and further explanation regarding these specifications, see Appendices A and B. Appendix A provides information about $\log(\text{Revenue}_{mst})$ regressions analogous to our main specifications. Appendix B discusses methods for balancing our panel as well as results using these data for both our main and alternative outcomes. These regressions are allocated to appendices, because they do not alter any conclusions from the main results.

5.6 Conclusion

Relying on the variation in timing of marijuana legalization and two distinct policy types, MMLs and RMLs, to capture the causal effects of these policies, this paper provides evidence that RMLs and MMLs interact with other markets in substantially different ways. While we find that MMLs decrease alcohol sales by approximately 11.8% acting primarily through liquor sales, RMLs actually increase alcohol sales by approximately 38.9% also due primarily to increased liquor sales. In a literature with mixed results on the complementarity or substitutability of alcohol and marijuana, the decrease in alcohol sales due to MMLs is consistent with Anderson, Hansen and Rees (2013) concluding that marijuana and alcohol are substitutes. However, it is important to note these results are only for MMLs. We find an increase in alcohol sales attributable to RMLs.

This is the first analysis of the effect of RMLs on alcohol sales, and in this setting, the two appear to be complementary. These estimated effects, from specification (1), rely on the level change from before to after and therefore pick up not only the immediate change, but any change that continues over time following these policy changes. While other changes may be occurring in states that pass marijuana legislation which bias our estimates, we believe these estimates are close to the true effect. To eliminate this potential bias, we include state specific time trends in specification (3). While these results restrict our identifying variation to the discontinuity immediately around the policy changes, we still find a significantly different effect for these two legalization types. This is an important contribution of this paper. The majority of previous literature has focused on MMLs, and we find that distinction between MMLs and RMLs is important when considering marijuana as a substitute or complement with alcohol. In addition to looking at the effect on alcohol sales, we are able to analyze OTC drug purchasing as well. While we expect to find substitution effects in both cases, we find no evidence to support this hypothesis. In fact, our results suggest that marijuana and OTC pain medication may act

as complements under RMLs. This is a surprising result which leads us to believe that marijuana may interact in many markets in unexpected ways which depend on the type of legislation.

We add to a growing literature on this topic in a meaningful way and has a data advantage over previous survey-based studies. Surveys have potential response bias issues especially when dealing with controversial topics like drugs and alcohol which we do not have to worry about. Additionally, the dataset covers 50% of total grocery sales in 48 states and Washington DC which is better coverage than previous studies which often focus on individual states.

As more states legalize medical and recreational marijuana, this will continue to be a topic of interest and additional time and data will provide further possibilities for study in this area. It is increasingly important to understand the unintended effects of these policies, specifically when considering a choice between these two distinct policy types. This paper provides compelling evidence that these two policies effect different populations and therefore, interact with other consumption in significantly different ways. These consequences are important when considering the potential effects of policies.

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5.7 Appendix A

Tables A1 through A3 correspond to Tables 3 through 5, respectively. In each regression, the original outcome variable $\log(Units_{mst})$ is replaced with a new outcome variable, $\log(Revenue_{mst})$ for this appendix analysis. What we would like to capture is the amount of total alcohol consumed which is not given in this dataset. Here I use revenue as a different measure of sales for a given product rather than the number of units sold as units is not a perfect measure for consumption. For instance, when measuring in units, a 750 ml bottle of liquor is indistinguishable from a 25 0ml bottle and a six pack of beer has the same measure as a 30 rack. Whereas, measuring purchases in dollars captures some of this difference in amount, but adds the noise from any changes in quality of the items purchased. Coefficients are robust to this change. While the significance of a couple coefficients is changed, the conclusions remain the same.

5.8 Appendix B

In the following regressions, we balance our panel of data at the state-module level. To do this, I remove any module which doesn't appear for a given state in every month. Table B1 compares summary statistics for the balanced and unbalanced panels. Tables B2 through B4 correspond to Tables 3 through 5 from the main analysis. With the balanced panel, I also run these regressions using the alternative outcome, $\log(Revenue_{mst})$. Results from these regressions can be seen in Tables B5 through B7. Coefficients are generally robust to balancing the panel in this way, however some are attenuated slightly. Overall, the conclusions remain the same.

5.9 Tables and Figures

Figure 1. Alcohol Sales in California

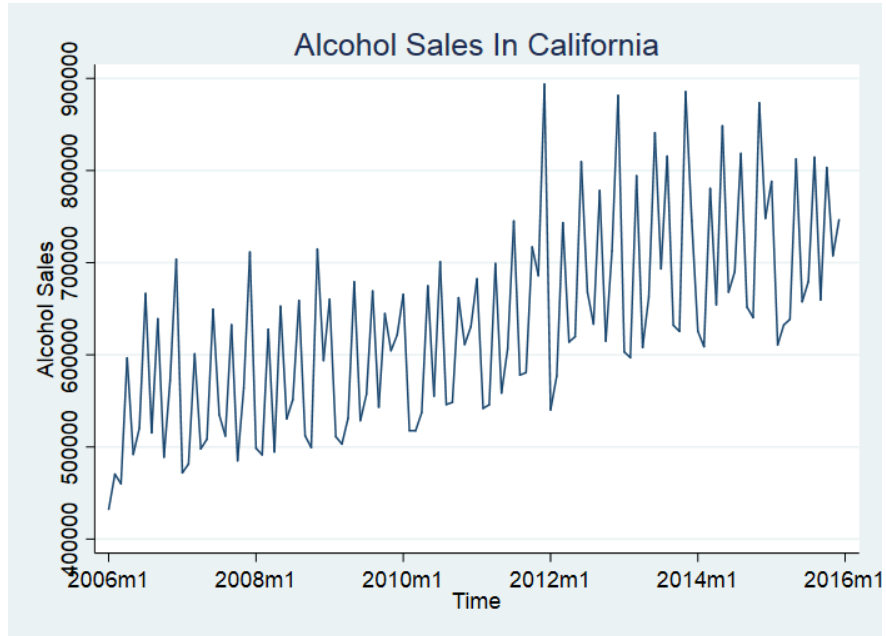


Figure 2. Identifying Variation

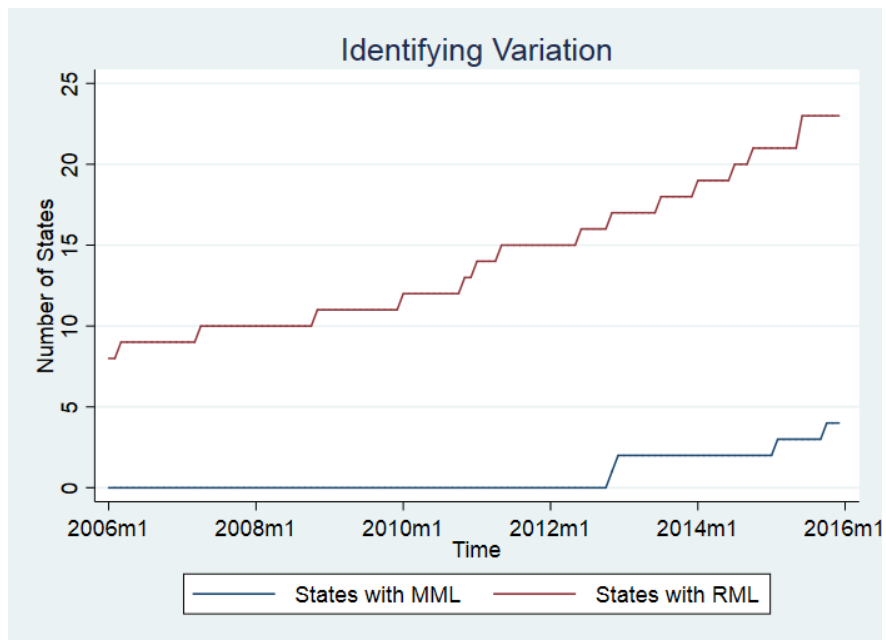


Table 1. Module Categories

Module:	Alcohol			Medicine		
	Beer	Wine	Liq.	Pain	Contractpt.	Other
COUGH DROPS						X
BEER	X					
NEAR BEER/MALT BEVERAGE	X					
STOUT AND PORTER	X					
ALE	X					
MALT LIQUOR			X			
BOURBON-STRAIGHT/BONDED			X			
BOURBON-BLENDED			X			
CANADIAN WHISKEY			X			
IRISH WHISKEY			X			
REMAINING WHISKEY			X			
SCOTCH			X			
GIN			X			
VODKA			X			
RUM			X			
TEQUILA			X			
BRANDY/COGNAC			X			
CORDIALS & PROPRIETARY LIQ.			X			
ALCOHOLIC COCKTAILS			X			
WINE-VERMOUTH			X			
COOLERS - REMAINING			X			
WINE-APERITIFS		X				
WINE-DOMESTIC DRY TABLE		X				
WINE-IMPORTED DRY TABLE		X				
WINE-FLAVORED/REFRESHMENT		X				
WINE-KOSHER TABLE		X				
WINE-SAKE		X				
WINE-SANGRIA		X				
WINE-SPARKLING		X				
WINE-SWEET DESSERT-DOMESTIC		X				
WINE-SWEET DESSERT-IMPORTED		X				
CONTRACEPTIVES-FEMALE					X	
CONTRACEPTIVES-MALE					X	
THROAT LOZENGES						X
NASAL PRODUCT INTERNAL						X
PAIN REMEDIES - HEADACHE				X		
COLD REMEDIES - ADULT						X
COUGH SYRUPS & TABLETS						X
SINUS REMEDIES						X
VAPORIZING PRODUCTS						X
Number of Modules:	4	10	16	1	2	7

Table 2. Summary Statistics

	N	Mean	SD
All Alcohol	139691	73055.83	304820.1
Beer	22763	145888.7	407536.2
Wine	41617	109358.8	446171.2
liquor	75311	30980.74	89668.7
Pain Medication	5880	338322.3	338070.7
Other Medication	41160	91782.99	177391.9
Contraception	11760	17283.26	29403.55

Table 3. Log Alcohol Quantities

	(1)	(2)	(3)
medical	-0.118 ⁺ (0.0690)	-0.109 (0.0662)	-0.0361 (0.0567)
recreational	0.389** (0.119)	0.409** (0.106)	0.166 (0.102)
demographics	No	Yes	Yes
module*state	Yes	Yes	Yes
module*date	Yes	Yes	Yes
state trend	No	No	Yes
N	139661	139661	139661

Standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 4. Log Alcohol Category Quantities

	Beer			Wine			Liquor		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
medical	-0.0580 (0.0980)	-0.0481 (0.0957)	-0.00638 (0.0477)	-0.0309 (0.0817)	-0.0266 (0.0804)	-0.0241 (0.0439)	-0.179* (0.0746)	-0.168* (0.0698)	-0.0479 (0.0677)
recreational	-0.0858 (0.104)	-0.0641 (0.113)	0.000226 (0.0990)	0.215 (0.185)	0.223 (0.188)	-0.0566 (0.0441)	0.638+ (0.379)	0.666+ (0.366)	0.325+ (0.181)
demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
module*state	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
module*date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state trend	No	No	Yes	No	No	Yes	No	No	Yes
N	22763	22763	22763	41610	41610	41610	75288	75288	75288

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 5. Log Medication Quantities

	Pain Medication			Contraception			Other Medication		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
medical	-0.00290 (0.0226)	-0.000620 (0.0223)	0.00884 (0.0110)	-0.00292 (0.0360)	-0.00131 (0.0349)	0.0255 (0.0205)	-0.00447 (0.0264)	-0.00434 (0.0261)	0.00544 (0.0165)
recreational	0.0606 ⁺ (0.0360)	0.0644 ⁺ (0.0380)	0.0157 (0.0272)	0.0342 (0.0438)	0.0401 (0.0447)	0.0175 (0.0227)	0.0425 (0.0419)	0.0375 (0.0403)	0.0125 (0.0357)
demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
module*state	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
module*date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state trend	No	No	Yes	No	No	Yes	No	No	Yes
N	5880	5880	5880	11760	11760	11760	41160	41160	41160

Standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Tables and Figures

Table 6. Log Alcohol Revenue

	(1)	(2)	(3)
medical	-0.124 ⁺ (0.0699)	-0.115 ⁺ (0.0666)	-0.0341 (0.0549)
recreational	0.424** (0.124)	0.445** (0.110)	0.187 ⁺ (0.111)
demographics	No	Yes	Yes
module*state	Yes	Yes	Yes
module*date	Yes	Yes	Yes
state trend	No	No	Yes
N	139661	139661	139661

Standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 7. Log Alcohol Category Revenue

	Beer			Wine			Liquor		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
medical	-0.0922 (0.0991)	-0.0822 (0.0968)	-0.00186 (0.0439)	-0.0510 (0.0848)	-0.0470 (0.0830)	-0.0269 (0.0486)	-0.171* (0.0770)	-0.159* (0.0710)	-0.0447 (0.0640)
recreational	-0.0723 (0.120)	-0.0491 (0.131)	0.0104 (0.0934)	0.232 (0.207)	0.239 (0.209)	-0.0594 (0.0479)	0.689+ (0.411)	0.720+ (0.396)	0.361+ (0.203)
demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
module*state	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
module*date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state trend	No	No	Yes	No	No	Yes	No	No	Yes
N	22763	22763	22763	41610	41610	41610	75288	75288	75288

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 8. Log Medication Revenue

	Pain Medication			Contraception			Other Medication		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
medical	-0.00275 (0.0236)	-0.000465 (0.0231)	0.00726 (0.0103)	-0.0203 (0.0365)	-0.0183 (0.0352)	0.0186 (0.0210)	-0.00599 (0.0271)	-0.00563 (0.0268)	0.00180 (0.0156)
recreational	0.0625 (0.0392)	0.0663 (0.0413)	0.00234 (0.0346)	0.00787 (0.0432)	0.0139 (0.0441)	0.0162 (0.0337)	0.0228 (0.0472)	0.0175 (0.0456)	0.0153 (0.0436)
demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
module*state	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
module*date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state trend	No	No	Yes	No	No	Yes	No	No	Yes
N	5880	5880	5880	11760	11760	11760	41160	41160	41160

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 9. Log Alcohol Quantities

	(1)	(2)	(3)
medical	-0.0992 (0.0706)	-0.0924 (0.0685)	-0.0311 (0.0572)
recreational	0.265** (0.0438)	0.280** (0.0428)	0.110 (0.0754)
demographics	No	Yes	Yes
module*state	Yes	Yes	Yes
module*date	Yes	Yes	Yes
state trend	No	No	Yes
N	129840	129840	129840

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 10. Log Alcohol Category Quantities

	Beer			Wine			Liquor		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
medical	-0.0153 (0.0825)	-0.00908 (0.0818)	-0.0108 (0.0478)	-0.0178 (0.0810)	-0.0150 (0.0803)	-0.0264 (0.0459)	-0.166* (0.0772)	-0.156* (0.0735)	-0.0385 (0.0679)
recreational	-0.0244 (0.0909)	-0.0108 (0.0991)	-0.0142 (0.0980)	0.242 (0.187)	0.247 (0.189)	-0.0824 (0.0520)	0.387* (0.168)	0.412* (0.157)	0.289+ (0.162)
demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
module*state	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
module*date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state trend	No	No	Yes	No	No	Yes	No	No	Yes
N	22080	22080	22080	39720	39720	39720	68040	68040	68040

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 11. Log Medication Quantities

	Pain Medication			Contraception			Other Medication		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
medical	-0.00290 (0.0226)	-0.000620 (0.0223)	0.00884 (0.0110)	-0.00292 (0.0360)	-0.00131 (0.0349)	0.0255 (0.0205)	-0.00447 (0.0264)	-0.00434 (0.0261)	0.00544 (0.0165)
recreational	0.0606 ⁺ (0.0360)	0.0644 ⁺ (0.0380)	0.0157 (0.0272)	0.0342 (0.0438)	0.0401 (0.0447)	0.0175 (0.0227)	0.0425 (0.0419)	0.0375 (0.0403)	0.0125 (0.0357)
demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
module*state	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
module*date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state trend	No	No	Yes	No	No	Yes	No	No	Yes
N	5880	5880	5880	11760	11760	11760	41160	41160	41160

Standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 12. Log Alcohol Revenue

	(1)	(2)	(3)
medical	-0.105 (0.0711)	-0.0975 (0.0686)	-0.0304 (0.0548)
recreational	0.295** (0.0435)	0.311** (0.0405)	0.122 (0.0818)
demographics	No	Yes	Yes
module*state	Yes	Yes	Yes
module*date	Yes	Yes	Yes
state trend	No	No	Yes
N	129840	129840	129840

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 13. Log Alcohol Category Revenue

	Beer			Wine			Liquor		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
medical	-0.0449 (0.0850)	-0.0386 (0.0843)	-0.00748 (0.0438)	-0.0349 (0.0834)	-0.0324 (0.0824)	-0.0299 (0.0505)	-0.158 ⁺ (0.0787)	-0.148 ⁺ (0.0740)	-0.0363 (0.0627)
recreational	-0.00708 (0.107)	0.00771 (0.116)	-0.00612 (0.0917)	0.256 (0.209)	0.261 (0.210)	-0.0820 (0.0561)	0.433* (0.207)	0.461* (0.196)	0.311 (0.188)
demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
module*state	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
module*date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state trend	No	No	Yes	No	No	Yes	No	No	Yes
N	22080	22080	22080	39720	39720	39720	68040	68040	68040

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 14. Log Medication Revenue

	Pain Medication			Contraception			Other Medication		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
medical	-0.00275 (0.0236)	-0.000465 (0.0231)	0.00726 (0.0103)	-0.0203 (0.0365)	-0.0183 (0.0352)	0.0186 (0.0210)	-0.00599 (0.0271)	-0.00563 (0.0268)	0.00180 (0.0156)
recreational	0.0625 (0.0392)	0.0663 (0.0413)	0.00234 (0.0346)	0.00787 (0.0432)	0.0139 (0.0441)	0.0162 (0.0337)	0.0228 (0.0472)	0.0175 (0.0456)	0.0153 (0.0436)
demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
module*state	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
module*date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state trend	No	No	Yes	No	No	Yes	No	No	Yes
N	5880	5880	5880	11760	11760	11760	41160	41160	41160

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Equity in Clean Energy Rebates: Evidence from California's Clean Vehicle Rebate Project

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Pollution reduction has become a central focus of policy around the world. California is looked to as a leader in putting forward ambitious policies regarding renewable generation, fuel switching, energy efficiency, and clean transportation. This paper focuses on the largest clean transportation program in California, the Clean Vehicle Rebate Project. The issue of equity in clean energy incentive programs led to the implementation of means testing the this program in 2016. Analysis highlights the importance of modeling choices and concludes that means testing led to increased electric vehicle adoption by low income households, estimating demand elasticity of -6.8 and identifying important interactions between equity and environmental impacts.

JEL Codes: H23, Q5

Keywords: Clean Vehicle Rebate Project, Electric Vehicle, Alternative Fuel Vehicle

6.1 Introduction

One of the most important issues of our time is climate change. It follows that pollution reduction has become the central focus of many policies around the world. California is looked to as a leader in putting forward ambitious policies regarding renewable generation, fuel switching, energy efficiency, and clean transportation. California began its efforts over three decades ago with the passage of Assembly Bill 4420 in 1988 which directed the California Energy Commission (CEC) to maintain an inventory of green house gas emissions and study the effects of these and climate change on the state. Since then, California has adopted various mandates, financial incentives, and a cap and trade market for CO₂ to address climate change, and policies similar to those adopted in California appear regularly in other places. This paper focuses specifically on the largest program¹ in California when it comes to clean transportation, the Clean Vehicle Rebate Project (CVRP).

The CVRP began offering rebates to Californians who chose to purchase or lease a neighborhood electric vehicles, plug-in hybrid electric vehicle, battery electric motorcycle, battery electric vehicle, or fuel cell vehicle in 2010, when there were few of these vehicles even available in the market. Not only has this program been running for over a decade, the size of individual rebates are also significant, offering consumers between \$900 and \$7000. Additionally, California is one of the largest markets for electric vehicles² (EVs) in the world accounting for almost half of EVs in the US with only 10% of the US population. It is, therefore, increasingly important to understand how these incentives impact the consumer.

With the plethora of climate change policies in existence, there is a wealth of existing literature in this area and still plenty of room further research in this area. Since the policy change used in this paper is the introduction of means testing, it is important to understand the motivation for this change. ? concluded that the majority of clean energy incentive

¹Size is determined by amount of government spending.

²Electric vehicles here are defined as neighborhood electric, plug-in hybrid electric, battery electric, and fuel cell electric vehicles.

dollars went to high income households. The issue of equity in these programs led to the adoption of increased CVRP incentives for low income EV consumers and an income cap on the standard CVRP rebate in 2016.

Previous literature focusing on clean vehicle rebates, subsidies, and tax credits includes robust work focusing primarily on hybrid electric vehicle incentives. For example, Chandra, Gulati and Kandlikar (2010) estimate that 26% of the hybrid vehicles sold during rebate programs in Canada could be attributed to the rebate and resulted in an carbon emission reduction cost of \$195 per tonne. Other papers focusing on incentives for hybrid vehicles, Diamond (2009), Sallee (2011), Gallagher and Muehlegger (2011), and Jenn et al. (2013), find a strong relationship between hybrid adoption and gasoline prices and a much weaker relationship between adoption and incentives, incentives are captured fully by the consumer, a 14.6% discount rate on future gas savings, and the Energy Policy Act of 2005 increased hybrid sales 3% to 20% depending on the vehicle, respectively.

More recently, the literature has turned to electric vehicle incentives. Zhang et al. (2014) looks at EV policies across countries. They identify relevant policies for encouraging EV adoption and remind us that countries should learn from each other what is the most effective. Holtmark and Skonhoft (2014) consider generous policies in Norway and conclude that the subsidy should be ended as soon as possible as it is encouraging households to purchase a second vehicle and decreasing alternative modes of transportation like public transit, bicycling, or walking. Antweiler and Gulati (2013) focus on EV subsidies and other alternative forms of transportation in Canada and conclude, among other things, that EV subsidies are not a cost effective way to induce switching from traditional vehicle transportation.

Literature focused specifically on the CVRP include papers utilizing survey data, data from the program, and DMV data. Only in the past couple years has there been sufficient data to consider the 2016 policy change which provides important identifying variation for this and a handful of other papers considering equity for this policy. Canepa, Hard-

man and Tal (2019) explore EV adoption in disadvantaged communities (DAC) in California in a descriptive way. They utilize CVRP application data to determine that the proportion of new EVs per household is lower in DAC than in other census tracts. Ju, Cushing and Morello-Frosch (n.d.) finds that with means testing introduced in 2016, the CVRP still issued more rebates per household to advantaged, White communities and the overall number of rebates distributed was reduced. Using data on CVRP applications and spatial autoregressive modelling, Bryan (2019) finds a positive, significant increase in rebate applications in areas with high concentrations of low income households with the 2016 policy change. Guo and Kontou (2021) evaluate the distributional equity of the CVRP. They find that, over time and with the 2016 policy change, the share of rebates distributed to DAC and low income groups increased. Additionally, they find high spatial clustering in metropolitan areas and significant neighborhood effects increasing rebate amounts in DACs next to communities with already high rebate amounts.

Building on these, my analysis concludes that means testing led to increased electric vehicle adoption by low income households, estimating demand elasticity for low income consumers of -6.8 which is slightly larger than estimates from other EV policies targeting this population. Unlike previous literature on the CVRP, this paper considers the proportion of households falling into the categories defined by the 2016 introduction of means testing as a continuous variable rather than defining communities as disadvantaged or not. It is important to keep in mind the 2016 context for the EV market. There was relatively limited supply at the time which could impact transactions. However, as long as this is not impacting the three income groups differently, it will not impact the results of this analysis.

The following section details the Clean Vehicle Rebate Project. The third section describes the data used in this paper. Section four elaborates on the methods used for analysis. Results are presented in section five. Policy analysis and implications are laid out in section seven, and the final section concludes.

6.2 Program Details

The Clean Vehicle Rebate Project is a rebate program for new electric vehicles purchased or leased by Californians. It is administered by the Center for Sustainable Energy (CSE) for the California Air Resources Board (CARB). The stated goals of the CVRP are to “bring many environmental and economics benefits, including less air pollution and reduced greenhouse gas emissions” through the promotion of clean vehicle adoption. It has undergone various changes since its inception in 2010, but the current program provides up to \$7000 for the purchase or lease of plug-in hybrid vehicles (PHEV), battery electric vehicles (BEV), and fuel cell electric vehicles (FCEV) to California residents who meet specific income restrictions. Table 15 summarizes the different rebate levels currently available and those available over the previous versions of this program. Rebates are also offered for neighborhood electric vehicles and battery electric motorcycles, though these rebates are smaller and since the Currently, the rebate amount depends not only of the vehicle, but also on the income level of the consumer. Households either qualify for a low income rebate, standard rebate, or no rebate at all. The means testing of this program began in March 29th, 2016 and has income brackets change over time due to changes in the policy and a dependence on the Federal Poverty Guidelines, colloquially known as the Federal Poverty Level, which is updated in the first quarter each year and updated in the CVRP on July 1st.

With the introduction of means testing in 2016, there are two income levels which determine one’s credit eligibility. To qualify for the standard rebates, the consumer must have less than the income limits laid out in Table 16. These limits were initially quite high, but ratcheted down eight months later. Those with income above these limits, currently \$150,000 for a single person, are not eligible for any rebate under the CVRP. In addition to these caps on income, consumers with household income under 300% of the Federal Poverty Guideline were made eligible for increased rebates called low income rebates.

Currently, these rebates were \$2,000 higher than the standard rebates. Since the Federal Poverty Guideline changes yearly, these updates are incorporated into the CVRP requirements each July. Table 17 lists the maximum income to qualify for the low income rebates since their inception in 2016.

Rebates under this program are also available to public entities. However, this paper will focus on individual consumers. These rebates also increased in 2018 for state, federal, and public entities who own and operate eligible vehicles in at least one disadvantaged community³.

It is important to note that these 2016 changes to eligibility were not unexpected. CARB approved these in late June 2015 with the Fiscal Year 2015-2016 Low Carbon Transportation Investments and Air Quality Improvement Program (AQIP) Funding Plan. Consumers and dealers were aware of the impending changes to the CVRP about 9 months in advance. Given this, one needs to recognize that any changes around the March 2016 is not solely due to means testing encouraging different people to purchase EVs, but also people change the timing off their purchase. For instance, a single individual with income exceeding the income cap of \$250,000 who is considering purchasing an EV in 2015 will be encouraged to do so before March 2016 to take advantage of the rebate. Low income households considering an EV purchase may be encouraged to hold off until their rebate amount increases. It is important to distinguish these intertemporal shifts from the long term effect of means testing.

Data from CVRP are dated based on application date which must be within 18 months of purchase or lease date⁴. Funding for the CVRP is yearly, so there are also situations in which funding for the year runs out prior to the end of the calendar year. In these cases, the program stops accepting applications temporarily and begins a waitlist for these ap-

³Disadvantaged communities are classified by census tract based on their population's exposure and vulnerability to pollution using multiple criteria. Low income communities are also included here and are defined as census tracts with at most 80% of the statewide median income or designated as low income by the California Department of Housing and community Development's list of State Income Limits

⁴For vehicles without a standard lease/purchase agreement, the date of first registration with the California DMV will be considered the date of purchase or lease

plications. Waitlisted applications will be fulfilled once funding is available and applications will be accepted again. This happens regularly for standard rebates. However, low income rebates are funded differently and are not subject to these limitations. Figure 1 graphs total applications and total sales and leases in California over time. The gap between these is due to the up to 18 month lag between the vehicle transaction and the fact that not all individuals apply either because they choose not to or due to ineligibility.

This paper focuses on the CVRP for three reasons. First, California is currently the largest market in the US for EVs as Figure 4 shows, so rebates offered in the state cover a large proportion of sales. Additionally, uptake of these rebates is high. A total of over 250,000 rebates were given out as of November 2018, and about 75% of eligible vehicles participated in the first five years of the program. The size of these rebates is also not insignificant relative to vehicle prices. Most eligible EVs sold to individual consumers for between \$25,000 and \$89,000⁵, making most standard and low income rebates between 2.8% and 18% of the selling price. Even a 2.9% decrease in price could potentially influence a consumer, and clearly an 18% is quite a significant rebate for the cheaper qualifying vehicles. Finally, with AB-1046 which is currently in committee, it appears that this project will only grow over the coming years. Data are explained and explored further below.

6.3 Data

Data on the universe of electric vehicle transactions in the state of California from April 2012 through December 2017 is the primary dataset. It provides detail about each EV transaction in California including the make and model, transaction price, date, and census tract and zip-code of registration. This is combined with demographic data for California from the American Community Survey (ACS). Crosswalks from the Department

⁵These numbers are approximately the 5th and 95th percentile of selling prices for new vehicles in this dataset. There were some even cheaper vehicles like the Smart fortwo, and some much more expensive vehicles such as Tesla's Model X with all the upgrades.

Housing and Urban Development (HUD) provide the overlap in residential addresses between zip-codes and census tracts. These data are used to allocate ACS demographics to the zip-code. Additionally, CVRP application data is made publicly available by CARB and CSE from March 2010 to November 2018. This dataset provides information on the type of rebate, which will be useful in determining the number of EVs purchased by low income households after the implementation of means testing. These datasets are detailed further below.

The main dataset used for analysis is the universe of EV transaction in California. During the data period of April 2012 to December 2017, over 350,000 battery electric and plug-in hybrid transactions occurred. Of these, 85% are new PHEVs or BEVs purchased by individuals and are the relevant group for the CVRP. These data include VIN, vehicle make, model, year as well as the census tract and zip code where the vehicle is registered. Transaction data over time is summarized in Figure 3.

Over this five year period, one can see that new EV transactions increased. This is partly due to the fact that very few BEVs were available in 2012. Specifically, only Nissan, Tesla, and Smart BEVs appear in beginning of this period, and the only models available in California at the time were the Tesla Roadster, Mitsubishi i MiEV, Nissan Leaf, and Smart ED. New BEV transactions are broken out by vehicle make in Figure 6. This figure reveals a large spike in Tesla transactions immediately after the implementation of means testing. Looking at Tesla's delivery numbers and trade publications does not provide an explanation for this spike. For example, InsideEVs tracks monthly EV sales in the USA by model beginning in 2010. Their Tesla estimates from quarterly reports, VIN data, and Tesla owners' information shared online provide no insight into this spike. Therefore, analysis will include robustness checks excluding the time period of this spike and Tesla vehicles altogether. Figure 5 breaks down new PHEV transactions by make. There is less of a clear trend for this group of vehicles as popular models are well established in California in 2012.

In addition to transactions varying over time, there is also significant variation across the state. Figure 7 maps the distribution of EV sales during this period by zip-code. Pairing these data with demographic information at the zip code level yields Figure 3 which summarizes EV transactions per thousand residents for zip-codes by income quartile. The ACS provides an estimate of the proportion of the population below 300% of the Federal Poverty Guideline which aligns perfectly with the low income rebate cutoff for the CVRP. This figure tracks new EV transactions by quartile of the proportion of the population below this 300% cutoff. The line for the first quartile is the average number of vehicles sold per thousand residents for the 25% of zip-codes with the lowest proportion of the population below 300% of the federal poverty line. In addition to estimating the proportion of the population below 300% of the federal poverty guideline, the ACS provides estimates of the proportions below 100%, 200%, 400% and 500% as well as the median income. These will be useful for additional robustness and falsification tests.

For the high income cutoff, the ACS provides the number of households with income over \$150,000 and \$200,000. Unfortunately, the proportion above \$250,000 is not directly provide and will need to be estimated based on the proportion above \$200,000.

While the raw data is at the transaction level, for analysis, data will be aggregated to the zip code level. A tabulation of data aggregated to the finest level, zip code-day, is presented in Table 18. Over 95% of zip-code days do not contain any new EV transactions, and coefficients are estimated using variation in data, so having all these zeroes is not useful. Aggregating to the zip code-month eliminates many of these zeroes without compromising much of the variation and increases the efficiency of estimation significantly. Table 19 presents a tabulation of these zip code-month level observations.

Transaction level data from the CVRP are available. These data will be useful for providing context for the proportion of EVs eligible for increased incentives after the policy change because it includes the size of the rebate for each application.

6.4 Methods

Since transaction level data is aggregated to the zip code month level, each data point is a count of the number of EVs registered during that month in that zip code. Working with count data with an abundance of zero observations generally requires the use of a nonlinear model. The main choice is the parametric Poisson model. Linear models are typically used as well as they have the most intuitive interpretation, and both will be utilized here. Poisson models became popular for use with count data in the 1980s. ? creates a framework for use with panel data, and ? lays out a variety of models for count data in their book focused on the econometrics behind these. This model is the workhorse for data which follows an arrival processes such as visits to the doctor, number of patents applied for or, the number of purchases per period. This makes these data the ideal application for this model. For information on alternative models, see Appendix A.

Equations 1 and 2 describe the preferred specification, a Poisson fixed effects model in which covariates determine λ_{zt} which in turn predicts a discrete probability distribution for each observation. $Transactions_{zt}$ is the number of new EVs in zip code, z , at time, t , where time is aggregated to the month year level. The coefficients of interest are β_{low} and β_{high} which capture the difference in purchases after the introduction of means testing and scales by proportion of low income and high income individuals, respectively, in each zip code. $\mathbb{1}(t > March28, 2016)$ is an indicator variable which equals 1 for all EVs purchased or leased after March 28th, 2016. These are the vehicles that are potentially impacted by the introduction of means testing. They may be eligible for an increased rebate, or they may no longer qualify for a rebate depending on the income of the purchaser/lessee. This variable only varies over time and is the same for all zip-codes in the state. $PPLI_z$ is the estimated proportion of the population in a zip-code, z , which would be categorized by this policy as low income and eligible for the increased rebate. $PPHI_z$ is the estimated proportion of the population in a zip-code categorized as high income and made ineligi-

ble for a rebate on their EV purchase/lease. To control for the fact that purchase patterns in different zip-codes may vary, zip code level fixed effects, α_z , are included. Due to the clear time trends in overall EV purchases, date fixed effects, δ_t are also included. The interpretation of the coefficients of interest, β_{low} and β_{high} , are elasticities for this model.

$$\Pr[Transactions_{zt} = y_i] = \frac{\exp(-\lambda_{zt})\lambda_{zt}^{y_i}}{\Gamma(1 + y_i)}, y_i = 0, 1, \dots \quad (4)$$

$$\begin{aligned} \ln \lambda_{zt} = & \beta_{low,3} \mathbb{1}(t > 3/28/16) * PPLI_z \\ & + \beta_{low,11} \mathbb{1}(t \geq 11/01/16) * PPLI_z \\ & + \beta_{high,3} \mathbb{1}(t > 3/28/16) * PPHI_z \\ & + \beta_{high,11} \mathbb{1}(t \geq 11/01/16) * PPHI_z \\ & + \alpha_z + \delta_t + u_{zt} \end{aligned} \quad (5)$$

Identification within this model relies on the proportional change in relative monthly sales across zip codes with varying proportions of the population eligible for the increased incentive and those made ineligible for any incentive from before to after the policy change. Since the incentives change twice during this period, both events dates are interacted with these population proportions to capture the impact of the first change relative to before any means testing and the impact of the second change relative to the first change. The resulting coefficient estimates are interpreted as the proportion change in monthly EV purchases in a zip code with 100 percentage points higher relative proportion of the population within the low or high income qualification on average. A 100 percentage point difference is not within realistic variation for these proportions, so it's better to interpret one tenth of the estimate as the proportion change in monthly EV purchases in a zip code with 10 percentage points higher relative proportion of the population within the low or high income qualification on average. This captures the proportional increase in purchases after the introduction of means testing created only by the purchases

made by those in the impacted populations.

Threats to this strategy are any other factors that are correlated with the proportion of population that fall into the low or high income categories and change over time differently based on these proportions and impact the purchasing of EVs. For example, if there was an increase in outreach regarding this incentive specifically in areas with lower average income around the time of the incentive increases for that population that was effective, that would bias the coefficient upward.

Classic linear and log linear specifications as well as a linear regression with a standardized transaction variable by location are allocated primarily to Appendix A.

In addition to using these models on the entire set of EV transactions, I utilize these methods to analyze subgroups, perform robustness checks, and run placebo and falsification tests.

6.5 Results

Results utilizing all new EV transactions for the four specifications described in the previous section are presented in table 21. These results confirm that the choice of model is integral to capturing the intended effect. Recall that all data for these are aggregated to the zip-code by month level.

Column 1 is a linear regression where the outcome variable is EV transactions per hundred thousand residents. This specification represents a downward biased estimate of the policy impact on low income households and an upward biased estimate of the effect on high income households. Column 2 of Table 21 presents results using a log-linear model. These are similarly counter-intuitive for the policy impact on the low income population and biased downward due to the abundance of zeroes in these data which are dropped when taking the log. Estimates for the high income population are more likely accurate than the linear regression because the dropping of zeroes is not an issue faced by wealthy areas which see EV transactions most months over this period.

In column 3 of Table 21, the number of transactions is standardized for each zip-code, $\frac{Transactions_{zt} - \bar{Transactions}_z}{s_z^{Transactions}}$. This scaling should allow one to capture proportional changes in transactions using a linear fixed effects regression where our interpretation of the coefficients of interest are measured as changes in the number of standard deviations when the policy is implemented. A zip-code with an additional 10% of the population categorized as low income is associated with increased sales of .6% after March 2016 and an additional 1.9% after October 2016 on average. These results, align with intuition and the preferred results in column 4, but lack the same statistical significance.

Finally, columns 4 present results for the preferred Poisson specification. This regression suggests a zip-code with higher PPLI by 10 percentage points is associated with higher EV transactions after the March 2016 policy change of .52%, on average and after the November 2016 policy change 3.2% on average. The former is not statistically significant. However, because the two changes happen within a year, they could confound one another. It also suggests that eliminating the incentive for those with annual income over \$250,000 in March did not have a statistically significant impact on transactions. In fact the direction of the coefficient suggests a possible increase in transactions which could be an indication that high income consumers were able to shift their EV purchases to the time period between the two changes. The elimination of the rebate for those with income over \$150,000 appears to be associated with lowered EV sales to this population. A zip-code with a higher proportion of people with income above \$150,000 by 1 percentage point is associated with lower EV transactions by 13% on average. The combination of these two changes would result in raised EV transactions from March to November and lowered transactions after, but little overall change in long run EV purchases for the high income population.

In a later section, elasticities will be estimated using these regression results. First, let us break down these results to further understand the ways in which this policy change is influencing EV adoption.

6.5.1 Subgroup and Additional Analysis

Tables 22, 23, and 24 present the preferred specification with slight variations. These provide support for the main specification and additional evidence for the ways in which means testing altered the dispersion of EVs in California.

First, this change in policy treats all people with income under 300% of the Federal Poverty Guideline the same, but people within the target group may respond differently to the increases subsidy. People with income just below this income cutoff may be more likely to purchase an EV than someone with income at 100% of the poverty level. Column 2 of table 22 breaks down the households with under 300% into those with between 200% and 300%, and those with under 100%. This confirms that this policy change is primarily encouraging EV purchases with income between 200% and 300% of the Federal Poverty Guideline. Increasing incentives for those at the very bottom of the income distribution does not seem to have a significant impact. These results are intuitive as we are focused only on new vehicles and EVs are more expensive than internal combustion engine (ICE) vehicles. For these reasons, EVs may not be accessible to those below 200% of the federal poverty line, even with a significant subsidy. Results in 22 confirm that the majority of the purchasing action caused by this policy change is concentrated among people who fall between 200% and 300% of the federal poverty guideline. The estimated impact of the November increase in the subsidy is a zip-code with 10% more of the population between 200% and 300% of the federal poverty guideline will see 13% higher EV transactions after the change, on average. Given the relatively high price tag on EVs, it is not surprising that an increased subsidy still fails to induce increased EV adoption for those with lowest income.

The second exercise accounts for vehicle price. Since this policy change provides additional resources to low income households, one may think that the policy lead to differing effects on various vehicles due to their prices. On average, EVs have higher price tags than internal combustion engine vehicles. Households qualifying as low income are

more likely to consider an EV with a lower price tag. Table 23 confirms that the effect of increased rebates was primarily focused on lower priced EVs. Column 1 presents the impact for EVs with price in the bottom quartile. Each column after this adds vehicle transactions in an additional price quartile. For EVs priced in the bottom 50% of all EVs, a 10% increase in the proportion of people in the low income group is associated with around a 12% increase in EV adoption after these policy changes, on average. These results confirm that the increase in the rebate for low income households increased transactions of primarily lower priced EVs.

Table 24 breaks down EV transactions into plugin hybrid and battery electric vehicles (PHEVs and BEVs) as well as separating out leases and purchases. Accounting for range concerns and number of vehicles per household, it may be that the additional low income subsidy impacts these two types of EVs differently. Low income households are more likely to have only one vehicle and due to concerns about the limited range of BEVs, these may be less attractive than a PHEV or other gasoline vehicle. Additionally, there is a trend specifically in increased leasing among low income households because of increasing difficulty in qualifying for a loan for a used vehicle. Leasing a new vehicle can also result in lower monthly payments than a used vehicle loan. Columns 1 through 3 present sales and leases of various types of vehicles, columns 4 through 6 focus solely on purchases, and columns 7 through 9 include only leases. One thing to notice is a significant decrease in BEV purchases and leases with the first increase in the subsidy which is more than made up for by a larger increase in BEV purchases and leases after the second change. This pattern is not replicated for PHEVs where the main increase in transactions occurs after the first increase in the subsidy. It is important to recognize this heterogeneous response by vehicle type. This could suggest that those buying or leasing BEVs have more ability to shift their transaction intertemporally to take full advantage of the increased subsidy relative to those buying or leasing PHEVs.

6.5.2 Demand Elasticities

From these regressions, elasticities can be estimated. Using the estimates from the preferred specification in column 4 and measures of rebate sizes and vehicle prices, I can calculate a back of the envelope average demand elasticity for an income group according to equation 3 where $\beta_{i,3}$ and $\beta_{i,11}$ are the coefficients for the i income group for the March and November policy changes respectively, $\Delta rebate_i$ is the average total change in the rebate amount for income group i , \bar{p} is the average vehicle price, and $rebate_{before}$ is the average rebate for everyone before the implementation of means testing.

$$\epsilon_{d,i} = \frac{\beta_{i,3} + \beta_{i,11}}{\Delta rebate_i / (\bar{p} - rebate_{before} - .5\Delta rebate_i)} \quad (6)$$

This calculation results in an average demand elasticity of 6.8 around the \$40,000 price level for people below 300% of the federal poverty guideline. This relies on the underlying assumption that all people within the income group have the same underlying propensity to purchase EVs, which we know is false. It also uses the overall average purchase price, which we know is false. Taking these facts into account, table 25 provides elasticity estimates under a variety of assumptions which suggest a local elasticity of around -6.8 for EVs for those below the income threshold on average. As EVs become increasingly more affordable, a 1% decrease in prices from the current price faced by this group will increase sales 6.8% on average or more. The other elasticity estimates in Table 25 use coefficients and prices based on the subgroup analysis which shows increased impact of the price change for the those with income just below the 300% of the federal poverty guideline and for less expensive vehicles. Overall, these elasticities are slightly larger than price elasticities estimated for electric vehicle in the existing literature.

6.5.3 Robustness Checks

Two different types of robustness checks are presented in this sections. First, doughnut regression results are described which are intended to convince the reader that the effect is not coming from intertemporal shifting of purchases. Second, an event study is used to provide additional information about any intertemporal shifts due to the policy.

Doughnut regressions are presented in table ?? as one might be concerned that consumers shift their purchases over time due to this policy change. Since the change was announced nine months in advance, it is possible that low income consumers delayed their EV purchases to take advantage of the increase and high income consumers did the opposite. This intertemporal shifting will bias estimates of the policy effect away from zero in previous regressions. To account for this, regressions are run excluding the months between the two changes and immediately before and after.

Eliminating the time in between the two changes means one can no longer estimate two separate coefficients for the low income group. Low income estimates for these will be comparable to the sum of the two original estimates. Table ?? presents results of these doughnut regressions using the preferred Poisson specification and various amounts of data removed around the policy implementation date. Column (1) presents the same regression using data excluding the period between March and November. Each subsequent column eliminates one additional month on either side of the policy change. Column (2) excludes data between February and December 2016, column (3) excludes data between January 2016 and January 2017, and so on. The low income estimates are robust to these exclusions which suggests that the results are not due to intertemporal shifting of transactions and that the main specification captures the overall intended impact of the policy change on transactions and not unintended consequences of the policy change. However, the high income estimates are no longer statistically significant and are not stable over the various exclusion periods. This may suggest that these groups are shifting the timing of their EV purchase to take advantage of the subsidy before it phases out for

them.

One could, on the other hand, not believe the assumptions put on the growth of the underlying propensity of consumers to purchase or believe there are too many other things happening over this period and changes can not be attributed to this policy change. This leads to using an event study framework with a limited data window around the policy changes. Table ?? presents results for event studies using various windows of time. Since we have two events, the time between them was left alone for this exercise. The first column provides the estimates for the two policy changes limiting the data window to fourteen months before the first change and 14 months after the second change. Each subsequent column eliminates an additional two months from the edges of this window until there are only two months left before and after the changes. As the window of time changes, the estimates do not vary in a statistically or economically significant way. This provides solid evidence that the changes in EV adoption are due to the policy change and not something else.

6.6 Policy Evaluation and Implications

Results suggest that the introduction of means testing in the CVRP increased electric vehicle adoption for low income individuals and potentially decreased adoption for high income individuals. This section focuses on putting the benefits of means testing in context with respect to equity and the environment.

Before getting into the specific environmental and equity benefits of means testing, one can back out an estimate of the number of additional electric vehicles put in the hands of lower income households by this policy change and therefore the cost incurred to get each of these vehicles purchased. CVRP application data provide information on whether an application received a low income or standard rebate. This is shown in figure 8. Using this and the regression results, I can estimate the number of additional EVs purchased because of the increased rebate. A 31% increase in purchases after the policy changes means

that about 26% of purchases by low income households was due to the increased incentive. The average number of EV applications for the increased rebate after the second policy change is 379 vehicles. This amounts to about 99 vehicle purchases per month on average at a cost of about \$7,700 in subsidy per vehicle on average. This is the amount of increased subsidy (\$2000) to all the vehicles qualifying for the increase distributed across only the marginal vehicles purchases.

Let's consider this cost in context of the estimated cost of carbon. A reasonable estimates for the cost per metric ton of carbon fall between \$50 and \$200. This leads to the question: does an EV in the hands of a low income buyer result in decreased carbon emissions between 38.5 and 154 metric tons of carbon. Of course, there may be other costs or benefits to the environment outside of carbon reduction, but this is an important piece of the picture and has been researched. ? estimate a reduction in life cycle carbon emissions between 45 and 74 tonnes when comparing light duty BEVs to their ICEV counterparts.

In addition to purely environmental benefits, means testing also has impacts on equity and equity, in turn, interacts with the environmental benefits. The following sections focus on these.

6.6.1 Increased Equity

Equity is an important consideration when it comes to any government policy because governments have limited budgets and redistribution is an important part of a government's job. Policies that are regressive should be recognized and offset when possible whether by complementary policy or through means testing within the policy itself. Clean energy and transportation policies are known to be regressive. ? analyze equity within many existing and former green policies and confirm they are highly regressive. 9, from their paper, shows the estimated concentration of dollars across the income spectrum for three clean vehicle policies. CVRP is an early example of an attempt to limit the regressive nature of these policies via means testing.

One way to visualize equity within a policy is to map the proportion of government dollars spent against income of recipients as in ?. Unfortunately, data available on this policy does not include income levels for recipients, so assumptions similar to those used in the analysis will be necessary. Data from CVRP applications include the size of the rebate received, but not further income information. Using these data, the graph in figure 10 present this relationship after the implementation of means testing under the assumption of equal distribution of funds within the three groups. The low income group accounts for approximately 51% of the population in the state, the unchanged group is 48%, and the high income group is 1% of the population. This assumption of equal funding distribution within groups represents an upper bound on the equitable distribution.

There is a natural extension to these graphical depictions of equity. One can simply use percentiles for both income and government spends and take the integral of this function to create a numerical value on the progressiveness of a policy. This will result in a score between 0 and 1, where the higher the score, the more progressive the policy. For example, an income neutral policy which treats all people equally graphically appear as a 45 degree line from the origin and would score .5 on this scale. The higher the score, the more progressive the policy. The upper bound score for CVRP funding is .29. Even with means testing, the vast majority of government spending on the CVRP ends up in the hands of those with above average income.

6.6.2 Environmental and Equity Interactions

Finally, there are specific ways in which equity and environmental benefits interact. The environmental benefits of means testing can be categorized into global and local pollutant reductions. There are many angles to consider when it comes to the interactions between equity and environmental benefits of putting EVs in the hands of low income households. First of all, it is estimated in California that low income individuals spend longer in their vehicles, on average, than others. This means that in terms of both global and

local pollutants an EV in the hands of a low income individual has higher environmental benefits. Second, the average vehicle replaced by an EV for low income individuals is, on average, dirtier than the vehicle a medium or high income individual replaces. Again increasing the environmental benefits. Finally, it is generally true that the lower income individuals are located in areas with higher levels of local pollutants. Therefore, there is a higher benefit of having EVs on the road in the hands of low income individuals because local pollutants have nonlinear environmental costs. Decreasing local pollutants in areas of high pollution is more beneficial than the same decrease in areas of low relative local pollution.

Using data from CVRP paired with air quality data from the EPA, the map in Figure 11 illustrates the correlation between the proportion of EVs purchased by low income households and the proportion of the year the EPA's air quality index is at least unhealthy for sensitive groups in 2014 by county. The maps on the left represent two different measures of air quality. The top right is the proportion of days where the air quality index (AQI) is in the "Unhealthy" range or higher in 2014, at least 150. The bottom right is the proportion of days the AQI is in the "Unhealthy for Sensitive Groups" range or higher in 2014, at least 101. On the right are two different measures for low income rebates. The top is the raw number of applications for the increased low income rebate and the bottom is the proportion of applications in the county that are specifically for the increased rebate. Both use CVRP application data from March 2016 through December 2017. One can see that in all maps the Los Angeles county and valley counties have the worst air quality and some of the highest low income rebate numbers. In fact, using these two measures for each, the correlations are between .1675 and .3175.

There are potentially increased benefits from EVs when they are in the hands of low income drivers. This is an area for further research.

6.7 Conclusion

In this paper, I utilize nonlinear econometric techniques to capture the impact of means testing within California's Clean Vehicle Rebate Project. Results show that increased incentives for plug-in and battery electric vehicles increase purchases of these vehicle by low income households. The impact is specifically focused on the purchase of more affordable EVs by households that qualify for the increased rebate but are closer to the income cutoff. This impact is robust to doughnut specifications accounting for intertemporal shifting of purchasing and event study models limiting the time window which limits any other potential influences.

On the other hand, the decreased incentive for households with high income are not statistically significant when accounting potential intertemporal shifting of EV purchases. In the future, more could be done on this end of the income spectrum to elicit the impact of price and incentives on purchasing patterns.

Building on a literature which aims to provide feedback for policy makers and important information on the impact of green incentives, this paper shows that even with means testing, the vast majority of funding ends up going to households with above average income.

Equity is an important consideration when it comes to climate action and adaptation, and this paper shows that increased incentives for EVs are successful in increasing purchases by low income individuals. Additionally, means testing with an aim at making policies more equitable could potentially increase funding for successful policies without putting the same political arguments about the fiscal impacts of increased government spending front and center in the conversation.

Means testing clean energy and transportation programs is not enough. Clean energy investments often require significant financial resources even when accounting for subsidies. In addition, lower income households face other barriers to adopting clean

energy and transportation solutions. In the case of EVs during the data period, barriers other than price faced primarily by lower income households include access to parking and charging, range-exceeding commutes, lack of access to dealers, and general knowledge about EVs as an alternative. Creating inclusive climate policy will require not just targeted dollars, but targeted policies.

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6.8 Appendix C: Other Specifications

In addition to the Poisson Model, it is useful to run a basic linear fixed effect model as a reference point. This model, described by equation 3 below, utilizes the same notation for covariates and parameters as the previous model, and is, in effect, an intensity of treatment model where the policy change is two treatments and the proportions of the population which qualify for the increased rebate and which are made ineligible represent the intensity of these two treatments on a given zip-code.

$$\begin{aligned}
 Transactions_{zt} &= \beta_{low,3} \mathbb{1}(t > 03/28/16) * PPLI_z \\
 &= \beta_{low,11} \mathbb{1}(t \geq 11/01/16) * PPLI_z \\
 &= \beta_{high,3} \mathbb{1}(t > 03/28/16) * PPHI_z \\
 &+ \beta_{high,11} \mathbb{1}(t > 11/01/16) * PPHI_z \\
 &+ \alpha_z + \delta_t + \epsilon_{zt}
 \end{aligned} \tag{7}$$

However, using a linear model in this situation will lead to biased results. There is a significant and differential time trend in EV purchases over this period which will result in a downward bias for β_{low} and an upward bias for β_{high} because people with higher income are much more likely to purchase EVs and this increases over time proportionally.

Given the nature of the data, a log-linear model may seem more appropriate than a linear one. People making \$200,000 are much more likely to purchase an EV than those making \$50,000. Therefore, when measuring the increase in purchases for a low income group relative to a medium income group, it makes sense to measure it as a proportionate increase as opposed to a level increase. Equation 4, below, details the log-linear specification. Covariates are the same as in the linear specification with β_{low} and β_{high} now interpreted as the partial elasticity of transactions with respect to the policy intensity of treatment.

$$\begin{aligned}
\ln(\text{Transactions}_{zt}) &= \beta_{low,3} \mathbb{1}(t > 03/28/16) * PPLI_{300\%,z} \\
&= \beta_{low,11} \mathbb{1}(t \geq 11/01/16) * PPLI_{300\%,z} \\
&= \beta_{high,3} \mathbb{1}(t > 03/28/16) * PPHI_{250k,z} \\
&+ \beta_{high,11} \mathbb{1}(t \geq 11/01/16) * PPHI_{150k,z} \\
&+ \alpha_z + \delta_t + \epsilon_{zt}
\end{aligned} \tag{8}$$

However, this specification comes with its own drawback. Many, in fact, the majority of the observations for EV transactions in a given month and zip-code are zero. Utilizing a log-linear format means dropping these observations. These zeros are important and dropping them eliminates much of the variation for areas with a significant low income population. In other words, observations for the zip codes with the highest treatment intensity are more likely to be dropped which means these areas will be under weighted when calculating coefficient estimates. This will lead to a downward bias in our estimate for β_{low} and an upward bias in our estimate for β_{high} .

Alternatively, one could standardize purchases within each zip-code to allow for the comparison of changes across zip-codes. Equation 5, below, describes this specification. The outcome of interest is now the standard (0,1) variable representing the number of standard deviations away from the average an observation is for a given zip-code. Here β_{low} and β_{high} are interpreted as a standard deviation change in transaction levels due to treatment intensity.

$$\begin{aligned}
\frac{Transactions_{zt} - \bar{Transactions}_z}{sd(Transactions_{zt}|z)} &= \beta_{low,3} \mathbb{1}(t > 03/28/16) * PPLI_{300\%,z} \\
&= \beta_{low,11} \mathbb{1}(t \geq 11/01/16) * PPLI_{300\%,z} \\
&= \beta_{high,3} \mathbb{1}(t > 03/28/16) * PPHI_{250k,z} \\
&+ \beta_{high} \mathbb{1}(t > 3/28/16) * PPHI_{150k,z} \\
&+ \alpha_z + \delta_t + \epsilon_{zt}
\end{aligned} \tag{9}$$

6.9 Tables and Figures

Figure 3. Cumulative EV Transactions

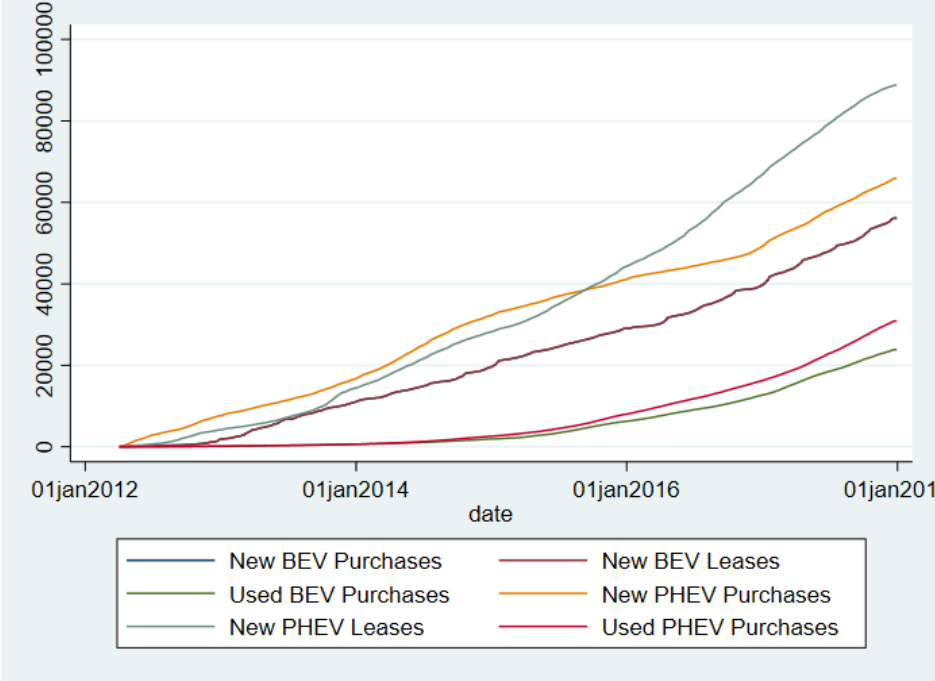


Figure 4. Cumulative New EVs: US and CA

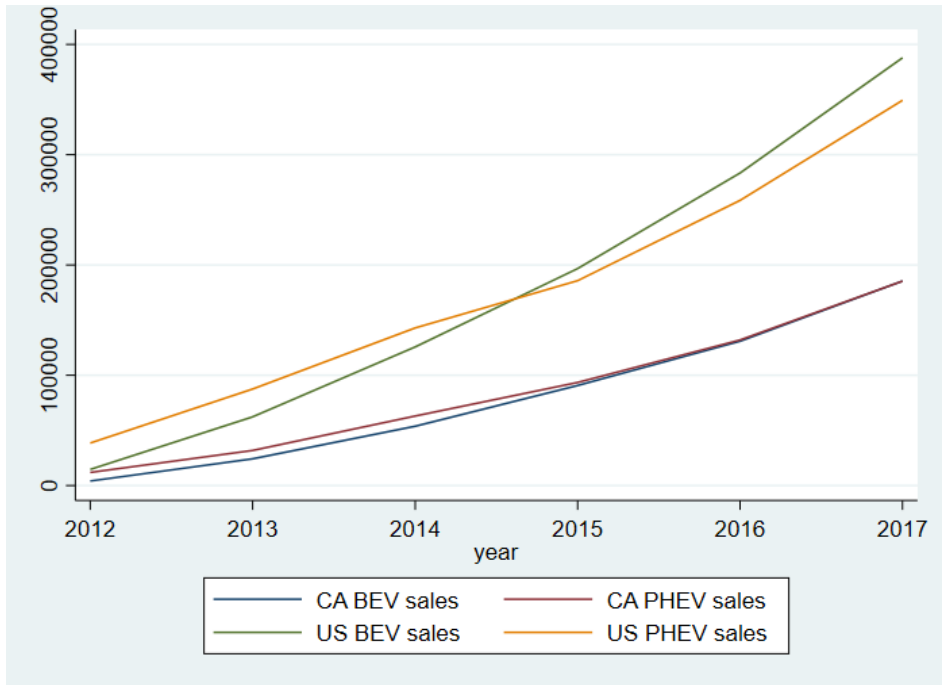


Figure 5. New PHEV Transactions by Make

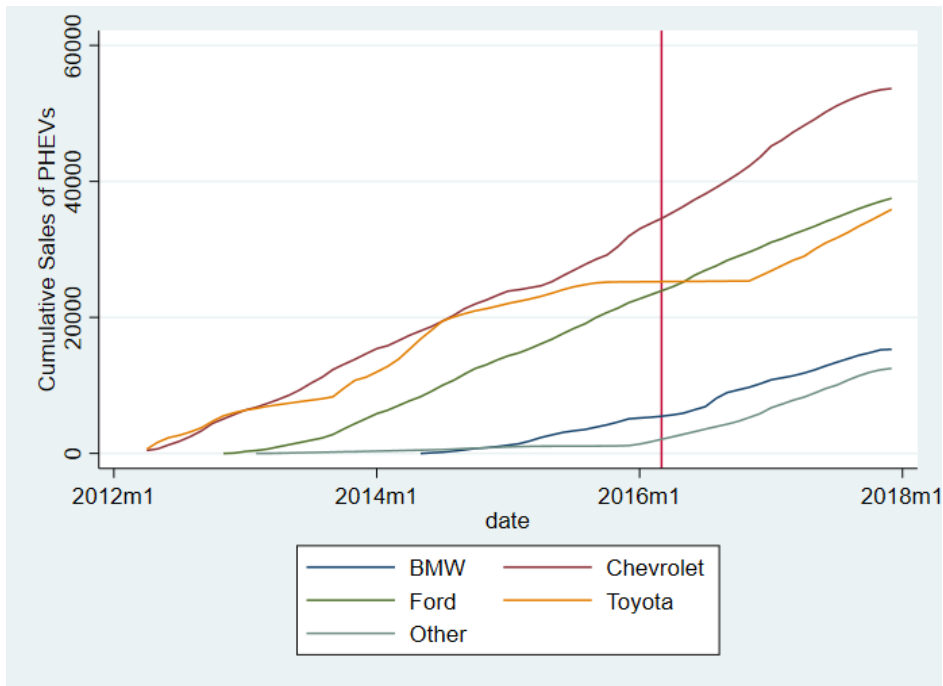


Figure 6. New BEV Transactions by Make

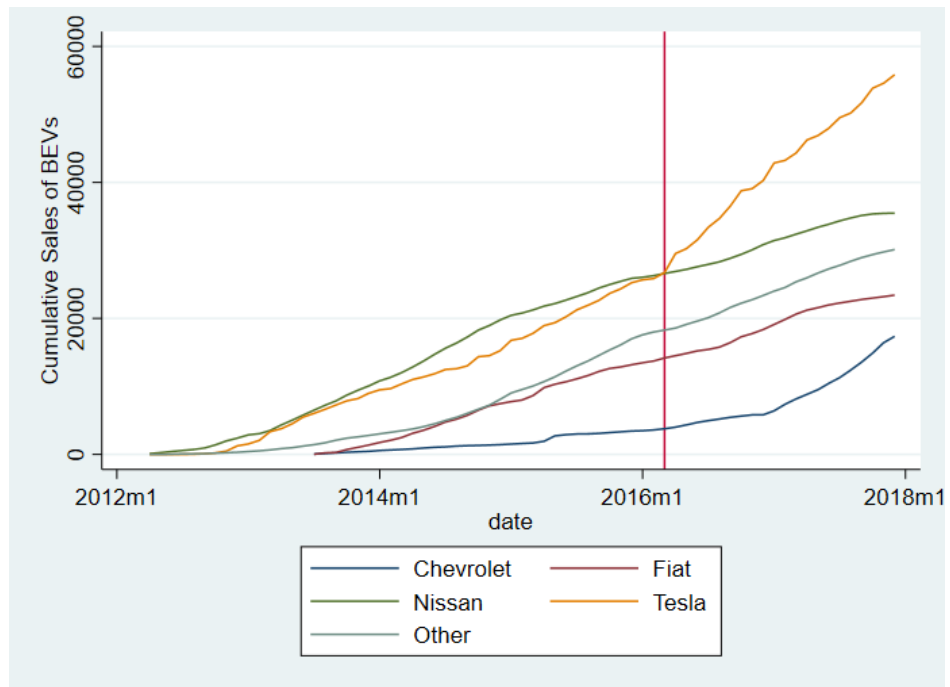


Figure 7. Geographic Dispersion of EV Transactions (through 2015)

Total New EV Transactions (through 2015)

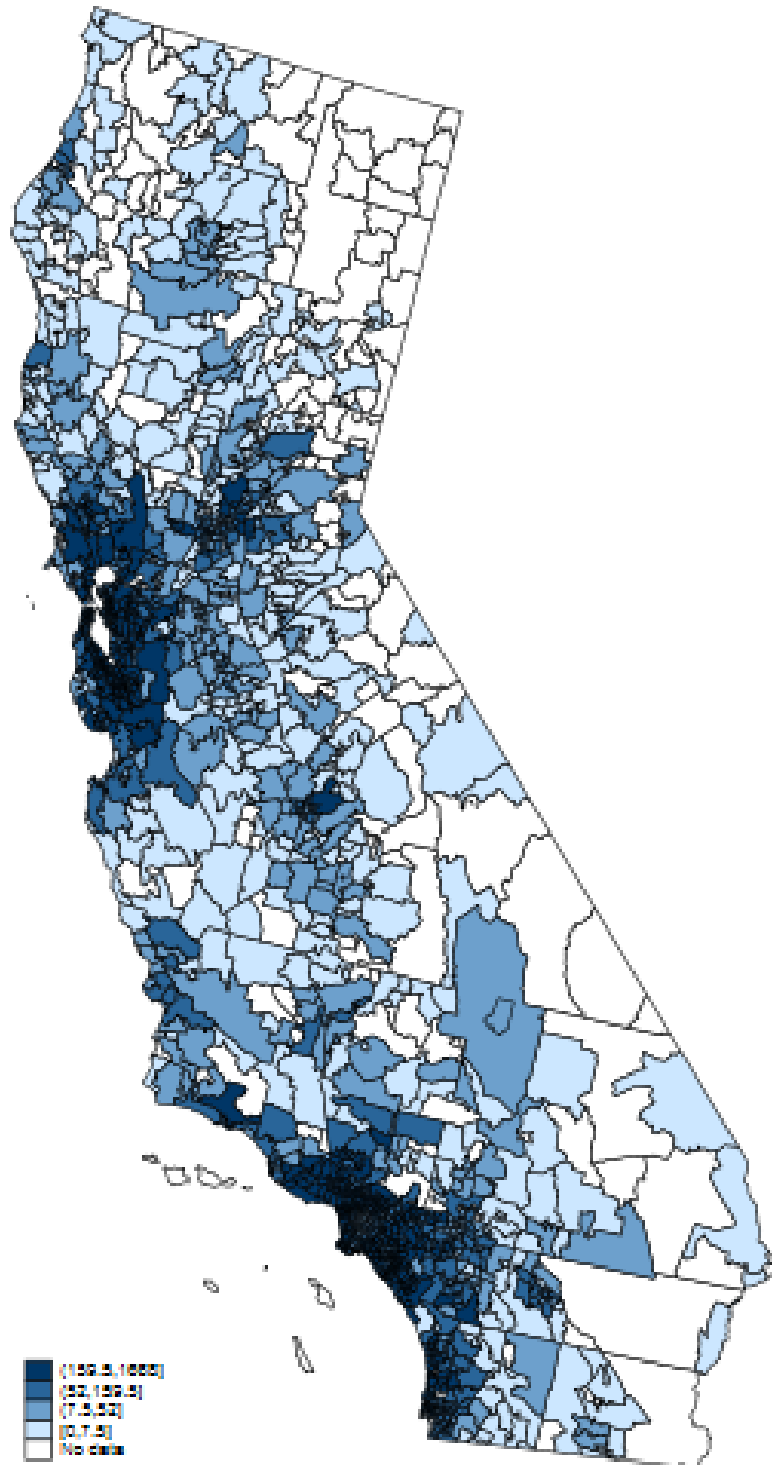


Figure 8. Number of CVRP Applications Over Time

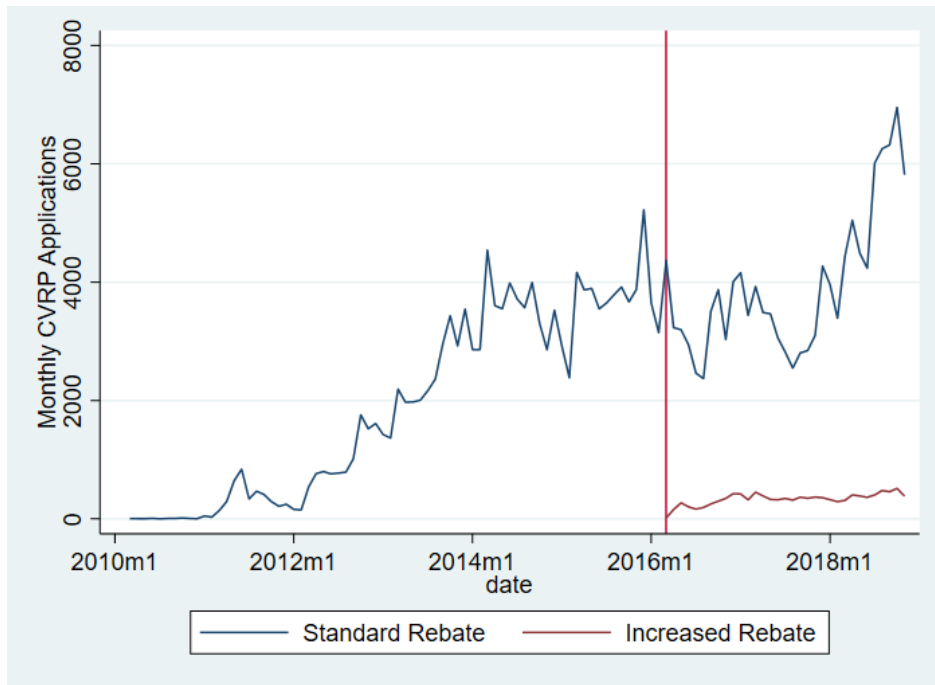
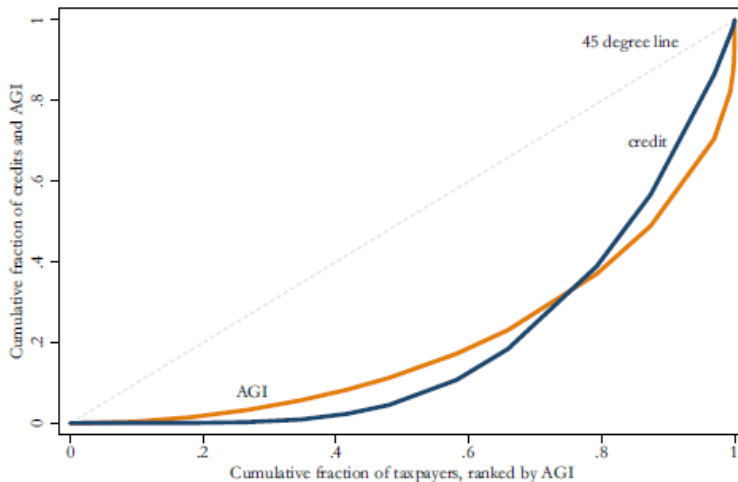


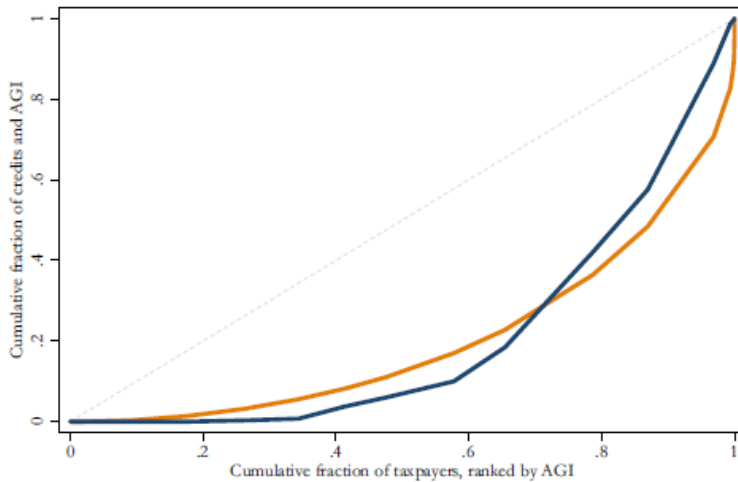
Figure 9. Borenstein and Davis

Figure 7: Concentration Curves

A: Residential Energy Credits, 2006-2012



B: Alternative Motor Vehicle Credit, 2007-2012



C: Qualified Plug-in Electric Drive Motor Vehicle Credit, 2009-2012

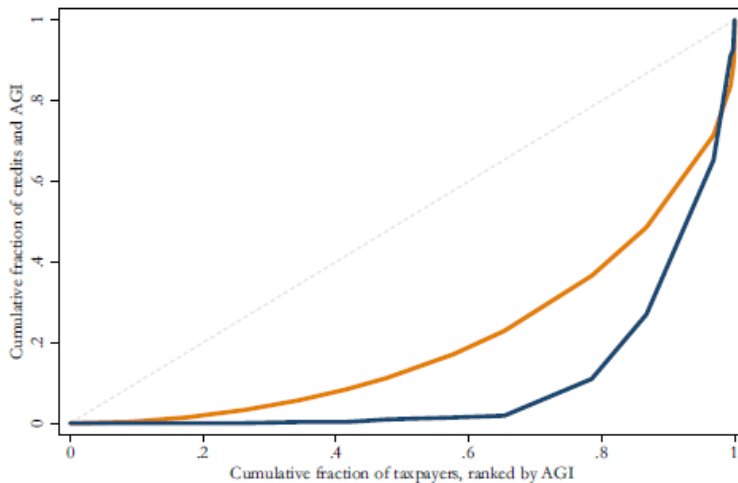


Figure 10. CVRP Funding Concentration Curve

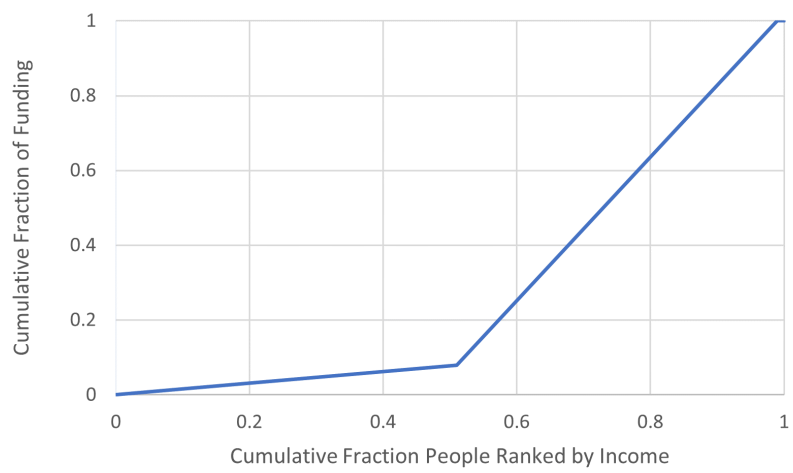
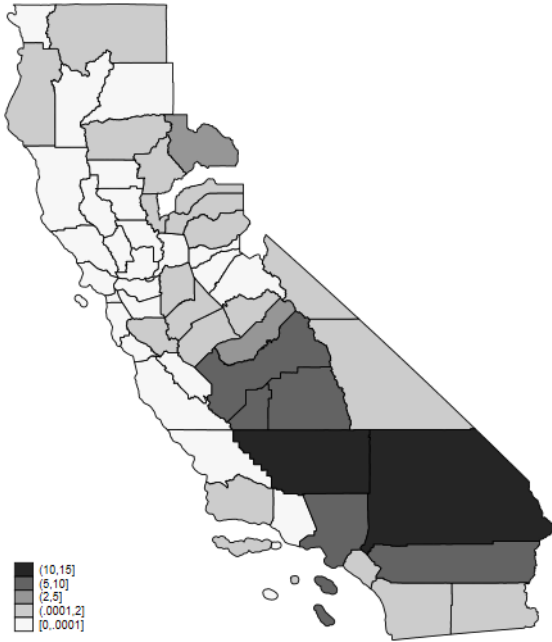
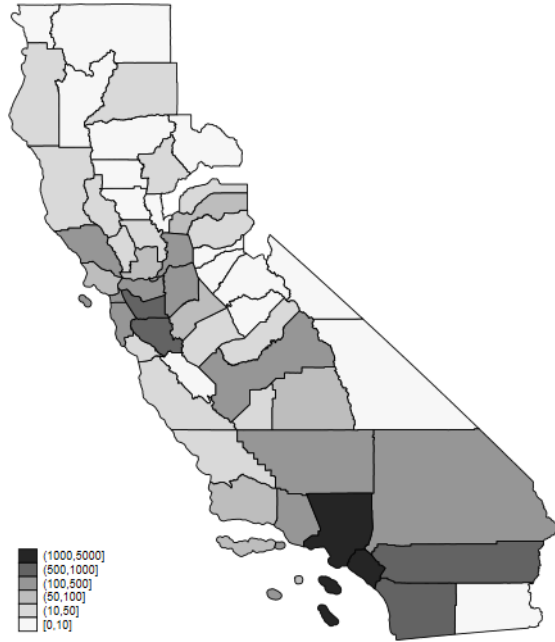


Figure 11. Air Quality (Left) and Low Income Rebates (Right)

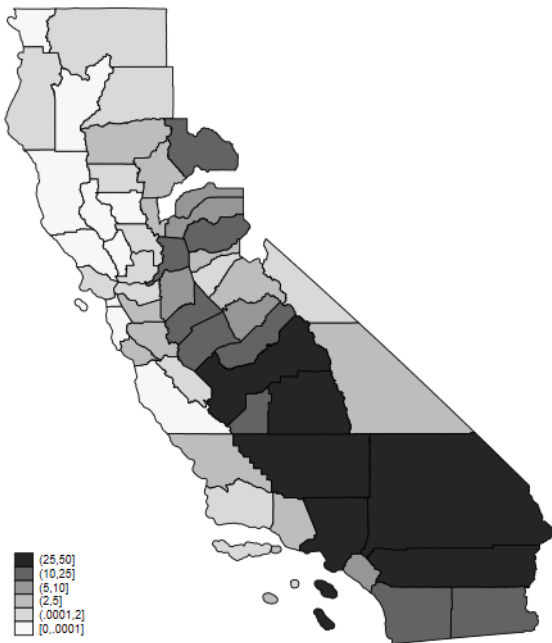
Proportion of Days with Unhealthy AQI in 2014



Number of CVRP Applications Applying for Low Income Rebate



Proportion of Days Unhealthy for Sensitive Groups AQI in 2014



Proportion of CVRP Applications Applying for Low Income Rebate

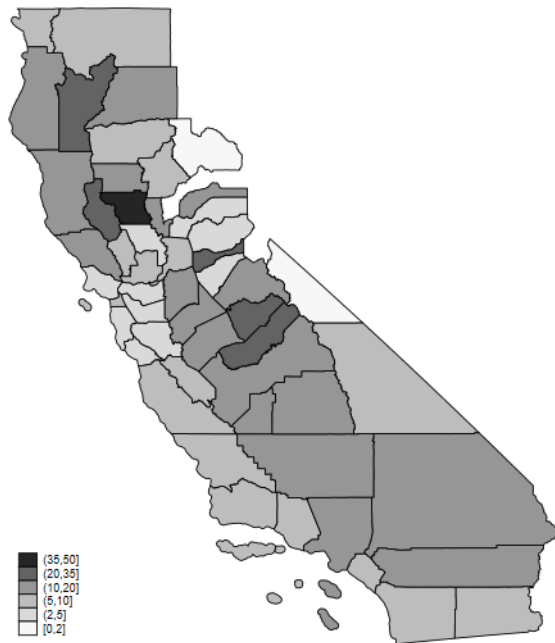


Table 15. CVRP Rebate Amounts

Start:	3/1/10	6/18/11	7/4/13	6/1/14	3/29/16	11/1/16	12/3/19
PHEV	3,000	1,500	1,500	1,500	1,500	1,500	1,000
BEV	3,000-5,000 ¹	1,500-2,500	2,500	2,500	2,500	2,500	2,000 ⁴
FCEV	3,000-5,000	1,500-2,500	2,500	5,000	5,000	5,000	4,500
PHEV	-	-	-	-	3,000	3,500	3,500
BEV	-	-	-	-	4,000	4,500	4,500
FCEV	-	-	-	-	6,500	7,000	7,000

¹ In the early years of the program, the rebate amount for BEVs and FCEVs depended on the range of the vehicle. Vehicles were either short or long range and received rebates of 3,000 and 5,000 respectively.

² Increased Low Income Rebates were available beginning March 29th, 2016.

³ Rebates are offered for neighborhood electric vehicles and battery electric motorcycles as well.

⁴ Tesla vehicles eligibility ends on March 15th, 2022.

Table 16. Maximum Income to Qualify for Standard Credit

	3/29/16	11/1/16	2/24/22
single filers	250,000	150,000	135,000
head of household filers	340,000	204,000	175,000
joint filers	500,000	300,000	200,000

¹ Prior to March 29th, 2016 there is no income cap.

² Income requirements do not apply to fuel cell electric vehicles.

Table 17. Maximum Income to Qualify for Low Income Credit

	Start:	3/29/16	11/1/16	7/1/17	7/1/18	7/1/19
Household Size	1	35,310	35,640	36,180	36,420	37,470
	2	47,790	48,060	48,720	49,380	50,730
	3	60,270	60,480	61,260	62,340	63,990
	4	72,750	72,900	73,800	75,300	77,250
	5	85,230	85,320	86,340	88,260	90,510
	6	97,710	97,740	98,880	101,220	103,770
	7	110,190	110,160	111,420	114,180	117,030
	8	122,670	122,670	123,960	127,140	130,290
+1 Member		12,480	12,480	12,540	12,960	13,260

¹ All requirements are based on 300% of the Poverty Guidelines (also referred to as Federal Poverty Levels). These are released in the first quarter of the year and going forward this program will update requirements the following July.

² In 2022, this changed to 400% of the Poverty Guideline.

Table 18. EV Data Tabulation

	Daily	Monthly
0	2,541,331	38,212
1	192,242	13,993
2	37,589	8,749
3	9,106	6,644
4	2,596	4,890
5	840	4,018
6	290	3,227
7	124	2,550
8	50	2,120
9	29	1,706
10	8	1,393
11-20	24	6,000
21-30	10	1,303
31-40	0	363
41-50	6	128
51-100	6	63
> 100	0	3

Table 19. Monthly Data Tabulation

Quantity of Vehicles	all EVs	BEVs	PHEVs
0	40,895	55,706	50,474
1	14,540	15,425	16,821
2	8,939	8,486	10,071
3	6,760	5,298	6,643
4	4,952	3,624	4,427
5	4,069	2,451	3,107
6	3,246	1,768	2,148
7	2,570	1,293	1,577
8	2,129	954	1,159
9	1,716	759	755
10	1,398	542	527
11-20	6,010	2217	1,299
21-30	1,303	427	71
31-40	363	99	4
41-50	128	21	0
51-100	63	11	1
> 100	3	3	0

Table 20. Data Summary for Zip Codes By PPLI_{300%} *Quartile*

	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
Demographics				
PPLI ₃₀₀	.0863 (.0434)	.1986 (.0279)	.3053 (.0354)	.4702 (.0860)
PPLI ₅₀	.0124	.0272	.0424	.0676
PPLI ₁₀₀	.0256	.0576	.0944	.1583
PPLI ₁₅₀	.0408	.0934	.1539	.2548
PPLI ₂₀₀	.0507	.1292	.2098	.3396
PPLI ₄₀₀	.1145	.2598	.3769	.5542
PPLI ₅₀₀	.1425	.3137	.4327	.6106
Median Income	82,300 (35,338)	72,992 (21,952)	58,669 (16,138)	49,068 (13,051)
PPHI _{100k}	.00234	.00261	.00362	.00273
PPHI _{150k}	.00132	.00128	.00173	.00090
PPHI _{200k}	.00085	.00068	.00104	.00041
New EV Transactions				
Pre-Means Testing per month	3.702 (6.591)	3.530 (4.674)	2.373 (4.116)	1.310 (2.120)
Post-Means Testing per month	6.202 (9.970)	6.439 (7.332)	4.325 (5.153)	2.543 (3.223)

Standard deviations in parentheses.

Table 21. Main Regressions for all EVs

	(1)	(2)	(3)	(4)
	Linear	Log Linear	Standardized	Poisson
$\mathbb{1}(\text{date} > 3/16) * PPLI_{300\%}$	-64.85 (53.61)	-0.196** (0.0693)	0.0602 (0.0964)	0.0517 (0.0931)
$\mathbb{1}(\text{date} > 10/16) * PPLI_{300\%}$	9.448 (20.47)	0.0489 (0.0646)	0.190* (0.0943)	0.320*** (0.0709)
$\mathbb{1}(\text{date} > 3/16) * PPHI_{250k}$	-356.7 (849.6)	4.528 (5.147)	4.710 (9.664)	7.441 (7.428)
$\mathbb{1}(\text{date} > 10/16) * PPHI_{150k}$	230.6 (342.9)	-9.797*** (2.846)	-7.260 (5.024)	-12.97*** (3.442)
Date FE	X	X	X	X
Zip Code FE	X	X	X	X
N	94323	57150	92805	92805

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 22. Regressions with all EVs and Finer Income Categories

	(1)	(2)
$\mathbb{1}(\text{date} > 3/16) * PPLI_{300\%}$	0.0517 (0.0931)	
$\mathbb{1}(\text{date} > 10/16) * PPLI_{300\%}$	0.320*** (0.0709)	
$\mathbb{1}(\text{date} > 3/16) * PPLI_{200-300\%}$		-0.499 (0.552)
$\mathbb{1}(\text{date} > 10/16) * PPLI_{200-300\%}$		1.313** (0.501)
$\mathbb{1}(\text{date} > 3/16) * PPLI_{100-200\%}$		-0.0892 (0.450)
$\mathbb{1}(\text{date} > 10/16) * PPLI_{100-200\%}$		0.130 (0.468)
$\mathbb{1}(\text{date} > 3/16) * PPLI_{100\%}$		0.641 (0.342)
$\mathbb{1}(\text{date} > 10/16) * PPLI_{100\%}$		-0.229 (0.353)
$\mathbb{1}(\text{date} > 3/16) * PPHI_{250k}$	7.441 (7.428)	7.488 (7.339)
$\mathbb{1}(\text{date} > 10/16) * PPHI_{150k}$	-12.97*** (3.442)	-13.37*** (3.381)
Date FE	X	X
Zip Code FE	X	X
N	92805	92805

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 23. Regressions with EVs by Price Quartile

	(1) Bottom 25%	(2) Bottom 50%	(3) Bottom 75%	(4) All
$\mathbb{1}(date > 3/16) * PPLI_{300\%}$	0.232 (0.195)	0.550*** (0.127)	0.642*** (0.101)	0.0517 (0.0931)
$\mathbb{1}(date \geq 11/16) * PPLI_{300\%}$	1.255*** (0.182)	0.727*** (0.115)	-0.0634 (0.0772)	0.320*** (0.0709)
$\mathbb{1}(date > 3/16) * PPHI_{250k}$	-28.11 (27.04)	-50.84** (17.87)	-14.01 (9.484)	7.441 (7.428)
$\mathbb{1}(date \geq 11/16) * PPLI_{150k}$	-20.85 (15.12)	7.077 (9.776)	2.910 (5.330)	-12.97*** (3.442)
Date FE	X	X	X	X
Zip Code FE	X	X	X	X
N	88872	91770	92943	92805

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 24. Regressions by EV and Transaction Types

	All Transaction Types			Only Purchases			Only Leases		
	(1) EV	(2) BEV	(3) PHEV	(4) EV	(5) BEV	(6) PHEV	(7) EV	(8) BEV	(9) PHEV
$\mathbb{1}(\text{date} > 3/16) * PPLI_{300\%}$	0.0517 (0.0931)	-0.361* (0.152)	0.734*** (0.105)	-0.474*** (0.133)	-0.108 (0.231)	0.745*** (0.171)	0.156 (0.122)	-0.445* (0.203)	0.704*** (0.108)
$\mathbb{1}(\text{date} \geq 11/16) * PPLI_{300\%}$	0.320*** (0.0709)	0.475*** (0.0994)	-0.0418 (0.0907)	1.363*** (0.129)	0.923*** (0.150)	0.530** (0.164)	-0.134 (0.0755)	0.271* (0.117)	-0.472*** (0.0974)
$\mathbb{1}(\text{date} > 3/16) * PPHI_{250k}$	7.441 (7.428)	8.940 (6.341)	0.513 (12.67)	-2.467 (7.425)	-29.45** (11.29)	-24.34 (22.77)	17.40* (8.848)	36.57** (12.90)	-4.200 (10.07)
$\mathbb{1}(\text{date} \geq 11/16) * PPHI_{150k}$	-12.97*** (3.442)	-9.176* (4.124)	-14.92*** (4.455)	-12.83* (5.732)	1.588 (8.601)	-6.044 (9.808)	-14.76*** (3.688)	-16.99** (5.288)	-11.34* (4.571)
Date FE	X	X	X	X	X	X	X	X	X
Zip Code FE	X	X	X	X	X	X	X	X	X
N	92805	88182	90321	90666	83145	88458	87837	83697	82938

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 25. Elasticity Estimates for Low Income Group

Assumptions	Elasticity Estimate
All people below 300% are the same Car types are evenly distributed (46% BEV) (Avg. car price \$47,000)	-6.8
All people below 300% are the same Cars bought by below 300% are below 75th price percentile (29% BEV) (Avg. car price \$37,000)	-7.5
All people below 300% are the same Cars bought by below 300% are below median price (17% BEV) (Avg. car price \$31,000)	-10
Focusing on people between 200%-300% Car types are evenly distributed (46% BEV) (Avg. car price \$47,000)	-12

Table 26. Donut Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Month Radius	1	2	3	4	5	6	7
$\mathbb{1}(date > 3/16) * PPLI_{300\%}$	0.377*** (0.0916)	0.421*** (0.0941)	0.407*** (0.0953)	0.390*** (0.0980)	0.402*** (0.101)	0.340*** (0.102)	0.304*** (0.109)
$\mathbb{1}(date > 3/16) * PPHI_{250k}$	7.087 (25.24)	0.593 (26.36)	3.903 (26.79)	1.392 (29.25)	-12.47 (31.45)	-11.57 (32.82)	-23.78 (37.12)
$\mathbb{1}(date \geq 11/16) * PPHI_{150k}$	-12.42 (15.71)	-8.930 (16.11)	-10.34 (16.70)	-9.353 (18.05)	-0.414 (18.99)	-2.003 (19.92)	4.224 (22.43)
Date FE	X	X	X	X	X	X	X
Zip Code FE	X	X	X	X	X	X	X
N	78942	76095	73315	70384	67626	64876	61946

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 27. Event Study

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Month Radius:	14	12	10	8	6	4	2
$\mathbb{1}(\text{date} > 3/16) * PPLI_{300\%}$	0.0353 (0.0924)	0.0367 (0.0930)	0.0628 (0.0933)	0.0727 (0.0954)	0.0668 (0.0977)	0.0582 (0.101)	0.0776 (0.104)
$\mathbb{1}(\text{date} \geq 11/16) * PPLI_{300\%}$	0.320*** (0.0708)	0.302*** (0.0699)	0.337*** (0.0716)	0.351*** (0.0726)	0.275*** (0.0774)	0.262** (0.0829)	0.286** (0.108)
$\mathbb{1}(\text{date} > 3/16) * PPHI_{250k}$	7.481 (7.386)	7.887 (7.435)	8.883 (7.430)	9.059 (7.590)	7.990 (7.761)	6.349 (8.067)	5.835 (8.257)
$\mathbb{1}(\text{date} \geq 11/16) * PPHI_{150k}$	-13.08*** (3.459)	-12.64*** (3.699)	-13.66*** (3.712)	-13.28*** (3.601)	-14.86*** (3.851)	-15.16*** (4.587)	-15.59** (5.347)
Date FE	X	X	X	X	X	X	X
Zip Code FE	X	X	X	X	X	X	X
N	89847	84357	78529	72820	67014	61335	55814

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Free Money: When Subsidy Take-Up is Less than 100%

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Abstract

This paper focuses on the incomplete take-up of California Clean Vehicle Rebate Project rebates. In the first six years of the program, all new electric vehicle owners in California were eligible for rebates ranging from \$900 to \$5000, but only around 75% of the buyers submitted applications for a rebate. Due to data limitations, this paper focuses on the period from April 2012 through March 2016. Using linear probability and Probit models, I find that those purchasing more expensive vehicles and buyers living in areas with a higher proportion of low income households were less likely to apply. Additionally, buyers living in the same zip code as the dealer where they purchased the vehicle all applied for a rebate. Finally, purchasing an electric vehicle from a dealer that is larger or further away from the buyers registration location both were correlated with higher likelihood of applying for CVRP. These results are then put into the context of policy implications.

7.1 Introduction

Over the past decade, governments around the world both locally and nationally have spent billions of dollars (USD) subsidizing alternative fuel vehicles. With the proposal of the Inflation Reduction Act, the Congressional Budget Office (CBO) estimates that the United States federal government alone is expected to spend \$1.775 billion dollars from now through 2026 on new clean vehicle tax credits. This amount does not take into account the planned used clean vehicle credits, commercial clean vehicle credits or the clean vehicle charging infrastructure credits which would increase this total during that time to \$3.689 billion¹. Given the size of these programs, it's important to understand the realized impacts. One of the largest subsidy programs for alternative fuel vehicles is the California Clean Vehicle Rebate Project (CVRP).

This paper focuses on take-up the CVRP - where are dollars going and who is taking advantage of these significant rebates. During the period focused on here, 2012 through 2015, California was the largest market for electric vehicles (EVs) in the world. Since then the EU and China have surpassed the US. Hundreds of millions of dollars have been given out through this program.

CVRP is a rebate program for new electric vehicles (EVs) purchased or leased by Californians which began in 2010. The Center for Sustainable Energy (CSE) administers this program on behalf of the California Air Resources board (CARB). The rebate amounts available to owners of new EVs varies over time and across vehicle types, including battery electric vehicles (BEVs), plug-in hybrid electric vehicles (PHEVs), and fuel cell electric vehicles (FCEV). Rebate amounts are summarized in table 28. The table shows that during the early years of the program, the only necessary requirement for eligibility was the purchase or lease, with length of at least three years, of an EV. Eligibility requirements changed in 2016 when means testing was introduced to the program. These income re-

¹More information on HR5376 can be found on the CBO's website: https://www.cbo.gov/system/files/2022-08/hr5376_IR_Act_8-3-22.pdf

quirements sorted people into three categories, for the middle group, nothing changed, but rebates increased for low and medium income households and rebates were eliminated for very high income households. The inclusion of means testing makes determining take-up of the program more difficult, so this paper will use data from before this change.

Specifically, I will look at the association between the existence of a CVRP application and vehicle, dealer, and buyer characteristics. The available data on the buyers themselves are limited, but the location of vehicle registration is known, so demographic variables at the zip code level from the Census can be used to get an idea of who the buyers are. VIN is provided in these data, so vehicle characteristics are known, and purchase price is also available. Finally, data is available on the selling dealers and their location. Using both linear probability and Probit models, I find that both vehicle price and the proportion of the buyer's zip code population that falls below 300% percent of the federal poverty guideline are negatively associated with application for CVRP, and that buyers who purchased their EV from a larger dealer or a dealer further away were more likely to apply for a rebate. Interestingly, I also find that all buyers who live in the same zip code as the dealer they purchase from apply for a CVRP rebate. Analyzing program take up is important to understand the fiscal implications, understand program equity, and inform information and educational campaigns.

Given the size and timing of this programs, there is existing literature focusing specifically on this program. In addition, there is a plethora of literature focusing on the take up of government programs. This paper aims to fill the gap in the overlap of these two topics and help understand why CVRP take up is incomplete.

Existing literature on the CVRP includes papers utilizing survey data, data from the program, and DMV data. Canepa, Hardman and Tal (2019) explore EV adoption in disadvantaged communities (DAC) in California in a descriptive way. They utilize CVRP application data to determine that the proportion of new EVs per household is lower in

DAC than in other census tracts. Ju, Cushing and Morello-Frosch (n.d.) finds that with means testing introduced in 2016, the CVRP still issued more rebates per household to advantaged, White communities and the overall number of rebates distributed was reduced. Using data on CVRP applications and spatial autoregressive modelling, Bryan (2019) finds a positive, significant increase in rebate applications in areas with high concentrations of low income households with the 2016 policy change. Guo and Kontou (2021) evaluate the distributional equity of the CVRP. They find that, over time and with the 2016 policy change, the share of rebates distributed to DAC and low income groups increased. Additionally, they find high spatial clustering in metropolitan areas and significant neighborhood effects increasing rebate amounts in DACs next to communities with already high rebate amounts. The literature on this policy generally focuses on the introduction of means testing but does not touch on the incomplete take up through the entire history of the program.

There is a significant amount of literature on incomplete take-up within governmental programs. However, this literature focuses primarily on welfare programs. Currie (2006) and Ko and Moffitt (2022) both provide overviews of existing literature on take-up. Currie (2006) identifies patterns across this literature including administrative barriers and potential stigma. Ko and Moffitt (2022) focus on theories for incomplete take-up as well. These include the small size of gains from participation, stigma, costs to participation, imperfect information, administrative barriers, and measurement error. Tempelman and Houkes-Hommes (2016) use administrative data, like this paper, to identify eligible households and take-up. They focus on a large Dutch social welfare program and find that in addition to private cost benefit analysis causing people to decide not to apply for the program because they deem the costs high than the benefits, the very lowest income households were not the most likely to take up the program, which does not align with this hypothesis. Many papers identify that simplification is needed to increase take-up and keep eligible parties participating in a variety of social welfare programs including

SNAP (Guyton et al., 2017). Finkelstein and Notowidigdo (2019) utilize an RTC to explore ways to increase SNAP participation. They find, "only 6% of the control group enrolls in SNAP over the next nine months, compared to 11% of the Information Only group and 18% of the Information Plus Assistance group. The individuals who apply or enroll in response to either intervention have higher net income and are less sick than the average enrollee in the control group. We present evidence consistent with the existence of optimization frictions that are greater for needier individuals, which suggests that the poor targeting properties of the interventions reduce their welfare benefits." (Finkelstein and Notowidigdo, 2019) Recent work, Fuchs et al. (2020), discusses the changes made in Austria to monetary social welfare that improved take up "A higher degree of anonymity within the claiming process, the provision of health insurance, binding minimum standards, the limitation of the maintenance obligations, new regulations related to the liquidation of wealth, as well as the general coverage of the benefit reform in the media and in public discussions led to an improved access to the benefit." (Fuchs et al., 2020)

While the same themes of administrative barriers, costs to participation, small size of gains from participation, stigma, and imperfect information are important when it comes to social welfare programs, most of these theories do not apply to CVRP. First, the amount of money offered from this program relative welfare programs is much larger. Second, welfare programs often have significant barriers with applications and interviews. Whereas, CVRP is a simple application with proof of registration. Third, there is often a stigma associated with social programs that does not necessarily exist for green initiatives. Finally, this program is targeting a very different population on average, a population where these hypotheses are largely untested.

While there is significant literature on social welfare program take-up, there is little written about the take up of subsidies for green programs. While there has been increased interest in where dollars end up going from clean energy programs, there has not yet been a focus on the other side of this equation. For the purpose of program design going

forward and analysis of program impacts, understanding who is and is eligible and not taking advantage of these dollars is an important piece of the puzzle. Incomplete take up in the case of CVRP is especially interesting because of the size of the subsidies and the simplicity of measuring who is eligible, particularly in the early years of the program before the implementation of means testing.

Analysis in this paper suggests that incomplete take up of the CVRP is at least partially due to buyers of expensive EVs choosing not to take advantage of the available subsidies either due to a barrier or by choice. In addition, there is some suggestive evidence that those with income below 300% of the Federal Poverty Guideline are also less likely to take advantage of CVRP rebates. More information is needed to know why this is occurring. It could be do to a lack of information or a lack or something else entirely.

The following section describes the data sets used in this paper. Section three elaborates on the methods used for the descriptive analysis. Results are presented in section four, and the final section concludes.

7.2 Data

Two main data sources provide information on electric vehicle purchases and applications for the California Clean Vehicle Rebate Project. First, DMV data from Experian for EVs provide the VIN of all battery electric and plug-in hybrid electric vehicles in California from April 2, 2012 to December 22, 2017. Second, data provided by CVRP provide VIN of all vehicles with applications for the subsidy from the introduction of the program in 2010 through June 30, 2019. These can then be matched which allows one to explore those data can be matched by VIN and those that do not.

Experian data includes VIN, vehicle characteristics, registration date, zip code of registration, purchase price, and dealer information. CVRP data includes VIN, application date, whether the application was accepted or rejected, and rebate amount among others. In matching the Experian and CVRP datasets, Table 29 describes the observations that

appear in both datasets, just over 43%. The second row provides the observations that appear in the Experian data as newly registered vehicles but do not have an associated application in the CVRP data which is about 29% of the observations. The third row summarizes the vehicles that are observed as having an application in the CVRP data, but do not appear in the Experian data.

Many of the vehicles that appear in the CVRP application data do not appear in the Experian data because of the time period covered by each. While the CVRP application data begins at the beginning of the program and ends in June of 2019, the Experian data begins in April 2012, so there are many vehicles for which applications are submitted, but purchase data was before the start of the data. Additionally, there are vehicles at the end of the CVRP dataset that were purchased after the end of the Experian data in December 2017. Figure 12 provides insight into the timing of the vehicles that are missing from the Experian data but have a CVRP application. One can see that the majority of these observations are from applications that occur after the Experian data period has ended or predate the start of the Experian data. The figure also shows a spike in application observations that are not found in Experian at the beginning on 2016. Further exploration of this anomaly is presented in Appendix A which concludes that there does not appear to be any specific pattern to these missing observations. However, main analysis will exclude this period in case.

Figure 13 through 19 further explore the observations for which registration data exists. 13 shows time series for the Experian data split into those with CVRP application and those without. With a change to the eligibility requirements in March 2016, this figure suggests that a higher proportion of vehicles do not have associated CVRP applications. Whether that is due to limited eligibility or personal choice, it is not possible to tell. The matching of these data can also be presented by make which begins to provide insight into where CVRP funds are going in practice.

Figures 14 through 16 break down matching into the top 9 makes of EVs and group

the rest into the "other" category. Figure 14 shows in green that it is more common for Hondas and Teslas to not appear in the Experian data and that is because of the vehicles offered during the period covered are not covered by this dataset. Tesla was increasing its volume significantly over this period, so many of these sales happen after the end of the registration data and are picked up only in the CVRP application data. Figures 15 and 16 focus only on the vehicles that appear in the registration data, but splits the data into two periods, the period where all EV owners were eligible and the period where means testing is implemented limiting eligibility. In both periods, the more expensive makes have a larger proportion of vehicles without CVRP applications. This will be explored further later.

Finally, out of curiosity, time to application is explored in figures 17 through 19. Application for CVRP is required within 180 days of registration to be eligible. Figure 17 provides a histogram of all time to application. There is a wide range which exceeds the parameters set by CARB, and the median time to application is actually negative, meaning that applications are taking place before registration. This is likely due to applications submitted at time of purchase and delays in processing vehicle registration relative to application date. Figure 18 breaks down time to CVRP application by make and figure 19 shows variation in time to CVRP application over the registration data period.

In addition to these, I use demographic data from the Census which includes zip code, median income, and the proportion of the population below 300% of the Federal Poverty Guideline. While this dataset cannot be matched by VIN to the others, they can be matched by geographic area, zip code. Since we do not have information about the individuals purchasing or leasing EVs, this allows us to say something about their likely income level.

Summary statistics for all variables used in the analysis can be found in 30. This table limits the number of observations to those that appear in the DMV data between April 2, 2012 and December 31, 2015. During this period, just over 63% of EVs have an associ-

ated application for the CVRP. Average sales price for these vehicles was approximately \$37,500 and when the average by vehicle model is taken, the prices vary from around \$18,800 to \$121,100. The variation in sales price for EVs during this period is enormous. The following rows summarize the demographic data. The mean of median income by the zip code of the buyer is just over \$89,000 annually and the proportion of people with income that falls under 300% of the federal poverty guideline (FPG) is around 34%. Both vary a lot across zip codes with median income from \$10,700 to \$220,400 and proportion of the population below 300% of the FPG from 9.9% to 93.4%. Finally, this table explores the data on dealerships which includes their location and sales. Sales is calculated by summing all EV sales that appear in this data for each unique dealer which ranges from 1 to almost 5,000 EVs. Dealer zip code is also provided, so distance from the center of the dealer and buyer zip codes can be calculated using the haversine formula which appears in equations labeled (10). Where ϕ are the latitudes of the two zip code latitudes, λ are the two longitudes, and R is the radius of the Earth, 6371 km. There is also significant variation in this variable with around 5% of these to be zero because the buyer and dealer are both located in the same zip code.

$$a = \sin^2(\Delta\phi/2) + \cos(\phi_1)\cos(\phi_2)\sin^2(\Delta\lambda/2) \tag{10}$$

$$d = 2R * \text{atan2}(\sqrt{a}, \sqrt{1-a})$$

Figures 27 and 28 provide more insight into the distance between the dealer and buyer zip codes and the dealer size variables. Both are skewed right. Most EV sales are done by dealers within 25 km of the buyer and by dealers who sold less than 500 EVs over the almost four year study period. The significant distance between the some buyers and dealers could be due to the limited supply of EVs during the period which differs from that of an average gasoline vehicle. It seems that some buyers of EVs during this period had to look over a broader geographic area than the average new vehicle buyer, but most buyers were able to purchase an EV quite close to home.

7.3 Methods

This paper is descriptive in nature. Methodology, therefore, is primarily utilizing regression frameworks to identify correlations between various demographic, vehicle characteristic, and dealer data and CVRP take-up. I present these patterns in intuitive tables and figures. Both linear and nonlinear regression models are used.

Due to the binary nature of whether a buyer applied or did not apply to CVRP, Logit and Probit models are preferable. This paper utilizes Probit models. Linear probability models are also presented, but the underlying assumptions do not hold. The linear probability model is represented in regression 11. Regression 12 is the Probit model where $\Phi(\cdot)$ is the cumulative standard normal distribution function. The $P(app = 1|\mathbf{X})$ is the probability that binary variable app is equal to 1 meaning that the individual applied for a CVRP rebate. given the explanatory variables, \mathbf{X} .

$$P(app = 1|\mathbf{X}) = \mathbf{X}\beta + \epsilon \quad (11)$$

$$P(app = 1|\mathbf{X}) = \Phi(\mathbf{X}\beta + \epsilon) \quad (12)$$

The benefit of linear probability models is the simplicity of interpreting the coefficients. Each coefficient represents the marginal change in probability of applying for CVRP associated of a one unit increase in the independent variable, on average.

The Probit regression coefficients give the change in the z-score of the probability of CVRP application for a one unit change in the independent variables. In order to interpret the Probit models at the margins, we have to set values for all other independent variables. Typically these are set to their mean value for this evaluation. It's important to keep this in mind when looking at regression results.

Explanatory variables included in \mathbf{X} are the sales price of the vehicle, the average sales price for the vehicle model, the median income for the zip code where the vehicle is

registered, the proportion of the population below 300% of the federal poverty guideline in the zip code where the vehicle is registered, whether the buyer and dealer share the same zip code, the distance from the dealer's zip code to the registration zip code, and the number of electric vehicles sold by the dealer during the period of interest. I expect the sales price or average model sales price to be negatively associated with the probability of CVRP application because people who buy expensive vehicles are less likely to care about the rebate money. Similarly, I expect median zip code income to be negatively associated with application probability. However, these two explanatory variables may be capturing a similar cause, so when controlling for one, I do not have a solid expectation for what the impact of the other will be. I include the proportion of the zip code population with income below 300% of the federal poverty guideline because this becomes an important cutoff point in the months following the period studied here. I would expect this to capture the lower end of the income spectrum and how they take up the program, which given the lack of barriers to application, I would expect to result in a positive correlation with probability of application. Finally, I would expect that the closer the location of dealer and buyer, the higher the probability of application and the larger the number of EVs the dealer sells, the higher the probability of application because I think these dealers are more likely to pass on information to the buyer and even provide application assistance.

7.4 Results

Before getting into regression results, bivariate relationships are explored graphically. These are followed by multivariate regression analysis which provides further insight on the bivariate relationships as scatter plots and a bivariate linear probability regressions are illustrated in Figures 20 through 23. Note that the data used for all of these figures and regressions only includes vehicles registered before 2016 due to the data inconsistencies in early 2016 and the introduction of means testing in March 2016.

Figure 20 shows a scatter plot of binned data as well as a the relatively strong relationship between vehicle price and the probability of applying for CVRP. This is great news if we are concerned that people taking advantage of CVRP are primarily wealthy people buying very expensive cars that they would buy regardless of the rebate or if there was concern that those not taking up this are lower income households that are lacking information or the time and energy to apply.

Figure 21 illustrates the negative relationship between the median income of the zip code where the owner registered the vehicle and the existence of a CVRP application. While this relationship is less extreme relative to price, it is still statistically significant in a bivariate regression. Again, policy makers would likely be pleased with this result as it suggests that take-up is higher among EV buyers with relatively lower income. Additionally, the positive correlation between the proportion of the population with income below 300% of the Federal Poverty Guideline and the probability of CVRP application illustrated in 22 suggests relatively higher take up for lower income households.

In the fourth bivariate graph, Figure 23, the linear regression tells a story of negative association between the number of EVs sold by a dealer and probability the buyer applies for CVRP, but looking at the scatter plot, this is clearly driven by one large dealer, Tesla. In fact, ignoring Tesla, the data tell the opposite story where customers at larger dealers were more likely to apply.

The final scatter plot, figure 24, shows a negative correlation between the distance between the two parties and the probability that a buyer applies for a rebate. This could provide evidence that the relationship between buyer and dealer is important for passing on information or assistance to the buyer around CVRP impacting their propensity to apply.

Tables 31 and 32 present initial regression results which utilize registration data through the end of 2015 as there is a measurement error issue where data is missing in the early months of 2016 and then the introduction of means testing in March of 2016 which lim-

its rebate eligibility. These two tables are replicated utilizing registration data up until March, 29th 2016 and are summarized in Appendix A.

Linear probability model regression results are presented in 31. Column 1 utilizes the sales price of the vehicle as an explanatory variable and standard errors are clustered at the zip code. The negative coefficient is a good sign for the efficiency of this subsidy as households purchasing higher priced EVs are likely less influenced to purchase an EV because of the existence of the subsidy. Column 2 utilizes the average price for the vehicle model instead of the actual recorded sales price. This average price takes the mean over all observed sales of the same make and model which eliminates much of the price variation and doesn't allow for differences within models or across model years, but this is done because the recorded sales prices in the Experian registration data appear to have potential recording errors with vehicle prices above \$800,000 and below \$10,000. Since the coefficient on the average price is not significantly different from that on the sales price, average price will be used for remaining regressions. Column 3 focuses on the impact of demographic variables, specifically zip code median income (measured in thousands of dollars) and the proportion of the population below 300% of the Federal Poverty Guideline. Both explanatory variables are negative and significant. However, in column 4 which combines vehicle price and zip code demographic variables, median zip code income is no longer significant and switches signs. Column 5 and 6 add variables focusing on the dealer location relative to the buyer's zip code. Column 5 adds a dummy variable that is equal to 1 if the dealer and the buyer share a zip code. This is very positive and significant. Dealer distance from the buyer in kilometers measured from the center of the zip codes is added in column 6 which is weakly positive and significant.

Table 32 follows the same sequence as table 31. In columns 1 and 2, the price of vehicles, sales price and average price respectively, have significant negative coefficients. This aligns with the priors that those purchasing expensive vehicles are less likely to care about the rebate money. In the case of column 2, a vehicle with average model price higher

by one thousand dollars is associated with a .0329 lower z-score probability of CVRP application on average. Column 3 examines the impacts of median zip code income and the proportion of households with earnings under 300% of the Federal Poverty Guideline. Both coefficients are negative and significant. Adding average model price into a Probit regression with these income variables in column 4, median zip code income is no longer significant. Again, this is due to the high correlation between income and vehicle price. Column 5 is especially interesting in the Probit model. The variable *sameZip* which is a dummy variable equal to one if the dealer and buyer share a zip code and zero if they do not is excluded from the model because it perfectly predicts CVRP application. In these data, if the dealer and buyer are located in the same zip code, there is always a CVRP application associated with that vehicle. While I expected this to be a positive predictor of CVRP application, I did not expect living in the same zip code as the dealer would perfectly predict the filing of a CVRP application. Finally dealer distance from the buyer is added to the regression in column 6. The coefficient is positive and significant which is counter-intuitive given the previous result.

To interpret these coefficients as point changes in probability of application, we must utilize the normal distribution function. This is typically done by evaluating the derivative at the means of all independent variables. These conditional marginal effects are presented in Table 33. Not that the variable Same Zip Code is not included as it perfectly predicted application to CVRP and therefore, is set to zero to evaluate the effects of all other variables. Evaluated at the means, an EV purchase by someone in a zip code with 10% more of the population with income below 300% of the federal poverty guideline is associated with a 1.27% lower probability of the buyer applying for CVRP, on average. I did not expect this to be negative and this may suggest that there are barriers to application that are not obvious to an outside party. Evaluated at the means, the buyer of an EV from a dealer that sold 100 more EVs between April 2012 and December 2015 is 1.027% more likely to apply for CVRP, on average. This fits with the prior that perhaps there is

information and assistance passed on by experienced EV dealers. Evaluated at the means, a buyer that lives 100 km closer to their dealer, 3.75% more likely to apply for CVRP, on average. Evaluated at the means, a vehicle purchased at a price \$1000 lower is associated with a 2.319% higher probability of the buyer applying for a CVRP rebate, on average.

These results lead to a few policy implications. First, with lower take up in low income zip codes, it is important to target informational campaigns and application assistance to these areas and also to buyers of low price EVs vehicles as well to dealerships that sell lower priced EVs. In addition, targeting dealership outreach to dealers who sell fewer EVs is also important according to the regression results to pass on information and assistance around CVRP. When it comes to the negative relationship between average vehicle price and the probability of applying for CVRP, this is good news for policy makers. The implementation the means testing shows us that there is little appetite for subsidizing the sales of these vehicles to people with high incomes and while median zip code income is not significant in the final regression, that is because of the strong positive correlation between income and vehicle price, so the negative coefficient on vehicle price is telling us that whether by choice or because of design, high income people are already less likely to take up these rebates. This is good news for policy makers and may suggest that rebates that differ by vehicle price or model may do a similar job to means testing which is easier to measure and creates less friction in the application process.

7.5 Conclusion

This paper provides descriptive analysis surrounding the incomplete take up of the CVRP. While these cash rebates offer significant sums of money to owners of new EVs in California, take up is less than 75% even during the initial period of the program when all new EV owners in California are eligible.

This is unusual as the typical reasons for incomplete take up of governments do not apply in this situation. The application requires proof of purchase, proof of registration in

California, and proof of California residency. Meaning that the application is fairly simple. During these early years of the program used in these analyses, buyers had 180 days from purchase to complete the application, so there is plenty of time and the required steps are not very complex. Additionally, there is not the same stigma surrounding take up of this program relative to other governmental programs like food stamps or unemployment.

The reasons for incomplete take up that could potentially apply given the descriptive results include lack of information surrounding the program, time/energy/focus constraints, and personal choice. However, on the high income end, it is less likely that these constraints are leading to less EVs on the road, so I would suggest that information and educational programming focused on lower income neighborhoods could increase take up and increase EVs on the road. In addition, the implementation of means testing, which increased rebate size for low income households could also help them overcome the barriers to take up, specifically making it more worth their time to apply. However, income requirements may increase the subsidy, but also increase the cost to applying as there is now a chance that buyers will need to prove their income levels. Rather than adding means testing, targeting low income populations using location specific subsidies or subsidies negatively correlated with EV price would not add the same increase barrier to take up. On the high income end of the spectrum, the addition of means testing eliminating the subsidy may not create the saving the government expects as it appears these EV owners were less likely to apply before regardless of their eligibility. This is important knowledge for policy makers to have in predicting the budgetary effect of the introduction of means testing.

Further work exploring the take up of clean energy and transportation programming is necessary as this has become a large budget item for many governmental bodies over the last decade. Additionally, options for individuals looking for clean alternatives are more available now than they were a decade ago. The supply side availability of EVs

during the years studied here is a significant limitation to note here which provides opportunity to build on this further. With the implementation of means testing in CVRP, there is an opportunity to revisit take up of this program with an added barrier to identify the impact of means testing on take up and therefore on EV adoption. When governments spend money, it's important to understand where those funds are ending up and if there are ways to analyze take up and improve the impact of these programs.

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7.6 Appendix D: Missing Experian Data in Early 2016

Figure 12 revealed an anomaly in matching the vehicle registration data from Experian with the CVRP application data. In early 2016, there is a spike in vehicles that have CVRP applications, but do not appear in vehicle registration data. This appendix explores these data further to attempt to identify any patterns within the missing data and provide information on whether the inclusion of these data would change results.

First, these data are broken down by vehicle make in figure 25 which can be compared to the overall make mix of the data that matches across the DMV data and CVRP application data in 26. One can see that there is a small increase in missing vehicles across most makes, but that there seem to be a disproportionately large number of BMWs missing from the period. Since this appears to be a larger proportion than what could be attributed to random missing data, it is important to exclude this period.

However, tables 34 and 35 replicate the linear probability model and Probit model respectively but using data for all vehicles registered up until March, 28th, 2016 which is the date means testing first comes into effect. Even with the inclusion of the potentially problematic data, the coefficients in these tables do not lead one to a different conclusion than the main results in 31 and 32.

7.7 Tables and Figures

Figure 12. Application Data by Presence in DMV Data

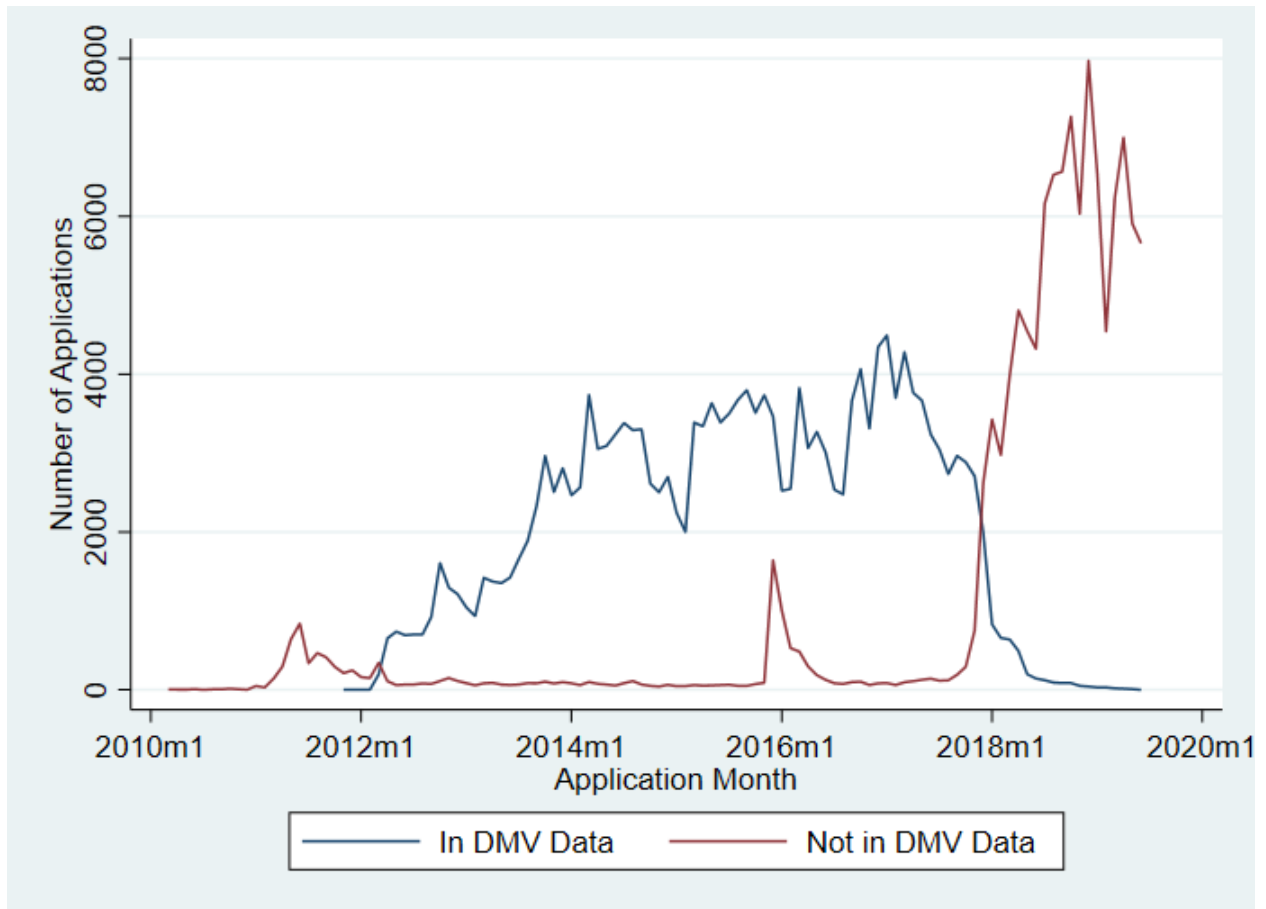


Figure 13. DMV Data by Presence of CVRP Application

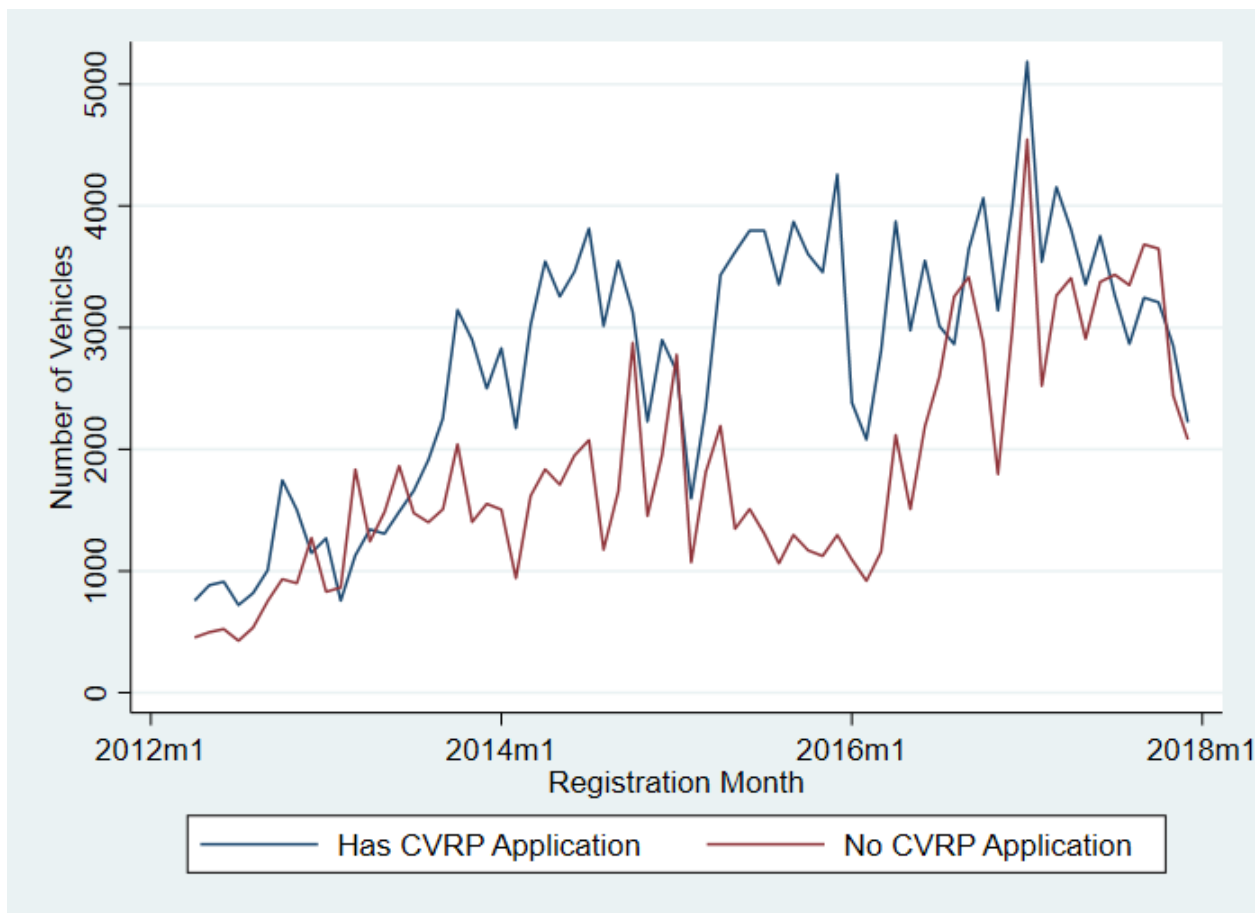


Figure 14. Categories of Matching by Make

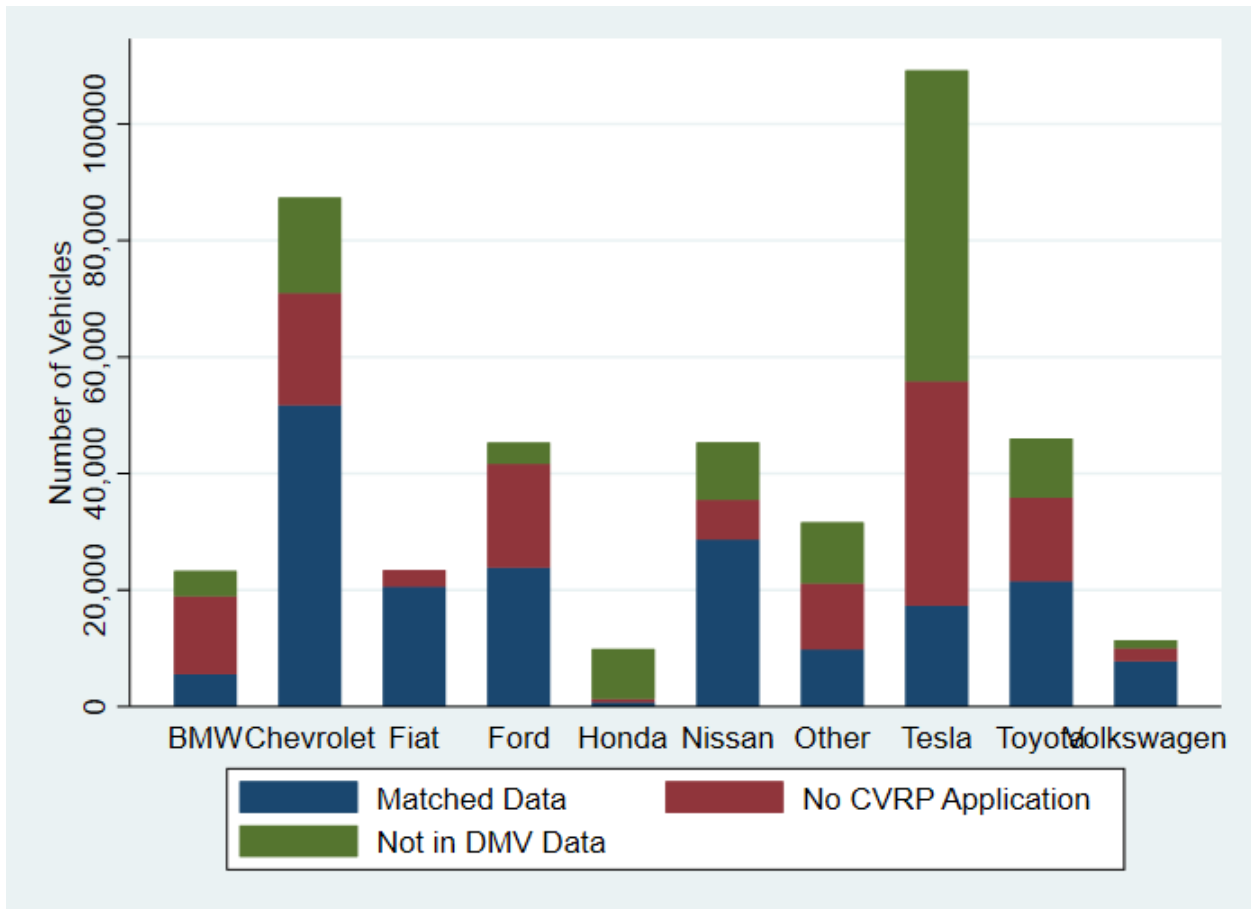


Figure 15. Presence of Applications Before Means Testing

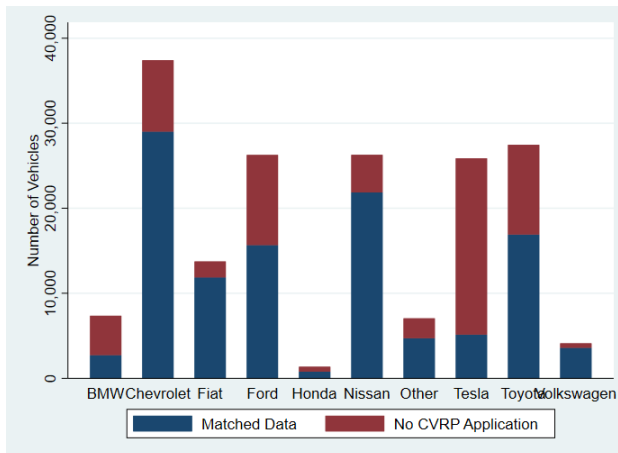


Figure 16. Presence of Applications After Means Testing

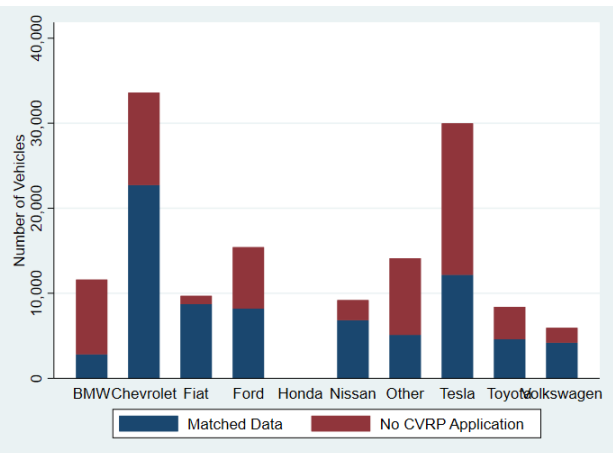


Figure 17. Time to Application

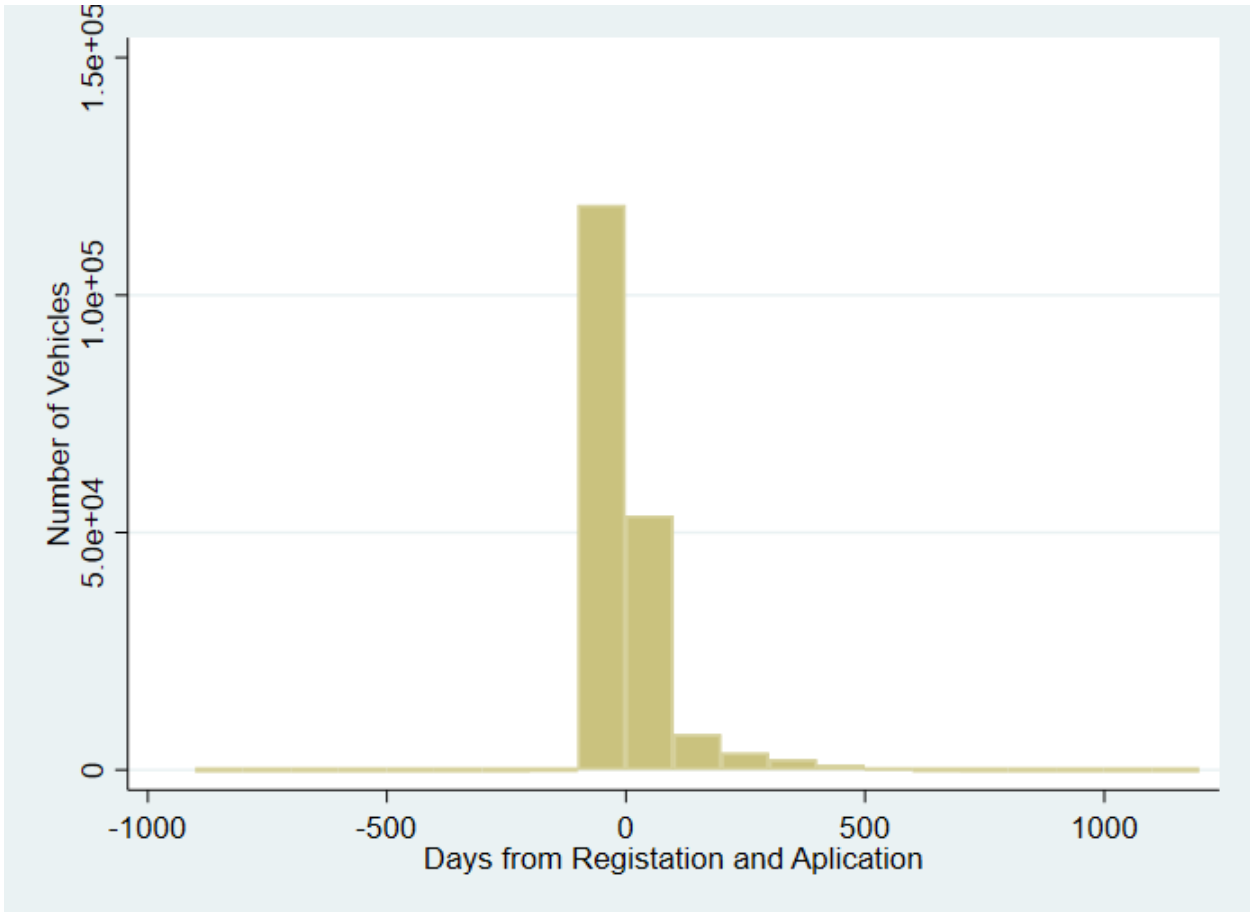


Figure 18. Time to Application by Make

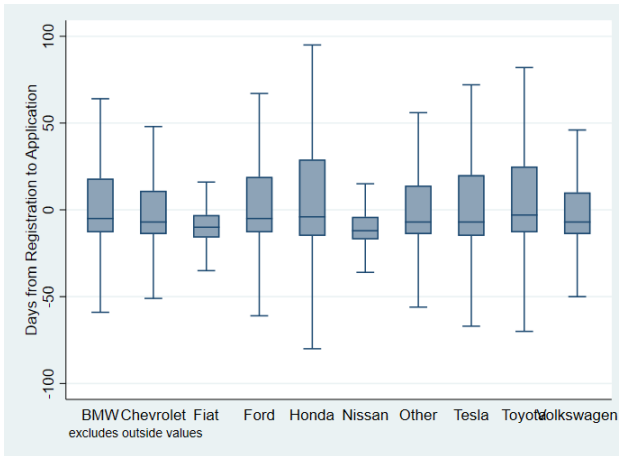


Figure 19. Time to Application over Time

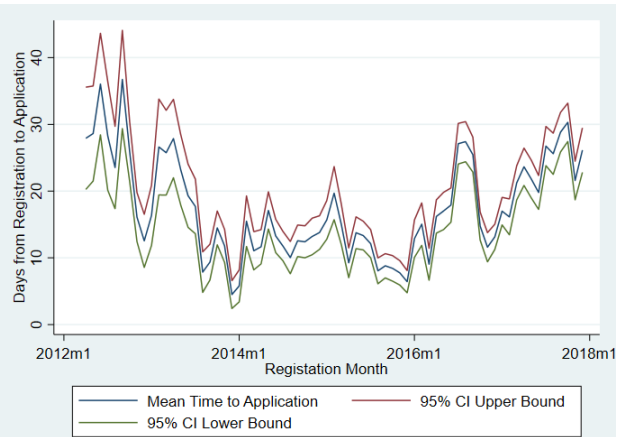


Figure 20. Application and Vehicle Price
Data and Linear Fit

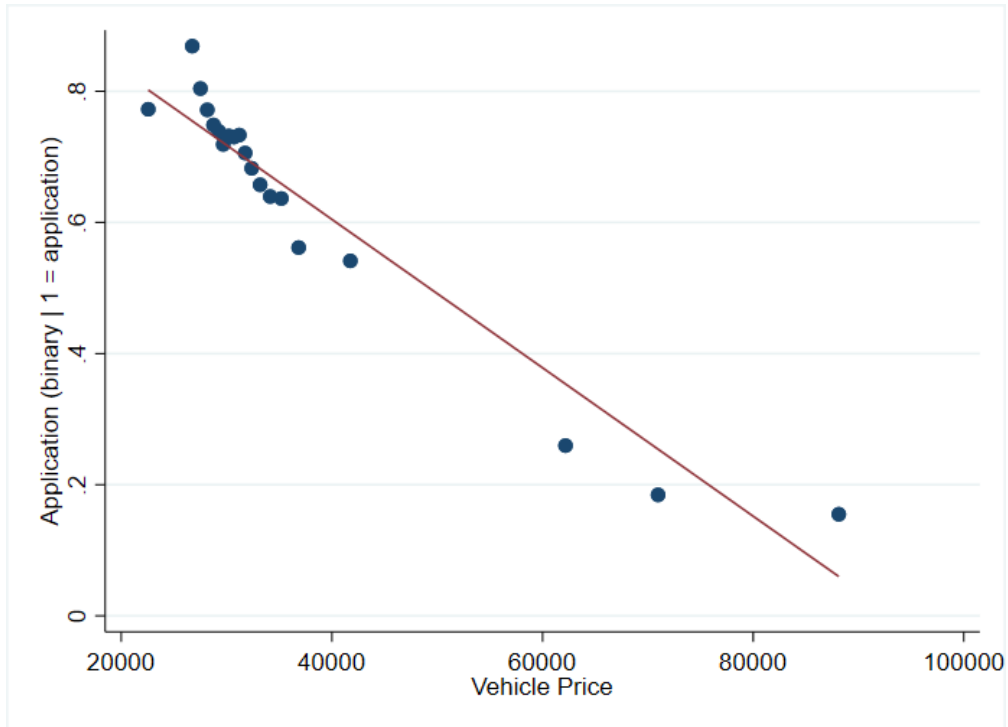


Figure 21. Application and Zip Code Median Income
Data and Linear Fit

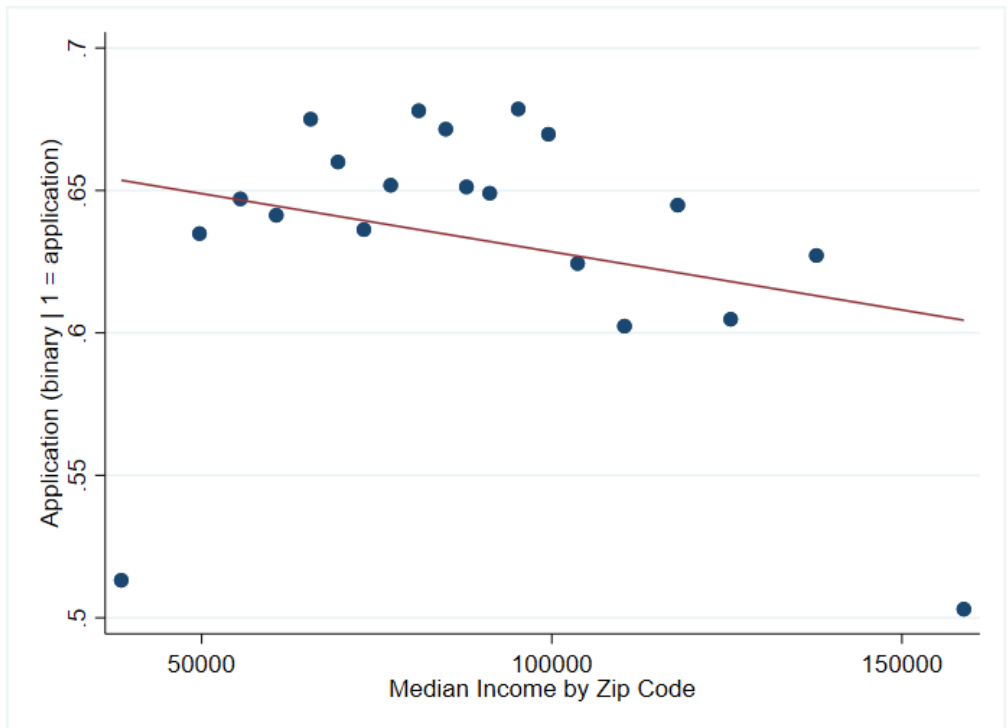


Figure 22. Application and Zip Code Proportion of the Population under 300% of the Federal Poverty Guideline Data and Linear Fit

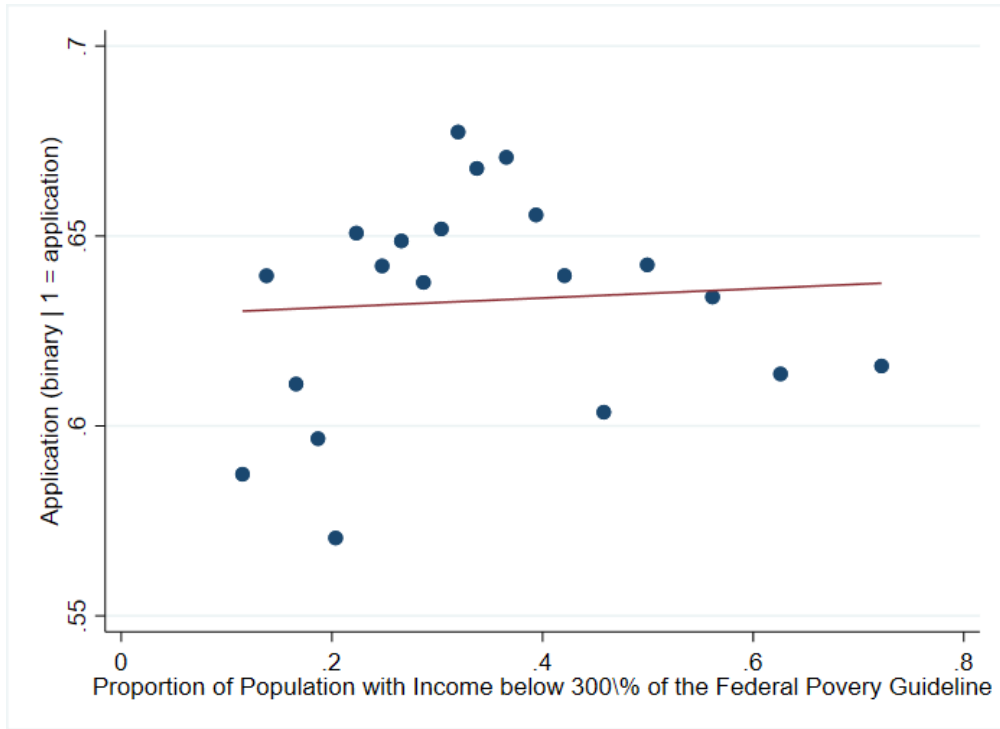


Figure 23. Application and Dealer Size Data and Linear Fit

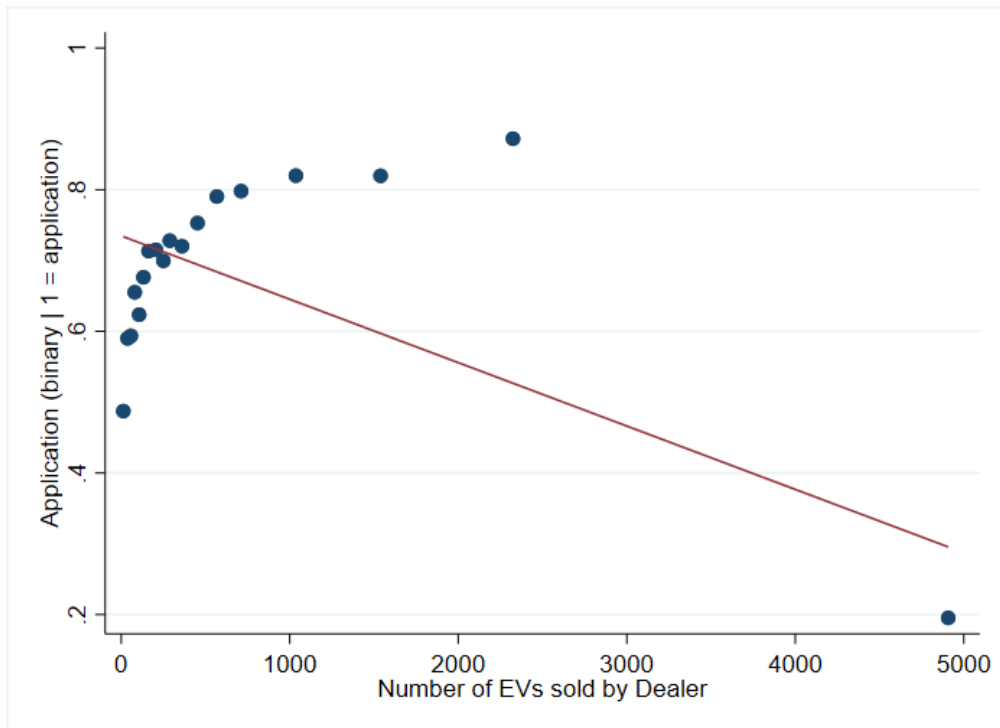


Figure 24. Application and Dealer Distance from Buyer
Data and Linear Fit

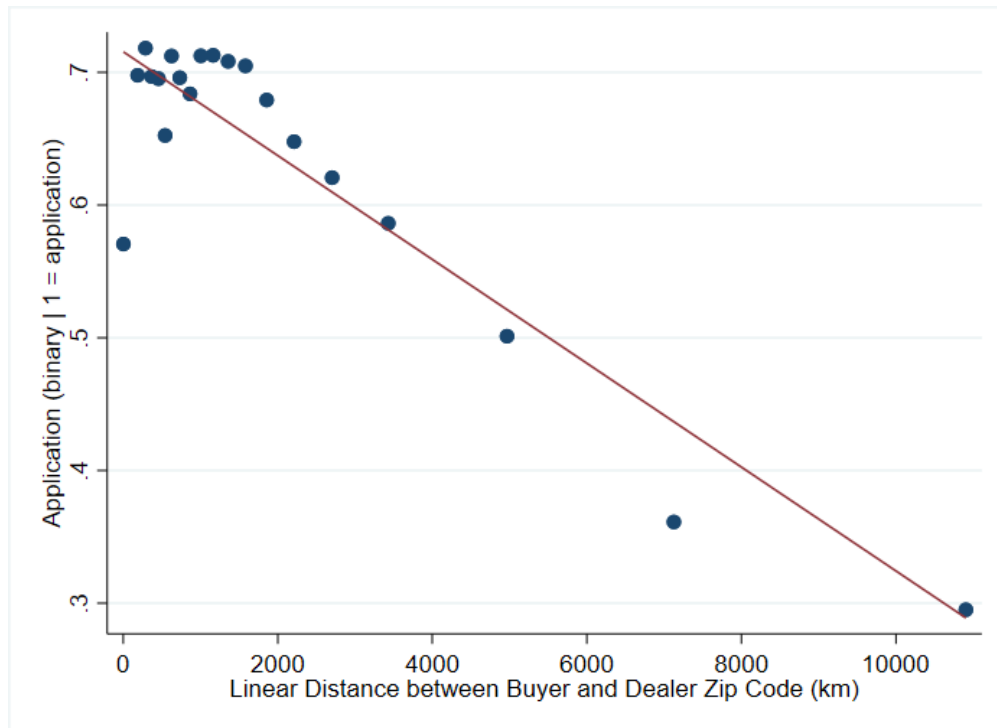


Figure 25. Vehicles with Applications but no Registration

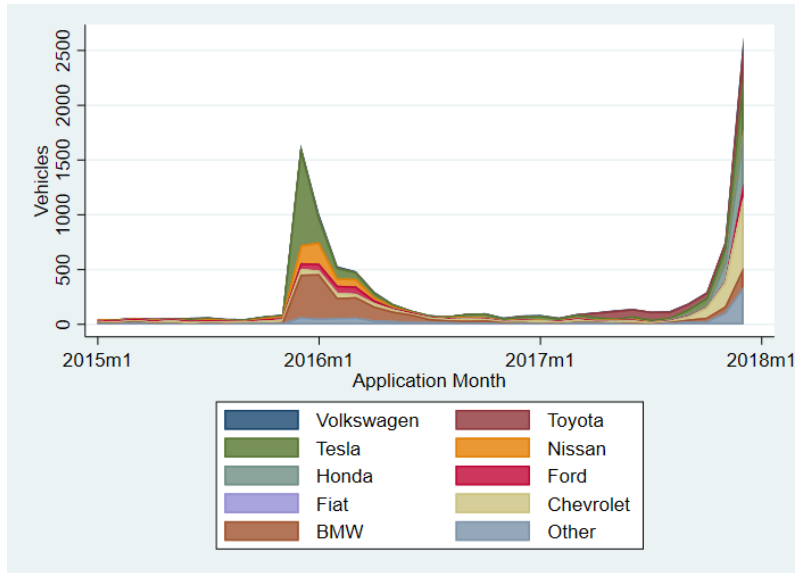


Figure 26. Matched Vehicles by Application Month

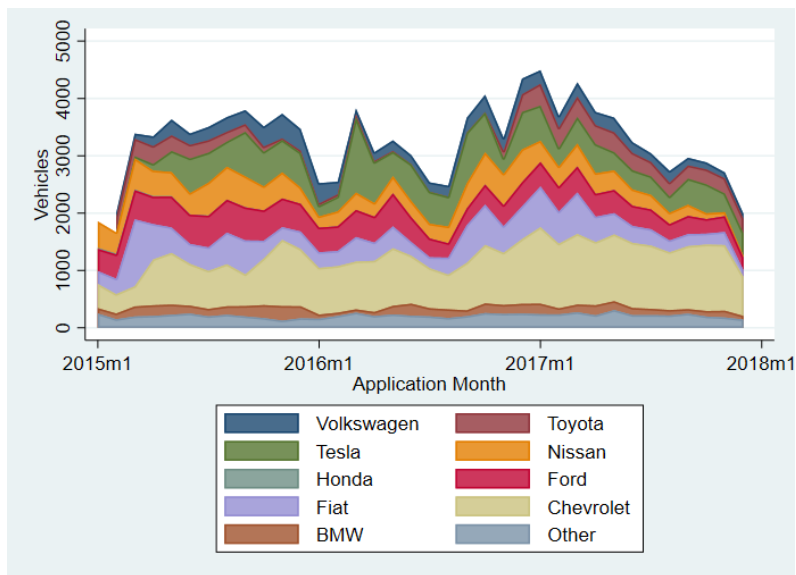


Figure 27. Histogram of the Distance between Dealer and Buyer Zip Code (km)

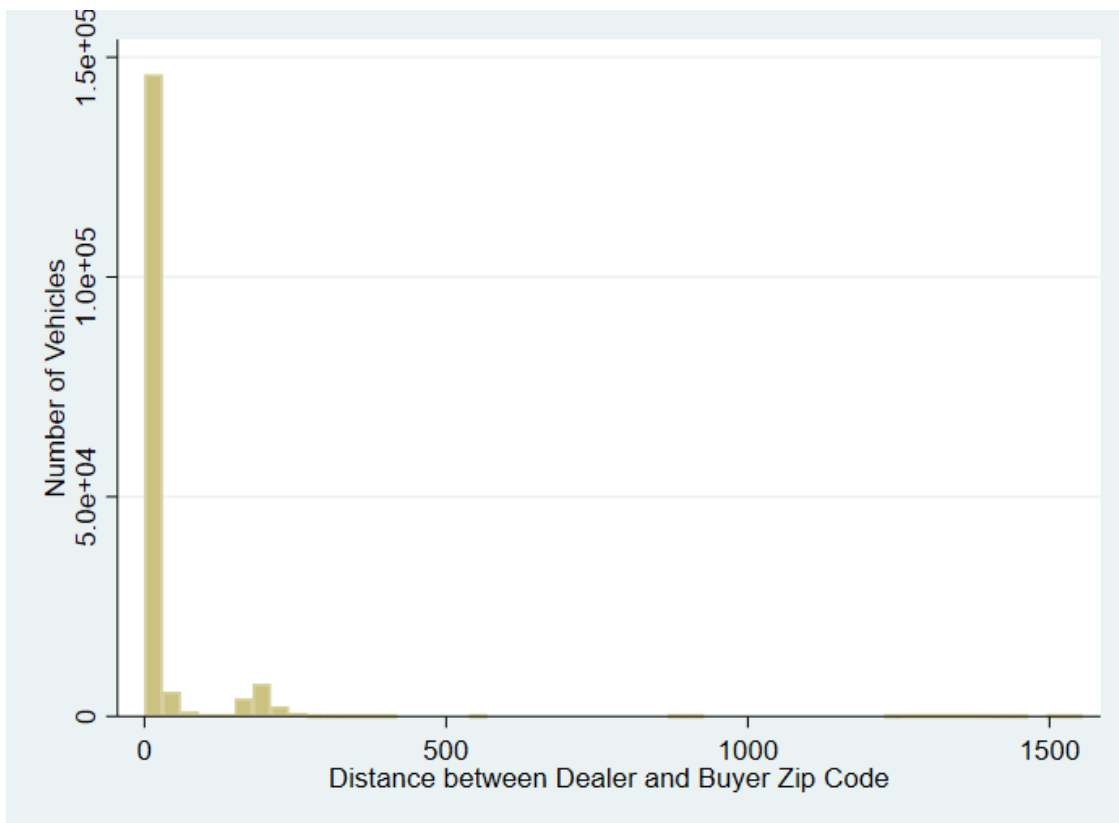


Figure 28. Histogram of the Size of Dealers by EV Sales

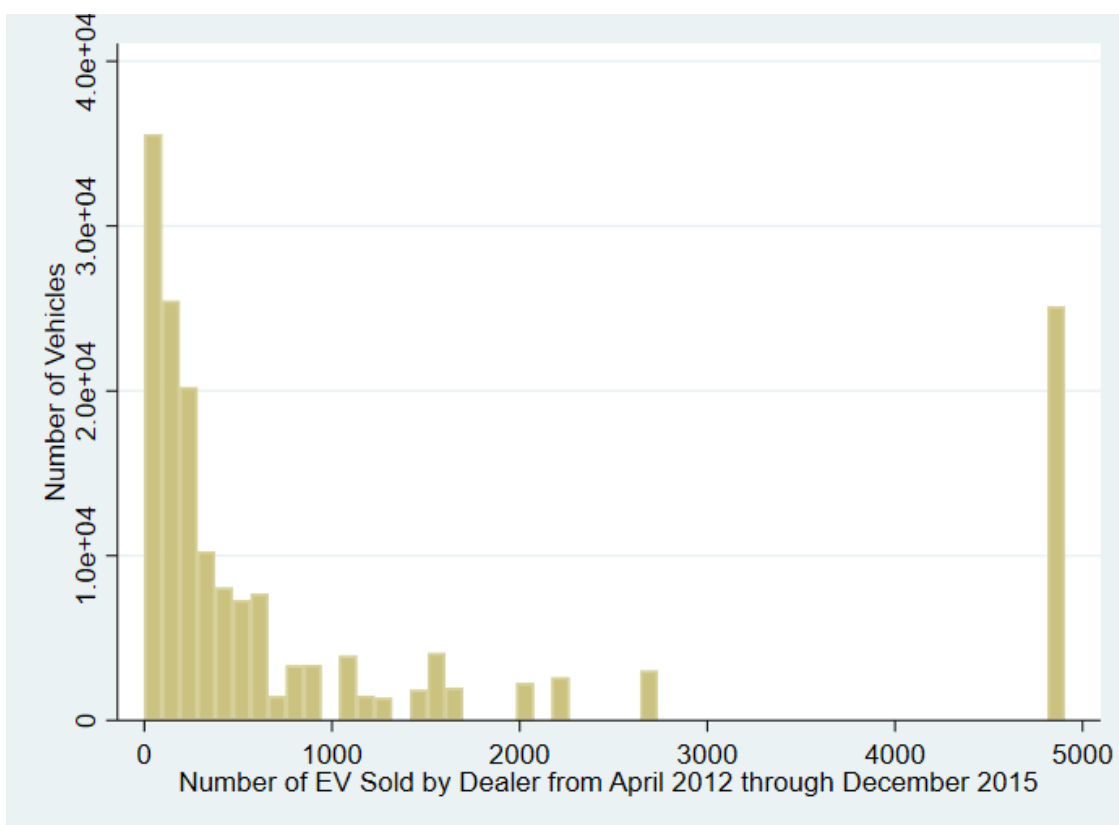


Table 28. CVRP Rebate Amounts

Start:	3/1/10	6/18/11	7/4/13	6/1/14	3/29/16	11/1/16	12/3/19
PHEV	3,000	1,500	1,500	1,500	1,500	1,500	1,000
BEV	3,000-5,000 ¹	1,500-2,500	2,500	2,500	2,500	2,500	2,000 ⁴
FCEV	3,000-5,000	1,500-2,500	2,500	5,000	5,000	5,000	4,500
PHEV	-	-	-	-	3,000	3,500	3,500
BEV	-	-	-	-	4,000	4,500	4,500
FCEV	-	-	-	-	6,500	7,000	7,000

¹ In the early years of the program, the rebate amount for BEVs and FCEVs depended on the range of the vehicle. Vehicles were either short or long range and received rebates of 3,000 and 5,000 respectively.

² Increased Low Income Rebates were available beginning March 29th, 2016.

³ Rebates are offered for neighborhood electric vehicles and battery electric motorcycles as well.

⁴ Tesla vehicles eligibility ends on March 15th, 2022.

Table 29. Matching by VIN

	Frequency	Percent
Matched	187,723	43.37
No Application	127,144	29.38
Not in DMV	117,957	27.25
Total	432,824	100.00

Table 30. Summary Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Application	170,401	.6329775	.4819941	0	1
Sell Price (in thousands)	170,401	37.50689	16.49235	7.4	177.4
Average Model Price (in thous.)	170,401	7.50689	15.4873	18.79904	121.1
Median Zip Code Income (in thous.)	170,401	89.06003	30.08529	10.67988	220.3505
Proportion of Zip Pop. \leq 300% FPG	170,401	.3413274	.1619227	.0991335	.9337355
Total Dealer EV Sales	170,401	1137.805	1662.749	0	4907
Dealer and Buyer Same Zip	170,401	.0500408	.2180298	0	1
Distance from Dealer to Buyer (km)	168,687	28.93171	84.84904	0	1554.733

All data limited vehicle purchases in Experian data from April 2, 2012 through December 31, 2015.

Table 31. Linear Probability Models

	(1)	(2)	(3)	(4)	(5)	(6)
SellPrice	-0.0113*** (0.000249)					
avg_p		-0.0123*** (0.000276)		-0.0125*** (0.000213)	-0.0205*** (0.00104)	-0.0199*** (0.00107)
MedianInc			-0.00146** (0.000505)	0.000109 (0.000406)	0.0000579 (0.000431)	-0.0000527 (0.000417)
under300			-0.224*** (0.0676)	-0.148** (0.0535)	-0.121* (0.0537)	-0.120* (0.0538)
dealersize					0.0000847*** (0.00000879)	0.0000856*** (0.00000899)
sameZip					0.302*** (0.0123)	0.297*** (0.0116)
dealer_distance						-0.000313* (0.000125)
_cons	1.057*** (0.0138)	1.093*** (0.0149)	0.840*** (0.0670)	1.144*** (0.0609)	1.335*** (0.0829)	1.327*** (0.0833)
N	170401	170401	170401	170401	170401	168687

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 32. Probit Models

	(1)	(2)	(3)	(4)	(5)	(6)
applied						
SellPrice		-0.0330*** (0.000964)				
avg_p		-0.0339*** (0.000937)		-0.0348*** (0.000727)	-0.0620*** (0.00417)	-0.0602*** (0.00422)
MedianInc			-0.00380** (0.00131)	0.000426 (0.00129)	0.000259 (0.00144)	-0.000102 (0.00140)
under300			-0.587*** (0.177)	-0.424* (0.169)	-0.353* (0.178)	-0.348 (0.179)
dealersize					0.000284*** (0.0000351)	0.000290*** (0.0000354)
sameZip					0 (.)	0 (.)
dealer_distance						-0.00102** (0.000382)
_cons	1.579*** (0.0486)	1.617*** (0.0483)	0.879*** (0.175)	1.758*** (0.195)	2.434*** (0.299)	2.421*** (0.301)
N	170401	170401	170401	170401	165691	163977

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 33. Conditional Marginal Effects

	mean	dy/dx	Std. Err.
Median Income	89.27545	.0000233	.0005083
Under 300%	.3404677	-.1271438*	.0647791
Dealer Size	1292.147	.0001027***	.0000115
Dealer Distance	29.46767	.0003753**	.000146
Average Model Price	37.87148	-.0231853***	.0015157

Based on 32 regression (6)

Marginal effects calculated at the mean of other variables.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 34. Linear Probability Models through March 2016

	(1)	(2)	(3)	(4)	(5)	(6)
SellPrice	-0.0111*** (0.000234)					
avg-p		-0.0119*** (0.000260)		-0.0122*** (0.000202)	-0.0202*** (0.000966)	-0.0196*** (0.000988)
MedianInc			-0.00142** (0.000493)	0.000117 (0.000390)	0.0000662 (0.000414)	-0.0000429 (0.000401)
under300			-0.216** (0.0662)	-0.142** (0.0516)	-0.118* (0.0519)	-0.117* (0.0521)
dealersize					0.0000748*** (0.00000718)	0.0000764*** (0.00000741)
sameZip					0.300*** (0.0115)	0.295*** (0.0109)
dealer_distance						-0.000318** (0.000122)
_cons	1.053*** (0.0131)	1.085*** (0.0141)	0.836*** (0.0653)	1.132*** (0.0582)	1.328*** (0.0790)	1.323*** (0.0793)
N	180072	180072	180072	180072	180072	178318

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 35. Probit Models through March 2016

	(1)	(2)	(3)	(4)	(5)	(6)
applied SellPrice		-0.0321*** (0.000892)				
avg_p		-0.0329*** (0.000877)		-0.0337*** (0.000684)	-0.0630*** (0.00424)	-0.0615*** (0.00428)
MedianInc			-0.00370** (0.00128)	0.000445 (0.00123)	0.000285 (0.00138)	-0.0000618 (0.00135)
under300			-0.565** (0.174)	-0.408* (0.162)	-0.343* (0.172)	-0.337 (0.173)
dealersize					0.000265*** (0.0000314)	0.000273*** (0.0000317)
sameZip					0 (.)	0 (.)
dealer_distance						-0.000996** (0.000386)
_cons	1.559*** (0.0457)	1.593*** (0.0457)	0.871*** (0.171)	1.727*** (0.185)	2.473*** (0.295)	2.465*** (0.296)
N	180072	180072	180072	180072	175091	173337

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$