The Effects of Regulation in the Presence of Multiple Unpriced Externalities: Evidence from the Transportation Sector

By ANTONIO BENTO, DANIEL KAFFINE, KEVIN ROTH,
AND MATTHEW ZARAGOZA-WATKINS*

Abstract

In transportation systems with unpriced congestion, allowing single-occupant low-emission vehicles in high occupancy vehicle (HOV) lanes to encourage their adoption exacerbates congestion costs for carpoolers. The resulting welfare effects of the policy are negative, with environmental benefits overwhelmingly dominated by the increased congestion costs. Exploiting the introduction of the Clean Air Vehicle Stickers policy in California with a regression discontinuity design, our results imply a best-case cost of $124 per ton of reductions in greenhouse gases, $606,000 dollars per ton of nitrogen oxides reduction, and $505,000 dollars per ton of hydrocarbon reduction, exceeding those of other options readily available to policymakers.

For reasons discussed in Harberger (1974), the estimation of the overall welfare effects of government interventions to correct externalities is more challenging

* Bento: Cornell University, Charles H Dyson School of Applied Economics and Management. 424 Warren Hall, Ithaca, NY 14853 (e-mail: amb396@cornell.edu); Kaffine: Colorado School of Mines Division of Economics and Business, 328 Engineering Hall, Golden CO 80401 (email: dkaffine@mines.edu); Roth: Cornell University, Department of Economics (email: kdr42@cornell.edu); Zaragoza-Watkins: University of California at Berkeley, Agricultural and Resource Economics, 207 Giannini Hall, Berkeley, CA 94720 (email: mdzwatkins@berkeley.edu). The authors thank Alex Anas, Richard Arnott, Lucas Davis, Matthew Freedman, Kenneth Gillingham, Sumeet Gulati, Ryan Kellogg, Shanjun Li, Jordan Matsudaria, Justin McCrary, Erich Muehlegger, James Sallee, and Hendrik Wolff for helpful comments as well as seminar participants at AERE-Seattle, Colorado School of Mines, Columbia, EAERE, Oregon State University, Stanford, TREE Seminar Series, UC-Berkeley, UEA-Miami, WCERE Montreal and Yale. Finally we thank Arthur T Degaetano for weather data.

than first outlined by Pigou (1920). First, unpriced externalities in a single market can interact with one another. As a consequence, policies levied to correct an externality can exacerbate or alleviate related unpriced externalities in that market. Second, policies levied to correct an externality in one market can also generate additional interaction effects in related markets, provided these markets also have unpriced externalities or other preexisting distortions. A priori, theory cannot shed light on the relative importance of the primary welfare effect of the policy – defined by the welfare gain from correcting the externality addressed by the policy– and the interaction effects – defined as the welfare effect that results from the interaction of the new policy with other unpriced externalities.

Multiple unpriced externalities are particularly prevalent in the transportation sector, where unpriced congestion and air pollution interact in nontrivial ways across space and time.[1] Recently, in an attempt to reduce automobile-related emissions, policymakers have introduced policies to stimulate the demand for ultra-low-emission vehicles (ULEVs) such as gas-electric hybrids.[2] A popular policy, in place in nine states and under consideration in six others, consists of allowing solo-hybrid drivers access to high occupancy vehicle (HOV) lanes on major freeways.[3] In this paper, we take advantage of the introduction of this policy in Los Angeles, California to study interactions between multiple unpriced externalities. We demonstrate the first-order importance of the interaction effect between the policy and unpriced congestion and show that it generates substantial welfare losses, dominating the expected primary welfare gain of the policy.

[1] Automobiles are major contributors to local and global air pollution, including carbon monoxide (CO), nitrogen oxides (NOx), and hydrocarbons (HC), and the transportation sector accounts for 20% of greenhouse gas (GHG) emissions in the United States (EPA, 2007). Automobile use also leads to significant congestion costs. In 2003 these were estimated at $63 billion dollars, with drivers in our area of focus losing 93 hours annually to congestion delays (Shrank and Lomax, 2005).

[2] Policies to promote the purchase of these new hybrid vehicles are also motivated by the perceived lag in adoption that often results from a lack of acceptability of new technologies by consumers (Greene, 2010; Helfand and Wolverton, 2011).

[3] Of nearly twenty states with HOV lanes, nine allow hybrid vehicles to drive in HOV lanes: Arizona, California, Colorado, Florida, New Jersey, New York, Tennessee, Utah and Virginia.

While it may not be too surprising that allowing solo-hybrid drivers into HOV lanes is likely an inefficient policy to promote the adoption of green technologies, we stress the remarkable variation of the interaction effect between unpriced congestion and the policy across space and time. Congested locations and peak travel periods make the HOV lane exemption attractive to hybrid drivers, but create the greatest congestion costs for existing carpoolers. While adding a single hybrid to any HOV lane at 2AM creates zero social costs of congestion, adding one daily hybrid driver at 7AM to a very congested road in our study area (the I-10W) generates $4500 in annual social costs. On these exceptionally congested roads, HOV lane traffic may be up to 30% above socially optimal levels, implying significant congestion costs from allowing hybrid access. As such, estimates of the effects of the policy that rely on average values would underestimate the impact of a marginal hybrid on HOV travel times, leading to erroneous estimates of the welfare effects of the policy.

Our findings imply a best-case cost of $124 per ton of reductions in greenhouse gas emissions, $606,000 dollars per ton of nitrogen oxides (NOx) reduction, and $505,000 dollars per ton of hydrocarbon reduction in the most optimistic calculations. These costs exceed those of other options readily available to policymakers. Further, a policy that was perceived as 'free' was far from free. We find that it costs carpoolers $3-$9 for every $1 of benefit transferred to hybrid drivers. If instead a tax-credit were implemented, funded through a broad-based tax, the cost to taxpayers would have been only $1.4 per dollar transferred.

To measure the magnitude of these interaction effects, we have assembled a rich dataset that includes real time data from the Freeway Performance Measurement System (PeMS) in California. PeMS reports hourly travel time for major routes and traffic flow for detectors located in HOV and mainlines. We use both travel time on a single route (I-10W) and detector level traffic flow for District 7, which corresponds to the greater Los Angeles metropolitan area and more than 36% of all HOV lanes in California. The analysis controls for possible confounding factors through the use of a regression discontinuity (RD) design where travel time and traffic flow in the HOV and mainline lanes are compared before and after the start of the policy. A common tension within the RD literature is the use of short run RD estimates to establish long run welfare effects. As a robustness check, we also take advantage of the recent ending of the policy to test the persistence of these changes in travel time. Similar changes at the end of the policy suggest that there is a sustained, upward shift in congestion levels, validating the use of our estimates for the welfare analysis.

This paper contributes broadly to the literature on environmental policy in a second-best setting. When examining the welfare effects of environmental policies in a second best setting, Bovenberg and de Mooij (1994), Bovenberg and Goulder (1996), and Parry and Small (2005), emphasize the importance of considering the interactions between preexisting distortions caused by distortionary taxes in factor markets and the new environmental policy.[4] As in Parry and Small (2005), here the interactions occur between the policy and unpriced congestion externalities.

In contrast with nearly all prior work in this area, which has typically relied on analytical and calibrated simulation models, our analysis provides one of the first econometric estimates of the interaction effect between an environmental motivated policy and a competing unpriced externality.[5] These econometric estimates are incorporated into a general equilibrium welfare framework, which is used to calculate the overall welfare and distributional impacts of the policy. A key insight of the second-best literature is that revenue-raising instruments can

[4] In the context of automobile-related policies, Parry and Small (2005) derive rules for the optimal second-best gasoline tax and examine the interactions between a gasoline tax and preexisting distortions caused by labor taxes and unpriced externalities within the transportation system, such as congestion, air pollution and accidents with a simulation model.

[5] The exception is West and Williams (2007) who estimate the parameters necessary to calculate the optimal second-best gasoline tax using household level data.

alleviate some of the welfare losses associated with interaction effects (Goulder, Parry, and Burtraw 1997). In this context, we compare the welfare effects of this non-revenue raising policy with alternative revenue raising policies, including auctioning of the hybrid stickers and high-occupancy toll lanes (HOT) that vary with fuel economy. We also compare this policy against tax incentives, as recently examined in Sallee (2011).

I. Background on the Policy

California has long had a reputation for being at the forefront of environmental policy. Because of perceived costs, many environmental policies typically face resistance from taxpayers and industry. In contrast, the Clean Air Vehicle Sticker (CAVS) policy was popular among nearly all interested parties. In the words of Assemblywoman Fran Pavley (D-Agoura Hills), author of the measure, "This is a win, win, win -- cleaning up our air, reducing dependence on foreign oil and saving money at the pump."[6]

Beginning August 10, 2005 and ending June 30th, 2011, owners of hybrid vehicles achieving 45 miles-per-gallon (mpg) or better were able to apply for a special sticker that allowed them access to HOV lanes regardless of the number of occupants in the vehicle. The goal of the CAVS policy was to stimulate the demand for highly fuel-efficient vehicles, particularly of ultra-low-emission vehicles (ULEV), such as the Honda Insight, Honda Civic Hybrid, and Toyota Prius. The sticker had limited transferability and was allocated to the vehicle and not the driver. The original bill allowed for the issuance of 75,000 stickers, but later legislation eventually increased the limit to 85,000. Stickers were available for $8 dollars to all owners of eligible vehicles, including the owners of the over 65,000 estimated hybrids already registered in California at the start of the policy.

[6] Salladay, Robert. 2004. "Hybrids Move Closer to Using Carpool Lanes," *Los Angeles Times*, May 7, 2004.

The rationale for this decision rested on the idea of not penalizing earlier adopters. In Los Angeles County, a total of 27,228 stickers were distributed over the course of the program. By August 20th, more than 12,000 applications for stickers were submitted to the DMV, implying that a substantial number of hybrids entered the HOV lanes at the beginning of the program.[7] By February 2007, all 85,000 stickers had been issued. While the original CAVS hybrid policy expired June 30th, 2011, it is nonetheless crucial to understand the effects of these programs as California introduced a new HOV exception program with 40,000 stickers for electric, hydrogen fuel cell, and plug-in hybrid vehicles on January 1st 2012.[8]

II. Data

Travel time on major highways in California is collected by the Freeway Performance Measurement System (PeMS), a joint effort by the California Department of Transportation (Caltrans), the University of California, Berkeley, and the Partnership for Advanced Technology on the Highways (PATH). PeMS obtains 30-second loop detector data in real-time across 12 Caltrans districts. Each detector compiles data on traffic flow and lane occupancy, which are then used to calculate traffic speed.[9] We use both travel time on a single route and detector level traffic flow for District 7, corresponding to the greater Los Angeles metropolitan area.

A. Travel Time on the I-10W

[7] Gledhill, Lynda. 2005. "Drivers race for carpool permits for hybrids: At 1,000 applicants a day, some predict gas-saver gridlock," *SF Chronicle*, August 20.

[8] The CAVS program was initially designed to expire on December 31st, 2008, however, as a result of the popularity of the program, an organized group of hybrid drivers has successfully lobbied for subsequent extensions of the program. Barringer, Felicity. 2011. "Hybrid Owners Seek to Extend Carpool Privilege" *New York Times,* May 18.

[9] Lane occupancy is the fraction of time the detector is "on" due to the presence of a vehicle. Based on average vehicle length and this lane occupancy measure, the speed of traffic is computed. See PeMS FAQ for more information: http://pems.eecs.berkeley.edu/?dnode=Help&content=help_faq.

Because commuters are primarily concerned with the time it takes to commute along a particular route, our initial analysis focuses on hourly travel time over a single freeway route. Routes are defined as a segment of the freeway system from a fixed starting point to a fixed destination and are predetermined by PeMS. A route level measure of travel time combines information from multiple detectors that a commuter would typically drive.[10]

The data set obtained from PeMS reports the hourly travel times along a 17.5-mile section of the I-10W for the HOV lane and each of the four mainline lanes (Map 1). With the exception of a three-plus occupant requirement during peak travel times in the HOV lane, this route is fairly representative in terms of size and design for the Los Angeles metropolitan area.[11] In addition to the I-10W, travel time data is also collected for the I-210W to broadly capture demand on competing freeways. While a large window of data around the policy is desirable, our window of analysis is limited by the availability of I-210W data to be January 2004 through December 2007. Finally, as weekend and holiday travel demand is substantially different from weekday demand, these observations are removed from the initial analysis resulting in a total of 34,980 hourly travel time observations by lane.

Figure 1 plots the 2004-2007 average travel times for the HOV and mainline lanes of the I-10W across the hours of the day. Morning peak is defined by Caltrans as 5 A.M. to 9 A.M. and afternoon peak as 4 P.M. to 7 P.M. The mid-day off-peak corresponds to 10 A.M. to 4 P.M. and the night off-peak from

[10] For example, drivers commuting to downtown LA from West Covina typically use the I-10 route, those commuting from Thousand Oaks to downtown LA use US Hwy 101; drivers in San Francisco commute to San Jose using US Hwy 101. PeMS computes travel time over a freeway segment by dividing the length of the segment by the calculated traffic speed at that detector and summing travel time across the segments that form the route.

[11] The I-10W, westbound from West Covina to Los Angeles, was selected on the criteria of data availability, data on a competing route, and high detector density. See Appendix A for more detail on the selection of the I-10W and further discussion of the data used.

8.P.M. to 4 A.M.[12] The figure reveals substantial variation in travel times over the course of the day, with maximum travel time levels of over 35 minutes in the mainline reached during the morning peak. This figure also underscores the large differential between mainline and HOV lane travel times during the morning peak, with a maximum difference of nearly 10 minutes at 7 A.M.

While the route level data allows us to explore the heterogeneity of the CAVS policy across various times of the day, it does not allow for exploration of the effect across various locations of the LA metropolitan area. To explore the spatial effects of the program and generality of the results, we also consider a detector level dataset of traffic flow spanning the LA area.

B. Detector Level Data

We estimate the citywide effect of the CAVS policy during peak hours using a comprehensive dataset of 677 detectors collected by PeMS. These detectors record hourly observations of traffic flow for HOV and mainline lanes on 18 freeways in Los Angeles. The wide spatial distribution of the final set of detectors used in the analysis can be noted in Map 1. Three months of data, July-September 2005, provide hourly traffic flow observations for 1,750 detectors across the 18 freeways, which includes all freeways with HOV lanes.[13] In addition, we also collect three months of data for 331 detectors around the end of the policy, June 30th, 2011.[14] Detectors located at on- and off-ramps are deleted from the analysis. Each detector is also required to have at least 50 observations after all deletions.[15] As is the case for the I-10W, many routes have a dominant

[12] See Tables E.2 and E.3 in Appendix E for weekday travel time averages for each lane and route, including the I-210W, during the four peak and off-peak periods. The westbound direction implies peak demand in the morning period.

[13] A larger window than three months would strain the assumptions of the local linear regressions used below.

[14] To ensure comparability with the start of the policy, we limit the detectors used in this analysis to those active at the start of the policy. Removal of decommissioned detectors and those with insufficient data further reduces the sample size.

[15] These deletions include hours outside of the specified peak period, weekend observations and observations labeled as less than 100 'percent observed.' Where detectors are not properly functioning, PeMS imputes missing values. By

commuting pattern such that only one peak time of day experiences congestion. We estimate the effect of the CAVS policy for each detector during the particular peak period corresponding to maximum traffic flow for that detector, during the three-month period.[16] This ultimately yields 677 individual detector level estimations of the effect of the CAVS policy on traffic flow, 200 of which are located in HOV lanes for our main analysis of the start of the policy.[17]

C. Other Covariates

The PeMS data is supplemented with hourly measures of weather from the National Weather Service at nine airports in the Los Angeles area. These measures include rainfall in inches, visibility in miles, cloud cover as a percentage of the sky, temperature in degrees Fahrenheit, and wind speed in miles per hour. The Fullerton airport station is closest to the I-10W, and data from this station is matched to the travel time data.[18] Nominal weekly retail gasoline prices (regular reformulated) for Los Angeles from 2004 to 2011 were obtained from the Energy Information Administration.[19]

III. Empirical Strategy

We begin by describing the empirical strategy utilized to estimate the effect of the CAVS policy on travel time on the I-10W. We employ a regression discontinuity (RD) design where logged hourly travel time in lane *i* at hour *h* on

---

dropping all observations where 'Percent Observed' is less than 100, all data with PeMS imputation are removed from the analysis.

16 This method implies that for nearly all of the I-10W detectors, the morning peak is selected as the most congested time of day, as expected. Detectors for which the maximum traffic flow occurs outside the peak periods are also excluded, as a detector with maximum flow occurring at 2 A.M. is of questionable quality.

17 For the 331 detectors that comprise the end of policy analysis, 82 are HOV lane detectors.

18 For the Fullerton station, of the 35,064 total observations, 840 had at least one weather measure missing. These missing weather measures are imputed from the other stations in the Los Angeles area (Chino, Hawthorne, Hollywood, Long Beach, Los Angeles, Ontario, Santa Monica and Van Nuys), following the algorithm used in Auffhammer and Kellogg (2011). See Appendix A.

19 These values were not deflated for the analysis because the time span is relatively narrow and deflation would introduce discontinuities into the data.

date t, TT_{ht}^{i}, is regressed separately by lane on $1(Hybrid_t)$, an indicator variable for observations after the implementation of the CAVS policy, a vector of covariates X_{ht}, and a flexible n^{th}-order polynomial in date $f(Date_t)$:

$$(1)\ TT_{ht}^{i} = \alpha^{i} + \beta^{i} \cdot 1(Hybrid_t) + \gamma^{i} X_{ht} + f(Date_t) + \varepsilon_{ht}^{i}.$$

The coefficient of interest, β^{i}, is the treatment effect of the CAVS policy on travel time in lane i.[20] Policy date is taken to be August 20th, 2005, which is when stickers first became available. The vector of covariates X_{ht} includes indicator variables for month of the year interacted with day of the week, and indicators for hour of the day. Additional controls include weather variables (linear and quadratic rainfall, linear and quadratic visibility, and indicators for cloud cover in the central specification), logged gas prices, and travel time on competing routes (I-210W).[21] Finally, as people often choose freeway routes based on travel updates in the hour before they leave home, we include travel time on the I-210W lagged by one hour.[22] While the introduction of the stickers identifies the short run-effect of the policy, our interest is on the overall welfare effects and distributional impacts of the policy, typically a long run calculation. Therefore, the welfare estimates presented below are based on the assumption that the unobserved, latent travel-time function is a vertical translation of the observed travel-time function. However, one may be concerned with the non-instantaneous adjustment for the rest of the stickers, which were distributed over a longer period

[20] As the dependent variable is logged, a one-unit increase in $1(Hybrid_t)$ would imply a percentage increase in travel times of $\exp(\beta^{i} - 1)$. Because this transformation does not significantly change any of our results, we ignore it and simply discuss parameter estimates in the results for the sake of exposition.

[21] Several robustness checks also include measures of temperature, (prolonged) wind speed and wind gusts. Temperature is included as three indicator variables, below 80 degrees, 80 to 100 degrees, and above 100 degrees. Wind is included as an indicator variable for sustained wind speeds above 20 miles per hour and similarly an indicator for wind gusts indicates gusts above 20 miles per hour.

[22] While the inclusion of the I-210W is justified by economic theory, as a robustness check, estimations without the inclusion of the I-210W are also performed, yielding results similar to the key findings presented below. See Appendix E.

of time. To address this concern, we also estimate the effect of the policy's recent termination date of July 1st, 2011.[23] Estimating the policy effects at both the beginning and end of the policy provides insight into the transition and evolution of the policy effect.

RD: Global Polynomial—The potential for omitted time-varying factors to confound our estimation make observations substantially before or after the introduction of the policy less informative about the effect of the policy on travel time. Without controlling for these time-varying factors, the error term may be correlated with time, and thus with $1(Hybrid_t)$, producing biased estimates of β^i. Under reasonable assumptions, regression discontinuity methods yield consistent estimates of β^i in the presence of time-varying omitted variables. Hahn, Todd, and Van der Klaauw (2001) show that nonparametric identification of a constant treatment effect with a sharp RD design requires that the conditional mean function is continuous at the threshold. In other words, provided that all other factors affecting travel time besides the CAVS policy are continuous at the policy date, the RD design will yield a consistent estimate of the effect of the policy.[24] Equation (1) includes a single, flexible n^{th} order polynomial in date, $f(Date_t)$, which controls for unobserved, time-varying factors that evolve smoothly and may influence travel times but are unrelated to the policy.[25]

[23] As discussed below, due to data window limitations arising from the recent termination of the policy, we are restricted in terms of the types of analysis we can perform at the end of the policy. As such, most of our analysis focuses on the effects estimated at the introduction of the policy. We interpret the estimates as a conservative estimate of the congestion effects of the policy, and where possible, also incorporate the end of policy estimates to support our analysis.

[24] If other discontinuous, unobserved changes occurred at the policy date, the effect of those unobservables would be indistinguishable from the effect of the policy. For example lane closures due to construction would cause discontinuities in traffic flow. To reduce the likelihood that the effects observed below are caused by other such scenarios we perform several robustness checks including an examination of weekend travel time that would likely be affected by construction but not the CAVS policy.

[25] An alternative approach to the regression discontinuity design would be to use a difference-in-differences approach. However, it is difficult to construct an appropriate control group. As all freeways in California were subject to the policy, no freeway in California could be considered untreated. Comparing against a freeway outside of California would strain the necessary assumptions of the difference-in-difference approach.

Following the approach of DiNardo and Lee (2004) and Davis (2008), an eighth-order polynomial was selected as the most parsimonious specification that adequately describes the underlying time trend with a reasonable degree of smoothness, with estimates for sixth through tenth-order polynomials also reported below.[26] As standard tests suggest serial correlation is of some concern, robust standard errors clustered at the week level are calculated for all regressions.[27] Finally, we note that we do not have a sufficient time window to estimate a global polynomial model for the end of the policy estimates.

RD: Local Linear—In a local linear regression discontinuity design, time varying factors are controlled for with a linear trend within some local bandwidth of the policy discontinuity (Imbens and Lemieux 2008). In Appendix E, robustness checks on the route level analysis using a local linear method - where $f(Date_t)$ is linear and interacted with the policy variable - are performed, yielding similar results to the global polynomial estimates reported below. The local linear specification is also used for the detector level analysis of the effect of the CAVS policy on traffic flows both at the introduction of the policy in 2005 and its expiration in 2011.[28] The estimating equation is similar to equation (1), with logged-hourly traffic flow as the dependent variable and weather covariates as well as hourly fixed effects included. Traffic flow effects are estimated for each

[26] The most common method of polynomial selection in the literature chooses the order that smoothly describes the underlying trend in the data, while presenting estimates for alternative polynomial orders (Lee and Lemieux, 2009). As this involves a substantial element of modeler discretion, misspecification is a concern. As DiNardo and Lee (2004) note, misspecification of the order can lead to biased estimates of the discontinuity and erroneous interpretations of statistical significance. We report results for other polynomial orders as well as the order chosen by the Bayesian Information Criterion (BIC) (Matsudaira, 2008). Graphs of other polynomial orders are included in Figures D.1-11 in Appendix D.

[27] Clustered standard errors are reported for our results as they yielded more conservative estimates of the standard errors than Newey-West standard errors; Newey-West standard errors are presented in robustness checks in Appendix E.

[28] Local linear regression is utilized in the detector analysis because of the challenges associated with correctly specifying global polynomial controls for each of the 677 detectors.

detector using an Epanechnikov kernel with a 30-day bandwidth.[29] To determine the citywide effect of the policy on traffic flows in HOV and mainline lanes, the RD estimates across detectors are averaged. Standard errors are calculated using 5,000 bootstrap samples.[30]

IV. Route Level Results

A. The Effect of the CAVS Policy on Travel Time

Figure 2 illustrates the regression discontinuity strategy for estimating the effect of the CAVS policy on travel time. Panel (a) depicts travel time residuals in the HOV lane during the morning peak and panel (b) for the mainline. Similarly, panel (c) depicts the HOV lane residuals during the afternoon peak and panel (d) displays the residuals for the mainline during the afternoon peak. Panels (e) – (h) present travel time residuals during off peak periods. The plotted points represent the averaged, biweekly residuals of log hourly travel time regressed against the covariate vector X_t. These residuals should reveal any underlying, time-varying trends as well as any discontinuous changes in travel time at the policy date.[31] The fitted lines are the predicted values of a regression of these residuals on the eighth-order polynomial time trend and the CAVS policy variable.

Panels (a) and (c) reveal that travel time in the HOV lane increased during peak hours due to the CAVS policy, and that these effects are larger during the

[29] A 30-day bandwidth may not be optimal for all detectors; a robustness check Table E.19 in Appendix E presents results using the Silverman Rule for bandwidth selection and Table E. 21 presents results using the method outlined in Imbens and Kalyanarama (2011).

[30] We also correct these bootstrapped errors for spatial correlation and the imprecision of the detector level estimates. To account for the imprecision of the estimates from the detector level regressions, we first generate 5,000 sets of detector-level effects using the estimated means and standard deviations on each detector. These effects are then spatially partitioned by route-direction into disjoint blocks. For each of the 5,000 bootstrap renditions of the data we sample blocks with replacement to account for the spatial correlation in the estimates. These samples are then used to generate the mean and standard deviation of the estimated effect of the policy.

[31] These underlying trends can produce biased estimates when using OLS. For example OLS finds a statistically significant increase of 4.6 percent for travel time in the mainline during the morning peak, despite the absence of a discontinuity in panel (b) of Figure 2. See Table E.4 in Appendix E.

morning peak than in the afternoon.[32] Figure 2 also shows no policy effect on travel time in the mainline. Figure 2 reveals that congested lanes and times of day display noisier plots than less congested conditions. This is consistent with Shrank and Lomax (2005) who document that over half of congestion delays, especially in peak periods, are the result of nonrecurring events.[33]

Regression Discontinuity Estimates—Table 1 presents the regression discontinuity estimates of the effect of the CAVS policy on travel time.[34] For each time window and lane, the table reports the point estimate and standard error for the coefficient of interest, the percentage effect of the CAVS policy on travel time, broken down by morning peak, afternoon peak and off-peak periods. The preferred specification of the eighth-order polynomial is presented in column III. Consistent with Figure 2, the results in column III confirm that the increase in travel time during the morning peak on the HOV lane is 9.0 percent and is statistically significant at the 1 percent level; this effect corresponds to an increase of travel time of 2.2 minutes. Columns I-VI of Table 1 also present results for sixth-order through tenth-order polynomials, as well as the results for the

[32] When the time differential between the HOV and mainline lanes is the largest, stickered hybrids will have the strongest incentive to move into the HOV lane - not just from the I-10W mainline but also alternate routes without an HOV lane option. Times of day with low levels of congestion will give hybrid drivers less incentive to drive in the HOV lane and reduce their travel time. During off-peak hours, traffic flow is often below the threshold level where congestion occurs (Vickrey, 1969). In these free flowing periods, travel time will be invariant to the addition of hybrids and the policy should have a small (or no) effect.

[33] Slight differences between Figure 2 and the estimates in Table 1 are due to the fact that the polynomial and discontinuity are fit to the residuals in Figure 2, while they are included jointly in the regression in Table 1.

[34] These regressions include gas price, lagged I-210W travel time, weather covariates, indicator variables for hour of the day, and for month-day of week or day of week as noted. Robust standard errors clustered by week are included in parenthesis for all regressions. Robustness checks of the global polynomial regression discontinuity estimates are presented in Tables E.8-E.12 in Appendix E, and the results are in general consistent with those in Table 1. These robustness checks test the sensitivity of the results in our main specification by altering covariate specifications (fixed-effects, controls for competing routes, additional weather covariates, inclusion of holidays and gas prices), removal of days near the beginning of the policy, estimating separate polynomials on either side of the discontinuity, Newey-West standard errors, various windows of data and altering weather aggregation methods. Local linear regression estimates are generally consistent with the global polynomial estimates and are reported in Tables E.13-E.17. Appendix E also includes all estimated coefficients in Tables E.5-E.7.

polynomial order chosen under the BIC.[35] Across all polynomial orders, the estimate of the effect of the CAVS policy on travel time in the HOV lane is relatively stable, ranging between 8.4 and 11.6 percent, and is statistically significant at the 1 percent level.[36] The point estimates in the afternoon peak, 5.7 to 6.7 percent, are smaller than those estimated during the morning peak, consistent with the fact that congestion is most severe in the morning peak.

Table 1 also reports the estimated effect of the CAVS policy on mainline travel times. Broadly speaking, these point estimates are statistically insignificant and unstable (-6.4 to 7.0%) across polynomial orders and times of day. These provide little evidence that the CAVS policy affected mainline travel times. As we discuss in section VI the mainline estimates do not enter in our welfare calculations, but are nonetheless useful to establish that travel time changes are unique to the HOV lane. While suggestive that the policy had no effect in the mainline, we recognize that the mainline estimates are not as precise as would be ideal. In particular, the fact that the mainline and HOV lane estimates cannot be statistically distinguished raises concerns that the increase in HOV travel times is driven by something other than the CAVS policy.[37] We examine this concern and several others related to the mainline below and in the detector level results in Section V.

Effects of the Policy by Time of Day—Thus far, we have examined the effects of the CAVS policy at peak and off-peak periods and calculated the average

[35] The BIC selects the 8th order polynomial as the preferred specification in the mainline during the morning peak. For the morning peak HOV lane, a 10th order polynomial specification is chosen.

[36] As a check on the size of the policy effect, we note that Caltrans occasionally performs vehicle counts detailing the number, type, and occupancy of cars passing a point on major freeways in California, including the I-10W. A two-hour car count conducted between 6:30 A.M. and 8:30 A.M. on a weekday in 2006 found 167 single occupancy hybrids traveling in the HOV lane. This represents 5.8 percent of total vehicles counted in the HOV lane by Caltrans during that time. Several other plausibility checks are detailed in Table E.22 in Appendix E.

[37] We provide p-values for tests of the null hypothesis that the HOV and mainline effect are identical as well as a test for induced demand. This 'no induced demand' test examines the null hypothesis that the true mainline effect is the decrease in travel time expected (one-fourth the flow increase of the HOV lane) if all hybrid drivers had originated in the mainline lanes prior to the policy. These tests are generally inconclusive given the mainline standard errors.

treatment effect. However, it is likely that these effects will vary even within peak periods, especially during the morning peak. Small (1982) finds that individuals will adjust work-trip departure times in response to changes in congestion and that such behavior can result in heterogeneous responses during peak hours. Hybrid drivers previously commuting in the mainline may depart closer to their preferred time, given access to the less congested HOV lane.

Table 2 presents the effect of the policy by hour on HOV lane travel time, under the preferred specification of an eighth-order global polynomial. The distribution of magnitudes mimics the congestion levels noted in Figure 1, such that congested times of day are most affected by the CAVS policy. Point estimates are insignificant at 5 A.M. and at their largest from 8 to 9 A.M. These magnitudes grow as rush hour progresses from 8.8 percent at 6 A.M. to 12.2 percent at 9 A.M.[38] The effect of the policy on travel time is again most pronounced during the congested evening peak hours of 5 and 6 P.M. with effects of 6.0 and 8.2 percent respectively.

B. Further Exploration

The estimates presented above provide strong evidence that the CAVS policy increased travel times during peak hours on the HOV lane while mainline travel times remained unchanged. A skeptic could still argue, however, that the pattern of effects found here are the result of standard seasonal changes such as school year effects, or that overall demand for driving increased around the CAVS policy date, or that there are changes in commuting demand unrelated to the policy, perhaps driven by macroeconomic factors such as unemployment. Here we briefly discuss several robustness checks presented in Table 3.

First, one may be concerned that the effects found in the HOV lane are the

[38] The asymmetric nature of travel times is also noted by Arnott, de Palma, and Lindsey (1993) and Small, Winston, and Yan (2005) who document that travel times grow until late in the peak due to persistence of events earlier in the day.

result of seasonal variation. Table 3 columns II and III present the regression estimates of placebo tests using global regression discontinuity, where in column II, the policy variable becomes active on August 20, 2004, and for column III, it becomes active on August 20, 2006.[39] None of the point estimates are significant at the 10 percent level in the HOV lane. Second, it is possible that total demand for driving increased around the time of the policy implementation. Table 3 column IV presents global RD estimates using only weekend observations. These estimates show no evidence of an increase in travel time on weekends, suggesting that our results are not driven by a general increase in demand. Table 3 column V shows that adding unemployment to the model has almost no effect on the estimates, suggesting that the polynomial trend is capturing any work-week-specific demand changes related to employment. Finally, Table 3 column VI pools HOV and mainline observations with a single polynomial trend, yielding point estimates similar to our central specification with tighter standard errors. Paired t-tests find that the peak estimates in the HOV and mainline lanes can be rejected as identical, suggesting there was not an across-the-board increase in commuting demand.[40]

V. Detector Level Results

Thus far, we have established convincing evidence of the effect of the CAVS policy on travel time on the I-10W. It is unclear, however, if these changes are unique to the I-10W. The detector-level analysis, which is performed across a

[39] Some might be concerned that the use of the global polynomial implies an unbalanced number of observations on either side of the placebo policy date. In Table E.15 in Appendix E we present local linear results that confirm the global polynomial analysis. In Table E.10 in Appendix E we also present regression estimates of the 2005 policy effect using a traditional difference-in-differences analysis where 2004 or 2006 serve as the control year. Under these specifications, HOV lane point estimates are significant at the 1 percent level and are qualitatively similar to the point estimates from our central specifications.

[40] The paired t-test, accounting for the covariance between the HOV and mainline estimates, yields t-statistics of 5.07 in the morning peak and 5.26 in the afternoon peak. The detector level regressions below also allow us to statistically distinguish the HOV and mainline estimates in the most congested parts of the city where the effect is most pronounced, but without the restrictive assumptions of a single polynomial trend

larger set of roads at both the start and end of the policy, can help generalize these results. Furthermore, the larger data set allows for more precise estimates of the policy effect on mainline lanes. Figure 3 panel (a) plots the kernel density smoothed citywide distribution of the effects of the CAVS policy on hourly traffic flow for mainline and HOV lanes at the start of the policy. This figure is generated by separately estimating policy effect coefficients for the hundreds of detectors across the city. While the mainline detectors indicate little evidence of an effect of the policy on traffic flow, as shown in Table 4 column I, there is a citywide positive increase of 5.7 percent in the HOV lane flow, statistically significant at the 1 percent level.

Exploring the heterogeneity of the policy effect across the city, Table 4 columns II-IV report the average estimated effect of the policy on traffic flow by distance from downtown LA.[41] For detectors within 10 miles of downtown LA, column II reports a mean effect in the HOV lanes of 9.1 percent, while the mean estimated effect in the mainline lanes is a statistically insignificant 1.5 percent. Figure 3 panel (b) plots the smoothed distribution of hourly flow effects for different spatial subsets, including the subset of detectors on the I-10W. Despite the different HOV lane passenger restrictions, the distribution of effects found on the I-10W is similar to those for detectors located on other freeways near downtown Los Angeles.[42] Furthermore, for detectors located 0-10 miles from downtown LA, this estimation has sufficient power to statistically distinguish the effect of the policy on HOV and mainline lanes, allowing us to reject the possibility that there was a common increase in traffic across both HOV and mainline lanes as a

[41] As noted by Anas, Arnott, and Small (1998), L.A. has multiple Central Business Districts (CBD). Here, downtown LA was chosen to be the intersection of the I-10 and I-5 freeways. This corresponds to the area near Union Station in LA.

[42] The segment of the I-10W analyzed in the route level analysis runs from 3 to 20 miles of downtown LA. The I-10W detectors also confirm the route level analysis. The traffic flow increase of 9.6 percent observed for the I-10W HOV detectors implies that travel time in the I-10 HOV lane would have increased 6.7, which is statistically indistinguishable from the estimate of 7.2 percent found in the route level analysis using local linear regression (Table E.13 column II in Appendix E).

result of increased aggregate demand.

Columns III and IV increase the distance from downtown LA to 20 and 30 mile rings. Moving further from downtown LA, the HOV lane effect drops to zero and becomes statistically insignificant, suggesting that the increases observed are closely tied to congestion and not due to a general increase in demand.

Next, we turn to the estimates at the end of the policy. We note that though the city-wide effect is somewhat larger, it is not statistically distinguishable from the start of policy estimates. Nonetheless, the increase in the point estimate is consistent with expectations that the effect of the policy would increase as additional stickers were distributed over time.[43]

Finally, we note that the estimates in Table 4 reveal no evidence of a change in flow in mainline lanes. While policy makers may have expected congestion decreases in the mainline to be a potential benefit of the policy, these results are suggestive of the presence of induced demand per the fundamental law of highway congestion (Downs, 1962; Vickrey, 1969; Duranton and Turner, 2011). Although our results are consistent with the presence of induced demand, it is not possible to determine whether it is the result of new Vehicle Miles Traveled or of diverted demand from other routes or times of day. The source of this induced demand is important, as new VMT generated by individuals commuting more frequently or switching from other modes of travel can increase emissions, undermining a stated goal of the policy.[44] In the best-case scenario for emissions reductions, diverted demand from other routes or times of the day is the source of induced demand. In the welfare analysis below, we argue that our final conclusions are robust to any of these scenarios.

[43] The larger point estimate at distances far from the CBD at the end of the policy is also consistent with the idea that commuters in more congested areas of the city would have been more inclined to acquire stickers at the start of the policy, while more remote commuters may have delayed their acquisition of stickers.

[44] New VMT from public transportation users is likely to be small as only 4.1 percent of commuters in Los Angeles use bus and 0.7 percent use rail (State of the Commute Report, 1999).

VI. Welfare

A. Conceptual Framework

Overall Welfare Effects—In the spirit of the literature on taxation in a second best setting (Harberger, 1974; Bovenberg and Goulder, 1996), here we outline the overall welfare effects of the CAVS policy with the aid of diagrams. This provides a simple conceptual framework for interpreting the subsequent numerical results. We provide more details and mathematical formulas used to calculate the effects in Appendix B. Consider a classical transportation network (Vickrey, 1969) where agents select across a set of alternative transportation options, such as freeways, back roads, public transit, etc. Figure 4 depicts the equilibrium in the HOV lane and the mainline lanes, which we assume to be the only distorted markets in the economy. It also displays the market for vehicles with stickers, where given the fixed supply of stickers, rents will be generated per Bento and Jacobsen (2007). In both HOV and mainline lanes markets, distortions stem from the fact that agents ignore external congestion and pollution costs when making driving decisions, generating a wedge between the marginal private cost and the marginal social cost of using a vehicle. Consistent with the empirical evidence provided below, we assume that both the mainline and the HOV are over-utilized prior to the CAVS policy.

Suppose a sticker is issued that allows a driver of a qualified hybrid vehicle to drive in the HOV lane without having to carpool. The general equilibrium welfare effects from this policy consist of the welfare impacts in each of the distorted markets and changes in rents from the value of hybrid vehicles with stickers.

We define the *primary welfare gain* as the social benefit of emission reductions, arising from the fuel economy improvements associated with the adoption of the hybrid vehicles induced by the CAVS policy. This effect equals the reduction in the number of drivers in the mainline times the external cost of emissions (given by the rectangle abcd) net of the increased social costs of emissions that these drivers cause in the HOV lane, (given by the rectangle efgh).[45]

Second, the *cost-side congestion interaction effect,* defined as the welfare loss for HOV lane drivers that results from the policy's goal of inducing hybrid vehicles into the HOV lane. This effect equals the rectangle fgji and reflects the value of the travel time delays in the HOV lane, as hybrids clog the HOV lane.

Third, the *rent effect* defined as the implicit value of hybrid with stickers and given by the rectangle klmn. This rectangle reflects the maximum willingness to pay for a hybrid vehicle with a sticker and reflects the value of travel time savings for drivers who were initially in the mainline and move into the HOV lane. This rectangle is effectively equal to the difference between the rectangle padq (in the mainlaine market) and the rectangle rehs (in the HOV lane market). As we shall see below, a priori, it is not obvious whether hybrid drivers appropriate this rent.[46]

In addition to these three key sources of welfare, removing drivers from the mainline also has the potential of lowering congestion in the mainline, and throughout the transportation system. We define the *system-wide benefit congestion interaction effect* as the upper bound of the congestion relief benefits for all other drivers in the freeway system.[47] This effect is given by rectangle btuc.[48] We explicitly note that the estimates of the effect of the policy on mainline travel times do not enter the calculation of this potential source of welfare. This

[45] When moving from the mainline to the HOV lane, some agents may have to switch vehicles if they were not driving a qualified vehicle in first place. Agents will only switch lanes if welfare increases. Implicitly the maximum willingness to pay for a hybrid with the sticker should reflect the private travel time's savings associated with the change in lanes.

[46] Because the program was relatively small, we abstract from any potential general equilibrium changes in the value of all other vehicles. Further, there could also be second order welfare losses that result from individuals choosing a less preferred vehicle as a result of the policy

[47] These benefits occur as, for example, drivers on less congested alternative routes may now replace the exiting hybrids on the congested travel route, dissipating congestion relief for the original drivers. In turn, they may be replaced by drivers from backroads, or even new VMT. As noted in Table 5, we assume new VMT accounts for 15% of these trips.

[48] In Appendix C, we demonstrate the conditions under which a *partial equilibrium congestion interaction effect* calculated for the mainline will upper-bound the *system-wide congestion interaction effect*. The intuition is simple. The hypothetical congestion relief benefit is larger the greater the external costs of congestion. Therefore in other freeways or travel options not as congested as the mainline, the potential benefit cannot be as large as the benefit in the mainline.

effect is exclusively derived from the number of drivers that leave the mainline for the HOV lane, and the initial level of congestion in the mainline.

Creating congestion relief benefits within the freeway system leads to re-allocation of agents across freeways and potentially new trips and vehicle miles travelled (Hymel et al., 2010). To the extent that policy induces new vehicle miles travelled, it may generate an additional negative source of welfare corresponding to the value of the external costs of emissions associated with these trips. This effect is represented by the rectangle abvw.

While our welfare calculations are general equilibrium in nature, the partial equilibrium calculations derived from the estimates in the preceding sections, in particular the effects of the policy on travel time in the HOV lane and the implied number of drivers leaving the mainline to the HOV lane, serve as the key parameters for the welfare analysis.

Distributional Impacts: Who appropriates the rents generated by the program?— While it is obvious that carpoolers will be made worse off by the policy, a priori it is not obvious who benefits from the policy. We note that for a mainline driver to move into the HOV lane, he must experience some gain. If the agent already owns a qualified hybrid, he will appropriate the overall benefits of travel time reductions and will only pay for the cost of the sticker; alternatively, if the agent does not own a qualified hybrid, his maximum willingness to pay for a hybrid vehicle with a sticker will reflect the benefits of travel time reductions he would experience (given by the difference between dq and hs). This is to say that other agents in the system, including dealers of new hybrid vehicles or used hybrid sellers, have the potential to extract part of this willingness to pay for the sticker and appropriate some of the rents generated by the program. Therefore, the value of travel time savings for hybrid drivers presented below should be interpreted as an upper bound of the potential welfare gain of the policy to these agents.

B. Welfare Effects

Table 6 displays estimates and confidence intervals of the annual and present value welfare effects of the CAVS policy on the I-10W, broken down by welfare source.[49] It underscores the following key results. First, the net welfare effect of the policy was negative and equal to -$1.6 million dollars with congestion relief benefits for other drivers included and -$3.3 million dollars without. Importantly, when varying relevant parameters within reasonable distributions (detailed in Appendix B), the confidence interval is always statistically different from zero. Over the nearly six years of the policy, this represents a discounted net welfare loss of -$8 million to -$18 million dollars. The *primary welfare gain* from the policy is roughly $28,000 per year, representing the emissions savings benefits if all hybrid vehicles on the I-10W were purchased because of the CAVS policy. However, as Shewmake and Jarvis (2011) note, roughly two-thirds of all stickers were distributed to hybrid vehicles registered before the start of the CAVS policy, suggesting the primary welfare gain may be smaller.[50] By contrast, the interaction effects of the policy are substantially larger, a reflection of freeway overuse. The *cost-side congestion interaction effect* from increased HOV lane congestion is substantial and is estimated to be approximately -$4.0 million per year.[51] The *rent*

[49] The welfare calculations rely on the estimates from Sections IV and V, additional PeMS and Caltrans data, as well as parameters from the literature. Table 5 displays these parameters, with additional discussion of parameter choices in Appendix B. Travel time and flow are linked following Burger and Kaffine (2009) and discussed in Appendix B. Confidence intervals are generated from the standard errors of the hourly travel time effects in Table 2. All calculations in Table 6 (including emissions effects) are for a one-way commute on the I-10W. The results in Section V suggest that the policy would have a similar effect on travel time and emissions on other core freeways throughout Los Angeles. The end-of-policy results in Section V are slightly larger than those from the beginning of the policy. We use the beginning-of-policy estimates as a lower bound and report welfare effects using the end-of-policy estimates in Appendix E Table E.26. At the conclusion of this section, we present back-of-the-envelope calculations of the statewide effect of the CAVS policy.

[50] Furthermore, Diamond (2009) and Gallagher and Muehlegger (2011) find no evidence that the CAVS policy stimulated hybrid purchases, implying emissions savings and the primary welfare gain may be near zero.

[51] These values are an upper bound on congestion costs if some marginal carpoolers broke their carpools. However, it is unlikely that this represents a significant effect, as our estimates of the number of hybrids entering the HOV lane based on increased travel time are consistent with physical hybrid counts conducted by Caltrans.

effect associated with HOV lane access for hybrids is $672,000 per year.[52] The *system-wide congestion interaction effect* for all other drivers in the transportation system is upper-bounded at $1.7 million dollars per year, representing the partial equilibrium congestion relief benefit for I-10W mainline drivers.[53] Finally, the *emissions interaction effects* arising from induced VMT are very small, at -$6,275 annually for greenhouse gas emissions, -$714 for NOx emissions, and -$251 for hydrocarbons.[54] Thus, the congestion costs arising from the policy interaction dominate the overall welfare effect. Given the large negative effects of the policy on congestion, sensitivity analysis in Appendix B finds that varying relevant parameters within reasonable distributions does not affect the key finding that the overall welfare impacts are negative. Because congestion is not priced, and therefore lanes are overused, policies promoting clean technologies by lowering the total cost of driving may exacerbate the congestion externality, increasing the cost of clean technology promotion. This is particularly true in congested areas like LA, as adding a car to the HOV lane requires either an occupancy of 10 people or a solo hybrid driver with a value of time of $200 in order to compensate for the congestion externality generated.

Second, Table 6 illustrates the distributional costs of the policy. Our estimates find that the number of hybrids induced into the I-10W HOV lane was 904 per day or an average of 100 hybrids per peak hour. The *rent effect* divided by the number of hybrid vehicles gives the maximum rent generated per sticker as $743

[52] Small, Winston and Yan (2005) find that drivers may be willing to pay for increased reliability of travel times. Following their procedure, an additional reliability benefit of $100,000-$150,000 per year is created. However, Small, Winston and Yan consider the choice between an uncongested route with a certain travel time and a congested route with uncertain travel time, and it is unclear if drivers would value the choice between two congested routes with uncertain travel times in a similar manner. Furthermore, hybrid owners entering the HOV lane will decrease reliability for carpoolers. Due to the relatively small magnitude of benefits and offsetting effects, we exclude reliability benefits and focus on travel time.

[53] This upper-bound is likely to be a substantial overestimate. Estimates in Section V find evidence of induced demand, implying that either hybrids originated from less congested transportation options or drivers re-optimized their travel decisions and replaced exiting hybrids.

[54] Greenhouse gas emissions from induced new VMT (agents induced to drive by the absence of hybrid vehicles in the mainline) are calculated to be 300 tons, a small increase equivalent to the yearly emissions of 50 average fuel-economy vehicles. Similarly, NOx and hydrocarbon emissions increased by a slight 0.05 and 0.06 tons.

dollars per year.[55] Ultimately these rents are appropriated by three sets of agents, individuals who owned hybrids before the policy, individuals who purchased hybrids after the policy, and the manufacturers or dealers. For the two-thirds of stickers distributed to hybrids that were registered before the policy, the full rent of $743 was appropriated by the owner. For the remaining third going to new vehicles, the rent was divided between the new owner and the manufacturer. The supply constraint on vehicles during this time would suggest that manufacturers or dealers would be able to appropriate the rents via higher prices; however, Sallee (2011) finds that hybrid tax credits were appropriated by hybrid drivers and not the manufacturers or dealers. It is possible that hybrid drivers were similarly able to capture the rent generated by the stickers. Finally, nearly 22,000 carpoolers on the I-10W were affected by the CAVS policy, for an average cost per carpooler of $176 dollars per year. We highlight several key findings related to the distributional impacts: First, while the total congestion cost for carpoolers substantially outweighs hybrid benefits, the cost per individual carpooler is relatively small and less than the rents generated per sticker. Second, to the extent that hybrid owners are wealthier than the average carpooler, this policy is likely to be regressive. Finally, the benefits of the policy are concentrated to a small number of hybrids, even when the manufacturer or dealers captured one-third of the rents. These concentrated benefits and diffuse costs across a large number of carpoolers may have enabled the approval of the policy.

Third, we also calculate the costs of transferring $1 dollar to hybrid owners. The transfer ratio is at minimum 3.31 with a maximum of 8.87, implying a

[55] To validate our calculation of the benefit per sticker, we investigated what this value would imply for the premium a household is willing to pay for a hybrid with a sticker. Doubling the $743 dollars a year benefits (for a two-way commute) and privately discounting it (5 percent) over the initially proposed life of the policy gives a net present value of the sticker of $4,800 on the I-10W. Our implied estimate is similar to that presented in Shewmake and Jarvis (2011), who estimate an average premium of $3,200 for a stickered hybrid, as well as suggestions of a $3,000-$5,000 premium for a stickered hybrid from some in the auto industry (http://hffo.cuna.org/12433/ article/2599/html).

striking cost of roughly $3-9 dollars to transfer $1 dollar of benefit per hybrid.[56] Alternatively hybrid tax credits could be used to stimulate hybrid purchases. Such a credit could be financed through taxes, at a cost of approximately $1.40 per dollar transferred, at a standard labor tax marginal excess burden of 0.4 (Browning, 1987). While access to HOV lanes is very valuable, the large number of HOV lane users and heavy preexisting congestion implies substantial costs are created when transferring benefits to hybrids via the CAVS policy.

Finally, under the most optimistic scenario for the CAVS policy – every sticker stimulated a hybrid purchase and induced demand does not occur – we generate back-of-the-envelope-calculations of the statewide cost per ton of emission reductions. Because the I-10W is a particularly congested freeway, it may be inappropriate to simply apply the estimated welfare effects calculated above. Appendix B presents our methodology for the back-of-the-envelope calculation and sensitivity analysis on key parameters. We find that the best-case cost per ton of GHG emissions reductions is $124 per ton, with costs per ton of NOx and hydrocarbon reductions of $606,000 dollars and $505,000 dollars.[57] Even under this extremely generous scenario, this is roughly an order of magnitude larger than estimates of the marginal social cost of GHG emissions and substantially larger than other emission control options.[58]

[56] The lower bound of 3.31 assumes that hybrid drivers appropriated all the rents and includes congestion relief, while the upper bound of 8.87 assumes that manufacturers appropriated one third of the rent and excludes congestion relief. Using the end of policy estimates, the bounds increase to 4.20 and 11.26.

[57] To the extent that the end-of-policy estimates in Section V are suggestive of larger policy impacts on flow and thus congestion costs, scaling up the cost of GHG reductions accordingly yields a GHG cost per ton of $160. Accounting for the fact that it is unlikely that all stickers stimulated purchases of hybrid vehicles will raise the cost per ton; removing the two-thirds of stickers received by preregistered hybrids increases the GHG cost to $482 per ton. On the other hand, using mean estimates of the effect of early adoption on mainstreaming hybrid adoption (0.6 additional purchases of hybrids per early adopter) from Heutel and Muehlegger (2012), the cost per ton would fall to $310.

[58] California Assembly Bill 32 Scoping Plan is a comprehensive study of the cost of reducing greenhouse gases prepared by the California Air Resources Board. The Scoping Report considered a wide-range of policies, with estimated costs per ton of emissions ranging from -$300 for greenhouse gas standards for vehicles, to $300 for additional solar water heaters. Chandra, Gulati, and Kandlikar (2010) estimate that the cost of GHG emissions reductions from tax rebates for hybrid drivers was $195 per ton, while Li, Linn, and Spiller (2010) estimate that the Cash-for-Clunkers program reduced greenhouse gas emissions at a cost of $91-$301 per ton.

VII. Conclusion

This paper employs a regression discontinuity design to estimate the interaction effect between the Clean Air Vehicle Sticker policy and unpriced congestion in Los Angeles. Although policies that allow single-occupant hybrid vehicles in HOV lanes are viewed as a 'free' method to stimulate hybrid demand, we provide evidence that the CAVS policy in California resulted in substantial welfare losses. We show that the losses from the interaction between the policy and unpriced congestion overwhelmingly dominated the primary welfare gain from increased environmental benefits associated with the adoption of new hybrid vehicles. Our results also underscore the remarkable variation of the interaction effect across space and time. While adding a single hybrid to the HOV lane at 2AM creates zero social costs of congestion, adding one daily hybrid driver at 7AM on the I-10W generates $4500 in annual social costs.

When incorporated into a welfare analysis, our econometric estimates imply a best-case cost of $124 per ton for reductions in greenhouse gas emissions, $606,000 dollars per ton of nitrogen oxides (NOx) reduction, and $505,000 dollars per ton of hydrocarbon reduction in the most optimistic case. These are substantially above other readily available options to policymakers.

The results presented here have important implications for policy. Given the substantial cost of the CAVS policy, at a minimum it is worth considering alternative policies that may have achieved similar goals at lower cost. While a hybrid tax credit would cost taxpayers $1.40 per dollar transferred to hybrid owners, this cost increases to $3.31-$8.87 under the CAVS policy. Therefore tax credit incentives funded through distortionary taxes would be preferred to the CAVS policy despite its perception of being 'free.'

Earlier literature on environmental policy in a second best setting highlighted the superiority of revenue-raising instruments over nonrevenue raising instruments (Goulder, Parry, and Burtraw 1997). In our context, a natural option would be to auction the stickers to hybrid drivers. In this case, recycling auctioned revenues broadly by cutting preexisting distortions would only reduce the costs per dollar transferred to hybrid owners to \$2.91-\$8.47. If revenues were instead used to compensate the carpoolers in full, hybrid drivers' value of time would need to be in excess of \$200/hour to raise enough revenues to offset carpoolers for the value of lost travel time. Whether auctioned or not, the major source of the inefficiency of the CAVS stickers comes from the fact that this policy is 'blind' to the heterogeneity of the external costs of congestion across time and space.

Alternatively, policymakers could ration HOV access via congestion pricing. Ideally, a High Occupancy Toll (HOT) could consider both the external costs of congestion and air pollution. While the discount for hybrid vehicles would be invariant across space and time (roughly 0.7 cents/mile), the congestion fee itself would still adjust to reflect prevailing congestion conditions, as in Keeler and Small (1977). For example, on the I-10W during peak periods, the congestion fee would be roughly 45 cents/mile.[59] The fact that the congestion fee would be at least 60 times higher than the discount to hybrid vehicles underscores the significance of the interaction effect studied here.

Moving forward, policymakers are already replacing CAVS with new policies to promote the adoption of plug-in hybrids. Starting January 2012 40,000 stickers are being issued to plug-in hybrid vehicles allowing them to drive in HOV lanes. They have also allowed for an unlimited number of stickers for electric, 'zero emissions' vehicles. More broadly, our findings imply that, even if these vehicles were truly zero-emission, promoting their adoption at the expense of exacerbating congestion will still generate substantial welfare losses. In contrast, promoting the

[59] The discount per mile for hybrid drivers reflects the fact that the emissions reduction benefits they generate are time and space invariant. The congestion fee per mile varies with the level of congestion, and is calculated as the cost per mile delay imposed on other drivers.

use of buses in HOV lanes, although a far less celebrated technology, may represent the win-win in terms of pollution and congestion that policymakers were hoping with the CAVS policy.

REFERENCES

Anas, Alex, Richard Arnott, and Kenneth Small. 1998. "Urban Spatial Structure." *Journal of Economic Literature,* 36(3): 1426-1464

Arnott, Richard, André de Palma, and Robin Lindsey. 1993. "A Structural Model of Peak-Period Congestion: A Traffic Bottleneck with Elastic Demand." *American Economic Review* 83(1): 161-179.

Auffhammer, Maximilian, and Ryan Kellogg. 2011. "Clearing the Air? The Effects of Gasoline Content Regulation on Air Quality." *American Economic Review,* 101(6): 2687-2722.

Bento, Antonio, and Mark Jacobsen. 2007. "Ricardian Rents, Environmental Policy and the 'Double-Dividend' Hypothesis." *Journal of Environmental Economics and Management,* 53(1): 17-31.

Bovenberg, A. Lans, and Lawrence H. Goulder. 1996. "Optimal Environmental Taxation in the Presence of Other Taxes: General Equilibrium Analyses." *American Economic Review,* 86(4): 985-1000.

Browning, Edgar K. 1987. "On the Marginal Welfare Cost of Taxation."*American Economic Review* 77(1): 11–23

Burger, Nicholas, and Daniel Kaffine. 2009. "Gas Prices, Traffic, and Freeway Speeds in Los Angeles." *Review of Economics and Statistics*, 91(3): 652-657.

Chandra, Ambarish, Sumeet Gulati, and Milind Kandlikar. 2010. "Green Drivers or Free Riders? An Analysis of Tax Rebates for Hybrid Vehicles." *Journal of Environmental Economics and Management,* 60(2): 78-93.

Davis, Lucas W. 2008. “The Effect of Driving Restrictions on Air Quality in Mexico City.”*Journal of Political Economy,* 116(1): 38-81.

DiNardo, John, and David S. Lee. 2004. “Economic Impacts of New Unionization on Private Sector Employers: 1984–2001.” *Quarterly Journal of Economics,* 119(4): 1383–1441.

Diamond, David. 2009. “The Impact of Government Incentives for Hybrid-Electric Vehicles: Evidence from US States.” *Energy Policy*, 37(3): 972-983.

Downs, Anthony. 1962. “The Law of Peak-Hour Expressway Congestion.” *Traffic Quarterly,* 16(3):393–409.

Duranton, Giles, and Matthew Turner. 2011 “The Fundamental Law of Road Congestion: Evidence from US Cities.” *American Economic Review,* 101(6): 2616-2652.

Environmental Protection Agency. 2007. *Inventory of U.S. Greenhouse Gas Emissions and Sinks: 1990–2005.* EPA 430-R-07-002

Gallagher, Kelly, and Erich J. Muehlegger. 2011. “Giving Green to Get Green? Incentives and Consumer Adoption of Hybrid Vehicle Technology.”*Journal of Environmental Economics and Management,* 61(1): 1-15.

Goulder, Lawrence H., Ian W.H. Parry, and Dallas Burtraw. 1997. “Revenue-Raising vs. Other Approaches to Environmental Protection: The Critical Significance of Pre-Existing Tax Distortions.” *RAND Journal of Economics,* 28(4): 708-31.

Greene, David. 2010. “How Consumers Value Fuel Economy: A Literature Review.” Office of Transportation and Air Quality, U.S. Environmental Protection Agency, Report EPA-420-R-10-008.

Hahn, Jinyong, Petra Todd, and Wilbert Van der Klaauw. 2001."Identification and Estimation of Treatment Effects with a Regression Discontinuity Design." *Econometrica,* 69: 201–209.

Harberger, Arnold. 1974. *Taxation and Welfare.* Chicago: University of Chicago Press.

Helfand, Gloria, and Ann Wolverton. 2011. "Evaluating the Consumer Response to Fuel Economy: A Review of the Literature." *International Review of Environmental and Resource Economics,* 5(2): 103-146.

Hymel, Kent M., Kenneth A. Small, and Kurt Van Dender. 2010. "Induced Demand and Rebound Effects in Road Transport." *Transportation Research B,* 44(10): 1220-1241.

Imbens, Guido W., and Karthik Kalyanaraman. 2011. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *Review of Economic Studies,* forthcoming.

Imbens, Guido W., and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics,* 142(2): 615-635.

Keeler, Theodore E., and Kenneth A. Small. 1977. "Optimal Peak-Load Pricing, Investment and Service Levels on Urban Expressways." *Journal of Political Economy*, 85(1): 1-25.

Lee, David S., and Thomas Lemieux. 2009. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48(2): 281-355.

Li, Shanjun, Joshua Linn, and Elisheba Spiller. 2010. "Evaluating "Cash-for-Clunkers": Program Effect on Auto Sales, Jobs and the Environment." Resources for the Future Discussion Papers, August 2010.

Matsudaira, Jordan. 2008. "Mandatory Summer School and Student

Achievement." *Journal of Econometrics,* 142(2): 829-850.

Parry, Ian W.H., and Kenneth A. Small. 2005. "Does Britain or the United States Have the Right Gasoline Tax?" *American Economic Review,* 95(4): 1276-1289.

Pigou, Arthur C. 1920. *Economics of Welfare*. 4th Edition 1932. London: Macmillan.

Sallee, James. 2011. "The Surprising Incidence of Tax Credits for the Toyota Prius." *American Economic Journal: Economic Policy*, 3(2): 189-219.

Shewmake, Sharon, and Lovell S. Jarvis. 2011. "Hybrid Cars and HOV Lanes." Working paper. http://ssrn.com/abstract=1826382

Shrank, David, and Tim Lomax. 2005. *The 2005 Urban Mobility Report.* Texas Transportation Institute, Texas A&M University System.

Small, Kenneth A. 1982. "The Scheduling of Consumer Activities: Work Trips." *American Economic Review*, 72(3): 467-479.

Small, Kenneth A., and Camilla Kazimi. 1995 "On the Costs of Air Pollution from Motor Vehicles." *Journal of Transport Economics and Policy*, 29(1): 7-32.

Small, Kenneth A., Clifford Winston, and Jia Yan. 2005. "Uncovering the Distribution of Motorists' Preferences for Travel Time and Reliability." *Econometrica*, 73(4): 1367–1382.

Vickrey, William S. 1969. "Congestion Theory and Transport Investment." *American Economic Review Papers and Proc,* 59 (2): 251–60.

West, Sarah E., and Roberton C. Williams III. 2007. "Optimal Taxation and Cross-Price Effects on Labor Supply: Estimates of the Optimal Gasoline Tax." *Journal of Public Economics*, 91(1): 593-617.

MAP 1.PEMS DETECTORS IN DISTRICT 7

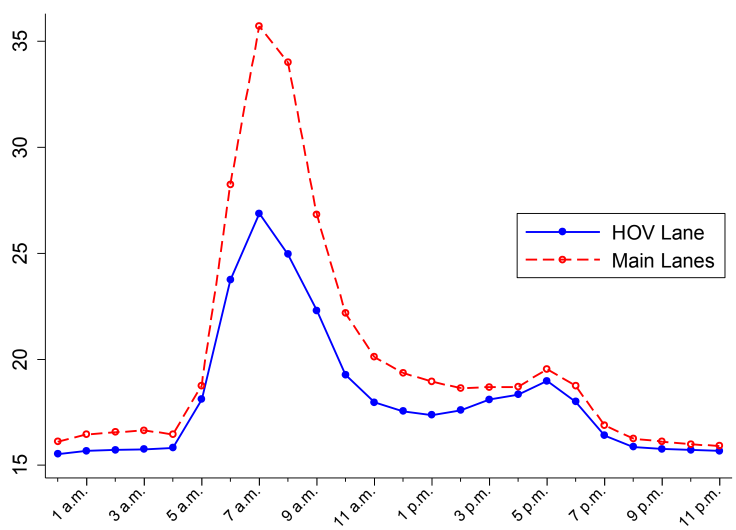

FIGURE 1.AVERAGE TRAVEL TIME BY HOUR

Notes: The figure displays the average hourly travel time from 2004-2007 in the indicated lane for each hour of the day on the I-10 W. Weekends and holidays as well as the day before and after a holiday are dropped.

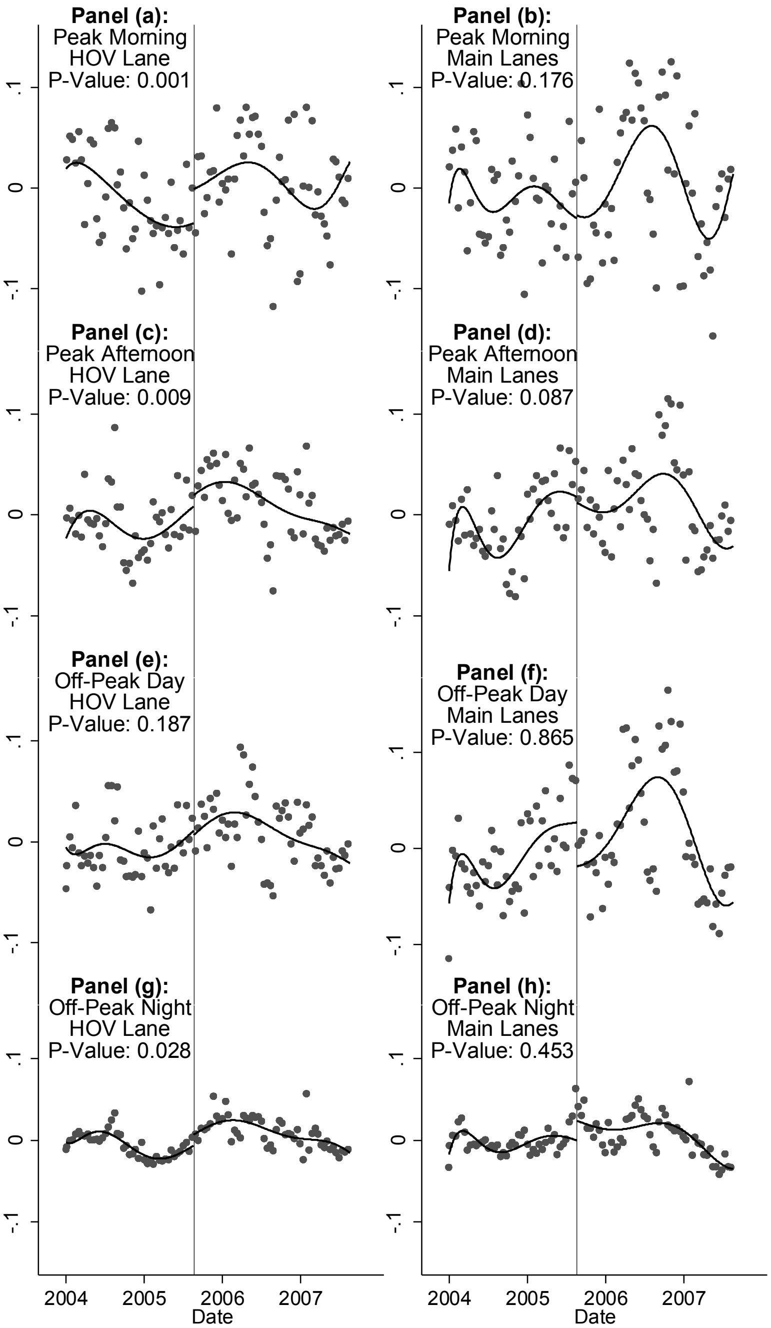

Figure 2. Interstate 10 West Travel Time

Notes: Values plotted are biweekly averaged hourly residuals of a regression of log(travel time) for the stated lane during the stated time of day on logged gas price, travel time for the 210 W, dummies for day of the week-month, dummies for hour of the day, quadratic rainfall, quadratic visibility, and five dummies for sky cover. Fitted lines are the predicted values obtained after regressing the residuals on hybrid exemption policy dummies and an eighth-order polynomial on date.

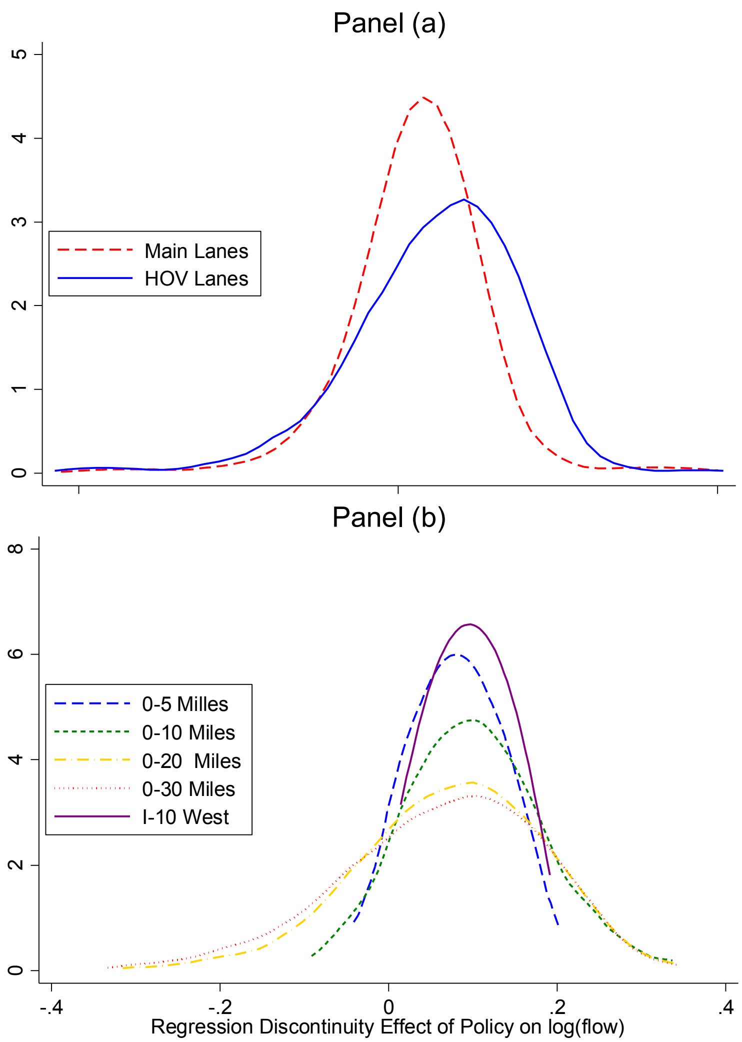

FIGURE 3.DISTRIBUTION OF DETECTOR LEVEL RD ESTIMATES FOR FLOW

Notes: The figure displays the smoothed distribution of local linear regression discontinuity detector estimates. Panel (a) plots the distribution for all HOV and mainline detectors. Panel (b) plots the distribution for HOV detectors for various distances from the CBD. The smoother uses an Epanechnikov kernel with a bandwidth of 0.05. Effects estimated from a regression of log(flow) on logged gas price, dummies for day of the week, dummies for hour of the day, quadratic rainfall, quadratic visibility, and five dummies for sky cover using a 30-day bandwidth and an Epanechnikov kernel.

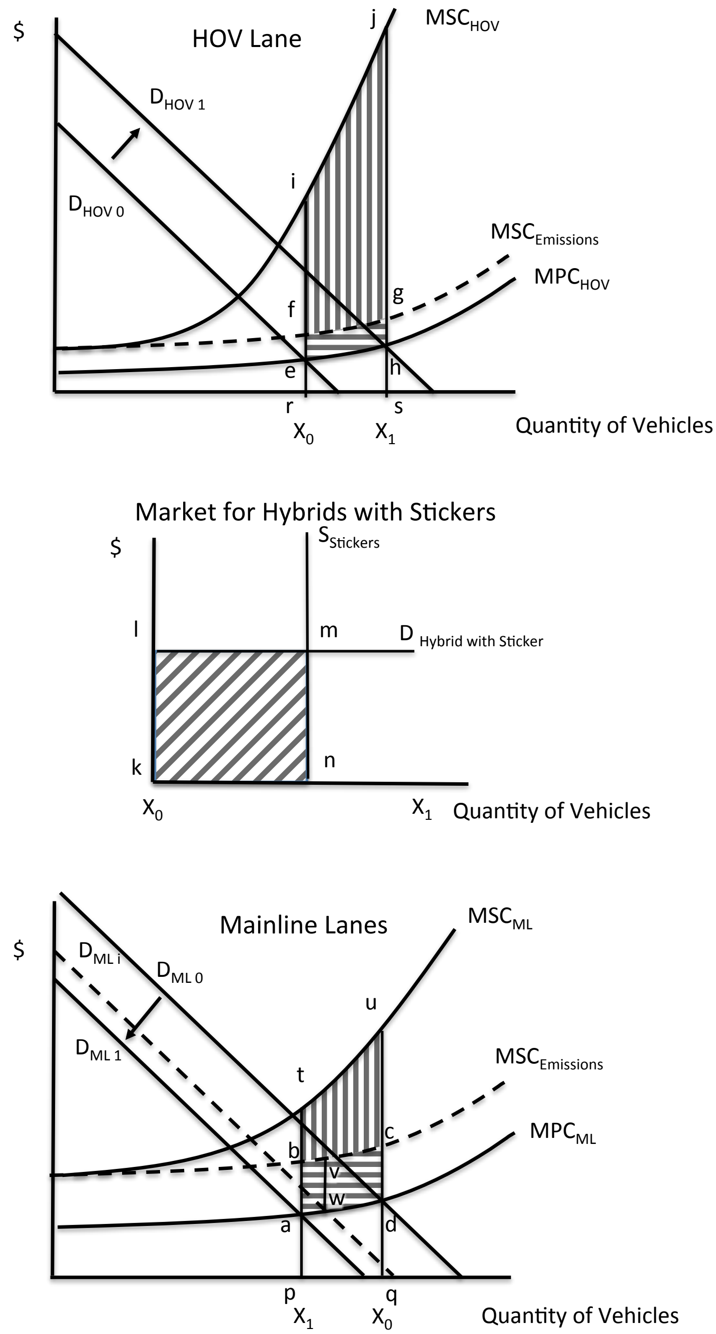

FIGURE 4. WELFARE EFFECTS OF THE CAVS POLICY

TABLE 1—REGRESSION DISCONTINUITY ESTIMATES: GLOBAL POLYNOMIAL RESULTS

	I	II	III	IV	V	VI
Polynomial order	6	7	8	9	10	BIC
			Morning peak			
CAVS policy/ HOV	0.088***	0.084***	0.090***	0.090***	0.116***	0.116***
	(0.026)	(0.028)	(0.028)	(0.027)	(0.026)	(0.026)
CAVS policy/ Mainline	-0.011	-0.022	0.060	0.060	0.070*	0.060
	(0.049)	(0.053)	(0.051)	(0.051)	(0.049)	(0.051)
Observations	4944	4944	4944	4944	4944	4944
P-Value for Induced Demand[a]	0.823	0.989	0.112	0.146	0.052	0.093
P-Value for Test of Difference between HOV and Mainline[b]	0.084	0.088	0.607	0.770	0.410	0.332
			Afternoon peak			
CAVS policy/ HOV	0.088***	0.084***	0.090***	0.090***	0.116***	0.116***
	(0.026)	(0.028)	(0.028)	(0.027)	(0.026)	(0.026)
CAVS policy/ Mainline	-0.001	0.004	0.059*	0.059*	0.055	0.059*
	(0.031)	(0.028)	(0.036)	(0.036)	(0.037)	(0.036)
Observations	3952	3952	3952	3952	3952	3952
P-Value for Induced Demand[a]	0.640	0.472	0.051	0.051	0.062	0.046
P-Value for Test of Difference between HOV and Mainline[b]	0.100	0.087	0.963	0.973	0.782	0.934
			Mid-day off-peak			
CAVS policy/ HOV	0.048**	0.050**	0.027	0.027	0.041	0.041**
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
CAVS policy/ Mainline	-0.064	-0.063	-0.008	-0.009	-0.007	-0.009
	(0.042)	(0.042)	(0.049)	(0.051)	(0.051)	(0.050)
Observations	5927	5927	5927	5927	5927	5927
P-Value for Induced Demand[a]	0.227	0.240	0.975	0.969	0.955	0.975
P-Value for Test of Difference between HOV and Mainline[b]	0.021	0.021	0.511	0.518	0.386	0.363
			Night off-peak			
CAVS policy/ HOV	0.033***	0.033***	0.016**	0.016**	0.022***	0.022***
	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)
CAVS policy/ Mainline	-0.008	-0.005	0.016	0.017	0.023	0.023
	(0.019)	(0.017)	(0.022)	(0.021)	(0.021)	(0.021)
Observations	8894	8894	8894	8894	8894	8894
P-Value for Induced Demand[a]	0.976	0.836	0.361	0.343	0.197	0.197
P-Value for Test of Difference between HOV and Mainline[b]	0.062	0.055	0.971	0.964	0.992	0.992

Notes: Values shown are the coefficients from 48 separate regressions of log(travel time) by lane on the regressands. Standard errors, clustered by week, are in parentheses. Covariates include logged gas price, travel time for the I-210W, dummies for day of the week-month, dummies for hour of the day, quadratics in rainfall and visibility, and five dummies for sky cover. R^2 ranges from 0.34 to 0.71 in HOV lane, and 0.41 to 0.76 in mainline lanes. Weekends and holidays, as well as the day before and after a holiday, are dropped. The polynomial orders chosen by the BIC in column VI are: morning peak HOV 10, morning peak M.L 8, afternoon peak HOV 5, afternoon peak ML 8, mid-day off-peak HOV 10, mid-day off-peak ML 9, night off-peak HOV 10, night off-peak ML 10.

[a] This test is calculated as the difference between the observed mainline estimate and the null hypothesis where all hybrids originated in the mainline and no replacement occurred due to induced demand. This null is calculated as the coefficient on the HOV lane divided by 4.

[b] This conservative test does not include the covariance between the two estimates, which would further increase the significance.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE 2—HOURLY GLOBAL POLYNOMIAL REGRESSION DISCONTINUITY ESTIMATES

	I	II	III	IV	V	VI
	Morning peak					
	5 A.M.	6 A.M.	7 A.M.	8 A.M.	9 A.M.	10 A.M.[a]
Policy (HOV)	0.027	0.088**	0.096**	0.125**	0.122	0.072*
	(0.025)	(0.040)	(0.042)	(0.037)	(0.038)	(0.038)
Observations	989	989	989	988	989	988
	Afternoon peak					
	4 P.M.	5 P.M.	6 P.M.	7 P.M.	8 P.M.[a]	
Policy (HOV)	0.045	0.060**	0.082***	0.038	0.007	
	(0.027)	(0.024)	(0.028)	(0.025)	(0.017)	
Observations	988	988	988	988	988	

Notes: Values shown are the coefficients from 11 separate regressions of log(travel time) in the HOV lane by hour on the regressands. Standard errors, clustered by week, are in parentheses. Covariates include an 8th order polynomial trend in time, logged gas price, travel time for the I-210W, dummies for day of the week-month, dummies for hour of the day, quadratics in rainfall and visibility, and five dummies for sky cover. R^2 ranges from 0.28 to 0.59. Weekends and holidays, as well as the day before and after a holiday, are dropped.

[a] Not part of the official peak.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE 3—REGRESSION DISCONTINUITY ESTIMATES: FURTHER ROBUSTNESS CHECKS

	I	II	III	IV	V	VI
	Morning peak					
	True Date	Placebo		Weekend	Macroeconomic	
Policy (HOV)	0.090***	-0.006	-0.073*	0.002	0.091***	0.102***
	(0.028)	(0.034)	(0.039)	(0.014)	(0.027)	(0.035)
Observations	4944	4944	4944	1980	4944	
Policy	0.060	-0.008	-0.024	0.011	0.060	0.048
	(0.045)	(0.032)	(0.060)	(0.020)	(0.045)	(0.036)
Observations	4944	4944	4944	1980	4944	9888
	Afternoon peak					
	True Date	Placebo		Weekend	Macroeconomic	
Policy (HOV)	0.057***	-0.032	-0.006	0.023	0.056***	0.081***
	(0.022)	(0.025)	(0.029)	(0.043)	(0.021)	(0.026)
Observations	3952	3952	3952	1584	3952	
Policy	0.059*	-0.042**	0.040	0.023	0.059*	0.034
	(0.035)	(0.021)	(0.048)	(0.046)	(0.034)	(0.027)
Observations	3952	3952	3952	1584	3952	7904
	Mid-day off-peak					
	True Date	Placebo		Weekend	Macroeconomic	
Policy (HOV)	0.027	-0.017	-0.002	-0.020	0.027	0.028
	(0.021)	(0.027)	(0.030)	(0.031)	(0.021)	(0.031)
Observations	5928	5928	5928	2376	5928	
Policy	-0.008	-0.028	0.048	-0.020	-0.008	-0.009
	(0.049)	(0.026)	(0.058)	(0.038)	(0.048)	(0.036)
Observations	5927	5927	5927	2376	5927	11855
	Night off-peak					
	True Date	Placebo		Weekend	Macroeconomic	
Policy (HOV)	0.016**	-0.003	-0.008	0.008	0.015**	0.039***
	(0.007)	(0.010)	(0.012)	(0.019)	(0.007)	(0.02)
Observations	8894	8894	8894	3554	8894	
Policy	0.016	-0.005	-0.014	0.002	0.017	-0.007
	(0.022)	(0.006)	(0.022)	(0.021)	(0.021)	(0.014)
Observations	8894	8894	8894	3554	8894	17788
Policy date	8/20/05	8/20/04	8/20/06	8/20/05	8/20/05	8/20/05
L.A. Unemployment	N	N	N	N	Y	Y
Pooled, single	N	N	N	N	N	Y

Notes: Values shown are the coefficients from 44 separate regressions of log(travel time) by lane on the regressands. Standard errors clustered by week are in parentheses. Covariates include an 8th order polynomial trend in time, logged gas price, lagged travel time for the I-210W, dummies for day of the week-month, dummies for hour of the day, quadratics in rainfall and visibility, five dummies for sky cover, and, where noted, the monthly unemployment rate for Los Angeles. R^2 ranges from 0.34 to 0.71 in HOV lane, and 0.41 to 0.76 in mainline lanes. Holidays, as well as the day before and after a holiday, are dropped. 'Weekend' specifications include only observations from weekends. ‘Placebo’ specifications use only weekday observations with policies starting one year before the true policy and one year after the true policy.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE 4—AVERAGE OF LOCAL LINEAR TREATMENT EFFECTS BY DISTANCE FROM CBD: DETECTOR LEVEL FLOW ESTIMATES

	I	II	III	IV
Distance from CBD	0-10	10-20	20-30	0-30
	Start of Policy			
CAVS policy/ HOV	0.091**	0.058**	-0.009	0.055**
	(0.028)	(0.029)	(0.102)	(0.025)
Detectors[a]	50	124	26	200
Observations[b]	8,297	20,662	4,110	33,069
Flow[c]	915	845	686	842
CAVS policy/ Mainline	0.015	0.008	0.021	0.012
	(0.017)	(0.030)	(0.022)	(0.017)
Detectors[a]	152	254	71	477
Observations[b]	24,461	40,863	10,682	76,006
Flow[c]	5,108	5,248	4,616	5,109
P-value test of difference in HOV and Mainline	2.4%	13.3%	35.5%	19.2%
Implied Number of Vehicles Removed from Mainline[d]	-83	-49	6	-46
Mainline Null Hypothesis without Induced Demand[e]	-0.016	-0.009	0.001	-0.009
P-value for test of Induced Demand[f]	7.8%	55.9%	39.8%	21.9%
	End of Policy			
CAVS policy/ HOV	0.088***	0.048*	0.116***	0.071***
	(0.025)	(0.024)	(0.043)	(0.018)
Detectors[a]	21	46	15	82
Observations[b]	3,579	7,677	2,409	13,665
Flow[c]	917	885	557	833
CAVS policy/ Mainline	0.003	0.008	0.012	0.007
	(0.008)	(0.005)	(0.013)	(0.005)
Detectors[a]	74	131	44	249
Observations[b]	12,674	22,441	7,040	42,155
Flow[c]	5,078	5,195	4,568	5,049
P-value test of difference in HOV and Mainline	2.4%	13.3%	35.5%	19.2%
Implied Number of Vehicles Removed from Mainline[d]	-81	-42	-65	-59
Mainline Null Hypothesis without Induced Demand[e]	-0.016	-0.008	-0.014	-0.012
P-value for test of Induced Demand[f]	2.5%	0.4%	5.5%	0.1%

Notes: Values shown are the means of policy coefficients on detectors within the stated number of miles from the city center for the indicated lane from local linear regressions of log traffic flow on the regressands. Standard errors of the means are given in parentheses. These are calculated by drawing a detector level effect from each detector's estimated mean and standard deviation and are block-bootstrap sampled 5,000 times at the route-direction level. Covariates for individual detector regressions include logged gas price, dummies for day of the week, dummies for hour of the day, quadratics in rainfall and visibility, and five dummies for sky cover. Weather variables are aggregated using the averaging across all stations. Weekends are dropped from the analysis. An Epanechnikov kernel and 30-day bandwidth is used in all regressions. Detector-level regressions include only those observations where Percent Observed is 100%. A minimum of 50 observations per detector are required after all deletions. Effects are reported for peak time of day when maximum traffic flow occurs. The End of Policy regressions omit two extreme outlier days May 30, 2011 and July 21, 2011. To aid in comparability across time we restrict the End of Policy detectors to be the same sample of detectors present at the Start of Policy. Route I-10W detectors between 3.3 and 19.2 miles from downtown LA give estimates of 0.096 in the HOV lane and 0.004 in the mainline lane.

[a] Detectors is the number of detectors within the listed distance from the CBD that comprise each average treatment effect.

[b] The total number of observations entering the detector level regressions.

[c] Flow is the number of cars passing the average detector entering the regressions.

[d] The implied increase in flow on the average HOV lane detector, i.e. the CAVS policy multiplied by flow.

[e] The implied percentage decrease in flow that would be observed if the hybrids entering the HOV lane had originated in the mainline lanes.

[f] Test of the difference between the observed mainline effect and the induced demand null. This test incorporates the HOV lane and mainline standard errors but, conservatively, we do not include any covariance of these estimates.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE 5—ADDITIONAL WELFARE ANALYSIS PARAMETERS

	Value	Source
I-10W route length	17.5 miles	PeMS
Value of time (Hybrid drivers)	$32.86	Small, Winston, and Yan (2005)
Value of time (Carpoolers)	$20.87	Small, Winston, and Yan (2005)
I-10W HOV lane occupancy per vehicle	3.1	Caltrans
I-10W mainline occupancy per vehicle	1.1	Caltrans
Elasticity of new VMT (short-run)	0.15	Hymel, Small, and Van Dender (2010)
Hybrid fuel efficiency	45 mpg	EPA
Fleet fuel efficiency	20 mpg	EPA
Hybrid NOx emissions per mile	0.02 grams	CA SULEV standards
Hybrid hydrocarbon emissions per mile	0.01 grams	CA SULEV standards
Fleet NOx emissions per mile	0.07 grams	EPA Tier II standards
Fleet hydrocarbon emissions per mile	0.09 grams	EPA Tier II standards
Marginal social damage of GHG emissions	$21/ton	US Interagency Working Group
Marginal social damage of NOx emissions	$15,000/ton	Small and Kazimi (1995)
Marginal social damage of hydrocarbon emissions	$4,100	Small and Kazimi (1995)

Notes: This table lists the additional parameter values used to supplement our estimates of the travel time and traffic flow in order to determine the implications of the CAVS policy on distributional impacts and greenhouse gas emissions. VOT estimates from Small, Winston, and Yan (2005) are income adjusted.

TABLE 6—WELFARE EFFECTS OF THE CAVS POLICY FOR THE I-10 WEST

Panel A Welfare Effects	Annual	Present Value
Primary welfare gain	$28,127	$283,449
Cost-side congestion interaction effect	-$3,990,620 [-$7,024,200, -$936,100]	-$21,495,008
Rent effect	$671,882 [$186,650, $1,153,750]	$2,363,240
System-wide congestion interaction effect	$1,744,620 [$423,500, $3,057,000]	$9,397,209
Emissions interaction effect	-$7240	-$38,998
Net welfare effect of the CAVS policy for the I-10W	-$1,553,225 [-$2,776,700, -$321,200]	-$8,366,274
Excluding system-wide congestion interaction effect	-$3,297,846 [-$5,829,700, -$750,000]	-$17,763,483
Panel B Distributional Effects		
Carpoolers using I-10W HOV lane (daily)	21,943	-
Hybrids using I-10W HOV lane (daily)	904 [192, 1615]	-
Congestion cost per carpooler	-$176	-$948
Rent per sticker[a]	$743	$4,002
Transfer ratio- lower bound	3.31 [2.86, 3.46]	-
Transfer ratio- upper bound	8.87 [8.09, 9.07]	-

Notes: Central estimates - peak hours. Present value calculated over the policy or hybrid vehicle lifetime. Transfer ratio defined as the cost of transferring $1 dollar to hybrid drivers. Author calculations.

[a] Calculation represents benefits of HOV lane access per sticker. Two-thirds of stickers went to hybrids purchased prior to the policy, whose drivers likely received this full benefit. However, the benefits associated with the remaining stickers may have been appropriated by agents other than hybrid drivers.