UNIVERSITY OF CALIFORNIA,
IRVINE

# Essays on Transportation Externalities

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Cody Scott Nehiba

Dissertation Committee:
Professor Linda Cohen, Chair
Professor Jan Brueckner
Professor Matthew Freedman
Professor David Brownstone

2019

# DEDICATION

To my family and friends

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# CURRICULUM VITAE

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## Research Papers

### *Peer-Reviewed Publications*

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### *Under Review and Working Papers*

"Correcting heterogeneous externalities: Evidence from local fuel price controls"

"Choking on oil: Production severance taxes and well-level extraction"

"Taxed to Death? The effects of diesel taxes on freight truck collisions"

"The effects of employee hours-of-service regulations on the U.S. airline industry" (with A. Luttmann)

### *Works in Progress*

"The time-of-day travel demand elasticity paradox"

"Circadian rhythms and collisions: Rest and duty hour regulations and freight truck-involved collisions"

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| US Association of Energy Economics North America Meeting, Annual Meetings of the Public Choice Society, Western Economics Association International Annual Meeting, UCI Transportation, Urban, and IO Seminar, UCI IO/Corporate Welfare Workshop (x2), UCI Job Market Seminar, Pacific Southwest Region University Transportation Center Advisory Council (poster), UCI Economics Poster Session | 2018 |
| CU Environmental and Resource Economics Workshop, International Transportation Economics Association Annual Conference, S4C Colloquium Velo-City, UCI Transportation, Urban, and Environmental Seminar, UCI Economics Poster Session | 2017 |
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## Research Experience

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# ABSTRACT OF THE DISSERTATION


Essays on Transportation Externalities

By

Cody Scott Nehiba

Doctor of Philosophy in Economics

University of California, Irvine, 2019

Professor Linda Cohen, Chair



This dissertation concerns the measurement and regulation of externalities with a focus on the numerous and interrelated external costs in the transportation sector. The research touches on pollution, congestion, and collisions as well as various modes of transportation. The results have important implications for public policy and the regulation of externalities both within and outside of transportation.

Chapter 1 investigates the effectiveness of traffic laws which require drivers to provide a minimum amount of distance between their vehicle and cyclists when passing them on roadways in improving cyclist safety. Many believe these laws are ineffective in reducing the number of bicyclist fatalities because they are difficult for police to enforce, contain loopholes, and the minimum distance required is inadequate. This chapter tests this claim empirically using data on 18,534 bicyclist fatalities from the Fatality Analysis Reporting System and a differences-in-differences approach, in a negative binomial model, to identify the effect of minimum distance passing laws on bicyclist fatalities. The analysis fails to find a significant effect of enacting a minimum distance passing law on the number of cyclist fatalities after controlling for differences in weather, demographics, bicycling commuter rates, state level traffic, and time variation.

Chapter 2 examines the effects of freight truck weight and miles traveled on both the quantity and severity of truck-involved collisions using a unique panel data set covering the universe of truck-

involved collisions and 3.5 billion truck-weight observations. Estimates reveal that, though both measures of trucking activity can increase collisions, increases in truck weight skew the distribution of collisions towards more severe outcomes involving either injury or death. The results are applied to a welfare analysis of diesel fuel taxes, which have been shown to both reduce truck miles traveled and increase truck cargo weight. Though diesel taxes are shown to slightly reduce the total number of collisions, the remaining collisions become more severe. Societal gains from pollution, congestion, and collision reductions are entirely offset by the increased fatal collision costs, reducing total welfare by $39.9 billion annually.

The final chapter examines the regulation of heterogeneous externalities. When demand for or damages from an externality producing good vary, uniform policy instruments are an inefficient tool for correcting market failures. This chapter examines how a policy that reflects this heterogeneity can improve efficiency. County travel demand elasticities and congestion damages are estimated to compare the efficiency of a uniform fuel tax to county-specific fuel taxes. Because elasticities, congestion damages, and pollution damages exhibit heterogeneity across regions, county-specific fuel taxes, levied in a subset of major metropolitan areas, provide welfare gains between $7-$26 per capita annually in addition to equity gains relative to a revenue neutral uniform fuel tax.

# Chapter 1

# Give me 3’: Do minimum distance passing laws reduce bicyclist fatalities?

## 1.1 Introduction

Policy makers have long sought to reduce the external pollution and congestion costs created by automobiles. Estimates have put air pollution costs at \$0.03 per mile and congestion costs at \$0.065 per mile (Small and Kazimi, 1995; Fischer et al., 2007). While many policies aim to make vehicles more fuel efficient or divert traffic to off-peak times, reductions in these costs could also occur by promoting alternative modes of transportation. Persuading a portion of the US population to switch from automobiles to bicycles could drastically decrease these external costs.

Though bicycling exhibited the largest percentage increase in modal split share of all commuting modes between 2000 and 2012, the number of bicyclists in the US is still modest (McKenzie, 2014). To alleviate pollution and congestion through increases in bicyclist numbers, policy makers must make bicycling an appealing substitute for automobiles. While bicycles and vehicles differ along many dimensions, it appears that many marginal bicycle users are deterred by the dangers of bicycling. When asked what factors prevent them from bicycling, individuals consistently report safety concerns are a major deterrent (Goldsmith, 1992). Bicyclists are some of the most vulnerable users of roadways, and, as their numbers have increased, safely integrating them onto roadways has become a major concern. Recognizing this, many states are seeking to make bicyclists safer to encourage more bicycle use.

One solution used in recent years by state legislatures to improve bicyclist safety is minimum distance passing laws (MDPLs). To date, 26 states and several cities have passed laws that require motorists to leave a minimum amount of distance between their vehicle and bicyclists when overtaking them on roadways (NCSL, 2015). The first of these laws was passed in Wisconsin in 1974, but 24 of these 26 states have enacted MDPLs since April 2000. Generally, the laws stipulate drivers may not come within 3 feet of bicyclists while passing.

Many welcome these laws as much needed passing guidelines, but others, including some bicycle advocacy groups, question their effectiveness (Brown et al., 2013). Skeptics argue that vague language, loopholes, inadequate minimum distance, and the difficulty police officers have enforcing such laws means they have little or no impact on cyclist safety.

The actual effect of MDPLs on bicyclist safety has remained ambiguous in the absence of substantive evidence. This paper uses data on 18,534 bicyclist fatalities to empirically test the impact of MDPLs on this aspect of bicyclist safety. The identification strategy utilizes a differences-in-differences approach which compares fatalities in states with MDPLs to those without MDPLs and the variation in timing of state level MDPL enactment to examine their effect while controlling for other factors.

The results, which are insensitive to numerous robustness and endogeneity checks, fail to find a statistically significant effect of MDPLs on bicyclist fatalities. Further, MDPLs do not appear to have an economically significant effect on bicyclist fatalities either as the estimates within the 95% confidence interval predict, at best, a reduction of 1 bicyclist fatality per state every 20.41 months after MDPL enactment. These results suggest that current MDPLs are ineffective in reducing bicyclist fatalities.

The remainder of this paper is organized as follows. The next section introduces where this paper fits in the related literature. Section 3 provides an in depth background on MDPLs. Section 4.1 describes the data, section 4.2 the empirical methods and identification assumptions, section 5.1 the main results, and section 5.2 provides robustness checks. Section 5.3 discusses limitations of the empirical work, and section 6 concludes with a brief overview of the policy implications.

## 1.2 Literature Review

One branch of existing literature on MDPLs has focused on driver compliance with these laws. Though this research doesn't test the impact of MDPLs on bicyclist safety, it provides important information about why these laws may or may not be effective.

Love et al. observed vehicles passing five bicyclists over 10.8 hours of riding in Baltimore, Maryland approximately one year after a MDPL took effect (Love et al., 2012). The results suggest MDPLs may not be effective as a sizable portion of Baltimore's population does not abide by the MDPL with 89 of the 586 recorded passes occurring within 3 feet or less of cyclists (Love et al., 2012). A second study of a MDPL in Queensland Australia also found a sizable portion of the population did not comply with the new law(Schramm et al., 2016).

In addition, there has been some preliminary analysis of the effect of the Queensland MDPL on bicyclist safety. This work looks at bicyclist fatalities before and after the passage of the law in Queensland, but due to the short time frame of the study, small number of bicyclist fatalities (33 in total), and the lack of a control group, the authors cannot infer the effect of MDPLs (Schramm et al., 2016).

Another literature examines cyclist safety perceptions finding that many individuals believe bicycling is too dangerous. It has been shown that the perceived danger associated with bicycling plays a large role in deterring new riders (Fernández-Heredia et al., 2014). Thus, an increase in cycling after a MDPL is enacted is expected if it makes cyclists safer, or makes them *feel* safer. There is some evidence that providing an additional buffer between cars and cyclists increases the number of cyclists. Habib *et al.* (2014) find that the factor with the largest positive effect on bicycle use in Toronto is providing on-street and protected bike lanes.

This paper also adds to the literature on the impact of legislation on road safety. In this literature, MDPLs are particularly related to distracted driving laws (texting and handheld phone bans while driving) as both are subject to enforcement and loophole concerns. The effectiveness of distracted driving laws has been questioned as they can be difficult to enforce and many contain loopholes that could render them useless. The literature has not reached a consensus on the impact of distracted driving laws on crashes or fatal crashes, with results ranging from no effect to significant decreases.

Recent contributions have found that texting and driving bans decrease single vehicle single occupant fatalities if the laws are sufficiently strict (Abouk and Adams, 2013). It has also been shown that this reduction may diminish quickly over time, implying drivers may simply be reacting to the announcement of the ban (Abouk and Adams, 2013). Other researchers argue that distracted driving laws have zero effect because of enforcement issues, low compliance rates, and driver heterogeneity (Burger et al., 2014; Prieger and Hahn, 2005).

This strictness requirement and persistence issue are of particular concern when examining MDPLs. There are only slight variations in the strictness of MDPLs, so it is possible that most may be too weak to significantly reduce fatalities. It is also possible that the effect of passing laws could diminish quickly after passage due to lack of enforcement, or it may take months for the law to become effective if officers must learn how to enforce it.

Broadly, this work connects existing research on MDPLs and bicyclist safety to literature on the quantitative analysis of road safety policies. This is done by performing a nation-wide analysis of state MDPLs using econometric methodology.

## 1.3 Policy Context

Motor vehicles and bicycles routinely travel at differential speeds which frequently leads to overtaking maneuvers when drivers and bicyclists meet. These interactions become more dangerous as the distance between bicyclists and vehicles decreases not only because the margin of error for direct collisions decreases, but also because automobiles may generate enough air displacement to affect a cyclists balance at close proximity (Kahn and Bacchus, 1995). MDPLs are intended to increase the distance between vehicles and cyclists to lower the probability of a collision.[1]

Prior to the passage of Wisconsin's MDPL in 1974, no well established rules outlining how to pass bicyclists on roadways existed. Formalizing bicyclist passing requirements caught on slowly, as can be seen in Figure 1.1. However, in the past 16 years, policy makers, motivated by a desire to improve safety and increase the number of bicyclists, have passed MDPLs in 24 more states. Although MDPLs are enacted with good intentions there are several common critiques including: insufficient distance required, loopholes in the legislation, and lack of enforcement.

MDPLs usually require motorists to provide 3 feet of space between their vehicle and cyclists when passing on roadways. Bicycle advocacy groups argue this is not enough space.[2] Some believe setting the minimum distance at 3 feet actually makes cyclists worse off (Brown et al., 2013). They argue the law effectively sets a benchmark that leads drivers who previously gave bicyclists a wider berth to adjust their behavior to provide only the minimum required distance.

Loopholes in the legislation allowing motorists to drive closer than the minimum distance specified

[1] MDPLs are one of many possible safety measures policy makers can enact. These include protected bike lanes, bicycle helmet laws, and strict liability laws that find drivers at fault in all automobile-bicycle collisions.

[2] Advocacy groups prefer MDPLs like South Dakota's which requires 4 feet of space (NCSL, 2015).

if they slow to a safe speed may also undercut the effect of MDPLs. For example, California's law allows for passing with less than 3 feet if the driver slows to a "reasonable and prudent" speed (CA State Legislature, 2013). This wording leaves the determination of reasonable and prudent speeds entirely up to the vehicle's driver, who likely has a different interpretation of reasonable and prudent than the bicyclist being passed. This wording also undermines police officers' ability to effectively enforce the law.

Another enforcement problem also exists. If police officers are trained in bicyclist traffic laws at all, it is often only secondary training provided by local bicycle advocacy groups.[3] This means many officers are inexperienced in handling bicyclists, and often do not monitor bicycling areas. If the laws are not being enforced, they are likely not an effective deterrent(Schramm et al., 2016).

## 1.4 Empirical Analysis

### 1.4.1 Data

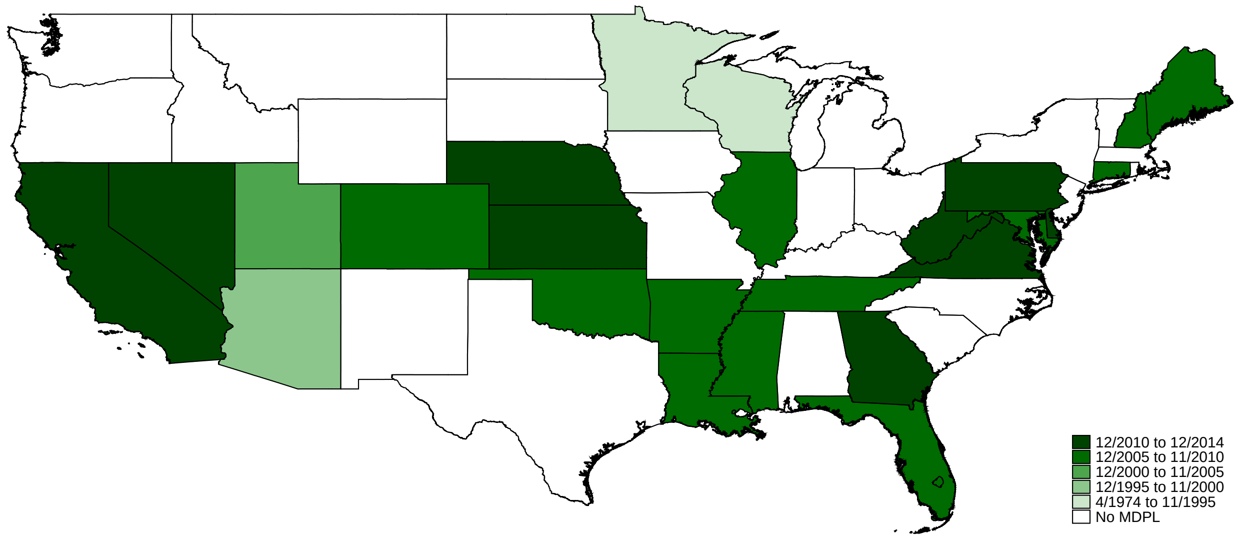
A unique panel data set was created for the empirical estimation of this paper. Information about traffic fatalities (driver, bicyclist, pedestrian, etc.) was provided by the Fatal Accident Reporting System (FARS) of the National Highway Traffic Safety Administration (NHTSA) for the years 1990-2014.

Although data on all crashes (fatal or not) that occurred while a motorist was overtaking a bicyclist would provide a clearer picture of cyclist safety, serious reporting concerns exist.[4] Importantly, it is possible that the rate of under-reporting may change after the passage of a MDPL as the costs of

[3]According to records provided by the League of American Bicyclists, new officers were trained in bicycling enforcement as part of the police academy curriculum in only 23 states in 2014. In the same year, officers in 31 states were provided with secondary bicycling enforcement training by bicycle advocacy groups.

[4]For example, in New York City, up to a third of bicycle collisions that required hospitalization were not recorded in traffic collision databases (Brustman, 1999).

Figure 1.1: Effective Date of Minimum Distance Passing Laws




Notes: While only 3 states enact MDPLs prior to 2000, the numbers sharply increase after this point. The majority of MDPLs are past after 2005. Hawaii and Alaska are not depicted; neither state has enacted a MDPL.

reporting have increased (due to increased fines). As this type of behavior would bias estimates of the effect of MDPL, FARS fatality data is used in lieu of data on all bicyclist collisions.

The effective dates of minimum distance passing laws were collected from state legislature archives, and cross referenced using news reports and information provided by The League of American Bicyclists. Figure 1.1 illustrates the effective month of MDPLs by state.[5] In total, 23 states and the District of Columbia enacted MDPLs during the sample period, while Wisconsin did so in 1974.[6]

State level monthly average temperature and total precipitation data were provided by the National Oceanic and Atmospheric Administration. The temperature variable is placed into 5 degree bins as the analysis uses a quadratic temperature term, and there are several states that have negative average temperature observations.

[5]Effective dates: AZ 4/2000, AR 3/2007, CA 9/2013, CO 5/2009, CT 10/2008, DE 7/2011, DC 10/2013, FL 10/2006, GA 7/2011, IL 8/2007, KS 7/2011, LA 9/2009, ME 6/2007, MD 10/2010, MN 4/1995, MS 7/2010, NE 8/2012, NV 5/2011, NH 1/2009, OK 6/2006, PA 2/2012, TN 5/2007, UT 4/2005, VA 7/2014, WV 6/2014, WI 4/1974

[6]WI enacted a MDPL over 20 years before any other state, which causes some concern about how similar Wisconsin is to the other treatment states. Removing WI from the sample does not significantly alter the results.

Unemployment and labor force data were gathered from the Bureau of Labor Statistics, while monthly population estimates, and annual age, sex, and median income estimates were obtained from the Census Bureau. State level annual estimates of the share of commuters using bicycle as a mode of transportation from 2005-2014 were obtained from the American Community Survey.

Monthly state means and percentages for all variables are presented in Table C.1. This information is split into control and treatment state groups, with the treatment groups further separated into all months, pre-law months, and post-law months. Table C.1 illustrates several interesting state characteristics, the most notable being states that enact a passing law, on average, have higher bicyclist fatalities than the control states. However, this is true for pedestrian, driver, and total fatalities as well, and is likely due to the treatment states being more populous.

Table C.1 also shows that monthly bicyclist fatalities decrease after minimum distance passing laws are enacted. While it is possible this decrease is caused by MDPLs, there is a downward trend in all fatality types in the treatment states.

### 1.4.2 Estimation Strategy and Identifying Assumptions

The model explains the count of bicyclist fatalities in state, $i$, and month, $t$, as a function of minimum distance passing laws and control variables, $x$, described in the previous section,

$$FATALITIES_{i,t} = f(MDPL_{i,t}, x_{i,t}).$$

A count model is used as the dependent variable takes the form of non-negative integers. The frequency of monthly bicyclist fatality counts can be seen in Figure 1.2. Despite many zero observations in the data, there are no theoretical reasons to suspect the data generating process to differ between zero and non-zero observations which precludes the use of hurdle and zero-inflated models, and deems negative binomial or Poisson models more appropriate.

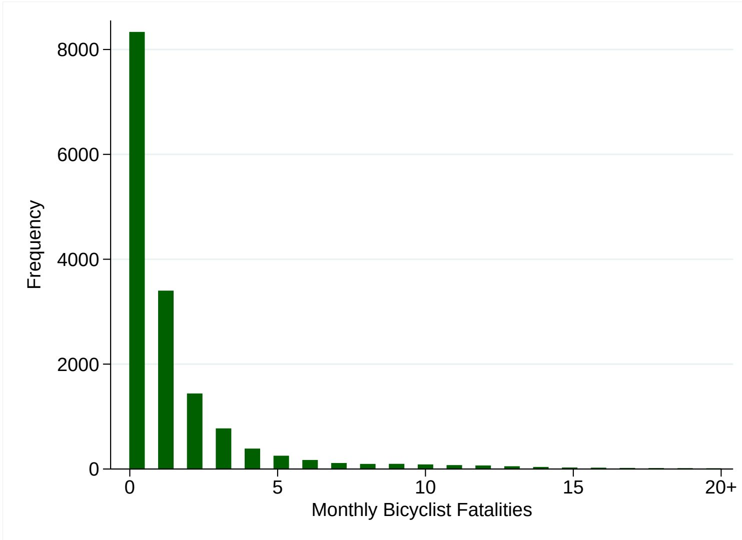
Table 1.1: Descriptive Statistics

Monthly State Means or Percentages

| | | Treatment States | | |
|---|---|---|---|---|
| | Control States | Total | Pre-law | Post-law |
| Bicyclist fatalities | 0.97 | 1.45 | 1.46 | 1.33 |
| Driver fatalities | 37.05 | 42.2 | 43.58 | 37.68 |
| Pedestrian fatalities | 7.26 | 9.12 | 9.46 | 7.44 |
| All traffic fatalities | 53.75 | 62.75 | 65.17 | 54.08 |
| MDPL | 0 | 0.26 | 0 | 1 |
| Temperature | 50.65 | 54.31 | 54.29 | 52.69 |
| Precipitation (in.) | 3.36 | 3.21 | 3.27 | 2.96 |
| Population (millions) | 4.02 | 4.53 | 4.52 | 4.49 |
| Share male | 0.48 | 0.48 | 0.48 | 0.48 |
| Unemployment rate | 0.06 | 0.06 | 0.06 | 0.06 |
| Median income | 54.00 | 54.84 | 55.53 | 53.20 |
| Share commuting by bicycle | 0.002 | 0.002 | 0.001 | 0.005 |

Notes: All traffic fatalities is not limited to bicycle, driver, and pedestrian fatalities. Average temperature is reported in degrees Fahrenheit, unemployment rate has been seasonally adjusted, median income is adjusted to thousands of 2014 dollars.

Figure 1.2: Frequency of monthly bicyclist fatality counts




Notes: Monthly bicyclist fatalities has a median and mode of zero while the mean is 1.211. The final bin contains observations of 20 or more monthly fatalities.

A key assumption of the Poisson model is equidispersion: the conditional mean, $\mu$, must be equal to the conditional variance, $var(y_{it}|x_{it})$. Stated explicitly, $\mathbb{E}(y_{it}|x_{it}) = var(y_{it}|x_{it}) = \mu_{it}$, where $y_{it}$ is the dependent variable (Cameron and Trivedi, 2013a). In this work overdispersion, $\mu_{it} < var(y_{it}|x_{it})$, is present in the data. The negative binomial model corrects for overdispersion without any further adjustments, while the Poisson model requires moving away from complete distributional assumptions to handle overdispersion (Cameron and Trivedi, 2013b). Further, the negative binomial collapses to a Poisson model when the estimated overdispersion parameter, $\alpha$, is zero (Cameron and Trivedi, 2013a). Negative binomial regression is thus chosen to estimate the model.[7]

The probability density function of the model[8] is then,

[7]The results of the paper do not qualitatively change when the Poisson model is used in place of the negative binomial, see Appendix Table A.2. More explicitly, the "NB2" model discussed in more detail in Cameron and Trivedi (2013a) is used.

[8]A derivation of the likelihood function from this density can be found in (Cameron and Trivedi, 2013a)

$$f(y_{it}|\mu_{it},\alpha_i) = \frac{\Gamma(y_{it}+\alpha_i^{-1})}{\Gamma(y_{it}+1)\Gamma(\alpha_i^{-1})}\left(\frac{\alpha_i^{-1}}{\alpha_i^{-1}+\mu_{it}}\right)^{\alpha_i^{-1}}\left(\frac{\mu_{it}}{\alpha_i^{-1}+\mu_{it}}\right)^{y_{it}},$$

where $\Gamma$ is the Gamma function, the overdispersion parameter is $\alpha_i > 0$, the mean is specified as $\mu_{it} = exp(\delta_i + \Psi_t + MDPL_{it}\Omega + x'_{it}\beta)$ with $\delta_i$ being a state fixed-effect, $\Psi_t$ being season fixed-effects, and $y_{it} = 1, 2, 3, ...$ (Cameron and Trivedi, 2013a; Allison and Waterman, 2002).

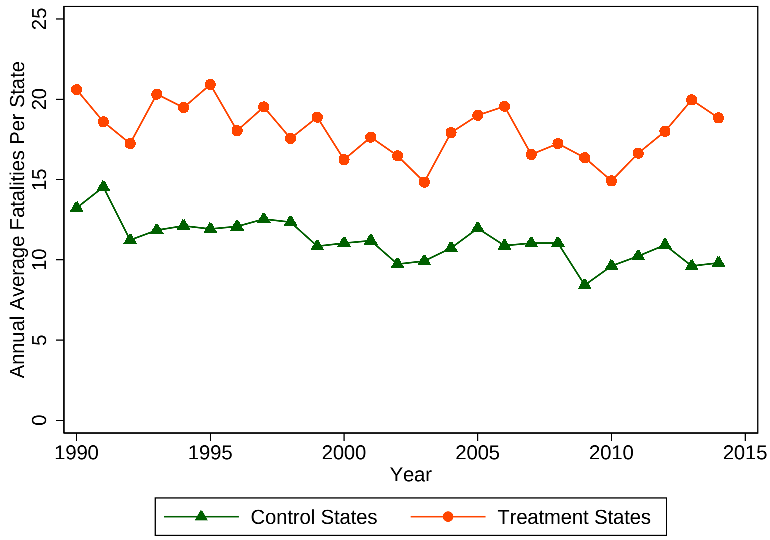
Due to the panel structure of the data, a grouped error term is a potential problem in this analysis (Bertrand et al., 2004). Because the observations within a state are likely correlated with each other over time, the standard errors are clustered at the state level. This clustering assumes that the errors are independent between states, but correlated within the state.

A differences-in-differences (DID) approach is used to identify the effect of MDPLs on cyclist fatalities. The identification of the effect of MDPLs relies on the assumption that states that do not enact a MDPL are comparable to states that do enact a MDPL conditional on the fixed-effects and time varying controls included (Angrist and Pischke, 2008). That is to say, in the absence of a MDPL, changes in cyclist fatalities must be determined by the controls and fixed-effects in the model. This means changes outside the trend are caused by the enactment of MDPLs. The inclusion of state fixed-effects means that this assumption is violated only if trends vary differentially over time for states with and without MDPLs. Further, the goal of this paper is to isolate the effect of MDPLs on bicyclist fatalities independent of state-level fluctuations in bicyclists. For this reason, controls that have been shown in the literature to influence the number of cyclists or collisions, such as temperature, precipitation, unemployment, etc., are included in the regressions (Leard and Roth, 2017).[9]

A visual inspection of bicyclist fatalities over time, illustrated in Figure 1.3, suggests the trends are the same over time, with fatalities in the control and treatment states following very similar trends,

[9] Failing to include such controls could potentially bias the estimated effect of MDPLs if, for example, temperatures (and thus the number of cyclists) change differentially in treated and control states.

Figure 1.3: Annual Average Bicyclist Fatalities Per State




Notes: Averages are constructed by summing all fatalities in the control or treatment states in a given year and then dividing by the number of states in that group, 26 for control and 25 for treatment (the total of 51 is due to the inclusion of D.C.

albeit with the treatment states experiencing more volatility.

However, this visual inspection is not conclusive, and it is still possible that state and time trends that violate this assumption could emerge. To control for this, season fixed-effects, state trends, and time trend variables are included in the model.[10] Following the literature, the state trend is modeled as linear deviations from the monthly national mean of cyclist fatalities (Shirley and Gelman, 2015). The time trend is incorporated as a simple linear month trend.

[10]Though fixed-effects and trend controls should eliminate any violations of the identification assumptions a selection problem may persist. MDPLs may have been enacted in states that policy makers believed they would be effective and not enacted in states where they would be ineffective. If this were true, in a counter-factual situation where the control states enacted MDPLs the effect of the laws might be even smaller. By this reasoning the insignificant results found in the treatments states would also apply to the control states.

## 1.5 The effect of MDPL on bicyclist fatalities

### 1.5.1 Main Results

The empirical work begins in Table 1.2 by estimating several baseline specifications of the reduced form model. Coefficients can be interpreted as semi-elasticities.[11] Column 1 gives results from a negative binomial regression that includes only the MDPL variable and fixed-effects.[12] Column 2 then adds weather and demographic controls while column 3 adds trend controls.

MDPLs have a positive effect on bicyclist fatalities in all specifications. Put simply, the model predicts MDPLs may *increase* bicyclist fatalities. There are several reasons this may occur. First, there is a possibility that the MDPL coefficient suffers from an endogeneity bias. This would occur if MDPLs are positively correlated with unobserved state characteristics that vary over time (for example, bicyclist miles traveled, lack of bicycle lanes compared to control states, or a spike in bicyclist fatalities before MDPL passage), that increase bicyclist fatalities. This would cause an upward bias on the MDPL coefficient. However, this is only problematic to the extent that these unobservables evolve differently over time in the treatment states relative to the control states as the inclusion of state fixed-effects controls for initial levels of these variables. While these factors cannot be controlled for explicitly, numerous robustness checks are performed in the next section to investigate the possibility of upward bias, all of which suggest that this is not the case.

There are also several plausible reasons to believe this positive coefficient may be estimated correctly, and MDPLs may in fact slightly increase bicyclist fatalities. MDPLs may effectively set a benchmark that could lead drivers who previously gave cyclists a much wider berth to adjust their behavior to provide only the minimum required distance. Another possibility is that MDPLs make

[11] This interpretation utilizies incident rate ratios. A commonly used rule of thumb approximation is $exp(1*x) \approx 1+x$ if $x < .1$ (Prieger and Hahn, 2005).

[12] Season fixed-effects are used throughout the body of this work, but the main results do not change if month or year fixed-effects are used. See Appendix Table A.1. Further, the results are insensitive to the use of Poisson Regression, or censoring of the dependent variable. See Appendix Table A.2

cyclists feel safer, but have no effect on driver behavior. This could potentially lead to increased cyclist numbers, less cautious bicycle riding, or both.[13]

The MDPL variable becomes insignificant when controls for weather and demographics are added. This indicates MDPLs either have no effect, or an effect so small these specifications cannot precisely estimate it. More information about the size of the MDPL effect can be gleaned by examining the effects within the 95% confidence interval. For example, column 3 estimates a decrease in bicyclist fatalities of more than 4.12% can be ruled out. Further investigation of the other covariates can help determine if this effect is estimated correctly.[14]

The weather controls behave as predicted. Precipitation decreases bicyclist fatalities, but its quadratic term is insignificant. Temperature should have a non-linear relationship with bicyclist fatalities as people enjoy bicycling in warm temperatures, but if it becomes too hot they will avoid riding. Temperature has a positive coefficient while the quadratic temperature variable has a negative effect on bicyclist fatalities, confirming a concave relationship between fatalities and temperature.[15] As temperature increases bicyclist fatalities also increase, but at a decreasing rate.

A rise in unemployment significantly predicts decreases in bicyclist fatalities after trends have been controlled for. The unemployment rate can be thought of as a proxy for vehicle miles traveled; if unemployment is high, fewer people will be commuting and traveling (Cotti and Tefft, 2011). It is also possible that an increase in the unemployment rate would increase the number of people bicycling, as it is a low cost substitute for automobiles.

Population is a strong predictor of cyclist fatalities before the addition of the control variables. This loss in significance is expected as the control and trend variables are correlated to population trends.

[13] Controlling for changes in the number of cyclists is discussed more in the next subsection.

[14] If the other explanatory variables are well behaved it is less likely that the specifications suffer from severe omitted variable bias.

[15] When interpreting the coefficient on the temperature variable, it is important to remember temperature has been binned into groups (1°-5°,6°-10°,..).

As previously stated, the assumptions of the identification strategy are violated if any trends in bicyclist fatalities that are not accounted for exist. To control for this several different trend variables are utilized (other types of traffic fatalities, state, and time), but their addition does not change the results.

### 1.5.2 Robustness Checks

This subsection examines the robustness of the baseline estimates. In particular, the precision of the estimates, the possibility of MDPL endogeneity, and time varying effects are discussed.

Though the effect of MDPLs has not been statistically significant in any specification with controls, it is consistently positive, small, and has a relatively precisely standard error. Despite the small standard errors reported, this model may not have the statistical power to detect the effect of MDPLs if it is very small. To increase the statistical power, a random-effects negative binomial model is also estimated. Random-effects models have fewer free parameters to estimate than their fixed-effect counterpart, which could potentially yield tighter estimates and produce statistically significant results.

In column 3 of Table 1.3, state level random-effects replace state fixed-effects. The state random-effects specifications do estimate the effect of MDPLs with more precision than full fixed-effects models, but this does not lead to significant estimates of the MDPL coefficient. According to these results, any effect that MDPLs have would be infinitesimal given the size of the standard errors.

Total bicyclist miles traveled are not recorded precisely by any agency in the US, and, as such, the baseline specifications do not directly control for the amount of bicycling in a state. Though state level fixed effects control for the initial level of bicyclist miles traveled in a state, if this variable changes differentially over time between treatment and control states the identification assumptions will be violated. This introduces two endogeneity problems to the analysis. As discussed

Table 1.2: Baseline Specifications

Dependent Variable: Count of bicyclist fatalities in a state per month

| | (1) | (2) | (3) |
|---|---|---|---|
| MDPL | 0.0783** | 0.0393 | 0.0307 |
| | (0.0360) | (0.0408) | (0.0364) |
| Temperature | | 0.7055*** | 0.5450*** |
| | | (0.0526) | (0.1290) |
| Precipitation | | -0.0481 | -0.0613*** |
| | | (0.0335) | (0.0223) |
| Temperature$^2$ | | -0.0186*** | -0.0147*** |
| | | (0.0016) | (0.0042) |
| Precipitation$^2$ | | -0.0008 | -0.0022 |
| | | (0.0008) | (0.0020) |
| Precipitation*Temp | | 0.0007 | 0.0016*** |
| | | (0.0005) | (0.0005) |
| Unemployment Rate | | 0.1366 | -0.8342*** |
| | | (0.4553) | (0.3095) |
| *ln*(Population) | | 0.5826*** | 0.4370 |
| | | (0.1822) | (0.2710) |
| Pedestrian Fatalities | | | 0.0014 |
| | | | (0.0016) |
| Driver Fatalities | | | -0.0019 |
| | | | (0.0014) |
| State Trend | | | -0.1986* |
| | | | (0.1095) |
| Time Trend | | | 0.0058 |
| | | | (0.0100) |
| Observations | 15,300 | 15,300 | 15,300 |
| State FE | YES | YES | YES |
| Seasonal FE | YES | YES | YES |
| $\alpha$ | 0.0692 | 0.0081 | 0.0065 |
| Pseudo $R^2$ | 0.271 | 0.295 | 0.374 |

Notes: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

previously, bicyclist miles traveled in a state may increase after MDPL enactment. Therefore, bicyclist fatalities per mile biked may have decreased after enactment, but the change is unobserved due to the effect of MDPLs being offset by increases in bicycle miles traveled. Second, the choice to enact a MDPL may be influenced by unobservable state characteristics, namely states that enact MDPLs may be experiencing more rapid increases in bicyclist miles traveled than control states, and correspondingly high increases of bicyclist fatalities. Both of these problems would lead to upward bias on the MDPL coefficient. As a first endeavor to address these issues, Table 1.3 includes several proxies for the number of bicyclists in a state.[16]

Column 1 of Table 1.3 adds state median annual income to the analysis. It has been reported that an individual's income influences their decision of whether or not to bicycle, with individuals with very high or very low incomes being the most likely to bicycle (McKenzie, 2014). In addition, high incomes are positively correlated with commuting by bicycle (Caulfield, 2014). Therefore, changes in a state's median income to either end of the distribution may be correlated with higher numbers of bicyclists. Income is a marginally significant predictor of bicyclist fatalities in one specification, but the size of the effect is essentially zero.

Column 1 also adds the percent of the population that is male to the model. Past studies indicated that males are much more likely to commute via bicycle than females, so larger male shares of the population may be correlated with more bicyclists (McKenzie, 2014). The coefficient is always positive and significant, indicating states with a higher male share of the population experience more bicyclist fatalities. This signals percent male may be a good proxy for bicyclist miles traveled.

Age is a likely determinant of bicycle use for a multitude of reasons. While Caulfield (2014) shows the probability of commuting to work by bicycle increases with age, it is likely that very old individuals are less frequent bicyclists, and very young individuals are not likely to be affected by MDPLs as most of their bicycling is done off of busy roadways (Caulfield, 2014). This is

[16] All of the variables added in Table 1.3 (percent male, age distributions, and median income) are provided at the annual level, so there is less variation over time for these predictors.

controlled for by including state age distributions in column 2.[17] Only a few of these age bins are significant predictors of bicyclist fatalities (coefficients suppressed for convenience), so they are not likely to be effective proxies for bicyclist miles traveled.

Including the proxies discussed has not qualitatively changed the results for the MDPL variable.[18] Though the percent of the population male and median income (in one specification) are significant predictors of bicyclist fatalities, the model still estimates that MDPLs have a statistically insignificant effect. This is suggestive evidence that endogeneity, as discussed above, is not biasing the results.

While detailed accounts of bicyclist miles traveled are unavailable, the American Community Survey does produce annual estimates of the share of commuters that use bicycle to travel to work in each state. Table 1.4 shows results from models that include an exposure term for the estimated number of bicyclist commuters in a state. This exposure term is created using data on the unemployment rate, labor force size, and the bicyclist commuter share information discussed above. While the ACS data are annual estimates of state bicycle commuter rates, monthly unemployment and labor force size are used in the estimates, so the count of bicyclist commuters varies by month. Because this variable is a measure of exposure (the number of bicyclist fatalities is directly related to the number of bicyclist in a state), the coefficient is constrained to equal unity (Cameron and Trivedi, 2013a).

The ACS bicycle commuter data is only available from 2005-2014, so a large portion of the panel is dropped in these specifications.[19] Because of this, specifications with more covariates than those in Table 1.4 fail to converge.

[17] *ln*(Population) is still included in specifications with age controls because the frequency of observations differ between the two variables. The population estimates are at the month level, while the age distribution estimates are at the annual level. Though a multicollinearity problem may exist, including both sets of variables does not have a large influence on the standard errors.

[18] Several other proxies are also considered in Appendix Table A.3.

[19] Appendix Table A.4 uses multiple imputations to estimate pre-2005 data and shows that the results are robust to this shortcoming.

Table 1.3: Proxy and Random-Effects Specifications

| Dependent Variable: Count of bicyclist fatalities in a state per month | | | |
|---|---|---|---|
| | Fixed-effects | | Random-effects |
| | (1) | (2) | (3) |
| MDPL | 0.0338 | 0.0363 | 0.0061 |
| | (0.0378) | (0.0358) | (0.0331) |
| Percent Male | 0.8688** | 1.1430*** | 1.1011*** |
| | (0.4135) | (0.4264) | (0.3843) |
| Median Income | 0.0067* | 0.0059 | 0.0022 |
| | (0.0040) | (0.0037) | (0.0032) |
| Observations | 15,300 | 15,300 | 15,300 |
| Controls | YES | YES | YES |
| Trends | YES | YES | YES |
| Age Controls | NO | YES | YES |
| State FE | YES | YES | NO |
| State RE | NO | NO | YES |
| Seasonal FE | YES | YES | YES |
| $\alpha$ | 0.0063 | 0.0045 | - |
| Pseudo $R^2$ | 0.374 | 0.375 | - |

Notes: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1. "Controls" includes: temperature, $(\text{temperature})^2$, precipitation, $(\text{precipitation})^2$, precipitation*temperature interaction, unemployment rate, and *ln*(population). "Trends" includes: state trends and pedestrian and driver fatalities.

Table 1.4: Specifications With Exposure

Dependent Variable: Count of bicyclist fatalities in a state per month

| | Fixed-effects | | | Random-effects |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| MDPL | 0.0588 | 0.0599 | 0.0605 | 0.0661 |
| | (0.0546) | (0.0557) | (0.0554) | (0.0471) |
| Exposure | 1 | 1 | 1 | 1 |
| | - | - | - | - |
| Pedestrian Fatalities | | 0.0027 | 0.0027 | 0.0027 |
| | | (0.0025) | (0.0025) | (0.0018) |
| Driver Fatalities | | 0.0006 | 0.0006 | 0.0011 |
| | | (0.0006) | (0.0006) | (0.0008) |
| Percent Male | | | -0.0913 | -0.0839 |
| | | | (0.9238) | (0.8766) |
| Observations | 6,120 | 6,120 | 6,120 | 6,120 |
| Controls | YES | YES | YES | YES |
| State FE | YES | YES | YES | NO |
| State RE | NO | NO | NO | YES |
| Seasonal FE | YES | YES | YES | YES |
| $\alpha$ | 4.44e-07 | 3.59e-07 | 2.85e-07 | - |
| Pseudo $R^2$ | 0.182 | 0.183 | 0.183 | - |

Notes: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1. "Exposure" is the estimated number of bicyclist commuters in a state. "Controls" includes: temperature, $(\text{temperature})^2$, precipitation, $(\text{precipitation})^2$, precipitation*temperature interaction, and unemployment rate.

Despite these issues, the results from models that include the exposure term provide an excellent robustness check. Columns 1-3 of Table 1.4 show results from fixed-effects models while column 4 is a state random-effects model. The main result remains unchanged; the MDPL is still statistically insignificant in all specifications, again suggesting that bicyclist miles traveled changed in a similar manner for treatment and control states. Interestingly, percent male becomes statistically insignificant in columns 3 and 4. This provides additional support that percent male is a strong proxy for bicyclist miles traveled.

As discussed earlier, a state legislature's decision to enact a MDPL may be influenced by the

Table 1.5: Effect of Count of Bicyclist Commuters on Probability of MDPL

| Dependent Variable: MDPL | | |
|---|---|---|
| | (1) | (2) |
| | Probit | Probit |
| Exposure | -0.0536*** | -0.0752*** |
| | (0.0099) | (0.0147) |
| Observations | 6,120 | 6,120 |
| Controls | YES | YES |
| Trends | NO | YES |
| Age Controls | NO | YES |
| State FE | NO | NO |
| Seasonal FE | YES | YES |
| Pseudo $R^2$ | 0.121 | 0.236 |

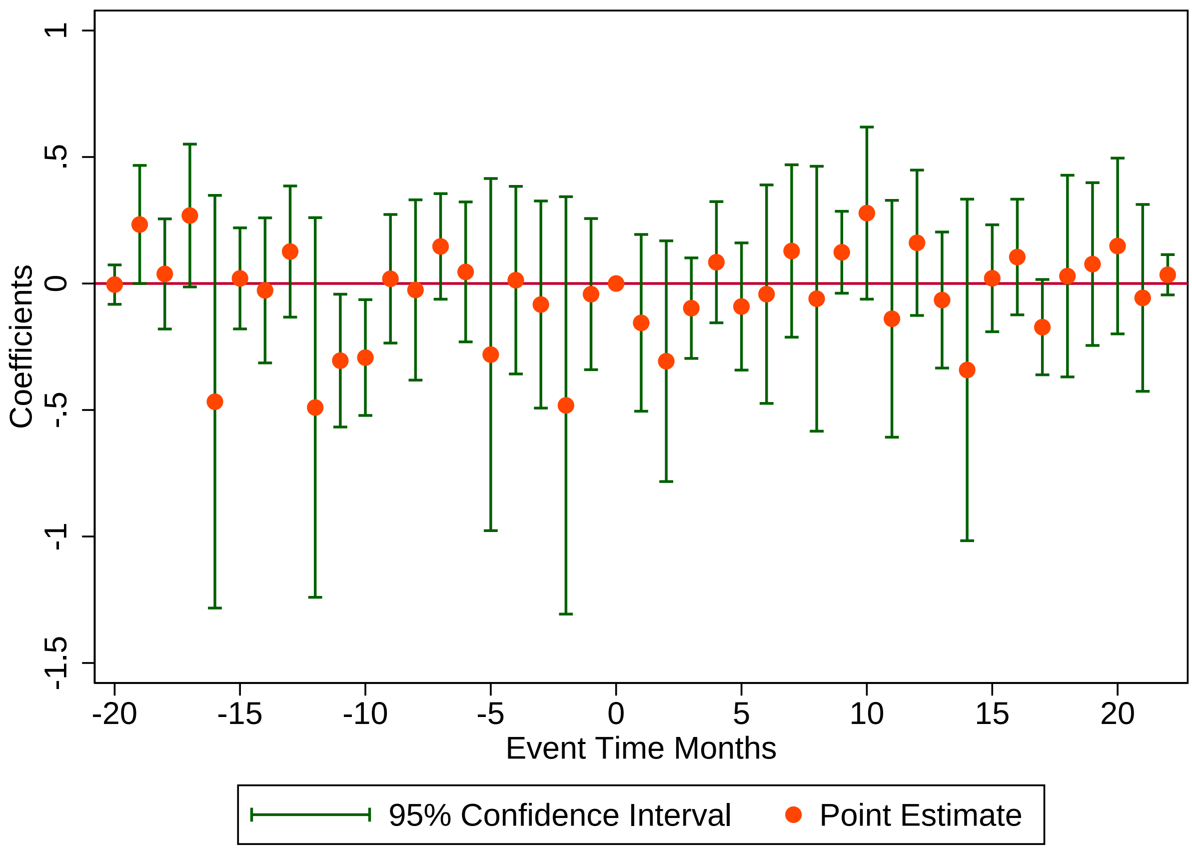
Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. "Exposure" is the estimated number of bicyclist commuters in a state measured in tens of thousands. "Controls" includes: temperature, (temperature)$^2$, precipitation, (precipitation)$^2$, precipitation*temperature interaction, unemployment rate, and *ln*(population). "Trends" includes: state and time trends.

number of bicyclists or bicyclist fatalities a state experiences. Large numbers of bicyclists and bicyclist fatalities this period are correlated with high numbers of bicyclists and bicyclist fatalities in future periods, and if MDPL enactment is based on either of these factors the MDPL coefficient is likely biased upwards. Two methods are employed to investigate this possibility. First, a probit model is estimated regressing MDPL on the count of bicyclist commuters in a state (measured in tens of thousands). This is somewhat like a "first stage" of an instrumental variables approach, used to see if high numbers of bicyclists lead to MDPL enactment. The results, in Table 1.5, show that the opposite is true. The model shows that increasing bicyclist commuters decreases the probability that a state has a MDPL. Further, studying the relationship between the date of MDPL enactment and count of bicyclist commuters on that date reveals a positive correlation, implying that states with fewer bicyclist commuters were the first to pass MDPLs. This evidence suggests that state legislatures did not enact MDPLs because of high number of bicyclists in their states.[20]

A second approach is used to investigate if bicyclist fatalities, in particular recent bicyclist fatalities

[20]It is also possible that treatment states pass MDPLs while control states use alternative, and potentially more effective, measures to protect bicyclists. This is explored briefly in the appendix, and the results suggest that the opposite is more likely, states that enact MDPLs are overall more "bicycle friendly".

Figure 1.4: Event Study




Notes: The point estimates show the change in MDPL effect relative to the effect during the month prior to enactment. The 95% confidence intervals are illustrated by bars stemming from the point estimates. If the point estimate is significant at the 95% level, its confidence interval does not intersect the horizontal line at 0.

led to MDPL enactment. To investigate this possibility an event study is performed.[21] The results can be seen in Figure 1.4.

The event study also illustrates if the effect of MDPLs varies over time. If drivers react to the announcement of MDPLs and then return to original behavior, or are slow to change their behavior due to learning processes the MDPL variable may only be statistically significant in certain time spans.

[21]This is done by transforming the MDPL variable into a set of dummy variables indicating the number of months before or after the passage of a MDPL, and excluding the month prior to the enactment of a MDPL. The tails of this variable are then binned to show the effect of all periods before or after the 20 month time window used. The model specification from column 2 of Table 1.3 is then used in estimation.

The point estimates show no discernible pattern, and only two coefficients are statistically significant. In fact, the significant point estimates denote a drop in bicyclist fatalities around one year *before* the enactment of a MDPL.[22] This rules out a systematic increase in bicyclist fatalities in states before they enact MDPLs. The event study also shows there is not a noticeable change in the effect of MDPLs over time, indicating that MDPLs do not have a delayed effect, and any effect they do have does not diminish over time.[23]

It is possible that the MDPL variable in the previous regressions suffers from omitted variable bias that is not correlated to the number of bicyclists in a state, and captures an overall shift in traffic fatalities in the treated states. To ensure that this is not occurring, a falsification test using pedestrian and then driver fatalities as the dependent variable is performed. The results in Table 1.6 alleviate these concerns by confirming that MDPLs do not significantly affect pedestrian or driver fatalities.

### 1.5.3 Limitations

As mentioned previously, reliable estimates of bicyclist miles traveled are not available. The robustness of the results to these issues has been tested by: including proxies for bicyclist miles traveled, estimating the effect of MDPLs on a shorter panel that incorporates an exposure variable that accounts for the number of bicyclist commuters in a state, performing an event study to ensure bicyclist fatalities did not increase before enactment of MDPLs, and examining the effect of bicyclist commuters on enactment of MDPLs. These methods provide similar results, suggesting that changes in bicyclist miles traveled have been controlled for sufficiently.

Though the use of state fixed-effects, linear time trends, and state level trends should eliminate

[22] Specifications with leads and lags also produce similar results, with the leads having a negative coefficient. These can be seen in Appendix Table A.6.

[23] Results from event studies with longer time frames (25, 35, and 45 months before and after MDPL passage) are very similar.

Table 1.6: Falsification Tests

| | Dependent Variable: Pedestrian Fatalities | | Dependent Variable: Driver Fatalities | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| MDPL | -0.0105 | -0.0038 | 0.0034 | 0.0044 |
| | (0.0263) | (0.0237) | (0.0256) | (0.0231) |
| Pedestrian Fatalities | | | 0.0053*** | 0.0050*** |
| | | | (0.0007) | (0.0007) |
| Driver Fatalities | 0.0025*** | 0.0026*** | | |
| | (0.0006) | (0.0005) | | |
| State Trend | | -0.0008 | | -0.0019** |
| | | (0.0021) | | (0.0009) |
| Percent Male | | 0.0044 | | -0.3686* |
| | | (0.2149) | | (0.2041) |
| Median Income | | 0.0002 | | -0.0018 |
| | | (0.0018) | | (0.0018) |
| Observations | 15,300 | 15,300 | 15,300 | 15,300 |
| Controls | YES | YES | YES | YES |
| Age Controls | NO | YES | NO | YES |
| State FE | YES | YES | YES | YES |
| Seasonal FE | YES | YES | YES | YES |
| $\alpha$ | 0.0122 | 0.0121 | 0.0103 | 0.0098 |
| Pseudo $R^2$ | 0.320 | 0.320 | 0.317 | 0.318 |

Notes: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1. "Controls" includes: temperature, $(\text{temperature})^2$, precipitation, $(\text{precipitation})^2$, precipitation*temperature interaction, unemployment rate, and *ln*(population).

any violations of the identification assumptions, it is possible that policy makers enacted MDPLs because they believed the laws would be more effective in their states than policy makers in other states do. This means expected bicyclist fatalities for states without MDPLs could have responded differently to MDPLs than the treatment states had they enacted MDPLs during this time period.

If this is true, policy makers in the control states likely thought MDPLs would be very ineffective in their areas. This means that in a counter-factual situation, where the control states enacted MDPLs, the effect of the laws might be even smaller. By this reasoning, the insignificant results found in the treatment states would also apply to the control states.

## 1.6 Conclusion

This paper identifies the effect of minimum distance passing laws on bicyclist fatalities in the US using a differences-in-differences approach, in a negative binomial model. Baseline estimates of this effect are insignificant, a result that proves to be time invariant and robust to alternative modeling choices, controlling for bicycle miles traveled using proxy variables and estimates of bicyclist commuters in a state as an exposure term, and exploring endogeneity concerns through an event study.[24]

In addition to finding that MDPLs do not have a statistically significant effect on bicyclist fatalities, the results suggest an economically significant result can also be ruled out. In the preferred specification (column 2 of Table 1.3), the average treatment effect of MDPLs indicates a 0.041 monthly increase in fatalities with a standard error of 0.045. Inspecting the effect within the 95% confidence interval, the model predicts at best a decrease of 0.049 fatalities monthly, and at worst an *increase* of 0.131 in monthly fatalities. This means a state with a MDPL is saving 1 life every 20.41 months compared to a state without a MDPL. This equates to a slim reduction implying that

[24]Although specifications using the proxy and exposure variables have shortcomings, they provide similar results. Taken together they suggest that changes in bicyclist miles traveled have been controlled for sufficiently.

MDPLs are not an effective way to reduce bicyclist fatalities. This insignificance may be caused by driver non-compliance, inadequate minimum distance required, or other issues (Love et al., 2012).

As MDPLs do not reduce bicyclist fatalities, the only benefits they generate are possible increases in bicyclist miles traveled. The costs of implementing MDPLs are relatively small and include: publicity, police patrol time, court-system, education program, program management, and more. Child bicycle helmet laws have been estimated to cost $8 million to implement in 26 states (Ecola et al., 2015). Assuming MDPLs have similar costs as child bicycle helmet laws, and using the costs of congestion and pollution per mile of $0.03 and $0.065 , respectively (Small and Kazimi, 1995; Fischer et al., 2007). This means a reduction of 84 million vehicle miles traveled, a mere drop in the bucket when compared to US annual vehicle miles traveled, is required to offset the costs of MDPLs.

Given the low costs of MDPLs, policy makers may still wish to enact them even if they are ineffective in reducing fatalities. However, it might be possible to decrease the number of bicyclist fatalities by implementing stronger versions of MDPLs and improving enforcement. These measures may include: increasing the minimum distance, eliminating loopholes, increasing police training, and altering drivers education courses to better disseminate information of the laws. Though further research is needed to determine the effectiveness of these alterations, they could be implemented with either zero or very low additional costs.

# Chapter 2

# Taxed to death? The effects of diesel taxes on freight truck collisions

## 2.1 Introduction

Virtually all economic activity in the U.S. is intertwined with freight carried by truck. An estimated 67% of the 19,662 million tons of freight shipped in the U.S. in 2012 was transported by truck (FHWA, 2013).[1] The economic significance of freight trucking is clear, but this industry also imposes enormous negative externalities on society that have received little attention.[2] Though a rich economics literature examines the "arms race" on American roads and the effects of increasing vehicle weight on collisions, the effects of truck weight on collisions has not previously been estimated (Li, 2012; Anderson and Auffhammer, 2014; Anderson, 2008). The disproportionately large number and immense severity of collisions that involve freight trucks means that even small regulatory changes that affect truck weight may have large effects on societal welfare.

[1]This constituted a total of 1,894,029 ton-miles (Bureau of Transportation Statistics).

[2]Notable exceptions include Cohen and Roth (2017), He (2016), and Muehlenbachs et al. (2017).

This paper fills this gap in the literature by examining the effects of both truck weight and truck miles traveled on collisions. The analysis not only quantifies how changes in trucking behavior influence the quantity of collisions, but also how it affects the severity of those collisions. Further, the effects of increased diesel taxes, which have previously been shown to decrease truck miles traveled while simultaneously increasing truck weight, on truck collisions is investigated (Cohen and Roth, 2017). Estimates reveal that both measures of trucking activity can increase collisions, but increases in truck weight skew the distribution of collisions towards more severe outcomes involving either injury or death. In application, diesel taxes are shown to slightly reduce the total number of collisions, but the remaining collisions become more severe. Societal gains from pollution, congestion, and collision reductions are entirely offset by the increased fatal collision costs, reducing total welfare by $39.9 billion annually.

Estimating the effects of truck weight on collisions is empirically challenging for several reasons. First, in contrast to passenger vehicle weights that are relatively fixed throughout a vehicle's lifetime, freight truck weight is highly dependent on the amount of cargo being carried. This means truck weights cannot easily be obtained by matching vehicle identification numbers (VINs) to listed curb weights as previous research on passenger vehicle collisions has done.[3] A second related issue arises because the amount of cargo on each truck is proportional to the number of trucks on the road. To move the same tonnage of freight, a firm may either dispatch many trucks with light cargo loads or fewer trucks carrying heavy cargo loads. Because the number of truck miles traveled will also affect the quantity of collisions, the level of trucking demand must be controlled for to obtain unbiased estimates of the effect of truck weight on the quantity of collisions. Finally, because there are few reliable data sources on non-fatal collisions, and collision severity is of paramount concern, a new data set that categorizes truck-involved collisions by severity level must be obtained.

The first challenge is overcome by utilizing over 3.5 billion individual truck weight observations obtained from Weigh-In-Motion (WIM) traffic sensors embedded in roadways across 35 states.

[3] See for example, Anderson and Auffhammer (2014) or Bento et al. (2017).

These WIM sensors record the the weight of each individual truck as it travels over the sensor – allowing for remarkably rich data on truck weight to be used in the analysis. The WIM data also resolve the second empirical issue by recording the number of trucks driving over a sensor during a given time period. This allows for the effects of two measures of trucking activity, truck weights and truck counts, on truck-involved collisions to be estimated separately. The final issue is overcome using a data set acquired from the Federal Motor Carrier Safety Administration (FMCSA) through a Freedom of Information Act (FOIA) request. This data set provides highly detailed information on the universe of reported truck-involved collisions in the U.S. Among other characteristics, these data include a crash severity rating indicating if the collision resulted in a vehicle being towed away, an injury requiring hospitalization, or a fatality.

The empirical methodology employs these data in two distinct exercises. The first estimates the effects of truck cargo weight and truck counts on the quantity of each collision type as well as the total count of all collisions within a state-week while controlling for differences in weather, passenger-vehicle travel, and unobservable differences location and time characteristics. This model identifies how the quantity of each type of collision changes using variations in truck weight and truck counts between states within a given month. The analysis provides a greater understanding of how firm dispatching decisions may affect the total quantity of truck-involved collisions, as well as a general idea of how these decisions affect collision severity. To provide a more rigorous investigation of how these decisions affect collision severity, an ordinal logit model is then estimated. This model estimates how truck weights and truck counts affect the probability of a collision being in a higher severity classification, conditional on a collision occurring.

Next, the estimates derived in this paper as well as those in Cohen and Roth (2017) are used to perform a welfare analysis of diesel fuel taxes. Cohen and Roth (2017) show that raising diesel prices induces freight carriers to alter their dispatch decisions. Their analysis shows that a firm's dispatch decisions vary along two dimensions– changes in weight per truck-mile, and changes in the total cargo-miles, or vehicle ton-miles. As fuel prices rise, firms balance the quality of the service they provide (as measured by the frequency of deliveries) with the costs of transportation. This trade-off leads firms to decrease the total number of shipments and increase the quantity of goods on each shipment. Firms respond to fuel price increases on both the intensive (cargo on each shipment) and extensive margins (number of shipments). Higher diesel fuel prices lead to fewer, but heavier, trucks on the road. Cohen and Roth (2017) find truck weight rises to such an extent that the additional road damage caused by diesel taxes and the ensuing heavier trucks more than offsets the welfare gains from reductions in diesel fuel consumption. This paper finds that the distortion in truck weight brought about by diesel taxes also exacerbates the collision externality.

The results from this policy analysis provide an empirical example of the theory of the second best, where, in the presence of multiple market failures, a policy implemented to correct one externality may reduce economic efficiency (Lipsey and Lancaster, 1956). A policy that can be used to mitigate carbon pollution (a diesel fuel tax) is shown to reduce societal welfare by increasing the average severity of collisions despite gains from reductions in pollution, congestion, and the quantity of collisions.

The remainder of the paper proceeds as follows. Section 2.2 provides a review of related literature. Section 3.3.1 describes the data used in the empirical analysis. Section 2.4 outlines the empirical methodology. Section 3.4 presents results, Section 3.5 discusses the implications of these results, and Section 3.6 concludes.

## 2.2 Related Literature

This paper is not the first to investigate the link between vehicle weight and traffic collisions. The existing literature examines this relationship in the context of passenger vehicles, focusing on either the "arms race" on Americans roads – wherein drivers purchase increasingly heavy vehicles to increase their own safety despite heavier vehicles being more dangerous for other road users – or the relationship between reductions in vehicle weight due to energy policy, namely the Corporate Average Fuel Economy (CAFE) standards, and collisions.

The arms race literature has examined how the rising prevalence of light-duty trucks and sport-utility vehicles (SUVs) in the U.S. may be affecting collisions. Anderson (2008), Gayer (2004), Li (2012), and White (2004) have used varying methodological approaches and data to quantify these effects. Their results show that larger and heavier vehicle classes (pick-up trucks, SUVs, and vans) impose large costs on other road users because they increase the risk of being in a severe accident relative to smaller vehicles. These results likely apply to freight trucks as well because they are significantly larger than passenger vehicles, pick-up trucks, and SUVs, suggesting they may impose even larger costs on road users.

Some research on the link between CAFE standards and collisions has also focused on vehicle classes, particularly how CAFE standards may influence vehicle choice. This research has shown that CAFE standards will shift drivers toward more fuel efficient vehicles, which changes vehicle weight dispersion (Jacobsen, 2013). Another branch has focused on changes in weight regardless of vehicle class (Bento et al., 2017; Crandall and Graham, 1989; Anderson and Auffhammer, 2014). Collectively, these papers have determined that average vehicle weight as well as the dispersion of vehicle weights within the fleet affect collisions. This dispersion is particularly interesting in the case of freight trucks, which are generally the heaviest vehicles on the road. Because these trucks are in the tail of the weight distribution, increasing their weight will lead to more dispersion, likely increasing the severity of collisions.

A final related literature has examined the trucking industry in an attempt to either estimate external costs or develop policies to mitigate externalities. Muehlenbachs et al. (2017) estimates the accident externality from trucks. Their analysis uses plausibly exogenous increases in the number of trucks on the road caused by the drilling of hydraulically fractured wells in North Dakota. The authors determine the average number of trucks required to transport drilling materials, determine the most plausible routes for the trucks to take to well locations, and examine the changes in reported accidents on those roadways. Their results suggest that increasing the number of trucks increases truck-involved collisions. Interestingly, increases in the number of trucks have an even larger effect on car-on-car collisions. The present analysis aims to disentangle the effects of two measures of trucking activity, cargo weight and truck counts, as opposed to focusing solely on the number of trucks as is done in Muehlenbachs et al. (2017).

Cohen and Roth (2017) examine the “dispatch effect” of freight trucking in response to fuel price increases. They show that shipping firms respond to higher fuel prices through two channels– reducing the number of shipments made and increasing the cargo on each shipment. Their results show that because road damage increases rapidly a function of truck weight, increasing a diesel fuel tax may actually be welfare reducing as it exacerbates the road damage externality. The estimates of the dispatch effect in Cohen and Roth (2017) are used in a policy simulation in Section 3.5 of this paper which illustrates that diesel fuel taxes also exacerbate the collision externality.

He (2016) analyzes how policy differentiation for freight trucking may improve societal welfare. He (2016) estimates the fuel price elasticity of truck miles traveled, finding significant heterogeneity across trucks in different weight classes and in different business sectors. The author then illustrates how a differentiated fuel tax policy that incorporates these elasticities provides welfare improvements in terms of reduced congestion, pollution, collisions, noise, and road damage relative to a uniform fuel tax. He (2016) suggests that differentiated taxes that levy a larger tax on trucks with higher weight ratings are welfare improving because of heterogeneous elasticities and (in large part) due to the increased road damage from heavier vehicles. The results from this paper and Cohen and Roth (2017) may bring these findings into question, as they show increasing diesel fuel taxes exacerbate both road damage and accident externalities associated with freight trucks.

## 2.3 Data Description

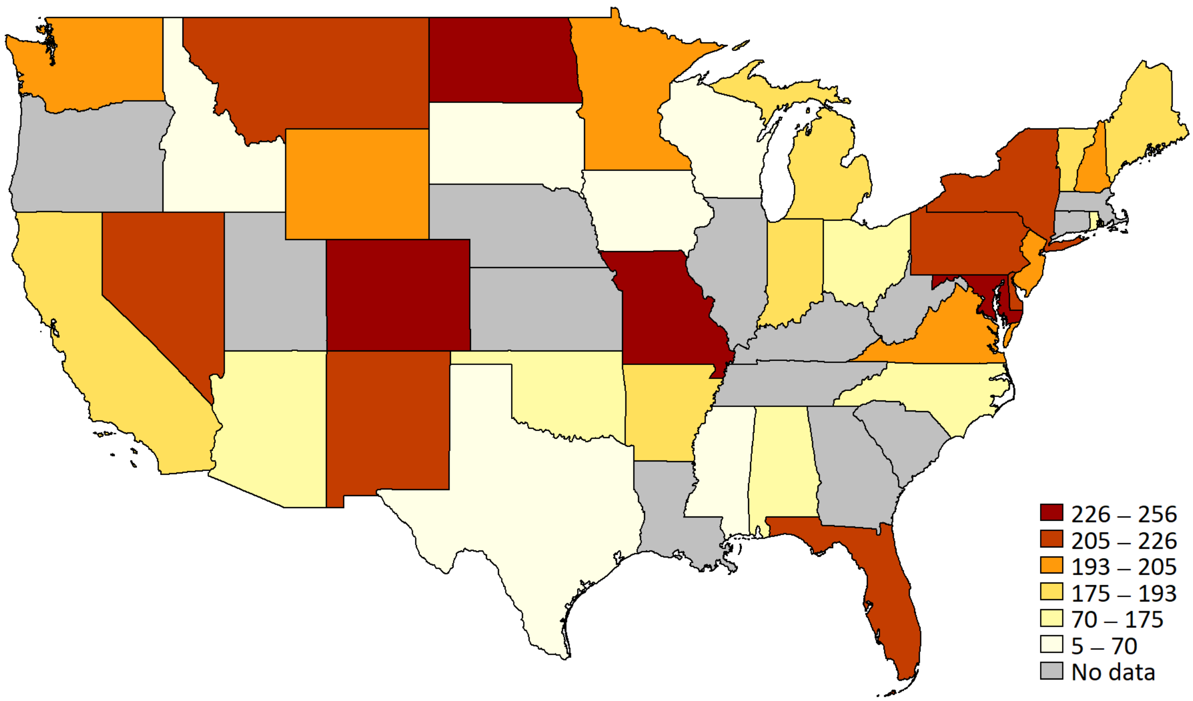
The empirical analysis utilizes one of the most expansive data sets ever compiled on trucking activity. The main data sources cover 3.5 billion individual truck weight observations and the universe of reported truck-involved collisions in the U.S. between 2012-2016. Several other ancillary data sources, including 770 million hourly vehicle counts from passenger-vehicle traffic sensors to control for variation in travel demand, and weather data from the National Oceanic and Atmospheric Administration are also used. This section provides additional information on these data sources and descriptive statistics.

***Weigh-In-Motion Data***

Data from weigh-in-motion (WIM) sensors are used to analyze both truck cargo weight and truck counts. These sensors are distinct from static weigh stations commonly observed on U.S. highways. Weigh-in-motion sensors are embedded in roadways and record metrics for all trucks as they travel over the sensors, as opposed to static scales that require truckers to exit along highways and stop on top of a scale to be weighed. The metrics recorded for each truck by WIM sensors include gross truck weight, individual axle weight, axle spacing, and (for some sensors) travel speed. From these data, variables for the average cargo weight of trucks in a state and week as well as the average number of truck observations per sensor in a state and week are constructed.

In total, WIM data from 35 states between 2012-2016, consisting 3.5 billion individual truck observations, are used in the analysis. However, most states do not report WIM data for the full sample period, and many states do not consistently report WIM data, which generates an unbalanced panel structure. The states included in the analysis and the number of weekly observations for each state are provided in Figure 2.1.

Figure 2.1: Number of Observations By State




*Notes:* The panel begins in January of 2012 and ends in December of 2016. The lowest number of states reporting data in a week is 6. The sample covers 260 weeks in total. 35 states are represented in the data set.

Following Cohen and Roth (2017), the sample of trucks used in the analysis is restricted to 5-axle trucks with gross weights between 15,000-120,000 pounds. Imposing this restriction increases the probability that the remaining trucks are diesel powered and transporting cargo.[4] Moreover this restriction eliminates erroneous and outlying WIM observations.

Unfortunately, WIM sensors are prone to outages. During outages sensors either report no or implausibly heavy truck observations. While this is of little concern for the construction of the cargo weight variable, as long as the outages occur randomly (which they seemingly do), outages may be problematic for the average number of truck observations per sensor in a state and week.

[4]Trucks with fewer than 5 axles are less likely to be diesel powered. Most states in the U.S. have a maximum allowable truck weight of 80,000 pounds, and the empty carry weight of the average 5-axle truck is higher than 15,000 pounds. The implication being that trucks outside the 15,000-120,000 pound range are likely to be either errors or extreme outliers.

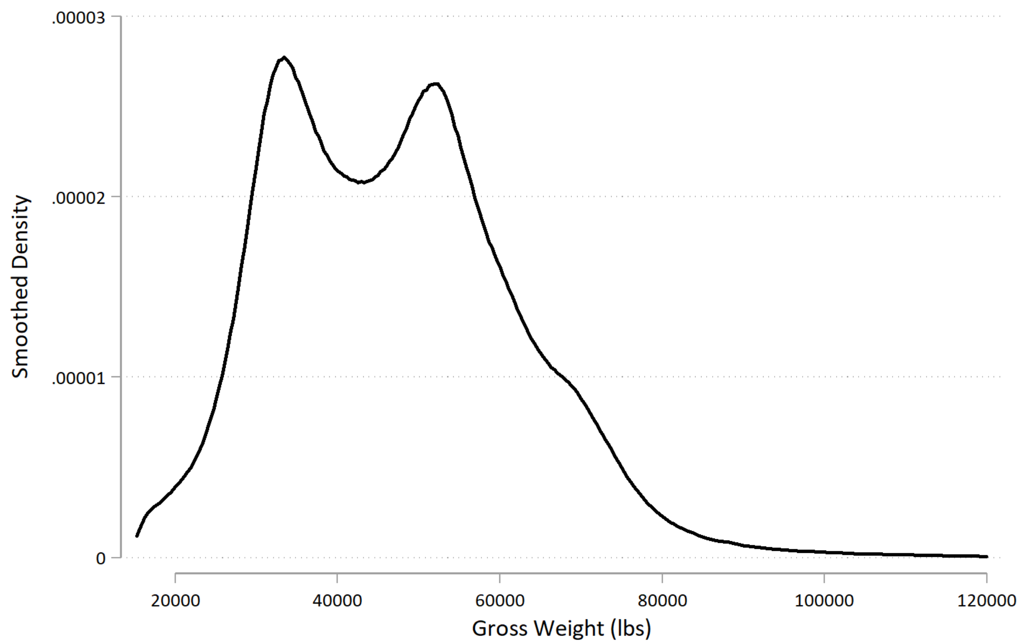
This issue is addressed using imputations by chained equations which imputes missing values using neighboring WIM sensors within the state. This imputation process is described in more detail in Appendix Section B.1.

Finally, these data are aggregated to the state week level. For truck counts, each observation is the average hourly truck count at a sensor across the entire state and week. A similar aggregation is done for gross truck weight, and for sensors that report it, truck speed. To determine cargo weight from gross weight, the methodology of Cohen and Roth (2017) is followed and the average weight of a containerless truck (23,000 pounds) is subtracted from gross weight.

The weight observations from the WIM sensors generally display a bimodal distribution with numerous trucks having gross weights near 30,000 pounds (likely empty trucks) and many near 55,000 pounds. An example of this is seen in Figure 2.2 which provides a smoothed density plot from all truck observations in the state of Wisconsin.[5] Because most states have weight limits of 80,000 pounds for trucks, this distribution suggests that most firms have the ability to increase cargo weight within legal limits.

[5]This density plot is constructed using nearly 3 million individual truck observations. Due to the magnitude of the raw data, it is computationally intensive to construct a kernel density plot using all 3.5 billion truck observations. Data from other states are qualitatively similar.

Figure 2.2: Bimodal Weight Distribution of Trucks




*Notes:* This figure plots the distribution of almost 3 million truck weight observations from Wisconsin in 2013. The data were generated from 39 distinct WIM sensors in the state.

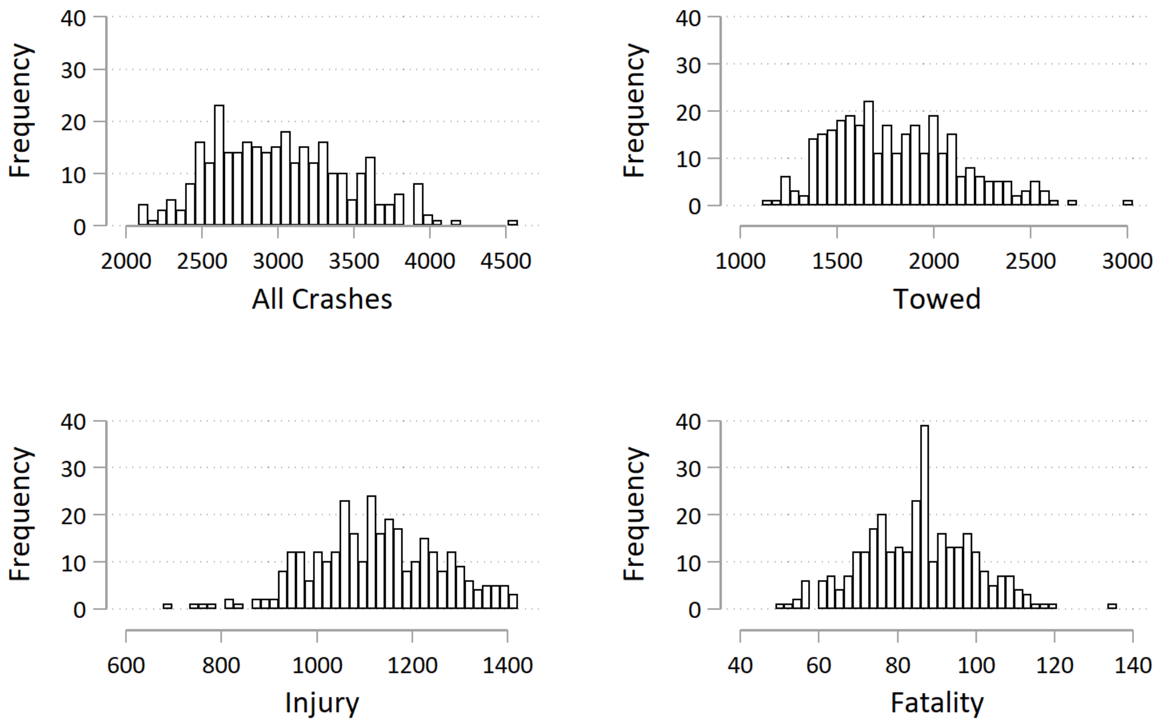
***Collisions Data***

Data on the universe of reported truck-involved collisions from 2012-2016 comes from the Federal Motor Carrier Safety Administration (FMCSA). These data, obtained through a freedom of information act request, provide the location, time, severity, as well as the type and number of vehicles involved for each of the almost 400,000 collisions used in the analysis.

The FMCSA classify collisions into three ranks of increasing severity, tow-away, injury, and fatality. Tow-away collisions are those that result in at least one vehicle being towed-away from the scene. Injury collisions are those that involve the hospitalization of at least one individual. Fatality collisions are those that involve the death of at least one individual. To illustrate the startling frequency of these collisions, Figure 2.3 provides histograms of the weekly number of each type of collision across the U.S. during the sample period. Less severe accidents are more frequent, but injury and fatal collisions are abundant in the data. There are no less than 40 fatal collisions per week across the U.S. during the sample and an average of 85 per week.

These data are either aggregated to the count of each collision type within a state and week in the analysis of the quantity of collisions or matched individually to truck count and cargo weight data for the state and week the collision occurred in the ordinal model. When analyzing the total quantity of collisions in a state and week, there are some observations with zero fatal or injury collisions. As will be discussed in the next section, these collision variables are log transformed after adding one to each observation to prevent ln(0). This transformation is performed to obtain elasticity estimates that are easily incorporated in the policy discussion in Section 3.5, but count data models are also estimated in the Appendix Tables B.2 and B.3 as robustness checks.

Figure 2.3: Weekly frequency of collision by severity class in the U.S.




*Notes:* Panels provide histograms of total number of each collision type per week across the entire U.S. The x-axis depicts the number of collisions while the y-axis displays the number of weeks that experienced that number of collisions. One week with a total number of 1,865 crashes is excluded from the first panel for clarity.

***Other Data***

To control for variation in passenger-vehicle travel that may be correlated with the level of trucking demand and collisions, data from passenger-vehicle traffic sensors obtained from the Federal Highway Administration are used. These data, consisting of 770 million hourly vehicle counts, come from the universe of passenger-vehicle traffic sensors in the U.S. The data are aggregated to the average hourly vehicle count at a sensor within a state and week.

Weather data was obtained from the National Oceanic and Atmospheric Administration. This data set covers all weather stations within the U.S. during the sample period. The data provide controls for temperature including heating degree days and cooling degree days (both base 65 degrees Fahrenheit) as well as precipitation measures including precipitation, snow, and snow depth. To correct for weather station outages, nearest neighbor imputations are performed following Auffhammer et al. (2013). The data are then aggregated to state week average values.

Descriptive statistics for all variables used in the analysis can be found in Table C.1.

Table 2.1: Descriptive Statistics

| | Obs. | Mean | Std.Dev. | Min | Max |
|---|---|---|---|---|---|
| Total Crashes | 5821 | 66.62 | 66.05 | 1 | 522 |
| Tow | 5821 | 40.49 | 40.38 | 0 | 339 |
| Injury | 5821 | 24.31 | 26.19 | 0 | 173 |
| Fatal | 5821 | 1.81 | 2.41 | 0 | 30 |
| Cargo Weight | 5821 | 312 | 3823.20 | 17842.48 | 44398.02 |
| Truck Count | 5821 | 23.84 | 13.60 | 3.05 | 75.19 |
| Truck Speed (km/hr) | 1899 | 79.96 | 19.44 | 51.69 | 108.49 |
| Passenger-Vehicle Count | 5821 | 456.45 | 402.11 | 46.51 | 3175.59 |
| Temperature | 5821 | 51.49 | 18.28 | -8.75 | 86.23 |
| Precipitation (in.) | 5821 | 0.27 | 0.27 | 0 | 2.55 |
| Snow | 5821 | 2.79 | 7.07 | 0 | 84.46 |
| Snow Depth | 5821 | 54.98 | 125.47 | 0 | 834.76 |

*Notes:* Crashes include all truck-involved crashes. Heating degree days and cooling degree days are calculated from the temperature variable with a base of 65 degrees Fahrenheit.

## 2.4 Empirical Methodology

The empirical strategy uses a two part approach to examine the effects of truck miles traveled and truck cargo weight on the quantity and severity of truck-involved collisions. The analysis begins by estimating the effects of these two variables of interest on the quantity of truck-involved collisions in different severity classes. Then the analysis turns to a more formal analysis of how these variables affect the severity of collisions. This part of the analysis uses an ordinal discrete choice model that estimates how, conditional on a collision occurring, changes in truck miles traveled and cargo weight affect the probability of a collision being in a more severe category rank.

***Estimation of the effects of truck counts and cargo weight on the quantity of collisions***

To begin, the WIM sensor data are utilized to estimate the effects of truck counts and cargo weight on the quantity of collisions in different severity rankings in a state and week. These effects are estimated using the OLS equation (1) below[6],

$$(1) \quad ln(y_{it}) = \beta_1 \cdot ln(\text{Cargo}_{it}) + \beta_2 \cdot ln(\text{Truck Count}_{it}) + \psi \cdot ln(\text{V}_{it}) + \gamma \cdot X_{it} + \nu_i + \mu_t \epsilon_{it}$$

where $i$ denotes a state and $t$ denotes the week. $ln(y_{it})$ is the natural logarithm of the sum of all collisions, the sum of tow-away collisions, the sum of collisions resulting in an injury requiring hospitalization, or the sum of fatal collisions. The coefficients of interest are $\beta_1$ and $\beta_2$ which estimate the elasticity of collisions with respect to cargo weight and truck counts, respectively. $\epsilon_{it}$ is an error term clustered at the week level. This level of clustering is used because the unbalanced panel data do not include enough states to cluster at the state level. However, because a large amount of freight truckers cross state lines it is unclear if clustering at the state level would be appropriate. Clustering on week allows for the error term to be correlated across states within a week, which is

[6] Although the dependent variables take the form of non-negative integers, an OLS model is estimated for ease of interpretation in policy simulation. However, Poisson and negative binomial models are also estimated in Appendix Tables B.2 and B.3 and provide qualitatively similar results.

appropriate given the mobile nature of freight trucking and dependence on macroeconomic shocks.

To ensure that the coefficients of interest are not biased by other factors, several controls are also included in the regressions. To control for changes in passenger vehicle travel which may be correlated with trucking activity, V, the average hourly passenger vehicle count from passenger vehicle sensors within a state and week is included. X is a matrix of weather controls (HDD, CDD, precipitation, snow, and snow depth) that controls for differences in weather between states and over time. Finally, state and month fixed effects control for unobservable static differences between states and macroeconomic trends over time. Conditional on the inclusion of these controls, the model uses variation between states within a month to estimate the effects of cargo weight and truck counts on the quantity of different types of collisions.

This model provides insights into the relationship between truck miles traveled, cargo weight, and the quantity of collisions. By comparing the magnitude of coefficients across regressions with different dependent variables, it is also possible to determine if these variables are more or less important in determining the quantity of different classes of collisions.

***Estimation of the effects truck counts and cargo weight on the severity of collisions***

The analysis now turns to a more rigorous estimation of how truck miles traveled and cargo weight change the probability of a collision being in a more severe category, conditional on a collision occurring. This is done by matching each individual collision to the average number of truck counts and cargo weight of trucks within the state and week the collision occurred. Because the collisions fall into three separate severity rankings with a a natural ordering, an ordinal logit can be used to determined how changes in truck counts and cargo weight affect the probability of a collision being in a higher severity class. Here, the observed ordinal dependent variable can be written as,

$$y_{jit} = \begin{cases} 1 & \text{if Tow} \\ 2 & \text{if Injury} \\ 3 & \text{if Fatality} \end{cases}$$

with an underlying unobservable process determining the outcomes characterized as,

(2) $y^*_{jit} = \beta_1 \cdot \text{Cargo}_{it} + \beta_2 \cdot \text{Truck Count}_{it} + \beta_3 \cdot X_{it} + \epsilon_{jit}.$

This latent process influences the observed outcomes as follows,

$$y_{jit} = \begin{cases} 1 & \text{if} -\infty < y^*_{jit} \leq \gamma_1 \\ 2 & \text{if} \gamma_1 < y^*_{jit} \leq \gamma_2 \\ 3 & \text{if} \gamma_2 < y^*_{jit} < \infty \end{cases}$$

where $\gamma_1$ and $\gamma_2$ are cutoff points determining which discrete outcome the dependent variable takes. Here, $j$ denotes a collision, $i$ denotes state, and $t$ denotes week. Assuming that the error term is distributed logistic allows for the estimation of this model via maximum likelihood with parameters $\beta_p, p = 1, 2, 3$ and $\gamma_k, k = 1, 2$ (Train, 2009). The estimated $\beta$ coefficients will describe the impact of the explanatory variables (cargo weight, truck counts, and X, a matrix of weather controls and fixed effects) on the severity of collisions. Because these coefficients represent changes in log-odds, marginal effects are calculated and presented in the Results section.[7]

[7] These coefficients will denote the change in the ordered log-odds that occurs when an independent variable is increased by one unit.

## 2.5 Results

***Effects of Truck Counts and Weight on Collisions***

The analysis begins by investigating the effects of truck weight and truck counts on the quantity of truck-involved collisions. The goal of this section is to quantify the relationship between these two measures of trucking activity and the frequency of truck-involved collisions of different severity classifications. Collisions are categorized into four distinct groups. Total, which is the sum of all collisions within a state and week, Towed, the sum of all collisions in a state week that resulted in at least one vehicle being towed away, Injury, the sum of all collisions in a state week that resulted in at least one person being hospitalized, and Fatal, the sum of all collisions in a state-week that resulted in at least one fatality.

The empirical model described in the previous section is then estimated four times, once for each type of collision. The results are provided in Table 2.2. Each dependent variable has been log transformed after adding one to avoid zero observations. This transformation allows for coefficients to be interpreted as elasticities. Because the dependent variables take the form of positive integers, Poisson and negative binomial models are also estimated as robustness checks in Appendix Tables B.2 and B.3, though the results are qualitatively similar.

Table 2.2: Effects of truck count and cargo weight on quantity of collisions

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Dependent Variable:* | ln(Total) | ln(Tow) | ln(Injury) | ln(Fatal) |
| ln(Truck Count) | 0.562*** | 0.472*** | 0.634*** | 0.321*** |
| | (0.065) | (0.073) | (0.066) | (0.064) |
| ln(Cargo Weight) | 0.328*** | 0.343*** | 0.730*** | -0.093 |
| | (0.073) | (0.081) | (0.114) | (0.124) |
| ln(Passenger-Vehicle Count) | 0.092*** | 0.061* | 0.177*** | 0.018 |
| | (0.029) | (0.035) | (0.043) | (0.044) |
| Observations | 5,821 | 5,821 | 5,821 | 5,821 |
| R-squared | 0.933 | 0.908 | 0.889 | 0.528 |
| Weather Controls | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |

*Notes:* Standard errors are clustered at the week level. All regressions include a constant. Weather Controls includes HDD, CDD, precipitation, snow, and snow depth. Truck count, cargo weight, and passenger-vehicle count are all state-week average measures. Dependent variables are the natural logarithms of state-week counts of collisions in different severity rankings plus one. Total is the sum of all other collision rankings.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Each regression includes a constant, weather controls, state fixed effects, and month-of-sample fixed effects with standard errors clustered at the week level.[8] Regressions also control for passenger vehicle travel demand by including vehicle counts from non-WIM traffic sensors. The inclusion of these controls allows for the investigation of how truck miles traveled (which are proportional to truck counts) and cargo weight affect the quantity of each type of collision. The effects are estimated using between state and within month variation in the variables of interest.

Truck count and cargo weight have a positive and statistically significant effect on collisions in

[8]Because the data set consists of an unbalanced panel that doesn't cover the entire US, there are too few states in the analysis to cluster at the state level. However, because a large amount of freight truckers cross state lines it is unclear if clustering at the state level would be appropriate. Clustering on week allows for the error term to be correlated across states within a week, which is appropriate given the mobile nature of freight trucking and dependence on macroeconomic shocks.

all regressions, with the exception of the effect of cargo weight on fatal collisions. This result is intuitive, as increasing the demand for trucking (which truck count and cargo weight are measures of) is expected to increase collisions. However, comparing the magnitude of the cargo weight coefficients across regressions suggests that increasing cargo weight may have a larger effect on more severe collisions. Column 2 suggests that a 1% increase in cargo weight increases the total quantity of tow-away collisions by 0.34% while column 3 suggests the same increase in cargo weight increases injury-involved collisions by 0.73%. While the effect of cargo weight on fatal collisions is statistically insignificant (potentially due to the lower frequency of fatal collisions) this provides suggestive evidence that heavier trucks increase collision severity.

Though the regressions include a control for passenger vehicle travel demand, as well as state and month fixed effects, that should control for the level of congestion, there is a possibility that increased truck counts lead to higher congestion. If this is the case, increasing truck counts will reduce the speed vehicles are traveling at. This may lead to two issues. First, conditional on a collision occurring, lower speeds will lead to less severe collisions. Second, increased congestion means there will likely be more interaction (passing) between passenger vehicles and freight trucks. This increase in interaction may lead to more collisions. Either of these issues may lead to potentially biased estimates of the effects of truck count or cargo weight.

To investigate this possibility, a subset of the data which also reports truck speed can be used. Table 2.3 estimates regressions identical to those in Table 2.2 while controlling for truck speed using only WIM sensors that report speed information. The results are qualitatively similar to those in Table 2.2, alleviating omitted variable bias concerns. Interestingly though, the speed variable is negative and statistically significant in most specifications. This provides evidence that trucks traveling at high speeds are involved in fewer collisions, all else held constant. This suggests that trucks in more heavily congested areas are more likely to be involved in a collision.

Table 2.3: Effects of truck count, cargo weight, and speed on quantity of collisions

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Dependent Variable:* | ln(Total) | ln(Tow) | ln(Injury) | ln(Fatal) |
| ln(Truck Count) | 0.628*** | 0.402*** | 0.680*** | 0.397*** |
| | (0.088) | (0.099) | (0.113) | (0.136) |
| ln(Cargo Weight) | 0.404*** | 0.463*** | 1.127*** | -0.238 |
| | (0.126) | (0.146) | (0.202) | (0.174) |
| ln(Truck Speed) | -0.406*** | -0.356*** | 0.308 | -0.558*** |
| | (0.119) | (0.125) | (0.223) | (0.191) |
| ln(Passenger-Vehicle Count) | -0.014 | -0.067 | 0.154* | -0.119* |
| | (0.050) | (0.051) | (0.087) | (0.069) |
| Observations | 1,899 | 1,899 | 1,899 | 1,899 |
| R-squared | 0.886 | 0.868 | 0.775 | 0.348 |
| Weather Controls | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |

*Notes:* Standard errors are clustered at the week level. All regressions include a constant. Weather Controls includes HDD, CDD, precipitation, snow, and snow depth. Truck count, cargo weight, speed and passenger-vehicle count are all state-week average measures. Dependent variables are the natural logarithms of state-week counts of collisions in different severity rankings plus one. Total is the sum of all other collision rankings.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

***Effects of truck counts and cargo weight on the severity of collisions***

The analysis next turns to a more formal treatment of how truck counts and cargo weight affect collision severity, conditional on a collision occurring. This portion of the analysis estimates an ordinal logit model where the dependent variable takes the form of an individual collision classified into one of three severity rankings. Here, the dependent variable is equal to one if the collision involved a vehicle being towed, two if the collision involved an injury, and three if the collision involved a fatality. Marginal effects from an ordinal logit model that controls for state by week average values of truck counts, cargo weight, HDD, CDD, precipitation, snow, snow depth as well

Table 2.4: Marginal effects of truck counts and cargo weight on the severity of collisions

| Variable | (1) Truck Counts | (2) Cargo Weight |
|---|---|---|
| Marginal effect on pr(Tow-Away collisions) | 0.03* | -0.13*** |
| | (0.02) | (0.03) |
| Marginal effect on pr(Injury collisions) | -0.05* | 0.20*** |
| | (0.03) | (0.05) |
| Marginal effect on pr(Fatal collisions) | -0.08* | 0.32*** |
| | (0.05) | (0.09) |

*Notes:* Standard errors are clustered at the week level. All regressions include a constant, weather controls including HDD, CDD, precipitation, snow, and snow depth as well as state and month fixed effects. Truck count and cargo weight are state-week average measures. Dependent variable is an ordinal measure of collision severity. Coefficients are marginal effects computed as elasticities.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

as state fixed effects, and month effects with standard errors clustered at the week level can be seen in Table 2.4. These marginal effects have been computed as elasticities for ease of interpretation.

The results in Table 2.4 provide marginal effects that depict the effects of truck counts and cargo weight on the probability a collision is in a particular severity ranking. As can be seen in column (1), increasing truck counts by 1% will slightly increase the probability that a collision is in the tow-away rank, but slightly decrease the probability it is either in the injury or fatal rank. The small marginal effects suggest that truck miles traveled play on a small role in determining the severity of a collision.

In contrast, cargo weight appears to play a relatively large role. Column (2) suggests that increasing cargo weight by 1% will decrease the probability that a collision is in the tow-away rank by 0.13%, but increase the probability it is in the injury or severity rank by 0.20% and 0.32%, respectively. The increasing magnitude of these marginal effects across severity rankings provides evidence that increasing truck cargo weight will skew the distribution of collisions towards more severe outcomes.

## 2.6 Discussion

The estimates reported in the previous section as well as results from Cohen and Roth (2017) allow for the analysis of the cumulative effect of a change in diesel taxes on both the quantity and severity of truck-involved collisions. These effects are hypothesized to occur through two channels– increases in truck cargo weight, which are expected to increase the number of collisions and make collisions more severe, and decreases in truck miles traveled, which are expected to decrease total collisions and (weakly) increase fatal collisions as well as decrease congestion and pollution. Table 3.2 analyzes the effect of a \$0.37 per gallon diesel fuel tax increase, equivalent to a carbon tax of \$36 per metric ton of $CO_2$, and the corresponding welfare implications. This discussion utilizes the estimates in column (1) of Table 2.2 to calculate changes in the total quantity of all collisions and the results in Table 2.4 to calculate how the probability a collision is fatal will change.

Beginning with cargo weight, a \$0.37 increase in the federal diesel tax is, based on the results of Cohen and Roth (2017), estimated to increase cargo weight by 5.328%, which yields an estimated 1.748% increase in total collisions and a 1.705% increase in a collision being fatal conditional on a collision occurring. Conversely, the \$0.37 increase in the federal per gallon diesel tax reduces truck-miles traveled by an estimated 6.403%, which yields to a 3.598% decrease in total collisions. However, based on the estimates in Table 2.4, reducing truck miles traveled will slightly increase the probability that a collision is fatal, conditional on a collision occurring. As such, this decrease in truck miles traveled will increase the probability that a collision is fatal by 0.512%.

Cumulatively, these estimates imply that a \$0.37 increase in the federal diesel tax will decrease the total quantity of truck-involved collisions by (1.748%-3.598%)=-1.85%, which translates into a 7,677.5 decrease in the total quantity of collisions. The probability that a collision is fatal condi-

Table 2.5: Welfare implications of a \$0.37 increase in federal diesel tax

| | |
|---|---|
| *Panel A: Parameters* | |
| Cost of average truck-involved collision[1] | \$172,292 |
| Cost of fatal truck-involved collision[1] | \$7,633,600 |
| Cost of truck pollution per million miles traveled[2] | \$44,000 |
| Cost of truck congestion per million miles traveled[2] | \$7,000 |
| Truck-involved collisions in 2015[3] | 415,000 |
| Fatal truck-involved collision in 2015[3] | 3,598 |
| Millions of truck miles traveled in 2015[4] | 170,246 |
| Tax increase per gallon[5] | \$0.37 |
| *Panel B: Estimated effects of diesel tax increase* | |
| $\Delta$ Average truck cargo weight[6] | +5.328% |
| $\Delta$ Truck miles traveled[6] | -6.403% |
| $\Delta$ Total truck-involved collisions from $\Delta$ in cargo weight | +1.748% |
| $\Delta$ Total truck-involved collisions from $\Delta$ in truck miles traveled | -3.598% |
| **Net $\Delta$ in quantity of all collisions** | **-7,677.5** |
| $\Delta$ in pr(Fatal \| collision) from $\Delta$ in cargo weight | +1.705% |
| $\Delta$ in pr(Fatal \| collision) from $\Delta$ in truck miles traveled | +0.512% |
| **Net $\Delta$ in fatal collisions** | **+5,467.7** |
| *Panel C: Welfare estimates* | |
| Pollution benefit | +\$481.7 million |
| Congestion benefit | +\$76.3 million |
| Non-fatal collision benefits | +\$1,322.8 million |
| Fatal collision costs | -\$41,737.9 million |
| **Total** | **-\$39,857.1 million** |

*Notes:*
[1] FMCSA (2008)
[2] GAO (2011)
[3] FMCSA (2017)
[4] BTS (2015)
[5] Equivalent to a carbon tax of \$36 per metric ton of $CO_2$
[6] Based on author's calculations and Cohen and Roth (2017)

tional on a collision occurring changes by (1.705%+0.512%)=2.217%. Applying this increase in the probability of a fatal collision to the slightly lower total quantity of collisions remaining after the tax change implies an increase in fatal collisions of 5,467.7.

The FMCSA's estimates for the costs of the average truck-involved collision and truck-involved fatal collision for truck-tractors towing a single trailer are $172,292 and $7,633,600, respectively. Using the FMCSA's recorded number of truck-involved collisions (415,000) and fatal truck-involved collisions (3,598) in 2015, and the cumulative effects above, it is possible to monetize the costs of increased crashes (FMCSA, 2008, 2017). The benefits from a decrease of 7,677.5 non-fatal crashes are valued at $1.3 billion.[9] However, the changes in fatal crashes estimated here leads to increased costs of $41.7 billion. Together, these figures imply an increase in the costs of truck-involved collisions of $40.4 billion annually.

To assess the welfare implications, the collision costs calculated above must be compared to the benefits from the decreases in pollution and congestion costs garnered from a rise in the diesel tax. This comparison is done using the Government Accountability Office's estimated costs of pollution and congestion from freight trucks of $44,000 and $7,000 per million miles traveled, respectively and the Bureau of Transportation Statistics' estimate of 170,246 million miles traveled by combination trucks in 2015 (GAO, 2011; BTS, 2015). Thus the decrease in pollution resulting from a 6.403% decrease in truck miles traveled produces a benefit of $481.7 million while the reduction in congestion is valued at $76.3 million.[10]

These results show that the pollution and congestion benefits produced from a $0.37 per gallon diesel fuel tax are enormously outweighed by the increase in fatal collision costs. Further, though there is a a decrease in the total quantity of collisions, the skewing of the collision distribution towards more severe outcomes is so extreme that the gains from decreases in non-fatal collisions

[9] The reductions in total collisions are assumed to be non-fatal collisions given the simultaneous increase in the probability a collision is fatal. This assumption provides more conservative estimates of the welfare effects.

[10] For pollution, $(0.06403) \cdot (170,246) \cdot$ ($44,000)=$481.7 million, and for congestion, $(0.06403) \cdot (170,246) \cdot (\$7,000) = \$76.3$ million.

are an order of magnitude smaller than the increased costs of fatal collisions. The net welfare change is estimated to be approximately -$39.9 billion annually in the U.S.

It is also worth noting that this is likely a lower bound on the true size of the welfare effects because this analysis ignores the shift in collisions from tow-away only to those that experience an injury significant enough to require hospitalization. While it can be assumed the costs of these collisions lie somewhere between the cost of the average collision and a fatal collision, the FMCSA does not provide estimated collision costs at a more granular level, and, as such, this welfare analysis uses only these figures in the calculations.

In light of these results, as well as those in Cohen and Roth (2017) which similarly illustrate that diesel taxes exacerbate road damage, truck weight appears to be a first-order policy concern. This would suggest policies that directly price truck weight, like an axle-weight-mile tax, would dominate currently used diesel taxes.

## 2.7 Conclusion

This paper examines how truck cargo weight and truck miles traveled affect both the quantity and severity of truck-involved collisions. The analysis utilizes 3.5 billion observations from Weigh-In-Motion sensors embedded across roadways in the U.S. which provide highly detailed information on both the weight of individual trucks and the number of trucks on the road. These data are combined with the universe of truck-involved collisions in the U.S. between 2012-2016 broken down into different severity rankings. The empirical results illustrate that truck count and cargo weight both play a critical role in the quantity of truck-involved collisions experienced. However, as truck weights increase, the distribution of collision severity skews towards more severe collisions that result in either injury requiring hospitalization or death.

A policy analysis is performed to analyze how diesel fuel taxes, which have previously been shown to affect both truck miles traveled and cargo weight (Cohen and Roth, 2017), affect truck-involved collisions. This analysis shows that, while diesel fuel taxes provide substantial reductions in pollution, congestion, and the total quantity of truck-involved collisions, they also substantially increase the number of fatal collisions. Despite the large benefits, the shift in the collision distribution towards fatal collisions from a $0.37 per gallon federal diesel fuel tax increase (equivalent to a $36 per metric ton tax $CO_2$) leads to an estimated welfare loss of $39.9 billion annually in the U.S. This welfare analysis provides an example of the theory of the second best in action, where a policy correcting one market failure may in fact decrease economic efficiency (Lipsey and Lancaster, 1956).

The results suggest a move from diesel fuel taxes to policies that explicitly price truck weight, such as an axle-weight-mile tax. An axle-weight-mile tax has the advantage of being easily implementable, as current diesel taxes for interstate truckers are use taxes based on miles driven in each state and trucks are regularly weighed. These taxes are also politically feasible, as illustrated by a policy of this exact type already being enforced in Oregon.

# Chapter 3

# Correcting heterogeneous externalities: Evidence from local fuel price controls

## 3.1 Introduction

Negative externalities occur when agents do not face the full social costs of their actions. Making agents internalize these costs can correct the market failure, but this task can be difficult when the demand for and damages from the externality producing good are heterogeneous. Under such heterogeneity, a uniform regulation that charges agents the average external cost will be inefficient (Peltzman and Tideman, 1972). This situation can lead to policy instruments being under, or even completely, unused.[1]

This paper examines a particularly extreme example of this phenomenon in the transportation sector. Despite economists lobbying for the use of gasoline taxes to correct congestion and local

[1] For example, "sin taxes" on the consumption of goods associated with negative outcomes like soda, cigarettes, and alcohol are uniform taxes, but demand and external costs (burden on health care system, drunk driving, secondhand smoke) are heterogeneous across agents. In these settings, demand is most inelastic for agents that are heavy users and most likely to impose the largest external costs on society, which may explain what is perceived as an underutilization of these taxes in many areas.

pollution externalities[2], they are generally only used to fund road infrastructure. It is possible that these potential benefits have gone unrecognized by voters and politicians because of the heterogeneity in travel demand and damages across regions. To illustrate this, consider increasing gasoline taxes to reduce congestion. As a result of the nonlinear relationship between travel demand and congestion, average congestion in the U.S. is much higher than the congestion level faced by an average driver.[3] This means that increasing the federal, or even a state, gasoline tax to reduce congestion would substantially improve welfare in congested urban areas, but the same cannot be said for uncongested rural areas. While economists have recognized this issue, they have struggled to recommend a more efficient fuel tax regime because of a lack of data on how these damages and travel demand elasticities vary over space.

This paper fills this gap in the literature by comparing the efficiency of a uniform fuel tax increase to a suite of revenue neutral county-level fuel taxes set using estimates of county-level travel demand elasticities and damages. The results reveal several empirical facts. First, county-level fuel taxes are significantly more efficient than the current uniform tax regime. The welfare benefits of county-level taxes relative to the revenue neutral uniform fuel tax increase are approximately $26 per capita annually. Second, these efficiency gains are realized by levying taxes on a small subset of large urban areas — just 5% of the sample counties. Finally, it is shown that county fuel taxes are likely less regressive than uniform fuel taxes.

These conclusions are reached by maximizing a societal welfare function constructed using county-specific travel demand and travel cost functions. The evaluation of this welfare function requires the estimation of county-level travel demand elasticities and damages[4], a task that is empirically

[2]As noted in Parry and Small (2005), taxes on local emissions or peak-period congestion may be better instruments for correcting these externalities, but are not widely implemented due to equity concerns, administrative burdens, and political opposition relative to gasoline taxes which are administratively simple and well established, even at very high rates.

[3]The relationship between traffic flows and speed is often characterized as a backward bending curve, with the backward bending segment described as "hypercongestion." See Small and Verhoef (2007), Li et al. (2018), and Kim (2018) for more information on this relationship.

[4]Congestion and pollution externalities are the focus of this paper due to data limitations. Because vehicle travel also produces collision and noise externalities, the welfare effects estimated in this paper are likely lower bounds to the true effects.

challenging for two reasons.

First, unbiased estimates of local travel demand elasticities must be recovered despite the endogenous relationship between vehicular travel and fuel prices. Reductions in fuel prices are expected to increase travel demand which, in turn, will increase fuel prices. This reverse causality biases estimates of the effect of fuel prices on travel demand towards zero. Previous literature has established two solutions for this issue — instrumenting for fuel prices using gasoline taxes (Coglianese et al., 2017; Li et al., 2014; Davis and Lutz, 2011) or utilizing the exogenous timing of vehicle inspections in areas with smog or safety check programs (Knittel and Sandler, 2018; Gillingham and Munk-Nielsen, 2016; Gillingham et al., 2015). However, these solutions are not applicable in this research setting. An instrumental variables (IV) approach that instruments for fuel prices using gasoline taxes does not provide sufficient identifying variation during the sample period (2013-2016). Over half of the contiguous U.S. states did not change their gasoline tax during the sample, and, as such, gasoline taxes provide no identifying variation for counties within these states. Further, even states with gasoline tax variation have only limited variation as gasoline taxes are not changed frequently. In addition, there is evidence that gasoline tax changes are also endogenously related to travel demand (Coglianese et al., 2017; Davis and Lutz, 2011).[5] Though vehicle inspection programs provide variation at the vehicle level, this strategy is only available in very limited geographic regions and would limit this paper's ability to examine the heterogeneity of damages and demand across the U.S.

Second, while Muller and Mendelsohn (2012) provide estimates of pollution damages at the county level, and a recent literature has investigated congestion damages in large cities (Couture et al., 2018; Kim, 2018; Li et al., 2018), reliable congestion damages are nonexistent for all areas other than the 50 or 100 most populous portions of the U.S. Intuitively, congestion damages are likely to be low in rural areas and high in urban areas. Data limitations and these small rural damages have almost certainly led to a lack of empirical evidence on rural congestion damages, yet it is exactly

[5]This relationship may arise if gasoline taxes are changed in response to changes in driving behavior, or if drivers engage in anticipatory fuel purchasing just prior to fuel tax changes.

this relationship between damages and urban areas which motivates this paper.

This paper addresses the first issue by utilizing an expansive data set on county-level travel demand and fuel prices with a novel instrument for local gasoline prices. The data on travel demand are derived from over 770 million hourly vehicle counts reported by the universe of traffic sensors across the U.S. while the county fuel prices are scraped from a leading industry website.[6] These geographically and temporally detailed data are combined with exogenous shocks to local fuel prices generated by county-level gasoline content regulations (often referred to as summer and winter fuel blends). These gasoline content regulations are local pollution controls that aim to reduce emissions of volatile organic compounds and ground-level ozone formation, but have been shown to increase fuel prices because they increase refining costs and segment fuel markets (Auffhammer and Kellogg, 2011; Brown et al., 2008; Chakravorty et al., 2008; Muehlegger, 2006).[7] These regulations are levied on the entire U.S. with the stringency of a region's regulation being a function of its proclivity to produce ozone and, potentially, its level of vehicle travel — two issues that can be controlled for empirically.[8]

By combining these data with an instrumental variables (IV) strategy that instruments for county fuel prices using gasoline content regulations, it is possible to identify travel demand elasticities for the full sample as well as county-specific elasticities. Though it has long been hypothesized that travel demand elasticities vary substantially across geographies, few papers have set out to empirically establish this fact.[9] As such, this paper estimates the most comprehensive set of geography

[6]These data are scraped from GasBuddy.com. GasBuddy.com is a website which reports retail gasoline prices at over 150,000 retail locations uploaded by a large group of users.

[7]Gasoline content regulations vary at the state, county, and even city level. For example, Phoenix, AZ has a more stringent gasoline content requirement than the rest of AZ. In addition to the variation in regulation stringency, there also exists variation in the dates of the year the regulations are active.

[8]More background on ozone formation and a discussion of the exogeneity and relevance of gasoline content regulations is provided in Section 3.3.2.

[9]Spiller et al. (2017) estimates the heterogeneous effects of a gasoline tax by examining variation in elasticities over space. Knittel and Sandler (2018) examine the efficiency of county fuel taxes in California for correcting pollution externalities, finding that regional heterogeneity in demand elasticities plays only a small role in the inefficiency of a uniform tax. This finding, potentially due to the focus on California in Knittel and Sandler (2018), is at odds with this paper's results which show that regional heterogeneity in travel demand elasticities significantly affect transportation policy efficiency.

specific travel demand elasticities available in the literature, which provides a richer understanding of the effects of transportation policy across regions in general.

Next, the lack of reliable congestion damages is overcome by developing a new strategy for estimating the effects of traffic flows on travel speeds. This strategy utilizes the previously mentioned traffic sensor data as well as data on projected trip durations in traffic at varying departure times obtained from Google Maps. These data allow for the estimation of the effect of vehicle counts on average trip speeds, which can be used to determine how congestion damages vary with travel demand. Like the travel demand elasticities, the methodology used and the congestion damages estimated are useful beyond the current analysis. For example, while the current estimates provide only an average congestion damage, the methodology can easily be extended to produce estimates of time varying county (or city) specific pigouvian congestion taxes.[10]

These county-level travel demand elasticities, congestion damages, and pollution damages from Muller and Mendelsohn (2012) are then combined in a simulation which maximizes societal welfare by setting county-level fuel taxes. To examine the relative efficiency of county fuel taxes and uniform fuel taxes, the simulation requires that the county fuel taxes must generate the same amount of revenue as a uniform \$0.10 fuel tax increase on all counties.[11]

Estimation of these models reveals significant heterogeneity in both travel demand elasticities and congestion damages across regions. The full sample travel demand elasticity estimates range from -0.357 to -0.458, slightly larger in magnitude than other recent estimates.[12] Estimation of county-specific travel demand elasticities reveals significant heterogeneity across regions. In particular, large central metro areas are shown to have travel demand elasticities almost twice as large as

[10]In theory, performing this analysis would simply require a larger amount of Google Maps data. The current work does not delve into this issue because, unlike pigouvian congestion taxes, fuel taxes cannot vary by time-of-day. Therefore, the analysis focuses on the average cost of congestion throughout the day.

[11]An unconstrained version of the maximization problem is also optimized to calculate optimal fuel taxes in each of the sample counties. These results can be seen in Appendix Figure C.5.

[12]Recent estimates have ranged from -0.1 to -0.4 (Gillingham, 2014; Gillingham et al., 2015; Bento et al., 2009; Small and Van Dender, 2007; Knittel and Sandler, 2011; Graham and Glaister, 2002).

those in small metro areas.[13] Because of this heterogeneity, the somewhat large full sample elasticity estimate is not surprising given that this paper uses a sample which skews towards urban areas, to produce county-specific estimates for many areas of the U.S., while previous research has focused on estimating an average elasticity for the entire U.S.[14] Estimates of the effect of vehicle counts on trip speeds are also correlated with urbanization levels. The estimated effects again show significant heterogeneity with increases in vehicle counts leading to decreases in average speed over four times larger in magnitude in large central metro areas than in small metro areas. Finally, the simulation reveals that county-specific fuel taxes improve welfare by approximately \$1-\$3 billion dollars (\$7.10-\$25.84 per capita) annually in the 339 sample counties relative to a revenue neutral \$0.10 per gallon uniform fuel tax increase. Higher income counties also pay larger shares of the total of tax burden under the county-specific tax scheme, and, as such, it is likely to be less regressive than uniform fuel taxes. Though this paper focuses on the transportation sector, these insights apply to the regulation of externalities in general.[15]

The remainder of this paper is organized as follows. Section 3.2 provides a theoretical framework for the welfare analysis. Section 3.3 describes the data and methodology used in the empirical analysis, and section 3.4 explores the results. Section 3.5 examines the welfare and policy implications of the results, and section 3.6 concludes.

[13]This urban-rural divide in travel demand elasticities is similar to that found in Spiller et al. (2017) for gasoline demand elasticities.

[14]Further, estimating demand elasticities at a coarse temporal level, as is done in most of the aforementioned works, has also been shown to bias results towards zero (Levin et al., 2017).

[15]Examples include pollution from electricity production, noise pollution from construction or airplanes, reductions in property values caused by tall buildings which restrict views, and many others.

## 3.2 Background and Theory

### 3.2.1 Regulating externalities under heterogeneity

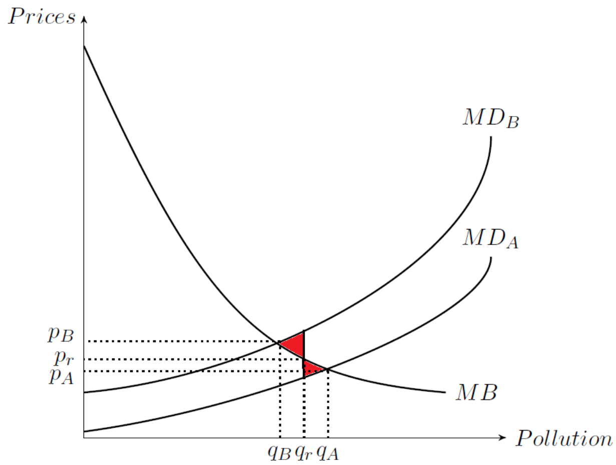
A simple framework can be employed to illustrate how locally targeted regulations may be welfare improving over uniform regulations when correcting heterogeneous externalities. Assume that spatially heterogeneous pollution externalities exist in regions A and B. For simplicity, marginal benefits from pollution are the same across regions, but marginal damages vary.[16] In the presence of such spatially heterogeneous externalities, policy makers have several options.[17] Regulators may set separate policies in the two regions or opt to set one regulation that governs both regions.

Figure 3.1 illustrates the outcomes for both uniform regulation and targeted regulation. When a regulator may set multiple prices, the prices $p_A$ and $p_B$ are selected, resulting in the socially optimal quantity of pollution in both regions. In contrast, when a regulator can set only a single uniform price for pollution across both regions, price $p_r$, between the socially optimum prices in both regions, is selected. Even if a regulator sets $p_r$ to minimize dead weight loss (DWL), it is impossible to reach the socially optimal level of pollution in both regions with a single price control. Therefore, setting a single price control by definition results in DWL, illustrated by the shaded regions in the figure. The goal of this paper is to both draw attention to this DWL and measure its general magnitude.

[16]This framework ignores the potential for spillover effects between the two regions.

[17]For simplicity, it is assumed that regulators may only set price controls, but this framework may also be employed using quantity controls.

Figure 3.1: Regulation of Heterogeneous Externalities




*Notes*: The figure illustrates the outcomes of socially optimal regulation and uniform regulation for two regions with identical marginal benefits from pollution, but heterogeneous marginal damages. A regulator required to place a single price (or quantity control) in regions A and B will choose some regulation in between the social optimum of both regions. While the regulator chooses the price (or quantity) control that minimizes dead weight loss (DWL) across the regions, having one broad regulation, $p_r$ will result in at least some DWL relative to the socially optimal outcomes achieved by setting two prices, $p_A$ and $p_B$. The DWL is illustrated by the shaded areas.

This example has shown that imposing uniform regulations when damages vary across geographies culminates in DWL to society. However, the phenomenon being studied in this paper involves both heterogeneous damages and demand elasticities. It is useful in this situation to consider how marginal benefits translate into demand for the good. As will be shown in Section 3.4, demand for externality producing goods is more elastic in regions with higher damages. This relationship will lead to a steeper marginal benefits curve in region B, and even larger DWL.

### 3.2.2 Theoretical Framework

The welfare implications of uniform fuel taxation relative to county-specific fuel taxation based on local travel demand elasticities and damages can be calculated by optimizing the following welfare function.

$$\max_{V_i \ \forall \ i} \sum_{i=1}^{339} \int_0^{V_i} P_i(v_i) dv_i - C_i(V_i) \tag{1}$$

$$\text{Subject to } \sum_{i=1}^{339} t_i \cdot V_i \cdot \theta_i = \sum_{i=1}^{339} \bar{t} \cdot \bar{V}_i \cdot \theta_i \tag{2}$$

$$\text{and } V_i \leq V_i^0 \tag{3}$$

Where $\int_0^{V_i} P_i(v_i) dv_i$ represents the total benefits from vehicle travel and $C_i(V_i)$ represents the total damages from vehicle travel in county $i$ (with 339 total counties). The equality constraint mandates that the total revenue generated from county-specific taxes, $t_i$, are equivalent to the revenue generated by a uniform fuel tax increase, $\bar{t}$. That is to say, the two tax regimes are revenue neutral.[18] The inequality constraint mandates that equilibrium vehicle travel must be less than or equal to vehicle travel when there is no fuel tax increase. In other words, the county-specific price controls must be taxes—not subsidies ($t_i \geq 0$ for all $i$).[19]

The inverse demand function for vehicle travel, can then be defined as

$$P_i(V_i) = (\frac{A_i}{V_i \cdot \theta_i})^{\eta_i} \tag{4}$$

[18]The revenue neutrality constraint is levied for two reasons. First, it makes for a simple welfare comparison between county fuel taxes and the uniform federal fuel tax. Second, it alleviates concerns about public infrastructure funding in the county fuel tax regime. Interestingly, the county fuel tax regime may shift the funding of public road infrastructure to areas with higher funding demands, thus eliminating some political economy concerns about the centralized provision of local public goods (Knight, 2004).

[19]Relaxing this assumption results in qualitatively similar results, but small metro areas may receive fuel subsidies which increase vehicle travel. This allows for even larger fuel taxes to be levied on large metro areas with more elastic demand and higher damages.

This function represents per mile fuel costs as a function of vehicle counts.[20] $V_i$, the choice variable, is the average hourly vehicle count in the county. $\theta_i$ scales vehicle counts to VMT and is determined by $\theta_i = \frac{VMT_i^0}{V_i^0}$, where $VMT_i^0$ is average county daily VMT obtained from the Highway Performance Monitoring System and $V_i^0$ is the average hourly county vehicle count in the sample.[21] $\eta_i$ is the travel demand elasticity to be estimated in Section 3.3.[22] $A_i$ can then be determined using average vehicle counts and per mile fuel prices in the county.

The total costs of vehicle travel in a county, the average external cost per mile driven multiplied by the total miles driven in a county, can be written as,

$$C_i(V_i) = \left(\frac{VOT_i}{\alpha_i + \beta_i \cdot V_i} + \kappa_i\right) \cdot V_i \cdot \theta_i \tag{5}$$

where $VOT_i$, a county's value of time, is the county's hourly median wage divided by two[23], $\beta_i$ is the effect of increasing vehicle counts on county speed to be estimated in the next section, and $\alpha_i$ is calculated using county average speed and vehicle count values. $\kappa_i$ is the per mile pollution damages for the county. This value is determined by scaling the per ton emission damages calculated in Muller and Mendelsohn (2012) to per mile damages for a representative vehicle using the EPA's estimate of average vehicle fuel economy in miles per gallon (MPG) and emissions per gallon.[24]

[20] Per mile fuel costs are determined for a representative vehicle in a county. This is done using the EPA's estimate of average miles per gallon in the U.S. vehicle fleet of 24.1022.

[21] The Highway Performance Monitoring System gives estimates of daily MSA VMT. These estimates are transformed into daily county VMT by weighting an MSA's VMT by each county's population. This assumes that, within an MSA, each county's VMT is proportional to its population.

[22] As will be seen in section 3.4, several of the county-specific travel demand elasticities estimated are positive. Positive travel demand elasticities imply that vehicle travel increases when fuel prices increase. As such, each county is assigned the average travel demand elasticity for its urbanization level.

[23] There are a wide range of value of time estimates in the literature. Following Small and Verhoef (2007), this paper uses half the hourly wage as the value of time.

[24] A limitation of this paper is the lack of data on the distribution of vehicles within a county. However, Knittel and Sandler (2018) find that the dirtiest vehicles are those with the most elastic demand. Results from this paper also show that more urban areas tend to have more elastic travel demand. This suggests that urban areas may also contain the dirtiest vehicles, and, as such, assuming a representative vehicle should not qualitatively change the result that large urban areas require the largest taxes.

Damages from VOC, $NO_x$, $PM_{10}$, and $PM_{2.5}$ emissions are included in the calculation.[25] The first term captures the costs of increasing vehicle travel by one mile in terms of increased congestion and pollution costs in county $i$. Multiplying this by $V_i \cdot \theta_i = VMT_i$ produces the total costs of vehicle travel in the county.

After estimating a vector of travel demand elasticities, $\eta$, and a vector of the effects of vehicle counts on speeds, $\beta$, it will be possible to maximize this societal welfare function, subject to the constraints, by choice of a vector of vehicle counts, $V$. Then, county-specific fuel tax values, defined as deviations from the county's average fuel price during the sample, can be determined by rearranging and evaluating the inverse demand function at the optimum $V^*$. By evaluating the welfare function at the optimal vehicle counts and the vehicle counts that would arise under a uniform fuel tax increase, $\bar{V}$, it is possible to compare the welfare effects of targeted fuel tax regimes and uniform fuel tax regimes.

## 3.3 Empirical Setting

The empirical section begins by describing the data used in the analysis before outlining the identification strategy used to estimate travel demand elasticities. The discussion then turns to the methodology used in estimating the effects of traffic counts on trip speeds.

[25] Muller and Mendelsohn (2012) provides damages for the above pollutants, NH3, and S02. Unfortunately, the EIA does not provide emissions estimates for NH3 or S02, and limits the pollutants included in the analysis. While the analysis could also be extended to include global pollutants, it would require additional assumptions. The approach taken in this paper constructs conservative estimates of pollution damages which, in turn, provide conservative welfare estimates.

### 3.3.1 Data

#### Overview

An expansive data set was compiled from several unique sources to analyze the welfare effects of county-level fuel taxes. The data cover the period from 2013-2016. To estimate county travel demand elasticities, the analysis combines traffic flow data from the universe of traffic sensors in the U.S., local fuel price data from 339 counties scraped from a major industry website, information on regional gasoline content regulations, and data on weather conditions. To estimate congestion effects, trip distances and projected trip durations in traffic at various departure times obtained from Google Maps' application program interface are also combined with the traffic sensor data to estimate county-specific congestion effects. The county-specific travel demand elasticities, congestion effects, and preexisting estimates of county pollution damages from Muller and Mendelsohn (2012) are then combined to simulate the welfare effects of a uniform fuel tax levied on all counties in the sample and a revenue neutral suite of county-specific fuel taxes set based on local parameters. This section describes the data sources used and provides summary statistics.

#### Travel demand data

Data on travel demand come from over 770 million hourly vehicle counts observed by the universe of traffic sensors across the U.S. obtained from the Federal Highway Administration (FHWA) between 2013 and 2016. These hourly vehicle counts are the standard measurement of traffic flow used in the transportation engineering literature (Small and Verhoef, 2007), and are reported to the FHWA by state departments of transportation.

Each hourly vehicle count includes the hour and date of observation and a traffic sensor identifier that indicates the direction and lane of travel. In other words, each individual lane is defined as a separate traffic sensor. These data are matched to more detailed information on the traffic sensors (also provided by the FHWA) using their identifiers. These data include county FIPS codes and latitude and longitude observations that allow traffic sensors to be assigned to counties.

The hourly sensor data contain some missing observations caused by sensor outages. These missing data are random, and, as such, only introduce measurement error into the analysis. This measurement error will bias estimates away from statistical significance. However, the counts for the hours immediately before the outages may be biased towards zero if the sensors were not active for the entire hour. This means sensor outages may lead to a sensor only reporting a partial count for an hour. To correct for this, sensor days with less than 24 hours of data are dropped from the analysis, and the data is then aggregated to the average hourly sensor count for each day.[26]

**Fuel price data**

Local fuel price data was obtained by web scraping price observations from GasBuddy.com, a website that crowd-sources gasoline prices at retail locations from a large base of users. GasBuddy's goal is to provide drivers with up-to-date fuel prices at nearby gasoline stations to help drivers find the best prices. When users purchase fuel or observe fuel prices at any of the 150,000+ stations monitored by GasBuddy they can update the fuel prices on the website to earn points that can be used to enter drawings for free gasoline. In addition, fuel prices are also updated by gasoline stations and credit card partners through purchases.

While GasBuddy only displays gas station level data contemporaneously, they do provide historical data that is slightly more aggregated. This fuel price data is reported for over 400 locations across the U.S. with 2-3 price observations per week during the sample period, 2013-2016. These areas are assigned to their corresponding counties and the data are then aggregated to county weekly average fuel prices before being adjusted to 2017 Q4 dollars. Because the fuel price data does not cover all counties in the U.S., these data are the limiting factor determining which counties are

[26] Appendix Figures C.1 and C.2 illustrate average hourly traffic counts and fuel prices for a sensor in a rural and urban area respectively. Appendix Figure C.3 illustrates a sensor that experiences intermittent outages.

included in the sample. In total, 339 counties can be assigned fuel price data during the sample period.

### Gasoline content regulations data

Gasoline content regulations were obtained from the Energy Information Administration (EIA) (and cross-referenced using State Implementation Plans). The regulations may cover cities, partial counties, entire counties, or even an entire state. Because the unit of analysis in this paper is the county, and some counties may have multiple regulations, counties are coded to have the most stringent content regulation on record within the county. For example, while Phoenix, AZ has a more stringent content regulation than the rest of Maricopa county, the entire county is listed as having the more stringent regulation. While this introduces some measurement error into the content regulation variables, this method of coding the content regulations provides a conservative estimate of their effect. These conservative estimates are preferred over a more precise coding of the variable focused on sensor locations because of the possibility that unobservable regulation spillover may occur, biasing the results towards finding a significant effect.[27]

### Congestion data

Data from Google Maps were obtained to estimate county-specific effects of traffic volume on trip speeds. To begin, random coordinates within each of the sample counties were drawn from a uniform distribution placed over the U.S. and matched together to create 16 origin-destination (OD) pairs per county. Trip distance, trip travel times in traffic, and trip travel times without traffic were then obtained for each of these OD pairs by querying Google Maps' application program

[27]For example, assume half a county requires RFG while the other half requires RVP 9.0. If sensors within each county are assigned a separate treatment variable based on their location it is possible that RFG fuel may be sold in the RVP 9.0 portion of the county. This creates a spillover effect which may bias the coefficient on the RVP 9.0 treatment variable towards a significant effect. However, coding all areas with the RFG treatment variable, the more stringent regulation, provides a conservative estimate of the effect of RFG.

interface.[28] This process was then repeated for each OD pair with different "departure times" to provide information on how trip travel times in traffic vary with the hour-of-day and day-of-week. Trip distances and travel times in traffic are then combined to determine a trip's average speed at a particular departure time.

Travel times in traffic between points were calculated using Google Maps' "best guess" travel time algorithm. Travel times between points were calculated every three hours with a "departure time" starting at 01:00 Monday, February 3rd, 2020 and ending 23:00 Wednesday, February 5th, 2020. Google does not allow users to access historical data directly. This means determining travel times at points in the past is not possible. The year 2020 was used partly because of this lack of historical data, and because traffic conditions at the time the data are accessed receive more weight in predicting trip durations if the set "departure time" is in the near future. Using estimates for the year 2020 means the data are not likely influenced by the time of draw (which occurred between 8/31/2018 and 9/5/2018).

**Other data sources**

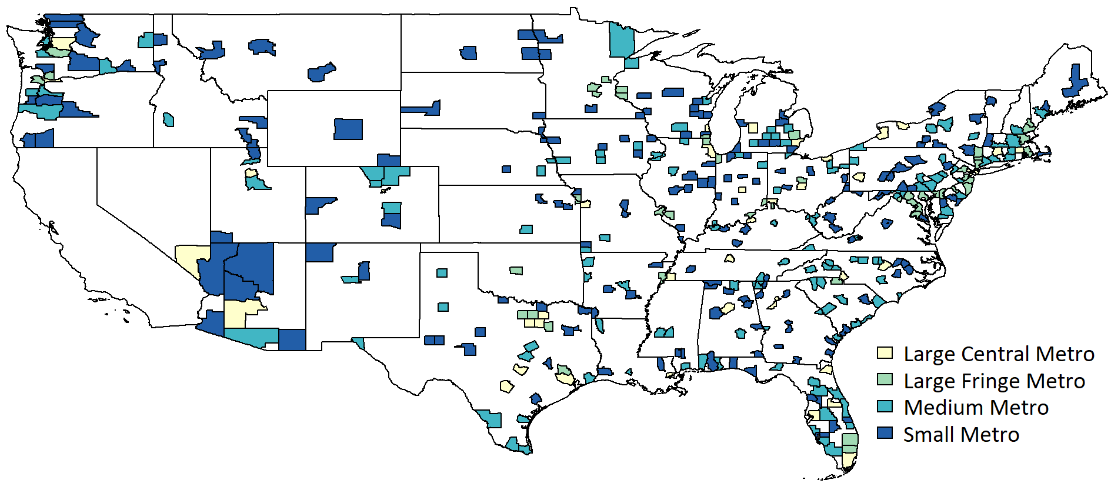
Daily weather data come from the National Climatic Data Center Global Historical Climatology Network-daily. These data provide daily minimum and maximum temperatures, precipitation, snowfall, and snow depth. Weather station data are aggregated to county-level daily weather. Because missing weather station data can explain a significant portion of weather variation, missing weather station data are imputed using methods established in the literature prior to aggregation (Auffhammer et al., 2013).[29]

County urban-rural levels were obtained from the National Center for Health Statistics (NCHS). The NCHS defines six levels of urbanization for counties in the U.S. These levels include four metropolitan classifications (large central metropolitan, large fringe metropolitan, medium metropoli-

[28]This application program interface was queried using Python's "gmaps" package.
[29]Missing station observations are imputed via a regression that uses data from a station's nearest active neighbor.

tan, and small metropolitan) and two nonmetropolitan classifications (micropolitan, and non-core). Due to the nature of the fuel price data, the analysis includes only counties in the metropolitan classifications.[30] Figure 3.2 provides a map indicating the urban classification of each county in the sample.

Figure 3.2: Map of Counties by Urbanization Level



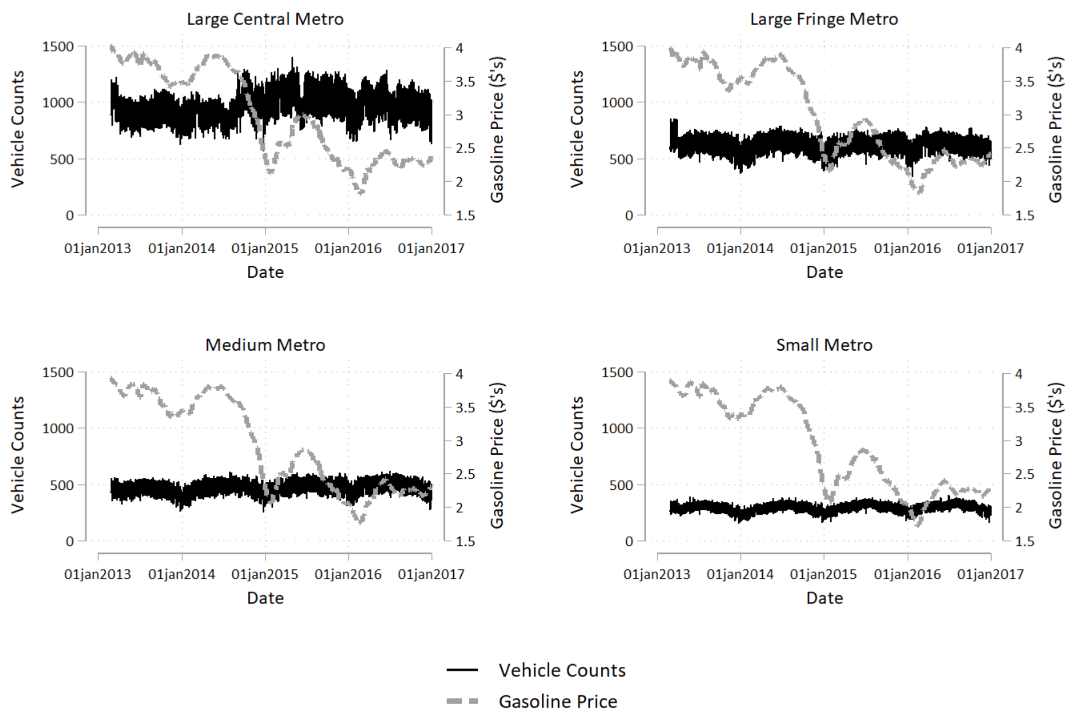

*Source*: National Center for Health Statistics.

### Summary statistics

Figure 3.3 depicts the evolution of vehicle counts and gasoline prices for each of the urbanization levels in the analysis over the sample period. Vehicle counts are the average hourly count across all sensors within each urbanization level for each day. Gasoline prices are the average weekly price across each urbanization level reported in 2017 Q4 dollars.

[30]The metropolitan classifications show considerable variation in urbanization. For example, large central metropolitan counties must contain MSAs with a population of at least one million and meet several other criteria while small metros are defined as counties in MSAs of populations less than 250,000. The smallest county by population in the data set has fewer than 20,000 residents. Further details on County Urbanization levels can be found at `https://www.cdc.gov/nchs/data/series/sr_02/sr02_166.pdf`.

Figure 3.3: Vehicle Counts and Gasoline Prices

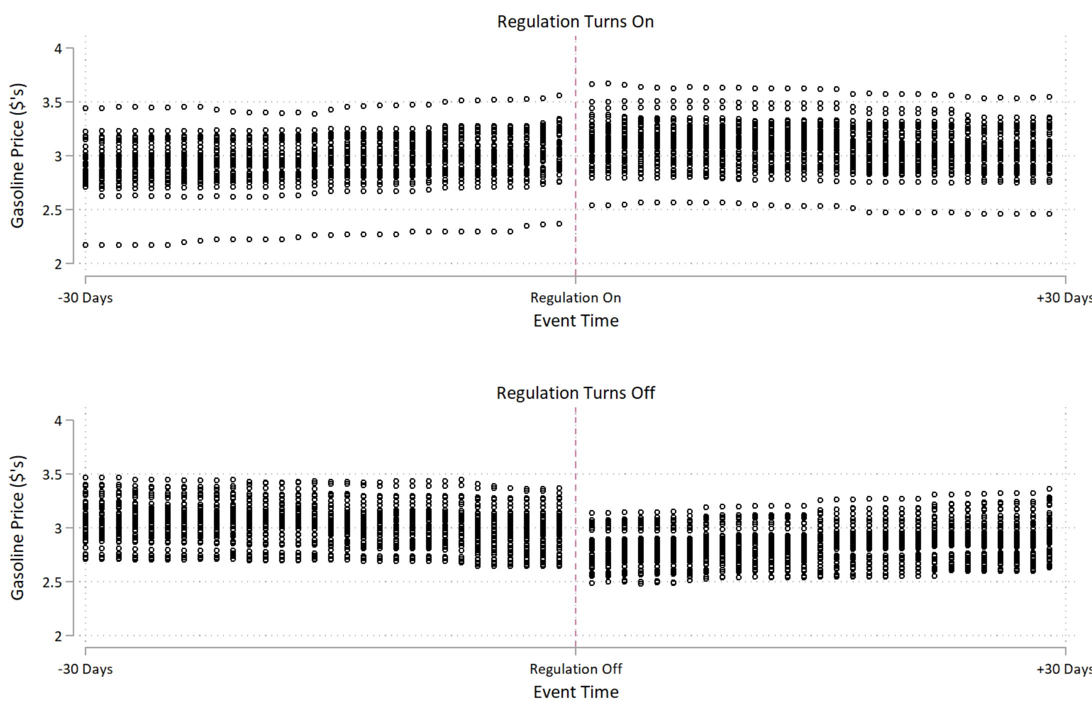



*Notes*: The vehicle count data reports the daily average hourly count across all sensors in an urbanization level. Gasoline prices are urbanization level week averages converted to 2017 Q4 dollars.

This figure illustrates several key facts for the analysis. First, average vehicle counts in urban areas are approximately double those in rural areas. This highlights the disparity in the level of travel demand across regions and, due in part to the nonlinear relationship between vehicle travel and congestion, suggests large central metros are significantly more congested. Second, despite the disparity in the level of travel demand, all regions follow a similar trend. A general increase in vehicle counts is shown during summer months with a decrease in the winter months. Gasoline prices also show a remarkably similar pattern with fuel prices rising in the summer and declining in the winter. This relationship suggests that travel demand and fuel prices are endogenously related, and that the IV identification strategy outlined in the following section is necessary to recover unbiased travel demand elasticities. Interestingly, gasoline prices across regions appear to be almost identical in both trends and levels across all urbanization levels. This suggests that there is little correlation between urbanization level and fuel prices. Finally, this figure shows that vehicle counts vary significantly over short periods of time. This volatility can be attributed to the variation in travel demand between weekends and weekdays and suggests the use of day-of-week fixed effects to improve estimate precision.

Figure 3.4 depicts fuel prices around the time of a gasoline content regulation change. Because there are four content regulations that vary across counties both in timing of regulation and stringency, this figure depicts gasoline prices in event time around two windows—when the gasoline regulations turn on for a county in a year and when they turn off. The vertical line depicts the day the regulation either turned on or turned off and the plotted points depict the average fuel price for a state at that event time. The figure begins at -30, thirty days before the regulation change, and ends at +30, thirty days after the regulation change.

Figure 3.4: Gasoline Prices and Gasoline Content Regulations




*Notes*: Event time denotes the number of days before or after the regulation has changed with 0 being the day of change. Points plotted are average fuel price for a state event day in 2017 Q4 dollars. Regulations turning on is expected to increase prices and regulations turning off is expected to decrease prices.

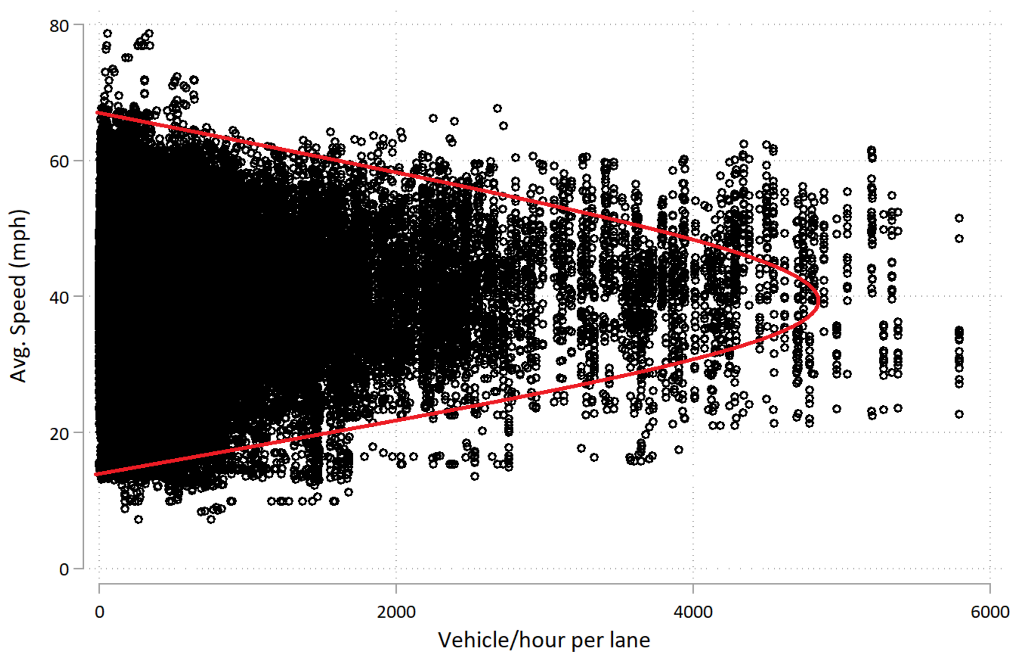
This figure shows that gasoline content regulations substantially influence gasoline prices. There is a large increase in gasoline prices when the regulations turn on in the spring or summer and a large decrease when they turn off in the fall or winter. While the simple average values plotted here do not provide conclusive evidence that gasoline content regulations are a relevant instrument for fuel prices, this figure provides strong suggestive evidence.

Figure 3.5 plots Google Maps predictions of trip average speed against the sample average hourly vehicle count in that county for that particular hour-of-day and day-of-week. While the classic speed-flow diagram[31] plots speeds and traffic flows (hourly vehicle counts) for a single traffic sensor, this figure does so for all of the OD pairs in the sample. The standard nonlinear relationship between speed and volume can be seen clearly. Traffic flow can be zero when there are no vehicles or too many vehicles. At the top of the red curve, there are few vehicles on the roads, roads are uncongested, and traffic moves at free flow speeds. Moving to the right, more vehicles enter the road and vehicles begin to slow. The roads capacity is eventually exceeded and the curve bends backwards, indicating “hyper congestion.” On this portion of the curve, there are too many vehicles on the road and speeds plummet which decreases vehicle flows. This figure both illustrates the nonlinear relationship between congestion and travel demand and validates the use of Google Maps’ predictive travel time and historic vehicle counts to establish the relationship between speed and traffic flows.

Further descriptive statistics are available in Appendix Table C.1 which provides metrics on each of the variables used in the analysis.

[31] For more information on speed-flow curves see Small and Verhoef (2007).

Figure 3.5: Speed-Flow Relationship




*Notes*: Vehicle counts are day-of-week hour-of-day averages across all sensors for the corresponding county throughout the sample period. Speed is the average trip speed predicted using the "best guess" algorithm via Google Maps.

### 3.3.2 Travel demand elasticities

#### Background on gasoline content regulations

Gasoline content regulations are used as instruments for local fuel prices in the analysis. These gasoline content regulations are environmental controls mandated by the Environmental Protection Agency under the Clean Air Act Amendments. There are several types of gasoline content regulations which vary in stringency and regulatory style, but all share the goal of reducing mobile source emissions and, in particular, ground-level ozone formation. The regulations studied in this paper include Reid Vapor Pressure (RVP) Regulations and Reformulated Gasoline (RFG) Regulations.[32] Because the regulatory goal of these programs is to reduce ground-level ozone formation they are seasonally enforced in the summer months when warm sunny weather creates conditions conducive to ozone formation.[33] This seasonal variation has led these regulations to be known broadly as "summer fuel blends."

RVP regulations are the most commonly used gasoline content regulations in the U.S. RVP is a pounds per square inch (psi) measure of the intensity that volatile organic compounds (VOCs) are emitted from gasoline. These regulations place an RVP psi limit on the gasoline that can be sold in an area. Currently, RVP regulations are mandated at three stringency levels, RVP 9.0 psi, RVP 7.8 psi, and RPV 7.0 psi, where lower psi ratings indicate a more stringent regulatory scheme that requires removing more VOCs from the fuel.

Federal RFG regulations are similar RVP regulations in that they also mandate that VOC emissions are reduced, but they also restrict emissions of benzene, a toxic air pollutant, and include minimum oxygenation requirements. For these reasons, RFG regulations are more stringent than any of the

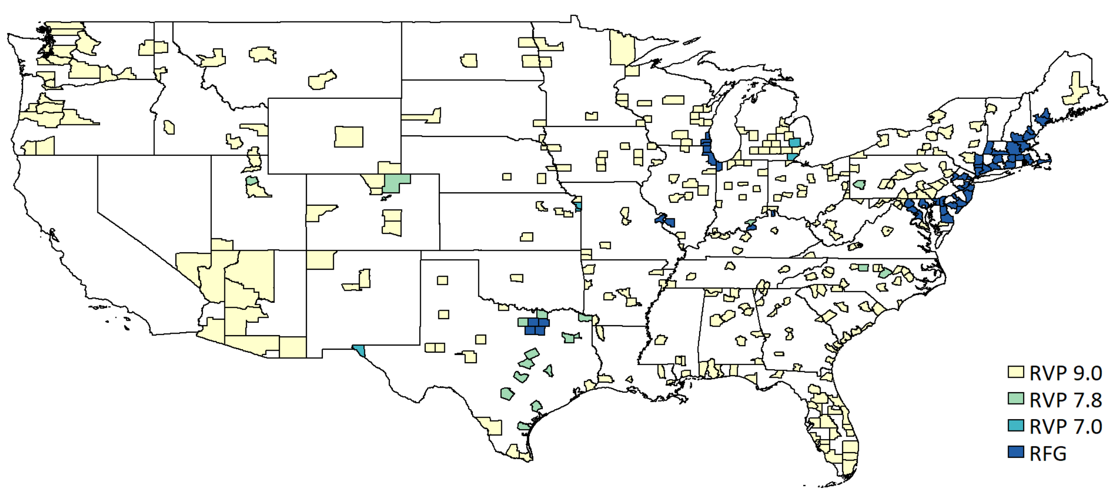
[32]There are additional fuel regulations, such as California's RFG program, the Oxygenated Fuel Program, and Texas' Low Emission Diesel Regulations, that are not included in this analysis. These regulations are excluded as they either pertain to only diesel fuel, are found in only a few areas, or fuel price data for the regions governed by these policies is unavailable.

[33]Ground-level ozone formation exhibits a Leontief production function where inputs are nitrogen oxides and volatile organic compounds which only react in warm sunny weather. For a more in-depth discussion of ground-level ozone formation and gasoline content regulations see Auffhammer and Kellogg (2011).

RVP regulations and meant for areas which were in severe nonattainment of the EPA's ozone standards.

These separate gasoline content regulations were implemented in various parts of the country as the EPA deemed necessary, or regions opted into them as part of their State Implementation Plans to reach ozone standard attainment. The variation in regulations across the U.S. can be seen in Figure 3.6. While regulatory decisions were made on the basis of ozone levels, which are in part determined by vehicular traffic, the Leontief-like production function of ground-level ozone formation which combines nitrogen oxides and VOCs in warm sunny conditions makes the distribution of regulations across the country largely a function of weather. The extent that gasoline content regulations are influenced by travel demand should be irrelevant for this study as the regulations have changed very little over the past decade with almost no variation during the sample period. This means that the regulations are unlikely to be influenced by contemporaneous travel demand and, as such, the regulations, conditional on the inclusion of a region fixed effect, should meet the exogeneity condition for an instrumental variable.

Figure 3.6: Gasoline Content Regulations




*Notes*: The figure illustrates gasoline content regulations across the U.S. Unshaded areas are counties that do not have matching price data in the analysis sample, but all counties in the U.S. are governed by at least RVP 9.0 regulations. The map depicts regulations as of December 2016.

The regulations should also meet the relevance condition for an instrumental variable. Past evidence that gasoline content regulations increase gasoline prices can be found in (Auffhammer and Kellogg, 2011; Brown et al., 2008; Chakravorty et al., 2008; Muehlegger, 2006). These papers find that higher refining costs incurred by more stringent gasoline content regulations increase gasoline prices. Further, as the regulations vary considerably across regions, they segment fuel markets. This market segmentation results in higher fuel prices during periods of high demand or supply disruption as fuel prices cannot be arbitraged across regions.

**Identification of travel demand elasticities**

The empirical analysis begins by estimating travel demand elasticities. The goal is to estimate the causal effect of fuel price shocks on the vehicle counts observed at sensors. A least squares model that directly identifies the effects of fuel prices on vehicle counts is not appropriate here because it does not address the endogeneity of fuel prices. Because increases in travel demand cause an increase in the price of fuel, there will be a spurious correlation between fuel prices and the error term in a least squares regression. This correlation will bias the estimates of the effect of fuel prices towards zero.

This paper proposes an instrumental variables strategy which employs a novel instrument for fuel prices, gasoline content regulations, as a solution to this endogeneity. This technique overcomes the documented lack of instruments for fuel prices that are both exogenous and relevant (Coglianese et al., 2017; Davis and Lutz, 2011). As mentioned previously, these gasoline content regulations have been shown to increase fuel prices because they increase refining costs and segment fuel markets due to varying regulation stringency across regions (Auffhammer and Kellogg, 2011; Brown et al., 2008; Chakravorty et al., 2008; Muehlegger, 2006). This means that gasoline content regulations are relevant instruments for fuel prices.

Further, gasoline content regulations are exogenous to contemporaneous travel demand for several reasons. First, they were originally implemented as seasonal pollution controls to combat ground-level ozone formation, and there has been little variation in the regulations at the county level during the sample period. This suggests that there is no contemporaneous correlation between a county's travel demand and the content regulations in that county, and, as such, a county or traffic sensor fixed effect should resolve any endogeneity between an area's content regulation and its travel demand. Second, because ozone formation requires warm and sunny weather, more stringent gasoline content regulations are, for the most part, found in regions with warmer climates. This correlation can easily be resolved by controlling for weather, and, again, including county or traffic sensor fixed effects to control for the average climate of a region. Finally, because these are seasonal regulations and there is well known seasonal variation in travel demand, month-of-year fixed effects are included to control for this correlation.

The model is then estimated on the full sample using the following system of equations,

$$\ln(P_{ijt}) = \tau + \delta \cdot R_{ijt} + \phi \cdot X_{ijt} + \S_i + \rho_t + \nu_{ijt} \tag{6}$$

$$\ln(V_{ijt}) = \omega + \eta \cdot \ln(\widehat{P_{ijt}}) + \psi \cdot X_{ijt} + \S_i + \rho_t + \epsilon_{ijt} \tag{7}$$

where $i$ denotes a traffic sensor, $j$ denotes county, $t$ denotes day, $P$ is gasoline price, $R$ is a matrix of indicator variables for the gasoline content regulations used as instruments, $X$ is a matrix of weather controls, $\S_i$ is a traffic sensor fixed effect, $\rho_t$ includes month-of-year and day-of-week fixed effects, and $\nu_{ijt}$ is the error term clustered at the county level. The predicted prices generated in equation (6) are then used in equation (7), which predicts average hourly vehicle counts, $V$, as a function of the fuel prices. Depending on specification, $\rho_t$ may be month-of-year by county and day-of-week by county fixed effects, to bring the estimates more in line with the county-specific elasticity regressions discussed below. Here, the estimated price coefficient, $\eta$, is interpreted as the sample average travel demand elasticity.

After travel demand elasticities are estimated using the full sample, the analysis moves to estimating county-specific elasticities. This is done by estimating a series of 339 county-specific

IV models. This analysis employs identical estimating equations, with the exception that the $j$ subscripts can be dropped, and the time fixed effects are now county-specific day-of-week and month-of-year fixed effects. In addition, the estimated price coefficients, $\eta$, are interpreted as the county-specific travel demand elasticity. This distribution of elasticities can then be used to analyze the heterogeneous effects of exogeneous fuel price shocks on local travel demand.

### 3.3.3 The effects of travel demand on trip speed

The effects of travel demand on trip speed are estimated using data on vehicle counts and projected trip durations and trip distances from Google Maps. To begin, the four years of traffic sensor data are aggregated to the county hour-of-day day-of-week level (e.g. the average vehicle count across all sensors in a county on Monday at 14:00 or Tuesday at 02:00). These data are then matched to corresponding Google Maps predictions of a trip's average speed with the same departure time. In this fashion, it is possible to estimate the effect of increasing the number of vehicles on roads in a county at a particular hour-of-day and day-of-week on trip speeds.

The county-specific estimation equation (8) is,

$$\text{Speed}_{int} = \mu + \beta \cdot (V_{nt}) + \lambda_i + \omega_{int} \tag{8}$$

where $\text{Speed}_{int}$ is an origin-destination pair $i$'s average speed on day $n$ and hour $t$, $(V_{nt})$ is a county's average vehicle count across all sensors, $\lambda_i$ is an origin-destination pair fixed effect, and $\omega_{int}$ is the robust error term. $\lambda_i$ controls for route specific unobservable characteristics and the average level of traffic on the route. The intercept, $\mu$, can be interpreted as a county fixed effect. The coefficient of interest, $\beta$, indicates the effect of increasing vehicle counts on the average trip speed in a particular county. It should be noted that the simultaneity of speed and an individual's decision to drive at a given point may bias the estimates of $\beta$. Individuals may postpone or even cancel vehicle trips when congestion increases their personal costs of travel. This relationship will bias the results away from finding a significant effect. Further, one might expect the degree of bias to increase with the level of urbanization. In other words, this simultaneity is more important for congested urban areas than it is rural areas. This means that the estimates produced from this equation are likely a lower bound to the true effect, and, in particular, the true effects for more urban areas may be even larger in magnitude. Because the estimates of $\beta$ show a pattern of decreasing absolute value as urbanization levels decrease, the true effects may accentuate this pattern even more. If this is the case, the welfare effects found in the section 3.5 are also likely lower bounds.

## 3.4 Results

### 3.4.1 Full sample fuel price elasticity

The analysis begins by estimating the effects of fuel prices on travel demand. This analysis is performed using an instrumental variables approach where exogeneous shocks to local fuel prices, caused by variation in gasoline content regulations, serve as instruments for local gasoline prices in the first stage.[34] In the second stage, the effect of the exogenous fuel price shocks on traffic sensor vehicle counts is estimated. Therefore, identification of the effects comes from the seasonal variation in gasoline content regulations at the county level.

The results of this estimation strategy on the full sample of counties can be seen in Table 3.1. The first stage results can be seen in Panel A and the second stage results in Panel B. The fixed effects included in the model vary across columns. The model begins with day-of-week and month-of-year fixed effects and ultimately includes county by day-of-week and county by month-of-year fixed effects to be in line with the county-level travel demand elasticities estimated in the next

[34] An IV approach is used because gasoline prices and travel demand are endogenously related. This relationship should bias OLS estimates of the effect of gasoline prices on travel demand towards zero. This is tested in Appendix Table C.2, which presents travel demand elasticities estimated by OLS that are indeed much smaller in magnitude than those estimated using the IV strategy.

subsection. All regressions include weather controls (HDD, CDD, precipitation, snow, and snow depth). All content regulation variables are indicators with a one week linear ramp in and ramp out which provides an adjustment period where gasoline stocks in both automobile fuel tanks and fueling stations are depleted and replenished to ensure gasoline content regulations are affecting fuel markets.[35] Standard errors are clustered at the county level.

Table 3.1: IV Estimates of Travel Demand Elasticities

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Panel A: First Stage* | Dependent Variable: ln(Gasoline Price) | | | |
| RVP 9 | 0.048*** | 0.054*** | 0.048*** | 0.054*** |
| | (0.004) | (0.003) | (0.004) | (0.003) |
| RVP 7.8 | 0.079*** | 0.081*** | 0.079*** | 0.081*** |
| | (0.007) | (0.023) | (0.007) | (0.023) |
| RVP 7 | 0.088*** | 0.075*** | 0.088*** | 0.075*** |
| | (0.011) | (0.005) | (0.011) | (0.005) |
| RFG | 0.052*** | 0.057*** | 0.052*** | 0.057*** |
| | (0.009) | (0.006) | (0.009) | (0.006) |
| F-stat of excluded instruments | 64.65 | 82.43 | 64.62 | 82.41 |
| R-squared | 0.272 | 0.294 | 0.273 | 0.294 |
| *Panel B: Second Stage* | Dependent Variable: ln(Traffic Count) | | | |
| ln(Gasoline Price) | -0.458** | -0.359*** | -0.457** | -0.357*** |
| | (0.198) | (0.042) | (0.197) | (0.042) |
| Observations | 8,593,629 | 8,593,629 | 8,591,140 | 8,591,140 |
| R-squared | 0.929 | 0.934 | 0.939 | 0.943 |
| Weather Controls | Yes | Yes | Yes | Yes |
| Sensor FE | Yes | Yes | Yes | Yes |
| DOW FE | Yes | Yes | No | No |
| MOY FE | Yes | No | Yes | No |
| County*DOW FE | No | No | Yes | Yes |
| County*MOY FE | No | Yes | No | Yes |

*Notes:* Standard errors are clustered at the county level. Weather Controls includes HDD, CDD, precipitation, snow, and snow depth. All gasoline content variables are indicator variables with a one week linear ramp in and ramp out to allow for an adjustment period where gasoline stocks (in both automobile and fueling station) are depleted and replenished. Gasoline prices are adjusted to 2017 Q4 dollars.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

[35] Table C.3 in the Appendix examines the models robustness to choice of ramp in period, finding that the results are not significantly affected by this choice.

The first stage F-statistic for the excluded instruments (RVP 9, RVP 7.8, RVP 7, and RFG) are all in excess of 64. The first stage results suggest that the gasoline content regulations increase fuel prices by approximately 4.8-8.8%. The magnitude of these effects are comparable to previous estimates.[36] The content regulation coefficients are expected to increase in magnitude as the regulations become more stringent. This implies that as the RVP psi decreases, the effect on fuel prices should increase and, because RFG is more stringent than any of the RVP regulations, RFG should have the largest effect. The RVP results are consistent with this hypothesis with RVP 9 having the smallest effect and RVP 7 having the largest. However, the RFG coefficient is similar in magnitude to the RVP 9 regulation. This result is likely a function of content regulation proliferation. RFG is the most common content regulation in the U.S. behind RVP 9, with 30% of all fuel sold in the U.S. meeting RFG regulations.(EPA, 2017) Because such a large portion of the U.S. is mandated to meet RFG regulations, there is little market segmentation in RFG counties. In contrast, RVP 7.8 and RVP 7 requirements are far more infrequent and may lead to less fungible fuel markets and larger price increases than RFG requirements.

The second stage results present the effect of gasoline prices on vehicle counts. As can be seen in Panel B, the elasticities estimated are between -0.357 and -0.458. While these estimates are near the higher end of the range previously estimated in the literature, it has been shown that estimating price elasticities of fuel consumption at low temporal frequency (month or quarter level) may bias estimates toward zero (Levin et al., 2017). Because this paper estimates elasticities at a very high temporal frequency relative to past studies, the larger estimates are not surprising. Further, past literature has largely estimated an average travel demand elasticity for the entire U.S. In contrast, this paper examines only 339 counties with the ultimate goal of estimating county-level travel demand elasticities. The next subsection illustrates that the somewhat nonrepresentative sample

[36]For example, Brown et al. (2008) find that content regulations increase wholesale gasoline prices by 3-11 cents per gallon (average wholesale fuel prices ranged from $0.56-$0.58 for their sample groups), and Chakravorty et al. (2008) finds that implementing RFG or an oxyfuel regulation in an unregulated state increases wholesale fuel prices by 16%.

required to estimate these county elasticities can explain these results as urban areas are shown to have a more elastic travel demand than rural counties.

### 3.4.2 County-specific travel demand elasticities

The analysis now turns to estimating county-specific travel demand elasticities. The estimation strategy is identical to that used in the previous subsection, with the exception that an IV model is estimated for each individual county in the sample. The IV model still includes weather controls and traffic sensor fixed effects as well as month-of-year and day-of-week fixed effects, which can now be interpreted as county by month-of-year and county by day-of-week fixed effects. The entire distribution of estimated travel demand elasticities can be seen in Figure 3.7.[37] This distribution mirrors the results in column 4 of Table 3.1 with the mean elasticity near -0.4. However, this distribution highlights that there is a great deal of heterogeneity in travel demand elasticities across counties in the U.S.[38]

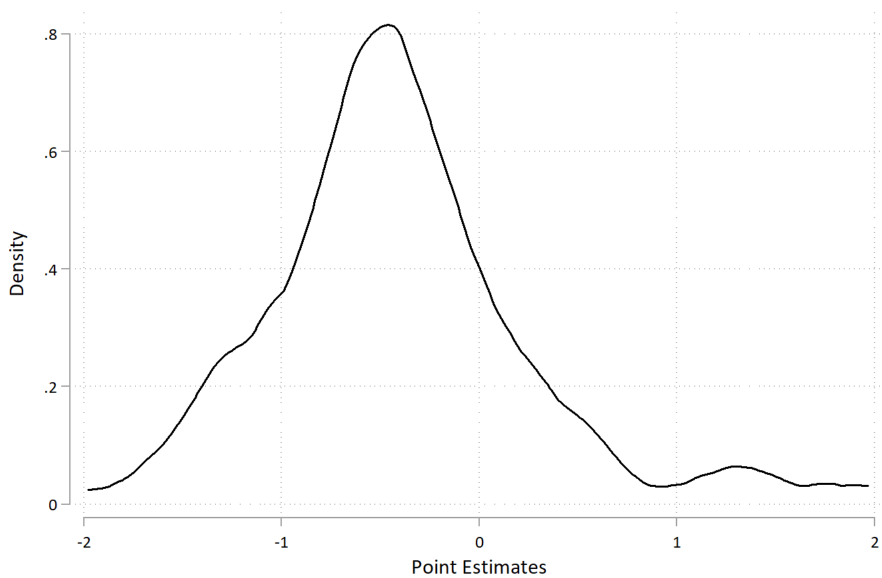
Figure 3.8 further illuminates this heterogeneity by presenting travel demand elasticities for counties in different urban classifications in the top panel, and a corresponding distribution of z-statistics in the bottom panel estimated with standard errors clustered at the traffic sensor level to illustrate statistical validity. In addition, a distribution of F-statistics of the excluded instruments can be seen in Appendix Figure C.4.

As can be seen, large metropolitan counties are more elastic than small metropolitan areas and large fringe metropolitan and medium metropolitan counties fall between these two. These results suggest that the travel demand of consumers in large cities is more responsive to fuel price shocks

[37]Standard errors for all regressions are clustered at the traffic sensor level. The distribution of z-statistics is discussed below.

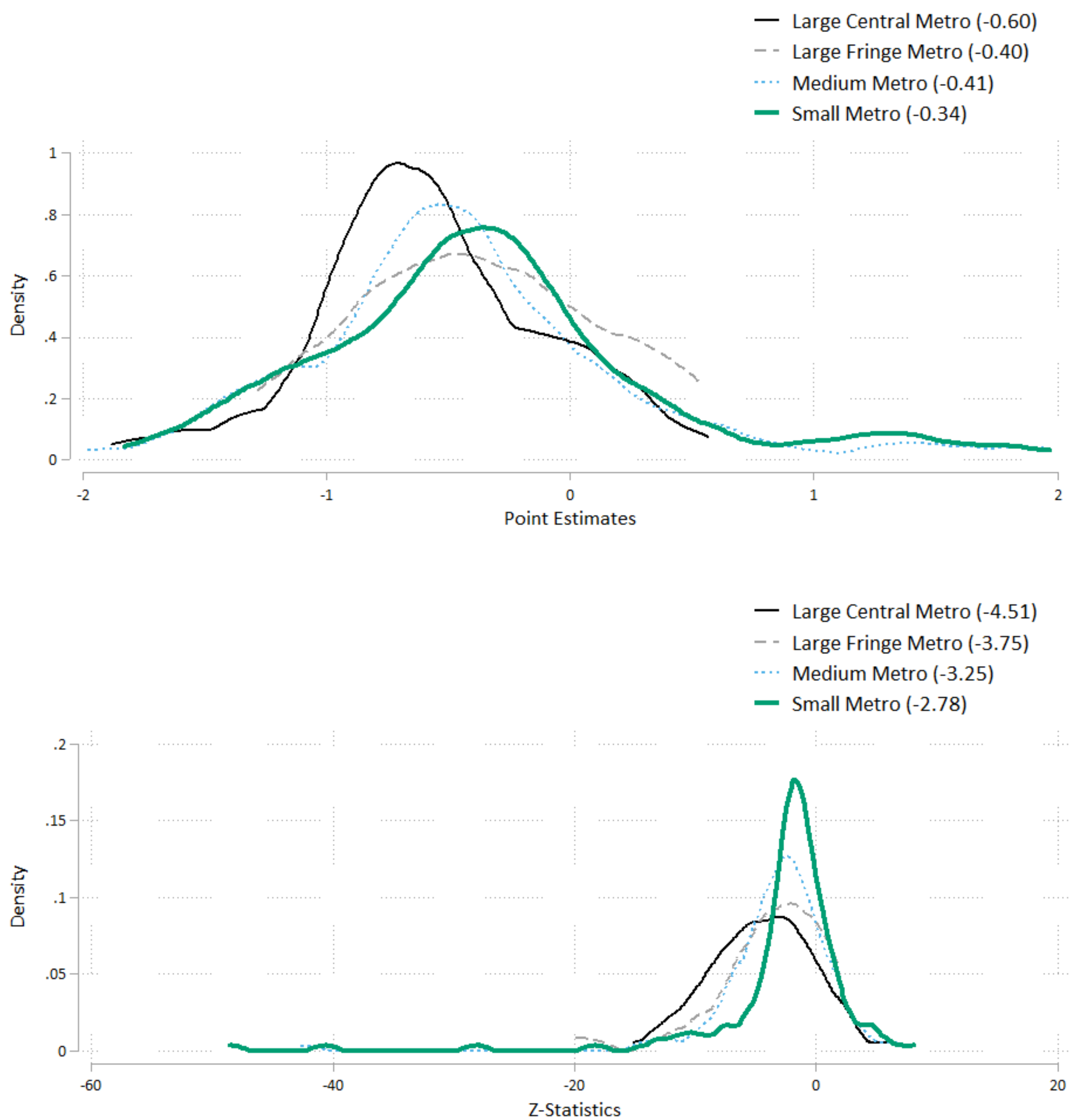
[38]This distribution, as well as the distributions in Figure 3.8 show both negative and positive travel demand elasticities. The majority of the positive travel demand elasticities estimated are statistically insignificant. These elasticities again, for the most part, are estimated for more rural counties which, on average, have 30% less data than the other urbanization levels in the analysis.

Figure 3.7: Distribution of County Travel Demand Elasticities



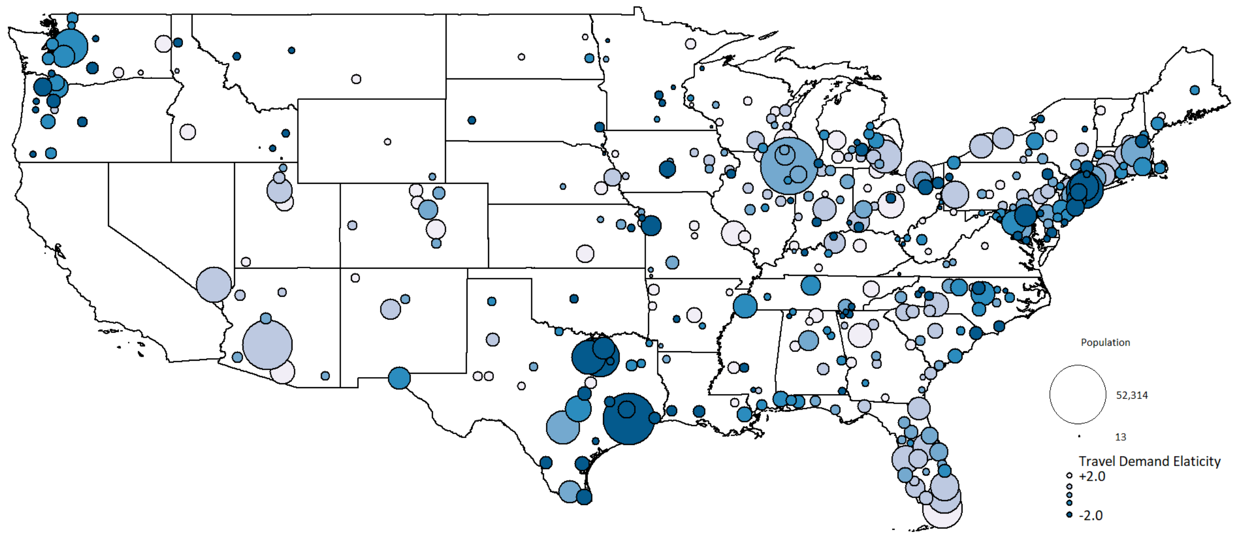

*Notes*: County-level travel demand elasticities were estimated using an IV model which instrumented for gasoline prices with gasoline content regulations. This model was estimated for each county individually controlling for HDD, CDD precipitation, snow fall, and snow depth and included traffic sensor, day-of-week, and month-of-year fixed effects. Counties with extreme outlier elasticities are not reported.

Figure 3.8: Distribution of County Travel Demand Elasticities by Urbanization Level




*Notes*: County-level travel demand elasticities were estimated using an IV model which instrumented for gasoline prices with gasoline content regulations. Z-statistics in the bottom panel are calculated using standard errors clustered at the traffic sensor level. This model was estimated for each county individually controlling for HDD, CDD precipitation, snow fall, and snow depth and included traffic sensor, day-of-week, and month-of-year fixed effects. Urban-Rural classifications were obtained from the National Center for Health Statistics. Urban classification mean values are reported in the legend. Outliers with elasticity absolute values in excess of 2 and one outlier z-statistic (value 144.54) in a small metro area were removed from the distribution to improve readability.

Figure 3.9: Map of County Travel Demand Elasticities and Populations




*Notes*: County populations, measured in thousands, were obtained from the NCHS. County-level travel demand elasticities were estimated using an IV model which instrumented for gasoline prices with gasoline content regulations. This model was estimated for each county individually controlling for HDD, CDD, precipitation, snow fall, and snow depth and included traffic sensor, day-of-week, and month-of-year fixed effects.

than those in rural areas, a finding that is supported by prior literature which found rural drivers have more inelastic demand for gasoline (Spiller et al., 2017). This result is consistent with urban travelers having access to lower cost alternatives to vehicle travel than rural travelers. In other words, urban travelers are more likely to have access to high quality public transit or the ability to use an active transportation mode (walk, bicycle, etc.) than rural travelers who may have no other viable travel mode. This leads to urban areas having a more elastic vehicle travel demand as they have a margin to adjust their behavior along. This hypothesis is consistent with past research which has shown that gasoline tax increases lead consumers to shift mode choices to public transit (Spiller et al., 2012).

As a final piece of visual evidence that travel demand elasticities are larger in urban areas, Figure 3.9 maps county-specific travel demand elasticities and populations. In this figure, darker markers indicate more elastic travel demand and larger markers indicate county population. As can be seen, areas with larger populations tend to have more elastic travel demand.

### 3.4.3 County-specific congestion damages

To determine county-specific congestion damages, traffic sensor data, trip distances, and trip duration times in traffic are combined. This allows for estimation of the effect of increasing the average number of vehicles on a road at a given day-of-week and departure time on average trip speed in a county. For ease of illustration, Figure 3.10 displays the distribution of estimated trip speed elasticities in the top panel and the corresponding distribution of t-statistics in the bottom panel.[39] The mean point estimate values for each urban level can be seen in parenthesis in the legend.

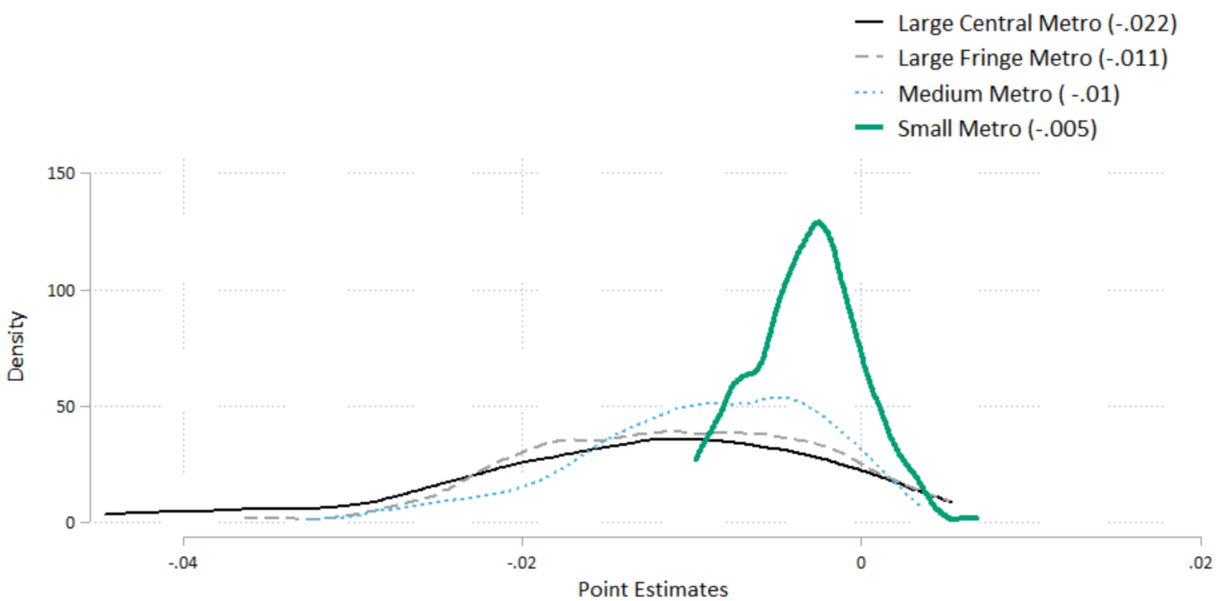
The estimates show a significant amount of heterogeneity. Similar to Figure 3.8, urban classification is strongly associated with the magnitude of the effect. The average effect ranges in size from an approximately 0.022% decrease in average speed when vehicle counts increase by 1% in large central metros down to a 0.005% decrease in average trip speed for a 1% increase in vehicle counts in small metros. Considering the highly nonlinear nature of congestion, and the disparities in travel demand across urbanization levels shown in Figure 3.3, these results are not shocking. However, while intuitive, these are some of the first estimates to examine congestion effects on such a large scale at a fine geographic level.

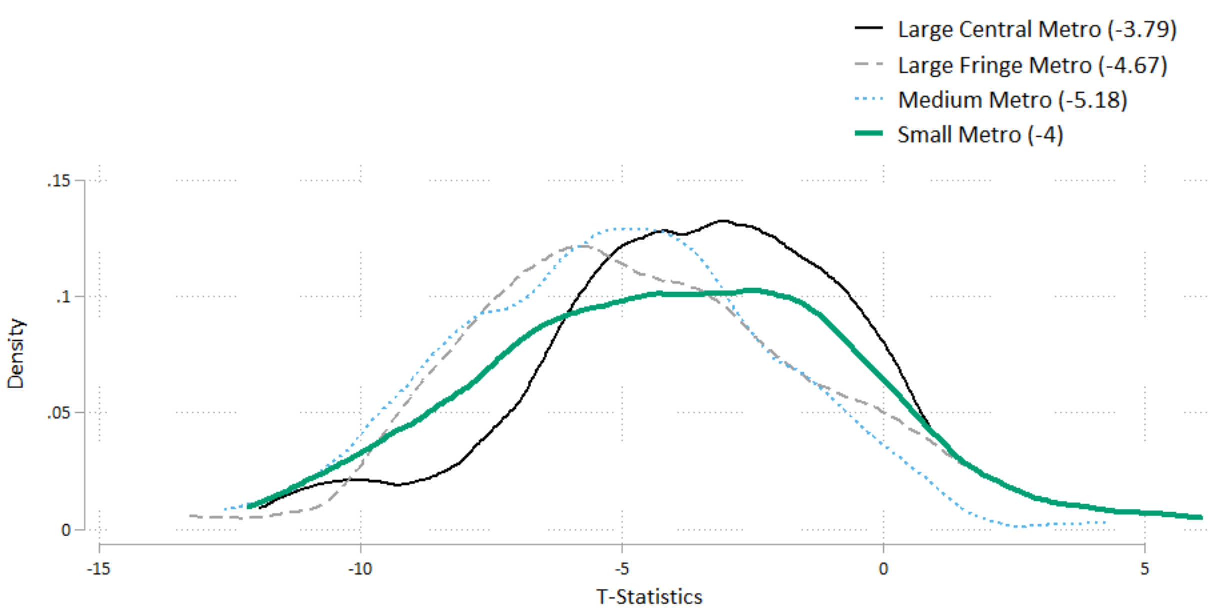
## 3.5 Welfare Implications of County-Specific Fuel Taxes

The welfare effects of county-specific fuel taxes relative to a revenue neutral uniform fuel tax increase levied on all counties can now be estimated. This comparison is accomplished by maximizing the societal welfare function in section 3.2.2, which is dependent on the county-specific travel demand elasticities and congestion damages estimated in section 3.4 as well as pollution damages from Muller and Mendelsohn (2012). This function is maximized by choice of vehicle

[39]Note that the model presented in equation (3) is written in levels, but Figure 3.10 illustrates point estimates from a log-log model. This is done to make the results easier to interpret, but the $\beta$ coefficients used in the welfare calculations are estimated using the levels model. Results from both models are qualitatively similar.

Figure 3.10: Distribution of County Speed Effects by Urbanization Level




*Notes*: County-specific speed effects were estimated using a regression of natural log of average trip speeds on the natural logarithm of average vehicle counts and an origin-destination pair fixed effect. The point estimates can be interpreted as the corresponding change in a average mph when vehicle counts increase by 1%. The bottom panel shows t-statistics calculated using robust standard errors. One outlier large central metro county is not depicted to improve readability.

counts in each county, $V_i \; \forall \; i$, allowing the optimal county-specific fuel tax, subject to the revenue neutrality and positive tax increase constraints, to be determined. The welfare gains from county-specific fuel taxes relative to a uniform fuel tax increase of \$0.10 per gallon levied on all counties can be determined by evaluating the welfare function at the optimal vehicle counts, $V^*$, and the vehicle counts that arise under the uniform \$0.10 fuel tax increase regime, $\bar{V}$. Subtracting societal welfare under the uniform tax regime from societal welfare under the targeted tax regime illustrates the welfare gains from targeted regulation.

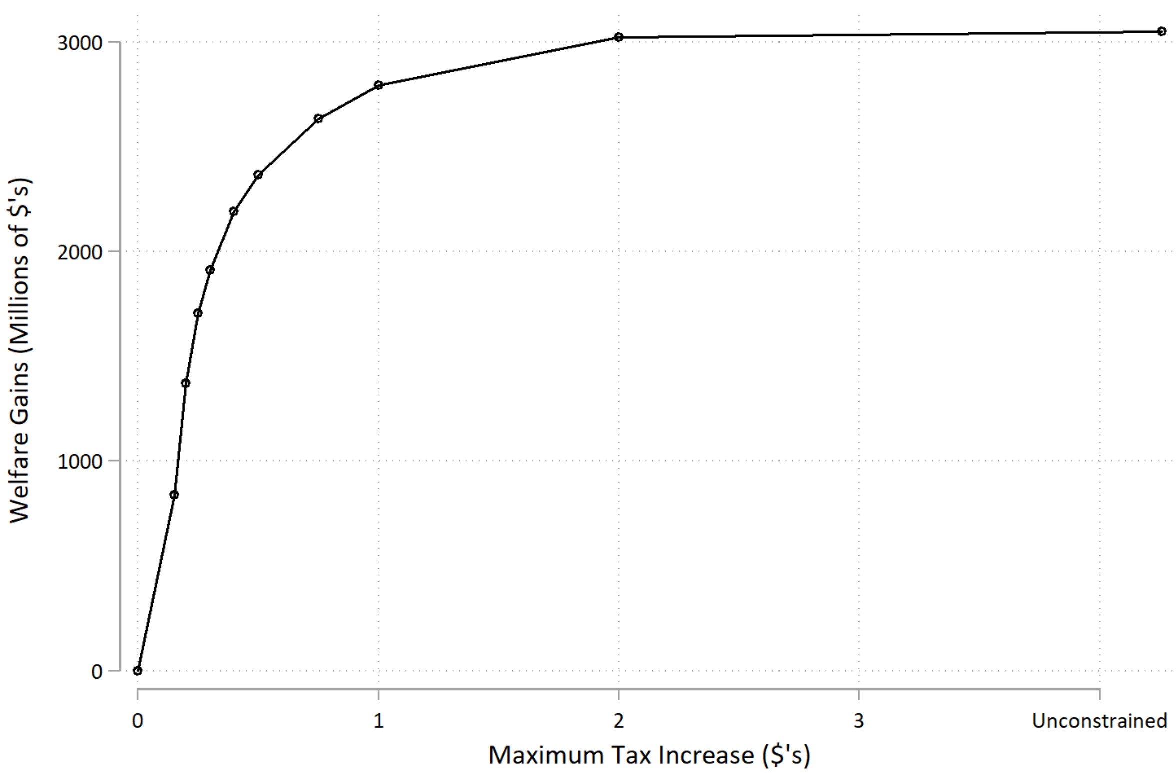
In addition to the constraints requiring revenue neutrality of the tax regimes and tax changes greater than or equal to zero, a maximum allowable fuel tax increase constraint is set. That is to say, a county may not increase their fuel tax by more than \$X. This is imposed to make the simulation more realistic. Political pressures and the unpopularity of gasoline taxes may make large increases in a county's fuel tax unfeasible. By varying the maximum fuel tax increase allowed, the simulation also sheds light on the welfare effects of both modest and large increases in county fuel taxes.

The results of these simulations can be seen in Figure 3.11. The initial point forces the maximum allowable tax increase to be \$0.10, and, as such, is equivalent to the uniform fuel tax increase across all counties, resulting in no welfare gains. The simulation is then run with maximum allowable tax increases between \$0.15 and infinity (unconstrained)[40] with the relevant gain in welfare relative to the uniform tax increase plotted on the y-axis.

The potential welfare gains increase with the maximum allowable tax increase at a decreasing rate. This is because as the maximum tax increase rises, more counties are able to enact their socially optimal fuel tax, beyond which there is no incentive to increase fuel taxes. The results show that county-specific fuel taxes would increase societal welfare between \$1-3 billion dollars annually relative to a \$0.10 per gallon uniform fuel tax increase in the 339 sample counties. This equates to welfare gains between \$7.10 and \$25.84 per capita annually, depending on the maximum

[40]The largest tax increase exhibited under this unconstrained simulation is \$4.26 per gallon, and the mean increase is \$0.053 per gallon.

Figure 3.11: Welfare Gains at Varying Maximum Tax Increases




*Notes*: Welfare gains are reported in millions of dollars. The first point forces the maximum allowable tax increase to be equal to \$0.10, and, as such, is equivalent to the uniform fuel tax across all counties, resulting in no gains in welfare. The simulation is then run with maximum tax increases ranging from \$0.15 to an unconstrained maximum tax increase with the relevant gain in welfare relative to the uniform tax increase plotted on the y-axis. The unconstrained welfare maximization's largest fuel tax increase is \$4.26 per gallon.

allowable fuel tax increase. It is worth noting that a majority of the welfare gains are realized even at relatively low maximum tax increases. Indeed, at a $0.75 maximum tax increase 86% of the potential welfare gains are already achieved.

For additional context, it is possible to determine the total dead weight loss that optimally set second-best fuel taxes can eliminate by maximizing the welfare function without the revenue neutrality constraint.[41] The revenue neutral county-specific and $0.10 uniform fuel tax regimes are found to eliminate 43.9% and 27.2% of this total dead weight loss, respectively. This suggests that, while both regimes improve societal welfare (because gasoline taxes are currently lower than the social optimum in all counties), the county-specific fuel taxes are substantially more efficient.

To illustrate where and to what extent the county-specific fuel taxes are levied, Table 3.2 illustrates tax changes for each urbanization level under maximum allowable tax increase constraints of $0.25, $0.50, and $0.75. As can be seen, large central metros experience the greatest frequency and magnitude of fuel tax increases. As the maximum tax increase constraint rises, the number of counties with tax increases falls, but these areas are taxed at higher levels. This pattern persists as the maximum tax increase constraint is loosened. These results suggest that levying hefty fuel taxes on large central metros, where damages from vehicle travel are high and consumers are relatively more price elastic, improves societal welfare.

[41] Appendix Figures C.5 and C.6 present results where the revenue neutral constraint is removed from the maximization problem. The taxes reported in these figures correspond to the county-specific fuel tax that achieves the socially optimal outcome in each county, i.e. the second-best optimal fuel tax in each county. Taxes are reported for each county in the sample.

Table 3.2: Welfare Simulation

| | Maximum tax increase constraint | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | $0.25 | | | $0.50 | | | $0.75 | | |
| County Urbanization | Max | Interior | Zero | Max | Interior | Zero | Max | Interior | Zero |
| Large Central Metro | 33 | 2 | 1 | 16 | 7 | 13 | 10 | 8 | 18 |
| Large Fringe Metro | 8 | 1 | 22 | 3 | 1 | 26 | 2 | 1 | 28 |
| Medium Metro | 4 | 4 | 104 | 2 | 0 | 110 | 1 | 1 | 110 |
| Small Metro | 1 | 0 | 158 | 1 | 0 | 158 | 1 | 0 | 158 |

*Notes:* Maximum tax increase constraint refers to the maximum tax increase allowed within the simulation. Interior refers to a solution between the maximum and zero. Each suite of tax increases is revenue neutral to a uniform $0.10 fuel tax increase in all counties.

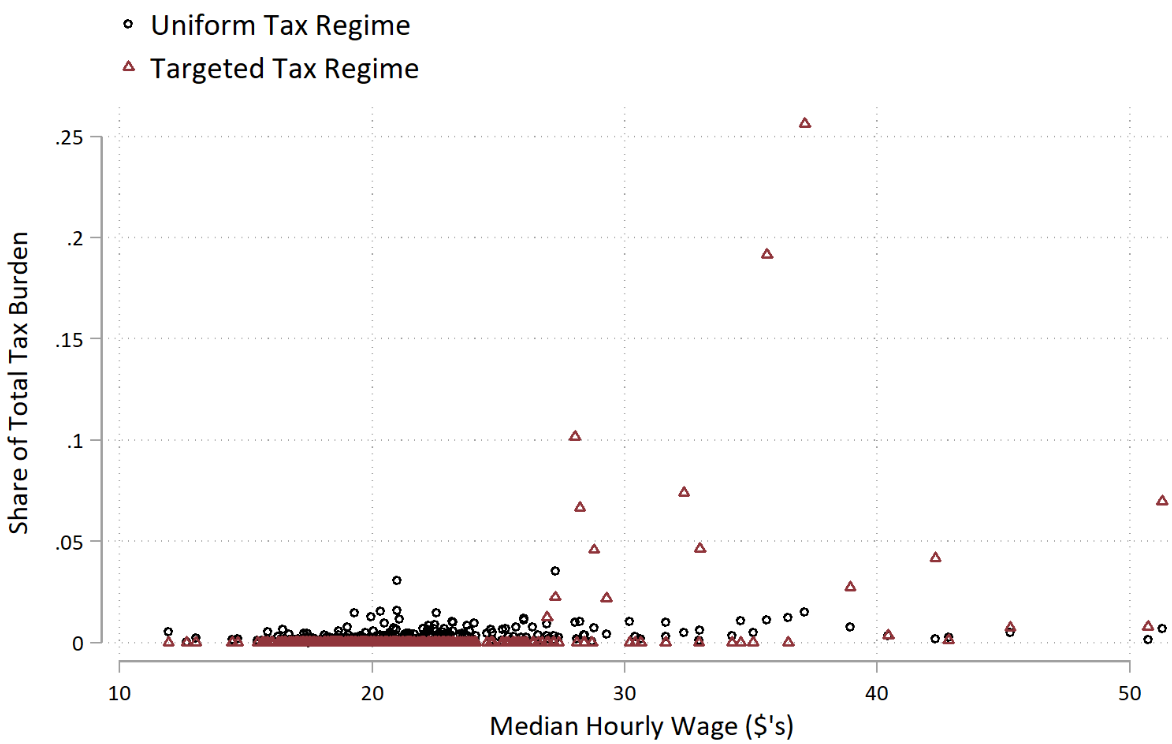
When counties are unconstrained in the amount they may increase their fuel tax, only 5% of the sample counties (which are almost entirely large central metro areas) choose to increase their fuel taxes. This result is interesting for several reasons. First, if there are concerns that consumers may drive from high-tax to low-tax counties to purchase fuel, it is beneficial to have only a small number of high-tax counties to lessen the potential for tax avoidance. However, it should be noted that this behavior is not likely a major concern as time (especially in congested metro areas), fuel, vehicle depreciation, and other costs quickly make this irrational. In addition, gas tax discontinuities already exist at state borders[42], and research has shown differential pass-through of taxes near these borders which further disincentivizes this behavior (Hurtado, 2018).[43] Second, levying taxes on a small subset of counties is unlikely to increase the administrative costs of gasoline tax collection by an amount large enough to offset the gains from the policy change. Finally, these taxes are to be levied on large central metro areas which are frequently more accepting of higher gasoline taxes than their rural neighbors.

The relative regressivity of uniform and targeted fuel tax regimes can also be examined. Figure

[42]While counties are generally smaller than states and may produce more opportunity for tax evasion, this is not always true. For example, many states in the northeast are smaller than counties in the western half of the United States.

[43]Hurtado (2018) finds that less of a gasoline tax's burden falls on consumers in the high-tax state near a border with a low-tax state. This difference diminishes as you travel further from the state boundary though.

Figure 3.12: Tax Burdens by Median Hourly Income




*Notes*: The share of the total tax burden paid by each county under the uniform fuel tax regime and county-specific fuel tax regime are plotted against the county's median hourly wage. The tax burdens depicted were generated without restricting the maximum allowable tax increase in a county. The total tax burden under both regimes is $305 million.

3.12 illustrates the share of the total tax burden that each county faces under both regime types plotted against the county's median hourly wage. These tax burden shares are generated without restricting the maximum allowable tax increase for the targeted regime. This figure shows that, under the uniform tax regime, there is relatively little variance in a county's tax burden across income levels. However, under the targeted tax regime, there is substantial heterogeneity. In this regime, a handful of high-income counties pay significant portions of the increased tax burden and most low-income counties pay 0% of the increased tax burden. This provides suggestive evidence that a more targeted fuel tax regime would be more efficient *and* less regressive than the current uniform fuel tax regime in the U.S.

## 3.6 Conclusion

This paper reveals several empirical facts about the transportation sector. First, travel demand elasticities vary significantly across regions. In particular, travel demand elasticity and urbanization are positively correlated, such that more urban areas have more elastic demand. This finding is consistent with large urban areas having low-cost transportation substitutes (train, bus, walking) relative to rural areas where few high-quality substitutes for vehicle travel exist. Though intuitive, this paper is the first to establish this result for a large portion of the U.S. Second, as expected, congestion damages are higher in urban areas than in rural areas. Again, this result has been hinted at in Couture et al. (2018) and Kim (2018), but this paper applies a novel data set on a wider range of counties in the U.S to solidify this result. Third, the heterogeneity in these parameters and pollution damages across geographies leads to large disparities in the welfare effects of a uniform tax and a revenue neutral suite of county-level taxes. The simulation shows that societal welfare is improved by \$1-3 billion (\$7.10-25.84 per capita) annually by levying hefty fuel taxes on just a few large urban areas. Finally, because the urban areas taxed under the targeted regime are also the highest income counties in the sample, the targeted fuel tax regime is likely to be less regressive than the uniform fuel tax regime.

These findings have important implications for the transportation sector and the regulation of externalities in general. The results could be directly applied to fuel tax policy in the U.S. to improve societal welfare. While fuel tax increases are politically unpopular, the results of this paper suggest taxing wealthy urban areas which generally find gasoline taxes more palatable. Anecdotal evidence of this can be found in France, where in 2018 low-income rural residents took to rioting in response to a proposed uniform fuel tax increase.[44] Further, policies that increase local fuel prices already exist. Indeed, gasoline content regulations, the regulations used to instrument for fuel prices in this analysis, are an example of federal policy that increase local fuel prices based

[44] See https://www.npr.org/2018/12/03/672862353/who-are-frances-yellow-vest-protesters-and-what-do-they or https://www.nytimes.com/2018/12/04/world/europe/france-fuel-tax-yellow-vests.html for more information on the “Yellow Vest” Movement and riots.

on environmental damages. In addition, the county-specific estimates of travel demand elasticities and congestion damages are useful in their own right when calculating optimal fuel taxes, considering rebound effects, or setting other transportation policy ranging from electric vehicle subsidies to congestion tolling. The results highlight the need for policymakers to account for heterogeneity in *both* external damages and demand elasticities when implementing price controls.

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# Appendix A

# Appendix 1: Give me 3': Do minimum distance passing laws reduce bicyclist fatalities?

## A.1 Alternative Fixed-Effects

The body of the paper only shows results from models that use season fixed-effects. However, the results are not sensitive to the use of alternative time fixed-effects decisions. Table A.1 shows results from several specifications using month and year fixed-effects. The MDPL remains positive and statistically insignificant when controls are included, but some variables also become insignificant when year fixed-effects are used. Season level fixed-effects are used in favor of month level fixed-effects in the body of the paper because of the similar estimates and greater statistical power that comes from reducing the number of fixed-effects that must be estimated.

## A.2 Poisson Regressions

Table A.2 shows results from models estimated using Poisson regression as opposed to the negative binomial regressions in the body of the paper. Further, column 2 censors bicyclist fatalities greater than ten in an attempt to reduce the standard erros. Neither of these modeling choices change the main results.

## A.3 Additional Proxies and Trends

Table A.3 shows results from models that include additional proxy and trend variables. The annual number of bicycle sales in the US is added to control for potential national level trends in bicycling. If bicycle sales increase, the number of bicyclists on the roads would be expected to increase. This variable is likely insignificant as it is at the national level and fixed-effects are included in the model.

Columns 2-3 add state legislature party majorities as a final proxy for bicycling participation. These variables enter as dummies that are turned on if either the Democratic or Republican party holds a majority in *both* of a state's legislative bodies. The excluded baseline is a split legislature where neither party holds majorities in both the state house and senate. The number of observations for specification with these variables falls to 14,700. This is because the District of Columbia does not have a state house or senate, and Nebraska has a unicameral legislature, so observations from these areas are dropped.

The validity of these variables as a proxy for bicyclist miles traveled is dubious at best. The results confirm this as they are insignificant predictors of bicyclist fatalities.

## A.4 Specifications with Exposure and Imputed Data

The bicycling commuter data obtained from the ACS only goes back to 2005 while the full panel used in this paper begins in 1990. This means a sizable portion of the data is not being used when controlling for the number of bicyclist commuters in a state. To show that the results from these specifications are robust, pre-2005 data for bicyclist commuting is imputed and re-estimate the models.

The multiple imputations approach is utilized here. To account for the within state correlations, separate imputations are created for each state. A negative binomial model is also used for the imputations as the measure of exposure is the count of bicyclist commuters in a state.

The estimates in Table A.4 are produced using two different lengths of imputation. The first two columns have data imputed for 2000-2004 while the final two columns have data imputed for 1995-2004. All specifications have 10 imputations.

Table A.4 shows that the results would likely hold even if more bicyclist commuting data were available. None of the specifications have a statistically significant MDPL coefficient.

## A.5 "Bicycle Friendly" Rankings and MDPLs

It is possible that states enacting MDPL are using them as a low cost alternative to things like bicycle lanes or educational programs while the control states are investing more heavily into these, potentially more effective, measures. If this is the case MDPLs may reduce bicyclist fatalities, but to a lesser extent than the measures implemented by control states. This causes MDPL to appear ineffective as the model compares the relative changes in bicyclist fatalities between treatment and control groups.

To explore this possibility, the 2015 "bicycle friendly states" scores were obtained from The League of American Bicyclists. This system assigns points to states in several categories that improve bicycling: legislation and enforcement, policies and programs, infrastructure and funding, education and encouragement, and evaluation and planning.[1] If states with MDPLs are less bicycle friendly compared to the control states it would be expected that MDPLs would have a negative effect on score. Results from an ordinary least squares show a positive and significant effect of MDPLs on score, potentially suggesting that states that enact MDPLs are, in general, more apt to improve bicycling conditions than the control states.

While Table A.5 supports the results of the paper, strong conclusions should not be made based on this regression alone for several reasons. As only 2015 scores for states are used, the analysis reduces to a cross sectional model with 50 observations. Also, as score is determined in part by state legislation about bicyclists, the two should have some positive correlation regardless of the other actions states undertake.

## A.6 Leads and Lags

As mentioned in the body of the paper, it is possible that the effect of MDPLs vary over time. In this situation, the MDPL variable might only be statistically significant for certain time spans. To determine if this is the case, specifications with dummy variables that indicate 6 month blocks of time prior to and after a MDPL became effective are estimated in Table A.6.

The leads and lags do not significantly predict bicyclist fatalities, and do not change the significance of the MDPL. Results from specifications using different lead and lag lengths (12, 18, and 24 months) produce similar results. This means that MDPLs more than likely have the same effect over time.

[1] Scores ranged from Alabama with 17.4 to Washington with 66.8 points.

Table A.1: Alternative Fixed-Effects

| Dependent Variable: Count of bicyclist fatalities in a state per month | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| MDPL | 0.0782** | 0.0910** | 0.0385 | 0.0422 |
| | (0.0365) | (0.0360) | (0.0414) | (0.0421) |
| Observations | 15,300 | 15,300 | 15,300 | 15,300 |
| Controls | NO | NO | YES | YES |
| Trend Controls | NO | NO | YES | YES |
| State FE | YES | YES | YES | YES |
| Month FE | YES | NO | YES | NO |
| Year FE | NO | YES | NO | YES |
| $\alpha$ | 0.0595 | 0.1050 | 2.29e-08 | 0.0137 |
| Pseudo $R^2$ | 0.280 | 0.242 | 0.300 | 0.293 |

*Notes:* Robust standard errors clustered by state in parentheses. "Controls" includes: temperature, $(\text{temperature})^2$, precipitation, $(\text{precipitation})^2$, precipitation*temperature interaction, unemployment rate, and *ln*(population). "Trend Controls" includes: pedestrian and driver fatalities.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Including these leads and lags also addresses concerns that high numbers of bicyclist fatalities may cause states to enact MDPLs. The coefficients on the MDPL leads, though statistically insignificant, are mostly negative. This suggests that the decision to enact a MDPL is exogenous to the number of bicyclist fatalities states experience in previous periods. These results mirror the findings of the event study in the main paper.

Table A.2: Poisson Regressions

Dependent Variable: Count of bicyclist fatalities in a state per month

| | Poisson<br>(1) | Censored Poisson<br>(2) |
|---|---|---|
| MDPL | 0.0372<br>(0.0344) | 0.0366<br>(0.0365) |
| Observations | 15,300 | 15,300 |
| Controls | YES | YES |
| Trend Controls | YES | YES |
| Age Controls | YES | YES |
| State FE | YES | YES |
| State RE | NO | NO |
| Seasonal FE | YES | YES |

*Notes:* Robust standard errors clustered by state in parentheses. "Controls" includes: temperature, $(\text{temperature})^2$, precipitation, $(\text{precipitation})^2$, precipitation*temperature interaction, unemployment rate, and *ln*(population). "Trend Controls" includes: time and state trends as well as pedestrian and driver fatalities. The dependent variable has been censored at 10 fatalities per month in column 2.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

## Table A.3: Additional Proxies

| Dependent Variable: Count of bicyclist fatalities in a state per month | | | |
|---|---|---|---|
| | Fixed-effects | | Random-effects |
| | (1) | (2) | (3) |
| MDPL | 0.0365 | 0.0479 | 0.0061 |
| | (0.0361) | (0.0323) | (0.0349) |
| Percent Male | 1.1439*** | 1.1487*** | 1.1394*** |
| | (0.4282) | (0.3977) | (0.3868) |
| Median Income | 0.0059 | 0.0052 | 0.0017 |
| | (0.0037) | (0.0035) | (0.0032) |
| National Bicycle Sales | 0.0287 | 0.0275 | -0.0180 |
| | (0.0292) | (0.0291) | (0.0294) |
| Democratic Majority | | 0.0003 | -0.0129 |
| | | (0.0285) | (0.0289) |
| Republican Majority | | -0.0202 | 0.0021 |
| | | (0.0376) | (0.0286) |
| Observations | 15,300 | 14,700 | 14,700 |
| Controls | YES | YES | YES |
| Age Controls | YES | YES | YES |
| State FE | YES | YES | NO |
| State RE | NO | NO | YES |
| Seasonal FE | YES | YES | YES |
| $\alpha$ | 0.0045 | 0.0028 | - |
| Pseudo $R^2$ | 0.373 | 0.373 | - |

*Notes:* Robust standard errors clustered by state in parentheses. "Controls" includes: temperature, $(\text{temperature})^2$, precipitation, $(\text{precipitation})^2$, precipitation*temperature interaction, unemployment rate, and *ln*(population). "Trend Controls" includes: pedestrian and driver fatalities.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

## Table A.4: Specifications with Exposure and Imputed Data

| Dependent Variable: Count of bicyclist fatalities in a state per month | | | | |
|---|---|---|---|---|
| | 2000-2014 | | 1995-2014 | |
| | (1) | (2) | (3) | (4) |
| MDPL | 0.0864 | 0.0906 | 0.0526 | 0.0150 |
| | (0.0756) | (0.0706) | (0.0837) | (0.0822) |
| Percent Male | | -1.0705 | | -1.2882 |
| | | (0.8242) | | (0.8562) |
| Median Income | | 0.0006 | | 0.0030 |
| | | (0.0052) | | (0.0053) |
| Observations | 9,180 | 9,180 | 12,240 | 12,240 |
| Controls | YES | YES | YES | YES |
| Trend Controls | YES | YES | YES | YES |
| Age Controls | NO | YES | NO | YES |
| State FE | YES | YES | YES | YES |
| Seasonal FE | YES | YES | YES | YES |
| Imputations | 10 | 10 | 10 | 10 |

*Notes:* Robust standard errors clustered by state in parentheses. "Controls" includes: temperature, $(\text{temperature})^2$, precipitation, $(\text{precipitation})^2$, precipitation*temperature interaction, and unemployment rate. "Trend Controls" includes: state trends as well as pedestrian and driver fatalities.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

## Table A.5: Effect of MDPL on Bicycle Friendly State Score

| | Dependent Variable: Bicycle Friendly State Score |
|---|---|
| | OLS |
| MDPL | 6.6169** |
| | (3.1734) |
| Temperature | 19.3041* |
| | (10.2866) |
| Precipitation | 6.6892 |
| | (6.0474) |
| Temperature$^2$ | -0.6628** |
| | (0.3248) |
| Precipitation$^2$ | 0.5270* |
| | (0.2900) |
| Precip*Temp | -0.1795 |
| | (0.1367) |
| *ln*(Population) | 5.9814*** |
| | (1.8186) |
| Observations | 50 |
| $R^2$ | 0.399 |

*Notes:* Standard errors in parentheses.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table A.6: Specifications with MDPL Lags and Leads

| Dependent Variable: Count of bicyclist fatalities in a state per month | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| MDPL | 0.0477 | 0.0458 | 0.0503 |
| | (0.0347) | (0.0354) | (0.0377) |
| Lead 24-19 Months | | | -0.0442 |
| | | | (0.1723) |
| Lead 18-13 Months | | | -0.4450 |
| | | | (0.3986) |
| Lead 12-7 Months | | 0.0175 | -0.0095 |
| | | (0.1394) | (0.1354) |
| Lead 6-1 Months | -0.0233 | -0.0228 | -0.0519 |
| | (0.1559) | (0.1593) | (0.1650) |
| Lag 1-6 Months | -0.0819 | -0.0800 | -0.0879 |
| | (0.0663) | (0.0698) | (0.0769) |
| Lag 7-12 Months | | 0.0184 | 0.0077 |
| | | (0.0546) | (0.0616) |
| Lag 13-18 Months | | | -0.0915 |
| | | | (0.0783) |
| Lag 19-24 Months | | | -0.0344 |
| | | | (0.0753) |
| Observations | 15,300 | 15,300 | 15,300 |
| Controls | YES | YES | YES |
| Trend Controls | YES | YES | YES |
| Age Controls | YES | YES | YES |
| State FE | YES | YES | YES |
| Seasonal FE | YES | YES | YES |
| $\alpha$ | 0.0028 | 0.0028 | 0.0030 |
| Pseudo $R^2$ | 0.375 | 0.375 | 0.375 |

*Notes:* Robust standard errors clustered by state in parentheses. "Controls" includes: temperature, $(\text{temperature})^2$, precipitation, $(\text{precipitation})^2$, precipitation*temperature interaction, *ln*(population), unemployment rate, percent male, median income, *ln*(population), and age controls. "Trend Controls" includes: state trends and pedestrian and driver

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

# Appendix B

# Appendix 2: Taxed to death? The effects of diesel taxes on freight truck collisions

## B.1 Imputation technique

The imputation strategy uses imputations by chained equations to impute missing observations using readings from other sensors within the same state. The imputation process is as follows. First, the data must be cleaned to only include trucks of interest and only sensors that appear to be in working order. As stated before, all but 5-axle truck observations with weight between 15,000-120,000 pounds are removed from the data set. Then, sensors are defined as active or inactive for each hour of the sample with active sensors having at least one truck observation during the hour. WIM sensors that are inactive for more than two thirds of their state's sample are removed from the data set. Because these sensors report so infrequently, they are assumed to have equipment issues that may result in erroneous observations. States are then defined as active or inactive. For a state to be active at least two sensors must be active within the time period.

Following these steps, the actual imputation is performed. The number of hourly truck observations, average gross weight, and average speed per active sensor in the sample is first computed. Then, using only data from other sensors within the same state, data for sensors that are inactive within a state and hour is imputed. This imputation is performed using imputations by chained equations–an imputation technique that uses switching regressions. This technique begins by assigning a sensors mean value to each missing value. In the second step, for each sensor, a regression is estimated using data from all other sensors as the independent variable. In the third step, predicted values from this regression are then used to replace the observations that were initially missing. Steps 2 and 3 are then repeated a through a number of cycles. A more detailed description of this process can be found in Azur et al. (2011)

## B.2 Main result robustness

This section provides additional robustness checks for the analysis of the effects of truck counts and cargo weight on the quantity of collisions.

Table B.1 includes week-of-sample fixed effects in lieu of the month-of-sample fixed effects used in Table 2.2. The inclusion of these more granular time fixed effects will better control for macroeconomic trends such as fuel prices or the demand for trucking. The results are qualitatively similar to those presented in the body of the text, suggesting that these unobservable trends are not driving the results.

Tables B.2 and B.3 present results that utilize Poisson and negative binomial models, respectively. These tables should alleviate concerns that estimates in Table 2.2 may be biased due to the log transformation of the dependent variables. Again, the results are qualitatively similar to those in the body of the text, suggesting that the choice of model is not driving the results.

Table B.1: Effects of truck count and cargo weight on quantity of collisions: Week FE

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Dependent Variable:* | ln(Total) | ln(Tow) | ln(Injury) | ln(Fatal) |
| ln(Truck Count) | 0.171** | 0.059 | 0.296*** | 0.215*** |
| | (0.067) | (0.088) | (0.085) | (0.081) |
| ln(Cargo Weight) | 0.241*** | 0.252*** | 0.661*** | -0.112 |
| | (0.077) | (0.088) | (0.118) | (0.128) |
| ln(Passenger-Vehicle Count) | 0.105*** | 0.073** | 0.192*** | 0.026 |
| | (0.029) | (0.035) | (0.044) | (0.044) |
| Observations | 5,821 | 5,821 | 5,821 | 5,821 |
| R-squared | 0.939 | 0.915 | 0.895 | 0.545 |
| Weather Controls | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| Week FE | Yes | Yes | Yes | Yes |

*Notes:* Standard errors are clustered at the week level. All regressions include a constant. Weather Controls includes HDD, CDD, precipitation, snow, and snow depth. Truck count, cargo weight, and passenger-vehicle count are all state-week average measures. Dependent variables are the natural logarithms of state-week counts of collisions in different severity rankings plus one. Total is the sum of all other collision rankings.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table B.2: Poisson estimates of effects of truck count and cargo weight on quantity of collisions

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Dependent Variable:* | Total | Tow | Injury | Fatal |
| Truck Count | 0.018*** | 0.019*** | 0.016*** | 0.018*** |
| | (0.002) | (0.002) | (0.002) | (0.004) |
| Cargo Weight | 6.070*** | 2.585 | 15.694*** | -9.838 |
| | (2.059) | (2.253) | (2.897) | (7.901) |
| ln(Passenger-Vehicle Count) | 0.135*** | 0.122*** | 0.172*** | 0.073 |
| | (0.028) | (0.030) | (0.034) | (0.099) |
| Observations | 5,821 | 5,821 | 5,821 | 5,821 |
| Weather Controls | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |

*Notes:* Estimates are produced using a Poisson regression model. Standard errors are clustered at the week level. All regressions include a constant. Weather Controls includes HDD, CDD, precipitation, snow, and snow depth. Truck count, cargo weight, speed and passenger-vehicle count are all state-week average measures. Dependent variables are the state-week counts of collisions in different severity rankings. Total is the sum of all other collision rankings.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table B.3: Negative binomial estimates of effects of truck count and cargo weight on quantity of collisions

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Dependent Variable:* | Total | Tow | Injury | Fatal |
| Truck Count | 0.020*** | 0.021*** | 0.019*** | 0.019*** |
| | (0.002) | (0.003) | (0.002) | (0.005) |
| Cargo Weight | 8.992*** | 6.998*** | 19.174*** | -9.558 |
| | (2.005) | (2.211) | (2.927) | (7.826) |
| ln(Passenger-Vehicle Count) | 0.105*** | 0.073*** | 0.182*** | 0.073 |
| | (0.026) | (0.028) | (0.037) | (0.095) |
| Observations | 5,821 | 5,821 | 5,821 | 5,821 |
| Weather Controls | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |

*Notes:* Estimates are produced using a negative binomial regression model. Standard errors are clustered at the week level. All regressions include a constant. Weather Controls includes HDD, CDD, precipitation, snow, and snow depth. Truck count, cargo weight, speed and passenger-vehicle count are all state-week average measures. Dependent variables are the state-week counts of collisions in different severity rankings. Total is the sum of all other collision rankings.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

# Appendix C

# Appendix 3: Correcting heterogeneous externalities: Evidence from local fuel price controls

## C.1 Additional Descriptive Statistics

Table C.1 provides descriptive statistics for the variables used in the empirical analysis. Further descriptions of these variables and their sources can be found in Section 3.3.1.

## C.2 OLS Estimates of Travel Demand Elasticity

Table C.2 provides ordinary least squares (OLS) estimates of travel demand elasticities. The elasticities estimated using OLS are much smaller in magnitude than those estimated using the IV strategy in Table 3.1. As expected, this suggests that OLS estimates are indeed biased towards zero, and that the IV strategy undertaken in Section 3.3 is necessary to recover unbiased travel demand elasticities.

## C.3 Additional Elasticity Robustness Checks

Table C.3 provides first and second stage results from IV models estimating the effect of gasoline prices on traffic counts. These models are identical to those shown in Table 3.1 except the gasoline content regulations now include a two week linear phase in and phase out period instead of a one week phase in and phase out. The results are qualitatively similar to those in Table 3.1, suggesting that the results are not particularly sensitive to the choice of regulation phase in period.

## C.4 Sensor Data Visualization

Figures C.1 and C.2 illustrate average hourly traffic counts and fuel prices for a sensor in a rural and urban area respectively. Figure C.3 illustrates a sensor that experiences intermittent outages. From these three figures it can be seen that traffic counts are highly seasonal and vary significantly within a short period of time. This high variance can be explained as a day-of-week effect as traffic patterns vary considerably between weekdays and weekends. There also appears to be a strong correlation between fuel prices and traffic counts. The visible relationship between traffic counts and local fuel prices suggests that the instrumental variables approach used in the empirical section is necessary to properly identify the effects of fuel price shocks on traffic counts.

## C.5 F-Statistics for County Travel Demand Elasticities

Figure C.4 depicts the Kleibergen-Paap Walk F-Statistics for the excluded instruments of the regressions depicted in Figure 3.8. The distribution is truncated at 5,000 which removes 24 counties with larger F-statistics. In total 33 of the counties have F-statistics with a value less than 10.

## C.6 Optimal Fuel Taxes

Figure C.5 depicts the distribution of county optimal fuel taxes while Figure C.6 depicts each county's optimal fuel tax defined as their average per gallon fuel tax during the sample plus the optimal fuel tax change calculated by maximizing the unconstrained welfare function. Several outlier results with optimal fuel taxes in excess of $3 were removed from the sample. The average optimal fuel tax in the sample counties is $0.606 per gallon, more than double the average fuel tax actually levied on these counties of $0.25.

Table C.1: Descriptive Statistics

| Variable | Observations | Mean | Std. Dev |
|---|---|---|---|
| Vehicle Count | 8,593,629 | 524.930 | 718.446 |
| Gasoline Price | 8,593,629 | 2.898 | 0.711 |
| RVP 9 | 8,593,629 | 0.307 | 0.461 |
| RVP 7.8 | 8,593,629 | 0.016 | 0.126 |
| RVP 7 | 8,593,629 | 0.003 | 0.051 |
| RFG | 8,593,629 | 0.065 | 0.247 |
| HDD | 8,593,629 | 11.863 | 14.640 |
| CDD | 8,593,629 | 4.113 | 6.418 |
| Precipitation | 8,593,629 | 29.176 | 72.831 |
| Snow | 8,593,629 | 1.998 | 12.065 |
| Snow Depth | 8,593,629 | 33.556 | 116.608 |
| | | | |
| Trip Distance | 104,448 | 24.324 | 21.248 |
| Trip Duration in Traffic | 104,448 | 0.600 | 0.506 |
| Avg. Trip Speed | 104,448 | 40.162 | 8.743 |

*Notes:* Vehicle counts are average hourly vehicle counts at a sensor across each sample day. Precipitation, snow, and snow depth are measured in millimeters. HDD denotes heating degree days and CDD denotes cooling degree days. Both HDD and CDD have a base of 65 degrees Fahrenheit. Gasoline prices are deflated to 2017 Q4 dollars. Trip distances are measured in miles. Trip duration in traffic is measured in hours. Average trip speed is measured in miles per hour.

Table C.2: OLS Estimates of Travel Demand Elasticities

| | Dependent Variable: ln(Traffic Count) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ln(Gasoline Price) | -0.120*** | -0.128*** | -0.120*** | -0.128*** |
| | (0.008) | (0.008) | (0.008) | (0.008) |
| Observations | 8,593,629 | 8,593,629 | 8,593,629 | 8,593,629 |
| R-squared | 0.933 | 0.935 | 0.935 | 0.938 |
| Weather Controls | Yes | Yes | Yes | Yes |
| Sensor FE | Yes | Yes | Yes | Yes |
| DOW FE | Yes | Yes | No | No |
| MOY FE | Yes | N | Yes | No |
| County*DOW FE | No | No | Yes | Yes |
| County*MOY FE | No | Yes | No | Yes |

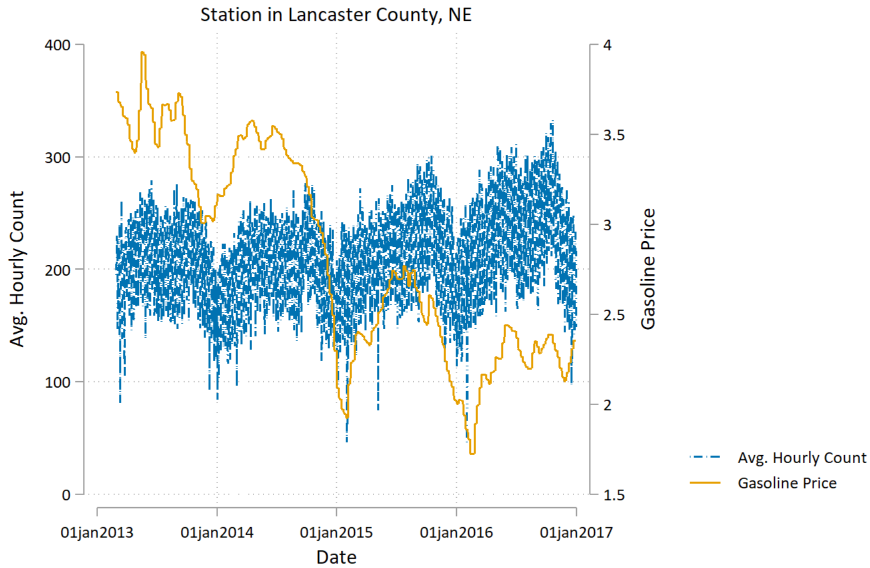
*Notes:* Standard errors are clustered at the county level. Weather Controls includes HDD, CDD, precipitation, snow, and snow depth. Gasoline prices are adjusted to 2017 Q4 dollars. Each regression was performed using OLS.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table C.3: IV Estimates of Travel Demand Elasticities—Two Week Regulation Phase In

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Panel A: First Stage* | Dependent Variable: ln(Gasoline Price) | | | |
| RVP 9 | 0.053*** | 0.062*** | 0.058*** | 0.067*** |
| | (0.004) | (0.003) | (0.004) | (0.004) |
| RVP 7.8 | 0.086*** | 0.092*** | 0.091*** | 0.100*** |
| | (0.007) | (0.021) | (0.007) | (0.022) |
| RVP 7 | 0.095*** | 0.101*** | 0.101*** | 0.107*** |
| | (0.010) | (0.008) | (0.010) | (0.007) |
| RFG | 0.056*** | 0.061*** | 0.063*** | 0.075*** |
| | (0.010) | (0.010) | (0.011) | (0.011) |
| F-stat of excluded instruments | 71.69 | 91.64 | 71.65 | 91.69 |
| R-squared | 0.273 | 0.294 | 0.200 | 0.225 |
| *Panel B: Second Stage* | Dependent Variable: ln(Traffic Count) | | | |
| ln(Gasoline Price) | -0.482** | -0.320*** | -0.481** | -0.321*** |
| | (0.203) | (0.042) | (0.203) | (0.042) |
| Observations | 8,613,475 | 8,613,475 | 8,613,529 | 8,613,529 |
| R-squared | 0.273 | 0.294 | 0.200 | 0.225 |
| Weather Controls | Yes | Yes | Yes | Yes |
| Sensor FE | Yes | Yes | Yes | Yes |
| DOW FE | Yes | Yes | No | No |
| MOY FE | Yes | N | Yes | No |
| County*DOW FE | No | No | Yes | Yes |
| County*MOY FE | No | Yes | No | Yes |

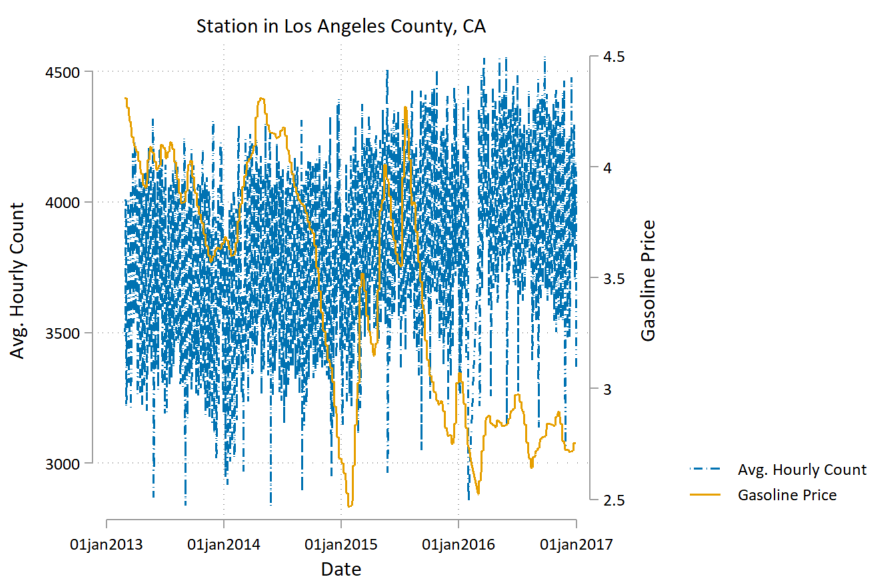
*Notes:* Standard errors are clustered at the county level. Weather Controls includes HDD, CDD, precipitation, snow, and snow depth. All gasoline content variables are indicator variables with a two week linear ramp in and ramp out to allow for an adjustment period where gasoline stocks (in both automobile and fueling station) are depleted and replenished. Gasoline prices are adjusted to 2017 Q4 dollars.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Figure C.1: Rural Sensor Data



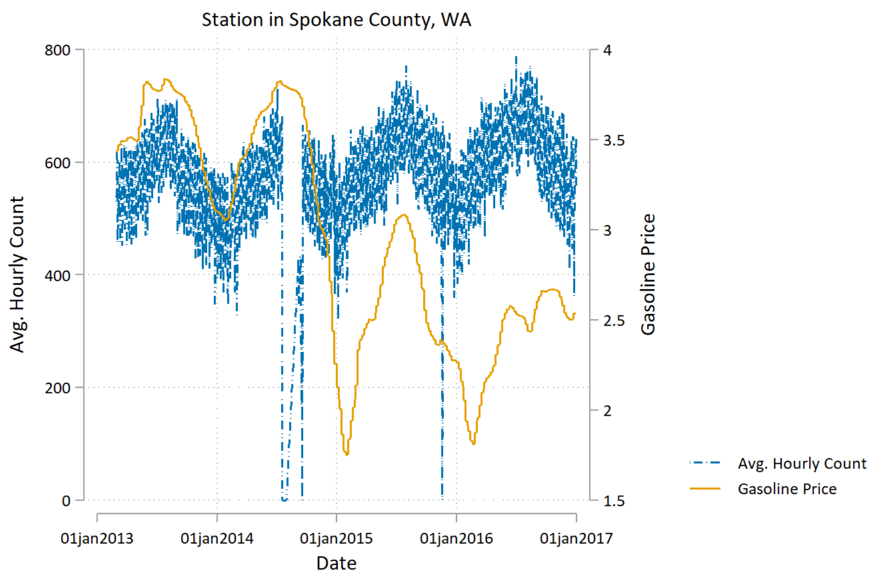

*Notes*: The vehicle count data reports the daily average hourly count. Only days that reported data for the full 24 hour period are shown. Gasoline prices are county week averages.

Figure C.2: Urban Sensor Data



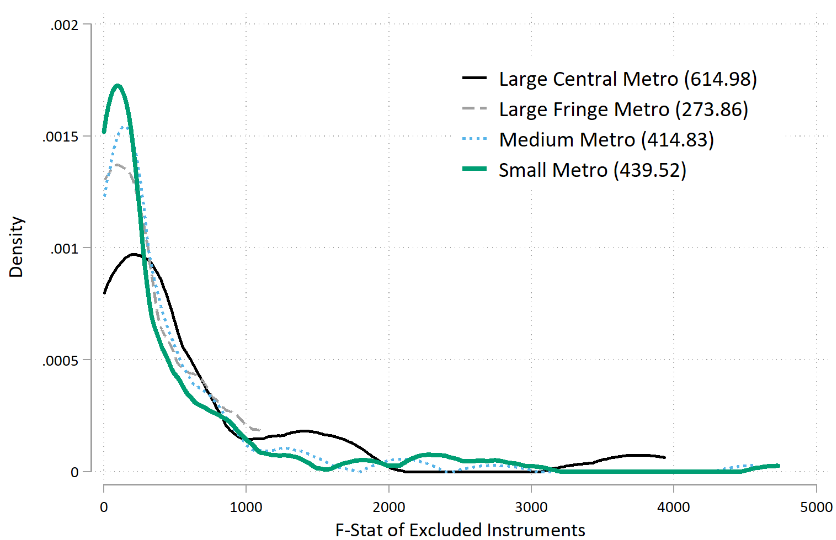

*Notes*: The vehicle count data reports the daily average hourly count. Only days that reported data for the full 24 hour period are shown. Gasoline prices are county week averages.

Figure C.3: Sensor With Outages Data



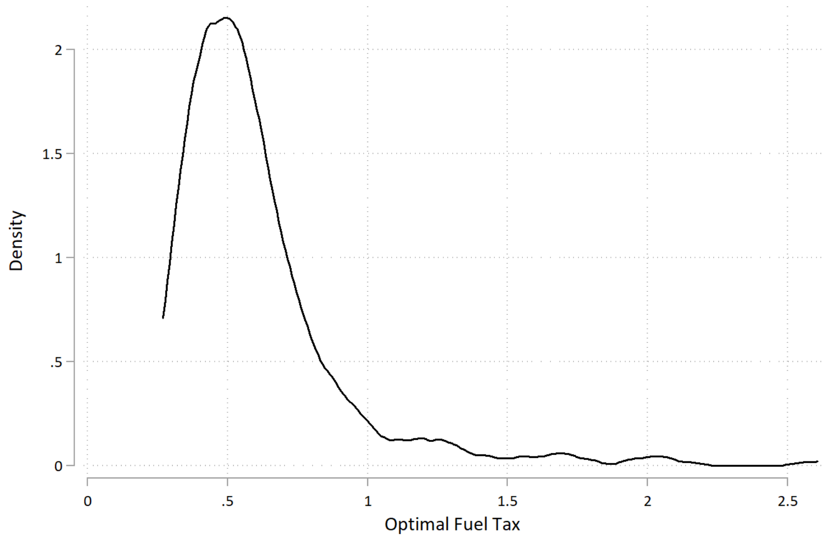

*Notes*: The vehicle count data reports the daily average hourly count. Only days that reported data for the full 24 hour period are shown. Gasoline prices are county week averages.

Figure C.4: F-statistics of Excluded Instruments



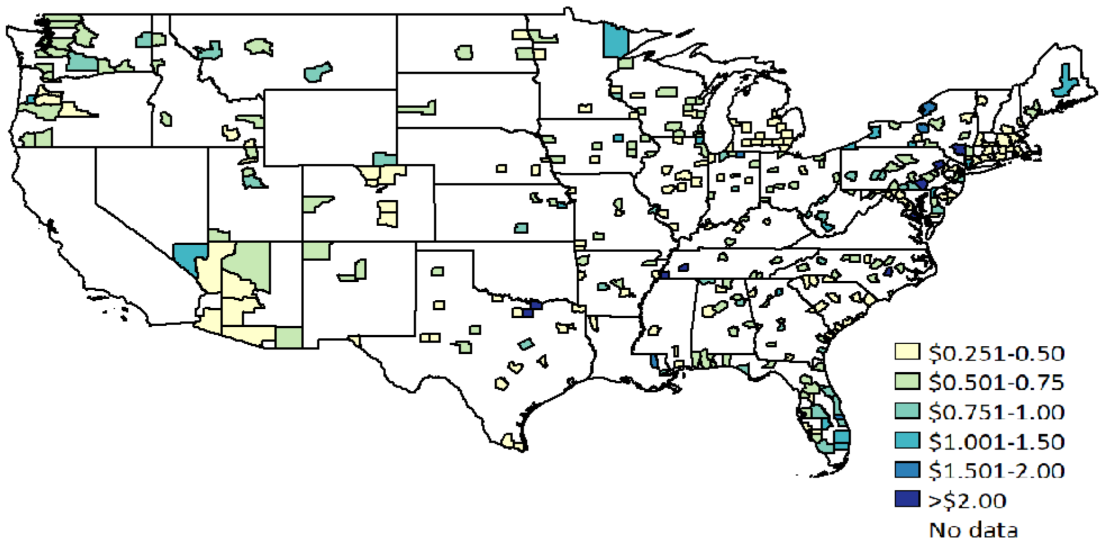

*Notes*: F-statistics are Kleibergen-Paap Wald F-statistics. The distribution is truncated at 5,000 for readability, removing 24 counties with larger F-statistics. In total, 33 counties have F-statistics less than 10.

Figure C.5: Optimal Per Gallon Fuel Taxes




*Notes*: This figure depicts each county's optimal fuel tax defined as their current per gallon fuel tax plus the optimal fuel tax change calculated by maximizing the unconstrained welfare function. Fuel taxes are in $'s. Six outlier counties with optimal fuel taxes in excess of $3 were removed.

Figure C.6: Map of Optimal Per Gallon Fuel Taxes




*Notes*: This map depicts each county's optimal fuel tax defined as their current per gallon fuel tax plus the optimal fuel tax change calculated by maximizing the unconstrained welfare function. Fuel taxes are in $'s.