Essays on the Effectiveness and Implications of Governments' Green Product Incentives

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Essays on the Effectiveness and Implications of Governments' Green Product Incentives

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Summary

Growing environmental concerns such as global warming along with a heightened sensitivity to dependence on foreign oil have recently led to an increased number of sustainability initiatives across the world (e.g., Jenn et al. 2013). Manufacturers in various industries have taken part in these initiatives by developing products that are less harmful to the environment. These products are called "green products" due to their use of green technologies that intend to mitigate or reverse the effects of human activity on the environment. For instance, green products such as hybrid vehicles offer solutions to reduce carbon emission and local air pollution. Although many consumers express concern for the environment, and green products become more available (Chen et al. 2012; Chen et al. 2014), green products represent a small fraction of global demand (Chabowski et al 2011). As a result, federal, state, and local governments have adopted various incentive programs such as tax credits, rebates, or special exemptions [e.g., waivers from high-occupancy vehicle (HOV) lane restrictions] to promote green goods and services. However, the impact of such incentives on consumers' adoption of green products remains controversial. Also, little is known about where these incentives are most effective and how these incentives influence the demand for products that are not covered by the incentives (e.g., non-green products) (Carley 2011).

In my dissertation, I empirically study the impact of two of the most common types of state-level government incentives, i.e., HOV-lane exemption and tax credit, in the U.S. automobile industry. Assessing the impact of monetary and non-monetary green-product incentives is challenging given the endogenous nature of governments' incentive provisions. To identify the effect of government incentives on unit sales of green and non-green vehicles, both essays take advantage of policy changes in various U.S. states. From a methodological standpoint, I employ a a multitude of quasi-experimental methods, including difference-in-differences with Coarsened Exact Matching, regression discontinuity in time, and border strategy.

The first essay explores the impact of HOV incentive launch and termination in California and Utah on the unit sales of green and non-green vehicles. Unlike previous studies that only examine the launch of the HOV incentive and find an insignificant impact on green vehicle sales,

this essay concentrates on its termination. The findings suggest that the termination of the HOV incentive decreases unit sales of green vehicles covered by the incentive by 14.4%. I also provide suggestive evidence that this significant negative effect of HOV incentive termination is through a mechanism related to the primary benefit the incentive provides: time saving. More precisely, the results indicate that the negative effect is more pronounced in counties where consumers value time saving more (i.e., counties with higher income and longer commute to work). In addition, the termination shifts consumers to non-green vehicles with higher performance. Importantly, in line with prior literature, the launch of the HOV incentive is not found to have a significant effect on green vehicle sales. This means that 1) the effect of termination is not simply the opposite of that of launch and 2) the net effect of the HOV incentive is negative. Combined together, the findings imply that governments' green product incentives could backfire.

The second essay examines the impact of tax-credit incentive launch in South Carolina on the unit sales of green vehicles and non-green vehicles. Using quasi-experimental methods, I find that the introduction of the tax-credit incentive leads to a 25.1% increase in the unit sales of green vehicles, while the unit sales of non-green vehicles remain unchanged. I also provide suggestive evidence that the tax-credit incentive is effective through the cost-saving mechanism. Specifically, the tax-credit incentive is more effective in counties where consumers value cost saving more (i.e., counties with lower income). Additionally, the incentive induces substitution from non-green vehicles with high fuel efficiency. The results also indicate that the tax-credit incentive does not result in market expansion for green vehicles. Therefore, the increased demand for green vehicles covered by the tax-credit incentive mainly comes from demand substitution from gasoline vehicles with high fuel efficiency.

Essay I

The End of the Express Road for Hybrid Vehicles: Can Governments' Green Product Incentives Backfire?

Abstract

In response to growing environmental concerns, governments have promoted products that are less harmful to the environment—green products—through various incentives. We empirically study the impact of a commonly used non-monetary incentive, namely the single-occupancy permission to high-occupancy vehicle (HOV) lanes, on green and non-green product demand in the U.S. automobile industry. The HOV incentive could increase unit sales of green vehicles by enhancing their functional value through time-saving. On the other hand, the incentive may prove counterproductive if it reduces the symbolic value (i.e., signaling a pro-environmental image) consumers derive from green vehicles. Assessing the effectiveness of green-product incentives is challenging given the endogenous nature of governments' incentive provisions. To identify the effect of the HOV incentive on unit sales of green and non-green vehicles, we take advantage of HOV incentive changes in two states, and we employ a multitude of quasi-experimental methods using a data set at the county-model-month level. Unlike previous studies that only examine the launch of the HOV incentive and find an insignificant association between incentive launch and green vehicle demand, we concentrate on its termination. We find that the *termination* of the HOV incentive decreases unit sales of vehicles covered by the incentive by 14.4%. We provide suggestive evidence that this significant negative effect of HOV incentive termination is due to the elimination of the functional value the incentive provides: time-saving. Specifically, we find that the negative effect is more pronounced in counties where consumers value time-saving more (i.e., counties with a longer commute to work and higher income). Additionally, in line with prior literature, the *launch* of the HOV incentive is not found to have a significant effect on green vehicle sales. Combined, our findings reveal that the effect of termination is not simply the opposite of that of launch, implying that governments' green product incentives could backfire.

Keywords: sustainability, green products, public policy, government incentives, climate change, technology adoption, policy evaluation, quasi-experiments, difference-in-differences, coarsened exact matching

1.1 Introduction

Global carbon emissions from fossil fuels–a major driver of climate change–have increased by 82% in the past three decades (Boden et al. 2017). Such growing environmental concerns along with a heightened sensitivity to dependence on foreign oil have recently led to a proliferation of sustainability initiatives across the world (e.g., Jenn et al. 2013). Manufacturers in various industries have taken part in these initiatives by developing new products that are less harmful to the environment relative to their extant counterparts. These products are called "green products" due to their use of green technologies that intend to mitigate or reverse the effects of human activity on the environment.[1] For instance, green products such as hybrid vehicles offer solutions to reduce greenhouse gas emissions, around one-third of which come from the transportation sector in the U.S.[2]

Although many consumers express concern for the environment, and green products become more available (Chen and Chang 2012; Chen et al. 2014), green products represent a small fraction of global demand (Chabowski et al. 2011). As a result, federal, state, and local governments have adopted various monetary incentive programs such as tax credits and rebates to promote green goods and services. Recently, governments have increasingly turned to "free" methods of stimulating green vehicle demand: non-monetary incentives such as waivers from high-occupancy vehicle (HOV) lane restrictions. While only 19 states in the U.S. adopted monetary incentives as of 2019, 36 states implemented non-monetary incentives.[3] Despite the prevalence of such non-monetary incentives, their impact on consumers' adoption of green products remains controversial. Also, little is known about how these incentives influence the demand for products that are not covered by the incentives (e.g., non-green products) as well as other sustainable behavior (e.g., carpooling) (Carley 2011). The lack of empirical evidence on these relationships results in an unclear picture of the effectiveness of governments' non-monetary green product incentives in

[1] https://en.oxforddictionaries.com/definition/green_technology. Other terms used for green products include "eco-friendly," "sustainable," "clean," and "environmentally friendly" (e.g., Sheldon and DeShazo 2017).

[2] https://www.epa.gov/ghgemissions/sources-greenhouse-gas-emissions.

[3] https://afdc.energy.gov.

boosting demand for green products and reducing greenhouse gas emissions.

In this paper, we empirically study the impact of HOV lane exemption on consumers' green (i.e., hybrid, plug-in hybrid, and electric) and non-green (i.e., gasoline) vehicle purchases in the U.S. automobile industry.[4] The HOV incentive has been adopted by 15 states in the U.S. to date, making it one of the most common types of state-level non-monetary government incentives for green products.[5] On one hand, the HOV incentive could increase unit sales of green vehicles by enhancing their functional value through the time-saving benefit. On the other hand, the HOV incentive may prove counterproductive if it reduces the symbolic value (i.e., signaling a pro-environmental image, or "greenness") consumers derive from buying green vehicles (Dittmar 1992). Signaling greenness through purchases can be particularly strong for sustainable innovations such as hybrid vehicles because 1) these green options typically have inferior functional value relative to their non-green counterparts (e.g., Gneezy et al. 2012), and 2) they are conspicuous products. Indeed, the New York Times reported "it makes a statement about me" as the most important reason for buying a hybrid Prius.[6] The HOV incentive could negatively influence green vehicle sales driven by the signaling motivation if the incentive renders green vehicles less inferior (or even better) than non-green vehicles in terms of functional value. In such a case, purchasing a green vehicle may produce a weaker (if any) signal of costly pro-environmental behavior in the presence of the incentive. This can, in turn, result in fewer green vehicle purchases by consumers who aim to signal a green image. As a result of the abovementioned countervailing forces, the direction of the net effect of the HOV incentive on green vehicle demand is an empirical question.

The research on the effectiveness of government incentives in promoting green-product (or technology) adoption is limited in the marketing literature, despite the recent calls for more marketing studies on sustainability (Sudhir 2016) and public policy (Stewart 2015). One notable ex-

[4]HOV lanes are also referred to as express lanes, carpool lanes, transit lanes, diamond lanes, or commuter lanes. There are two types of hybrid vehicles in the automobile market in our study period: conventional hybrid and plug-in hybrid. Both types of vehicles have an electric motor and a rechargeable battery. While conventional hybrid vehicles can only be fueled by gasoline, plug-in hybrid vehicles can be plugged in and recharged from an outlet. In the remainder of the paper, we use "hybrid vehicle" to refer to conventional hybrid vehicles, and "plug-in hybrid vehicle" to refer to plug-in hybrid vehicles.

[5]https://afdc.energy.gov.

[6]http://www.nytimes.com/2007/07/04/business/04hybrid.html.

ception is Bollinger (2015), which studies the effectiveness of various government policies in the Southern California garment cleaning industry using counterfactual analyses based on a dynamic structural approach. In the same research stream, Shriver (2015) develops a structural econometric framework incorporating network effects, and he shows that subsidies for fuel retailers in certain geographic markets can be effective in boosting the demand for ethanol-compatible vehicles.[7]

Another research stream that is more directly related to our study exists in energy policy (see Jenn et al. 2018 for a summary of this literature). Papers in this stream have mostly examined the effects of monetary government incentives (e.g., tax waivers) on green-vehicle adoption. These studies have shown that monetary incentives are positively related to green-vehicle adoption (e.g., Gallagher and Muehlegger 2011). In contrast, only a small body of literature has investigated the effectiveness of non-monetary incentives such as HOV lane access by examining changes in consumer demand around incentive launches. In one of the pioneering studies, Diamond (2009) documents an insignificant relationship between the HOV incentive launch and hybrid car market share based on a state-year-level analysis.[8] In line with this finding related to the ineffectiveness of the HOV incentive launch, Gallagher and Muehlegger (2011) report that, on average, single-occupancy permission to HOV lanes is not significantly correlated with hybrid car sales based on a state-quarter-level analysis.

Although these studies lay the foundation for understanding important factors related to the effectiveness of non-monetary green-vehicle incentives, the empirical evidence on the causal effect of the HOV incentive on unit sales of green and non-green vehicles is limited and inconclusive. First, the previously reported insignificant relationship between the HOV incentive and hybrid-car adoption is mostly correlational, as it is based on hedonic regressions using state-level aggregate data. Second, importantly, earlier studies concentrate only on the launch of the HOV incentive. That is, they do not examine the effect of the termination of the HOV incentive, which may not

[7]There is a related but separate marketing literature that examines the role of social effects in the adoption of green technology. Bollinger and Gillingham (2012) investigate the role of social effects in the diffusion of solar photovoltaic panels in California. Narayanan and Nair (2013) propose solutions to identify installed-base effects and they apply these approaches to the case of Toyota Prius electric car adoption in California.

[8]The author finds a positive association between the HOV incentive launch and hybrid car market share only in Virginia.

simply be the opposite of the effect of launch. Therefore, they cannot guide policymakers and managers regarding the long-term "net effect" of the HOV incentive. Third, papers in this literature do not investigate the underlying mechanisms for the incentive (in)effectiveness. This is in part because these studies exploit temporal variation using state-level aggregate data, which does not allow for exploiting local-market (e.g., county) characteristics that might affect consumer demand for green vehicles. As a result, previous studies have called for more in-depth studies to take into account and understand more local factors than state-level data (Jenn et al. 2018). Finally, since previous literature has not investigated the effects of the HOV incentive on the demand for non-green vehicles and carpooling behavior, it can provide little guidance on the broader implications of the HOV incentive for overall greenhouse gas emissions.

In light of these limitations of the extant papers, this study aims to contribute to the literature on the effectiveness of governments' green-product incentives in four ways. First, unlike prior research that has mainly studied the relationship between the HOV incentive launch and green vehicle demand, we examine the causal impact of the HOV incentive *termination* on consumer demand for the green vehicles covered by the incentive. Second, using market characteristics (e.g., commute time) at the local (county) level, we explore possible mechanisms underlying the effect of the HOV incentive termination. Our quasi-experimental analyses at the county level allow for 1) a stronger case for causal inference, and 2) more detailed insights related to the local heterogeneity in incentive effectiveness than previous studies based on state-level data. Third, importantly, we compare the effect of the HOV incentive termination with that of launch to investigate the overall effectiveness of the HOV incentive in boosting demand for green vehicles. Fourth, to shed light on the total environmental impact of the HOV incentive, we examine several previously unexplored emissions-related consequences of the incentive, including potential substitution from or to non-green vehicles, market size, as well as carpooling behavior.

To accomplish these goals, we collect a unique data set that involves county-level vehicle sales around the HOV incentive terminations and launches in California and Utah. The main empirical challenge in measuring the effect of these incentive changes on vehicle sales is that the assignment of "treatment" (i.e., incentive change) is a strategic decision by state governments, and thus potentially not random. As such, a simple comparison of unit sales between counties in states "treated" by the incentive change and those in "non-treated" states may be misleading, if there are persistent differences (e.g., commute times or preference for green vehicles) across counties in different states.

We address this endogenous incentive-selection issue by employing several quasi-experimental methods using granular analyses at the county-month level. First, we primarily use the difference-in-differences (DiD) approach with Coarsened Exact Matching (CEM) (Iacus et al. 2009) to ensure that treated and non-treated counties are comparable in terms of pre-treatment sales trends and several important variables. Specifically, we exploit variables related to policymakers' incentive decisions (e.g., traffic conditions) as well as consumers' green-vehicle purchase likelihood (e.g., income, and political inclination) documented in the literature (e.g., Potoglou and Kanaroglou 2007; Ozaki and Sevastyanova 2011). Besides, the panel nature of our data allows us to control for time-invariant differences across counties via county fixed effects and time-varying differences across vehicle models via model-month fixed effects. Second, we also employ a "border strategy" by leveraging the variation in the HOV incentive around state borders (e.g., Shapiro 2018). To the extent that neighboring counties in the same market but in different states are similar in terms of unobserved demographic variables, this strategy complements our primary strategy that relies on matching based on observable demographic variables. Third, we provide a regression-discontinuity-in-time (RDiT)-style analysis (Hausman and Rapson 2017). Assuming that there are no concomitant unobservables influencing unit sales that discontinuously change at the incentive change period, this approach uses a vehicle model's own unit sales in a given county just (e.g., a month) before an incentive change as the counterfactual for those sales just after that incentive change. In line with the identifying assumption underlying the RDiT-style analysis, our subsequent analysis of news articles as well as the availability of charging stations reveals no major changes related to infrastructure or other adoption barriers for green vehicles around the incentive changes.

We find that, on average, the HOV incentive termination decreases unit sales of vehicles covered by the incentive (i.e., hybrid vehicles) by 14.4%. In contrast, on average, green vehicles that are not covered by the HOV incentive (i.e., plug-in hybrid and electric vehicles) and non-green (i.e., gasoline) vehicles do not experience a change in unit sales after the HOV incentive termination.[9] Additionally, we find that the HOV incentive termination has an immediate negative effect after the announcement of the termination, and it persists in the medium term (i.e., six months after the incentive termination). These results are robust to an extensive set of robustness checks, including 1) different functional forms, 2) different sets of control variables and fixed effects, 3) falsification exercises, 4) different treatment states (i.e., California vs. Utah), 5) differential trends across groups based on matching, 6) alternative treatment dates, and 7) alternative identification strategies.

We then explore potential mechanisms underlying the negative effect of the HOV incentive termination on unit sales of vehicles covered by the incentive. Specifically, we investigate whether this negative effect is due to the elimination of the functional value the HOV incentive provides to consumers: time-saving. In doing so, we present two types of suggestive evidence that supports the time-saving mechanism. First, we find that the HOV incentive termination has a more negative sales effect on vehicles covered by the incentive in counties where residents spend more time commuting to work. Second, we show that the negative effect of the HOV incentive termination is more pronounced in counties with higher income levels. However, we do not find support for a change in symbolic value (i.e., signaling greenness) for the vehicles covered by the incentive in response to the HOV incentive termination. We also consider several alternative explanations.

Additionally, in line with prior studies, we show that the launch of the HOV incentive leads to only an insignificant 1.61% increase in hybrid vehicle sales. We provide evidence that this insignificant effect is due to an increase in functional value that is offset by a reduction in symbolic value. Specifically, in support of a raise in functional value, we show that the incentive launch has a positive sales effect on vehicles covered by the incentive in counties with longer commute

[9]Note that there were only a few plug-in hybrid and electric vehicle models available in our analysis period. Additionally, the insignificant average sales effect for vehicles that are not covered by the HOV incentive does not necessarily mean that there is no substitution between vehicles that are covered by the HOV incentive and those that are not. We explore this issue in our subsequent analyses.

times. In contrast, the incentive launch has a negative sales effect on vehicles covered by the incentive for those vehicle models and counties that are more conducive to signaling greenness. This finding, combined with the lack of support for a change in symbolic value following the HOV incentive termination, implies that while the symbolic value of purchasing a green vehicle covered by the HOV incentive is reduced after the incentive launch, it is not restored after the termination. As a result, the effect of termination is not simply the opposite of that of launch, implying that governments' green product incentives could backfire.

Finally, we provide additional analyses related to the implications of the HOV incentive for greenhouse gas emissions. We show that, after the HOV incentive termination, 1) consumers shift to non-green vehicles with high tailpipe emissions, 2) the market for new cars shrinks, and 3) the percentage of carpoolers increases. As we detail in our conclusion section, collectively, our findings have important implications for policymakers and manufacturers in assessing the effects of governments' HOV incentives on green and non-green product demand as well as on greenhouse gas emissions in a comprehensive manner.

1.2 Background Information and Empirical Context

This section provides details on the incentive changes we study to evaluate the effectiveness of the HOV incentive in stimulating demand for green products and curtailing greenhouse gas emissions.

1.2.1 HOV Incentive in California

HOV lanes were constructed to encourage carpooling by providing shorter and more reliable commute times. Before California passed a bill to allow hybrid vehicle owners to drive solo in HOV lanes, there was excess capacity in those lanes. The reason for this is that the laws at the time limited the HOV lane access to vehicles with two or more people and motorcycles, and there were not enough carpoolers.[10] To relieve the pressure to convert HOV lanes to general-purpose lanes, state officials looked for ways to shift a small portion of vehicles from congested general-

[10] http://articles.latimes.com/2004/apr/09/local/me-hybrid9.

purpose lanes to carpool lanes. This goal, combined with officials' desire to promote less-polluting and more energy-efficient vehicles than conventional cars led to the launch of the HOV incentive for hybrid vehicles in California.[11] In September 2004, California signed Assembly Bill 2628 to reduce tailpipe emissions. The California governor Arnold Schwarzenegger supported the bill as part of his vision of a "hydrogen highway." This bill, also referred to as the "Yellow Clean Air Vehicle Decals" program, allowed single-occupant use of HOV lanes by hybrid vehicles—the lowest-emission vehicles available at the time. The program started in July 2005. It was initially valid until July 2008, but its termination date was extended twice.

In 2011, the Department of Motor Vehicle (DMV) decided to terminate the incentive for various reasons. First, DMV aimed to reduce congestion in HOV lanes that started to experience more traffic by terminating the incentive for hybrid cars.[12] California could lose federal highway funding if speeds observed in HOV lanes fall below 45 miles per hour during rush hour, and therefore HOV lanes become "degraded" as defined under federal law.[13] Second, as hybrid cars became popular enough, some believed that the original goal of the incentive was accomplished.[14] Third, DMV was eventually planning to provide carpool stickers for a new generation of plug-in hybrids, which did not start until September 2012. In May 2011, California DMV sent letters to the owners of hybrid vehicles to inform them about the termination of Yellow Clean Air Vehicle decals for hybrid vehicles.[15] This program officially ended on July 1, 2011, beyond which hybrid vehicles were no longer allowed to use the HOV lanes without meeting the minimum passenger number requirement. Importantly, there were no coincident incentive changes for green vehicles other than the HOV incentive termination for conventional hybrid vehicles around the HOV incentive termination in California. That said, there was a planned HOV incentive for a new generation of plug-in hybrids, which took effect more than a year after the HOV incentive termination for conventional hybrids.[16]

[11] http://articles.latimes.com/2004/apr/14/opinion/oe-poole14.

[12] http://articles.latimes.com/2011/jul/02/local/la-me-07-02-carpool-lanes-20110702.

[13] http://articles.latimes.com/2007/sep/12/local/me-carpool12.

[14] http://articles.latimes.com/2009/sep/28/business/fi-hybrid-stickers28

[15] https://www.thecarconnection.com/news/1062439_california-hybrid-drivers-lose-car-pool-lane-privileges.

[16] We discuss the potential implications of this feature of our context in the subsequent sections. Electric cars would

1.2.2 HOV Incentive in Utah

Facing excess capacity in HOV lanes, Utah also passed a similar incentive (Administrative Code 63G-3-102) to promote hybrid vehicles in July 2008.[17] According to this incentive, which became effective in January 2009, hybrid vehicles were allowed single-occupant use of HOV lanes through the "C Plate Permit." Several years after the HOV incentive launch for hybrid vehicles, the Utah Department of Transportation (UDOT) became concerned with the explosion in the number of vehicles with C-decal and the potential degradation of its HOV lanes. As a result, UDOT announced the termination of the "C Plate Permit" for hybrid vehicles starting from May 2011. The program officially expired in July 2011. The termination of the program did not apply to plug-in hybrid and electric vehicles. There were no coincident incentive changes for green vehicles other than the HOV incentive termination for hybrid vehicles around the HOV incentive termination in Utah.

With these incentive changes for hybrid vehicles in California and Utah as our backdrop, we examine how the HOV incentive affects unit sales of green vehicles covered by the incentive, green vehicles that are not covered by the incentive, and non-green vehicles.

1.3 Data and Model-Free Analyses

We combine data from ten sources to assess the impact of HOV incentive changes in California and Utah on unit sales: 1) a major market research firm, 2) American Community Survey, 3) California Air Resources Board, 4) Office of Energy Efficiency & Renewable Energy, 5) Harvard Kennedy School, 6) Federal Highway Administration, 7) Factiva, 8) Alternative Fuels Data Center, 9) U.S. Energy Information Administration, and 10) California Distributed Generation Statistics (details of these data sources are provided below).

continue to retain their HOV lane privileges around the HOV incentive terminations we study.

[17] http://archive.sltrib.com/article.php?id=58094075&itype=cmsid.

1.3.1 Sales Data and Market Definition

Our primary data set contains information on business-to-consumer new vehicle transactions collected by a major market research firm. We obtain data on every transaction that occurs in a random sample of 15-20% of the census of new car dealerships in the U.S. for the 12-month symmetric window around, i.e., six months before and after, each incentive change. For each transaction, the data contain the price and detailed characteristics of the vehicle such as make, model (including trim level), model year, and body type. Also, we observe the customer's ZIP code associated with each transaction. We aggregate these transaction data to the vehicle model-county-month level for our subsequent analyses.[18] In analyzing the sales effects of the HOV incentive for hybrid vehicles, we need to use a clear market definition that allows us to account for market-level shocks. To do so, we follow an approach used in previous studies that examine auto purchases (Mian and Sufi 2012), and we use Core-based Statistical Areas (CBSAs) to define markets in our data. Defined by the Office of Management and Budget, a CBSA is a U.S. geographic area that consists of one or more counties (or equivalents) anchored by an urban center of at least 10,000 people plus adjacent counties that are socioeconomically tied to the urban center by commuting. Additionally, our analyses concentrate on geographically isolated markets to be able to identify potential customers in a given market. We employ a similar set of criteria to those used by Olivares and Cachon (2009) to determine isolated markets.[19] This leaves us a sample of 275 isolated CBSAs with 1,167 counties, which accounts for around 80% of total unit sales in our original transaction data.

1.3.2 Supplementary Sources of Data

Our second source of data consists of the American Community Survey[20], which provides annual county-level information on demographic variables. Specifically, we collect demographic data on income (median household income), education (the percentage of population with bachelor's degree or higher), gender (the percentage of male population), age (the median age of household

[18]The list of green vehicles available during our observation period is provided in Online Appendix A.
[19]The specific criteria are shown in Table B.1 in Online Appendix B.
[20]https://www.census.gov/programs-surveys/acs.

head), unemployment rate, and commute times (mean travel time to work in minutes). We exploit these variables to 1) match counties that are affected by the incentive change with those that are not (as discussed later in detail), and 2) examine the potentially heterogeneous impact of the HOV incentive across different demographics. American Community Survey also includes annual county-level data on the percentage of carpoolers, which we use to study the relationship between the HOV incentive and carpooling behavior. Besides, we obtain data on emission levels at the county-month level from California Air Resources Board to examine emission patterns around the HOV incentive termination in California. We also collect greenhouse gas emission levels (grams per mile) at the vehicle model level from the Office of Energy Efficiency & Renewable Energy (https://www.fueleconomy.gov) to explore the heterogeneity in the sales effects of the HOV incentive by vehicle emission levels. We then convert greenhouse gas emission levels to a categorical (i.e., low, medium, and high) measure for vehicle emission based on the terciles of emission levels at the vehicle model level.

Data on the election statistics of 2004, 2008, and 2012 presidential elections come from Harvard Kennedy School.[21] In particular, we collect data on the county-level percentage of votes for the Democratic Party as a proxy for the support of green technologies, given that the Democratic Party intends for the U.S. to become a clean energy superpower.[22] We use this variable in our matching procedure as well as in our analyses on the heterogeneous effects of the HOV incentive. To supplement the previously mentioned matching variables, we also acquire information on traffic congestion (i.e., average daily traffic volume) at the state level from Federal Highway Administration.

We rely on the Factiva news database to extract data on newspaper articles about green vehicles in California and Utah. This data set allows us to track whether there are any major changes related to infrastructure or other adoption barriers for green vehicles around HOV incentive changes. To complement this news search, we also obtain data on the number of charging stations (public and private) at the county-month level from the Alternative Fuels Data Center (afdc.energy.gov). We

[21] https://guides.library.harvard.edu/hks/campaigns_elections.

[22] https://www.democrats.org/issues/environment.

use this variable to assess the patterns in charging station availability and to explicitly control for its potential effect on unit sales of vehicles in our analyses. To control for the impact of gas prices on vehicle sales, we obtain state-month level gas prices from the U.S. Energy Information Administration.[23] Finally, we also collect data on all the installations of interconnected solar PV (NEM) systems (i.e., NEM Currently Interconnected Data Set) in a given county from California Distributed Generation Statistics. Based on the terciles of the number of solar PV installations, we create a categorical measure to proxy the level of social desirability of being seen as "green."

1.3.3 Raw Sales Patterns Around the HOV Incentive Termination

Table 1 provides descriptive statistics for key variables used in our analyses of the HOV incentive termination. It also shows the raw "difference-in-differences" in terms of percentage changes in unit sales for green vehicles covered by the HOV incentive, green vehicles that are not covered, and non-green (i.e., gasoline) vehicles for counties that are affected by the incentive termination (i.e., treatment group) and those that are not (i.e., control group).[24] These raw data patterns show that, after the incentive termination, unit sales of green vehicles covered by the incentive decreased more in treated counties relative to control counties. On the other hand, the differences in percentage unit-sales changes between the treatment and control groups are much smaller and insignificant for the vehicles that are not covered by the HOV incentive. We use econometric analyses to formalize these insights in Section 1.4.

1.3.4 Other Potential Changes Around the HOV Incentive Termination

In Online Appendix C, we examine other potential changes around the HOV incentive termination that might confound the relationship between vehicle sales and the termination of the HOV incentive. We first investigate whether there is any sudden change in the number of charging stations. We find that the availability of charging stations does not change sharply around the HOV incentive termination. We account for the gradual changes in the number of charging stations by explicitly

[23]https://www.eia.gov/dnav/pet/pet_pri_gnd_dcus_nus_w.htm.

[24]The details of these two groups are provided later in subsection 1.5.

controlling for it in our subsequent econometric analyses. Furthermore, following the approach used by earlier marketing studies (e.g., Tirunillai and Tellis 2017), we search for news articles on Factiva for potential news about major changes related to infrastructure or other adoption barriers for green vehicles in California and Utah. The search results did not provide any evidence of major changes around the HOV incentive termination.

1.4 Effect of the HOV Incentive Termination on Consumers' Green-Vehicle Adoption

This section examines the main effect of the HOV incentive termination on unit sales of green vehicles covered by the HOV incentive, green vehicles that are not covered, and non-green vehicles. The key empirical challenge in identifying the causal effect of governments' green-vehicle incentives is that the termination of the incentive may not be exogenous. In particular, as mentioned in Section 1.2, policymakers' incentive decisions may be influenced by state-specific factors such as traffic conditions, commute times, residents' preferences for green products, and the demographic composition of each state. Thus, a simple comparison of unit sales between counties in states with an incentive change and those in unaffected states may be misleading. To address this identification challenge, we employ several identification strategies that rely on different identifying assumptions.

1.5 Empirical Strategy: DiD with Coarsened Exact Matching

Since the availability of the HOV incentive varies over time and these incentive changes apply only to a subset of vehicles (i.e., hybrid cars) in a subset of counties (e.g., those in California), we can use a DiD identification strategy to estimate the main effect of each incentive change on unit sales. More precisely, we estimate the effect of a given incentive change on unit sales by comparing changes in unit sales before and after the incentive change takes effect in a given county (i.e., treatment group) with a baseline of changes in unit sales in counties with no incentive change (i.e., control group) in the same period. In other words, when estimating the average effect of the HOV incentive termination, the treatment group consists of counties where the HOV incentive becomes unavailable (i.e., counties in California and Utah); and the control group includes counties where the HOV incentive is in effect throughout our observation window (i.e., counties in Arizona, Colorado, Florida, Tennessee, New York, and Virginia).

The main identifying assumption of the DiD approach discussed above is that there are no unobserved, time-varying, county-specific variables that are correlated with both the incentive change and unit sales. To alleviate the potential concerns related to this assumption, we primarily rely on two supplemental approaches. First, we show that accounting for 1) important control variables (e.g., price, the number of charging stations), 2) unobserved county-specific time-invariant factors (e.g., preference for green products), 3) unobserved strata-specific time trends (e.g., changes in local economic conditions), and 4) unobserved time-varying factors specific for a given vehicle model but common across all counties (e.g., national advertising) do not change the estimated effect substantially. Therefore, the effect of any remaining unobservables would need to be relatively large compared to the factors we account for to result in a significant change in our qualitative findings.

Second, to further lessen the endogeneity concern, we combine the DiD with matching (see Singh and Agrawal 2011 and Zervas et al. 2017 for a similar approach). Intuitively, the goal of the matching procedure is to generate similar treated and control counties based on a set of observables to reduce the potential for unobservable differences between the two groups. Specifically, we apply the Coarsened Exact Matching (CEM) method in our main analyses (Iacus et al. 2009). To do so, we stratify counties based on observable characteristics related to policymakers' termination decisions, including local traffic conditions and commute times (as discussed in Section 1.2) as well as local preferences for green vehicles. Previous studies document several characteristics that distinguish U.S. consumers who have a high preference for green vehicles—green consumers—from those who do not (e.g., Potoglou and Kanaroglou 2007; Ozaki and Sevastyanova 2011). In particular, green consumers are more likely to have higher income and education levels. They are also more likely to be older females and to vote for the Democratic Party. As a result, we use income, education, gender, age, and political inclination along with traffic conditions and commute times as matching variables to generate strata.[25]

Our matching procedure results in 21 strata for the HOV termination analysis. We conduct t-tests to see whether counties in treatment and control groups are comparable in terms of the matching characteristics. Specifically, we perform a stratified t-test of the difference in the average of a given matching variable (e.g., income) between the two groups. None of the t-tests for all matching variables rejected the null hypothesis that the two groups have the same average at the 5% significance level. The results of these t-tests suggest that the counties in treatment and control groups are largely comparable in terms of key observables. Therefore, the variation in the treatment status (i.e., incentive change) across counties within the same stratum will allow us to identify the average effect of the HOV incentive termination.

In our subsequent analyses on the HOV incentive termination effect, we rely on a count data model, namely the fixed-effects negative binomial model, as our main specification. This is because our dependent variable (unit sales at the county-vehicle-month level) takes on the value zero for many observations in our data, especially for green vehicles. This is reflected in the summary statistics, which show a mean of 1.3 for unit sales of green vehicles covered by the HOV incentive. Faced with similar count variables as the dependent variable, previous literature modeling consumer demand in marketing have used count models, including the negative binomial and Poisson regressions (e.g., Busse et al. 2010; Wang and Goldfarb 2018; Ozturk et al. 2019). We later show that our findings are robust to alternative specifications such as a log-linear regression model as well as a Poisson model.

[25] Only 5 counties out of 27 counties are discarded after matching for the HOV incentive termination analysis. Our results remain similar when we estimate DiD using all counties (i.e., without dropping counties due to matching), as shown in Table D.1 in Online Appendix D.

1.5.1 The Effect of HOV Incentive Termination on Unit Sales

In this subsection, we estimate the average effect of HOV incentive termination on unit sales. We estimate this effect separately for three types of vehicles: green vehicles covered by the incentive (i.e., hybrid vehicles), green vehicles that are not covered (i.e., plug-in hybrid vehicles and electric vehicles), and non-green vehicles (i.e., gasoline vehicles).[26] Our unit of analysis is county (i) - vehicle model (j) - month (t), e.g., New York county - Toyota Prius - August 2011. Given that our dependent variable, i.e., unit sales, is a count variable, we identify the effect of interest by using the following negative binomial model with log link:

$$\begin{aligned} ln(Unit\, Sales_{ijt}) &= \beta_{HOV\, term} HOV\, termination_{it} \\ &\quad + \alpha_i + \delta_{s(i)j} + \lambda_{jt} + X'_{ijt}\gamma + \varepsilon_{ijt}. \end{aligned} \tag{1}$$

The dummy variable $HOV\, termination_{it}$ is equal to 1 after the HOV incentive termination is announced if a given county belongs to a state where there is an HOV incentive termination, and 0 otherwise.[27] County fixed effects, α_i's, capture time-invariant unobserved factors of each county such as the location of the market. $s(i)$ is the stratum s to which county i belongs, which is determined by the CEM algorithm as explained in subsection 1.5. Accordingly, $\delta_{s(i)j}$ is the stratum-vehicle fixed effect, which captures the baseline demand of a given vehicle model j for a given stratum. Vehicle model-month dummies, λ_{jt}'s, allow us to control for vehicle-month-specific unobservables such as manufacturer-level advertising as well as promotions to dealers and consumers (see Ozturk et al. 2016 for a similar approach). Note that as λ_{jt} subsumes vehicle model fixed effects, it also accounts for vehicle model-specific factors that do not vary over time such as manufacturer (e.g., GM), brand (Chevrolet), and nameplate (e.g., Volt). Since λ_{jt} subsumes month fixed effects, it also takes into account time-varying factors that might impact demand common

[26] Our conclusions do not change when we combine data across the three vehicle types and estimate a specification where we interact the incentive change with vehicle type.

[27] One concern with any study of the regulation of incentives is that the dates of enactment might not give precise measurement of the incentive's effect due to the anticipation or awareness of the target population. Therefore, in our main specification, we use the DMV's announcement about the HOV incentive termination to define the treatment start date. In subsection 1.5.2, we show robustness to an alternative treatment start date, i.e., the actual month of incentive termination.

to all vehicle models and counties such as consumer confidence in the economy (e.g., Ozturk et al. 2019). The vector X_{ijt} consists of county-, vehicle model-, and/or month-varying control variables. It includes the following annual county-level demographic variables: income, education, age, gender, unemployment rate, and mean travel time to work. The vector X_{ijt} also involves the average transaction price of a vehicle model j in county i in month t, monthly average gas price in each state, and the number of charging stations at the county-month level.

The coefficient of interest in Equation 1 is $\beta_{HOV\,term}$. Since there are no other coincident incentive changes for green vehicles in our analysis period, as mentioned in Section 1.2, we interpret $\beta_{HOV\,term}$ as the average effect of the HOV incentive termination on unit sales. As we conduct separate analyses for green vehicles covered by the incentive, green vehicles that are not covered, and non-green vehicles, we obtain three different coefficients (one for each vehicle type). For instance, when we use $ln(Unit\,Sales_{ijt})$ for green vehicles covered by the incentive as our dependent variable, if $\beta_{HOV\,term}$ is smaller than zero, we interpret it as indicating that the average unit sales for green vehicles covered by the incentive decreases by 100 $\times$ [1-exp($\beta_{HOV\,term}$)]% after the HOV incentive is terminated. Similarly, when we use $ln(Unit\,Sales_{ijt})$ for non-green vehicles as our dependent variable, if $\beta_{HOV\,term}$ is greater than zero, we interpret it as indicating that the average unit sales for non-green vehicles increases by 100 $\times$ [exp($\beta_{HOV\,term}$)-1]% after the HOV incentive is terminated.

Identification Check Before we proceed to the estimation of the average effect of the HOV incentive termination on unit sales, we perform an identification check to examine whether our empirical strategy can recover the causal sales effect of the incentive change. Specifically, we estimate Equation 1 with a slight modification by splitting our main independent variable ($HOV\,termination_{it}$) into a sequence of dummy variables for the months before and after the treatment (see Wang and Goldfarb 2018 for a similar approach). The base month is six months before the implementation of the program.

In Figure 1, we plot the coefficients associated with these monthly dummy variables to examine the parallel-trends assumption of our DiD identification strategy on unit sales of green vehicles covered by HOV incentive termination. The solid line shows the estimated coefficient for each month, and the bars show the 95% confidence interval for each coefficient. Figure 2 shows the coefficients associated with these monthly dummy variables for green vehicles not covered by HOV incentive termination and gasoline vehicles. These two graphs show that the estimated coefficients are insignificant before the treatment, which indicates that pre-trends in unit sales are comparable between the treatment and control groups after controlling for the covariates and fixed effects in our main specification. Although no identification test is conclusive as with any quasi-experimental analysis, this identification check supports our empirical strategy.

Main Effect The columns in Table 2 present the estimated effect of the HOV incentive termination on unit sales of green vehicles covered by the incentive (i.e., hybrid vehicles), green vehicles that are not covered (i.e., plug-in-hybrid vehicles and electric vehicles), and non-green vehicles (i.e., gasoline vehicles) using Equation 1. The estimates in Column (1) suggest that the average effect of the HOV incentive termination on unit sales of green vehicles covered by the incentive is negative (-.155) and statistically significant ($p < .01$), which amounts to a 14.4% [1-exp(-.155)] decrease in unit sales in response to the HOV incentive termination.[28] On the other hand, as shown in Columns (2) and (3), we do not find a statistically significant effect of the termination of the HOV incentive on unit sales of green vehicles that are not covered by the incentive (-.041, p = .73) and non-green vehicles (.005, p = .82). In sum, the estimation results indicate that the HOV incentive termination hurts the adoption of green vehicles covered by the incentive. However, on average, we do not find significant evidence of a demand spillover to vehicles that are not covered by the incentive change.

We also investigate whether the negative effect of the HOV incentive termination on covered green vehicles is persistent over time. Recall that we created month-specific treatment coefficients

[28]We provide various specifications with different subsets of fixed effects as well as with and without controls in Table E.1 in Online Appendix E. The consistently negative and significant coefficients for the HOV incentive termination dummy suggest that the effect of any remaining unobservables would need to be relatively large compared to the factors we account for to result in a significant change in our qualitative results (Altonji et al. 2005).

in our earlier discussions of identification checks, and we used Figure 1 to compare pre-treatment trends between the treatment and control groups. We can examine the months after the HOV incentive termination in these figures to see whether the effects vary over time. The coefficient for the HOV incentive termination is negative and significant for green vehicles covered by the incentive across all months following the announcement (i.e., confidence intervals do not cover zero).

1.5.2 Robustness Checks

To further strengthen the causal interpretation of our previous findings, as suggested in Goldfarb and Tucker (2014), we investigate a set of robustness checks including 1) different functional forms, 2) falsification exercises, 3) different treatment states for the HOV incentive termination (i.e., California vs. Utah), 4) differential trends across strata, and 5) alternative treatment dates.

We first check the robustness of our findings to other functional forms. Our main specification in Equation 1 uses a negative binomial with log-link to estimate the effect of incentive changes on unit sales. To examine the sensitivity of our results, we re-estimate the effect using Poisson and log-linear regression models in Table E.2 in Online Appendix E. These estimates show a significant decrease of 18.6% and 8.1% for Poisson and log-linear models, respectively. These results are consistent with our earlier findings for the HOV incentive termination. We also check the robustness of our results to falsification exercises, different treatment states for the HOV incentive termination (i.e., California vs. Utah), differential trends across strata, and alternative treatment dates in Online Appendix E. The results from these sensitivity analyses are in line with our earlier findings.

1.5.3 Alternative Identification Strategies

We also conduct analyses using other identification strategies, i.e., the border and the RDiT strategies, that complement our main empirical strategy based on DiD with CEM.

Border Strategy The DiD approach uses the unit sales in counties that are not affected by an incentive change as counterfactual unit sales for counties that are treated by that incentive change. Although we use CEM to ensure that treatment and control groups are comparable in terms of a set of observable characteristics, there may still be differences between the two groups in terms of unobservable characteristics. To allay this worry, we employ the "border strategy" by leveraging the variation in green-vehicle incentives around state borders (e.g., Shapiro 2018).

In this strategy, counties in a state without the HOV incentive termination will serve as controls for counties within the same market but on the other side of the border in another state where the incentive is terminated. As such, we will attribute the unit sales difference for a given vehicle type between neighboring counties in different treatment conditions to the HOV incentive termination. To the degree that neighboring counties in the same market but in different states are comparable in terms of unobserved characteristics, this border strategy supplements our primary empirical strategy that relies on matching based on observable demographic variables. The details of our analyses based on the border strategy are provided in Online Appendix F. The estimation results based on the border strategy are provided in Table F.1. The coefficient for the HOV incentive termination is negative and significant for the vehicles covered by the incentive, supporting our earlier results based on the DiD with CEM.

Regression-Discontinuity-in-Time (RDiT)-Style Analysis In our estimations so far, we have used a vehicle model's unit sales in a county without the HOV incentive termination as the counterfactual for unit sales of the same vehicle model in a similar county that is affected by the incentive termination. An alternative way to generate counterfactual unit sales is to use a vehicle model's unit sales just before the HOV incentive termination as the counterfactual for those just after the termination. The key identifying assumption of this approach is that there are no concomitant unobservables influencing unit sales that discontinuously change at the start of the incentive termination, which is supported by our earlier analyses discussed in subsection 1.3.4. To further reduce the possibility of time-varying unobservables that sharply change in the close temporal vicinity of the HOV incentive termination, it is crucial to concentrate on a narrow window around the incentive change. This idea of narrowing the temporal windows around the focal incentive change was used by previous marketing studies to alleviate the concern about time-varying unobservables (e.g., Ozturk et al. 2016).

As we implement this identification strategy as a complementary approach to our primary empirical strategy, we do not provide a complete set of analyses required by RDiT (see Hausman and Rapson 2017). As such, we call the associated analysis an RDiT-style analysis. Specifically, we rely on a local linear strategy where we use various relatively narrow time windows—i.e., 4 weeks, 8 weeks, 12 weeks and 16 weeks before and after each incentive change—to examine the effect of HOV incentive termination on unit sales of different vehicle types. The details of our analyses based on the RDiT strategy are provided in Online Appendix G. Table G.1 shows the local linear estimation results for the impact of the HOV incentive launch and termination. The estimates suggest that our earlier findings are robust to the RDiT-style analysis.

1.6 Mechanism

Our results so far show that the adoption of green vehicles covered by the incentive is negatively influenced by the HOV incentive termination. This section investigates various mechanisms through which the HOV incentive termination affects the adoption of green vehicles covered by the incentive. We argue that the HOV incentive influences consumer demand via a mechanism related to the functional value it provides: time-saving. The HOV incentive is expected to be more effective for consumers that seek a time-saving benefit. Therefore, if time-saving is the underlying mechanism, we anticipate a larger drop in hybrid vehicle sales in response to the HOV incentive termination in counties where consumers value time-saving more. In what follows, we provide suggestive evidence that supports this time-saving mechanism. We then rule out several alternative explanations.

The key benefit of the HOV incentive is that the unrestricted access to HOV lanes significantly reduces travel time for drivers. Some have seen their commuting times halved.[29] Therefore, one would expect that the HOV incentive launch would result in more green vehicle sales covered by that incentive for consumers that value time-saving more. On the flip side, the HOV incentive termination will likely have a more negative effect on green-vehicle adoption for consumers that value time-saving more. To establish that the time-saving mechanism is at work, we provide the following suggestive evidence. First, we provide evidence that the HOV incentive termination has a more negative sales effect on vehicles covered by the incentive in counties where residents spend more time commuting to work. Second, we show that the HOV incentive termination has a more negative sales effect on vehicles covered by the incentive in counties with higher income levels. Along with their inherent value in better understanding the effectiveness of governments' green vehicle incentives, these mechanism checks aim to "help make casual identification more convincing" (Goldfarb and Tucker 2014, p. 31).

1.6.1 *HOV Incentive Termination Has a More Negative Impact in Counties with Longer Commute to Work*

The benefit of having access to the HOV lane is more for consumers who have longer commutes. As a result, we expect that the impact of the HOV incentive termination will be more negative in counties where residents spend more time traveling to work. To assess this conjecture, we estimate the following specification:

$$\begin{aligned} ln(Unit\,Sales_{ijt}^{covered\_green}) = {} & \mu\, HOV\,termination_{it} \\ & + \mu_{Commute\,int}^{HOV} HOV\,termination_{it} \times Commute_{it} \\ & + \alpha_i + \delta_{s(i)j} + \lambda_{jt} + X_{ijt}'\gamma + \varepsilon_{ijt}. \end{aligned} \tag{2}$$

$Commute_{it}$ is the county-level mean travel time to work. $\mu_{Commute\,int}^{HOV}$ measures the heterogeneous (if any) impact of the HOV incentive termination on unit sales of green vehicles covered by the incentive in terms of the commute level in a given county. The fixed effects and control

[29] https://www.thecarconnection.com/news/1062439_california-hybrid-drivers-lose-car-pool-lane-privileges.

variables are the same as those defined for Equation 1. Column (1) in Table 3 provides the coefficient estimates for the specification above. The coefficient of the $HOV\,termination_{it} \times Commute_{it}$ interaction is negative and significant (-.049, p < .01), which suggests that the reduction in unit sales for the green vehicles covered by the incentive in response to the HOV incentive termination becomes more substantial as commute to work increases.[30]

1.6.2 *HOV Incentive Termination Has a More Negative Impact in Counties with Higher Income*

Previous literature points out that consumers with higher income value their time more because of its opportunity cost (e.g., Stigler 1961). Similarly, Cesario (1976) shows that drivers with higher income value reducing the commuting time more. Therefore, we expect that the impact of the HOV incentive termination will be more negative in counties with higher income. To formally examine this prediction, we use the following specification:

$$\begin{aligned} ln(Unit\,Sales_{ijt}^{covered\_green}) \quad = \quad & \mu\, HOV\,termination_{it} + \mu_{Commute\,int}^{HOV} HOV\,termination_{it} \times Commute_{it} \\ & + \mu_{Income\,int}^{HOV} HOV\,termination_{it} \times Income_{it} \\ & + \alpha_i + \delta_{s(i)j} + \lambda_{jt} + X_{ijt}'\gamma + \varepsilon_{ijt}. \qquad (3) \end{aligned}$$

The parameter of interest in this specification is $\mu_{Income\,int}^{HOV}$. It measures the heterogeneous (if any) impact of the HOV incentive termination on unit sales of green vehicles covered by the incentive in terms of the median household income in a given county. The fixed effects and control variables are the same as those defined for Equation 1. Column (2) in Table 3 presents the coefficient estimates for the specification above. The negative and significant coefficient of the

[30]We have also estimated similar interaction specifications using HOV lane miles (which was used by previous studies such as Sheldon and DeShazo 2017) or the distance between the centroid of a county and the closest HOV lane point to that centroid, instead of commute time. The estimates indicate that the drop in unit sales for the green vehicles covered by the incentive following the HOV incentive termination is greater in counties with 1) more HOV lane miles and 2) shorter distance to HOV lanes. However, as a large majority of variation (around 85%) in these two measures of HOV access is captured by the $HOV\,termination_{it} \times Commute_{it}$ as well as other fixed effects and control variables, the interactions of these two measures with the HOV termination dummy become insignificant when we also include $HOV\,termination_{it} \times Commute_{it}$. Therefore, we only report the coefficient for the $HOV\,termination_{it} \times Commute_{it}$ interaction in Table 3.

$HOV\,termination_{it} \times Income_{it}$ interaction ($\mu^{HOV}_{Income\,int}$ = -.017, $p < .05$) suggests that the reduction in unit sales for the green vehicles covered by the incentive in response to the HOV incentive termination is more in counties with higher income relative to those with lower income.

1.6.3 Other Interactions and Alternative Explanations

Symbolic Value The HOV incentive could be counterproductive if it reduces the symbolic value consumers derive from buying green vehicles. Previous research has shown that the purchase of new goods is a way to symbolize personal and social identities and values, especially for conspicuous products such as cars (Dittmar 1992). Specifically, earlier studies suggest that some car buyers purchase hybrid cars (or sustainable innovations in general) to signal a pro-environmental identity or image, i.e., "greenness" (Noppers et al. 2014). These signals can be particularly strong for sustainable innovations such as hybrid vehicles because these green options typically have inferior functional value (e.g., higher prices or behavioral costs) relative to their non-green counterparts (e.g., Gneezy et al. 2012). The increase in the functional value offered by green vehicles due to the HOV incentive could negatively influence green vehicle sales driven by the signaling motivation. This is because the incentive could render green vehicles less inferior (or even better) than non-green vehicles in terms of functional value. Accordingly, buying a green vehicle could provide a weaker (if any) signal of costly pro-environmental behavior when the HOV incentive is in effect. This can, in turn, lead to fewer green vehicle purchases by consumers who want to signal a green image. Such a potential reduction in symbolic benefits can be crucial. Some studies have shown that evaluations of symbolic attributes are the only significant factor explaining the adoption of sustainable innovations, whereas evaluations of functional attributes did not explain actual adoption when the evaluation of symbolic attributes are taken into account (Noppers et al. 2016).

The preceding discussion suggests that the termination of the HOV incentive may result in increased symbolic value for hybrid vehicles as they lose the incentive-driven functional value once the incentive is removed. If there is an increase in the symbolic value of covered green vehicles after the termination of the HOV incentive, then our estimates related to the time-saving mechanism could be biased toward zero. To assess whether a change in symbolic value after the termination could confound our earlier findings, we add an interaction term related to the potential for signaling greenness in Equation 3: $HOV\, termination_{it} \times Green\, version\, only_j$. The $Green\, version\, only_j$ is a dummy variable that takes on value 1 for hybrid vehicle models that do not have a non-green (i.e., gasoline) counterpart (e.g., for Toyota Prius), and 0 otherwise (e.g., for Toyota Camry). As previous research suggests that some consumers would like to be seen driving an explicitly "green" vehicle such as Toyota Prius (Gallagher and Muehlegger 2011), $Green\, version\, only_j$ is used to proxy the potential of a given vehicle model for signaling greenness. As shown in Column (3) in Table 3, the $HOV\, termination_{it} \times Green\, version\, only_j$ interaction is insignificant. Therefore, we do not find support for a change in symbolic value following the termination of the HOV incentive.

As an additional robustness check for the insignificant change in symbolic value, we use the dummy variables $Medium\, Solar_i$ and $High\, Solar_i$ that represent counties with medium and high levels of solar PV installations (see subsection 1.3.2 for the details of this measure). These variables proxy the level of social desirability of being seen as "green." Thus, they can be seen as a measure of the potential of a given county for signaling greenness. When we add the interaction terms $HOV\, termination_{it} \times Medium\, Solar_i$ and $HOV\, termination_{it} \times High\, Solar_i$ to the specification estimated in Column (3) in Table 3, we find that the coefficients of both interactions are insignificant. In line with our earlier results, this finding provides no evidence of a change in symbolic value after the HOV incentive termination. One possible explanation for the lack of change in symbolic value is that even after the termination of the HOV incentive, there will be hybrid vehicles on the road that were purchased during the HOV incentive period because of the increased functional value. Therefore, it will still be difficult to signal greenness to others even after the incentive ends.

Interactions with Other Demographics To allay concerns related to the potential moderating effects of other demographic variables on the relationship between HOV incentive termination and unit sales, we also control for additional interactions. Specifically, we include the interactions of income, education, age, gender, and unemployment rate with the HOV incentive termination in Equation 3, and report the estimates for our key interactions in Column (4) of Table 3.[31] Our previous findings remain robust to the inclusion of these additional interactions.

Pending Availability of an HOV Incentive for Plug-in Hybrids As mentioned in subsection 1.2.1, DMV was planning an HOV incentive for a new generation of plug-in hybrids in California, which eventually started more than a year after the HOV incentive termination for conventional hybrids. Therefore, one could argue that some consumers might decide to wait for that pending incentive following the termination of the HOV incentive for conventional hybrids.[32] To assess the potential implications of this feature of our setting on our key finding, we perform a new analysis using aggregate future sales for plug-in hybrid vehicle models by county. If the above explanation were at work, one would expect to see a larger sales decline following the HOV incentive termination in counties where there are more plug-in vehicle sales after the pending HOV incentive takes effect for plug-in hybrids. To see whether this is the case, we created a variable that indicates total sales of plug-in hybrid vehicles by county during the six months following the launch of the HOV incentive for plug-in hybrid vehicles (September 2012-February 2013). We then include the interaction of this variable with our HOV termination dummy in Equation 3 along with all other interactions discussed earlier. If this new interaction were significant and negative, it could provide support for the explanation above. However, this interaction coefficient is positive (.069) and insignificant ($p = .48$), which is not in line with the above explanation.[33]

[31] The highest correlation across demographic variables is between income and education, which is equal to .604. All other correlations have an absolute value smaller than .5.

[32] We thank an anonymous reviewer for bringing this possibility to our attention.

[33] We also consider other ways to address this alternative explanation in Online Appendix H.

1.7 Overall Effectiveness of the HOV Incentive in Boosting Green Vehicle Demand and Reducing Emissions

Our results so far indicate that the termination of the HOV incentive led to a reduction in green vehicle sales covered by the incentive via the time-saving mechanism. However, previous literature has documented that the launch of the HOV incentive has an insignificant (or even negative) relationship with green vehicle demand (Diamond 2009; Gallagher and Muehlegger 2011). Combined, these findings raise the following questions: Can the net effect of the HOV incentive on the unit sales covered by the incentive be negative? To the extent that the net effect is negative, does that mean that the HOV incentive could result in more carbon emissions? Given that state governments aim to increase the sales of green vehicles, and thus reduce carbon emissions through the HOV incentive, the answers to these questions have important implications for policymakers related to the overall effectiveness of the HOV incentive.

In the subsections that follow, we first examine the average effect of the HOV incentive launch on hybrid vehicle sales in California and Utah. We then compare the sales effects of the HOV incentive launch with that of termination. Next, we further investigate the broader implications of the HOV incentive for carbon emissions by examining the relationship of the HOV incentive termination (and launch) with 1) substitution from or to vehicles with different emission levels, 2) market size, as well as 3) carpooling behavior.

1.7.1 The Effect of HOV Incentive Launch on Unit Sales of Hybrid Vehicles

In this subsection, we estimate the effect of the HOV incentive launch on unit sales of hybrid vehicles. We follow the same structure we used earlier for the analysis of HOV incentive termination. Specifically, we replace the $HOV\ termination_{it}$ variable in Equation 1 with the $HOV\ launch_{it}$ dummy variable that is equal to 1 after the HOV incentive is launched if a given county belongs to a state where there is an HOV incentive launch, and 0 otherwise. The fixed effects and control variables are the same as those defined for Equation 1. Also, the interpretation of the coefficient for the $HOV\ launch_{it}$ variable is the same as that discussed for $\beta_{HOV\ termination}$ earlier.

Column (1) in Table 4 presents the estimated effect of the HOV incentive launch on unit sales of hybrid vehicles using a symmetric 12-month window (6 months before and 6 months after) around the HOV incentive launch. The estimates suggest that the average effect of the HOV incentive launch on unit sales of green vehicles covered by the incentive is positive (.016) but statistically insignificant (p = .86), which amounts to an insignificant 1.61% [exp(.016)-1] increase in unit sales in response to the HOV incentive launch.[34] One might contend that the insignificant effect of the HOV incentive launch around the close temporal vicinity of the launch could be due to a potential lack of publicity of the incentive at the beginning. If this were the case, the effect of the HOV incentive launch could become positive and significant over time as more consumers become aware of the incentive. To examine this possibility, we re-estimated our main specification by extending the post-period to 12 months after the HOV incentive launch. As shown in Column (2) in Table 4, the effect of the launch remains insignificant with this longer window. This result is robust to an even wider 18-month post-launch window.[35] Albeit with a different effect direction, this insignificant main effect finding gives support to earlier studies that report an insignificant association between the HOV incentive launch and green vehicle sales (Gallagher and Muehlegger 2011; -7%, $p > .10$) and market share (Diamond 2009; -12%; $p > .10$).

In addition to our main effect analysis, we also explore the mechanism that underlies the insignificant effect of the HOV incentive launch on hybrid vehicle sales. Specifically, we check whether the insignificant effect could be due to an increase in functional value for hybrid vehicles that is offset by a reduction in symbolic value. Similar to the approach we used in our HOV termi-

[34]We provide various specifications with different subsets of fixed effects as well as with and without controls in Table I.1 in Online Appendix I. The consistently insignificant coefficients for the HOV incentive launch dummy suggest that the effect of any remaining unobservables would need to be relatively large compared to the factors we account for to result in a significant change in our qualitative results (Altonji et al. 2005). Additionally, in various Online Appendices, we show that our incentive launch findings are robust to: 1) DiD without matching (Table D.1), 2) different functional forms (Table I.2), 3) different treatment states (Table I.3), and 4) differential trends across strata (Table I.4).

[35]We also examined the web search interest (based on Google Trends) in specific search terms related to the HOV incentive launch in California, including "yellow sticker," "yellow decal," "HOV hybrid," and "carpool hybrid." The trend in the interest for these terms suggested that the interest in the HOV incentive was highest right after the actual launch of the incentive, and it diminished over time. Therefore, the 18-month post-launch window we used in our HOV launch analysis covers a longer period than the period when the interest was at its peak.

nation analysis, we add the following interactions to our specification: $HOV\ launch_{it} \times Commute_{it}$ and $HOV\ launch_{it} \times Green\,version\,only_j$ along with interactions with other demographics.[36] As shown in Column (3) in Table 4, the coefficient for $HOV\ launch_{it} \times Commute_{it}$ is positive (.058) and significant ($p < .01$). This result is in line with an increase in functional value due to time-saving.[37] Also, the coefficient for $HOV\ launch_{it} \times Green\,version\,only_j$ is negative (-.412) and significant ($p < .001$), providing evidence for a reduction in symbolic value (i.e., signaling greenness).

Besides, similar to our termination analysis, we also add the interaction terms $HOV\ launch_{it} \times Medium\,Solar_i$ and $HOV\ launch_{it} \times High\,Solar_i$ to the specification estimated in Column (3) in Table 4. While the coefficient of the $HOV\ launch_{it} \times Medium\,Solar_i$ is negative but insignificant (-.109; $p > .1$); the coefficient of the $HOV\ termination_{it} \times High\,Solar_i$ is negative (-.523) and significant ($p < .05$). This means that unit sales of green vehicles covered by the incentive reduce more in counties with high (vs. low) potential for signaling greenness. This result gives further support to a decline in symbolic value following the incentive launch. Combined, these findings are consistent with the view that an increase in functional value after the incentive launch is canceled out by a reduction in symbolic value, resulting in an insignificant launch effect.[38]

1.7.2 Comparison of the Sales Effects of HOV Incentive Launch and Termination

The analysis above suggests that, on average, the launch of the HOV incentive results in only an insignificant 1.61% increase in hybrid vehicle sales. In contrast, the termination of the HOV incentive leads to a decrease of 14.4% in hybrid vehicle sales. Besides, while the symbolic value of purchasing a green vehicle is found to decrease after the HOV incentive launch, this reduction

[36]Note that we do not include the $HOV\ launch_{it} \times Income_{it}$ interaction, because, in our data for the HOV launch analysis, there is not adequate variation in $HOV\ launch \times Income_{it}$ to exploit after incorporating $HOV\ launch_{it} \times Commute_{it}$ as well as other interactions, fixed effects, and control variables. When we exclude the $HOV\ launch_{it} \times Commute_{it}$ interaction, the coefficient for the $HOV\ launch_{it} \times Income_{it}$ interaction becomes positive and significant.

[37]This finding is in line with anecdotal evidence suggesting that the HOV incentive has helped to sell hybrid vehicles, especially if such a perk is a deal-breaker (https://www.wsj.com/articles/SB10001424052702303812104576441781190400052).

[38]We also consider an alternative explanation related to the pending availability of greener electric vehicle models in Online Appendix J, but we do not find support for this explanation.

is not restored after the incentive termination. In other words, green vehicle adoption has become partially reliant on the HOV incentive in the long term, because such incentives have crowded out the symbolic motivations to go green. As a result, the effect of incentive termination is not simply the opposite of that of launch, implying that governments' green product incentives could backfire in terms of boosting the demand for green vehicles.

1.7.3 HOV Incentive and Greenhouse Gas Emissions

This subsection studies various factors related to the effectiveness of the HOV incentive in reducing overall carbon emissions.

HOV Incentive Termination Shifts Consumers to Non-Green Vehicles with High Tailpipe Emissions An important factor that influences the emission-related implications of the HOV incentive is the potential substitution patterns between hybrid and gasoline vehicles after the incentive ends. To examine such substitution behavior, we estimate a new specification that estimates the heterogeneous (if any) effect of the HOV incentive termination on unit sales of gasoline vehicles in terms of the tailpipe emission category of a given vehicle model. We use a categorical (i.e., low, medium, and high) measure for vehicle emission. The categories are determined based on the terciles of tailpipe emissions of greenhouse gases. To estimate this heterogeneity based on vehicle emission categories, we add two interactions terms, i.e., $HOV\ termination_{it} \times Medium\ Emission_j$ and $HOV\ termination_{it} \times High\ Emission_j$ to Equation 1. The estimates reported in Column (1) in Table 5 reveal that the HOV incentive termination shifts consumers to gasoline vehicles with high tailpipe emissions (.102, $p < .05$).[39]

One possible explanation for this shift is that hybrid vehicles are typically more expensive vehicles relative to gasoline vehicles within a given category. Therefore, it is likely that the second-choice vehicle for consumers who do not purchase hybrid cars in the absence of the HOV incentive

[39] We also estimated a similar heterogeneity specification for the HOV incentive launch. The interactions of vehicle emission categories with the HOV incentive launch were insignificant, which is in line with the fact that the HOV incentive launch does not have a significant main effect on hybrid vehicle sales.

is relatively expensive gasoline models, which tend to have higher emissions.[40] To see if this explanation has any support in our data, we include two interaction terms, i.e., $HOV\,termination_{it} \times Medium\,Price_j$ and $HOV\,termination_{it} \times High\,Price_j$ to Equation 1. The price categories are determined based on the terciles. In support of the explanation above, the coefficients of both interactions are positive and significant: .128 ($p < .001$) for $HOV\,termination_{it} \times Medium\,Price_j$, and .161 ($p < .001$) for $HOV\,termination_{it} \times High\,Price_j$.

We also explore the potential substitution from hybrid vehicles to zero-emission (i.e., electric) vehicles that are not covered by the incentive following the incentive termination. Specifically, we re-estimate our main specification in Equation 1 by using only the sample of electric vehicles. The coefficient for the $HOV\,termination_{it}$ dummy is insignificant, as reported in Column (2) in Table 5. Therefore, we do not find evidence of demand substitution to electric vehicles after the HOV incentive termination.[41]

Market Shrinks After the HOV Incentive Termination In addition to examining substitution patterns, it is also important to know whether the HOV incentive results in market expansion after its launch and market shrinkage after its termination to assess the overall effectiveness of the HOV incentive in reducing carbon emissions. In Columns (1) and (2) of Table 6, we estimate the effects of HOV incentive termination and launch on total vehicle sales at the county level, respectively. These estimates show that, on average, the new-car market shrinks significantly by 4.7% following the HOV incentive termination. On the other hand, the new-car market size does not change significantly in response to the HOV incentive launch.

The Percentage of Carpoolers Increases After the HOV Incentive Termination As noted earlier, the HOV incentive was terminated to reduce congestion in HOV lanes, which were originally constructed to encourage carpooling. Thus, it is possible that the HOV incentive termination could lead to an increase in the percentage of carpoolers in a county due to potentially lower lev-

[40] We thank an anonymous reviewer for this insight.

[41] Note that demand substitution from electric vehicles following the HOV incentive launch was not possible, because electric vehicles were not available around the incentive launches.

els of congestion after incentive termination. We check whether this possibility has any empirical support by regressing the annual county-level percentage of carpoolers on the *HOV termination* dummy along with stratum and year fixed effects. This estimation, reported in Column (3) of Table 6, suggests that the percentage of carpoolers significantly increases by 1.9% ($p < .001$) following the HOV incentive termination. In contrast, as shown in Column (4) of Table 6, the percentage of carpoolers does not change significantly after the HOV incentive launch.

The findings above suggest that there are countervailing forces that influence the overall emission levels. While the substitution to non-green vehicles with high tailpipe emissions after the HOV incentive termination implies greater levels of tailpipe emissions; shrinkage in market size for new vehicles and the increase in the percentage of carpoolers imply lower levels of tailpipe emissions. Thus, the direction of the effect of the HOV incentive on overall emissions is mixed. To shed some light on the possible direction of the overall emission effect, we use annual county-level emission data from California around the HOV incentive termination (such data are not available for California around the HOV incentive launch, and Utah around incentive launch and termination). We then regress the emission variable on the *HOV termination* dummy along with county and year fixed effects. We find that the emission is negatively associated with the termination of the HOV incentive. This negative correlation provides some support for the claim that the negative effects of the reduction in market size for new vehicles and the increase in the percentage of carpoolers on emissions can dominate the positive effect of the substitution to non-green vehicles with high tailpipe emissions after the HOV incentive termination. Combined, our emission-related findings imply that although the net effect of the HOV incentive on the unit sales covered by the incentive could be negative, the HOV incentive may result in less carbon emission due to market size and carpooling effects. That said, we urge the readers to exercise caution in evaluating the evidence on overall emissions as the evidence is based on annual data for one state without a control group.

1.8 Conclusion

In a period when many federal and state governments consider adopting green-vehicle incentives (e.g., Georgia in the U.S[42], Denmark[43], and New Zealand[44]), it is crucial to understand whether and where these incentives are effective in boosting demand for green products and alleviating the negative environmental externalities of vehicle use. This study empirically examines the effectiveness of a commonly used non-monetary government incentive, i.e., HOV lane exemption.

The key findings of this study include the following: First, unit sales of vehicles covered by the HOV incentive (i.e., hybrid vehicles) reduce by 14.4% following the incentive termination. In contrast, on average, green vehicles that are not covered by the HOV incentive (i.e., plug-in hybrid and electric vehicles) and non-green (i.e., gasoline) vehicles do not experience a change in unit sales after the HOV incentive termination. Second, the HOV incentive termination has an immediate negative effect right after the announcement of the termination, and it persists in the medium term (i.e., six months after the incentive termination). Third, in line with the time-saving mechanism, the HOV incentive termination has a more negative sales effect on vehicles covered by the incentive in counties with longer commute times and those with higher income levels. Fourth, the launch of the HOV incentive results only in an insignificant 1.61% increase in hybrid vehicle sales. Combined with the 14.4% reduction in hybrid vehicle sales following the HOV incentive termination, this result suggests that the effect of termination is not simply the opposite of that of launch. Fifth, following the HOV incentive termination, 1) consumers substitute to non-green vehicles with high tailpipe emissions, 2) the new-car market diminishes, and 3) the percentage of carpoolers goes up.

[42] https://politics.myajc.com/news/state--regional-govt--politics/georgia-lawmakers-try-bring-back-electric-vehicle-tax-break/MJIY2sPBwYK4NXAtHsEqRJ/.

[43] https://cleantechnica.com/2018/05/01/denmark-rethinks-ev-incentives-after-market-collapses/.

[44] https://www.newshub.co.nz/home/politics/2018/09/government-promises-decent-incentives-for-electric-cars.html.

1.8.1 Implications

The findings above have important implications for policymakers and managers. First, given the negative effect of the HOV incentive termination on green vehicle sales, policymakers need to consider long-term, adverse consequences of a green-vehicle incentive. The significant decline in hybrid vehicle sales after the HOV incentive termination happened more than a decade after the introduction of hybrid vehicles into the market. This implies that green vehicle adoption has become partially reliant on the incentive even in the long term.

Second, our finding that the negative impact of the HOV incentive termination is greater than the insignificant positive impact of the launch implies that non-monetary incentives such as the HOV incentive can be counterproductive in terms of stimulating demand for green vehicles. The asymmetry in the effects of the HOV incentive launch and termination appears to stem from the fact that the symbolic value of buying a green vehicle covered by the HOV incentive declines after the launch, but it does not go back up following the termination. Thus, our results reveal an important tradeoff policymakers face in deciding whether to provide the HOV incentive: an increase in green vehicle sales due to increased functional value over the course of the incentive versus a reduction in green vehicle sales due to reduced symbolic value during and beyond the incentive period.

Third, although the HOV incentive may not be effective overall, our results suggest that it does generate additional sales in markets where consumers have longer commute times and higher income. Such demographic heterogeneity in the effects of the HOV incentive could help local governments choose an appropriate incentive based on the characteristics of the local market under consideration.

Fourth, our findings suggest that policymakers should also consider the impact of the HOV incentive on demand for non-green vehicles, the size of the market, and carpooling behavior while assessing the broader environmental impact of the incentive. Indeed, our findings reveal that the direction of the effect of the HOV incentive on overall emissions is not straightforward. Whereas substitution to non-green vehicles with high tailpipe emissions following the HOV incentive termination implies greater levels of greenhouse gas emissions; shrinkage in the market size for new vehicles and the increase in the percentage of carpoolers imply lower levels of emissions. The negative correlation we found between the termination of the HOV incentive and total emission levels in California implies that the adverse impact of the incentive termination on green vehicle sales might have had an unintended positive consequence in terms of gas emissions. This, in turn, provides support to a common but untested criticism that the HOV incentive runs counter to other policies designed to promote energy-efficient practices.

Fifth, companies increasingly engage in green branding activities to attract new customers, including those that value signaling a green image through their green product purchases. Our findings supporting a negative net effect of the HOV incentive on green vehicle sales imply that firms' ability to sell their products based on the "green image" could be undermined by counterproductive government policies. In particular, government incentives for green products can reduce the symbolic value consumers derive from green products, and thus they can have a direct harmful impact on the effectiveness of companies' green branding efforts. Also, our results regarding the heterogeneity of the effectiveness of the HOV incentive in terms of time (i.e., short vs. medium term), as well as local market characteristics (i.e., commute times and income) could help managers tailor their production schedules and inventory levels in response to government incentives.

Finally, managers need to take into account the impact of government incentives on the demand for non-green products along with their effect on green products. Our finding that the new-car market shrinks after the termination of the HOV incentive suggests that some consumers may not simply switch to other vehicle options. Instead, they may prefer carpooling, as revealed by our analyses. Furthermore, our results indicate that some manufacturers could be less vulnerable than others to the impact of governments' green-product incentives on their non-green vehicle sales. Since we show that the HOV incentive termination shifts consumers to non-green vehicles with high emissions, manufacturers with a larger portfolio of such vehicles could be less negatively affected after the termination of the HOV incentive. This points to another strategic consideration for companies that invest in their green-vehicle portfolio.

1.8.2 Limitations and Future Research

Although we provide an extensive set of robustness checks, including alternative identification strategies, readers should assess the evidence as they would in any study relying on observational data. In this study, we examine the sales effect of HOV incentive terminations for conventional hybrid vehicles in a context where a future HOV incentive was expected for newer models of green vehicles. Although we have provided several types of evidence to allay the concerns associated with this feature of our setting, readers should be cautious in extrapolating our findings to contexts without pending incentives. More research on the effectiveness of other types of monetary and non-monetary incentives would enhance our understanding of which incentives work best to promote green products and why. Furthermore, our analyses in this paper concentrate on incentive changes in two states. It is important to note that these states are part of different regions based on the Bureau of Economic Analysis. While California is part of Far West, Utah belongs to Rocky Mountain. As such, our results can be generalizable to two different regions. Also, our findings related to the impact of the HOV incentive launch are in line with prior studies using data from other states. We hope that future studies will shed further light on the generalizability of our results, as more states launch and terminate their HOV incentives. Finally, a more comprehensive structural analysis of the welfare implications of green-product incentives is another fruitful direction for future research.

Figure 1: The Impact of HOV Incentive Termination on Green Vehicles Covered by the Incentive

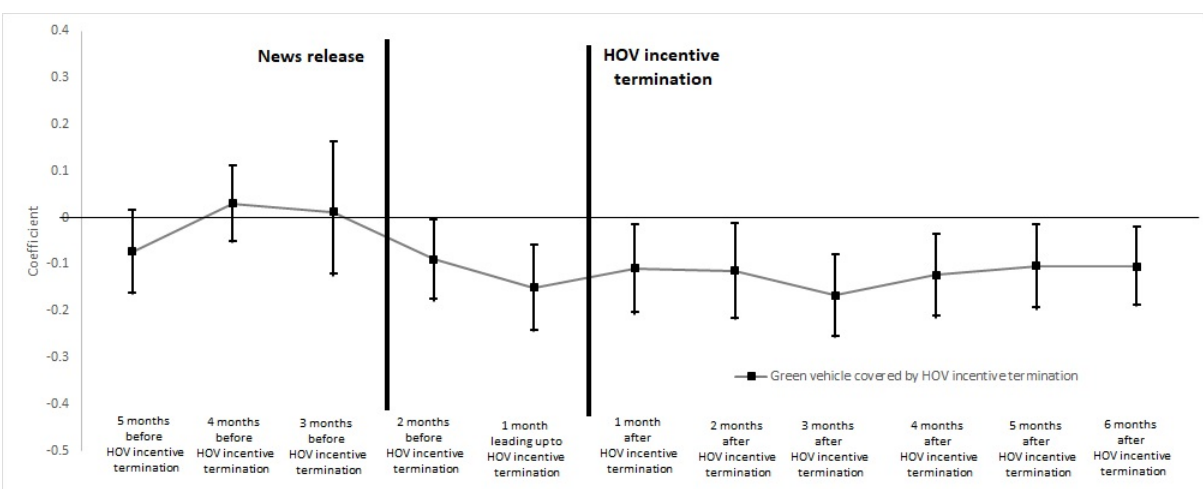


Figure 2: The Impact of HOV Incentive Termination on Vehicles Not Covered by the Incentive

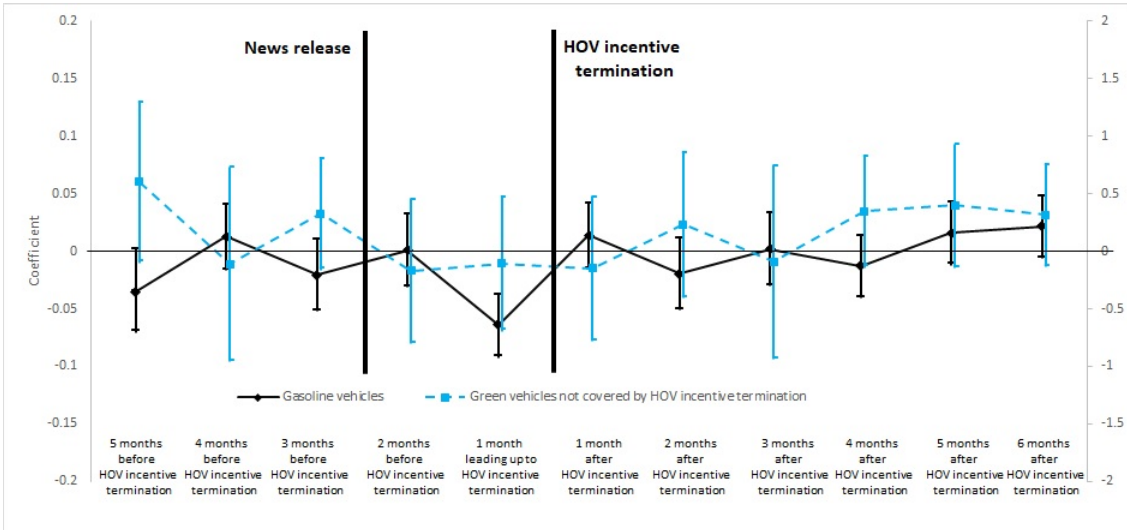


Table 1: Descriptive Statistics and Raw Data Patterns

Variables		**No. of obs.**	**Mean**	**Std. dev.**	**Max**	**Min**
County-vehicle model-month level variables						
Green vehicles covered by the HOV incentive	Price (in thousands USD)	7,420	35.0	5.4	46.5	16.9
	Unit sales	7,420	1.3	4.7	95	0.0
Green vehicles not covered by the HOV incentive	Price (in thousands USD)	232	41.0	4.7	55.1	32.6
	Unit sales	232	1.6	2.0	13	0.0
Gasoline vehicles	Price (in thousands USD)	77,713	35.3	16.9	198.3	10.0
	Unit sales	77,713	1.9	4.6	124	0.0
State-month level variable						
Gas Price (USD)		1,800	3.6	0.2	4.5	2.9
County-year level demographics						
Income: median household income (in thousands USD)		3,501	50.3	10.4	101.6	22.1
Education: % bachelor degree or higher achievement		3,501	22.9	9.3	58.3	4.6
Commute: mean travel time to work (in minutes)		3,501	24.6	4.8	44.2	14.5
Political inclination: % votes for the Democratic Party		3,501	41.8	16.8	90.0	10.1
Gender: % male residents		3,501	50.0	2.4	72.7	36.0
Age: Median age		3,501	37.4	1.5	41.7	32.6
Unemployment: % unemployment rate		3,501	9.2	3.1	22.9	1.5
% of carpoolers		3,501	0.11	0.02	0.21	0.05
State-year level variable						
Average daily traffic volume		150	4,841	2,159	9,223	1,211
Categorical vehicle model level variable						
Emission level (percentage occurrence)		403	High 42.9% ; Medium 44.2% ; Low 12.9%			

Model-free evidence for the HOV incentive termination

	Counties with Incentive Termination	Counties Without Incentive Termination	Difference (p-value of t-test)
% unit sales change for green vehicles covered by HOV incentive after termination	-18.1%	-7.6%	-10.5%* (0.03)
% unit sales change for green vehicles not covered by HOV incentive after termination	2.0%	4.7%	-2.7% (0.74)
% unit sales change for gasoline vehicles after termination	8.3%	7.1%	1.2% (0.89)

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001.

Table 2: The Impact of HOV Incentive Termination on Unit Sales of Different Vehicle Types

Dependent variable: Unit sales	(1) Green vehicles covered by the incentive	(2) Green vehicles not covered by the incentive	(3) Gasoline vehicles
HOV incentive termination	-0.155**	0.041	0.005
	(0.062)	(0.118)	(0.020)
Price	-0.015*	0.051+	-0.022***
	(0.008)	(0.031)	(0.001)
Stratum × vehicle model dummies	Yes	Yes	Yes
County dummies	Yes	Yes	Yes
Vehicle model × month dummies	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes
Number of observations	7,420	1,232	77,713
Log likelihood	-6,453	-1,235	-107,937

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table displays the results based on Equation 1, which measures the average sales effect of HOV incentive termination on green vehicles covered by the incentive change (i.e., hybrid vehicles), green vehicles not covered by the incentive change (i.e., plug-in hybrid vehicles and electric vehicles), and gasoline vehicles, respectively. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table 3: Possible Mechanisms

	(1) Commute	(2) Income	(3) Symbolic value	(4) Other demographics
HOV incentive termination	1.108**	1.928***	1.938***	3.569
	(0.400)	(0.533)	(0.535)	(6.189)
HOV incentive termination × Commute	-0.049**	-0.040**	-0.040**	-0.039+
	(0.021)	(0.019)	(0.018)	(0.023)
HOV incentive termination × Income		-0.017*	-0.017*	-0.018*
		(0.007)	(0.008)	(0.009)
HOV incentive termination × Green version only			-0.024	-0.023
			(0.079)	(0.083)
Other interactions (see the notes below)	No	No	No	Yes
Dummies: Stratum × vehicle model, county, vehicle model × month	Yes	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes	Yes
Number of observations	7,420	7,420	7,420	7,420
Log likelihood	-6,452	-6,451	-6,450	-6,448

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (county) are reported in parentheses. Control variables include vehicle price, state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute. Interactions with other demographics (not reported due to space constraints) include interactions of the incentive termination with the remaining demographics that are part of our control variables.

Table 4: The Impact of HOV Incentive Launch on Hybrid Vehicle Sales

Dependent variable: Unit sales	(1) Main effect (6-month post period)	(2) Main effect (12-month post period)	(3) Heterogeneity
HOV incentive launch	0.016	0.063	-1.648**
	(0.183)	(0.085)	(0.611)
HOV incentive launch × Commute			0.058**
			(0.021)
HOV incentive launch × Green version only			-0.412***
			(0.113)
Other interactions (see the notes below)	No	No	Yes
Dummies: Stratum × vehicle model, county, vehicle model × month	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes
Number of observations	25,860	54,006	54,006
Log likelihood	-28,861	-63,545	-63,521

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Control variables include vehicle price, state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute. Interactions with other demographics (not reported due to space constraints) include interactions of the incentive termination with the remaining demographics that are part of our control variables.

Table 5: Heterogeneous Effects of HOV Incentive Termination by Vehicle Emission Category

Dependent variable: Unit sales	(1) Emission category	(2) Zero-emission
HOV incentive termination	-0.075	0.081
	(0.093)	(0.192)
HOV incentive termination × Medium emission	0.067	
	(0.069)	
HOV incentive termination × High emission	0.102*	
	(0.044)	
Other interactions and control variables (see the notes below)	Yes	No
Dummies: Stratum × vehicle model, county, vehicle model × month	Yes	Yes
Number of observations	77,713	476
Log likelihood	-94,242	-523

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Column (1) uses the sample of gasoline vehicles and estimates the heterogeneous effect of HOV incentive termination on unit sales of gasoline vehicles in terms of emission category. Column (2) uses the sample of zero-emission vehicles and estimates the main effect of HOV incentive termination on unit sales of zero-emission vehicles. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute. Other interactions (not reported due to space constraints) include interactions of the incentive termination with vehicle price and category.

Table 6: Market Size and Carpooling Behavior

	Market Size		Carpooling	
	(1) HOV termination	(2) HOV launch	(3) HOV termination	(4) HOV launch (UT)
HOV incentive change	-0.047*	0.016	0.019***	-0.040
	(0.016)	(0.022)	(0.004)	(0.023)
County dummies	Yes	Yes	No	No
Month dummies	Yes	Yes	No	No
Year dummies	Dropped (due to month dummies)	Dropped (due to month dummies)	Yes	Yes
Stratum dummies	No	No	Yes	Yes
Control variables (see the notes below)	Yes	Yes	No	No
Number of observations	1,879	12,110	274	66
Log likelihood	-5,231	-48,097		
R^2			0.478	0.207

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. In Columns (1) and (2), we estimate the effects of HOV incentive termination and launch on total new vehicle sales at the county level using a negative binomial model, respectively. Specifications in Column (1) and (2) contain several control variables, including state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute. In Columns (3) and (4), we estimate an ordinary least squares regression model where the dependent variable is the annual county-level percentage of carpoolers. For the estimation in Column (4), we use data from Utah (UT), as carpooling data are not available around the HOV incentive launch period in California.

Online Appendix A: List of Green Vehicle Models

Table A.1: List of Green Vehicles in Our Analysis Period for the HOV Incentive Termination

Hybrid		Plug-in Hybrid	Electric
BMW ActiveHybrid 750I	Lexus HS 250H	Chevrolet Volt	Nissan LEAF
BMW ActiveHybrid 750LI	Lexus LS 600H L		
Cadillac Escalade Hybrid	Lexus RX 450H		
Chevrolet Silverado 1500 Hybrid	Lincoln MKZ Hybrid		
Chevrolet Tahoe Hybrid	Mercedes-Benz S400 Hybrid		
Ford Escape Hybrid	Mercury Mariner Hybrid		
Ford Fusion Hybrid	Mercury Milan Hybrid		
GMC Sierra 1500 Hybrid	Nissan Altima Hybrid		
GMC Yukon Denali Hybrid	Porsche Cayenne Hybrid		
GMC Yukon Hybrid	Toyota Camry Hybrid		
Honda CR-Z	Toyota Highlander Hybrid		
Honda Civic Hybrid	Toyota Prius		
Honda Insight	Volkswagen Touareg Hybrid		
Lexus GS 450H			

Online Appendix B: Criteria for Determining Isolated Markets

Table B.1: Isolated Market Criteria

	Minimum distance (in miles) to a CBSA with the following populations, Pop			
Population in CBSA (Thousands)	Pop > 50	Pop > 100	Pop > 200	Pop > 500
[0,100]	50	50	100	100
[100,200]	-	50	100	100
[200,500]	-	-	50	100
500+	-	-	-	100

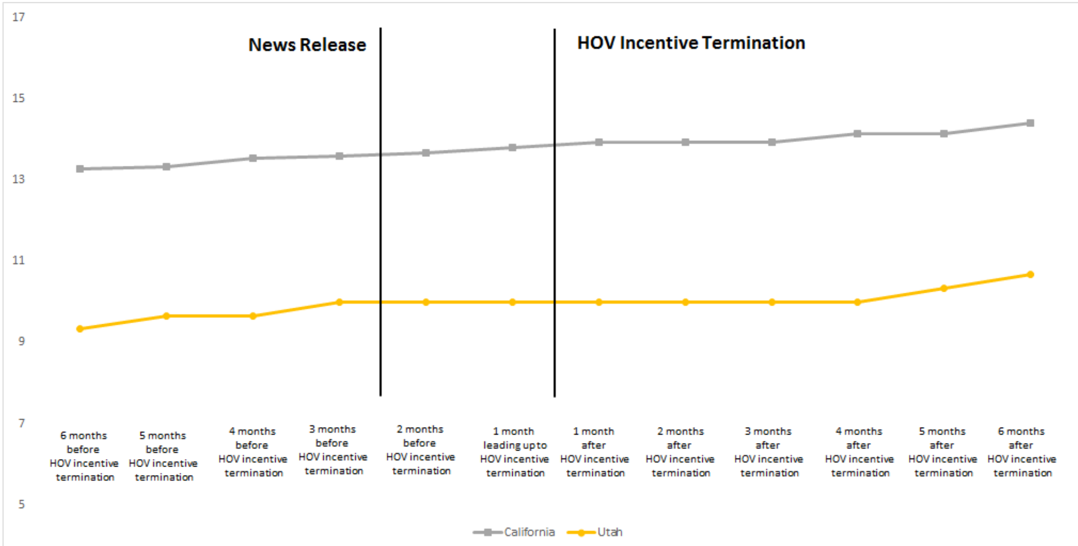
Notes: The above criteria are adapted from Olivares and Cachon (2009). A CBSA with population indicated in the first column is included in our analysis sample if it satisfies the criteria shown in the other columns.

Online Appendix C: Other Potential Changes Around the HOV Incentive Termination

In this Online Appendix, we examine other potential changes around the HOV incentive termination that might confound the relationship between vehicle sales and the termination of the HOV incentive in California and Utah. Specifically, we first investigate whether there is any sharp change in the number of charging stations around the HOV incentive termination. As shown in Figure C.1, the availability of charging stations does not change discontinuously around the HOV incentive termination. We account for the gradual changes in the number of charging stations by explicitly controlling for it in our empirical specifications.

Next, following the approach used by earlier studies (e.g., Tirunillai and Tellis 2017), we search for news articles on Factiva for potential news about major changes related to infrastructure or other adoption barriers for green vehicles. In particular, we collect data on the news reported in the two states where the HOV incentive is terminated, i.e., California and Utah, from April 2011 to August 2011. We use the following search terms: "charging station," "HOV," "electric vehicle infrastructure," "Assembly Bill 2628," "Toyota Prius," "Honda Insight," and "Honda Civic-hybrid." The search results did not provide any evidence of major changes around the HOV incentive termination.

Figure C.1: The Average Number of Charging Stations by County-Month in California and Utah



Online Appendix D: Difference-in-Differences Without Matching

Table D.1: Robustness to Difference-in-Differences Without Matching

	HOV termination			HOV launch		
Dependent variable: Unit sales	Green vehicles - covered	Green vehicles - not covered	Gasoline vehicles	Green vehicles - covered	Green vehicles - not covered	Gasoline vehicles
HOV incentive termination	-0.207*** (0.072)	-0.290 (0.457)	0.014 (0.023)			
HOV incentive launch				0.032 (0.037)	0.098 (0.144)	0.015 (0.075)
Dummies: Stratum × vehicle model, county, vehicle model × month	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes	Yes	Yes	Yes
Number observations	16,656	1,662	746,354	119,342	51,927	3,281,511
Log likelihood	-17,711	-1,084	-631,127	-119,846	-34,574	-3,482,724

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (county) are reported in parentheses. Control variables include vehicle price, state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Online Appendix E: Additional Robustness Checks

Falsification Exercise

We also perform a "falsification" test to alleviate any remaining concerns about endogeneity following earlier studies (e.g., Ozturk et al. 2016). Specifically, we re-estimate our main specifications for the two incentive changes to see whether there is a "treatment" group effect in a period when there is no incentive change–i.e., before May 2011. To do this, we use all the pre-incentive-change data and divide that into two halves, namely before and after the placebo incentive change. Then we test whether the coefficient for the placebo incentive change is significant. If we were to find a significant coefficient for the placebo incentive change on unit sales of green vehicles covered by the incentive in a period when no incentive change happens, this would suggest that unobservable differences that are correlated with incentive changes are contributing to our estimated effects. The results of this falsification exercise are reported in Table E.3. Column (1) indicates that the estimate for the placebo HOV incentive termination variable is insignificant. This falsification test reinforces the causal interpretation of our main effect estimate.

Different Treatment States

In our analyses of the HOV incentive launch and termination, we exploit incentive changes in two states, i.e., California and Utah. Given the leading role of California (which was the focal state in most of the previous studies) in the green movement, one might think that our results so far could be primarily driven by California, rather than Utah. To assess this possibility, we also investigate the impact of HOV incentive termination in California and Utah separately. The results associated with this analysis are provided in Table E.4. Our results indicate that HOV incentive termination hurts unit sales of green vehicles covered by the incentive change not only in California but also in Utah, albeit with a marginal significance.

Differential Trends Across Strata

To examine potential concerns regarding differential trends across strata, we repeat our estimation by adding strata-monthly trend interactions to our main specification in Equation 1. These interactions can capture gradually changing differential trends in variables such as gas prices or traffic patterns across strata. The results of this estimation are reported in Table E.5. The estimates show that our key finding is robust even after we allow for differential time trends across strata.

Alternative Treatment Dates

California and Utah made announcements before the actual enactment of the HOV incentive termination. As such, the date of enactment might not give a precise measurement of the effect of the HOV incentive termination due to consumers' anticipation of the incentive change. Therefore, in our main specifications, we use DMV and UDOT's announcements of the HOV incentive termination as the treatment. In Table E.6, we use the enactment of the HOV incentive termination as the treatment, and our results remain robust. Additionally, our conclusion does not change when we replicate this analysis after dropping the two months between the announcement and the enactment of the HOV incentive termination.

Table E.1: The Impact of HOV Incentive Termination - Robustness to Various Specifications

	Unit sales of green vehicles covered by HOV incentive termination			
HOV incentive termination	-0.339***	-0.308***	-0.380***	-0.155**
	(0.077)	(0.079)	(0.054)	(0.062)
Price		-0.045***	-0.023**	-0.015*
		(0.002)	(0.007)	(0.008)
Stratum $\times$ vehicle model dummies	No	No	Yes	Yes
Vehicle model $\times$ month dummies	No	No	No	Yes
Control variables (see the notes below)	No	Yes	Yes	Yes
County dummies	Yes	Yes	Yes	Yes
Number of observations	7,420	7,420	7,420	7,420
Log likelihood	-8,459	-8,173	-6,746	-6,453

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table E.2: The Impact of HOV Incentive Termination - Robustness to Other Functional Forms

	Unit sales of green vehicles covered by HOV incentive		Unit sales of green vehicles not covered by HOV incentive		Unit sales of gasoline vehicles	
	(1) Log-linear regression model	(2) Poisson model	(3) Log-linear regression model	(4) Poisson model	(5) Log-linear regression model	(6) Poisson model
HOV incentive termination	-0.084*	-0.206***	-0.028	-0.652	-0.007	0.022
	(0.039)	(0.045)	(0.198)	(0.708)	(0.008)	(0.014)
Price	-0.005+	-0.016*	-0.008	-0.087*	-0.007***	-0.032***
	(0.003)	(0.007)	(0.015)	(0.044)	(0.001)	(0.001)
Stratum × vehicle model dummies	Yes	Yes	Yes	Yes	Yes	Yes
County dummies	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle model × month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7,420	7,420	232	232	77,713	77,713
R square	0.629		0.558		0.530	
Log likelihood		-6,543		-248		-121,728

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table shows the robustness of the impact of HOV incentive termination to different functional forms. The dependent variable for the log-linear regression models is log(unit sales+1). The Poisson models use the log link. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table E.3: Falsification Exercise

	(1) Falsification
Placebo treatment	0.114
	(0.153)
Price	-0.003
	(0.013)
Stratum × vehicle model dummies	Yes
County dummies × month	Yes
Vehicle model × Month dummies	Yes
Control variables (see the notes below)	Yes
Number of observations	2,378
Log-likelihood	-2,293

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table shows the results from the falsification exercise for HOV incentive termination. March 2011 is the time of placebo treatment. We have 2 months (January and February 2011) as the pre-placebo-treatment period and 2 months (March and April 2011) as the post-placebo-treatment period. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute. Since our analyses use observations from the same year and we control for county fixed effects, all control variables at the county-year level are dropped.

Table E.4: The Impact of HOV Incentive Termination in Different States

	Unit sales of green vehicles covered by HOV incentive		Unit sales of green vehicles not covered by HOV incentive		Unit sales of gasoline vehicles	
	California	Utah	California	Utah	California	Utah
HOV incentive termination	-0.160**	-0.093+	-0.427	-1.063	0.005	0.030
	(0.065)	(0.057)	(0.531)	(2.752)	(0.021)	(0.043)
Price	-0.016*	-0.038	-0.107*	-0.077+	-0.024***	0.007*
	(0.008)	(0.036)	(0.048)	(0.048)	(0.001)	(0.003)
Stratum × vehicle model dummies	Yes	Yes	Yes	Yes	Yes	Yes
County dummies	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle model × month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7,022	398	184	48	68,896	8,817
Log likelihood	-5,907	-424	-205	-103	-93,650	-13,030

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table shows the estimation results of the impact of HOV incentive termination on green vehicles covered by the incentive change for California and Utah, separately. We use the same identification specification of Equation 1. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table E.5: HOV Incentive Termination - Robustness to Differential Trends Across Strata

	(1) Unit sales of green vehicles covered by HOV incentive	(2) Unit sales of green vehicles not covered by HOV incentive	(3) Unit sales of gasoline vehicles
HOV incentive termination	-0.217**	-0.073	0.002
	(0.068)	(0.754)	(0.024)
Price	-0.015*	-0.046	-0.022***
	(0.008)	(0.055)	(0.001)
Stratum × vehicle model dummies	Yes	Yes	Yes
Stratum dummies × month	Yes	Yes	Yes
Vehicle model × month dummies	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes
Number of observations	7,420	232	77,713
Log likelihood	-6,414	-212	-107,893

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table shows the robustness of the impact of HOV incentive termination to differential linear trends across strata. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table E.6: The Impact of HOV Incentive Termination - Robustness to a Different Treatment Date

	(1) Unit sales of green vehicles covered by HOV incentive termination	(2) Unit sales of green vehicles not covered by HOV incentive termination	(3) Unit sales of gasoline vehicles
HOV incentive termination	-0.090*	-0.504	0.033
	(0.050)	(0.620)	(0.028)
Price	-0.014*	-0.088*	-0.022***
	(0.007)	(0.044)	(0.001)
Stratum × vehicle model dummies	Yes	Yes	Yes
County dummies	Yes	Yes	Yes
Vehicle model × month dummies	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes
Number of observations	7,420	232	77,713
Log likelihood	-6,455	-247	-107,941

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table displays the results of Equation 1, which measures the average effect of HOV incentive termination on green vehicles covered by the incentive change (i.e., hybrid vehicles), green vehicles not covered by the incentive change (i.e., plug-in hybrid vehicles and electric vehicles), and gasoline vehicles, respectively. Here, instead of using the date of news release as our treatment start date, we use the enactment of HOV incentive termination. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

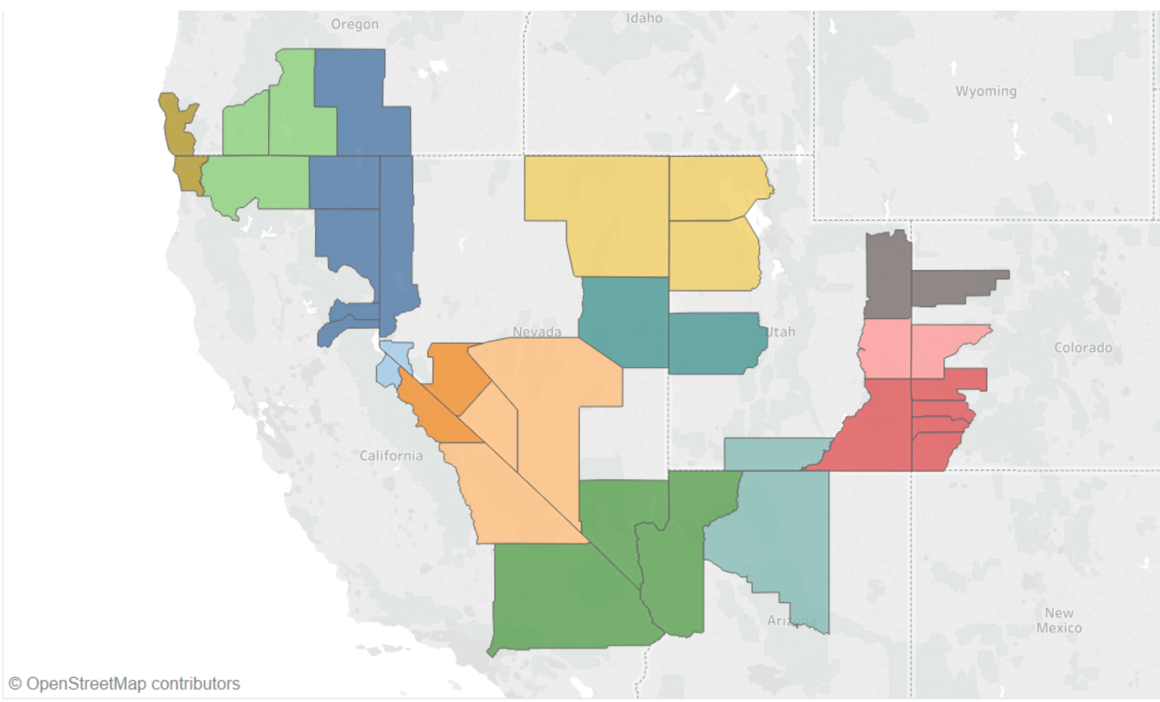
Online Appendix F: Additional Details on the Border Analysis

For our border analysis, we use counties in the following states neighboring California and Utah as our control group: Oregon, Nevada, Arizona, and Colorado (see Figure F.1). The data for our border analysis contain 37 counties across 16 markets. The unit of analysis for our border strategy is county - vehicle model - month. To estimate the effect of the HOV incentive termination using the border strategy, we use the following specification:

$$\begin{aligned} ln(Unit\,Sales_{ijt}) &= \beta^{border}_{HOV\,term} HOV\,termination_{it} \\ &\quad + \alpha_{c(i)j} + \lambda_t + X'_{ijt}\gamma + \varepsilon_{ijt}. \end{aligned} \tag{4}$$

The dummy variable $HOV\ termination_{it}$ is equal to 1 after the HOV incentive termination is announced if a given county belongs to a state where there is an HOV incentive termination, and 0 otherwise. The index $c(i)$ shows the market to which county i belongs. Market-vehicle model fixed effects, $\alpha_{c(i)j}$'s, allow the baseline unit sales of each vehicle model to vary across markets. Month fixed effects λ_t capture common time-variant unobservables. The vector X_{ijt} includes county-level demographic controls, including income, education, age, gender, unemployment rate, percentage of votes for the Democratic Party, and commute. The vector X_{ijt} also involves the average transaction price of a vehicle model j in county i in month t, monthly average gas price in each state, and the number of charging stations at the county-month level. $\beta^{border}_{HOV\ term}$ captures the impact of HOV incentive termination on unit sales.

Figure F.1: Map of Counties Used for the Border Analysis



Notes: Counties with the same color are in the same market.

Table F.1: The Impact of HOV Incentive Termination on Unit Sales - Border Strategy

	Green vehicles - covered	Green vehicles - not covered	Gasoline vehicles
HOV incentive termination	-0.452**	0.133	-0.019
	(0.158)	(26.937)	(0.025)
CBSA × vehicle model dummies	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes
Number of observations	674	84	30,462
Log likelihood	-597	-105	-32,906

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (CBSA) are reported in parentheses. Control variables include average vehicle price, state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, percentage of votes for the Democratic Party, and commute.

Online Appendix G: Additional Details on the RDiT-style Analyses

To estimate the effects of the HOV incentive termination using an RDiT-style approach, we estimate the following specification:

$$\begin{aligned} Unit\,Sales_{ijt} &= \beta^{RDiT}_{HOV\,termination} HOV\,termination_t + f(t) \\ &\quad + \alpha_i + \delta_j + X'_{ijt}\gamma + \varepsilon_{ijt}. \end{aligned} \tag{G.5}$$

$HOV\,termination_t$ is a dummy variable that is equal to 1 after the announcement of the HOV incentive termination, and 0 otherwise. $f(t)$ is a flexible polynomial (e.g., linear or quadratic) time trend that allows for separate trends on either side of the incentive change period. County fixed effects, α_i's, account for the impact of time-invariant county-level factors on unit sales. Vehicle model fixed effects, δ_j's, capture time-invariant vehicle model characteristics such as manufacturer, brand, and nameplate. The vector of control variables X_{ijt} includes the average transaction price of a vehicle model j in county i in month t as well as the number of charging stations in county i in month t. The key coefficient of interest is $\beta^{RDiT}_{HOV\,termination}$, which measures the impact of the HOV incentive termination on unit sales of the three vehicle types.

Table G.1: The Impact of HOV Incentive Termination on Unit Sales - RDiT-Style Analysis

	Unit sales of green vehicles - covered			
	4 weeks	8 weeks	12 weeks	16 weeks
Incentive change dummy	-0.220+	-0.376*	-0.292*	-0.355*
	(0.121)	(0.060)	(0.044)	(0.038)
Number of observations	1,135	2,085	3,068	3,973
	Unit sales of green vehicles - not covered			
	4 weeks	8 weeks	12 weeks	16 weeks
Incentive change dummy	0.228	0.097	-0.060	-0.019
	(0.523)	(0.109)	(0.065)	(0.081)
Number of observations	133	236	306	372
	Unit sales of gasoline vehicles			
	4 weeks	8 weeks	12 weeks	16 weeks
Incentive change dummy	-0.001	0.007	0.002	-0.008
	(0.011)	(0.014)	(0.001)	(0.008)
Number of observations	33,113	60,604	89,493	116,058

Notes: + $p<0.1$; * $p<0.05$; ** $p<0.01$; *** $p<0.001$. All estimations are based on the local linear approach (with separate trends pre- and post-incentive changes) using a rectangular kernel. Clustered standard errors (county) are reported in parentheses. Our findings remain robust when we use a second order polynomial (not reported due to space constraints).

Online Appendix H: Additional Discussion on an Alternative Explanation for the HOV Incentive Termination Effect

The logic behind the "pending availability of an HOV incentive for plug-in hybrids" explanation implies that the closer consumers get to the actual launch of the future HOV incentive, the higher the value from waiting because they will incur lower costs of waiting as they need to wait for fewer periods. Therefore, if this explanation played a major role in the sales decline for the hybrid vehicles around the HOV incentive termination, one would expect that the sales decline will be more pronounced as one gets closer to the launch of the future HOV incentive (i.e., September 2012). However, as shown in Figure 1, the negative effect of the HOV termination does not become more negative over time.

Furthermore, existing plug-in hybrid and electric vehicles at the time of the HOV incentive termination are also potential substitutes for future greener models with HOV lane access. As such, if consumers decided to wait for greener future plug-in models to come out to get a decal following the HOV termination for conventional hybrids, one would also expect to see a significant sales decline for existing plug-in hybrid and electric vehicles around the HOV incentive termination. However, as shown in Column (2) of Table 2, we do not find a significant sales effect of the HOV incentive termination on those vehicles. Additionally, it is important to note that, in Utah, there were no plans to offer the HOV incentive for other types of green cars following the termination of the HOV incentive for conventional hybrid cars. As discussed in Online Appendix E, we find a negative sales effect of the HOV incentive termination, albeit marginally significant, even in Utah, where there was no pending HOV incentive. Combined, the aforementioned analyses and discussions suggest that the future availability of an HOV incentive for newer plug-in hybrids might not be playing a primary role in the significant and sharp decline in sales for conventional hybrids around the HOV incentive terminations.

Online Appendix I: HOV Incentive Launch: Additional Analyses

Table I.1: The Impact of HOV Incentive Launch - Robustness to Various Specifications

	Unit sales of green vehicles covered by HOV incentive launch			
HOV incentive launch	0.181	0.391	0.361	0.016
	(0.268)	(0.264)	(0.215)	(0.183)
Price		-0.061***	-0.047***	-0.010***
		(0.003)	(0.004)	(0.004)
Stratum × vehicle model dummies	No	No	Yes	Yes
Vehicle model × month dummies	No	No	No	Yes
Control variables (see the notes below)	No	Yes	Yes	Yes
County dummies	Yes	Yes	Yes	Yes
Number of observations	25,932	25,860	25,860	25,860
Log likelihood	-34,434	-33,969	-29,780	-28,861

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table I.2: The Impact of HOV Incentive Launch - Robustness to Other Functional Forms

	Unit sales of green vehicles covered by HOV incentive		Unit sales of gasoline vehicles	
	(1) Log-linear regression model	(2) Poisson model	(3) Log-linear regression model	(4) Poisson model
HOV incentive launch	0.042	0.008	-0.020	0.017
	(0.078)	(0.153)	(0.013)	(0.011)
Price	-0.001	-0.010**	-0.002*	-0.016***
	(0.001)	(0.004)	(0.001)	(0.001)
Stratum × vehicle model dummies	Yes	Yes	Yes	Yes
County dummies	Yes	Yes	Yes	Yes
Vehicle model × month dummies	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Number of observations	25,860	25,860	62,367	62,367
R square	0.703		0.640	
Log likelihood		-29,654		-116,387

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. The dependent variable for the log-linear regression models is log(unit sales+1). The Poisson models use the log link. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table I.3: The Impact of HOV Incentive Launch in Different States

	Unit sales of green vehicles covered by HOV incentive		Unit sales of gasoline vehicles	
	California	Utah	California	Utah
HOV incentive launch	-0.049	0.028	0.022	-0.094
	(0.104)	(0.226)	(0.023)	(0.052)
Price	-0.069*	-0.002	-0.004*	-0.002
	(0.027)	(0.006)	(0.002)	(0.003)
Stratum × vehicle model dummies	Yes	Yes	Yes	Yes
County dummies	Yes	Yes	Yes	Yes
Vehicle model × month dummies	Yes	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes	Yes
Number of observations	324	25,536	50,449	11,918
Log likelihood	-629	-28,222	-89,495	-12,754

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table shows the estimation results of the impact of HOV incentive launch on green vehicles covered by the incentive change for California and Utah, separately. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table I.4: The Impact of HOV Incentive Launch - Robustness to Differential Trends Across Strata

	(1) Unit sales of green vehicles covered by HOV incentive	(2) Unit sales of gasoline vehicles
HOV incentive launch	-0.003	0.017
	(0.091)	(0.022)
Price	-0.024***	-0.004**
	(0.007)	(0.002)
Stratum × vehicle model dummies	Yes	Yes
Stratum dummies × month	Yes	Yes
Vehicle model × month dummies	Yes	Yes
Control variables (see the notes below)	Yes	Yes
Number of observations	25,860	62,367
Log likelihood	-28,845	-101,152

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table shows the robustness of the impact of HOV incentive launch to differential linear trends across strata. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price, county-month-level number of charging stations, and county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Online Appendix J: Additional Discussion on an Alternative Explanation for the HOV Incentive Launch Effect

We also consider an alternative explanation for the insignificant effect of the HOV incentive launch on conventional hybrid sales. According to this explanation, the "greenness" signal provided by conventional hybrid vehicles could be weakened by the pending availability of greener electric vehicle models, resulting in fewer sales for conventional hybrids following the HOV incentive launch. To empirically assess whether the above mechanism could explain the insignificant effect of the HOV incentive launch on conventional hybrid sales, we have examined this effect only for California.

Specifically, we aim to exploit the fact that the HOV incentive was launched in California in July 2005 and the first mass-produced plug-in hybrids (e.g., Chevy Volt) and all-electric vehicles (e.g., Nissan Leaf) did not become available until December 2010. In other words, given the long period between the launch of the HOV incentive in California and the availability of more green electric vehicles, it is reasonable to think that the above explanation should be largely inoperative around the HOV incentive launch in California.

Recall that our explanation for the insignificant effect of the HOV incentive launch on hybrid sales is that an increase in functional value after the incentive launch is canceled out by a reduction in symbolic value due to the incentive launch. In contrast, the above alternative explanation suggests that the increase in functional value after the incentive launch could have been counterbalanced by pending greener vehicles, rather than the incentive launch itself. If this alternative explanation were a major driver of our insignificant sales effect finding, it would be more likely to observe a more positive (and maybe even significant) effect of the HOV incentive launch in California, where the above mechanism is expected to be largely absent. In contrast, as shown in Table I.3 in Online Appendix I, the coefficient for the HOV incentive launch becomes more negative (-.049) and remains insignificant ($p > .10$) when we only examine California. Therefore, we do not find empirical support for the explanation that the "greenness" signal provided by conventional hybrid vehicles could be weakened by the pending availability of greener electric vehicle models.

Also, if the above explanation were at work, one would expect to see a more pronounced decline in the sales of conventional hybrids over time, i.e., as one gets closer to the launch of greener electric vehicle models. However, as shown in Column (2) in Table 4, the effect of the launch remains insignificant when we extend the post-launch window from 6-months to 12-months. This result is robust to an even wider 18-month post-launch window.

Our additional analyses (not reported due to space constraints) suggest that the pending availability of greener electric models does not appear to influence our termination-related findings either. These analyses are available from the authors upon request.

Essay II

Heterogeneity in the Effectiveness of Governments' Monetary Incentives for Green Products

Abstract

Monetary incentives such as a tax-credit incentive have been commonly adopted by governements to encourage the adoption of green technology. However the conclusion regarding the effectiveness of monetary incentives has not reached consensus. On one hand, monetary incentives such as a tax credit could increase unit sales of green vehicles by making them more attractive through cost saving. On the other hand, these incentives may prove ineffective due to important barriers for adoption such as the need for drastic changes in behavior (e.g., long charging times). To identify the effect of governments' tax credit incentives on unit sales of green and non-green vehicles, we exploit incentive changes in 46 counties and use several quasi-experimental methods. We find that unit sales of vehicles covered by the tax-credit incentive increase by 25.1% after the launch of the tax-credit program. Our results also show that unit sales of vehicles covered by the tax-credit incentive remain unchanged after its termination.We also provide suggestive evidence that the tax credit incentive is effective through the cost-saving mechanism. Specifically, the tax-credit incentive is more effective in counties where consumers value cost saving more (i.e., counties with lower income), and it induces substitution from non-green vehicles with higher fuel efficiency.

Keywords: sustainability, green products, public policy, government incentives, policy evaluation, quasi-experiments, difference-in-differences, coarsened exact matching

2.1 Introduction

Growing environmental concerns such as global warming have recently led to an increased number of sustainability initiatives across the world (e.g., Jenn et al. 2013). Green products, which use green technologies to mitigate or reverse the effects of human activity on the environment, have been developed by manufacturers in various industries. In automobile industry, for example, green vehicles such as hybrid vehicles and electric vehicles become increasingly popular as they offer solutions to reduce carbon emission and local air pollution. Many consumers express interest for green products which become more available (Chen and Chang 2012; Chen et al. 2014), however, green products represent a small fraction of global demand (Chabowski et al. 2011). As a result, various incentive programs such as tax credits, rebates, or special exemptions have benn adopted by governments to promote green goods and services. However, the effectivenss of these incentives on consumers' adoption of green products remains controversial.

In this paper, we empirically examine the effect of tax-credit incentive on consumers' green and non-green product purchases in the U.S. automobile industry between 2010 and 2013. Tax-credit incentive is one of the most common types of state-level government monetary incentives. Unlike rebate and sale tax exemption, the benefit offered by tax-credit incentives is not immediate. Consumers cannot attain the monetary benefit until the tax filing in the next year, which makes the effectiveness of tax-credit incentive controversial. On one hand, tax-credit incentive could increase unit sales of green (i.e., hybrid, plug-in hybrid, and electric vehicles) by making them more attractive relative to non-green vehicles through enhanced benefits like cost saving from the tax return.[45] On the other hand, tax-credit incentive may prove ineffective due to important barriers for adoption. These barriers include lack of awareness or knowledge by potential adopters, need for drastic changes in behavior (e.g., long charging times), lack of infrastructure (e.g., charging stations), low consumer risk tolerance, and significant price premiums for green vehicles even after taking into

[45]There are two types of hybrid vehicles in the automobile market in our study period: conventional hybrid and plug-in hybrid. Both types of vehicles have an electric motor and a rechargeable battery. While conventional hybrid vehicles can only be fueled by gasoline, plug-in hybrid vehicles can be plugged in and recharged from an outlet. In the remainder of the paper, we use "hybrid vehicle" to refer to conventional hybrid vehicles, and "plug-in hybrid vehicle" to refer to plug-in hybrid vehicles.

account government incentives (Jaffe and Stavins 1994; Stoneman and Diederen 1994; Diamond 2009; Sierzchula et al. 2014).[46] Therefore, it is not clear whether tax-credit incentive provided by governments can be effective. In addition, as different consumers potentially value various types of benefits differently, it is important to know the conditions under which this incentive work best.

Despite the recent calls for more marketing studies on sustainability (Sudhir 2016) and public policy (Stewart 2015), there is very few research on the effectiveness of government incentives in promoting green-product (or technology) adoption in the marketing literature. There is a related stream of research in the energy policy literature that examines the effects of government incentives on green-vehicle adoption (see Jenn et al. 2018). Previous literature find that financial incentives in general play an important and significant role on boosting the sales of green vehicles. For example, Sierzchula et al. (2014) applied ordinary least squares (OLS) regression analysis on national data taken from 30 countries for the year 2012. Their analysis find financial incentives to be significantly correlated with PEV sales shares along with other factors such as the presence of local production facilities, and the per capital number of charging stations. Slowik and Lutsey (2017) identified drivers of the US BEV and PHEV market. They analyzed data from 200 US metropolitan areas for 2016. Their outcomes indicated that financial incentives are one of the most important factors. However these studies do not distinguish between tax-credit incentive and other monetary incentives with immediate benefits such as rebate and sale tax exemption. The studies from Chandra et al. (2010), Gallagher and Muehlegger (2011), and Jenn et al. (2018) suggest that rebate and sale tax exemption have a positive association with adoption of green vehicles. However, the findings regarding the effectiveness of tax-credit incentive are inconclusive. In one of the pioneering studies, Diamond (2009) documents an insignificant relationship between monetary incentives and hybrid car market share based on a state-year-level analysis during the introduction period of hybrid cars into the automobile market (i.e., between 2001 and 2006). Hardman et al. (2017) conduct choice experiment and post-purchase survey, and their results show that tax-credit incentives have lower affectivity and they are the least effective incentives in changing the pur-

[46]For instance, while the most expensive Ford Focus is priced at $22,700, its electric counterpart lists for $39,995 (https://www.cbsnews.com/news/hybrid-and-electric-cars-losing-government-support/).

chase decisions. Münzel et al. (2019) find that income tax credit has a negative correlation with EV adoption by applying panel data regression on plug-in electric vehicle sales from 32 European countries from 2010 to 2017. In contrast, Gallagher and Muehlegger (2011) show that monetary incentives in the form of state-level income tax credits are positively associated with hybrid car sales. Although their studies also suggest that income tax credits have less impact on hybrid car sales than rebate and sale tax exemption. Jenn et al. (2013) complement these findings by showing a positive relationship between federal tax-credit incentives and the sales of hybrid vehicles using national sales data at the vehicle model level between 2000 and 2010. Using a lagged-dependent model specification at the vehicle model-state-month level to address the simultaneity concern, Jenn et al. (2018) show that tax credit increases average sales of electric vehicles.

Although these studies provide important insights to understand the effectiveness of monetary green-vehicle incentives, there is a lack of empirical evidence on the causal effect of such incentives on unit sales of green and non-green vehicles. First, these papers only examine the effect of green-vehicle incentives on the demand for green vehicles covered by the incentives. Therefore, little is known about the effects on vehicles that are not covered (e.g., gasoline vehicles), which have important implications regarding the effectiveness of green-vehicle incentives in terms of total carbon emission levels. Second, papers in this literature do not investigate the underlying mechanisms for the incentive effectiveness. Understanding these mechanisms is important in determining when and where various green-vehicle incentives are most appropriate and help explain the inconsistent results in previous literature. Third, the studies above that exploit temporal variation based on national- or state-level aggregate data, which does not allow for controlling local-market (e.g., county) characteristics that might affect consumer demand for green vehicles. As a result, previous studies have called for more in-depth studies to take into account and understand more local factors than state-level data (Jenn et al. 2018). Fourth, previous studies concentrate only on the launch of tax-credit incentive. That is, they do not study the effect of termination of tax-credit incentive, which may not simply be the opposite of the effect of launch. Therefore, they cannot guide policymakers and managers regarding the long-term "net effect" of the tax-credit incentive.

This study aims to contribute to the literature on the effectiveness of governments' green-product incentives in four ways. First, we examine the causal impact of tax-credit incentive on unit sales of all vehicles, including those that are covered by such incentives and those that are not. Second, we analyze the demographic (e.g., high vs. low income counties) and product (e.g., high vs. low performance vehicles) heterogeneity in the effects of tax-credit incentive to provide suggestive evidence regarding the underlying mechanisms. Third, our quasi-experimental analyses at the county level allow for 1) a stronger case for causal inference, and 2) more detailed insights related to the local heterogeneity in incentive effectiveness than previous studies based on national- or state-level data. Fourth, this study complements previous studies by examining a relatively recent period in the life cycle of green vehicles and documenting the effect of terminating besides launching the tax-credit incentive.

Motivated by the aforementioned goals, we exploit a unique data set that involves county-level vehicle sales around the events associated with green-vehicle incentive changes: South Carolina's launch of the tax-credit incentive for plug-in hybrid vehicles in January 2012, and Oregon's termination of tax-credit incentive for plug-in hybrid vehicles in December 2011. The main empirical challenge in measuring the effect of these incentives on vehicle sales is that the assignment of "treatment" (i.e., incentive change) is a strategic decision by state governments, and thus potentially not random. As such, a simple comparison of unit sales between counties in states "treated" by the incentive change and those in "non-treated" states may be misleading, if there are persistent differences (e.g., preference for green vehicles) across counties in different states.

We utilize several quasi-experimental methods using granular analyses at the county-month level to address this endogenous incentive-selection issue. First, our primary identification strategy is to use the "difference-in-differences" approach with Coarsened Exact Matching (CEM) (Iacus et al. 2009) to ensure that treated and non-treated counties are comparable in terms of pre-treatment sales trends and several important demographics related to green-vehicle purchases (e.g., income, and political inclination) documented in the literature (e.g., Potoglou and Kanaroglou 2007; Ozaki and Sevastyanova 2011). In addition, the panel nature of our data allows us to control for time-invariant differences across counties via county fixed effects and time-varying differences across vehicle models via model-month fixed effects. Second, we also use a "border strategy" by leveraging the variation in green-vehicle incentives around state borders (e.g., Shapiro 2018). To the extent that neighboring counties in the same market but in different states are similar in terms of unobserved demographic variables, this strategy complements our primary strategy that relies on matching based on observable demographic variables. Third, we provide a regression-discontinuity-in-time (RDiT)-style analysis (Hausman and Rapson 2017). Assuming that there are no concomitant unobservables influencing unit sales that discontinuously change at the incentive change period, this approach uses a vehicle model's own unit sales in a given county just before an incentive change as the counterfactual for those sales just after that incentive change.

We find that unit sales of vehicles covered by the tax-credit incentive (i.e., plug-in hybrids) increase by 25.1% after the tax-credit incentive launch. However, green vehicles that are not covered by the tax credit incentive (e.g., electric vehicles) and non-green (i.e., gasoline) vehicles do not experience a change in unit sales, on average.[47] These findings suggest that, at least for some consumers, the benefits provided by tax-credit incentive dominate the factors that hinder green-vehicle sales. Additionally, we document the dynamic effect of the tax credit incentive on green-vehicle sales. In particular, the tax-credit incentive effect becomes significant three months after the start of the incentive program with a peak in the fourth month, and its effect fades over the medium term (i.e., six months after the incentive launch). Our results also suggest that unit sales of vehicles covered by tax-credit incentive remain unchanged after the termination of tax-credit incentive. Combined with the results of tax-credit launch, it indicates that tax-credit incentive has a long-term net effect. These results are robust to an extensive set of robustness checks, including 1) different functional forms, 2) falsification exercises, 3) differential trends across groups based on matching, and 4) alternative identification strategies.

Next, we investigate several potential mechanisms that may explain the effectiveness of governments' tax-credit incentive for green vehicles. Specifically, we examine whether tax-credit in-

[47]Note that this insignificant average effect does not necessarily mean that there is no substitution between vehicles that are covered by an incentive change and those that are not. We explore this issue in our subsequent analyses.

centive are effective through the cost-saving mechanism. First, we find that the tax-credit incentive has a more positive impact on unit sales of vehicles covered by the incentive in counties where consumers value cost saving more (i.e., counties with lower income). Second, the tax-credit incentive leads to demand substitution from non-green vehicles with higher fuel efficiency. We also consider several alternative explanations based on potential interactions of the incentive changes with education level, political inclination (i.e., percentage of votes for the Democratic Party), gender, age, and unemployment rate as well as vehicle price, category, and brand.

2.2 Institutional Background

In the past few decades, policymakers have introduced various incentive programs to boost the adoption of green products in an effort to alleviate environmental concerns and dependence on foreign oil. Such incentives have been provided in the context of the automobile industry to induce consumers to buy hybrid, plug-in hybrid, and electric vehicles rather than gasoline vehicles. The primary benefit of monetary incentives is to reduce costs for adopting a green vehicle for consumers. In this paper, we study the effectiveness of one of the monetary green-vehicle incentives most commonly employed by state governments: tax credit.

2.2.1 Tax-Credit Incentive Launch in South Carolina

Effective in January 2012, tax payers in South Carolina were allowed a tax credit against income tax for the in-state purchases of new plug-in vehicles (South Carolina law of code 12-6-3376). The credit is equal to six hundred and sixty seven dollars, plus one hundred eleven dollars if the vehicle has at least five-kilowatt hours of battery capacity, plus an additional one hundred eleven dollars for each kilowatt hour of battery capacity in excess of five-kilowatt hours. The maximum credit allowed by this incentive was two thousand dollars. Only the plug-in hybrid vehicles that were purchased after January 2012 were eligible for this program. This program was terminated in December 2017.

2.2.2 Tax-Credit Incentive Termination in Oregon

Effective in December 2009, the State of Oregon's tax credit program became available to vehicles that run on electricity or natural gas as well as gasoline-electric hybrids that are designed for electric plug-in charging (i.e. plug-in hybrid). Individuals who purchase qualified vehicles are able to receive a $1500 tax credit. This program was terminated on December 31, 2011.

With these important green-vehicle incentive changes as our backdrop, we examine how the tax-credit incentive launch and termination affect unit sales of green vehicles covered by the incentive, green vehicles that are not covered by the incentive, and non-green vehicles.

2.3 Data

In this study, we employ data from four sources to evaluate the impact of tax-credit incentive change in South Carolina and Oregon on unit sales. Our primary data set contains information on business-to-consumer new vehicle transactions collected by a major market research firm. We collect data on every transaction that occurs in a random sample of 15-20% of the census of new car dealerships in the U.S. for the 12-month symmetric window around, i.e., six months before and after, each incentive change. For each transaction, the data include the price and detailed characteristics of the vehicle. These characteristics include the make, model, model year, body type, number of doors, number of cylinders, and engine displacement. In addition, we observe the customer ZIP code associated with each transaction. We aggregate these transaction data to the vehicle model-county-month level for our subsequent analyses. The list of green vehicles available during our observation period are provided in Table A.1 in the Online Appendix A. Combined, these green vehicles accounted for about 2.2% of total new automobile sales.

In analyzing the sales effects of green-vehicle incentives, we need to use a clear market definition that allows us to account for market-level shocks. To do so, we follow an approach used in previous studies that examine auto purchases (Mian and Sufi 2012), and we use Core-based Statistics Areas (CBSAs) to define markets in our data. Defined by the Office of Management and Budget, CBSA is a U.S. geographic area that consists of one or more counties (or equivalents) anchored by an urban center of at least 10,000 people plus adjacent counties that are socioeconomically tied to the urban center by commuting. Additionally, our analyses concentrate on geographically isolated markets to be able to identify potential customers in a given market. More precisely, we employ a similar set of criteria to those used by Olivares and Cachon (2009) to determine isolated markets.[48] This leaves us a sample of 275 isolated CBSAs with 1167 counties, which accounts for around 80% of total unit sales in our original transaction data.

Our second source of data consists of the American Community Survey[49], which provides annual county-level information on demographic variables such as population, educational attainment, and income as well as commuting level. We exploit these variables to account for the potentially heterogeneous impact of HOV and tax-credit incentives. Specifically, we examine heterogeneity in terms of median household income, the percentage of population with bachelor's degree or higher, the percentage of male population, the median age of household head, and mean travel time to work (in minutes). The third source of data contains the election statistics of the 2012 presidential election from Harvard Kennedy School.[50] We use the county-level percentage of votes for the Democratic Party as a proxy for the support of green technologies, given the Democratic Party's objective of becoming a clean energy superpower.[51]

Table I.5 provides descriptive statistics for key variables used in our estimations. It also shows the raw "difference-in-differences" in terms of percentage changes in unit sales for green vehicles covered by the tax credit incentive, green vehicles that are not covered, and non-green (i.e., gasoline) vehicles for counties that are affected by the incentive change (i.e., treatment group) and those that are not (i.e., control group).[52] These raw data patterns show that after the tax-credit incentive was launched, unit sales of green vehicles covered by the incentive increased more in treated counties relative to control counties. On the other hand, the differences in percentage unit-sales changes

[48]The specific criteria are shown in Table B.1 in Online Appendix B.
[49]https://www.census.gov/programs-surveys/acs.
[50]https://guides.library.harvard.edu/hks/campaigns_elections.
[51]https://www.democrats.org/issues/environment.
[52]The details of these two groups are provided later in subsection 2.4.1.

between the treatment and control groups are much smaller for the vehicles that are not covered by the incentives. We use econometric analyses to formalize these insights in the next section.

2.4 Effect of Governments' Monetary Incentives on Consumers' Green-Vehicle Adoption

In this section, we assess the main effect of the tax-credit incentive launch on unit sales of green vehicles covered by each incentive, green vehicles that are not covered, and non-green (i.e., gasoline) vehicles. The key empirical challenge in identifying the causal effect of governments' green-vehicle incentives is that the termination or introduction of policies may not be exogenous. In particular, the policymakers' tax credit incentive decisions may be influenced by state-specific factors such as residents' preferences for green products and the demographic composition of each state. Thus, a simple comparison of unit sales between counties in states with incentive change and those in unaffected states may be misleading. To address this identification challenge, we employ several identification strategies that rely on different identifying assumptions.

2.4.1 Empirical Strategy: Difference-in-Differences with Coarsened Exact Matching

Since the availability of tax-credit incentive changes over time and it applies only to a subset of vehicles (i.e., plug-in hybrid cars) in a subset of counties (i.e., those in South Carolina and Oregon), we can employ a difference-in-differences identification strategy to estimate the main effect of the tax credit incentive change on unit sales. More precisely, we estimate the effect of the tax-credit incentive change on unit sales by comparing changes in unit sales before and after the tax credit incentive is launched in a given county (i.e., treatment group) with a baseline of changes in unit sales in counties with no incentive change (i.e., control group) in the same time period. In other words, when estimating the average effect of the tax-credit launch, the treatment group consists of counties where the tax-credit incentive changes (i.e., counties in South Carolina and Oregon); and the control group involves counties without the tax-credit incentive during our analysis period.[53]

The main identifying assumption of the difference-in-differences approach discussed above is that there are no unobserved, time-varying, county-specific variables that are correlated with both the incentive change and unit sales. To alleviate the potential concerns related to this assumption, we primarily rely on two supplemental approaches. First, we show that accounting for 1) important control variables (e.g., price), 2) unobserved county-specific time-invariant factors (e.g., preference for green products), 3) unobserved strata-specific time trends (e.g., changes in local economic conditions), and 4) unobserved time-varying factors specific for a given vehicle model but common across all counties (e.g., national advertising) do not change the estimated effect substantially. Therefore, the effect of any remaining unobservables would need to be relatively large compared to the factors we account for to result in a significant change in our qualitative findings.

Second, to further lessen the endogeneity concern, we combine the difference-in-differences with matching (see Singh and Agrawal 2011 and Zervas et al. 2017 for a similar approach). Intuitively, the goal of the matching procedure is to generate more similar treated and control counties based on a set of observables in an attempt to reduce the potential for unobservable differences between the two groups. Specifically, we apply the Coarsened Exact Matching (CEM) method in our main analyses (Iacus et al. 2012). To do so, we stratify counties based on observable characteristics related to the preference for green vehicles. Previous studies document several characteristics that distinguish U.S. consumers who have high preference for green vehicles—green consumers—from those who do not (e.g., Potoglou and Kanaroglou 2007; Ozaki and Sevastyanova 2011). In particular, green consumers are more likely to have higher income and education levels. They are also more likely to be older females and to vote for the Democratic Party. Drawing upon these studies, we use income, education, gender, age, and political inclination to generate strata. Only 2 out of 46 counties could not be matched for the tax-credit launch analysis in South Carolina and 6 out of 36 counties could not be matched for the tax-credit termination analysis in Oregon.[54]

[53] The control states include Alabama, Arkansas, Connecticut, Delaware, Idaho, Indiana, Iowa, Kansas, Kentucky, Michigan, Minnesota, Missouri, Montana, Nebraska, Nevada, New Hampshire, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, South Dakota, Vermont, West Virginia, Wisconsin, and Wyoming.

[54] Our results remain similar when we use difference-in-differences using all counties (i.e., without dropping coun-

Our matching procedure results in 30 strata for the tax-credit launch analysis and 16 strata for the tax-credit termination analysis. We conduct t-tests to see whether counties in treatment and control groups are comparable in terms of the matching characteristics. Specifically, we perform a stratified t-test of the difference in the average of a given matching variable (e.g., income) between the two groups. None of the t-tests for all matching variables rejected the null hypothesis that the two groups have the same average at the 5% significance level. The results of these t-tests suggest that the counties in treatment and control groups are largely comparable in terms of key observables. Therefore, the variation in the treatment status (i.e., incentive change) across counties within the same stratum will allow us to identify the average effect of the tax-credit incentive launch.

2.4.2 The Effect of Tax-Credit Incentive Launch on Unit Sales

In this subsection, we estimate the average effect of tax-credit incentive launch on unit sales of green vehicles covered by the incentive (i.e., plug-in-hybrid vehicles), green vehicles that are not covered (i.e., hybrid and electric vehicles), and non-green vehicles (i.e., gasoline vehicles).[55] Our unit of analysis is county (i) - vehicle model (j) - month (t), e.g., New York county - Toyota Prius - August 2011. We identify the effect of interest by using the following log-linear model:

$$\begin{aligned} ln(Unit\,Sales_{ijt}) &= \beta_{Tax\,credit\,launch} Tax\,Credit\,launch_{it} \\ &\quad + \alpha_i + \delta_{s(i)j} + \lambda_{jt} + X'_{ijt}\gamma + \varepsilon_{ijt}. \end{aligned} \tag{G.6}$$

$Tax\,Credit\,launch_{it}$ is the dummy variable that is equal to 1 after the launch of the tax-credit incentive if a county belongs to a state where there is a tax-credit incentive change, and 0 otherwise. α_i's are the county fixed effects, which capture time-invariant unobserved factors of each county

ties due to matching), as shown in Table C.1 in Online Appendix C.

[55] Our conclusions do not change when we combine data across the three vehicle types and estimate a specification where we interact the incentive change with vehicle type.

such as the location of the market. $s(i)$ is the stratum s to which county i belongs, which is determined by the CEM algorithm as explained in subsection 2.4.1. Accordingly, $\delta_{s(i)j}$ is stratum-vehicle fixed effect, which captures the baseline demand of a given vehicle model j for a given stratum. λ_{jt}'s are vehicle model-month dummies, which allow us to control for vehicle-month-specific unobservables such as manufacturer-level advertising as well as promotions to dealers and consumers (see Ozturk et al. 2016 for a similar approach). Note that as λ_{jt} subsumes vehicle model fixed effects, it also accounts for vehicle model-specific factors that do not vary over time such as manufacturer (e.g., GM), brand (Chevrolet), and nameplate (e.g., Volt). Similarly, λ_{jt} also subsumes month fixed effects; as a result, it also takes into account time-varying factors that might impact demand common to all vehicle models and counties such as consumer confidence in the economy (e.g., Ozturk et al. 2019). X_{ijt} is a vector of county-, vehicle model-, and/or month-varying control variables. It includes the following county-level demographic variables, including income, education, age, gender, unemployment rate, and mean travel time to work. X_{ijt} also involves the average transaction price of a vehicle model j in county i in month t, monthly average gas price in each state, and the number of charging stations at the county-month level.

The coefficient of interest in Equation G.6 is $\beta_{Tax\,credit\,launch}$, which we interpret as the average effect of the tax-credit incentive launch on unit sales. As we conduct separate analyses for green vehicles covered by the incentive, green vehicles that are not covered, and non-green vehicles, we obtain three different $\beta_{Tax\,credit\,launch}$s (one for each vehicle type). For instance, when we use $ln(Unit\,Sales_{ijt})$ for green vehicles covered by the incentive as our dependent variable, if $\beta_{Tax\,credit\,launch}$ is greater than zero, we interpret it as indicating that the average unit sales for green vehicles covered by the incentive increases by 100 $\times$ [exp($\beta_{Tax\,credit\,launch}$)-1]% after the tax credit incentive is launched. Similarly, when we use $ln(Unit\,Sales_{ijt})$ for non-green vehicles as our dependent variable, if $\beta_{Tax\,credit\,launch}$ is smaller than zero, we interpret it as indicating that the average unit sales for non-green vehicles decreases by 100 $\times$ [1-exp($\beta_{Tax\,credit\,launch}$)]% after the tax credit incentive launch.

Identification Check Before we proceed to the estimation of the average effect of the tax credit incentive launch on unit sales, we perform an identification check to examine whether our empirical strategy can recover the causal sales effect of the incentive change. Specifically, we estimate Equation G.6 with a slight modification by splitting our main independent variable ($TaxCredit\,launch_{it}$) into a sequence of dummy variables for the months before and after the treatment (see Wang and Goldfarb 2018 for a similar approach). The base month is 6 months before the implementation of the program.

Figure I.1 graphs the coefficients associated with these monthly dummy variables for green vehicles covered by tax-credit incentive launch. The solid line presents the estimated coefficient for each month, and the bars show the 95% confidence interval for each coefficient. The base month is 6 months before the implementation of the tax-credit program. Figure I.2 shows the coefficients associated with these monthly dummy variables for green vehicles not covered by the tax-credit incentive and gasoline vehicles. The figures show that the estimated coefficients for all three vehicle types are insignificant prior to the tax-credit launch. The pre-trends in unit sales are similar between the treatment and control groups after controlling for the covariates and fixed effects in Equation G.6, thus providing support for our identification strategy.

Main Effect The columns in Table I.6 show the estimated effect of the launch of tax-credit incentive on unit sales of green vehicles covered by the incentive, green vehicles that are not covered, and non-green vehicles using Equation G.6. The estimates in Column (1) indicate that the average effect of the tax-credit program on unit sales of the green vehicles covered by the incentive is positive ($\beta^{cov\,green}_{TaxCredit\,launch} = 0.224$) and statistically significant ($p < .05$).[56] In other words, unit sales of green vehicles covered by the incentive increase by 25.1% [100×{exp(0.224)-1}] in response to the launch of the tax-credit incentive. In contrast, as reported in Column (2) and (3), we do not find a statistically significant effect on unit sales of green vehicles that are not

[56]We provide various specifications with different subsets of fixed effects as well as with and without controls in Table **??** in Online Appendix D. The consistently positive and significant coefficients for the tax-credit launch dummy indicate that the effect of any remaining unobservables would need to be relatively large compared to the factors we account for to result in a significant change in our qualitative results (Altonji et al. 2005).

covered by the incentive ($\beta^{non\,cov\,green}_{Tax\,Credit\,launch}$ =.033, p = .20) and non-green vehicles ($\beta^{non\,green}_{Tax\,Credit\,launch}$ = .004, p = .48). These estimates suggest that while the tax-credit incentive has a positive impact on the adoption of green vehicles covered by the incentive, there is no evidence of a demand spillover to the vehicles that are not covered by the incentive program, on average.

2.4.3 Time-Varying Effects

We also investigate whether the effect of the tax-credit launch is persistent over time. Recall that we created month-specific treatment coefficients in our earlier discussions of identification checks. More precisely, we used Figure I.1 to compare pre-treatment trends between the treatment and control groups. We can examine the months after the tax credit incentive launch in this figure to see whether the effects vary over time. The figure shows that the positive effect of the incentive change on unit sales of green vehicles covered by the incentive becomes significant three months after the implementation. This pattern might be partially explained by the potential unawareness of the consumers about the incentive change in the initial months. The positive effect is highest during the fourth month after the tax-credit launch, and then it tapers off. This is potentially because this month coincides with the period when consumers do their taxes. As such, tax-related benefits and associated decisions are potentially more salient for consumers. This result suggests that government should increase consumers' awareness of green-product incentives in order to improve their effectiveness (Jenn et al. 2018).

2.4.4 Robustness Checks

To further strengthen the causal interpretation of our previous findings, as suggested in Goldfarb and Tucker (2014), we investigate a set of robustness checks including 1) a falsification exercise, and 2) differential trends across strata. The results are shown in Online Appendix D, which are consistent with the findings of primary method.

2.4.5 Alternative Identification Strategies

We also conduct analyses using other identification strategies, i.e., the border and the RDiT strategies, that complement our main empirical strategy based on difference-in-differences with CEM.

Border Strategy The difference-in-differences approach uses the unit sales in counties that are not affected by the tax credit incentive launch as counterfactual unit sales for counties that are treated by that incentive change. Although we use CEM to ensure that treatment and control groups are comparable with respect to a set of observable characteristics, there may still be differences between the two groups in terms of unobservable characteristics. To allay this worry, we employ the "border strategy" by leveraging the variation in green-vehicle incentives around state borders (e.g., Shapiro 2018). In this strategy, counties in a state without a given incentive will serve as controls for counties within the same market (i.e., CBSA) but on the other side of the border in another state where that incentive exists. As such, we will attribute the unit sales difference for a given vehicle type between neighboring counties in different treatment conditions (i.e., existence vs. absence of the tax-credit incentive) to the tax credit incentive launch. To the degree that neighboring counties in the same market but in different states are comparable in terms of unobserved characteristics, this border strategy supplements our primary empirical strategy that relies on matching based on observable demographic variables. The details of our analyses based on the border strategy are provided in Online Appendix E.

The estimation results based on the border strategy are provided in Table I.7. As shown in Table I.7, the tax-credit incentive has a positive (.166) and significant ($p < .05$) effect on the unit sales for the vehicles covered by the incentive.[57] However, it does not significantly affect unit sales of other vehicles. In sum, these estimates demonstrate that our conclusions are robust to the border strategy for identification.

[57] The magnitudes differences between the two identification strategies for the tax-credit incentive could be due to the fact that the estimated effect based on the border strategy is local. In other words, they are identified at the border and not elsewhere.

Regression-Discontinuity-in-Time (RDiT)-Style Analysis In our estimations so far, we have used a vehicle model's unit sales in a county without a given incentive change as the counterfactual for unit sales of the same vehicle model in a similar county that is affected by that incentive change. An alternative way to generate counterfactual unit sales is to use a vehicle model's own unit sales just before an incentive change as the counterfactual for those just after that incentive change. The key identifying assumption of this approach is that there are no concomitant unobservables influencing unit sales that discontinuously change at the start of the incentive change. Therefore, it is crucial to concentrate on a narrow window around the incentive change. As we implement this identification strategy as a complementary approach to our primary empirical strategy, we do not provide a complete set of analyses required by RDiT (see Hausman and Rapson 2017). As such, we call the associated analysis an RDiT-style analysis.

Specifically, we rely on a local linear strategy where we use various relatively narrow time windows—i.e., 4 weeks, 8 weeks, 12 weeks and 16 weeks before and after the incentive launch—to examine the effects of tax-credit incentive launch on unit sales. The details of our analyses based on the RDiT strategy are provided in Online Appendix F. Table I.8 shows the local linear estimation results for the impact of the tax-credit launch. The estimates suggest that that the positive impact of the tax-credit incentive launch becomes significant about three months after the tax-credit incentive is implemented. This is consistent with our earlier findings discussed in subsection 2.4.3. As such, our conclusions are robust to the RDiT identification strategy.

2.5 Mechanism

Our results so far show that the adoption of green vehicles covered by the incentive is positively influenced by the tax-credit launch. Additionally, the demand for an average vehicle that is not covered by the tax credit incentive does not change in response to the incentive.

This section investigates various mechanisms through which the tax credit incentive could affect the adoption of green vehicles. We argue that the tax credit incentive influences consumer demand via the cost saving benefit. Specifically, we anticipate that the tax-credit incentive will be more effective for consumers that primarily aim to save costs. In what follows, we provide suggestive evidence that supports the cost-saving mechanism for the tax-credit incentive launch.

2.5.1 Cost-Saving Mechanism for the Tax-Credit Incentive

The tax-credit incentive provides consumers with a tax refund. As such consumers can save costs, if they purchase a green vehicle covered by the tax-credit incentive. Thus, we conjecture that the tax-credit incentive will be more effective for consumers who value cost savings more. In this subsection, we provide two types of empirical evidence to support the cost-saving mechanism for the tax-credit incentive. First, we demonstrate that the tax-credit incentive launch has a more positive sales effect on vehicles covered by the incentive in counties with lower income levels. Second, we show that the introduction of the tax-credit incentive results in demand substitution from gasoline vehicles with higher fuel efficiency.

Tax-Credit Incentive Has a More Positive Impact on Green-Vehicle Sales in Counties with Lower Income Previous studies show that consumers' valuation of cost savings depend on their income level (e.g. Evans and Viscusi, 1993). Specifically, customers with higher income are less sensitive to cost savings than those with lower income. Accordingly, it is likely that the tax-credit incentive launch has a greater positive impact on unit sales of the vehicles covered by the incentive in counties with lower income level. To formally test this conjecture, we estimate the model below:

$$\begin{aligned} ln(Unit\,Sales_{ijt}^{covered\_green}) &= \mu\, TaxCredit\, launch_{it} \\ &\quad + \mu_{Income\,int}^{Tax} TaxCredit\, launch_{it} \times Income_{it} \\ &\quad + \alpha_i + \delta_{s(i)j} + \lambda_{jt} + X_{ijt}'\gamma + \varepsilon_{ijt}. \end{aligned} \tag{G.7}$$

The parameter of interest in this specification is $\mu_{Income\,int}^{Tax}$. It estimates the heterogeneous (if any) impact of the tax-credit incentive launch on unit sales of green vehicles covered by the incentive in terms of the median household income in a given county. The fixed effects and control variables are the same as those defined for Equation G.6. Column (1) in Table I.9 shows the coefficient estimates based on the specification above. The negative and significant coefficient for the $TaxCredit\,launch_{it} \times Income_{it}$ interaction ($\mu^{Tax}_{Income\,int}$ = -.029, $p < .01$) suggests that the increase in unit sales for the green vehicles covered by the incentive in response to the tax-credit launch is more in counties with lower income than those with higher income.

Demand Substitution from Gasoline Vehicles After the Tax-Credit Incentive Launch is Greater for Vehicles with Higher Fuel Efficiency The automobile market provides a large set of gasoline vehicle options with varying degrees of fuel efficiency. While some vehicle models allow commuters to travel up to 33 miles per gallon (MPG) (e.g., Toyota Yaris), others can only get 15 (e.g., Ford Expedition). If the effectiveness of the tax-credit incentive is through the cost-saving mechanism, one would predict that there could be more demand substitution from gasoline vehicles with higher fuel efficiency to the green vehicles covered by the tax-credit incentive after the incentive becomes available. To see whether we have any empirical evidence supporting this prediction, we estimate the following specification:

$$\begin{aligned} ln(Unit\,Sales^{gasoline}_{ijt}) &= \mu\,TaxCredit\,launch_{it} \\ &+\mu_{MPG\,med\,incr}TaxCredit\,launch_{it} \times Medium\,MPG_j \\ &+\mu_{MPG\,high\,incr}TaxCredit\,launch_{it} \times High\,MPG_j \\ &+\alpha_i + \delta_{s(i)j} + \lambda_{jt} + X'_{ijt}\gamma + \varepsilon_{ijt}. \end{aligned} \tag{G.8}$$

The model above estimates the heterogeneous (if any) effect of the tax-credit incentive launch on unit sales of gasoline vehicles in terms of the MPG category (i.e., low, medium, and high) of a given vehicle model. The categories are determined based on industry conventions (i.e., Auto Trader): MPG below 20 is categorized as low, MPG between 20 and 30 is categorized as medium, and MPG above 30 is categorized as high. The fixed effects and control variables are the same as those defined for Equation G.6. Column (1) in Table I.11 presents the estimates based on Equation G.8. The estimates suggest that the tax-credit incentive launch results in a greater reduction in the demand for gasoline vehicles with medium ($\mu_{MPG\,med\,incr}$ = -.045, $p < .001$) and high ($\mu_{MPG\,high\,incr}$ = -.077, $p < .01$) fuel efficiency relative to those with low fuel efficiency. These findings imply that the introduction of the tax-credit incentive leads to more demand substitution from gasoline vehicles with higher fuel efficiency to green vehicles covered by the incentive. Collectively, the results above provide suggestive evidence that the cost-saving mechanism is at work.

2.5.2 Other Potential Mechanisms

Interactions with Demographics Variables To allay concerns related to the potential moderating effects of demographic variables on the relationship between tax-credit incentive launch and unit sales, we also control for additional interactions. Specifically, we include the interactions of income, education, age, gender, and unemployment rate with the tax credit incentive launch in Equation G.7, and report the estimates for our key interactions in Column (3) of I.9, respectively. Our previous findings remain robust. None of the interactions between these demographic variables and incentives are significant in both estimations.

Interactions with Vehicle Price, Category, and Brand We also examine the robustness of our previous findings regarding the impact of tax credit incentive launch on gasoline vehicle demand to the inclusion of the interactions of vehicle price, category, and brand with the incentive change. The estimation results that control for potential moderating effects of vehicle price, category, and brand on the relationship between the tax credit incentive launch and unit sales of gasoline vehicles are reported in Column (2) of Table I.11. Notably, our previous conclusions largely remain robust to the inclusion of the interactions of vehicle price, category, and brand with both incentive changes.[58]

[58] In a separate analysis reported in Online Appendix H, we also examine whether brands that have hybrid vehicles in their portfolio are affected differently than those without by the tax credit incentive launch. We find a significant and negative coefficient for the interaction between the tax-credit incentive launch and the dummy variable for brands with plug-in hybrid vehicles. This shows that the tax-credit incentive launch leads to a larger drop in the demand for gasoline vehicles associated with brands with plug-in hybrid vehicles relative to those without.

2.6 Overall Effectiveness of the Tax-Credit Incentive

Our results so far indicate that the launch of tax-credit incentive led to a increase in green vehicle sales via cost-saving mechanism. However, this financial incentive cannot last forever. Most states have terminated tax-credit incentive due to three primary reasons. First, governments have set up the expiration date of incentives when government passed the law to offer them. But governments can decide whether to extend it. Second, . As a result, to have a comprehensive understanding of the overall effectiveness of tax-credit incentive, we also need to examine the impact of the termination. In our observation window, Oregon has terminated tax-credit incentive for plug-in hybrid vehicles on December 31st, 2011. We use Oregon as an example to study the impact of tax-credit termination.

2.6.1 The Effect of Tax-Credit Termination on Unit Sales of Plug-in Hybrid Vehicles

In this subsection, we estimate the effect of the tax-credit incentive termination on unit sales of plug-in hybrid vehicles in Oregon. We follow the same structure we used earlier for the analysis of tax-credit incentive launch. Specifically, we replace the $TaxCredit\,launch_{it}$ variable in Equation G.6 with the $TaxCredit\,termination_{it}$ dummy variable that is equal to 1 after the tax-credit incentive is terminated if a given county belongs to a state where there is an tax-credit incentive termination, and 0 otherwise. The fixed effects and control variables are the same as those defined for Equation G.6. Also, the interpretation of the coefficient for the $TaxCredit\,termination_{it}$ variable is the same as that discussed for $\beta_{TaxCredit\,launch}$ earlier.

Column (1) in Table I.10 presents the estimated effect of the tax-credit incentive launch on unit sales of plug-in hybrid vehicles using a symmetric 12-month window (6 months before and 6 months after) around the tax-credit incentive termination. The estimates suggest that the average effect of the tax-credit incentive termination on unit sales of green vehicles covered by the incentive is positive (.041) but statistically insignificant (p = .45). Column (2) and (3) represent the effect of tax-credit incentive termination on uncovered vehicles, i.e.hybrid vehicles and gasoline vehicles respectively. The estimates suggest that unit sales of green vehicles uncovered by the incentive remain unchanged after the termination of tax-credit incentive.

2.6.2 Comparison of Sales Effects of Tax-Credit Launch and Termination

The analysis above suggests that, on average, the launch of the tax-credit incentive results in a significant 25.1% increase in plug-in hybrid vehicle sales. And the sale effect pike occurs in the fourth month after the launch, and the sales effect diminish afterwards. In contrast, the termination of the tax-credit incentive leads to an insignificant change in plug-in hybrid vehicle sales. One alternative explanation is that tax-credit incentive attracts potential early adopters who are worried about the price premium of green vehicles. And they act quickly after the launch of tax-credit incentive since usually tax-credit incentive has a hard deadline due to the limit of fund. So we see the unit sales of green vehicles increase in the early stage, and diminish over time. For those who do not have willingness to purchase green vehicles, tax-credit can hardly change their decision. So the termination also have limited impact on them. In other words, tax-credit incentive has a positive "net" effect when we take both launch and termination into account.

2.7 Discussion

In this study, we empirically examine the impact of governments' tax-credit incentive on consumers' green and non-green product purchases. We show that the launch of tax-credit incentive leads to a 25.1% increase in unit sales of green vehicles covered by it. We also document time-varying effects. However, the tax credit incentive does not affect the demand for vehicles, green and non-green, that are not covered by those policies, on average. We also show that the termination of tax-credit incentive results in an insignificant change of unit sales of green vehicles covered by it. These findings are robust to 1) different functional forms, 2) falsification exercises, 3) differential trends across strata, 4) alternative identification strategies.

Importantly, we provide suggestive evidence that the launch of tax-credit incentive influences green-vehicle adoption via the cost-saving mechanism. Specifically, the launch of tax-credit incentive leads to a larger increase in demand for green vehicles covered by the incentive in counties where consumers value cost saving more (i.e., counties with lower income). In addition, the launch of tax-credit incentive induces demand substitution from non-green vehicles with higher fuel efficiency.

2.7.1 Managerial and Policy Implications

As green vehicles are treated as a potential solution to ease the carbon emission of tailpipe, many federal and state governments consider adopting green-vehicle incentives. As a result, it is important to explore whether and when governments' monetary incentives are effective as well as how they influence the demand for non-green products.

First, with county-level data and alternative identification strategies, our results suggest the launch of tax-credit incentive has a significant positive impact on the adoption of green vehicles. The demographic heterogeneity in the effect of the tax-credit incentive could help local governments choose an appropriate incentive. Specifically, our results demonstrate that the tax credit is more beneficial in markets with consumers with lower income. In contrast to some news reports and public complaints that monetary incentives unfairly subsidize the rich[59], our findings suggest that a monetary incentive (i.e., tax credit) has a more positive effect on green-vehicle sales in counties with lower income.

Second, the result that the positive effect of tax-credit incentive launch is larger than the insignificant effect of tax-credit incentive termination implies that the tax-credit incentive, albeit more costly, has a better overall effectiveness on improving the adoption of green vehicles than other non-monetary incentive such as HOV lane exemption incentive which has greater negative impact of termination (He et al. 2019).

Third, importantly, our findings indicate that the tax-credit incentive induces substitution to green vehicles from gasoline vehicles with higher fuel efficiency. In order to assess the overall effect of the tax-credit incentive in reducing carbon emissions, policymakers need to also know whether it results in market expansion along with the aforementioned substitution patterns. In

[59] Electric Vehicle Subsidies: Environmentally-Friendly Or Just Welfare For The Rich? (Source: Forbes). Electric Vehicle Subsidies Hurt the Poor and Help the Rich. (Source: Real clean energy).

Online Appendix I, we provide an additional analysis that examines the effect of the tax-credit incentive launch on total vehicle sales at the county level. This analysis shows an insignificant estimate for the effect of tax-credit incentive in total vehicle sales (i.e., no significant market expansion). Taken together with the results from previous studies such as He et al. (2019), the above findings suggest an important trade-off for policymakers: the HOV incentive leads to more substitution from vehicles with higher carbon emission levels compared to the tax-credit incentive, but it also expands the market resulting in more vehicles with carbon footprint.

Fourth, for manufacturers, our findings imply that it is important to take into account the adverse impact of government incentives on demand for certain non-green products along with their positive effect on green products. Depending on the type of incentive, some manufacturers could be more vulnerable than others to the negative impact of governments' monetary green-product incentives on their non-green vehicle sales. For example, manufacturers with a larger portfolio of high-fuel-efficiency cars could be more at risk when state governments provide a tax credit for green cars. Speculatively, this may partially explain the recent push by the GM CEO for renewed tax breaks on electric vehicles, given that GM's product line consists of cars with lower fuel efficiency relative to its competitors.[60]

2.7.2 Limitations and future research

This study examines one of the most commonly provided monetary incentives (i.e., tax credit) for green vehicles by governments. Although we provide an extensive set of robustness checks, including alternative identification strategies, readers should assess the evidence as they would in any study relying on observational data. More research on the effectiveness of other types monetary and non-monetary incentives would enhance our understanding of which incentives work best to promote green products and why. Furthermore, our analyses in this paper concentrate on short- and medium-term effects of governments' monetary green-product incentives. Future empirical research on the long-term effect of the tax-credit incentive would nicely complement the findings

[60] https://www.wsj.com/articles/gm-ceo-pushes-for-renewed-tax-breaks-on-electric-vehicles-1520450100.

in this study.

Figure I.1: The Average Impact of Tax-Credit Incentive on Green Vehicles Covered by the Incentive

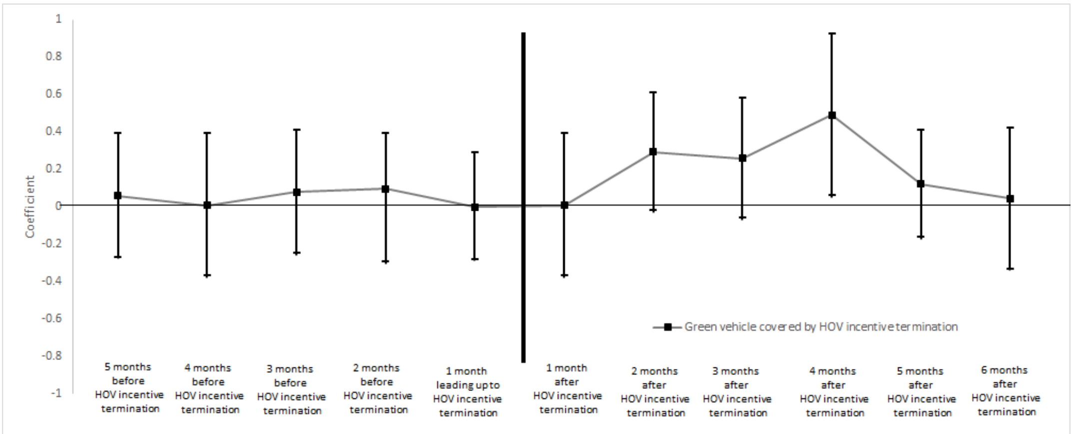


Figure I.2: The Average Impact of Tax-Credit Incentive on Vehicles Not Covered by the Incentive

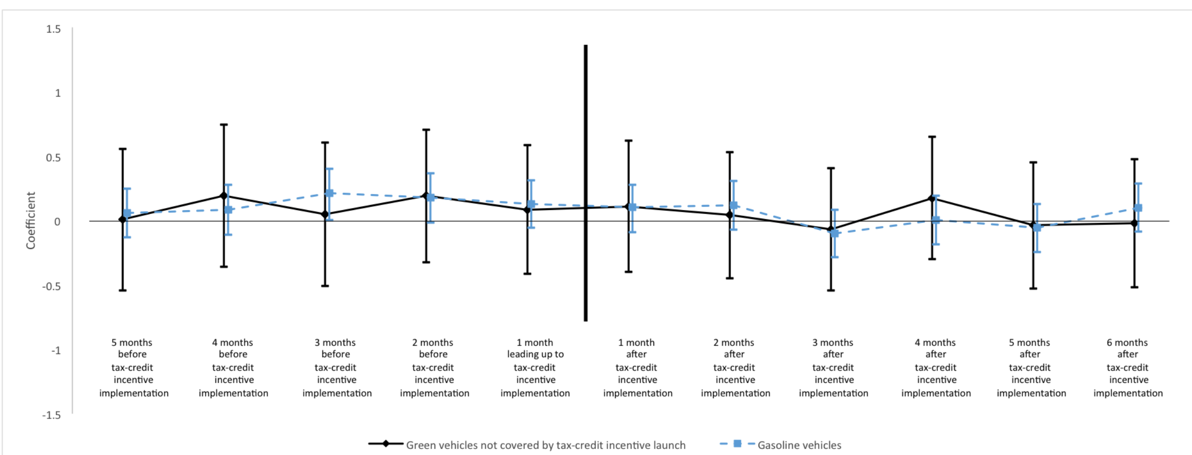


Table I.5: Descriptive Statistics and Raw Data Patterns

Variables	**No. of obs.**	**Mean**	**Std.dev.**	**Max**	**Min**
County-month level variables					
Price (in thousands USD)	5,604	33.1	3.2	46.2	24.5
Unit sales of all vehicles	5,604	74.5	116.5	969	0
Price of green vehicles (in thousands USD)	5,604	31.1	5.7	70.1	17.6
Unit sales of green vehicles	5,604	2.7	5.3	46	0
County-year level demographics					
Income: median household income (in thousands USD)	12,167	45.0	11.6	122.8	19.3
Education: % bachelor degree or higher achievement	12,167	19.2	8.6	74.4	3.2
Commute: mean travel time to work (in minutes)	12,167	23.0	5.3	44.2	4.5
Political inclination: % votes for the Democratic Party	12,167	40.7	19.3	92.5	5.7
Gender: % male residents	12,167	49.9	2.2	76.5	40.8
Age: Median age	12,167	40.3	4.9	63.8	21.6
Unemployment: % unemployment rate	12,167	8.6	3.7	28.8	0

Model-free evidence for the tax-credit incentive launch

	Counties with Incentive Change	Counties Without Incentive Change	Difference (p-value of t-test)
% unit sales change for green vehicles covered by tax-credit incentive after launch	98.8%	49.9%	48.9%** (0.004)
% unit sales change for green vehicles not covered by tax-credit incentive after launch	17.5%	11.5%	6.0% (.70)
% unit sales change for gasoline vehicles after launch	9.3%	8.7%	0.6% (.88)

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001.

Table I.6: The Impact of Tax-Credit Launch on Unit Sales for Different Vehicle Types

	(1) Unit sales of green vehicles covered by tax-credit incentive launch	(2) Unit sales of green vehicles not covered by tax-credit incentive launch	(3) Unit sales of gasoline vehicles
Tax-credit incentive launch	0.224*	0.033	0.004
	(0.106)	(0.025)	(0.005)
Stratum × vehicle model dummies × month dummies	Yes	Yes	Yes
County dummies	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes
Number of observations	287	5,409	136,602
R square	0.897	0.704	0.588

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table displays the results of Equation G.6, which measures the average effect of tax-credit incentive launch on green vehicles covered by the incentive change (i.e., plug-in hybrid vehicles), green vehicles not covered by the incentive change (i.e., hybrid vehicles and electric vehicles), and gasoline vehicles respectively. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price as well as county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table I.7: The Impact of the Tax-Credit Incentive on Unit Sales - Border Strategy

	Tax credit incentive launch		
	Green vehicles - covered	Green vehicles - not covered	Gasoline vehicles
Tax-credit incentive	0.166*	-0.027	-0.199
	(0.065)	(0.059)	(0.182)
CBSA × vehicle model dummies	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes
Number of observations	6,292	26,793	488,633
Log likelihood	-5,521	-20,185	-543,247

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (CBSA) are reported in parentheses. Control variables include state-month-level average gasoline price as well as county-level demographic variables that change annually: income, education, age, gender, unemployment rate, percentage of votes for the Democratic Party, and commute.

Table I.8: The Impact of the Tax-Credit Incentive on Unit Sales - RDiT-Style Analysis

	Tax-credit incentive launch			
	Unit sales of green vehicles - covered			
	4 weeks	8 weeks	12 weeks	16 weeks
Incentive change dummy	0.368+	0.082	0.099+	0.154*
	(0.195)	(0.058)	(0.060)	(0.067)
Number of observations	109	186	269	354
	Unit sales of green vehicles - not covered			
	4 weeks	8 weeks	12 weeks	16 weeks
Incentive change dummy	0.289	0.055	-0.044	-0.024
	(0.205)	(0.137)	(0.168)	(0.130)
Number of observations	167	318	445	585
	Unit sales of gasoline vehicles			
	4 weeks	8 weeks	12 weeks	16 weeks
Incentive change dummy	0.095	0.059	0.046	0.056+
	(0.066)	(0.037)	(0.030)	(0.029)
Number of observations	8,668	15,318	22,117	28,790

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. All estimations are based on the local linear approach (with separate trends pre- and post-incentive changes) using a rectangular kernel. Clustered standard errors (county) are reported in parentheses. Our findings remain robust when we use a second order polynomial (not reported due to space constraints).

Table I.9: The Impact of Tax-Credit Incentive on Plug-in Hybrids - Demographic Heterogeneity

	(1) Income	(3) Other demographics
Tax-credit incentive launch	1.461***	1.211*
	(0.352)	(0.635)
Tax-credit incentive launch × Income	-0.029**	-0.026*
	(0.008)	(0.009)
Interactions with other demographics (see the notes below)	No	Yes
Stratum × vehicle model dummies × month dummies	Yes	Yes
County dummies	Yes	Yes
Control variables (see the notes below)	Yes	Yes
Number of observations	287	287
R square	0.902	0.902

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price as well as county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute. Interactions with other demographics (not reported due to space constraints) include interactions of the incentive launch with the remaining demographics that are part of our control variables.

Table I.10: The Impact of Tax-Credit Termination on Unit Sales for Different Vehicle Types

	(1) Unit sales of green vehicles covered by tax-credit incentive launch	(2) Unit sales of green vehicles not covered by tax-credit incentive launch	(3) Unit sales of gasoline vehicles
Tax-credit incentive termination	0.041	-0.018	0.015
	(0.055)	(0.034)	(0.009)
Stratum × vehicle model dummies × month dummies	Yes	Yes	Yes
County dummies	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes
Number of observations	513	4,942	101,286
R square	0.865	0.798	0.696

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table displays the results of Equation G.6, which measures the average effect of tax-credit incentive launch on green vehicles covered by the incentive change (i.e., plug-in hybrid vehicles), green vehicles not covered by the incentive change (i.e., hybrid vehicles and electric vehicles), and gasoline vehicles respectively. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price as well as county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Table I.11: The Impact of Tax-Credit Incentive on Gasoline Vehicles - Product Heterogeneity

	(1) MPG	(2) Other vehicle characteristics
Tax-credit incentive launch	0.026**	0.090**
	(0.009)	(0.029)
Tax-credit incentive launch × Medium MPG	-0.077**	-0.102**
	(0.024)	(0.034)
Tax-credit incentive launch × High MPG	-0.045***	-0.057***
	(0.011)	(0.015)
Interactions with other vehicle characteristics (see the notes below)	No	Yes
Stratum × vehicle model dummies × month dummies	Yes	Yes
County dummies × month	Yes	Yes
Control variables	Yes	Yes
Number of observations	131,279	131,279
Log likelihood	0.683	0.683

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price as well as county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute. Interactions with other vehicle characteristics (not reported due to space constraints) include interactions of the incentive termination with vehicle price, category, and brand.

Online Appendix A: List of Green Vehicle Models

Table A.1: List of Green Vehicles in Our Analysis Period for the Tax-Credit Incentive Launch

	Hybrid		Plug-in Hybrid
Acura ILX Hybrid	GMC Sierra 1500	Mercedes-Benz ML450 Hybrid	Chevrolet Volt
Audi Q5 Hybrid	GMC Yukon Denali	Mercedes-Benz S400 Hybrid	Ford C-Max Energi
BMW ActiveHybrid 3	GMC Yukon Hybrid	Mercury Mariner Hybrid	Toyota Prius Plug-In
BMW ActiveHybrid 5	Honda CR-Z	Mercury Milan Hybrid	
BMW ActiveHybrid 750I	Honda Civic Hybrid	Nissan Altima Hybrid	Electric
BMW ActiveHybrid 750LI	Honda Insight	Porsche Cayenne	Ford Focus Electric
BMW ActiveHybrid X6	Hyundai Sonata Hybrid	Porsche Panamera	Mitsubishi i
Buick LaCrosse eAssist	Infiniti M35H	Toyota Avalon Hybrid	Nissan LEAF
Buick Regal eAssist	Kia Optima Hybrid	Toyota Camry Hybrid	Toyota RAV4 EV
Cadillac Escalade Hybrid	Lexus CT 200H	Toyota Highlander Hybrid	
Chevrolet Malibu Eco	Lexus ES 300H	Toyota Prius	
Chevrolet Silverado 1500	Lexus GS 450H	Toyota Prius c	
Chevrolet Tahoe Hybrid	Lexus HS 250H	Toyota Prius v	
Ford C-Max Hybrid	Lexus LS 600H L	Volkswagen Jetta Hybrid	
Ford Escape Hybrid	Lexus RX 450H	Volkswagen Touareg Hybrid	
Ford Fusion Hybrid	Lincoln MKZ Hybrid		

Online Appendix B: Criteria for Determining Isolated Markets

Table B.1: Isolated Market Criteria

	Minimum distance (in miles) to a CBSA with the following populations, Pop			
Population in CBSA (Thousands)	Pop > 50	Pop > 100	Pop > 200	Pop > 500
[0,100]	50	50	100	100
[100,200]	-	50	100	100
[200,500]	-	-	50	100
500+	-	-	-	100

Notes: The above criteria are adapted from Olivares and Cachon (2009). A CBSA with population indicated in the first column is included in our analysis sample if it satisfies the criteria shown in the other columns.

Online Appendix C: Difference-in-Differences Without Matching

Table C.1: Robustness to Difference-in-Differences Without Matching

	Tax credit		
	Unit sales of green vehicles - covered	Unit sales of green vehicles - not covered	Unit sales of gasoline vehicles
Tax-credit incentive launch	2.792***	0.035	0.020
	(0.592)	(0.124)	(0.047)
Price	-0.051	-0.010+	-0.019***
	(0.041)	(0.006)	(0.003)
Stratum × vehicle model dummies	Yes	Yes	Yes
County dummies	Yes	Yes	Yes
Vehicle model × month dummies	Yes	Yes	Yes
Control variables (see the notes below)	Yes	Yes	Yes
Number observations	3,529	30,235	936,593
Log likelihood	-2,102	-21,407	-913,226

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price as well as county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

Online Appendix D: Additional Robustness Checks

Falsification Exercises

We also perform "falsification" tests to alleviate any remaining concerns about endogeneity following earlier studies (Ozturk et al. 2016). Specifically, we re-estimate our main specifications for the two incentive changes to see whether there is a "treatment" group effect in a period when there is no incentive change–i.e., before May 2011 for the HOV incentive termination and before January 2012 for the tax-credit incentive launch. To do this, we use all the pre-incentive-change data and divide that into two halves, namely before and after the placebo incentive change. Then we test whether the coefficient for the placebo incentive change is significant. If we were to find a significant coefficient for the placebo incentive change on unit sales of green vehicles covered by the incentive in a period when no incentive change happens, this would suggest that unobservable differences that are correlated with incentive changes are contributing to our estimated effects. The result from these falsification exercises are reported in Table D.1. Column (1) shows that the estimate for the placebo-HOV incentive termination variable is insignificant. Similarly, Column (2) indicates that the estimate for the placebo tax-credit launch variable is insignificant. These falsification test results reinforce the causal interpretation of our main effect estimates.

Differential Trends Across Strata

To examine potential concerns regarding differential trends across strata, we repeat our estimations by adding strata-monthly trend interactions to our main specifications on Equation G.6. For instance, these interactions can capture gradually changing differential trends in gas prices or traffic patterns across strata. The results from these estimations are reported in Table D.2 for tax-credit launch. The estimates show that our key findings are robust even after we allow for differential time trends across strata.

Table D.1: Falsification Exercises for Tax-Credit Launch

	Tax-credit incentive launch
Placebo treatment	0.081
	(0.083)
Price	-0.015***
	(0.001)
Stratum × vehicle model dummies	Yes
County dummies × month	Yes
Vehicle model × Month dummies	Yes
Control variables (see the notes below)	Dropped
Number of observations	124,029
Log-likelihood	-145,762

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table shows the results from the falsification exercises for both incentive changes. Column (1) shows the estimation results for the HOV incentive termination. March 2011 is the time of placebo treatment. We have 2 months (January and February 2011) as the pre-placebo-treatment period and 2 months (March and April 2011) as the post-placebo-treatment period. Column (2) shows the estimation results for the tax-credit incentive launch. October 2011 is the time of placebo treatment. We have 3 months (July, August, and September 2011) as the pre-placebo-treatment period and 3 months (October, November, and December 2011) as the post-placebo-treatment period. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price as well as county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute. Since our analyses use observations from the same year and we control for county fixed effects, all control variables at the county-year level are dropped.

Table D.2: The Impact of Tax-Credit Launch - Robustness to Differential Trends Across Strata

	(1) Unit sales of green vehicles covered by Tax-credit incentive launch	(2) Unit sales of green vehicles not covered by Tax-credit incentive launch	(3) Unit sales of gasoline vehicles
Tax-credit incentive launch	0.224***	0.032	-0.005
	(0.106)	(0.023)	(0.006)
Price	-0.024+	0.002	-0.001***
	(0.014)	(0.002)	(0.000)
Stratum × vehicle model dummies	Yes	Yes	Yes
Stratum dummies × month	Yes	Yes	Yes
Vehicle model × month dummies	Yes	Yes	Yes
Control variables	Yes	Yes	Yes
Number of observations	287	5,409	136,602
R Square	0.897	0.709	0.589

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. This table shows the robustness of the impact of tax-credit incentive launch to differential linear trends across strata. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price as well as county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute.

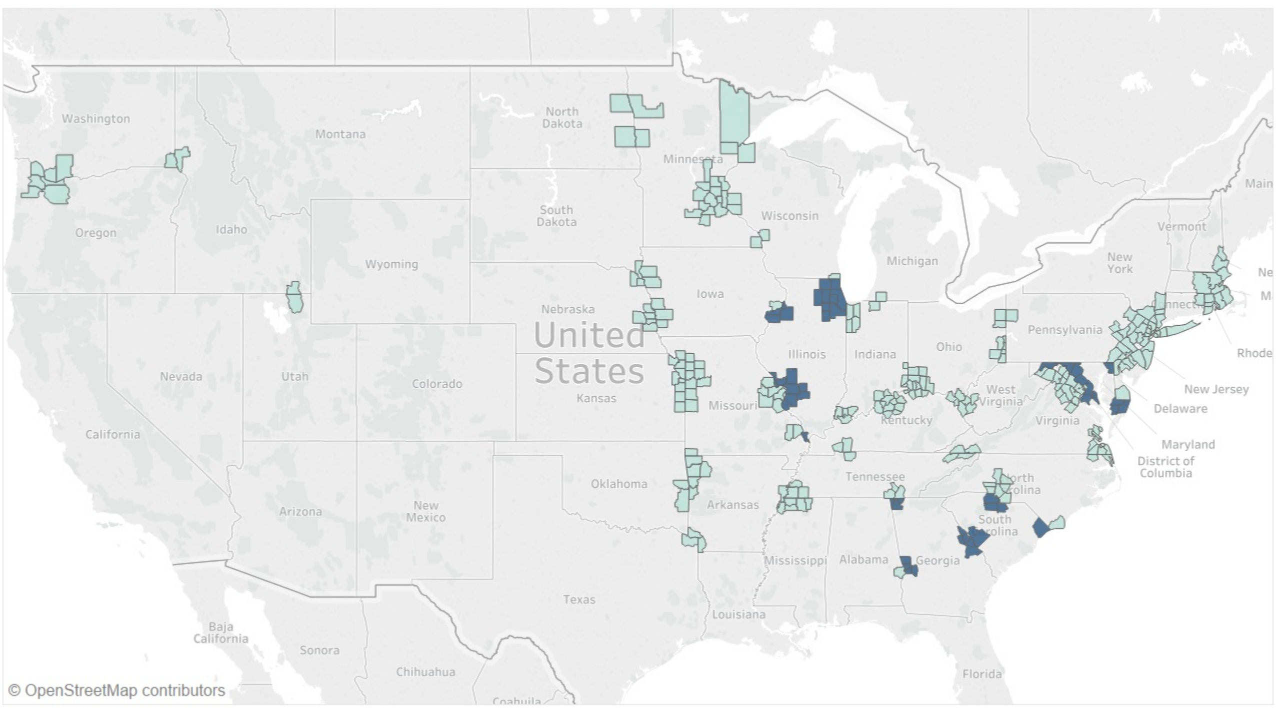
Online Appendix E: Additional Details on the Border Analyses

For our border analysis, we select CBSAs that are on the border of two or more states that are identical in terms of green-vehicle incentives except for the existence of focal incentive–i.e., tax-credit incentive. The CBSAs used for our border analysis are shown in Figure E.1 for the HOV incentive and the tax-credit incentive. The data for our border analyses contain 72 counties across 11 CBSAs. Six of them (48 counties) have the tax-credit incentive in place. The level of analysis for our border strategy is vehicle model-county-month. To estimate the effects of the HOV and tax-credit incentives using the border strategy, we use the following specifications, respectively:

$$\begin{aligned} ln(Unit\,Sales_{ijt}) &= \beta^{border}_{Tax\,credit} Tax\,credit_i \\ &\quad + \alpha_{c(i)j} + \lambda_t + X'_{it}\gamma + \varepsilon_{ijt}. \end{aligned} \tag{G.9}$$

The dummy variable $Tax\,credit_i$ is indicator for the availability of the associated incentive in a given county i. $c(i)$ is the CBSA to which county i belongs. $\alpha_{c(i)j}$'s are the CBSA-vehicle model fixed effects, which allow the baseline unit sales of each vehicle model to vary across CBSAs. λ_t are the month fixed effects that capture common time-variant unobservables. The vector X_{it} includes county-level demographic controls, including income, education, age, gender, unemployment rate, percentage of votes for the Democratic Party, and commute. The identifying assumption in these specifications is that after taking into account the common temporal shocks and demographic differences, any differences in demand between counties on two sides of the border within the same CBSA stem from the existence of the focal incentive.

Figure E.1: Map of Counties Used for the Border Analysis - Tax-Credit Incentive



Notes: Dark color shows the counties with the tax-credit incentive.

Online Appendix F: Additional Details on the RDiT-style Analyses

To estimate the effect of the tax-credit incentive on unit sales using an RDiT-style approach, we estimate the following specification:

$$
\begin{aligned}
Unit\,Sales_{ijt} \quad = \quad & \beta^{RDiT}_{Tax\,credit} Tax\,credit_t + f(t) \\
& \alpha_i + \delta_j + X'_{ijt}\gamma + \varepsilon_{ijt}.
\end{aligned}
\tag{E.10}
$$

The dummy variable $Tax\,credit_t$ is equal to 1 after the tax-credit incentive launch, and 0 otherwise. $f(t)$ is a flexible polynomial (e.g., linear or quadratic) time trend that allows for separate trends on either side of the tax credit incentive launch. County fixed effects α_i's account for the impact of time-invariant county-level factors on unit sales. Vehicle model fixed effects capture time-invariant vehicle model characteristics such as manufacturer, brand, and nameplate. The vector of control variables X_{ijt} includes the average transaction price of a vehicle model j in county i in month t. The key coefficient of interest is $\beta^{RDiT}_{Tax\,credit}$, which measure the impact of the tax credit incentive launch on unit sales of the three vehicle types.

Online Appendix G: Correlation Matrix for Demographic Variables

Table G.1: Correlation Matrix for Demographic Variables

	income	education	gender	age	commute	unemployment
education	0.604***					
gender	-0.056*	-0.134***				
age	0.007	-0.218***	-0.053*			
commute	0.155***	-0.267***	-0.013	0.317***		
unemployment	-0.479***	-0.366***	-0.085**	-0.053*	0.180***	
political inclination	-0.148***	0.046*	-0.068**	-0.131***	-0.100***	0.294***

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001.

Online Appendix H: Additional Interaction Estimations

Table H.1: The Impact of Tax-Credit Incentive on Gasoline Vehicles - Brands with Plug-in Hybrid Vehicles vs. Not

	Brands with plug-in hybrid vehicles vs. not
Tax-credit incentive launch	0.219+
	(0.115)
Tax-credit incentive launch × Medium MPG	-0.149***
	(0.014)
Tax-credit incentive launch × High MPG	-0.306***
	(0.026)
Tax-credit incentive launch × Dummy for brands with plug-in hybrid vehicles	-0.102***
	(0.010)
Interactions with other vehicle characteristics (see the notes below)	Yes
Stratum × vehicle model dummies	Yes
County dummies × month	Yes
Vehicle model × month dummies	Yes
Control variables	Yes
Number of observations	235,119
Log likelihood	-281,035

Notes: + p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Clustered standard errors (county) are reported in parentheses. Control variables include state-month-level average gasoline price as well as county-level demographic variables that change annually: income, education, age, gender, unemployment rate, and commute. Interactions with other vehicle characteristics (not reported due to space constraints) include interactions of the incentive termination with price and vehicle categories.

Online Appendix I: Analysis of Total Car Sales at the County Level

Table I.1: Analysis of Total Car Sales for Incentive Change

	Tax-credit incentive launch
Incentive change	0.025
	(0.261)
Price	-0.521***
	(0.027)
County dummies	Yes
Month dummies	Yes
Number of observations	3,345
Log-likelihood	-14,644

Notes: The results are generated by a negative binomial with log link. The dependent variable of the model is the monthly total unit sales of all vehicles (green and non-green) in a given county.

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